5-7-1866

Annual Report of the Board of Regents of the Smithsonian Institution, showing the operations, expenditures, and condition of the institution for the year 1865.

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ANNUAL REPORT

OF

THE BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING THE

OPERATIONS, EXPENDITURES, AND CONDITION OF THE INSTITUTION FOR THE YEAR 1865.

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1866.

IN THE SENATE OF THE UNITED STATES, *May* 9, 1866.

Resolved, That five thousand additional copies of the report of the Board of Regents of the Smithsonian Institution for the year ending the 30th of June, 1865, be printed—two thousand for the use of the Smithsonian Institution and three thousand for the use of the Senate. ***Provided***, That the aggregate number of pages contained in said report shall not exceed four hundred and fifty pages, without wood-cuts or plates, except those furnished by the Institution.

Attest:

J. W. FORNEY, *Secretary.*

IN THE HOUSE OF REPRESENTATIVES, *June* 8, 1866.

On motion of Mr. Laflin, from the Committee on Printing:

Resolved, That five thousand extra copies of the last report of the Smithsonian Institute be printed—two thousand for the Institution and three thousand for the use of the members of this house.

Attest:

EDWARD McPHERSON, *Clerk.*

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.

May 7, 1866.—Laid on the table and ordered to be printed.

SMITHSONIAN INSTITUTION,
Washington, May 7, 1866.

SIR: 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 Smithsonian Institution for the year 1865.

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

JOSEPH HENRY,
Secretary Smithsonian Institution.

Hon. SCHUYLER 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, 1866, AND THE PROCEEDINGS OF THE
BOARD UP TO MAY, 1866.

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 1865.
2. Report of the Executive Committee, giving a general statement of the Smithsonian fund, and also an account of the expenditures for the year 1865.
3. Report of the Building Committee.
4. Proceedings of the Board of Regents up to May, 1866.
5. Appendix.

Respectfully submitted:

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

OFFICERS OF THE SMITHSONIAN INSTITUTION.

MAY, 1866.

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.
W. W. SEATON, Treasurer.
WILLIAM J. RHEES, Chief Clerk.

A. D. BACHE,
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.
A. D. BACHE, citizen of Washington.
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.
WM. DENNISON, Postmaster General.
J. SPEED, 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.

JAS. HARLAN, 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 be made.

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

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

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

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

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

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

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

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

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

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

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

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

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

DETAILS OF THE PLAN FOR DIFFUSING KNOWLEDGE.

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

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

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

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

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

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

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

I. PHYSICAL CLASS.

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

II. MORAL AND POLITICAL CLASS.

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

III. LITERATURE AND THE FINE ARTS.

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

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

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

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*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.

To the Board of Regents:

GENTLEMEN: The principal object of the annual report of the Secretary is to present to the Board of Regents, at the beginning of their session, such an account of the events and operations of the previous year as may serve as the basis of their deliberation, as well as furnish the materials for a connected history of the Institution. Besides this, however, it is desirable that each report should contain such brief expositions as may tend to keep the ever-changing public informed as to the true character of the establishment and of the results it is intended to produce. The importance of these repetitions will be evident when it is recollected that these reports follow each other after a considerable interval of time, are in great part distributed to different persons, and that now, after an interval of little less than twenty years since the first one was published, but few individuals can obtain access to an entire set. Indeed, it is often a matter of surprise to meet so many intelligent persons, even in the city of Washington, who are entirely ignorant of the terms of the bequest on which the Institution is founded, and of the plan which has been adopted to execute the trust thereby devolved upon the government. It can, therefore, scarcely be too often repeated that the Institution is not, as our foreign correspondents often suppose, an association of learned men similar to the scientific societies of Europe and America; that it is not a university for the education of youth, nor an agency for the diffusion of useful knowledge among the people of the United States, but primarily a foundation for enlarging the boundaries of science by stimulating and assisting the researches of original inquirers, wherever found, and for gratuitously diffusing the results of such researches wherever they may conduce to the intellectual or material interests of men.

The general plan adopted for the realization of these benevolent purposes, and steadily pursued from the beginning, has been repeatedly explained in the successive reports, and might, indeed, be gathered from almost any one of them by an attentive consideration of the account given of the operations of each year. In the last report a brief sketch was presented of all that had been accomplished in the way of advancing science through the agency of the Smithsonian bequest, and in the present report it is purposed to continue this historical account to the close of the year just passed, accompanied with such other particulars as may be needed for better illustration, and such suggestions as may seem necessary in regard to the policy of the Institution.

The most important event of 1865 was the destruction of a part of the building and its contents by the fire of January 24. This event must continue to form an epoch in the history of the Institution; and though it cannot but be considered a most serious disaster, it may yet lead to changes of importance in the correction of tendencies which might ultimately have absorbed the annual income and neutralized the more liberal policy which has thus far been pursued. In view, therefore, of the character of the event, as well as the continuity of the history, it is deemed expedient, before proceeding with an account of the operations of the year, to repeat briefly the facts connected with the origin and results of the fire.

It may be well, however, for the better information of those not acquainted with the Smithsonian building, to premise in regard to it the following particulars: It consists of a main edifice two hundred feet long and fifty wide, with two large wings and two connecting ranges, having in all an extreme length, in an east and west direction, of four hundred and fifty feet. In front and rear of the middle portion are projections, terminated by high towers, two on the north and one on the south side; moreover, on each corner of the middle building is a smaller tower, and also one on each of the two wings.

The whole of the first story of the main building, in a single room, is devoted to the museum; the upper story, in three apartments, was assigned to the lecture room, the gallery of art, and the cabinet of apparatus. The west wing is entirely appropriated to the library; the east wing to the residence of the Secretary and a storeroom for publications and specimens of natural history. The east connecting range contains the laboratory and office rooms; the west range is an extension of the museum. In the large towers were the Regents' room, the offices of the Secretary, storerooms, and workshop.

Though the original plan was much admired for its architectural effect, it was soon found that, in relation to the means at the disposal of the building committee, it was too expensive to admit in its construction of the exclusive use of fire-proof materials; hence, while the exterior was to be constructed of cut freestone, it was concluded to finish the interior in wood and stucco. Fortunately this plan, which was carried out in regard to the wings, the connecting ranges, and the towers, was abandoned before the completion of the main building. After the exterior of this, including the roof, had been finished, and the framing of the interior was in place, the latter suddenly gave way and was precipitated into the cellar—a mass of broken timber. The attention of the Regents having been called by this accident to the insecurity of the wood-work, they directed that the further progress of the building should be stopped until means could be accumulated for finishing the remainder of the edifice in a more stable manner and with fire-proof materials. In accordance with this direction, after an interval of several years, the construction was recommenced under the direction of Captain (now General) B. S. Alexander, of the Engineer corps, and the whole of the main building, except the inside of the towers and the framing of the roof, which had previously been completed, was finished in a substantial manner in iron and brick work. The architect advised the removal of the roof, but as this would have swelled the cost of the building still further beyond the estimate and the means at command, and as the covering was of slate, the framing under it was thought to be in no danger from fire. This, however, was destined to be the part on which the first attack of the element was to be made. Through a mistake in some workmen, the pipe of a stove which had been temporarily used in one of the upper rooms was introduced through the wall into a furring space resembling a flue, but which discharged the heated air from the combustion into the loft immediately under the roof, instead of into the air, through the true chimney. The rafters were set on fire, and before the burning was discovered the entire wood work under the covering was in flames. The progress of the fire was so rapid, that but few of the contents of the upper rooms could be removed before the roof fell in. The flames soon extended to the large towers, and as these acted as high chimneys, they greatly increased the intensity of the combustion. The conflagration was only stayed by the incombustible materials of the main building. Had the original plan of constructing the interior of the edifice in wood and plaster been fully carried out, the whole structure would have been destroyed, and the valuable library and rich collections of specimens of natural history entirely lost.

The aperture which deceived the workmen was probably made by those who originally plastered the building. It occupied a middle point between two windows, and from its position would naturally lead to the inference that it was designed to conduct the products of combustion directly into the chimney, from which it was only separated by the thickness of a single brick. For what reason it had not been placed in the middle between the two windows is unknown. It is remembered that some ten years previous to the fire this opening was, during several weeks, used for the insertion of a stove pipe, without suspicion of accident at the time; but in the interval the wood had undergone a process of drying which rendered it more combustible.

Constantly impressed with the fact that the interior of the two wings and the connecting ranges were constructed of combustible material, I have always felt great anxiety on account of the liability to conflagration of these parts of the building. The rest of the edifice, with the exception of the interior of the towers, was supposed to be secure from injury of this nature. A night watch was constantly kept, barrels and buckets filled with water were placed at suitable points, and strict rules were adopted prohibiting the carrying of exposed lights, as well as the practice of smoking, in any part of the edifice. That these precautions were unavailing has been seen; the fire having been communicated at a point where danger was least suspected, and in a manner which rendered its occurrence sooner or later almost inevitable.

The weather at the time was extremely cold, and before the engines could be brought into operation the whole of the roof was in flames. Commencing at the west end of the centre building, the flames were driven by the wind, which blew from that direction, eastwardly, and fortunately away from the library, in the west wing. The destruction of the roof of the main building involved that of the contents of the rooms immediately beneath it, and also those of the three principal towers adjacent. In the former were the Indian portrait gallery, the lecture room, and the apparatus room. The first of these contained the large collection of paintings by Mr. Stanley and a series of Indian portraits belonging to the government. The lecture room was constructed on acoustic and optical principles, and not only answered perfectly the ends for which it was immediately intended, but had served also as a model for lecture rooms in various parts of the country. The apparatus room contained the principal part of the articles presented by the late Dr. Robert Hare, and a large number of instruments of recent construction, intended both for illustration and original research.

The losses in the south tower were, first, the contents of the Regents' room, including the personal effects of Smithson; second, those of a *large room above it, in which were stored the private library of Rev.* Dr. Johns, of Virginia, and the public library of Beaufort, S. C., deposited there at the request of Hon. Mr. Stanton, for preservation until the end of the war; and third, in the attic, a large collection of public documents and complete sets of the Smithsonian Reports, intended for distribution. The effects of Smithson had but little intrinsic value, and were chiefly prized as mementoes of the founder of the Institution. They consisted of a number of articles of chemical and physical apparatus, such as were used by him in his perambulatory excursions, two small cabinets of minute specimens of minerals, a silver-plated dinner service, and a trunk filled with manuscripts. The portrait of Smithson while a student at Oxford, a medallion likeness of him in bronze, his library, consisting of 150 volumes, and a small painting were saved. The manuscripts consisted principally of notes on scraps of paper, intended apparently for alphabetical arrangement in a common-place book, after the manner of a philosophical dictionary.

The losses in the north towers were the contents of the offices of the Secretary, including the records and copies of the correspondence of the Institution, the wood-cuts to illustrate the publications, the steel plates of an expensive memoir, several boxes of stereotype plates, a large number of manuscripts of the Secretary on scientific subjects, four memoirs accepted for publication, about a hundred volumes of valuable books from the library, used for constant and immediate reference; a large number of copies of the Smithsonian Reports and duplicate documents; the contents of the workshop, consisting of a lathe, forge, a full set of tools, and an assortment of hardware and materials for the construction and repair of apparatus; and of the upper room of the highest tower, including the clock-work of an anemometer for recording the direction and force of the wind. Not only was this instrument itself lost, but all the records which had been obtained by the use of it for the last seven years. Fortunately, nearly all the other meteorological records, which were in a lower room, were saved.

The Indian portraits, as far as they were the likenesses of particular individuals, in most cases can never be reproduced, but we are gratified to learn that the extensive collection of Mr. Catlin, of a similar character, has been purchased in Europe by Mr. Harrison, of Philadelphia, and will be rendered accessible to the student of ethnology. Besides this, there are in existence, particularly in Canada, other portraits, sufficient in number and variety fully to illustrate the characteristics of the race. At the same time the loss has fallen very heavily upon Mr. Stanley, the painter and owner of this collection. It was the result of the labor of many years among the Indians; it constituted the pride, as it has been the crowning effort, of his life, and he ardently desired that it might be transmitted to posterity as a monument of his enterprise and industry. The hope is entertained that the government will see fit to give him an order to paint a picture for the Capitol, in which the principal figures of this collection and the characteristics of the Indian race may be portrayed.

The apparatus presented by Dr. Hare was interesting on account of its association with the history of the advance of science in this country. The collection contained most of the articles invented by the donor, and which are described in the scientific journals of the first half of the present century. Among the chemical implements were those used by that distinguished chemist, in procuring for the first time, without the aid of galvanism, calcium, the metallic basis of lime. A number of the articles of apparatus presented by Dr. Hare, though injured by the fire, may be repaired, and I have taken measures for their restoration.

Among the articles of historic interest which were lost is the lens used by Priestley for the evolution of oxygen from the oxide of mercury, and by means of which the first distinct recognition of this elementary substance was effected. It had been presented to the Institution by the nephew of the celebrated philosopher, as was also the apparatus employed by Priestley in his experiments on bodies in condensed atmospheres. The latter was but slightly injured, and can readily be repaired. The other articles of apparatus may be replaced at an expense of about ten thousand dollars.

The most irreparable loss was that of the records, consisting of the official, scientific, and miscellaneous correspondence, embracing 35,000 pages of copied letters which had been sent, at least 30,000 of which were the composition of the Secretary, and 50,000 pages of letters received by the Institution; the receipts for publications and specimens; reports on various subjects which have been referred to the Institution; the records of experiments instituted by the Secretary for the government; four manuscripts of original investigations, which had been adopted by the Institution for publication; a large number of papers and scientific notes of the Secretary; a series of diaries, memorandum and account books. Fortunately, however, a detailed history of the general operations of the Institution is preserved in the printed reports; and a large amount of correspondence connected with natural history and meteorology was saved.

Since the occurrence of the fire all the operations have been carried on in the lower story of the east wing of the building, (the upper part still continuing to be the residence of the Secretary,) and in the several rooms of the adjoining east range. Connected with an office in the latter, the lower story of the tower attached to the southeast corner of the main building has been converted into a fire-proof vault, in which all the valuable papers and records are constantly kept, except for the usually short time they are required for consultation. To insure the wakefulness and fidelity of the watchmen, we have introduced the use of an instrument called a 'detector,' which records the number and the times of his visits to the several parts of the building. For this instrument, which has rendered good service during the past year, we are indebted to the liberality of its inventor, Mr. J. E. Bauerk, of Boston, who, in consideration of the loss which the Institution has sustained by fire, kindly presented it free of charge.

A circumstantial account is given by the building committee of what has been done toward the reconstruction of the edifice. From this it will be seen that the plan adopted contemplates not merely the repair of the damage by the fire, but the restoration of the several parts in fire-proof materials, and with such alterations in the division of the interior space as will better adapt it to the uses of the Institution.

The plans have been prepared and the work superintended by Mr. Adolph Cluss, an architect who was warmly recommended by the mayor of Washington as having been successful in designing and erecting the public school-houses of the city as well as a number of churches and other buildings. These plans have been critically examined and, in some cases, modified by the chairman of the building committee, General Delafield, who, by his knowledge and experience in the line of engineering, has rendered the Institution valuable service.

No appropriation has yet been made by Congress to aid in the restoration of the building. Considering, however, the large amount of government property intrusted to the care of the Institution, it can scarcely be doubted that in a normal condition of the national finances an appropriation for such purpose would have been readily granted.

In consideration of the extraordinary outlay required for the reconstruction of the building, an effort has been made to reduce as much as possible the miscellaneous expenses, and to engage in no enterprise that is not absolutely necessary to the continuance of the general operations. So many articles, however, of furniture and stores of hardware and stationery were to be replaced that we have not been able to reduce the expenditures to as low a point as we could wish; yet it will be seen that they fall somewhat below those of the preceding year.

A reference to the report of the Executive Committee will show that the following is the present condition of the fund:

First. The whole amount of money originally derived from the bequest of Smithson is still in the treasury of the United States, bearing interest at six per cent., paid semi-annually, and yielding $30,910 yearly.

Second. Seventy-five thousand dollars of an extra fund are in bonds of the State of Indiana, at five per cent. interest, also paid semi-annually, yielding $3,750.

Third. Fifty-three thousand five hundred dollars of the same fund are in bonds of the State of Virginia, twelve thousand in those of Tennessee, and five hundred in those of Georgia, from which nothing has been derived since the commencement of the war.

The southern State stocks have increased during the year in marketable value, though no interest has been derived from them.

The interest on the original fund for the past year has been paid by the Secretary of the Treasury in coin, under the advice of the Solicitor of the department, who, having investigated the subject, decided that in accordance with the usages of the government the Institution was entitled thus to be paid.

At the end of last year there was a balance in the hands of the treasurer of unexpended interest of $29,484 08, which, with the income on the original bequest and premium on coin, made a disposable fund of $84,956 37; of this sum $39,121 77 have been expended on the building, and $32,115 97 for the maintenance of the establishment and for carrying on all the operations of the Institution, leaving a balance of $13,718 63 to be further applied to the building.

In view of the great expenditure of the government on account of the war, the Institution did not at first claim, as it justly might have done, the payment of the annual income of the bequest in specie; but after the great loss sustained by the fire, the necessity could not be avoided of calling the attention of the Secretary of the Treasury to this subject. The claim, after consideration, was allowed, so far as related to the current income, but the question relative to the difference between the payment of the interest which had previously accrued in currency or in coin has not been settled by the department. That the Institution is entitled to the benefit resulting from this difference was the unanimous opinion of the Board of Regents at their last session, and among them of Chief Justice Chase. This learned jurist has since stated, that when Secretary of the Treasury he had in several cases ordered the currency received by parties who after investigation were found justly entitled to be paid in coin, to be again returned into the treasury, and had directed payment to be made to them in specie nominally of the same amount.

It has been mentioned in the two preceding reports, that a part of the original bequest had been left in England as the principal of an annuity payable to the mother of Smithson's nephew. The annuitant having died, a power of attorney signed by Abraham Lincoln, President of the United States, was sent to Fladgate, Clarke & Finch, solicitors, in England, authorizing them to collect the money and pay it to the order of the Secretary of the Smithsonian Institution. The proceeds from this, deducting the expenses of collection, were £5,262 0*s*. 3*d*., which were temporarily deposited with George Peabody & Co., who not only transacted the business without charge, but allowed four per cent. interest on the money while it remained in their hands. The total amount of this residuary legacy received by the Institution, including the interest, £153 19*s*. 4*d*., was $26,210 63 in gold, which being sold at the current premium, (about 207,) yielded $54,165 38 in United States currency. This sum was invested in government bonds, bearing interest at 7 3-10 per cent., and deposited for safe-keeping with General Spinner, the Treasurer of the United States.

It was at first supposed that the interest on this fund could be immediately applied to the uses of the Institution, but upon a critical examination of the enactments of Congress in regard to the bequest, it was found by the Solicitor of the Treasury that the act of Congress of 1846 appropriated only that portion of the money which was then in the treasury, and made no provision for the disposition of the residuary legacy. The Secretary of the Institution was therefore called upon by Mr. Fessenden, Secretary of the Treasury, to deposit this fund to his order in the treasury of the United States until Congress should authorize the appropriation of it to the maintenance or use of the Institution.

The cost of the restoration of the building in fire-proof materials without changing the external appearance has, as formerly stated, been far greater than was anticipated. Whether the portion of the work yet to be executed will much exceed in cost that which has already been completed, will depend upon the price of materials and of labor. The Institution may in time be able to finish this work without encroaching on its present capital, provided the Secretary of the Treasury shall recognize the inadequacy of the payments of interest which for three years were made in the depreciated currency of the time. If this allowance be not made, and no assistance be received from Congress, then, in order to secure the building and its contents from injury by the weather, the Institution will be obliged to sacrifice a portion of its extra fund, and to the extent of this forever diminish its power to "increase and diffuse knowledge among men."

I have always been opposed to asking appropriations from Congress for the maintenance of the Institution, believing that the government is called upon to do nothing further in its behalf than carefully to guard the original bequest, and see that it is faithfully applied in an efficient manner to the purposes intended by the donor. But the government, after having voluntarily accepted the trust, is bound in good faith to carry out the intentions of the testator, and to make up for any encroachments upon the funds which may have resulted from improvident or defective legislation. From abundant experience of at least the last fifteen years, it has been shown that the cost and maintenance of a building of the character which has been erected, so far from being necessary to the most efficient realization of the intentions of the founder, have been a constant source of extraneous expense, and have absorbed a large amount of money which ought to have been added to the active capital; and the question may now be asked with propriety, whether, since this building was erected by its own agents, in conformity with a law of Congress, an appropriation should not be made to restore it in fire-proof materials, and to devote it in whole or in part to purposes of the government. A single wing of the edifice is sufficient to carry on all the essential operations of the Institution, and the whole remaining part of the building might be applied to the national collections, which have been greatly enriched at the expense of the Institution, to the accommodation of the Army Medical Museum, or to the uses of the Agricultural Department.

Publications.—During the past year the general operations of the Institution have been continued with unabated energy, although, on account of the increased cost of paper and printing, the number of copies of publications distributed has not been so great as in some previous years. The papers, however, which have been printed are stereotyped, and all our domestic institutions will be supplied as soon as a reduction of prices or an increase of the income of the Institution shall warrant the expenditure for this purpose. The following is a list of the works in *quarto* published by the Smithsonian Institution in 1865:

Discussion of the Magnetic and Meteorological Observations made at Girard College Observatory, Philadelphia, in 1840, 1841, 1842, 1843, 1844, and 1845. Fourth section comprising Parts X, XI, and XII. Dip and total force. By A. D. Bache, LL. D. Pp. 44. (Published January, 1865.)

Palæontology of the Upper Missouri: A report upon collections made principally by the expeditions under command of Lieutenant G. K. Warren, U. S. topographical engineers, in 1855 and 1856. Invertebrates. By F. B. Meek and F. V. Hayden, M. D. Part I. Pp. 156 and five plates. (Published April, 1865.)

Cretaceous Reptiles of the United States. By Joseph Leidy, M.D. Pp, 142 and twenty plates. (Published May, 1865.)

These memoirs, which have all been described in previous reports, combined with section third of Professor Bache's discussion and Dr. Draper's article on the construction of a silvered-glass telescope, published in 1864, formed volume XIV of the "Contributions to Knowledge," (490 pp., 25 plates,) which was distributed in part during the past year.

Of other quarto works in press in 1865, the following are nearly ready for publication, and will constitute part of volume XV of the "Contributions:"

Astronomical, magnetic, tidal, and meteorological observations within the arctic circle, by Isaac I. Hayes, M. D., reduced and discussed by C. A. Schott, of the U. S. Coast Survey. 250 pp., with two charts and a number of diagrams.

Investigation of the Orbit of Neptune, with general tables of its motion. By Simon Newcomb, of the U. S. Naval Observatory. Pp. 116.

The memoirs actually completed and issued in the year therefore embraced 342 pp. and 25 plates, which, added to the 366 pp. stereotyped but not distributed, in the same period, makes an aggregate of 708 pp. and 25 plates in quarto as the record of the year.

The following works in *octavo* were also published or printed in 1865:

Instructions relative to Ethnology and Philology of America. Appendix A—physical characters of the Indian races; Appendix B—numerical systems. By George Gibbs. Pp. 18. (Published May, 1865.)

Review of American Birds in the collection of the Smithsonian Institution. By S. F. Baird. Pp. 143–320 or 172 pp. (Published May, 1865.)

Researches upon the Hydrobiinæ and allied forms, chiefly made upon materials in the museum of the Smithsonian Institution. By Dr. W. Stimpson. Pp. 64. (Published August, 1865.)

In addition to the above, continuations of the following works were printed in 1865:

Catalogue of transactions of societies and scientific journals in the library of the Smithsonian Institution. 200 pp.

Continuation of Parts II and III, by Mr. Binney, of the Synopsis of the Land and Fresh Water Shells of the United States. About 60 pp.

These make a total of 514 pages, and, including the Annual Report of 1864, (450 pages,) an aggregate of 964 octavo pages; and of all classes of publications, 1,672 pages.

Smithsonian Contributions.—The first paper which has been received for publication in the quarto series, and which has not yet been described, is that on the planet Neptune, by Professor Newcomb, of the National Observatory.

It will be recollected by those who are familiar with the history of the operations of the Institution, that shortly after the discovery of the planet Neptune, the first ephemeris of, or, in other words, a table for indicating, its position in the heavens at any time during the year was prepared and published at the expense of the Smithsonian fund. This ephemeris was computed from an orbit based on a remarkable discovery of Professor Sears C. Walker, then of the Washington Observatory. Beginning with observations of the movement of the planet during a period of four months, Mr. Walker traced its path among the stars through its whole revolution of 166 years, and was thus enabled to carry its position backward until it fell among a cluster of stars, each of which had been accurately mapped by the celebrated Lalande, near the close of the last century. From a critical scrutiny of these stars, Mr. Walker was led to conclude that one of this cluster observed by Lalande on the night of the 10th May, 1795, was the planet Neptune. Availing himself of this discovery, Mr. Walker had now a series of observations, embracing not merely a few months of the movement of the planet, but its entire motion during a period of fifty years. From these data he was enabled to deduce a perfect elliptical orbit, or one which the body would describe were there no other planets in the system, and from this to calculate an ephemeris.

The motion of Neptune was further investigated by another of our countrymen, Professor Peirce, of Cambridge. He calculated the action of all the other planets on Neptune, and obtained results which enabled Mr. Walker to correct his elliptical orbit, and to compare the calculated places of the planet with its actual position in the heavens. This led to a further correction of the orbit, and to a more perfect table of calculated places. The Ephemeris was published annually for several years, until the establishment of the American Nautical Almanac, when, in accordance with the general policy of the Institution, it was transferred to that work.

During the last nineteen years, which have elapsed since the investigation of Professors Peirce and Walker relative to the orbit of this planet, a series of accurate observations have been made upon its motion at the principal observatories of the world, and these have now *been discussed by Professor S. Newcomb, of the Washington Observatory*, a young mathematician in whose progress and advancement this establishment has taken great interest.

The objects of Mr. Newcomb's investigations, as stated in the introduction of his memoir, are as follows:

1. To determine the elements of the orbit of Neptune with as much exactness as a series of observations extending through an arc of forty degrees will admit of.

2. To inquire whether the mass of Uranus can be deduced from the motions of Neptune.

3. To inquire whether these motions indicate the existence of an extra neptunian planet, or throw any light on the question of the existence of such a planet.

4. To construct general tables and formulæ, by which the place of Neptune may be found at any time, and more particularly between the years 1,600 and 2,000.

The work is divided into five chapters.

The first is introductory, giving a brief review of the previous labors of astronomers in perfecting the theory of Neptune. The only approximately correct theory which had been published is shown to be that of Professor Sears C. Walker and Professor Benjamin Peirce, whose labors were given to the world in the second and third volumes of the Smithsonian Contributions to Knowledge, and in the first volume of the Proceedings of the American Academy. The elements of this theory were, however, far from correct, owing to the insufficiency of the observations which had been made when the theory was constructed. On account of the extremely slow motion of Neptune, the errors of the observations would be multiplied several hundred times in the final elements of the orbit.

The second chapter contains the computation of the perturbations of Neptune arising from the attractions of the other planets. This was the most difficult and laborious part of the work, the difficulty being greatly increased by the circumstance that the attraction of Uranus causes the orbit to pass through a regular change of form in a cycle of about 4,300 years.

The third chapter gives a discussion of the meridian observations of Neptune, made at the observatories of Greenwich, Paris, Washington, Cambridge, Hamburg, and Albany.

In the fourth chapter, the positions of the planet given by the observations are compared with a provisional theory, for the purpose of correcting the elements, after which, supposing Neptune to be attracted only by the known planets, its motions, as observed during the nineteen years since its discovery, are compared with the results of the theory of gravitation, and the greatest difference between the theoretical and observed longitudes is less than a quarter of a second in space, a minuteness so inconsiderable that, if multiplied by 300 times, it would still be too small to be perceptible by the naked eye, and the discrepancy is as likely to be due to the errors of the observations themselves as to that of the assumption of no exterior planet.

It is therefore concluded that there is no evidence of any unknown cause influencing the motions of Neptune, and consequently no evidence of the existence of an extra neptunian planet. At the same time, this is only negative evidence; for supposing the extra planet to exist, centuries might be required for its attraction to exert any appreciable influence upon the motions of Neptune. For a similar reason there are no reliable data for correcting the mass of Uranus.

The fifth chapter is devoted to the tables, which are founded on the theory finally concluded upon, and will probably not be subject to errors of more than a very few seconds during the remainder of the present century.

The history of the planet Neptune in relation to the perfection of its orbit exhibit a series of facts alike creditable to the science of this country and the policy of the Smithsonian Institution.

The next paper for the "Contributions" is an exposition of the results of the discussion of the observations made during the expedition under the direction of Dr. I. I. Hayes in the arctic regions, by Chas. A. Schott, of the United States Coast Survey.

The expedition, during which these observations were made, was organized and principally equipped through the enterprise of Dr. Hayes, assisted by contributions of a number of liberal gentlemen interested in the advance of physical geography. The principal object of the enterprise was the extension of the explorations of Dr. E. K. Kane north of Baffin's Bay, and to make such observations as would add to our knowledge of the physical condition of the arctic regions. The expedition was mainly furnished with instruments by the Coast Survey and the Smithsonian Institution, and after its return the records of its observations in their rough state, or as they were made, were presented to the Institution for reduction and discussion.

It is scarcely necessary to mention that scientific truths are not generally immediately deduced from the simple observation of phenomena, but that these require in most cases corrections to free them from the effects of extraneous and other causes. Thus in observing the place of the moon or a planet, the position as given directly by the instrument must be corrected for refraction, for parallax and for instrumental errors. So with the observations of the barometer, a correction must be applied for the relative expansion of the mercury and of the brass case in which the glass tube is contained, and also for capillarity, and in many cases for the elevation of the instrument above the level of the sea.

After the observations have been submitted to the process of correction, to which the name of *reduction* has been applied, they are then in a condition for scientific analysis, or for what is technically called *discussion.* It seldom happens that any phenomenon is the result of a single approximate cause. In almost all cases the effect observed is the result of a series of concurring causes, and it is the object of the scientific inquirer, if possible, to ascertain the separate effect of each. For example, the height of the tide at a given place and time is due to the conjoint action of the sun and moon modified by the form and direction of the coast, to concurring or adverse tidal waves, and also to the direction of the wind. In a successful discussion each of these effects should be separately exhibited, and the amount of the several influences of each critically ascertained. Without such reduction and discussion the crude observations exhibit a mass of figures without apparent connexion, and give no indication of the relation of phenomena. Unfortunately the labor attending these processes is so great, and in many cases the skill required so unusual, that individual enterprise and ordinary attainment are insufficient to accomplish the end, and on this account immense masses of physical observations are of little value to science. Even those made by Parry, Ross, and the other explorers in the arctic regions have not yet been subjected to the analytical processes by which all the interesting truths relative to the physical geography of the globe are to be deduced. It is in this line that the Smithsonian Institution has especially rendered good service in the way of advancing science. Of this fact the memoir in question, with those which the Institution has published relative to the observations made by the expedition under the direction of Dr. Kane, and that under Sir F. L. McClintock, and the discussion of the observations made at Girard College by Professor A. D. Bache, are obvious illustrations.

For investigations of this kind the Institution has been fortunate in obtaining the services of a computer so expert and sagacious as Mr. Schott, since few persons are to be found who combine the varied qualifications necessary to so difficult an undertaking.

The results of the observations made under the direction of Dr. Hayes are presented in four parts: the first relates to the astronomical and geodetic; the second, to the magnetic; the third, to the tidal; and the fourth, to the meteorological observations.

The first part contains all the geographical positions determined by the explorers, including a series of observations to ascertain the latitude and longitude of Port Foulke, the winter quarters of the expedition in 1860–'61; also a general survey of Kennedy channel, and a minute survey of Smith's straits. The memoir presents a table of eighteen geographical positions, all determined with astronomical accuracy, by means of which, and the help of angles and solar bearings, the outlines of a large map were traced. It is highly gratifying to find, says Mr. Schott, that a remarkable agreement exists with the prominent points observed during Kane's expedition, and, indeed, that the exploration of Dr. Hayes is truly an extension and verification of the labors of his predecessor in the same region.

Nor is this all; it is proper to state that a new sound was discovered, opening to the westward near Cape Sabine, and also that two points noticed by Kane and described as headlands were found to be parts of two islands at the entrance of a bay; also the shoreline of Whale sound was fully developed, as well as the whole of the western coast of Kennedy channel. All these results, with the positions of the glaciers, are exhibited on a general chart of the regions explored, and on a special chart of Smith's straits.

The agreement of the results of the observations under Dr. Hayes and Dr. Kane is due to the fact that in both expeditions the greater portion of the observations were made by Mr. August Sonntag, well known to science by his previous labors in astronomy and physics. By his early death the expedition sustained a great loss, since through his aid still greater additions would have been made to our knowledge of the regions explored.

Included in the first part of the reductions is also an account of the pendulum observations, intended to furnish information as to the relative intensity of the force of gravity, and, consequently, of the figure of the earth. The pendulum used in these observations is a simple bar of brass, five feet seven three-fourth inches in length and one inch and four-tenths in breadth, and seven-tenths in thickness, weighing nearly twenty-two pounds. It is furnished with two steel knife-edges, placed at 14.2 inches from either end, so that it may be vibrated first with one end downward, and then with the other, affording in each position a series of independent observations. The direction of the face of the pendulum could also be reversed, by means of which the results of their regularities of the knife-edge could at least in part be eliminated.

For comparison of the observations which had been made in the arctic regions, a series was instituted, previous to the sailing of the expedition, with the same pendulum, at the Harvard Observatory, in Cambridge, by the late director, George P. Bond. The result obtained by the comparison indicates a smaller value for the polar depression of the earth than that deduced from all previous pendulum observations in the northern region. If combined with these, it will bring the resulting figure of the earth nearer to that previously deduced from the measurement of arcs of the meridian in various parts of the world.

The compression, as deduced by Mr. Schott from all the observations of the expedition under Dr. Hayes, is $\frac{1}{372}$ part of the polar radius. The excess of the number of vibrations in a day at Port Foulke, over the number made by the same pendulum in the same time at the Harvard Observatory, was $129\frac{1}{2}$. The observations were corrected for the height above the level of the sea, for the expansion of the metal on account of variation of temperature, and other deviations from a normal condition. It is highly desirable that the same pendulum be vibrated at several points on the eastern coast of the United States, as nearly as possible, under the same meridian as Port Foulke, in order to obtain a series of independent determinations of the curvature of the earth; and for this purpose the instrument has been lent to the Coast Survey, to be vibrated at New York, Washington, and Key West. The observations at New York will afford the means of connecting the results of a similar kind in Europe and other parts of the Old World, through the series of observations made by General Sabine in that city in 1822–'23, at the old site of Columbia College.

The observations of part second, which relate to magnetism, are of two kinds, namely, differential and absolute; the first made at Port Foulke, between November and March, 1860–'61, on fifteen days, during each hour of the twenty-four, being intended to ascertain the diurnal variation of the magnetic needle. The results of these observations are presented in connexion with those of Dr. Kane, in the form of a curve in which the agreement is strikingly exhibited.

From both series it appears that the north end of the needle attains its greatest westerly deviation at one p. m.; its greatest easterly deflection, between two and three o'clock in the morning; its normal position, at seven in the morning, and at the same hour in the evening. From these additions to our knowledge of terrestrial magnetism we have been enabled to state the fact that when simultaneous observations are made at different places, the motions of the needle are found to be governed by the local time at each place. From this it has been inferred that the cause of the daily motion of the needle is connected with the diurnal motion of the sun. The one o'clock greatest western excursion is common to all localities in the northern magnetic hemisphere, and is the most constant feature of the daily motion of the needle.

The declination was determined at fourteen localities on the coast of Greenland, between latitudes 72 and 80. These have been discussed in combination with the observations of Dr. Kane, and the result is given on a magnetic chart of the vicinity of Smith's strait. The horizontal component of the magnetic intensity was determined at seven stations, and these, combined with those by Dr. Kane, are also represented on the same chart. The inclination or dip was observed at six localities, and were similarly combined with Kane's determination, and likewise exhibited on the same chart.

It is a remarkable fact that but three auroras of sufficient brilliancy to attract the attention of the observers were seen at Port Foulke during the winter of 1860–'61, though many were noticed during the same period in lower latitudes; and this fact is the more interesting since the position above mentioned is very nearly the centre of the auroral belt as marked out by Professor Loomis, and exhibited in the appendix of the report for this year. Although the aurora is now known to be an electrical phenomenon, it is certain that its region of greatest activity is not the magnetic pole of the earth, but is in a zone of several degrees in breadth, including the geographical poles eccentrically; nevertheless, it is also clear that it is influenced in its motions by the magnetism of the globe.

The third part, which relates to tidal observations, consists of two series: the first were made during November and December, 1860; the second in June and July, 1861. In the first series the height of the tide was noted every half hour; in the second series it was observed at every ten minutes about the time of high and low water. The apparatus employed consisted of a rope to which a stone was attached at the lower end resting on the bottom and passing over a pulley attached to a movable weight at the other. The pulley was supported by a tripod standing over a hole in the ice at a short distance from the vessel, and the changes of the height of tide were indicated by the vertical motion of the smaller weight. Corrections were applied for the irregular stretching of the rope and also for the errors of the watch. From the records thus corrected, Mr. Schott has determined the height of the average sea level for each day, and has compared this with the moon's declination parallax, or distance from the earth, the atmospheric pressure, and the direction and force of the wind. It is found that a variation of between one and two inches in the height of the water is due to the changes in the moon's declination from zero to its maximum value; also that a rise of one inch in the mercury of the barometrical column is accompanied with a fall of nearly four inches in the level of the sea. The effect of the wind, though small, is apparent. With a northeast wind there was a depression, and with a southwest an elevation, of the level; one probably blowing the water into and the other out of the strait.

The general character of the tide at Port Foulke is similar to that at Van Rensselaer harbor, exhibiting two ebbs and two flows each lunar day, with considerable diurnal variation, producing at Port Foulke at certain times the character of a single daily tide—the two waves as it were running into each other. The establishment, or, in other words, the average lagging behind of the high water, after the passage of the moon across the meridian of the place, is here half an hour earlier than at Van Rensselaer harbor, which is distant 55 miles in a northeast direction. This indicates a motion in accordance with the general direction of the tidal wave, which in this region is not due to the direct attraction of the moon, but is derived from the great tidal wave of the Atlantic ocean.

The investigation of the half-monthly inequality, or of the effect of the action of the sun in modifying the effect of the moon in time as well as in height of tide, gave the following results: The range of the inequality in time is one hour and twenty-six minutes. The mean establishment of high water is found to be eleven hours thirteen minutes and eight-tenths, and that of low water seventeen hours nineteen and a half minutes. The range of the variation in the height of the water, due to the action of the sun and moon, is two feet and a half. The absolute average variation in the height of water at Port Foulke is 7.7 feet, while Dr. Kane found at Van Rensselaer harbor 7.9 feet as the variation at that place. The extreme fluctuation observed in the water level was 13.8 feet. The retardation or the difference between the theoretic and observed time of high water is comparatively small.

The effect of the greater or less distance of the moon or parallax on the half-monthly inequality shows that diminution of distance produces a decrease in the time of the lagging of the water, and that the range of the tide is increased by three-tenths of a foot for an increase of one minute of parallax. The action of the sun must evidently be relatively less, with an increase of the declination of the moon, and the amount of this is found from the discussion of the observations to be in height of the value of only a fraction of a foot, and in time but a few minutes.

The moon, as it is well known, produces two high tides, at nearly the same moment, on opposite sides of the earth, and these must vary in altitude and extent from day to day, with the change of position of the moon in the heavens. The difference of these two tides in time and height is called the diurnal inequality. Mr. Schott has made of this phenomenon a special graphical study, and has found that the diurnal variation in height is greater for high water than for low water—that is, two successive high waters differ from each other more than two successive low waters. The maximum variation in height is found at Port Foulke to be 3.8 feet, and only 2.4 in low water, while this variation entirely disappears about two days after the moon passes the equator. This is for the high water; but for low water the disappearance does not take place until after a lapse of nearly ten days, and this fact is connected with a remarkable one relative to the magnitude of the variation. On the one hand, the less interval of time between two tides is accompanied in high water with a greater difference of height, while in low water a greater interval of time is connected with a smaller difference in the level of the two tides. The diurnal inequality is due to two waves, a diurnal and a semi-diurnal, which are exhibited by a diagram and fully analyzed.

Next, the form of the tide was investigated, and found to occupy a longer time in rising than in falling, or, in other words, its posterior slope is more rapid than its anterior—the difference in time is thirteen minutes at Port Foulke, and fifteen at Van Rensselaer harbor.

Finally, an investigation is given of the velocity of the tide wave in passing up along Baffin's bay. From this it appears that the crest of high water occupies eight hours in passing from the southern cape of Greenland to Smith's strait, at the head of the bay. From these investigations the velocity of the wave in Davis's straits is 194 statute miles per hour, correspoding to an average depth of 418 fathoms. In Baffin's bay the velocity is 177 miles, corresponding to 349 fathoms. In Smith's strait the velocity is 157 miles per hour, corresponding to 277 fathoms of depth. It may be interesting to state, as one of the results, that the free tide-wave in Davis's strait and Baffin's bay is about 2,300 miles in length, and only 7½ feet in height from hollow to crest.

These statements will serve to give some idea of the complex nature of the investigation of the phenomena of the tides, resulting not alone from the direct action of the moon and sun, but also modified by the superposition of the derived tide of the Atlantic, and by the influence of the configuration of the channels through which the waves are propagated.

Part fourth gives the observations and discussion of the meteorological investigations. It is divided into three divisions—temperatures, atmospheric pressures, and winds—with an appendix giving a record of the weather during the whole voyage, and miscellaneous remarks.

Port Foulke, the locality of the winter quarters, was in the vicinity of open polar water, which exercises a marked influence on the climate of the region. The comparative mildness of the station is illustrated by the fact that the simultaneous recorded observations at Port Foulke and Van Rensselaer harbor show that the temperature was 26 degrees lower at the latter than at the former place, though distant only 53 miles. The continuous records at Port Foulke extend over a period of eleven months, and were made at every other even hour, day and night, with a few exceptions, (as to the pressure,) during the whole time.

The expedition was provided with about two dozen thermometers, consisting of spirit, mercurial, and metallic, of which the index errors were several times ascertained by immersion in buckets of ice-water, and by comparison among themselves in the air, at very low temperatures. From all the records of these observations a table of corrections was made out by taking the mean of the temperatures as indicated by the several thermometers. The result was found to indicate temperatures nearly corresponding with those given by standard No. 3, *which had been previously selected as the most trustworthy of the* series of instruments. All omissions in the records were supplied by the known methods of interpolation.

The first results obtained are those which relate to the annual fluctuations of temperature. From these it appears that the warmest day was the 15th of July, with a temperature of 41°.6 Fahr., and the coldest the 16th of February, with a temperature of —28°.

The mean annual temperature falls on the 22d of April and the 14th of November, and is +6°.06. It must be observed that this difference in the temperature of the two localities, as we have said before, is due to the open water, and reached its maximum on the 20th of March, 1861, when the thermometer stood 46½ degrees lower at Rensselaer harbor than at Port Foulke.

The diurnal fluctuation—that is, the difference between the warmest and coldest hour of the day—exhibits also a remarkable accordance at the two places, its amount being 3°.38 at Port Foulke and 3°.64 at the harbor. At Port Kennedy it is 4°.12. In the month of December the diurnal variation almost vanishes, there being a difference of only two-tenths of a degree between the highest and lowest hour of the twenty-four. It attains its maximum value in March, when it is 8°.9. The fact that this greatest effect of the day influence of the sun takes place in March, Mr. Schott is disposed to attribute to the great amount of vapor which formed at a later period and obscured the direct action of the sun. On the average for the whole year the temperature rises until 2½ p. m. and falls till 2½ a. m., and the average temperature of the day is reached at 8 o'clock morning and evening.

The dependence of the temperature on the phases of the moon was also attempted to be deduced, but the situation of Port Foulke relative to the disturbing influence of the open water of Smith's straits rendered the result unsatisfactory.

The relation of the temperature to the direction of the wind was next studied, and it was found that the northeast and east winds, or those which flow over Greenland, are the coldest, while those from the south, southeast, and southwest, or those which pass over ocean surfaces, are the warmer. The northeast wind always depresses, while the southwest always increases the temperature, especially in the winter season. The most intense cold was experienced when the air was perfectly calm, and this appears to be the general rule in the arctic regions. The effect of the various winds on the whole is small, not exceeding an elevation or depression of more than a degree and a half from the mean.

The effect of the snow and rain on the temperature is far greater than that of the wind. On an average in winter, during every fall of snow, the temperature was elevated 8°.6, and in summer fell a degree and a half during a fall of snow or rain. The number of days in which snows fell was 94, those in which it rained were 15, during the whole period of eleven months.

The effect of clear and cloudy weather on the temperature is next considered, and from the result of 82 clear days in winter, it is found on an average 3½ degrees below the normal temperature of these days. In summer, from the observations of 41 clear days, the temperature was higher eight-tenths of a degree. In winter, during 31 cloudy days, the temperature was seven degrees above the normal; and in summer, during 48 cloudy days, the temperature was 2°.1 lower than the normal.

From the foregoing it appears that a clear atmosphere produces opposite effects in summer and in winter, and this is, without doubt, due to the greater amount of vapor in the former than in the latter season. During a clear day in winter the air is almost entirely deprived of vapor, and radiation goes on with full energy, uncompensated by rays from the sun, except those which are very oblique; while in summer the more intense rays of the sun penetrate the vapor, while the less intense heat from the earth cannot escape through the aqueous stratum.

The last discussion relates to the direct heating power of the sun and the record of temperatures observed during the excursion to the extreme northern point reached. The temperature in the month of May, 1861, was ten degrees lower along the coast of Kennedy channel, indicating a colder climate as the explorer went north, but whether this would be the case during the other parts of the year remains yet to be verified.

The observations on atmospheric pressure are not as complete as those on the temperature, the observations between 10 p. m. and 6 a. m. being frequently wanting. All the readings were reduced to the temperature of 32° Fahrenheit.

The diurnal fluctuation as given by these observations, as well as by those of Dr. Kane, is extremely small, scarcely exceeding one-hundredth of an inch. The maximum pressure occurs during the day at about 6½ p. m., and the minimum about 3 in the morning. These, however, agree generally with those indicated in other arctic localities.

At Port Foulke there is also a secondary maximum and minimum occurring at 8 and 10½ a. m. The annual monthly fluctuation of the barometer is fully twenty times greater than the diurnal variation. In spring there is a well-marked maximum pressure, and in the autumn an equally well-exhibited minimum.

It is found from all the observations that the mean atmospheric pressure of the mercurial column at the temperature of 32° is 29.83, which accords well with the observations of Kane and McClintock, and is considerably above the minimum pressure found in about 12 degrees lower latitude. The fluctuation was 1.8 inch—the highest, 30.74; the lowest, 28.93.

The effect of the direction of the wind on the barometer is that of a depression of .07 of an inch during the northeast, and an elevation of .04 during the southwest, and a similar elevation during the calms. The oscillations of the barometer during three storms are illustrated graphically, and an attempt is made to determine the elastic force of vapor, but the observations recorded were insufficient; the amount was very small, not exceeding .02 of an inch.

The direction of the wind was invariably recorded with reference to the true meridian, and its force estimated by an arbitrary scale between zero and ten. The general result was, that the quantity of the stream of air which passed over the place of observation in the course of a year was nearly 60,000 miles.

The resultant direction during the year is largely from the northeast. The relative frequency of the wind is given, the northeast being 47 per cent. of the whole, the southwest 17, and the calms 27 per cent. The average velocity of the wind was 19 miles per hour.

During the eleven months of the observations 25 storms were recorded, 19 from the northeast and 6 from the southwest.

Another paper presented for publication, also in the "Contributions," is an account of geological observations in China, Japan, and Mongolia, by Raphael Pumpelly, of New York. This paper was read before the National Academy of Sciences, and recommended by that association to the Smithsonian Institution for publication. In the summer of 1863 the author passed from Shanghai to Hunan and the boundary between Hupeh and Sz'chuen, and in the autumn and winter and following spring from Peking through the mountains of western Chihli, and again from Peking, beyond the great wall of China, and westward on the plateaus along its southern edge to its 112th meridian, returning by a route south of the great wall. In the winter of 1864–'65 he went from Peking across the plateau of Central Asia to Siberia.

The almost total absence of observations of a geological character throughout this wide field, renders any information in regard to it of considerable interest. The facts stated are principally derived from the personal observations of the explorer, together with such information as could be obtained from Chinese works which treat of the geography of the empire, or bear upon its mineral productions. The principal results arrived at are as follows:

There is reason to believe that there exists throughout China an immense development of Devonian limestone, which rises to the surface in all the larger ridges, and attains in some places a thickness of over 10,000 feet. The formations beneath this limestone, as far as they were seen, are either granitic rock or metamorphic schist unconformably stratified as regards the limestone. Overlying the limestone there exists in almost every part of the country a great coal-bearing formation of sandstones, shales, conglomerates, &c., in nearly, if not quite, conformable stratification as regards the floor on which they rest. The fossil plants obtained from this formation are considered supra-carboniferous, and it is supposed that the coal fields of China, which vie with our own in extent, are referable to the Triassic period. Although from the limited range of actual observation it would be too much to assert that there is a total absence of any later formation than these coal measures, still the author failed to observe any traces of them.

Only two systems of elevations occur in China of sufficient importance to have left a marked impress on the surface. These are the northeast–southwest and east–west.

The northeast system determines the outline of Asia east of the 110th meridian, and coincides with the middle course of the Yangtse Kiang and the lower course of the Amur.

The east–west system exists in western China in the Min mountains and in the Nanling range, and determines the general course from west to east of the three principal rivers of the south of China.

The upheaval of the northeast system began after the deposition of the great Devonian limestone formation, and appears to have risen slightly during the formation of the coal measures, but its greatest elevation was after the latter had been deposited.

The east-west system appears to have risen later than the other, since it has elevated the limestone and overlying rocks which rest upon its sides. Evidences are presented of recent oscillations extending over great areas in the form of terraces.

In the great plain of northeastern China is a delta deposit extending over nearly eight degrees of latitude, which is yearly increasing in extent. Through this delta the Hwang Ho varies its course every few centuries, emptying into the sea alternately to the north and to the south of a mountainous peninsula, thus presenting the remarkable phenomenon of one of the great rivers of the earth not only shifting its course through several degrees of latitude, but also of returning to the same bed after the lapse of a number of years.

The great table-land which lies between China and Siberia, where the author crossed it, consists of basins of undisturbed strata of sandstone, containing beds of gypsum. In the south this table-land generally terminates in a precipitous wall, formed of an immense development of lava, in some places more than 1,500 feet thick.

The abrupt termination of the plateau is owing to a great dislocation which marks approximately the coast-line of a former ocean to the north, in which the most recent deposits of the plain originate, and along whose southern shore there existed an extensive region of volcanic activity. The plateau is terminated on the east by parallel ridges, which descend by successive terraces to the low land.

Among the more economical results obtained may be mentioned a large number of extensive coal basins and the deposits of other useful minerals, which are so widely distributed throughout the empire as to warrant the belief that China scarcely stands second to any other country in regard to the quantity and quality of its coal and its other mineral resources.

Such gifts of nature, says the author, combined as they are with a variety of favorable circumstances, cannot long be unappreciated. They are the elements of the civilization of the present age, and in the natural course of events the country possessing them cannot long avoid being drawn into the stream of industrial and intellectual progress.

Among the papers which have been offered for publication is a vocabulary and grammar of the Nootka Sound language, by the Rev. C. Knipe. This was the result of a residence of a year and a half among the tribes inhabiting that portion of the northwest coast of America. The same language extends southward to Cape Flattery, and is one upon which very little correct information has been obtained. The present work contains lists of more than twelve hundred native words; with remarks on the genius of the language, and an account of roots, terminals, derivations, comparisons, &c. In the report *of one of our collaborators, Mr. George Gibbs, to whom this memoir was referred, it is stated:*

"The Nootka language is an exceedingly interesting one, not only as the earliest of those of Northwestern America with which we had any acquaintance, and which enters largely as an element into the 'jargon' of the coast, but also because the people who speak it in its various dialects form one of five great northern tribes of the Pacific, whose intelligence, courage, and ingenuity have rendered them conspicuous, and whose industrial arts and physiognomy have furnished an argument with many in favor of their Asiatic origin. The existing vocabularies of that language are very imperfect and meagre, and the more extended one of Mr. Knipe will be of great value to comparative philologists."

We had decided to put this production to press, when the author unexpectedly informed us that he was obliged to leave the country for England, and could not, therefore, give his personal attention to the work as it was passing through the press. As this was considered essential, it was thought better that he should withdraw his manuscript, and endeavor to procure its publication through some society in England. If he should fail in this, the Institution would at some future time undertake its publication, since it is intimately connected with other works of a similar character already given to the world through the agency of the bequest.

Another paper presented to the Institution for publication is by Mr. James G. Swan, upon the manners and customs of the Makah Indians of Washington Territory, a tribe belonging to the Takwaht or Nootka Sound family, illustrated with many drawings and accompanied by a vocabulary of their language. The size and cost of publication of this work has prevented us from considering its adoption at present as one of our series.

Miscellaneous Collections.—Under the class of publications called "Smithsonian Miscellaneous Collections," previous to the war, a series of manuals, intended to facilitate the study of different branches of natural history, were projected and a number of them actually commenced; but the subsequent diminution of our income, and the advance in the cost of materials and workmanship in the line of printing, has greatly interfered with the rapid completion of this enterprise. Of the works of this series as given in the report for 1860, some have been completed, others are still under way, and one or two not yet commenced. The following is a statement of their present condition:

1. Land and Fresh Water Shells—part I. Terrestrial Pulmonata. By W. G. Binney. Of this work the manuscript is entirely completed and the woodcuts engraved.

2. Land and Fresh Water Shells—part II. Fresh Water and Marine Pulmonata. By W. G. Binney. The whole of this work has been stereotyped, and will soon be ready to be issued.

3. Land and Fresh Water Shells—part III. By W. G. Binney. Including all the water-breathing univalve shells except the Melaniadæ. This work has likewise been stereotyped, and is ready for publication.

4. Land and Fresh Water Shells—part IV. Melaniadæ. By G. W. Tryon. The manuscript of this has been completed and most of the woodcuts engraved.

5. Land and Fresh Water Shells—part V. Corbiculadæ. By Temple Prime. Of this the stereotype plates are almost completed.

6. Land and Fresh Water Shells—part VI. Unionidæ. A work by Mr. Lea, of Philadelphia, supersedes for the present any other publication on this subject.

7. Marine Shells—part I. From the eastern coast of North America, by Dr. W. Stimpson.

8. Marine Shells—part II. From the western coast of the same continent, by P. P. Carpenter. The last two works have been commenced and numerous woodcuts drawn or engraved to illustrate them; but no definite period can be fixed for their completion.

9. Bibliography of North American Conchology to the year 1860, by W. G. Binney, parts I and II. This work, which is a supplement to those previously mentioned, has been stereotyped and copies distributed to institutions.

Another work belonging to the octavo series, but not included in the above list, is the Review of American Birds in the Collections of the Smithsonian Institution—part I. Northern and Middle America. By Professor S. F. Baird.

This work is intended to present a descriptive account of the very large collection of American birds in charge of the Smithsonian Institution, with an enumeration of such other specimens as may serve to illustrate the geographical distribution of the several species. In 1858 Professor Baird prepared an account of the birds sent to the Institution by the different expeditions for surveying the railroad routes to the Pacific, which was published in the series of reports ordered by Congress. To this was added an account of all the birds of the Atlantic States, and it thus formed a systematic and descriptive work on the ornithology of North America, which has since become the principal standard manual on this subject. A reprint by the Institution, from this volume, of the catalogue of species has been widely distributed and much used for labelling collections and preparing lists for distribution of specimens.

The number of specimens of birds of America in the collection of the Institution at the time the work was published, and upon which it was based, was less than 10,000; it now exceeds 40,000. Many portions of North America unexplored at that time—the whole arctic region, the recesses of the Rocky mountains, Cape St. Lucas, &c.—have since been investigated; the migration and distribution southward in winter of the species have been established by numerous collections from the West Indies, Mexico, Central and South America, and information generally has been collected, during the interval of seven years, which tends to complete the knowledge of the ornithology of North America. In this same period the specimens received from all parts of Mexico and Central America and the West Indies are so numerous as to represent nearly all the known species, and to embrace many new ones, forming, according to Professor Baird, an aggregate of species much larger than that of any other single collection.

In order, therefore, to bring up the subject to the present date, and at the same time to exhibit a connected account of the birds of Mexico, Central America, and the West Indies, Professor Baird undertook the work referred to, and 320 pages of it have thus far been published, each signature being dated, to show the time of actual issue. The portion printed embraces an account of the *oscine* or singing birds, with synoptical tables and detailed descriptions of the families, genera, and species, excepting where these have already appeared in the Pacific Railroad Report. As the account of each family is completed, the duplicate specimens are set aside for distribution to the principal museums at home and abroad as types of the "review."

The work of Professor Baird has met with much commendation from ornithological writers in this country and Europe. The editor of the London Ibis, a journal devoted exclusively to ornithology, remarks: "that it will be the book of authority on North American ornithology for a long time to come, there can be little doubt. The immense series of specimens, whether only temporarily lent or deposited permanently, (but the latter out of all proportion to the former,) in the collection of the Smithsonian Institution, gives him an advantage, such as probably no other ornithologist of what country soever has at any time previously enjoyed; and the professor, as our readers need not be told, is not the man to neglect opportunities of this kind. We are almost inclined to regard this work as the precursor of a new era in natural history. Hitherto a zoologist has thought he has done very well if he has closely examined some half dozen specimens, presenting the different appearances depending upon age, sex, or the like, of one species. He will now find, from an inspection of Professor Baird's labors, that an acquaintance with a much larger number of individuals, especially from different localities, is requisite if he intends to advance his science. One result of this attention to increased material, if generally followed, we suspect will be the very desirable one of nullifying the species makers—species makers, of course we mean, in a bad sense, for there are no more useful men, if they will but keep their hobbies under command.''

From the limited funds of the Institution and its plan of organization it cannot afford to support individuals while they are devoted exclusively to the advance of any branch of science; and were this to be done in one case, it might be demanded in many. But there can be no objection to an officer of the Institution availing himself of the materials which are gathered through its agency for prosecuting as an extra labor any investigation to which he may be inclined; provided that at the same time he faithfully discharges all the essential duties which pertain to his position. In the preparation of the work above mentioned every facility has been given to Professor Baird which the establishment could afford, and with this view special attention has been paid to the collection of specimens of ornithology, with the view that, after this work has been completed, similar attention will be given in succession to other branches of science.

Another paper in the Miscellaneous Collections is entitled Researches upon the Hydrobiinæ and allied forms, chiefly from materials in the collection of the Smithsonian Institution, by Dr. William Stimpson. This memoir gives the results of an investigation relative to the structure of a group of small and little known fresh-water Gasteropods, which Dr. Stimpson had undertaken to study with a view to their classification and arrangement in the museum of the Institution. Under the name of Gasteropods are included mollusks, with a distinct head, and which generally have a flat foot, adapted to crawling, and are also usually provided with a univalve shell. The search for the affinities and relations of these animals led to an examination of the allied types inhabiting the sea and brackish water. The results of the investigation were a more exact definition of the family to which they belong, and the extension of it so as to include other forms previously scattered; also the establishment on anatomical bases of a number of subordinate groups or sub-families, and the suggestions as to many new genera distinguished by peculiarities in the structure of the soft parts as well as the shell. This memoir not only furnishes an interesting addition to descriptive natural history, but a method of investigation which may be advantageously applied to other families of the class.

Dr. Stimpson, who was the naturalist to the Northwest Pacific Exploring Expedition, under Commodore John Rodgers, is now in charge of the Museum of the Chicago Academy of Sciences. He has spent several years, while preparing his report on the collections of the expedition, in the building of the Institution, and, without salary from the Smithson fund, has rendered us essential service in the classification and naming of specimens.

The work on the Myriapoda of North America, by Dr. H. C. Wood, jr., mentioned in previous reports, was completed and was awaiting its turn for publication, when it was unfortunately destroyed by the fire. It was subsequently rewritten, and as our funds did not permit its being immediately put to press, it was, with the consent of the Institution, offered to and accepted by the American Philosophical Society, and printed in the thirteenth volume of its transactions. The wood-cuts (about sixty) which had been prepared for the work were lent to the society to facilitate the publication. These facts are stated in the memoir, and full credit given to the Smithsonian Institution for the aid thus rendered.

The close of the war having released Dr. John Le Conte from his medical duties in connexion with the army, he has resumed his labors in entomology, and has already written a considerable portion of his "List of North American Coleoptera," and the "Description of New Species," which will be published during 1866. When these works are completed, he will commence the second part of his classification of coleoptera.

Reports.—During the last three years the government printing office was so busied with the preparation of documents connected with the war that the Annual Report of the Institution was incidentally delayed, but the report for 1864 was finished and distributed at an earlier period than had been possible for several previous years. The demand for these reports is every year increasing, and we learn from the members of both houses that no document printed by Congress is more frequently called for.

It is greatly to be regretted that the extra copies of all the volumes of the reports previous to 1863, which were stored in the rooms of the towers, were destroyed by the fire, and that we are therefore unable any longer to furnish complete sets or to supply missing volumes to various institutions and correspondents that have applied for them. The reports since 1861, inclusive, have been stereotyped, so that at any future time an edition of any of these volumes may be printed; but with the high price of paper and press-work, and with the heavy demands on the Institution, this is at present impracticable.

The report for 1864 contains in the appendix a eulogy of Delambre, the eminent astronomer, translated by C. A. Alexander, esq., and a continuation of the series of memoirs of distinguished members of the French Academy of Sciences; an essay on the velocity of light, by M. Delaunay, translated by Professor A. M. Mayer; an original compilation on ozone and antozone, made for the Institution by Dr. Charles M. Wetherill; translation of Jamin's essay on vegetation and the atmosphere; extract of a memoir on the preservation of copper and iron in salt water, by M. Becquerel, furnished by Admiral C. H. Davis, United States navy; translations of articles on the preservation of wood and caoutchouc and gutta-percha, from the German periodical "Aus der Natur;" an article on gun-cotton by Lieutenant Von Karolyi, with notes by Dr. B. F. Craig, of this city; a translation by Professor Ten Brook of a description of Pettenkofer's apparatus for testing the results of perspiration and respiration; a translation by the late Professor Hubbard, of the Naval Observatory, of Lamont's report on the solar eclipse of July 18, 1860; a report of the transactions of the Society of Physics and Natural History of Geneva, 1861 and 1862, translated by C. A. Alexander; a letter from F. Troyon on the crania Helvetica, with illustrations; a continuation of Plateau's researches on the figures of equilibrium of a liquid mass withdrawn from the action of gravity, with numerous illustrations; an original article on the artificial shell deposits in Monmouth county, New Jersey, and a continuation of Baegert's account of the aboriginal inhabitants of the California peninsula, translated by Professor Charles Rau; an article on the "intermixture of races," by George Gibbs; a lecture on the first steps in the study of high antiquity in Europe, prepared for the Institution by A. Morlot, of Switzerland; the prize questions proposed by various scientific societies in Europe; a report on the French scientific exploration of Mexico; an account of a journey to the Youcan, by W. W. Kirby, of an exploration in Upper California, by John Feilner, of an exploration of Western Missouri, by Dr. P. R. Hoy, with tables of weights and measures and chemical equivalents.

From this list it will be seen that the articles in the appendix consist principally of translations from foreign scientific publications not generally accessible to readers in this country, and of original articles prepared especially for the Institution.

At an early period it had been proposed to establish a printing office in connexion with the Institution, and the experiment was eventually tried. The result, however, conclusively proved that the expense of keeping up an establishment of this kind was far greater than the cost of having the printing done elsewhere. The "Contributions" and "Miscellaneous Collections" have principally been printed by Mr. T. K. Collins, of Philadelphia, whose execution of the work has been highly satisfactory, and who has had at his disposal unusual facilities for performing the various kinds of printing required in our diversified series of publications.

In consideration of the high price of paper and printing, and in order that it might be always in our power to issue new editions of any of our works, it has been thought advisable for the last four years to stereotype all our publications, and, for the security of the plates, to store them in a fire-proof receptacle. In view of this latter object, application was made to the Academy of Natural Sciences of Philadelphia for the use of a portion of the extensive fire-proof basement of its building. This proposition was cordially acceded to by the curators of the academy; a separate room was set apart for our accommodation, and all the plates not in actual use by the printer are now deposited in an apartment entirely secure from fire, and under the exclusive control of the Institution. The favor thus conferred by the academy is an illustration of the friendly relations which exists between this Institution and other establishments for the advancement of knowledge; and I scarcely need say that special thanks, in behalf of the Board of Regents, have been given to the society for its liberality and kindness in this matter.

Ethnology.—As we have stated in previous reports, this has been a subject to which the Institution has given particular attention. Indeed, it is one which especially commends itself to the science and literature of this country, since it is intimately connected with our history and politics. Unfortunately, however, the subject of races is one which involves questions as to their origin and characteristics which can scarcely be discussed at the present time with that dispassionate logic and strictness of induction which is necessary to the establishment of truth. Still, much can be done in the way of collecting and recording facts which may serve as the basis of future investigation. Some of these, such as those relative to the Indians, are rapidly passing into oblivion; and others, which regard the negro, require to be disentangled from much prejudice and misrepresentation. The peculiarities of these races should be critically examined and truthfully recorded; to do this is a duty we owe to science and humanity.

One of the most important clues we have to guide us in the labyrinth of ethnological research is language; and this is essentially the case with regard to the aboriginal races of this continent. The remains of their implements, and even their earthworks, may, in a considerable degree, be referred to the common wants and instincts of humanity, as in the use of the bow and arrow; but their language affords indications of affiliation or diversity not otherwise attainable. It has therefore been considered an important object to devise a system of general characters which would express to philologists in every part of the world identity of sounds. Unfortunately, however, though much labor has been expended on attempts to construct such a system, none has yet been presented which is entirely satisfactory, or has been generally adopted. Indeed, either from transmitted peculiarities or acquired habits, commencing with the instinctive use of the mother tongue, men of different languages apparently become incapable of accurately discriminating particular vocal sounds with which early usage has not familiarized them, and on this account a system of characters as a general alphabet which will give universal satisfaction is scarcely to be hoped for. We must therefore be content, at least for the present, to represent the sounds of the remains of the language of the red men of our continent in such characters as shall best serve to preserve their general features, with a view to future comparison and discussion. Such an alphabet has been adopted on the recommendation of Professor Whitney, of Yale College, and Mr. George Gibbs, of this city, to whom the subject was referred. It is accompanied by a vocabulary to be filled up with the equivalent words of the dialect under consideration, expressed in common letters, the sounds of which are fixed by reference to well-known English words in which these letters occur. In this way an attempt is made to express the several sounds without confusing the inquirer with new characters or numerous marks.

That the number of dialects which exist on this continent should be great is not surprising, when we reflect on the condition of the people previous to the advent of the Europeans. They had, without doubt, for a long time occupied the soil, and had probably arrived at that condition as to numbers in which the struggle for life is carried on with the greatest intensity, and in which sufficient food from the chase can only be obtained by separation into small tribes, or even families; a condition in which various dialects of one prevailing language would necessarily be produced.

Instructions for researches relative to ethnology in general have been prepared by Geo. Gibbs, esq., and were published in 1863, and a large number of copies distributed to officers of the United States government and others, particularly to those residing in the western portion of the continent. During the past year an appendix to these instructions, together with blank forms for systematic records, has been prepared by the same author and published by the Institution. The instructions and blank forms which have been sent out have produced a valuable return in vocabularies, weapons, implements, dresses, and other illustrations of the arts, manners and customs, and mental advancement of the aboriginal races of this continent. The vocabularies received have been given in charge to Mr. Gibbs, to whom the Institution is largely indebted for months of labor gratuitously rendered.

It is proposed, as soon as the funds will permit, to publish a descriptive catalogue of all the ethnological specimens in the possession of the Institution. These include, beside those from different parts of this country, those collected by the United States Exploring Expedition under Captain (now Admiral) Wilkes, from the islands of the Pacific, the East Indies, Africa, China and Japan, and Central and South America. Such a catalogue, properly illustrated with wood-cut engravings, from photograph drawings, would be an acceptable addition to the literature of ethnology.

At the commencement of the war preparations were made by the Institution to obtain records of the physical characteristics of the soldiers composing the army of the United States, embracing a large number of measurements of different parts of the body, to ascertain the peculiarities of the different nationalities represented. This work was afterwards prosecuted on a much more extended scale than was compatible with the means of the Institution by the Sanitary Commission, and the observations have since been discussed by Dr. B. A. Gould, of Cambridge, who has deduced from them a series of novel and interesting results, which were lately presented to the National Academy. It is proposed to extend similar measurements to the Indian tribes, and it is very desirable that the negro should be embraced in the same investigation. We have in this country at the present time a better opportunity to study the peculiarities of a number of races than is perhaps to be found in any other single portion of the earth, and the most casual observer cannot but be struck with the marked difference which exists between the Indian, the negro, and indeed between the descendants of the civilized inhabitants of different parts of Europe, thousands of whom are now flocking to our shores.

The stubborn self-reliance and impatience of control of the Indian are strikingly contrasted with the docility and imitative qualities of the negro. The inflexibility of the characteristics of the former, with the gradual changes and amelioration of the character of the latter in his association with the white man, are worthy of special attention.

It may be proper here to mention that we have received a communication from Dr. E. H. Davis, one of the authors of the first volume of Smithsonian Contributions, pointing out an error in Lubbock's account of Smithsonian publications on ethnology, copied from the Natural History Review, of London, in our report for 1862. In this article (page 322) the sculptured stone pipes found in the mounds are classed under the head of pottery. This error, says Dr. Davis, does injustice not only to American aboriginal art, but also misleads European ethnologists in regard to a series of sculptures pronounced by all who have seen them to be illustrations of the highest stage of art attained in the stone age of America. The same mistake is now reproduced in the publications of the Anthropological Society of London, and in Lubbock's Prehistoric Times. The fact is that the pipes described in the first volume of the Smithsonian Contributions were not of terra-cotta. None of this kind were found in the mounds, and but few anywhere in the country.

Intimately connected with ethnology and anthropology is archæology, or the study of remains of the ancient inhabitants of a country. To those who have paid any attention to the subject, it is well known that recently very interesting discoveries have been made of the remains of lacustrine villages in Switzerland, Italy, and Germany;

and also that memorials of the early inhabitants of the Scandinavian peninsula have been recognized in the extensive heaps of shells, mostly those of the oyster, which were for a long time considered as formations of the sea, but which have been shown by the Danish savans to be the accumulated household refuse of populations who lived in ages ascending beyond the records of history. The indications of the artificial origin of these accumulations consist in the total absence of stratification which always characterizes marine deposits, and in the fact that the rubbish contains rude flint implements, charcoal cinders, and the bones of various animals, some of which are, at present, extinct in the districts in which these mounds exist.

In our own country, besides the well-known Indian mounds constructed for special purposes, such as for sepulture and religious observances, and as monuments of events, there are found on various parts of the coast shell mounds, which, like those of Denmark, are composed of the refuse of the repasts of the aborigines, and which will undoubtedly reward the research of the archæologist with interesting facts in regard to the ancient inhabitants of the land we now inhabit. Among the first essays in this line are those of Mr. Charles Rau, of New York, published in the last report of this Institution. They relate to an examination of mounds of this kind on the shores of New Jersey. These indicate the places where the aborigines were accustomed to feast upon the spoils of the neighboring beach, remarkable for the abundance of oysters, clams, and other edible mollusks. The places selected for this purpose were at some distance inland, and sufficiently elevated to be out of reach of high tide. The direct evidence that these shell accumulations are of an artificial character consists in the presence of numerous fragments of pottery and stone implements.

In one of the heaps and in the adjacent fields Mr. Rau obtained more than 300 specimens of Indian manufacture, consisting of stone axes, arrows and spear points, flint knives, and many pieces of broken crockery. The axes are of greenstone, or of sandstone, of the usual shape, and encircled by a groove for the attachment of a handle. That the manufacture of arrow heads was carried on in this place is evident from the great number of flint chips and unfinished arrow heads which lie scattered among the shells. These places were probably camping grounds at certain seasons of the year. It is said that similar shell-beds occur on Long Island, where the shells are used for burning lime. They also exist in Georgia, on the coast of Massachusetts, in Newfoundland, and in California; and now that attention has been specially directed to the subject, they will probably be found and examined in various parts of this continent.

The occurrence of the Danish shell-heaps, whose history is merged in the twilight of civilization, and those of a similar character in America, show that the early condition of man is everywhere essentially the same, while the rude implements which are obtained from them indicate a similarity of wants and an identity of mental characteristics by which these wants are supplied.

The Institution has given special attention to the collection of specimens to illustrate the archæology of this country, and now possesses, with those procured by the Exploring Expedition under Captain Wilkes, those obtained from the various expeditions under the auspices of the Institution, and from the Hudson's Bay Company, a more valuable series than any to be found elsewhere in the United States. An effort will be made during the present year to properly arrange and fully display them for study. The comparison of the early savage implements in different countries is full of interest. We see from this that what is called the stone age is not a period of absolute time, but a stage of civilization, long past in one portion of the earth, while existing at present in another.

Meteorology.—It has been aptly said that man is a meteorologist by nature. He is placed in such a state of dependence upon the atmospheric elements, that to watch their vicissitudes and to endeavor to anticipate their changes become objects of paramount importance. Indeed the interest in this subject is so absolute that the common salutation among civilized nations is a meteorological wish, and the first introduction to conversation among strangers is a meteorological remark. Yet there is no circumstance which is remembered with so little exactness as the previous condition of the weather, even from week to week. In order that its fluctuations may be preserved as facts of experience, it is necessary that they should be continuously and accurately registered. Again, there is, perhaps, no branch of science relative to which so many observations have been made and so many records accumulated, and yet from which so few general principles have been deduced. This has arisen, first, from the real complexity of the phenomena, or, in other words, from the number of separate causes influencing the production of the ordinary results; second, from the improper methods which have been pursued in the investigation of the subject, and the amount of labor required in the reduction and discussion of the observations. Although the primary causes of the change of the weather are, on the one hand, the alternating inclination of the surface of the earth to the rays of the sun, by which its different parts are unequally heated in summer and in winter, and, on the other, the moisture which is elevated from the ocean in the warmer and precipitated upon the colder portions of the globe; yet the effects of these are so modified by the revolution of the earth on its axis, the condition and character of the different portions of its surface, and the topography of each country, that to strictly calculate the perturbations or predict the results of the simple laws of atmospheric equilibrium with that precision which is attainable in astronomy, will probably ever transcend the sagacity of the wisest, even when assisted by the highest mathematical analysis. But although such precision cannot be looked for, approximations may still be obtained of great importance in their practical bearing on the every-day business of life.

The greater part of all the observations which have been recorded until within a few years past has been without system or co-ordination. It is true that the peculiar climate of a given place may be determined by a long series of isolated observations, but such observations, however long continued, or industriously and accurately made, can give no adequate idea of the climate of a wide region, of the progress of atmospheric changes, nor can they furnish an approximation to the general laws of the recurrence of phenomena. For this purpose a system of observation must be established over widely extended regions within which simultaneous records are made and periodically transmitted to a central position, where, by proper reduction and discussion, such general conclusions may be reached as the materials are capable of yielding.

In discussing the records, the empirical method does not suffice. It is necessary that *a priori* assumptions should be provisionally adopted, not, however, at random, but chosen in strict accordance with well-established physical principles, and that these be finally adopted, rejected, or modified, as they are found to agree or disagree with the records. It is only by this method that the different causes which co-operate in the production of a series of complex phenomena can be discovered, as is illustrated in the history of astronomy, which, previous to the investigations of Kepler, consisted of an unintelligible mass of records of observations. But even with the application of the best possible process of discussion, the labor necessary to be expended on such large masses of figures, in order to deduce simple results, is far beyond any individual effort, and can only be properly accomplished by governmental aid.

The importance of a combined system of meteorological observations extending over a large area, and the peculiar advantages presented by our country for this object, were early appreciated, and such a system was commenced in 1819, under the direction of Dr. Lovell, Surgeon General of the army. The stations embraced the principal military posts, from which reports were made at the end of each month as to the temperature, the pressure, and the moisture of the air, the amount of rain, the direction and force of the wind, the appearance of the sky, besides casual phenomena, such as the aurora, thunder-storms, shooting stars, &c. In 1825 a similar system, of more numerous stations in proportion to the area embraced, was established in the State of New York, the points of observations being the several academies, under the direction of the board of regents of the university, an establishment having charge of the higher institutions of learning in that State.

In 1837 the legislature of Pennsylvania made an appropriation of four thousand dollars for instruments, which were distributed to voluntary observers. This system was continued about ten years; that of New York has been kept up with more or less efficiency until the present time; while the army system was continued until the commencement of the war.

The lake system, established by the engineer department, under the superintendence of Captain (now General) Meade, consists of a line of stations, extending from the western part of Lake Superior to the eastern part of Lake Ontario, and has been efficiently continued for several years.

The Smithsonian meteorological system was commenced in 1849, and, with occasional aid in defraying the expenses, has continued in operation until the present period. It was, however, much diminished in efficiency during the war, since from the southern States no records were received, and many of the observers at the north were called to abandon such pursuits for military service in the field. The efforts of the institution in this line have been directed to supplementing and harmonizing all the other systems, preparing and distributing blank forms and instructions, calculating and publishing extensive tables for the reduction of observations, introducing standard instruments, and collecting all public documents, printed matter, and manuscript records bearing on the meteorology of the American continent, submitting these materials to scientific discussion, and publishing the results. In these labors the Institution has been in continued harmonious co-operation with all the other efforts made in this country to advance meteorology, except those formerly conducted by the Navy Department under Lieutenant Maury. These were confined exclusively to the sea, and had no reference to those made at the same time on land. Without desiring to disparage the labors of Lieutenant Maury, I may say that his results would have lost nothing of their value by the adoption of a less exclusive policy on his part. The meteorology of the sea and that of the land pertain to a connected series of phenomena which can only be properly studied by a combined system of observations relating to both. The method pursued by Lieutenant Maury consisted in dividing the surface of a map of the ocean into squares of ten degrees on a side, and in recording within each of these the directions of the winds obtained from the log-books of the vessels which had traversed the several regions. In this way he accumulated a large amount of data, which, though published in connexion with many crude hypotheses, are of great value in the study of the meteorology of the globe.

In 1853 a meteorological system was commenced in Canada, the senior grammar school in each county being provided with instruments, and the observations have been continued to the present time. In regard to this system, Mr. Hodgins, of the educational department, remarks: "We have never lost sight of the great practical importance to a new and partially settled country, of establishing early in its history, before its physical condition is materially changed, a complete and comprehensive system of meteorological observations, by which may be tested theories of science which are yet unsettled, and which may be solved, relating to natural phenomena which have long remained among the sealed mysteries of nature."

The observations thus far have been taken without remuneration, but the importance of the system has become so well recognized that the Canadian government has decided to establish ten permanent stations, in addition to the observatories at Toronto and Kingston, distributed so as to afford the most complete information relative to the climatic features of the whole province. The points selected are Windsor, Goderich, Stratford, Simcoe, Barrie, Hamilton, Peterborough, Belleville, Pembroke, and Cornwall; that is, two stations on Lake Erie, one on Lake Huron, three on Lake Ontario, one on Lake Simcoe, one on the Ottawa river, one on the bay of Quinté, one on the St. Lawrence, near the eastern extremity of the province, and two in the interior of the country. The records made at the public schools of Canada have been furnished to the Smithsonian Institution, as well as to the committee on immigration of the House of Assembly, for the purpose of furnishing facts relative to the climate, of importance to settlers, and recently the department of royal engineers has applied for the returns, with a view to the consideration of their bearing on questions of defence.

To secure a greater degree of responsibility, and to promote the efficiency of the system, the government has provided for the payment of fifty cents a day to the teachers of the grammar schools at the stations before enumerated, as remuneration for the service rendered.

Under the direction of the distinguished academician Kupfer, there is established over the vast Russian territory a network of thirty meteorological stations, where are noted the various changes of the atmosphere as to temperature, pressure, moisture, &c. The most northern of these stations is at Hammerfest, in 70° 41′ north latitude, 21° 26′ east longitude from Paris, and the most southern is at Tiflis, in 41° 42′ north latitude, and 42° 30′ east longitude. A similar system of simultaneous observations has been for several years in operation in Great Britain and Ireland, in connexion with the Board of Trade, and under the direction of the late Admiral Fitzroy. Other and like systems have been established in France, Italy, and Holland. From these different organizations, as well as from insulated observatories, telegrams of the weather are sent every morning, at seven o'clock, from the principal cities of Europe to Paris, where, under the superintendence of the celebrated Leverrier, they are discussed, and the results transmitted by mail to all parts of the world in the successive numbers of the daily International Bulletin. A similar publication is periodically made in Italy, under the direction of M. Matteucci, so well and favorably known by his discoveries in physics. The British government has also established a system of observations for the sea, and furnished its navy with accurate instruments, carefully compared with the standards of the Kew observatory. It is estimated in a report to Parliament that, through an annual appropriation of about fifty thousand dollars, statistics may be collected in fifteen years sufficient, with what has already been obtained, to determine the average movement of the winds on every part of the ocean.

From the great interest which has been awakened in regard to meteorology throughout the world, and the improved methods which have been adopted in its study, it can scarcely be doubted that in a few years the laws of the general movements of the atmosphere will be ascertained, and the causes of many phenomena of the weather, which have heretofore been regarded as little else than the capricious and abnormal impulses of nature, will become adequately known; although, from the number of these causes, and the complexity of the resultant effect, it may never be possible to deduce accurate predictions as to the time and particular mode of their occurrence.

Indeed, the results which have been already derived from the series of combined observations in this country, fully justify the wisdom and forethought of those who were instrumental in establishing them. Although their organization was imperfect, the observers, in most cases, untrained, and the instruments of an inferior character, yet they have furnished data which, through the labors of Redfield, Espy, and Hare, whose memories are preserved in the history of science, have led to the establishment of principles of high theoretical interest, as well as of great practical value. Among these I need here mention only the fact now fully proved that all the meteorological phenomena of at least the middle and more northern portions of the temperate zone are transmitted from west to east. The passage of storms from one part of the country to the other was noticed by Dr. Franklin on the occasion of observing an eclipse of the moon. He showed that our northeast storms are felt successively later and later as the point of observation is further to the northeast; that they arrive last at the extreme northeastern portions of our continent. We now know, however, that the successive appearance of the storm at points further along the coast is due to the easterly movements, sideways as it were, of an atmospheric disturbance, greatly elongated north and south, and reaching sometimes from Canada to the Gulf of Mexico. Hence to persons residing along the seaboard the phenomenon would appear to have a northwardly progression, on account of the northeasterly trend of the coast; yet the storm not unfrequently reaches Bermuda simultaneously with Nova Scotia.

Few persons can have failed to observe the continued motion of the higher clouds from the west, or to have recognized the just meteoroscopy of Shakspeare in a well-known passage:

> "The weary sun hath made a golden set,
> And by the bright track of his fiery car
> Gives token of a goodly day to-morrow."

The breaking forth of the sun just before his setting shows that the rear of the cloud which has obscured his beams has, in its easterly course, reached our horizon, and will soon give place to an unobscured sky.

It must be observed, however, that all the storms which visit our coast are not of this nature; those denominated cyclones, and which seldom extend far into the interior, are probably of a rotatory character.

These usually commence in the Caribbean sea, move first toward the northwest, and gradually curving round before they reach our latitude, take an easterly direction, as has been shown by Redfield and others.

The first practical application which was attempted of the principle we have mentioned was made by this Institution in 1856; the information conveyed by telegraphic despatches in regard to the weather was daily exhibited by means of differently colored tokens, on a map of the United States, so as to show at one view the meteorological condition of the atmosphere over the whole country. At the same time publication of telegraphic despatches was made in the newspapers. The system, however, was necessarily discontinued at the beginning of the war, and has not yet been resumed. Similar applications have since been made in other countries, particularly in England, under the late Admiral Fitzroy; in France, under Leverrier; and still later, in Italy. In the last-mentioned country tabular statements are to be published annually, comparing the predictions with the weather actually experienced.

The British government has also recently introduced the system of telegraphic meteorological predictions into India. The cyclone of October, 1864, which did such damage to the shipping in Calcutta and destroyed the lives of sixty thousand persons, called special attention to the subject. The Asiatic Society of Bengal estimated the cost of such a system at 67,000 rupees, (about $30,000,) a sum which the government hesitated to appropriate, though it decided to furnish the necessary instruments and an allowance of fifty rupees a month to the assistant at the telegraph station at Saugor, on the seaboard to the southward of Calcutta, in the direction from which the most severe storms approach that port.

It must be evident, from what we have said in regard to the movement of storms, that a system of telegraphic meteorological predictions would be at once more reliable and of more benefit to the eastern coast of the United States, than those made in England and France, on the western coast of Europe, could possibly be to those countries, since the disturbances of the atmosphere which reach them advance from the ocean, while the majority of those of a similar nature which visit especially the middle and eastern portions of our coast, come overland from a westerly or southwesterly direction, and their approach may be telegraphed in some cases many hours before their actual arrival.

But the expense of the proper establishment of a system of this kind can only be defrayed by the general government, or some organization in possession of more ample means than can be applied by the Smithsonian Institution to such a purpose. This will be evident from the fact which we have mentioned of the cost of the establishment of a similar system in India, and from a report of a committee of the two houses of Parliament appointed to consider certain questions relating to the meteorological department of the Board of Trade. From this it appears that the amount expended during the eleven years ending with 1865 was 45,000 pounds sterling, or an average of about $20,000 a year. The same committee recommend that meteorological observations at sea be continued under the direction of the hydrographic office of the admiralty, and an appropriation of £1,500 annually be made for instruments, and for discussion and publication of results, £1,700; making a total of £3,200. For weather statistics on land, the annual sum of £4,250, including instruments, discussion, and publications, is recommended, and for telegram storm warnings, £3,000; making a total annual expenditure of £7,450 for the land, and a grand annual total for land and sea of £10,450, or $52,250.

The present would appear to be a favorable time to urge upon Congress the importance of making provision for reorganizing all the meteorological observations of the United States under one combined plan, in which the records should be sent to a central depot for discussion and final publication. An appropriation of $50,000 annually for this purpose would tend not only to advance the material interest of the country, but also to increase its reputation. It would show that although the administration of our government is the expression of the popular volition, it is not limited in its operation merely to objects of instant or immediate utility, but that, with a wise prevision of the future, it withholds its assistance from no enterprise, however remote the results, which has for its end to advance the well-being of humanity.

It is scarcely necessary at this day to dwell on the advantages which result from such systems of combined observations as those which the principal governments of Europe have established and are now constantly extending. I may, however, in passing, briefly allude to some facts which may not at once occur to the mind of the general reader. They enable the mariner to shorten the time and diminish the danger of the passage from one port to another by indicating to him the route along which prevail, at a particular season of the year, the most favorable winds for his purpose. They also furnish the means by which the sailor is taught the important lesson, which has saved thousands of lives and millions of property, namely, that of finding the direction of the centre of the cyclone, and of determining the course in which he must steer in order to extricate himself from the destructive violence of this fearful scourge of the ocean. To the agriculturist they indicate the character of the climate of the country, and enable him, with certainty, to select the articles of culture best adapted to the temperature and moisture of the region, and which, in the course of a number of years, will insure him the most profitable returns for his labor. They furnish the statistics of the occurrence of sterile years and of devastating storms, which may serve as the bases on which to found insurance institutions for protection against the failure of crops, and thus give to the husbandman the same certainty in his pursuits as that possessed by the merchant or the ship-owner. They may also afford warning of the approach of severe frosts and violent storms in time to guard, at least in some degree, against their injurious effects. To the physician, a knowledge of such results as can be obtained from an extended system of observations is of great importance, not only in regard to the immediate practice of his art, but also to the improvement of his science. The peculiar diseases of a region are principally dependent on its climate; an extreme variation of temperature in a large city is invariably attended with an increase of the number of deaths. The degree and variation of the moisture at different times and in different places have also a great influence on diseases, and the more the means of studying the connexion of these elements and the corresponding condition of the human body are multiplied, the more will the art and the science of medicine be improved. I may mention that scarcely a week passes at the Institution in which application is not made for meteorological information relative to different parts of this country, with the hope to improve the condition, if not restore the health, of some patient. The knowledge, however, which at present exists as to the connexion of climate and disease, particularly in relation to our own country, is, in comparison to what might be obtained, of little significance.

No other part of the world can at all compare with this country in the conditions most favorable to the advancement of meteorology, by means of a well-organized and properly sustained system of combined observations; such a system extending from east to west more than two thousand miles would embrace in its investigation all the phenomena of the great upper current of the return trade-wind, which, continually flowing over us at a high elevation, carries most of the disturbances of the atmosphere eastward. It would also include the effects produced by the polar and equatorial currents as they contend for the mastery along the broad valley which stretches without interruption from the arctic circle to the Gulf of Mexico, and would settle with precision the influence of the great fresh-water lakes in ameliorating the climate of the adjacent regions. But above all, in a popular view, it would furnish the means more effectually than any other system of predicting the approach of storms and of giving the ships of our Atlantic coast due warning of the probability of danger.

Collections.—In the preceding reports an important distinction has been made between the collections of the Institution intended for the immediate advance and diffusion of a knowledge of natural history, and the museum intended for popular exhibition; while the former is in strict conformity with the catholic spirit of the bequest, and can be prosecuted in due relation to the various other branches of knowledge, having each an equal claim on the bounty of the fund, the latter is principally local in its character, and demands a perpetual outlay of a portion of the annual income, which tends continually to increase with the additions to the number of objects exhibited, and finally to absorb all the resources of the establishment. Although the museum has been principally restricted to the maintenance and exhibition of the articles of the exploring expeditions of the government, and to such type specimens as might serve to illustrate the publications of the Institution, yet its cost has exceeded that of all the active operations which have rendered the name of Smithson favorably known in every part of the civilized world. This statement will not be thought incorrect when it is considered that to its account may justly be charged the absorption of the annual interest of the money expended on the building, a sum which will now be greatly enhanced by the cost of the restoration.

I have thought it important to refer to this point in almost every annual report, in order that what I deem a fundamental policy of the Institution should be kept constantly in view, namely, the preservation of the income of the Smithsonian fund as untrammelled as possible, and free to be applied to assist in the solution of any scientific problem which may present itself, or in any other way to extend the present bounds of human knowledge. A well replenished purse, unincumbered with debt and free to be applied to any purpose, is a source of power as important to an establishment for the advance or diffusion of knowledge as it is to an individual.

By these remarks I do not intend to disparage the value of public museums; so far from this, I can freely say that I consider them of great importance as a means of intellectual improvement, of rational enjoyment, and as receptacles of interesting materials for the use of the student in any branch of learning. By the foregoing remarks I merely wish to urge the fact that an establishment of this kind, worthy of the seat of government of the United States, can only be supported by appropriations from Congress, and to express the opinion that so large an expenditure in the imperfect attempt to found a museum by means of the Smithson bequest will in time be abandoned, and the whole of the income devoted to the more cosmopolitan objects of the Institution.

In the last report a general account is given of what has been done by the Institution towards forming such collections, and during the past year this portion of the general operations has been carried on with unabated success. Although it might appear that the older settled portions of the country had been thoroughly laid under contribution, yet new questions are continually arising, and attention is devoted to less obvious features, as the subject of natural history is more minutely pursued. But still there are vast portions even of the United States which remain in a considerable degree unexplored. In the eastern portion the explorations in 1865, under the auspices of the Institution, have been chiefly confined to the collection of materials for the illustration of the work on North American Oology, in preparation by Dr. Brewer. In the west, the labors interrupted by the death of the lamented Captain Feilner have been resumed, under the direction of General Sully, in the neighborhood of Devil's lake, Dakota. From Idaho, collections made by Dr. Hitz have been received, and from Arizona the valuable series collected by Assistant Surgeon Coues. The latter consist of mammals, birds, reptiles, insects, and plants. Doctor Coues has also made minute notes on the habits and peculiarities of the animals of the region explored, and is now engaged upon a report of the results of his labors.

It has been frequently stated in previous reports that the Institution has entered into friendly relations with the officers of the Hudson's Bay Company, and, from the active co-operation which has taken place, large collections of interesting specimens relative to the natural history of the country, valuable meteorological observations, and ethnological records have been derived.

In March last the Institution received upwards of thirty large cases from this source, containing immense numbers of specimens illustrating the flora, fauna, and the anthropology of these northern regions. For the larger portion of these collections we are specially indebted to Mr. R. R. Macfarlane of Fort Anderson.

The services of Governor Mactavish, in forwarding his own and other valuable notes which accompany the collections, were also of great importance, and, as in previous cases, the boxes were delivered by the Hudson's Bay Company at Fort Garry, after thousands of miles of transportation, free of charges.

The opportunity afforded of adding to the collections of the Institution by the expedition to connect the United States and Russia by a line of telegraphic communication, was too important to be neglected. The directors of the company engaged in this enterprise early called upon the Smithsonian Institution for information in regard to the contemplated route, and besides receiving maps and other data relative to the geography and climate of the country, Mr. Kennicott, who had spent several years under the auspices of the Institution in Arctic America, was warmly recommended to them as a person well qualified to assist in the undertaking. The company also received valuable suggestions as to the best line from Professor Baird, and, in return for these services, facilities have been afforded, under Colonel Bulkley, in charge of the expedition, for making collections in natural history, &c., on a liberal scale. A prominent position was given to Mr. Kennicott in the survey, and a number of assistants were selected from young men who had also been in training in this Institution; the notes and collections which may be made by them will be transmitted to Washington for discussion and the publication of such results as may be important in the advance of science.

The telegraph company not only afforded facilities for making the collections, but also contributed, as did the Chicago Academy of Sciences, to lessen the expense to the Smithsonian fund in the purchase of the necessary articles comprising the outfit of the naturalists of the expedition. The first set of duplicate specimens will properly belong to the Chicago Academy, and the remainder will be distributed in the manner best adapted to facilitate the researches of those who are engaged in the study of the special branches of science to which the specimens may pertain.

In the southern part of the continent, explorations have continued to be made. Colonel A. J. Grayson, who has previously been a contributor, has examined the islands off the coast of Mexico, and has discovered a number of new species of birds; also Mr. Charles

Laszlo and Dr. Sartorius, to whom we have been indebted for meteorological observations, have continued their contributions to the natural history of Mexico.

A scientific survey of the Isthmus of Yucatan has been undertaken by Governor Salazar y Ilarregui, and, on the recommendation of the Institution, Dr. Arthur Schott was engaged to superintend the natural history branch of operations; and the collections, of which many packages have already reached us, are all to be sent to this Institution for examination previous to the publication of an account of the results obtained.

Dr. H. Berendt has commenced, under the auspices of the Institution, an exploration of British Honduras and the interior of Guatemala. The outfit of physical instruments and apparatus, and supplies, for collections of natural history, were principally furnished from the Smithsonian fund, while the personal expenses were borne by a subscription of a number of gentlemen interested in the advance of science, and by the Academies of Natural Sciences of Philadelphia and Chicago.

The explorations in Costa Rica by Dr. Von Frantzius and Mr. Carmiol have continued to add large numbers of specimens to our collections, no region of America, according to Professor Baird, having yielded of late years so many new species of birds.

Valuable collections have been made by Mr. Holland, Captain Dow, Mr. Hicks, and Mr. Chapman, in Central America.

From South America collections have been received from Mr. Walter S. Church and Professor W. E. Nation, in Peru; Dr. Hering, from Paramaribo; Mr. De Lacerda, Mr. Goodwin, and the Natural History Museum of Rio, in Brazil. For the latter donation, made by order of the Emperor of Brazil, we are specially indebted to the kindness of Mr. Lisboa, former Brazilian minister to the United States.

Large collections have also been received from the West Indies, principally made by Mr. Charles Wright, Mr. N. H. Bishop, Dr. Gundlach, Mr. March, Professor G. N. Allen, Mr. Robert Swift, and Mr. George Latimer, the deficiencies in our series from this region having been largely supplied during the year.

In accordance with the policy of the Institution, collections are not generally requested from the Old World, except in certain cases where they are desired for comparison by those who are engaged in special investigations. We have, however, during the last year, received a series of the eggs and skins of the birds of Palestine, presented by Mr. Tristram.

Distribution and use of Specimens.—The policy of rendering the specimens as conducive as possible to the immediate promotion of science has been constantly observed, and for this purpose not only have new specimens been furnished to those engaged in original investigations, but the duplicates of such as have been examined have been made up into sets for distribution.

From the materials principally furnished by the Institution, Dr. Gill has prepared a synopsis of North American seals, about to be published by the Essex Institute of Massachusetts, and has continued the description of new species of fishes. He has also examined the skulls of Mammals in the collection, and among them has made the remarkable discovery of a peculiar generic type of tapirs, still living on the Isthmus of Panama, but which has escaped the notice of previous investigators.

Ample materials have also been furnished Professor Baird for the preparation of an additional part of his review of American birds, and the means have been furnished Dr. Bryant, Mr. Cassin, and Mr. Lawrence, for prosecuting their investigations in relation to ornithology. A large number of reptiles have been sent to Professor Cope, of Haverford College, Pennsylvania, including a series from Arizona and Central America; the new species will be described in the proceedings of the Philadelphia Academy of Natural Sciences, and a more elaborate monograph of the whole is to be presented for publication to the Institution. Dr. Brewer has been supplied with eggs and nests of birds, for the continuation of his work on North American Oology, the first part of which was published in the eleventh volume of Smithsonian Contributions.

In all cases where assistance is thus extended to individuals in the prosecution of their particular studies, there is an implied understanding that full credit shall be given to the Institution for the facilities afforded. This condition has generally been properly observed, although in some few cases the acknowledgment has not been quite so explicit as the benefits received would appear to demand.

So extended has become the field of modern science that division of labor is here as essential as in the mechanic arts, and a mastery of principles and details can only be the reward of attention concentrated on a few branches; as, therefore, the organization of this Institution does not contemplate the support of a corps of professors engaged in a comprehensive cultivation of science in all its branches, but would rather invite the aid and procure the collaboration of those who may be disposed to render gratuitous service for the furtherance and advance of knowledge for its own sake, many of the specimens which are sent to us for identification, particularly those of a rare character, are forwarded for examination to persons at a distance. During the past year the insects received, as usual, have been placed in charge of the Entomological Society, of Philadelphia; the plants have been sent partly to Dr. Torrey, and partly to Dr. Gray, for study and arrangement. The minerals have been transmitted to the School of Mines of Columbia College, New York. In all cases the specimens are to be assorted and labelled, the most perfect suite to be returned to the Institution, and the remainder properly divided into sets for distribution.

References have also been made during the past year of questions in the line of natural history to Professor G. J. Brush, Isaac Lea, esq., Dr. Jos. Leidy, A. Agassiz, J. P. Lesley, Dr. Haldeman, Dr. Allen, Thos. Bland, W. G. Binney, G. W. Tryon, jr., Dr. W. Stimpson, Dr. J. Le Conte, S. H. Scudder, P. R. Uhler, W. H. Edwards, Edw. Norton, E. T. Cresson, and Dr. H. C. Wood, jr.

It is gratifying to be able to state that in no case has a favor of this kind been refused by any one qualified to render the desired service ; indeed the time and labor bestowed on the affairs of the Institution by its collaborators might well be a matter of surprise to those who are themselves scarcely influenced by other motives than the pursuit of gain. In this respect the liberalizing tendency of scientific studies is strikingly manifest.

In the winter of 1859–'60 Mr. P. P. Carpenter, a well known conchologist, who had been requested by the British Association for the Advance of Science to prepare a report on the shells of the northwest coast of America, visited Washington, and was requested to arrange and label the extensive collection of shells of the Smithsonian Institution, preparatory to the distribution of the duplicates. These consisted of the specimens collected by the United States Exploring Expedition, under Captain Wilkes, and other expeditions instituted by government, together with those collected by individuals under the direction of the Institution:

The work, however, proved far too extensive to be completed by Mr. Carpenter before his return home, and it was therefore concluded to send the collection to him in England, where he would have an opportunity of performing the work under the more favorable condition of comparison with the great collections of that country. The boxes were transported across the Atlantic gratuitously, through the kindness of Sir E. Cunard, passed through the English custom-house without search, and arrived safely at the Warrington Museum, of which Mr. Carpenter was at the time one of the scientific curators.

"In one respect," says Mr. Carpenter, "it was fortunate for the future interests of American malacology that the work was undertaken at the time. It was part of the plan to duplicate, for American students, names in the celebrated Cumingian collection, the largest in the world, and containing the principal part of the types described and figured in the modern monographs. Mr. Cuming himself liberally and kindly undertook to compare the shells of the Institution with his own. This great and wearisome labor he performed gratuitously, in order that he might give a fair starting point to American students. The only expense was that of transportation and clerk-hire. Shortly after the completion of the work, Mr. Cuming died, to the irreparable loss of the students of malacology, to whom he was always ready to render assistance whenever the interests of science would be thereby advanced." "When the duplicates of the Smithsonian collection shall have been distributed to the schools, colleges, and museums of the American continent, and students not yet born shall be thereby enabled to make an accurate beginning in this interesting and useful branch of study, it is hoped that the name of Hugh Cuming will be remembered with grateful respect, as the man to whose incessant labors during a long life is due the gathering together of the largest series of known forms of shells, and to whom they owe the naming of the principal part of their collection."

Mr. Carpenter does not claim for the naming of these specimens entire freedom from error; the names given represent simply, neither more nor less, those in Mr. Cuming's cabinet, as identified by himself during the years 1861 to 1865, that is, the names of the more recent monographer, whether right or wrong. It is believed, however, that there is no other collection of shells on the continent of America of an equal number of species which can lay claim to even this moderate standard of accuracy.

The several objects to which Mr. Carpenter's attention was directed were as follows:

First. To make the permanent collection of the Institution as complete as possible by advantageous exchanges in England and elsewhere.

Second. To arrange the first-class duplicates of those which had a special scientific value for distribution to the establishments where they would be most generally useful.

Third. To make up the remaining specimens into series for distribution to colleges and other educational establishments.

Of the first-class series, the following distribution has been made, viz: Museum of Comparative Zoology, Cambridge; Academy of Sciences, Philadelphia; State Cabinet of Natural History, Albany; Geological Survey of Canada, Montreal; Academy of Natural Sciences, San Francisco. The specimens of these sets, as we have said before, are of great scientific value for original comparison in the way of determination of species, since they have been labelled after careful comparison by Mr. Carpenter and other authorities in conchology.

A very inadequate conception was at first entertained of the amount of labor which would be required to complete the assorting and labelling this part of the Smithsonian collection of natural history. It was thought that it might be completed in the course of a few months. Mr. Carpenter has, however, devoted to it about four years of continued labor. It must be observed at the same time that perhaps a considerable portion of this period was devoted to a completion of the series intended to be preserved in the Smithsonian museum.

The specimens examined by Mr. Carpenter have all been returned to the Institution, made up into sets, in accordance with the arrangement previously described. The number of these sets and of other collections of shells for distribution to colleges, academies, &c., is about 1,000, the whole including about 60,000 species and 250,000 specimens. Of these about one-fourth have been distributed.

According to the statement of Professor Baird, the whole number of specimens, including shells which have been distributed, amounts to upwards of 124,000, of which 19,000 have been presented to various establishments during the past year.

Museum.—As the public museum of the Institution occupied the portion of the building constructed of fire-proof materials, it escaped destruction by the fire; yet the smoke and water to which they were exposed caused some damage to the specimens, and much labor and expense were requisite to restore them to their proper appearance.

The museum has continued to be an object of interest to the citizens and visitors of Washington, and should the library of the Institution be transferred to the Capitol, space will be found in the cases of the west wing for increasing the number of articles placed upon exhibition. Among the collections we have a large number of specimens to illustrate ethnology. In addition to those collected by the Exploring Expedition under Captain Wilkes, are all those which have been obtained in the various explorations across the continent; from the Hudson's Bay Company; from the region of Nootka sound, Mexico, Central and South America. We regret that on account of the additional labor required in the renovation of the museum, and for want of space, these interesting collections have not yet been fully arranged and labelled for exhibition.

We may add, in this connexion, that if Congress shall adopt the proposition now under consideration to take charge of the library, we shall entertain the hope that it will in due time make provision for the establishment and care of a museum worthy the government of the United States, and thus relieve the Smithsonian fund of a burden to which, in strict accordance with what I have always conceived to be the proper interpretation of the will of the founder, it ought not to be subjected.

In the report for 1863 mention is made of the presentation of a large and remarkable meteorite to this Institution, to which the name had been assigned in California of the "Ainsa" meteorite. Acknowledgments were duly made to Dr. J. D. Irwin, surgeon United States army, for his services in behalf of the Institution in procuring this interesting specimen; but from additional facts which have come to our knowledge we are induced to add the name of this gentleman to the specimen, and to label it the "Irwin-Ainsa meteorite."

For an account of the work done in the museum, a complete list of donors to the museum and collections since the commencement of the Institution, I would refer to the annexed statement of Professor Baird.

Laboratory and experiments.—The conflagration which destroyed the cabinet of apparatus of physics did not extend to the chemical laboratory, and consequently the operations connected with the latter have not been interrupted. The series of experiments which were mentioned in the last report in relation to the examination of the air of the Capitol has been continued by Dr. C. M. Wetherill, and a very elaborate report prepared on the subject to be submitted to the Secretary of the Interior. This report not only gives the result of the observations made in the halls of Congress, and the experiments at this Institution, but also a synopsis of all the authentic facts from the bibliography of the subject, which it is believed will be considered of value to those who are practically engaged in those departments of mechanical construction which require attention to temperature and ventilation. From the investigations it appears that there is an abundant supply of fresh air forced into the chambers of the Capitol, but that in winter this is greatly deficient in the quantity of moisture necessary to form a comfortable and salubrious atmosphere. It must be evident that if in an isolated space in winter we desire to have air of the same salubrity and temperature as in an open space in pleasant weather in summer, we must artificially impart to this air not only the same degree of heat, but also an equal amount of moisture.

In an atmosphere entirely devoid of moisture the human body exhales with great energy from every pore of the skin, and especially from the lungs, and all the more delicate parts of the mucous membrane. Such an exhalation of moisture in the ordinary condition of the body would be far too great to allow of a healthy equilibrium between the natural excretion and assimilation, and it is with prevision, derived from long experience, that the savage anoints his body when exposed to an atmosphere rendered arid by refrigeration, to prevent excessive exhalation. The tendency of vapor to exhale from the body diminishes with the force of vapor already in the surrounding atmosphere, and a perfect equilibrium can only take place with air entirely saturated, at a temperature of 98°. But at the temperature of zero almost all the moisture of the atmosphere is condensed, and hence the tendency of the body in the open air in winter to give off its vapor would be excessive, were it not for the condensation of the vapor immediately around the body and retained in the interstices of the clothing. If, however, the surrounding air, without additional moisture, be heated to 70°, all obstruction to evaporation is removed, and excessive exhalation is the consequence. Beside this the equalization of the temperature is much more perfectly effected in case of air properly supplied with moisture.

These considerations, we think, are very much neglected in the processes which are adopted for warming and ventilating in this country. At least in the public buildings in the city of Washington, the means of supplying an adequate amount of moisture have not been provided, or, if provided, are not habitually employed. It is intended, however, during next winter, to continue the observations in regard to this matter, and to extend them to the principal public buildings connected with the government.

It has been mentioned in previous reports that a series of experiments was made in regard to the physical qualities and economical values of different kinds of illuminating materials for light-house purposes. Unfortunately all the notes of the experiments which had been made on this subject were destroyed in the fire, and during the past year the principal portion of time which I could spare from other duties was devoted to the reproduction and extension of the results previously obtained.

During the past year I devoted about three weeks, in connexion with Commodore Powell and Mr. Lederle, of the light-house service, to investigations in relation to sound as applicable to fog signals, and obtained results of sufficient practical value to determine with considerable precision the policy of the Light-house Board in regard to this branch of aids to navigation.

As usual, various questions have been referred to the Institution by different departments of the government for solution, and these have in all cases received proper attention, and such reports have been made as would suffice to an intelligent decision in regard to them.

The Institution is constantly applied to for the examination and analysis of specimens of ores. Where such examination requires no special labor, the information has been gratuitously given; but where quantitative analysis is desired, and the information is for the advancement of private interests, a charge is always made sufficient to repay the actual cost of the investigation.

Exchanges.—The system of international scientific and literary exchanges has been maintained and extended by the Institution during the past year. Seventy-seven large boxes, containing 1,176 parcels, were sent to our foreign agents in 1865, and 60 boxes, containing 5,000 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. The cost of this branch of the general operations is very large, and would indeed far exceed the means of the Institution were it not for the liberal aid received from various parties interested in the advance of science. For favors of this kind thanks continue to be due to the Bremen, the Hamburg, the Cunard, and the Pacific steamship lines, and to the Panama Railroad Company; all these have generously transported the packages of the Institution free of cost. Acknowledgments are due to the Adams, Harnden, and Wells & Fargo express lines for the carriage without charge of smaller packages, and of larger ones at a very reduced rate. To Sir E. Cunard, F. Probst & Co., Oelrichs & Co., and Kunhardt & Co., of New York, and to Leffman & Gutheil, of Vera Cruz, thanks have been tendered for important privileges granted by them or through their influence. Mr. George Hillier, of New York, and Mr. Samuel Hubbard, of San Francisco, agents of the Institution in those cities, have continued their valuable services.

It may be proper to state that great care is exercised in the instructions which have been given to our agents to transmit nothing in the boxes to the Institution which is not a donation, all purchases being excluded on account of the present tariff regulations.

For the statistics of the exchanges see the annexed tables in the report of Professor Baird.

Library.—The library has received during the past year, through our system of exchanges, 547 octavo, 201 quarto, and 19 folio volumes, 3,256 pamphlets and parts of volumes, and 183 maps and charts, making a total of 4,206.

The work on the catalogue of transactions of learned societies and of scientific journals has advanced so far that all those of foreign countries have been finished, while those of America are now in the hands of the printer.

The suggestion has been made in previous reports that considerable relief might be afforded to the Institution by the transfer of its library, under certain conditions, to the new and spacious halls which Congress is providing for its own library, and the importance of the proposition has been much enhanced by considerations connected with the recent disaster. The west wing of the building, in which the library is now contained, is not fire-proof, and is already filled to overflowing. To provide another depository for it, which shall render it entirely secure from fire and be sufficient for its continued increase, will far exceed the means of the Institution, and, although some inconvenience would be experienced in regard to ready access to the books, yet, in consideration of the great value of the collection, by far the most perfect of its kind in the United States, it has been thought proper to ask Congress to allow the deposit of this library to be made in one of the new fire-proof rooms preparing for the extension of its own collection of books.

I am informed by Mr. Spofford, the librarian of Congress, that these two new rooms will be sufficient to accommodate the Smithsonian library and to furnish space for the growth of the Congressional library for the next fifteen or twenty years. The object of the transfer is, of course, not to separate this unique and highly prized collection of books from its relations to the Smithsonian Institution, for it must still bear its name and be subject to its control, but merely to deposit it where its preservation will be more certain and its usefulness more extended.*

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* Since the preparation of this report an act of Congress has been passed authorizing the deposit of the library in one of the new rooms of the Capitol. This arrangement, while it secures the safety of the books, will facilitate the researches of the student, since, in the same suite of apartments, he will have free access to two libraries. (See Proceedings of the Board in this volume.)

"*Personnel*" *of the Institution.*—In the annual reports for a number of years past no other account has been given of the *personnel of* the Institution than a reference to the principal assistants or a casual allusion to the others; but as special inquiry on this point has been made by some of the new Regents, and as some changes will probably take place on account of the fire and the transfer of the library, it is deemed advisable on this occasion to give the official position and the duty of the several persons connected with the establishment. This information is perhaps the more necessary in order to prevent misapprehension, and in some cases to protect the public from the representations of designing persons, who, though never having had any connexion with the Institution, or only a very temporary or subordinate one, have assumed to belong to the corps of its officers.

The act of Congress which organized the Institution directed the appointment of but one executive officer, who, under the name of the Secretary, should have "charge of the building and property of said Institution," be keeper of the museum, and perform the duties of librarian, thus confiding to him the general direction of affairs, under the control of the Board of Regents, and investing him with the sole responsibility for the judicious and the efficient conduct of all transactions of the Institution within the prescribed conditions. In order, however, better to enable him properly to discharge the important and arduous duties devolved upon him, he is allowed, with the consent of the board, "to employ assistants." By the adoption of such an arrangement it was no doubt intended to secure unity of action and efficiency of co-operation among all who might be actually engaged in carrying out the novel and interesting objects of the bequest. The importance of this provision of the law was, however, either not apparent at first or was lost sight of in the early proceedings of the board. The Secretary, instead of being allowed the selection of assistants upon his own judgment and responsibility, when permission had once been obtained for making the appointments, was required to submit his choice to the approval of the Regents, and thus, in a considerable degree, to abridge his power of control. As might have been anticipated, this deviation from the original intention of the act did not succeed in practice; dissensions soon arose as to the exact apportionment of the income by the Secretary to the several objects of the programme, in which the superintending parties were differently interested, as well as in regard to the direction which each assistant might exercise as head of a department, and maintaining a separate and independent direction and official correspondence. To obviate these difficulties, and to determine for himself and his successors the character of his official position, the Secretary was constrained to resort to a rightful exertion of his authority, as plainly expressed in the law, and, without an appeal to the board, to reorganize his corps of assistants. The majority of the Regents fully approved of this course, and, to prevent difficulty in the future, repealed the regulation relative to the division of the income, adopting at the same time a resolution offered by the late Judge Douglas, that no official letter or communication pertaining to the affairs of the Institution should be written except under the authority and by the direction of the Secretary. Under this arrangment, which has now been in operation twelve years, all the affairs of the Institution have been conducted with harmony, and, as I venture to think, with an efficiency which evinces the wisdom of the provision for securing conformity of purpose and unity of action.

In the reorganization of the personnel of the establishment, the Secretary employed as his first assistant Professor Spencer F. Baird, and as chief clerk William J. Rhees, each of whom receives a permanent salary, affixed by a resolution of the Board of Regents to their respective offices. Other assistants are employed on such terms as the Secretary may think just.

It is evident, from the plan of organization, that the duty required of the assistants is of a clerical rather than a scientific character, since service of the latter kind, as has been fully shown, can always be obtained from the collaborators without charge, or from experts employed for the occasion and paid in proportion to the time of engagment. Although the terms of the reappointment of Professor Baird prescribe that he shall render assistance in any line of duty in which the Secretary may require his aid, yet on account of his zeal in the cultivation of natural history, and his skill and experience in the collection and arrangement of specimens, his labors have principally been assigned to such objects. He has likewise had charge of the business of exchanges, and in part also of the printing and correspondence. To Mr. Rhees has been assigned the duty of superintending the accounts and auditing all the bills of expenditures made by the Secretary and authorized under the general appropriation of the Regents, while he has at the same time acted as paymaster and assisted in the official correspondence.

Another assistant, Mr. William Q. Force, of this city, who has been for several years connected with the Institution, has charge of the meteorological materials which are constantly accumulating, and of the preparation of the monthly account of the weather for publication in the bulletins of the Agricultural Department; one half of his salary is paid by the Commissioner of Agriculture and the other half from the Smithson fund.

An assistant is also required to attend to the care of the books in the library, and another to prepare the catalogue of the transactions of learned societies now in the press, and to perform the clerical duties connected with the system of foreign exchange. The former service for the last few years has been rendered by Dr. Theodore Gill, who, at the same time, has continued his investigations in regard to zoology. The latter duty has been performed by Miss Jane Turner, the sister of the lamented Professor Turner, whose name occurs so frequently in the previous reports of the Institution.

As one of the maxims to be observed in the policy of the Institution, but few persons are to be permanently supported by its funds. This maxim has been strictly observed in regard to the principal assistants, but it must be evident that, in the case of so large a building and its contents, and so many and various operations to be attended to, a number of employés will be constantly required. The connexion of these, however, with the Institution is considered of a temporary character, their numbers being increased or diminished as circumstances may indicate. Of this class the following is an enumeration of those employed before the fire. The number has since been reduced:

1. A janitor, who resides in the building, on the grounds, gives information to visitors, acts as messenger, and has a general care of the property at all times. 2. A curator of the museum, who keeps the specimens in a proper condition for exhibition, performs the work of a taxidermist in setting up specimens of birds, mammals, and other animals. 3. A machinist, who assists in preparing experiments for the illustration of lectures or for original research, who has charge of the apparatus, the gas and water pipes, the furnaces, and the repairs of the metal parts of the building. 4. A carpenter, to make cases, trays, packing boxes, furniture, and to attend to the repairs constantly required for the building. It has been found from experience that the salaries of the last two employés are much less than the annual cost of repairs of carpentry and metal work by the employment of outside parties. 5. Night watchmen. Previous to the fire but one of these was employed, but it was found that the service was too much for the health of an ordinary individual, and consequently it has been necessary to employ another, the whole

twenty-four hours being nearly divided between them. 6. Laborers, daily required for cleaning the building after the hours in which it is open to the public, attending to fires, &c.

Besides the assistants who receive a salary, a large amount of labor has been given the Institution, without pay, by persons interested in the study of natural history, or who have been engaged in explorations and make use of the facilities afforded by the library and collections in preparing their reports. To most of these, rooms, warmed and lighted, have been assigned in the building, and lodging apartments in the towers. Among these we may mention the names of Mr. F. B. Meek, Dr. W. Stimpson, Dr. Gill, Mr. R. Kennicott, Dr. E. Coues, Dr. H. Allen, Dr. E. D. Cope, and Mr. A. D. Brown.

The laboratory has been under the care, for the last few years, of Dr. B. F. Craig, now of the medical department of the army, and also of Dr. Wetherill, just appointed Professor of Chemistry in the new college at Bethlehem, Pennsylvania. The remuneration they received was either from the government or from private parties, on account of researches principally of an economical character.

For the purpose of conducting the foreign exchanges, it has been found necessary to have agents in such central positions as may enable us to distribute the books and specimens most effectually to various parts of the world, and to collect the returns intended for this country. The following are the agents at present employed: Dr. Felix Flugel, Leipsic; Gustave Bossange, Paris; William Wesley, London; Fred. Muller, Amsterdam.

The collaborators, to whom references have so frequently been made, include all the prominent cultivators of original science in this country. They have all, with scarcely an exception, rendered assistance in supporting, directing, and advancing the Institution. Its opponents have been mainly those who have been misinformed as to its character and labors, or have been disappointed in the desire to advance personal interests through its means.

Respectfully submitted.

JOSEPH HENRY,
Secretary Smithsonian Institution.

JANUARY, 1866.

APPENDIX TO THE REPORT OF THE SECRETARY.

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

During the past year 678 principal packages of various kinds—boxes, bales, or bundles—were received at the Institution, and 508 sent out from it by the different express, railroad, or steamship lines.

The figures given above, however, do not represent the entire number of parcels received by the Institution, or made up and transmitted by it, since, as will be seen by the subsequent tables, seventy-seven alone of the parcels sent out were boxes for our agents of exchange, containing 1,176 parcels, thus increasing the preceding enumeration by 1,100. Again, of the packages received 60 were boxes from Europe, containing at least 5,000 parcels, making a total of at least 5,600 received, and of about 1,600 transmitted.

As usual, the principal statistics connected with the exchanges of the Institution will be given in a series of tables, marked, respectively, A, B, C, and D.

A.

Receipts of books, &c., by exchange, in 1865.

Volumes, Octavo	547	
Quarto	201	
Folio	19	
		767
Parts of volumes and pamphlets:		
Octavo	2,070	
Quarto	979	
Folio	207	
		3,256
Maps and charts		183
Total		4,206

As the corresponding receipts in 1864 amounted to 3,686, it will be seen that 1865 exhibits an increase of over 500 volumes and parts of volumes, thus indicating that this source of supply to the library of the Institution exhibits no symptom of diminution.

B.

Table showing the statistics of the exchanges of the Smithsonian Institution in 1865.

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	19			
Norway	7	11			
Denmark	9	14			
Iceland	1	2			
Russia	41	63			
Germany	293	426			
Switzerland	28	42			
Total	390	577	34	254	9,920
FREDERIC MÜLLER, *Amsterdam*—					
Holland	41	58			
Belgium	11	23			
Total	52	81	6	44	1,320
GUSTAVE BOSSANGE & CO., *Paris*—					
France	89	164			
Italy	49	75			
Spain	7	12			
Portugal	3	6			
Total	148	257	15	108	3,440
W. WESLEY, *London*—					
Great Britain and Ireland	140	273			
Africa	3	3			
China	2	4			
Australia	9	13			
Total	154	293	10	103	3,000
Rest of the world	39	68	12	48	950
Grand total	783	1,176	77	557	18,630

C.

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

	Number of packages.
Albany, N. Y.—	
New York State Agricultural Society	72
Boston, Mass.—	
American Academy of Arts and Sciences	131
Board of State Charities	1
Boston Society of Natural History	308

Cambridge, Mass.—	
Museum of Comparative Zoology	325
Nautical Almanac	90
Alexander Agassiz	55
Columbus, Ohio.—	
Ohio State Board of Agriculture	95
Dorchester, Mass.—	
Dr. Edward Jarvis	9
Des Moines, Iowa.—	
State of Iowa	5
Janesville, Wis.—	
Institution for the Blind	50
Madison, Wis.—	
Wisconsin State Agricultural Society	6
New Haven, Conn.—	
American Journal of Science	21
Professor J. D. Dana	1
Professor A. E. Verrill	4
New York, N. Y.—	
New York Lyceum of Natural History	110
Philadelphia, Penn.—	
Academy of Natural Sciences	176
American Philosophical Society	200
Historical Society of Pennsylvania	21
Portland, Maine.—	
Natural History Society	20
San Francisco, Cal.—	
California Academy of Natural Sciences	50
St. Louis, Mo.—	
St. Louis Academy of Sciences	16
Dr. George Engelmann	5
Toronto, Can.—	
Canadian Institute	4
Washington, D. C.—	
United States Patent Office	494
United States Coast Survey	315
National Observatory	135
National Academy of Sciences	39
Theodore Gill	5
	2, 563

D.

Addressed packages received by the Smithsonian Institution from Europe, for distribution in America, in 1865.

	No. of packages.
ALBANY, NEW YORK.	
Albany Institute	3
Dudley Observatory	14
New York State Agricultural Society.	24
University of the State of New York.	2
State Library	31
State Medical Society	2
Prof. J. Hall	7
Franklin B. Hough	1
Colonel Jewett	1
AMHERST, MASSACHUSETTS.	
Amherst College	13
Prof. C. U. Shepard	4
ANN ARBOR, MICHIGAN.	
Observatory	5
University of Michigan	1
Dr. Brunnow	1
Dr. J. C. Watson	2
Prof. A. Winchell	1
ATHENS, OHIO.	
Ohio University	1
AUBURN, NEW YORK.	
New York State Lunatic Asylum	1
AUGUSTA, MAINE.	
State Lunatic Hospital	2
AUSTIN, TEXAS.	
Lunatic Asylum	1
BALTIMORE, MARYLAND.	
Maryland Historical Society	4
Maryland Hospital for Insane	2
Mount Hope Institution	1
Dr. John G. Morris	3
Dr. A. Paetsch	1
BLACKWELL'S ISLAND, NEW YORK.	
New York City Lunatic Asylum	2
BLOOMINGTON, INDIANA.	
Indiana State University	1
BOSTON, MASSACHUSETTS.	
American Academy of Arts and Sciences	101
American Statistical Association	1
BOSTON, MASS.—Continued.	
Board of Agriculture	1
Boston Lunatic Hospital	2
Boston Society of Natural History	135
Bowditch Library	3
Geological Survey of Massachusetts	2
Historical Society of Massachusetts	2
New England Historico-Genealogical Society	2
North American Review	4
Prison Discipline Society	2
Public Library	18
State Library	14
F. Alger	1
Alvan Clarke	1
Dr. John Dean	1
Mrs. E. Everett	1
Prof. J. D. Everett	2
Colonel J. D. Graham	1
Dr. B. A. Gould	1
A. A. Hayes	1
John R. Motley	9
Prof. W. B. Rogers	1
S. H. Scudder	4
BRATTLEBORO', VERMONT.	
State Lunatic Asylum	2
BRUNSWICK, MAINE.	
Bowdoin College	9
Historical Society of Maine	2
BURLINGTON, NEW JERSEY.	
W. G. Binney	1
BURLINGTON, VERMONT.	
University of Vermont	2
CAMBRIDGE, MASSACHUSETTS.	
American Association for Advancement of Science	29
American Ephemeris and Nautical Almanac	2
Astronomical Journal	1
Harvard College	23
Museum of Comparative Zoology	4
National Academy of Sciences	10
Observatory of Harvard College	26
Perkins's Institution for Blind	1
Alex. Agassiz	1
Prof. L. Agassiz	42
Prof. G. P. Bond	1

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

	No. of packages.
CAMBRIDGE, MASS.—Continued.	
Prof. H. J. Clark	1
Prof. Ferrel	1
Prof. W. Gibbs	1
Dr. B. A. Gould	7
Colonel J. D. Graham	2
Prof. Asa Gray	14
G. W. Hill	1
T. Lyman	1
Prof. Jules Marcou	2
A. Ordway	1
Prof. B. Peirce	4
T. H. Safford	3
Prof. J. Winlock	3
J. E. Worcester	7
Prof. Wright	1
Prof. Wyman	1
CHAPEL HILL, NORTH CAROLINA.	
University of North Carolina	1
CHICAGO, ILLINOIS.	
Chicago Academy of Sciences	1
Mechanics Institute	1
CINCINNATI, OHIO.	
Cincinnati Lancet	1
Dental Register of the West	3
Historical and Philosophical Society of Ohio	1
Mercantile Library	1
Observatory	4
Prof. G. A. Schmidt	1
CLINTON, NEW YORK.	
Observatory of Hamilton College	5
Dr. C. H. F. Peters	5
COLUMBIA, SOUTH CAROLINA.	
State Lunatic Asylum	1
COLUMBIA, MISSOURI.	
Geological Survey of Missouri	12
State Library	1
University	1
COLUMBUS, OHIO.	
Lunatic Asylum	1
Ohio State Board of Agriculture	68
Leo Lesquereaux	1
W. S. Sullivant	1

	No. of packages.
CONCORD, NEW HAMPSHIRE.	
New Hampshire Historical Society	2
State Lunatic Asylum	2
DAYTON, OHIO.	
Southern Ohio Lunatic Asylum	2
DELAWARE, OHIO.	
Ohio Wesleyan University	1
DES MOINES, IOWA.	
State Library	21
DETROIT, MICHIGAN.	
Michigan State Agricultural Society	10
DORCESTER, MASSACHUSETTS.	
Dr. E. Jarvis	2
ELIZABETH, NEW JERSEY.	
O. A. Brownson	1
ERIE, PENNSYLVANIA.	
Rev. L. Olmstead	2
FALL RIVER, MASSACHUSETTS.	
Niels Arnsen	1
FLATBUSH, LONG ISLAND, N. Y.	
Kings County Lunatic Asylum	1
FRANKFORD, PENNSYLVANIA.	
Friends' Asylum for Insane	1
FRANKFORT, KENTUCKY.	
Geological Survey of Kentucky	10
FULTON, MISSOURI.	
Lunatic Asylum	1
GAMBIER, OHIO.	
Kenyon College	3
Prof. H. L. Smith	1
GEORGETOWN, D. C.	
Georgetown College	9
Dr. A. Schott	1
Mrs. Arthur Schott	1

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

	No. of packages.
GERMANTOWN, PENNSYLVANIA.	
Dr. Theiss	1
GREENCASTLE, INDIANA.	
Indiana Ashbury University	1
HAMDEN, MASSACHUSETTS.	
Rev. C. W. Everest	1
HAMILTON, NEW YORK.	
Madison University	1
HANOVER, NEW HAMPSHIRE.	
Dartmouth College	10
HARRISBURG, PENNSYLNANIA.	
State Lunatic Hospital	2
State Library	1
HARTFORD, CONNECTICUT.	
Historical Society of Connecticut	1
Retreat for Insane	1
Trinity College	7
Young Men's Institute	1
Hon. H. Barnard	1
HOPKINSVILLE, KENTUCKY.	
Western Lunatic Asylum	1
HUDSON, OHIO.	
Western Reserve College	6
INDIANAPOLIS, INDIANA.	
Indiana Historical Society	1
Indiana Hospital for Insane	1
Prof. Kirkwood	1
INMANSVILLE, WISCONSIN.	
Scandinavian Society	1
IOWA CITY, IOWA.	
Iowa State University	14
Prof. Hinrichs	4
IRVINGTON, NEW YORK.	
Clarence King	1
JACKSON, LOUISIANA.	
Insane Asylum	1
JACKSON, MISSISSIPPI.	
State Lunatic Asylum	1
JACKSONVILLE, ILLINOIS.	
Illinois State Lunatic Hospital	1
JANESVILLE, WISCONSIN.	
State Institution for the Blind	4
KALAMAZOO, MICHIGAN.	
Michigan Hospital for Insane	1
LANSING, MICHIGAN.	
State Agricultural College	1
LEBANON, TENNESSEE.	
Cumberland University	1
LEWISBURG, PENNSYLVANIA.	
University	1
LEXINGTON, KENTUCKY.	
Eastern Lunatic Asylum	2
LOUISVILLE, KENTUCKY.	
Historical Society of Kentucky	2
University	6
J. L. Smith	2
LYNN, MASSACHUSETTS.	
Miss Maria Mitchell	2
MADISON, WISCONSIN.	
Historical Society of Wisconsin	4
Observatory	1
Skandinaviske Presseforening	1
State Library	5
University	1
Wisconsin State Agricultural Society	12
MIDDLETOWN, CONNECTICUT.	
Wesleyan University	1
MILL CREEK, OHIO.	
Lunatic Asylum	1

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

	No. of packages.
MILLEDGEVILLE, GEORGIA.	
State Lunatic Asylum	1
MILLTOWN, MAINE.	
George A. Boardman	1
MONTPELIER, VERMONT.	
State Library	9
MONTREAL, CANADA.	
Natural History Society	4
MOUNT PLEASANT, IOWA.	
Iowa Wesleyan University	1
NASHVILLE, TENNESSEE.	
State Lunatic Asylum	2
University	1
NEW BRUNSWICK, NEW JERSEY.	
Geological Survey of New Jersey	8
NEWBURG, OHIO.	
Lunatic Asylum	1
NEW HAVEN, CONNECTICUT.	
American Journal of Science and Arts	39
American Oriental Society	19
Yale College	21
Frank H. Bradley	1
E. J. Chapman	1
Professor J. D. Dana	30
Professor E. Loomis	8
Professor O. C. Marsh	3
Professor H. A. Newton	3
Professor B. Silliman	17
Professor Twining	1
Professor W. D. Whitney	1
NEW OXFORD, PENNSYLVANIA.	
Dr. G. Pfeiffer	1
NEW ORLEANS, LOUISIANA.	
New Orleans Academy of Sciences	21
NEWPORT, RHODE ISLAND.	
United States Naval Academy	1

	No. of packages.
NEW YORK, NEW YORK.	
American Ethnological Society	5
American Geographical and Statistical Society	27
American Institute	12
American Missionary Society	1
Astor Library	12
Bloomingdale Asylum	1
Columbia College	7
Historical Society	1
Mercantile Library Association	2
New York Academy of Medicine	1
New York Journal of Medicine	1
New York Lyceum of Natural History	58
University of the City of New York	5
Professor Chandler	1
Dr. J. W. Draper	5
Professor T. Egleston	4
General J. C. Frémont	6
H. Grinnell	7
Mr. Harlan	2
G. N. Lawrence	3
C. F. Loosey, (consul general, Austria)	1
Colonel C. B. Norton	5
Edward Norton	1
Baron Ostensacken, (consul general, Russia)	3
Temple Prime	2
E. G. Squier	3
NORTHAMPTON, MASSACHUSETTS.	
State Lunatic Asylum	1
B. S. Lyman	2
J. D. Whitney	4
NORWICH, VERMONT.	
Norwich University	1
OSWEGO, NEW YORK.	
Raphael Pumpelly	6
OXFORD, OHIO.	
Miami University	1
PEORIA, ILLINOIS.	
Dr. F. Brendel	2
PHILADELPHIA, PENNSYLVANIA.	
Academy of Natural Sciences	150
American Pharmaceutical Society	11
American Philosophical Society	88

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

	No. of packages.
PHILADELPHIA, PENN.—Continued.	
Central High School	2
Central High School Observatory	3
Dental Cosmos	1
Entomological Society of Philadelphia	3
Franklin Institute	21
Girard College	1
Historical Society of Pennsylvania	4
Library Company	2
Pennsylvania Institute for the Blind	1
Pennsylvania Horticultural Society	8
Penn's Hospital	1
State Lunatic Hospital	1
Wagner Free Institute	13
C. A. Blake	1
Miss Bouvier	1
Breckinridge Clemens	1
H. C. Carey	2
John Cassin	3
E. D. Cope	2
E. T. Cresson	1
W. M. Gabb	1
A. B. Grote	1
Dr. Isaac Hays	2
Dr. Isaac Lea	7
Dr. John Le Conte	4
Professor J. Leidy	8
Professor J. P. Lesley	3
B. S. Lyman	1
B. V. Marsh	2
Dr. J. Aitken Meigs	6
Professor Morton	1
Franklin Peale	1
Dr. W. Sharswood	4
George W. Tryon, jr.	11
Professor Wagner	1
PINE LAKE, WISCONSIN.	
Scandinavian Society	1
PITTSBURG, PENNSYLVANIA.	
Western Pennsylvania Hospital for Insane	1
PORTLAND, MAINE.	
Society of Natural History	1
Neal Dow	1
PRINCETON, NEW JERSEY.	
College of New Jersey	8
Professor Alexander	1
A. D. Brown	2
Professor A. Guyot	3
PROVIDENCE, RHODE ISLAND.	
Brown University	8
Butler Hospital for Insane	2
Rhode Island Historical Society	2
Professor Caswell	1

	No. of packages.
QUEBEC, CANADA.	
Observatory	2
RALEIGH, NORTH CAROLINA.	
Insane Asylum	1
ROCHESTER, NEW YORK.	
University of Rochester	1
Professor Dewey	1
P. L. Holzer	2
Dr. Ward	1
SALEM, MASSACHUSETTS.	
Essex Institute	2
A. S. Packard	1
SAN FRANCISCO, CALIFORNIA.	
California Academy of Natural Sciences	29
Geological Survey	1
W. P. Blake	5
Mr. Trask	1
Dr. Wedekind	1
ST. LOUIS, MISSOURI.	
Deutscher Institut für Befördering von Wissenschaften	6
St. Louis Academy of Sciences	108
St. Louis University	2
J. G. Bernays	1
Dr. G. Engelmann	9
Dr. Adam Hammer	2
N. Holmes	1
Mr. Schuster	1
Dr. B. F. Shumard	14
Professor G. C. Swallow	1
Dr. H. A. Prout	1
SOMERVILLE, MASSACHUSETTS.	
McLean Asylum	1
SPRINGFIELD, ILLINOIS.	
Professor Esbjörn	1
STAUNTON, VIRGINIA.	
Western Lunatic Asylum	1
STOCKTON, CALIFORNIA.	
California State Lunatic Asylum	1
TAUNTON, MASSACHUSETTS.	
Massachusetts State Lunatic Hospital	2

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

	No. of packages.
TRENTON, NEW JERSEY.	
New Jersey State Lunatic Asylum	2
TORONTO, CANADA.	
Canadian Institute	4
Rev. Charles J. S. Bethune	4
URBANA, OHIO.	
Urbana University	1
UTICA, NEW YORK.	
State Lunatic Asylum	2
VANDALIA, ILLINOIS.	
Historical Society of Illinois	1
WASHINGTON, D. C.	
Army Medical Museum	4
Bureau of Ordnance and Hydrography	1
Department of Agriculture	29
Engineer Bureau	1
German Relief Association	28
Government Hospital for Insane	2
Library of Congress	5
National Observatory	98
Secretary of War	1
Statistical Bureau	1
Surgeon General's Office	23
United States Coast Survey	36
United States Patent Office	128
War Department	3
Prof. A. D. Bache	29
Prof. S. F. Baird	12
Gen. J. G. Barnard	1
WASHINGTON, D. C.—Continued.	
M. S. Bebb	2
Admiral C. H. Davis	1
General W. H. Emory	1
S. Ferguson	1
George Gibbs	1
Prof. T. Gill	3
Capt. J. M. Gilliss, U. S. N.	2
Mrs. Gillis	1
Dr. F. V. Hayden	1
M. Heilprin	2
J. E. Hilgard	1
J. C. G. Kennedy	2
Professor Newcomb	1
Dr. Peter Parker	1
Mrs. Schoolcraft	5
Charles A. Schott	1
Dr. W. Stimpson	6
WATERVILLE, MAINE.	
Waterville College	3
WEST POINT, NEW YORK.	
U. S. Military Academy	1
Captain J. H. Chase	1
WILLIAMSBURG, VIRGINIA.	
Eastern Lunatic Asylum	2
WORCESTER, MASSACHUSETTS.	
American Antiquarian Society	8
Massachusetts State Lunatic Hospital	2
ZELIENOPLE, PENNSYLVANIA.	
G. C. Holls	1

Total of addresses	345
Total of parcels	2,368

Additions to the collections of the Institution.

The total number of different donations in the year has amounted to one hundred and fifty-five, contained in two hundred and fifty-seven packages. This number is not equal to that of some previous years, as shown by the following tables; but the character of the collections received is in no way inferior, and embraces much new and unworked material.

Table showing the number of donations to the collections since 1859.

Received in 1859,	of different donations,	302
" 1860,	"	404
" 1861,	"	157
" 1862,	"	124
" 1863,	"	241
" 1864,	"	171
" 1865,	"	155

The table of receipts for the year, however, by no means expresses the resources in this respect of the Institution.

In addition to the mechanical work of cleaning the specimens which were exposed during the fire, and in restoring labels, &c., considerable progress has been made in the identification and systematic arrangement of the specimens. All the collections received have been unpacked, and proper labels of locality and other items of personal history affixed, the different specimens placed where they belonged, and those that would not admit of being immediately put in hand for investigation, were boxed, labelled, entered in the record-book, and stored where they will be readily accessible when wanted.

Much of the labor done in connection with the specimens received during the year, and to a less extent with those previously in the collections, has consisted in the writing labels for the same, and entering them with corresponding numbers in the record books of the Institution. The following tables will show what has been done in this respect, and a reproduction of a similar table from last year's report will show what additions have been made. In further illustration of the progress made in this work, I add also the records of two or three year's interval since the commencement of the collections of the Institution.

Table showing the entries in the record-books of the Smithsonian collections in 1864 *and* 1865, *as well as various preceding intervals.*

	1851.	1855.	1858.	1860.	1862.	1864.	1865.
Skeletons and skulls	912	2,050	3,413	4,350	4,750	6,275	6,609
Mammals		1,200	3,226	4,575	5,900	7,782	8,416
Birds	3,700	4,425	11,390	20,875	26,157	35,111	40,554
Reptiles			4,370	4,683	6,311	6,543	6,544
Fishes			1,136	2,975	4,925	5,404	5,588
Eggs of birds			1,032	4,425	6,000	8,700	9,939
Crustaceons			939	979	1,287	1,287	1,287
Mollusks				8,832	10.000	10,525	18,103
Radiates				1,308	2,675	2,725	2,725
Fossils				705	2,100	5,487	5,907
Minerals				1,132	3,725	4,925	4,940
Ethnological specimens				550	825	1,048	1,125
Annelids					109	110	110
Total	4,612	7,675	25,506	55,389	74,764	95,922	111,847

From the preceding tables it will be seen that the average entries per year, for fifteen years, have been about 7,450, and that those for 1865 amounted to 15,925, the difference between the summations for 1865 and 1864. This, however, includes about 7,600 entries of shells made during several years past by Mr. Carpenter, and only reported in 1865.

As repeatedly explained in previous reports, it is only in case of the osteological specimens, the mammals, and the birds, that each entry necessarily relates to but one specimen. In the other departments all of the same kind, from one locality, and collected at one time from the same donor, may have but a single entry number, although this number should be affixed to all the specimens. The average of specimens to each entry cannot be determined accurately, but it is probable that five will not be too high, thus giving as the total number of specimens catalogued over half a million.

The following tables present an enumeration, somewhat approximate indeed in some cases, of the specimens of different kinds, as well as the names and addresses of the parties receiving them, whether public or private. Many of the latter have made contributions of greater or less value to the Institution, which were then reciprocated; and some required the specimens for scientific investigation. From very few of the institutions, however, has anything been ever received, nor is any expected.

Approximate table of distribution of duplicate specimens by the Smithsonian Institution from the beginning to the end of—

	1863.		1864.		1865.		TOTAL.	
	Species.	Specimens.	Species.	Specimens.	Species.	Specimens.	Species.	Specimens.
Osteology	63	63	......	....	1	1	64	64
Mammals	581	845	168	665	21	33	770	1,543
Birds	5,774	7,784	1,490	2,708	233	1,038	7,497	11,530
Reptiles	1,506	2,396	51	69	74	126	1,631	2,591
Fishes	1,643	3,949	......	....	750	1,200	2,393	5,149
Eggs of birds	2,279	5,100	431	1,641	893	2,421	3,603	9,162
Shells	11,832	46,477	600	2,100	5,780	11,086	18,212	59,663
Radiates	551	727	......	....	....	....	551	727
Crustaceans	936	1,894	77	622	....	....	1,013	2,516
Other invertebrates.	528	712	1,072	3,798	200	550	1,800	5,060
Plants	....	....	......	....	10,000	12,975	10,000	12,975
Fossils	747	2,238	......	....	2,224	5,319	2,971	7,557
Minerals and rocks.	211	354	860	4,600	250	600	1,321	5,554
Ethnology	....	....	58	58	....	....	58	58
Total	26,651	72,539	4,807	16,261	20,426	35,349	51,884	124,149

LIST OF DONATIONS MADE TO THE COLLECTIONS OF THE SMITHSONIAN INSTITUTION IN 1865.

Abbot, G. J.—Indian relics, minerals, &c.; various localities.

Adamson, J. C.—Noddy tern, and other specimens in alcohol, from the Atlantic ocean.

Allen, Prof. Geo. N.—Collection of birds of Jamaica.

Armstrong, M. K.—Grasshoppers, Dakota.

Arnold, Jas. G.—Shells and marine animals, Bermuda.

Ash, H. C.—Fossil fish from Dakota.

Baird, S. F.—Skin of *Buteo pennsylvanicus*, Maine.

Bishop, N. H.—Collection of birds from Cuba.

Blackburn, Chas. and Geo.—Eggs of birds from Iowa.

Boardman, G. A.—Skins and eggs of birds.

Botteri, Sig. M.—Plants and shells of Orizaba.

Boston Society of Natural History.—Skins of hares from Massachusetts.

Brass, W.—Zoölogical collections from the Mackenzie River district, Hudson Bay Territory.

Brooks, O. N.—Skins and eggs of roseate tern from Connecticut.

Bulkley, Col. Chas. S., (Director Russian overland international telegraph expedition.)—Zoölogical and botanical collections made by Robert Kennicott and assistants in Nicaragua and California.

Butterfield, W. W.—Nest of bird, Indiana.

Carmiol, J.—Collection of mammals and birds of Costa Rica.

Cassin, J.—Cryolite, Greenland.

Chapman, W. G.—Twelve jars reptiles, U. S. of Colombia.

Christie, W. J.—Skin of black rabbit, Fort Edmonston.

Church, W. S.—Collection of birds of Peru.

Copeman, A. J.—Mounted slides for microscope containing infusoria.

Coues, U. S. A., Dr. Elliott.—Collection of vertebrata from Arizona.

Crocker, Allan.—Skins and eggs of birds of Kansas.

Dayton, E. A.—Collection of marls from Virginia.

Dow, Captain J. M.—Birds and marine invertebrates, west coast of Central America.

Edmonds, J. H.—Clay stones and concretions, Vermont.

Edwards, Amory.—Seven skins of birds of Honduras.

Eichwald, Dr. Ed.—Plumbago collected by M. Siderow from the Lower Tungouski, Siberia.

Elliot, D. G.—Mounted specimens of *Penelopinae*.

Evans, Gov. J.—Jaw of *Titanotherium*, Colorado.

Feilner, Captain John.—Skins of birds, mammals, &c., collected during General Sully's expedition on Upper Missouri.

Flett, Jas.—Zoölogical collections from the Mackenzie River district.

Frantzius, Dr. A. von—Collection of birds of Costa Rica.

Goss, B. F.—Nests and eggs of birds of Kansas.

Gaudet, C. P.—Zoölogical collections from the Mackenzie River district.

Gibbs, George.—Infusorial earths from Nevada Territory.

Goodwin, Mr.—Skins of birds of Brazil.

Grayson, Colonel A. J.—Collection of birds from Mazatlan, Tres Marias, and Socorro, Mexico.

Gundlach, Dr.—Mounted birds and eggs from Cuba.

Gunn, Donald.—Zoölogical collections from the vicinity of Red River settlement.

Hamlin, Prof. C. E.—*Vireo philadelphicus*, Waterville, Me.

Hayden, Dr. F. V.—Indian dresses, &c., Upper Missouri.

Hays, Dr. W. W.—Collection of birds, &c., from Southern California.

Hepburn, J.—Skins and eggs of birds from Pacific coast.

Hering, Dr.—Alcoholic vertebrates, Surinam.

Hicks, Fred.—Collection of birds from Panama, Chiriqui, &c.

Hill, Richard.—Specimens of bats, Jamaica.

Hitz, Dr. R. B.—Collection of birds, eggs, reptiles, &c., principally from Fort Laramie and Laramie Peak.

Holland, H. E.—Skins of birds of Nicaragua.

Hubbard, Samuel—Collection of California fishes.

Jones, R. Strachan.—Zoölogical collections from Yukon river.

Kennicott R.—See Bulkley.

Lacerda. A. D.—Collection of birds from Brazil.

Laszlo, Chas.—Living *Dasyproeta* and collection of vertebrata in alcohol, Mexico.

Latimer, Geo.—Collection of birds of Porto Rico.

Lea, Isaac.—Collection of minerals, Chester county, Penna.

Lewis, Geo. T.—Cryolite from Greenland.

Lincoln, C. D.—Four skins *Parus atricapillus,* Massachusetts.

Lockhart, Jas.—Zoölogical collection from Yukon river and Great Slave lake.

McCauley Jno.—Skin of *Neotoma drummondi,* Fort Edmonston.

Macfarland, R. R.—Twenty boxes zoölogical collections from Fort Anderson and vicinity, Mackenzie River district.

Mactavish, Gov. W.—Coleopterous insects and other specimens from Arctic America.

Mapes, H. H.—Insects, &c., in alcohol, Michigan.

March, W. T.—Skins, nests, and eggs of birds, shells, &c., Jamaica.

Merritt, E.—Indian relics, New York.

Middleton, E. J.—Collection of birds of the District of Columbia.

Moore, Carlton R.—Specimen of continental money.

Nation, Prof. W. E.—Collection of birds of Peru.

New Haven, Yale College.—Birds from Peru.

Poey, Prof. F.—Cuban fishes in alcohol.

Poston, Col. C. D.—Fragment of timber from the Casa Grande of Gila; hair bridles and other articles of Indian workmanship from the Pimo villages.

Powers, W. J.—Insects, &c., in alcohol, Cuba.

Rankin, Colin.—Skins of birds and skulls of bears, Lake Superior.

Riecksecker, S. E.—Eggs of birds, Pennsylvania.

Rio Janeiro, Royal Museum of.—One hundred and thirty skins of Brazilian birds.

Rothhammer, S. M.—Skins of birds and mammals, insects, eggs, &c., collected during General Sully's expedition on the Upper Missouri.

Salazar y Ilarregui, Gov.—Zoölogical, botanical, and other collections made with the "comision cientifica de Yucatan" by Dr. Arthur Schott, naturalist to the expedition.

Sartorius, Dr. C.—Birds, reptiles, shells, &c., Mexico.

Schott, Dr. Arthur.—(See Salazar.)

Sclater, Dr. P. L.—Skin of *Haliaetus leucocephalus,* Nova Scotia.

Sessions, Lewis.—Nest and eggs of birds from Connecticut.

Shimer, Henry.—Collection of birds from Illinois.

Spangler, Geo.—Fossils from Indiana.

Squier, E. G.—Fossils and shells from Peru.

Strebel, G.—Shells from Mexico.

Sully, General.—(See Rothhammer; Feilner.)

Sumichrast, Prof. F.—Collection of birds, mammals, and shells, Orizaba, Mexico.

Swan, J. G.—Collections of mammals, birds, shells, and Indian curiosities from Puget Sound.

Swift, Robert.—Collection of birds of St. Thomas and Porto Rico.

Taylor, A. S.—Grasshoppers, southern California.

Thomson, J. H.—Fishes from Buzzard's bay, Massachusetts.

Thurston, W. H.—Minerals from Massachusetts.

Tolman, J. W.—Eggs of birds from Illinois.

Torrey, W.—Stone pestles, New York.

Tristram, Rev. H. B.—Series of skins and eggs of birds of Palestine.

Twitchell, G. S.—Belemnites from New Jersey.

Walker, R. L.—Skins of birds and mammals, Pennsylvania.

White, Lieut. J. W.—Skins of seals and other mammals, birds, Indian curiosities, &c., Puget Sound.

Willis, J. R.—Eggs and skins of birds, shells, &c., Nova Scotia

Wolle, A.—Eggs of birds, Maryland.

Wyeth, John.—Petroleum from California.

LIST OF ADDRESSES OF FOREIGN INSTITUTIONS

ADDED TO THE

DISTRIBUTION LIST OF THE SMITHSONIAN INSTITUTION SINCE 1862, THE DATE OF THE LAST PRINTED LIST.

SWEDEN.

Stockholm.—Bureau Central de Statistique de Suède.
Bureau de la Recherche Géologique de la Suède.

NORWAY.

Christiania.—Kougelige Selskab for Norges Vel.
Physiographiske Forening.

DENMARK.

Kjöbenhavn.—Universitets-Museum.
Zoologisches Museum.

RUSSIA.

Moskwa.—Musée Publié de Moscou.
St. Petersburg.—Bibliothek der Evangelischen Gemeinden.
Commission Impériale Archéologique.
Entomologische Gesellschaft.
Nikolai Haupt-Sternwarte.

HOLLAND.

Amsterdam.—Genootschap ter Bevordering der Bouwkunst.
Genootschap ter Bevordering der Genees en Heilkunde.
Maatschappij : Tot Nut van't Allgemeen.
Vereeniging voor Statistik.
Breda.—Koninklijke Militaire Akademie.
Delft.—Kon. Instituut voor Taal-, Land en Volkenkunde voor Ned. Indië.
'sGravenhage.—Government of the Netherlands.
Groningen.—Genootschap te Groningen pro excolendo Jure Patrio.
Haarlem.—Nederlandsche Maatschappij ter Bevordering van Nijverheid.
Musèe Teyler.
Leiden.—Stolpiaansch Legaat.

GERMANY, INCLUDING AUSTRIA AND PRUSSIA.

Augsburg.—Red. des Wochenschrift für Thierheilkunde und Viehzucht.
Berlin.—Kön. Ministerium für Handel, Gewerbe und öffentliche Bauten.
Zoologische Museum der Universität.
Bremen.—Naturforschender Verein.
Brünn.—Naturforschender Verein.
Buda.—K. K. Sternwarte.
Chemnitz.—K. Gewerbschule.
Öffentliche Handels-Lehranstalt.
Red. der Deutsche Industrie Zeitung.
Darmstadt.—Grossherz. Hessische Centralstelle für die Landes-Statistik.

Dresden.—Handels Lehr-Anstalt.
K. Polytechnische Schule.
Verein für Erdkunde.
Eisenach.—Grossherz. Carl Friedrich-Gymnasium.
" Real Gymnasium.
Elberfeld.—Wupperthaler Thierschutz-Verein.
Göttingen.—Göttingische Verein Bergmannischer Freunde.
Zoologisches Museum.
Grätz.—Steiermärkisch-Landschaftliche Ober-Realschule.
Halle.—Königl. Ober-Berg-Amt.
Hannover.—Apotheker Verein in Nord-Deutschland.
Architecten und Ingenieur-Verein.
Historischer Verein für Niedersachsen.
K. Polytechnische Schule.
Jena.—Landwirthschaftliche Institut.
Medicinische Naturwissenschaftliche Gesellschaft.
Universitäts-Bibliothek.
Laibach.—Juristische Gesellschaft.
Landshut.—Historischer Verein für Niederbayern.
Leipzig.—Red. der Zeitschrift der Deutsche Landwirthe.
Verein von Freunde der Erdkunde.
Linz.—K. K. Landwirthschafts-Gesellschaft.
Luxembourg.—Société pour la Recherche et la Conservation des Monuments Historique dans le Grand-Duché de Luxembourg.
Meseritz.—Königliche Realschule.
München.—Baierische Gartenbau Gesellschaft.
Niesse.—K. Katholische Gymnasium.
Philomathische Gesellschaft.
Realschule.
Offenbach.—Grossherz. Handels-Kammer.
Olmütz.—K. K. Gymnasium.
K. K. Ober-Realschule.
Universitäts-Bibliothek.
Prag.—Verein für Geschichte der Deutschen in Böhmen.
Regensburg.—Universitäts-Bibliothek.
Reutlingen.—Red. der Illustrirte Monatshefte für Obst-und Weinbau.
Rostock.—Mecklenburgische Patriotischer Verein.
Schwerin.—Grossherz. Landes-Vermessungs-Comission.
Stuttgart.—Monatschrift für Pomologie und praktischen Obstbau.
Trieste.—Società Scientifico Letteraria della Minerva.
Tübingen.—Anstalt für schwachsinnige Kinder Mariaberg.
Weimar.—Grossherzogliche Gymnasium.
Wilhelm-Ernst Gymnasium.
Wien.—Hydrographische Anstalt der Kais. Osterr. Marine.
Handels und Gewerbekammer.
K. K. Marine Ober-Commando.
Wiesbaden.—Verein Nassauischer Land und Forstwiethe.
Würzburg.—Verein für Nassauische Alterthumskunde und Geschichtsforschung.

SWITZERLAND.

Bern.—Ökonomische Gesellschaft des Kantons Bern.
Zürich.—Bureau Central Météorologique de la Suisse.

BELGIUM.

Bruxelles.—Société Entomologique de Belgique.

FRANCE.

Douai.—Société Royale d'Agriculture de Douai.
Montpellier.—Académie de Montpellier; Faculté de Médecine.
Société Centrale d'Agriculture du Dept. de la Herault.
Paris.—Annales Télégraphiques.
Archives Générales de Médecine.
Societé Médicale Homœopathique.
Rennes.—Société des Sciences Physiques et Naturelles du Dépt. d'Ille-et-Vilaine.

ITALY.

Genova.—Società ligure di Storia Patria.
Milano —Accademia Fisico-medico-statistico di Milano.
Napoli.—Società Reale di Napoli.
Palermo.—R. Instituto d'Incoraggiamento di Agricoltura, Arti e Manifatture in Sicilia.
Siena.—Accademia di Fisiocritici.

SPAIN.

Madrid.—Real Observatorio.
San Fernando.—Real Academia.

GREAT BRITAIN AND IRELAND

Aberdeen.—University.
Armagh.—Public Library.
Birmingham.—Institution of Mechanical Engineers.
Dublin.—Natural History Society of Dublin.
Dumfries.—Dumfriesshire and Galloway Natural History and Antiquarian Society.
London.—Acclimatization Society.
Anthropological Society.
Cambrian Archaeological Association.
Quarterly Journal of Science.
Macclesfield.—Macclesfield Society for Acquiring Useful Knowledge.
Manchester.—Lancashire Independent College.
Nottingham.—United Lunatic Asylum.
Perth.—Murray Royal Institution.
Plymouth.—Plymouth Institution and Devon and Cornwall Natural History Society.
Sandhurst.—Royal Military College.

OTHER PARTS OF THE WORLD.

Constantinople.—Hellenic Philological Society of Constantinople.
St. Helena.—Magnetical and Meteorological Observatory.
Mauritius.—Meteorological Society of Mauritius.
Meteorological Observatory.
Melbourne.—Library of Parliament.
Mining Department.
Mining Institute of Victoria.
Royal Society of Victoria.
Sydney.—Entomological Society of New South Wales.
Buenos Ayres.—Museo Publico de Buenos Ayres.
Halifax.—Nova Scotian Institute of Natural Sciences.
Montreal.—Numismatic and Antiquarian Society.
Rio Janeiro.—Royal Museum.

LIST OF METEOROLOGICAL STATIONS AND OBSERVERS

OF THE

SMITHSONIAN INSTITUTION

FOR THE YEAR 1865.

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
Baker, J. C	Stanbridge, Canada East	45 08	73 00	....	T	7
Magnetic Observatory	Toronto, Canada West	43 39	79 21	†108	A	12
Murdock, G	St. John, New Brunwick	45 16	66 03	135	A	12
Rankin, Colin	Michipicoton, Canada West	47 56	85 06	660	B. T	12

MEXICO.

Laszlo, Charles	Frontera, Tabasco	18 32	92 40	12	A	
Sartorius, Dr. Charles	Mirador, Vera Cruz	19 15	96 25	3,600	A	

CENTRAL AMERICA.

Riotte, C. N	San José, Costa Rica	9 54	84 06	3,772	T. R	12
White, William T., M. D	Aspinwall	9 23	79 53	6	A	5
Kluge, J. P., M. D	Aspinwall	9 23	79 53	6	A	7

WEST INDIES.

United States Consul	Turk's Island	21 20	71 00	....	....	
Brayton, Milton	Sombrero Island	18 37	63 27	45	A	

BERMUDA.

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

SOUTH AMERICA.

Hering, C. T	Government Plantation Vossenburg, colony of Surinam, Dutch Guiana.	....	....	....	A	

* 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.

ARIZONA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Cones, Elliot, Assistant Surgeon, U. S. A.	Fort Whipple....	Yavapi	32 20	111 00	8,000	T	7

ARKANSAS.

[redacted] O. F.............	Helena	Phillipps	34 33	90 10	....	T. R	1

CALIFORNIA.

Ayres, W. O., M. D.......	San Francisco ...	San Francisco ...	37 48	122 27	30	A.......	6
Canfield, Colb't A., M. D..	Monterey.........	Monterey.........	36 36	121 52	40	T. P. R..	12
Logan, Thomas M., M. D.	Sacramento	Sacramento	38 32	121 30	65	A.......	11
Smith, Mrs. M. D.........	Meadow Valley..	Plumas..........	40 20	120 15	3,700	B. T. R..	10

COLORADO.

[redacted] rell, James	Montgomery.....	Park	39 00	106 00	13,000	T.......	5

CONNECTICUT.

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.

[redacted] Urban D., M. D ..	Wilmington	New Castle......	39 47	75 33	115	T. R	10

GEORGIA.

[redacted] Frederick	Atlanta	Fulton	33 45	84 31	1,050	T. R	2

IDAHO.

[redacted] A. F., M. D	Fort Laramie....	....	42 10	104 47	4,472	T	1

ILLINOIS.

Adams, W. H	Elmore..........	Peoria...........	40 56	90 04	612	R	9
Aldrich, Verry	Tiskilwa	Bureau	41 15	89 16	550	T	12
Babcock, E...............	Riley	McHenry.........	42 11	88 33	760	T. R	12
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
Dudley, Timothy	Waverly	Morgan	39 40	90 00	680	T. R	11
Ellsworth, J.............	Hoylton	Washington	38 30	89 00	....	T	1

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

ILLINOIS—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Gill, Joseph H., and others.	Evanston	Cook	42 02	87 38	588	B. T	12
Grant, John Grant, Miss Ellen	Manchester	Scott	39 31	90 34	683	A	12
Griffing, Henry	Hazel Dell	Cumberland	39 00	88 00		N	8
Kunster, H	Waterloo	Monroe				T	3
Livingston, Prof. William	Galesburg	Knox	40 55	87 11	795	A	12
Mead, S. B., M. D	Augusta	Hancock	40 10	91 00	*203	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	3
Phelps, E. S Phelps, Miss Lelia E	Wyanet	Bureau	41 30	89 45		T. R	12
Riblet, J. H	Pekin	Tazewell	40 36	89 45		B. T. R	10
Rogers, O. P. and J. S	Marengo	McHenry	42 14	88 38	842	B. T. R	5
Schauber, H. A	Centralia	Marion				T	2
Spencer, Wm. C	Dubois	Washington	38 14	89 16		T. R	7
Spaulding, Abiram	Aurora	Kane	41 48	88 23		T. R	2
Tolman, James W	Winnebago	Winnebago	42 17	89 12	900	A	12

INDIANA.

Burroughs, Reuben	South Bend	St. Joseph	41 39	86 41	600	T. R	6
Boerner, Charles G	Vevay	Switzerland	38 46	84 59		T. R	12
Butterfield, W. W	Indianapolis	Marion	39 45	86 20	698	T	12
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	6
Crozier, Dr. E. S	New Albany	Floyd	38 02	85 32	353	A	10
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	8
Hobbs, Miss Mary Anna	Bloomingdale	Parke	39 48	87 00	600	T	3
Loughridge, J. H., M. D	Rensselaer	Jasper	40 56	87 13	745	T. R	9
Mayhew, Royal	Indianapolis	Marion	39 45	86 20	698	T. R	2
McCoy, Dr. F McCoy, Miss Lizzie	Columbia	Whitney	41 10	85 30		T. R	4
Mulvey, Oliver	Madison	Jefferson				B. T. R	4
Redding, Thomas B	Newcastle	Henry	39 53	85 16	1,000	B. T. R	3
Valentine, John	Richmond	Wayne	39 52	84 39	850	A	9
Windle, Isaac E	Lafayette	Tippecanoe	40 20	86 57		T. R	6

IOWA.

Collin, Prof. Alonzo	Mount Vernon	Linn	42 00	91 00		T	12
Deering, D. S	Independence	Buchanan	42 30	92 16	850	T	11
Dorweiler, Phillip	Guttenburg	Clayton				T. R	11
Farnsworth, P. J., M. D	Lyons	Clinton	41 50	90 10	630	T. R	12
Hagensick, John M	Ceres	Clayton	42 45	91 11	825	T	8
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	3
Kridelbaugh, S. H., M. D	Clarinda	Page				T. R	2
McConnel, Townsend	Pleasant Plain	Jefferson	41 07	91 54	950	T. R	3
McCoy, Franklin, M. D. McCoy, Miss Elizabeth	Algona	Kossuth	43 01	94 04	1,500	T. R	7
McCready, Daniel	Fort Madison	Lee	40 37	91 28		T. R	12
Mead, Allen	Manchester	Delaware	42 30	91 30	925	T. R	4
Mead, Chauncey	Monticello	Jones	42 13	91 15	880	T. R	8
Nash, Rev. J. A	Des Moines	Polk	41 35	93 36		T. R	5
Parvin, Prof. Theodore S	Iowa City	Johnson	41 37		621	A	12
Pratt, George B	Davenport	Scott	41 30	90 40	737	A	12
Steed, F	Waterloo	Black Hawk	42 30	92 30	670	T. R	12
Townsend, Nathan	Iowa Falls	Hardin	42 32	93 20		T. R	12
Walton, Josiah P	Muscatine	Muscatine	41 25	92 02	582	A	12
Wheaton, Alex. Camp	Independence	Buchanan	42 29	91 50		T. R	12

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* Above low-water mark at Quincy.

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

KANSAS.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Agricultural College	Manhattan	Riley	39 13	96 45	1,000	T. R	9
Beckwith, W	Olatha	Johnson	38 50	94 30		T. R	12
Horn, Dr. H. B. and Miss Clotilde.	Atchison	Atchison	39 30	95 00		T	8
Post Surgeon	Fort Riley	Davis	39 00	96 30	1,300	T. R	12
Woodworth, Abner, M. D.	Council Grove					T	9

KENTUCKY.

Beatty, O	Danville	Boyle	37 40	84 30	900	B. T. R	12
Doak, W. S	London	Laurel	37 12	84 03		T	6
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

MAINE.

Dana, Wm. D	North Perry	Washington	45 00	67 06	100	A	4
Gardiner, Rev. Frederick Gardiner, Robert H	Gardiner	Kennebec	44 41	69 46	120	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	11
Parker, J. D	Steuben	Washington	44 31	67 57	50	A	12
Pitman, Edwin	Lee	Penobscot				T	12
Robinson, Almon	Webster	Androscoggin	44 04	70 04		T	1
West, Silas	Cornish	York	43 40	70 44	784	B. T. R	12
Wilbur, Benjamin F	West Waterville	Kennebec				T. R	12

MARYLAND.

Baer, Miss Harriott M	Sykesville	Carroll	39 23	76 57	700	T. P. R	10
	Frederick	Frederick	39 24	77 17		T. R	1
Goodman, William R	Annapolis	Anne Arundel	38 58	76 29	20	A	12
Grape, George E	Catonsville	Baltimore	39 17	76 42	42	T	1
McCormick, James O	Woodlawn	Cecil	39 39	76 04		B. T. R	10
Stephenson, Rev. James	St. Inigoes	St. Mary's	38 10	76 30	45	A	11

MASSACHUSETTS.

Astronomical Observatory.	Williamston	Berkshire	42 43	73 13	686	B. T. R	11
Bacon, William	Richmond	Berkshire	42 13	72 20	1,000	T. R	10
Barrows, N., M. D	Sandwich	Barnstable	41 45	70 30		T. R	4
Caldwell, John H	Newbury	Essex	42 45	70 55	25	T	11
Davis, Rev. Dr. Emerson	Westfield	Hampden	42 06	72 48	180	A	12
Dewhurst, Rev. Eli	Baldwinsville	Worcester	42 37	72 05	900	B. T. R	12
Fallon, John	Lawrence	Essex	42 42	71 11	133	A	6
Fendler, Augustus	Cambridge	Middlesex	42 28	71 11	60	B. T	3
Merriam, Arthur M	Topsfield	Essex	42 38	71 57		A	12
Metcalf, John Geo., M. D	Mendon	Worcester	42 06	71 34		B. T. R	12
Nelson, Henry M	Georgetown	Essex	42 42	71 00	225	T	11
Rice, F. H	Worcester	Worcester	42 16	71 48	528	A	11
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

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

MICHIGAN.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Ellis, Edwin, M. D	Garlick	Ontonagon	46 49	90 00	1,440	T. R.	4
Kedzie, Prof. R. C.	Lansing	Ingham	42 42	84 34	895	A	12
Mapes, Henry H.	Oshtemo	Kalamazoo				N.	12
Parker, J. B	Grand Rapids	Kent	43 00	85 30	680	T	1
Reasner, F. M., M. D.	Manchester	Washtenaw				T	5
Smith, Harmon M.	Kalamazoo	Kalamazoo	42 20	85 40		N.	5
Steele, George E	Homestead	Benzie	44 30	86 00		T	12
Weeks, James A.	Pontiac	Oakland	42 36	83 14	927	T	8
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	8
Cheney, William	Minneapolis	Hennepin	45 00	93 10	856	A	12
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	850	T. R	12
Smith, Henry L.	Forest City	Meeker	45 45	96 00		T. R	6
Stouffer, Andrew	Bowles' 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	8

MISSISSIPPI.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
McCary, Robert	Natchez	Adams	31 34	91 25	264	B. T. R.	[illegible]

MISSOURI.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Caldwell, J. T	Athens	Clark				T. R	10
Christian, John	Harrisonville	Cass	38 40	94 30		T. R	12
Engelmann, George, M. D.	St. Louis	St. Louis	38 37	90 15	481	A	12
Fendler, Augustus	Allenton	St. Louis	38 29	90 45	482	B. T. P.	7
Ray, George P	Canton	Lewis	40 12	91 37		T	12
Sibley, P. B.	Easton	Buchanan	39 46	94 22		T. R	3
Stuntebeck, F. H., S. J.	St. Louis	St. Louis	38 37	90 15	470	A	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
Hamilton, Rev. William	Bellevue	Sarpy	41 08	95 50		T. R	12
Hill, L. J	Jonin	Dixon	42 30		3,000	T. R	2
Thompson, R. O	Nursery Hill	Otoe	40 40	95 51	1,266	T	5

NEVADA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Johnson, R. C.	Star City	Humboldt	40 30	117 30	7,500	T	[illegible]

NEW HAMPSHIRE.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Brown, Branch	Stratford	Coos	44 08	71 34	1,000	T. R	12
Chase, Arthur	Claremont	Sullivan	43 22	72 21	539	B. T. R.	12
Mead, Stephen O	Claremont	Sullivan				T	12
Nason, Rev. Elias	Exeter	Rockingham	42 58	70 55	125	B. T	5
Odell, Fletcher	Shelburne	Coos	44 23	71 06	700	B. T	12
Pitman, Charles H	North Barnstead	Belknap	43 38	71 27		T. R	12
Wheeler, John T.	Concord	Merrimack	43 12	71 29	400	B. T. R.	3

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

NEW JERSEY.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Alexander, Archibald	Long Branch	Monmouth	....	....	....	T	3
Beans, Thomas J	Progress	Burlington	40 03	75 11	15	T. R	3
	Moorestown	Burlington	39 59	74 54	....	T. R	9
Brooks, William	Passaic Valley	Passaic	40 53	74 12	140	T. R	12
Cole, Barker	Seaville	Cape May	39 20	74 40	18	T	8
Cook, Ephraim R	Trenton	Mercer	....	....	....	B. T. R.	4
Cook, George H	New Brunswick	Middlesex	40 30	75 31	90	T	3
Deacon, John C	Burlington	Burlington	40 05	75 10	60	T. R	12
Lippincott, James S	Cole's Landing	Camden	39 54	75 02	50	T	11
Lippincott, Joseph W	Moorestown	Burlington	40 00	75 00	....	A	2
Rhees, Morgan J., M. D	Mount Holly	Burlington	39 59	74 47	30	B. T	12
Sheppard, Clarkson Sheppard, Miss R. C	Greenwich	Cumberland	39 20	75 25	30	A	12
Thompson, George W	New Brunswick	Middlesex	40 30	75 31	90	T	8
Whitehead, W. A	Newark	Essex	40 45	74 10	35	B. T. R.	12

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	6
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	11
Beauchamp, William M	Skaneateles	Onondaga	43 00	76 30	932	B. T	11
Bowman, John	Baldwinsville	Onondaga	43 04	76 41	....	T	12
Dill, John B	Auburn	Cayuga	42 55	76 28	650	B. T	12
Denning, William H	Fishkill on Huds'n	Dutchess	41 34	74 18	42	B. T. R.	12
Dewey, Prof. Chester	Rochester	Monroe	43 07	77 51	516	B. T. R.	12
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	12
Haas, Henry	Depauville	Jefferson	44 15	....	350	T. R	11
Haswell, Rev. James R	Sherburne	Chenango	....	....	....	T	7
Heimstreet, John W	Troy	Rensselaer	42 44	73 40	58	A	2
Howell, Robert	Nichols	Tioga	42 00	76 32	....	T	12
Hyde, Stephen	Palmyra	Wayne	43 04	77 20	466	T	3
Ingalsbe, Grenville M	South Hartford	Washington	43 15	73 21	400	T. R	12
Joy, Prof. Charles A	New York	New York	40 43	74 05	....	A	9
Mack, Rev. Eli T	Flatbush	Kings	40 37	74 02	54	B. T. R.	12
McMore, P. A	Fort Ann	Washington	42 39	73 44	1,430	T. R	8
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	7
Morris, Prof. Oran W	New York	New York	40 43	74 05	75	A	12
Morse, J. P	Warsaw	Wyoming	43 00	78 10	....	T	3
Paine, Horace M., M. D	Clinton	Oneida	43 03	75 15	600	T. P. R.	3
	Albany	Albany	42 39	73 44	75	P. T. R.	6
Roe, Sanford W	Jamestown	Chautauqua	42 06	79 29	1,454	T	12
Russel, Cyrus H	Gouverneur	St. Lawrence	44 19	75 29	....	B. T. R.	12
Smith, E. A Smith, Miss Naomi	Moriches	Suffolk	40 49	72 36	13	T. R	12
Soule, Prof. William	Cazenovia	Madison	42 55	75 46	1,260	B. T	6
Spooner, Stillman, M. D	Oneida	Madison	43 04	75 50	500	T. R	12
Trowbridge, David	Hector	Schuyler	42 30	77 00	850	N	4
Willis, Oliver R	White Plains	Westchester	41 05	73 40	....	T	11
Wilson, Rev. W. D., D. D	Geneva	Ontario	42 53	77 02	567	B. T. R.	12
Yale, Walter D	Houseville	Lewis	43 40	75 32	....	T. R	1

OHIO.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
Abell, B. F	Welshfield	Geauga	41 23	81 12	1,205	T. R	12
Bambach, Dr. G	Ripley	Brown	38 47	83 31	*106	A	12
Benner, Josiah F	New Lisbon	Columbiana	40 45	80 45	961	B. T. R.	12
Crane, George W	Bethel	Clermont	39 00	84 00	555	T. R	12
Engelbrecht, Lud	Portsmouth	Scioto	38 45	82 50	537	B. T. R.	8
Fraser, James B	Saybrook	Ashtabula	41 48	80 53	....	T	12
Hammitt, John W	College Hill	Hamilton	39 19	84 26	800	T. R	11
Harper, George W	Cincinnati	Hamilton	39 06	84 27	*305	A	12

* Above low water in the Ohio river.

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

OHIO—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Haywood, Prof. John	Kingston	Ross	39 29	83 00	692	A	11
Huntington, George C	Kelley's Island	Erie	41 36	82 42	587	B. T. R	12
Hyde, Gustavus A Hyde, Mrs	Cleveland	Cuyahoga	41 30	81 40	643	B. T. R	12
Larsh, Miss Ollitippa J	Eaton	Preble	39 54	84 25	1,400	T	4
McMillan, Smith B	East Fairfield	Columbiana	40 41	80 44	1,152	A	12
Mathews, Joseph McD	Hillsborough	Highland	39 13	80 53		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	12
Phillips, R. C	Cincinnati	Hamilton	39 06	84 27	588	B. T. R	12
Rankin, Rev. D. M	Cuyahoga Falls	Summit	42 00	81 00		T	5
Rodgers, A. P	Gallipolis	Gallia	39 00	82 00	600	T. R	9
Schauber, Hubert A	Centralia	Marion				N	1
Thompson, Rev. David	Milnersville	Guernsey	40 10	81 45		T. R	12
Thompson, Prof. H. A	Westerville	Franklin	40 04	83 00		A	11
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	3
Williams, Prof. M. G	Urbana	Champaign	40 06	83 43	1,015	B. T. R	12
Wilson, Prof. J. H	College Hill	Hamilton	39 19	84 25	800	B. T. R	8
Winchester, Electus D	Austinburg	Ashtabula	41 54	80 52	816	B. T	12
Winger, Martin	Wooster	Wayne	40 49	81 57	872	T	12

OREGON.

Hindman, S. M. W	Auburn	Baker	44 45	118 16	3,300	R	2
	Albany	Linn	44 22	123 00	600	R	2
Ironside, R. B	Auburn	Baker	44 37			T	1
Willis, P. L	Salem	Marion	44 56	123 01	120	B. T. R	1

PENNSYLVANIA.

Bentley, E. T	Tioga	Tioga	42 00	77 00	1,000	T. R	12
Boyers, W. R	Blairsville	Indiana	40 30	74 43	1,010	T. R	1
Bruckart, H. G	Silver Spring	Lancaster	40 05	76 45		T	12
Brugger, Samuel	Fleming	Centre	40 55	77 53	780	T. R	3
Clark, Prof. A. G Aldrich, Truman H	Westchester	Chester	39 57	75 36		B. T	2
Darlington, Fenelon	Pocopson	Chester	39 40	75 37	218	T. R	12
Day, Theodore	Dyberry	Wayne	41 36	75 19		T	12
Duffield, Henry, M. D	Oxford	Chester	39 50	75 51		A	5
Eggert, John	Berwick	Columbia	41 05	76 15	583	B. T. R	1
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	9
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 30		B. T. R	12
Jacobs, Rev. M Jacobs, H. E	Gettysburg	Adams	39 49	77 15	624	B. T. R	2
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
Martindale, Isaac C	Byberry	Philadelphia	40 05	75 00	70	N	12
Meehan, Thomas	Germantown	Philadelphia				T	9
Ricksecker, Lucius E	Nazareth	Northampton	40 43	75 21	525	T	12
Smith, Wm., D. D	Cannonsburg	Washington	40 16	80 10	936	B. T. R	12
Spencer, Miss Anna	Horsham	Montgomery	40 00	75 11	250	B. T. R	12
Spera, W. H	Ephrata	Lancaster				T. R	2
Taylor, John	Connellsville	Fayette	40 00	79 36		T	12

RHODE ISLAND.

Caswell, Prof. Alexis	Providence	Providence	41 49	71 25	120	A	
Crandall, William H	Newport	Newport	41 28	71 21	25	T. R	

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

SOUTH CAROLINA.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
			° ′	° ′	*Feet.*		
Marsh, M. M., M. D Marsh, Mrs	Beaufort	Beaufort	32 21	80 41	15	B. T. P	3
Suter, Major C. R., U. S. engineers.	Hilton Head	Beaufort	32 14	80 40	27	P. T. R	6

TENNESSEE.

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

UTAH.

Burgon, George A Pearce, Harrison	St. George	Washington	37 11	114 00		T. R	12
Phelps, W. W	Salt Lake	Salt Lake	40 45	111 26	4,320	T. R	12

VERMONT.

Buckland, Harmon	Brandon	Rutland	43 45	73 00	460	T. R	7
Cutting, Hiram A	Lunenburg	Essex	44 28	71 41	1,124	A	12
Paddock, James A	Craftsbury	Orleans	44 40	72 29	1,100	T. R	12
Paine, Charles L	East Bethel	Orange	43 35	72 36	700	T. R	2
Sheldon, Harmon A	Middlebury	Addison	43 59	73 10	398	A	12

VIRGINIA.

ver, Howard	Wytheville	Wythe	36 55	81 04	2,400	B. T	2

WASHINGTON.

, James G	Neeah Bay		48 41	124 37	17	T. R	12

WEST VIRGINIA.

Charles L	Ashland	Cabell	38 30	82 16	600	T. R	8

WISCONSIN.

Breed, J. Everett	Embarass	Waupaca	44 51	88 37		T. R	11
Curtis, W. W	Rocky Run	Columbia	43 26	89 19		T. R	12
Deckner, Frederick	Green Bay	Brown	44 34	88 07	732	T. R	9
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. R	8
Hachez, Ferdinand	New Holstein	Calumet	43 45	88 08		T	1
Hicks, John C	Rural	Waupaca	44 15	89 05	910	T. R	4
	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
Mead, H. C	Waupaca	Waupaca	44 20	89 11	1,000	T	12
Moeller, G	Plymouth	Sheboygan	43 44	88 07	870	B. T	8
Porter, Henry D	Beloit	Rock	42 30	89 04	750	A	12
Sterling, Prof. John W	Madison	Dane	43 05	89 25	1,068	A	8
Waite, M. C	Baraboo	Sauk	43 27	80 45	920	T. R	12
Ward, Prof. Wm. H	Ripon	Fon du Lac	43 54	88 59		B. T	2
Whiting, William H	Geneva	Walworth	42 30	89 41	600	T	11
Winkler, Carl, M. D	Milwaukee	Milwaukee	43 03	87 57	630	B. T. R	12

Deaths of observers.

Urban D. Hedges, M. D., Wilmington, Delaware, November 4, 1865.
Royal Mayhew, Indianapolis, Indiana, March, 1865.

Colleges and other institutions from which meteorological registers were received during the year 1865, *included in the preceding list.*

Nova Scotia	Acadia College	Wolfville.
Canada	Magnetic Observatory	Toronto.
Arkansas	Normal School	Helena.
Connecticut	Wesleyan University	Middletown.
Illinois	Lombard University	Galesburg.
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.
New Hampshire	St. Paul's School	Concord.
New York	Columbia College	New York.
	Institution for Deaf and Dumb	New York.
	Erasmus Hall Academy	Flatbush.
	Oneida Conference Seminary	Cazenovia.
	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.
Rhode Island	Brown University	Providence.
Tennessee	Stewart College	Clarksville.
Wisconsin	Beloit College	Beloit.
	Wisconsin University	Madison.

METEOROLOGICAL MATERIAL CONTRIBUTED IN ADDITION TO THE REGULAR OBSERVATIONS.

Armstrong, M. K.—Summary of observations at Yankton, Dacota, from June to December, 1865.

Barclay, Rev. J. T.—Observations at Jerusalem, from June, 1851, to January, 1855, by ————? and from September, 1853, to June, 1859, by R. G. Barclay, M. D., and M. T. Deniss. Also observations at Jaffa, Palestine, from April to October, 1859, (except July,) by Rev. J. T. Barclay. Manuscript.

Beauchamp, W. M.—Summary of observations for the year 1865, at Skaneateles, New York.

Brayton, William.—Record of the fall of rain in Phillipsburg, St. Martins, West Indies, during the months of July, August, September, October, November, and December, 1864, copied by William Brayton, from the records of Z. C. Z. Huntington.

British Museum.—Catalogue of the collection of meteorites exhibited in the mineral department of the British Museum, with the dates and places where found, and the weight of each specimen. Four pages 4to. August 1, 1863.

Bureau Central Météroloqique de la Suisse, Zurich.—Monthly bulletin of observations, 4to, at about eighty different stations.

Christiania Observatorium.—Meteorologische Beobachtungen; aufgezeichnet auf Christiania Observatorium. 1837–1863. Christiania, 1865. 4to, 711 pages.

Meteorologiske Iagttagelser paa Christiania Observatorium, 1864. Christiania, 1865. 4to, 50 pages.

Clark, Hendricks.—Table showing the highest and lowest temperature in each month from observations made daily at sunrise, noon, and sunset; also the quantity of rain and depth of snow, for the ten years from 1856 to 1865, inclusive, at New Creek station, Hampshire county, West Virginia.

Clough, J. B.—Diagram of daily curve of temperature in Hennepin county, Minnesota, from December 1, 1864, to December 31, 1865, from observations taken at morning, noon, and night.

Connolly, H.—Observations at Fort Nascopie, Hudson Bay Territory, from October, 1864, to June, 1865, inclusive.

Delaharpe, J.—Les variations de la pression barométrique ont-elles un effet sensible sur l'homme dans les Alpes? Par J. Delaharpe, docteur-médecin. (Extrait du bulletin, No. 43, de la Société vaudoise des Sciences naturelles.) 8vo., 6 pages.

Doyle, Joseph B.—Observations made at 7 a. m., 12 m., and 6 p. m. at Steubenville during the year 1865. One small manuscript book.

Engelmann, George, M. D.—Summary of observations for each month of 1864, and also of the year, at St. Louis. From the St. Louis Medical and Surgical Journal. 22 pages, 8vo.

France, Minister of Public Instruction.—Archives de la Commission scientifique du Mexique. Publiées sous les auspices du Ministère de l'Instruction Publique. Tome premier. Paris, 1865, 8vo, 467 pages.

This volume contains the following articles on meteorology: Météorologie et Physique du globe, par M. le maréchal Vaillant, pages 62–73. Reproduction des instructions de l'association pour l'avancement de l'astronomie, de la physique, et de la météorologie, pages 190–210. Des variations hororaires du baromètre, par M. le maréchal Vaillant, pages 224–239. Instruction pour les observations météorologiques des écoles normales, pages 367–390. Lettre de M. le maréchal Vaillant à M. Charles Sainte-Claire Deville, sur les phénomènes barométriques, pages 391–397.

Gardiner, Rev. Frederic.—On the ice in the Kennebec river, by Rev. Frederic Gardiner. 3 pages, 8vo.

Gay, V. P.—Summary of temperature, rain, and cloudiness, for each month during the years 1864 and 1865, at York Neck, Adams county, Illinois. 1 page, foolscap.

Gesellschaft "Isis."—Zusammenstellung der Monats- und Jahresmittel aus den zu Meissen, 1865, angestellten täglich dreimaligen meteorologischen Beobachtungen. Im Auftrage der Gesellschaft Isis. Gebauer. One sheet.

Hough, G. W.—Description of an automatic registering and printing barometer. By G. W. Hough, A. M., director of the Dudley observatory. 22 pages, 8vo.

Huntington, Z., C. Z. Huntington.—(See William Brayton.)

Hyde, Gustavus A.—Summary of observations at Cleveland, Ohio, for the year 1865, and for a period of ten years.

Hydrographischen Anstalt der kaiserlich-königlichen Marine.—Reise der österreichischen Fregatte Novara um die Erde, in den Jahren 1857, 1858, 1859, unter den Befehlen des Commodore B. von Wüllerstorf-Urbair. Nautisch-physicalischer Theil, III. (letzte) Abtheilung. Meteorologisches Tagebuch. Mit 22 beigebundenen lithographirten Courskärtchen, und einer verbesserten Auflage des Planes No. II. (Mittheilungen der hydrographischen Anstalt der k. k. Marine, I. Band, 3 [letztes] Heft.) Vienna, 1865. 4to, 386 pages. [The observations were taken according to the directions published by the maritime conference held at Brussels in 1853, for devising a uniform system of meteorological observations at sea.]

Ingalsbe, Grenville M.—A list of trees and plants in blossom in the Central Park, New York city, on the first day of May, 1865, copied from an authentic source. Observations upon periodical phenomena in plants and animals from 1860 to 1865, inclusive, by Grenville M. Ingalsbe, South Hartford, Washington county, New York.

Ives, William.—Monthly and annual summary of observations kept for the Young Men's Association at Buffalo, New York, during the year 1865.

Kaiserlich Königlich Sternwarte.—Magnetische und meteorologische Beobachtungen zu Prag. Auf öffentliche Kosten herausgegeben von Dr. Jos. G. Böhm, Director, und Dr. Moritz Allé, Adjunct, der kaiserlich königlich Sternwarte. Fünfundzwanzigster Jahrgang: vom 1. Januar bis 31. December, 1864. Prag, 1865. 4to, 154 pages.

Kaiserliche Leopoldino-Carolinische deutsche Academie der Naturforscher.—Die jährliche, periodische Aenderung des atmosphärischen Ozons, und die ozonoskopische Windrose als Ergebniss der Beobachtungen zu Emden von 1857 bis 1864. Von Dr. M. A. F. Prestel, M. d. K. L.-C. d. A. Mit drei Figuren. Eingegangen bei der Akademie am 13. März, 1865. Dresden, 1865. 4to, 141 pages.

Koninklijk Nederlandsch Meteorologisch Instituut.—Meteorologische Waarnemingen in Nederland en zijne bezittingen, en Afwijkingen van temperatuur en barometerstand op vele plaatsen in Europa. Uitgegeven door het Koninklijk Nederlandsch Meteorologisch Instituut, 1864. Utrecht, 1865. Oblong folio, 304 pages.

Kongl. Svenska Vetenskaps-Akademien.—Meteorologiska Jakttagelser i Sverige utgifna af Kongl. Svenska Vetenskaps-Akademien anställda och bearbetade under inseende af Er. Edlund. Femte bandet, 1863. Stockholm, 1865. Oblong folio, 180 pages.

Lapham, I. A., LL. D.—A table showing the monthly and yearly amount of rain at Milwaukee, Wisconsin, for the years 1841 to 1865, inclusive, except 1842 and 1853, prepared for the Daily Wisconsin; the years 1843–1848, by E. S. Marsh, M. D.; 1855–1859, by Charles Winkler, M. D.; and the other years by I. A. Lapham, LL. D. Newspaper slip.

Leipziger Universitäts-Sternwarte.—Resultate aus den meteorologischen Beobachtungen angestellt an mehreren Orten im Königreich Sachsen in den Jahren 1848 bis 1863, und an den zweiundzwanzig Königl. Sächsischen Stationen im

Jahre 1864. Nach den monatlichen Zusammenstellungen im Statistischen Bureau des Königlichen Ministerium des Innern bearbeitet von Dr. C. Bruhns, Director der Sternwarte und Professor der Astronomie in Leipzig. Erster Jahrgang. Leipzig, 1866. 4to, 152 pages.

———— ————. Meteorologische Beobachtungen angestellt auf der Leipziger Universitäts-Sternwarte von 1860 bis 1863. Herausgegeben von Professor Dr. C. Bruhns, Director der Sternwarte. Mit drei graphischen Darstellungen der Beobachtungen von G. Schreiber. (Separatabdruck aus dem vierten Jahresbericht des Vereins der Freunde der Erdkunde zu Leipzig.) Leipzig, 1865. 8vo, 170 pages.

Logan, Thomas M., M. D.—Report on the climate of California, with a comparative table of observations at various stations in the Pacific States. By Thomas M. Logan, M. D., meteorologist to the Board of Agriculture. Printed slips from the Transactions of the California State Agricultural Society for the years 1864 and 1865.

Lösche, Dr. Gustav Eduard.—Meteorologische Abhandlungen von Dr. Gustav Eduard Lösche, Professor der Physik am Königl. Polytechnikum zu Dresden. I. Ueber periodische Veränderungen des Windes an der Erdoberfläche, nach Beobachtungen zu Dresden von 1853 bis 1858. Mit 1 lithographirten Tafel. Dresden, 1865. 8vo, 205 pages.

Lyman, Henry M., M. D.—Observations of the thermometer at sunrise, 9 a. m., 2 p. m., and 9 p. m., and notes of the weather, kept at Hilo, Hawaii, from June 14, 1852, to June 22, 1853. One small manuscript book.

Macgregor, Charles John, M. A.—Abstract of meteorological observations for the years 1861 and 1862, taken at Stratford, Canada West. 5 pages, 8vo.

Matzenauer, Engelbert.—Erdmagnetismus und Nordlicht. Ein Versuch ihren Zusammenhang mit Zugrundelegung der B. T. Meissner'schen Wärmelehre zu erklären. Bearbeitet von Engelbert Matzenauer, k. k. Telegraphen-Inspector in Innsbruck. Zweite vermehrte Auflage. Innsbruck, 1861. 8vo. 31 pages.

Matzenauer, Engelbert.—Planeten, Monde, und Meteore. Nachtrag zu der Brochure; Vortrag über Kometen und Sonnenlicht, von Engelbert Matzenauer, k. k. Telegrafen-Directionsrath, Vienna, 1865. 8vo., 15 pages.

May, R. L.—One small manuscript volume, containing notes of the weather and state of the thermometer, kept at Reading, Pennsylvania, by John H. Raser, during the years 1857 to 1863.

Meteorological Society.—Proceedings of the Meteorological Society; published monthly, London. 8vo.

Meteorological Society of Mauritius.—Transactions of the Society, volume 4, 1854, 70 pages, 8vo.; volume 5, 1861, 220 pages, 8vo.; volume 6, 1864, 260 pages, 8vo. Proceedings of the Society, 1861, 22 pages, 8vo.; 1862, 46 pages, 8vo. Observations taken at the Meteorological Observatory, Port Louis, Mauritius, in 1862, 7 pages, folio; in 1864, 9 pages, folio.

Monnet, P. A.—Nouveau Procédé pour étudier l'Electricité atmospherique. Par Monnet, P.-A., employé au télégraphe, membre de la Société industrielles de Lyon, de l'Association scientifique de France. 8vo., 12 pages, and plate.

Morris, Prof. Oran W.—Comparative view of the temperature for the month of July for the years 1854 to 1865, inclusive, showing the maximum and minimum temperature at 7 a. m., 2 p. m., and 9 p. m. of the month, with the mean temperature of the month for each of the years, at the Institution for the Deaf and Dumb in the city of New York.

Comparison of the weather for the first six months of the years 1855 and 1865, including the means of the barometer and thermometer with the maximum and minimum; the quantity of rain and melted snow, and the difference between the two years. Also the mean monthly temperature, with the quantity of rain, &c., for each of the years from 1855 to 1865, inclusive; the total mean for the six months for those years, and the warmest and coldest of the different months for the years named, at the Institution for the Deaf and Dumb in the city of New York.

Mühry, Adolf, M. D.—Supplement zür klimatographischen Uebersicht der Erde. Mit einem Appendix enhaltend untersuchungen ueber das Wind-system und eine kartliche Darstellung des Systems der Erd-meteoration. Von Adolf Mühry, M. D., Verfasser Von "Allgemeine Geographische Meteorologie," "Beitrage zur Geo-Physik," u. a. Hierbei 2 Karten in Steindruck und 6 Kärtchen in Holtzschnitt. Leipzig und Heidelberg, 1865. 8vo., 320 pages.

München, konigliche Sternwarte.—Wöchenberichte No. 12, vom 17 September bis 23 September, 1865: I. Meteorologische Beobachtungen; II. Magnetische Variations Beobachtungen. 4to. 4 pages.

Navy Department.—Monthly registers from the naval hospitals at Chelsea, New York, Philadelphia, and Portsmouth.

Newton, H. A.—The determination of the height of auroral arches from observations at one place. By H. A. Newton. 4 pages, 8vo. [From the American Journal of Science, May, 1865.]

New York Skating Club.—Report of the meteorologist of the New York Skating Club for the season 1864–'65. New York, 1866. 16 mo., 32 pages.

Observatario magnético y meteorológico del Real Colegio de Belen de la Compañia de Jesus en la Habana.—Monthly bulletins of observations. 8vo.

Observatoire Physique Central de Russie.—Annales de l'Observatoire Physique Central de Russie, publiées par ordre de sa Majesté Impériale, sous les auspices de S. Exc. M. de Reutern, dirigeant la Ministère des Finances et chef du Corps des Ingénieurs des Mines, par A. T. Kupffer, Directeur de l'Observatoire Physique Central. Année 1862. St. Pétersbourg, 1865. 4to, 955 pages. [This volume contains meteorological observations during the year 1862 at nine stations in Russia; also, in a supplement, observations at Glasof and Ourjoum for the years 1853 to 1863, inclusive.]

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Paine, Horace M., M. D.—Summary of observations for the year 1865 at Albany, New York, on two large sheets.

Plantamour, E.—Recherches sur la distribution de la temperature a la surface de la Suisse pendant l'hiver 1863–'64, par E. Plantamour, professeur. Mémoire lu à la Société Helvétique des Sciences Naturelles dans sa Séance à Zurich, le 23 Août, 1864. 8vo., 46 pages.

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Prestel, Dr. M. A. F.—Die jährliche und tägliche Periode in der Aenderung der Windesrichtungen über der deutshen Nordseeküste, sowie der Winde an den Küsten des rigaischen und finnischen Meerbusens und des weissen Meeres. Von Dr. M. A. F. Prestel, M. d., K. L. C. D. A. D. N. Mit zwei Figuren. Eingegangen bei der Akademie den 1. September, 1863. Besonderer Abdruck aus Band XXX, der Verhandlungen der K. L.-C. d. A. Dresden, 1864. 4to. 46 pages.

Die Aenderung des Wasserstandes der Flüsse und Ströme in der jährlichen Periode, als der jährlichen periodischen Bu-und Abnahme des atmosphärischen Niederschlags und Verdunstung genau entsprechend, en Beobachtungen nachgewiesen von Dr. M. A. F. Prestel in Emden. Extra-Abdruck aus der Zeitschrift des Architecten- und Ingenieur-Vereins für des Königreich Hannover. Hannover, 1865. 4to, 7 pages.

Raser, John H.—See R. L. May.

Rogers, F. M.—Register of temperature and times of rain during the first twenty-four days of March, 1865, at Virgin bay, Nicaragua.

Royal Society, London.—Greenwich magnetical and meteorological observations for the year 1863. Quarto, 320 pages.

Sartorius, Charles.—Summary of observations for the year 1865, at Mirador, Mexico.

Schweizerisch Meteorologisch Commission.—Bericht über die organisation meteorologischer Beobachtungen in der Schweiz, August, 1864. 8vo., 120 pages.

Scottish Meteorological Society.—Journal of the Scottish Meteorological Society for the year 1865, published quarterly.

Société Météorologique de France.—Annuaire, 1865.

Spaulding, S. C.—Temperature at sunrise and noon, also notes of the weather during the day, observed at South Pass, Union county, Illinois, from December, 1862, to December, 1865, inclusive.

Trembly, J. B., M. D.—Annual meteorological synopsis for the year 1865 of observations made at Toledo, Ohio. Pamphlet, 12 pages, 8vo.

Valentine, John.—Summary of observations for the year 1865, near Richmond, Indiana. Also a table showing the depth of water which fell in each month and year from January, 1852, to September, 1865, inclusive.

Victoria, Government of.—Results of the meteorological observations taken in the colony of Victoria during the years 1859 and 1862, and of the nautical observations collected and discussed at the Flagstaff Observatory, Melbourne, during the years 1858 and 1862. George Neumayer, director of the Melbourne Flagstaff Observatory, Melbourne, Australia, 1864. 4to, 392 pages of text and 49 pages of diagrams. Appended to the above are meteorological tables for the north and south Atlantic and the southern Indian ocean, 81 pages.

Wilbur, Benjamin F.—Summary of observations for the year 1865, at West Waterville, Maine. Newspaper slip.

Whitehead, William A.—Summary of observations for the year 1865, at Newark, New Jersey. Newspaper slip.

Wilson, Rev. W. D.—A table exhibiting the average temperature at Geneva, New York, for each week in the year, together with the greatest heat and the greatest cold that has been known in that week, during the fifteen years ending December, 1865. Also a table exhibiting the average temperature for each month, with the hottest and coldest days in each month, together with the average amount of water-fall for each month, and the average number of days on which water falls. Newspaper slip.

Winnepissiogee Lake Cotton and Woollen Manufacturing Company.—Depth of rain and melted snow collected in the rain-gauge kept by the company at the outlet of Lake Winnepissiogee, in the town of Laconia, New Hampshire; also depth of rain and melted snow collected in the gauge at Lake Village, New Hampshire, about four miles south, on the same stream of water, for the year 1864.

Wolf, Professor Dr. Rudolf.—Schweizerische meteorologische Beobactungen herausgegeben von der meteorologischen Centralanstalt der schweizerischen naturforschenden Gesellschaft unter Direktion von Professor Dr. Rudolf Wolf. Zurich. 4to, Sept., 1864—Feb., 1865.

Unknown.—Summary of temperature and rain at Pennsylvania Hospital, Philadelphia, for each month in 1864 and 1865; also average of each month for twenty-six years. Newspaper slips from North American and United States Gazette.

REPORT OF THE EXECUTIVE COMMITTEE.

The Executive Committee respectfully submit to the Board of Regents the following report of the receipts and expenditures of the Smithsonian Institution during the year 1865:

RECEIPTS.

The whole amount of Smithson's bequest deposited in the treasury of the United States is $515,169, from which an annual income at 6 per cent. is derived, of	$30,910 14
The extra fund of unexpended income is invested as follows:	
$75,000 in Indiana 5 per cent. bonds, yielding in 1865	3,750 00
$53,500 in Virginia 6 per cent. bonds, yielding in 1865	....
$12,000 in Tennessee 6 per cent. bonds, yielding in 1865	....
$500 in Georgia 6 per cent. bonds, yielding in 1865	....
$100 in Washington 6 per cent. bonds, yielding in 1865 $6, (but not collected.)	
Premium for sale of coin received as interest from the United States	20,333 79
Interest on temporary deposits with the United States Treasurer	478 36
Total receipts during 1865	55,472 29
Balance in hands of the treasurer January, 1865	29,484 08
	84,956 37

EXPENDITURES.

For building and furniture	$39,121 77	
For general expenses	14,149 82	
For publications and researches	9,528 03	
For library, museum, and gallery of art	8,438 12	
		71,237 74
Balance in the hands of the treasurer January, 1866		13,718 63

STATEMENT IN DETAIL OF THE EXPENDITURES DURING 1865.

Building:		
Reconstruction and incidentals	$37,930 71	
Furniture and fixtures	1,191 06	
		$39,121 77
General expenses:		
Meetings of the board, (hack-hire, &c.)	123 64	
Lighting and heating	1,207 95	
Postage	568 61	
Transportation, general, (freights)	1,084 13	
Exchanges, (books sent and received to institutions, expenses of agents, &c.)	1,453 63	

Stationery	539 01	
General printing, (circulars, labels, &c.)	270 00	
Apparatus	2 50	
Laboratory, (chemicals, fixtures, glass, &c)	135 26	
Incidentals, general, (hardware, tools, materials for cleaning, packing twine, general repairs, &c.)	1,683 89	
Extra clerk-hire, (copying)	273 20	
Salaries, (Secretary, chief clerk, book-keeper, messenger, watchmen, and laborers)	6,808 00	
		14,149 82
Publications:		
Smithsonian Contributions to Knowledge	5,651 18	
Smithsonian Miscellaneous Collections	2,113 84	
Smithsonian reports	728 00	
Other publications	207 51	
Meteorology	827 50	
		9,528 03
Library and museum:		
Cost of books	611 86	
Assistants in library	1,300 00	
Transportation for library, (freights)	100 00	
Museum, salary of assistant secretary, and assistants in museum	4,774 14	
Incidentals for museum, (alcohol, mounting specimens, &c.)	954 12	
Transportation for museum, (freights)	500 00	
Gallery of art, (engravings, frames, &c.)	198 00	
		8,438 12
		71,237 74

The foregoing statement shows the expenditure during the year, and the balance in the hands of the treasurer. The receipts, however, did not actually come into the hands of the treasurer in full from the government until the 11th of January, 1866, although credited for 1865.

In accordance with the decision of the Solicitor of the Treasury, the interest on the Smithsonian fund, due 1st of January and 1st of July, 1865, and the 1st of January, 1866, was paid by the Secretary of the Treasury in coin, which, being sold at the current prices, yielded the following sums, viz:

1865.			
April 28.	Sale of $15,455 07, at 48¾, yielded	$7,534 34	
	Less brokerage and United States tax	61 64	
			$7,472 70
Aug. 8.	Sale of $15,455 07, at 43½, yielded	6,722 96	
	Less brokerage and United States tax	60 82	
			6,662 14
1866.			
Jan. 31.	Sale of $15,455 07, at 40½, yielded	6,259 30	
	Less brokerage and United States tax	60 35	
			6,198 95
			20,333 79

This sum is placed among the receipts for the past year, which, together with the balance in the hands of the treasurer at the end of the previous year, made the total available funds $84,956 37 for the year 1865.

The incidental expenses and the cost of the care of the museum are nearly the same as in 1864, but in the previous year $4,000 were received from Congress to defray the expenses, in part, of the care of the government collections of the exploring expeditions, while, owing to the failure of the appropriation bill, but $2,000 were received for this purpose during 1865.

The great expenditure, therefore, of the year has been on account of the building, the particulars of which will be given in the report of the building committee.

The appropriation received for the preservation of the collections of the exploring expedition of the United States, has been expended, as heretofore, under the direction of the Secretary of the Interior, in assisting to pay the expenses of extra employés in the museum, and the cost of arranging and preserving the articles. The specimens intrusted to the care of the Institution by government are in a good condition, and the distribution of duplicates to other museums has been continued during the year.

From the examination made by the committee it appears that, notwithstanding the loss and inconvenience in consequence of the fire, the operations of the establishment have been continued with unabated energy, and that especially the foreign correspondence and exchanges have been increased rather than diminished during the past year.

It appears from the statement of the Secretary, and the accounts rendered by Riggs & Co., bankers of the Institution, that the remainder of the legacy of Smithson, which amounted to $26,210 63 in gold, was sold at a premium from 105 to 107½ per cent., yielding, after deducting the cost of sale and United States tax, $54,165 38. This amount was expended in the purchase of United States bonds bearing 7$\frac{3}{10}$ per cent. interest at par. The following is a detailed statement of the whole transaction:

		£	s.	d.
1864.				
June 11.	The amount received from Fladgate, Clarke & Finch, attorneys, London, as the residuary legacy of James Smithson, was	5,262	0	3
	This amount was deposited with George Peabody & Co., bankers, London, who allowed interest on it to the 5th of March, 1865	153	19	4
		5,415	19	7

This amount was equivalent to $26,210 63 in gold, which was sold by Riggs & Co., under the direction of the Secretary of the Institution, as follows:

$10,000 00 at 207¼		$20,725 00	
15,000 00 at 206⅞		31,031 25	
1,000 00 at 207		2,070 00	
210 63 at 205		431 79	
26,210 63		54,258 04	
Less brokerage, ¼	$65 53		
Less United States tax, $\frac{1}{20}$	27 13		
		92 66	
Net amount realized from sale of gold			$54,165 38

1865.		
February 17.	United States bonds bearing $7\frac{3}{10}$ per cent. interest were purchased at par for	54, 150 00
	Balance, which could not be invested on account of there being no bonds for less than $50	15 38

After the Secretary had purchased these bonds and deposited them for safekeeping with the Treasurer of the United States, it was claimed by the Secretary of the Treasury that this money was *not under the control of the Regents* of the Institution, inasmuch as the original act of Congress of 1846, establishing the Institution, referred to only so much of the bequest of Smithson as was then in the treasury of the United States, and that a special act of Congress would be required to apply this money, or the interest on it, to the uses of the Institution. The Executive Committee would therefore recommend that an application be made to Congress for such a disposition of this money.

It is impossible to make at this time an exact estimate of expenditures for the year 1866. The committee would therefore recommend that $34,660, the regular income of the Institution, be devoted to the maintenance and current expenses of the operations of the establishment, and that the $13,724 63, balance in the hands of the treasurer on the 1st January, 1866, together with the premiums which may be received for the sale of coin, be applied to the reconstruction of the building.

The committee have carefully examined the accounts of the treasurer, and the books as posted by Mr. Randolph for the past year, and find them to be correct.

In conclusion, it appears that the entire bequest of Smithson remains undiminished in the treasury of the United States, and that all the expenditures, from the organization of the establishment to the present time, have been made exclusively from the interest of the original sum, and from the income on accrued interest invested in State stocks.

Respectfully submitted.

RICHARD WALLACH,
RICHARD DELAFIELD,
Executive Committee.

WASHINGTON, *March*, 1866.

REPORT OF THE BUILDING COMMITTEE.

It has been stated to the Board that the fire which occurred on the 24th of January, 1865, destroyed the roof and all the interior of the upper story of the main building, the interior of the two large north towers, and also of the large south tower.

The first step toward the reconstruction of the building was to secure the services of a competent person as architect and engineer to prepare plans and superintend the work. For this purpose Mr. Adolph Cluss, who had designed and directed the building of the principal school-houses of the city, was employed.

The next thing to be done was the making of a critical survey to ascertain the actual state of the walls, and to determine what parts it was necessary first to rebuild. This survey forced upon the committee the conviction that the original construction of the building, as a whole, was very defective, and, in many respects, unsuited as a receptacle of records and other valuable articles, the loss of which could never be repaired. The exterior of all the walls consists of a facing of red sandstone, bound to an irregular backing of bluestone of very bad workmanship. In the main building, and in the lower portion of the large south tower, was inserted a four-inch brick lining, separated by an air space from the main walls. This lining is not bound to the walls, and, therefore, does not add to their strength. It is merely a furring, intended to prevent dampness by the condensation of moisture from the atmosphere. This furring is open at the top, and it was into this that the stove-pipe was inserted which led to the accident by fire. In all the other rooms of the towers the plastering was upon the rough rubble work.

The heavy projecting cornice of the south tower was merely set in place without fastening, and, consequently could not withstand any disturbing action.

The parts of the building which were not injured by fire, namely, the two wings and connecting ranges, as far as the committee have had the opportunity of examining, are defective in materials and construction. The floors, in some cases, though covered with flagging and filled in with deafening, rest upon beams of pine wood, which is decayed, and in the course of a few years the interior of these parts will require renewal.

It is proper to state that the foregoing remarks on the character of the materials, and the construction of the building, are not applicable to the work on the main edifice, subsequently executed under the superintendence of Captain (now General) B. S. Alexander, of the United States engineers. This work, which principally consisted in the arching of the basement and main story of the upper building, was executed in fire-proof materials, and prevented the extension of the fire, and, consequently, the destruction of the entire edifice and all its contents.

From the foregoing account of the original construction of the building, it will not be surprising that the effect of the fire was found to be much more serious than previous to this survey it had been supposed, and that the work to be done could not be confined to the mere repairing of the injury caused by the fire, but would include also the rebuilding of a considerable part of the edifice; and this was particularly the case on account of the decision of the Board that the restoration should be in all parts indestructible by fire.

The heavy projecting cornice of the south tower had fallen down, in part, and the remainder was unfit to receive a new roof.

The high brick columns, extending from the cellar to the eaves of the main building, and supporting the northern wall of the south tower, were so much damaged by the fire as to require to be removed, and, consequently, with them the above-mentioned wall itself. The lining of the upper story of the main building was also so much injured that the greater portion of this will require renewal. But the most instable portion of the building, and that which gave rise to most anxiety, was the principal northern tower. This, which is one hundred and forty feet high, starts from a square base, and is gradually transformed into a regular octagon of smaller dimensions. Four sides of this octagon rest upon the sides of the original square, but project into the interior, while its other four sides extend diagonally across the angles of the square, and are supported by rough and imperfect corbel work, consisting of masses of bluestone very seriously affected by the fire. The tower was originally divided into a series of stories by transverse wooden beams and plank floors, which were entirely destroyed. The anxiety in regard to this tower was increased by observing a vertical crack extending a considerable portion of the height of the tower, but whether this had previously been produced by unequal settling, and had merely been increased by the unequal expansion of the exterior and interior walls, due to the fire, or entirely produced by the latter cause, could not be definitely ascertained. As this part of the building imperatively demanded immediate care, the architect was directed to give it his first attention. After a due consideration of its then present condition and its future use as a receptacle of heavy articles, it was considered necessary to erect within it a lining of solid brick-work nine inches thick, laid in cement, from the bottom to the top, firmly united to the original wall, and serving as the support to iron beams of the brick floors. And, furthermore, it was concluded to fill up, in brick-work, a number of the high, narrow windows in each story, which would add to the strength of the structure without affecting externally its architectural appearance.

A similar construction was directed in the other principal north tower, and the work in both has been executed in such a manner as to give assurance that these parts of the building will not merely be restored, but will also be rendered more stable than they were before the conflagration. The crack above mentioned has been found, by the undisturbed condition of a thin stratum of plaster placed over it, to have remained the same, and the walls, for several months previous and during the winter, have not undergone any perceptible change.

While the work immediately required for the safety of the front towers was in progress, plans were discussed and prepared for the interior of these as well as for that of the south tower, with a view to their better adaptation to the wants of the establishment.

The original plan of the building included four principal staircases leading to the upper story of the edifice, one on each side of the north entrance, and a similar arrangement on the right and another on the left of the south entrance. As these occupied a large portion of useful space, it was thought best to increase the size of those at the north entrance, dispense with those on the southern, and so arrange the heights of the stories of all the towers as to render them more available for the business operations of the establishment.

The work which has been done on the southern tower consists in the removal of the north wall and a considerable part of the upper portion of the other three walls; the preparation of a part of the freestone, from which to reconstruct the exterior wall; the greater portion of the brick-work of the basement, and the furnishing of the cast-iron columns intended to replace the brick piers which supported the northern wall of this tower.

Immediately after the fire, measures were taken by the Secretary to secure the property from the weather by a temporary roof over the main building, and this was effected through the kind assistance of the Hon. E. M. Stanton, Secretary of War, who authorized General Meigs, Quartermaster General, to construct, under the direction of General Rucker, the covering required, though at the expense of the Institution. The work was executed, during the most inclement period of the year, in the short space of two days. This temporary roof, covered with felt saturated with tar, has served the purpose intended. It will, however, rapidly deteriorate, and, consequently, the first object of the committee, during the coming season, will be to decide on the character of the roof, and to hasten its completion as rapidly as the work can properly be accomplished.

In the restoration of the building the committee have been governed by the following considerations:

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

2d. To render it thoroughly fire-proof.

3d. In view of the great cost at present of material and workmanship, and the condition of the funds of the Institution, at first to do such work as should be necessary to preserve the stability of the several parts of the building, and prevent injury to the property by the weather.

The following is a detailed account of the expenditures on the building up to the close of the operations for the winter. It includes not only the items of expenditure immediately connected with the reconstruction, but also those which were necessary as preliminaries in the security of the property and the temporary repair of such parts as could not be deferred:

Expenditures on the Smithsonian building from January, 1865, *to April,* 1866.

PRELIMINARY WORK AND CURRENT EXPENSES.

Pay of laborers removing debris after the fire, taking down walls, and general cleaning up	$1,055 29	
Temporary roof, constructed under direction of Quartermaster General	1,974 25	
Pay of carpenters—repairs	254 75	
Pay of blacksmiths	28 25	
Pay of glaziers	121 95	
Pay of plasterers	98 00	
Glass, oil, paints	544 50	
Nails, tools, and hardware	849 33	
Water and gas pipes, new plugs, extensions, and repairs	1,569 44	
Tin work, new roof on tower, and repairs	256 20	
Repairs to felt roof, and miscellaneous items	93 92	
		6,845 88

RECONSTRUCTION OF THE BUILDING.

Iron work, beams, doors, frames, &c	9,052 22
Stone	400 00
Hard brick (240,333, at $15)	3,605 00
Pressed brick (32,200, at $23)	740 60
Lumber	2,185 84
Cement, 774 barrels	1,436 52
Sand, 545 loads	546 05
Lime	12 64
Hardware, nails, steel, iron clamps, tools, &c	632 82
Rope, blocks, and derricks	135 95
Blacksmiths' coal	18 00

Bricklayers	6,066 86	
Stonecutters	6,609 29	
Carpenters and laborers	4,464 35	
Blacksmiths	465 37	
Riggers	475 75	
Painters	46 00	
Architect	925 00	
Freight and hauling	822 82	
		38,641 08
		*$45,486 96

The following are the estimates of the architect for the work to be completed as far as possible during the present year:

1st. To finish the floor, ceiling, and walls of the northern vestibule or principal entrance to the main building, and doors from this into rooms in the two towers, one on each side; also to put sashes in the tower windows and openings, further to roof in main tower, and space between the two front towers; also to finish the space occupied by main stairs, $8,000.

2d. For iron work and necessary masonry for the principal staircases and doors into rooms and apartments from the lower hall, about $6,500.

3d. For roofing the main building, $17,500.

4th. For the masonry of the south tower, so far as the completion of the enclosure, and for roofing the same, $9,600.

Respectfully submitted.

RICHARD DELAFIELD,
RICHARD WALLACH,
JOSEPH HENRY,
Building Committee.

WASHINGTON, *April* 28, 1866.

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* The difference between this sum and that given in the report of the Executive Committee is due to the difference of the dates of the two accounts.

JOURNAL OF PROCEEDINGS

OF

THE BOARD OF REGENTS.

WASHINGTON, *January* 17, 1866.

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 Saturday, February 3, 1866.

WASHINGTON, *February* 3, 1866.

A meeting of the Board of Regents was held at 12 m. in the laboratory of the Smithsonian Institution.

Present: Hon. L. F. S. Foster, Vice-President of the United States, Hon. Garret Davis, Hon. Lyman Trumbull, Hon. J. W. Patterson, Hon. J. A. Garfield, Hon. J. F. Farnsworth, General Richard Delafield, Hon. Richard Wallach, and Professor Henry, Secretary.

Mr. Foster was called to the chair.

The Secretary stated that since the last annual session the following gentlemen had been appointed as Regents by the Speaker of the House of Representatives:

Hon. J. W. Patterson, of New Hampshire; Hon. J. A. Garfield, of Ohio; Hon. J. F. Farnsworth of Illinois.

Mr. Patterson was reappointed a Regent, and Messrs. Garfield and Farnsworth were appointed to succeed Hon. S. S. Cox and Hon. H. Winter Davis, whose terms as members from the House of Representatives had expired.

The Secretary gave a general account of the objects and operations of the Institution, the nature of the will of Smithson, &c.

General Delafield, from the Executive Committee, presented a report of the financial condition of the Institution, and gave an account of what had been done towards the reconstruction of the building.

The Secretary presented a notice from Roswell C. Brainard, esq., surrogate of the county of Kings, New York, requiring the Smithsonian Institution to appear at his office, in Brooklyn, New York, on the 19th of March next, at 10 o'clock a. m., to attend the final settlement of the account of Francis Vinton, as executor of and trustee under the last will and testament of Thomas Wynns, deceased, of whose estate the Institution is a residuary legatee.

On motion of Mr. Davis, it was

Resolved, That the Secretary be authorized to have the Institution represented at the surrogate's office, Brooklyn, New York, on the 19th of March next.

The Secretary stated that during the past year the interest on the Smithsonian fund in the treasury of the United States had been paid in coin; but that Chief Justice Chase, Chancellor of the Institution, had recommended that application be made to the Secretary of the Treasury for the payment of all the interest in coin, which for the previous three years had been received in currency, since he had while Secretary of the Treasury authorized a similar course in regard to other applications of the same character.

On motion of Mr. Garfield, it was

Resolved, That the Secretary of the Institution be directed to apply to the Treasury Department for the payment of the difference between the interest actually received in currency during the years 1862, 1863, and 1864, and the amount in coin to which the Institution was justly entitled.

Mr. Patterson addressed the Board in relation to the great value of the library of the Institution, and recommended that some action be taken to secure it from danger of destruction, and advised the placing of it, under proper restrictions, in the library of Congress.

After remarks by Messrs. Delafield, Garfield, Davis, and Trumbull,

On motion of Mr. Patterson, it was

Resolved, That a committee be appointed, to consist of one Regent from the Senate, one from the House of Representatives, one resident member, and the Secretary of the Institution, to confer with the Committee on the Library of Congress in relation to an arrangement for the removal of the library of the Smithsonian Institution to the Capitol.

The Chair appointed Mr. Patterson, chairman, and Messrs. Trumbull, Delafield, and Professor Henry as the committee.

The Board then adjourned to meet on Saturday, February 17.

WASHINGTON, *February* 23, 1866.

A meeting of the Board of Regents of the Smithsonian Institution was held this day at 12 m. in the room of the Senate Committee on the Judiciary, United States Capitol.

Present: Hon. L. F. S. Foster, Vice-President United States, Hons. W. P. Fessenden, L. Trumbull, and G. Davis, of the United States Senate; Hons. J. W. Patterson, J. A. Garfield, and J. F. Farnsworth, of the House of Representatives; General R. Delafield, Hon. R. Wallach, and Professor Henry, Secretary.

Mr. Foster took the chair. The minutes of the last meeting were read and approved.

The Secretary stated that, in accordance with the instructions of the Board, he had addressed the following communication to the Secretary of the Treasury relative to the payment of the interest on the Smithsonian fund in coin; but as yet had received no answer:

SMITHSONIAN INSTITUTION,
Washington, February 7, 1866.

SIR: I am directed by a resolution of the Board of Regents of the Smithsonian Institution, adopted at a meeting on the 3d of February, to make application for the repayment in coin of the six instalments of interest on the Smithsonian fund for the years 1862, 1863, and 1864.

These payments were made in currency, but it has since been decided by the legal adviser of the Treasury Department that the Institution was entitled, by the usages of the government in paying the interest on the permanent debts of the United States, to receive its interest in coin.

The Board of Regents have been informed by Chief Justice Chase that, when Secretary of the Treasury, he had made several repayments of this kind, and therefore they consider that the Institution is not only in justice entitled to this claim, but that also a precedent has been established by which it can be readily allowed.

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

JOSEPH HENRY, *Secretary.*

Hon. H. MCCULLOCH,
Secretary of the Treasury.

The Secretary stated that Joseph H. Patton, esq., attorney, 112 Broadway, New York, had kindly consented to attend at the settlement of the account of the executor of Mr. Wynns, that gentleman having from the beginning been fully acquainted with all the facts of the bequest.

Mr. Patterson, from the special committee appointed at the last meeting, reported that a conference had been held with the Library Committee of Congress, and that certain propositions had been discussed relative to the transfer of the library of the Institution, and he recommended the adoption of the following as the conditions on the part of the Institution:

1. That the Smithsonian library be deposited in the library of Congress, subject to reclamation when the Regents may so desire.
2. The public to have access to the library for purposes of consultation every ordinary week day.
3. The Institution to have the use of its own books as at present, and through its Secretary to have the use of the library of Congress, under the same regulations as senators and representatives.
4. That the books, maps, charts, &c., of the Smithsonian library be properly cared for as are those of the library of Congress.

After considerable discussion, in which Messrs. Fessenden, Trumbull, Delafield, Patterson, and Farnsworth took part, on motion of Mr. Trumbull the following was adopted as a substitute for the first proposition reported by the committee.

1. That the library of the Smithsonian Institution be placed on deposit with the library of Congress, not to be withdrawn except on reimbursement by said Institution to the United States of the expenses incurred in taking care of said library, or on such terms and conditions as shall be mutually agreed upon between the United States and the Regents of the Institution.

On motion of Mr. Patterson, it was

Resolved, That a committee be appointed to make the necessary arrangements with the Library Committee of Congress for the proposed transfer.

Mr. Patterson, Mr. Trumbull, Mr. Garfield, and Professor Henry were appointed the committee.

Professor Henry presented his annual report of the operations of the Institution during 1865, which was read in part.

On motion, the Board adjourned to meet at the call of the Secretary.

WASHINGTON, *March* 24, 1866.

A meeting of the Board of Regents was held this day in the laboratory of the Institution, at 7½ o'clock p. m.

Present: Chief Justice Chase, Chancellor; Hon. G. Davis, Hon. J. A. Garfield, Hon. R. Wallach, General Richard Delafield, and the Secretary, Professor Henry.

The minutes of the last meeting were read and approved.

The Secretary stated that the Secretary of the Treasury had granted the request made by the Board relative to the payment of the interest in coin on the Smithsonian fund, in the following manner: The amount which had been paid to the Institution for six instalments of interest, from January, 1862, to July, 1864, in currency, was repaid into the treasury of the United States by Riggs & Co., bankers of the Institution, who received in exchange for it the same amount in coin. This coin was sold immediately in New York, as follows:

March 14, 1866,	$50, 000	at 129¾	$64, 875 00
	5, 000	at 129½	6, 475 00
	37, 730	at 129⅜	48, 813 73
	92, 730		120, 163 73
		Less brokerage and tax	351 98
			119, 811 75

The profit, therefore, to the Institution by the change of the currency to coin, and the sale of the latter, is $27,081 75.

The report of the Executive Committee was presented by the chairman, Mr. Wallach, and after explanations in detail by Generald Delafield, on motion of Mr. Davis, was adopted.

General Delafield, chairman of the Building Committee, presented plans and estimates for the reconstruction of the building.

The subject of the disposition of the money in possession of the Secretary of the Treasury, resulting from the residuary legacy of Smithson, was next considered. The Secretary suggested that so much of this sum as was received from England, independent of the premium on the coin, viz: $26,210 63, should be added to the amount originally deposited in the treasury of the United States by Mr. Rush, making $541,379 63 as the total bequest of Smithson, and that the premium and the interest since accrued be applied to the current uses of the Institution, and to assist in defraying the cost of the restoration of the building. By this arrangement the interesting fact could be stated that, after all the Institution has done in the way of increasing and diffusing knowledge, the entire sum derived from the bequest of Smithson is still undiminished in the treasury of the United States.

The Chancellor recommended that the sum thus added to the money now in the treasury of the United States should be sufficient to make up the amount to $550,000.

On motion of Mr. Wallach, it was

Resolved, That the Secretary be directed to apply to Congress for an act by which the residuary legacy of James Smithson, now in the possession of the Secretary of the Treasury, amounting to $26,210 63, be added to the sum originally received; and that also from the income of the above-mentioned residuary legacy the further sum of $8,620 37 be added, making the total amount deposited in the treasury of the United States $550,000 as the trust fund, the interest on which alone is to be applied to the maintenance and uses of the Institution; and further, that the Regents be authorized to apply the remainder of the income of the residuary legacy to the current expenses of the Institution and the reconstruction of the building.

The Secretary stated that at the last annual session of the Board the disposal of the State stocks held by the Institution was left to the discretion of the Chancellor, Secretary, and Executive Committee; it having been found that it was not necessary, in order to meet the expenditures on the building during the year, to make a sale, it had been concluded not to dispose of these stocks, it being thought that the value of those of Virginia and Tennessee would increase, and that the accumulated interest due would in time be paid.

On motion of Mr. Garfield, it was

Resolved, That the Secretary present a statement at the next meeting of the value of the State stocks held by the Institution.

In the absence of Hon. Mr. Patterson, chairman of the Committee on the transfer of the Library, the Secretary stated that a bill had been presented in the House of Representatives by Mr. Patterson, which had been referred to the Library Committee of Congress, who had reported an act providing for the transfer of the Smithsonian library on the terms agreed to by the Board at its last meeting.

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

On motion of Mr. Wallach, the report was accepted.

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

WASHINGTON, *April* 28, 1866.

A meeting of the Board of Regents was held this day, at 11 o'clock a. m., in the laboratory of the Smithsonian Institution.

Present: Chief Justice Chase, Chancellor; Hon. L. F. S. Foster, Vice-President United States; Hon. L. Trumbull, Hon. J. W. Patterson, Hon. J. F. Farnsworth, Hon. R. Wallach, General Richard Delafield, Dr. T. D. Woolsey, and Professor Henry, the Secretary.

Chief Justice Chase, Chancellor, took the chair.

A note from General Garfield, stating the cause of his absence from the meeting, was read.

The minutes of the last meeting were read and approved.

The Secretary, in accordance with a resolution of the Board at its last meeting, presented the following statement relative to the present value and original cost of the State stocks forming the extra fund of the institution:

Extra fund.	Rate at which bought.	Cost, including brokerage.	Present rate.	Present value.
$53,500 Virginia	93	$49,832 50	68	$36,380 00
12,000 Tennessee........	94	11,167 50	91	10,920 00
75,000 Indiana...........	84	63,000 00	85	63,750 00
500 Georgia..........	100	500 00	100	500 00
100 Washington......	100	100 00	100	100 00
141,100		124,600 00		111,650 00

The Secretary presented to the Board the following copy of the act of Congress which had been approved by the President of the United States, April 5, 1866, relative to the Smithsonian library:

[PUBLIC—No. 20.]

AN ACT to provide for the transfer of the custody of the library of the Smithsonian Institution to the library of Congress.

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That the library collected by the Smithsonian Institution under the provisions of an act approved August tenth, eighteen hundred and forty-six, shall be removed from the building of said Institution, with the consent of the Regents thereof, to the new fire-proof extension of the library of Congress upon completion of a sufficient portion thereof for its accommodation, and shall while there deposited be subject to the same regulations as the library of Congress, except as hereinafter provided.

SEC. 2. *And be it further enacted,* That when such library shall have been so removed and deposited, the Smithsonian Institution shall have the use thereof in like manner as it is now used, and the public shall have access thereto for purposes of consultation on every ordinary week day, except during one month of each year, in the recess of Congress, when it may be closed for renovation. All the books, maps, and charts of the Smithsonian library shall be properly cared for and preserved in like manner as are those of the Congressional library, from which the Smithsonian library shall not be removed except on reimbursement by the Smithsonian Institution to the treasury of the United States of expenses incurred in binding and in taking care of the same, or upon such terms and conditions as shall be mutually agreed upon by Congress and the Regents of said Institution.

SEC. 3. *And be it further enacted,* That the Smithsonian Institution, through its Secretary, shall have the use of the library of Congress, subject to the same regulations as senators and representatives.

SEC. 4. *And be it further enacted,* That the librarian of Congress shall be authorized to employ two additional assistants, who shall receive a yearly compensation of eight hundred dollars and one thousand dollars, respectively, commencing July one, eighteen hundred and sixty-six, to be paid out of any money in the treasury not otherwise appropriated.

SEC. 5. *And be it further enacted,* That the sum of five hundred dollars, or so much thereof as may be necessary, shall be appropriated, out of any money in the treasury not otherwise appropriated, to defray the expenses of the removal herein provided for.

Approved April 5, 1866.

The subject of selling the State stocks forming the extra fund, and of requesting Congress to receive the amount on the same terms as the original bequest of Smithson, was considered.

On motion of Mr. Trumbull, it was

Resolved, That, in addition to the direction given at the last meeting, the Secretary be instructed to apply 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, such 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.

On motion of Mr. Patterson, it was

Resolved, That, in case the privilege is granted to increase the capital of the Institution, the Executive Committee, with the Chancellor and Secretary, be authorized to dispose of any or all of the stocks now held by the Institution, and to deposit the proceeds in the treasury of the United States.

General Delafield presented the report of the Building Committee; which was read and adopted.

The Secretary presented a number of communications to illustrate the correspondence of the Institution.

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

EXTRACTS FROM THE CORRESPONDENCE OF THE INSTITUTION TO ILLUSTRATE ITS OPERATIONS, ADDRESSED TO THE SECRETARY, PROFESSOR JOSEPH HENRY.

From Joseph Leidy, Curator Academy of Natural Sciences.

PHILADELPHIA, *May* 1, 1866.

DEAR SIR: I write in answer to your letter of February 20, in relation to the donation of shells by the Smithsonian Institution to the Academy of Natural Sciences. I was obliged to wait until now to give the committee an opportunity to make out an account of the shells, which account I have just received from the chairman, Mr. Tryon. He reports as follows:

"The collection of shells recently presented to the Academy of Natural Sciences by the Smithsonian Institution embraces over 1,300 species, of which 793 are new to our collection; an extraordinary increase, due in great part to the fact that many of the species are those collected by the Wilkes exploring expedition, (described by Dr. Gould,) never before distributed. There are also a large number of new species from the west coast of North America, recently described by Mr. Carpenter.

"We were indebted to the Smithsonian Institution last fall for a donation of 800 species, including 300 new to our collection, being a first portion of the expedition shells. Uniting the two donations, we have thus received over 2,100 species, including 1,100 new to us, within six months. The accuracy of the names and localities renders the collection a valuable addition to our museum."

From J. Miguel Arroyo, Perpetual Secretary of the Mexican Society of Geography and Statistics.

MEXICO, *March* 24, 1865.

ESTEEMED SIR: This society has been highly gratified by the communication of your note of November last, in which notice is given it of the books which the Smithsonian Institution has had the goodness to remit. In effect, it has just received, at the hands of Señor D. José Ramon Pacheco, three large boxes containing the said books, which the society, with a high appreciation of the gift, has ordered to be placed in its library as a valuable addition to the collection which it already possesses regarding the United States. I shall not forget to seek, and will very soon send you the "Registro trimestro," which you have had the condescension to inquire for; and begging you to accept for your distinguished institution the thanks of this society for the favor conferred, I have the honor to subscribe myself your obedient servant.

From Jno. Evans, Governor of Colorado Territory.

DENVER, *July* 29, 1865.

DEAR SIR: The fossil jaw referred to in yours of the 12th instant was presented to me by Arapahoe Chief "Friday," who said he found it on Rock creek, a tributary of the Republican fork of the Kansas river, about one hundred and fifty miles nearly due east of this place.

In September, 1863, I visited that region of country, vainly endeavoring to get the Indians together in council.

The general character of the country on the head of the Republican, corresponds with other parts of the great plains, in being covered with the buffalo grass on the clayey soils and bunch grass on the sandy hills.

The valleys along the streams are exceedingly fertile, being covered with tall grass, rushes, and other growths of luxuriant vegetation common to such localities in other parts of the country.

On the Whiteman's fork, Rock creek, and Arickaree fork, I found high bluffs, bounding rather narrow bottoms, the general surface of the country being slightly rolling on the general level of the bluffs. For some twenty-five or thirty miles east and west these bluffs show outcrops of a cretaceous limestone, corresponding with the mauvaises terres north of it.

This region has as yet been but little known. Captain J. C. Frémont crossed it in 1843, and Lieutenant Bryan, in his explorations for a wagon road from Fort Riley to Bridger's pass, in the year 1856, on his return trip, passed down Rock creek, but they appear to have given the country but a slight examination, as might be expected on such expeditions. From reports of parties who have crossed the country south of this region on the dividing ridge between the waters of the Platte and Arkansas rivers, near the 103° of west longitude, I am led to believe this cretaceous formation extends for some distance southward from the place that I visited on the occasion referred to.

It is a mistake to call this region a desert, for throughout my trip across the region at the head of the Republican, I found it everywhere covered with grass, furnishing the best of pasturage for stock.

Any further information that I can give will be cheerfully furnished.

From William F. Given, United States Vice-Consul.

MARTINIQUE, ST. PIERRE, *January* 6, 1865.

SIR: I have the honor to acquaint you with the circumstances attending a very remarkable electric phenomenon which occurred in this city on the afternoon of the 23d of November last, while I was absent in the United States.

A heavy temperature, charged with electricity, had for several days pressed on the town, during which time there had been many and violent showers of rain. A sudden and heavy shower of rain had just ceased, when there came a sharp and short detonation like the booming of a cannon, accompanied by a flash of red light, and followed by the smell of burnt powder. At the same instant cries were heard from a house in the principal street, the north end of which had been struck by the lightning. On this end of the house there was a projection, which was shattered, and the rubbles and tiles were almost symmetrically thrown across the street. The fluid then passed along the houses to the right, and down the front of one of these to a distance some two or three feet below the level of the pavement; then passing outwardly, without in any manner deranging the pavement, which was of brick, it extracted from the side of the gutter a flat stone, nearly half a yard in length by about ten inches wide and eight thick. This stone was taken out of the gutter at a level with the bottom without interfering in any way with the surrounding mason-work, and, being taken by an oblique line upwards, was thrown against the house on the opposite side of the street. Here it took out six slats of a Venetian window and falling on the round table of the saloon, shattered its marble slab.

From Charles Hale, United States Consul General.

ALEXANDRIA, EGYPT, *November* 1, 1864.

SIR: I have the honor to enclose for the Smithsonian Institution a map, prepared by the Venetian voyager Miani, in which his explorations upon the river Nile are contrasted with those of the English travellers Speke and Grant. The object of Dr. Miani in requesting that his map (which is accompanied by a printed explanation) should be communicated to some of the learned societies in America is sufficiently explained in his communication to me, of which I enclose a copy. Should you be able to respond in any way to the wishes of Dr. Miani, it will give me pleasure to make any communication to him which you may address to me for that purpose, through the Department of State at Washington.

[This subject was referred to the American Geographical and Statistical Society.]

From R. Brough Smyth, Secretary for Mines.

MINING DEPARTMENT,
Melbourne, January 25, 1865.

SIR: I have the honor to acknowledge the receipt of your letter of the 8th November, 1864, and I am directed by the honorable the minister of mines to forward to you, in accordance with your request, a complete set of papers, maps, and plans, &c., as noted on the margin, relating to mining in Victoria, which have been published by this department; and I am to state that Mr. Sullivan will be glad to receive in return the annual reports, geological reports, and other books, &c., which you offer to forward, and which, on receipt, will be bound and placed in the library of this department. Parcels intended for transmission to this department may be sent, as may be most convenient, either to Messrs. J. M. Mackay & Co., Leadenhall street, London, or to Messrs. Gibbs, Bright & Co., Liverpool, addressed to the honorable the Minister of mines, Melbourne, Victoria, Australia.

From General James H. Carleton, U. S. A.

HEADQUARTERS DEPARTMENT OF NEW MEXICO,
Santa Fé, N. M., August 23, 1865.

MY DEAR SIR: Last week I received from Surgeon B. J. D. Irwin, U. S. army, the enclosed pamphlet in relation to two *aerolites* which were found near Tucson, Arizona, one of which you have in the Smithsonian, and the other I had the honor to present to the city of San Francisco.

In speaking of these aerolites to his excellency Henry Connolly, governor of New Mexico, he informed me that he knows of one far surpassing, in point of size, either of these. As the one he speaks of is probably larger than any one now to be found in any cabinet in the world, I should like very much for the United States to secure possession of it.

In regard to the place where it may be found; the following are the governor's words:

"In the State of Chihuahua, and at the hacienda of Don *Juan Nepumocena Urquida,* say one hundred and eighty miles south of the city of Chihuahua, and directly on the road from that city to Mexico, and directly among the houses of the above-named hacienda, on the left-hand side of the road going to the city of Mexico, and within from thirty to fifty yards of the main road, is, what is supposed to be, an aerolite." Governor Connolly saw it nearly every year for twenty years, the last time in 1846, and he describes it as follows:

"It is a large mass of solid iron, standing like a post in the earth, from which it projects vertically about four feet. Its diameter at the surface of the earth is from two to three feet. It diminishes in size a little from the earth to its apex, which is irregularly rounded. How far it is imbedded in the earth had never been ascertained. Some small pieces, or chips, had been detached by cold chisels and carried off as curiosities; but these pieces were insignificant in point of size, and their removal has not disfigured the general mass as a specimen." The governor says he thinks the portion above ground would weigh a ton or more.

[It would appear from this and other information received at the Institution, that an immense fall of meteorites must have taken place, in a recent geological period, in New Mexico.]

From T. A. Conrad.

PHILADELPHIA, *February* 16, 1866.

"Chalk has at last been found in this country—genuine chalk, with flints and abundance of fossils.

"Smoky Hill, Colorado, is an outlying mass of chalk, probably the only remainder of a vast mass which denudation has removed. If any expeditions should be going that route, it is well the scientific members of it should know this."

UNIVERSITY OF THE STATE OF NEW YORK,
Albany, January 12, 1866.

At a meeting of the regents of the University, held this day, the following resolutions were unanimously adopted:

Resolved, That the regents of the University of the State of New York gratefully acknowledge the receipt of the following valuable additions to the collections of the State cabinet of natural history, presented by the Smithsonian Institution, viz:

A series of specimens of rocks, minerals, and building stones, and a collection of nearly five thousand shells belonging to almost twelve hundred species, properly labelled and distinguished.

Resolved, That the secretary transmit to the Smithsonian Institution a copy of the foregoing resolution.

I hereby certify that the preceding is a true copy from the minutes of the regents of the University.

S. B. WOOLWORTH, *Secretary.*

[The following letter was referred to the Institution by Hon. I. DONNELLY, of the House of Representatives:

From S. Y. McMasters.

ST. PAUL, MINNESOTA,
January 23, 1866.

MY DEAR SIR: I have recently received a communication from the Rev. Charles Reynolds, missionary in New Mexico, in which is the following:

"I had a call last evening, (September 1, 1865,) from Lieutenant Colonel Samuel Tappan, who has lived and served as a soldier in Colorado and New Mexico for several years. He begged me to write you in regard to the Navajo tribe in New Mexico, 250 miles south of Santa Fé.

"'They are 15,000 strong, on a reserve; are the best farmers in New Mexico; have no religious instruction; live in good houses; take but one wife; do the outdoor work, and treat their wives as women; the most hopeful of all Indians.

From another source I learn that their language is almost pure Welsh; that a Welshman can understand them at once; and that the blankets which they make so beautifully are made in precisely the same way as the domestic blankets in South Wales."

The last paragraph of the above is very startling. If true, it suggests a new train of thought for those engaged in the study of American ethnology. Nay, it may go far to favor the views of Judge Hall, that the southern Indian is of Phœnician origin, seeing that the Phœnician impress was strongly made on the Welsh, while the former were developing the tin mines of Britain. Nay, there is not a little to favor the idea that the natives of Wales and Cornwall were themselves of Phœnician derivation.

Now, my dear sir, would it be too much trouble for you to look into the records of the Indian bureau, and ascertain whether there is any record of the fact of the Navajo Indians having anything in language, manners, or customs resembling Wales? If you can learn anything of the kind, you will do a great service to the public, and eliminate, at once, more than half the mysteries of Indian history. True, this may not, in any way, affect the history of the more northern tribes, but it will strongly suggest the idea that Indian history, *in general*, is far more simple than Indians themselves think, or would have us believe.

If you can give me any information on this subject I shall be greatly obliged, and will endeavor to follow it up. If you should not have time to examine the matter, Professor Henry, of the Smithsonian Institution, might look into it.

Yours, faithfully,

[The following are the remarks of Mr. GEORGE GIBBS, to whom the foregoing letter was referred.]

WASHINGTON, *February* 12, 1866.

MY DEAR SIR: I have the pleasure of acknowledging your request that I would answer the queries of Mr. Donnelly's correspondent, and herewith submit such information as I can give, without more research than I now have time for.

The reports of the existence of Indian tribes speaking Welsh are very old, and their alleged location has been as fugitive as that of the Amazons. The story, I presume, took its origin in the tradition of Prince Madoc's voyage, the patriotism of his countrymen leading to the desire that they should participate with the northmen in the glory of discovering this continent. Whatever may have been his fate, it is certain that neither his followers nor the Scandinavians ever left their impress on an American language. As the knowledge of the various Indian tongues has advanced with the progress of settlement and more enlightened inquiry, the identical tribe, speaking Welsh, has receded like the mirage, until it is now sought in almost that last place upon the continent to which a foreign colony could have reached. The Zuñi Indians, a "pueblo" or settled tribe, living in the neighborhood of the Navajos, have enjoyed this reputation of late on the strength of the number of albinos found among them.

The Hon. John R. Bartlett, now secretary of the State of Rhode Island, many years ago prepared a very elaborate paper, giving all the "authorities" on this subject, and, if I am not mistaken, published it at the request of the Welsh citizens of New York. He certainly can elucidate the history of the legend if Mr. McMasters desires to pursue the inquiry in that direction.

I do not, however, think it necessary to dwell upon that point, or the improbability of a change of color and feature. The character of the language of the Navajos is well known. Vocabularies, more or less extensive, have been obtained from time to time by various officers of the army, government agents, and by Mr. Bartlett himself. From these the late Professor Wm. W. Turner demonstrated, long since, its affinity with the great Athapascan or Chepewyan stock, a family occupying the northern part of the continent, next south of the Esquimaux, and extending from the shores of Hudson's bay to those of the Pacific. To this family the Apaches, neighbors of the Navajos, also belong. Mr. Hale, the philologist of the United States exploring expedition, had previously obtained vocabularies of the Tahculli or "Carriers," of Fraser river, of certain bands near the mouth of the Columbia, and of the Umpquas, to which he gave the collective name of "Tá-ka-li-Um-kwa." These Mr. Gallatin proved to be Athabascan tribes. In 1851 I collected new vocabularies on Rogue river and the Klamath, still further south, which Mr. Tnrner recognized as belonging to the same family. All these form links in the chain connecting the Navajos and Apaches with the parent stock, and show that the migration southward of the Athabascans took place by routes west of the Rocky mountains.

In fine, the Navajos and Apaches are offshoots of an extreme northern race who have wandered southward, just as the Camanches are a branch of the Shoshonees or Snakes of Oregon. I have by me vocabularies of almost every tribe of this great family, and can assure you that the verbal similarity is conclusive as to their common origin. But my opinion is not needed in corroboration of that of Turner and of Buschmann.

As regards the blankets, they are the common pattern of the Mexican "sarape," made, it is true, exceedingly well.

Very respectfully, your obedient servant.

From M. M. Lisboa, late Brazilian minister to the United States.

RIO DE JANEIRO, *December* 17, 1864.

MY DEAR SIR: I did not forget my offer to you, and immediately on my arrival here, in July last, I made application directly to the Emperor for the remittance of contributions to the Smithsonian Institution, and through his Majesty's gracious intervention I obtained orders which gave me satisfaction, and will, I have no doubt, be agreeable to you also.

I have since crossed the ocean again, and am just arrived from Europe; this absence has delayed my addressing you this letter.

The work of Descourtilz is ready to be offered to the institution—that is to say, the first volume, for the continuation has not yet been published; and they are preparing at the museum a collection of birds for the same destination. This collection is not complete; but I have taken upon myself to delcare that that was not an objection, as a complete collection can only be made by degrees.

I have also obtained an order from the Historical and Geographical Institute of Brazil that a set of their three-monthly publication (the Revista) be sent to you. You will find that some of the volumes are missing, because the boxes in which they were kept have been destroyed; but if they are republished they will be sent to you.

All these presents will be delivered here to Mr. Monroe, the United States consul, with whom I have communicated, to insure their remittance, as I agreed with you and Mr. Seward. I find, however, that he has received no instructions from the State Department as I expected, and beg to suggest to you the propriety of speaking again to the Secretary of State to request him to forward said instructions to the consul.

Please to let me know that you have received this letter, and you may deliver your answer to Mr. Fleury, attaché to the Brazilian legation at Washington.

Yours, very truly.

From Don F. L. L'Burlamaqui, director.

BRAZILIAN NATURAL HISTORY MUSEUM,
Rio de Janeiro, January 4, 1865.

MY DEAR SIR: I am ordered by the Brazilian government to send to the Smithsonian Institution, of which you are the worthy secretary, a collection of natural history specimens of this country, taken from the duplicates existing in this museum.

Performing this duty, I should be very happy if I were enabled to send you objects worthy of being ranked among your magnificent collections; but, unhappily, I cannot do it in this opportunity, because the Brazilian museum is exhausted with the returns made to other museums.

I send a few ornithological and entomological specimens and some bones of fossil animals.

I send also a copy of the "Ornithologie Brésilienne," a work which shall continue and form four or five volumes.

I profit by this opportunity to let you know that it would be very agreeable to this museum to possess the interesting memoirs of your illustrious society.

I am, sir, with the highest consideration and respect, yours, very truly.

From the Museum of the city of Bern.

BERN, *March* 25, 1865.

HONORED SIR: By order of the commissioners of the museum of the city of Bern, the undersigned beg leave to trouble you with the question whether it will be practicable for them to obtain, through your kind intervention, a specimen of the bison, by which is meant the well-preserved skin, with skull and extremities, of a full-grown male animal of that species, and what will be the price of such an acquisition? On account of the narrowness of our financial means we find ourselves necessitated to place this question in the foreground, and on that account also address ourselves directly to your interposition, holding ourselves always ready to acknowledge the favor by any reciprocal service which may lie in our power. Encouraged by the friendly assurances of Mr. Fogg, envoy of the United States of North America to the Swiss confederation, we prefer our request to you in the hope of being favored with an obliging reply to the above-proposed questions.

Be pleased to accept the assurances of the high consideration with which we have the honor to remain, (in the name of the commission of the museum of Bern,)

C. FISCHER, *President.*
B. STUDER, *Secretary.*

From Charles F. Loosey.

AUSTRIAN CONSULATE GENERAL,
New York, am. February 13, 1865.

SIR: The Imperial Library of Vienna having published a repertory of the oriental manuscripts of the library, translated into Latin by the philosophical-historical class of the Academy of Sciences, under the title of "Tabulæ codicum manuscriptorum in biliotheca palatina Vindobonemi asservatorum," and placed a number of copies of the same at the disposition of the imperial royal lord steward's office, requesting that those public institutions and scientific societies which have favored the imperial library with donations may be presented with a copy of the repertory, I am directed by the imperial royal ministry of foreign affairs to transmit to the Smithsonian Institution such copy, and beg to inform you that I have forwarded to you by mail the repertory above referred to.

I have the honor to subscribe, yours, most respectfully.

From the British minister.

WASHINGTON, *November* 6, 1865.

MY DEAR SIR: Allow me to introduce to you Dr. H. Berendt, an American gentleman of science, who is about to visit Honduras and its neighborhood, under the patronage of the Smithsonian Institution of this place. His objects are to obtain a more accurate knowledge of the geography and natural history of that region, and to explore what is still unexplored. If you can assist him, you will oblige,

Yours, faithfully,

FREDERIC W. A. BRUCE.

JNO. GARDINER AUSTIN,
Lieutenant Governor of Honduras.

From Señor Irisarri, minister from Guatemala.

BROOKLYN, *November* 2, 1865.

SIR: In answer to your favor of the 30th ultimo, I have to say that I am not personally acquainted myself with the corregidors of Peten and Verapaz. Therefore it seems to me more conducive to the object of having Dr. H. Berendt well recommended to the authorities of the different departments he intends to visit, to write by the steamer of the 16th instant to the minister of foreign affairs of Guatemala, telling him Dr. Berendt's object, and the minister will no doubt write to the corregidors recommending them to tender the doctor any assistance or information in their power, to further such useful undertaking.

In case Dr. Berendt wishes it, I send for him a passport, recommending him officially and especially to whatever authorities he may meet in Guatemala.

The secretary to this legation is the member for Peten in the chamber of representatives of Guatemala, and although not personally acquainted with the corregidor, is known to the latter by name and may, if desirable, give a letter for the corregidor.

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

A. T. DE IRISARRI.

Don Antonio José de Irissarri, envoy extraordinary and minister plenipotentiary of the republic of Guatemala for the United States of America, to the authorities of the departments of Yzabal, Vera Paz, and Peten.

Inasmuch as Dr. H. Berendt is about to undertake a scientific exploration in those countries under the auspices of the Smithsonian Institution, the advantages resulting from which will inure as well to Central America as the rest of the world, in the advancement of historical and geographical knowledge, and inasmuch as such enterprises ought to be promoted by the authorities of the countries in which they are undertaken, I would hope that this may serve as a general recommendation for Dr. Berendt, to the effect that he may be received in all places in a manner suitable to the meritorious objects of his expedition.

Given at Brooklyn the second of November, 1865.

A. J. DE IRISSARRI.

From Señor Luis Molina, minister from Costa Rica, &c.

LEGATION OF HONDURAS,
Washington, November 8, 1865.

DEAR SIR: In answer to your favor of 31st of October, ultimo, which only came to my hands three days ago, I have the pleasure to send you, as requested, two letters, enclosed herewith, for the collectors of Omoa and Trujillo, respectively; and, besides, another to the minister for foreign relations, because there is plenty of time before Dr. Berendt may go to Honduras, and I think the best plan is to get the necessary orders from the government at Comayagua to speed his good work.

I have the honor to be, with great respect, your obedient servant.

WASHINGTON, *November* 8, 1865.

At the instance of the worthy secretary of the Smithsonian Institution, established in this capital, in conformity with the will of its founder, for the promotion of human knowledge, I have the honor to recommend to you Dr. H. Berendt, a naturalized citizen of the United States, an accomplished gentleman and man of science of great merit, who, under the auspices of the Institution and as its agent, purposes a visit to Central America with a view to exploring the less known portions of Guatemala and the coasts of that republic, in order to augment the knowledge which we at present possess of their geography and natural history.

The enterprise is purely scientific, interesting to the learned world, and cannot fail especially to redound to the advantage of the people and government of Honduras as far as concerned in its execution; under which impression I can entertain no doubt that you will extend to Dr. Berendt and his assistants the protection, aid, and facilities which may be in your power towards the furtherance of their objects, agreeably to the request of the representative of the republic in the United States.

Be pleased to accept the assurance of the distinguished consideration of your obedient servant,

LUIS MOLINA.

The ADMINISTRATOR
of the Puerto de Omoa, Honduras.

WASHINGTON, *November* 7, 1865.

Dr. Berendt, accompanied, perhaps, by an assistant, is about to proceed to Central America, as an agent and under the auspices of the Smithsonian Institution in this capital, for the purpose of increasing the knowledge which we already possess respecting the geography and natural history of certain unexplored parts of Guatemala and the coasts of Honduras.

This purpose recommends itself by its intrinsic interest to the scientific world, and more especially to the government and people of that republic; and Dr. Berendt has been strongly represented to me by Professor Henry, the distinguished secretary of the above institution, as being a gentleman of great attainments in science, and of much personal merit.

Upon these considerations, I pray you to accord to the Doctor the regard which he merits, and to his enterprise the protection, aid, and facilities which may be in your own power, or to procure them for him from the functionaries with which you may be in correspondence.

Dr. Berendt is a native of Germany, and a naturalized citizen of the United States.

It remains only to subscribe myself, with the highest respect, your obedient servant,

LUIS MOLINA.

The ADMINISTRATOR *of Trujillo, Honduras.*

WASHINGTON, *November* 7, 1865.

I have been informed by the highly esteemed secretary of the Smithsonian Institution, Professor Henry, that Dr. H. Berendt, a citizen, by naturalization, of the United States, has formed the design of prosecuting, under the auspices of the Institution, either alone or accompanied by an assistant, an expedition into certain unexplored regions of Central America, with a view to increasing the amount of our present knowledge of their geography and natural history. This visit, purely scientific, will embrace the departments of Peten, Vera Paz, and Golfo Dulce, in Guatemala, and will terminate on the coast of Honduras.

As far as regards the exploration of their own coasts, the people and government of Honduras will find themselves especially interested in this enterprise, so strongly commended, as Professor Henry well observes, to the favor of the whole scientific world; and I have therefore consigned to the worthy professor the papers which he has been pleased to request of me in the name of the Institution for the authorities of the northwest coast, recommending to them to extend to Dr. Berendt all the protection, aid, and facilities in their power and which may be necessary to secure success to his meritorious undertaking. I have sent to him, moreover, this letter, directed to your excellency, in order that the [illegible]preme government may have knowledge of the projected enterprise, and that, [illegible] the opinion of its merits be as favorable as I think it will be, orders may be [illegible]ciously issued to the authorities of the northwest coast in conformity with [illegible] objects indicated in my recommendations above referred to.

I should add that Professor Henry speaks of Dr. Berendt as "a gentleman of great merit and of high accomplishments in point of science."

Allow me the honor of subscribing myself, in conclusion, your excellency's very obedient servant,

LUIS MOLINA.

His Excellency Señor Don FRANCISCO CRUZ,
Minister of Foreign Relations of Honduras, &c.

From J. Rosing, Chargé d'Affaires.

HANSEATIC LEGATION,
Washington, D. C., February 25, 1865.

SIR: I beg to inform you that a society has been constituted in Bremen for the promotion and dissemination of natural science and knowledge.* They have requested me by their secretary, Dr. Phil. Franz Buchenan, to further their ends on this continent, and I think I cannot do better than to recommend the young society to your kind consideration. They will be very grateful for any communication on the part of your Institution, and endeavor to give in exchange whatever may be of interest for you and in their reach. They propose publishing regular annual reports and periodicals, and dare to offer regular exchanges, although conscious that their doings, owing to the smallness of their means, will by no means compare with those of your proud Institution. The more happy you could make this little fraternity ardently devoted to science.

Allow me, sir, again to offer you the assurance of my very high esteem as your most obedient servant.

From the same.

SIR: The government of Bremen, sensible of so many acts of liberality of yours, have directed me to offer you the accompanying volume, of entirely Bremen origin and workmanship, as a contribution to the library of the Institution. It is a publication made by the society for Bremen history and antiquities, and gives an illustrative description of that most venerable and accomplished monument of the history of the republic, the court-house, at the same time the seat of the government and senate.

I shall be gratified, sir, if you would kindly accept this small gift as a token of good will on the part of the Bremen government and scientific societies towards your most useful institution, and beg to solicit the continuance of your highly estimated favor.

From the same.

HANSEATIC LEGATION,
Washington, D. C., April 8, 1865.

SIR: I am happy to learn, from your kind note of the 5th instant, that the Smithsonian Institution will be pleased to enter into scientific correspondence with the new society for the promotion of natural science, at Bremen, whose establishment I had the honor of announcing to you recently.

It is with gratification that I accept your liberal offer of a package of your publications for the society; if it could be ready by the end of next week, I shall have an opportunity of sending it off directly with other official matter.

With many thanks for your favors, believe me, sir, to be, with high regard, your most obedient servant,

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From J. George Hodgins, Department Superintendent.

DEPARTMENT OF PUBLIC INSTRUCTION FOR UPPER CANADA,
Toronto, March 26, 1866.

SIR: I have the honor to state, in reply to your letter of the 10th ultimo, that the numbers of the Journal of Education for which you have applied have been sent to you.

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* Naturwissenschaftlicher Verein.

You are already aware, from previous correspondence with this department, that the legislature of the province, at the instance of the chief superintendent of education, authorized the establishment of a meteorological station in every county in Upper Canada in connexion with the department of public instruction, the observers being the head masters of grammar schools. The following instruments were obtained from England for each station: Barometer by Negretti and Zambra; dry and wet bulb thermometers by the same; and maximum and minimum thermometers by Cassella. These were compared with standards at the new observatory by Mr. Glaisher, and again at the Toronto observatory. They are excellent instruments, and may be relied on. Each station is also supplied with a wind vane and rain gauge. Full instructions and tables, together with forms for periodical reports, are provided for the observers.

As some of the counties have hesitated to pay for the instruments, and in others the observations were not duly taken, it was deemed necessary in 1865 to obtain further legislation and regulations on the subject. Although some observers faithfully performed their duty under the former system, it was found that more satisfactory results would be obtained by restricting the number of stations and making a pecuniary allowance to observers for their labors. Our stations are now ten (10) in number, situated at the most favorable points between longitude 83° and 74° west, and latitude 42° and 46° north. The observers are educated men, and graduates of universities. Arrangements have also been made for the careful examination and comparison of the records of the observations at this office. The results will appear monthly in our official journal.

I send herewith copies of some recent regulations which we have issued to our stations.

As our meteorological establishments are now being placed on a more satisfactory footing, we may hope to contribute information of permanent value; and your institution would confer a favor on this department by sending us as complete a series of its meteorological reports, with any papers bearing on the subject, as it may be able to afford.

I have the honor to be, sir, your obedient servant,

JOSEPH HENRY, Esq., LL. D.,
Secretary Smithsonian Institution, Washington, D. C.

From A. Panizzi, Principal Librarian.

BRITISH MUSEUM, *July* 20, 1865.

SIR: I have to acknowledge the receipt of your letter of the 18th May last, informing me that on behalf of the Smithsonian Institution you have forwarded to the British Museum, as a present to the trustees, upon certain conditions, a type series of fossils from the upper Missouri, collected by Lieutenant Warren, and Dr. Hayden.

In reply, I have to express the thanks of the trustees of the British Museum for the very obliging offer which the managers of the Smithsonian Institution have made to them, and I am to assure you of the readiness of the trustees to reciprocate the kind feeling of interest which the managers have shown in the improvement of the collections of the British Museum. Although the trustees cannot accept a present under restrictive conditions, they are prepared, in this instance, to meet the wishes of the managers of the Smithsonian Institution so far as may be in their power, and I am accordingly directed to send you herewith the copy of a letter on this subject from Professor Owen, the superintendent of the departments of natural history in this museum, on whose views the trustees are disposed to act.

I have the honor to be, sir, your most obedient servant,

[The condition referred to as required by the Smithsonian Institution is that suitable returns be made from the duplicates in the collections of the Museum when called for. This condition is made on the part of the institution to favor the formation of museums in this country.]

BRITISH MUSEUM, *July* 20, 1865.

DEAR SIR: I enclose the letter and form which you brought me on the 17th instant from the principal librarian. In reference to the fourth condition, after due inquiry and inspection, I should be prepared, when required, to submit to the trustees a series of duplicates, in my opinion suitable in the sense of equivalency, as a return for the type series of fossils proposed to be presented conditionally by the Smithsonian Institution, Washington, United States.

I remain, dear sir, yours truly,

RICHARD OWEN.

THOMAS BUTLER, Esq.,
Assistant Secretary British Museum.

From Professor William Hincks.

TORONTO UNIVERSITY, *March* 26, 1866.

DEAR SIR: The additional proof just received of the liberality with which the Smithsonian Institution uses its duplicates in promoting science claims something more than the mere formal expression of our gratitude, and is, I assure you, very highly appreciated by myself and my colleagues in the management of our museum, and by all the authorities of our University.

Permit me to explain that the objects of the museum are first to afford the best attainable materials for instruction in the several branches of natural science to the professor of that department in University College; secondly, to afford opportunities for private and special study to any persons seeking them; and thirdly, to offer a pleasing and instructive exhibition to the public at large, which is opened at all proper hours without any payment, and is extensively visited both by our own citizens and the numerous travellers from the United States, the most intelligent of whom have expressed themselves in the most flattering terms respecting its interest and beauty. We perhaps excel most in birds, and so far as the representation of the few families that remain unrepresented in the collection, and the completion of our North American series, nothing is more desirable to us. In mammalia we aim chiefly at obtaining the moderate-sized native animals, and a few of the more deviative forms, not having either space or funds for attempting more. We have a good instructive series of shells and some special collections, and this branch is a favorite one. We have many fine insects, and greatly desire to extend and improve that collection. We have some excellent specimens of fishes, both British and Canadian, and a few Chinese and West India. We have some crustacea and echinodermata and a few good polypifera, but are comparatively deficient in these interesting branches. Our botanical collection includes about 7,000 species, chiefly European and North American, with many fine forms from all parts of the globe. We are somewhat crippled in funds, which checks very rapid increase, and we have no means of accumulating duplicates to any extent, but we should feel the sincerest pleasure in rendering any service within the reach of our efforts to the Smithsonian Institution as a proof of our estimate of the value of its contributions to science and the liberality with which it seems to be conducted. Our specimens are throughout systematically arranged and handsomely exhibited. I should have said above that we have some good reptilia and amphibia, yet very few comparatively.

From the Rev. E. Petitot to Wm. L. Hardisty, Esq., (communicated by Mr. Hardisty.)

[Translation.]

FORT RAE, HUDSON'S BAY TERRITORY, *June* 20, 1864.

MY DEAR SIR: I take advantage of Mr. Smith's departure to offer you the assurance of my respect and perfect consideration. I thank you for myself and my converts for the permission you have kindly given me to take up my quarters within the bounds of Fort Rae.

As Mr. Smith will perhaps tell you, I have had an opportunity of visiting the tribes which inhabit the interior of the country comprised between Great Bear lake, Copper Mine river and Fort Rae. It is through the kindness of that gentleman that I have been able to comply with the desire of the Indians, and I am infinitely indebted to him for it. As many incidents of this journey were of a kind to interest a "voyageur," I shall allow myself to amuse you with a few.

In the first place I will spare you the fatigues of a journey on snow-shoes, which, notwithstanding your reputation as a pedestrian, you will doubtless not regret performing without stirring from your easy-chair. I will transport you, therefore, at once to Lake Kleritié, eight days' travel by that method to the north-northeast of Fort Rae, and ten or twelve days by canoe. There, upon a pretty high hill, is situated the camp.

A magnificent view is enjoyed from this point of the above-named lake and of Lake Kamitié, which empties into it. Their immovable and frozen surfacs winds between feldspathic mountains, sometimes naked or eaten into by lichene and mosses, sometimes covered with forests of thorns. But these trees are only pigmies of five or six feet in height; wretched shrubs whose roots are buried in a thick bed of yellowish lichens, and whose dwarfed and vertical branches allow the rays of the sun to pass through. On the left extend arid steppes, dotted with pools of stagnant water, serving as a pasturage for herds of rein deer which run unceasingly over the surface of the lake. This country is a true Arabia Petræa, where the eye takes in only blocks of granite, masses of coarse porphyry, diorite, and especially of feldspathic orthose. Here there is no stratification, no talus of debris or metamorphism; the mountains have undergone no degradation, and the waves which beat against their foundations dash themselves in vain. Upon the slope opposite to these rocks stretches the Ot'-el-néré or flat country of the Esquimaux, which, despite its name, is composed only of mounds and rounded hills. I did not go there only because I had more work than I could perform among my Indians. It is time that I spoke of them.

They belong to the great Montagnais or Ténè nation and to the Slave tribe, but their idiom is very different from the language of the Ténès. Many of these Indians have already made the voyage from Portage la Roche, and this present year two of them are preparing to repeat it. The young people and grown men alone visit Fort Rae, or that at the Forks, or have intercourse with the whites. The rest of the tribe, the old men, women and children, not only have never seen the missionaries, but even a white man of any sort. I except, however, King Beaulieu, who visited their midst in May, 1863, but did not ascend as far as I, by nearly three days' journey.

It is a singular spectacle, that of a horde of these savages on their march over a frozen lake, and it was the first time that I have been permitted to witness it. As far as the eye could reach, a long file of sledges and dogs, women loaded with burdens and young children, the cries of infants, the barking of dogs, and the shouts which their conductors uttered—the whole forming a picture as curious as wild.

I have told you that these poor Indians have never yet seen a missionary, which is saying that they had not a trace of Christianity. Thus disease (I speak of moral disease) has made frightful ravages among these unfortunate tribes. It cannot be denied, and it is my conviction, that we shall assist at the obsequies of the Dog Rib nation, (Plats cotés de Chiens.) The incredible venereal excesses to which these wretched people were formerly addicted has destroyed the constitution, although so robust, of the Indians, and abridged half their existence. Among them young persons are only seen emaciated and frail, with bony and hectic faces. Pulmonary consumption slowly undermines the tribe. During the forty-four days which I passed among them, I ministered to two of them on the death-bed. On my return to Fort Rae I found five graves yet fresh, and upon the journey of the Toaut Onédés river two others; a tenth savage is dying at the moment of my writing you these lines. If you add to this figure, already unhappily too great, the thirty-four Indians who died during the last winter, you have forty-four deceased in the space of six months, an enormous total considering the number of this tribe, (about 1,200, according to my enumeration.) To pthysis, which appears to be, with the venereal disease, the scourge of this people, is added influeneza, which extends its ravages especially among the children.

Poor people, they are very different in their morals from what they were formerly. The beneficent light of the gospel, in entering their hearts, has opened their eyes to their past excesses; but as Adam, converted, they carry the chastisement of their guilt with them.

It is to these causes, venereal excess and incestuous unions, that I attribute the general stuttering of these Indians. Among ten there was not a single one who was not a stammerer.

I have dwelt long upon these Indians, my dear sir. What I have told you is not for the purpose of exposing the scourges of humanity. My duty is to hide them. But they speak eloquently in showing that the theory of the primitive man, the happiness of man free and cut off from religion, is but an Utopia, worthy of the philosophers who invented it.

Once more I thank you for having afforded me the means of doing a share of good to these poor Indians. They will repay you some day. I refrain. It is time to close this long farrago, which has become tedious.

Will you, while excusing my loquacity, receive anew the assurances of the distinguished consideration with which I am, sir, your very humble servant.

EULOGY

ON THE LATE

JOSEPH G. TOTTEN,

BREVET MAJOR GENERAL,

LATE CHIEF ENGINEER U. S. ARMY, AND REGENT OF THE SMITHSONIAN INSTITUTION.

By J. G. Barnard, Lieutenant Colonel of Engineers, and Brevet Brigadier General U. S. A.; Brigadier and Brevet Major General U. S. V., A. M., LL. D., N. A. S.

[Reprinted from the Annual of the National Academy of Sciences for 1866.]

[Instead of preparing a eulogy myself, as requested by the Board of Regents, of their lamented associate General Totten, I have thought the service would be better rendered by presenting the facts I had gathered on the subject to General Barnard, and by adopting his tribute to the memory of one so long and so efficiently connected with the Institution. J. H.]

Mr. President and Gentlemen of the Academy:

In conformity with a clause of the constitution of this Academy, and in obedience to your instructions, I am here to render the tribute of a formal biographical notice in commemoration of one who was numbered among our most venerable and most honored associates. If, in the language of one of our body, on a previous and similar occasion, "it is no unreasonable assumption that public benefit and individual incentives may be derived from the history of any man whose scientific services have rendered him worthy of admittance to your number," that assumption must have a peculiar force when it applies to one who has "finished his course," and has filled a life, protracted beyond the usual term, with scientific labors of no ordinary variety and magnitude.

It is but little more than two years since we first met for the great and important work of organizing this National Academy, and with us—of our number, if not personally present—were "both the gray-headed and very aged men." But, alas! these, like autumnal leaves, are rapidly falling away, and already the places of a Totten, a Hitchcock, and a Silliman know them no more, save in the records of their lives and deeds, and in the grateful memories of their associates. What a trio of names, glorious in the annals of science, is this! Well may they be incentives to us who yet remain to strive that we may worthily replace them, and establish for this Academy a reputation for usefulness and science which their honored bearers have acquired for themselves.

Although there may be many among us more capable than myself of doing justice to the memory of our departed colleague, I feel grateful that the lot has fallen to me. Placed under General Totten on my first entrance into the military service—almost in my boyhood—my relations to him, both personal and professional, have ever since been continuous and intimate. Under obligations to him of no ordinary nature, I could not do otherwise than regard him with reverence and affection. If I fail, therefore, it shall not be because my heart is unmoved, nor because I am insensible to the magnitude of my task.

JOSEPH GILBERT TOTTEN was born in New Haven, Connecticut, on the 23d of August, 1788. His grandfather, Joseph Totten, came from England before the war of the Revolution, and engaged in mercantile pursuits in New York. Attached to the cause of the mother country, he left that city, after the acknowledgment of our independence, for Annapolis, Nova Scotia. It would appear that his two sons remained in this country, since one of them, Peter G. Totten, married in 1787 Grace Mansfield, of New Haven, a very beautiful woman, who died a few years after her marriage, leaving two children, the subject of this memoir and a daughter, Susan Maria, who married Colonel Beatty, an English officer, and who is still living, a widow, in London. After the death of Mrs. Totten, which occurred when her infant son was but three years old, the father, having been appointed United States consul at Santa Cruz, West Indies, took up his future abode on that island, leaving his son under the care of his maternal uncle, Jared Mansfield, a graduate of Yale College, 1777, and a learned mathematician. The boy continued to be a member of Mr. Mansfield's family until the latter removed to West Point, having been appointed captain of engineers and a teacher in the United States Military Academy, then just organized by act of Congress of 1802. Young Totten's first teacher was Mr. Levi Hubbard, brother to the rector (at that time) of Trinity church, New Haven; afterwards his education was carried on under the personal superintendence of his uncle. Of the period of his schoolboy life we have some glimpses, through the recollections of an old friend and schoolmate, Mr. Ralph Ingersoll of New Haven, who speaks of him as a bright, noble youth, of fine mind, fond of study, and always at the head of his class, gentlemanly in his deportment, and greatly beloved.

Young Totten went to West Point with the family of his uncle in 1802. He was soon after appointed a cadet. He remained at West Point one term, that of 1803, and perhaps part of that of 1804. He was promoted to a second lieutenancy in the corps of engineers, July 1, 1805.

The venerable General J. G. Swift, recently deceased, his brother engineer officer and life-long friend, describes him at West Point as "a flaxen-headed boy of fourteen years of age, a good scholar, and to me a most interesting companion."

Captain Mansfield, having been appointed surveyor general of Ohio and the western Territories, November 4, 1803, induced his nephew to accompany him to the west as an assistant on that first systematic survey of any of the new States of the Union. Here that faculty which so distinguished him through life, of keen observation of whatever was most interesting connected with or incidentally brought under his notice by his professional pursuits, displayed itself at this early age in a noteworthy manner. The vestiges of an earlier race than the red man, which have since been made the subject of the researches of a Squier and a Davis, of a Lapham and of a Haven, and to which, during recent times, fresh attention has been directed by the developments of the high antiquity of the human race in Europe as shown by similar relics over the surface of that country and by the lacustrine remains in Switzerland, attracted his notice and were made the subjects of survey. Although these investigations were not published, they are, I believe, the first we have record of; those of Caleb Atwater, who is called by Squier and Davis "the pioneer in this department," not having been published until 1819. Full descriptions and measurements of several of these mounds, particularly that of Circleville, were made and sent to his friend, J. G. Swift. To most youths of his age those remains of structures, built

> "while yet the Greek
> Was hewing the Pentelicus to forms
> Of symmetry, and rearing on its rock
> The glittering Parthenon,"

would have been passed over with vague curiosity or listless indifference. Not so with young Totten. Although not able, perhaps, to perceive all the ethnological importance which has since been attached to them, he could yet appreciate them as objects of high interest, as vestiges of the races which had inhabited the country, and give his time to their examination and measurement.

During the two years which he passed in the office of his uncle at Ludlow's station near Cincinnati, he was a companion of several young men who subsequently became conspicuous, among whom were Nicholas Longworth, Samuel Petry, Daniel Duke, Thomas Pierce, and Peyton Symmes, all of whom are now dead. His tastes, however, led him back to the army, (from which he had resigned shortly after his promotion,) and, February 23, 1808, he was reappointed a second lieutenant of engineers, his commission bearing the same date as that of his subsequent friend, brother engineer officer, and professional associate, Sylvanus Thayer, of national fame as for so many years superintendent of the Military Academy, and as the officer to whom is mainly due its present high grade among the military and scientific institutions of the world. Lieutenant Totten commenced his career as a military engineer under Colonel Jonathan Williams, the first chief of the corps, and was engaged on the construction of Castles Williams and Clinton, New York harbor.

At the commencement of the war with England Lieutenant Totten was assigned to duty as chief engineer of the army under Brigadier General Van Rensselaer, in the campaign of 1812, on the Niagara frontier, and in that capacity took a conspicuous part in the battle of Queenstown. He was subsequently chief engineer of the army under the command of Major General Dearborn, in the campaign of 1813, and of the army under Major General Izard and Brigadier General Macomb, in the campaign of 1814, on Lake Champlain. Having been promoted to a captaincy in 1812, he was in June, 1813, brevetted major, for "meritorious services," and September 11, 1814, lieutenant colonel, for "gallant conduct at the battle of Plattsburg;" his efficient services as an engineer in the defensive arrangements of that field having contributed powerfully to the successful issue.

The termination of the war may be considered as the close of one period in the life and services of General Totten, and the commencement of another; or rather it may be said, that the events of which we have traced a faint outline were but the preparation and training of his mind for the real work of his life. Reared under the eyes and guardianship of a relative distinguished for his mathematical attainments, receiving as extensive a military and scientific education as West Point at that early day could give, called by his position in Surveyor General Mansfield's office, not only to exercise the science which the duties involved, but to take extended views of our country as to the interconnection of its parts, and their relations to commerce or war, then practically taught the duties of a military engineer in what concerns the defence of harbors, and finally carried through the ordeal of actual war in the campaigns of armies in the field, he was now prepared for the great work of his life—the fortification of our seaboard frontier. When I call this the great work of his life, I am not unaware that it is but a *part* of that work—still the most important part, and one to which his other labors may be considered incidental.

A brief reference to the condition and progress of sea-coast defence at that period is here appropriate. Previous to the Revolution, our seaport towns had not grown into large cities, nor were there great naval establishments or military depots to invite the enterprises of an enemy. During that contest, the harbors of Boston, New York, Philadelphia, Charleston, &c., had been, to a certain extent, "fortified" against naval attack by slight earthen batteries, or in some few cases by small and (as we would now call them) insignificant earthen forts. A work of palmetto logs and sand on Sullivan's island, Charleston harbor, mounting but 30 guns, decisively repulsed, early in the revolutionary war, the attack of the British fleet under Sir Peter Parker, consisting of two frigates and six sloops-of-war, carrying about 270 guns, destroying four of the smaller vessels, and inflicting a loss of 205 in killed and wounded (eleven times as many per gun employed against them as the English lost at Trafalgar;) thus decisively demonstrating the value of fortifications, and the superiority of land batteries to ships. But with an immense sea-coast line and sparse population, it was impossible to hold our seaports against the great naval power of the mother country, and the war of the Revolution was mainly a contest of land forces. After the attainment of our independence, the importance of fortifying our harbors impressed itself on the mind of General Washington, and the political agitations which grew out of the French Revolution, and which threatened to involve the new-born Power of the West, prompted early action in this direction. In that day war, though a science, had not grown into one which makes tributary to it all other sciences, as it has since done. Fortification, indeed, had reached a high degree of perfection, but the elaborate treatises on that subject scarcely touched the subject of harbor defence, so little art was apparently supposed to be involved in throwing up batteries to defend the entrances of ports. The art of a Vauban and Cormontaigne was little concerned in the war from which we had just emerged, and the circumstances were too dissimilar, the theatre too large and too thinly populated, the armies engaged too small, to afford to the precepts of a Lloyd or a Templehoff much apparent applicability. While the war developed generals of unquestionable ability in the spheres in which they acted, it seemed to be conceded, that for military science, and especially for the art of fortification, we must look to Europe. Hence we find so many of the early harbor defences of our principal seaport towns to have been built under the direction of foreign officers who had found employment among us, and who did not always possess the knowledge of the art to which they laid claim.

The importance of a Military Academy for the training of officers for the military service, and especially for the engineers and artillery, had been acknowledged even from the very outset of the struggle for independence. We find even the Continental Congress appointing a committee "to prepare and bring in a plan of a Military Academy," and the first Secretary of War, General Knox, in an official report to the President, discusses the subject at much length. The establishment of such an institution is known to have been a favorite object of General Washington, and in his annual message in 1793 he suggests the inquiry, "whether a material feature in the improvement" of the system of military defence "ought not to afford an opportunity for the study of those branches of the art which can scarcely ever be attained by practice alone;" and in 1796 he states that "the desirableness of this institution had constantly increased with every new view he had taken of the subject."

An act of Congress of 1794 had provided for a corps of artillerists and engineers, to consist of four battalions, to each of which eight cadets were to be attached, and made it the duty of the Secretary of War to procure books, instruments and apparatus for the benefit of said corps; and in 1798 Congress authorized the raising of an additional regiment, increased the number of cadets to fifty-six, and empowered the President to appoint four teachers of the arts and sciences necessary to the efficiency of this "corps." Of the four teachers, none were appointed prior to January, 1801, at which time Mr. George Barron was appointed teacher of mathematics, and the institution, "which was nothing more than a mathematical school for the few cadets then in the service," was nominally established.

It was soon discovered that the regiment of artillerists and engineers could not combine with effect the two duties assigned to its members, and a law was therefore framed separating them into two corps, and declaring that the corps of engineers should be stationed at West Point, New York, and should constitute a Military Academy. This act of March 16, 1802, which is the organic law of the corps of engineers and of the Military Academy, provided for the appointment of a certain number of officers and cadets,* (not to exceed twenty in all,) and declared that "the principal engineer, or, in his absence, the next in rank, shall have the superintendence of the Military Academy, under the direction of the President of the United States."

It is not my purpose here to follow further the history of that institution; I have alluded to its initiation as a step taken to provide for an acknowledged want of the period—an institution for teaching the military sciences to young men entering the army, and for creating a competent *corps of engineers.* It was soon found, however, that the duties of engineer officers were inconsistent with their remaining at West Point, and themselves constituting "a Military Academy." Most of them were soon called to duties along the seaboard, in constructing our fortifications, while, as the wants of the service and of the Academy have been more clearly seen, the number of cadets has been increased, to supply not only the engineers and artillery, but officers of all arms of the service, and the various professorships and departments of instruction now existing have been established.

As the duties of the corps became more and more extensive, its chief, though charged with the administration of its affairs, could not be constantly present at the Academy, and it ultimately became apparent that the immediate superintendency of such an institution was incompatible with his proper functions. In 1817, an officer selected from the corps (Brevet Major Sylvanus Thayer, to whom allusion has already been made) was appointed permanent superintendent of the Academy, and made subject only to the orders of the President of the United States.

Major (afterwards Colonel) Jonathan Williams, a near relative of Dr. Franklin, whom he accompanied, as secretary, to France, where he studied the military sciences, and made himself acquainted with the standard works on fortification, was the first chief engineer of the United States under the law of 1802. He was an officer of decided merit, much beloved by his subordinates, and is justly styled the father of the corps of engineers and of the Military Academy.

While exercising his superintendence of the Academy, he devoted himself personally to the fortification of New York harbor, and most of the forts which constitute the inner line of defence of that harbor—Fort Columbus, Castles Williams and Clinton, (Castle Garden,) and a work similar to the last named, located two or three miles higher up the river (Fort Gansevoort)—were planned by him, and built under his immediate supervision.

Castle Williams was the first "casemated" battery erected in this country, (built in 1807–10,) and was planned after the system of Montalembert, with which, as we have seen, Colonel Williams had made himself acquainted in France. This and other works of Colonel Williams, though they have been superficially and ignorantly criticised, were really meritorious, and do not suffer by comparison with European structures of the same or even much more recent dates.

The indications of an approaching war with England, and the obvious inadequacy of existing fortifications, had led to renewed exertions, and prompted the works just mentioned and others at all our seaports, so that when the war broke out there was not a town of any magnitude upon the coast not provided with one or more batteries. But most of the works so thrown up before the subject had been studied and systematized as a whole were defective in design, small, weak, and, being built, for present economy, of cheap materials and workmanship, very perishable. In the main, however, they answered their purpose—more, perhaps, through an undue respect for them on the part of our foe than

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* Besides ten cadets of engineers, forty cadets "of artillery" were authorized by this law; making fifty cadets in all.

through their intrinsic strength. It was not till after the close of the war with England that a permanent system of coast defence was entered upon by our government. Indeed, without the experience of that war it is doubtful whether a measure, always so unpopular and generally so little understood as a national system of fortifications, could have gained the support of Congress and of the people. A "board of engineers" was constituted in 1816, with instructions to make examinations of the sea-coast, and to prepare plans for defensive works, subject to the revision of the chief engineer and the sanction of the Secretary of War.

Up to this period the Military Academy had maintained a sort of embryo existence, without definite form or a prescribed system. The annual term of study lasted from April to November, all the intermediate months being vacation. No fixed number of terms was necessary to graduation, nor was it prescribed what should be studied. Some cadets remained but a single term before being commissioned; others, several years. Although this period produced officers who afterwards became highly distinguished in engineering, (as well as in other branches of military art,) it is not surprising that the government yet entertained the common notion that only in Europe, and especially in France, could high military science be found; nor that, in undertaking so vast and costly a work as the fortification of our sea-coast, distrust should have been felt in the unaided abilities of our own engineer officers. A distinguished French engineer, General Simon Bernard, was invited to this country, and, as "assistant" in the corps of engineers, (an office created for the purpose by Congress,) made a member of the board which, as first constituted, November 16, 1816, consisted of himself as president, Colonel William McRee, and Lieutenant Colonel J. G. Totten. In 1817 Colonel Totten was relieved, and appears to have been stationed at Rouse's Point, Lake Champlain, in charge of fortifications at that place, and the board to have been composed of Brigadier General J. G. Swift, Chief Engineer, Brigadier General Bernard, and Colonel McRee; but Colonel Totten was again made a member in 1819, and (both General Swift and Colonel McRee having resigned) the permanent board came to consist of Bernard and Totten alone, and the labor of working out the fundamental principles of the system, and of elaborating the projects of defence for the great seaports, thus devolved mainly upon these two officers, though naval officers of rank and experience were associated with them whenever their examinations included positions for dock-yards, naval depots, or other objects which concerned the naval service.

Though the advent of a foreign officer, and his assignment to this duty, under the anomalous designation of "assistant" in the corps of engineers, naturally caused some feeling, yet it can scarcely be doubted that the influence of the proceeding was beneficial. If in Swift, McRee, Totten, Thayer, and many others, were found high engineering abilities and acquirements, it is no less true that professional association with such a man as Bernard was calculated to stimulate to higher attainments and more zealous exertion. The spirit of emulation alone would induce our own officers to prove to the country that they were not inferior to others. To high military and scientific acquirements and great experience in his professional duties, General Bernard united to the qualities of an amiable and accomplished gentleman the tact to adapt himself to his peculiar position without wounding the pride of those with whom he was thus associated. The prestige of his name aided powerfully in sustaining, with the administration and with Congress, the measures which the board found necessary to recommend, and in establishing firmly, as a part of our national policy, the system of sea-coast defence by fortifications. In recounting the origin and growth of the system, it is but just to give that name an honorable mention.

By the board of engineers of which I have been speaking a series of reports was drawn up, which, mostly from the pen of our departed associate, form his best memorial, and exhibit in a masterly manner the principles of sea-coast and harbor defence, and their application to our own country. In a paper of this kind it will not be out of place to give some idea, at least, of the arguments and views contained in these documents. An elaborate report of 1826, from which I quote, gives a general *résumé* of the principles which have guided the labors of the board, and of the results arrived at:

" The means of defence for the seaboard of the United States, constituting a system, may be classed as follows : First, a navy ; second, fortifications ; third, interior communications by land and water; and fourth, a regular army and well-organized militia.

" *The navy* must be provided with suitable establishments for construction and repair, stations, harbors of rendezvous, and ports of refuge, all secured by fortifications defended by regular troops and militia, and supplied with men and materials by the lines of intercommunication. Being the only species of offensive force compatible with our domestic institutions, it will then be prepared to act the great part which its early achievements have promised, and to which its high destiny will lead.

" *Fortifications* must close all important harbors against an enemy, and secure them to our military and commercial marine; second, must deprive an enemy of all strong positions where, protected by naval superiority, he might fix permanent quarters in our territory, maintain himself during the war, and keep the whole frontier in perpetual alarm; third, must cover the great cities from attack; fourth, must prevent as far as practicable the great avenues of interior navigation from being blockaded at their entrances into the ocean; fifth, must cover the coastwise and interior navigation by closing the harbors and the several inlets from the sea which intersect the lines of communication, and thereby further aid the navy in protecting the navigation of the country ; and sixth, must protect the great naval establishments.

" *Interior communications* will conduct with certainty the necessary supplies of all sorts to the stations, harbors of refuge, and rendezvous, and the establishments for construction and repair, for the use both of the fortifications and the navy ; will greatly facilitate and expedite the concentration of military force and the transfer of troops from one point to another; insure to these also unfailing supplies of every description, and will preserve unimpaired the interchange of domestic commerce even during periods of the most active external warfare.

" *The army and militia,* together with the marine, constitute the vital principle of the system.

"From this sketch it is apparent that our system of defence is composed of elements whose numerous reciprocal relations with each other and with the whole constitute its excellence ; one element is scarcely more dependent than the whole system is on any one. Withdraw the navy, and the defence becomes merely passive; withdraw interior communications from the system, and the navy must cease in a measure to be active for want of supplies, and the fortifications can offer but a feeble resistance for want of timely re-enforcements ; withdraw fortifications, and there only remains a scattered and naked navy. "

The relation of the navy to fortifications is one of those subjects not always well appreciated, and hence the cause of mischievous notions and much misrepresentation. No pains is spared in these reports to make this subject clearly understood. After the quotation just given, Colonel Totten remarks :

" It is necessary to observe, in the first place, that the relation of fortifications to the navy in a defensive system is that of a sheltering, succoring power, while the relation of the latter to the former is that of an active and powerful auxiliary ; and that the latter ceases to be efficient as a member of the system the moment it becomes passive, and should in no case (we allude to the navy proper) be relied on as a substitute for fortifications. This position may be easily established.

"If our navy be inferior to that of the enemy, it can afford, of course, unaided by fortifications, but a feeble resistance, single ships being assailed by whole fleets; if it be equal, or superior, having numerous points along an extended frontier to protect, and being unable to concentrate, because ignorant of the selected point of attack, every point must be simultaneously guarded: our separate squadrons may therefore be captured in detail by the concentrated fleet of the attacking power. If we attempt to concentrate under an idea that a favorite object of the enemy is foreseen, he will not fail to push his forces upon the places thus left without protection. This mode of defence is liable to the further objections of being exposed to fatal disasters, although not engaged with an enemy, and of leaving the issue of conflict often to be determined by accident, in spite of all the efforts of courage and skill. If it were attempted to improve upon this mode by adding temporary batteries and field works, it would be found that, besides being weak and inadequate from their nature, the most suitable positions for these works must often be neglected, under a necessary condition of the plan, that the ships themselves be defended; otherwise, they must either take no part in the contest, or be destroyed by the superior adversary."

It is hardly to be expected that a system affording so much room for discussion, and by its importance inviting it, should, especially in this country, escape adverse judgment. Military and naval men, congressmen, and even cabinet officers, have assailed it, called in question the principles on which it is based, or denied the judiciousness of their application. The forms and sources of assault have been varied, but there has been really no great difference in the substance, of which, perhaps, as good an expression as any may be found in these dogmas; forming the pith of a criticism from no less a source than the Secretary of War, Mr. Cass, approved by the President, General Jackson:

"1st. That for the defence of the coast, the chief reliance should be on the navy;

"2d. That in preference to fortifications, floating batteries should be introduced wherever they can be used;

"3d. That we are not in danger of large expeditions; and, consequently,

"4th. That the system of the board of engineers comprises works which are unnecessarily large for the purposes which they have to fulfil."

Owing to these strictures, the House of Representatives, by resolutions of April 9, 1840, called upon the War Department for a report of a full and connected system of national defence. The duty was committed by the Secretary of War to a board of officers of the army and navy, among whom was Colonel Totten, and by whom the report was drawn up. It was entirely approved by the Secretary of War, Mr. Poinsett, and is universally admitted to be one of the most able and comprehensive expositions of the whole subject of sea-coast defence extant, and a complete refutation of the objections made to our existing system. The discussion of the first and principal proposition—that of defence by the navy—is so interesting and instructive that, though long, I venture to quote it:

"The opinion that the navy is the true defence of the country is so acceptable and popular, and is sustained by such high authority, that it demands a careful examination.

"Before going into this examination, we will premise that by the term 'navy' is here meant, we suppose, line-of-battle ships, frigates, smaller sailing vessels, and armed steamships, omitting vessels constructed for local uses merely, such as floating batteries.

"For the purpose of first considering this proposition in its simplest terms, we will begin by supposing the nation to possess but a single seaport, and that this is to be defended by a fleet alone.

"By remaining constantly within this port, our fleet would be certain of meeting the enemy, should he assail it. But if inferior to the enemy, there would be no reason to look for a successful defence; and as there would be no escape for the defeated vessels, the presence of the fleet, instead of averting the issue, would only render it the more calamitous.

"Should our fleet be equal to the enemy's, the defence might be complete, and it probably would be so. Still, hazard, some of the many mishaps liable to attend contests of this nature, might decide against us; and in that event, the consequences would be even more disastrous than on the preceding supposition. In this case the chances of victory to the two parties would be equal, but the consequences very unequal. It might be the enemy's fate to lose his whole fleet. but he could lose nothing more; while we in a similar attempt would lose not only the whole fleet, but also the object that the fleet was designed to protect.

"If superior to the enemy, the defence of the port would in all respects be complete. But instead of making an attack, the enemy would, in such case, employ himself in cutting up our commerce on the ocean; and nothing could be done to protect this commerce without leaving the port in a condition to be successfully assailed.

"In either of the above cases the fleet might await the enemy in front of the harbor, instead of lying within. But no advantage is apparent from such arrangement, and there would be superadded the risk of being injured by tempests, and thereby being disqualified for the duty of defence, or of being driven off the coast by gales of wind, thus for a time removing all opposition.

"In the same cases also, especially when equal or superior to the enemy, our fleet, depending on having correct and timely notice as to the position and state of preparation of the enemy's forces, might think proper to meet him at the outlet of his own port, or intercept him on his way, instead of awaiting him within or off our own harbor. Here it must be noticed that the enemy, like ourselves, is supposed to possess a single harbor only; but, having protected it by other means, that his navy is disposable for offensive operations. If it were attempted thus to shut him within his own port, he, in any case but that of decided inferiority, would not hesitate to come out and risk a battle; because, if defeated, he could retire under shelter of his defences to refit; and if successful, he could proceed with a small portion of his force—even a single vessel would suffice—to the capture of our port now defenceless, while, with the remainder, he would follow up his advantage over our defeated vessels, not failing to pursue into their harbor should they return thither.

"Actual superiority on our part would keep the enemy from volunteering a battle; but it would be indispensable that the superiority be steadily maintained, and that the superior fleet be constantly present. If driven off by tempests, or absent from any other cause, the blockaded fleet would escape, when it would be necessary for our fleet to fly back to the defence of its own port. Experience abundantly proves, moreover, that it is in vain to attempt to shut a hostile squadron in port for any length of time. It seems, then, that whether we defend by remaining at home, or by shutting the enemy's fleet within his own harbor, actual superiority in vessels is indispensable to the security of our own port.

"With this superiority the defence will be complete, provided our fleet remains within its harbor. But then all the commerce of the country upon the ocean must be left to its fate; and no attempt can be made to react offensively against the foe, unless we can control the chances of finding the enemy's fleet within his port, and the still more uncertain chance of keeping him there; the escape of a single vessel being sufficient to cause the loss of our harbor. Let us next see what will be the state of the question on the supposition of numerous important ports on either side, instead of a single one, relying on our part still exclusively on a navy.

"In order to examine this question, we will suppose our adversary to be fortified in all his harbors, and possessed of available naval means, equal to our own. This is certainly a fair supposition; because what is assumed as regards his harbors is true of all maritime nations, except the United States; and as regards naval means, it is elevating our own strength considerably above its present measure, and above that it is likely to attain for years.

"Being thus relatively situated, the first difference that strikes us is, that the enemy, believing all his ports to be safe without the presence of his vessels, sets himself at once about making our seas and shores the theatre of operations, while we are left without choice in the matter; for if he thinks proper to come, and we are not present, he attains his object without resistance.

"The next difference is, that while the enemy (saving only the opposition of Providence) is certain to fall upon the single point, or the many points he may have selected, there will exist no previous indications of his particular choice, and, consequently, no reason for preparing our defence on one point rather than another; so that the chances of not being present and ready on his arrival are directly in proportion to the number of our ports—that is to say, the greater the number of ports, the greater the number of chances that he will meet no opposition whatever.

"Another difference is, that the enemy can choose the mode of warfare as well as the plan of operations, leaving as little option to us in the one case as in the other. It will be necessary for us to act, in the first instance, on the supposition that an assault will be made with his entire fleet; because, should we act otherwise, his coming in that array would involve both fleet and coast in inevitable defeat and ruin. Being in this state of concentration, then, should the enemy have any apprehensions about the result of a general engagement, should he be unwilling to put anything at hazard, or should he, for any other reason, prefer acting by detachments, he can, on approaching the coast, disperse his force into small squadrons and single ships, and make simultaneous attacks on numerous points. These enterprises would be speedily consummated, because, as the single point occupied by our fleet would be avoided, all the detachments would be unopposed; and after a few hours devoted to burning shipping, or public establishments, and taking in spoil, the several expeditions would leave the coast for some convenient rendezvous, whence they might return, either in fleet or in detachments, to visit other portions with the scourge.

"Is it insisted that our fleet might, notwithstanding, be so arranged as to meet these enterprises?

"As it cannot be denied that the enemy may select his point of attack out of the whole extent of coast, where is the prescience that can indicate the spot? And if it cannot be foretold, how is that ubiquity to be imparted that shall always place our fleet in the path of the advancing foe? Suppose we attempt to cover the coast by cruising in front of it, shall we sweep its whole length? —a distance scarcely less than that which the enemy must traverse in passing from his coast to ours. Must the Gulf of Mexico be swept as well as the Atlantic; or shall we give up the Gulf to the enemy? Shall we cover the southern cities, or give them up also? We must unquestionably do one of two things: either relinquish a great extent of coast, confining our cruisers to a small portion only, or include so much that the chances of intercepting an enemy would seem to be out of the question."

The report then goes on to discuss the uses for defensive purposes of gunboats, floating batteries and steam batteries, as distinguished from the navy proper. Admitting their usefulness, and, even in some cases, their necessity, it argues with great force that they are not a substitute for and cannot supersede fortifications, and it sums up its argument concerning naval defence with the following broad propositions, to which it challenges opposition:

"1st. If the sea-coast is to be defended by naval means exclusively, the defensive force at each point deemed worthy of protection must be at least equal *in power* to the attacking force.

"2d. As from the nature of the case there can be no reason for expecting an attack on one of these points rather than on another, and no time for transferring our state of preparation from one to another after an attack has been declared, each of them must have assigned to it the requisite means; and,

"3d. Consequently this system demands a power in the defence as many times greater than that in the attack as there are points to be covered.

"There has been but one practice among nations as to the defence of ports and harbors, and that has been a resort to fortifications. All the experience that history exhibits is on one side only; it is the opposition of forts or other works, comprehended by the term fortification, to attack by vessels, and although history affords some instances wherein this defence has not availed, we see that the resort is still the same. No nation omits covering the exposed points upon her seaboard with fortifications, nor hesitates in confiding in them."

The most prominent cases of such successful attacks, viz. Copenhagen, Algiers, San Juan de Ulloa, &c., are then described and discussed, to show that the deductions drawn from them are erroneous, or that they are not cases in point, or that the disastrous result has been owing to the neglected condition, imperfect armament, or unskilful and inadequate defence of the forts.

The report, of which I have given some of the main points, may be said to have silenced opposition to our system of fortifications for the next ten years; but, in a form modified by the alleged changes in the condition of the country, increase of population, construction of railroads, &c., it again found expression in a resolution of Congress in 1851; and the Secretary of War, to enable himself to respond, called upon numerous distinguished army and navy officers for an expression of their opinions. The following questions were addressed to several of the principal engineer officers, among whom the chief of corps, General Totten:

"1st. How far the invention and extension of railroads have superseded or diminished the necessity of fortifications on the seaboard?

"2d. In what manner and to what extent the navigation of the ocean by steam, and particularly the application of steam to vessels of war, and recent improvements in artillery, and other military inventions and discoveries, affect this question?

"3d. How far vessels of war, steam batteries, ordinary merchant ships and steamers, and other temporary expedients, can be relied upon as a substitute for permanent fortifications for the defence of our seaports?

"4th. How far the increase of population on the northern frontier and of the mercantile marine on the northern lakes obviates or diminishes the necessity of continuing the system of fortifications on these lakes?"

General Totten's response to these critical interrogations is, as usual with him when this great subject has to be dealt with, full and exhaustive. The following pithy paragraphs exhibit his views on the influence of railroads:

"Suppose a hostile fleet to lie in front of the city of New York—which nothing would prevent, if the channels of approach were not fortified—in what way could the 100,000 or 200,000 new men poured into the city and environs by railroads, although armed with muskets and field-pieces, aid the half-million of people already there? It seems to me very clear that these additional forces would, like the population of the city, be utterly powerless in the way of resistance, with any means at their command, and, if resistance were attempted by the city, would but serve to swell the list of casualties, unless they should at once retreat beyond the range of fire. If the enemy's expedition were intended, according to the second supposed mode of attack, for invasion, or occupation for some time, of a portion of the country, then in many places this resource of railroads would be of value, because then the duty of defence would fall upon the army and militia of the country, and these communications would swell their numbers.

"But of all circumstances of danger to the coast, this chance of an attempt by an enemy to land and march any distance into a populous district is least to be regarded, whether there be or be not such speedy mode of receiving re-enforcements, and our system of fortifications has little to do with any such danger. In preparing against maritime assaults, the security of the points to be covered is considered to be greatly augmented whenever the defence can be so arranged as to oblige an enemy to land at some distance; for the reason that opportunity is thereby allowed, in the only possible way, for the spirit and enterprise of the people to come into play.

"Instead of being designed to prevent a landing upon any part of the coast, as many seem to suppose, and some allege in proof of extravagant views on the part of the system of defence, the system often leaves this landing as an open alternative to the enemy, and aims so to cover the really important and dangerous points as to necessitate a distant landing and a march towards the object through the people. It is because the expedition would easily accomplish its object without landing, and without allowing the population to partake in the defence, that the fortifications are resorted to. For instance, without Fort Delaware, or some other fort low down on Delaware bay, an enemy could place his fleet of steamers in front of Philadelphia by the time his appearance on the coast had been well announced throughout the city. And in spite of all New Jersey, Delaware, and lower Pennsylvania, he could levy his contributions, and burn the navy yard and shipping, and be away, in a few hours. But being obliged, by the fort above mentioned, to land full forty miles below the city, the resistance to his march may be safely left to the courage and patriotism that will find ample time to array themselves in opposition."

Concerning the application of steam to vessels of war he says:

"The application of steam to vessels of war acts upon the question of seacoast defence both beneficially and injuriously. It acts injuriously in several ways; but chiefly, first, by the suddenness and surprise with which vessels may fall upon their object, and pass from one object to another, in spite of distance, climate, and season; and, secondly, by their ability to navigate shallow waters.

"The first property, by which squadrons may run into our harbors, outstripping all warnings of their approach, affords no chance for impromptu preparations; accordingly, whatever our preparations are to be, they should precede the war. If seems past all belief that a nation having in commission—as France and England always have—a large number of war-steamers, ready for distant servicein twenty-four hours, receiving their orders by telegraph, capable of uniting in squadrons, andin two or three days at most speeding on their several paths to fall upon undefended ports—it is not to be expected, I say, that they should delay such enterprises until temporary resorts could be got ready to receive them. And yet there are those who insist that we should leave defensive measures to a state of war—that we should let the day supply the need!

"Inadequate as all such measures must prove, there would not be time to arrange even these. By the second property, due to their light draught of water, these vessels will oblige the defence to be extended in some form to passages or channels or shoals that were before adequately guarded by their shallowness. The bars at the mouth of the Mississippi formerly excluded all but small vessels-of-war, and the strong current of the river made the ascent of sailing vessels exceedingly uncertain and tedious. Now these bars and currents are impediments no longer; and all the armed steamers of Great Britain and France might be formed in array in face of the city of New Orleans before a rumor of their approach had been heard.

"Had the English expedition of 1814, attended by a squadron of armed steamers, arrived at the mouth of the Mississippi, a few transports might have been taken in tow, and in a few hours the whole army would have been before the city. Or twelve or fifteen such steamers could have carried the whole army up in half a day, without the delay of transports. Will it be contended that the attack in that form would have been repulsed with the means then in General Jackson's hands? Would the landing, or even the presence on board these steamships, of the British troops have been necessary to burn the city or put it under contribution? Is there anything now, but the existence of forts on the river, to prevent the success of such an attack by fifteen or twenty steamers of war, allured there by the vastly increased magnitude of the spoil?"*

While the enemy's means of attack are thus enhanced by the use of war steamers, General Totten contends that they cannot be relied upon, as a substitute for fortifications, for defence.

"I do not assert," he says, "that armed vessels would not be useful in coast defence. Such an idea would be absurd. I shall even have occasion to show a necessity for this kind of force, in certain exceptional cases. It is the general proposition, viz., that armed vessels, and not fortifications, are the proper defences for our vulnerable points—a proposition the more dangerous, because seemingly in such accordance with the well-tried prowess and heroic achievements of the navy, that we have now to controvert.

"Boston, New York, Philadelphia, Baltimore, Charleston, and New Orleans are, we will suppose, to be guarded, not by forts, but by these vessels, on the occurrence of a war with a nation possessing large naval means. We know that it is no effort for such nations to despatch a fleet of twenty line-of-battle ships and frigates, or an equal number of war steamers, or even the combined mass—both fleets in one.

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"What, then, shall we do at the above-named ports severally? Each is justly felt to be an object worthy of an enemy's efforts, and each would be culpable in sending elsewhere any part of the force required for its own defence. Each, therefore, maintains a naval force equal, at least, to that the enemy is judged to be able to send promptly against it. Omitting any provision for other places scarcely less important, what is the result? It is, that we maintain within the harbors of, or at the entrance to, these places, chained down to this passive defence, a force at least six times as large as that of the enemy.

"He does not hesitate to leave his port, because it will be protected in his absence by its fortifications, which also afford him a sure refuge on his return. He sails about the ocean, depredating upon our commerce with his privateers and small cruisers, putting our small places to ransom, and in other ways following up appropriate duties; all which is accomplished without risk, because our fleet, although of enormous magnitude, must cling to ports which have no other defence than that afforded by their presence. They cannot combine against him singly, for they cannot know where he is; and must not, moreover, abandon the object which they were expressly provided to guard.

"It would really seem that there could not be a more impolitic, inefficient, and dangerous system, as there could not certainly be a more expensive one."

I have thus extensively quoted from the reports of General Totten, because they are themselves the best expressions of the life labors and services of the subject of our memoir, and because I think they treat of matters which should be, in an eminent degree, interesting to the mêmbers of this National Academy, and which, moreover, should demand its attention.

To preserve the continuity of my subject, I have followed these reports down to a late date. It is necessary now to revert to an earlier period. It has

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* The experience of the rebellion has proved the truth of General Totten's words. The moment the *forts* were passed, the city of New Orleans was, notwithstanding the land forces under Lovell, at Commodore Farragut's mercy. I have alluded elsewhere to the failure of the forts.

already been observed that, as soon as the original board of engineers had sufficiently matured the general system of defence, and completed plans for the works first required, its members applied themselves to the duty of construction. In 1828 General (then Colonel) Totten took charge of the construction of Fort Adams, Newport harbor, and continued on this duty, making his residence in the town of Newport, until December, 1838, the date of his appointment as chief of the corps of engineers. This work, the second in magnitude of the fortifications of the United States, is one of the best monuments of his genius as a military engineer. From its peculiar relations to the land defence, it called for the application of most of those rules of the art and many of those special arrangements which form the themes of treatises upon "fortification," and which, generally, have but a very limited application to works of harbor defence. In these respects it has no parallel with us; and in the treatment of the case and happy adaptation of means to the end, Colonel Totten exhibited a mastery of all the details of the art, which proves his technical skill and minute knowledge to be fully equal to the power of broad generalization I have already endeavored to illustrate. But Colonel Totten found here yet another field for professional usefulness—another track to explore. The art of the civil engineer (I use the phrase in its application to mere *construction*, whether it be of a military or civil work) was yet in its infancy in this country. Our resources in building materials were almost unknown, their qualities and adaptabilities to different purposes of construction undeveloped. Thus far the matter had excited little attention; the building material, whether brick or stone, lime or timber, nearest at hand was indiscriminately used, and its aggregation left much to the skill of the mechanic. In commencing constructions on so great a scale, it was of the first importance that the work should be both durable and economical—a result only to be attained by the most careful selection of materials, and the most skilful manipulation. Besides, our forts called for arrangements unknown in other branches of building—arrangements for which the execution and the most suitable materials had to be studied out *ab initio,* since on many of these points there were neither experience nor extant rules to guide.

In the years 1830 and 1831 a series of experiments was instituted by Colonel Totten at Fort Adams, on the expansion and contraction of building stone by natural changes of temperature, and the effects of these variations on the cements employed to secure the joints of stone copings. An account of them was prepared under his direction by Lieutenant (now Professor) W. H. C. Bartlett, a member of this Academy, and published in the American Journal of Science for July, 1832. The methods employed were at once simple and ingenious, and the result was such as to leave no doubt that in this climate the joints of copings formed of stone of four or five feet in length will always be insecure, no matter what description of cement may be employed to close them.

This result is one of great practical importance. Previously to the experimental examination of the subject by Colonel Totten, the walls of our most expensive works of masonry were protected by copings cemented at their joints; and while the failure of the cement was constantly noticed, the cause of the failure was not understood. The experiments showed that the changes of longitudinal dimensions of granite coping-stones, five feet only in length, under the extreme temperatures to which they were exposed at Newport, would be sufficient to pulverize the hardest cement between them, or to leave cracks in it thicker than common pasteboard. With marble as a material, these destructive effects are considerably increased, and with sandstone, nearly doubled.

About the same time Colonel Totten caused some experiments to be made to ascertain the relative stiffness and strength of the following kinds of timber, viz: White pine, *(Pinus strobus,)* Spruce, *(Abies nigra,)* and southern pine, *(Pinus australus)*, also called long-leaved pine.

These experiments, made by his assistant, Lieutenant T. S. Brown, of the corps of engineers, were published in the American Journal of Science and Art, and afterwards, having been revised by the author, in the Journal of the Franklin Institute, a note being added, the calculations extended, and practical inferences drawn therefrom. This memoir and additions are found in vol. vii, new series, Journal of the Franklin Institute, 1831. Lieutenant Brown's account concludes with the following remarks:

"In Tredgold's Carpentry, and other similar works, may be found the constant numbers (a) and (c) for nearly all the kinds of wood useful in the arts; but besides that the numbers are in many instances calculated from insufficient experiments, most of the specimens used in the trials were of European growth, and of course the results obtained are inapplicable to American timber, though bearing the same name. It is much to be desired that numerous and accurate experiments be made in this country by those having the requisite zeal and opportunities; our architects will then know with certainty the qualities of the different kinds of woods they are using, and instead of working at hazard and in the dark, as they now too often do, they will be guided by the sure light of practical science to certain and definite results. If these experiments contribute ever so little to the attainment of so important a result, the object of their publication will be fully accomplished."

A subject of such vital importance in the art of construction as the composition of mortars could not fail to invite, or rather compel, the researches of Colonel Totten. No species of masonry is subject to such severe deteriorating influences as the walls and arches of fortifications, especially in our climate; so severe, indeed, that they almost drive the engineer to despair. Next only to the importance of having the building stones or bricks of a suitable character, is that of uniting them by a strong and durable mortar. Few persons whose attention has not been called to the subject conceive its magnitude, the variety of materials it embraces, and the laborious investigations to which it has given rise. Colonel Totten commenced his researches at an early date, and continued them actively during the whole period of his connexion with Fort Adams.

His work on "Hydraulic and Common Mortars" was published in 1838 by the Franklin Institute of Philadelphia. It contains, besides original experiments and observations on mortars, hydraulic cements and concretes, translations of essays by Treussart, Pitot, and Courtois, the best French writers on the same subject, and constitutes to this day an authority relied on by American engineers. Colonel Totten's experiments extend over the period from 1825 to 1838; they are especially valuable for the variety of limes and cements, and the tests of different modes of slacking the lime, mixing the mortars, and preparing the cements and concretes. The mortars were tested, after periods ranging from five months to four years and five months, for tenacity, by the force required to separate two bricks joined together by means of them, and for hardness by the weight which they would support, applied over a small circular area. The experiments on concretes or factitious stones are equally comprehensive, being directed to the composition and consistency of the cement, whether best used as a stiff mortar or a semi-fluid grout; to the effect of additions of common lime and sand or rounded pebbles and gravel, and to ascertaining the proportion of each that would be used to the best advantage. The results developed by these investigations are of the greatest value, and having been applied in the construction of the fort, have now had the test of many year's experience.

It would be almost impossible to enumerate the various objects of Colonel Totten's researches while at Newport. There is scarce a subject connected with the art or science of the engineer, civil or military, which did not engage his attention, and of which he has not left some record. The thickness of sustaining walls, the thrust of arches, among the more important, and the composition of stuccoes, of paints, lackers, washes for stone or brick work, among the less so, may here be mentioned.

Perhaps no period of his life is so interesting and so affectionately remembered by his professional associates. Indeed, a large proportion of the young officers of the corps of those days passed a portion of their time under his command, and acquired their first professional experience in the performance of duties under his eye and direction. The disposition to cultivate science, physical and natural, led him to original researches, while his influence stimulated and led to improvement the educated young men who from time to time came into his military family. Fond of exercise, bodily and mental, he sought in natural history, as in geology, mineralogy, and conchology, objects for the long walks and drives conducive to health, while the arrangement of the specimens, their care and classification, and the study of the habits of the animals which occupied the shells, gave scope to his wonderful powers of observation. Instead of finding his young officers a trouble, he was fond of their companionship, suggesting modes and objects of experiment, and encouraging them to do so likewise, thus cultivating originality of thought. His laboratory was at their service, and his companionship and example at their disposal. After a day's labor he retired to this laboratory, glad to have with him such of the young companions of the day as desired to join him. The honored president of this Academy can recollect, year after year, the computations, under Colonel Totten's direction, of the thickness of revetments, the analysis of minerals collected in the field, classifications of shells gathered in days' walks on the seashore, discussions of the curious structure of geological specimens in the neighborhood of Newport, and of the curious mineralogical specimens of the upper portion of Rhode Island, which he encouraged them to find. So upon the fort itself, the various researches which I have described were marked out for successive experimenting, with a generosity to his assistants which almost persuaded them that they were original with them. The determination of the measures used in laying out the fort, and the practical apparatus employed in the measurements, received his careful study. The practical character of these works impressed themselves upon the minds of the young officers, and furnished the fitting complement to the theoretical training received at West Point.

Not least pleasant among the memories of this period of Colonel Totten's life, to those who had the good fortune to be associated with him, is the recollection of the social enjoyments of his house. Married in 1816 to Catlyna Pearson, of Albany, he was surrounded by a young family, among whom his happiest moments were spent, and to whom he was everything that such a relation can imply. None could be happier in his social intercourse. Genial and eminently hospitable, he cultivated as a duty those smaller amenities of society by which the cares of life are lightened and its joys augmented. His house was the home of his friends, and was seldom without some one of them. Though dignified and courteously reserved in his intercourse with the external world, few more highly enjoyed real humor, or could with more true *bonhommie* give themselves up to the gayety of the moment. In his relations to his young officers he was kind and affable, encouraging freedom of expression, and inviting inquiry in everything that related to professional matters, while there was always that in his manner which inspired the most profound respect and forbade undue levity of conduct in his presence.

Before quitting the scene of so important a portion of Colonel Totten's official labors, it is proper to remark that, in addition to the duties of his particular charge, he, as a member and for the last six years president of the board of engineers, was engaged in the planning of the new works for which Congress from time to time made the necessary appropriations.* To this duty he usually devoted the winter months, during which all construction on Fort Adams was suspended. In the execution of his designs he was usually assisted by young officers of the corps, who found therein a practical application of the theoretical knowledge acquired at West Point instructive and useful.

The works of harbor improvement on the seaboard and on the lakes were likewise under the control and direction of the Engineer Bureau; and Colonel Totten, though not directly engaged therein, was not infrequently called on to inspect and advise concerning them. Most of these, and especially those of the lake shores, afforded curious and interesting problems in this branch of civil engineering, and his reports and notes on these subjects, yet extant, are additional proofs of the wide range of his professional knowledge and of his powers of accurate observation and of skilful deduction from the phenomena of nature.

Colonel Totten was appointed colonel of the corps of engineers and Chief Engineer December 7, 1838. At this time the construction of Fort Adams was so far advanced towards completion as to need no longer his personal supervision, and the city of Washington became thenceforth his home and the seat of his official duties. Identified, as we have seen, with the origin and growth of the great system of sea-coast defence of the United States, it was eminently proper that he should become the head of that bureau of the War Department to which its execution was committed, and no one could be more eminently fitted for that important station.

At the date of his appointment the system of coast defence had been for about twenty years in progress of construction, and during that period most of those ports and harbors of the United States deemed most important to ourselves or most assailable by a naval foe had been, at least, partially fortified. At many such points, indeed, no new work had been as yet constructed, owing to the existence of forts or batteries more or less adequate built before or during the war of 1812. These works, where possible, were absorbed into the new system with some repairs and alterations. Among such points may be mentioned the harbors of Portland, Portsmouth, New London, Philadelphia, Baltimore, and Charleston. New and powerful works had, however, been built or far advanced to completion, for the defence of Boston, Newport, New York, Hampton Roads, the Savannah river, Pensacola, Mobile, and New Orleans. But the strictures on the system, to which we have before made reference, proceeding from such an authority as the Secretary of War and sanctioned by the President, had not failed to shake the confidence of Congress and of the people. For several years the annual appropriations had been wholly denied or made so inadequately that the work had languished and at some points had been wholly suspended. But however much opposition may grow up in time of profound peace, no sooner is there a probability of seeing a foe at our doors than all eyes are turned to these protecting works, and the most urgent demands are made that our seaport towns shall be speedily put "in a state of defence." Such an impulse was given by the Maine boundary and McLeod questions, soon after the advent of Colonel Totten to the Chief Engineership. In fulfilling the urgent duty which thus devolved upon him, he did not content himself with the mere issuing of orders from his office at Washington. He made it his business to inspect personally the works, and in less than two years, besides

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* By the Regulations, the local engineer officer, upon whom the construction of the proposed work was to devolve, was *ex officio* a member of the board. This brought together during the winter months engineer officers from various parts of the country—from the shores of the Gulf, from the seaboard of North and South Carolina and Georgia, as well as from nearer points, and added not a little to the charm of the professional and social life of the young engineer officers at Newport.

the enormous office labor he found necessary to attend to on the first assumption of charge of the bureau, he had visited every fort and battery on the seacoast of the United States. His inspections were not superficial and hasty; they were most thorough and searching. His investigations embraced, at the same time, the general scope and purpose of the work, its adaptability to its great objects, and the minutest detail in its construction. It was now that the country derived the full benefit of his indefatigable researches while at Newport.

I have already alluded to the lack of knowledge and experience in this country of the art of construction, especially in its applications to the peculiarities of fortification. To supply this lack was a great end of Colonel Totten's labors at Fort Adams. At few other points did the locality or circumstances of the construction render practicable such researches. This remark will apply particularly to the works on the Gulf of Mexico. The regions bordering the Gulf were, at the close of the war of 1812, but recent acquisitions to the territory of the United States. Sparsely populated and isolated from the rest of the Union as (before the application of steam to the navigation of the Mississippi) they were, they would be defended, if defended at all, only by the aid of fortifications. The fact that New Orleans had been almost wrenched from our grasp, and the impression then everywhere felt that if it had been captured it would not have been relinquished, stimulated the government to secure the possession of this important place and of other strategic points on the Gulf by immediate fortification. Accordingly, designs for works—mostly prepared by General Bernard—were among the first labors of the board of engineers, and the forts on the river and lake approaches to New Orleans, at the entrances to Mobile bay and Pensacola harbor, were almost simultaneously commenced. Around New Orleans especially the engineers had to contend with formidable difficulties. The deadly climate, the treacherous soil, on which no art could build a structure so massive as a fortification that should not sink one or more feet, warping and dislocating the walls and arches, the difficulties of procuring the services of mechanics and laborers, the want of building materials, &c., all combined to make construction exceedingly difficult, to forbid any of its niceties, and to hinder all research or experiment. Some of these works had been entirely finished at the period we have arrived at, others nearly so, and left to "séttle" before the weight of the earthen parapets was added.

Considering all these unfavorable circumstances, these works had been built in a manner creditable to the energy and skill of the engineers; but a few years' neglect, aided by a damp and tropical climate, had given many of them an appearance which, to the superficial observer, promised anything but efficiency. Indeed, it was a popular belief in New Orleans at this time that Fort Jackson, on the Mississippi, had sunk so much that its guns could not be brought to bear on the river—a belief doubtless due to the unnecessarily highness of the levees by which it had been surrounded to protect its site from inundation, and to the rapid growth of vegetation on and about the fort. Such was the condition of this work when Colonel Totten first visited it in 1841, and the author of this paper, who had but recently taken charge of it, has yet a vivid recollection of the thorough inspections of this and other works, the tedious voyages in open boats through the intricate "bayou" navigation about New Orleans, in company with his chief, as well as the copious and most minute instructions which he received. Destitute of American experience on such points, the designer had followed European precedents, or the constructing engineer had been left to his own devices as to much that relates to the interior arrangements. The wood-work of magazines, inadequately ventilated, had rotted and fallen in ruins; the covering of the bomb-proof casemates, imperfectly understood, had failed to exclude water, which percolated through the piers and arches, or gathered in muddy pools on the floors. The work to be done to bring the forts to speedy efficiency was vast; embrasures and floors of casemates were to be raised to compensate the settlement the work had undergone; earth to be removed from the arches, in order to repair or renew the roofing; magazines and quarters to be refitted, and all this before a gun could be mounted in a proper manner. On all these points Colonel Totten was rich in the experience of his long researches, and ready at once to give the proper directions. Following his detailed instructions, the works speedily reached such a condition of efficiency as to permit the mounting and service of their guns.*

What the writer here relates from his own experience at New Orleans serves but to illustrate the indefatigable labors and personal agency of Colonel Totten at this period, along the whole seaboard of the United States, in bringing all its ports and harbors into a defensible condition. Nor should I confine these attributes to any particular period. During the whole time of his chief engineership he continued the same laborious supervision. Generally once in about every two years he inspected every fort of the United States, and scarcely was the local engineer officer more thoroughly familiar with each detail of his own particular works than was the Chief Engineer with those of all under charge of the Engineer bureau. Besides attending to the routine duties of his office at Washington, he found time to design plans for new works, as well as for alterations or enlargement of old ones. An admirable draughtsman, executing his work with a delicacy and finish that defied competition on the part of his subordinates, he would be usually found, if visited at his office, engaged at his drawing-table. Indeed, if he had a fault as Chief Engineer, it was the habit of doing everything himself. It was contemplated by the Regulations lations that all plans of fortifications should be made by a board of engineers, and General Totten, in one of his reports, alludes to the fact that this has *not* always been the case, in these words: "In rare cases it has happened that plans have been made under the particular direction of the Chief Engineer, owing to the difficulty, at moments, of drawing the widely dispersed members of the board from their individual trusts." It may be said, too, in justice to him, that when he assumed control of the bureau, it was almost indispensable to take much upon himself, in the direction of the repairs and prosecution of many of the works, owing to the great pressure thrown upon the corps by the circumstances of the period, and the want of a sufficient number of experienced officers.

The excitement produced by the anticipation of war with England was followed by an actual war with a weak neighbor, a war inaugurated by the same influences which, in a more potent form, produced the rebellion, or rather of which the rebellion was but the legitimate and natural sequel. Called on by General Scott, who reposed in his professional skill the most unbounded confidence, Colonel Totten assumed, in 1847, the immediate control of the engineering operations of the army destined to invade the Mexican capital, directing in this capacity the siege of Vera Cruz. For his successful services he was breveted a brigadier general, March 29, 1847, "for gallant and meritorious conduct at the siege of Vera Cruz." Having thus successfully accomplished the special task for which he had been selected, he left the army and resumed his station at Washington.

In addition to the onerous duties of his office, involving, besides the labors described, the inspectorship and supervision of the Military Academy, his position and high reputation subjected him to calls for incidental labors by the government, by the States, or by municipal bodies. A few months prior to his

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* When Forts Jackson and Philip, on the Mississippi, were attacked by the fleets of Commanders Farragut and Porter, they were not provided with the armaments intended for them, and the garrisons were demoralized by a long bombardment. It is not in place to discuss this subject here.

appointment as Chief Engineer, 1838, he was, at the invitation of the Secretary of the Navy, ordered to visit the navy yard at Pensacola, and to prepare plans for dry-docks, wharves, sea-walls, and other improvements. Save a wretched failure in the shape of a wharf, the place—a navy yard in name—had been, up to this period, destitute of everything that characterizes such an establishment, except an imposing row of officers' quarters, and some few storehouses. A board of naval officers had been convened two years previously to consider the wants of the yard, and had recommended an extensive system of improvements, involving, among other things, no less than four dry-docks. Such constructions, reaching thirty or more feet below the level of low water in the loose sand of the bay shores, were difficult, demanding all the resources of the engineer, and it was on account of General Totten's eminent abilities and high authority in such matters that the Navy Department had recourse to his services. He made a report on the manner of construction, with plans which, if I mistake not, have been a guide in the subsequent operations. Unfortunately, to this day no permanent dry-dock exists, a floating wooden one having, through some influence, been substituted, at enormous expense, for the intended masonry structure.*

The legislature of the State of New York having, March 30, 1855, passed "An act for the appointment of a commission for the preservation of the harbor of New York from encroachments, and to prevent obstructions to the necessary navigation thereof," the commission so appointed invited and obtained the co-operation, as an "advisory council," of General Totten, Professor Bache, and Commander Davis, United States navy. The nature of the services thus rendered is best understood by reference to the reports of the commissioners themselves:

"The distinguished reputation of General Totten, Professor Bache, and Commander Davis for scientific attainments, their diversified experience in the construction of hydraulic works, and long observation of the influence of tidal currents in the formation and removal of shoals, indicated them as the best qualified to assist the commissioners in the discharge of their duties, while their high personal character precluded the possibility of their advice being affected by other than the single purpose of arriving at a just decision on the questions submitted to them." And again, after a particular allusion to the services of Professor Bache: "It is the gratifying duty of the commissioners to present to the notice of the legislature the important services which have been gratuitously rendered to the State by General Joseph G. Totten, chief engineer of the United States army, and Commander Charles H. Davis, of the United States navy, who, with Professor Bache, formed the advisory council of the commissioners. Animated by the single desire of preserving the port of New York in all its usefulness, they brought to the consideration of the subjects referred to them the diversified experience of many years spent in the examination and improvement of harbors. The several reports they have made on the exterior lines, on the improvement of Hell Gate, and on the preservation of Gowanus bay, are profound dissertations on the forces and actions of currents, and, while they evince, in some degree, the extent of the labors of those gentlemen, they demonstrate how just is the public estimate of their scientific attainments."

Following the example of New York, Massachusetts soon organized a similar commission for the port and harbor of Boston, on which the same gentlemen were invited to serve, receiving similar testimonials of the high value of their services.

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* The "questionable shape" and suspicious object of this novel craft, set afloat and towed out into the bay by the rebels in 1861, caused anxious surmises on the part of Colonel Brown and the gallant garrison of Fort Pickens, reminding us of the famous "Battle of the Kegs" of the Revolution. The probable object was to sink it in the channel to prevent the entrance of our gunboats. But Colonel Brown's interference prevented the accomplishment of the design. It was abandoned by the rebels, and set fire to by Colonel Brown's orders.

Of the many scientific men of the country who were associated with him in such duties, (of whom most usually was our eminent president,) none exhibited greater zeal and assiduity, few took a more prominent and useful part. The resolutions of the Light-house Board, on the occasion of his decease, which are appended to this memoir, would be, with slight modifications, applicable in reference to all his connexions of a similar nature. Inflexible in his integrity, uncompromising in his notions of duty, and watchful to the highest degree for all the interests of the government in all that concerned his charge, it is not strange that the shameless Floyd soon found him an obstacle to his peculiar operations. He was virtually banished from his office, or at least relieved from its duties, which he did not resume until Floyd left the War Department. He took this opportunity—perhaps the very first and only release during his lifetime from the unceasing demand of duty—to visit Europe in company with Mrs. Totten, travelling through France, Italy, Germany, and England. Endued with those keen perceptions and that harmonious adjustment of faculties which render the mind susceptible to the beautiful, whether in nature or art, he was, in the true sense of the term, an artist. For music, for painting, for sculpture, he had a high relish and a most accurate and discriminating judgment.

By such a one the treasures of art and antiquity of Europe can only be adequately appreciated and enjoyed, as we know they were appreciated and enjoyed by General Totten. He did not fail, however, to take the opportunity to examine, as far as he was able, the fortifications of Europe, of the character and peculiarities of which, however, he had little to learn. On his return he was sent by Floyd to the Pacific coast, with directions to inspect the fortifications in construction, and to report on the defensive requirements of that region. This duty and the report thereon he executed in his usual thorough and exhaustive manner. It furnished him with the opportunity to acquire the same personal knowledge of all that concerned the seaboard defence of our newly acquired territories on the Pacific which he already possessed, beyond any other man, in reference to the Atlantic and Gulf coasts.

In the year 1851 General Totten inaugurated, and continued through the years 1852, 1853, 1854, and 1855, a series of experiments at West Point "on the effects of firing with heavy ordnance from casemate embrasures," and also "on the effects of firing against the same embrasures with various kinds of missiles." It will be interesting and conducive to a better understanding of the objects and results of these experiments to say a few words as to the origin and meaning of the term "casemate," and to give an account of General Totten's previous labors in connexion with the "casemate embrasure." The word is from the Spanish *casa-mata*, (a compound, most likely, of *casa*, house, and *matar*, to kill; though it is said also to mean a low or hidden house; but the etymology is not settled,) and seems to have been used to signify a countermine as well as a concealed place, arranged in connexion with a fortification, for containing and using a piece of artillery. According to Bardin* it appears to have been applied to the double or triple tier of uncovered gun platforms used by the early Italian and German engineers for flanking the ditch, as well as to vaulted galleries along the scarp wall. The term finally came to mean, in fortification, any vaulted room under the earthwork of the rampart or glacis, whether intended for service of guns, or for quarters of troops, or for containing stores. A *gun casemate* is such a vault abutting against the scarp or counterscarp wall through which an "embrasure" is pierced to permit the discharge of the gun; and in the naval service the term has been adopted to signify the part of an iron-clad vessel containing the guns, and which is, for that reason, especially protected by the iron plating. Hence the essential notion of the word seems to involve one or more of the attributes of concealment, shelter, and destructive purpose.

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* Dictionnaire de l'Armée de Terre, &c.

The use of the casemate, in some of its forms, for flanking purposes goes back to Albert Durer and San Micheli, in the early part of the sixteenth century, and it was resorted to by Vauban in his second and third systems, of which the tower-bastions are casemated throughout. But it was reserved for the Marquis de Montalembert, in the latter part of the eighteenth century, to give it an extraordinary development, and to make the casemate the essential element of a system of fortification. This "most intrepid of authors upon fortification" (as he is styled by Chasseloup) boldly attempted to apply to his art the same principles by which Napoleon won his victories—the concentration of superior forces upon the decisive points. In his projects we find, upon all parts where there must be a decisive contest of artillery, an extraordinary concentration of guns, amounting in some cases to ten times those of the attacking batteries, the construction of which it is intended to prevent, or which shall be promptly overpowered, if constructed. This concentration he effected, and could only effect, by the use of casemates, upon which, numerous and well constructed, he bases all the strength of his fortifications.

No author on this art has displayed greater genius or a greater affluence of resources, and no author has given occasion for so much acrimonious discussion. Rejected by the French, the principles of Montalembert have been made the basis of the modern German, or "Polygonal," system.

For sea-coast fortification the casemates of Montalembert had a singular applicability, and he has the merit, at least, of being the first writer who has seen in this branch of the art a subject of particular treatment, and who had given special designs for forts and batteries "for the defence of ports."

In no warlike structure was there so great a concentration of artillery as in a ship-of-war, such as it was fifty or even twenty years ago. And as there is no limit to the number of ships which may be brought to bear upon a shore battery save that of the range of artillery and the area of navigable water, it is easy to see to what overwhelming hostile fire such a work may be subjected. On the other hand, it frequently happens that the site otherwise most advantageous for a battery is low and contracted, rendering any accumulation of guns impracticable, if mounted on an ordinary rampart, and exposing the unprotected gunners to the fire of the sharpshooters with which the enemy's topmasts are filled.*

It is no small merit of Montalembert to have devised a method of mounting guns which should meet this case. Notwithstanding that the French corps of engineers rejected the system in its intended application, and disclaimed, as an engineer, its author, it nevertheless constructed, in 1786, for the defence of the roadstead and harbor of Cherbourg, forts which are in reality almost copied from his designs.† Following the example of the French, other European nations have adopted, for the defence of their seaports, works of the same character, of which the forts of Cronstadt and Sebastopol, once made familiar to us, in their outward appearance, by the pictorials, are recent specimens; and, as we have already seen, Colonel Williams introduced them into our country in 1807, by the construction of Castles Williams and Clinton, and Fort Gansevoort, New York harbor.

An objection urged against casemates, and a grave one, since it is aimed at one of their most important attributes, is that the embrasures of masonry are dangerous to the gunners, from their outward flaring surfaces reflecting into the interior the enemy's missiles. Montalembert was well aware of this objection, calling the embrasure, in its ordinary form, a "murderous funnel," (*entonnoir*

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* The topmasts of many of the vessels of Commodore Farragut's fleet in the attack on Forts Jackson and St. Philip contained boat-*howitzers*, destined to fire canister at the gunners of the low batteries of those works.

† The celebrated Carnot, then an officer of French engineers, but who adopted the views of Montalembert, writes to him: "You have wrung from your adversaries the admission that well-constructed casemates are a good thing," &c. (*Zastrow, Histoire de la Fortification.*)

meurtrière,) and his sagacity did not fail to prescribe the best remedy by rules intended to reduce to a minimum the external opening. He directed that the throat should be no larger than necessary to receive the muzzle of the gun and to endure the shock of its discharge; that it should not be more than two feet from the exterior surface of the wall; that the cheeks should be parallel to the sides of the sector of fire; and to render practicable these arrangements he invented the "*affut à aiguille,*" (carriage with tongue,) which has served as the type of nearly all subsequent casemate gun-carriages. It is strange that, even while adopting the plans of Montalembert, European engineers should have almost wholly overlooked these maxims, and that it was reserved for our own illustrious engineer to make their application, and, in perfecting the casemate and the embrasure, to become a co-worker with Montalembert, by bringing the casemated water-battery to its highest degree of perfection.

I now revert to General Totten's labors in this connexion, and in reference thereto I quote from his report to the Secretary of War:

"The first casemated battery was completed in 1808. It has two tiers of guns in casemates and one in barbette. The exterior openings of the lower embrasures are 4′ 8″ by 6 feet, giving an area of 28 square feet; and of the second tier 3′ 8″ by 5 feet, area 18⅓ square feet; the horizontal traverse of the guns being limited to 44 degrees.

"Within three or four years of the time just mentioned two other casemated batteries were built, each having a single tier of guns in casemates, with exterior openings of 4′ 5″ by 5 feet, area 22 square feet; one with horizontal scope of about 42 degrees, and the other of about 45 degrees.

"In 1815 the author of this report was called on to prepare a project for the defence of an important channel; and, having been convinced, while employed as an assistant in the construction of two of the batteries just mentioned, that the principles and the details by which the embrasures and the dependent casemates had thus far been regulated were erroneous and defective, set about a careful study of the conditions to be fulfilled in providing for the heavy guns of that period mounted on a casemate carriage that had already been approved and adopted. The result was an embrasure having an exterior opening of 4 feet wide by 2′ 6″ high at the outside line of the cheeks, and 3 feet high at the key of the covering arch, the throat being 1′ 10″ wide. This provided for all the depression and elevation of the gun that the carriage permitted, and also for a horizontal scope of full 60 degrees. Covered with a lintel instead of an arch, the height of the exterior opening might be a little less than 3 feet.

"The plan of this embrasure shows that the interior opening is 5′ 6″ wide, and that the plane of the throat is within 2 feet of the outside of the wall, which, just at the embrasure, is 5 feet thick.

"A slight modification fitted this embrasure, when applied to flanking or interior defence, to receive at first a carronade of large calibre, and of later years a howitzer instead. When these latter were liable to be assailed by musketry, the outer cheeks were made *en cremailliere,* (notched,) a long-known device.

"It was with timidity and hesitation that the cheeks of this embrasure were placed so near the track of the ball, when fired from the casemate, with the maximum obliquity, and the results of an early trial with experimental embrasures at Fortress Monroe gave some sanction to the doubt. The first two under trial were built of lime-mortar, and were soon shaken to pieces by the blast of the gun. Another one, however, constructed of bricks laid in cement-mortar, sustained without injury several hundred discharges. These last results have been confirmed wherever there has been practice from our embrasures, which, with immaterial differences, have, since 1815, been constructed in all our casemated batteries according to the preceding description."

It will be seen from the foregoing quotations how thoroughly General Totten, in adopting the casemated battery, was imbued with the spirit of its illustrious originator. If, as is likely, he was aware of the latter's rules on this subject, he was the first to appreciate their essential importance, and to prove the practicability of their application. It is probable, however, that the close study of the subject, critical observation, and keen sagacity which so distinguished him on all occasions, and which taught him to accept nothing as the best which was susceptible of improvement, led him to recognize as "murderous funnels" the embrasures of routine—to create anew the rules of Montalembert, and to make, for the first time, a successful application of them. He reduced the throat to nearly an absolute minimum ; he placed it at two feet from the outer face of the wall, diminishing the external openings from eighteen, twenty-two, and twenty-eight, down to about ten square feet, while he increased the sector of fire of the gun from forty-five to sixty degrees ; thus adding one-third to its field of fire, and consequently to its value.

The embrasures, thus modelled in 1815, remained unchanged until the year 1858, but the casemate continued a subject of study and experiment during most of his life. The perfecting of ventilation, the determination of the dimensions and height of the piers, of the span and rise of the arches, their thickness and manner of covering, so as to obtain perfect drainage and to avoid the injurious effects of frost, &c., were problems of prolonged research and skilful solution, establishing for General Totten the right to be considered the author of the American casemate.

In connexion with these researches may be mentioned those also which were directed to the determination of the manner of mounting guns "en barbette." * As the dimensions of sea-coast ordnance increased, more and more elaborate structures became necessary for their mounting and management. The planning and construction of the carriages belonged to the Ordnance Bureau, but it was General Totten's task to adapt the platforms and parapets thereto. None but the engineer or artillerist can thoroughly understand the difficulty and complexity of the problems therein involved. To provide a platform which shall support, without the slightest deflexion, the weight, and resist the shock of discharge, while it provides for the training or pointing of the gun—which is so adapted to the parapet as to allow the maximum horizontal sector of fire, and to afford the most perfect cover to the gunners consistent with allowing all the depression demanded by the circumstances of the case—such are the conditions to be fulfilled, separately, for each calibre of gun. After years of experience, and after our sea-coast ordnance had attained its highest development prior to the introduction of the rifled gun and fifteen-inch columbiad, General Totten embodied his results in a lithographic sheet exhibiting to the eye of the engineer for every kind of gun and for every probable case the particular solution. This single sheet exhibits strikingly the characteristics of the author's mind—the profound study which he brought to bear on every subject, the scrupulous accuracy of his determinations, which neglected no appreciable magnitude, and the thoroughness and generality of his solutions.

When the embrasure of 1815 was designed, ships' armaments contained no gun heavier than a twenty-four or thirty-two pounder. As the calibres increased it became a matter of doubt whether the five feet thickness of wall immediately about the embrasure was sufficient. At the same time the progress made in the art of forging large masses of iron had suggested that by its use the funnel form of the mouth might be entirely done away with, and the exterior opening reduced to an absolute minimum. Nothing but *experiment* could lead to sound conclusions, and the experiments referred to on a former page were instituted, the principal objects of which were (in General Totten's own language)—

I. "To ascertain the effects of firing with solid balls, with shells, and with grape and canister, from heavy ordnance at short distances, upon various materials used in the construction of casemate embrasures.

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* A barbette gun is one which is fired over a parapet.

II. "To determine whether these embrasures might have a form that would shut out most of these missiles, and resist for a time the heaviest, without lessening the sector of fire, horizontal and vertical, of the casemate gun.

III. "To determine the degree to which, without injury from the blast of the gun, or lessening its scope of fire, the throat of the embrasure, and also the exterior opening, might be lessened.

IV. "To determine whether all smaller missiles might not be prevented from passing through the throat into the battery; and whether the smoke of the blast might not also be excluded by simple and easily managed shutters."

Targets were constructed representing the wall of a fortification pierced with its embrasures. All varieties of materials were employed in the walls, and every suggested method of constructing the embrasure was tried. General Totten's report shows that the minutest detail of construction was directed by himself, and that he personally superintended the experiments. They were carried on at intervals during four successive years, the results of each year suggesting the object of experiment for the next.

It would be out of place here to follow the report through its detailed accounts of the firings, or even to attempt to sum up the conclusions arrived at, referring as they do to such a variety of subjects; but those concerning the thickness of the scarp-wall and the use of wrought iron may be properly quoted as among the most important:

"The general conclusion from these trials is, that, whether of cement concrete, of bricks, or of hard stones, the portion of the wall at and around each embrasure having the thickness of five feet only should be no larger than is indispensable for the adaptation of the gun and carriage to the embrasure; if restricted to a small area, this thickness will suffice—not otherwise.

"The thickness of five feet will resist a number of these balls, impinging in succession on that space, provided the bond expand promptly above, below, and on each side, into a thickness greater by some two and a half feet or three feet or more. Were the wall no thicker generally than five feet, being reinforced only by piers some fifteen feet apart, it would soon be seriously damaged by battering at short distances."

And in reference to iron it is stated: "First, it may be fairly assumed that a plate eight inches thick of wrought iron of good quality, kept in place by a backing of three feet of strong masonry, will stop a solid ball from an eight-inch columbiad fired with ten and a quarter pounds of powder from the distance of two hundred yards. The plate of iron will be deeply indented at the point of impact, the ball carving for itself a smooth bed of the shape and size of one hemisphere, in which it will be found broken into many pieces easily separable, and it will, besides, be somewhat bent generally. The masonry behind will be much jarred, and, unless strongly bonded, be considerably displaced; moreover, unless the thickness of three feet is well tied into thicker masses immediately adjacent on the sides and above and below, the general damage will be severe.

"Second, this plate will be much the stronger for being in a single mass, and not made up of several thinner plates. The continuity effected by bolts and rivets of the made-up plates is broken even by weak assaults, so that afterwards the stronger, instead of a joint opposition, finds only a succession of feeble resistances.

"Third, a thickness of two inches is ample for shutters designated to stop the largest grape-shot. With this thickness they will be neither perforated nor deformed by anything less than cannon balls or shells. These shutters also, for the reason just given, should be made of a single thickness. The firings show the necessity of concealing entirely, even from the smallest iron missile, their hinges and fastenings.

"Fourth, a wrought iron plate of half an inch in thickness is adequate to protect the outer margins and the offsets of embrasures from injury by grape or canister shot."

These facts established, the effect of the form and dimensions of the embrasures in carrying in the smaller missiles was investigated; the recorded results will enable us to appreciate the force of Montalembert's expression, "murderous funnels," as even its author could not do.

"Suppose a hundred-gun ship to be placed within good canister range of a casemated battery of about the ship's length and height, to the fifty guns of the ship's broadside there would be opposed about twenty-four guns in two tiers in the battery. The ship would fire each gun once in three minutes, or ten times in half an hour; the fifty guns would therefore make five hundred discharges within that time.

"With one hundred and fifty-six balls in each thirty-two-pound canister, (weighing in all thirty-one and a half pounds,) there would be thrown seventy-eight thousand balls in thirty minutes. Supposing one-half to miss the fort, which, considering the size of the object and the short distance, is a large allowance, there would still remain the number of thirty-nine thousand balls to strike a surface of (say) six thousand square feet—that is,

"On each square foot	6½ balls.
"Or within the exterior opening of one of the embrasures of our second target, of which the area is 8.9 square feet, there would fall	58 balls.
"Within the European embrasure above mentioned, having fifty-four square feet of opening,* there would be received in half an hour	351 balls."

And if the ship carried modern eight-inch guns, and fired canister of musket balls, these figures would be, in the three cases, fifty-one, four hundred and fifty-three, and two thousand seven hundred and fifty-four. These theoretical conclusions were verified by the experimental firing with grape and canister, and it is thus seen how greatly superior General Totten's embrasure of 1815, which is but little larger than that of the second target, is to the European one, and how thoroughly he had, at that early day, mastered the subject. He had, indeed, perfected the embrasure so far as it could be done with masonry alone.

But the quantity of small missiles which even that embrasure would receive is dangerously great, and would be much diminished if the funnel-form of the mouth could be done away with, and the throat reduced to an absolute minimum. This could be accomplished only by the use of iron, and the conclusions I have just quoted furnish the data necessary to its successful application.

The throat (still placed two feet back from the outer face of the wall) being formed of iron plates, it became practicable to cut away the flaring surfaces of masonry, so as to present others parallel or perpendicular to the face of the wall, and by this change of form to exclude all missiles not directed within the limits of the throat itself. Still more completely to accomplish the object, wrought-iron shutters of two inches thickness (as determined by the experiments) were applied, by which, except at the moments of aiming and firing, the embrasure was entirely closed.

Such is the history of the casemated battery and casemated embrasure in the United States. We have seen that the perfection to which they have been brought is due to General Totten, and to General Totten alone. Nor is it to the experiments which I have been describing, laborious, skilful, and thorough as they were, that we may solely attribute such results. We must look back to

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* Reference is made to the embrasure of a European work built within the last twenty-five years.

the time when, a first lieutenant of engineers, he saw and aided in the construction of our first casemated fort, and when he, fully appreciating its merits and recognizing the defects which a disregard and want of appreciation of the illustrious projector's own principles had entailed upon it, set himself to the task of enhancing the one and correcting the other.

The ten years which have elapsed since 1855 have witnessed changes in the character of sea-coast and naval artillery, and an increase in the calibres and weight of their projectiles, which no one at that date would have anticipated; hence some doubt may be entertained whether our casemated masonry works are adequate to contend with iron-clad vessels armed with the modern artillery. This is a question which it remains for experiment or experience to decide. It has, as yet, not been demonstrated that a masonry fort, constructed as our more recent works are, will not, armed with the powerful guns now being introduced, endure the contest quite as long as its iron-clad antagonist can protract it.

In this connexion it is due to General Totten to say that he has himself been ever the most strenuous advocate of "big guns," the most urgent instigator of their production. The writer well remembers when, seated with him on the piazza of the officers' quarters at Fort Jackson, our eyes resting on the mighty stream flowing past us, upon the defence of which our thoughts and conversation had been turning, he exclaimed, "We must have a 20-inch gun." The idea was novel to me at that time, and I exhibited some surprise. He went on to say that, thoroughly to prevent the passage or attempted passage of an armed steamship, there must be not only danger but almost a certainty of destruction. "Let us have guns such that (to use his own phrase) 'every shot shall be a bird.'" The invention of armored ships, not then foreseen, has increased the necessity of having such guns as he, on other grounds, so strongly advocated. He expressed the greatest confidence that a gun of the dimensions he named would yet be made and introduced into our batteries, and added the interesting statement that in his earlier days he had found much difficulty in impressing upon the members of boards on which he had served the necessity of having guns in our harbor defences larger than 24-pounders. To the labors and genius of a Rodman we owe the actual invention of the art of constructing fifteen and twenty-inch guns; but without the unceasing stimulus of General Totten's known and urged views, it is doubtful whether Rodman's labors would have been called for or sustained.

The preceding pages have been mainly devoted to the illustration of our departed associate's career as an officer and as the Chief Engineer of the United States. Before turning our attention to other spheres of his usefulness, it seems fitting to quote from one of his eulogists the following summary of his official characteristics:

"In wielding the influence of his office as Chief Engineer, the prominent traits exhibited by General Totten were strict justice and scrupulous integrity. No sophistry, no blandishments, no arbitrary exercise of superior authority could turn him in the least from his steadfast adherence to his own sense of duty. Avoiding all useless collisions with his official superiors, showing due respect to their station, he never failed to call their attention to any errors committed by them with respect to the department under his charge; nor did he ever leave them any excuse for wilful wrong-doing by remaining silent, even when he knew that his suggestions would not only be ill-received and of no use, but might be visited by the exercise of those petty vexations which official superiors can employ against those under them who thwart their misdoings.

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"The individual traits of General Totten were strongly marked. Powerfully built, of a constitution of the most vigorous stamp, cool, potent, and persevering, of sound judgment and variety of intellectual capacity, nature seemed to have endowed him for the profession that he had chosen. His attention to the performance of his professional duties amounted to a devotion.

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"Whilst steadily adhering to what had been well settled by experience, and withstanding the ill-directed efforts of that class of men of whom some are to be found in all bodies, who seize upon every novelty and press it into the service of their own crude notions, he was far from rejecting well-reasoned projects of improvement, and encouraged, as his own immediate works show, every step towards real progress. Although not belonging to the class of mere inventors, he had that invaluable faculty to one holding a position of so great public responsibility, of detecting the fallacies with which this class too frequently deceive themselves as well as others."

In 1863, under the law uniting into one the two corps of engineers and topographical engineers, General Totten was advanced to the full grade of brigadier general. A few days before his death the Senate unanimously confirmed his nomination by the President to be "major general by brevet, for long, faithful, and eminent services." Never were such distinction and such commendation more fitly bestowed.

Giving the precedence in order to duties most intimately connected with his profession, I now turn to General Totten's important labors in establishing and maintaining our present light-house system.

The attention of Congress having been called to the pressing necessity for introducing certain reforms, administrative and executive, into the light-house system of the United States, that body, after full discussion of the subject, passed an act (approved March 3, 1851) stipulating that from and after that date, in all new light-houses and all light-houses requiring illuminating apparatus, the lens or Fresnel system should be adopted.

Another chapter of the same act provided for the appointment of a commission, to be composed of two officers of engineers of the army, and such civil officers of high scientific attainments as might be under the orders or at the disposition of the Treasury Department, and a junior officer of the navy as secretary, whose duty it should be to inquire into the condition of the light-house establishment of the United States, and to make a general detailed report and programme to guide legislation in extending and improving our present system of construction, illumination, inspection, and superintendence.

The board, as constituted by the President, consisted of Commander W. B. Shubrick, General J. G. Totten, Colonel James Kearney, Captain S. F. Dupont, United States navy, Professor A. Dallas Bache, superintendent United States coast survey, and Thornton A. Jenkins, United States navy, as secretary.

Its labors were directed first to demonstrating the evils, irregularities, and abuses which had crept into the light-house service under the management of the Fifth Auditor of the treasury, (the late venerable and highly respected Stephen Pleasonton,) among which were found to be those arising from defective principles of construction, renovation, and repair of light-houses, inadequate protection to sites and badly planned and poorly constructed sea-walls. It may readily be understood how the peculiarly practical mind of General Totten, brought to bear upon these and kindred subjects of inquiry, developed and demonstrated the necessity of at once employing proper scientific systems and plans of construction. His assistance in collecting data was found invaluable, and his lucid, clear mind was equally to be trusted in detecting faults and in devising the remedy.

Without entering into a detailed account of the labors of this board of inquiry, it is sufficient to state that the mass of evidence collected by it was so irresistible in proof of existing errors, that Congress, under date of August 31, 1852, passed an act which created a permanent light-house board, to which was confided all the duties of the establishment. General Totten was appointed to this board, and served as a valued and honored member, with but a short interruption, until his decease. Its early labors were arduous and onerous. A new system was to be founded where before had been none; order should come from chaos, error was to vanish before science, economy to succeed to wastefulness, darkness to give place to light. The task, great as it was, fell upon no shrinking hearts or feeble brains. The work was accomplished; and long before his lamented death General Totten had the satisfaction of witnessing the labors of himself and his associates crowned with full success. The board in its deliberations derived great benefit from his presence and participation, and relied with entire assurance upon the correctness of his judgment upon all subjects concerning which he would express an opinion. He served almost continuously as chairman of the committee of finance, and the decisions of that committee owe not a little of their sound wisdom to the searching scrutiny joined to the generous and liberal views of its chairman. He was also a member of the committee on engineering, in which department his peculiar merit was most conspicuous. The principal works with which his name is associated, and which claim our attention, are the light-houses on Seven-Foot Knoll, near Baltimore, Maryland, and on Minot's Ledge, off Cohasset, Massachusetts.

The former is an iron pile structure standing in some ten feet of water. It was erected at a time when the science of iron pile construction was in its infancy, and was one of the first works of the kind undertaken by the board. Hence it was a matter of deep interest and solicitude. It was successfully completed, and the light-house stands to-day a signal reward for the thought and labor bestowed upon its conception and construction.

The light-house at Minot's Ledge was a work of far greater difficulty, and to its proper location and plan General Totten lent the resources of his great experience and exhaustless knowledge. As his intimate acquaintance with the whole coast of the United States, acquired while acting as a member of the board of engineers, and during his annual inspections as Chief Engineer, enabled him, with the aid of the Coast Survey, to indicate with almost unerring certainty the proper location and character of all new light-houses, so his practical knowledge of construction, in laying the foundation of our sea-coast fortifications and the sea-walls by which the sites of many of them had to be protected, prepared him to grapple with the difficulties of constructing a masonry tower in this exposed situation, and to bring to their solution all the known and tried resources of engineering.

Minot's Ledge is situated about twenty miles southeast of Boston. It is the outer rock of a very dangerous group called the "Cohasset Rocks," lying at the very wayside of navigation to the harbor of Boston. A light-house of iron had been erected here a few years previous to the organization of the Light-house Board, but it was carried away in a fearful storm which swept along the coast of New England on the 16th of April, 1851.

Not only the commercial interests of the country, but humanity demanded that it should be replaced, and Congress promptly made an appropriation for this purpose, stipulating that the tower should be erected on the outer Minot, and confiding its construction to the Topographical Bureau. This bureau, having publicly advertised, received sixteen distinct proposals to erect the proposed structure, but finally recommended, in view of the difficulties to be overcome, and the fearful fate of its predecessor, that it should be located on one of the inner rocks. In accordance with this recommendation, an act of Congress was passed authorizing the Secretary of the Treasury to "select, instead of the outer Minot's Ledge, any more suitable site." Before further action had been taken, the whole subject fell into the hands of the newly created Light-house Board. A joint resolution of Congress was then passed (1854) giving to this board the decision as to the location and the mode of construction.

The question of location being thus widely reopened, a committee of the board was sent to make a personal examination of the locality. General Totten was, of course, a member of this committee, and was not long in making up his mind that the outer and not the inner Minot was the proper site. His arguments on this subject proved conclusive with the board. He urged that if the light were placed on any of the inner rocks the desired object would be but partially accomplished, since in a dense fog or thick snow-storm vessels might approach within a few hundred feet without being able to see it, and thus be lost upon the outer ledge.

When the question of practicability was broached, his professional pride seemed to be roused. He argued that, after what had been done on the coast of England in the erection of the Eddystone light-house a century ago, and more recently of the Bell Rock and Skerryvore lights, it would be a humiliating admission that the requisite science and skill were not to be found in this country to erect a similar structure where, as all admitted, one was so much needed.

He carefully studied the accounts of the construction of the Eddystone, Bell Rock, and Skerryvore light-houses, by Smeaton, Robert Stephenson, and Allan Stephenson, but the fact that the Eddystone was begun at high-water mark, that the ledge of the Bell Rock was extensive, and elevated several feet above low-water, and that the Skerryvore presented still less difficulties, while the surveys show that the outer Minot's ledge was very contracted, and that the proposed structure must commence even below low water, did not deter him from advocating and designing a work for this formidable position more difficult to accomplish than anything which had ever preceded it.

The plans which he prepared were drawn with his usual minuteness of detail. The problem was one peculiarly fascinating to engineers—the uniting into a single mass the several component stones of the structure so that no one can be detached from the rest, that each shall be a bond of connexion to those adjacent, that the whole shall be an integral, having a strength ample to defy the most powerful foe to human structure, the fury of the ocean's winds and waves. Though not himself the constructor of the work, yet to have insisted against authoritative adverse opinion on its practicability, to have planned the building and selected the engineer who should rear it, and to have overlooked the work from its commencement to its completion, entitles him, even were this his only work, to recognition among the Smeatons and Stephensons and Brunels, as one of the great engineers of the age.

For the execution, he selected Captain (now Brevet Brigadier General) Barton S. Alexander, of the Corps of Engineers, an officer whose experience, energy, boldness, and self-reliance eminently fitted him for the task. It is for him to recount the history of the work, to give to the world the interesting narrative of difficulties met and overcome, of patience requited and energy triumphant. General Totten watched its progress with unflagging interest, making frequent visits to the superintending engineer, aiding him with his counsels and encouraging him in his difficulties. He lived to enjoy the proud satisfaction of inspecting the finished structure; and when at last from its towering summit flashed o'er the troubled waters the beacon-light of safety to the tempest-tossed mariner, he might well exclaim, with the Latin poet, though in a nobler sense and in a less boastful spirit, "Exegi monumentum ære perenius."

General (then Colonel) Totten was named in the act of Congress organizing the Smithsonian Institution in 1846 as one of the Regents to whom the business transactions of that celebrated establishment are intrusted. At an early meeting of the Board of Regents he was appointed one of the Executive Committee, and was continued in these offices by repeated election to the time of his death, a period of nearly eighteen years. He evinced a lively interest in the organization of the Institution, and after a careful study of the will and character of Smithson, gave his preference to the programme prepared by Professor Henry, which was finally adopted. His advocacy of the plan was the more important since he was well acquainted with the scientific character of James Smithson, and had himself, as we shall see in a subsequent statement, been engaged in a line of research similar to one of those pursued by the founder of this Institution.

In the reconstruction of the interior of the main part of the Smithsonian building which had partly been completed in wood, but which had given way, he strongly urged the employment of fire-proof material, to the adoption of which the preservation of the valuable collections of the Institution is indebted. In the discharge of his duty as one of the Executive Committee, he acted with the same conscientious regard to the sacredness of the trust which characterized all his official labors, and critically examined all the accounts, assured himself as to the proper expenditure of the funds, and advised as to the general policy to be pursued. In him the Secretary ever found a firm supporter, a sympathetic friend, and a judicious adviser. Unostentatious, unselfish, and only desiring to advance whatever cause he might be connected with, he gave the most valuable suggestions as if they were of little moment, and in such a way that they might appear to be deductions from what others had said or done, being more anxious that his suggestions should be properly carried out than that they should be accredited to himself.

As a recreation from the more arduous studies of his profession, he devoted in the early part of his life his spare hours to natural history, paying much attention to the mollusca of the northern coast of the United States; and he was perhaps the first, or at least one of the first, to introduce into this country the use of the dredge for the search of these animals, thus not only obtaining many species which would otherwise have escaped attention and getting fresh and unmutilated specimens of species previously known only from dead imperfect shells, but enabling us to learn something of the habits and associations of the animals—information of much greater scientific value than the discovery of a few new species. His observations and studies in conchology were embodied in an article entitled "Descriptions of some Shells belonging to the Coast of New England," published in the American Journal of Science and Arts for 1834 and 1835, and Dr. A. A. Gould was largely indebted to him for material employed in his "Invertebrata of Massachusetts," many of the species of shells contained in which were first found to inhabit our coast by General Totten; others were new species discovered by him, though described by Dr. Gould, while some nine or ten specimens were not only discovered but described by him. The descriptions of species and remarks evince his powers of observation and critical acumen, and almost all of the forms described have stood the test of subsequent examination, and the validity of their specific distinction been confirmed, although several of them are among the most common shells of the coast; on account of their small size, they had been previously overlooked or neglected, but their insignificance in size did not diminish their interest in the eyes of one who viewed nature in all her manifestations as worthy of contemplation. One of the most beautiful and almost the smallest of the bivalves of our coast, called by him *Venus gemma*, has since been dedicated to him under the name of *Gemma Tottenii* by Dr. William Stimpson.

General Totten collected principally on the shores of New England, and his explorations with the dredge were almost entirely made in the vicinity of Newport, R. I., and of Provincetown, Mass. A list of the shells of Massachusetts was contributed by him to one of the preliminary reports on the natural history of that State. The principal species described by him are as follows: *Modiola glandula,* (now known as *Mytilus decussatus,*) *Venus gemma,* (*Gemma Tottenii,*) *Solemya borealis, Bulla oryza, Natica immaculata, Turbo minutus,* (*Rissoa minuta,*) *Turritella interrupta,* (*Chemnitzia interrupta,*) *Acteon trifidus,* (*Chem-*

nitzia trifida,) and *Pasithea nigra.* This last-named species he described from young shells, and afterwards finding the adult shell, which is very different, called it *Cerithium reticulatum.* It has for many years been called *Cerythium Sayi,* but a late author has again credited it to him, under the name of *Bittium nigrum.*

A species of *Succinea* (*S. Totteniana*) was dedicated to General Totten by Mr. Isaac Lea, of Philadelphia.

Conchologists are also indebted to General Totten for the discovery of means for the preservation of the epidermis or periostraca of shells, which is in many species so liable to crack, and this recipe has been received with much approbation by many collectors who have found it to supply a want much felt. The valuable collection of rare shells which he made at this period of his life he presented to the Smithsonian Institution, without the usual condition that it should be preserved separately, but to be used most advantageously for the advancement of science, to complete the general collection of the museum, or for distribution as duplicates to other establishments.

In the "Annals of the Lyceum of Natural History of New York" for 1824 (vol. i, pp. 109–114) he published "Notes on some new Supports for Minerals, subject to the Action of the Common Blow-pipe." These researches on the use and power of the blow-pipe appear to have been incited by an article of James Smithson, the subsequent founder of the Smithsonian Institution, and the memoir of Totten commences with a reference to and rehearsal of the experiments of that gentleman, as detailed in a letter to the editor of the Annals of Philosophy. Smithson, it was remarked, had communicated several ingenious modifications of Saussure's process with supports of splinters of sapphire, which process, he observes, "has been scarcely at all employed; owing partly to the excessive difficulty, in general, of making the particles adhere, and in consequence of the almost unpossessed degree of patience required, and of the time consumed by nearly interminable failures." Detailing the processes of Mr. Smithson, three in number, and the success of that gentleman, he adopted a modification of Smithson's third process, having recourse, as a support, to a portion of the mineral itself, which he designed to expose to the action of the flame. "Instead, however, of taking upon the point of platinum wire a very minute portion of the paste made of the powdered mineral," according to Mr. Smithson's method, he "formed a paste by mixing the powder with very thick gum-water, and, rubbing a little of it under the finger, formed a very acute cone, sometimes nearly an inch in length, and generally about a twentieth of an inch in diameter at the base." To the apex of such cones the most minute particles would adhere under the strongest blast of the blow-pipe, and being insulated by the destruction of continuity of the particles of the cone, the flame could be directed upon it with undiminished fervor. Experiments were made on a number of minerals, confirming those of Mr. Smithson, and greatly extending the power of the blow-pipe, and he was thus led to add to the three classes divided in relation to this instrument a fourth, namely, "such as are fusible, *per se,* in microscopical particles."

The attention of the inhabitants near the shores of the great lakes of the north had often been arrested by the sudden disappearance in the spring of the ice on the surface. The lakes would be covered with a continuous sheet of solid ice in the evening, and in the next morning all would have vanished. Wild speculations had been entertained as to the explanation of this phenomenon previous to the investigation of the subject by General Totten, who presented an article on the subject to the American Association for the Advancement of Science at the Springfield meeting in 1859.

From this it appears that his attention had been directed to it forty years before, at Plattsburg, New York. Ice is composed of a congeries of prismatic crystals, whose axes are at right angles to the surface of the mass. "Examinations then and afterwards made of floating fresh-water ice have shown that the natural effect of the advancing year is gradually to transform ice, solid and apparently homogeneous, into an aggregation of these irregular prismatic crystals, standing in vertical juxtaposition, having few surfaces of contact, but touching rather at points and on edges, and kept in place at last merely by want of room to fall asunder. Until this change has somewhat advanced, the cohesive strength of ice of considerable thickness is still adequate to sustain the weight and shock of the travel it had borne during the winter; but becoming less and less coherent by the growing isolation of the prisms, or more and more 'rotten,' as the phrase is, though retaining all its thickness, the ice will at last scarcely support a small weight, though bearing upon a large surface, the foot of man easily breaking through, and very slight resistance being made to the point of a cone." The points of contact of the particles being destroyed, each will drop into the position in the water below required by the place of its own centre of gravity—that is to say, it will be upon its side, exposing large surfaces to the action of the warm water. With the ice in such condition, a heavy wind will cause the disruption of the particles, and the speedy disappearance would be the consequence. This remark of General Totten as to the crystallization of ice has since been extended to nearly all substances which, in becoming solid, assume the crystallized form. The axes of the crystals tend to assume a position at right angles to the surface of cooling.

As illustrative of the mind of General Totten, it may be stated that he seldom failed to give valuable hints for the improvement of processes or inventions which were brought before him in the course of the discharge of his numerous official duties. Among these was an instrument for ascertaining the daily amount of evaporation from a given surface by means of the descent of water contained in an inverted graduated tube, the open end of which was immersed in the basin from which the evaporation took place. With a slight correction for variation in barometrical pressure, this instrument gives, with more precision than any other with which we are acquainted, the amount of evaporation.

I have, gentlemen, thus faintly and inadequately sketched the life and services of our departed friend and associate; but, faint and inadequate as my sketch may be, I feel confident that every one will recognize in it the lineaments of a great and true man. Labors so protracted, results so important and varied, it is the destiny of but few to achieve, and for him who achieves them may justly be claimed a high niche in the temple of fame, and the grateful homage of the patriot and of the seeker after truth. One of the oldest of the corporators of this academy, it was permitted him only to contribute his past labors and his shining example. But these are indeed a rich legacy. Proud, indeed, may this youthful institution be that it can enrol among its members the name of Joseph Gilbert Totten; proud, too, may each one whom I now address—each one of its members—be, if he shall achieve but a far less claim to recognition among men of science. To the aged among us—to those who were young with him, and like him have crowned a life of toil by honorable achievements—I need not speak. *They* require no example, and they may feel in contemplating his history an additional assurance that their own works, too, "shall praise them." To the more youthful or to the middle-aged, who have just commenced, or but partially accomplished, the steep ascent which leads to honorable fame, his life is precious in its teachings.

He was a patriot in the broadest and best sense of the term. To his country he had given himself, and every faculty of his being was devoted to her honor and welfare—realizing almost literally the thought of Rousseau, "the child on entering life ought to see his country, and to the hour of his death see but her."

Like all who have left lasting results for the benefit of their country or of mankind, he was a hard worker. But ill-regulated labor, however arduous, could never have accomplished what he accomplished. Beyond all men I ever knew, he was *systematic*; and few indeed are the examples of a life, in *all* things, so perfectly regulated. The beautiful *order* which pervaded all that he did is scarcely less worthy of study and admiration than the achievements to which it so materially contributed.

He was no trifler with the realities of life, who dallied with them for his pleasure or who wielded them as instruments of ambition or self-interest. To him, as to all true men, the meaning of life was concentrated in one single word, DUTY. This "chief end of man," which is to glorify God by obedience to his laws in the use of the faculties he has bestowed, was his ruling principle—the celestial cynosure to which his eyes were ever directed, and from which no allurement of lower motives could divert it. Nor was his sense of duty of that frigid, repulsive nature which reduces the conduct of life to a formula, and, substituting rules for emotions, seems but a refined selfishness. He was warm and sympathetic, finding his chief happiness in the pleasures of domestic and social intercourse, but singularly susceptible to everything that ministers to innocent enjoyment.

Perhaps no more striking illustration than his history affords could be found of the truth that the path of duty is the path of happiness. His life was eminently a happy one, and his, indeed, was that "peace of mind which passeth understanding." Though devoted from his youth to the military service of his country, and doomed to the vicissitudes of a soldier's lot, he was permitted, to a greater degree than most men, to enjoy the blessings of the domestic circle. There, indeed, he sat enthroned, the idol of a family of whose supreme affection and immeasurable devotion he was the object. Nor dare we call those blows by which a Heavenly Father reminds us that this world is not our "abiding place," and teaches us to look beyond to "an house not made with hands, eternal in the heavens," sources of unhappiness to him who receives them as from the hand of One "who chasteneth whom he loveth." One by one, he lived to see all his three sons, two of his four daughters, and finally the companion of the joys and sorrows of so many years, precede him to the grave.

Beautiful beyond all else that earth presents is that conjugal companionship, so touchingly depicted by Burns, which, beginning in youth, is permitted to continue unbroken till the Psalmist's period of life is overpassed. During the later years of their lives, Mrs. Totten, no longer bound to the domestic hearth by the cares of a growing family, became truly an inseparable companion. Never, when it was at all practicable to have her with him, did he ride or walk, or make a journey, or perform one of his periodical tours of inspection, without her companionship; nor could one see them together without feeling that they presented a model of whatever is amiable and lovely in the conjugal state. If he was to her the embodiment of all that is most worthy of respect and love in man, not less marked was his deference to her. In her own sphere—as woman, wife, mother—she was supreme, and her judgment his law. When, but two years before his own death, she was somewhat suddenly called away, it seemed as if he regarded it as a message from on high, "set thy house in order, for thou shalt die and not live." No murmur escaped his lips, and no long-continued sadness clouded his brow, but there was an unwonted gentleness and quietude in his demeanor, a softening, as it were, of his nature, which revealed how deeply "the iron had entered his soul." His health and bodily strength seemed to continue little impaired, and his devotion to the duties of his office undiminished. But once, during a life protracted beyond the usual span, had that powerful frame submitted to the sway of sickness, and he seemed to have unusual promise of a still further protracted life. But such promises proved deceitful. Early in March, 1864, he was attacked with pneumonia. His illness was not at first deemed alarming, and, indeed, at one time he was supposed to be convalescent, but a relapse ensued, and on the 22d of April he expired, having borne the sufferings of his sickness with cheerfulness and resignation, and retained to the last the perfect use of all his mental faculties. He had long been a member and communicant of the Episcopal church, and died in the Christian's hope of a joyful resurrection.

Gentle, kind, and good, mild, modest, and tolerant, wise, sagacious, shrewd, and learned, yet simple and unpretending as a child, he died as he had lived, surrounded by hearts gushing with affection, and the object of the respect and love of all with whom he had ever been associated.

The greatest of sculptors, the greatest of painters, a man unsurpassed in boldness and originality of thought, and whose name is among those of the few whose genius overpasses the limits of country and claims homage from all mankind—Michael Angelo—in a work stamped with the maturity of his powers, carved a figure known to the world as "Il Pensiero," or Thought. There exists in art no other personification of meditation, no other type of self-collectedness and profound thought.

The sculptor arrayed it not as a philosopher, as a monk, as a poet, as an artist, as a theologian, as a scholar, nor even as a pope. And yet these different types of thinkers were not wanting in the past or present of the age and country of a Raphael, of a Correggio, of a Leonardo da Vinci, of a Dante, of a Savonarola, of a Marco Polo, of a Columbus, of a Machiavelli, of a Galileo, of a St. Francis de Assis, of a St. Thomas Aquinas, of a Julius II, of a Leo X, and of a Clement VII.

How, then, has Michael Angelo arrayed his personified "Thought?" In the garb of a Soldier, upon the breast the cuirass, upon the brow, wrapt in meditation, the iron casque of the man of war. The great sculptor has divined the mysterious cause why, among all people, among all classes, and in all epochs, the soldier is honored. Instinct teaches the people, and genius taught Michael Angelo, that among so many glorious examples, among so many immortal victims, so many illustrious martyrs or devotees of thought, illustrating an age or a country, the soldier stands forth pre-eminently, in all ages and in all countries, the victim always ready, the defender always armed, the servant, the apostle, and the martyr.

It is the Christian version of the ancient allegory which made Minerva issue from the brain of Jupiter: Minerva, or *wisdom armed*, the helmet upon her brow, the sword in her hand.

Will the foregoing paragraphs, which I have translated somewhat freely from the "Soldat" of Joachim Ambert, a work devoted to the illustration of the soldier's career, be deemed an immodest or extravagant glorification of the profession of arms? Far be it from me to exalt unduly that profession, but I would at least make a claim for it, the more necessary since popular apprehension tends to lose sight of the thinker in the man of force and of blood, that, more than any other, it embraces all sciences and all branches of human knowledge, and leads its followers into vast and diverse fields of thought. Let the illustrious dead be our witnesses; that idea which a genius of a Michael Angelo inspired and embodied in marble; that idea which the lives of a Cæsar, a Frederick, a Washington, a Napoleon, and a Wellington have justified; the union of Force and Thought finds yet another and a varied illustration in the accomplished soldier and profound thinker whose life and works we now commemorate.

RESOLUTIONS OF THE LIGHT-HOUSE BOARD.

Resolved, That the members of the Light-house Board feel most deeply the loss sustained by the branch of the public service under their charge in the death of Brevet Major General Joseph Gilbert Totten, who has been one of the most useful and active members of the board from its first appointment in pursuance of law in 1851, under the Secretary of the Treasury, as a temporary board of inquiry into the light-house establishment of the United States, through all the years of organization of the establishment and of its executive duties.

Resolved, That the high scientific attainments, the admirable administrative qualities, the perfect knowledge of general principles, and attention to every minute detail of the system, impressed the mental and moral qualities of General Totten upon his associates in a way to make his mind eminently a leading one of the board, while his suavity, patience, perfect amiability, and retiring modesty rendered him one of the most charming of associates in executing work to which he was so much more than sufficient.

Resolved, That in the discharge of the duties of inquiry of the first board, the resulting organization, the adoption of the present system of lighting by lenses, the subject of construction, theoretical and practical, and the use of materials, the experience and experimental knowledge of General Totten were of the highest value to the board, and his careful application of the sciences were of the greatest importance to the light-house system; and that in the large qualities of common sense in all the transactions of the board, general as well as technical, and in his high sense of justice directing great mental power, the board constantly felt the support of General Totten as one to be relied upon for guidance in all difficult questions of administration.

Resolved, That the affectionate qualities of General Totten's heart so endeared him to his colleagues, that in now expressing themselves in regard to his death, they are fully prepared to share to the utmost the deep grief of his family, to whom they offer their sincere condolence for the loss of one not to be replaced, but to be ever mourned as the true, devoted, and sincere friend.

Resolved, That a copy of these resolutions be transmitted to the family of General Totten, and to the honorable Secretary of War, and to the honorable Secretary of the Treasury.

Resolved, That these proceedings be published in the Washington newspapers.

GENERAL APPENDIX

TO THE

REPORT FOR 1865.

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

DUCROTAY DE BLAINVILLE,

BY M. FLOURENS,

PERPETUAL SECRETARY OF THE FRENCH ACADEMY OF SCIENCES.

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

"There is no pursuit in the world so toilsome," says La Bruyere, "as that of making for one's self a name." Undeterred by this reflection, and stimulated by the charm of satire, La Bruyere braved the annoyances of which he spoke, and made for himself a very considerable one. The member of the academy whose memory I am about to recall had too much energy to be daunted by such a saying as the above, and seems in no small degree to have been stimulated in his arduous labors by the spirit of contradiction. Having by persevering efforts thrown light on some of the highest points of the science of organized beings, he also enjoyed the success which seldom fails to attend criticism and attracted the fervid interest which opposition constantly excites, even when its attacks are directed against genius.

Born at Arques, February 17, 1777, son of Pierre Ducrotay and Catharine Pauger, Marie-Henri de Blainville was fond of recounting that, although his family was not numbered among the most illustrious of the province, it ascended, nevertheless, to the fourteenth century; that it was the issue of a Scotch gentleman who, holding nothing except by the tenure of cloak and sword, had received from the place of his landing the name of Ducrotay. Having thus sheltered the nobility of his family under the ægis of Scottish loyalty, he would add that, under Francis I, the government of the castle of Arques, which its position then rendered an important post, was confided to one Robert Ducrotay; that the fortunes of the family had been still further enhanced through a descendant of the latter, who had the address to secure the favor of five successive monarchs, had received particular marks of esteem from Henry III, and the confirmation of his titles and franchises from Henry IV, to whom he had rendered valuable service at the battle of Arques. It was in the bosom, therefore, of a family proud of its historical recollections and jealous of its privileges that the first moral impressions of the young Ducrotay de Blainville were formed.

He was the youngest son, and had the misfortune to lose his father at an early age. For the rudiments of education he was indebted to a neighboring curate, and was transferred at a later period to the military school of Beaumont en Ange, which was under the direction of the Benedictine monks of Saint Maur, and of which it is eulogy enough to say that it had the honor of counting Laplace among its pupils.

The revolutionary tempest, in dispersing the religious congregations, closed too soon for the young De Blainville this excellent source of instruction. He was scarcely fifteen when he returned to a mother, weak and broken in spirits, whose blind affection could maintain no adequate restraint over a youth of wayward disposition. All that depends on the life of a father—all that avails the experience of the head of a family who conceals from the son who should support the honor of his name none of the rude obligations of existence, is often only appreciated after a long series of deceptions. At the age of nineteen, wishing to enter into public service as an engineer, Henri de Blainville passed some months at Rouen in a school of design. The director of this establishment wrote to the mother of his pupil: "The character of the young man is intractable; his heart, though vindictive, is not unrelenting; his greatest passion is a love of learning; all the rest is a chaos of ill-combined ideas."

To finish his studies he came to Paris, and scarcely was he there when even the shadow of authority disappeared; he lost his mother. Delivered thenceforth to his own guidance, too much independence became to him a dangerous snare; he abandoned himself to all the passions of his age, and surrounded by trifling companions, succeded very quickly and very gaily in dissipating his whole patrimony.

Having attained this natural result of the life he was leading, he began to reflect, and comprehended the necessity of supplying the resources of which he had robbed his future existence. In his first efforts he did no more than put forth a restless activity. By turns he appeared as a poet and essayist among his friends, a zealous musician at the conservatory, and, in a celebrated studio, a painter and designer of no little skill. Two lofty principles, in the mean time, survived in the soul of this young man—an exalted respect for his birth and a love of knowledge.

The first of these two sentiments had, in truth, its perils; it gave rise to singular pretensions. M. de Blainville had preserved all the illusions of the noblesse of the preceding age to such an extent that he could never, even when his views had become sobered, entirely divest himself of the idea that by royal prescript he was endowed with peculiar privileges. Among these, as that of censure and authoritative assertion appeared to him the most precious, he made use of it always and everywhere, and this rendered intercourse with him somewhat impracticable to such as did not choose to admit these obsolete claims of feudalism.

The ardor for instruction, combined with the pious respect for family, saved this restless nature by directing its extraordinary energy towards a noble aim. When, shaking off the last delusions of an idle youth, our fiery gentleman found, on attaining his twenty-eighth year, that he was ruined, without career and without family, if a bitter regret sprung up within his heart he repressed it, and appealing to a vigorous and unsubdued spirit, he put forth, in order to retrieve himself, a courage worthy of his ancestors.

The crafty Phrygian slave, in ancient comedy, might exclaim: *Buy your master.* M. de Blainville, though not indisposed to the same course, judged it more prudent to comply with the tendencies of his age. Chance had conducted him to the course of physics which Lefevre Gineau was then holding at the College of France; and here was revealed to him a new charm, that of serious application. He had presented himself to the professor as a modest neophyte, but soon made himself sufficiently appreciated to be admitted into a house where the associates of M. Gineau, all connected with the highest class of instruction, were accustomed to assemble. It was in this circle of eminent men that, for the first time, he recognized his vocation. Nothing harmonized better with his tastes and turn of mind than the authority of the chair and the dogmatic tone of the master. The commanding influence which superiority of knowledge exercises over men appeared to him the most enviable of attainments. He believed that he had discovered the path which would one day conduct him to distinction. From this moment persevering and ardent labor absorbed all his powers. Submitting to judicious counsels, he entered, by a scrutinizing analysis of the human organization, upon a career of original research, and made such extraordinary efforts and rapid progress that after two years passed in the amphitheatres and hospitals, he proved himself a not unworthy competitor even of Bichat, by a remarkable disquisition on experimental and comparative physiology.*

The report of this transformation of character, which must have been a matter of no little surprise, and perhaps chagrin, to the noble and gay companions of his early youth, penetrated at length into the paternal manor-house, where the eldest of the family of De Blainville still resided. "Do you know what has become of your younger brother?" said one day a communicative traveller. "Nothing good, I suppose." "Let me tell you, then, that he is in a path which will lead to great renown." "Impossible!" exclaimed the feudal Norman; "he never had the least inclination for employment of any sort."

The range of his earliest labors, his address, his birth, the singularity of his outset, caused this new adept of science to be remarked from the first. In pursuing all the branches of instruction at the museum he met everywhere with generous sympathy; and it was in this great and first school of modern natural history that were developed, during years of profound study, the pre-eminent faculties of an intelligence destined to mark its passage by force of meditation, boldness of views, and tenacity in controversy.

He first attached himself to zoology, and to this he gave a distinctive character. Especially is this character observable in what he has left us respecting the *mollusks* and the *zoophytes.*† When he began to occupy himself with these two groups of beings, all the principal divisions had already been established, the type had been definitely determined, the classes formed, these classes divided into orders; but there remained the genera, a labor which required peculiar sagacity, and in this De Blainville excelled.‡ His conceptions of the genera were such as Linnæus had entertained; nor is this the sole parallel which I discover between himself and that naturalist of so rare a cast. These

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* This disquisition, which was his thesis, bore the following title: ***Propositions** extracted from an Essay on Respiration, followed by some experiments on the influence of the eighth pair of nerves on respiration, presented and maintained at the School of Medicine of Paris, August* 30, 1808.

† His researches on the *mollusks* are his best labors in practical zoology. His *Manual of Malacology* forms an eminent work in anatomy, physiology, and especially analytical classification. This work, undertaken in 1814 for the *Supplement of the British Encyclopedia*, was not published till 1825. Several fragments of it had appeared in the *Dictionaire des Sciences Naturelles*, and the article *Conchology* of that compilation is reproduced in the Supplement, with numerous additions. The article *Mollusks* is also given, with extensive developments and new monographs. "I have drawn much," says M. de Blainville, "upon the work of Lamarck for the number and distribution of living shells, and upon that of Defrance for fossil shells. I think," he adds, very judiciously, "that the species have been generally too much multiplied. We may sometimes derive benefit from these approximations of identical or analogous fossil species, although, as I intentionally repeat, we ought not to place an unlimited confidence in them. In all parts of the natural sciences, what is laid down to-day is almost always susceptible of being modified to-morrow." (He had adopted as a general device of his writings, *Dies diem docet*—day teaches day.) He thus recapitulates the spirit of his book: "It has been my object to show that the classification of molluscous animals may very well accord with that of shells, and that consequently their simultaneous study must have an influence on that of each of them."

The *Manual of Actinology or Zoophytology* is also an important work, but must be ranked after the former. It is the reproduction of the article *Zoophytes* of the Dictionary of Natural Sciences, but much improved. "The plan I have followed," he says, "is the same with that which I had adopted for my *Manual of Malacology;* I have stated, in distinct chapters, the generalities pertaining to the organization, physiology, and natural history of all the animals heretofore confounded under the name of *zoophytes*. * * * * * I have had in view to cite all the *genera* which have been proposed, in order to supply the *lacunæ* which might exist in the Dictionary of Natural Sciences, which is not a proof, however, that we adopt them all. * * * *." The last phrase is, by its turn, characteristic of the manner of M. de Blainville.

‡ Here de Blainville had two peculiar merits—merits which also distinguished Linnæus—that of marking the true character of each *genus*, and that of ranging the *genera*, one in relation to the others, agreeably to an analytical view. See in another note what I shall say of the series of beings.

two are perhaps the only methodical writers whose fire is not extinguished in the treatment of details. Linnæus gives life to those details by inventiveness of expression; De Blainville animates them in another manner, by making them the vehicle of his preconceived and impassioned ideas.*

From zoology De Blainville passed without delay to comparative anatomy. In these galleries, then so new, everything recalled to him the profound admiration which he had felt when, confounded in the crowd, he had for the first time heard the eloquent voice of the inspired restorer of the ancient science of Aristotle.† But this admiration itself awaked all his critical instincts, and already the daring resolution was formed within him of some day venturing upon opposition. While he was thus musing upon the grounds of dissent and independence, the penetrating regards of the man of genius had more than once rested upon him. Cuvier coveted for science such proselytes; he sought them out, welcomed them, opened to them his house and library, gave them a share in his affections, and all in the utmost good faith, so long as they remained satellites of his renown; but when, once become strong, they ventured to contest *the part of the lion*, the alliance was broken.

One day, De Blainville, absorbed in meditation, saw Cuvier approach him—the great Cuvier, then at the apogee of his brilliant career. "I have a proposal to make to you," said the man of science to him whom labor alone had as yet designated to him, and whom he addressed for the first time. "Are you disposed to unite your efforts with mine in the completion of a great work on comparative anatomy with which I have been occupied for a long time? You shall have a share in my success: we shall aid one another." Tempted by the gratification which a man of merit feels at being appreciated, appreciated, too, by a superior intelligence, De Blainville promptly accepted the offer of collaboration. No sooner, however, was he thus established in the first rank among the disciples, many of them already celebrated, who lent their efforts to the execution of works whose projection belonged exclusively to the master, than M. de Blainville, who could never bear even the shadow of subordination, gave place in his bosom to the feelings of a jealous susceptibility. He took umbrage, complained with acrimony, and was heard with indulgence, even with kindness; for much should be pardoned to him who merits much. But from the time that the right of censure was conceded, the intractable disciple established it on so wide a base that M. Cuvier used to say pleasantly: "Ask M. de Blainville his opinion on any subject whatever, or even simply say to him *good day*, and his reply will be, 'No.'"

Compelled to a state of permanent warfare, Cuvier at least knew how to profit by it; it discovered to him all the exposed points of his doctrines; all were promptly seized upon by a watchful antagonist, who, in these attacks, seemed charged with the office of those priests of antiquity who daily repeated to kings, in the midst of their grandeur, *Forget not that you are men.* In requital of services so gratuitously rendered, the master, at once judicious and adroit, neglected nothing to promote the interests of this singular collaborator. After having for ten years fulfilled a course at the athenæum, he asked the succession for De Blainville; selected him to supply his appointments at the College of France and the Museum; and when the faculty of sciences was to choose a professor of anatomy and zoology, took care to environ him as a candidate with all the means of success. M. de Blainville was nominated, and, with independence thus secured, acquired an absolute liberty of opposition which he used by no means sparingly.

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* Since he proceeds from ideas to facts, each new detail found is necessarily, as regards the preconceived idea which guides him, a peril or a proof; there is no room for indifference.

† M. de Blainville himself cheerfully acknowleged that the brilliant success of Cuvier as a professor had greatly contributed to the impulse which directed all his own energies towards natural history.

He had made no mistake in the choice of a vocation; it was in the chair of the professor especially that De Blainville succeeded in giving lustre to his scientific career. He possessed in the highest degree that ready affluence of ideas, that animated turn of expression, that authoritative tone, which at once overmaster and allure the hearer. He preferred to a calm and judicious caution in sowing the seeds of knowledge, the forms of a bold and imposing logic. To some young and inflammable heads he thus succeeded in communicating an ardent sympathy for the disciple who reared himself in contradiction to a great master; and yet that master was Cuvier, in whose fame the youth of France so justly exulted, but in whom they now sought, with a certain malice, to assail the superiority of the savant, forgetful of the claims of a noble and independent simplicity.

Such successes on the part of De Blainville were not calculated to render relations between the two more complacent. At the close of a sojourn in England, De Blainville returned enriched with scientific materials, and Cuvier, supposing his just supremacy to be still respected, asked to have them communicated to him. The traveller contented himself with saying: "In order that they may be more readily at your disposal, I am going to publish them." Thus all things portended a rupture, a pretext for which, with so unconformable a nature as that of De Blainville, could not long be wanting. Cuvier might regret the opposition of an original and powerful intellect, but he knew at least how to derive from it the advantages of contradiction. As for De Blainville, he deprived himself of the benefit of intimate contact with an exalted mind, endowed with every quality requisite for counselling and directing: right reason, luminous tranquillity of thought, and that *good sense* which is the real sovereign and final judge of everything in this world.

In the rudest shocks of life, the energetic man whose character I am considering seemed to find in labor renewed strength. His cotemporaries could not but wonder at the vigor infused into his studies: profound researches, bold discussions, exhaustive historical retrospects,* nothing, in fine, seemed to weary the indefatigable elasticity of this ardent and active mind. In 1822 he published the first volume of a general treatise on *comparative anatomy,*† and with this work a new doctrine made its appearance. Cuvier had just reared the science on the experimental method, which proceeds from facts to ideas. In the efforts of De Blainville this order was reversed, and all his labors were based upon the opposite method.

His first care was to form an abstract type of the living being. Buffon had said: "We can distinguish in the animal economy two parts, of which the first acts perpetually, without any interruption, and the second acts only at intervals. The action of the heart and lungs appears to be that first part; the action of the senses and the movement of the body and members seem to be the second." This view became the principle of Bichat's celebrated distinction of two lives—organic life and animal life. Buffon proceeds to say: "If we clothe the interior part with a suitable envelope—that is to say, if we give to it senses and members—the animal life will presently manifest itself, and the more senses, members, and other exterior parts the envelope contains, the more complete will

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* The Dictionary of Natural Sciences contains a great number of very considerable articles by M. de Blainville, who was at the same time one of the most active collaborators of the *Bulletin de la Société Philomathique.* It may be added, and with literal truth, that during the whole militant life of our savant there appeared nothing on natural history which did not undergo on his part a sort of adverse discussion. Placed by the legacy of his friend M. de Lamétherie at the head of the Journal of Physics, he furnished, from 1818 to 1822, a series of historical *resumés*, in reading which one cannot fail to be struck with the extent and variety of the knowledge displayed.

† This volume, the only one he published, is entitled "*De l'Organization des Animaux, ou Principes d'Anatomie Comparée,*" and is occupied with the study of the *skin* and *apparatus of the senses* in all the classes.

the animal life appear, and the more perfect will be the animal." M. de Blainville combines the two ideas of Buffon. In effect, there are, in life itself, two lives, the life of nutrition and that of sensation. Of the general envelope, Buffon saw but the exterior part, the seat of the sensations; M. de Blainville sees this envelope continued, turned inward, penetrating into the interior, and becoming there the seat of the respiratory and digestive functions. And, as there are two lives, so are there two grand systems of apparatus, the vascular and the nervous apparatus; and on these two depend all the organs: on the first the organs of sense and of motion; on the second the organs of secretion and nutrition.

The abstract type of the living being once established, a new frame-work is furnished to M. de Blainville wherein all the details of comparative anatomy—details almost infinite in number—become classified and concentrated. The different structures appear only as realized instances of one first conception. The dogmatic process is substituted for the experimental, and M. de Blainville, having impressed the science with the form of his own genius and originality, might also consider himself a master, and a great master.*

So many and such strenuous labors had long since marked out for M. de Blainville a place in the Academy; he was called to it in 1825.† In 1830, a royal ordinance having divided into two that part of the instruction of the Museum devoted to the demonstration of invertebrate animals, he was naturally designated, from his admirable labors on the mollusks and zoophytes, for the occupancy of one of the chairs. Thus, though late in his application to the sciences, he had acquired the best position which they can confer, and saw the destiny accomplished which he had traced for himself when, in a moment of spleen, he had said to Cuvier: "I shall take my seat one day at the Institute and the Museum, beside you, in face of you, and in spite of you." The last phrase was an injustice, for it assumed an animosity which did not exist; but it would have been to diminish the enjoyment to have ceased to believe in it: experience had simply proved to Cuvier the difficulty of their relations, and had made him distrustful of them.

M. de Blainville had now arrived at that age when a man of superior intellect feels the necessity of connecting his collective ideas by some philosophic bond. His long studies on zoology had led him to see in the animal kingdom only a *continuous series* of beings, which, becoming at each stage more animated, more sensitive, more intelligent,‡ ascend from the most inferior animals up to man; an elevated view, which was that of Aristotle in antiquity, as it was that of Leibnitz in modern times. "The continuity of gradations," says Aristotle, "conceals the limits which separate beings, and withdraws from the eye the point which divides them." "I love maxims which are self-supporting," said Leibnitz; and we know that, to have such maxims, he had conceived the idea of reducing them all to one. His philosophy has but one principle, that of *continuity*. Each being, in the globe that we inhabit, is connected with all others, and that globe itself with all globes. "With M. Leibnitz," said Fontenelle, "one would have seen either the end of things, or that they have no end."

Never has a scientific idea experienced more vicissitudes than that of the *scale of beings*. All the naturalists of the eighteenth century admit it. "The progression of nature is effected by insensible shadings," says Buffon. "Nature makes

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* It is to be regretted that this production, conceived with so much vigor, should have remained incomplete. It would be difficult to find a mind suited for the continuation of the work, and capable of reducing the whole of comparative anatomy to the dogmatic form.

† He had been presented as early as 1814, and even at that early period with just title, to replace M. Olivier.

‡ This idea of series was with him always predominant. See especially his remarkable *Prodrome d'une Nouvelle Distribution Systématique du Règne Animal*, 1816; his article on the word *Animal*, in the *Supplément du Dictionaire des Sciences Naturelles*, published in 1840. and his great work *Osteographie Comparée*.

no leaps," exclaims Linnæus. Bonnet exhausts himself in well-meant efforts to find everywhere equivocal species with which to supply vacancies. At length Cuvier appears, and all idea of *continuity*, of *sequence*, is excluded. The animal kingdom is distributed into groups; definite, circumscribed, profoundly separated, without connexion, without transition. Cuvier is followed by M. de Blainville; and with him the *series of beings* reappears, and now at least with more development and completeness,* more nearly demonstrated throughout, and, what is here the last step, essentially connected with the doctrine, every day better understood and more respected, of *final causes*.†

The chain of beings thus linked together and adapted one to another evidently implies a fixed design, a consistent plan, an end foreseen. Final causes are the highest philosophic expression of our sciences, and at the same time the most cheering; it is a pleasure of a high order to discover and contemplate that wonderful assemblage of so many different forms and forces combined in proportions so just. The spectacle of an infinite wisdom diffuses calm over the human spirit. "It is no small thing," said Leibnitz, "to be content with God and with the universe."

In 1832 a severe blow was sustained by science; Cuvier was too soon lost to us. The administration of the Museum decided to transfer M. de Blainville to the chair in which the modern Aristotle had achieved immortality. From that time, it was in the close neighborhood of the collections, due to a half century of inappreciable labors, that M. de Blainville, a vigilant and almost jealous guardian, pitched his tent; it was a true tent, an abode worthy of our savants of the middle age, where he reproduced both their long meditations and their exhaustless enthusiasm.

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* In order properly to understand M. de Blainville in his different labors, regard must everywhere be had to the profound influence exercised upon him by M. Cuvier. The proof of this influence will be found even in this question of the *animal series*, which is one of those on which he has most constantly opposed him.

M. Cuvier, taking the nervous system as a guide, had established four principal divisions of the animal kingdom—the *vertebrata*, the *mollusca*, the *articulata*, and the *radiata* or *zoophytes*. It is on the nervous system also that M. de Blainville constructs his theory, only he separates the last division of M. Cuvier, that of the radiata, into two, which gives him five divisions instead of four—the *osteozoa*, which answer to the *vertebrata;* the *entomozoa*, which correspond to the *articulata;* the *malacozoa* to the *mollusks;* the *actinozoa* and the *amorphozoa*, which represent the *radiata*. Such are the five grand types of the animal kingdom, and it is easy to perceive how upon these is established the ascending series or scale. Mounting by successive steps from the *amorphozoa* to the *osteozoa*, he passes to the consideration of this latter great type, and instead of the four classes—*mammals*, *birds*, *reptiles*, and *fishes*—he subdivides it into seven—*mammals*, *birds*, *pterodactyls*, (a lost class of reptiles,) *reptiles*, *icthyosauri*, (another lost class of reptiles,) *amphibia*, (the batrachians of Cuvier,) and *fishes*. Here also it may easily be seen how the ascending scale is developed; it remounts from *fishes* to *amphibia*, from *amphibia* to *icthyosauri*, from these to *reptiles*, from *reptiles* to *pterodactyls*, from the latter to *birds*, and from *birds* to *mammals*. The class of *mammals* is divided into three sub-classes—*monodelphs*, *didelphs*, and *ornithodelphs;* and here again the same ascending gradation is seen, from *ornithodelphs* to *didelphs*, and from these to *monodelphs*. Without entering into further details, it will be seen from what has been stated how M. de Blainville modifies, and almost always multiplies, by subdividing, the groups of M. Cuvier; how he connects, while intercalating in his *scale*, the lost with the living species; how he applies to the groups themselves, to types, to classes, to orders, &c., the ideas of series, gradation, ascent, which had till then been more particularly applied to species. His scale is, in the first place, the scale of groups; but he does not stop there. Just as in the entire kingdom there is the series of principal groups or *types*, there is in each type the series of *classes*, in each class the series of *orders*, in each order the series of *genera*, in each genus the series of *species*. It is a succession of series superposed in line, always ascending and always direct.

† "The conception of *final causes*," says M. de Blainville, "leads rigorously and necessarily to the demonstration of a Being whose intelligence is infinite, and enables us to discern, not only for each created being in itself, but for each group of beings, and in the whole assemblage of beings, a plan, a necessary harmony, and within the preconceived limits." * * * * * (Article: *Animal* of the *Suppl. du Dict. des Sciences Naturelles.*)

Passing his life in a sombre apartment, buried in the depths of a vast armchair, encompassed with a triple rampart of heaps of books, original drawings, anatomical preparations and disordered instruments, if sometimes a studious disciple obtained admittance to him, it was necessary to surmount more than one obstacle, and not less difficult to find a chair than a place for it when found. If at length, after this difficult installation, reference to some volume became necessary in the heat of research, it must be drawn generally from the base of a mountain of books, whose displacement was not the less chaotic and tumultuous for being often repeated. Did an adventurous visitor, after much solicitation, obtain access to the inviolable asylum, when as yet he was scarcely more than on the threshold, and without a sense of his presence being manifested by any movement, a grave and sonorous voice would address to him the invariable question: *What is needed for your service, Monsieur?* The stranger, sometimes, disconcerted by the apparently inextricable confusion of the labyrinth before him, or aware too late of the inconvenience imposed on a profound thinker by the derangement of his ideas, would seek safety in a hasty retreat. But if the first expressions of the visitor disclosed a personage worthy of a learned conference, M. de Blainville, at once raising his head and divesting himself of the thoughts in which he was absorbed, would employ all the advantages which his facile elocution placed at the service of a vast fund of knowledge, and the auditor, charmed by so much courtesy, might expose himself, by prolonging his visit, to the danger that after his departure the laborious savant should once more repeat the phrase: *Another hour lost.* Was it a former pupil, on the other hand, who came to clear up some questionable point, he might with confidence surmount every barrier and count on the most cordial reception; for, if M. de Blainville exacted from his disciples a species of feudal *fidelity and homage,* he at least requited it by an affection which was little less than paternal.

It was from this sanctuary of study that, after having been long held in reserve, there issued one day, in full panoply, like Minerva from the brain of Jupiter, the emphatic contradiction of all the arguments on which Cuvier had founded the new science of *palæontology.*

The first germ of this surprising science of *lost existences* rested on the old belief of a general and ancient deluge. In vain did the scholastic philosophy pretend that fossil shells were only *sports of nature;* in vain did the philosopher Voltaire, who, for very unphilosophic reasons, would not admit, on any terms, that there had been a deluge, send forth his pilgrims to seek for an explanation of the dispersion of marine shells: neither *sports of nature* nor pilgrims availed anything. Sustained by the evidence of the fact and by ineffaceable tradition, the common sense of mankind asserted its right of dissent.

In the seventeenth century, attention, which had been excited by the *fossil shells,* was transferred to the *gigantic bones* preserved in the bowels of the earth, and whose origin was not involved in less obscurity. In 1696 some bones of the elephant were discovered in the principality of Gotha. The Grand Duke called together his council of savants, and the council pronounced, with unanimity, that these were *sports of nature.* About the same time were found in the province of Dauphiny some bones of the animal which we now call the *mastodon.* A surgeon of the country buys these bones and has them brought to Paris, where he exhibits them for money, affirming in his advertisement that they were taken from a sepulchre thirty feet long, and that they are the remains of a giant, a king of one of the tribes of barbarians who were defeated near the Rhone by Marius. All Paris was eager to see this trophy of the glory of Marius; and, agreeably to its almost constant usage, Paris, after having at first believed all that was told it, presently made a mock of all that it had at first believed.

With the eighteenth century comes at last a serious study of the subject. Gmelin and Pallas bring to our knowledge the fossil bones of Siberia; they inform us that these bones are found there in prodigious quantity, comprising those of the rhinoceros, the elephant, and gigantic ruminants. Who shall be the fortunate interpreter of these strange facts? Gmelin and Pallas conclude that a vast irruption of the sea, from the southeast, could alone have transported into the regions of the north these extraordinary relics, which all pertain to animals of the south. Buffon, now almost an octogenarian, conceives, with more penetrating insight, the idea of *lost species.* "The bones preserved in the bosom of the earth are witnesses," he says, "as authentic as unexceptionable, of the past existence of different colossal species of all the races now in existence." And with eloquent emotion, he adds: "It is with regret that I quit these precious monuments of ancient nature, which my advanced age does not leave me time to examine. This study of beings which have disappeared would alone require more time than remains for me to live, and I can only recommend it to posterity. Others," he continues, "will come after me * * *;" and the prophecy has been fulfilled. To the honor of our age, Cuvier creates for himself a new art; he touches these scattered remains, and recalls before our astonished eyes the *extinct races* of the earth.

He interrogates each stratum of the earth, and each yields him a peculiar population. He finds first the crustacea, the mollusks, the fishes; then reptiles, then mammals, but mammals of which the race no longer exists: the races which exist to-day he finds only on the present surface of the earth. It follows that life is developed only gradually, progressively; and the admirable theory of the *succession of beings* arises and offers itself as the surest deduction from the best-established observations. There have been, according to Cuvier, repeated but *partial and successive* creations: these multiple populations have gone on improving at the same time that they were diversified; and for the sudden disappearance of so many species at once, nothing less could have been necessary than violent and abrupt causes.

M. de Blainville takes up each of these propositions, one after the other, and contests them all.* He adopts a single and simultaneous creation; a first and

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* The following four propositions, whose elements are drawn from his great work on *Osteography*, form a comprehensive summary of the ideas of M. de Blainville on *paleontology:* First, a creation, single and consequently complete; secondly, that creation, complete at the moment when it proceeded from the hand of God, becomes afterwards incomplete in proportion as species perish, for each race becoming extinct leaves a gap; thirdly, causes the most natural, the most simple, the action of man, &c., have sufficed to destroy the extinct races, as they still suffice to destroy before our eyes the living races; fourthly, there is therefore no need, in order to explain these continuous destructions, of having recourse to general and extraordinary revolutions, to *cataclysms.*

Proposition 1. There has been but one creation. "We may find here," says M. de Blainville, with reference to the *manatee*, "a new proof that the fossil species, whose analogues we no longer recognize, are but extinct terms of the animal series produced by the thought of the creative power, and by no means, as has been too often said, and is still repeated every day, the remains of an ancient creation, which has given place to a new and more perfect one—an assertion easy to make, but incapable of being sustained by any legitimate proof in favor of so rash an opinion." (*Manatus*, p. 128.) In speaking of *palæotherium*, he says: "Although none of these species have been found alive, we are yet forced to conclude that it is impossible to admit with certain naturalists that they can be considered as a primitive form of some existing species which are but a transformation of them, and still less that these have replaced them in consequence of a new creation, as many say, without good reasons it is true, since we have shown that they fill an actual chasm in the intelligible series created by divine power for an intelligent purpose." (*Palæotheriums*, p. 183.) With reference to two or three fossil species of the *rhinoceros*, he says: "There are two or three links of the animal series which have been destroyed before other congeners, existing still in less inhabited parts of the ancient continent, and which can in no manner be considered as transformations of the former, and still less as the product of a new creation, as it is at present somewhat the fashion to suppose for each stratum of the sedimentary formations." *Rhinoceros*, p. 222.)

Proposition 2. This single creation, at first complete, presents at present vacancies which extinct species supply. "These mammals," says M. de Blainville, alluding to certain complete population, subject to incessant extinctions; and for this continuous destruction he requires nothing but slow and ordinary causes. You pretend, he exclaims, that at each of your supposed revolutions the great Author of created beings has recommenced his work! But observe first the general resemblance which allies the living with the lost species; notwithstanding all your sagacity, you have not succeeded in distinguishing, by any certain criterion, the fossil elephant from the present elephant of the Indies.* You are forced to acknowledge that, among animal fossils, there are many found which differ in nothing from living animals.† The facts on which you found your theory are therefore insufficient and incomplete; and incomplete facts cannot be prescribed as a limit to our conjectures.

In default of complete facts of which he, no more than Cuvier, is possessed, M. de Blainville seeks a higher reason which may supply its place and deliver his impatient spirit from the pain of hesitation. This higher reason seems to him to consist in the *unity* of the kingdom of nature; and here science is indebted to him for an important step in advance. So long as he had confined himself to the study of present species, the *animal series* had everywhere presented to him gaps and vacuities; everywhere beings were found wanting. At this

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species of ***smaller bears*** pertaining to the same orders, the same families, and to the same Linnæan genera with those which still live on the earth, "are not, however, always of like species; but they fill in an admirable manner the gaps which the living animal series at present offers." (***Sub-ursus.***) "As a definite conclusion," says M. de Blainville, "we find in the *dinotherias*, which seem to have disappeared at a very early period from the surface of the earth, a step, a term in that animal series which religious philosophy, the only true and good one, unavoidably accepts, but which science demonstrates the more easily in proportion as the question is judiciously considered, and a greater number of elements can be employed." (*Dinotherium*, p. 61.)

Proposition 3. The extinct races have perished through natural causes, which are still acting every day. "The largest species are those which have first disappeared; and we may even now observe that the same thing is taking place under our eyes in regard to the species still existing on the surface of the earth." (***Sub-ursus***, p. 116.) "The rhinoceros is in the condition of the elephants, which, because of their great size and their biennial uni-parturition, perish earlier—that is, first among terrestrial animals—as a consequence especially of the multiplication of the human species upon the earth." (*Rhinoceros*, p. 221.) He says of some species of fossil *viverræ:* "These species have disappeared, as we see disappear at present, by little and little, the genet, the civet, and the ichneumon, though half domesticated." (*Viverra*, p. 94.)

Proposition 4. There has not since the creation of living beings been any general and extraordinary revolution of the globe. M. de Blainville says, in speaking of bears: "A single species of this genus has ceased to exist, a species which in Europe completed the genus, as it is complete in Asia and America; a feebler species, and inhabiting the part of Europe most anciently civilized, and at the same time, perhaps, the most populous, which must have hastened its disappearance from the number of beings at present in existence; so that the state of things in relation to this genus would demand no cataclysm, no change in the present conditions of existence of the earth, but only incessant progress in the development of the human species in Europe." (***Bears***, p. 88.) "The bones of fossil ***small bears*** might have been carried, whether united or separated, and often already broken, with materials of different nature borne by the atmospheric waters into the places of deposit, where we now find some of them by hazard, without there having been required catastrophe or change in the ambient mediums to determine their destruction." (***Sub-ursus***, p. 115.)

* The ***fossil elephant*** of M. Cuvier—the ***mammoth*** of Russia—is, according to M. de Blainville, only the present elephant of Asia. "The definite result, to which we are conducted by a rigorous logic, is that in the actual state of our collections, at least at the Museum of Paris, it is still nearly impossible to prove that the fossil elephant, of which so many remains are found in the earth, differs specifically from the still existing elephant of India."

† "There are some doubtful species, which will affect more or less the certainty of results so long as precise distinctions shall not have been reached respecting them. Thus, the horses, the buffaloes, which are found with the elephants, have not as yet peculiar specific characters; and the geologists who do not choose to adopt my different epochs for the fossil bones will still be able to derive from them an argument so much the more convenient as it is from my book they will take it." (Cuvier. *Discourse on the Revolutions of the Surface of the Globe.*)

point, by a happy inspiration of genius, he discerns in nature which has perished the beings which are wanting in nature as it exists,* and, with surprising skill, he intercalates among living species the fossil species; thus asserting, and, first among naturalists, discovering to us the *unity of the animal kingdom.* That kingdom is therefore one, and the unity of that kingdom seems the first demonstrated point in the *unity of creation.*

Having thus stated the opposite opinions of the two authors, let us turn to their methods, which will be found not less opposed. Cuvier follows facts, alike resolved to wait for them however slowly they may arrive, and to accept the result which they yield him, whatever it may be: whether the theory of *successive creations,* if species continue to be found everywhere separated and superposed, or the theory of a *single and simultaneous creation,* should it be found eventually that they occur anywhere intermingled and confounded. M. de Blainville assumes a great fact, which he transforms into a principle: the fact of the *unity of the animal kingdom,* and from that unity he boldly deduces the unity of creation. Thus there is, on one side, always the experimental method, with its process sure and its results uncertain; on the other, always the dogmatic method, with its result presented as certain, but obtained by a process which is not sure.

The human mind in making use of methods and judging them has this quality of excellence, that it finds no repose except in the full and entire knowledge of things. It is this restless seeking for truth, a movement impressed upon the mind by a divine impulse, which constitutes its force in labor and its joy in discovery. In the new study which we have been considering, a multitude of facts, I mean necessary facts, are still wanting to us. We have explored but a part of the surface of the globe. There are places where, in reference to so grave a discussion, nature may well be surprised at not having been interrogated. There will arise intrepid explorers who will lay open unknown regions. There will arise new thinkers. The noble science of Cuvier and De Blainville—for, from the very opposition of ideas, the two names will remain united—has reached that elevated point at which it is able to propound with precision the problem upon which it is divided; and this problem of the *successive* or *simultaneous* order of created beings is surely, in the domain of natural history, one of the grandest which the genius of men has ever conceived.

Absorbed in contemplations of so high a nature, M. de Blainville became less and less disposed to comply with those relations of amenity which render life easy. To excuse himself to his own conscience, he attributed to rigidity of principle what was at best but error of judgment. He was now in possession of the substantial *privileges* of success; but this did not diminish his pretensions. He brought them all into this Academy, in spite of the admonition given us by Fontenelle: "Here it was intended that everything should be simple; that no one should think himself under an obligation *to be in the right;* that no system should govern, and that the door should always remain open for truth." To one who had but too well learned in the professor's chair the full value of the *law of the strongest,* this privilege of *being in the right* appeared intolerable when it no longer applied to himself alone. In replies marked by a tone of peremptory authority, M. de Blainville was apt to forget that he had descended from the chair, and that here all the seats are equal. "Doubtless," as was said by the sagacious historian just cited in speaking of one of his colleagues, "the search for truth demands in the Academy liberty of contradiction; but all society

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* Nothing in the book of M. de Blainville is at the same time more ingenious and true than this remark, namely, that the more *lacunæ* a group of mammifers presents, the more vacancies between its living species, so much the greater is the number of fossil species which it counts. The actual *pachyderms* afford only scattering species, and there are many fossil *pachyderms.* The *monkeys,* on the other hand, present numerous and closely crowded species, and there are few fossil *monkeys,* &c.

exacts in contradiction a certain deference, and he did not recollect that the Academy is a society. We did not cease to discern his merit through his manner, but for this some little effort of equity was required, and that effort it is always better to spare mankind the trouble of making."

Not that these "efforts of equity" were wanting in the case of M. de Blainville, any more than the terror with which, by his fierce attacks and stubborn disputativeness, he had succeeded in inspiring the most hardy academicians. He seemed at the last to have adopted an extreme resolution; and,

——————— As if he had designed
To break all terms of commerce with mankind,

he withdrew from our reunions, and, in the spirit of Moliere's Alcestis, who yearned to find—

Some nook of earth, if earth such nook can give,
Where honest candor might have leave to live,

he fairly barricaded himself, as has been shown, in the depths of his cabinet.

He had undertaken to give, in a great work on *comparative osteography*,* the description and demonstration of the collections confided to him, and superintended, with characteristic severity of attention, the drawings which none could better judge of than himself. This enterprise involved enormous expense, and had every claim to the encouragement which authority everywhere extends to vast and important publications. It was but simple justice that the work should be placed under the patronage of the government. But to obtain this, it would have been necessary to make suit, or, at least, submit his claims, and never was misanthrope more singularly bent on preserving all the prerogatives of an intractable humor. Taking high ground, and with reason, in regard to the value of the author and the work, he assumed that his wishes should have been anticipated and his acceptance solicited; for, over and above the *hatred* which he had vowed to the *human race*, he endowed with a superior and privileged degree of irritation all that bore the guise of authority, and that by which we were then governed chafing him in his prepossessions as a gentleman, he could not be brought to condescend so far as to honor it with a request. He suffered of course, complained bitterly, and had the satisfaction of accusing all the world: colleges, Academy, ministry, government, all were culpable, all except himself, who would abate no jot of his punctiliousness, and thereby only succeeded in dispelling all possibility of finishing his learned and gigantic catalogue.

This man, whose captious spirit took fire at the very appearance of a favor conferred by power, and whose antecedents by no means announced a conciliator, employed himself, about this time, in a work of the most delicate conciliation. Under the title of a *History of the Sciences of Organization, adopted as a Basis of Philosophy*,† he published, in 1845, a work whose object, he said, was the alliance of philosophy and religion.

Always led away by preconceived views, he carries into history the same method as into philosophy. He constitutes types: Aristotle is the type of the natural sciences in antiquity, Albert the Great in the middle ages, and, in our

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* The title of this work is: *Osteography, or a Comparative Iconographic Description of the Skeleton and Dental System of the Five Classes of Vertebrate Animals, Recent and Fossil, to Serve as a Basis for Zoology and Geology.* 1839–'50. It is from the ideas scattered in different parts of this great work, incomplete as it unfortunately is, that I have derived the *paleontological* doctrine of M. de Blainville; for he had not the same good fortune with M. Cuvier, of collecting in a single *discours* the sum of his researches and views. Death surprised him before he had finished his task; and to reproduce now the doctrine which he labored with so much fearlessness and ardor, we have but scattered elements, often left incompletein unfinished pages.

† In this work M. l'Abbé Manfried co-operated with him, and it is scarcely necessary to say that my remarks only apply to that part of the book pertaining to M. de Blainville.

own days, M. de Lamarck. He nearly suppresses all the rest of naturalists, and, in his impassioned delineations, fails to remember that history is a judge, and that the first duty of a judge is impartiality. Nor is he less rash as a diplomatist than historian: seeking the first principles of his philosophy in Lamarck, Gall, and Broussais, whom he calls the three great philosophers of our age, and thus encumbered with no light baggage of materialism, he ventures into uncertain paths, and misses the only sure one, which Bossuet had followed in his immortal treatise of the *Knowledge of God and of ourselves.* But it was labor and time lost. The science of organization cannot be the basis of philosophy. The domains are separated. What we now call philosophy, what Descartes called, by a more precise term, metaphysics, has but one object, profoundly circumscribed—the study of the soul.

As an analytical appreciation of the progress of the human mind in the natural sciences, the book of M. de Blainville had been preceded by one of M. Cuvier on the same subject,* a production slowly matured and of a calmer spirit. In comparing the latter work with the other, one is involuntarily reminded of the well-known line:

My phlegm's as philosophic as your spleen.

A wide interval separates the penetrating sagacity which detects the weak side in the ideas of others from the deliberate reflection which sits in judgment on its own. Too impatient to subject his theories to a severe analysis, but too prudent to leave them exposed to attacks which might incur danger, M. de Blainville made use of stratagem: he carried the war among his adversaries, and, allowing them neither peace nor truce, compelled them to hold themselves always on the defensive.

The necessity of success, an implacable tyrant, in him inspired by turns the stubborn disputant and the fascinating professor; and it was because in the latter character success was certain that in entering upon the functions of the master, not only did he put forth all his intellectual superiority, but he displayed likewise his better moral qualities: the confidence of being useful, the hope of being loved, the charm of appreciation, removed then all the asperities of the surface. The sentiment of recognized pre-eminence sufficed to dispel all roughness and pretension; and confident of his strength, nor yet affecting any concealment of his efforts, he gained much by being seen in this light. One day, at the exit from a lecture, a former scholar drew near in order to congratulate him on the happy manner in which he had just treated an important question. "I am glad that you are satisfied," rejoined M. de Blainville; "the subject was difficult, and for eight days I have meditated upon this lecture from nine o'clock in the morning until midnight." This avowal discovers a strict conscientiousness, for no one ever possessed more than he the gift of brilliant improvisation. He has been known, after an hour and a half occupied in rich and animated lecturing, on being excited by some objection, to begin anew to discourse and argue, with closed doors, regaining at once all his strength and resources, conceding nothing, and remaining always the last champion in the field.

An ardor like this for disputation subjected to singular vicissitudes friendships which certainly ran no risk of growing languid through dull acquiescence. The faithful associate,† the sage Pylades of this impetuous Orestes, once said to me: "For nearly half a century that our intimacy has lasted, it has been rather cherished and cemented by discussion than by perfect agreement." In effect, if M. de Blainville obtained, sooner than suited him, a triumph for the thesis

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* I speak of the reproduction of the lectures of M. Cuvier at the College of France, published under the title of *Histoire des Sciences Naturelle Depuis leur Origine Jusqu'à nos Jours.*

† Our learned colleague, M. Constant Prevost, who pronounced at the tomb of his fiend a discourse full of the sensibility which is inspired by profound affection.

which he was supporting, he would presently take in hand the opposite thesis. But what, it would be impatiently asked, what then is decidedly your opinion? Is it *yes?* No, it is not *yes.* Is it *no?* I have just proved to you that it can not be *no.* It must needs, however, be one or the other; decide. *Oh ho!* he would exclaim, *you forget, then, that I am a Norman.* And in him everything, physical as well as moral, recalled that origin. He was of medium stature, but of a remarkable vigor. His eye, lively, penetrative, observant, revealed a superior nature. The simplicity of his exterior denoted his confidence in a personal value which chose to borrow nothing from honorary distinctions, distinctions for which he manifested a plenary indifference. No ostentation, no petty vanity sunk this man to a lower level. He seemed to have settled in his own mind that by study alone can life be invested with dignity or value.*

Under all its envelopes, and however seemingly impenetrable, the heart, when once touched, was but the more unreserved in its effusions. Become possessor of the small manorial domain of his ancestors, M. de Blainville returned yearly to visit its shores and hills, to breathe the invigorating air of the sea, and to recall the tender images which had soothed his earlier years. During the time that he thus occupied his little manor, the man of science disappeared, and the gentleman showed himself no cynic. An unaffected amiability accompanied him into society, and in that of ladies especially he displayed a playfulness and good taste which banished into the distant and lowering horizon of science every misanthropic impulse. His delight in the revival of associations found other aliment in the reunion of the representatives of all the epochs of his life. Frequently assembled around him on such occasions, this circle of friends was open to all philosophies, to the most opposite opinions, to all social positions, to every age. For the youngest among them the severe critic and profound thinker ever entertained a warm and watchful regard, which, it is but just to say, was requited, not only by unlimited devotion, but, since his death, by a pious care for the memory of the distinguished savant.†

At the beginning of the year 1850, M. de Blainville thought it his duty, notwithstanding the alteration of his health, to open his course at the Faculty of sciences. In his first lectures, he reappeared with a talent which had lost nothing of its force or brilliancy. Impelled, however, by sad presentiments, he quitted, on the 1st of May, his modest habitation at the Museum, promising a speedy return. He was but going, he said, to breathe his native air, and see the sun of spring once more shine on the fair coasts of Normandy.‡

His purpose was not fulfilled. Scarcely had he taken his place in the vehicle which was to convey him, when, by a sudden stroke, this noble existence was terminated. On the public authority it devolved to extend that protection to his last moments which it owes to the humblest citizen, and to restore to his friends and colleagues the mortal remains of one so worthy of respect, one by whom the nothingness of life had never been forgotten.

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* I scarcely need say that M. de Blainville belonged to most of the learned societies of the world, and of course to the Royal Society of London. He was a member of the Legion of Honor, and if he remained a simple *chevalier* it was only because he preferred it.

† All the manuscripts of the great naturalist were scrupulously collected by his young friend M. Nicard, who prepared, also, a *notice* breathing a spirit of enthusiastic veneration.

‡ This love of his native place was one of the prominent features of his character, as was also the love of his family. The son and daughter of his elder brother were long the only ties of kindredship which remained to him.

REPORT ON THE TRANSACTIONS

OF THE

SOCIETY OF PHYSICS AND NATURAL HISTORY OF GENEVA,

FROM JULY, 1863, TO JUNE, 1864.

BY DR. CHOSSAT, PRESIDENT.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION.

In conformity with article 7 of our laws, I am about to present to the society an account of its transactions and its progress during the year in which I have had the honor of being called to preside over it. The society, I regret to say, publishes no special bulletin of its sittings; the annual report of the president, at the close of his official term, is intended to supply its place. This, however, it can only do imperfectly, because, from the necessarily tardy date of its publication, some of the results communicated must have partially lost the character of finality. However this may be, the greater portion of the labors of the year have been successively inserted either in the present volume of memoirs, or in the archives of the physical and natural sciences of the *Bibliothèque Universelle;* so that my task to-day will be limited to a concise recapitulation of those labors.

Since the end of September last, the society has been deprived of the special collaboration of our excellent colleague, Professor Claparède, whose infirm health has obliged him to withdraw from the functions of secretary of our sittings—functions which for many years he discharged in so distinguished a manner. His place has been filled, provisionally at first, by MM. Alexander Prevost and de Loriol; and definitively since, by M. Alexander Prevost alone, whom you designated for this office 21st January last. Such has been the obligingness of these gentlemen, and so clear and detailed their report of the current proceedings of the society, that the execution, always more or less difficult, of my present duty has been facilitated to the utmost possible degree; and I shall be permitted, I am sure, to present to them, as well in the name of the society as my own, the most sincere acknowledgments.

Agreeably to the usage adopted in former reports, the present account will be divided into two principal parts: that of the physical and that of the natural sciences; parts which will be then subdivided into as many special sections as the nature of the communications made to the society may prescribe. In each of these sections we shall speak, first, of the original memoirs which have been read; and next, say something of the verbal reports which have been made to the society during the year. We commence with astronomy.

PHYSICAL SCIENCES.

Astronomy—*Memoirs.*—M. Emile Gautier read two memoirs on the constitution of the sun. With M. Kirchhoff, he regards this body as a globe in fusion, incandescent, and surrounded with a vast atmosphere; he conceives that this atmosphere, constituted chiefly of thick and metallic vapors, must be incomparably more dense than the terrestrial atmosphere, and he attributes to it the protuberances and roseate border observed in total eclipses, as well as the dappled appearance of the surface of the sun. But he denies to it the action attributed by M. Kirchhoff in the production of the solar spots.

As regards these spots, M. Gautier considers them to be connected with oxydations, with masses of salts and scoriæ, with solidifications, in a word, which are temporarily formed on the surface of the sun; and this, under the influence of exterior refrigeration or interior chemical action, in much the same manner as is observed in great masses of metals in fusion, in our industrial operations. The author admits the results of M. Spœrer and those of M. Carrington regarding the apparent differences in the duration of the rotation of the sun, according to the heliographic latitude of such of the spots as are adopted for the calculation of this rotation. And in reference to the acceleration in longitude of the points of the equatorial zone, he considers it, with our colleague M. Cellerier, as resulting from the action exerted upon the solidified masses floating on the surface of the sun, whether by the friction of the heavy and metallic atmosphere of that orb, or by the interior rotary movement of the strata of its mass in fusion. These two components being both a function of the velocity and of the cosine of the latitude, (though this cosine has a different power for each of the two components,) they vary with each parallel, and their resultant may supply the reason of the acceleration in question. For the rest, although the author does not consider the mean density of the sun to be inferior to that of water, yet he admits that its low degree of density might form a serious objection to his theory of the spots, an objection which will be met on his part by a deliberate and thorough examination.

Professor Plantamour read us an extract from a very interesting memoir on the horary and telegraphic operations, by means of which the longitude of the observatory of Neuchâtel has been connected with that of the observatory of Geneva. These operations have led to some new results on the employment of the telegraph in determinations of this nature. But, as the paper of M. Plantamour appears in the present volume of the collection of our memoirs, we confine ourselves to a simple mention of it. The same physicist presented a note on the rectifications to be applied to the general system of levelling for Switzerland, and on the choice to be made, as a point of departure above the sea, between the mean level of the Mediterranean at Marseilles and the mean level of the ocean, as the latter results from very exact measurements executed in nineteen of the principal ports of France between Bayonne and Dunkirk. M. Plantamour would prefer the level of the ocean, the mean of which is $0.^{m}80$ higher than the mean level of the Mediterranean at Marseilles.

Verbal reports.—Professor Alfred Gautier presented statements, full of interest, on several astronomical labors executed in different observatories of Europe and America. These statements bore more particularly, firstly, on the spots of the sun, their reciprocal occultation observed at Altona, and their presumed connexion with the aurora borealis and magnetic variations; likewise, on the two periods, one of eleven and the other of fifty-six years, recognized by M. Wolf in the number of these spots; secondly, on the photometric researches of M. Alvan Clarke, relative to the intensity of the light of the sun compared with that of the fixed stars; whence it would result that our sun cannot be one of the brilliant stars of the heavens; thirdly, on a slight augmentation to be applied to the value of the parallax of Mars as hitherto recognized, an augmentation which would imply that our distance from the sun is a little less than that at present admitted; fourthly, on the observation of shooting-stars by P. Secchi, from which it results that these meteors are situated at a height of 100 to 150 kilometres, and hence that they are within the limits of our atmosphere; fifthly, on the lines of the solar spectrum, which, at an elevation of four miles above the ground, remain identical with those on the surface of the earth, only the spectrum diminishes in extent in proportion to the elevation attained; sixthly, on the discovery of a satellite of Procyon; on the light of η of the Ship, which, in the space of twenty years, has passed from the first to the sixth magnitude; and finally, on a deviation of 10″ of the plumb-line in the environs of Moscow, at a distance from any description of mountain.

METEOROLOGY.—*Memoirs.*—The great and valuable labors of our future president, Professor Plantamour, on the climate of Geneva, pertain by peculiar right to our society, and would have occupied a distinguished place in the collection of our memoirs, if their extent had not compelled the author to have them printed and published separately. This work, one of true scientific importance, inasmuch as it is based on observations executed with improved instruments, and repeated every day and several hours of each day for thirty-five consecutive years, and finally discussed with all the resources of modern science, could not be analyzed in a report necessarily so much circumscribed as the present; besides that, it would now be the more useless to attempt such analysis, since Professor Aug. de la Rive has recently given, in the archives of the physics and natural sciences of the *Bibliothèque Universelle*, a detailed and highly interesting account of the enterprise.

Verbal reports.—Professor Plantamour communicated some of the results obtained during the month of January last, at the meteorological stations of the valley of the higher Rhone. Some singular anomalies of temperature have there been realized. Thus, among others, it was found that it was colder at the village of Rechingen (valley of Conches) than at the *hospice* of Saint Bernard, though the latter is situated 1,140 metres higher than the village. These anomalies may be explained sometimes by the presence or absence of the sun, and sometimes also by the cold air flowing from the mountains and accumulating gradually in the bosom of the narrow valleys. Professor Marcet informed us of the results of M. Glaisher on the diminution of the temperature of the air in proportion to the elevation attained—results gathered in England and by means of balloon ascensions. The diminution is not regular, most probably from circumstances purely accidental, such as momentary currents of cold air, or enormous strata of vapor, which arrest the solar heat and reflect it toward the higher spaces. After traversing cold mists of some thousands of feet in thickness, M. Glaisher found at 11 or 12,000 feet of elevation the same temperature as at the surface of the earth. Professor Wartmann reports, in relation to atmospheric electricity on high mountains, that it had been observed this year in an ascension of the Jungfrau, as had been done the year before on the Diablerets, that at the approach of a storm the ironed staves of the tourists commenced *intonating*, and that singular sounds were heard in the air. Professor Gautier spoke of torrents of rain having fallen in Italy, in February last, accompanied, at Rome, by a furious hurricane, which transported thither sand entirely similar to that of the desert of Sahara. Professor Marcet remarked upon the relatively very mild temperature of the winter of 1863–'64, in Canada, a fact which navigators believe they account for by a change observed, as they suppose, in the direction of the Gulf Stream. M. Chaix read to us a report on the results of late travels in Arabia, and particularly those of Palgrave, who succeeded in traversing the country by passing for a Syrian.

MATHEMATICAL AND EXPERIMENTAL PHYSICS.—*Memoirs.*—M. Ch. Galopin read an extract of a memoir on the mathematical theory of double refraction. After reciting the principles on which rests the theory of Fresnel, and having indicated the process followed by that eminent physicist for applying analysis to transcendental researches, our colleague adopts as his own the views of Cauchy, who regards the movement of light as a particular case of the movement of a system of molecules, very slightly diverted from their position or equilibrium, and solicited by mutual forces of attraction and repulsion; and he gives the differential equations, whose integration would furnish the value of the molecular displacements. By a method peculiar to himself, M. Galopin, with the help of certain artifices of analysis, arrives at the equation of the velocities of luminous waves already given by Fresnel, an equation which may be regarded as representing a surface called that of elasticity, and from which he deduces the equation of the surface itself of these waves. Besides the two surfaces in question, there still exist six others, the study of which enables him to arrive rapidly at the properties of the surface of the waves, at those which concern the peculiar points and planes of that surface, the conic and cylindrical refractions, and finally the ordinary and extraordinary rays of crystals of two axes.

M. Lucien de la Rive has made researches on the differences of density of a gaseous mass revolving around an axis in a cylindrical vessel, and has arrived at a formula which enables him to calculate these different densities. The differences in question are little appreciable for volumes of gas and dimensions of vessels of inconsiderable quantity; but they increase in proportion as the diameter of the vessels is larger. The last named physicist read a memoir on the conductibility of ice for heat. After having given the detail of his experiments, he recapitulates them mathematically, and proceeds to deduce to the value of the co-efficient k, of the conductibility of ice, a co-efficient which he finds $=0.25$, that of glass being 0.13, and that of porcelain 0.24. Then applying his results to the formation of ice on a surface of water below 0°, he seeks for the law according to which this formation takes place, and he arrives at three equations corresponding to three different epochs of the formation in question. Now, as the last of these equations is that of a parabola, he thence deduces that after the lapse of quite a few days of frost, the ice can only increase very slowly. The author finally establishes the agreement of his theory with known facts, with the observations of Flauguergues particularly, and concludes by indicating briefly the application which may be made of it to the formation of the polar ices.

Verbal reports.—Professors Wartmann and Marcet, in several successive verbal communications, brought to the notice of the society the interesting discussion in progress between MM. Magnus and Tyndall, on the absorption of heat by gases. M. Tyndall, in repeating his experiments without employing the diaphragm of rock-salt, has removed one of the most serious objections of M. Magnus. From his results he derives consequences of importance for the theory of Wells on dew, and for other atmospheric phenomena. Professor Marcet made the additional remark that Dulong, in his investigation regarding the specific heat of gases, had set out with the hypothesis that gases do not radiate; now, since M. Tyndall has established that they do radiate, the results of Dulong would seem to call for revision, account being taken of the radiation. Professor Plantamour occupied our attention with the views of M. Hipp respecting the establishment of electric clocks in cities. Their employment at Geneva has greatly conduced to a determination of the conditions by which their disposal should be governed. Their position in gas-lanterns exposes them to great variations of temperature from summer to winter, to the unfavorable influences of dust and humidity, and to the disturbing effects of the discharges of atmospheric electricity and of concussions produced, whether by gusts of wind or the frequent washing of the lanterns—concussions which have sometimes caused temporary loss of the current. By reason of these difficulties, M. Hipp would prefer that such clocks should be placed in the wall of the fronts of houses rather than in the gas-lanterns. M. Philip Plantamour presented an analysis of the researches of M. Edlüng on the formation of ice in the northern seas. According to the latter, the sea begins to freeze from the bottom; the water being there cooled below the point of congelation, the least shock, the passage of a fish for instance, suffices to determine solidification and to produce the sudden formation of masses of ice more or less considerable, which rise and float on the surface of the water. The analysis of M. Plantamour has been inserted in the Archives of the *Bibliothèque Universelle.*

CHEMISTRY.—*Memoirs.*—Professor Marignac communicated to the society the continuation of his researches on the silico-tungstates. He has recognized three distinct acids formed by the combination of tungstic acid and silicic acid, namely: 1st. Silico-tungstic acid, containing 12 equivalents of tungstic acid for 1 of silicic acid; 2d. Silico-decitungstic acid, 10 equivalents of tungstic acid for 1 of silicic acid; 3d. Tungsto-silicic acid, which has the same composition as the first, but which differs by its crystalline form. He remarks that a great number of salts of these acids present crystalline forms almost identical, although not by any means so in their composition. This fact seems to him to indicate the necessity of admitting the following extension of Mitscherlich's principle of isomorphism, viz: that two compounds including an element or a group of common elements, which constitutes by much the greater part of their weight, *may be isomorphous,* even when the elements in which they differ do not constitute by themselves an isomorphous group. M. Delafontaine read a memoir on the atomic weight of thorine or thorium. He has repeated the analysis of the sulphate of thorium after the method of Berzelius. The mean of several accordant results yielded him for the equivalent of thorine the figure 823.3, and admitting that the formula of this is Tho^2, the weight of its atom referred to oxygen would be 1,646.6, and that of thorium 1,446.6. To the same author we owe a note on the place which thallium should occupy among the elements. Several chemists place it among the alkaline metals, while others consider it as being related to lead. Of these two views our colleague adopts the former.

Verbal reports.—M. Clusius has modified his theory on the atomic composition of ozone. It is not this body, it would seem, but oxygen which is formed of atoms grouped two and two—atoms which are dissociated when oxygen passes into the state of ozone. But it is objected to this new theory that ozone having more density than oxygen, it is the former, not oxygen, which must be composed of grouped atoms.

NATURAL SCIENCES.

GEOLOGY.—*Memoirs.*—M. Favre has continued his communications on the geological constitution of the Chablais. The soil of this province is composed of new formations superposed in the following order: the glacial, fucoid schists, kimmeridgian limestone, collovian limestone, liasian, lower lias, triassic, coal, and serpentine. One of the characters of the region is the absence of cretaceous and nummulitic formations; an absence which results probably from the soil in question having been already elevated above the surface of the water at the epoch of the cretaceous and nummulitic seas. The author afterwards presents a geological description of that part of Savoy traversed by the valleys of Mégève and of Haut-Luce. Among other formations he there recognizes the black slates of the jurassic period, forming the crest of Mont Joli; a fine deposit of vegetable fossils of the carboniferous era, near Bonhomme; and near Beaufort deposits of anthracite. He also shows that the granitic group of Mont Blanc is separated from that of Beaufort by sedimentary rocks, a continuation of those of the valley of Chamounix, and thus the granite of Beaufort would seem to be a prolongation of that of Valorsina.

M. Favre also gave an account of an investigation in which he is engaged of the deposits of translation between Jura and the Alps. These deposits present four principal stages: 1st, the present alluvium; 2d, the alluvium of the terraces, deposited by great currents of water above the glacial formation, and at a maximum elevation of 30 to 33 metres above the lake; 3d, the glacial deposit, composed of loam, of rolled pebbles, and of some erratic blocks; 4th, the old alluvium, composed of facilites (pudding-stone) and of rolled pebbles. Among these, the euphotide, found below Geneva, comes from the mountains of the Valais, a considerable distance doubtless, and the translation of which, by reason of the interposition of the lake, it is not easy to explain.

M. Loriol read to us a memoir on the nummulitic formation of Egypt. To eight species of echinoderms already known in the nummulitic of that country he adds four others entirely new. He also communicated a series of researches on the classic mountain of Saléve. The fauna of the coralline stratum which forms the base of that mountain has furnished some new fossils, among others a large species of Diceras. The deposits between the corralline and the middle neocomian belong to the valengian stratum, as their fossils (*Natica leviathan*) testify. The Urgonian stratum offers three species of invertebrata in common with the deposits of Orgon, without speaking of several new species, and in particular of a fine terebratula (*T. Ebrodunensis,*) which has not yet been published, and which has been compounded with the T. semistriata. Finally, in this Valengian stratum, M. Loriol has distinguished four new species of brachiopods.

Verbal reports.—Professor A. de la Rive called the attention of the society to the researches of M. Frankland on the physical cause of the glacial epoch. This cause he finds in the generally admitted fact, that the ocean must, at the precipitated epoch, have had a temperature much superior to that which it now has; that hence the evaporation of the seas would have been considerably augmented, and with it the aqueous precipitations of the atmosphere. Now, these enormous precipitations, falling in the form of snow, and during millenary periods, on the elevated table-lands of the high latitudes of the globe, would eventually occasion the vast accumulations of ice which characterized the epoch in question. In support of this theory, M. de la Rive added, that since 1815 he had observed the great extension which the glaciers of Switzerland had acquired after the two rainy years of 1816 and 1817. Further, that other savants had already announced ideas upon the glacial epoch in close analogy with those of M. Frankland. This communication of Professor de la Rive has been inserted in the *Bibliothèque Universelle.*

Professor Desor communicated some of the results of his late researches on the lacustrian deposits of lake Neuchatel. He has studied two stations at Auvergnier; one of the age of stone, situated near the shore, at a depression of about five feet below the mean level of the water; the other of the age of bronze, which is found somewhat in front of the other and at a greater depth. He supposes that the stations of the age of stone are the remains of artificial islands formed of pebbles heaped around stakes planted in the bottom of the lake.

M. de Heer announces the discovery of the wing of an insect of the genus blatina in the anthracites of the Valais, near St. Maurice. This insect, found under fossil plants of the epoch of coal, is a near neighbor of those of the coal series. Professor Pictet has continued to give us information both of the facts relative to the discovery of the jawbone of Moulin-Quignon and of the different scientific inquiries which bear upon that discovery. These inquiries have been successively published in the scientific and literary journals of the epoch; and as they are too numerous for us to give here even a simple enumeration of them, we must be content with a reference to the journals themselves. The last-named naturalist spoke also of the discovery of a tooth of a gigantic crocodilian in the oolite of Poitiers; according to the savants who have examined it, this animal appears to have been about one hundred feet in length. Finally, M. Alexandre Prevost, our secretary, showed us a fragment of a human skull, found in the valley of Chamounix, immediately above the Aiguilles by which the glacier of Bossons terminates in that valley. This fragment is doubtless a relic of one of the three guides who perished on the great plateau during the expedition of Dr. Hummel in 1820. Other bones, of the same origin, have been found last spring and still more recently in the same localities.

BOTANY.—*Memoirs.*—Dr. Muller read a memorandum of the monstrosities which he had met with in the flower and fruit of the jatropha pohliana, and deduced therefrom some conclusions on the theory of the anther. He thinks that this is formed neither by the combination of two ordinary leaves, nor by a leaf whose edges are incurvated towards the median rib so as to form the two chambers of the pollen. He believes that the anther represents only a simple leaf, and that the pollen is developed in the incrassated tissue of the parenchyma of this leaf. The anthers heretofore recognized are of 1, 2, 4, and 8 chambers, and according to the commonly received theory, the existence of trilocular anthers would be an impossibility. Now, in confirmation of his own theory of the antherian leaf, M. Muller read a note on the existence of trilocular anthers in the species pachystema of the family of the euphorbiacæ, (Java). The same colleague afterwards presented a notice of two modes of inflection of the stamens in the euphorbiaceæ. The only one of these two modes which is noteworthy is that in which the anther is inflected in the bud, its summit below and its base above. This form of inflection is important, inasmuch as it serves to characterize the great tribe of the crotoneæ.

Verbal reports.—Professor de Candolle, in his *Botanic Geography*, has remarked that the beech and chestnut have not been discovered in Algeria. Now, Professor Martius has recently found chestnut trees in the forest of l'Edding, in the neighborhood of a Roman aqueduct. M. de Candolle conceives that in such a locality the chestnut may well have been introduced by the Romans, and he persists in thinking that the tree in question does not exist in the Atlas. He also noticed a memoir of Dr. Hooker on the arctic flora, a memoir in which that savant seeks to explain why certain regions of the north possess a very rich flora, (Lapland,) while others have an extremely poor one, (Greenland.) M. Hooker thinks that after the glacial epoch the vegetable species, in proportion as the ice withdrew, would ascend into the arctic regions, when those regions were continental, while in the regions which became insular the sea would oppose the reascension of vegetables. The same naturalist presented the society with grains of the indigenous coffee of Peru; these grains are more voluminous than those of the coffee of Asia, but it is not yet possible to determine their species. He exhibited also the male flower of a begoniacean of Africa, very different from the usual type of its family, for which he had been indebted to the kindness of Dr. Hooker. Lastly, M. Reuter presented to the society the leaf, fruit, and part of the flower of the *tormelia fragrans*, which is the first time that the fruit of this aroid of Mexico has been seen at Geneva.

ZOOLOGY—*Memoirs.*—M. Henri de Saussure read a paper on the incessant dispersion of the hymenoptera on the surface of the globe, a dispersion which would have for its apparent consequence a successive modification of individuals, and consequently the development of series of *graduated species*, marking the stages traversed by the migrations of each type. This hypothesis would explain the parallel series which may be observed on the same continent or on different continents. Among these successive modifications, one of the most interesting is that which pertains only to one of the two sexes, the feminine. In the genus Elis, for instance, we can distinguish as many as twenty varieties, spread over all the continents, varieties extremely distinct from one another in regard to the female, but of which the males seem identical or nearly so, which would constitute a series of types successively polygamous. The author concludes by indicating certain species which may have passed from Europe to America, and from America into Africa or Europe. M. Alois Humbert read a memoir, in which he showed, by means of mollusks which he had brought from Ceylon, that in the pulmonate gastropods there exists no essential difference between those with an external and those with an internal shell, and that it is possible to establish all the requisite transitions between the two extremes of this family, extremes which may be represented by the slug, (internal shell,) and the snail, (external shell.) M. Victor Fatio communicated a memoir on the vertical distribution of species in certain families of birds. Leaving the basin of Lake Leman with twenty-four different species of sylviadæ, he loses some one of these species in proportion as he ascends the mountains; and when arrived at the Haute-Engadine finds himself accompanied only by redstarts, one of which alone, the *ruticilla tithys,* ascends still higher. In this comparative study, the author first establishes an approximation between the north pole and our higher Alps, which leads him to signalize the relations which exist, for birds, between their horizontal passages and vertical migrations. Next, passing to this transport of species to heights more and more considerable, he recognizes the influence which climate and the nature of the soil exercise on the production of nourishment.

Verbal reports.—M. V. Fatio took notice of the appearance at Geneva of a bird, the *syoraptes paradoxus,* which inhabits Siberia, Tartary, and China, and which entered Europe in 1863, directing its course from the northeast of Germany to the southwest of France. An extraordinary drought in its native country has probably been the cause of this unusual migration of the bird in question.

ANATOMY AND PHYSIOLOGY—*Memoirs.*—Professor Thury communicated his important memoir on the law of the production of sexes. In the case of plants, the fundamental identity of the pistils and stamens is admitted by those botanists who regard the organs in question as modified leaves. Now, according to the experiments of Knight, heat favoring the production of male flowers in diœcious plants, M. Thury has thence concluded that the caloric acted on plants by occasioning a more complete elaboration and maturation of the juices and organs, so that the production of the male element would correspond with a more perfect development of the germ. Applying these ideas to the animal kingdom, our colleague has deduced from them the consequence, that the production of one or the other sex depends only on the degree of maturation of the egg, a maturation which would continue to advance during the time which elapses between the moment of the detachment of the ovum and that of its impregnation, in such manner that the ova promptly fecundated would yield females, and those more slowly fecundated males. Such is the filiation of ideas by which M. Thury has arrived at the theory to which his name will remain attached; a theory which was confirmed by experiment in each of the twenty-nine cases in which, at the instance of its author, trial was made by M. Cornay. These ideas of our colleague have excited very general interest; they were immediately submitted to investigation in France, in England, and in Germany, and this on so large a scale that we may hope to arrive promptly at results altogether decisive. Certain objections, however, have been raised against this theory, of which we shall here notice only such as have been advanced at our own sittings. Thus, for instance, in regard to hemp, which grows in very different climates, it has been said that nothing has heretofore indicated that more male individuals are produced in the hemp of warm countries than in that of cold ones. Again, M. Pagenstecher has sought to demonstrate that the theory of M. Thury was in opposition with observations on the parthenogenesis, and he substitutes for it another theory, in which account is equally taken of the age of the egg at the moment of its fecundation. In effect, at the session of the Helvetic society of natural sciences in 1863, he expressed the idea that, by maturation, the pellicle of the ovule became hardened, which might prevent a more or less considerable number of zoospermes from penetrating into the interior of the egg, and thus influencing its sexuality. Some of these objections, it must be confessed, rest only upon hypotheses and need demonstration in order to obtain assent.

Professor Claparède read a memoir on the circulation of the blood in arachnidæ of the genus lycosa. The examination of this circulation conducts the author to a very unexpected result: that in these animals the blood, in almost the whole of the heart, moves from front to rear, contrary to what takes place in all the arthropods hitherto studied. The memoir of M. Claparède having been published in the previous volume of our collection dispenses with the necessity of entering into the further details of this interesting investigation, which, besides, it would be difficult to understand without plates. The same naturalist presented another memoir in which he sets forth the result of his interesting researches made at Port Vendres, during the summer of 1863, on the anatomy and classification of marine annelidæ. He first occupies himself with a type which has not been studied heretofore except in a manner probably very imperfect, that of the polyophthalmæ, which forms among the chetopods an intermediate link between the oligochetæ and the polychetæ. He next examines the annelidæ, degraded from the family of the terebellacæ, in which the disappearance of the vascular system is accompanied by the formation in the general cavtiy of the animal, of a liquid holding in suspension red globules, very similar to the corpuscles of the blood of mammifers, and thrown into continual movement to and fro by the contractions of the walls of the body. Lastly, he passes to the examination of the family of syllidæ, of which the species present some the normal and others the alternating generation. Among more than twenty species pertaining to this family, and found by him at Port Vendres, one species only was already known. This memoir, accompanied with plates, is inserted in the present volume of memoirs of the society.

M. Victor Fatio read a memoir on the reproductive male apparatus of the accentor alpinus, one of the pretty sparrows of our Alps. In the spring, at the approach of rutting time, its testicles acquire an enormous development, attaining a volume of about one-third of that of the entire trunk. Their different vessels, instead of opening directly into the cloaca, are wound upon themselves, and form on the sides of the anus two large balls which hang beneath the tail in pouches covered by the skin. On issuing from these balls, the vessels in question are directed towards the common vestibule, and terminate at the extremity of a small sexual papilla. In autumn all this temporary development disappears. Doctor Dor read a memoir on the physiological effects of the bean of Calabar, (physostigma venenosa.) Studied specially in its effects on the eye, this substance produces contraction of the pupil, and occasions a sort of cramp of the accommodator muscle. In this double relation it acts as an antagonist of the atropima. Professor Valentin presented a note on the stretching of the motor nerves. This stretching, by producing elongation of the fibrillous sheath of Schwan, must narrow the diameter, and thus occasion, *probably*, a certain degree of compression of the nervous pulp which it envelopes.* M. Valentin has sought to measure the effects of the stretching in question, and that by help of a measuring apparatus at once rotary and graphic, and of a slight current of induction, as a means of excitation of the nerves examined. The same physiologist made a communication on the effects of the separation of the motor nerves from the nervous centres. He has ascertained: 1st, that a discontinuity of one demi-millimetre suffices to produce all the effects resulting from the separation in question, which excludes the idea that the nervous force can act at a distance, as in induction; 2d, that when the two separated ends of the nerve are examined with the microscope, it is found that the peripheric portion presents only degenerated fibres, while the end attached to the nervous centres is composed only of fibres perfectly normal. For the great sympathetic the effects are different, on account of the anastomoses of the peripheric end.

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* We say *probably*, because if the elongation of the nervous substance is proportional to that of the sheath which envelops it, there will not be compression, but a stretching only of the nervous pulp enveloped.

Verbal reports.—There have been some interesting communications to the society, on physiological researches conducted by foreign savants, namely: By Professor de la Rive, on the experiments of M. Scouttetten relative to the elasticity of the blood, and to the current which, through the galvanometer, passes from the arterial to the venous blood; and this as well in the living animal as in the blood freshly drawn from the vessels. By Professor Claparède, on the process of M. Wandt for measuring the rapidity with which thought may be transferred from an impression of the sense of hearing to an impression of the sense of sight, (about ⅛ of a second.) The personal equation of the observer ought, evidently, to be here taken into consideration. On the animal grafts of Dr. Best; besides the subcutaneous grafts of different members effected with success, this physiologist, in the case of two animals of the same species, *but of different sex*, has succeeded in engrafting the genital organs of one into the abdominal cavity of the other. He has also united two rats by the skin of the lateral part of the body, and has thus succeeded in establishing between them a complete vascular communication. Although these graftings have succeeded between animals of the same species, they have failed between those of different species. Again—and with this I shall end—Professor Claparède detailed to us the last results at which, each for himself, MM. Knoch, in Russia, Leuckart, in Germany, and the late Bertholus, in France, have arrived in their valuable researches on the evolution of the bothryocephalus. In this worm, common enough in Russia, Poland, the south of France and Geneva, the eggs are developed in water, and at the end of seven months give birth to embryos furnished with remarkably long cilia. These embryos continue to live freely in the water, and there produce a larvæ armed with hooks very similar to that of the tænias. Now, what still remains to be discovered is, whether it is sufficient to swallow water containing the larvæ of the bothryocephalus in order to introduce this worm into the human body; or whether, as with other tænias, the passage through an intermediate animal is necessary for effecting its ultimate evolution. The question being reduced to this degree of simplicity, its solution may be easily attempted, and we may hope will be attained before the lapse of any long interval.

Such is the analysis, complete enough, I think, but still summary and dry, of the different labors which have occupied us during the year with which my official term closes. It remains to say a few words on—

THE PERSONNEL AND PROGRESS OF THE SOCIETY.—I had hoped, till quite a recent period, to have been enabled, notwithstanding the somewhat precarious state of the public health during the year 1863, to felicitate you that no vacancy had occurred in the ranks of our society; but those anticipated felicitations have been changed into sincere regrets by the death of M. Wartmann, senior, at the age of 71 years. Louis François Wartmann was born at Geneva, January 6, 1793; and was, from the first, destined by his father, who knew how to give in his own person an example of perseverance and the love of labor, to that hardy education which the difficult circumstances of the epoch demanded. Of a ready intelligence and happy aptitude for the serious occupations of the mind, it was the physical sciences towards which he felt most strongly attracted, and the study of which, under such men as Schaub, Gasp. de la Rive, M. A. Pictet, Maurice, and Gautier, definitely decided the course of his life. Endowed with an easy and agreeable elocution, and knowing how to place himself on a level with intellects of every degree, he devoted himself more particularly to instruction, and, for half a century, attained in that line well-merited success. Thus occupying a position in which he could clearly recognize the wants of our population, he soon perceived that our classical college answered but imperfectly the educational requirements of a part of our youth, and, associating himself with a few capable and devoted colleagues, he opened, July, 1831, an industrial school, which continued in successful operation till 1838, when its functions were superseded by the establishment of the present industrial and commercial college by the government.

Astronomy was, with M. Wartmann, always the science of predilection. He discovered, September 6, 1831, in all probability, one of the small telescopic planets of our system; but the complete discovery eluded him through a deficiency, at that epoch, of instruments of precision, and the consequent impossibility of determining the elements of the orbit. More recently he drew up charts of the trajectories of the comets of Halley, Enke, and Biela, and constructed, on a new plan, two large planispheres, which comprised, to the number of 2,800, all the stars from the first to the sixth magnitude, visible in Europe at a mean latitude of 45° to 47°, and calculated for the 1st of January, 1850. His attention was also directed to the shooting stars, and he published, (Corresp. Mathem. and Phys. of Quetelet, vol. xl,) on those which he had observed in the night of 10th to 11th of August, 1838, a memoir, accompanied by a chart, in which he traced the trajectories of 372 of these mysterious meteors. Having become a member of our society in 1832, he acted as its treasurer from 1834 to 1858. Besides his own researches in astronomy and meteorology, he often communicated to us extracts from his correspondence with foreign astronomers. His scientific labors will be found dispersed in the *Bibliothèque Universelle*, the *Comptes Rendus* of the Academy of Sciences of Paris, the Bulletins of the Academy of Brussels, and in certain other foreign scientific collections. But this brief notice would still be imperfect, did I not cite the admirable qualities of heart and temper of our deceased colleague—qualities which endeared him to all, and which will dispose us long to recall his courteous and prepossessing presence. Let us not forget also to commemorate the punctual observance with which it was his habit to attend each of our meetings, and to express the hope that this example of persistent assiduity may not be lost upon any of us.

I will not conclude this report without adding that the progress of the society has been satisfactory, as in the past. In effect, during the whole year, the number of memoirs, notes and verbal reports which have been read or presented to us, shows that the scientific impulse has suffered no abatement among us. Our sittings have been replete with interest, of which your zealous attendance, as witnessed by our records, is the surest exponent; and in each of our reunions the most agreeable forms of mutual kindliness and simplicity have never for a moment ceased to predominate. If to these considerations we add the steady augmentation of our resident membership, an augmentation which is for us the proof of the increasing interest which our society excites in the enlightened part of our population, you will be justified in concluding that our position is firm, our advance well sustained, and that we need only desire to see this favorable state of things maintained in the future. But for that, gentlemen, it must not be forgotten that youth imposes obligation; for it is the hour of genius and the age of great labors. Such is the reflection which an old colleague thinks it not unsuitable to present to you at the close of what he has been called upon to say touching the progress and prospects of the society.

REPORT ON THE TRANSACTIONS

OF THE

SOCIETY OF PHYSICS AND NATURAL HISTORY, OF GENEVA,

FROM JULY, 1864, TO JUNE, 1865.

BY M. E. PLANTAMOUR, PRESIDENT.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION.

According to established usage, I proceed, on quitting the presidency, to submit a report of our transactions during the year which has just elapsed. The statement which I shall read is an abridged reproduction of the proceedings of our meetings, though the chronological order has been changed for the purpose of classifying the different communications according to the subject to which they relate. In this classification I conform to the customary division between the physical and natural sciences, although the line of demarcation between these two branches of human knowledge is far from being distinctly drawn; in not a few cases, in fact, it is somewhat difficult to decide under which of the two categories certain researches, relating to physiology or geology, ought to be ranged.

PHYSICAL SCIENCES.

M. Galopin presented the result of some theoretical researches which he had made on the resistance opposed by fluids to the movement of solid bodies. He has been occupied especially with the effect which would be produced by different solids of revolution moving in the direction of their axis, and he concludes that bodies operate differently, according to their form, in modifying the density of the fluid.——— M. Achard communicated to the society a memoir, (*Bibliotheque Universelle, Archives des Sciences Phys. et Nat.*, vol. xxii,) directed to the study of the second principle of the mechanical theory of heat, discussing therein the researches of Rankine and Clusius on this subject. He also made a report on an improvement applied by M. Foucault to the regulator with centrifugal force.——— M. Lucien de la Rive read a note on an application of the calculation of probabilities. He proposes to determine the functions of time expressing the probability of the recurrence of an event a certain number of times within a given period.——— General Morin, being present at one of the meetings, made a communication on his studies relating to ventilation, and on the results obtained by the processes of ventilation established in the amphitheatres of the Conservatory of Arts and Trades, and in some of the hospitals of Paris.

Professor Gautier has continued to keep the society apprised of the researches of foreign astronomers, particularly of the discussions which are taking place in England on the constitution of the surface of the sun, on the nature of the spots, and on the appearances, likened by some to grains of rice, by others to leaves of the willow. At different sittings he analyzed the investigations of M. Wolf on the spots of the sun; of M. Webb on the changes which are effected at the surface of the moon; of M. Quetelet on the proper movement of the stars; of M. Auwers on the orbit of Sirius. He gave an account, also, of the geodesic operation undertaken by a Swedish expedition for the measurement of an arc of a degree of the meridian at Spitsbergen in the latitude of 78°, and of different inquiries into the spectral lines of the light of planets, stars, and nebulæ. He further exhibited to the society specimens of autographic telegraph despatches. some of which were accompanied with drawings obtained by the system Caselli established between Paris and Lyons.——— M. E. Gautier presented a communication on the researches of M. Howlet, relative to the constitution of the sun, and submitted to the society photographs of the sun obtained in different English observatories. These representations show the spots and feculæ with remarkable distinctness. The same associate, after his return from Rome, where Father Secchi had, with much kindness, placed the great refractor of the Roman college at his disposal, gave us an account of the observations which he made on the appearance of the surface of the sun, and of his researches on the physical constitution of that orb.

M. Plantamour presented the result of experiments which he had made with a pendulum of inversion, in order to determine, at Geneva, the length of the pendulum which beats seconds. The instrument, executed by M. Repsold, of Hamburg, was confided to him by the federal geodesic commission, and the same apparatus will successively serve to ascertain the gravitation at different points of Switzerland. He also exhibited to the society a portable sun-dial, constructed on a model analogous to those which have been established at a certain number of federal meteorological stations, when the distance from a telegraphic office or the absence of regular postal communications rendered it necessary to furnish observers with the means of obtaining the exact time. These sun-dials have been constructed by MM. Herrmann and Studer, of Berne. The apparatus exhibited to the society differs from those established permanently at the several stations by certain modifications which allow of their being adjusted at any place of which the latitude and the declination of the magnetic needle are known. For this purpose the instrument is provided with a circle, by means of which the axis may be fixed according to the elevation of the equator, and with a compass for adjusting it. The true solar hour may thus be obtained to within a fraction of a minute.

General Dufour presented a report on the attempts which have been made to construct topographical plots by the help of photography, and showed that complete ones can scarcely be obtained in this way, because salient points would necessarily mask the others.——— Professor Marcet communicated some of the results deduced by M. Glaisher from his meteorological discussion of a series of observations made for about ninety years at Greenwich. He called notice particularly to the gradual elevation of mean temperature which seems to have been manifested in the course of that period.

Professor de la Rive communicated to the society his new researches on electricity, particularly those relating to the influence exerted on the molecular constitution of bodies by the combined action of electricity and magnetism. He has resumed the experiments which he had made in 1846, on the sounds rendered by conducting bodies traversed by discontinuous currents, when they are submitted to the influence of a strong electro-magnet, and he is disposed to attribute this rupture of molecular equilibrium to an effect of orientation of the molecules, analogous to that which takes place in magnetic bodies simply traversed by a discontinuous current. He has also made experiments on the influence exerted by the vicinity of a very intense magnetic force on the arrangement of the metallic particles when these are in process of deposition at the negative electrode in the decomposition of the salts of magnetic metals. Again, the passage of the luminous jet of the Ruhmkorff apparatus across metallic vapors, produced by means of a voltaic arc, has furnished to M. de la Rive very curious and interesting results. This luminous jet assumes, in its passage through these vapors, a well-defined color, varying from one metal to another, so that this color becomes a characteristic indication of each metal. In experimenting on metallic alloys, he found that there is a decomposition of their elements in metallic vapors, such as results from the deposit of particles on the electrodes.

M. de la Rive has further occupied the attention of the society on several occasions with researches directed to the study of the relations existing between the variations indicated by magnetic instruments, variations of the atmospheric condition and the telluric currents which are manifested by putting in communication, through a telegraphic wire, two plates sunk in the earth at a great distance from each other. (*Bibliotheq. Universelle, Archives des Sci., Phys. et Nat.*, vol. xxii.) These communications were made either on the occasion of letters addressed to our colleague by Father Secchi and read to the society, or of memoirs published by the latter. It was to researches on the same subject that the note presented by M. de la Rive to the Helvetic Society of Natural Sciences, during its last session at Zurich, related, which note concluded with the proposition that the society should nominate a commission charged with making experiments on terrestrial currents, analogous to those which Father Secchi has executed in two directions in the environs of Rome. The proposition having been accepted and the commission named, authority has been obtained from the federal directory of telegraphs, which has manifested herein the utmost complaisance and interest, for making use, during a certain number of hours, of the direct wire connecting two distant stations, whether in the direction of the magnetic meridian or in the perpendicular direction. Experiments were first made this spring by Professor de la Rive and Louis Dufour on the line between Berne and Lausanne, and are to be continued. If we are authorized in assuming, in conformity with the opinion of those physicists who have been most occupied with the subject, that the regular magnetic variations consist of two periodical variations superposed on one another, having different laws and due to different immediate causes, it is evident that the efforts of science should be directed to a means of isolating the effect produced by one of the causes whose combined action is manifested on the magnetized bars. It is in the sun, according to M. de la Rive, that we must seek the origin of these two different modes of action on the terrestrial magnetism; one of them is due to the direct influence of the sun, which varies according to the distance of that body and the nature of its surface, as is proved by the correlation between the period of the magnetic variations and that of the spots of the sun; the other would seem due to the currents produced by the positive electricity of the atmosphere and the negative electricity of the earth, which tend constantly to neutralize one another in the polar regions, while the cause, which operates also constantly to separate the two electricities, resides in the sun. Now, as M. de la Rive states, it would appear most easy to isolate the terrestrial currents in order to study their variations and the phenomena to which they give rise, and it is with this view that he has proposed to make in Switzerland also, experiments on the currents derived from the great terrestrial current.

M. Louis Soret read a memoir* on researches undertaken to verify the electrolytic law in a particular case, and which completes previous labors of the same savant. This memoir was directed to a comparison of the intensity of discontinuous currents (particularly when the Ruhmkorff apparatus is introduced into the circuit) with the chemical action of those currents measured by the weight of the deposit of copper which is produced. M. Soret also called the attention of the society at different times to the more recent labors of M. Tyndall, and, among others, to the researches of the English physicist on the relations which exist between calorific and luminous radiations, and on the singular property of a

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* *Recherches sur la correlation de l'electricité dynamique et des autres forces physiques.* Memoires de la Société de Physique et d'Histoire Naturelle, tome XVIII.

solution of iodine in the sulphuret of carbon; this substance completely obstructs the passage of the light without intercepting the heat. The interesting investigations of M. Tyndall on the invisible rays of the electric light and on the calorescence were also analyzed by M. Soret.

Professor Wartmann made a report on the observations of M. Castracane, and on the employment of monochromatic light applied to the microscope. M. Castracane has availed himself of this process in his researches on living and fossil diatomeæ, of which he will soon publish an atlas, and M. Wartmann exhibited to the society photographic images of these minute organisms which the learned Italian has succeeded in obtaining. M. Wartmann also presented a report on the memoir of M. Plateau relative to a curious problem of magnetism, the possibility, namely, of maintaining a magnetic body in stable equilibrium by magnetic forces. The same problem was made the subject of remarks offered by MM. Lucien de la Rive and Cellerier. The latter examined particularly the case in which the force, instead of acting in the inverse ratio of the square of the distance, would act in the inverse ratio of the fifth power, as this occurs with bodies electrified by induction, and he proved that neither in this case could a stable equilibrium be obtained.

Professor Marignac communicated the first results of a series of researches which he has undertaken on the niobium. (*Bib. Univers., Archives*, &c., vol. xxiii.) He first examined the double fluorides which the hyponiobic fluoride forms with other metallic fluorides, and has arrived at the conclusion that the hyponiobium is not an allotropic modification of niobium, but rather an oxide of niobium. He has also recognized the association of tantalic acid with hyponiobic acid in the colombites of America, and he indicates a means of separating them. He proves, finally, that tantalic acid comprises five equivalents of oxygen, like the hyponiobic acid. Professor Marignac also read a note (*Bib. Univers., Archives*, &c., vol. xxii) on certain consequences which result from the researches of M. Karl Than, relative to the anomalous density of the vapor of sal ammoniac, and from those of M. Deville on the decomposition of water by heat. M. Marignac pointed out that the combination of two bodies frequently gives rise to a temperature much more elevated than that which is necessary in order to effect the decomposition of the compounds which they form. It results from this, that the time requisite for the accomplishment of a combination or a combustion depends on the rapidity with which the surplus of heat produced by that act is capable of being dissipated by communicating itself to the surrounding bodies; this interval might even become very long, if the question related to bodies forming a considerable mass entirely isolated in space, and unable to part with heat except by the slow process of radiation.

M. Delafontaine read to the society a memoir, in which he recites a first series of researches on the earths of the gadolinite; his experiments related to erbine, terbine, and yttria, of which oxides he has determined the atomic weight by analyses of their sulphates, and he combats the opinion pronounced by M. Popp, that erbine is one of the oxides of cerite. The same member presented a series of researches on the salts of molybdic acid, (*Bib. Univers., Archives*, &c., vol. xxiii,) and on their comparison with the tungstates; he has detected two series of salts: one of which is composed of neutral salts, and the other is parallel to the paratungstates. He exhibited to the society a fragment of meteorite, picked up after the fall of a large number of aerolites, which occurred May 14, 1864, in the environs of Montauban. The matter composing this fragment is of a porous appearance and a relatively slight density; it contains 13 per cent. of water, and 6 per cent. of a substance very similar to lignite. Another memoir which M. Delafontaine read to the society relates to his researches on the spectra of absorption, (*Bib. Univers., Archives*, &c., vol. xxii); when the rays of the solar spectrum are made to pass through a solution of certain metals, we obtain a spectrum much modified; certain bands more or less large may then be observed, according to the degree of concentration of the solution. The metals on which these experiments were made are didyme, erbium, and terbium.

Dr. W. Marcet read two memoirs: one on a colloid acid derived from urine, in which some new details were added respecting the chemical properties and atomic weight of this acid; the second memoir (*Bib. Univers., Archives*, &c., vol. xxii) had for its object the muscular dialysis, M. Marcet proving in this paper that the muscular substance is permeable for colloid as well as crystallizable substances. ——— M. Chaix offered some remarks on the accumulation of volcanoes, whether extinct or in activity, with which recent explorations have made us acquainted in certain regions of the globe, particularly in the republic of Nicaragua and in New Zealand. He also indicated, on the authority of a memoir of M. Abich, the appearance of several new islands, which have emerged in the Caspian sea, as a sequel of volcanic movements in 1857 and 1864. ——— Dr. Pitschner read an account of his ascension of Mont Blanc in 1859, during which he took occasion to make observations on different points of terrestrial physics, physiology, and zoology.

NATURAL SCIENCES.

Professor Favre presented to the society a memoir, (*Bib. Univers., Archives*, &c., vol. xxii,) in which he recapitulates, from an historical point of view, the discussion which has taken place on the subject of the coal formation of the Alps, and announces the conclusions at which he has arrived from his observations made in different parts of those mountains. The study of the chain of Mont Blanc formed the subject of two memoirs read by M. Favre; in the first, he occupies himself with the fan-shaped structure of that group, and after having discussed the different theories advanced on this subject, he concurs in the opinion pronounced by M. Lory, adding some considerations on the protogene of which the chain is formed. In the other memoir he undertakes to investigate the succession and thickness of the strata which must have covered that part of the surface of the earth before the mass of Mont Blanc made its appearance and upheaved the formations which covered it. These formations have been gradually removed by the action of atmospheric agents, and thence has resulted an enormous denudation, laying bare the protogene. M. Favre seeks to calculate the volume gauged by the formations thus removed, and he shows that the greatest elevation of the chain, before these denudations, may explain in part the greatest extension of the glaciers. Our colleague, lastly, read to the society his letter, (*Bib. Universelle, Archives*, &c., vol. xxii,) addressed to Sir R. Murchison, in which he combats the theory of the excavation of the alpine lakes and valleys by glaciers. It was the study of the lake of Geneva and of the direction of the geological strata on the two shores, whether in the eastern or western part, which furnished M. Favre with proof that the depression of the bed of the lake must have proceeded from a cause wholly different from an excavation by glaciers.

Professor Pictet presented a memoir (*Bib. Univers., Archives*, &c., vol. xxi) on the succession of gasteropod mollusks in the cretaceous lakes of the Jura and the Swiss Alps. The study of the fossils collected at Sainte Croix has enabled him to recognize in that locality the existence of nine successive faunas, independent of one another and almost without mixture, between the epoch of the lower valangian and that of the chloritic chalk of Rouen. A comparison with the cotemporaneous faunas of the neighboring countries shows that the species are there associated somewhat differently, and hence M. Pictet concludes, from analogy with what occurs in our present seas, that we cannot consider each species as characteristic of the whole of a period. It must rather be admitted, contrary to an opinion quite widely entertained, that the greater part of species have a variable signification, according to the geographic region where they are found. Professor Pictet also presented a memoir, (*Bib. Univers., Archives*, &c., vol. xxii,) by MM. d'Espine and E. Favre, in which these two young savants, who had just finished their studies at the academy of Geneva, record their researches made in certain localities in the region of the Alps, where the faunas of the lower and the upper gault are found intermingled. The localities studied by them are la Goudinière, near the Grand-Bornand, the mountain of Criou, above Samoëns, and the Wannen-Alp, in the canton of Schwytz. To this memoir, presented for the annual competition founded as a parallel to the Davy prize, the premium was awarded.

M. de Loriol read a memoir (*Memoires de la Société de Phys. et d'Hist. Nat. de Geneve*, vol. xviii,) on the infracretaceous fresh-water strata of Villers-le-Lac, (Doubs,) in which he arrives at the following conclusions: the Portlandian dolomites of the Jura are the equivalent of the *Plattenkalk* of Hanover and of the limestones *à plaquettes* of the Charente; they do not pertain to the Portlandian, and they form the base of the Purbeckian group. The fresh-water limestones and marls of Villers are the equivalent of the *Mundener Mergel* and serpulite of Hanover, as well as of the gypsiferious clays of the Charente. Again, this infracretaceous group of Villers and the Jura is the equivalent of the Purbeck beds of England, of which they represent the middle and the lower part.

M. Humbert presented a memoir (*Memoires de la Soc. de Phys. et d'Hist. Nat. de Geneve*, vol. xviii) on the myriapods of Ceylon, of which he had an opportunity of collecting a great number of individuals during his sojourn in that island. The author draws the attention of naturalists to certain organs, heretofore little studied, and which furnish important characters for the establishment of genera and species. M. de Saussure presented to the society a new number of his work on the orthoptera of Mexico. This number is devoted to the family of Blattæ, several specimens of which the author exhibited, and respecting which his researches have led him to verify some interesting facts. He points out, among others, a very singular peculiarity in the structure of the wing of a tribe of this family, to which he has given the name of diplopterians. In these insects the wing is folded into four duplicates which are exactly superposed by means of a longitudinal and a transversal crease; it is by gradual modifications that the structure of the wing deviates more and more, in the three genera established in this tribe by M. de Saussure, from the normal type of duplicature which occurs in the orthoptera.

M. Fatio exhibited to the society an apparatus, (*Bulletin de la Soc. Ornithologique Suisse*, vol. i,) to which he has given the name of oometre, constructed with a view of determining the dimensions of the eggs of birds measured in all directions, and consequently their exact form. It might serve as well for the analogous measurement of shells. The same member gave information of a colony of ash-colored herons which he has discovered on the shore of the lake of Lucerne, at the foot of Mount Pilate; during an excursion made in that locality he ascertained the existence of from 200 to 300 nests of these birds.

Professor Claparede made a report (*Bib. Univers., Archives*, &c., vol. xxii) on certain interesting results at which Dr. Fritz Müller has arrived in his studies of the crustacea of the island of St. Catharina, on the coast of Brazil. On comparing the respiratory apparatus in the families of the land crabs which are derived from those of the marine crabs, M. Müller found that the adaptation of this apparatus to aerial life is not accomplished by the same process in each family, whence the author draws a conclusion favorable to the theory of Darwin. Professor Claparede likewise presented us an analysis of the researches of Professor Wagner, at Casan, respecting certain larvæ of flies, in the interior of which small larvæ are developed, which issue forth by piercing the skin, and become like their mother; thus a series of generations of larvæ is presented, without the perfect insect having been obtained. Our colleague noticed also the discovery made by M. Leuckardt of an interesting case of alternating generation in the *Ascarides nigrovenosa.*

M. Duby presented a report on the investigations of M. Bary, of Fribourg, in Brisgau, relative to certain parasitical fungi which are observed on the leaves of the cruciferæ. M. de Bary has discovered a sexual generation among these fungi, and has observed also a true alternating generation in certain kinds; hence, he has felt authorized to refer to one and the same species, fungi, which have been heretofore classified in different species and even in different tribes. It is to one of these fungi, the *Perenospora infestans,* that M. de Bary attributes the malady of the potato.

Personnel.—Our society has, in the course of the year, sustained the loss of one of its members in ordinary, M. Pyrame-Louis Morin, whom a premature death has torn from science and his country before he had completed his fiftieth year. In rapidly sketching the scientific career of our deceased colleague I shall not pretend to offer a complete portrait of a life so usefully and honorably occupied. I shall not speak of the devoted citizen who was animated with so ardent a love for his country, and who gave proof of an enlightened patriotism through the political agitations of the last twenty-five years. I shall not speak of the pharmaceutist who bestowed upon his preparations the same care and exactness which a consummate chemist applies to the most delicate analyses. Nor shall I speak of the services rendered by Morin to the industry of our city, for even now a voice much more eloquent than mine retraces the part which he has filled in the Society of Arts, and more especially in the class of industry to which he had dedicated for many years all the time at his disposal. I shall confine myself to a sketch of his career as a man of science and a member of our society.

Born at Geneva, in March, 1815, he was placed at the age of ten years in the institute of M. Naville, at Vernier, where he pursued his early studies, till admitted, in 1832, as a pupil at the Academy of Geneva to prosecute the scientific courses then comprised in the faculty of philosophy. His taste for chemistry, which was thus developed, naturally pointed out the path which he was to follow, and he joined his uncle, M. Antoine Morin, in order to fulfil, as a student of pharmacy, the apprenticeship of his new vocation. He afterwards passed two years at the University of Zurich, where he became preparator for M. Lœwig and director of the laboratory of practical chemistry; he gave also in that city courses of chemistry applied to the arts.

He thence proceeded to Berlin, where he had the advantage of being placed under the special direction of M. Mitscherlich, and he here published his first scientific memoir, which had for its object researches on the bisulphurate of ethyle; this paper was inserted in the *Annals of Poggendorf.* He completed his practical studies in Paris, at the establishment then conducted by M. Soubeiran, and returning to Geneva in 1840 was admitted as pharmaceutist, after undergoing the examinations required at that period, embracing, as a qualifying test, an analytical disquisition on the red quinquina. At the close of that year he was received into our society, of which he was an assiduous attendant to the last, and an active participant in its labors. Among the memoirs which he published, most of which were presented to this body, are several which relate to the waters of Saxon, and to the long controversies which he was called to sustain in reference to that subject. In his second analysis, published in 1853, he had shown the intermission of iodine in that fountain, a result which was at first contested by MM. Rivier and Fellenberg, who were not slow, however, in recognizing the exactness of the facts advanced by Morin, and in conforming to his opinion. At a later date M. Ossian Henri, whose name was an authority, maintained the constant presence of the iodine, but contended that it was sometimes masked by a sulphurous principle. On this occasion Morin made new researches and a complete study of the subject, proving, among other things, that there was no sulphurous principle in the water of Saxon, and establishing irrefutably, by a series of numerous quantitative analyses, that the constant existence of iodine therein was an illusion, and its intermission a reality.

From the very titles of the memoirs published by Morin it may be seen that his researches were chiefly directed towards a practical end, and that the numerous analyses which he conducted were undertaken with a view to application rather than to theory. But that in which we recognize the chemist, conversant with the entire progress and with every demand of the science, is the exactness and care with which all these analytical researches were made. It was this tendency towards the application of science to the arts which impelled Morin to devote himself more and more to the class of industry, that section of the Society of Arts in which he found the field of activity that best suited him. Although his health had been seriously affected for more than two years, his zeal and activity were not for a moment relaxed, and it was only since the month of September last that the progress of the malady obliged him to renounce his occupations. He died 1st of December, 1864, after many months of suffering, bearing with him the regrets of his colleagues and of all who had known and could appreciate the worth of the man and the savant.

I shall recall, lastly, the different nominations which have been made in the course of the year: M. Arthur Achard has been named as member in ordinary; M. Berthelot, professor of the normal school, and General Morin, director of the conservatory of arts and trades at Paris, have been elected honorary members; and Dr. Ed. Dufresne, associate at large. In the elections which have taken place for the renewal of the bureau, at the commencement of the year, you have called to the presidency Dr. Gosse; whence it will result that, by a happy coincidence, the same year which is destined to the celebration at Geneva of the fiftieth anniversary of the Helvetic Society of Natural Sciences, will be marked in our own association by the presidency of the son of that savant to whom Switzerland is indebted for an institution whose utility is every year more highly appreciated.

THE AURORA BOREALIS, OR POLAR LIGHT:

ITS PHENOMENA AND LAWS.

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THE Aurora Polaris is a luminous appearance frequently seen near the horizon as a diffuse light like the morning twilight, whence it has received the name of aurora. In the northern hemisphere it is usually termed aurora borealis, because it is chiefly seen in the north. Similar phenomena are also seen in the southern hemisphere, where it is called the aurora australis. Each of them might with greater propriety be called aurora polaris or polar light.

The aurora exhibits an infinite variety of appearances, but they may generally be referred to one of the following classes:

1. A horizontal light, like the morning aurora or break of day. The polar light may generally be distinguished from the true dawn by its position in the heavens, since in the United States it always appears in the northern quarter. This is the most common form of aurora, but it is not an essentially distinct variety, being due to a blending of the other varieties in the distance.

2. An arch of light somewhat in the form of a rainbow. This arch frequently extends entirely across the heavens from east to west, and cuts the magnetic meridian nearly at right angles. This arch does not long remain stationary, but frequently rises and falls; and when the aurora exhibits great splendor, several parallel arches are often seen at the same time, appearing as broad belts of light stretching from the eastern to the western horizon. In the polar regions five such arches have been seen at once,* and on two occasions have been seen *nine* parallel arches separated by distinct intervals.†

3. Slender luminous beams or columns, well defined, and often of a bright light. These beams rise to various heights in the heavens, 30°, 50°, 70°, and sometimes, though rarely, they pass the zenith. Frequently they last but a few minutes; sometimes they continue a quarter of an hour, a half hour, or even a whole hour. Sometimes they remain at rest, and sometimes they have a quick lateral motion. Their light is commonly of a pale yellow, sometimes reddish, occasionally crimson, or even of blood color. Sometimes the tops of these beams are pointed, and having a waving motion, they resemble the lambent flames of half-extinguished alcohol, burning upon a broad, flat surface. Sometimes the luminous beams are interspersed with dark rays, resembling dense smoke.

4. The corona. Luminous beams sometimes shoot up simultaneously from nearly every part of the horizon, and converge to a point a little south of the zenith, forming a quivering canopy of flame, which is called the corona. The sky now resembles a fiery dome, and the crown appears to rest on variegated fiery pillars, which are frequently traversed by waves or flashes of light. This may be called a complete aurora, and comprehends most of the peculiarities of the other varieties.

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* Franklin's First Expedition, p. 588.

† Voyages en Scandinavie, 1838, pp. 170–171.

The corona seldom continues complete longer than one hour. The streamers then become fewer and less intensely colored; the luminous arches break up, while a dark segment is still visible near the northern horizon; and at last nothing remains but masses of delicate cirro-cumulus clouds. During the exhibition of brilliant auroras, delicate fibrous clouds are commonly seen floating in the upper regions of the atmosphere; and on the morning after a great nocturnal display, we sometimes recognize the same streaks of cloud which had been luminous during the preceding night. Sometimes during the day these clouds arrange themselves in forms similar to the beams of the aurora, constituting what has been called a *day aurora.*

5. Waves or flashes of light. The luminous beams sometimes appear to shake with a tremulous motion; flashes like waves of light roll up towards the zenith, and sometimes travel along the line of an auroral arch. Sometimes the beams have a slow lateral motion from east to west, and sometimes from west to east. These sudden flashes of auroral light are known by the name of merry-dancers, and form an important feature of nearly every splendid aurora.

The *duration* of auroras is very variable. Some last only an hour or two; others last all night; and occasionally they appear on two successive nights, under circumstances which lead us to believe that, were it not for the light of the sun, an aurora might be seen uninterruptedly for 36 or 48 hours. For more than a week, commencing August 28, 1859, in the northern part of the United States, the aurora was seen almost uninterruptedly every clear night. In the neighborhood of Hudson's bay the aurora is seen for months almost without cessation.

Auroras are characterized by *recurring fits* of brilliancy. After a brilliant aurora has faded away, and almost wholly disappeared, it is common for it to revive, so as to rival and often to surpass its first magnificence. Two such fits are common features of brilliant auroras; and sometimes three or four occur on the same night.

The *color* of the aurora is very variable. If the aurora be faint, its light is usually white or a pale yellow. When the aurora is brilliant, the sky exhibits at the same time a great variety of tints; some portions of the sky are nearly white, but with a tinge of emerald green; other portions are of a pale yellow or straw color; others are tinged with a rosy hue, while others have a crimson hue which sometimes deepens to a blood red. These colors are ever varying in position and intensity.

Auroras are sometimes observed simultaneously over large portions of the globe. That of August 28, 1859, was seen over more than 140 degrees of longitude from California to eastern Europe; and from Jamaica on the south to an unknown distance in British America on the north. The aurora of September 2, 1859, was seen at the Sandwich Islands; it was seen throughout the whole of North America and Europe; and the magnetic disturbances indicated its presence throughout all northern Asia, although the sky was overcast, so that at many places it could not be seen. An aurora was seen at the same time in South America and New Holland. The auroras of September 25, 1841, and November 17, 1848, were almost equally extensive.

Dark segment.—In the United States an aurora is uniformly preceded by a hazy or slaty appearance of the sky, particularly in the neighborhood of the northern horizon. When the auroral display commences, this hazy portion of the sky assumes the form of a dark bank or segment of a circle in the north, rising ordinarily to the height of from five to ten degrees. This dark segment is not a cloud, for the stars are seen through it as through a dense smoke.

M. Struve says, "the stratus that rests on the northern horizon, and appears to be the base of all the auroræ boreales that I have seen for a long time at Dorpat, (latitude 58° 21′ N,) is not a cloud, but merely the sky somewhat darkened. Very frequently, when it was quite black, and very high above the horizon, we have seen the stars without any diminution in their brilliancy. Its dark appearance is the effect of contrast with the luminous arc.*"

In the year 1838 the French government sent out a scientific expedition to make explorations in the Arctic seas. Five members of this commission spent the winter of 1838–'39 at Bossekop, in the north of Europe, (latitude 69° 58′ N., longitude 1^h 33^m E. of Greenwich,) for the purpose of making observations upon the aurora borealis, and other meteorological phenomena. The observers were MM. Lottin, Bravais, Lilliehook, and Siljestrom, while M. Bevalet made sketches of the most remarkable auroras. These observers, in their final report, say: "The dark segment was situated near the magnetic meridian towards the north, and was generally illumined by auroral light. Sometimes the illumination prevailed throughout the entire extent of the upper border of the segment; sometimes the illumination was only local and partial. Sometimes it presented the ordinary appearance of an auroral arch; but generally the source of the illumination seemed to be *behind* the segment, near the horizon, or even below it. The lower edge of the luminous band which crowned the segment was diffuse, and seemed to be the result of the increasing density of the hazy stratum traversed by the visual ray.

"Once the dark segment was observed at the *south* point of the horizon, and appeared bordered by the auroral light. At other times the haze extended towards the east and west part of the horizon; it then appeared to overlap the lower extremity of the arches which passed near the zenith, and concealed their point of intersection with the horizon.

"The appearance of an aurora is not necessarily preceded by this dark segment, several brilliant auroras having been seen when the sky was clear and of a deep blue quite down to the horizon.

"The light of the stars is but little diminished by passing through the substance of the aurora. The smallest stars can be seen through the rays of the aurora, especially when its light is feeble and diffuse."†

The highest point of this dark segment is generally found in the magnetic meridian. Exceptional cases, however, frequently occur, and in certain regions there appear to be constant causes which tend to deviate this point uniformly in the same direction. Thus at Abo, (latitude 60° 27′ N.,) M. Argelander found that this summit was 11° west of the magnetic meridian.‡

Auroral arches.—The dark segment is bounded by a luminous arc, whose breadth varies from a half degree to one or two degrees. The lower edge is well defined, but the upper edge is only so when the breadth is very small. As the breadth increases the upper edge becomes less definite, and at length its light becomes confounded with a general brightness of the sky. If the aurora becomes brilliant, other arcs usually form at greater elevations, sometimes passing through the zenith.

The summit of these arcs is situated nearly in the magnetic meridian, and the arc sometimes extends symmetrically on each side towards the horizon. During the winter of 1838 and 1839 numerous measurements of auroral arches were made in Scandinavia (latitude 70°) by MM. Lottin and Bravais, with the aid of a theodolite, and the result of 225 observations gave an average deviation of 10° towards the west of the magnetic meridian. The deviation was only about 6° for arcs rising but little above the northern horizon; it was about 12° for arcs passing near the zenith. This result is almost indentical with that obtained by Argelander at Abo.

The auroral arches observed by Captain Parry, in his Arctic voyages, did not always have their centres in the magnetic meridian.§ A splendid auroral arch

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* Poggendorff Ann., XXII, p. 456.

† Voyages en Scandinavie, pp. 437–442.

‡ Kaemtz's Meteorology, by Walker, p. 453.

§ Parry's Second Voyage, p. 135.

observed at New Haven, Connecticut, March 27, 1781, touched the eastern horizon at E. 2° S. by compass, and it touched the western horizon at W. 20° N. by compass, indicating a deviation of 11° to the east of the magnetic meridian. Observers in most parts of Europe, and also in the United States, have generally described auroral arches as perpendicular to the magnetic meridian. It is desirable that an extended series of observations should be made, to determine the azimuth of each extremity of these arches. It is not improbable that such observations would show not merely that auroral arches are occasionally not perpendicular to the magnetic meridian, but that, for many localities, there is a small constant deviation from this position.

Form of auroral arches.—Auroral arches are not arcs of great circles; that is, they do not cut the horizon at points 180° from each other. This is shown conclusively by the observations made by Lottin and Bravais in latitude 70°. The following table shows the average result of 145 observations, arranged in seven groups, according to the height of the arcs.*

1st group	height =	20°.2	amplitude =	137°.7	16 observations.
2d group	height	41°.4	amplitude	154°.4	20 observations.
3d group	height	70°.1	amplitude	163°.9	17 observations.
4th group	height	90° 0	amplitude	175°.4	41 observations.
5th group	height	108°.5	amplitude	185°.0	27 observations.
6th group	height	136°.8	amplitude	184°.2	21 observations.
7th group	height	157°.3	amplitude	190°.0	3 observations.

The altitudes are supposed to be measured from the north horizon, and the amplitude is the angular distance between the two extremities of the arc, measured on the north side.

If we divide the observations into three groups, we have—

Northern arcs	height =	44°.2	amplitude =	152°.4	53 observations.
Zenith arcs	height	90°.0	amplitude	175°.4	41 observations.
Southern arcs	height	123°.0	amplitude	184°.9	51 observations.

Careful measurements made at five or six points of some of the most remarkable arcs showed that, except near the horizon, they may be regarded as portions of small circles parallel to the earth's surface. Near the horizon there is sometimes a sensible deviation from this circular form, and the appearance is sometimes that of a portion of an ellipse, the extremities of the arch being bent inward, as shown in the annexed figure. Such appearances were frequently noticed by Lottin and Bravais in Scandinavia. Hansteen says that, at Christiania, latitude 59° 54′, he has twice seen an ellipse almost entire.†

Observations made at New Haven accord remarkably with the preceding. Of 27 auroral arches whose average height was 8½° the average amplitude was 80°. A single arch, whose altitude was 66°, had an amplitude of 165°.‡

During the splendid aurora of September 2, 1859, near Cape Horn, in latitude 57° south, there was noticed a bright yellowish light forming an ellipse whose diameters were as two to one, the centre of the ellipse being elevated about 15° above the south horizon.§

Number of auroral arches.—On the 2d of January, 1839, Lottin and Bravais saw nine different arches at the same time; November 2 and January 24, they

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* Voyages en Scandinavie, pp. 466–478.

† Memoires de l'Academie de Belgique, t. 20, p. 119.

‡ American Journal of Science, n. s., v. 39, p. 289.

§ Ibid., v. 30, p. 89.

saw seven; January 3 and 21, six arches; January 7, five arches; September 30, October 15, and December 25, four arches. The number of cases of three arches seen simultaneously was considerable; and examples of two arches were extremely frequent.

Breadth of auroral arches.—This element is not the same when an arch appears near the horizon as when it is seen near the zenith. At Bossekop, in Scandinavia, for arches seen in the north at altitudes less than 60°, from a mean of twenty observations the average breadth of the arches was 7°. Near the zenith and between the limits of 30° zenith distance either north or south, the mean of fifteen measurements gave a breadth of 25°. For arches seen south of the zenith at altitudes less than 60°, the mean of nine observations gave a breadth of 8°.

The following example shows the change in the breadth of an arch during its progress across the sky. December 16, the arch being in the south, at an elevation of 52°, its breadth was 5°; a little later the arch passed the zenith and had an altitude of 70° towards the north; its breadth was then 40°. The arch continued to sink, and when its altitude was 61°, its breadth was only 28°; and at an altitude of 41° its breadth was about 6°. If the distance of an arch from the earth remained constant during its movement of translation, and the arch were of the form of a solid ring whose section was a circle, its breadth when in the zenith should be double that at an elevation of 30°. But the average of many measurements gave its breadth in the former case three or four times as great as in the latter; showing that the greatest breadth of a section of the ring is parallel to the earth.

Anomalous forms of arches.—Sometimes an auroral arch consists of rays arranged in irregular and sinuous bands of various and variable curvature, presenting the appearance of the undulations of a ribbon or flag waving in the breeze. Sometimes the appearance is that of a brilliant curtain whose folds are agitated by the wind. These folds sometimes become very numerous and complex, and the arch assumes the form of a long sheet of rays returning into itself, the folds enveloping each other and presenting an immense variety of the most graceful curves. Sometimes these curves are continually changing, and develop themselves like the folds of a serpent.

It is evident, therefore, that a variety of disturbing causes may prevent an auroral arch from taking up a position perpendicular to the magnetic meridian.

Movements of auroral arches.—An auroral arch does not maintain, invariably, a fixed position. It is frequently displaced, and is transported, parallel to itself, from north to south, or from south to north. An arch which first appears near the northern horizon sometimes rises gradually, attains the zenith, descends towards the southern horizon, remains there for a time stationary, and then, perhaps, retraces its course. The observations made by Lottin, at Bossekop, presented 60 cases in which auroral arches moved from north to south, and 39 cases from south to north. There were 25 nights upon which only the first of these movements was observed; 11 nights upon which only the opposite motion was observed; and 17 nights upon which both movements were observed, successively. Thus the motion from north to south appears to have been about twice as common as that from south to north. In the United States, from a considerable collection of observations it is inferred that the motion from north to south is about ten times as frequent as the motion from south to north.*

Sometimes there is a movement of the arch from west to east, or from east to west. Sometimes while the height of the arch remains constant, the entire arch seems to turn around the vertical, either in the direction of the diurnal motion, or in the opposite direction. Lottin and Bravais observed three cases in which

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* American Journal of Science, n. s., v. 34, pp. 41–45.

the movement of rotation was from east to west by the south, and seven in which the motion was from west to east.

The rate of motion of arches is very variable. The angular motion of translation sometimes amounts to 17° per minute, and frequently amounts to 5° per minute. With a vertical elevation of 125 miles above the earth, the last rate of motion would imply an actual velocity of 1,000 feet per second. We shall find, hereafter, that the movement of auroral beams is still more rapid than that of auroral arches.

Light of auroral arches.—The light of auroral arches is generally of a yellowish white; the lower edge is better defined than the upper; the latter is usually very indefinite and blends with the general tint of the sky. This difference in the sharpness of the two edges is less noticeable in southern arches.

The greater distinctness of the lower edge of the arch may in part be explained by its greater distance from the observer; but it seems probable that the substance of the upper part of the arch is really less dense and more diffuse than the under part of the arch.

Structure of auroral arches.—Auroral arches generally tend to divide into short rays running in the direction of the breadth of the arch, and converging toward the magnetic zenith. They frequently seem to be formed of transverse fibres, terminating abruptly in a regular curve which forms the lower edge of the arch. Arches entirely nebulous and homogeneous are not the most frequent; arcs composed of rays, or striated arcs, are very common, and they present every intermediate shade between those two extremes. Frequently a nebulous arc resolves itself into a striated arc, without changing its general form. Sometimes the rays are distinct and isolated. In this case, the arch generally increases in breadth, extending on the side of the zenith. Sometimes auroral beams arrange themselves in the form of an arch, which is subsequently replaced by an arch of nebulous matter. If the rays of the arch are broader than the dark intervening spaces, and their light is uniform, we have the singular appearance of *dark rays*, or *black striæ* perpendicular to the arch, and projected upon a luminous surface. This fibrous constitution of auroral arches is most noticeable when they pass near the zenith. On the evening of April 9, 1863, there was noticed at New Haven an auroral arch spanning the heavens, and formed of short streamers parallel to each other. Most of them were from 10° to 15° in length, and for some time presented the appearance of a row of comets' tails, all parallel to each other.*

Auroral beams.—Auroral beams present every variety of length from 2° or 3° up to 90° or more. Their breadth varies from 10′ up to 2° or 3°. The most brilliant beams have their edges sharply defined. Sometimes by the side of such a beam the sky appears darker than elsewhere, the effect, probably, of contrast. The lower part of a beam is generally better defined than the upper part. Stars are frequently visible through the substance of the beams.

Motion of auroral beams.—This motion is either longitudinal, in virtue of which the beam extends towards the zenith or the horizon, or it is a lateral movement which displaces the beam parallel to itself, either from right to left, or from left to right. Both of these motions may be very rapid. A beam has been seen to move over an angular space of 90° in 27 seconds.† Beams advance either from north to south, or from south to north; but the former motion is the most common. They sometimes move laterally from east to west, and sometimes from west to east; but in the United States, the former motion is the most common.‡ Frequently a beam extends suddenly either upward or downward. This motion is most common downward, and sometimes with very

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* American Journal of Science, n. s., v. 35, p. 461.

† Voyages en Scandinavie, p. 498.

‡ American Journal of Science, n. s., v. 34, p. 45.

great velocity. It is sometimes observed simultaneously in a large number of neighboring beams. When a beam rises and falls alternately without any considerable change of length, it is said to *dance*. This is a common occurrence in high latitudes, where it is known by the name of the *merry dancers*.

The corona.—When the atmosphere is filled with a large number of separate beams, all parallel to each other and to the direction of the dipping needle, according to the rules of perspective these beams will seem to converge to one point, viz: the magnetic zenith. Hence results the appearance of a corona or crown of rays, whose centre is generally (but not always) dark. The observers at Bossekop made 43 measurements of the position of this corona, the mean of which differed less than one degree from the magnetic zenith. In one instance the position of the corona differed 15°, and in two other instances it differed 12° from the magnetic zenith. A portion of these differences may be ascribed to the difficulty of making such observations with precision; but it seems necessary to conclude that the auroral beams are not always rigorously parallel to the direction of the dipping needle.

Observations made in other parts of Europe as well as in the United States* show that the centre of the corona is always very near the magnetic zenith, but not always exactly coincident with it. From a series of careful measurements of the aurora of November 17, 1848, Professor Challis found that the corona had almost exactly the same altitude as the magnetic zenith, but was situated 1° or 2° more to the west. The observations, however, showed considerable discordances, which seemed to indicate that the centre of the corona was continually shifting its position.†

The corona is sometimes incomplete, sectors of greater or less extent being deficient. At Bossekop these incomplete coronæ generally occupied the northern part of the visible hemisphere. It follows from this that the beams which form a corona, although covering at times a very large region, are nevertheless limited, and frequently did not extend south of Bossekop. The passage of a striated arch over the magnetic zenith frequently presents the appearance of a corona. If the arch advances from north to south, before reaching the magnetic zenith it forms a half crown on the northern side; at the instant of passing the magnetic zenith we have a complete corona of an elliptic form, whose rays descend nearly to the horizon on the eastern and western sides; after the arch has passed the magnetic zenith, there is formed a half crown on the southern side.

Auroral clouds.—When an aurora becomes less active, its beams become more feeble, their edges more diffuse, their length diminishes and their breadth increases, and they assume the appearance of luminous clouds. Their outline is rounded and a little less brilliant than the centre. Sometimes they exhibit a fibrous structure, and present a strong resemblance to cirrus clouds. These auroral clouds generally correspond to an hour of the night more advanced than arches and beams. According to observations made at Bossekop on 37 nights upon which the three forms of arches, beams, and auroral clouds were all observed on the same night, the average time of first appearance was as follows:

	Hours.	Minutes.
Auroral arches	7	52
Auroral beams	8	26
Auroral clouds	11	18

The average hour of disappearance was 14 hours 3 minutes.

Auroral vapor.—Frequently, during the exhibition of a brilliant aurora, there is an appearance of general nebulosity or luminous vapor covering large por-

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* American Journal of Science, n. s., v. 40, p. 286.

† Cambridge Phil. Trans., v. 8, p. 628.

tions of the heavens, and sometimes almost the entire celestial vault. Sometimes its light but little exceeds that of the milky way. Its light is generally faint, especially in the upper part of the sky, but sometimes its accumulation near the horizon produces a pretty intense light resembling a vast conflagration. The great disparity between the light of auroral vapor when viewed near the zenith and near the horizon is a proof that its vertical thickness is small in comparison with its horizontal dimensions. Sometimes this distant light resembles the twilight. This auroral vapor may appear during any phase of a grand aurora, and is probably due to an extreme diffusion of the substance of auroral arches. It is frequently seen during the intervals between the disappearance and reappearance of arches and beams.

Colors of the aurora.—The color of the aurora is ordinarily white; sometimes it is of a pale yellow, and occasionally it becomes reddish. When the movements of the auroral beams become rapid, the yellow color flows from the extremities of the beam towards the centre, while one of its extremities becomes red and the other green. The red of the aurora is usually tinged with violet, and does not correspond to the red of the prismatic spectrum. The green is tolerably pure, blended, perhaps, with a slightly bluish tint. The red tint is the brighter of the two, and that which disappears the last. Sometimes the red and green are arranged parallel to the length of the beam, but more frequently the lower part of the beam is red and the upper part green. Sometimes the entire auroral illumination becomes of a red color. The coloring of the aurora is most frequently observed between 10 and 11 o'clock, and this is usually the period of greatest brilliancy of the aurora.

Geographical distribution of auroras.—Auroras are very unequally distributed over the earth's surface. They occur most frequently in the higher latitudes, and are almost unknown within the tropics. At Havana (latitude 23° 9′) but six auroras have been recorded within a hundred years; and south of Havana, auroras are still more unfrequent. As we travel northward from Cuba, auroras increase in frequency and brilliancy; they rise higher in the heavens, and oftener attain the zenith. In order to determine the law of distribution of auroras over the northern hemisphere I have collected observations from 128 localities, showing, as far as possible, the average number of auroras seen annually at each place.* These observations are not as complete as could be desired, frequently comprehending a period of less than a year; nevertheless, when we project them upon a chart we find them unexpectedly consistent with each other. If we travel from the equator northward along the meridian of Washington, we find on an average, near the parallel of 40°, only 10 auroras annually. Near the parallel of 42°, the average number is 20 annually; near 45°, the number is 40; and near the parallel of 50°, it amounts to 80 annually. Between this point and the parallel of 62° auroras are seen almost every night. They appear high in the heavens, and as often to the south as the north. Further north they are seldom seen except in the south, and from this point they diminish in frequency and brilliancy as we advance towards the pole. Beyond latitude 62°, the average number of auroras is reduced to 40 annually. Beyond latitude 67°, it is further reduced to 20; and near latitude 78°, to 10, annually. If we make a like comparison for the meridian of St. Petersburgh, we shall find a similar result, except that the auroral region is situated further northward than it is in America; the region of 80 auroras annually, being found between the parallels of 66° and 75°.

Upon the accompanying chart the dark shade indicates the region where the average number of auroras annually amounts to at least 80; and the lighter shade indicates the region where the average number of auroras annually

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* American Journal of Science, n. s., v. 30, pp. 89–96.

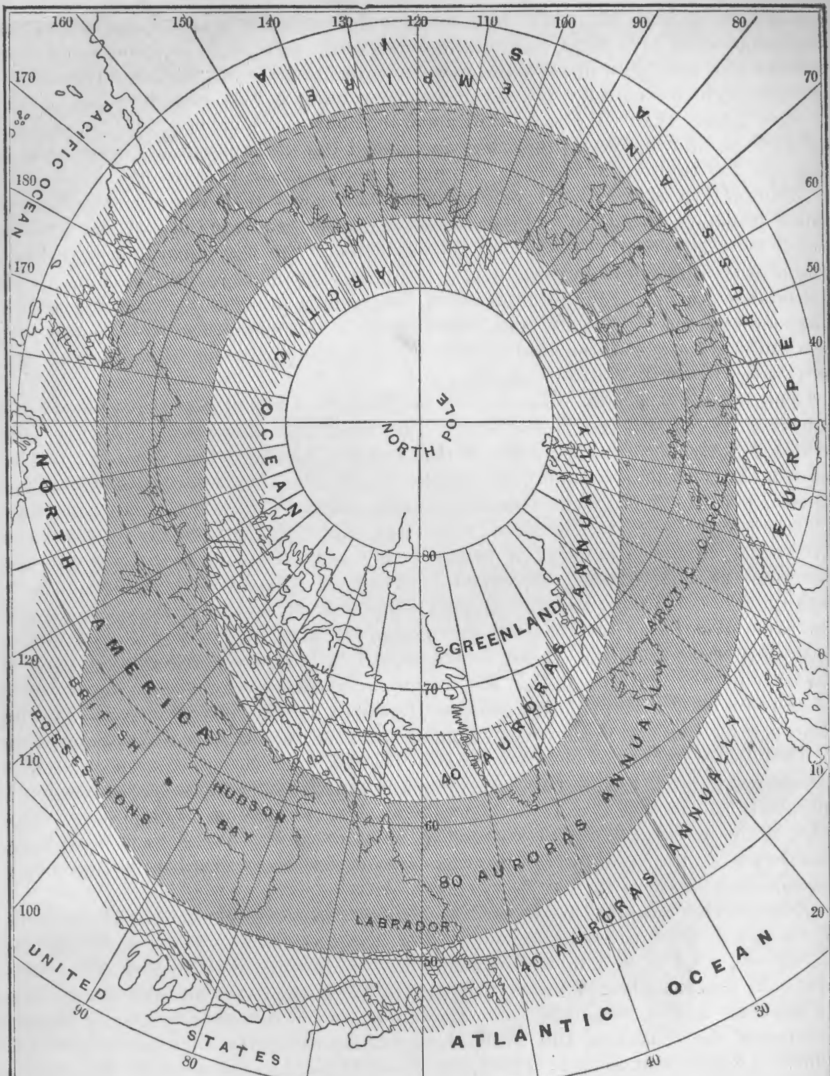

CHART
showing the distribution of
AURORAS
in the northern hemisphere, by
Prof. ELIAS LOOMIS.
1860.

amounts to at least 40. We thus see that the region of greatest auroral action is a zone of an oval form, surrounding the north pole, and whose central line crosses the meridian of Washington in latitude 56°, and the meridian of St. Petersburgh in latitude 71°. Accordingly, auroras are more frequent in the United States than they are in the same latitudes of Europe. On the parallel of 45°, we find in North America an average of 40 auroras annually, but in Europe less than 10.

The form of this auroral zone does not bear any resemblance to the lines of equal magnetic intensity, but it does bear some resemblance to the lines of equal magnetic dip. Throughout Asia the line of 80° dip runs nearly through the centre of the auroral zone, but in America it runs sensibly south of it. It bears also considerable resemblance to a magnetic parallel, or line everywhere perpendicular to a magnetic meridian; and the coincidence of this result with the uniform position of auroral arches, naturally suggests the idea of a real connexion between the two phenomena.

Auroras in the southern hemisphere.—We have but a few observations of the aurora in the southern hemisphere. The most complete record of this kind which I have found is that made at the British Magnetic Observatory, at Hobarton, Van Dieman's Island, during the years 1841–'48. These observations have been published by the British government, and embrace 5 auroras in 1841, 12 in 1842, 2 in 1844, 1 in 1846, 9 in 1847, and 5 in 1848, making 34 auroras in 8 years, being an average of 4¼ per year; or if we leave out of the account the years 1843 and 1845, we have an average of 5⅔ per year. Hobarton is in latitude 42° 52′ south, and the magnetic dip in 1845 was 70° 35′. This dip is the same as is found in the southern part of England, or in the United States near Baltimore, and the average number of auroras seen annually in each of these regions is from 6 to 7. We hence infer that auroras in the southern hemisphere are nearly if not quite as frequent as they are in corresponding magnetic latitudes of the northern hemisphere. From August 28 to September 2, 1859, throughout the southern part of South America and also in Australia, the aurora exhibited a magnificence such as is seldom witnessed in corresponding northern latitudes. The observations are too few to enable us to infer what is the geographical distribution of auroras in the southern hemisphere, but they are quite consistent with the supposition that this distribution bears considerable analogy to that in the northern hemisphere.

Auroras seen simultaneously in both hemispheres.—By comparing the records of auroras in the northern hemisphere with the observations made at Hobarton, already referred to, we find the coincidences of dates are very remarkable. Out of the 34 auroras observed at Hobarton, in 11 of the cases an aurora was seen on the same day at New Haven. These observations were not strictly cotemporaneous, for Hobarton and New Haven being in nearly opposite longitudes, when an aurora was seen at Hobarton it could not be seen at New Haven, on account of the presence of the sun; but in 11 cases, an aurora was seen within about twelve hours of its appearance at Hobarton. In several cases when an aurora was seen at Hobarton it was cloudy at New Haven, and there were eight other corresponding cases in which an aurora was seen at some one of the academies in New York, although not noticed at New Haven. In four additional cases an aurora was seen at Toronto, when none was recorded at New Haven or in the State of New York. There remain, then, only 11 cases of auroras at Hobarton for which we do not find corresponding observations from one of these three sources in the northern hemisphere, and in eight of these cases the sky was overcast from New Haven to Toronto. In each of these 11 cases an aurora was observed in England, or there were observed unusual disturbances of the magnetic instruments, indicating the existence of an aurora at no very remote station. So far, then, as a conclusion is authorized from so small a number of observations we should infer that whenever an aurora is seen at Hobarton, where the magnetic dip is —70°, an aurora occurs at some place in the northern hemisphere as far south as where the magnetic dip does not much exceed 75°; in other words, *an unusual auroral display in the southern hemisphere is always accompanied by an unusual display in the northern hemisphere;* or an exhibition of auroral light about one magnetic pole of the earth is uniformly attended by a simultaneous exhibition of auroral light about the opposite magnetic pole.*

Height of the aurora.—The great auroral exhibition of August and September, 1859, was very carefully observed at a large number of stations, and these observations afford the materials for determining the height of the aurora above the earth's surface. The southern limit of these auroral displays was not the same upon all meridians. In North America, the aurora of August 28 appeared in the zenith as far south as latitude 36° 40′; and it attracted general attention as far south as latitude 18°. In Central Europe this aurora extended to the zenith of places as far south as about latitude 45°. It was brilliant at Rome in latitude 42°, but was not noticed at Athens in latitude 38°; neither was it seen in western Asia in latitude 40°.

In North America, the aurora of September 2 appeared in the zenith at places as far south as latitude 22° 30′, and attracted general attention in latitude 12°; and if the sky had been clear, some traces of the aurora might probably have been detected even at the equator. In Europe this aurora was noticed at Athens in latitude 38°. Both of these auroras conformed to the general law of auroral distribution already explained, the region of greatest auroral action being in America, about 15° further south than in eastern Europe.

At the most southern stations where these auroras were observed, the light rose only a few degrees above the northern horizon; at more northern stations the aurora rose higher in the heavens; at certain stations it just attained the zenith; at stations further north the aurora covered the entire northern heavens, as well as a portion of the southern; and at places further north the entire visible heavens, from the northern to the southern horizon, were overspread with the auroral light. The following table presents a summary of a few of the most definite observations on the aurora of August 28, 1859, at about $8^h 42^m$ p. m., New Haven time:

TABLE I.

Locality.	Latitude.	Extent of auroral display.
	° ′	
North side of Jamaica	18 20	Like the light of a fire.
Inagua, Bahamas	21 18	Remarkably brilliant.
Havana, Cuba	23 9	Rose 23° above the north horizon.
Key West, Florida	24 33	Rose about 30° above the north horizon.
Savannah, Georgia	32 5	Rose some 45° above the north horizon.

The following table presents a summary of observations of the same aurora, made at the same hour, at places where the auroral light covered the entire northern heavens, as well as a portion of the southern:

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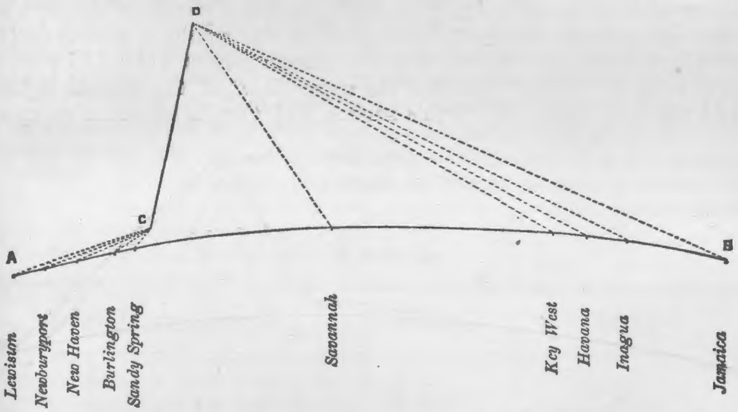
* American Journal of Science, n. s., v. 32, pp. 11–14.

TABLE II.

Locality.	Latitude.	Extent of auroral display.
	° ′	
Sandy Spring, Maryland	39 9	Extended to 51° from south horizon.
Gettysburg, Pennsylvania	39 49	Extended to 30° from south horizon.
Philadelphia ....do	39 57	Extended to 22½° from south horizon.
Burlington, New Jersey	40 5	Extended to 20° from south horizon.
New Haven, Connecticut	41 18	Extended to 10½° from south horizon.
West Point, New York	41 23	Extended to 12° from south horizon.
Newburyport, Massachusetts	42 48	Extended to 6° from south horizon.
Lewiston, Maine	44 5	Extended to 5° from south horizon.

If we combine the preceding observations in Table II, we shall find that *the lower limit* of the auroral light was elevated forty-six miles above the earth's surface, and that its southern margin was vertical over the parallel of 38° 50′ north latitude in Virginia.

Now it is considered as established that the auroral streamers are luminous beams sensibly parallel to the direction of the dipping needle. But the dip of the needle in latitude 38° 50′ in Virginia is 71° 20′; and if we draw a line C D, making an angle of 71° 20′, with the curve line A B, which represents a portion of the earth's surface, we may assume that the line C D represents the southern boundary of the auroral illumination. If, then, we assume that the observations of Table I were made upon the point D, we shall find that the *upper limit* of the auroral light was elevated 534 miles above the earth's surface, and that its southern margin was vertical over the parallel of 36° 40′ north latitude in Virginia.

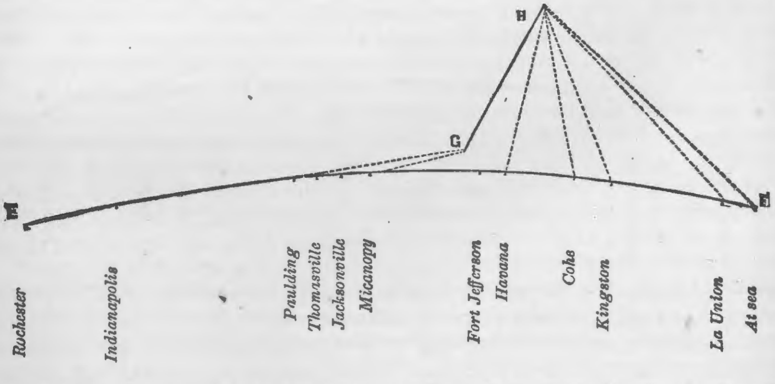


The following table presents a summary of the most definite observations of the aurora of September 2, 1859, made generally about 2 a. m., Havana time.

TABLE III.

Locality.	Latitude.	Longitude.	Hour.	Extent of auroral display.
	° ′	° ′		
At sea	12 23	88 28	Midnight	Sky lurid; wavy appearance.
La Union, San Salvador	13 18	87 45	10.3 a. m.	About 30° above the north horizon.
Salvador	13 44	88 55	....	Same as at La Union.
Kingston, Jamaica	17 58	76 50	1.5 a. m.	Appeared like a colossal fire.
Cohe, Cuba	20 0	76 10	....	Extended upward about 72°.
Havana, Cuba	23 9	82 22	2 a. m.	More than 100° in height.
Fort Jefferson, Florida	24 37	82 52	2 a. m.	Extended beyond the zenith.
Micanopy, Florida	29 30	82 18	2.30 a. m.	Corona very distinct.
Jacksonville, Florida	30 15	82 0	3 a. m.	Extreme south in a red glow.
Thomasville, Georgia	30 50	84 0	2 a. m.	Corona formed.
Paulding, Mississippi	32 20	89 20	2.10 a. m.	Whole visible heavens overspread.
Indianapolis, Indiana	39 55	86 5	....	Down to south horizon.
Rochester, New York	43 8	77 51	2 a. m.	Do. do.

If we combine the last seven observations of the preceding table, we shall find that *the lower limit* of the auroral light was elevated fifty miles above the earth's surface, and that its southern margin was vertical over the parallel of 25° 15′ north latitude in Florida. Now the dip of the magnetic needle in Florida, in latitude 25° 15′, is 55° 40′; and if we draw G H, making an angle of 55° 40′ with the curve line E F, which represents a portion of the earth's surface, and assume that the line G H represents the southern boundary of the auroral illumination, and that the first five observations of Table III were made upon the point H, we shall find that *the upper limit* of the auroral light was elevated 495 miles above the earth's surface, and that its southern margin was vertical over the parallel of 22° 30′ north latitude in Cuba.* Combining these results with the numerous observations of this aurora contained in the American Journal of Science, volume 32, we find that the aurora of September 2, 1859, formed a belt of light encircling the northern hemisphere, extending southward in North America to latitude 22½°, and reaching to an unknown distance on the north; and it pervaded the entire interval between the elevations of 50 and 500 miles above the earth's surface. This illumination consisted chiefly of luminous



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* American Journal of Science, n. s., v. 32, pp. 319–322.

beams or columns, everywhere nearly parallel to the direction of a magnetic needle when freely suspended; that is, in the United States, these beams were nearly vertical, their upper extremities being inclined southward at angles varying from 15° to 30°. These beams were, therefore, about 500 miles in length; and their diameters varied from five to ten and twenty miles, and perhaps, sometimes, they were still greater.

The aurora of August 28, 1859, formed a belt of light of nearly equal extent, and it pervaded the entire interval between the elevations of 46 and 534 miles above the earth's surface.

The height of a large number of auroras has been computed by similar methods, and the average result for the upper limit deduced from 31 examples is about 450 miles.*

Professor Potter, of London University, from a comparison of a very large number of uncommonly good observations, has determined the height of the auroral arches of September 17 and October 12, 1833. His results were for the mean heights of the upper edge of the arches from eight comparisons, 72 miles; mean height of the under edge of the arches from two comparisons, 63 miles.† Dr. Dalton determined the height of the auroral arch of March 29, 1826, to be 100 miles or upwards‡

From these and a multitude of similar results it is concluded that the aurora seldom if ever appears at an elevation above the earth's surface less than about 45 miles, and that it extends upwards sometimes to an elevation of at least 500 miles.

It is believed that these conclusions correspond substantially with the views of those whose opinions on this subject are entitled to the greatest weight; nevertheless, there are some who contend that the aurora is sometimes seen at elevations of less than one mile above the earth's surface. In the Philosophical Transactions of London for 1839, pp. 277–280, Professor James Farquarson, of Scotland, has given an account of observations upon an auroral arch made at two stations distant but a little more than a mile apart, from which it was concluded that the height of the lower edge of the arch was only 2,481 feet, or less than half a mile. It is difficult to decide wherein consisted the fallacy of this determination. One observation was made at 7^h and another at 7^h 5^m. The observations at the two stations indicated an apparent parallax of 7°; the arch was about 12° broad; clouds were visible during both observations; and "at 7^h 10^m the whole sky had become too much obscured to admit of longer continued contemporaneous observations." It is possible that this cloud hid a portion of the arch from one of the stations while it was visible at the other, so that the two observers were not viewing the same object; or the object of observation might have been simply a thin cloud illumined by auroral light. I believe Mr. Farquarson's conclusions to be erroneous, because they differ so widely from those of more practiced observers under at least equally favorable circumstances.

Similar observations for the determination of the parallax of the aurora were made by the French observers in Scandinavia in 1839. On the 9th of January M. Bravais left Bossekop and went to Jupvig, distant less than ten miles, for the purpose of observing the height of auroral arches, while M. Lottin remained at Bossekop to make similar and simultaneous observations. The altitudes of the arches were measured with a theodolite, and the times were noted by a chronometer. On comparing the observations it was discovered that the aurora *always* presented nearly the same appearance at the two stations.§ Care-

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* Annals of Philosophy, December, 1814, p. 431.

† Transactions of Cambridge Philosophical Society, v. 8, pp. 322–325.

‡ Philosophical Transactions Royal Society 1828, p. 298.

§ Voyages en Scandinavie, p. 537.

ful drawings of numerous auroras were made at both stations which clearly established this coincidence. The parallaxes resulting from the observations were sometimes positive and sometimes negative; the average parallax but little exceeding one degree. These observations were regarded as demonstrating that the average height of auroral arches is from 60 to 100 miles above the earth's surface.

Some persons maintain that the small height of auroras is proved by their being sometimes seen between the observer and a cloud, or between the observer and a hill of moderate elevation. Cases do unquestionably occur in which the aurora appears to be situated between a cloud and the observer; but this appearance is believed to result from a cirrus or cirro stratus cloud of very small density being strongly illumined by auroral light which shines through the cloud, so as to produce the same appearance as if the aurora prevailed on the under side of the cloud.

Sometimes the lower extremity of an auroral streamer appears to be prolonged below the summit of a mountain or hill. Captain Parry states that on one occasion he observed a bright ray of the aurora shoot suddenly downward between him and the land, which was then distant only 3,000 yards * This appearance is believed to have been an illusion. A similar phenomenon was twice noticed by the French observers at Bossekop, and was ascribed by them to the reflection of the auroral light from the snow which covered the mountain.

On the whole, then, we conclude that although it is *possible* the aurora may sometimes descend nearly to the earth's surface, there is no sufficient evidence to prove that the true polar light has ever descended so low as the region of ordinary clouds.

Noise of the aurora.—There is no satisfactory evidence that the aurora ever emits any audible sound. It is a common impression, at least in high latitudes, that the aurora sometimes emits sound. This sound has been called a rustling, hissing, whizzing. crackling noise. But Scoresby, Ross, Back, and other distinguished travellers who have spent several winters in the Arctic regions, where auroras are seen in their greatest brilliancy, have been convinced that this supposed rustling is a mere illusion. The observers at Bossekop never heard any noise which they could ascribe to the aurora, although their attention was specially directed to this subject; and they concluded that the sounds which have been ascribed to the aurora must have been due to other causes, such as the whistling of the wind, the whirling of the snow, the distant murmur of the sea, or the cracking of the snow when it congeals after having been partially melted.

When we see a brilliant light shooting like a rocket across the sky, it is natural to expect an accompanying sound. People generally hear what they expect to hear. Tacitus informs us that the ancient Germans heard a noise whenever the setting sun descended into the western ocean.†

No observer has ever spoken of the interval that had elapsed between the darting of the auroral rays and the alleged noise. But, on account of the elevation of the aurora, this interval should be a long one. Sound requires four minutes to travel a distance of fifty miles. It is probable, therefore, that the sounds which have been heard during exhibitions of the aurora are to be ascribed to other causes than the aurora.

Diurnal periodicity of auroras.—Auroras appear at all hours of the night, but not with equal frequency. In Canada, the number of auroras increases uninterruptedly from sunset till an hour before midnight, from which time the number diminishes uninterruptedly till morning. At more northerly stations in North America, auroras are most frequent at midnight; and at places still more northerly, as far as the Arctic ocean, they appear to be most frequent an hour

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* Parry's Third Voyage, p. 61.

† Tacitus Germania, c. 45, l. 4.

after midnight. This is shown in the following table, in which column first shows the hour of observation; column second shows the number of auroras reported by Captain Lefroy for the years 1848 and 1849 at London, Kingston, Montreal, Quebec, and Newfoundland, in latitudes from 43° to 47½° *; column third shows the number observed during the winter of 1857–'58 at Carlton Fort, latitude 52° 52′, longitude 106° 30′ west†; column fourth shows the number observed during the winter of 1843–'44 at Lake Athabasca, latitude 58° 43′ north, longitude 143° 49′ west‡; column fifth shows the number observed during the winter of 1852–'53 at Point Barrow, latitude 71° 21′ north, longitude 156° 15′ west§; and column sixth shows the sum of the numbers in the four preceding columns.

Hour.	Canada.	Carlton Fort.	Athabasca.	Pt. Barrow.	Sum.
6 p. m.	25	5	1	30	61
7 p. m.	60	13	8	56	137
8 p. m.	124	26	14	56	220
9 p. m.	149	35	17	60	261
10 p. m.	191	41	19	77	328
11 p. m.	194	53	23	88	358
Midnight	154	59	32	85	330
1 a. m.	128	56	36	103	323
2 a. m.	102	46	21	98	267
3 a. m.	81	46	18	95	240
4 a. m.	49	40	13	80	182
5 a. m.	24	26	12	71	133
6 a. m.	2	10	3	66	81

Annual periodicity of auroras.—Auroras occur in each month of the year, but not with equal frequency. This period would be more obvious if it were not disguised by the unequal length of the days in the different seasons. Suppose the aurora occurred with equal frequency at every hour of the day and night throughout the year, then the number of those that would be seen in winter must be greater than the number seen in summer, because the prolonged darkness permits us to see them more frequently. But, on the contrary, we find in the northern part of the United States more auroras recorded in summer than in winter. This is shown in the following table, in which column second shows the number of auroras observed at New Haven and Boston during a period of 113 years, from 1742 to 1854, according to tables recently published by the Connecticut Academy of Arts and Sciences; column third shows the number of auroras observed at the academies of the State of New York in 25 years, from 1826 to 1850;|| column fourth shows the number reported by Captain Lefroy for the years 1848 and 1849, in Canada and Newfoundland;¶ and column fifth shows the sum of the numbers in the three preceding columns.

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[*] N. Y. Reg. Reports, 1850, p. 292.

[†] St. Helena Mag. Obs., v. 2, p. CX.

[‡] Lake Athabasca Obs., p. 144.

[§] Phil. Trans., 1857, p. 512.

[||] Hough's N. Y. Meteorology, p. 472.

[¶] N. Y. Regents' Report, 1850, p. 229.

	Boston and New Haven.	New York.	Canada.	Sum.
January	81	76	16	173
February	93	86	31	210
March	110	106	24	240
April	104	125	38	267
May	86	83	22	191
June	83	79	17	179
July	123	100	21	244
August	102	122	14	238
September	143	131	19	293
October	99	110	27	236
November	115	74	26	215
December	83	60	16	159

If we classify the entire series of observations by seasons, we shall have in—

	Boston and New Haven.	New York.	Canada.	Sum.
Spring	300	314	84	698
Summer	308	301	52	661
Autumn	357	315	72	744
Winter	257	222	63	542

These observations show a decided minimum in December, and there is apparently another minimum in June. There are apparently two maxima—one in April and the other in September. The slight diminution in the number of auroras in summer as compared with the spring may be ascribed, at least in part, to the longer continuance of daylight. If we increase the number of auroras recorded for the month of June, in the ratio of the number of hours in the two months upon which ordinary auroras might be seen, we shall conclude that auroras are nearly as abundant in June as in April. We infer, then, that there is a very decided diminution in the frequency of auroras in December, and a period of maximum frequency from April to September, with perhaps a slight diminution during the intervening month of June.

Secular periodicity of auroras.—The number of auroras seen on different years is extremely variable. Sometimes, for several years, auroras are remarkable for their number and magnificence, and then succeeds a barren interval during which auroras are almost entirely forgotten.

If we compare the observations made at any one station for a long period of years, we shall discover not merely an inequality in the number of auroras upon successive years, but this inequality bears a strong resemblance to a secular periodicity. This is shown in a long series of observations made at Boston and New Haven. The observations for Boston have been published by Professor Lovering in the Memoirs of the American Academy, vol. ix, pp. 101–120. The observations for New Haven have been recently published by the Connecticut Academy of Arts and Sciences.* The combined series of observations extends from 1742 to 1854, and embraces in the aggregate 1,222 auroras, not counting duplicates. The following table exhibits the number of auroras observed each year at New Haven and Boston, according to this combined series of observations:

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* Transactions of the Conn. Acad. of Arts and Sciences, vol. 1, pp. 9–172.

Summary of auroras observed at New Haven and Boston.

Years.	No. of auroras.	Years.	No. of auroras.	Years.	No. of auroras.	Years.	No. of auroras.	Years.	No. of auroras.
1742	2	1765	7	1788	38	1811	0	1834	9
1743	2	1766	0	1789	51	1812	0	1835	6
1744	0	1767	4	1790	13	1813	0	1836	5
1745	0	1768	7	1791	12	1814	3	1837	41
1746	7	1769	18	1792	6	1815	1	1838	39
1747	10	1770	14	1793	8	1816	0	1839	47
1748	6	1771	15	1794	2	1817	0	1840	44
1749	10	1772	7	1795	2	1818	4	1841	42
1750	17	1773	17	1796	0	1819	6	1842	11
1751	5	1774	20	1797	0	1820	2	1843	10
1752	2	1775	5	1798	0	1821	0	1844	10
1753	1	1776	4	1799	0	1822	1	1845	22
1754	0	1777	15	1800	0	1823	0	1846	30
1755	0	1778	18	1801	0	1824	0	1847	22
1756	0	1779	4	1802	2	1824	2	1848	53
1757	6	1780	25	1803	5	1826	0	1849	20
1758	4	1781	25	1804	4	1827	7	1850	30
1759	5	1782	24	1805	4	1828	6	1851	21
1760	6	1783	22	1806	4	1829	2	1852	42
1761	5	1784	4	1807	2	1830	6	1853	22
1762	7	1785	9	1808	0	1831	2	1854	15
1763	6	1786	55	1809	2	1832	2		
1764	12	1787	47	1810	0	1833	3		

These numbers exhibit an increase from 1742 to 1786–'89, with certain exceptions which will be noticed hereafter. The middle of the period of maximum abundance may be fixed at 1789, the average number of auroras for four years amounting to 48. From this date the number declined rapidly, with slight interruptions, to near 1820. The middle of the period of minimum frequency may be assigned for 1816, when the average number of auroras did not exceed one per year. From 1827 the numbers increase, and after 1837 the number is very remarkable, the average for five years from 1837 to 1841 being 42 per year. Then for three or four years there is a marked decline, and a subsequent revival, which is most decided in 1848 and 1852. Regarding this as a single period of maximum abundance, the middle of the period occurred not far from 1845, making thus an interval of 58 years from the maximum in 1787 to that of 1845.

It is, then, established beyond question that during the past century the frequency of auroras in New England has been subject to an inequality bearing considerable resemblance to an astronomical periodicity, the period being about 58 years; but to enable us to decide whether this period is uniform would require observations continued for a much longer interval of time.

For the purpose of making the comparison for a longer period, I have sought for auroral observations from different parts of Europe. At first, I intended to make the comparison with Bouè's catalogue,* which gives a very extensive collection of auroras from 500 B. C. to 1856, but after mature deliberation decided to abandon it. This catalogue, although very extensive, is still quite incomplete. Professor Wolf has given two supplementary catalogues† amounting in the aggregate to about 1,000 auroras; and I have, myself, found a number of cases not enumerated in either catalogue. Moreover, by combining indis-

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* Sitzungsberichte der Akad. der Wiss., Wien, b. 22, pp. 1–74.

† Vierteljahrs Schrift der Nat. Ges. in *Zurich, 1857, pp. 83 and 401.*

criminately in one catalogue all the auroras reported from any part of the world, and thus incorporating occasional lists embracing, perhaps, one or two years' observations from polar regions where auroras are seen almost every clear night, the total number of auroras for the different years exhibits an inequality not due to any real change in the frequency of auroras, but rather to a change in the place of observation. I have, therefore, sought to obtain, as far as possible, continued series of observations from single localities. In this I have been but partially successful, but think the data are sufficient to warrant some important conclusions. The following table embraces several such partial lists, and extends from 1685 to 1864.

The column marked 1 is taken from a catalogue of auroras seen throughout Europe, as collected by Mairan in his Traité de l'Aurore Boreale, 2d ed., pp. 552–554.

The column marked 2 contains the auroras observed in Sweden, chiefly at Upsala and Christiania, latitude 59° 52′ or 54′. The observations from 1716 to 1733 are given in Mairan, p. 497. The observations from 1739 to 1762, and from 1846 to 1853, are from the Bulletin de l'Academie R. de Belgique, t. 21, pp. 284–300; those from 1837 to 1846 are from the Memoires de l'Acad. R. de Belgique, t. 20, p. 117; and the remaining observations are from Wolf's Vierteljahrs Schrift, 1863, p. 108.

The column marked 3 contains the auroras observed at St. Petersburgh, latitude 59° 56′. Those from 1726 to 1739 are from Mairan, p. 512. The others have been collected from the successive volumes of the Memoirs of the Academy at St. Petersburgh.

The column marked 4 contains a catalogue of auroras observed in different parts of Europe, collected by Cotte, in his Memoires sur la Meteorologie, v. 1, p. 366.

The column marked 5 contains Dalton's catalogue of auroras as published in his Meteorological Essays, pp. 54–58 and 218–226. The first seven years are for Kendall and Keswick, Scotland, latitude 54° 17′ and 54° 33′ north. The other observations are for Great Britain generally. The numbers for 1835 and 1836 I have added from Bouè's catalogue, in order to supply a gap in the series.

The column marked 6 contains the auroras observed at Mannheim, latitude 49° 29′, taken from Ephemerides Met. Palatinae, 1781–1792.

The column marked 7 contains observations at Dunse, Scotland, latitude 55° 47′, from Phil. Soc. Abstracts, vi, p. 291.

The column marked 8 contains observations at Makerstoun, Scotland, latitude 55° 35′, from the Edinburgh Phil. Trans., v. 19, p. 81.

The column marked 9 contains observations from every part of Europe, as reported in Heis's Wochenschrift.

Year.	1.	2.	3.	Year.	1.	2.	3.	4.	Year.	3.	5.	6.	Year.	5.	2.	7.	8.	9.
	Mairan.	Sweden.	Petersburg.		Mairan.	Sweden.	Petersburg.	Cotte.		Petersburg.	Dalton.	Mannheim.		Dalton.	Sweden.	Dunse.	Makerstoun.	Heis.
1685	1	....	....	1740	2	36	11	....	1782	29	....	14	1824	0	....	....	....	...
1686	4	....	....	1741	21	76	28	....	1783	17	....	16	1825	1	....	....	....	...
1692	2	....	....	1742	14	46	30	....	1784	7	....	8	1826	2	....	....	....	...
1693	2	....	....	1743	9	47	7	....	1785	14	....	12	1827	10	....	....	....	...
1694	2	....	....	1744	8	18	....	....	1786	40	16	21	1828	11	....	....	....	...
1695	4	....	....	1745	3	....	....	....	1787	10	27	39	1829	18	....	....	....	...
1696	4	....	....	1746	1	56	....	....	1788	10	53	27	1830	32	....	....	....	...
1697	1	....	....	1747	7	33	....	....	1789	15	45	11	1831	23	27	....	....	...
1698	9	....	....	1748	3	39	....	....	1790	4	36	17	1832	5	....	....	....	...
1699	40	....	....	1749	3	33	....	....	1791	4	37	10	1833	12	....	....	....	...
1702	1	....	....	1750	12	24	....	....	1792	1	23	9	1834	2	....	....	....	...
1704	1	....	....	1751	2	24	....	....	1793	2	....	....	1835	[6]	....	....	....	...
1707	12	....	....	1752	....	39	....	....	1794	2	6	....	1836	[8]	....	....	....	...
1708	1	....	....	1753	....	29	....	....	1795	2	3	....	1837	....	25	....	....	...
1709	3	....	....	1754	....	17	....	....	1796	1	0	....	1838	....	28	27	....	...
1710	1	....	....	1755	....	9	1	....	1797	1	13	....	1839	....	30	38	....	...
1711	1	....	....	1756	....	15	2	....	1798	0	0	....	1840	....	40	43	....	...
1714	1	....	....	1757	....	....	0	....	1799	1	2	....	1841	....	35	42	....	...
1716	11	1	....	1758	....	....	2	....	1800	0	3	....	1842	....	49	9	....	...
1717	12	3	....	1759	....	48	8	....	1801	0	4	....	1843	....	38	10	20	...
1718	27	9	....	1760	....	53	7	....	1802	1	4	....	1844	....	22	13	30	...
1719	32	3	....	1761	....	50	12	....	1803	0	6	....	1845	....	18	10	47	...
1720	28	6	....	1762	....	34	18	....	1804	3	6	....	1846	....	39	16	17	...
1721	19	5	....	1763	....	....	4	....	1805	1	4	....	1847	....	38	30	18	...
1722	46	15	....	1764	....	....	9	....	1806	2	3	....	1848	....	38	....	26	...
1723	30	16	....	1765	....	....	8	....	1807	1	0	....	1849	....	42	....	26	...
1724	26	10	....	1766	....	....	0	....	1808	1	1	....	1850	....	25	....	....	...
1725	30	18	....	1767	....	....	5	....	1809	0	0	....	1851	....	17	....	....	...
1726	46	13	2	1768	....	....	2	....	1810	0	0	....	1852	....	45	....	....	...
1727	67	46	10	1769	....	....	10	....	1811	0	0	....	1853	....	26	....	....	...
1728	86	38	25	1770	....	....	13	....	1812	....	0	....	1854	....	36	....	....	...
1729	65	43	6	1771	....	....	29	13	1813	....	0	....	1855	....	20	....	....	...
1730	116	69	33	1772	....	....	21	26	1814	....	4	....	1856	....	20	....	....	...
1731	57	37	18	1773	....	....	31	31	1815	....	0	....	1857	....	15	....	....	...
1732	100	43	51	1774	....	....	48	46	1816	....	1	....	1858	....	34	....	....	19
1733	27	9	5	1775	....	....	21	44	1817	....	1	....	1859	....	46	....	...	30
1734	38	....	25	1776	....	....	12	31	1818	....	2	....	1860	....	33	....	....	36
1735	51	....	30	1777	....	....	26	57	1819	2	3	....	1861	....	....	....	....	35
1736	43	....	17	1778	....	....	30	54	1820	5	2	....	1862	....	....	....	....	33
1737	40	....	11	1779	....	....	37	88	1821	....	2	....	1863	....	....	....	....	36
1738	9	....	7	1780	....	....	20	16	1822	....	0	....	1864	....	....	....	....	47
1739	27	45	21	1781	....	....	29	....	1823	....	0	....						

A careful examination of this table will lead to conclusions similar to those already derived from the American observations. We perceive a period of unusual abundance, extending from 1830 to the present time, the maximum occurring apparently from 1840 to 1845. This period was preceded by one of great barrenness, extending from 1793 to 1826, and its middle occurred about 1812. This period was preceded by one of great abundance, extending from 1771 to 1792, its middle occurring about 1780. This period was preceded also by one of great barrenness except in very high latitudes, and extended from 1742 to 1770, its middle occurring about 1755. This period was preceded by another of great abundance, extending from 1716 to 1741, its middle occurring about 1728. This period was preceded by another one of barrenness, whose middle was not far from 1697. Combining all these results, we perceive a considerable degree of uniformity, approximating towards a period of fifty-nine years from one maximum to another.

At the same time we cannot overlook the considerable exceptions to this rule; and these exceptions seem to point to a subordinate period of ten years. In each of the preceding lists we notice this alternation of meagre and abundant years, and the intervals do not generally differ much from ten years. The following table shows the dates of these periods of maximum and of minimum frequency, as far as they can be gathered from the American and European observations independently:

	European observations.	American observations.		European observations.	American observations.		European observations.	American observations.
Maximum.	1707		Maximum	1760	1760	Maximum.	1819	1818–9
Minimum.	1713		Minimum.	1766	1766	Minimum.	1823	1823–4
Maximum.	1718		Maximum.	1771	1769–71	Maximum.	1830	1827–8
Minimum.	1721		Minimum.	1776	1776	Minimum.	1834	1832
Maximum.	1730		Maximum.	1779	1780	Maximum.	1840	1839
Minimum.	1733		Minimum.	1784	1784	Minimum.	1843–5	1843
Maximum.	1741		Maximum.	1788	1789	Maximum.	1849	1848
Minimum.	1745–6	1744–5	Minimum.	1798	1798–9	Minimum.	1856	
Maximum.	1750	1750	Maximum.	1804	1804–5	Maximum.	1859–64	
Minimum.	1755	1755	Minimum.	1811	1811			

It will be observed that the two series of observations accord pretty well with each other, and they show a mean interval of eleven years between the successive maxima. It should be remarked, however, that from 1792 to 1802 the number of observed auroras is small, and there is an appearance of another maximum in 1797. If we count this as a period of maximum abundance, then the mean interval between the successive maxima will be reduced to ten years, and the evidence at present appears to be in favor of this conclusion. Thus the observations of the aurora seem to indicate a *maximum* every ten years, and a *maximum maximorum* every fifty-nine or perhaps sixty years. If any doubt should still remain whether this phenomenon exhibits a true astronomical periodicity, it will probably be removed when we discover its connexion with the movements of the magnetic needle.

Disturbance of the magnetic needle.—The aurora is ordinarily accompanied by a considerable disturbance of the magnetic needle. This magnetic influence of the aurora has been known for more than a hundred years, and within the last thirty years it has been studied with great care. When an aurora consists merely of a bank of light like the dawn, and rises but little above the horizon, the disturbance of the magnetic needle is slight, while the effect increases with the brilliancy and extent of the aurora. Auroral beams generally cause a disturbance of the needle, particularly when the beams themselves are in active motion. Auroral waves or flashes, especially if they extend to the zenith, cause a violent agitation of the needle, consisting of an irregular oscillation on each side of its mean position. During the aurora of September 2, 1859, the entire range of the needle at Toronto was 3° 45′, and at Rome was 4° 13′.

These extraordinary deflections of the needle prevail almost simultaneously over large portions of the globe, even where the aurora itself is not visible; and they have been termed, by Baron Humboldt, magnetic hurricanes. On the 25th of September, 1841, an extraordinary disturbance of the magnetic instruments was observed at Greenwich. This disturbance affected both the direction and intensity of the magnetic needle. The changes in the direction of the needle were by sudden impulses; after each impulse the needle was stationary for a few seconds, then it was jerked to another position and was again stationary.* On the same day a remarkable disturbance of the magnetic instruments was

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* Greenwich Magnetic Observations, 1841, p. 48. London and Edinburgh Philosophical Magazine, December, 1841, p. 505.

observed at Toronto, distant from Greenwich more than 3,500 miles. The disturbances at Toronto commenced at nearly the same absolute time as at Greenwich, and they were generally simultaneous at both stations. The same extraordinary disturbance was noticed at St. Helena, distant from London 4,800 miles, and from Toronto 6,000 miles. A similar disturbance occurred at the Cape of Good Hope, still more distant, and also at Trevandrum, in India, which is almost diametrically opposite to Toronto. This phenomenon was noticed simultaneously over an entire hemisphere, and, not improbably, was sensible at every point of the earth's surface.

At the same time there occurred an auroral display of unusual extent. An aurora was observed throughout Canada and the northern parts of the United States,* as well as in England and Norway, and also in the southern hemisphere, at Van Dieman's Island.

During the great auroral display of September 2, 1859, the disturbances of the magnetic needle were still more remarkable throughout North America, Europe, and northern Asia, as well as in New Holland. At Toronto the declination of the needle changed 3° 45′ in half an hour. The inclination was observed to change 2° 49′ when the needle passed beyond the limits of the scale, so that the entire range of the needle could not be determined. The horizontal force was observed to change to the extent of *one-ninth* of its whole value when the needle passed beyond the limits of the scale, so that its entire range could not be determined.†

At Rome, September 2, at $7^h\ 10^m$ a. m., the declinometer pointed 2° 50′ to the west of its ordinary position. After this the needle returned rapidly to the east, and at $7^h\ 30^m$ pointed 1° 23′ east of its mean position, thus describing an arc of 4° 13′ in one-third of an hour. The bifilar indicated a diminution of the horizontal component amounting to about *one-eighth* of its mean value ‡

At Paris the magnetic instruments were very much disturbed, and were carried beyond the range of their scales, so that the extreme range could not be determined.§

At St. Petersburg the declination of the needle changed 4° 24′ when the needle passed beyond the range of its scale, so that the entire range could not be determined. For a similar reason the entire change of the horizontal intensity could not be determined.||

At Christiania the variation of the horizontal intensity amounted to nearly one-thirteenth of its whole value.¶

At Melbourne, Australia, at the same time the magnetic instruments were very much disturbed, the range of the declination being 1° 9′, and that of the horizontal intensity *one-thirtieth* of its whole value.**

In the volume of the Greenwich Magnetical and Meteorological Observations for 1862, Professor Airy has given an abstract of the magnetic observations from 1841 to 1857, made on days of great magnetic disturbance.

The magnetic force of the earth is resolved into forces acting in the direction of three rectangular axes; two of which lie in a horizontal plane, one pointing north and south, the other east and west, while the third axis has a vertical position. From Professor Airy's abstract, it appears that out of 170 magnetic storms observed at Greenwich in seventeen years, 63 per cent. of the whole number *began* with westerly force +; and 60 per cent. *ended* with westerly force +. Of the whole number, 66 per cent. *began* with northerly force —,

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* Hough's N. Y. Met., p. 480.

† American Journal of Science, n. s., v. 28, p. 390.

‡ American Journal of Science, n. s., v. 29, p. 397.

§ American Journal of Science, n. s., v. 29, p. 391.

|| American Journal of Science, n. s., v. 30, p. 80.

¶ American Journal of Science, n. s., v. 29, p. 387.

** American Journal of Science, n. s., v. 32, p. 8.

and 91 per cent. *ended* with northerly force —. The disturbance in a vertical direction was sometimes positive and sometimes negative, with about equal frequency.

The following table presents a summary of the results for twenty cases of the most remarkable disturbance, including all the cases in which the mean disturbance of the declination magnet amounted to fourteen minutes. In the column headed "declination magnet," the plus sign shows when the western declination was *greater* than the mean, and the minus sign when it was *less* than the mean. In the two columns headed "horizontal force" and "vertical force magnet," the plus sign shows when the force was greater than the mean, and the minus sign when it was less than the mean.

In order to reduce the table to convenient limits, the numerical values of several of the changes are not given; but the direction and number of the changes are indicated by the repetition of the signs +, —, +, etc.

Magnetic storms at Greenwich, from 1841 *to* 1857.

Date.	DECLINATION MAGNET.			HORIZONTAL FORCE MAGNET.		VERTICAL FORCE MAGNET.	
	Beginning and end of wave.	Mean disturbance.	Equivalent in terms of horizontal force.	Beginning and end of wave.	Mean disturbance.	Beginning and end of wave.	Mean disturbance.
	h. *m.* *h.* *m.*	′		*h.* *m.* *h.* *m.*		*h.* *m.* *h.* *m.*	
September 25, 1841	0 0 — 3 40	+ 14.0	+ .0041	0 2 — 6 59	+ .0057	1 47 — 13 3	+ .0089
	3 40 — 3 51	— 14.3	— .0041	....	....	....	....
	3 51 — 6 49	....	+ — +	....	....	....	....
November 18, 1841	6 0 — 17 17	— 26.0	— .0075	6 2 — 23 55	— .0007	5 57 — 11 9	+ .0007
	17 17 — 23 54	+ 5.9	+ .0017	....	....	11 9 — 23 57	— +
December 14, 1841	6 0 — 8 35	+ 1.0	+ .0003	6 2 — 16 2	— .0013	5 57 — 15 58	+ .0024
	8 35 — 16 0	— 14.2	— .0041	....	....	....	....
July 3, 1842	14 0 — 16 39	— 18.4	+ .0054	14 2 — 24 2	— .0065	13 57 — 20 40	— .0024
	16 39 — 23 41	+ 6.9	+ .0020	....	....	20 40 — 23 57	+ .0015
May 6, 1843	10 0 — 14 24	— 16.7	— .0049	10 2 — 14 8	— .0055	9 58 — 12 36	— .0013
						12 36 — 14 8	+ .0006
March 19, 1847	0 0 — 6 16	....	+ — +	0 2 — 5 13	+ .0011	1 48 — 9 10	+ .0013
	6 16 — 9 17	— 18.8	— .0055	5 13 — 20 2	— .0061	9 10 — 19 58	— .0037
	9 17 — 20 0	....	+ — + — +	....	....	....	....
May 7, 1847	14 0 — 22 0	+ 14.8	+ .0043	14 2 — 17 46	+ .0011	13 58 — 23 58	— .0004
				17 46 — 22 2	— .0017	....	....
September 24, 1847	0 0 — 2 23	— 17.0	— .0049	0 2 — 1 23	— .0026	0 59 — 9 43	+ .0038
	2 23 — 3 37	....	+ —	1 24 — 3 16	+ .0132	9 43 — 17 58	— .0020
	3 37 — 6 2	+ 27.8	+ .0081	3 16 — 3 48	— .0048	....	....
	6 2 — 9 19	....	— +	3 48 — 6 39	+ .0307	....	....
	9 19 — 10 15	— 20.0	— .0058	6 39 — 18 2	— .0064	....	....
	10 15 — 18 0	....	+ — +	....	....	....	....
October 24, 1847	0 14 — 10 20	....	+ — + —	0 14 — 1 27	— +	0 14 — 8 48	+ .0033
	10 20 — 11 43	+ 20.0	+ .0058	1 27 — 2 11	— .0014	8 48 — 10 24	— +
	11 43 — 13 40	— 16.6	— .0048	2 11 — 8 1	+ .0040	10 24 — 14 36	— .0025
	13 40 — 23 33	+ 14.1	+ .0041	8 1 — 23 33	— .0149	14 36 — 23 58	+ — +
December 19, 1847	13 57 — 21 11	+ 14.6	+ .0042	13 58 — 23 58	— .0091	....	Small.
	21 12 — 23 57	— 16.0	— .0047	....	....	....	....
December 20, 1847	0 0 — 1 35	— 14.5	— .0042	0 0 — 1 59	+ —	....	Small.

	1 35 — 4 25	+ 13.2	+ .0038	1 59 — 5 1	+ .0110	....	....
	4 25 — 5 32	— 17.1	— .0050	5 1 — 5 29	— .0056	....	....
	5 32 — 18 2	....	+ — + — + — +	5 29 — 18 3	+ —	....	....
October 18, 1848	1 52 — 10 27	....	+ — +	1 42 — 7 35	— +	6 5 — 13 13	— .0053
	10 27 — 13 22	— 15.6	— .0045	7 35 — 12 15	— .0058	....	....
November 17, 1848	1 0 — 8 1	+ 12.4	+ .0036	1 20 — 2 37	— .0006	1 47 — 9 2	+ .0023
	8 1 — 9 57	....	— +	2 37 — 3 43	+ .0003	9 2 — 10 2	— .0006
	9 57 — 13 33	— 28.2	— .0082	3 43 — 20 40	— .0115	10 2 — 10 33	+ .0020
	13 33 — 21 0	....	+ — +	....	....	10 33 — 20 43	— + — +
September 7, 1851	0 2 — 7 9	....	+ — +	0 7 — 7 15	+ — + — +	0 5 — 7 26	+ .0030
	7 9 — 9 5	— 14.4	— .0042	7 15 — 7 38	+ .0016	7 26 — 16 30	— .0049
	9 5 — 23 0	....	+ — +	7 38 — 23 59	— .0048	16 30 — 23 55	+ .0005
September 29, 1851	1 12 — 8 58	....	— +	0 3 — 3 32	— .0005	1 30 — 19 45	— .0082
	8 58 — 17 9	— 17.5	— .0051	3 32 — 11 18	+ .0032	19 45 — 23 55	+ .0001
	17 9 — 23 55	....	+ — +	11 18 — 23 55	— .0056	....	....
October 28, 1851	0 32 — 14 1	— 8.0	— .0023	0 30 — 14 48	+ —	1 0 — 23 55	— .0031
	14 1 — 23 38	+ 16.4	+ .0048	14 48 — 23 10	+ —	....	....
December 6, 1851	0 12 — 7 59	+ 3.6	+ .0010	0 30 — 23 55	— .0054	0 12 — 22 50	+ .0014
	7 57 — 14 55	— 15.7	— .0046	....	....	....	....
	14 55 — 23 30	....	+ —	....	....	....	....
February 14, 1852	1 30 — 22 0	....	+ — +	0 30 — 21 46	+ .0036	1 28 — 8 33	— .0022
	22 0 — 23 36	— 23.0	— .0067	21 46 — 23 40	— +	8 33 — 23 32	+ — +
February 19, 1852	2 38 — 4 35	+ 2.4	+ .0007	0 28 — 11 3	— .0049	1 0 — 23 58	— .0044
	4 35 — 15 43	— 18.1	— .0053	11 3 — 15 36	+ .0059	....	....
	15 43 — 23 35	+ 13.9	+ .0040	15 36 — 23 58	— + —	....	....
April 10, 1854	0 7 — 13 26	....	— +	0 5 — 13 19	+ .0021	1 23 — 11 36	— .0008
	13 26 — 18 2	— 16.6	— .0048	13 19 — 16 3	— +	11 36 — 12 10	+ .0001
	18 2 — 23 59	....	+ — +	16 3 — 23 59	— .0047	12 10 — 23 59	+ .0021

These irregular deflections of the magnetic needle do not occur everywhere simultaneously. From a comparison of a very large number of observations made in the years 1836 to 1841, at twenty-seven stations scattered over Europe from latitude 45° to 60° N., I have discovered that they are propagated over the surface of Europe in a direction from N. 28° E. to S. 28° W., at the rate of about 100 miles per minute.* From a similar comparison of observations made at Washington, Philadelphia, Cambridge, and Toronto in the years 1840 to 1842, I have discovered that in North America those irregular deflections of the magnetic needle are propagated in a direction from N. 68° E. to S. 68° W., at the rate of about 100 miles per minute.† Mr. C. V. Walker has determined that the direction of this motion in England was from N. 42° E. to S. 42° W.

Influence of the aurora upon the telegraph wires.—Auroras exert a remarkable influence upon the wires of the electric telegraph. During the prevalence of brilliant auroras the telegraph lines generally become unmanageable. The aurora develops electric currents upon the wires, and hence results a motion of the telegraph instruments similar to that which is employed in telegraphing; and this movement being frequent and irregular, ordinarily renders it impossible to transmit intelligible signals. During the aurora of September 2, 1859, the currents of electricity on the telegraph wires of the United States were so steady and powerful that, on several lines, the operators succeeded in using them for telegraph purposes as a substitute for the battery; that is, telegraph messages were transmitted from the auroral influence alone, without the use of any voltaic battery.‡ This result clearly proves that the aurora develops on the telegraph wires an electric current similar to that of a voltaic battery, and differing only in its variable intensity.

These electric currents during the auroras of August 29 and September 2, 1859, moved alternately to and fro over the earth's surface, their average direction being probably from about N. 45° E. to S. 45° W.

Similar effects were noticed upon the telegraph lines of Europe. In Switzerland the intensity of the currents was measured by a galvanometer, and was found to be *three-fold* the ordinary current employed in telegraphing. Two currents were found to succeed each other, having a general direction nearly along a meridian line; the one proceeding from north to south having a double intensity and a double duration, the other proceeding from south to north having a less intensity and a less duration.

From careful observations of galvanometers upon the telegraph lines of England during the auroras of August 29 and September 2, Mr. C. V. Walker discovered that there was a stream of electricity of indefinite width drifting across the country, moving to and fro along a line directed from N. 42° E. to S 42° W.

THEORY OF THE POLAR LIGHT.

1. Some have ascribed the polar light to a rare nebulous matter occupying the interplanetary spaces, and revolving round the sun at such a distance that a portion of this matter occasionally falls into the upper regions of the atmosphere with a velocity sufficient to render it luminous, from the condensation of the air before it. But we can see no reason why matter, reaching the earth from such a source, should be confined to certain districts of the earth, and be wholly unknown in other portions. During a single month, or possibly an entire year, the fall of such matter might be limited to certain parts of the earth; but that certain portions of the earth should *always* be exempt from such visits while other portions receive them uninterruptedly from night to night, is quite

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* American Journal of Science, n. s., v. 32, p. 334.

† American Journal of Science, n. s., v. 34, p. 38.

‡ American Journal of Science, n. s., v. 29, pp. 92–97.

incredible. Now we have found that, throughout a large portion of the torrid zone, auroras have never been known to occur; while throughout a zone surrounding the magnetic pole they are seen almost uninterruptedly during the period that the sun's light does not obscure them from our view. The aurora, then, does not result from nebulous matter encountered by the earth in its progress round the sun.

2 Auroral exhibitions take place in the upper regions of the atmosphere, and partake of the earth's rotation. All the celestial bodies have an apparent motion arising from the rotation of the earth; but bodies belonging to the earth, including the atmosphere and the clouds which float in it, partake of this rotation, so that their relative position is not affected by it. The same is true of the aurora. Whenever a corona is formed, it maintains sensibly the same position in the heavens during the whole period of its continuance, although the stars meanwhile revolve at the rate of 15° per hour. Auroral exhibitions are therefore to be regarded as terrestrial phenomena.

3. The light of the aurora is caused by the movement of atmospheric electricity. This is proved by its effect upon the telegraph wires. The electric telegraph is worked by a current of electricity generated by a battery, and flowing along the conducting wire which unites the distant stations. This current flowing round an electro-magnet renders it temporarily magnetic, so that its armature is attracted, and a mark is made upon a roll of paper. During a thunder-storm the electricity of the atmosphere affects the conducting wire in a similar manner, so as to set in motion the recording pen in the telegraph office; and thus, during a thunder-storm, telegraphing generally becomes quite impossible. A similar effect is produced by the presence of an aurora. During the great aurora of November 17, 1848, the electro-magnets of the telegraph lines were rendered magnetic, even when no voltaic battery was attached to them, so that, for three hours, communication by telegraph was rendered impracticable.*

During the aurora of September 2, 1859, the aurora caused so strong and steady a current of electricity on the telegraph wires, that it was possible to transmit telegraph messages by the use of this current without any voltaic battery whatever. During this aurora there were remarked all those classes of effects which are considered as characteristic of electricity.

A. In passing from one conductor to another, electricity exhibits a spark of light. During the auroras of August 28 and September 2, 1859, brilliant sparks were drawn from the telegraph wires, even when no battery was attached. At Springfield, Massachusetts, a flash was seen about half the size of an ordinary jet of gas. At Boston, Massachusetts, a flame of fire followed the pen of Bain's chemical telegraph. At Pittsburg, Pennsylvania, streams of fire were seen when the telegraph circuit was broken. At Washington, D. C., a spark of fire jumped from the forehead of a telegraph operator when his forehead touched a ground-wire. Bright sparks were noticed on the conductors of the telegraph lines to Bordeaux, in France. On the telegraph lines of Norway sparks and uninterrupted discharges were observed.†

B. In passing through poor conductors electricity develops heat. During the auroras of August 28 and September 2, paper and even wood were set on fire by the auroral influence alone. At Boston, Massachusetts, a flame of fire burned through a dozen thicknesses of paper. The paper was set on fire and produced considerable smoke. At Springfield, Massachusetts, the heat was sufficient to cause the smell of scorched wood and paint to be plainly perceptible. At Pittsburg. Pennsylvania, the magnetic helices became so hot that the hand could not be kept on them. On the telegraph lines of Norway, pieces of paper were set on fire by the sparks of the discharges from the wires, and the

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* De La Rive's Elec., v. 3, p. 287.

† American Journal of Science, n. s., v. 32, p. 323.

current was at times so strong that it was necessary to connect the lines with the earth in order to save the apparatus from destruction.*

C. When passed through the animal system, electricity communicates a shock which is quite peculiar and characteristic. During the auroras of August 28 and September 2, some of the telegraph operators received severe shocks when they touched the telegraph wires. At Philadelphia the current gave a severe shock. At Washington, D. C., the telegraph operator received a severe shock, which stunned him for an instant.†

D. A current of electricity develops magnetism in ferruginous bodies. The aurora of September 2 developed magnetism so abundantly and so steadily that, on several lines, it was used as a substitute for a voltaic battery in the ordinary business of telegraphing. The intensity of this effect was estimated to have been at times equal to that of 200 cups of Grove's battery upon a line 230 miles in length. In Switzerland the currents were at least three-fold the ordinary current employed in telegraphing.‡

E. A current of electricity deflects a magnetic needle from its normal position. In England the usual telegraph signal is made by a magnetic needle surrounded by a coil of copper wire, so that the needle is deflected by an electric current flowing through the wire. Similar deflections were caused by the auroras of August 29 and September 2, and these deflections were frequently greater than those produced by the telegraph batteries.§

F. A current of electricity produces chemical decompositions. During the display of September 2 the auroral influence produced the same marks upon chemical paper as are produced by an ordinary voltaic battery; that is, the auroral influence decomposed a chemical compound, the cyanide of potassium. The same effect was produced by the aurora of February 19, 1852.‖

G. Certain bodies, such as fluor spar, the solution of sulphate of quinine, and several vegetable infusions possess the remarkable property of so dispersing some part of the light passing through them that the course of the luminous rays become visible, as though the body were self-luminous. This phenomenon has been termed *fluorescence.* This fluorescence is produced in a very remarkable degree by the light of an electric discharge, and the same effect is found to be produced by the light of the aurora. On the 14th of March, 1858, during the exhibition of a brilliant aurora, Professor Robinson, of Armagh observatory, found that a drop of disulphate of quinine on a porcelain tablet seemed like a luminous patch on a faint ground; and crystals of platino-cyanide of potassium were so bright that the label on the tube which contained them (and which by lamplight could not be distinguished from the salt at a little distance) seemed almost black by contrast. These effects were so strong in relation to the actual intensity of the light that they appeared to afford additional evidence of the electric character of the aurora.¶

The preceding facts are regarded as proving, conclusively, that the fluid developed by the aurora on the telegraph wires is indeed electricity. This electricity may be supposed to be derived from the aurora either by transfer or by induction. If we adopt the former supposition, then the auroral light is certainly electric light. If we adopt the latter supposition, then we must inquire what known agent is capable of inducing electricity in a distant conductor. We know of but two such agents—magnetism and electricity. But the auroral fluid is luminous, while magnetism is not luminous. We seem, then, compelled to admit that the auroral light is electric light.

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* American Journal of Science, n. s., v. 32, p. 323.

† American Journal of Science, n. s., v. 32, p. 323.

‡ American Journal of Science, n. s., v. 32, p. 324.

§ American Journal of Science, n. s., v. 32, p. 324.

‖ American Journal of Science, n. s., v. 32, p. 324.

¶ Lond. Ed. and Dub. Phil. Mag., v. 15, 4th ser., p. 326.

4. The *colors* of the aurora are the same as those of ordinary electricity passed through rarefied air. When a spark is drawn from an ordinary electrical machine, in air of the usual density, the light is intense and nearly white. If the electricity be passed through a glass vessel in which the air has been partially rarefied, the light is more diffuse, and inclines to a delicate rosy hue. If the air be still further rarefied, the light becomes very diffuse; it flows readily through a great distance, and its color becomes a deep rose or purple. The same variety of colors is observed during the aurora. The transition from a white or pale straw color to a rosy hue, and finally to a deep red, depends, probably, upon the height above the earth and upon the amount of condensed vapor present in the air.

The emerald green light which is seen in some auroras, is thought to be due to the projection of the yellow light of the aurora upon the blue sky; for a combination of yellow and blue light always produces green. So also during the evening twilight there is frequently a brief period when the western sky exhibits a delicate shade of green. This is caused by a combination of the yellow light of the sun with the blue of the celestial vault. If this explanation should not seem to account for the intensity of the green light which has been noticed in some auroras, the difference may perhaps be ascribed to that well-established physiological principle that when two complementary colors are placed near each other, each color appears more brilliant by contrast with its complementary color.

5. The formation of an auroral corona near the magnetic zenith is the effect of perspective, resulting from a great number of luminous beams all parallel to each other. A collection of beams parallel to the direction of the dipping needle would all appear to converge towards the pole of the needle, as is actually observed; and no other supposition will explain all the appearances. Each observer, therefore, sees the auroral crown in his magnetic zenith, and it is not the same crown which is seen at different places, any more than it is the same rainbow which is seen by different observers.

6. The auroral beams are simply illumined spaces, caused by the flow of a stream of electricity through the upper regions of the atmosphere. During the aurora of August 28, 1859, these beams were nearly 500 miles in length, and their lower extremities were elevated about 45 miles above the earth's surface. Their tops inclined towards the south; the angle with the vertical at New York amounting to 17°. When electricity flows through good conductors, it emits no light. Dry air of the ordinary density is a non-conductor of electricity; but water is a conductor, and so is rarefied air. When electricity forces its way through dry air of common density, it exhibits a brilliant spark. Through rarefied air electricity passes with less resistance to a much greater distance, and with a pale diffuse light.

It was formerly supposed that the electric current necessarily moved in the direction of the axis of the auroral beams; that is, that the electric discharge was between the upper regions of the atmosphere and the earth or the lower regions of the atmosphere. But recent discoveries throw some doubt upon this conclusion. When a current of electricity flows through a vessel from which the air is almost wholly exhausted, under certain circumstances the light is not uniformly diffused through the vessel, but becomes stratified, exhibiting alternately bright and dark bands crossing the electric current at right angles. From this experiment it might be inferred that electricity flowing *horizontally* through the upper regions of the atmosphere might exhibit alternately bright and dark bands, having a position nearly vertical like the auroral beams. But this stratification of the electric light is generally ascribed to *intermittences* in the intensity of the electric discharge, and it does not seem probable that such intermittences could take place in nature with sufficient rapidity to produce a similar effect. It seems, therefore, more probable that auroral beams are the result of a current of electricity travelling in the direction of the axis of the beams.

7. The slaty appearance of the sky, which is remarked in all great auroral exhibitions, arises from the condensation of the vapor of the air; and this condensed vapor probably exists in the form of minute spiculæ of ice or flakes of snow. In the Arctic regions fine flakes of snow have been repeatedly observed to fall during the exhibition of auroras, and this snow only slightly impairs the transparency of the atmosphere, without presenting the appearance of clouds.* The presence of these minute flakes of snow produces that turbid appearance of the atmosphere which invariably attends bright auroras, and causes that dark bank or segment which in the United States rests on the northern horizon. This turbidness is more noticeable near the horizon than it is at great elevations, because near the horizon the line of vision traverses a greater depth of this hazy atmosphere, while the effect is increased by contrast with the light above it. When the aurora covers the whole heavens, as in the neighborhood of Hudson's bay, the entire atmosphere is filled with this haze; and if the aurora goes far beyond the zenith of the observer towards the south, he sees for the same reason a dark segment resting on the southern horizon.

8. *What is the source of the electricity of the atmosphere?*—Philosophers are by no means agreed as to the origin of atmospheric electricity. It has been ascribed successively to friction, combustion, and vegetation, but these causes seem entirely inadequate to account for the enormous quantities of electricity sometimes present in the atmosphere.

Evaporation is probably the principal source of atmospheric electricity. The following experiment shows the production of electricity by evaporation. If upon the top of a gold leaf electrometer we place a metallic vessel containing salt water, and drop into the water a heated pebble, the leaves of the electrometer will diverge. The vapor which rises from the water is charged with positive electricity, while the water retains negative electricity. The water used in this experiment must not be perfectly pure, but must contain a little salt or some foreign matter. The evaporation of the water of the ocean must, therefore, furnish a large amount of electricity; and fresh water must also furnish some electricity, for the water of the earth is never entirely pure. The vapor that rises from the sea, therefore, constantly carries away positive electricity, while the solid part of the earth must be charged with negative electricity.

9. The vapor which rises from the ocean in all latitudes, but most abundantly in the equatorial regions of the earth, carries into the upper regions of the atmosphere a considerable quantity of positive electricity, while the negative electricity remains in the earth. This positive electricity, after rising more or less vertically with the ascending currents of the atmosphere, would be conveyed towards either pole by the tropical current of the upper regions of the atmosphere. This tropical current, setting out from the equator where it occupies the most elevated regions of the atmosphere, descends in proportion as it advances towards the higher latitudes, until in the neighborhood of the poles, where it approaches the earth's surface.

The earth and the rarefied air of the elevated atmospheric regions may be regarded as forming the two conducting plates of a condenser, of which the insulating stratum is the inferior portion of the atmosphere. The two opposite electricities must then be condensed by their mutual influence in those portions of the atmosphere and of the earth to which they are nearest; that is, in the regions near the poles, and there neutralize themselves in the form of discharges whenever their tension reaches a certain limit. When the air is humid, it becomes a partial conductor between the upper regions of the atmosphere and the earth, by which means a portion of the electricity of the atmos-

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* Franklin's First Expedition, pp. 583 and 600.

phere is conveyed to the earth. On account of the low conducting power of the medium, the neutralization of the opposite electricities would not be effected instantaneously, but by successive discharges, more or less continuous, and variable in intensity. These discharges should take place almost simultaneously at the two poles, since the electric tension of the earth should be nearly the same at each pole.

Figure 5 represents the system of circulation here supposed, the north and south poles of the earth being denoted by the letters N. and S.; and this, as I understand it, is substantially the theory of Professor De la Rive.

10. When electricity from the upper regions of the atmosphere discharges itself to the earth through an imperfectly conducting medium, the flow could not be everywhere uniform, but would take place chiefly along certain lines where the resistance was least; and if the air be sufficiently rare, this current must develop light, forming thus an auroral beam. It might be supposed that these beams must necessarily have a vertical position, but their position is controlled by the earth's magnetism. Professor Plucker, of Bonn, has shown that "when magnetic forces act upon a perfectly flexible conductor through which an electric current passes, equilibrium can only exist when the conductor assumes the form of a magnetic curve."* Now, the axis of the dipping needle at any point on the surface of the earth lies in the magnetic curve passing through that point. Hence the axis of an auroral streamer must lie in the magnetic curve which passes through its base.

During the prevalence of a brilliant aurora the inclination of the needle sometimes changes to the extent of two or three degrees. Hence the auroral streamers cannot always preserve the same position, but their average inclination should not differ much from the mean dip of the magnetic needle. Hence results an apparent convergence of all the beams towards the magnetic zenith, forming the auroral corona.

11. Auroral arches assume a position at right angles to the magnetic meridian, in consequence of the influence of the earth's magnetism. Auroral arches generally consist of a collection of auroral beams all nearly parallel to each other. These beams tend to arrange themselves upon a curve which is perpendicular to the magnetic meridian, forming thus a ring about the magnetic pole. The same law has been discovered to hold true for a stream of electricity under the influence of an artificial magnet. When electricity escapes from a metallic conductor under a receiver from which the air has been exhausted, it escapes in streams of diffuse rosy light which appear to diverge from the conductor. But Professor De la Rive has shown that if this conductor be the pole of a powerful magnet, the electric light forms a complete luminous ring around this conductor, and this ring has a movement of rotation around the pole of the magnet, sometimes in one direction and sometimes in another, according to the direction of the discharge and the direction of the magnetization.†

A similar effect takes place on a grand scale during auroral exhibitions. The auroral arch is a part of a luminous ring sustained everywhere at about the same elevation above the earth, having the north magnetic pole for its centre, and cutting all the magnetic meridians at right angles. The influence of the north magnetic pole of the earth determines this position, as the pole of an artificial magnet determines the electricity which escapes from it to assume the form of a ring.

12. We have found that auroral arches are not always exactly perpendicular to the magnetic meridian, and that in some places this deviation is pretty uniform and is considerable in amount. At Bossekop (latitude 70°) the average deviation is 10° towards the west of the magnetic meridian. We can ascribe

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* Lond. Ed. and Dub. Phil. Mag., 4th ser., v. 18, p. 2.

† De la Rive's Elec., vol. 2, p. 248; and Lond. Ed. and Dub. Phil. Mag., June, 1862, p. 2.

occasional deviations of two or three degrees to the changes in the position of the magnetic needle which are observed during great auroral exhibitions; but permanent deviations indicate the operation of some constant cause. The following is substantially the explanation suggested by M. Bravais.* The direction of the magnetic needle at any place is determined mainly by its position with respect to the magnetic poles of the earth, but partly by local causes, such as the conformation of the land and sea, the structure of the earth in that vicinity, &c. In consequence of these local causes, the direction of the magnetic needle at some places differs several degrees from what it would be if it were controlled entirely by the magnetic poles. Now this local and disturbing influence probably diminishes as we rise above the earth's surface, so that at the height of one or two hundred miles the direction of the magnetic needle may differ several degrees from that at the surface of the earth. In northern Europe the north end of the magnetic needle points several degrees more easterly than it should if the magnetic meridians were entirely symmetrical. Hence it seems not improbable that in this region the declination of the magnetic needle increases as we rise above the earth's surface at the rate of one degree to about ten miles' deviation, and this supposition will reconcile our theory with the observations.

13. The flashes of light so frequently observed in great auroral displays are due to *inequalities* in the motion of the electric currents. In consequence of the imperfect conducting power of the medium through which it passes, the flow of electricity through the upper regions of the air is not perfectly uniform. It experiences more or less resistance to its motion, and hence escapes through the air by paroxysms. The flashes of the aurora are therefore feeble flashes of lightning.

14. *Cause of the magnetic disturbances.*—The disturbance of the magnetic needle during an aurora is due to the currents of electricity flowing through the atmosphere or through the earth. A magnetic needle is deflected from its mean position by an electric current flowing near it through a good conductor, like a copper wire. A stream of electricity flowing through the earth or the atmosphere must produce a similar effect. The direction in which the magnet is deflected may always be known from the rule given by Ampère: "If you conceive yourself lying in the direction of the current, the stream of positive electricity flowing through your head towards your feet, with the north pole of the magnet before you, the north pole will always be deviated toward the right."

It is probable that the directive power of the magnetic needle is due to electric currents circulating round the globe from east to west. If there were such electric currents circulating round the globe in planes parallel to the magnetic equator, the effect of such currents would be everywhere to cause the magnetic needle to assume a position corresponding very nearly with what is actually observed. M. Lamont, of Munich, thinks he has proved, by direct observation, the existence of such currents constantly circulating from east to west, over the surface of the earth.†

According to the theory of Professor De la Rive, already explained, there is a general system of circulation of positive electricity from the equator towards either pole, through the upper regions of the atmosphere; in the higher latitudes this positive electricity makes its way to the earth, and it travels thence towards the equator to restore the equilibrium which is continually disturbed by evaporation from the waters of the equatorial seas. This current through the earth from the north polar regions southward, must modify the regular current of electricity which we suppose is constantly circulating from east to west. Hence during the period of great auroral displays we should expect a

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* Voyages en Scandinavie, p. 458.

† Bib. Univ. de Genève, 1861, v. 12, p. 357.

current from the northeast to the southwest, and such a current has been positively indicated in a most decisive manner—

1. By observations on the telegraph lines of England with a galvanometer needle.*

2. By observations on the telegraph lines of the United States and other countries.†

3. By simultaneous observations of the magnetic declination made in 1836 to 1841 at numerous stations scattered over Europe.‡

4. By similar observations at several stations in North America, made from 1840 to 1842.§

This current of electricity does not, however, flow steadily and uninterruptedly from northeast to southwest, but alternates at short intervals with a current in the contrary direction. This fact was distinctly noticed in September, 1859, upon the telegraph lines of the United States. It was also distinctly observed and measured upon the telegraph lines in Switzerland, where a northerly current continued for two or three minutes and then slowly declined, when it was succeeded by a southerly current of less intensity, which continued for sixty or ninety seconds and then declined, to be succeeded by another current from the north—the northerly current having a double intensity and a double duration; the other, proceeding from south to north, having a less intensity and a less duration.‖

In England, the northerly currents are also generally stronger than the southerly, and they continue for a longer time, but the difference is less than was observed in Switzerland.¶

Such currents of electricity must produce a continual disturbance of the magnetic needle, and they seem sufficient to account for the disturbances actually observed. Mr. C. V. Walker has compared magnetic observations made at Greenwich and Kew, and has discovered that the deflections of the magnets there observed were such as should be produced by the electric currents observed on the telegraph wires.**

15. *Effect of the aurora upon telegraph wires.*—The effect of the aurora upon the telegraph wires is similar to that of electricity in thunder-storms, except in the intensity and steadiness of its action. During thunder-storms the electricity of the wires is discharged instantly with a flash of lightning, while during auroras there is sometimes a steady flow for a few minutes, which may even be employed as a substitute for the voltaic battery, in transmitting telegraph messages.

16. *The geographical distribution of auroras.*—The geographical distribution of auroras appears to depend chiefly upon the relative intensity of the earth's magnetism in different latitudes. The circumstances favorable to a grand display of the auroras appear to be—1. The upper portion of the atmosphere must be highly charged with electricity. 2. The atmosphere must be filled with particles of condensed vapor, probably in the form of minute crystals of ice. 3. This condensed vapor must form an imperfect conductor of great extent, for the passage of the electricity from one portion of the heavens to another, and from the upper atmosphere to the earth. According to the experiments of De la Rive with artificial magnets, the electric light should be most noticeable in the neighborhood of the magnetic pole, but not directly over the pole, since the electric light tends to form a ring around the pole, and at some

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* London Phil. Trans. 1861, p. 106.

† American Journal of Science, n. s., v. 32, p. 325.

‡ American Journal of Science, n. s., v. 32, p. 334.

§ Ibid., v. 34, p. 39.

‖ Comptes Rendus, t. 49, p. 662.

¶ Lond. Phil. Trans. 1861, pp. 128, 129.

** Ibid., pp. 111, 112.

distance from it.* Auroras are therefore most abundant along a certain zone which follows nearly a magnetic parallel, being everywhere nearly at right angles to the magnetic meridian of the place.

17. *Why auroras do not occur within the tropics.*—Auroras do not prevail within the tropics, on account of the high intensity of the electricity, combined with the high temperature of the lower atmosphere. By the rapid evaporation within the tropics a vast amount of electricity is daily elevated into the air; but on account of the general dryness of the air this electricity is to a great extent insulated, and cannot flow back again to the earth. Whenever there is a general condensation of the vapor of the air, the precipitation is copious on account of the large supply of vapor, and dense clouds are formed which are pretty good conductors of electricity. The electricity thus accummulates and acquires great intensity, moving with explosive violence in thunder-showers, instead of the slow and silent discharges of the aurora.

By an extensive comparison of observations,† I have determined that—

Between latitude 0° and latitude 30° the average number of thunder-storms annually is 52
Between latitude 30° and latitude 50° the average number of thunder-storms annually is 20
Between latitude 50° and latitude 60° the average number of thunder-storms annually is 15
Between latitude 60° and latitude 70° the average number of thunder-storms annually is 4
Beyond latitude 70° 0

Thus we see that atmospheric electricity is most abundant in the equatorial regions, where the causes which develop it are the most active; and as we recede from the equator, thunder-storms diminish in frequency, while auroras increase in frequency, because circumstances favor a slow and quiet rather than a rapid and violent discharge. The aurora and lightning differ, then, chiefly in the *mode* of discharge of electricity from the atmosphere to the earth.

18. *Cause of the diurnal inequality in the frequency of auroras.*—The diurnal inequality in the frequency of auroras is probably due to the same causes as the diurnal variation in the intensity of atmospheric electricity. The intensity of atmospheric electricity is found to vary with the hour of the day. From the mean of three years' observations made at London, it appears that at 4 a. m. the electric tension is represented by 20 on Volta's electrometer; from this hour the electricity increases to 10 a. m., when it is represented by 88; from that time it decreases to 4 p. m., when it is represented by 69; it then increases to 10 p. m., when it is represented by 104; from which time it decreases till 4 a. m.; that is, there are two daily maxima of intensity and two daily minima.‡

The variations in the intensity of atmospheric electricity are to be ascribed partly to real changes in the amount of electricity present in the air, and partly to variations in the conducting power of the air. Just before sunrise the electricity has a feeble intensity, because the moisture of the preceding night has transmitted to the earth a portion of the electricity which was previously present in the air. After the sun rises new vapor ascends, and carries with it positive electricity, and the amount of electricity in the air increases. Towards noon the air becomes dry, and transmits less readily the electricity accumulated in the upper regions of the atmosphere; so that, although the amount of electricity in the air is continually increasing, an electrometer near the earth's surface indicates an apparent diminution. Towards evening the air grows cool, again becomes humid, and transmits more readily to the earth the electricity accumulated in the upper regions of the atmosphere. The effect produced upon an electrometer, therefore, increases until some hours after sunset; but since during the night there is a constant discharge of electricity from the air to the earth, the

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* De la Rive's Elec., v. 2, p. 248.
† American Journal of Science, n. s., v. 30, p. 97.
‡ Report British Assoc. 1849, pp. 117–191.

electrometer soon indicates a diminished intensity, which continues until towards morning.

The same causes which favor the escape of electricity from the upper atmosphere to the earth will produce an aurora whenever the electricity of the upper air is sufficiently intense, and the conducting power of the air is favorable for the slow transmission of an electric current.

19. *Cause of the annual inequality in the frequency of auroras.*—The unequal frequency of auroras in the different months of the year appears to depend partly upon the amount of electricity present in the upper air, and partly upon the humidity of the air by which this electricity may be discharged. The supply of electricity must be greatest when the evaporation is most rapid, that is, in summer; and this is probably the reason why in North America auroras are more frequent in summer than in winter; and it is not improbable that, were it not for the longer continuance of daylight in summer, auroras would then be more frequent than at any other period of the year. In Europe auroras are seldom seen in midsummer, because, in those latitudes to which auroras are almost exclusively confined, twilight in midsummer continues all night.

20. *Cause of the secular inequality in the frequency of auroras.*—The secular inequality in the frequency of auroras seems to indicate the influence of distant celestial bodies upon the electricity of our globe. The periods of auroras observe laws which are very similar, if not absolutely identical, with those of at least two other phenomena, viz: the mean diurnal variation of the magnetic needle, and the frequency of black spots upon the sun's surface.

It is found that the north end of the magnetic needle has, in the morning, a regular motion eastward amounting to from one to three minutes, when the declination in New England is usually less than at any other hour of the day, and may, therefore, be called the minimum. This minimum during the winter is attained about nine o'clock, but during the summer months commonly as early as seven. The needle then gradually deviates to the west, and attains its greatest westerly bearing about two o'clock in the afternoon, when the declination is greater than at any other hour of the day, and may, therefore, be called its maximum. From this time the needle again returns to the eastward, till it attains its original bearing, about 10 o'clock. During the night another small oscillation occurs, the north pole moving west until 3 a. m., and returning again as before. The mean daily change of the magnetic needle not only varies with the locality, but also varies from one year to another at the same locality; and these variations present a decided appearance of periodicity. In the following table, column third shows the mean daily variation of the magnetic needle at the stations named in the fourth column for the years mentioned in the first column. These numbers are derived mostly from a table furnished by Professor R. Wolf, and published in Poggendorff's Annalen for 1862, v. 193, p. 503.

Column second of the same table shows the relative frequency of the solar spots, as determined by a collection of about 20,000 observations made by Professor Wolf.

Column fifth shows the years in which auroras exhibited a maximum or a minimum frequency.

Year.	Relative number of spots.	Magnetic variation.	Magnetic station.	Auroras.	Year.	Relative number of spots.	Magnetic variation.	Magnetic station.	Auroras.
		′					′		
1749	63.8				1807	10.0?			
1750	68.2			Maximum	1808	2.2			
1751	40.9				1809	0.8			
1752	33.2				1810	0.0			
1753	23.1?				1811	0.9			Minimum.
1754	73.8				1812	5.4			
1755	6.0			Minimum	1813	73.7	6.56?	London	
1756	8.8				1814	20.0?	7.62	do	
1757	30.4				1815	35.0?	7.66?	do	
1758	38.3?				1816	45.5			
1759	48.6?	10.76	London		1817	43.5	8.55?	do	
1760	48.9			Maximum	1818	34.1	8.81	do	Maximum.
1761	75.0				1819	22.5	7.77	do	
1762	50.6				1820	8.9	7.79	do	
1763	37.4				1821	4.3	9.10	Paris	
1764	34.5				1822	2.9	8.83	do	
1765	23.0				1823	1.3	8.18	do	Minimum.
1766	17.5?			Minimum	1824	6.7	8.20	do	
1767	33.6				1825	17.4	9.67	do	
1768	52.2				1826	29.4	9.76	do	
1769	85.7				1827	39.9	11.31	do	
1770	79,4			Maximum	1828	52.5	11.52	do	
1771	73.2				1829	53.5	13.74	do	Maximum.
1772	49.2				1830	59.1	12.40	do	
1773	39.8				1831	38.8	12.17?	do	
1774	47.6?				1832	22.5			
1775	27.5				1833	7.5			Minimum.
1776	35.2?			Minimum	1834	11.4	7.79?	Göttingen	
1777	63.0	11.2?	Montmorency		1835	45.5	9.57	do	
1778	94.8	10.0?	do		1836	96.7	12.34	do	
1779	99.2	8.5?	do	Maximum	1837	111.0	12.27	do	
1780	72.6?	5.5?	do		1838	82.6	12.74	do	
1781	67.7	9.12	Mannheim		1839	68.5	11.03	Prague	Maximum.
1782	33.2?	8.11	do		1840	51.8	8.84	do	
1783	22.5?	8.77	do		1841	29.7	7.43	do	
1784	4.4?	6.98	do	Minimum	1842	19.5	6.34	do	
1785	18.3	8.56	do		1843	8.6	6.57	do	Minimum.
1786	60.8	14.00	Paris		1844	13.0	6.05	do	
1787	92.8	15.14	do		1845	33.0	6.99	do	
1788	90.6	13.48	do	Maximum	1846	47.0	7.65	do	
1789	85.4?	12.60?	London		1847	79.4	8.78	do	
1790	75.2	14.85?	do		1848	100.4	10.75	do	Maximum.
1791	46.1	12.27?	do		1849	95.6	10.27	do	
1792	52.7?	8.87?	do		1850	64.5	9.97	do	
1793	20.7?	8.43	do		1851	61.9	8.32	do	
1794	23.9	8.27?	do		1852	52.2	8.09	do	
1795	16.5	7.48?	do		1853	37.7	7.09	do	
1796	9.4	8.02?	do		1854	19.2	6.81	do	
1797	5.6	8.30?	do		1855	6.9	6.41	do	
1798	2.8	7.44?	do	Minimum	1856	4.2	5.98	do	Minimum.
1799	5.9	7.56?	do		1857	21.6	6.95	do	
1800	10.1	7.14?	do		1858	50.9	7.41	do	
1801	30.9?	7.74?	do		1859	96.4	10.37	do	
1802	38.3?	8.58?	do		1860	98.6	10.05	do	
1803	50.0?	9.16?	do		1861	77.4			Maximum.
1804	70.0?	8.48?	do	Maximum	1862	59.4			
1805	50.0?	8.72?	do		1863	44.4			
1806	30.0?				1864	45.6			

Upon inspecting this table we perceive that the mean annual range of the magnetic needle exhibits periodical variations, and these changes show a remarkable regularity during the last twenty-five years, embracing a period during which the needle has been observed with much greater care than formerly. The interval from one maximum to another is a little more than ten years. We also perceive that the successive maxima are not equal to each other, but exhibit variations which indicate a periodicity, showing a tendency to attain their greatest value after an interval of about five periods, or from fifty to sixty years.

The relative frequency of the solar spots exhibits a similar periodicity, and the maximum number of spots corresponds in a remarkable manner with the maximum value of the magnetic variation. Indeed, for the past forty years the times of maxima and minima of the two phenomena have been almost absolutely identical, and seem to favor the conclusion that the apparent anomalies occasionally noticeable in the earlier observations are due mainly to the incompleteness of the observations; for it is only since 1826 that any one has undertaken to keep a continuous record of all the spots visible at any time upon the sun's disk; and before 1818 the observations of the magnetic declination were, for the most part, only occasional, and are generally insufficient to determine, in a perfectly satisfactory manner, the *mean annual* range of the magnetic needle. The observations seem also to indicate that the successive maxima are not all equal to each other, but are themselves subject to a periodicity; one period extending from 1779 to 1839, comprehending an interval of fifty-eight years

We have found that auroras exhibit a periodicity, the last period extending from 1779–'80 to 1839–'40, embracing an interval of about sixty years; and during this period there have been indications of alternate maximum and minimum abundance, corresponding in a remarkable manner with the maxima and minima of the solar spots, if we except the period from 1800 to 1820, during which auroras were too infrequent to afford the basis for a safe deduction.

It seems, then, pretty well established that these three phenomena, the solar spots, the mean daily range of the magnetic needle, and the frequency of auroral exhibitions, manifest two distinct periods; one a period of from ten to twelve years, the other a period of from fifty-eight to sixty years. The first of these periods corresponds to one revolution of Jupiter, and it has been suspected that Jupiter might be the occasion of these periodical disturbances of the sun's surface. If Jupiter does, indeed, exert such an influence, then it is to be presumed that Saturn must exert a similar, though less powerful influence, which would have a period of about thirty years; and since five revolutions of Jupiter correspond nearly to two of Saturn, embracing a period of fifty-nine and a half years, at the end of this period the two planets return to nearly the same relative positions, and their joint action should exhibit a period of fifty-nine and a half years.

If Jupiter and Saturn do, indeed, exert such an influence upon the sun's surface, then it seems probable that each of the other planets must also exert an influence, (though perhaps inappreciable;) and the earth may exert a direct influence upon the sun's surface, causing an inequality in the solar spots, whose period is one year; and Venus and Mercury may exert a similar influence.

If we inquire for the mode in which the planets might exert an influence upon the sun's surface we are left almost entirely to conjecture. But one plausible hypothesis is that there are continually circulating around the sun powerful electric currents, which currents may possibly be the source of the sun's light; that these currents act upon the planets, developing in them electric currents, or modifying the currents developed in them by the action of other forces; that these currents circulating around the planets react upon the solar currents with a force depending upon the variable distance of the planet, and therefore having periods corresponding to the times of revolution of the planets. These disturbances of the solar electric currents may be one cause of the solar spots, and an unusual disturbance of the solar currents may cause a simultaneous disturbance of the electric currents of the earth's surface, giving rise to unusual manifestations of electric light—that is, to unusual displays of the aurora.

There is also another cause which may produce a change in the number of auroras visible at one station, viz., the gradual change of position of the terrestrial magnetic poles. The dip of the magnetic needle at Paris in 1671 was 75°, while in 1864 it was only 66° 3′, showing a diminution of 8° 57′ in 193 years, being an average diminution of 2.7′ annually. Observations made in other parts of Europe give similar results. In the northeastern part of the United States the dip attained its minimum about 1843, and is now slowly increasing.* This change of dip implies a change of distance from the magnetic pole, and probably a corresponding change in the frequency of auroral displays. We do not know what is the cause of this motion of the magnetic poles, and whether the motion is periodical. It has been conjectured to be the result of great geological changes going on in the crust of the earth. But whatever may be its origin, this cause must modify and complicate the influence of extra-terrestrial bodies upon the earth's magnetism.

21. *Why do great auroral exhibitions take place simultaneously in both hemispheres of the earth?*—During the years 1841–'8, as has been mentioned before, there were recorded at Hobarton, latitude 42° 52′ south, 34 auroras. In 29 of these cases an aurora was recorded either in Europe or America, and in the 5 remaining cases there was recorded an unusual disturbance of the magnetic needle, indicating the existence of an aurora at no very remote station. The great auroral displays of August 29 and September 2, 1859, were among the most remarkable ever recorded in the northern hemisphere. Both of them were conspicuous at Cuba, where but *four* auroras had ever before been recorded. The aurora of September 2 was seen in latitude 12° north, where there was no tradition that such a phenomenon had ever been seen before. In the southern hemisphere an aurora occurred simultaneously, and was almost equally remarkable for its brilliancy as well as its geographical extent.

We cannot explain the great auroral displays in the northern hemisphere by supposing that the electricity of the atmosphere is temporarily diverted from one hemisphere and concentrated in the other. Such an idea is entirely refuted by observations of the mean range of the magnetic needle, which exhibits its maxima simultaneously in both hemispheres. This is shown by the following table, which exhibits the amount of the mean diurnal variation at Prague, latitude 50° 5′ north; Toronto, latitude 43° 40′ north; and Hobarton, latitude 42° 52′ south.

Years.	Prague.	Toronto.	Hobarton.
	′	′	′
1841	7.43	9.50	8.28
1842	6.34	8.67	7.75
1843	6.57	8.90	7.66
1844	6.05	8.87	7.84
1845	6.99	9.41	8.39
1846	7.65	9.27	9.06
1847	8.78	10.40	9.93
1848	10.75	12.11	10.63
1849	10.27	11.77	8.13
1850	9.97	10.88	8.57
1851	8.32	10.15	6.65

The great disturbances of the earth's magnetism, therefore, take place simultaneously in both hemispheres, and they exhibit the same periods. Now, we cannot suppose that the absolute amount of electricity for the entire globe, as developed by evaporation from the water of the ocean, should undergo any such periodical variation, for we know that the mean temperature of the earth's surface does not change sensibly from one year to another, and hence we seem compelled to ascribe these great auroral displays in no small degree to the direct action of the sun, through the agency, perhaps, of its magnetism, or of the

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* U. S. Coast Survey Report 1856, p. 244.

electric currents circulating around it, which electric currents are sensibly disturbed by the action of the larger planets. Such an effect might be expected to take place simultaneously in both hemispheres, and in conformity with the results of experiments with artificial magnets, the exhibition of light should take place chiefly in the region about the magnetic poles of the earth.

We are thus led to regard great auroral displays as no longer an exclusively atmospheric phenomenon, and as being to an important extent the result of the influence of extra-terrestrial forces. But if these extraordinary electric currents are mainly determined by extra-terrestrial forces, then since the earth exhibits many of the properties of a great and permanent magnet, the two magnetic poles of the earth ought to exert opposite influences, and we should expect that the currents in the neighborhood of the two poles would move in *contrary* directions. We are thus naturally led to infer a system of circulation somewhat similar to that suggested by Mr. B. V. Marsh*, and which is illustrated by figure 6, where N and S are supposed to represent the north and south magnetic poles of the earth, *n* and *s* the poles of an imaginary magnet, representing the magnetism of the earth. The east and west bands represent auroral arches upon which stand auroral streamers. The dotted lines represent magnetic curves passing from auroral streamers in the northern hemisphere to streamers in the southern hemisphere, showing the path pursued by the currents of electricity in passing from one hemisphere to the other above the atmosphere. It is not clear from Mr. Marsh's paper that he supposed a regular flow of positive electricity through the earth from north to south, and above the atmosphere from south to north, but this seems to me to be necessary to render his hypothesis complete.

If, then, we regard great auroral displays as mainly determined by terrestrial forces, the system of circulation previously described seems the most natural one; but if they are determined mainly by extra-terrestrial forces, the system of circulation just described appears the most probable. The two hypotheses substantially agree, so far as the phenomena can be observed in the northern hemisphere, but they lead to opposite results in the southern hemisphere, where the first hypothesis supposes that the motion of positive electricity along the surface of the earth is from south to north, and the latter supposes it to be from north to south. If the direction of this motion could be determined by direct observation, it would decide between the two hypotheses; and such observations might doubtless be made in Australia. During the auroras of August 28 and 29, 1859, the wires of the electric telegraph in Australia were disturbed to such a degree that it was almost impossible to transmit any continuous message.† It does not appear that any measures were adopted to determine the direction of these electric currents. If, during some future auroral display, such observations could be made, they would probably furnish the experimentum crucis to decide between these two hypotheses.

In attempting to explain the phenomena of the aurora, I have been led to describe hypotheses which, by the progress of science, are liable any year to be disproved. These hypotheses, therefore, must not be regarded as established principles, but simply as convenient formulæ for connecting facts which otherwise appear disjointed. Such hypotheses often prove useful for the promotion of science by suggesting new observations or researches, like the one just mentioned respecting the direction of electric currents in the southern hemisphere.

In conclusion, I will make a few suggestions addressed especially to the meteorological observers of the Smithsonian Institution:

1. It is desirable that there should be preserved a continuous and complete record of all visible auroras at a variety of stations. Such observations are de-

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* American Journal of Science, n. s., v. 31, p. 311.

† American Journal of Science, n. s., v. 32, p. 8.

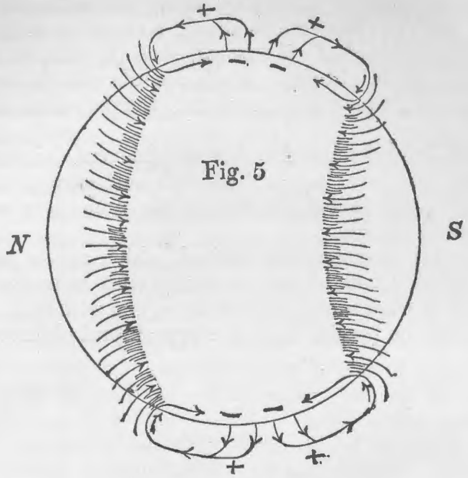

Fig. 5

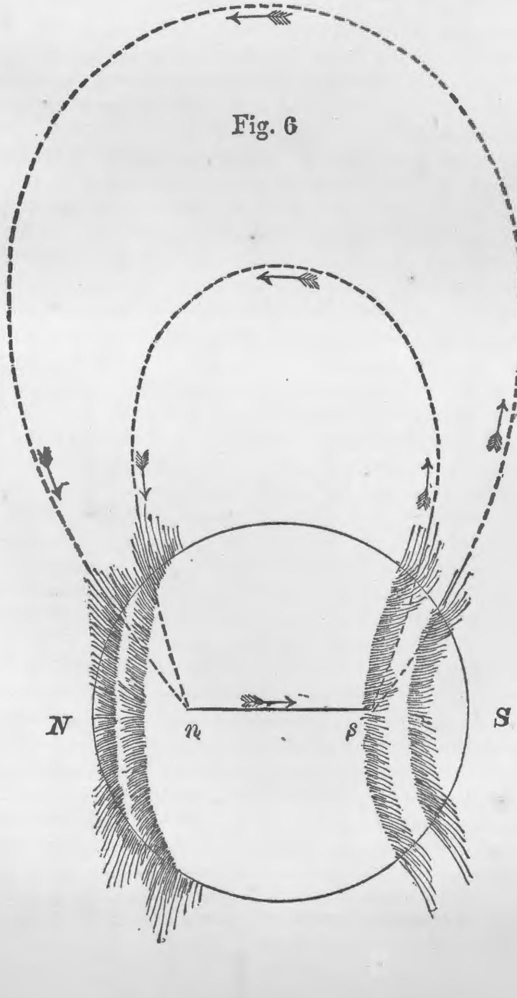

Fig. 6

sirable from a series of stations stretching along the entire northern frontier of the United States, and the observations should be continuous from year to year. They should embrace a record of *all* auroras, even the faintest, and should state concisely their degree of brightness. Such records will, in time, enable us to decide the questions which may arise respecting the periodical character of these phenomena.

2. Whenever well-defined auroral arches are observed, it is important to locate accurately their east and west extremities with reference to the magnetic meridian. We may thus find that the vertex of the arch does not ordinarily lie *exactly* in the magnetic meridian.

3. Whenever auroral streamers are noticed, which are so peculiar that they may be easily identified, locate them accurately with reference to neighboring stars, and determine in what direction they move, and with what angular velocity.

4. Record carefully the precise time of every auroral observation.

ON THE SENSES.

1.—THE SENSE OF FEELING.

Translated for the Smithsonian Institution from the German periodical, "*Aus der Natur, u. s. w.*," Leipzig.

The senses, those open portals of the soul through which its perception of external things is constantly streaming in; the sources from which, whether consciously or unconsciously, it derives its impulses to thought and action, are, to many even of the educated, possessions so little understood and so wrongly appreciated in many important respects, that the attempt to afford a clear insight into their manifold and marvellous sphere of activity can scarcely be otherwise than acceptable. It is not the mechanism of that activity, the structure of the apparatus and the complex and connected series of incidents which occupy the seemingly slight interval between the vibration of a string or flash of a sunbeam, and the well recognized sensations of sound or light, which form the sole problems of such a discussion. The physiologist, with his vast apparatus of knowledge and expedients, stands here before a mysterious deep, into whose darkness no hypothesis sheds light, for penetration into whose recesses no accessible path or guiding hand offers itself to his keenest researches. We may wonder at the rapid and brilliant development of modern physiology, even as regards its inquiries into the theory of sensation, at the dexterity of the microscopist in disentangling the structural complexities of the organs of sense, the accuracy with which the path of light has been traced in the *camera obscura* of the eye, and the form of the vibration communicated by the air-wave to the apparatus of the ear; but everywhere, in regard to every organ, we come upon an abrupt boundary to our researches—the edge of that enigmatical deep, within which lies hid the true and intrinsic germ of the physiology of sense. What passes in the delicate filament of the optic nerve when a wave of light strikes its extremity in the retina of the eye? That is the first great problem, whose solution may now, perhaps, have been brought within reach, but which has heretofore lain at an unapproachable distance. We know that it is a motion which, in the fibre of the eye, telegraphs to the brain the arrival of the light-wave; an ingenious savant has even measured the velocity of that motion, but the moving force, the matter moved, and the form of the movement, are as yet unknown. But were even this problem solved, did the mechanism of the organs and the processes in the nervous fibres, which convey the impressions of the outer world to the soul, lie before us in noonday clearness, there would yet remain for us the last and most difficult problem: How does this physical motion, which shoots along the nerve-filaments, become in the brain a conscious sensation? Sensation and current (since thus we must express ourselves) are in the nervous fibre two wholly different and in nowise comparable things—just as much so as the despatch of the telegraph and the electric stream which traverses the wires; we know that the sensation is produced and its conditions determined by the current, but the nature of the causative connexion is to us a mystery, like that of the sensation itself. These passing intimations, which will assume in the course of our inquiries a more definite and intelligible form, are here premised only to show that we should undertake an impossibility did we aim to communicate to the novices of science a complete comprehension of the mechanism of the action of the senses. We fear, indeed, that for some of the teachings of the theory of physical sensation, which are clear enough to the learned, it will be difficult to find in the circuit of popular ideas pencil or colors with which to trace a clearly comprehensible image. No doubt, indeed, that at the present day, when both fashion and profit conspire to popularize the treasures of science, there may be found limners who, in comparison with effectiveness, consider any plain portraiture of the objects of sober science quite a secondary affair; but we eschew all affiliation with artists of this sort; we willingly exchange the cheap glitter of the parade for the honor of standing as a sentry before the sanctuary of science. There is a vast deal in the sphere of the physiology of the senses, which may be plainly translated into the language of the laity of science, and which is of the more interest to them as false representations and notions are deeply rooted and widely prevail in the ideas and expressions of mankind.

How little do men understand the operations of their senses, how little are they qualified to form a right conception of the nature and import of a simple sensation, pertain to whichever of the senses it may, so as to separate, on the one hand, this bare sensation from the multifarious impressions which insensibly combine with it, and to distinguish, on the other hand, the qualities of the sensation from the properties of the external objects and incidents which occasion it. One or two examples will substantiate this charge, and many a reader, we are convinced, will with surprise hear that named an error which he holds to be unquestionable truth and the result of direct observation. You hear the sound of a string which is struck, and speak of the "resounding string;" you see the leaves of the trees and designate the green color as a property of the leaves; you taste sugar and impute to the sugar the sweet taste: these are all errors! The string only vibrates—it does not sound; the sound originates in yourself, is the peculiar and no further to be explained sensation which arises when the vibrating string has by its oscillations set in motion the particles of air, a motion which these particles convey to the tympanum of the ear, this in turn to the small bones of that organ, these to the fluid of the so-called labyrinth, and this to the extremities of the auditory nerves, when it is propagated through these nerves by the unknown movement which we have designated above as "a current" to the brain, and by a suitable adjustment of that apparatus produce the condition of our sentient nature of which we are conscious as sound. The original vibrations determine the character of the sound, but have nothing in common with it. The same is the case with the green color of leaves: the leaves are not green; they but possess the quality of producing the sensation which we term green, without knowing what green is, except that it depends on the presence between the eye and the leaf of an impalpable fluid or ether whose tremulous palpitations are propagated to the organ of sight in the form of waves. When a wave of this sort strikes upon the extremities of the nerves in the back of the eye, that unknown current is again produced in the appropriate filaments, and this it is which calls into being in the brain the sentient condition which we call the sensation of light. This wave of the ethereal particles in vibration has, like the waters of the agitated sea, a determinate length, a determinate velocity, which science has measured, though the ether itself be wholly inscrutable. Were the waves which proceed from the leaf longer or shorter, were the velocity of the vibrations greater or less, they would excite in the perceptive faculty, through the nerves, a differently modified sensation which we should designate as a red, blue or yellow color. This then is the first striking error, that each of us considers the qualities of his own sensation to be the qualities of the external object or incident which is the cause of the sensation. It is an error hard to be eradicated, as it is entwined with our habits both of thought and speech, and finds countenance not only in the language of novices and poets, but even in that of sober science, physicists and physiologists still continuing to write of blue and red light, sounding strings, &c., as of old. But to proceed to other misconceptions. You press with the sensitive points of your fingers against some object, and believe that you feel this object immediately as one existing without your own body; in other words, the perception of the object touched seems a direct sensation, seems to be the substance of the sensation. Now, this is an illusion, a confounding of sensation with idea.

The perception of the object touched is a representation which the mind forms for itself from the sensation by help of certain recognitions derived from experience—an interpretation of the simple sensation, which the mind has patiently learned to supply in the years of childhood, but it is by no means the sensation itself. Could you be suddenly carried back to the first days of life, and with matured understanding observe the first sensations of your self-educating mind, you would become aware that from the touching of an object there results at first only what we may call, but scarce define in words, a sensation of pressure; that in the simple, original, *subjective* sensation there exists no perception of the place of contact or part of the skin pressed. The mind first gradually learns, by circuitous procedures, to be described below, that certain qualifying differences in the sensation are determined through the different points at which the skin is pressed, and thus first learns to set itself right as regards its widespread organ of feeling. The mind knows nothing at the outset of objects without us; this knowledge it first learns through the conscious movements of the organs of feeling, whereby it is taught that the same movement of a finger is now accompanied with a sensation of pressure, and now is not. The idea of external objects of sensation once acquired, it is easy, even without the help of the sense of sight, which must itself pass through the same schooling, to recognize an outer object as cause of the sensation, thus imparting to the latter the character of *objectivity*. The mind rapidly acquires practice in the interpretation of its sensations, no longer needs reflection to assign to each of them the appropriate idea, and finally connects this last so unconsciously and quickly with the sensation that they present themselves simultaneously. Forgetting the mental application by which this association has been brought about, the adult man takes the sensation and idea for one, or rather conceives the last to be the very essence of the sensation. This error is a general one, and becomes established in the process of mental development, so that nothing less was needed than the perspicacity and penetrating psychical analysis of one of our most eminent physiologists to fix irrevocably for science the precise boundary between sensation and idea, (*Vorstellung*.) This service we owe to C. H. Weber, and yet, despite the light which he has thrown upon it, a young physiologist has lately striven anew and obstinately to defend the existence of an objective sensation of touch, (tastempfindung,) a sensation, the immediate and real essence of which is the perception of an object touched. We could adduce many examples of the confusion of sensations, and representations immediately derived therefrom, for each sense affords them in abundance; nay, we are prone wholly to forget that the sensations are subjective, and think that our senses penetrate into the outward world, while, on the contrary, it is the outward world which penetrates through the senses into us. We speak of a force of vision, which carries our sight forth as it were into the immeasurable distance, while it is from the immeasurable distance that the waves of light pulsate into the interior of our eyes, and it is in the secret recesses of the brain that the mind first gathers the sensation from the appulse of the currents of the nerves.

I could hope, by these preliminary remarks, to have excited in my readers an interest in the mysterious functions of the senses, and the desire to bear me company in a survey of their practical workings, to the end that we may catch their sign-language in its natural simplicity, and eventually analyze the high and inexhaustibly varied signification of that language, the living sense which the dead sign acquires through the operation of the thinking spirit. It is the sense of feeling, whose nature and operations I shall first address myself to discuss, as it is in relation to this sense probably that intelligible ideas can be most readily conveyed to the general reader. At another time, perhaps, I may be permitted to attempt a popular exposition of the rest of the senses, especially the more complex ones of seeing and hearing, for which certainly a more comprehensive apparatus of previously acquired physical knowledge will be requisite.

If we ask, in the first place, what a sensation of touch is, no other reply can be given than an enumeration of the manifold kinds of sensation which pertain to this class; what is felt we cannot define, nor assign any characteristic token of the sensation. An explanation through the cause of the sensation, as, for instance, that the sensation of heat is that which results from the touch of a heated body, is no definition, merely a paraphrase, which gives us not the slightest insight into the nature of the sensation itself. Neither the feeling of pain nor hunger admits of being described, but can only be experienced, and were there any fortunate individual who had never felt bodily pain, and wished to know what pain is, we should never be able to satisfy his curiosity by words, but only by communicating to him a sensation of pain, from which he might prosecute his study of the idea at leisure. We must rely then entirely upon the experience of the reader when we recount the impressions of this sense; the knowledge of their nature can only consist in a remembrance of the sensations experienced. Sensations of *pain, tickling, shuddering, pleasure, hunger, thirst, pressure, heat, and cold,* are the different qualities of this feeling; other distinctions, it is true, obtain in common language, but, as will readily be seen, without reason. The generality of mankind are prone, as regards pain alone, to distinguish many various qualities, such as boring, burning, pricking, tearing, &c.; the sensation of pain, however, has probably but one quality, and the varieties named are to be referred merely to differences in its duration and intensity. Since we cannot, in general, more closely characterize any of these qualities of feeling, it is, of course, impossible to compare them and express in words the distinction between them. The above-named sensations admit, however, of a division into two distinctly marked classes, one of which, being the more definite, may be designated as the *sensation of touch,* (tastempfindung,) the other as *common feeling,* (gemeingefühl.) To the latter pertain sensations of pain, shuddering, tickling, pleasure, hunger, and thirst; to the former those of pressure and of temperature. The distinction between the classes will be elucidated by the following example. If we touch an object with the finger there arises a sense of pressure, but at the same time, as has been already stated, an idea not alone of the existence of an external object as cause of the pressure, but also of its size and form, the condition of its surface, its solidity and weight. If, on the other hand, we touch hot iron, there results a purely subjective sensation of pain, from which alone the mind derives no representation of the object which inflicts the pain and of its qualities; or, to choose a striking example, when pain is felt within the body, with but an obscure perception of the place of suffering, the mind is unable to form from the painful sensation the representation of an object through contact with which the pain may be occasioned, or of the properties, form, and extent of that object. The essential difference, to be more fully explained hereafter, between a common sensation and one of touch, consists, then, in this—that in the former it is the isolated subjective sensation which excites consciousness, while with the latter are associated objective ideas, through which alone the mind obtains a knowledge of the outward world in its manifold relations. And hence is the sensation of touch a genuine function of the sense, which the common feeling of pain is not.

In order to render clear the distinction between the sense of touch and common feeling, we are forced to take a somewhat wider view. Few of my readers, perhaps, have a correct idea of a nerve and its functions; and many, who rightly or wrongly impute every evil to the poor nerves, know as little how to justify the charge, as to appreciate the services of these scape-goats of the bodily machine. Honestly speaking, physiology itself can afford us no certain explanation of the nevous energy; it has a prudent distrust, however, and has chased from the temple many a time-honored error and empty phrase respecting the action of the nerves; and the elimination of error, we know, is the first step towards the knowledge of truth.

I assume that my readers have derived from other sources some knowledge of the nature, causes, and operations of the electric current. Few will be ignorant that such a current, however generated, may be conducted through a metallic wire, and, according to the nature of the apparatus with which that wire is connected, produce the most surprising and varied effects; that we may thus explode powder, drive the wheels of a car, and set in motion the machinery of a clock or a telegraph. Now, the innumerable nerve-filaments which traverse our bodies in all directions are similar to that metallic wire, and the unknown element of activity which shoots along the nerves is analogous to the electric current which flashes along the wire. We wish it to be observed, that we use the word analogous as implying that it is by no means the electric current itself which circulates in the nerves as in the conducting wires when in a state of activity. Long and persistent efforts have been made in the province of physiology to identify the active principle of the nerves with the electric current on account of some superficial resemblance in the phenomena; but more recent and exact inquiry has conclusively repudiated this hypothesis, by which the nervous fibres were consigned to the simple office of conductors, and might, it would seem, have been replaced by metallic wires. But though the nervous and the electrical currents are not identical, they are yet strikingly comparable with one another, and we know no better images by which, for general readers, the functions of the nerves can be made intelligible. For, as the electric stream can be produced in the conducting wire by different means and apparatus, so the process in the nerves which we call "a nervous current" can, in various ways, be excited; if, for instance, we lay a nerve bare in a living animal, that nerve is thrown into a state of activity; or, in other words, the current is produced by wounding or by pressure, by an electrical shock or the application of any irritating or corrosive substance. The agent by which such an effect is produced we call an irritant, and this, according to its nature, may be either mechanical, electrical, thermal, or chemical. But besides these irritants which excite the nerves by their direct action, there are a number of others, and those among the most important, which are only competent to that effect under certain circumstances, and with the help of an appropriate apparatus. In this class light and sound are the most prominent. We can easily satisfy ourselves by experiments brought to bear upon the exposed nerve of a leg for instance; neither of those active principles produces the slightest excitement. Lay bare even the optic nerve, and allow the sun to shine upon its fibres, no irritation will be manifested; but how different the effect when light is admitted to that living and wonderfully constructed apparatus by which the ends of those fibres are connected with the pupil of the eye. Analogous effects are witnessed with regard to sound, which then only is operative as a nervous irritant when its undulations have reached the extremities of the auditory nerves after having passed through the exterior mechanism of the ear and the fluid of the labyrinth. The undulations of light and sound, therefore, are only mediate or indirect irritants of the nerves, and excite the latter by means of a peculiar intermediate apparatus, in which most probably they produce some action, whether chemical or physical, by which the nervous current is set in motion. We might presume, for example, that the waves of light, when they strike upon this terminal apparatus of the optic nerves, produce in its substance a chemical change, and thus supply a chemical irritant, a conjecture not without support in the many well-known chemical effects of light, though not susceptible of previous proof. The nervous current excited by these different mediate or immediate irritants traverses the nerves, and, like the electric current of the wires, is adapted to the production of the most diversified effects according to the organization and apparatus with which the nervous fibre is in connexion. There is a large class of these filaments which, having their origin in the brain and spinal column, penetrate the flesh, and, in some manner not clearly understood, connect themselves with its elements, the muscular fibres. If a current be excited by some irritant in a nerve of this sort, that current will flow from the point of irritation to both extremities of the fibre; at the outward extremity it takes effect upon the muscular fibre, which is so constituted that it necessarily contracts under the operation of the nervous current. The part of the current which flows to the inner extremity in the brain and spine meets there with no arrangement through which any phenomenal effect is manifested; on the other hand, there exists at the inner extremity of every nerve proceeding to a muscle, an apparatus through which the will can excite a current in the particular fibre. The microscope shows us this apparatus in the form of a small bulbous body occupied by a turbid fluid and a nucleus, and physiology teaches us that through this bulb the physical energy acts upon the nervous fibre; but though we thus learn the state and purpose of the mechanism, not the slightest intimation do we gather of the nature of its action, of the way and manner in which through this bulb the presumed immaterial principle of the will communicates to the fibre the impulse from which results the contraction of the muscle and the varied movements of the limbs. This riddle we shall, perhaps, never solve; it would still be one if the process within the active fibre of the nerve were laid open; nay, could physiology surprise the vital activity of the terminal bulb itself, the problem would still remain inexplicable so long as the will, the last term of the causative series, subsists as a force independent of matter. The riddle only becomes more wonderful when we see that, at the inner end of most probably all the remaining nerve fibres in the brain and spine, are found the so-called *nerve cells,* being bulbous bodies, existing under wholly similar conditions. We distinguish from the nerves of motion, whose destination we have been considering, a second great class of nervous fibres as *nerves of sensation,* being all those whose excitation, by whatever irritant it be determined, produces some sort of sensation. Their fibres fully resemble those which effect the movements of the limbs, and here also we have to seek the nature of the apparatus at both extremities, as well as that which communicates the excitation as that through which the latter produces an effect.

We find the nerves of sensation stretching between the brain and spine on the one hand, and the exterior surface of the body, as well as almost all the internal organs, on the other. The apparatus of the inner extremity, therefore, is seated in the central part of the nervous system, that of the outer extremity in the organs of sense, the whole external surface of the skin, and in all sensitive internal organs, that is in all parts which yield a sensation on being wounded or irritated. The destination of the terminal apparatus is here, however, reversed, as compared with the nerves of the muscles, inasmuch as in the nerves of sensation the apparatus through which their current produces its sensitive effect is placed at the inner extremity in the spine and brain, while the organization connected with the outer extremity is destined to render the nerve susceptible of excitation by certain external agents. If we irritate a nerve of sensation, the optic nerve for instance, at any point of its course, as by compressing or electrifying it, a current flows to both ends of the irritated nerve: that which arrives at the inner extremity takes effect on the appropriate apparatus and produces in it a wholly unknown condition, whose result, as regards consciousness, is a sensation of light; the current, on the other hand, which is transmitted to the outer extremity of the fibre situated in the globe of the eye, meets there with no apparatus by which it can manifest any effect of a phenomenal nature. If, therefore, we divide the optic nerve near the brain, so as to sever its collective fibres from their terminal apparatus in the central nerve system, all irritation applied between the point of division and the outer extremity remains ineffectual, and no sensation can arise, just as no intelligence can be transmitted to a telegraph station between which and the place of operation the wire has been sundered. Heretofore it was thought that generally a nerve of sensation was capable only of conducting a current in one direction, namely, from its external to its internal extremity, while the nerve of motion, on the other hand, had no conducting capacity except in the opposite direction. At present, we know with certainty that every nerve fibre is alike qualified to conduct in both directions, the earlier and erroneous view having arisen from the circumstance that no means were known for demonstrating that the current may, in a nerve of sensation, be propagated towards the outer extremity, and, in a nerve of motion, towards the inner one, and hence the fact was rashly denied. Now that we possess an admirable expedient for rendering the existence of a current perceptible at any point of a nerve, we know that, for example, the optic nerve can conduct a current from the brain to the globe of the eye, though we also know that in life no current is conducted in this direction, because at the central extremity of the nerve there exists no apparatus of excitation; and even were an irritant applied at that extremity the current flowing outwardly must remain without effect, because at the external extremity there exists no apparatus adapted to the manifestation of its activity and the production of the related phenomena.

If we now ask further, how it happens that excitation of the optic nerves always produces a sensation of light, excitation of the auditory nerves a sensation of sound, and therefore by what causes single nerves are adapted to the transmission of different specific sensations, but one general answer can be given in advance. Formerly a solution was supposed to have been found by ascribing to each different nerve a different specific faculty, which was called its "specific energy," without proof, and without further explanation of the expression. At present, when clearer views have been obtained of the properties and powers of the nerves, a conviction has prevailed that all nerve-fibres possess like essential properties, and therefore like capacities for action. If this be the case, the causes of the difference in their actual operation should be sought for solely in the apparatus on which they operate; and we must suppose that what occurs in the excited optic nerve is the same with what occurs in the excited auditory nerve, and also in the nerves of motion, but that at the extremity of the first there is an appropriate apparatus which converts the nervous current into a sensation of light, at the extremity of the auditory nerve another which produces from the same current a sensation of sound, while the union of other nerves with the muscles enables the current to accomplish the phenomenon of motion. Mankind in general are disposed to believe, without closer inquiry, that the optic nerve transmits its peculiar sensation because it is stimulated by light, as does the nerve of hearing, because it is stimulated by sound, &c.; but it is easy to prove that the difference in the effect produced is not dependent on the difference in the means of excitation. Electricity is an irritant which stimulates all the nerves, but when we excite the optic nerve by an electric stroke, a sensation of light is invariably the result, as is that of sound when the auditory nerves are similarly assailed, the same agent of excitation thus producing a different effect, and in the case of each nerve the effect proper to it, which we must therefore regard as being determined by the nature of the apparatus of the inner extremity. If we examine this apparatus in different nerves with a view to detect supposed differences, we at once encounter the hitherto inexplicable riddle before referred to. So far as microscopic examination goes, it reveals to us, at the end of all the nerves of sensation in the brain and spinal marrow, organizations in which we seek in vain to distinguish any essential difference. Everywhere, there are seen the seemingly simple nucleated bulbs which we have designated as nerve-cells, and singularly enough they are to appearance the same bulbs which we find at the inner extremity of the nerves of motion, but which, as regards the latter, we have learned to consider as organisms for producing excitation, not as in the case of nerves of sensation organisms upon which the excited nerve is destined to operate, and through which its phenomenal action is manifested. And yet differences must exist, since it is incredible, even upon the most strictly spiritualistic principles, that an immaterial soul should be capable, from the same processes in the nervous matter, of creating for itself different sensations. The procedure by which a sensation of light is produced in the mind must of necessity be different from that by which a sensation of sound or of touch is provoked. If we can detect no such differences in the terminal apparatus of the nerves, the fault lies with our present defective means of investigation, and we must await further light from future researches.

Having thus seen that a numerous class of nerves are adapted by the nature of their inner terminal apparatus to become the vehicles of sensation, it remains for us to cast a glance at their outer terminations and the arrangements there *provided. Here we find those wonderful adaptations, the organs of sense, of* whose destination something has already been said. The capacity of the optic nerve, by virtue of its interior apparatus, to produce on being excited the perception of light, would be of little account if the nerve lay bare to the day, so as to respond by a sensation of light at the solicitation of every casual excitement, whether proceeding from a blow, from heat, cold, electricity, or chemical agency. It becomes, however, of inestimable value when the nerve, besides being adapted to one only definite kind of excitement, is carefully sheltered from every other, and when this specific excitement is one to which all other nerves are insensible. The optic nerve is destined for excitation through the undulations of light, and is provided at the end directed towards the outward world with the organs necessary for that purpose. Those undulations form naturally its sole and exclusive means of excitation, and are hence designated in science as the "adequate" irritant of the optic nerve. Only thus is it possible for the excitability of this nerve to convey to the sensorium an authentic impression of the outward world, so far as it is to be derived from the definite external agent, light. But how imperfect were the communications of the visual nerve if it simply apprised us of the presence and absence of light, and, perhaps, through the degree of excitation, of the intensity of its beams, if the only function of our eyes were to distinguish daylight and darkness! How vastly is their value enhanced through their power of placing external objects before us in an endless diversity of shapes, magnitudes, and colors! We can here only depict with a few strokes the principle of those exquisite performances which we hope hereafter to discuss in a more detailed manner. The first condition of this action is the capacity of the optic nerves to produce those differently qualified sensations which correspond to the different sorts of external light, that is to say, to the waves of different lengths formed by the vibrating particles of light, and which induce in our minds the perception of different colors. Each perception of color corresponds to a wave of the vibrating light-element of determinate length. That through this faculty alone the circle of intimations which the mind receives by means of the nerves of vision is greatly widened is at once evident. But as thus far we have spoken of one nervous current in general, as of one like process in all nerves, it is now proper to suggest a slight modification of this expression, without thereby vitiating, however, our comparison of the electric and the nervous currents. It is, indeed, very conceivable, and even probable, that the "current" in the nerves of motion which occasions the contraction of the muscles, is the same with that in the optic nerve through which the latter produces a sensation of light; the wholly unlike effects of the currents of both nerves may be completely explained by the difference of the apparatus on which they operate. But it is impossible that in one and the same fibre of the optic nerve exactly the same current should evoke now the sensation of red and now of blue light, according as the extremity of the fibre is struck by one or the other of the corresponding undulations; yet we know with absolute certainty that the very same fibre really produces differently qualified sensations of color, and that there are not fibres, some for the transmission of red and others of blue light. Since, then, one and the same sort of nervous current in the same fibre, with the same terminal apparatus, can by no possibility produce different effects, we must necessarily assume that there exist just as many modifications of the nervous current for the fibre of the optic nerve as we experience different qualities in sensations pertaining to color. Wherein these modifications consist we are not at present in a condition to explain, but we may with confidence assert that they can by no means pertain to processes essentially differing from one another, but are rather slight modifications of a process which is substantially one and the same. To this conclusion we are not led by observation of the results, that is to say, of the different sensations; for the sensations of blue and of red color are so little comparable with one another, that without other proofs of the relation we could scarcely infer from themselves that they are only different modifications of the same principle. It is by a comparison of the external and qualifying causes that we are guided to the above conclusion. Since we know that the sensations of red and of blue color are occasioned by the vibrations of the same luminous ether, and that it is only a small difference in the length of the waves of the vibrating medium which causes the qualitative difference of the sensations, we must also conclude that the qualities of the nervous currents differ as inconsiderably from one another as the external causes. It might be conjectured that the difference in the sensations proceeds only from a difference of velocity in the nervous current; but we guardedly use the word "conjectured" to avert the possibility of what is merely a suggestion being regarded as an assertion.

The most important function of the eye is certainly that by which it affords a perception of the relative position of the objects from which the rays of light are directed upon it. For this purpose there is placed before the superficial expansion of the outer extremities of the fibres of the optic nerve the *retina*, a complete optical apparatus, a *camera obscura*, which delineates on this surface formed by the ends of the nerves a small and distinct image of the objects before the eye, just as the *camera obscura* of the photographer projects such an image on the blank plate or sensitive collodion in the rear. On the structure of this natural *camera obscura* we need not here dilate. In the mere presence, indeed, of an image of external objects on the retina there lies no effective reason that the image should be perceived as such; on the contrary, it is clear that there can be here no question of direct perceptions of space or proportion; the question with which we have to deal is this: how do the sensations occasioned by an image connect themselves with such localizing representations as correspond with the actual local relations of the object? This takes place after the follow[illegible] manner: like the small stones in a Roman mosaic, the delicate points consisting the extremities of the optic nerve-fibres are disposed near one another [illegible] the above-mentioned surface of the eye in regular order. If one such point receive an impression of light there arises in the nerve-fibre proceeding from that point a current of excitation which is propagated to the brain, but which, (and this is a most important law,) being confined to this one fibre, remains "isolated," and is not taken up by the other fibres of the trunk of the optic nerve lying in immediate contact with the former. Now, if two different points receive simultaneously two different impressions from the light, so that two minute images fall at two different places on the retina, a nervous current will be directed from either point along the respective fibres to the brain, and each will produce its separate, and not a blended, sensation. Thus the mind, from two impressions, when different fibres are excited, acquires two sensations, and from three impressions three separate sensations; but the divided sensation conveys no immediate perception of a relative position. The place at which the three particles of light struck the retina, the situation of the ends of the irritated nerves, forms no part of the sensation; from the mere sensation we obtain no intelligence whether the rays of light struck the retina in a right line or angularly, or how widely separated are the points from which they proceeded. At the knowledge of these relations of space the mind first arrives by more circuitous processes. We must imagine that the sensations which the individual nerve fibres produce are distinguished from one another in some manner which cannot be clearly indicated; that the sensation of blue light, for instance, always bears with it, according to the fibre excited, a certain definite *token*, which, while it is constant for that one fibre, is distinguishable from the token of every other fibre. It is from these tokens that the mind learns to form for itself representations of relative position; or, in other words, learns to refer every such token to a determinate place in its instinctive representation of space, and consequently to assign every sensation, which is accompanied by this token, to the corresponding place in the space-picture. When, therefore, as was above supposed, three separate sensations of light arise simultaneously from three impressions, each of the impressions bears with it that localizing token, according to which the mind forms for itself a representation or idea of the relative situation and distance of the points from which the irritating rays proceed. If it be the image of a candle flame which falls on the retina, the separate sensations which arise are as many in number as the ends of the nerves irritated, and these separate sensations disclose through their localizing tokens that the exciting impressions lie near one another in the form of a flame. In what manner the mind learns to refer the visual sensations with the attending representations of position to objects exterior to the body and from which the rays proceed, to give them *objectivity*, in a word, has been shown above. It is needless to say that the mind originally knows nothing of the image on the retina as an object of sight, nor is aware of its presence; scientific research has first shown its existence and causative relation to the perceptions of light.

Thus much we have thought proper to say, by way of example, respecting the purpose and structure of the apparatus adapted to the exterior terminations of the nerves of sensation; it would detain us too long to dwell at present upon other provisions of this sort, such as that by which the undulations of sound are converted into a suitable irritant for the nerves of hearing, &c. We return from this digression upon the general physiology of the nerves and senses to a more particular consideration of the sense of feeling, with a hope that the foregoing discussion may have rendered what we have to say upon that and other special topics more easily intelligible.

Nearly all parts of our body are furnished with nerves of feeling—that is, with nerves whose excitation, through its effect on the cells at the interior termination of the nervous fibres, produces some one of the above recited sensations of feeling; and indeed there is one quality of such sensations which all these nerves are capable of generating, and which is therefore characteristic of them—the sensation, namely, of pain. Hence, would we ascertain, respecting any branch of nerves in the body, whether it contains any fibres of sensation, we have only to search for it in a living animal and observe whether the irritation of it is followed by indications of uneasiness, (outcry, attempt at flight,) a kind of experiment which, in the eyes of the laity, has rendered the ideas of physiologist and cruelty almost inseparable, but with as little justice, on the whole, as the association which, in the mind of a child, connects the physician with the suffering which it is his purpose to relieve.

Still, were the function of the nerves of feeling limited to the production of pain, we could blame no one for incredulity in regard to their utility. There are a great number, however, of the nerves of feeling, and, indeed, all those whose outer terminations are imbedded in the skin of the general surface of the body, and in the mucous membrane of the cavity of the mouth, which, besides the general feeling of pain under certain conditions, are destined, through a more than ordinary activity, to produce those peculiar modifications of feeling which, from their external causes, we denominate sensations of pressure and temperature, and which, on grounds above indicated, we contradistinguish from the common feeling as true sensations of the sense of touch. The nerves of the outer skin and of the mucous membrane of the mouth we term, therefore, nerves of touch. A pressure applied to the skin gives rise not only to the corresponding sensation of pressure, but to the perception of an object which presses, as well as of its form and size. Touching the skin with a moderately warm or cold body imparts a feeling of warmth or cold. On the other hand, if a muscular part, which likewise possesses nerves of sensation, be laid bare by incision, there results, not a sensation of pressure, but of pain; not the idea of an incisive object, but only a consciousness of subjective hurt; so, too, from the touch of a hot body there results, not a feeling of warmth, but of pain. On what conditions these functional activities of the nerves of the skin depend may be shown by a simple and easily repeated experiment. These nerves produce the specific sensations of pressure and temperature above referred to, then only, when the external irritants, whether of pressure, of heat, or of cold, operate upon the ends of the nerves existing in the skin, but not when those irritants act upon the nerve-fibres in their intermediate course between the skin and the brain. The nerves of sensation which terminate in the skin of a part of the finger become combined in their passage to the brain into one stem, which, at the place of the elbow, called by Germans "das Mäuschen," runs deep under the outer skin. Now, if we dip the finger of a hand in cold water we perceive a sensation of cold; but if, on the other hand, we immerse the elbow in the same water, though a sensation of cold is at first felt through the skin, after some moments, when the cold has penetrated the skin and reached the nerve-stem below, there arises a sensation of pain, which has nothing in common with that of cold, and suggests no idea of an external cold object as cause of the uneasiness.

How is this to be accounted for? The same irritant operating on the same nerve-fibres, and with so different a result! The difference is founded in this: that, at the *extremities* of the nerves on which the cold acts in the skin of both finger and elbow, an organic apparatus exists, which, set in action by the application of cold, produces in the nerve-fibres a peculiar sort of current, which conveys to the brain a sensation of cold. If, on the other hand, the cold strikes these nerve-fibres in their passage, it occasions, indeed, an excitation; but, as here the apparatus is wanting to effect a modification of the excitement, only the ordinary nerve-current is set in motion, which communicates the simplest sensation, the common feeling, namely, of pain. The same is the result in the case of pressure, which, producing at the *extremities* only a sensation of pressure, occasions pain when applied to the stem. In making the experiment mentioned, another noticeable distinction will occur to every attentive observer. On immersing the fingers we perceive the cold in the portions of skin touched by the cold water; with bandaged eyes we can exactly determine from the sensation on which finger and what parts of the same the cold takes effect. In dipping the elbow, however, till pain arises, we feel this pain, strangely enough, not at the place which is immersed, but in the skin of the hand and finger; in like manner. these are the parts which pain us when we strike that portion of the elbow under which runs the nerve in question; and thus, likewise, it is the foot and toes which are benumbed when, by pressure on that part of the thigh under which lies the nerve running to those members, the feeling is produced which we call being "asleep." Thus, while with the sensations of touch there is connected a *right* perception of the point at which the exciting cause operates, there is a *false* perception in the case of the common feeling of pain; we transfer the place of uneasiness to those parts where the nerves of the irritated member terminate. A striking instance of this erroneous transposition is frequently witnessed in cases of amputation; if the ends of the divided nerves in an amputated arm or leg become irritated, it is usual for the patient to feel the pain in the finger or toes of the removed member. The error is easily explained if we investigate the origin of the localizing sensation. In no sensation, either that of touch or of pain, is the perception of place originally comprised; we can have as little direct perception of a place as of an object; we acquire only indirectly and mediately, by help of the sensation, an idea of the place of excitation and of an exciting external object. The origination of both ideas is intimately connected. Further on, when we specially consider the sense of place, (Ortsinn,) it will be shown that the localization of a sensation of touch and the perception of space through the sense of touch depend on analogous conditions, as we have already incidentally stated in regard to the perception of space through the sense of sight. In like manner, every sensation of touch has, for each fibre which causes it, a peculiar sign or token from which we learn to form the idea of the situation of the terminal point of that fibre in the skin, and we soon attain a proficiency which enables us instantaneously to distinguish the local sign which corresponds with the sensation. But the same sign also accompanies the sensation generated by any particular fibre when this is irritated, not at its extremity, but at some point in its passage; no wonder then that the mind is deceived, and assigns the origin of the sensation to that point to which, by the customary token, it has been taught at first to refer it. Thus the pain communicated by the nerve-fibres of the elbow is referred to all the points in which those collective fibres terminate. That, moreover, it is only the sensations of touch and temperature—sensations arising from excitation of the ends of the nerves in the skin—which relate to *outer objects*, while the sensations of pain, on the other hand, are always *subjective*, may be shown in the following manner, thus establishing a still wider and substantial difference between the two classes of sensations—that of touch and that of common feeling. We learn to distinguish external objects as causes of sensation by satisfying ourselves of the endless variations of the sensation arising from the movement of the sensitive parts of our body towards exterior objects, or of those objects towards our organs of sensation. That we arrive at this knowledge is owing to two essential conditions: first, that our organ of touch, the sensitive surfaces of our skin, are, in the highest degree and the most different directions, movable; and secondly, that we possess a peculiar, but generally unnoticed sense, which continually apprises us that our limbs are in a state of rest or of motion; and if the former, what is their actual position; if the latter, what is the extent and direction of the movement. Close your eyes and execute all possible movements, you will be always and accurately conscious what motion has been performed by each member, what has been at each moment the relative position of each limb in regard to every other.

To return to the important help afforded by this sixth sense in the operations pertaining to touch: the organs of this sense are the muscles, each of which in acting, or by the contraction through which the points of attachment at its ends are brought nearer together, produces a sensation whose intensity is proportioned to the effort and the extent of the actual contraction. The mind learns to interpret these feelings of effort like other sensations; it ascertains that, with every change of position of the limbs, a sensation of definite quality and intensity is connected, and hence, when experience is sufficiently matured, involuntarily associates with every such sensation a correct idea of the nature and extent of the corresponding movement. How, now, do those sensations of movement co-operate with the sensations of touch in the formation of objective ideas? We experience that one and the same movement, of the hand, for instance, and therefore one and the same feeling of movement is at one time connected with a sensation of touch, and at another is not, (according as some external object is or is not encountered,) and we thence conclude, in the first place, that the sensation of touch is no essentially concomitant phenomenon, no invariable consequence of the sensation of movement. We further experience that often, in the conscious and entire rest of our limbs, a sensation of touch arises, (if an object be brought into contact with our organs of touch,) that the state of rest still continuing, the sensation changes, and different organs of touch became successively sensitive, (if the object be moved along the surfaces of this species of sensibility.) We experience, finally, that if by a movement a sensation of touch commences, the same becomes changed, on further movement, in various ways, both qualitative and quantitative. It is evident that all these experiences must force upon us the inference that the causes of our sensations of touch are external; that the movement of the limbs brings our organs of touch into communication with different outward excitants of sensation. From these external objects of touch we learn to distinguish, as belonging to our own body, those whose touch produces a *double* sensation, as well in the part touching as in that which is touched. If a part of our body be insensible, it seems to the touching finger, provided no other sense correct the illusion, to be a foreign and external object. Thus it often happens that through protracted pressure in sleep on the nerves of the arm, the latter is rendered, for a time, so entirely insensible that when, on awaking, the arm which is "asleep" is touched with the hand of the other, we can scarcely but believe that it is the arm of a stranger with which our hand has come into contact.

Finally, we might here adduce an important general distinction between the sense of touch and of pain, drawn from the relations of both to the exciting cause. How sensitive an apparatus of measurement as regards pressure and temperature, is our organ of touch; what small degrees of pressure, what slight changes of temperature does it indicate! Not that, like the thermometer and barometer, it announces the absolute amount of the weight pressing upon the skin, or of the temperature of a medium to which it is exposed; but it informs us by sensations, to which we can assign no numerical value, whether the skin is compressed or stretched, whether heat be supplied or withdrawn, and the perfection of its performances rests chiefly on this, that of two pressures or temperatures taking effect one after the other on the skin, it can rightly discriminate the higher and lower, even to slight differences. How exactly, besides, does the duration of a sensation of touch correspond with the duration of the exciting cause; it has, indeed, been observed that the sensation does really endure for a minute point of time longer than, for instance, the actual compression of the skin, but the duration of this excess is so inappreciably small as not to detract sensibly from the exact synchronism of the touch and the sensation. If we pass a finger rapidly over sandstone, we distinctly feel that the surface is rough and beset with prominences, and why? Because we are able to distinguish the alternation of impressions caused by the minute projections and the pauses in those sensations produced by the intervening depressions, however short these pauses may be rendered by rapidity of movement. How different is it in these respects with the common feeling of pain. In the first place, for its excitation, proportionably far higher degrees of external influence are requisite; at the same points of the skin where the slightest pressure or the least alternations of heat and cold are promptly perceived, no inconsiderable amount of either must be present in order to produce pain. Next, the degree of pain by no means maintains so exact a proportion to the degree of the excitement; they do not observe so parallel a course that from the intensity of the pain we can form a certain determination respecting the magnitude of the exciting cause. Finally, the relative duration of the pain and of its cause depends on very different conditions; the pain often first arises long after the access of the cause which provokes it, and outlasts the latter in many cases for a considerable time, as we need not show by special examples.

We hope that this general outline has sufficiently satisfied our readers of the important characteristic differences between the proper perceptions of the sense of touch and of the common feelings produced by the nerves of sensation. We turn now to a closer consideration of the individual action of those nerves, and especially to the operations of the sense of touch.

As it has been above stated that the sensations of touch, and hence the perceptions of pressure, of warmth and of cold, only arise when the corresponding irritants act in a moderate degree upon the ends of the nerves in the outer skin and the cavity of the mouth, the task which next awaits us is to inquire into the nature of these outer ends, to seek, with the help of the microscope, to discover the structure of those terminal points which we regard as organs of sense, in order to explain, if possible, the specific action which is only exerted from those outward points. But, alas, respected reader, no satisfactory solution of this important problem has as yet proved practicable to science. With the best microscopes it is extremely difficult, and in many cases wholly impossible, to follow the individual nerve-fibres to their final terminations; we often see them, after growing extremely thin and pale, disappear among the elements of the tissue into which they enter, without our being able to say what has become of them. Until lately we possessed, regarding the destination of the nerves in the skin, scarcely anything more than conjectures, and of these the two principal were contradictory. According to the one the nerves terminated in loops—that is, they were bent round under the cuticle, and the fibres thus bent returned again into the nerve stem; according to the other, the ends in which the fibres terminated were free. Recently, the latter conjecture has been fully confirmed, no less by direct observation than upon physiological grounds, in which the free termination was *a priori* assumed as a postulate; but the nature of the free extremities and the structural arrangements which we must suppose to be connected with them remained, till even a recent day, just as obscure in regard to the skin as to other organs of sense. Within a short time past, however, we are indebted for much light to the investigations of a physiologist of great merit, R. Wagner, and his scholar Meissner. For their better comprehension we must premise that the skin consists of two distinct layers; the outer, superficial cuticle (*epidermis,*) which appears to be only a protective covering, consists of merely flat microscopic plates or scales overlaying and connected with one another. As the exterior layers of this cuticle are constantly wearing away and falling off, they are still replaced by new layers which form on the under side. The second or proper skin, the cutis (*lederhaut,*) is a closely interwoven tissue, whose substratum is a somewhat soft mass penetrated by numberless small and flexible fibres. In this mass, which gives to the skin its firmness as well as extensibility, a network of fine blood-vessels is imbedded; nerve-stems also enter it in great number and pass with many convolutions towards the upper surface of the cutis, where the single nerve-fibres become disentangled and terminate close under the cuticle. The surface, where the lower skin is bounded by the cuticle, is by no means a smooth or even one, but is beset with countless cone-shaped prominences, which fit, as the fingers do in a glove, into corresponding cavities of the cuticle. It is into these prominences, called papillæ, that the nerve-fibres enter, and it is in these that we are to seek their free extremities. Attempts had accordingly been long made, but in vain, to follow the nerves under the microscope to their termination in the papillæ; it was reserved for the above-named observers to descry, at certain points of the skin, and just those indeed which are distinguished by the finest sense of touch, as, for instance, the inner side of the finger ends, the appropriate apparatus in which the nerves of feeling terminate. They found that in all the papillæ, into which nerve-fibres enter from the deeper portion of the skin, there exist small soft bulbous bodies, seemingly filled with fluid; that to each of these little bulbs one, two, or even three nerve-fibres find their way, penetrate its walls at some point, and having entered, each becomes divided into a tuft of very delicate branches, which branches again run off each into a fine point. These bulbs with their tufts of nerve-branches have received the name in German, of "tastkörperchen," (touch-corpuscles,) because they unavoidably suggested to the discoverers a close connexion with the operation of the nerves to which they are attached, and for which they presumably serve as sense-organs. But if we ask what takes place in the bulbs, how they operate as an intermediary between the outward exciting cause and the extremities of the nerves, we remain at a loss for an answer. We are not yet able to indicate even in a general way the mode of operation of these wonderful organs, nor have we a full understanding of their mechanism. We have shown above that for an explanation of the origin of those specific sensations of pressure and temperature which only arise through an excitation of the extremities of the nerves, we must suppose a peculiar apparatus for those extremities, by the intervention of which the corresponding and appropriate modifications of the nervous current are effected. The hope of finding this apparatus in the corpuscles just referred to vanishes, when we see that these corpuscles only occur in very circumscribed portions of the skin, while the sensations of touch may be elicited from the whole surface. We have seen that for an explanation of perceptions relating to space, it must be supposed that each fibre possesses some peculiarity in its excitation, which serves for the mind as a local sign or token. The office of impressing this token on the fibres cannot possibly be ascribed to the corpuscles in question, as well because the latter only occur in limited places as because these local tokens, as we have seen, also accompany the excitation when the fibres are excited, not at their extremities, but in their intermediate passage. After amputation of a limb, the pain is referred to the no longer existing part in which the ends of the excited nerves were once situated. Lastly, we find in places of the skin which possess these corpuscles no action different from that of places which at least do not seem to possess them. In short, we are forced to confess that here are organs which we do not understand, and the value of their discovery remains to be determined hereafter.

Inasmuch as the operations of the sense of touch consist in the production of sensations of *pressure* and of *temperature*, each of which constitutes a peculiar sphere of intelligence for the mind, we draw a distinction between a sense of pressure and a sense of temperature. Inasmuch as each sensation of pressure and temperature is attended by a perception of the place of the skin whence it is excited, and with this perception again a peculiar circle of intimations is associated, we establish by the side of the former a sense of *locality*, (ortsinn,) though it must always be recollected that the perception of place is not a sensation like those of pressure and of temperature, but only a mediate idea connecting itself with those sensations, from which indeed it is acquired.

We first turn our attention to the sense of pressure. The proximate cause of a sensation of pressure is either a pressure by which the portions of skin between the nerve-ends and the object exerting the pressure are more or less compressed, or a *negative* pressure or traction by which a stretching of the skin is effected. It is highly probable that the quality of the simple sensation is precisely the same in both cases. We learn only from circumstances, especially from the accompanying muscular feeling, whether pressure or pulling, compression or stretching of the skin, has occasioned the sensation, and associate with the idea corresponding to the sensation that of the direction of the force moving the portions of skin—an idea which, as well as many others, we falsely regard as essentially a part of the sensation. Every one thinks he sees the direction in which a ray of light comes to the eye, and thus the direction in which a visible object lies; that he hears in what direction a sound reaches his ear, because the idea of the direction, to which in these respects the muscular feeling chiefly contributes, connects itself immediately and unconsciously with the sensation of light or of sound. The bare sensation can, in itself, just as little express the direction of the exciting force as the electric current, which moves the index of the telegraph, can furnish an indication of the direction from which it proceeded. A simple example will illustrate the formation of such an idea of direction in the case of sensations of touch. If some one plucks us by the hair, without our seeing him, we immediately conceive the direction in which he has drawn us, but not from the sensation, not even mediately from this, but from the feeling which arises in those muscles of the neck that offer resistance to the turning of the head in consequence of the traction; and it is again necessary that we should have previously learned from experience to interpret all these muscular feelings, so as to know with what movement each of them is connected. That it is really the muscular feeling from which we divine the direction of the traction, is evident from the fact that we no longer know that direction, when our head is so firmly held by a third person as to prevent its following the communicated impulse.

For compression of the skin to produce a sensation of pressure, it is necessary that the former should not fall below a certain degree of intensity nor rise above another certain degree.

Pressure, if too strong, creates, instead of such sensation, pain; if too light it does not excite the nerves; we feel not, for instance, the pressure which a small bit of paper exerts by its weight on the skin, especially not on the parts of the latter where the epidermis is of greater thickness and hence embarrasses the propagation of the pressure to the ends of the nerves in the under skin. Within these limits of intensity but one and the self-same sensation, as to quality, is produced by whatsoever pressure; no matter by what force or by what object the latter is occasioned, whether the organ of touch move towards the object or this towards the organ, and the pressure be thus created by the resistance of either to the movement of the other, there is in effect only one kind of sensation as regards pressure. This assertion may sound strangely to one not conversant with such discussions, who has been accustomed to assume that it is from the different qualities of the sensation arising from the touch of an object that he forms a judgment of the properties of such object, the material of which it consists, &c. Yet is it one and the same kind of sensation upon which is founded the perception of roughness and smoothness, hardness and softness, dryness and moisture, &c.; the self-same sensation of pressure arises whether the object touched be of wood, metal, gum or clay. What enables us to know those properties and materials may be made clear by a single example. If with closed eyes you receive a ball in your hand, a moment's examination by touching enables you to pronounce a complete judgment respecting it You feel that the object is, in form, round; you can perhaps indicate its size, and can certainly decide whether it be rough or smooth, heavy or light, hard or soft, elastic or otherwise; you can even rightly conjecture whether it consist of wood or metal. From what impressions do you form this comprehensive judgment, which affords a complete image of the performances of the sense of touch in general? To the knowledge of the form and size conduces, partly what we have termed the sense of place in the skin, which, in a manner to be hereafter explained, announces the extent and situation of the points of the skin which are touched, partly the muscular feeling, which, as already shown, represents to us at every moment the relative position of our members, and informs us in the present case that the parts of the hand touched by the object enclose an orbicular space. Were the ball too large to be encircled by the hand the touching of a section of it would for the most part suffice to convey the idea of rotundity, the known form of part of the surface affording an inference as to the rest, but a certain determination is arrived at if a finger be exploringly moved, here and there, over the surface of the whole object, and the muscular feeling which one has learned from experience to regard as the inseparable concomitant of movement in a circle accompanies all these movements. The similarity of this feeling, in whatever direction the finger be moved over the object, apprises you that the form corresponds to the conception which you have learned to form of a ball. Whether the ball be rough or smooth is determined by the sense of place in the skin; we call it smooth, when the intensity of the sensation is precisely the same at all the points of the skin, which through the sense just mentioned, we know to be situated near one another; we call it rough when several parts of the skin at certain small distances from one another are felt to be more strongly pressed than the intermediate parts, whence we refer the occasional stronger impressions to corresponding prominences in the surface touched. We yet more sensibly distinguish the roughness and smoothness when we move the finger-end over the surface, to try whether the successive impressions made on the same points of the skin are equally strong, or alternately stronger and weaker from gliding over the prominences and depressions. The weight of the ball is conceived of either directly from the intensity of the sensation of pressure in those parts of the skin on which it rests, or from the intensity of the muscular feeling—that is, from the sensible degree of effort which must be put forth to sustain or raise it. We compare the weight with the acquired idea of the size of the ball, and thence form a judgment of its relative heaviness or the specific gravity of the material of which it is composed. We decide that the ball is hard or soft, elastic or firm, from the amount of resistance which it opposes to compression, and of the amount of this resistance we judge from the degree of conscious exertion of the muscles, and from the increase of strength in the sensation of pressure with the increasing effort employed to produce compression. If, finally, we ascertain that relatively to its circumference the weight of the ball is considerable, that a sensation of cold is communicated, (what that is we shall presently see,) we conclude that the object is of metal, since we know from experience that it is to metal these properties pertain. This example will serve to show by what complex and circuitous processes the comprehensive judgment, which almost at the moment of the touching stands ready formed before the mind, is elaborated; will serve to show the mechanism of the manifold operations of the soul, through whose co-operative working, schooled by experience, the raw material of the mere sensations is, with wonderful rapidity, transformed into an harmonious and colored image which we are accustomed thoughtlessly to receive as the substance, simply and directly, of the sensation; will serve, in fine, to show what part in this is borne by the sensation of pressure with its single characteristic.

Every one knows that the strength of the sensation of pressure corresponds to the force of the pressure on the skin, the one increasing or decreasing with the other. Without this proportionality, one of the most valuable functions of our sense of touch, the comparative estimate, namely, of the force with which different objects exert a pressure, and consequently the discrimination of weight would be impossible, or at least in the highest degree uncertain. The appreciation of pressures according to the intensity of the sensation is tolerably exact and certain, as we shall presently show; but the scale by which we measure is in nowise to be compared with the weights from which, with the help of the balance, we determine the precise numerical value of these same pressures. We have no scale on which the mind can read the absolute value of a sensation of pressure of a determinate intensity; we are able, indeed, of two successive sensations to say which is the stronger, which the weaker; we can decide whether the intensity differs little or much, but we cannot express this difference in numerals and pronounce one sensation to be twice, thrice, or half as strong as another with which it is compared. While undergoing its training, the mind gradually learns to connect with different sensations the idea of corresponding differences of intensity in the forces of pressure, and the different degrees of sensation with the ideas of the corresponding forces become so stamped upon memory that at a moment's warning we are in a condition to interpret every such sensation and to refer it to a certain amount of outward pressure. We form for ourselves, for instance, an accurate recollection of the sensation which arises when the weight of pressure stands, as the balance announces, at the ratio of 1 : 2, and in other absolute proportions, and in this way attain a faculty of discrimination, gradually becoming through practice more sensitive and certain, for amounts and differences of pressure. If we inquire as to the manner in which we essay the trial of weight by help of the sense of touch, an interesting circumstance must be noticed. It has already been seen that, for the estimate of weights we have in our muscles a second means to which we principally recur for the trial of greater weights. We lift the object and estimate its gravity by the intensity of the feeling of effort in the exerted muscles, after we have gradually learned to interpret this feeling in reference to its intensity, as in the case of sensations of pressure. If, therefore, we would employ the latter only in the trial, we must exclude all aid of the muscular feeling; for which purpose the organ of proof, the hand, must be fully supported and left at rest with its back on the support, while the weights to be essayed are laid upon the palm. For comparison of two different weights the determination is simple and easy, if both are laid at the same time on two different places of the hand and the relative weight be estimated by the intensity of the separate sensations simultaneously excited. But our judgment is more accurate and our power of discrimination nicer if we deposit the weights, one after the other, on the same place of the hand, in such manner that the second and actual sensation is compared with the first and simply remembered one, instead of two actual and simultaneous sensations being compared as in the former instance; indeed, the recollection of the previous sensation is so tenacious that no inconsiderable interval may elapse between the two trials without rendering our judgment as regards the first sensation uncertain, and the greater the difference of the two compared weights, the longer may this interval be without prejudice to the accuracy of our estimate. If we lay on the hand of an individual whose eyes are bandaged or averted, so that the sight shall lend no assistance to the judgment, first a weight of two ounces, and afterward on the same place a weight of two and a half ounces, the lapse of two minutes will occasion no error as to the greater heaviness of the second. If the weights be deposited on the hand in immediate sequence we distinguish with certainty fourteen and fifteen ounces as being of different weight, and recognize which is the heavier. We should observe, however, that the weights to be compared must be deposited on the same place of the hand; were we to place one of two equal weights on the finger-end, so rich in nerves, the other on the palm, in which the nerves are so much rarer, we should certainly decide in favor of the preponderance of the former. Again: it is necessary that the base with which the weights rest on the skin should not in the one occupy a much greater surface than in the other; we cannot accurately compare two weights, of which the one presses on a few square lines, the other on the whole surface of the hand. Both these conditions seem to be founded on the fact that in our judgment we do not closely discriminate between the intensity and the extension of the sensation, but are led erroneously to infer, from the greater expansion of the feeling or, what is the same thing, from the greater number of nerve-fibres involved, a greater intensity of pressure. A third requisite for accurate appreciation of weight through the sense of touch is, that the weights to be compared should have an equal temperature; of two equal weights, having different temperatures, we should hold, under circumstances otherwise equal, the warmer to be the lighter, the colder the heavier, and this probably because the effect of pressure on the particles of the skin expanded by warmth is different from that exerted on the same particles corrugated by cold. Lastly, it will be readily conceived that in the act of placing both weights on the hand no greater degree of pressure should be exerted in case of one than the other.

Thus much of the sense of pressure and its offices; how the mind makes use of it, what knowledge is derived from its communications, the above examples sufficiently indicate. To this, and to the closely allied muscular feeling, beyond all things else, we are indebted for a right conception or idea of force.

We turn now to the second faculty of the sense of touch, the sense of temperature. That we are not able to give a nearer definition of the nature of a sensation of heat or cold has been already explained in our introduction; our next thesis, therefore, is to inquire in what manner these sensations are evoked. A sensation of this sort arises in general as soon as the temperature proper to the skin, and maintained by the blood, undergoes a change, either to a higher or lower grade. If the temperature of the skin be raised by an accession of heat from without, an excitation of the ends of the nerves is produced, which creates the sensation of warmth; if the temperature be lowered by a withdrawal of heat, there arises a sensation of cold. This is really all that can be said about the causes of the sensations in question; in what manner the increase or dimunition of temperature in the skin surrounding the ends of the nerves excites these sensations, and determines in the fibres running to the brain two different modifications of the nervous current corresponding to these opposite changes; why the alteration of temperature operating on the ends of the nerves occasions not pain, as is the case when it takes effect on those nerves in their intermediate course, but the specific sensations above-mentioned is altogether obscure. We may imagine that warmth expands the particles around the nerve extremities, as it expands all bodies, while cold is transformed into an excitant, through the contraction of these same particles; but this is only a conjecture, which is not proved, and does not satisfactorily answer all the questions involved.

We possess in the apparatus of these sensations, at the inner extremities of the nerves of the skin, a sort of thermometer, if we may so express ourselves, which, like the instrument mentioned, indicates to us the addition or abstraction of warmth through two different qualities of sensation, and through the intensity of the sensation the degree of the change; but the difference of the two instrumentalities is sufficiently striking. In the first place, analogous to what was said in regard to sensations of pressure, our sensations of temperature admit of no reduction to a graduated scale; we perceive two such sensations to differ in intensity, but are unable to express the numerical ratio of their intensity. A further difference is the following: In the mercurial thermometer, the zero of the scale, from which the degrees of heat upwards, of cold downwards, are reckoned, corresponds to a definite and constant height of the column—to that indeed which it occupies when the metal has the temperature of melting ice. It is otherwise with our thermometer of sensation. Here it is not a sensation of determinate quality and intensity which, in a certain manner, represents zero; it is the absence of all feeling of temperature; it is nothing positive, therefore, like the measured height of the mercurial column, but something negative which exists between a sensation of heat and of cold. This repose of sensation occurs not when the skin has acquired some definite and absolute degree of temperature, but when that temperature, whether high or low, remains unchanged, while a change immediately produces a sensation of the kind in question, and this sensation again only subsists as long as the addition or abstraction of heat, but ceases when the temperature becomes stationary at any supposed degree. Our animal thermometer thus indicates, not the grade, but the change of temperature in the skin. It follows, of course, that what we have designated as the zero of the sensational scale is inconstant. If we dip our hand in water of some ten degrees of temperature we feel at first cold, because the water is colder than the skin, and abstracts heat from it; but after a time, when the temperature of the skin and water has become equalized, the sensation ceases. If we let the hand rest for a short time in water at eight degrees, which produces a feeling of cold, and then transfer it to water at sixteen degrees R., we at first feel warmth, because the skin, whose temperature has been considerably lowered by the first immersion, absorbs heat from the warmer water; but the feeling is soon transmuted into one of coldness, because the skin, as soon as it is brought to a temperature of sixteen degrees, takes up heat from the much warmer blood and gives this over in turn to the water. Our thermometric sensations are, therefore, an uncertain criterion in judging of the absolute temperature of external things; and there are circumstances also under which this uncertainty is increased. If we grasp, in winter, an iron rail, it seems to us extraordinarily cold, much colder than the air, much colder than a wooden rail under precisely the same circumstances; and yet it is easily shown by the thermometer that the three objects possess absolutely the same temperature, and that hence the judgment founded on our sensations is false. The ground of this illusion lies in the fact that iron is a very good, wood a very bad, conductor of heat, whence the former robs our skin of its heat much more rapidly than the latter. Since we have seen that it is the change of temperature, not its absolute degree, which stimulates the nerves, it will readily be conceived that a rapid change produces a more intense sensation than a gradual one—that the feeling of cold is greater when the heat is withdrawn from the skin in a relatively shorter time. Lastly, there is another circumstance to be noticed which may be the occasion of deception in our estimate of temperature through the sense of touch, a circumstance which conduces also to render our estimate of weights uncertain, and exerts considerable influence on the intensity of the sensation of pain. Just as two equal weights, when they press upon surfaces of the skin of different extent, seem to us unequally heavy; so does the warmth or coldness of an external medium produce in us the impression of a greater intensity in proportion to the greater extent of the surface on which it takes effect. If we dip into water at eight degrees of temperature, for instance, the finger-end of one hand and the whole of the other hand, we feel in the hand wholly immersed a much more intense feeling of cold than in the finger-end. The ground of this error is probably that above given, that the mind, from the greater number of nerves simultaneously excited, is betrayed into the idea of a greater intensity of the sensation.

As all thermometers are not equally sensitive, but the thickness of the walls of the tube will in some cases prevent an equally rapid transmission of the external temperature and the consequent rise or fall of the quicksilver, so not every part of our organ of touch is endued with the same sensibility to changes of temperature in the surrounding medium; and here, too, it is the thickness of the envelope which occasions the difference. The thicker the epidermis, the more slowly does the sensation of warmth or cold penetrate to the nerve-ends inbedded in the cutis. In the human hand the thickness of the epidermis is much more considerable in the palm than on the back; when, therefore, we immerse our hand in cold water, the sensation of cold is sooner perceived in the back than the palm; but, because a proportionably larger number of nerves terminate in the latter, the feeling eventually reaches a greater degree of intensity in the palm than the back of the hand. The difference of two temperatures is, however, appreciable with competent exactness by the sense of touch, notwithstanding the sources of error which have been noticed; if we take two vessels with water of different temperatures and in quick succession, dip the same finger first in one and then in the other, we can distinguish which is the warmer, which the colder, if the difference be only a fraction ($\frac{1}{2}$—$\frac{1}{6}$) of a degree. But the delicacy of discrimination is not the same with all persons, and practice greatly improves the exercise of this sense. It is further to be remarked that we habitually give *objectivity* to our perceptions of temperature, as we do to those of pressure, and to all true perceptions of the sense in question. If we touch an object which abstracts heat from the skin, it is not the mere subjective sensation, not the idea of a change of temperature in the skin, which presents itself to our consciousness, but the immediate idea of the cold object, to which we impute as a property that quality of our own sensation which we denominate coldness. Only in the case when no sensation of pressure accompanies that of temperature, and the skin therefore gives us no perception of an exciting object, are we sensible of the coldness as something subjective, as a state of our own sentient organism.

We turn now, in the third place, to the *sense of place or position*—that is, the faculty of our organs of touch which gives us *a perception of the situation, size, and form of the portions of the skin on which either pressure, or heat or cold, takes effect.* It has already been cursorily noticed by what steps we arrive at the ideas of position which are connected with the sensation of touch; and we here recall the most important particulars. First of all, it is to be observed that there is no room for supposing a direct primary sensation of place; the new-born child feels at first only the simple sensation of pressure, without learning therefrom that the skin is the place whence the sensation was excited, or recognizing the part of the skin on which the excitation was produced; it, as yet, knows not that it possesses in its skin an outspread organ composed of so many distinct points of sensibility. It is easy to comprehend, moreover, that it cannot be the course of a nerve-fibre which furnishes the idea of place; that hence it cannot be that simply because some particular nerve terminates in the leg, for instance, the idea of pressure of the leg connects itself with the sensation generated by that nerve; in the mere course of the nerve there can be nothing to determine the nature of the resulting idea, nothing to qualify the excitation which acts upon the mind. If the process in the nerve-fibre be the same, the sensation produced will also be the same, whatever may be the direction in which the nervous current is conducted to the brain; just as the movements of the hand of the electro-magnetic clock continue the same, whatever the direction of the conducting wire; the movements furnish no indication of the origin of the operating current. If, therefore, the sensation itself gives occasion for the formation of ideas of place, we are obliged to suppose that through some modification a slight but constant peculiarity exists for each separate fibre which is stimulated, and that thus the sensation which is effected by exactly the same compression possesses a somewhat different shade when it proceeds, not only from widely separated nerves of the arm or leg, but also from two fibres termininating in close proximity with one another. These presumed peculiarities of the currents in separate fibres of the nerves we have above indicated by the name of *local tokens.* Through these are we originally endowed with the possibility of associating ideas of place with the sensation of touch, but we do not thence derive the ideas themselves. The child at first receives only the impressions differently toned according to the place of the excitation; it gradually learns by circuitous means that the sensation is qualified by the place from which the excitation proceeds, and that a particular point in the skin corresponds to each particular token. The interpretation of these local tokens is the result of the joint and reciprocal self-training of the sense of sight, the sense of touch, and the muscular sense, by which last is meant the feeling of effort in the muscles moving the limbs, a feeling whose invaluable services, as regards the point in question, we have before had occasion to signalize. Were the organs of touch not movable in themselves and in relation to one another, and were their movements not accompanied by this muscular feeling, we should be destitute of one of our most important sources of information, and scarcely in a condition, as we actually are, to judge accurately in regard to the performances of our organ of touch. If with the finger of one hand we touch a point in the palm, for instance, of the other, there arises a feeling in the touching part as well as in that touched, each feeling being attended by the local token appropriate to itself; if we touch with the same finger another point of the palm, the local token in the part touched will be a different one. If we move the finger gradually over the palm we receive a continuous series of sensations with different tokens. It is not alone that we see the course described by the finger; we know its extent and direction also from the feelings generated by the moving muscles, and we thus arrive at the idea that the sensations with the distinctive local tokens belong to distinct but contiguous points of the skin, while the form of the movement, whether in a right line or a circle, is made known to us by this same muscular feeling. Thus by long and attentive study the mind is qualified rightly to interpret each local token, and, in the case of two simultaneous impressions, to judge correctly of the relative situation and distance of the two corresponding points of the skin; whence the whole cuticle may be conceived of as a mosaic of sensitive points, in which we have learned to distinguish each point by its characteristic coloring, and to refer with the rapidity of intuition each local token to its definite place within the general outline.

It results from what has been said, that while two simultaneous impressions on the skin, when the sensations convey different local tokens, give rise to the idea of two different local excitations, the mind will, on the other hand, necessarily refer both impressions to one place when the tokens are alike, and hence the resulting sensation will be single. We have further supposed that each different nerve determines a different shade of sensation, whence it follows that the sensation occasioned by each fibre conveys the idea of that point of the skin in which the fibre terminates, and that hence pressure at the points, whose compression stimulates one and the same fibre, will generate only one and the same perception of place. This is a supposition which we must here submit to a closer examination. That one and the same fibre cannot simultaneously convey to the brain two currents of different quality, or with different local tokens, and that hence two sensations cannot be generated at the same time through the same fibre, is a physiological law, which we must request the reader to take for granted, as its demonstration here would lead us into too wide a digression. Another question, however, is, whether in reality each fibre imprints on the sensation a separate stamp; whether, because two different fibres are simultaneously excited, therefore, the *double* sensation is necessarily accompanied with a conscious separation of the impressions. Every one knows that there is not the smallest portion in the whole surface of the skin which is not sensitive, and which, if touched with the point of a needle, will not yield a sensation. But each point, though sensitive, is not endowed with its own nerve; on the contrary, the microscope shows us that the papillæ in which the nerves terminate are separated from one another by a greater or less number of papillæ which are devoid of nerves, so that in many parts of the skin there exists quite a considerable interval between the ends of these fibres. With this anatomical fact, however, it is easy to reconcile the further fact that even those points of the epidermis, situated over a portion of the cutis which is destitute of nerves, should be perfectly sensitive. In order that the pressure produced, for instance, by a needle's point should stimulate the end of a nerve, it must necessarily be propagated through the whole thickness of the integument under which the nerves occur. This propagation of the pressure will not take place in a perpendicular direction only, but will also extend laterally so as to involve to a certain degree the particles of the skin in proximity with those directly compressed. Just as a circular wave spreads in the water, when the fall of a stone disturbs however small a part of the surface, does there arise in the skin, from the pressure of the needle, a propagated motion which affects the particles within a certain circuit, and not merely in a perpendicular line. However light the touch or minute the instrument, the wave, if so we may call it, in the skin is always wide enough to reach the end of a nerve, even in parts where the nerves are most sparingly distributed. But the microscope further shows us that a special fibre is not appropriated to each separate extremity of a nerve, but that frequently the same fibre is divided close under the skin into several branches, so that in many places a greater or less extent of the surface (of a line, for instance, in diameter) is supplied with these nerve-extremities by a single fibre. From which it follows, of course, that within this extent each single point will be sensitive, and yet if, within that circuit, we press simultaneously upon two separate points, the two impressions can produce but one simple sensation, since, however distinct the extremities of the nerve, the impressions act as an excitant only upon one fibre communicating with the brain, and do not, as in the case of a double sensation, convey through their local tokens the idea of two distinct points of contact.

Few, probably, of my readers have so nicely tested their organ of touch as to have acquired the knowledge that under certain conditions two simultaneous impressions on the skin produce but one simple sensation; that the touch of two points, for instance, may seem but a single touch, when the sense of sight does not betray us into the belief of a twofold sensation by disclosing its twofold origin. A simple and easily repeated experiment will strikingly confirm the truth of this proposition. Let the eyes of any one, who is not previously advised of what is intended, be carefully bandaged, in order that there may be no such betrayal of the judgment as that we have just spoken of; then let a common but blunt pair of compasses, opened to the extent of half an inch, be applied to the back of the hand of the person upon whom the experiment is tried, care being taken that both points shall touch the skin at exactly the same time. If we now ask the person in question whether the impression be single or double, we shall with certainty receive the assurance that it is a single one. Place the points of the compasses, with the same extension, upon the skin of the finger's end, and the subject of experiment will immediately and correctly inform us that the impression is double. If the points be applied to the skin of the back, we may separate them to the extent of even two inches without causing the sensation of a double pressure, provided the judgment is unbiassed and the subject carefully considers the nature of his sensation. Next, let the compasses, with the points separated to the distance of ¾ inch and placed perpendicularly to one another, be applied just in front of one ear; the subject will again be conscious of only a single impression. Now move both points, still preserving the same distance from one another and in constant contact with the skin, across the face as far as the other ear; at a certain place in the transit the single sensation will be felt to change into a double sensation; as the points approach the mouth it will seem that they are gradually separating further from one another, and when on the other side of the face they approach the other ear, the points will seem anew to draw closer together, and the double impression will presently be again lost in a single one. How are we to explain these singular effects, and especially the remarkable illusion in the case last mentioned? In the following manner: the two points simultaneously applied always and necessarily produce only a *single* sensation, whenever, by their pressure, they excite one and the same nerve-fibre; when both, therefore, touch the skin within one of those circuits whose sensibility is supplied by the same fibre. This proposition is incontrovertible. The skin is to be regarded as divided into a countless number of these spaces supplied by a single nerve, which have received the name of *circles of sensation*, (Empfindungskreise;) as a mosaic of such circles, whose size is regulated by the variable affluence of different portions of the cuticle in sensitive fibres. The further these are from one another, and the greater the division to which they are destined for the supply of the sensibility of the part, the larger in that tract of the skin are the circles of sensation. Within any one such circle all impressions bear the same local token; two, three, or more impressions, therefore, which, within the limits of that circle, take effect simultaneously upon the skin, communicate but a single or blended sensation.

It would seem now to be a necessary consequence of these facts, that inversely two simultaneous impressions should produce a double sensation and a perception of local separation, when they take effect on two different circles of sensation, and thus excite two different nerve-fibres; that hence the two points of the compasses should always be separately felt when each of them touches a different circle. In this simple form, however, is the conclusion not tenable—not consistent with the results of experience, as is shown by the following facts and observations. If we make the experiment, for instance, on the skin of the forearm, we find that the separation of the points of the compasses must amount to at least 18 lines if we would produce a double sensation; that a separation of 16 lines always produces a single one, let the points be placed where and in whatever direction we will. Should we thence infer that the circles of sensation of this portion of the skin must have a diameter of 17 lines, we should encounter an inexplicable contradiction. If we imagine, for example, these divisions to be squares of 17 lines lateral length, we know from the condition that every point is sensitive, and that these squares border upon one another without the least interval. If we conceive now two such coterminous squares, it is readily seen that the points of the compasses need not be 18 lines apart to touch at the same time two different squares, but that with a separation of ½ line we can so place the points that one shall touch a certain square and the other a second square close to the boundary at which these squares meet. There should now arise a double sensation, since here the supposed condition of touching two different circles of sensation is fulfilled, and we ought to be able to detect some place in the forearm where, with a distance of ½ line, this would be the case. But as this is not so, as a constant distance of 18 lines in all directions is here required for a double sensation, the supposed condition cannot be tenable, that it suffices, namely, to touch two contiguous circles in order to produce such a sensation. To reconcile, therefore, the facts just cited with the theory of circles of sensation constituted by the spreading of single nerve-fibres, it has been found necessary to resort to a further supposition, and to assume, as a condition of the double sensation, that at least one or more circles which are not touched must lie between those which are. This, indeed, is a proposition not directly proved, and which, on close consideration, introduces some further difficulties into the discussion, but it is the only one as yet by which the facts are reconciled, all others which have been suggested being much less consistent with recognized physiological facts and laws. Agreeably to this hypothesis, the explanation for which we were seeking may be concisely stated in the following manner: The two points, when simultaneously applied to the skin, always produce a single sensation, not only when they touch one and the same circle of sensation, but also when they touch two contiguous ones; the larger the diameter of the circles, so much the more considerable must be the distance in order to reach beyond the boundaries of two neighboring circles, so that one, perhaps several, circles which remain untouched shall be included between the points of the compasses. When this latter case occurs, the double sensation, the idea of locally separated impressions, seems to result from the circumstance that the mind has within itself a consciousness of the intervening circles, be they one or more, which remain untouched; that in the ideal image of the cuticular surface which is present to it, the mind, while it assigns a place to each of the received impressions, perceives that there is a free space between the two which is characterized by other local tokens. It is this consciousness of the situation and number of the circles of sensation which also enables the mind to form an estimate of the distance between the two points of the compasses, as soon as it acquires a perception of their duality, an estimate, however, which, from examples before cited, we know to be subject to much error. We mentally estimate the distance in question by the number of the circles of sensation lying untouched between those to which the points are applied, and this appreciation can have no absolutely correct value, because the unit of measure on which it is founded, the diameter of a circle of sensation, is of very variable magnitude, differing greatly in different parts of the cuticle. Place, for instance, the two points of the compasses an inch apart, first on the skin of the cheek and then on that of the finger, and if the same precautions be observed as were before stipulated, the person subjected to the experiment will conceive the distance between the points to be considerably greater on the finger than on the cheek; because the circles of sensation of the finger are much smaller than those of the cheek, whence in the former more of them lie untouched between the points than in the latter. In this way also may be easily explained the illusion above noticed of the seeming withdrawal of the points further from one another when, preserving the same distance apart, they are gradually moved from the ear towards the mouth; the nearer we approach the latter the smaller become the circles of sensation; the greater the number of them which passes at the same time between the points, the greater, therefore, seems to be the separation of these points.

It is impossible to measure the absolute magnitude of the circles of sensation in different parts of the skin, because we do not know the number of those circles which, on the above theory, must remain untouched between the points of the compasses in order that the sensation should be double. But their relative magnitude we may measure with considerable accuracy, and thus determine the comparative delicacy of the sense of place in different portions of the skin; for it is obvious that the degree of that sense assigned to any particular portion must be so much the finer in proportion as the distance between the points of the compasses can be reduced without a cessation of the divided sensation, or a blending of the two simultaneous impressions into one. These measurements have been executed with great care by the same eminent physiologist, C. H. Weber, to whom the whole doctrine of the sense of touch is indebted for its present form and clearness, as well as for the ingenious theory we have been discussing. It has been ascertained that the sense of place is most delicate in the end of the tongue, and next on the inner side of the last finger joints, the points of the compasses requiring only a separation of ½ line in the former case, and 1 line in the latter, to produce the double sensation. Much more obtuse is the sense in the second finger joints, still more so in the palm of the hand, and becoming duller and duller in other parts of the skin, it manifests, finally, the greatest degree of obtuseness in the cuticle of the back, the upper arm, and the thigh.

It is an old and well established usage to inquire into the design of all arrangements in nature, and especially in our own wonderful and complicated organism, and to judge all observed facts by the principle of conformity to that design. This theological principle, which to the laity is almost indispensable in their contemplation of the wonders of creation, and which, not without reason, seems to them the surest guide to an understanding thereof, has in later times been warmly attacked; its value has been denied, and all employment of it in scientific observation and inquiry been sedulously discarded. We shall not here attempt to settle this troublesome controversy, nor to decide how much too far, in one direction or the other, the views of the parties have carried them. Our readers will not take it amiss, we are sure, if, with the aid of the above principle, we seek to explain the unequal degree of delicacy of the sense of place in different parts of the skin, and afford them the opportunity of admiring, in this connexion, the adaptation and design which everywhere prevail in the works of nature. We find, then, this sense in the highest perfection precisely in those parts which, through their arrangement, situation and freedom of movement, are best suited and most clearly destined to the operations of touch. Observe the functions of your tongue, which you are accustomed only to regard as an organ of speech and taste, during that most indispensable of acts, eating. It indefatigably examines by touch the morsel which you are chewing and separates what is not yet sufficiently comminuted from that which is prepared for swallowing, brings the former again under the molars for further reduction, carefully removes from the mass whatever is unsuitable for deglutition, fish-bones, fruit-stones, &c., and gives you accurate notice of the form and size of the objects in the cavity of the mouth. All this, whose importance cannot be mistaken even when it relates to so trivial an affair as mastication, would be impossible without that exquisite delicacy of the sense of touch which resides in the tongue. Still more evident perhaps will appear to you that nice discernment of the place of external objects which pertains to the finger-ends, those pre-eminently active organs of touch, with which you are accustomed to test everything tangible, and whose services cannot be replaced by any other portion of the wide-spread surface endued with sensibility, because no other part is possessed of so much delicacy combined with such manifold mobility. You may have often wondered at the accurate decisions of the groping finger of the blind, in whom that organ seems to have replaced, as far as it can be replaced, the priceless sense of vision, and you forget that your own finger has exactly the same power, the same innate and delicate sense of place and position. This sense, you say, is refined in the case of the blind; but that is only true in so far as the blind man has been compelled to consider in a more careful manner the communications of the sense, to devote a more attentive study to the interpretation of its tokens, the translation of the simple sensations into ideas, and has thence acquired greater practice and certainty in understanding its intimations; the sense itself has become no finer. In your own judgment of the properties of objects which this sense, assisted by the muscular sense, is capable of ascertaining, you have been accustomed to allow the sight to interpose and to modify through its perceptions the ideas derived, as has been shown above, from the sense of touch. Take in your hand a three-cornered stick of three inches length with two triangular bases; your sight informs you, at a glance, of all these relations of shape and size. The blind judges of them with equal accuracy through the sense of touch, as you yourself do if the trial is made in darkness or with averted eyes. You first take the stick between the thumb and forefinger, so that the two bases touch the skin of both, and perceive at the moment that you hold a solid body with two triangular bases, while at the same time an idea is formed of its length. From the circumstance that every effort to close the fingers is accompanied by an insuperable resistance you form the idea of solidity, and it is what we have termed the muscular sense which enables you to do so. The sense of place in each finger-end informs you of the shape and size of the bases of this solid object, through the intimations which that sense conveys to you that a certain number of circles of sensation are in contact with the surfaces, and that these circles lie near one another in a triangular position. If you apply one of the bases to the end of your tongue, the base will seem larger, because here a greater number of circles come into contact with the triangular surface, while, if this same surface be applied to the forearm, you no longer distinguish its form, provided the diameter be not more than 18 lines, because you receive only a single impression, and no idea of geometrical arrangement can result. It is again the muscular sense which ascertains the length of the supposed stick, for it is this which gives you notice of the relative distance of the finger-ends, and the same sense would be still available for the same purpose, but in a different manner, if the stick were too long to be held between the fingers. Were it, for instance, an ell long, you might estimate its length by holding it between a finger of one and of the other hand, or by moving a finger along its surface from end to end, and it is to the muscular sense that you would owe the consciousness in the former case of the relative distance between the fingers, and in the second of the extent of the movement executed. By similar means you obtain an idea of the surfaces of the object, as under what angles they meet, whether they be straight or bent, rough or smooth, &c. No further exemplifications are deemed necessary; the one here given will suffice to place in a right point of view the operations of the sense of touch, especially as applicable to the determination of the relations of place or position, and will serve at the same time strikingly to illustrate the conformity of arrangement and design in the endowment of the finger with so exquisite a degree of the sense in question.

So much, respected reader, for the doctrine of the sense of touch. This were the place to bestow a closer consideration on the common feeling which we have contradistinguished as an indirect or imperfect sensation from those of touch, had we not limited our purpose to a discussion of the true perceptions of the sense. Add to this, that the distinction between the two classes of sensations has been already made sufficiently clear, and indeed that one description of the common feeling, that of muscular effort, has, in the foregoing remarks, been the subject of examination with a view to a right appreciation of its action and offices.

I would fain hope that in the attempt to convey to the general reader an insight into the nature of his most important sense, I may not have fallen too far short of my purpose. At the close, even more sensibly than at the outset of my undertaking, do I feel the conviction that it is a more difficult and a more responsible task to open to the novice the gates of the temple of a physical science than even to penetrate therein by the laborious process of investigation; more difficult, because we cannot lead the novice by our own path, but only by the common one which alone he can tread; more responsible, because every adulteration or charlatanical exhibition of the treasures of science seems a profanation, calculated to satisfy a childish curiosity, not the intelligent inquiries of a cultivated mind. There are numbers, doubtless, who think that the scope of science, and especially of the physical sciences, consists in the alchemy of money-making, who hold no discoveries worth the trouble of examination, except such as can be turned to account in the kitchen or the workshop; who wish, therefore, to learn nothing from science but what conduces to these "useful" objects. It is indeed the duty and a grateful duty of science to place its acquisitions at the service of industry and art; but he errs who ascribes to science the inferior rôle of a handmaid to the latter. He who, with the question, *cui* *bono?* (of what use?) only thirsts after popular expositions, will probably derive not the least satisfaction from the preceding observations. For such alone are these pages intended who desire the possession of scientific truths for their own sake; and it is to such only that I deem it possible to convey a popular representation of physiology—a subject which, in the foregoing article, appears for the first time in this work, (*Aus der Natur.*)

2.—THE SENSE OF SMELL.

MAN is, in a certain sense, the slave of his nose, and even a strong will is often powerless in its struggle against the force, partly original, partly developed by habit, of that unseemly tyrant. Not to be suspected of a trivial allusion to the snuff-taking members of mankind, and in order to allay in my readers every sentiment of indignation which this imputed slavery might give rise to, I hasten to explain and prove my assertion, stating at the outset that we share this slavery with the animals, to which we, in general, and even in many a physical relation, are certainly not so incomparably superior as the proud lord of creation would fain believe. Who would deny that what we call our disposition is but a soft wax, moulded in manifold ways by sensual impressions, and, according to their nature and power, incessantly changed into endlessly varying shapes, comparable, in this respect to the photographer's plate, which, yielding to the influences and inviolable laws of light and shadow, reproduces in faithful images the local relations under which light and shadow act upon it? Who, indeed, would be so presumptuous as seriously to assert himself complete master of his disposition, and able to force it to withstand the most powerful impressions of the senses, without changing color or form? He who asserts this has not yet cast a profound discerning look into the machinery of his own mental life, whether prevented by a lack of talent for self-observation or by a lack of modesty. He who earnestly examines and tries to understand his inner life will have found that, in a thousand cases, our longings, our desires, as well as the opposite antipathies, always wear the color of the momentary disposition; that, as its effluences, they are, like itself, the indirect products of the workings of the outer world upon our soul through the medium of our senses; and that from the same source flow thousands of our actions which we regard as entirely spontaneous, as springing from our free will, and not from the force of external influences. These may appear to some as commonplace speculations; they certainly are truths which the lyric poetry of all ages has hounded to death, and which, coated with new phrases, are reproduced in every novel; but they are, no doubt, nevertheless obscure to many a reader who receives them with a contemptuous smile as stale, and in a given case deceives himself, unable to discern the source of his disposition, the hidden external causes which, through the senses, have necessitated his "voluntary" desires and actions. We shall leave it to the poets to sing the commanding voice with which nature speaks to our soul through the senses, charming forth, in varied alternation, joy and sorrow, longing and horror, and only in a few allusions shall allow ourselves a slight encroachment upon their domain. Whose heart is so ice-bound as not to be warmed by the charms of nature awakening from its slumber on an early and a serene spring morning? Behold, its youthfully stirring life pours into your inner being through all your senses, paints every thought with its gaudy tints, rouses all the merry spirits of your heart, (an innocent instrument, indeed, but which the poets have transformed into a cornucopia of the feelings.) The fresh and sapful verdure excites your nerves of sight, the tepid air your nerves of touch, the fragrance of the young spring flowers your nerves of smell, the returning birds your nerves of hearing, inspiring long-missed sensations, and if you so want it, you can procure a vernal enjoyment even to your nerves of taste. And what is there in that verdure, in that fragrance, in that soft breeze, that so works upon your soul? Why is the green carpet unable to breathe into your heart a desire for travelling, or the warm stove that secret longing so often sung by the bards of spring? Physiology has no better answer to these questions than poetry, but in these and other innumerable facts it sees evidences of the normal dependence of the actions of the soul upon the qualities of the physical processes in the excited nerves, and the processes of sensation unavoidably conditioned by the former. In other words, the outer world compels our soul not only to feel, but, indirectly, also to think, to form conceptions, even to will, and thus leads, as if by a thread, that proud being that fancies to walk in such unconditional independence of the laws of the physical world. What a poor thing would the human soul be without the senses; nay, it is not even imaginable without them! A man born without any senses could scarcely vegetate; it would be absurd to talk of his *living* psychically. The soul develops its abilities only in the school of the senses; only the senses convey to it the materials for thinking, for the formation of conceptions and ideas; only the senses give it the primary objective points for the development and exercise of its volition; the will, even if an imminent and inborn faculty of the soul, would, without the senses, be a latent force. I leave it to the reader to picture to himself what I have only hinted at, to form an idea of the spiritual activity of a man cut off from his birth from all sensuous perception. After an unprejudiced examination everybody will arrive at the same result, at the conviction that psychical life without senses is to us something entirely inconceivable, the best proof of which is that at every attempt to imagine the soul surviving and separate from the body, we are compelled to endow it with senses and the ability of reacting upon external bodily things, else our attempt fails from the beginning. The *instinct*, however, of forming such an image is necessary, and deeply rooted in the human soul, educated as the latter is under earthly circumstances, from which it borrows all its conceptions; no one can free himself of that instinct.

In short, the soul, in its earthly career, is first the pupil, and subsequently, through life, the slave of the senses, inseparably connected with them, as the steam-engine is with the fire, which engenders its motive power and renders its activity possible. Deprive a grown-up person, who from childhood has been in full possession of all his senses, of only one of them—the sense of sight, for instance—and observe how poor his educated soul becomes through this single loss, how narrowed its circle of ideas, how one-sided the exercise of its volition; deprive him of several, and see into what pitiable poverty even the richest soul will sink.

It is true the sense of smell is of all the least necessary, and still it is a tyrant like the others, and plays its part as such openly or behind the scene. It, too, moulds our disposition, awakens desires—most material desires, too—and these are followed by actions which, when speaking of man, we boldly designate as entirely voluntary, (because they can be prevented by our will,) and when of animals, we put to the account of that universal wizard called instinct. Here are a few examples. You speak of an innate instinct when the hound follows the track of the game, which his extremely keen sense of smell makes him discover; you justly deny this action of the dog to be an entirely voluntary one, caused by reflection. When, your stomach being empty, the delicious odor of a savory meal engenders in you an appetite—that is to say, literally, a desire for that meal, and you satisfy this desire; when the drunkard, seduced by the vapors of spirituous liquors, yields to the tempting odor, in spite of all his good determinations and of energetic exertions of the will to prevent it, and satisfies what he emphatically calls his thirst, I do not see what difference there is from a physiological point of view between your and the drunkard's case and that of the hound. Like the latter, both of you obey the incitement produced by the nerves of smell; in most cases, it is true, you can refrain from satisfying your desire, but just of this desire you cannot free yourself, whether you be one of the strong or the frail. When the new-born child grasps at its mother's breast, and tries to suck it, as it also does with every finger offered it, you cannot avoid supposing a human, inborn instinct, and attributing to it that inconceivable action so beneficial to the child. The grown-up man who grasps at the *odorous meal* differs from the new-born child only in that he understands his action; the desire itself and the action resulting from it are as necessary consequences of the working upon the soul of the singularly excited nerves of smell, as is the child's grasping at and sucking its mother's teat. The slavish dependence of our soul upon the impressions of the sense of smell is very clearly evinced by the well-known fact that the same food, the flavor of which seems to you delicious, and invites you to eat when you are hungry, disgusts you in a state of satiety, so that the strongest will can scarcely induce you to eat of it. The replenishing of your stomach and the saturation of your blood with nourishing matter so much changes the disposition of your soul that it *reacts upon the impressions* of smell in a quite opposite way to its action in hours of hunger, and obstinately rejects what it previously desired, commanding your organs to convey it to the alimentary canal. You say you want to follow no more the odor of the food when you have eaten enough; but this freedom of will is not a whit better than that of the fox who did not want the unapproachable grapes—than that which makes you grasp after food when hungry.

How slavishly bending and winding does our *disposition* follow the lead of the nose in its various smelling exercises. A habitual smoker is a hypochondriac when deprived of his cigar; with the smoke he scatters his grief and cares to the winds; the flavor of coffee opens the sluices of eloquence and all the gates of the heart to the matron, while the tender fragrance of flowers charms forth a thousand sweet emotions in the soul of the maiden; the smell of a corpse oppresses our breast and fills us with horror, just as the odor of balmy incense inspires us with pious exaltation. A profound recognition of this dependence of our disposition upon the impressions of smell is involved in the superstition of the ancients who sought to propitiate their gods, whom they imaged to themselves purely human, by the odor of burned sacrifices, which odor, of course, we would not include among the pleasant ones—a superstition which even now, though in an altered form, finds its expression in the incense burnings of the Catholics. How often does it happen that an accidental impression of smell, such as we remember having felt another time under certain circumstances, becomes the cause of a long migration of the soul through the events of the past, deploying before us a long series of pleasant or gloomy pictures, and thus determining for a time the activity of our soul. But these physiological sketches may suffice; in drawing them we had no other object in view than to awaken in our readers the desire of becoming more closely acquainted with the tyrant whose mysterious influence so powerfully rules our spirit. Unfortunately, however, the physiology of the sense of smell still occupies an exceedingly low place, considerably below that occupied by the physiology of the other senses, so that we can satisfy that desire only in a very imperfect degree. We not only have no idea of what passes in the nerve of smell while it produces a sensation of smell in the brain, no idea of the way in which that nerve becomes affected in the cuticle of the nose, but we have also not the slightest knowledge of the external irritation which causes that affection, or of the qualities of substances which render them odorous. While, as regards the senses of sight and hearing, and also that of touch, as above treated, we possess the exact physical knowledge of the external agencies which the nerves of the eye, of the ear, and of the skin react upon, while the laws of the oscillations of the luminous ether, of the velocity and extent of its waves, of the vibrations of sound,

&c., have been elucidated with wonderful acuteness, neither physics nor chemistry answers the questions, what agent excites the nerve of smell; by what force musk or the ether of rose-leaves acts in this or that specific way upon the organ of smell; why the oxygen of the air, a gaseous substance like the strong-scented musk vapor, is odorless. If, then, in spite of this *testimonium paupertatis*, we still endeavor to produce a popular essay on this branch of physiology, we look for our justification in various reasons. Firstly, we hope to be able to give our readers something of general interest in the little that has been made out with positive exactness; secondly, we shall regard it as a merit if we can enlighten our readers concerning what we do not know, and if we succeed in dispelling some of the manifold deep-rooted erroneous ideas and conceptions which still haunt the imaginations of those uninitiated in scientific research. This negative part of our dissertation may even be the most meritorious part of the task before us.

Unfortunately, our very beginning must be with a negative truth, to which we have already alluded in our introduction on the theory of the senses. We do not know what a *sensation of smell* is, just as we are unable to explain and describe the nature of a sensation of touch, sight, hearing, or taste. Those conscious *conditions of the soul*, which we call sensations, defy all definition. Let one try to describe the fragrance of a rose, or the scent of musk, or what we call a spicy odor or a putrid smell, or to tell what distinguishes the scent of a violet from the smell of putrescent meat. Indeed, this is a problem which admits of no solution; everybody knows how a violet smells, everybody preserves in memory the often-felt nature of that sensation of smell, embodying it in imagination, and recognizing it even when the sense of sight does not present a violet as its apparent cause, but none is able to designate any characteristic mark of that sensation, distinguishing it from the nature of other similar ones. The designations which are used are therefore all borrowed from the external causes; they either directly name the object from which an odorous substance is evolved, or which is itself odorous in a gaseous form, (smell of roses, smell of oil of roses,) or they are selected in accordance with qualities and conditions under which external objects become smellable, (putrid scent, *roast smell*) When we enter a scented atmosphere, and, without knowing the object which makes it so, desire to describe our sensation, we cannot do it without mentioning some object which on a previous occasion caused in us a sensation of smell of the same quality, and therefore we say it smells like violets, like varnish, &c. Sometimes we recollect only having experienced previously a similar odor, without our memory having also preserved its cause, and in such cases we stand there helpless, completely unable to designate to any one the character of the sensation of smell produced in us. That we are still less able to compare a sensation of smell with a sensation of another sense—with a sensation of light or sound, for instance—hardly needs an elucidation. Not even sensations of smell and taste, or of smell and touch, which, as we shall soon see, the uninitiated are so prone to mix up with each other, can in any way be compared with one another, however paradoxical this may sound. We speak, for instance, of the keen and pungent smell of spirits of sal ammoniac, and yet the sensation thus designated as keen and pungent is no sensation of smell at all, but a so-called *common sensation*—a sensation of pain produced, not by an affection of the nerves of smell, but by an irritation which the vapors of that substance cause in the fibres of the nerves of touch spread through the cuticle of the nose—a sensation which has nothing in common with the true sensation of smell *simultaneously* produced by the same substance. We mistake it for a sensation of smell, because it simultaneously arises with one of that sense, and because we find that, like the latter, it comes from the nostril; not every one knows that the sensation of smell is produced in the upper parts of the cuticle of the nose, and the sensation of touch in the lower. Physiology, however, can prove with certainty, by experiments and observations on sick persons, that all the presumed acute burning, itching, or pungent sensations of smell are brought about, not by an excitement of the real and exclusive nerve of smell, but of the same nerves which, when gently touched, produce a ticklish feeling. There are individuals who do not feel the scent of either roses or violets, because their nerve of smell has been deprived by sickness of its active faculty, but who, nevertheless, are very well accessible to the pungent sensation caused by the vapor of sal ammoniac rising into the nostril. Everybody knows, from personal experience, that when deprived of all smell by a severe cold—that is, when, in consequence of an inflammation of the cuticle of the nose, smelling substances have ceased to act upon the extremities of the nerve of smell, he is still accessible to the acute sensation caused by spirits of sal ammoniac or mustard, just because this sensation is produced by the vapors of these substances penetrating the nerves of touch. On the other hand, it also frequently happens that we mistake a sensation of smell for one of taste, because the two happen to coincide. Thus we speak of the aromatic taste of a substance which has an aromatic scent; while we have that substance on our tongue, deriving from it a real (bitter or sweet, &c.) sensation of taste, vapors from it also enter our nasal cavity, which by the throat is connected with the mouth, and there produce the aromatic sensation of smell. As this latter coincides with a real sensation of taste, as well as with a sensation of touch on the tongue, which makes us believe the cause of the sensation to be within our mouth, while there is no distinct indication of the nasal seat of the sensation of smell, we place this too in the oral cavity, mistaking it for a sensation of taste. Many a *connoisseur* of wine may, indeed, be surprised to hear that while tasting that liquor his nose is as actively engaged as his tongue; that his praise or *blame* is being determined by smell no less than taste all the time he, with closed eyes and all kinds of grimaces, moves the noble juice of the vine to and fro on his tongue.

In spite of much earnest research, we know very little of the organs of smell, and that little cannot easily be made clear to the non-scientific inquirer. Before all, we must refer to the explanations which we have given in our introduction on the services of nerves of sense in general, and the means which enable them to perform such services. A sensation of smell takes place when a certain nerve, called the nerve of smell, which rises in the brain and spreads its extremities through the nose, becomes affected by a gaseous smelling substance; that is to say, when the process which we above designated as the "nervous current" is produced in that nerve, rapidly extending from the seat of affection, the cuticle of the nose, along the fibres of the nerve, to the brain, where, by means of a peculiar apparatus, it acts upon the mind. In itself, the nerve of smell is a nerve like all others; its fibres have the same appearance, the same qualities as the nerves of touch or sight, or even as a muscle nerve, the current of which in the muscle produces contraction, and, through it, motion of the limbs. Even the current which in the nerve of smell rushes to the brain, while conveying a sensation of smell, is, as we have seen, very probably, essentially like the current in the fibre of the nerve of sight, by which this produces a sensation of sight; or like the current in the fibre of the motive nerve, which, spreading from the brain to the extremity of the fibre, acts upon the muscle and causes a compulsory contraction. What, then, constitutes this nerve, essentially the same with all others, a nerve of smell? What explains, notwithstanding this identity with other nerves, the specific kind, incomparable with others, of its faculties? Similar reasons to those which explain why the same copper wire, with the same electric current, now moves the hand of a clock and now ignites powder; that is, the nature of the apparatus at the extremities of the nerve of smell in the cuticle of the nose and in the brain. In the cuticle of the nose we must suppose some specific apparatus arranged at the extremities of the nerve fibres, which effect that a current is produced in the fibres by a smelling substance; in the brain we must suppose apparatus at the extremities of the same fibres in which the current causes a specific process, still entirely unknown to us, out of which the soul forms a sensation of smell. This is the general answer which we have already previously attempted to establish; a special answer in regard to the nerve of smell we can as yet give only in the most imperfect way. In vain do we look with the microscope for apparatus at both ends of the tender fibres of this nerve, the mechanism of which would be so visible to us that we could explain by it the action of the nerve of smell, and its difference from the action of other nerves. With astonishment we discover at the extremities of the brain the same small, grainy, dark vesicles (nervous cellules) which we also find on all other nerves of sense. None of the powerful means of discernment of physiology gives us yet the slightest clue to the process which the nervous current produces in these vesicles; by what that process is distinguished from one taking place in the vesicles of the nerve of sight; and, still less, an answer to the question how this process can work upon the soul and force upon it that sensation which we call a sensation of smell. We do not fare much better when attempting to analyze the external extremities of the same nerve. Quite a short time ago nothing more could be stated than that its fibres probably ended in free points on the fundamental tissue of the cuticle of the nose, which tissue is precisely like that of every other cuticle. No mention was made of separate apparatus, the necessity of the existence of which the physiology of the time did not apprehend. Quite recently, however, a philosopher of great merit took an important step in advance, by showing the formerly presumed, but not seen, extremities of the nerve fibres, and by proving the existence of peculiar elements of tissue at those extremities. We would fain give up the attempt to produce a sketch of this, as we fear we shall not be able to accomplish a clear picture; but, being afraid, on the other hand, lest our elaboration be censured for incompleteness, we risk to be reproached with a want of lucidity. Imagine a nose cut open through all its length from right to left, or the whole external nose cut off, from the nostrils to the forehead, so that you can look without hindrance into the inner cavity, the aspect will be represented by the figure here attached. The whole nasal cavity is divided into two halves by a perpendicular partition (marked *S*) extending from the rear to the front.

You feel the beginning of this partition at the entrance of the nose, which is divided by it into two entrances, the two nostrils N N. Our figure shows you the very irregular shape of the nasal cavity, the external walls of each half forming a multitude of conspicuous projections jutting into the cavity. Three principal projections are noticed on each side which are designated nasal shells in consequence of their resemblance to a muscle-shell. In general, they form a kind of bent ledges extending from the front to the rear, projecting into the cavity with a kind of free list, and partly supplied with secondary ledges. Our figure represents these shells *a b c* in the cut, showing how they divide each half of the nasal cavity into layers overlying or freely communicating with each other, and, above all, how through them and their secondary projections a considerable increase of the surface of the nostril partition is brought about. The whole surface, with its prominences and corresponding hollows, is lined with a soft skin, the so-called cuticle, as shown by the double contours. This cuticle consists of two layers of a soft membrane directly overlying the bony wall, the base of which is formed by a net of thin fibres and a softish substance filling up the meshes of the net, and secondly, of an external coating in the direction of the nasal cavity. A microscope shows us this coating to consist of innumerable small, oblong, cylindrical vesicles, standing thick and perpendicular, in regular order, on the described base. Each of these vesicles has a pointed rear extremity by which it is attached to the base, and a broad front extremity directed towards the nasal cavity. On this broad basis of each single vesicle stands a wreath of extremely fine and tender hairs, continually shaken by a kind of lashing motion. Each little hair, in rapid motion, bends in a certain direction, assuming the shape of a hook, then raises itself, again bends as before, and so on. When we look, through the microscope, at a large row of such cellules, with their numberless little lashing hairs, we have in miniature the same aspect which a waving cornfield presents. The motion is so rapid that one at first perceives only a kind of glimmering along the edge formed by the bases of the vesicles, and only when the movements under the microscope gradually slacken the single hairs become visible and recognizable. On account of this phenomenon the said coating, consisting of vesicles, (cellules,) set with oscillating hairs, is called cilliated epithelium. Physiology has yet no explanation for this wonderful phenomenon; we entirely ignore what force, inherent in the vesicles, or working upon the hairs from without, causes the regular rhythmic oscillations of the latter, we only know that it is a force which is rapidly extinguished on the expiration of the organism. It is true, the epithelium separated from the organism, nay, even the single entirely isolated cellule continues its motion for a while, but the movement soon expires before we can discover a death change in the little mechanism. Some of my readers may expect me to designate the motive force of the hairs as "vital force," but I beg leave incidentally to remark that this is a name and a conception long buried in the lumber-chamber of the past, the resurrection of which would fill every conscientious physiologist with horror. I defer the justification of this horror to some other time, observing here that the forces which keep up the animal organism are no new or special ones, but the same physical and chemical forces which rule inanimate nature, and that they act in the organism according to the same inviolable laws which govern them in the outer world. But enough for digressions. Into this thus described cuticle of the nasal cavity, and through it in all directions, spread the tender and even microscopically hardly distinguishable fibres of the nerve of smell. According to recent discovery, each of these fibres most probably, approaching one of the vesicles of the outer coating, fixes itself to its rear extremity. If this observation be correct, those vibrating cellules have the significance of end-organs of the nerve of smell, that is, they are to be regarded as the apparatus upon which the odorous substance acts, producing a physical or chemical process, which excites the nervous fibres springing from the cellule, develops a nervous current and sends it to the brain. This view is exceedingly well supported. The fact that an odorous substance causes a sensation of smell on the slightest contact with the cuticle of the nose becomes explicable when we assume that the substance acts first on the cellules bordering on the nasal cavity; it remains an enigma if we have to assume that the odoriferous substance has to soak through these cellules into the tissue of the membrane under it in order to reach the nerve of smell which it is going to effect. But how an odorous substance, whatever it be, acts upon the vesicles, and upon their contents; what takes place in the vesicles; how by this process the nervous fibre becomes affected; how the influences of the various odorous substances differ from each other; all these are problems toward the solution of which no path offers as yet to lead us, or is likely soon to be discovered. We cannot hope to see the torchlight of scientific inquiry illumining the mysterious processes which we have just hinted at before accomplishing two essential tasks: First, the understanding of the external affection of the nerve of smell, that is, of the qualities which render a substance odorous, and of the forces by which it indirectly acts upon the nerves; and, secondly, a clear conception of the conducting process in the nerve itself of the so-called nervous current. The unavoidable necessity of making these preliminary scientific steps is as obvious as the sad truth that physiology is yet helplessly ignorant of how to make them. Of our ignorance of the nature of the affections of the nerves, we have spoken in our first article; the complete obscurity of the exciting agent has been alluded to in this. If we take, for instance, a simple smellable substance, like oil of roses, or a simple, elementary one, as chlorine, we know precisely all its physical and chemical qualities, but we are unable to state which of these qualities makes it odorous, why another gas, as, for instance, oxygen or hydrogen, is inodorous, that is, does not act in the required way upon the nerve of smell, or rather, on the substance of its end organs. Our scientific ancestors, it is true, knew well how to get over this difficulty; they invented a fine name for the unknown principle of odorous substances, and with that they were perfectly satisfied. They assumed a special *spiritus rector* inherent in odorous substances, without, however, being able to connect any clear idea with that name. We must here again decidedly come forth to combat an erroneous conception, though by so doing we risk being accused of useless repetition, having already in the general introduction commenced our warfare against the same deep-rooted error. Every one uninitiated in science, when asked about the qualities which render a substance smellable, will designate the odor itself as the quality inquired after, believing that a certain odorous essence, from without, has come to our consciousness, as a detached part of the essence of that substance which, by way of experience, we have found out to be the nearest cause of our sensation. Every one will attribute the quality of the sensation to the external object which is the cause of the sensation, as properly belonging to that object; this is the fundamental error we speak of, the same which makes us attribute the blue or green color to the external light, or to the light-spreading objects, to the "blue sky" or the "green meadow," and the sound to the vibrating cord. We repeat: the quality of the sensation of smell of which we become conscious has nothing in common with any quality of a so-called odorous essence, just as little as light developed by two carbon poles on the passage of a galvanic current has anything in common with the properties of the two metals which have produced the galvanic current, and have become the indirect causes of the carbon light, as essence of violets is the indirect cause of the odor of violets. We need not repeat how we are brought to attribute the qualities of all our sensations to the external objects which the soul habitually imagines to be the cause of sensation; we have seen how this way of rendering our sensations objective is, on the one hand, the necessary result of the education of our senses in the service of the soul, and, on the other, the principal condition of the measureless benefits they bestow on it. It is, therefore, an error, the origin of which is unavoidable, and its existence indispensable; which science must recognize as error, but which it cannot prohibit or destroy. The physiologist himself, who teaches you that color is not the quality of light, regards, like yourselves, the sky as blue, and the trees as green, and as obstinately as yourselves places the sound in the cord, and the scent in the violet; he fares like one seized by giddiness, who sees tables and chairs whirling around him, and though convinced of their standing firmly at their place is unable to resist the delusion. You all firmly believe in the theory of the astronomers, that the sun is at rest and the earth in motion, and yet your eyes again and again give the lie to the great dogma enunciated by Galileo. From sunrise to sunset they whisper to your soul of the sun what he told of the earth; "and yet it moves," and your credulous spirit, which piously follows the demonstrations of its first teachers, the senses, is easily seduced, and, in spite of its better knowledge, accommodates all its conceptions to the insinuations of the eyes.

If we cannot explain what makes a substance smellable, what the sensation of smell is, nor what series of physiological processes intervenes between the action of an odorous substance on the cuticle of the nose and the rise of the sensation, at least we are able to elucidate, in a popular way, some of the interesting *conditions* within which a sensation of smell takes place.

Everybody knows that odors are inhaled by the nose, that is, that a sensation of smell arises when odorous substances, blended with the atmospheric air, enter the nasal cavity in consequence of an inhalation. We cannot explain here the mechanism of breathing; we shall say, incidentally, only that our chest resembles a pair of bellows; when expanded by force of muscles, it receives a current of air, which, like that entering through the orifices of the bellows, serves to fill up the increasing space. Our bellows has two orifices, the oral and the nasal cavities, we can open and close both at will, so that the air is made to rush into the expanded chest either by both or one of them. At the time of regular and quiet breathing the oral cavity is generally closed, so that the air enters the chest only through the nose, and leaves it when contracted through the same; in this way light currents of atmospheric air continually pass by the cuticle of the nose, and each conveys to it whatever of odorous substance it has absorbed. Daily experience further teaches us that when regular, quiet breathing through the nose takes place the sensations of smell are comparatively faint; that their intensity increases considerably when the air is inhaled through the nose by strong and rapid blasts; that we thus repeatedly execute short but strong inhalations through the nose by intentional smelling or snuffing. Finally, it is a well-known fact that, even in an atmosphere saturated with most intensely odorous substances, we do not smell anything when inhaling the air exclusively through the oral cavity; though without closing our nostrils with our fingers, so that the odorous air enters the nasal cavity through the open orifices, but is not carried throughout it *in motion*. A closer analysis of these simple observations leads us to correct conclusions regarding the conditions of smelling. First, it is obvious that, together with atmospheric air, only such substances reach the cuticle of the nose which can be contained in the former, which are blended with it in an aerial form, whether they be originally gases, or changed into such, in the form of steam or vapor, from a solid or liquid state. Solid or liquid bodies which become gaseous at an ordinary, or only at a high temperature, are, as is well known, called *volatile*. Neither a liquid nor a solid substance, even if possessed of the necessary properties, can, under ordinary circumstances, penetrate the nose and cause there a sensation of smell. The question then arises whether there are among the non-volatile, solid or liquid, bodies such as would be able, when brought in contact with the cuticle of the nose, to affect the nerves of smell, it appearing probable that we only, therefore, never smell the non-volatile bodies because the ordinary carrier of odorous substances, the air, cannot convey them to the nose. The question can be answered, and is solved by very simple experiments; solid bodies can be blown into the nasal cavity, as powders, for instance; liquid substances can be brushed, squirted, and poured into it, as we soon shall see, when it becomes manifest whether they produce sensations of smell. Experiments of this kind have established it as a law, which knows no exception, that *all non-volatile bodies are odorless*, being devoid of that unknown property which is the exciting element for the nerve of smell. Only originally gaseous or volatile substances are smellable. But that the gaseous or volatile state is not the only condition which enables a substance to affect the nerve of smell is proved with certainty by the fact already alluded to, that not all gaseous or volatile substances are smellable, as, for instance, the odorless oxygen and carbon, and among the latter, water. There must, therefore, be another condition besides the one already mentioned, to render a gaseous or volatile substance odorous, and that most essential condition is the great, unsolved enigma. Another question is, whether the volatile or gaseous substances, which experience teaches us to be odorous, can only then act excitingly on the nerves of smell when they touch the cuticle of the nose in an aerial state, or whether they evince the same faculty also when dissolved in water. One is inclined to expect with certainty a smelling effect also in the latter case, as every odorous substance, even when coming in contact with the cuticle in a gaseous state, is probably imbibed by the vesicles of the epithelium, from which it acts upon the nerves only after being dissolved in the moisture of the cuticle, and as it would seem proper to assume that such a substance would preserve, in a liquid solution, all its essential qualities. The question is not so easily solved as would appear at the first glance, and therefore not yet solved with complete certainty. In a surprising way the experiments made prove more against than in favor of the smellability of odorous substances in liquid solution; but as in most cases there is a strong presumption of the possible existence of other reasons for inodorousness than the state of solution, no decided judgment ought to be formed. We shall only briefly indicate the way of proceeding, what regards must be had, and what precautions taken. Were we only to wet the cuticle of the nose with the liquid solution of an odorous substance, we could by no means conclude from the resulting sensation of smell that the liquid solution has been effective, as while, besides it, there is air in the nasal cavity, a part of the odorous substance combines with it, thus coming in contact with the cuticle also in an aerial state. The first condition of such experiments will, therefore, be to exclude from the nasal cavity all air which could absorb a part of the odorous substance from the solution, and to fill up with the latter the whole cavity. Such a filling up of the nose with liquid will probably appear to our readers not only as a very disagreeable experiment, but also as impracticable. Neither the one nor the other is really the case, as we can assure from experience; the experiment is very easily made, and neither painful nor in any other way particularly disagreeable; it by no means belongs to the list of torturings with which physiologists are so eagerly reproached. Let one man recline upon a long table and so hang down his head off the edge as to have the nostrils turned upward, and another will be able to completely fill up each of the latter with water abundantly poured into them. The liquid remains in the nostril as in a tumbler, without flowing down, as we would expect, into the throat, which is in open communication with the nasal cavity, of which breathing through the nose alone is a sufficient evidence. The mechanism which interrupts this communication, and forms a close partition between the nasal and guttural cavities, is the following: When looking into the wide-open mouth of a man, pressing down the back of the tongue with our finger or with a spoon, and thus glancing beyond it, we perceive in the background of the oral cavity an arched gate, leading into the guttural cavity, so that, with the help of a favorable illumination, we can see through the gate the hind wall of the throat. Hanging down into this gate like a curtain there is a soft fold of skin, with a prolongation in its middle, known as the uvula, which, reaching down almost to the threshold of the gate, divides this into two halves. This perpendicularly hanging-down fold, which is moved by peculiar muscles, can be bent down backward so as to stand horizontally, the uvula touching the wall of the throat. In this position the soft palate forms a valve entirely closing the throat, so that nothing can pass from the latter into the interior openings of the nasal cavity above it, or *vice versa*. This separation of the two cavities through the described mechanism takes place every time on the swallowing of food or beverage, which are by this means prevented from straying into the nasal cavity; it is the same valve which, in the experiment in question, prevents the flowing down to the throat of the water poured into the nostrils. Now, if we in this way fill the nasal cavity with water, mixed with a small quantity of *eau de Cologne*, of which it smells when held before the nose, a sensation of smell arises at the time of the pouring in of the liquid, and vanishes when the filling up is complete. This might appear at first glance as an unequivocal evidence of the odorlessness of the odorous substances when brought in contact with the cuticle of the nose in liquid solution, and thus also of the necessity of their being in a gaseous state for the purpose of smelling; but considerable doubts arise against this conclusion. Microscopic examinations of the cuticle of the nose have shown that the vesicles with which the latter is internally lined, and which seem to perform such an important mediating part between the odorous substance and the nerve of smell, are exceedingly tender and fragile, being easily burst by water which soaks into them. If those cellules be really the end apparatus of the nerves of smell in which a nervous affection is first produced by the odorous substance, we cannot be surprised to find that after their destruction by water odorous substances are unable to excite the nerve of smell. It has been proved, indeed, by direct experiments, that pure water poured into the nasal cavity in the above-mentioned way for some time suspends the faculty of smell, so that after its removal from the nose even strong-scented gases are not smellable when inhaled; and so it is more than probable that this destructive influence of the water causes the negative result of the experiments made with liquid solutions of odorous substances.

It would be very desirable to find a liquid which would not affect the cellules of the cuticle of the nose, and to repeat with it the same experiments. Moderately thinned sugar, or albumen solutions, or liquefied blood containing its red globules, would probably be found available in this respect. To my knowledge, however, no such experiments have been made. But should even these produce the same negative result—should, for instance, *eau de Cologne*, dissolved in water and poured into the nasal cavity, cause no sensation of smell, there would still remain one last possible objection to the validity of the conclusion that odorous substances are powerless in a state of solution, and that the gaseous form is the only condition of their smellability. For with the simple filling of the nasal cavity with a solution of an odorous substance there is still wanting another condition of the rise of a sensation of smell, to wit, the *motion* on the cuticle of the medium saturated with the odorous substance. We have already alluded to the daily experience which indubitably proves the ineffectiveness in the nasal cavity of odorous air in a state of rest. If we enter an ill-smelling atmosphere we best defend ourselves by closing the nose; but we avoid the sensation of smell also by breathing through the mouth alone. There can be no doubt that when the nostrils are open the external air which is saturated with odorous substance mingles with the air in the nasal cavity, and that the odorous substance is thus brought in contact with the cuticle. The absence of a sensation of smell can, therefore, have no other cause but the immobility of the air. As soon as we cause the lightest draught by opening the nasal channel, sensation immediately arises. Every smoker can convince himself in an agreeable way, and without exposing himself to a bad atmosphere, of the correctness of this statement. When holding the smoking cigar as closely near his nostrils as possible, without burning himself, he will feel warmth and a certain tickling in the nasal cavity from the entering smoke, but no trace of a real smell as long as he entirely avoids breathing through the nose; but at the lightest draught caused by nasal inhalations a more intense sensation of smell will immediately take place. Ladies can use, instead of the smoking cigar, a little flask of perfume with the same satisfactory result. The question here arises: In what way does the motion of the scented air cause the rise of a sensation of smell? This question, too, we must unfortunately leave without a precise answer. The nearest we find is not sufficient. The faculty of receiving impressions of smell by no means extends all over the wall of the double nasal cavity, being limited to the upper sections, to the middle and upper nasal shells, and the upper part of the partition. Through the cuticle of these parts only the nerve of smell spreads its fibres. The cuticle of the lower nasal shell, of the bottom of the nasal cavity, and of the lower part of the partition, is supplied with fibres by another nerve, which has been proved to be a nerve of touch, and serves as a medium for the sensation of smell of the nasal cavity. It is the excitement of this other nerve, by a light touch, that causes the tickling affection which produces sneezing, and the affection of which by sharp steam, for instance, from burning sulphur or spirit of sal ammoniac, produces those acute burning or itching sensations of touch which, by the common people, are mistaken for real sensations of smell. Now, as odorous substances can produce sensations of smell only when in contact with the upper parts of the nasal cavity, the motion of the odorous air caused by the nasal inhalation seems to have no other aim but to make it stream towards the upper smelling regions of the cuticle; and it would not appear irrational to suppose that when the air is in rest in the nasal cavity there arises no smell from the odorous atmosphere, because the gradual blending of the external with the internal air, in consequence of so-called diffusion, would not be sufficient to convey an adequate quantity of the odorous substance to the upper parts of the cuticle. But besides its being evident that after a sufficient lapse of time that blending must in all cases become so complete as to equalize the amount of odorous substance in the nose with that in the external air, the insufficiency of that explanation can also be proved by a direct experiment. If we blow some odorous air directly against the upper parts of the nose outside by means of a small tube introduced into the nasal cavity, there arises no sensation of smell, or at least a considerably fainter one than when drawing the same air through the nose by a motion of breathing. This shows that the motion of the odorous air which is caused by inhalation must have in it something peculiar, and that this peculiarity is the conditional something for the affection of the nerve of smell. But in what this peculiarity consists is another enigma for the solution of which physiology as yet offers but a suggestion itself in need of a further explanation. The lower nasal shell, which by itself is no medium for sensations of smell, seems yet to bear a relation to the odorous air entering from without, which is important for the development of a sensation of smell. It has been observed that the loss of the projection of the nasal partition, caused by disease, for instance, is generally connected with a considerable lessening or with an entire loss of the sense of smell, though it must be remarked that in these observations it may hardly have been ascertained whether the upper nasal shell had not undergone simultaneously with the lower such alterations as would directly cause the loss or a weakening of that sense. The part to be played by the lower nasal shell can only be a subject of conjecture. To these we are led by an examination of the position and form of the lower shell. This (as shown also in the above figure) forms a ledge extending from front to rear in a crooked oblique direction, and having its concave plane turned towards the bottom of the nasal cavity, and its convex plane towards the upper shells, its interior list being strongly bent downward. When propelling the atmospheric air into the nose by an expansion of the chest, the current of air receives such a direction from its position and form of the nostrils that it strikes the list of the lower shell, the nostrils extending downward, so that the nasal entrance presents a funnel turned upward. Were the nostrils perpendicular, and therefore the nasal entrance on each side a funnel turned backward, the current of air thus introduced would receive a direction rearward, streaming by the shortest possible way, along the bottom of the nasal cavity, towards its rear opening toward which it would be attracted, and being prevented by the gorge of the lower shell, as if by an umbrella, from ascending to the smelling region. But as it is, the stream of air receives an oblique direction upward, and must so strike the opposite list of the lower shell as to break on it, and thence to stream partly along its under plane towards the opening, and partly along its oblique convex upper plane towards the upper shell. The more effectively we inhale by the nose, the more surely and effectively the current of air is led towards the lower shell, and the greater becomes the part of the current branching off upwards. The streaming of the air against the lower shell can further be promoted by changing the form of the nasal entrance; the operation of snuffing, at least, seems to be explainable in this way. The wings of the nose are by it drawn upward so as to turn the nasal hole a little rearward, in consequence of which the inhaled current of air is necessarily turned still more directly upward, and a greater part of it carried upon the upper side of the shell. The mechanical part performed by the lower shell in conveying the air to the smelling region being thus established with tolerable certainty, the conjecture is natural, on the other hand, that its essential service to the sense of smell cannot consist in this simple act of conveying, as we have just seen that an odorous current blown directly against the upper shells produces only faint sensations. There must be something peculiar in that conveying operation of the lower shell; some change of the odorous current must there take place by which its faculty of affecting the nerves of smell in the upper cuticle is enhanced. In what that consists we do not know; all the suggestions made rest on slender foundations, and can expect belief only from confirmations as yet to come. It may be discovered on some future day that the current of air impregnated with an odorous substance receives on its way over the cuticle of the lower shell some admixture by which its power of affecting the end-organs of the nerves of smell is increased. As long as we do not know the effective principle of odorous substances we have little hope of obtaining full light as to the necessary conditions of their efficiency or of satisfactorily explaining what has been empirically discovered to be such.

That effective principle must be different in quality in different odorous substances; it must have as many different modifications as there are distinguished qualities of sensation; but its intensity is also exceedingly varying in different odorous substances, as some of these, even when received in large quantities from the atmospheric air, produce only faint sensations, while others, even when contained in the breathing air in an infinitely small quantity, in imponderable particles, still intensely affect the nerve of smell. We shall mention some well-known instances as evidences of the enormous power which that effective principle must have in some bodies; or, as may be differently expressed, of the extraordinary sensitiveness of our organ of smell in regard to certain substances. The most wonderful substance in this respect is undoubtedly musk, which, with incredible tenacity, preserves its efficacy in the very minutest parts. It is enough slightly to touch a musk-bag with one's fingers or a garment to have the smell in the touching part for days; nay, even if we spent only a short time in a musky atmosphere, our dresses will for a long time fill the rooms we enter with a musky odor. In order to offer an inexact idea of the minuteness of the particles of musk which are still capable of imparting some odor, we state, after a well-known experimenting physiologist, that a certain liquid, containing as much of an extract of spirit of musk as $\frac{1}{2000000000}$th part of its whole weight, was at times still distinctly odorous. A grain's weight of a liquid of which $\frac{1}{2000000}$th part was of that extract spread an intensely penetrating odor. These figures are naturally but of little value, as the other moments which are to be taken into account, in ascertaining the intensity of smell, cannot be precisely measured, and the intensity of the sensation not even inexactly. However, they facilitate the forming of an idea, and people like to see endless greatness or minuteness represented in figures, although a millionfold increase by means of a solar microscope is as little apt to create a clear conception as the number of miles, learned by heart, can do it in regard to the distance of stars from the earth. Next after musk are to be mentioned certain flower ethers, especially the oil of roses, a little drop of which is sufficient to fill with odor an immense atmosphere. The same physiologist states that a certain space filled with air of which, at the highest, only $\frac{1}{1000000}$ part was vapor of oil of roses, still diffused a distinct odor of roses. Figures like these could easily be presented in numbers, but we prefer restricting ourselves to those given.

The intensity of a sensation of smell depends not only on the quality of the odorous substance, and on the quantity in which it is carried upon the nasal cuticle, but also on the sensitiveness of the latter and of its receiving apparatus. This sensitiveness, it is well known, greatly differs in different persons, and still more in animals of different species. It varies also, and that considerably, in the same person, according to circumstances. It is positively certain that the sense of smell, like all other senses, can be refined by exercise; we gradually learn more easily to apprehend faint odors—to distinguish between different ones—to recognise the shades of one and the same odor. The nose of a connoisseur of wines boasts of being able to find out the land and the year which have produced a certain wine; the nose of a chemist is in many cases a reliable agent for discovering the presence of smellable chemical combinations. On the other hand, we often meet with a gradually developed obtuseness of the sense of smell, be it in consequence of old age, in which all senses suffer from a decrease of nourishment, of a habitual stuffing of the smelling channel with snuff, or of a diseased condition of the nasal cuticle. That every bad cold carries with it a considerable weakening or temporary loss of our smelling faculty is an experience which few men will not have gained. Some persons are inaccessible only to certain odors, their sense of smell being otherwise in a perfectly normal condition—a partial want, the cause of which is yet unexplained.

We shall say but few words on the aesthetic side of smelling. The opposite conceptions of fragrance and stench denote the fact that certain qualities of smell produce an agreeable, and others a disagreeable sensation. But it is hardly necessary to remark that there is no universally acceptable division of odors, without reference to the nose, into agreeable and disagreeable ones; that one person is delighted by an odorous substance which is loathsome to another; nay, that the same person may under certain circumstances call an odorous substance fragrant, and detest it under others. Habit, custom, bodily conditions, determine and modify the aesthetic effects of impressions of smell. Thousands of persons find the smell of valerian greatly disagreeable, while others rank it, as do the cats, among the perfumes; some are attracted, others are repulsed, by the smell of old cheese or of garlic. Even the most disgusting odors meet with particular favor; there are hysteric ladies to whom the smell of burned hair is incense. The Laplander rejects such tallow as would not, by its rancid smell, stand the test of his nose. It is true, we take the liberty of blaming their taste, or even to class them with the animals on account of this their presumed perverseness; but we do it, perhaps, with no more right than they would have to reproach us for placing musk among the perfumes. Very likely, the *esbouquet* atmosphere of a European fashionable lady is as distasteful to an Esquimaux as is to us his fish-oil smell, and neither he nor we have a right to declare his or our judgment exclusively acceptable. *De gustibus non est disputandum.* As regards the change of judgment in the same person concerning certain odors, we have already spoken of that daily experience that in a state of hunger we are powerfully attracted by a smell of food which disgusts us in a state of satiety. Finally, we must also remark that our judgment varies with intensity of an odor; that most persons find a feeble odor of clove oil pleasant, and a strong odor of the same disagreeable, and so on. The mentioned facts admit of no physiological conclusion; we have not yet made the faintest step towards explaining the question how a sensation of smell acts upon our imagination, or on what conditions depends the quality of a feeling produced by smell. It need not be further explained that to call a sensation of smell in itself agreeable or disagreeable is to speak incorrectly. In our introduction we have designated the sense of smell as a tyrant; it is but just that we should represent also its good qualities in their real light, and try to satisfy in our readers the desire of knowing the vocation and functions of that sense. A few hints will suffice; may the reader sketch out to himself the rest. The observation of animals is our best guide for that purpose; in many of them the sense of smell performs a more important, more obvious part than in man, who, if it must be, can even do without it. The finding of their necessary aliment, the discovery of their natural enemies, depends, with thousands of animals, on the services of the organ of smell, and hence the extraordinary fineness of that sense with them. The fox scents a dead body from a distance of many miles, and finds it by following the smell; the wind conveys to the game the exhalation of man over a large tract of land; it is the smell which, in the pairing season, brings together the males and females of a large number of animals, thus playing an important part also in the preservation of the species. With man, it is true, the functions of the nose are less conspicuous and apparently of less vital importance; but though he makes no use of it for finding out his nourishment or his enemies, still its vocation is not restricted to causing delight to the soul by sweet odors. Is it necessary to adduce special examples to show in how many ways we are indebted to our sense of smell for information regarding the presence or absence of substances or certain proceedings in the outer world, from which all kinds of judgments and actions may occasionally result? We shall specify only one side of our nasal activity. The organ of smell is frequently designated as a guard of respiration, as it informs us about the quality of the air which we inhale, and teaches us to shun certain noxious irrespirable gases, which are distinguished by a peculiar odor. This designation ought not to be misunderstood, nor the importance of that office overestimated. First, there is a number of irrespirable gases which produce no sensation of smell, against the inhaling of which, therefore, the nose is incapable of warning us. Secondly, it must be observed that the quality of a sensation of smell does not directly indicate the noxious or innoxious character of a certain gas, or of a certain admixture in the atmosphere, but that in order to find this out we must have acquired by other means the necessary experience concerning the effects of gases and vapors characterized by peculiar odors. Were we to form our judgment according to an idea immediately attaching itself to our sensation, and thus believe all agreeable impressions of smell to be innocent or useful, and all disagreeable ones to be injurious, we would fall into quite dangerous deceptions; for instance, we would be inclined to sip prussic acid without hesitation. Thirdly, we must still remark that we recognize a number of noxious aerial substances by the nasal sense of touch, and not by that of smell, and that these sensations of touch, on account of their highly disagreeable quality, more easily persuade us to avoid the inhaling of those gases than would the sensations of smell. Those pungent sensations of touch which are produced in the nose by the vapors of sulphur leave us not a single moment in doubt whether we shall inhale those vapors or not, and they would prevent us from inhaling them even if we knew them to be conducive to our health. In this limited sense only the organ of smell deserves the title of guard of respiration.

This much, or rather this little, dear reader, do we know of the sense of smell. We have candidly unveiled the weak sides of physiology, being convinced, as we have stated, that it must be of great use to the common reader to learn what we do *not* know, and that it is a merit to show the erroneous character of popularly current physiological notions, even when we are unable to replace them by exact truths. May our reader share with us the hope that the power of science, so nobly developed in our age, may once, and perhaps ere long, lift the veil still unpenetrated, and, according to some desponding minds, eternally impenetrable, which covers the mysteries of nervous life. Though the soul, the immaterial principle, must forever remain a *noli me tangere* of physiological research, the machinery by which it works will once lay open before the eye of physiology with all its innermost recesses and finest particles. We would not like to quarrel with the most genial of our poets, but it grates on our ear to hear Faust thus despairingly speak of nature. What it reveals not to thy spirit thou canst never extort with levers and screws. A little but essential alteration of this sentence will make it the device of the physiologist, with which he will calmly follow the trace of the highest problems. Our device is: Thou must extort it with levers and screws! The microscope, the chemical balance, and other manifold and ingenious apparatus which fill the armory of the physiologist, are our lever, our screw.

ELECTRO-PHYSIOLOGY:

A COURSE OF LECTURES BY PROF. CARLO MATTEUCCI, SENATOR, &c.

TURIN, 1861.

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

LECTURE I.—Introduction.—Definition of electro-physiology.—Distinction between the electro-physiological effects and the physical and chemical effects of electricity.—Apparatus for experiments in electro-physiology.—Measure of muscular power produced by electricity.

Grateful for the welcome which my auditors extend to this course of lectures, I ascribe their kind reception in great part to the common sentiment by which at present we are all animated. A professor of the University of Pisa, presenting himself as a lecturer at Turin, where at the same time he resides as senator, affords but another and significant token of our national union.

I do not now propose to entertain you with novelties, but I hope to communicate some portion of the warm interest which I feel in one of the most fertile and attractive sciences of modern times, *the science of the physico-chemical phenomena of living bodies.* It is a science whose discoverers made their first appearance in Italy, where it has since never ceased to be cultivated—a fact which needs no other proof than the names of Redi, Fontana, Spallanzani, and Galvani.

What I have said would have received the assent of that great man, one of the most extraordinary intellects of our century, to whom Lagrange is said to have once made the remark that there could be no further discovery so great as that made by Newton of the law of universal attraction, because there was but one world. There is still another, replied Napoleon—*the world of details.* This world is peculiarly that of the living organism.

But let us enter upon the subject. What is electro-physiology?

In former times, soon after the discovery of the electrical machine and Leyden jar, such was the wonder excited by the electrical phenomena that to electricity were attributed the most extraordinary effects on animals and on vegetables. Naturally this wonder was not diminished after the discovery by Galvani of the contractions occasioned in the frog by the passage of electricity. It was then the received belief, though founded on mere imagination, that plants electrified grow much more rapidly and luxuriantly than those not thus treated; that the soil might be fertilized by electricity, and that an amalgam of zinc substituted for the brain of an animal could restore its sensibility and intelligence. These presumptions of false science could not fail, of course, to be dispelled by those rigorous experiments which have led in later times to the foundation of the science of electro-physiology, a part of physics which has established certain laws on well-demonstrated facts, and has also advanced some hypotheses which are found to interpret a considerable number of those facts.

Electro-physiology falls readily under two divisions: the action of electricity on vegetables and animals—an action manifested by appropriate effects relative to the vital organism—and the development of electricity within the living organism itself.

I shall begin by briefly citing some instances of phenomena which have been referred to the domain of animal electricity, but which do not in reality pertain to it. Thus it was customary, as may be learned from books not of recent date, to cite as a proof of animal electricity the phenomenon developed by stroking with the hand a living cat, or in taking off silk stockings in dry weather. These are effects of electricity developed by rubbing, and may be equally obtained by rubbing with the hand a muff made of a cat's fur. Those also were called currents of animal electricity which resulted from touching with the two ends of the galvanometer the tongue and the forehead when wet with perspiration, or from introducing these ends into the liver and stomach of a living animal. Currents indeed arise, but they are of the same nature with those which we obtain by dipping the extremities, one in a solution of potassa, the other in sulphuric or nitric acid, thus bringing the two liquids into communication. In this experiment a piece of cloth is immersed in the acid, another piece in the alkali, the two pieces are placed in contact, the circuit closed by the extremities of the galvanometer, and we have a direct current from the alkaline to the acid cloth. Now the sweat is an acid, as is also the gastric juice of the stomach, the saliva and bile have an alkaline reaction, and hence there are direct currents in the animal from the alkaline to the acid liquids. Bellingeri, a distinguished Turinese physiologist, in a memoir on the electricity of animal liquids, thought that he had discovered electro-physiological phenomena, properly so called. He used to operate with a voltaic pair of plates, sometimes on the arterial blood, sometimes on the venous, and again on the urine or saliva. These phenomena, rightly considered, are, and indeed cannot be other than, electro-chemical, and the differences, whatever they may be, depend on the chemical composition of those liquids and their different conductibility. It has been also said that the electric current being passed into albumen, or white of egg, produced organization. The fact is that albumen is coagulated around the electrodes, because these and the liquid grow warm from the passage of the current, and because the acid and alkali produced by the electrization cause the albumen to coagulate.

I have multiplied these examples, because no doubt should be permitted to remain, especially in the beginning of this course, respecting the distinction which it is requisite to make between electro-physiological phenomena really pertaining to the living organism, and the electrical effects which are produced in animal or vegetable tissues whether alive or dead, and which are due to known physical or chemical actions. Of this kind are certain other effects of electricity on vegetation in which there has been a more persistent disposition to see a relation between electricity and the living organism.

You are, perhaps, not ignorant that there is an aquatic plant, the *chara*, whose stalk, observed with the microscope, presents a singular phenomenon. It is divided into compartments, in each of which are seen regularly moving or circulating globules or cellules. These movements, the cause of which is unknown, stop when an electric discharge passes through the stalk, and all circulation finally ceases if the discharge is very strong. We must infer that the discharge acts either by mechanically destroying the structure of the plant, or coagulating the liquid which it contains, or by altering the chemical composition, effects which all suffice to explain the observed results.

An eminent physicist of Turin, Vassalli-Eandi, made a series of ingenious experiments to show that seeds exposed frequently in the focus of an electric machine germinate before others not thus electrified. I know not whether there has been any verification of this fact, but it might be accounted for by attributing the effect to the oxygen, or rather to the *ozone*, which is known to favor germination; and ozone is formed in contact with the electrical focus. I have here the usual ozonemetric paper covered with iodine of potassa and baked starch, and it will be seen that, under the electric discharge, the paper becomes blue, an effect which is due to the ozone.

Your attention is finally called to what has taken place from causing an electrical current to pass for several days into a large piece of flannel moistened with a slightly salt solution, and on which were scattered seeds of the mustard, millet, and vetch. You observe that, in contact with the negative electrode, the seeds have already germinated and have even the small leaves, but this is not the case with those in contact with the positive electrode. Neither is this phenomenon an electro-physiological one, properly speaking, but a secondary effect of electricity. At the negative electrode is developed the alkali, and at the positive the acid, as is shown by the reactive paper. Now, in the slightly alkaline solution seeds germinate more readily than in pure water, and in the acid solution they do not germinate at all. In germination the diastasis should act upon the fecula in order to render it soluble, and convert it into dextrine and glucose, and this does not take place in presence of the acid, or, rather, takes place better in presence of a slightly alkaline solution. This is also shown by experiments in which, independently of a current, I have placed the same seeds, some in contact with slightly alkaline solutions, and some in contact with acid solutions. The seeds have germinated in the first case and not in the second.

Let us come now to true electro-physiological phenomena; that is, to the contractions which are excited in an animal, either living or recently dead, by the passage of electricity. It is thus that we designate the *electric shock;* the pain and involuntary muscular contraction which occur when we touch the two armatures of a charged Leyden jar, or the poles of a battery formed of several pairs, or the extremities of a spiral of an apparatus of induction. Before proceeding it is, perhaps, not superfluous to observe, that the property which the muscles possess of contracting is inherent in their nature, and that this property is chiefly manifested through the excitation of the nervous fibres distributed in the muscle. There is no muscle entirely destitute of nerves, by which may be demonstrated the truth first announced by Haller, that the muscles possess contractility. This truth is conformable to all physiological analogies, and the experiments are various by which it is established.

I have here two prepared frogs: one of them was poisoned with *curare,* the other killed while the first was dying. If I touch the nerves of the poisoned frog with the poles of a battery no muscular contraction is excited, but on operating with the current on the muscles contraction is manifested. In the other frog contraction takes place in both modes. The muscle then will contract under direct irritation when the nerves have lost their excitability. Lately a young French physiologist, M. Faivre, has shown that several hours after death, and when the nerves have lost all excitability, the muscular irritability is augmented. We have here, then, two distinct things: the irritability of the muscles, and the capacity of the nerves to awaken that irritability.

I will remind you, further, that from one of the finest experiments of physiology, for which we are indebted to Charles Bell, we know that *roots* of nerves issue from the spinal medulla, which, before uniting to form the so-called mixed nerves distributed in the muscles and in all parts of the body, have distinct properties. If the anterior roots be irritated, very strong muscular contractions occur, and nothing else; if the posterior roots, the animal utters cries and gives signs of pain, but there is no contraction. By irritating the mixed nerves in a living animal we obtain at the same time contractions and signs of pain.

Let us begin by preparing a frog in such a manner as may serve to exhibit contraction with the electric current. For that purpose it is divided in half below the upper members, the skin taken off, the viscera are removed from the lower section, and by introducing the scissors under the spinal or lumbar nerves a part of the pelvis is separated, by which means the animal is reduced to a portion of the spine, the lumbar plexus, and the two hinder legs. This is called the frog prepared after the manner of *Galvani.* I take a small pair of plates of Volta, formed with a wire of zinc and one of copper or platina twisted or soldered together at one extremity, and I touch the frog with the other and free extremities of the pair. If I touch the nerves the whole frog is strongly contracted, and is equally so when I touch a nerve and muscle; while, on the other hand, if the surface of the muscle alone be touched with the two extremities there is only a slight contraction in the portion of muscle interposed between them. The greater effect in the first case evidently results from the fact that the current, and consequently the excitation, is conveyed by the nerves into the whole muscular mass in which those nerves terminate. We thus arrive at a comprehension of the special effects of the so-called electric shock.

It is well known that the pain is only felt at the articulations, or at least that it is strongest at those points, and that when the quantity of electricity is increased, the pain is not felt alone at the joints of the fingers, but reaches to the articulation of the hand and arm. The shock increases if the hand with which the jar or battery is touched be wet, and diminishes if it be dry; in the former case the skin conducts electricity much better then when it is deprived of moisture. When a chain of persons is formed by contact of hands, those stationed at the extremity experience the greater shock. These various effects are readily understood through the principles of the propagation of electricity. It should be also remarked that the electric conductibility of the muscular substance is not less than five or six times greater than that of the nervous matter; and this is intelligible because the muscle is full of blood, and consequently of serum and saline solutions, while much solid matter enters into the constitution of the nerve. When electricity is discharged through the hands and arms the section of the conductor is narrowed at the joints where the muscular mass is smaller, and the *electrical density* in the nervous fibres at those points is much greater than in the same fibres imbedded in a large muscular mass: hence the shock and greater pain in the articulations. Franklin exhibited to his auditors an experiment which is explained in the same way. He caused the discharge of a strong battery to pass through a living rat when wet, and the animal was not injured; but when the rat was dry the same discharge killed it. There have been cases in which pregnant women have been killed by lightning without injury to the fœtus. Under both these circumstances the stratum of water served to conduct the electricity and protected the fœtus and the rat.

These general remarks being premised, and before proceeding to an exposition of the fundamental propositions of electro-physiology, it is proper that I should say a word of the apparatus of measurement applied to these phenomena.

Till recently those who studied electro-physiology contented themselves with saying that the shock produced by the discharge of the jar, or by the current, was more or less strong; that the frog was convulsed in a greater or less degree. We have now an apparatus of measurement which we call a *dynamometer*. Let us take a muscle, the gastrocnemius for instance of a frog, and fasten it by one extremity to a hook or pin; at the other extremity let us suspend with a hook a small weight of a gram or half a gram. When a current is passed into this muscle there is a momentary contraction of the latter, and the weight is proportionally raised. This is the force of the muscular contraction, which is computed as is that of steam or falling water, from the product of the weight by the space or height to which the weight was raised. It is necessary, therefore, in the first place, to measure this height. If we would render the elevation more conspicuous, resort may be had to an expedient which was once practiced in physical investigation, the movement may be made more extensive by means of wheels or a lever. But this procedure is now generally discarded, the direct measurement by which the movement is not altered being preferred, and this is obtained by the employment of small telescopes provided with micrometers. To render the movement of contraction visible at a distance I use a very light lever with very unequal arms. The end of the long arm moves over a graduated quadrant, and to the end of the short arm is fastened the leg of the prepared frog; the spine of the frog is fastened above to another hook or to a pin. When the current is passed into the lumbar nerves, the muscles contract, the short arm of the lever is raised, the long one moves in the opposite direction over a line 20, 50, or 100 times longer than that described by the short arm; that is, in the ratio of their lengths.

This movement is of course very rapid, and with the naked eye it would be impossible to fix precisely the point at which the index arrives in the act of contraction. For this reason Brequet, in a dynamometer which he constructed for me some years since, placed before the index, and in contact with it, a light ivory index, which the former pushed before it when the contraction occurred, and which, when that had ceased, remained at the point to which it had been carried. I repeat that, for precise measurement, it is necessary to read directly the elevation or contraction of the muscle. We should, therefore, operate on a single muscle, and not on a collection of muscles, such as are thóse which form the members of a frog, because, as anatomists know, there are in the same member some muscles which tend to raise the members, others to restore it to its position, and whose effects partially neutralize each other. Nor to obtain an exact measure does it suffice merely to use a single muscle, but this must be formed of fibres all equally long and parallel among themselves, and such a muscle, I think, is found under the tongue of the frog. For direct measurement there must be fixed beneath the muscle a fine metallic cylinder, on which the divisions are accurately marked, and this we observe with the telescope furnished with a micrometer; by which means we arrive at a rigorous determination of the elevation produced by the contraction. I should add that, to succeed fully in the experiment, it is requisite that the muscle should be kept tense, because it is raised vertically, but at the same time the strain should not exceed a certain limit, lest it be too much stretched and its structure altered. And to conclude what relates to the measurement of the contraction, I would state that we may determine also the times corresponding to the various phases of this movement. In effect, when the electric current passes through the nerve there is a certain time needed for the nerve to become excited, then for the excitation to traverse the nerve and reach the muscle; the muscle next contracts, and in a very brief space of time the contraction ceases and the muscle is again relaxed.

There is an ingenious method devised by Watt for measuring the velocity with which the pistons of the steam-engine move, and it is this method which I have used for ascertaining the duration of the muscular contraction. Let us suppose a small brush attached horizontally, after having been dipped in ink, to the muscle which is to be operated upon. It is obvious that if the point of this brush touch lightly a card of blank paper it will describe, when the muscle contracts and rises, a straight line on the card. But if the card be a disk which has an uniform movement of rotation, we at once perceive that the line will be no longer straight, but curved; and that, knowing the velocity with which the disk turns, we can, from the curve which is traced, deduce the time occupied by the contraction and elevation of the muscle. In a word, the length taken on the axis, called by geometers the *abscissa*, from the point at which the pencil begins its course to the point which corresponds normally to the vertex of the curve, determines the time which the disk occupies in describing this interval, and this time is that which corresponds to the duration of the contraction.

From many experiments it results that, in the first moments, when the vitality of the muscle is still considerable, the duration of the contraction is $\frac{1}{100}$ of a second; the entire contraction, that is, the contraction and relaxation of the muscle, occupies $\frac{1}{4}$ or $\frac{1}{3}$ of a second; which implies that the period of relaxation of the muscle is much longer than that of the contraction, strictly so called.

But if we employ a very strong discharge, the muscle remains contracted and shortened for several minutes, and sometimes permanently.

I shall here only add that, operating on a prepared frog in the manner which I have described, and using, for the purpose of closing the circuit, a wheel with teeth, part of metal and part of wood, we are able, by causing the wheel to revolve, to produce in extremely rapid alternation the passage of the current and its interruption in the frog. This will then be seen to contract many times in succession: if the wheel turns rapidly, that is, if the passages and interruptions of the current are numerous and at short intervals, the frog remains contracted and rigid; if the wheel turns slowly, the contractions are distinct, but diminish in intensity and presently cease. If we would renew the contractions, it is necessary to leave the muscle in repose for some seconds.

After having thus shown the best means for experimenting on the physiological effects of the electric current, we should proceed to state the laws of those phenomena, which will be done in the following lecture.

LECTURE II.—General facts of electro-physiology.—The physiological effects of electricity depend on the variations of the electrical state.—Quantity of zinc or of electricity extremely small in order to produce excitation of the nerve.—Principle of the preservation of living forces.—Mechanical theory of heat.—Application to electro-physiology.—Electricity excites the nerve, and the excited nerve occasions the chemical action of the muscular respiration.—Electricity acts as the spark which kindles a mass of powder.

In the first lecture I endeavored to define with exactness the phenomena which pertain to electro-physiology, and to distinguish them from others attributed to the electricity of vegetables and animals solely through the imperfection of our knowledge. With this view different examples were adduced in which electricity produces in the living organism effects dependent either on electro-chemical action or on known physical properties, and hence, independent of the condition of life and of organization properly so called. We have seen that the principal effect of electricity on animals consists in the shock—that is, in the pain and muscular contraction, which the discharge or electric current excites in traversing the muscles or nerves of an animal either alive or recently killed. And in order to observe more precisely this electro-physiological effect, I have proposed dynamometers suitable for measuring the muscular action, and described the manner of operating with them. We will now proceed to consider the laws of electro-physiological phenomena, commencing with an attempt to investigate their nature.

Whenever a Leyden jar is discharged by a metallic arc or any liquid conductor, and when with one of these arcs the circuit of a battery is closed, the different physical or chemical phenomena which are produced depend on the time and on the quantity of electricity which circulates, and which is the cause of those phenomena. Thus, if the current passes through the solution of sulphuric acid of a galvanometer and is constant, the quantity of water which is decomposed, and the quantity of zinc which is oxidized in the battery, are proportional to the time—that is, to the quantity of electricity which passes. And if the circuit instead of being constantly closed be alternately closed and opened by means of one of the common wheels of interruption having metallic teeth of the same length with that of the isolating intervals, it will be found that in the circuit constantly closed and in that interrupted the quantity of water decomposed in the same time will be in the ratio of $1 : \frac{1}{2}$. The calefaction of the conductors traversed by the current or discharge depends also on the quantity of electricity in a unit of time—that is, on the intensity of the current; and the same may be said of the action which the voltaic conductor exerts on the magnetic needle and of the property of magnetizing soft iron.

It is not so with the electro-physiological action. If the circuit of a battery be closed with wet hands, or if the current be made to pass into a prepared frog, whether in the muscles or nerves alone, or in both muscles and nerves of the frog or of any animal living or lately dead, it will always be found that the contraction and the pain are produced at the first moment when the electricity begins to pass, and that none is any longer felt or observed while the circuit remains closed. It is not, then, the quantity of electricity, not, as we might say, the passage of the current in the nerves and muscles, which occasions those effects. If, instead of keeping the circuit in which the frog is included closed, the passage of electricity is after a certain time interrupted, the signs of pain and the contraction are then manifested anew. And if, to effect these closures and openings of the circuit, a wheel of interruption be employed, the frog will be seen to contract and its muscles to be relaxed in succession at each passage and each interruption of the current. If these alternations be very rapid the frog is seized with tetanic contractions, which shortly destroy the nervous power and kill the animal. In this way it is possible with certain kinds of apparatus of induction to give even with a very weak current a series of repeated shocks which shall kill large and strong animals. This effect, therefore, is not owing to the quantity of electricity, but rather to the variation of the electric condition which arises in the nerves and muscles of a living animal at the moment of opening and closing a voltaic circuit. This variable state of the electric tension of a voltaic conductor which was heretofore admitted by physicists as a consequence of the well known theory of Ohm respecting the battery, is now placed beyond doubt by experiments made on the long wires of telegraphs. When the circuit of a battery is closed by touching its poles with a conducting arc, we know that the electric state, and hence the flow of electricity, does not attain at all points and at the same instant that permanent degree at which it arrives after a certain time, though a very minute one, but which has yet been measured by operating upon long telegraphic circuits. We now know that in a circuit of iron wire 500 or 600 kilometres in length, between the moment in which the circuit is closed, and that in which the intensity of the electric current is perceptibly constant at all points, there is an interval of time which has been found to be 15 to 18 thousandths of a second. This is the duration of the so-called variable state, which, according to the nature of the circuits and the apparatus which give the electric discharge, continues for a greater or less time. The electro-physiological action seems to depend on the velocity with which the permanent electric state is established in the nerves and muscles of the living animal: the less the duration of this variable state, so much the greater is the electro-physiological effect. We know that in the discharges of the Leyden jar or in the sparks of the electric machine, the quantity of electricity is extremely small. By comparing the heat developed in a wire of platina by a discharge of the Leyden jar, which lasts for a very minute space of time, less perhaps than $\frac{1}{24000}$, with the heat obtained by a small battery which with the same wire lasts for several minutes, the conclusion has been arrived at, if not with absolute rigor, certainly with great probability, that the quantity of electricity developed by the battery, in proportion to the very small quantity of zinc oxidized, is many times greater than that which constitutes the lightning of a heavy storm.

It is in our power at present, if not to measure the smallest duration of the electric spark, at least to determine the limit—that is, a quantity still less than that which represents the actual duration of the spark. Nor must I omit to make mention of that highly ingenious application of the measurement of very small intervals of time for the correction of certain indications which, through the persistence of the images on the retina, assume for us a false appearance. When a body revolves with a certain velocity, instead of seeing it at the successive points which it occupies, we see it in the form of a continuous ring, because our eye *still retains the impression of the body in a certain position for* a short time, about the tenth of a second, after the body has left that position; hence it is clear that, if the rotation is accomplished in less than $\frac{1}{10}$ of a second, our perception will be that of a ring or circle, because the body will return to the point from which it started before the first impression on the eye will have ceased. But this appearance is no longer realized if the body and our eye are illuminated by a light which lasts for an interval of time much shorter than that of the rotation of the body. This is precisely the case with the electric spark, whose duration has been measured upon principles already indicated, and has been found to be less than $\frac{1}{24000}$ of a second. A cord which seems enlarged when it vibrates, the insect which seems of greater size when its wings move rapidly, the liquid vein which appears continuous, are all illusions which cease when these bodies are illuminated with the electric spark:

The spark which endures for so short a time, and which is due to a very small quantity of electricity, at least in comparison with that produced in a battery, gives very strong shocks, or, in other words, violent pains and contractions. I take a prepared frog, and submitting it to the action of a small Leyden jar, I discharge the latter two, three, or four times in succession with a metallic arc, until not only do I no longer obtain sparks, but on testing the jar with a delicate electroscope I perceive no sign of electricity; still, as you see, the frog repeatedly contracts when I discharge the jar through it. It is not, then, I repeat, on the absolute quantity of electricity that the electro-physiological effect depends, but rather on the duration of the variable electric state of the circuit in which the nerves of the frog are included. This duration is, with an equal quantity of electricity, smaller in the case of the discharge of the jar than with the voltaic current. It is thus that the intense physiological effects of the discharge of the jar or of the inducted currents are to be explained.

These considerations have led some physicists to suppose a certain analogy between the electro-physiological effects and the action of the current in developing inductive currents when it begins and ceases to act. To represent to ourselves mechanically, and yet perhaps not inaccurately, how the excitation of a nerve arises under electricity, we may suppose that there then occurs in that nerve what occurs in a mass of soft iron or even in certain transparent bodies under the action of a strong electric current: there is a new molecular equilibrium which in the one case accompanies the new magnetic state, and which in the other gives to the body the property of causing the rotation of the polarized ray. The excitation of the nerve—that is, the faculty of causing the muscle to contract, would consist in, or at least be accompanied by a new molecular state, and the excitation of the nerve and the physiological effect would depend on the passage from one molecular state of the nerve to another excited by electricity and by the greater or less rapidity with which it is accomplished.

I deem it important to dwell upon these considerations, and shall employ for exciting the muscular contractions, not the discharge of the jar, of which it is impossible to know the quantity of electricity, but the electric current of a battery, made to pass for a very short time into the nerves of a prepared frog. In this experiment we can know the duration of the current, and hence the quantity of zinc which is oxidized in the battery in that time, as well as the quantity of muscular labor which the electric excitation of the nerve produces. This investigation will conduct us to important consequences regarding the nature of the electric excitation of the nerves. The experiment is quickly arranged: I take a battery formed of very small elements of platina and zinc amalgamated, because oxidation does not take place except when the circuit is closed. In the circuit of this battery I place a galvanometer and a prepared frog attached to the dynamometer.

This circuit is interrupted at a point where the two extremities formed by two springs of steel or brass are established very near, but not so as to touch one another. If we take a strip of brass and with it touch the two springs at the same time, the frog will at that moment be observed to contract and the needle of the galvanometer to deviate; in order that this metallic contact between the two springs may continue for a very short, yet still measureable, space of time, or, in other words, that the current may occupy a very minute but ascertained interval, it will suffice to have a large wooden wheel, and to fix the strip of brass on the rim of this wheel. Let us suppose the wheel to be three metres in diameter, to revolve in a third of a second, and the strip of brass to be one millimetre in extent. It is evident, the wheel having this velocity, that when the two springs which are in contact with its rim, touch the plate of brass, there will be a closure of the circuit, and the current will pass for an interval of time, which, under these conditions, would be $\frac{1}{9000}$ of a second. You will first observe that while the frog contracts as if the circuit had remained closed for some time, the needle of the galvanometer does not deviate, which evinces that the frog is an instrument more sensitive to electric discharges and to sudden and extremely evanescent variations of electricity than the needle of the galvanometer, which has a degree of inertia, and requires that the action of the current should endure for a certain time in order to move it. In the next place, if the passage of the current be repeated until a sensible quantity of zinc becomes oxidized in the battery, or the circuit be kept closed for a given time, we may succeed in measuring with exactness what is the quantity of zinc oxidized during that short closure of the circuit, that is, what quantity of electricity by passing in that minute interval produces a certain contraction, which may be calculated, as I said in the first lecture, from the product of the weight raised by the muscle and the height to which it is raised.

We have thus deduced from experiment, and with much exactness, two quantities between which a close connexion must exist—the connexion, in general terms, of cause and effect. These two numbers are the amount of zinc oxidized in the battery or the quantity of electricity produced, and the mechanical labor represented by the muscular contraction excited by that electricity. In order to explain more clearly the connexion here spoken of, I must bring to your notice very briefly one of the finest inductions of modern physics, the mechanical theory of heat.

Even a slightly attentive observation of physical facts, which are constantly reproduced before us, suffices to vindicate a principle which rational mechanics had demonstrated *a priori:* the principle of living forces. In a word, neither matter nor force is created in nature, and consequently neither matter nor force is destroyed. In every machine which is in motion there is always a motive force and a resistance to be overcome, and, if we would not admit the absurdity of perpetual motion, we must necessarily conclude that the motive labor, so to speak, and the labor of resistance must be equal in the same interval of time. When, by means of a lever with unequal arms, we see a small weight attached to the extremity of the long arm produce an equilibrium with a much greater weight at the extremity of the shorter arm, we might for a moment deem it an illusion; but no sooner is the lever put in motion than we see the principle verified of which we have spoken, namely, that the product of the arm of the lever by the weight will be equal on one side and on the other, which implies that the greater weight will traverse a space proportionably less than that traversed by the smaller weight, exactly in the ratio of the weights.

Although these truths were demonstrated in rational mechanics, they seemed to meet with some contradiction in physics and in experimental mechanics. In the collision of bodies, as when a body falls from a certain height to the earth or moves in the midst of water, in all cases of pressure, of resistance, of attrition, we were content to say that the forces were dissipated in vibratory movements, diffused themselves gradually into the great masses, were in effect annihilated. On the contrary, all these cases are only examples of transformation of forces—that is to say, of the living force which is transformed into caloric, or *vice versa,* of caloric transformed into living force. From numerous accordant experiments a quantity has been found which is called the *equivalent of heat.* This quantity, which is about 420 kilograms, signifies that when a body of that weight falls from one metre of height to the ground, this movement, or rather this quantity of labor, is not extinguished, but is converted into a quantity of heat capable of raising the heat of a kilogram of water by one degree centigrade, and that, *vice versa,* this quantity of heat may be transformed into the mechanical labor expressed by 420 kilograms.

I must not omit to mention the experiment which incontestably demonstrates the transformation of the living force into heat, an experiment which we owe to the genius of Davy. During a very cold season, when the temperature of the air was several degrees below 0°, he conceived the idea of causing two disks of ice to rotate in contact with one another. Of course a greater force was necessary to produce this rotation when the disks were in contact, and then both were found to be in part melted and converted into water. The heat thus developed could be nothing else than the living force consumed in the action of attrition. Another striking example of this transformation is constantly going on in nature; the water which falls from the clouds and which descends from the mountains to the main, producing so great an amount of labor, is nothing but the solar heat transformed.

This principle once admitted, it becomes necessary to extend it to the battery and to electro-magnetic motors, and finally, also, to the animal mechanism. The chemical action which takes place in the interior of the battery is a combustion, and whether it arises without development of a current, or when there is a current, the quantity of heat developed will be always constant, and only the seat of it will be changed, because the current transports that heat into various parts of the circuit. If we have an electro-magnetic motor, or a machine which produces a certain quantity of labor, and in which a certain quantity of zinc is burned, it will be found that when the machine is not at work there is developed in the circuit and in the battery more heat than when it is in action, and that the difference is just equivalent to the labor produced according to the quantity which we have given.

This digression was necessary to the conclusion which I would draw from our electro-physiological experiments, and it is time to return to that which we were making with the frog.

I have said that it is possible to determine the quantity, however minute, of the zinc which is oxidized in that evanescent moment in which the circuit with the two springs remained closed. By making this experiment with care we can ascertain the mechanical labor produced by the contraction of the frog—that is, the product of the weight raised and the height. We can make the application of the mechanical theory of heat to this case, by inquiring whether the zinc consumed in that instant of time develops a quantity of heat, and hence a quantity of mechanical labor equivalent or equal to that produced by the muscle. In this calculation it is assumed that the current which excites the nerve is transformed entirely into muscular contraction, and yet in reality it is not so, because the whole voltaic circuit is heated, and in the act of contraction the muscle also is heated. Upon that calculation, however, it would result that the mechanical labor of the contraction is, according to the theory, the same given, or rather a little less than the equivalent of the heat developed by the zinc oxidized in the battery. On the contrary, through the numbers deduced from rigorous experiments an opposite result is arrived at; the labor produced by the muscular contraction is at least twenty-five or thirty thousand times greater than that which would correspond, according to the mechanical theory of heat, to the quantity of zinc or to the current by which the nerve was excited.

This result necessarily involved a conclusion which experiment has plainly confirmed, and which serves greatly to enlighten our ideas on the properties of the nerves. To explain that result it was necessary to suppose that the electric current which excites a nerve acts as does the spark of fire which kindles a great mass of powder, or as a small force which causes a heavy mass in an unstable state of equilibrium to fall from a great altitude. It must be supposed, in effect, that when the nerve is excited by the electric current, the excited nerve instantaneously occasions in the muscle certain chemical actions which, by a concatenation as yet unknown, are transformed either into heat, or more probably into electricity, and eventually into mechanical labor; in a word, the chemical actions requisite to explain the muscular labor are within the muscle; it is in the muscle that they are produced and exhausted after having been aroused by the excitation of the nerve. It was known that a muscle exposed to the air absorbed oxygen and exhaled carbonic acid; it was known also that exercise of the body enhanced the chemical phenomena of the pulmonary respiration.

I shall be able to prove to you by a simple and conclusive experiment that muscular contraction is accompanied by an augmentation in the chemical action of the so-called respiration of the muscle—that is, by a greater absorption of oxygen, by a greater exhalation of carbonic acid. Here are two glass bottles of the same capacity, namely, about 100 cubic centimetres each. The openings are closed by a cork stopper, through which pass two wires of iron or copper which are bent horizontally in the interior of the bottle so as to leave between them an interval of twenty to twenty-five millimetres. I hastily prepare ten frogs *a la Galvani*, and suspend five in each bottle to the wires, by inserting the two extremities of the wires, one in the portion of spine, and the other in the inferior part of the pelvis. Everything should be arranged alike in the two bottles, and the difference will consist alone in connecting the two wires of one bottle with the extremity of an electrical apparatus, by which, for four or five minutes, I excite the greatest possible number of contractions in the five frogs of this bottle, while the five of the other remain at rest. After that time I promptly remove the cork stoppers and the frogs, and again close the bottles with stoppers of glass. In order to discover and measure the difference in the composition of the air which has been produced in the bottles, I ought to make, as has in fact been done, a strict eudiometric analysis; but this is not possible during the delivery of a lecture, and I must, therefore, content myself with showing in a rough but quite evident manner, that in the vessel in which the contractions took place there is much more carbonic acid than in that in which the muscles were left in repose. For that purpose I promptly pour the same quantity, ten cubic centimetres of lime water, into the two vessels and shake them. In the vessel where the contraction occurred great discoloration is produced, and consequently there is here a much greater quantity of carbonate of lime than in the other vessel in which the lime water is scarcely whitened.

It must not be supposed that all the carbonic acid developed by the muscular respiration is exhaled externally. If muscles be placed, after long and repeated contraction, in an atmosphere of hydrogen gas or in a vacuum, it will be found, as some years ago I proved by actual experiment, that those muscles exhale a great quantity of carbonic acid. Bernard, in analyzing the gases of the blood which traverses a muscle after a long contraction, found no longer any trace of oxygen, but nearly all the gas was carbonic acid. It may be said, therefore, that the immediate cause of the exhaustion of a muscle is an asphyxy, owing to the disappearance of the oxygen as well as of the air of the blood, and to the presence of the carbonic acid which is substituted for them. Perhaps the day is not distant when chemistry will inform us what are the immediate products of the greater combustion which occurs in a muscle through contraction; it is not by the carbon alone that the oxygen is fixed; the carbonic acid is a last term, and *there are, perhaps, also some fixed acids which are produced in that muscle.* In fact it has been proved by Dubois Reymond that the acid reaction increases in the muscles after contraction, and that the difference does not disappear by keeping the muscle in a vacuum in order to cause the carbonic acid to exhale.

It should be further observed that the mechanical labor of the contraction is not all the force which is excited in a muscle in that act. We shall see hereafter that electricity is developed. What I wish to point out here is that heat also is developed in a muscle in contraction. Bequerel did not make this fact apparent, because he operated on an animal entire and alive, from which it resulted that in the act of contraction the blood flowed into the muscles, and to this circumstance was attributed the rise of the temperature.

I shall place before you direct proof, derived from a frog prepared, and, therefore, without blood, of the heating of a muscle by simple contraction. I take two thermo-electric pairs of bismuth and antimony so arranged that the metals of the same name communicate with each other; the two bismuths, for instance, are placed in communication and the two pieces of antimony are respectively united to two extremities of a galvanometer. In this arrangement, if the two pairs are heated to the same temperature there is no current produced, nor deviation in the needle; but if one of the two pairs is a little warmer than the other, there suddenly arises a current, which we call differential. It is in this way only that we succeed in making quickly and satisfactorily a comparative experiment of this kind.

Let us return to our experiment on the frogs; we prepare two *a la galvani*, and into a thigh of one of them is introduced a thermo-electric pair, while the other pair is placed in the thigh of the other frog. We wait till the equilibrium of temperature is well established and the needle stands at zero, and then cause one of the frogs to contract, at the instant the needle deviates, as if the thermo-electric pair imbedded in the thigh of the frog which contracts were heated by contact with a warm body. Hence the simple contraction of a muscle developes heat.

All these considerations, on which, perhaps, I have dwelt too long, conduct us necessarily to the conclusion—a very important one for the theory of electro-physiological phenomena—that "the excitation of a nerve by means of a current, as in the kindling of a mass of powder by a spark, gives rise in the muscle to chemical phenomena; that is to say, increases the so-called muscular respiration; and it is through these chemical phenomena and the mechanical labor of the contraction, taking into account also the development of heat, that we verify in effect the relation demanded by the mechanical theory of heat." Thus we see the important progress which this part of electro-physiology has made in quite recent times.

Since from the chemical action of the muscular respiration there is a transition to the contraction of the muscle, it remains to discover by what mysterious concatenation this transformation is accomplished. It is certainly not my intention to show, by an experiment, how these things take place in the muscle, but only to make it better understood in what this mystery consists. I have here a large electro-dynamic spiral, in the interior of which I have suspended a piece of elastic wire or spring of soft iron. This last is fixed at top, and below is united to a silk thread which is wound around a very delicate pulley. Around the same pulley is wound in the contrary direction a silk thread, bearing a weight which stretches the elastic wire to a certain point. Lastly, to the axis of the pulley is attached a long index of ivory or straw. Every time that an electric current passes in the spiral, the elastic iron wire is shortened, its coils approach one another, and the index manifests the raising of the weight; when the electric current ceases the weight sinks, and the index moves in the opposite direction. It is precisely the same alternation which you have witnessed in the case of a muscle suspended in the dynamometer when the current begins to act and when it ceases. In the experiment which I now exhibit the current of the spiral magnetizes the coil of iron wire, and the constituent parts thus magnetized are drawn towards one another. It is a well-known effect controlled by recognized laws; the electric current is generated in the battery when there is zinc which is oxidized. In the muscle, also, there is this oxidation—there is carbon which combines with the oxygen and burns, and through this chemical action there is heat, and therefore force, developed. But what are the muscular elements corresponding to the magnetic elements? By what laws and with what force are these elements moved, in order to produce the contraction? Herein consists the mystery; or, rather, let us call it one of the most subtle of problems which physics and chemistry will one day assist physiology to resolve.

After these general propositions respecting the mechanism of contraction excited by the passage of the electric current in the nerves, we shall employ ourselves in the next lecture in studying the relations existing between these electro-physiological effects and the direction of the current, its intensity, and its course in traversing the nerves.

LECTURE III.—Manner of representing the action of the current in contraction.—Experiment of magnetic attraction within a spiral.—Laws of electro-physiology.—Different effect of the current according to its direction in the nerves.—Errors introduced into experiments of electro-physiology by derived currents.—Periods of Ritter and of Nobili.—Experiments of Masianini.—Galvanoscopic frog and its use.—Electric excitation of the nerve reduced to half when a given current is divided between two nerves.—The current does not act by passing in a nerve transversely.

The whole of my last lecture was occupied—and I hope not uselessly—in presenting some experiments and general considerations which give us an idea of the mechanism by which electricity, acting on nerves and muscles, produces muscular contraction. Unlike what takes place through the chemical, calorific, and magnetic effects of the electro-current, which depend on the quantity of electricity and on the duration of its action, electro-physiological effects are manifested only during those variations of the electrical state which occur at the closing and opening of the circuit; that is, in that minute interval of time in which any conductor traversed by electricity is passing from the natural state to the state of electro-dynamic equilibrium and *vice versa*. Independently of the quantity of electricity, the electro-physiological action is proportional to the velocity with which this variable state of the beginning and of the end of the current is produced, and this explains how the least sparks or discharges of the Leyden jar act strongly on the nerves and muscles, and how their action is diminished or extinguished by causing these discharges to pass slowly through conductors very long and imperfect.

When the quantity of the electricity and the velocity of the discharge combine, we have, as in the case of lightning, the most violent electro-physiological effects. It will be remembered also that the electric excitation of the nerve does not determine muscular contraction, except by previously exciting the chemical action of the muscular contraction.

Our attention should now be turned to the laws of electro-physiology—that is, to the relations which exist between the physiological effects and the direction of the electrical current in the nerves, its intensity, the physiological properties of the nerves, &c. Few are the parts of physics with which experimentalists have occupied themselves so much as with this, and yet too few are the rigorous and general conclusions at which they have arrived. For this reason I shall abstain from detailing to you all that is known or is supposed to be known on this subject, and which may be more or less confirmed, more or less contradicted in general, but shall restrict myself to the few propositions which have been demonstrated by experiment, and regarding which there is no dispute.

Our consideration will first be directed to the different electro-physiological effect of the discharge or electric current, according to the direction in which it traverses the nerves. By the direction of the current, we know, is meant the direction in which the positive fluid developed by the chemical action undergone by the zinc, and which is diffused in the liquid, moves in the interpolar arc. In a Leyden jar, of which the internal armature gives a discharge of positive electricity and the external of negative, the discharge is also said to be directed from the internal armature to the external in the interpolar arc. In speaking of the current transmitted in the nerves, it will be understood that this may be propagated either from the nervous centres to the extremities, in which case it is called *direct, descending,* or *centrifugal,* or it may be propagated from the extremities of the nerves to the nervous centres, being then called *inverse, ascending,* or *centripetal.*

This being premised, our first proposition is as follows: "In the mixed nerves, the first and sole effect obtained is the contraction produced at the moment when the direct or descending current, rendered as little intense as possible or propagated with the greatest slowness, begins to pass. On increasing the intensity of the current or the velocity of the discharge, the second electro-physiological effect which arises is the contraction excited, at the opening of the circuit, by the inverse or ascending current; on still increasing the intensity of the current the contractions occur at two other instants, namely, when the direct current ceases and when the inverse begins to act. These different phenomena embrace to a certain point the known electro-physiological periods of Ritter and Nobili, which chiefly consist in obtaining at first and while the excitability of the nerve is yet very great, contraction as well at the opening as at the closing of the circuit, whatever may be the direction of the current in the nerve; and, during a succeeding period of less excitability, contraction at the closing of the circuit only with the direct current, and contraction at the opening only with the inverse current."

I shall not describe all the different means which have been employed to establish this proposition experimentally, but shall limit myself to an account of that which has been practiced with most success, briefly indicating, however, in passing, the various causes of error which occur in other modes of operating.

Our experiment consists in preparing the frog after the manner of Galvani, in removing the muscles and bones of the pelvis, and cutting the symphysis of this pelvis. In this way the frog is reduced to the two upper members, which only remain united by means of the lumbar nerves connected with a portion of the spine. It is readily perceived that by touching one of the extremities of the frog or the corresponding nerve with a pole of the battery, and the other member or its nerve with the other pole, we shall have at the same time and in the same animal one of the nerves traversed by the direct, and in the other by the inverse current. In this way we can test with more certainty and exactness the effects developed by the current according to its direction in the nerves, since it is the same current which passes at the same time in two directions in two like nerves of the same animal. The nerve is not alternately subjected to the current in opposite directions; moreover, the direction in which the nerves are traversed by the current is well known, which is not the case when the poles of the battery are applied to a nerve laid bare in an animal and surrounded on all sides by muscles. We know in effect that when the poles of a battery are immersed in a liquid and much extended mass, or two points of an extended metallic plate are touched by two poles, the electricity is distributed through all the points of those conductors, forming, as it were, so many minute threads radiating from the points touched by the poles.

In the present case we must not forget that, by this diffusion of the current, it may happen that while the nerve is traversed between the two poles by a direct current from the positive to the negative pole, there may be in the same nerve, beyond the poles, another portion of current, called *derivative*, opposite in direction, and which may have a greater effect than the first. To represent this case, let us suppose we have a piece of string or pack-thread slightly wet and bent like the letter ∩ inverted, the extremities of which touch a surface forming a good conductor, such as a metallic plate. If we now apply two poles of the battery to the string, we know that the electricity will divide into two parts—that is, into one portion which traverses the string between the two poles, and into another portion which circulates beyond them, and which, meeting in the plate with a much better conductor than the string, may be even greater than the first. What it most imports for us to notice is, that if the current in the intermediate part of the nerve between the two poles be, for instance, *direct*, in the lateral parts of the same nerve it will be *inverse*. Hence the confusion which may arise in making an experiment under these conditions in order to judge of the effects due to the direction of the current.

Let us recur to the frog divided in half, and cause the discharge from the jar to pass from one member to the other, or from one nerve to another, while the animal is stretched on the metallic arms of the universal discharger. I have recourse to the small Leyden jar, and which, after being charged by a few turns of the machine, I discharge two or three times with a metallic arc. Thus reduced, so as no longer to yield signs of electricity with an electroscope, the discharge is turned upon the extended frog, when that member alone which communicates with the external armature, and the nerve of which, therefore, is traversed by the direct discharge, will be observed to contract two or three times in succession. In using the discharge of the jar we can only take account of the first effect, although in reality this must be composed of two opposite phases, which immediately succeed one another. With induction, also—that is, on generating an inductive current, as with the physiological effect, the discharge of the jar acts in that phase only which corresponds to the closing of the circuit in the case of the battery.

Let us substitute the battery for the jar, employing, however, the weakest possible current. With this view I take a glass tube, having an internal diameter of three or four millimetres, and a length of one metre; this tube is bent to the form of an U, and is fixed upon a wooden table. Having filled it with distilled water and plunged the metallic rheophores of the battery more or less into the liquid column, we obtain in the circuit a strong resistance, varying according to the length of the column. The frog having been prepared in the usual manner, and extended on the two wires of the discharger, so that the current shall traverse it from one member to the other, it follows that the current in one of the nerves will be direct, in the other inverse. I begin by using as long a column of water as possible, so that, whether from the greatly diminished intensity or the slowness with which the permanent electric state is established, neither of the members of the frog is contracted, either at the opening or closing of the circuit. I gradually diminish the height of the column of water, and the first contraction which ensues is always that of the member whose nerve is traversed by the direct current in the act of closing the circuit. I continue to diminish the liquid column, or, in other words, to increase the intensity of the current, and a second contraction supervenes, that, namely, of the member traversed by the inverse current at the opening of the circuit. If the current be still augmented, both contractions show themselves as if at the same instant; that is, in the member traversed by the inverse current at the closing, and in the other traversed by the direct current at the opening of the circuit. By proceeding with our experiments on the same frog I might show that if the current be now again reduced we shall repass through an inverse succession of the same phenomena, and in the end obtain, with the weakest current, only a contraction in the member traversed by the direct current and at the closing of the circuit.

These effects are transformed into the periods of Ritter and of Nobili, by using, at the beginning, an intense current. We should then see from the first the two members of the frog contract simultaneously as well at the closing as the opening of the circuit. But in that case, whether it be that the nerves lose excitability naturally or that this loss is hastened by the passage of the current and the contractions excited, it results that, after twenty or thirty minutes, more or less, according to the force of the current and the vitality of the animal, we have the phenomena of the so-called second period, which correspond to those we obtained by using the fresh nerve and very weak current. When the excitability of the nerve is diminished it is requisite that the current be strong; when the nerve is very excitable a feeble current is required.

The exposition which has been given establishes as the first, most simple, and most general of electro-physiological phenomena the excitation of the nerve at the commencement of the direct current. We also find it verified with the dynamometer that a given direct current excites at the commencement of its action the greatest muscular force. This simplicity, however, is no longer manifested when we operate on nerves entire and united to the nervous centres. It is easy to lay bare in a rabbit or a dog two long tracts of the sciatic nerves, and to cause a direct current to pass through one of them and an inverse current through the other. In this case it is a constant result that the greater contraction of the leg occurs at the closing of the circuit with the direct current, and the same contraction scarcely ever fails at the opening of the circuit in the member traversed by the inverse current. But the phenomena are not so distinct as in the frog, nor indeed as in the rabbit and dog when the nerves are divided at their exit from the spine. When this division is made, contraction occurs at first with the inverse current at the closing of the circuit, and it is necessary to prolong the passage of this current through the nerve if we would cause the anomaly to disappear and the contraction again to occur at the cessation of the inverse current.

By operating upon dogs and rabbits we have the great advantage of being able also to follow up the sensational effects which the current produces in the animal. The most constant phenomenon which is remarked in these cases, and which Bellingeri and Marianini have also verified in the living frog, is that the symptoms of pain arise when the direct current ceases and the inverse commences; that is, when the contractions terminate. At these two instants we realize likewise contractions in the back of the animal, occasioned by the so-called *reflex action*, as is proved, in effect, by dividing the spine at different points, when those contractions become successively circumscribed between the nerve irritated by the current and the point at which the spine is divided.

To these phenomena pertains an old experiment of Marianini, which has been of late extensively studied and varied by an eminent French physiologist, Dr. Chauveau, of Lyons. Marianini employed a voltaic pile of 50, or 60, or 100 elements; Chauveau uses apparatus of induction—that is, inductive currents. The former closed the circuit of the battery with his two hands, after having moistened them. This, it will be readily seen, is the experiment which we have witnessed with the divided frog, one nerve of which is traversed by the direct and the other by the inverse current. In the experiment of Marianini in like manner there is an arm traversed by the direct current, being the one which touches the negative pole, and in which the contractions are excited by the electric streams which traverse the muscular mass, as well as those which traverse the nerves in the direction of their ramification. In this arm, therefore, exist the conditions for rendering the shock and the contractions greater than in the other arm.

In the experiments of Dr. Chauveau the two electrodes are applied at two points, more or less widely separated, of the same muscular mass or of two different muscles. He found that the greater contraction is always in the proximity of the negative pole. This result, it will be seen, bears an analogy to that of Marianini, and, without resorting to other hypotheses, we may establish ourselves on that analogy. On this proposition is founded the use of the prepared frog, to which I have given the name of the galvanoscopic frog, for detecting not only the existence of the smallest variations of the electric state, but discovering also the direction in which the discharges or electric currents flow. The galvanoscopic frog consists of a leg of a frog to which is united a long nervous filament, being the sciatic portion, and also, if possible, the lumbar portion. It is necessary to make this preparation rapidly in order that the nerve and muscle may have the greatest possible vitality. When this instrument is to be used, we place the leg on a surface of gutta-percha, or in the interior of a glass tube which is held in the hand, and we touch two different points of the nervous filament with any two points of the body in which the electro-motor or power is supposed to exist. This being done, however weak, the electric current there will be a contraction in the galvanoscopic frog either at the closing or the opening of the circuit. By considering the position of the nerve and the moment of the contraction, we can judge of the direction of the current in the nerve, whether it be direct or inverse.

We shall see more fully, in the next lecture, why it is proper, in using the galvanoscopic frog to support it on an isolating surface; the frog, like every muscle, is an electro-motor, and without that precaution we should obtain the contractions and signs of the current through the effect of the muscular electromotor, and therefore through a cause which resides in the instrument itself. When the above precaution is used, the galvanoscopic frog becomes the most delicate instrument which we know, and it will be seen in the ensuing lecture on animal electricity what frequent use is made of it. In the mean time I propose to offer a single experiment with the galvanoscopic frog for the purpose, not only of conveying an idea of the sensibility of that instrument and of the mode of using it, but of exhibiting an instance of electricity developed entirely without metals. In this experiment I employ two long glass tubes filled with quartz or powdered glass moistened with water, one of which is plunged in a solution of potassa, and the other in a vessel of porous earth immersed in that solution and full of nitric acid. With the nerve of the galvanoscopic frog I touch the extremities of the columns of powdered glass or quartz which communicate with the two liquids of the so-called battery of Becquerel. We then see the frog contract, now at the closing, now at the opening of the circuit, according to the position of the nerve. The current, we know, passes from the potassa to the acid in the liquid, and circulates in the nerve from the tube of glass immersed in the acid to the tube of glass immersed in the potassa. Hence the frog immediately contracts when the free portion of the nerve touches the tube immersed in the acid, and the portion next to the leg touches the tube immersed in the potassa. We are justified, therefore, in the assertion that not only is the galvanoscopic frog in the greater number of cases the most delicate instrument we possess for discovering the presence of electricity, but that, properly used, it serves also to indicate the direction of the current.

A second proposition of electro-physiology is this: "That by using, for the excitation of a nerve, an electric current of very slight intensity, and such, therefore, that, being still further diminished, there would be perceived a corresponding diminution in the muscular contraction, if this current be forced to divide itself in half between two nerves, the effect excited in the muscle is reduced to half what it was at the first instant when the current passed entire in the nerve."

At first sight this proposition seems contrary to the general definition which we have given of electro-physiological effects: it has been our object to show that these effects arising only at the beginning and at the end of the current, and not during the time when the circuit remained closed, they were in some manner independent of the quantity of electricity. Relying upon a physical fact, of which the law and theory are known, I now add that the development of the inducted currents only takes place when the action commences and when it ceases; which does not, when all other circumstances are equal, prevent the intensity of the inducted currents from being proportional to the intensity of the inductive currents.

I arrange the experiment to demonstrate the above proposition by placing a prepared frog in one of the dynamometers, using a very weak battery, and introducing into the circuit a long column of pure water. In this way we employ the current reduced to the point at which a greater diminution of it is manifested by a diminution of the physiological effect. Having, then, already introduced into the circuit great resistance, when the current is forced to pass through a second nerve we see from the galvanometer that its intensity remains perceptibly the same. We have thus the certainty that there now passes in the first nerve half the current which passed in the first experiment, when that nerve alone was engaged. And if the experiment is well conducted, if we use in the dynamometer a single muscle, and operate under the same conditions, we shall see that the muscular force is approximately half as much as at first—that is, that the weight attached to the muscle is no longer raised to more than half the former height. We may therefore add that not only is the galvanoscopic frog, properly employed, the most delicate galvanoscope we have, but it is also a galvanometer.

The third well-established proposition in electro-physiology is this: "That the electric current does not act, or that its action is at least extremely feeble, when it is transmitted across the nerves, instead of traversing them in the direction of their ramifications."

In our present ignorance of the nature of the nervous agent and of its dependence on the structure of the nerve, it is impossible to determine precisely the significance of the relation; but we will admit *a priori* that it has its significance, and that it must be important. It would detain us too long to recite here the various experiments upon the strength of which the above proposition has, since the days of Galvani, been affirmed or contradicted. After having been long occupied with this subject, I will present to you that which seems to me most conclusive.

You see fixed upon this cube of wood two parallel plates, one of zinc and one of copper. I place between them a strip of moistened paper so as to form a battery. In effect, when I touch the two plates at the same time with a metallic wire, I am certain that the current circulates in this wire, and that at the same time the strip of paper is traversed by as many threads of the electric current all parallel to one another, of equal intensity, and directed from one plate to the other. We have the means of rendering this propagation of electricity in the liquid stratum evident. It consists in dividing the liquid stratum into two parts with a metallic diaphragm, which is a lamina of platina, and in using for a liquid that mixture of acetate of lead and of copper with which the celebrated electro-chemical exhibitions of our Nobili are found best to succeed. On causing the current to pass, if electrodes be used which have the same section with the liquid stratum, we shall see in a few moments the interposed lamina become colored equally at all points; and as this color proceeds from a stratum of matter deposited by an electro-chemical effect, and as its color depends, through a most delicate optical property, on the thickness of the deposited stratum, if the current had not the same intensity at all points, we should be quickly apprised of it by the difference of color. As this is not so, the filaments of the current which traverse the liquid from one electrode to the other have the same intensity at all points and are parallel.

Let us return to the former experiment. I prepare the galvanoscopic frog and dispose the long fibre of its nerve on the moistened paper which is between the two plates of the battery. If the fibre is disposed transversely, in which direction it is certain there can be no filament of electricity which traverses the nerve parallel to its axis, there will be no contraction seen in the frog when I set the battery in action by touching the two plates with the metallic wire. And observe that while I thus operate, the whole long nervous fibre is stretched upon the paper and is perpendicularly traversed by the current at all points.

I now invert the position of the nerve and stretch it, though to a much shorter extent, in such a way that the electric stream traverses it parallel to its length. Immediately contractions ensue, now at the opening, now at the closing of the circuit, provided the current is very weak, or the nerve has already lost a little of its excitability. It is beyond doubt, therefore, that the effect of the current upon a nerve is null, or at least extremely feeble, when the former traverses the nerve in a direction perpendicular to its length. It is not without importance to remark that whether we conceive the excitation of the nerve by the current to be due to a species of mechanical effect which is propagated towards the muscles or towards the nervous centres, or whether we consider that excitation under the analogy which it may bear to electro-dynamic action in general, we may, to a certain point, comprehend this difference of the action of the electric current according as it is propagated along the axis or perpendicularly to the axis of the nerve.

Whether we use in making these experiments the discharges of the jar or the inducted currents, when the two electrodes are applied directly on the nerve, now transversely and now along the axis, it may be easily understood how difficult it must be, with this arrangement, to realize the conditions of the experiment, and how it may happen that the currents should be always very strong, and that there should be always a portion of the electric filaments which traverse the nerve in a direction more or less parallel to the axis. In the mode in which we have operated these causes of error are removed, and therefore this proposition may be received as demonstrated by experiment.

LECTURE IV.—Electro-physiological laws.—Effect of the continuous current on the excitability of the nerve.—The inverse current exalts the excitability of the nerve and the direct extinguishes it.—Tetanic contraction produced in a muscle, the nerve of which was traversed by the inverse current at the moment of opening the circuit.—Secondary electro-motive power of the nerves and its application to the phenomena excited by the inverse current at the opening of the circuit.—Action of the current on the roots of the nerves.—Correction of the mode of interpreting the results of Longet and Matteucci with derived currents.—Action of electricity on the ganglionic system.—Medical uses of electricity.—Aneurism.—Tetanus.—Cure of paralysis.

To complete the first part of this course, that, namely, relating to the phenomena which the current produces in its passage through the nerves and muscles of an animal either living or recently killed, it remains to consider particularly the action which the current exerts on the properties of the nerve by continuing to pass for a long time through it. On this occasion I shall be enabled to give an explanation of the electro-physiological phenomena which are manifested at the opening of the circuit; an explanation which, from being founded on known physical laws, constitutes one of the most important advances made in the science of electro-physiology in later times.

Let us resume the usual preparation of the frog—that is, the frog, after Galvani's manner, cleft in the middle and traversed by the electric current from one member to the other. With this arrangement we can have, as was said in the preceding lecture, two nerves derived from the same animal and traversed simultaneously, one by the direct and the other by the inverse current, and be thus enabled conveniently to compare their state. The experiment may be prepared either by placing the frog astride between two small goblets filled with common water in which are plunged the two electrodes of a battery, or by directly applying these electrodes to the two nerves.

It is known in physics that when the current traverses a liquid by means of two electrodes of platina, the products of the electrization—that is, the oxygen and the acids which go to the positive pole, the hydrogen and oxides which go to the negative pole—become tenaciously fixed on the metals of the electrodes, so as to give place, by closing the circuit between these alone, to a current which we call *secondary*, and which has a contrary direction to the current of the battery; because, as is known in electro-chemistry, the current developed between hydrogen and oxygen, between an oxide and an acid, passes in the liquid from the first to the second. And that current, in effect, would be obtained in the same manner if those plates of platina which form the electrodes had been for a certain time immersed, the one in the hydrogen gas or the potassa, the other in the oxygen gas or the acid.

When the current is made to pass by means of the electrodes of platina placed on the nerves of the prepared frog, then, too, the secondary polarity is produced, and hence at the opening of the circuit of the battery and by closing the circuit of the electrodes alone we have the secondary current, which is capable of exciting the muscular contraction. If we wish, in some very delicate experiments, to avoid this secondary polarity, we use for electrodes, instead of wires of platina, wires of zinc amalgamated, and between these and the nerves is interposed a card moistened with a saturated solution of sulphate of zinc. Under these conditions, as I shall show in the sequel, the secondary polarity disappears entirely or nearly so, and hence there is a method, the great utility of which we shall presently see, of excluding from the circuits into which animal electro-motors enter, such as the muscles or organs of electrical fish, the secondary current, which, as being opposed to that of these electro-motors, fails not to diminish and even entirely destroy it.

Let us turn to the experiment with which I propose to show the action exerted by the continuous current on the excitability of the nerves, according to the direction in which it traverses them. In order that there may be no uncertainty in the results, and that you may have before you different proofs of the same truth, I have arranged four similar experiments, viz: four frogs prepared alike and all traversed from one extremity to the other by the electric current.

If the experiment is newly prepared we already know what occurs. When I open the circuit the member traversed by the inverse current alone contracts; and when I again cause the current to pass, that member contracts through which flows the direct current. This first experiment never fails when we use the frog recently prepared and possessing much vitality or a very strong current, for at first the two members contract as well at the opening as the closing of the circuit; but after a few moments the phenomenon appears, which may be distinguished as normal, namely, contraction in the member, which for brevity we will call direct, at the closing of the circuit, and contraction in the inverse member when the circuit is opened.

Before proceeding further, I must notice an important observation of Marianini: that the contraction corresponding to the opening of the circuit is manifested even when there has been no contraction at the closing. To show this, instead of causing the current to pass by immersing the electrodes of the battery in the water, I close the circuit with a conductor moist but not thoroughly wet, such as would be supplied by our fingers or a piece of flannel or cotton dipped in the water of one of the goblets. In this way the water penetrating layer by layer into the material or the hand, the current also enters and increases from degree to degree in the circuit, and hence the contraction is absent; not so, however, when the circuit is interrupted—then the passage of the current ceases of a sudden and the contraction is obtained.

I should also point out a mode of producing the contractions by the opening of the circuit, which, however simple, might, in minds not habituated to reasoning upon these experiments, give occasion to erroneous interpretations. I take a frog prepared as usual, and, after passing the current for some time, touch with a small pencil, wet with water or some other liquid, the nerve traversed by the direct current—no result ensues; I touch the other nerve with the wet pencil, and instantly violent contractions take place in this member. When this experiment is seen for the first time, it is not easy to hit upon the cause of the phenomenon, though very simple, and one might almost be induced to surmise a specific action of the liquid on the two nerves. But with a little reflection it is readily understood that when the nerve traversed by the inverse current is wet with a drop of water, this drop, which envelopes the nerve, has upon that nerve the effect of an opening of the circuit; the current leaves the nerve to enter into the liquid, which conducts it much better, and we have the contraction at the opening of the circuit in the member traversed by the inverse current.

These considerations being premised, it is time to see what takes place in the case of the prepared frog submitted from fifteen to twenty minutes or more to the passage of a current. At the moment I open the circuit the member traversed by the inverse current, and that alone, undergoes contraction, and for the most part remains tetanized or rigid for several seconds. If I then close the circuit there is no contraction either in the inverse member or in that permeated by the direct current; or if there be any in the latter it is slight, and ceases after the same experiment has been repeated three or four times. If the circuit be reclosed immediately after having been opened, that is during the time when the inverse member is still tetanized, we shall see this member relax and contraction cease. This experiment is equally verified if the current be made to pass directly from nerve to nerve without traversing the muscles; only in this case the described effect is produced in less time than when the current traverses the entire animal. The same results are witnessed when we operate on the living frog or the higher animals; but here the effect is produced more slowly, and it is with difficulty that we realize in the living animal that prolonged tetanic contraction displayed in the inverse member of the prepared frog on opening the circuit.

These results conduct us to the following proposition: "A continuous current transmitted in a mixed nerve modifies the excitability of the nerve in a different and it may even be said an opposite manner, according to its direction; the direct current enfeebles and destroys the excitability of the nerve, while the inverse increases it within certain limits. The time necessary for the current to produce these effects is proportional to the degree of excitability of the nerve and in inverse ratio to the intensity of the current. After the opening of the circuit the effects of the current have a tendency to disappear, and so much the more rapidly as the excitability of the nerve is greater and the current employed was weaker."

It is important to show that independently of the use of the electric current, the two nerves which have been traversed by that current have acquired a difference of excitability—that is to say, that in the nerve traversed by the direct current the excitability is much diminished or even extinguished, while on the other hand it is preserved or increased in the nerve traversed by the inverse current. For this purpose I take away one of the prepared frogs which has been subjected to the passage of the current, and touch first one and then the other of its nerves with a piece of potassa or hot iron, or wound them with the forfex. Whichsoever of these means is employed to excite the nerve, you will see contraction take place if I operate on the nerve which transmitted the inverse current, while none occurs from similar action on the nerve traversed by the direct current.

The above proposition brings to view and at the same time explains the phenomenon discovered by Volta, which is still called in electro-physiology the phenomenon of *voltaic alternatives.* It was Volta who first observed that on causing an electric current to pass for a long time in a frog until contraction had ceased, this phenomenon was realized anew when the current was made to pass in the opposite direction.

The exact analysis of this phenomenon is as follows: We have here the frog cleft, as usual, in the middle and traversed for a certain time by the current; it is thus reduced to the state in which there is no longer contraction, except in the inverse member at the opening of the circuit. I reverse the position of the electrodes or of the frog, previously staining with a drop of ink the member which was traversed by the direct current, and which no longer contracted. The current being allowed again to pass for a certain time, the limb which no longer contracted, and which is now traversed by the inverse current, begins anew to be contracted, while at the closing of the circuit you have observed the other limb contract through the action of the direct current, which before did not occur, and this from the excitability maintained in its nerve by the inverse current. In a word, the voltaic alternatives proceed from the different action which the current exerts on the nerves according to its direction: in the inverse nerve the excitability is preserved and augmented, so that when the direct current begins to pass, the contraction is excited, which would not be the case if the nerve had not undergone the effect of the inverse current or had been subjected to the direct current; in the direct nerve the transmission of the inverse current restores the excitability, so that it again contracts at the opening of the circuit.

It is by means of the dynamometer that this important proposition has been rigorously demonstrated. We easily contrive to have both halves of the same frog with a portion of the spine so fixed in the dynamometer that we can insert the lower hook now in one, and now in the other leg, and cause successively the same direct current to pass in one of the nerves and the inverse in the other. By then measuring the elevations obtained in the different experiments we arrive at an exact determination of the proposition in question. Thus we see that, in causing the direct current to pass at certain intervals of time by opening and immediately closing the circuit, the contraction due to this current decreases with great rapidity; yet it may be restored and with greater strength, at least within certain limits, either by abbreviating the passage of the current or by repeating its action after having left the circuit open long enough for the effect of the current to disappear. It is quite different with the inverse current. With this we see the contraction remain constant in the other half of the frog under a frequent repetition of the passage of the current. The effect of the inverse current may also be distinctly seen by causing it to pass, first for a fraction of a second, and then for three, four, or five seconds. In this way we obtain a contraction at the opening of the circuit which increases within these limits in proportion to the time of the passage of the current.

This experiment is even more conclusive when the inverse current is made to act upon a nerve whose excitability has been weakened by the passage of the direct current; to obtain the maximum of contraction it is necessary that the passage of the inverse current should be prolonged for a space of time extending from twenty-five to thirty seconds. When we operate on the animal alive or recently killed, it is sufficient, in order to dispel the effects of the continuous passage of the current direct or inverse, to leave the frog to itself, that is, outside the circuit. It will be found that the nerve which has lost its excitability through the direct current becomes from repose again capable of contraction by means of the same current.

I cannot leave this subject without calling your attention anew to the fact already noticed of the tetanic and prolonged contraction, by which the member traversed by the inverse current is attacked after a passage of thirty or forty minutes. This fact has in latter times furnished, through a known physical theory, an explanation of the electro-physiological phenomena which arise at the opening of the circuit, and has thus laid a foundation for the most important progress recently made in electro-physiology. The frog which I exhibit is prepared as usual and has been subjected to the passage of the current. I open the circuit, and on the instant the member traversed by the inverse current is attacked by the tetanic contraction. It may be shown by a very clear experiment that this contraction depends on a particular state into which the lumbar nerve has been thrown by the passage of the inverse current. In effect, if I interrupt the circuit by cutting this nerve at the point of its exit from the spine, the tetanic contraction ensues as usual, while it fails entirely if the nerve be cut precisely at its entrance into the muscles. But in what consists this particular state of the nerve? It is to this question that a recent physical discovery furnishes an answer.

We know what secondary polarity is, and I have before said that it is developed in the nervous fibres as in any moist conductor, such as is afforded by a strip of paper or of flannel imbued with a saline solution. Let us observe now the principal experiment. I take the sciatic nerve of a fowl, place it upon a handle of gutta-percha and convey it to the galvanometer; after having ascertained, by touching with the extremities of the galvanometer two points in the surface of this large nerve, that there is no sign therein of a current, I deposit the nerve on the two electrodes of platina or of moistened paper of a battery of eight or ten elements of Grove. I cause the current to pass for several minutes, and then return with the nerve to the galvanometer. I now obtain a very strong current in a direction opposite to that of the battery, and which, according to all analogy, is a current due to secondary polarity. In effect, this current is manifested in a nerve deprived of life for many hours or even several days, as in the nerve taken in the living animal; it is manifested in a wet string, the stalk of a green plant, or any solid body imbued with a liquid.

The passage of the current collects hydrogen and the bases at the points touched by the negative pole, oxygen and acids at the points touched by the positive pole. These products of electrization are gradually diffused through the moist solid body, and transform it through its whole length into a secondary electro-motor. It may be shown, indeed, that in a fowl which has been prepared precisely like the frog, reduced, namely, to the pelvis, a piece of spine, the two large lumbar nerves, and the legs, and which has been traversed by the current from one leg to the other, the two nerves have acquired the secondary electromotor power, without having been touched directly by the electrodes of the battery.

After having discovered the secondary electro-motive property in the nerves, I have also ascertained that this has not the same intensity at all points, and that the portion of the nerve placed near the positive pole, and in which the current enters, has acquired this property in a much stronger degree than the portion situated near the negative pole. This experiment also may be easily shown. I take the nerve after it has been subjected to the passage of the current, cut it in half, and reverse the position of one of the halves, reproducing the piece of nerve as before. As we know that the nerve has become, by the passage of the current at all its points, an electro-motor, the operation I have performed would be the same as if I had ten pairs of Volta, all in series, and then opened the battery in the middle by reversing five of these pairs, and reestablished the communication by replacing two zincs and two coppers together. Thus we have two opposite half batteries, whence, if the pairs have all the same electro-motive force, the two batteries are neutralized. But if one of the batteries is the stronger, we at once obtain the differential current, that is, a current due to the excess of the stronger battery. In like manner, if those two portions of the nerve which the current has converted into a secondary electro-motor have not the same force, I shall at once perceive it after having performed the division and placed one of the halves in opposition to the other. And, in fact, when I apply the nerve thus prepared to the galvanometer, I find a strong differential current, which is in the direction of the piece of the nerve which was next to the positive pole.

Hence it is a plain consequence deduced from a known physical theory, that, namely, of the secondary polarity, that a nerve becomes an electro-motor by the passage of the current, and is therefore in a condition to give a secondary current when the current of the battery has ceased. In a word, a nerve after having been traversed by a voltaic current is traversed by the secondary current, which is in an opposite direction to that of the battery, and which may circulate in those parts of the nerve in which the secondary polarization is not developed, and which is certainly neutralized and produces a discharge through the great difference of the secondary electro-motor power in different points of the same nerve. Thus is explained, with all probability, in what consists the state which we termed unknown, into which the nerve traversed by the inverse current which excites the tetanic contraction in the muscles of the frog is thrown. This nerve is none other than that portion next to the positive pole which is traversed by the current, and which has been shown to have acquired the strongest secondary electro-motor power; it is therefore that piece of the nerve which, after the opening of the circuit, is traversed by the discharge or by the secondary current, which is at once the strongest and most prolonged; and as the secondary current is always in an opposite direction to the current of the battery which has excited it, it follows that in the inverse nerve the secondary current is direct—that is to say, is in the direction which was shown from the first of our propositions to have the strongest electro-physiological action. Moreover, the effect of this secondary current must be further enhanced, because we know that the inverse current maintains the excitability of the nerve, and this perhaps in turn arises from the fact that the inverse current does not excite contraction, and does not, so to speak, consume the muscle, as does the direct current.

In conclusion, the contractions occasioned in a muscle, the nerve of which has been traversed by the continuous inverse current, are attributable, in all probability, to the physiological effect of the secondary current, which is direct. And that this current or direct secondary discharge exists is proved by the galvanometer, as has been already made apparent, and is proved also by the galvanoscopic frog. In effect, if I apply the nerve of this frog to the nerve of the fowl which was traversed by the inverse current, we see, immediately after opening the circuit, the galvanoscopic frog undergo contraction, and especially if we place its nerve close to those points which are nearest to the positive pole and in a suitable direction.

I must not omit to cite an important observation recently made by Pfluger, which will perhaps some day find a place among the electro-physiological phenomena, depending on the secondary electro-motor power of the nerves. Pfluger has found that while a nerve is traversed by a continuous current, "the excitability is augmented beyond the region or points touched by the negative electrode, and so much the more as the point observed is nearer to the electrode. The contrary is the case beyond the positive electrode, where, on the other hand, the excitability is diminished." To make this experiment, Pfluger causes a continuous current to pass in the sciatic nerve of a galvanoscopic frog, and then submits this nerve at the points not traversed by the current to the action of different stimulating bodies, as, for instance, water impregnated more or less with salt, and the action of which had been previously determined. He then finds that the stimulus, which applied to a certain point of the nerve does not excite contraction, is capable of producing that effect when the nerve is traversed by the current, if that point be near the negative electrode. On the other hand, a stimulus, capable of exciting contraction, excites it no longer when the nerve is traversed by the current, if applied towards the positive electrode. These curious facts have been attributed by the physiologists of Germany to a particular state of the nerve, with which we shall occupy ourselves further on, and which they term *electro tonic.* Before acquiescing in that hypothesis, it would have been proper to inquire what influence may be exerted by the products of the electrization collected on the nerve in contact with the electrodes upon the composition, and hence upon the physiological properties of the stimulating substances used by Pfluger. Moreover, we have found that the effects of the secondary electromotor power of the nerve extend beyond the points touched by the electrodes. Thus, between the portion of the nerve outside of the positive electrode, and the points touched by that electrode, there is a secondary current in the nerve in the direction of the current of the battery. Beyond the negative electrode also there is a secondary current, which has been found much stronger than that produced beyond the positive electrode, and which is also directed like the current of the battery. The origin of these currents and their direction is understood without difficulty, when we remember that they depend on the currents excited between the portions of the nerves which we will term neutral, and the points which, by having been in contact with or very near the electrodes, are bathed, one with an acid, the other with an alkaline liquid. These secondary electro-motor powers outside of the poles may well intervene in the effects observed by Pfluger.

After having explained, perhaps somewhat too diffusely, the application of the secondary electro-motor power of the nerves to the electro-physiological phenomena which are excited at the opening of the circuit, I shall proceed to speak briefly of the knowledge we possess, imperfect though it be, respecting the action of the current in the muscles alone, in the central parts of the nervous system, and upon the nerves of the ganglionic system.

When a current or a discharge is made to pass through a muscle, contraction is excited, and this takes place, as is natural, independently of the direction of the current. This contraction is sometimes persistent: thus, when the discharge of a jar is made to pass through the gastrocnemian detached from a living frog, this muscle is seen to be shortened by even one-fourth or more of its length and to remain shortened. We might say that the parts of a muscle, when the nervous action is extinguished or *quasi* extinguished, tend to approach one another, and, once in this condition, remain so, as occurs after death in regard to the cadaveric rigidity.

I have already said that Chauveau has, in late times, varied the experiments made on the muscles by applying the inducted current on the muscular mass, and has found that the greater and more persistent contraction always occurs in contact with the negative electrode. We have explained this phenomenon as far as is yet in our power, by attributing it to the action of the *direct* current on the nerves of this muscular mass. I will further recall that the contractions excited by the current in the muscles of a frog killed with the poison of *curare,* and the nerves of which have lost all trace of excitability, prove that the muscular fibre alone, independently of the nerves, contracts under electricity as under other stimulants.

Many years since Longet and I studied the action of the electric current on the spinal radicles; that is, on those simple nerves which, according to the discovery of Charles Bell, possess separately either the property of exciting contraction or that of occasioning pain when they are irritated. A long series of experiments, which were afterwards verified in Germany and elsewhere, conducted us to results which were chiefly noticeable for being opposed to those set forth in our first proposition, which are obtained in the mixed nerves. In effect, by operating with the direct current on the anterior radicles, no contraction was obtaining at the closing, as there was at the opening of the circuit; with the inverse current contraction takes place at the closing and none at the opening. We are now able to say that this anomaly is removed in consequence of an ingenious observation made by the French physiologists, Rousseau and Martin-Magron. In the experiments made by Longet and myself, the spinal radicle was not separated from the spine. but was simply raised to the handle *(ansa)* with a silk thread or by an isolating surface placed beneath it. In this mode of operating, as has been already shown, the greater portion of the current does not pass in the nerve directly from one electrode to the other by the shortest interval, but flows beyond the poles, being discharged into the neighboring conductor, whence it is that in the portion of the nerve next to the muscle the *derived* current has a direction opposite to that of the portion of the current which passes from pole to pole by the shortest interval. I recur to the experiment, before exhibited, of the wet string or cord bent in the form ∩, and which descends upon a stratum which is a good conductor. If I cause the current to pass in the upper portion of the string we shall see a contraction in the galvanoscopic frog whose nerve is extended on the string or on the conducting surface beyond the poles; this contraction is certainly the effect of the so-called derived current. Therefore, to explain the effects of the current on the spinal radicles, it is necessary to decide which of these two portions of current is that whose physiological action is in the ascendency. I will first state that if, instead of operating on the spinal radicles still united to the medulla and lifted to the handle with a silk thread, we cut these radicles at their departure from the spine and keep their ends free, it will then be found that the electric current acts as on the mixed nerves.

In this arrangement the derived current beyond the poles can no longer exist; it was this current, therefore, which interfered in the case of the anomalies observed by Longet and myself. And, in fact, it is easy to prove, in regard to any mixed nerve, that in proportion as its excitability is extinguished, this excitability continues to retreat towards the muscles if the current is so employed as to excite contraction, and, on the other hand, that it retires towards the nervous centres when the effects of sensation are contemplated. No wonder, then, if in our experiments on the spinal radicles it was the derived current acting in the portion of the nerve nearest to the muscle whose action was the prevalent one. Of this an experimental proof can be given. Take, in order to form one of the electrodes, a small fork of two branches and apply this fork to the nerve of a galvanoscopic frog, while the other electrode is placed midway between the points of the fork. If now the middle electrode be, for example, the positive one, the current is divided into two branches, one descending and the other ascending; and although these two portions of current be equal, it will be found that the prevailing effects are those due to the descending portion which is nearest to the muscle.

The same explanation applies to the experiments recently made by Dr. Radcliffe, by causing the current to pass through one of the lumbar nerves of a frog prepared in Galvani's manner. If this nerve is raised to the handle and the current be made to pass in the portion thus raised, contractions are excited in the member whose nerve is not traversed by the current and which evidently depend on the portion of the current which circulates beyond the poles, and which, hence, invades the other lumbar nerve.

The investigation has been extended to the action of the current on different parts of the brain of an animal alive or recently killed, and it was simply found that the current acted there like mechanical stimulants applied to those parts. By operating with electricity on the ear or eye, the effects occasioned are sensations characteristic of the excitation of the nerves of those organs. As regards the ear it is known that the passage of the current excites a continuous sound; in the eye a luminous sensation is produced at the beginning and at the termination of the passage of the current, as if the effect depended on a mechanical action undergone by the eye and due to the contractions of the muscles of the eye itself. These studies are deserving of greater development.

I shall say a word, finally, of the action of the current on the ganglionic nerves. Humboldt first studied the action of the current on the cardiac plexus and on the ganglionic system of the lower belly. In the former case he observed, and it was afterwards verified by others, that on keeping the circuit closed for a certain time the pulsations of the heart show no difference, but that if the electric excitation be continued these pulsations become more frequent, and that this frequency lasts for a certain time after the current has ceased to pass. When the ganglionic system of the lower belly is operated upon with the current, an analogous fact is noticed. The vermicular motion of the intestines is by degrees accelerated, and this acceleration also continues for a certain time after the opening of the circuit. In these two effects the electric excitation of the ganglionic nervous system would seem to differ from that of the mixed nerves in being, as regards the former, continuous during the passage of the current, slower in manifesting itself and slower in ceasing.

This subject, as well in its therapeutic applications as its physiology, calls for renewed investigation and more thorough research, especially as regards man himself; and its prosecution should be directed, among other things, to determining with exactness the quantity of urea in the urine, the quantity of carbonic acid exhaled, the varied composition of the bile and of the products of digestion, according as man or the animal experimented upon has undergone for a longer or shorter time the action of the current, both interrupted and continuous.

This study would be of so much the more interest as we now know that the ganglionic system acts on the sanguineous vessels at one time by constricting, at another by dilating them. This has been verified in the valuable experiments of Budge and Bernard. After having divided, in a living animal, a certain nervous fibre which receives at least in part its action from the ganglionic system, through which division the ear of the corresponding part becomes much warmer than the other and is engorged with blood, the physiologists just named found that, by exciting with the current the peripherous portion of the same fibre, the circulation of the blood was restored and the elevation of temperature, which was but a secondary effect, disappeared. It is probable that in the functions of secretion and in the physio-chemical action of nutrition analogous effects would present themselves.

I cannot conclude this lecture, which completes the study of the physiological phenomena excited by electricity, being the first part of this course, without a glance at the therapeutic uses of electricity. After the discovery of the Leyden jar, and even later, after that of the voltaic pile, the new and singular effects of electricity had so exalted the imagination of the students of therapeutics that the mysterious agent of life, the universal medicine, was supposed to have been found. These fantastic expectations were of course soon dissipated, and there remained, as there will continue to remain, in the science only the results of observations well and diligently made and specifically founded on electro-physiological researches.

As it had been observed that albumen coagulated around the positive electrode, and this through the acid developed by electrization and which coagulates the albumen as any free acid would do, it was inferred that the electric current might dispel the cataract, if in the no longer transparent crystalline an alkali were generated with the negative electrode. In making the experiment of the electrization of albumen, the coagulum is seen to be formed around the two electrodes, although, as might be expected, in a greater degree around the positive than the negative one, but it is never found that, by inverting the current, the albumen which has coagulated around the positive electrode is dispersed, because this electrode has become negative. Hence it is that with the electric current not only can the cataract not be cured, but, on the other hand, it may with great certainty be created.

The use of the electric current has been also proposed for dissolving calculi in the bladder. To judge of this method it is sufficient to recollect that the calculi are formed of materials insoluble in the urine, and that the current cannot decompose an insoluble compound. For the proposed purpose it would be necessary to fill the bladder in which the calculus exists with a concentrated alkaline solution, through which a very intense electric current should be made to pass.

Some cases have been cited of the cure of aneurismal sacs by the method of Petrequin. This surgeon having introduced into the sac needles of steel, brought into close proximity or nearly into contact, caused a strong electric current to pass through them. Through the heat produced or other cause, coagulated masses are formed in the sac, an inflammatory process is excited in its parietal surfaces, and favorable results seem to have been realized from the practice, as in the case also of incipient and limited aneurisms. The employment of a very fine wire of platina, rendered incandescent by the electric current, has been proposed in surgery for the purpose of cauterizing and promptly separating the polypus in any concealed situation. Ruhmkorff has suggested the use of the electric light to render visible the morbid state of certain deeply-seated parts of the body. Professor Burci, by causing a strong electric current to pass through an extra-uterine fœtus, destroyed its vitality, and the fœtus was then gradually dislodged.

It is, however, in the cure of tetanus and paralysis that the application of electricity has been most confidently and persistently relied on. These cures rest upon the electro-physiological facts with which we have become acquainted. The direct and continuous current destroys the excitability of the nerve—that is, places the nerve in a state which may be considered analogous to that of a nerve paralyzed. The inverse current, on the other hand, increases that excitability, and within certain limits restores it when it is lost. This is the case of the voltaic alternatives already brought to your notice. It will be remembered also that at the opening of the circuit we have seen the member which had been traversed for a certain time by the inverse current attacked by a strong tetanic contraction which lasted many seconds; to cause a cessation of this contraction it is sufficient to re-establish the continuous passage of the current. We have further seen, by causing to pass in the nerves of a living animal a current interrupted by means of a wheel of interruption, or an apparatus of electro-magnetic induction, which has the interrupter of De la Rive in the circuit of the battery, that the animal is seized with violent tetanic convulsions which soon destroy it. These strong and continued contractions naturally consume a large supply of nervous power, and in so short a time that it cannot be restored by the organism; in this lies, perhaps, the mysterious action of certain poisons which operate on the nervous system with so much energy.

I will finally adduce an observation which is easily comprehended, and which may assist in explaining the mode of the action of electricity in paralysis. Let us suppose that we divide the motor nerves of the two lower members of a living frog, and that, for a certain time, we every day irritate one only of these nerves. If, after that time, we test with the usual stimulants the degree of muscular irritability of the muscles of the two members, we shall always find that this irritability is much greater in the muscles made to contract daily than in those which have continued in repose.

Such are the facts and principles upon which is scientifically founded the use of electricity for the cure of tetanus, and especially of paralysis. With a view to overcome the tetanic contractions, it is necessary, upon the principles above set forth, to subject the patient to a continuous current. I know of but one case of an attempt of this kind. During the passage of the current the sufferings of the subject were alleviated; but unfortunately, because perhaps in this instance tetanus was the effect of an inflammation excited by the presence of extraneous bodies, the melioration was but transient. But if the electric treatment had no other object than to mitigate the tortures of so intractable a malady, I deem it incumbent on physicians to make new trials of the application of the continuous electric current on this form of disease.

The electric treatment of paralysis is founded on such a number of cures as to justify us in regarding it as an important resource. I shall abstain, however, from minutely describing all the distinctions which have been drawn by therapeutists between the effects of the currents, according as they are obtained directly with the battery or inducted in different modes. These distinctions are, in a scientific, view so obscure that, before taking them into consideration, it would be necessary to be assured, as we certainly are not at present, that they are founded on a large number of observations accurately made.

The cure of paralysis is conducted with interrupted currents, by applying the electrodes on the moistened skin of the extremities of the paralyzed members. According to the principles above established, if the paralysis is that of sensation, the direct current should be employed, while, on the other hand, the inverse current should be preferred for the paralysis of contraction. It is proper to commence with very weak currents, to suspend frequently the application of electricity, to make at first short applications and gradually to prolong their duration. All physicians who have conscientiously tried these electric cures acknowledge that recovery is not to be expected till after treatment of long duration, whence both physician and sufferer should arm themselves with patience, and not be discouraged by delay in realizing the effects of electricity.

LECTURE V.—Animal electricity.—Principal facts of Galvani and Nobili.—Methods of experimenting in electro-physiology.—Precautions for destroying secondary polarity.—Differential method by the confrontation of electro-motive powers.—Muscular batteries.—Fundamental facts of muscular electricity.—Entire muscles.—Divided muscles.—Laws of Matteucci and Du Bois Reymond.—Propositions on muscular electricity.—Muscular electro-motor, independent of the thickness of the muscle and the integrity of the nerve, proportional to the length of the muscle.—Relation of muscular irritability.—Distinction between the voltaic pile and the muscular electro-motor.—Electric current of the nerves.—Electro-tonic state and its explanation by the secondary electro-motive power.

We commence to-day the second part of this course—that is, the study of electricity developed within the living organism. In this investigation it will be shown that there are in nature certain animals provided with a particular organ by which electricity is constantly developed. We shall further see that in all animals there are certain tissues which are true electro-motors, and that the electricity developed in these stands in relation to the proper functions of those tissues. But, before proceeding with this subject, I should explain and describe the apparatus and the methods which are requisite to guide us to exact results in a field once so intricate and obscure. It is by virtue of these methods and apparatus that this part of electro-physiology forms at present a department of general physics, founded on exact experiments reproduced with facility, and in which our acquisitions are every day extended and verified.

Such as it exists to-day, this part of electro-physiology has derived its impulsion from two capital experiments which were made at two epochs very distant from one another, and which remained without development till 1840. The first of these experiments had its origin in the well-known discussion between Galvani and Volta, towards the close of the last century. Galvani, after having discovered that the prepared frog furnished a very delicate instrument for indicating by strong muscular contractions the passage of electricity, inferred from his experiments that electricity was developed within the animal, and that the nerves and muscles of a frog thus prepared were as the two armatures of a charged jar, discharged through the metallic arcs between which those animal parts were placed and in contact with them. Volta, after having invented the pile and conceived the hypothesis of the electro-motive force, rejected the idea of Galvani respecting animal electricity, and concluded that in all the experiments made upon frogs the electricity was developed by the external arcs, because they were heterogeneous, and that the animal did nothing but discharge it.

Galvani opposed to these conclusions an experiment which I have termed capital, because it was, in reality, the point of departure of electro-physiology; that is, a true phenomenon of electricity developed by the living organism. The experiment is this: The frog being prepared in the usual manner, a leg of the same is bent so as to bring it into contact with the lumbar nerves. At that instant the frog undergoes contraction, and this effect is repeated as long as the frog is excitable, and each time that the contact is renewed. If the muscles and the nerves continue to be kept in contact for a certain time, the contraction often occurs even at the moment in which the contact is interrupted.

Galvani demonstrated, as far as was then possible, by many sagacious and well-conducted experiments, which were subsequently varied and extended, by Humboldt and Aldini particularly, to other animals, that if the contact between muscles and nerves was effected by means of interposed bodies, contraction manifested itself only when those bodies were conductors of electricity, and that it no longer occurred if isolating bodies were employed. Thus contraction results at the opening as well as closing of the circuit, if, the galvanoscopic frog being held on an isolating surface, the extremities of the leg and of the nerve touch a stratum of water or any homogeneous and conducting surface. Humboldt used to place a portion of muscle between the nerve and the leg of the galvanoscopic frog, and in this manner also contractions were excited. Aldini observed the same fact discovered by Galvani in reference to the frog, by rapidly preparing in birds and rabbits the leg united with the sciatic nerve, and folding that nerve on the muscles. Aldini further varied this experiment by taking with the fingers the leg of a galvanoscopic frog, and bringing the nerve into contact with the brain or the muscles of other living animals. As is readily perceived, in thus operating he did but repeat the former experiments of Galvani and Humboldt, with the difference that the arc between the muscles and the nerves of the galvanoscopic frog was formed by the hand and by the body of the observer, by the earth and by the animals touched with the nerve; wherefore the contraction obtained in this manner could not be taken for a sign of electricity in the animals touched. It was for this reason that I said, in one of the preceding lectures, that it was proper, in using the galvanoscopic frog, to support it with an isolating handle, and to touch the electro-motor which we wish to study at two points of the single nervous fibre of the frog

All these various forms of the leading experiment of Galvani left no doubt in regard to the inference of the existence of a proper electricity of the muscles and nerves in a state of life, nor was this demonstration in any manner contradicted by the observation of Volta, viz: that the nerves and muscles represented the two metals of the pair of plates, since it always proved that the nerves and muscles act not as electro-motors, if not taken in an animal living or recently killed.

In 1827, about fifty years after Galvani, Nobili, who had then improved the galvanometer and rendered it very sensitive, made, with the frog prepared in the manner of Galvani, the second capital experiment to which I have alluded. Let us suppose we have the galvanometer with a long wire, and on the astatic system devised by Nobili, the extremities of which are laminæ of platina immersed in two goblets filled with salt water. Before commencing the experiment on the frog, Nobili takes a wick of cotton well steeped with the liquid and unites with this wick the liquor of the two goblets. If the two laminæ of platina are homogeneous, a thing very difficult to realize, the needle remains unmoved; usually, however, there is deviation which results from the imperfect homogeneity of the laminæ, and especially from their having the secondary polarity. The circuit being left closed, the deviation after some time amounts to nothing, or very little. A frog is then rapidly prepared, the wick is removed, and the frog is substituted by immersing the lumbar nerves in one vessel and the legs in the other. At that instant the frog contracts, and the needle deviates by an arc of twenty-five or thirty or more degrees, and settles at a much less angle, which continues eventually to decrease. If the experiment is repeated on another frog, inverting at the same time its position with respect to the extremities of the galvanometer, the deviation occurs in the opposite direction. Nobili also ascertained, by operating upon two or three frogs similarly prepared and united in form of a battery—that is, by placing in contact the muscles of one with the nerves of another, that the deviation of the needle increased in proportion to the number of the frogs. It results from these experiments that, by uniting through a homogeneous arc the nerves and muscles of a prepared frog, there is established in this arc a direct electrical current, so that the muscles represent the zinc, and the nerves the carbon or platina of a voltaic pair, by which the current is directed in the animal from the muscles to the nerves, or from the feet to the head of the frog, as it is customary to say. This current was, according to all analogy, the cause of the contraction observed in the experiment of Galvani.

Nobili did not sufficiently vary his experiments to be enabled to arrive at an exact interpretation of the fact which he had discovered, and, led by a false analogy, he overlooked the true origin of the electricity of which, however, he had rigorously demonstrated the existence in animals. He imagined that the nerves and the muscles, by their different structure and composition, were bodies which parted unequally with the water by evaporation, whence they must have, as he reasoned, since they are exposed to the air, a different temperature, from which he inferred that the current discovered in the frog was a thermo-electric current between the nerve and muscle, and, therefore, independent of the living organism. A slight consideration, however, of the conditions under which his experiment was made, will satisfy us how dissimilar and even opposite they are to the conditions in which thermo-electric currents are produced; and, moreover, we shall presently see that the true electro-motor in the experiments of Nobili, as in those of Galvani, (which do not differ except through the addition of the circuit of the galvanometer and the electricity of the frog being proved not only by the contraction, but also by the deviation of the magnetized needle,) is not formed by the union of the nerves and muscles, but by the various parts of the muscle alone.

I shall endeavor to show the laws of the muscular electro-motor by simple and exact experiments, and must, therefore, premise a description of the instruments and methods used in these experiments.

You are already acquainted with the preparation of the galvanoscopic frog and its use. The leg of a frog rapidly prepared, and with which a long nervous filament remains united, is placed upon a slip of gutta-percha; the nerve is to be slightly dried by placing it upon a sheet of felt paper, and then the piece of muscle of which we would ascertain the electrical state is touched with two points of this nerve. By repeating this experiment several times, changing the points at which the muscle is touched by the nerve, and observing the instant at which the contraction occurs, we may determine with some certainty the direction of the current. In such an experiment we may also use the whole frog divided in half, after the manner described in former lectures, and with this, if the current is sufficiently intense, it is still more easy to discover the direction of the current, because commonly one of the members, that, namely, which is traversed by the direct current, contracts when the circuit is closed, and the other at the opening.

The galvanometer of very fine and long wire, with a good magnetic system, is the essential instrument for the researches of electro-physiology. Since it is practicable to unite several muscular elements in series, forming of them one battery, a galvanometer of two or three thousand coils may suffice, though in more delicate experiments, such as those made on a single muscle and those pertaining to the variation of the muscular electro-motor power under contraction, and the electro-motor power of the nerves, it will be necessary to have recourse to a galvanometer of from twenty to thirty thousand coils.

In every experiment of animal electricity with the galvanometer there are encountered even to this day causes of error, introduced by the laminæ or strips with which the extremities of the galvanometer terminate. In effect the improvement of this instrument by Nobili led immediately to the multiplication of experiments in electro-physiology, but from want of proper attention to the errors introduced by the use of the laminæ just spoken of, the recurrence of erroneous results arrested the progress of this part of physics. The more delicate the galvanometer, so much the more necessary was it to resort to a method which should be guarded against the influence of the extraneous currents attributable to the extremities of the galvanometer and the chemical action of the liquids placed in contact with those extremities. It is not necessary that I should here speak of the experiments of electro-physiology made by using, as extremities of the galvanometer, laminæ of iron, silver, or copper, placed directly in contact with the muscles, the brain, or the spinal marrow of a living animal. Perhaps in some of these experiments the effects obtained by the galvanometer should be attributed to the electro-motor power of those parts, but most frequently the uncertainty of the direction and the variable intensity of the currents obtained under equal circumstances depend either on the secondary polarity or the heterogeneity of the laminæ, or on the different action of the animal liquids in contact with them. These uncertainties and irregularities are not to be effectually excluded, except when with the muscular batteries we have increased the intensity of the currents, or, better still, when we shall have succeeded in entirely eliminating the secondary polarity of the laminæ of the galvanometer.

It is easy to understand the method of the muscular batteries. For the present the muscular element which we are considering is the frog prepared after the method of Galvani, and used in the experiments of Nobili. We will suppose that we have a certain number of these frogs possessing about the same degree of vivacity and similarly prepared. We stretch them on an isolating surface, arranging them in battery—that is, placing the legs of one frog in contact with the legs of the succeeding frog. The extremities of the galvanometer used at first were two equal laminæ of platina soldered to copper wires; each of which laminæ has an isolating handle of ivory or gutta-percha, and is covered with a coating of sealing-wax, leaving exposed only a small and equally extended portion of their surfaces. The two laminæ must have been first cleansed with potassa, then plunged in a diluted acid, then several times in distilled water, and finally left immersed for a considerable time in this liquid, or in a solution of marine salt. In this way we succeed in rendering the laminæ homogeneous, and in divesting them of currents; if agitated too much, or unequally immersed in the liquid, currents again make their appearance.

The frogs being arranged, as already shown, in battery, their two extremities—that is, the nerves on the one hand and the legs on the other, are immersed in little cavities filled with distilled or salted water. Having succeeded in obtaining homogeneity in the platina laminæ of the galvanometer, we close the circuit of the muscular battery by immersing them in the extreme cavities of this battery. We have thus a current which, by employing eight or ten elements and a galvanometer of but two thousand coils, is sufficiently strong to make the needle deviate a full quadrant. It may be thus demonstrated that the deviation increases with the number of the elements, and that the direction of the current obtained by this battery is independent of the nature of the liquid in which are plunged the extremities of the battery, These results have, for the first time, placed beyond doubt, that the currents thus obtained do not depend on the heterogeneity of the laminæ of the galvanometer, or on the action of the animal liquids on those laminæ; in a word, the experiments with muscular batteries have furnished the most indubitable proof of the existence of the proper electricity of the muscles of animals living or recently killed.

Du-Bois Reymond, to whom we owe so many important experiments in electrophysiology, was the first to construct and to use a galvanometer of very fine and long wire which makes at least twenty thousand coils around the needles. He was thus enabled to study muscular electricity without resorting to batteries—that is, by operating on a single piece of muscle. The extremities of the galvanometer used by him were two laminæ of platina immersed in salted water. This liquid is contained in two small cups of glass, in each of which a considerable stratum of flannel or paper passes from and beyond the cup, and is bent horizontally to a short distance. These two appendages of paper or flannel, called cushions, (*cuscinetti,*) being imbibed with the liquid of the cups, serve to close the circuit when they are brought into contact, which is effected by their directly touching one another, or, still better, by placing upon them a third similarly imbibed with the same liquid. After it is ascertained that there is no current between the laminæ, the third cushion is removed and the prepared frog is substituted, which is done by placing it with the nerves on the cushion of one cup and the legs on the cushion of the other. We shall then see a very strong deviation in the needle, referrible to the usual current, which Nobili termed the current of the frog, directed in the animal from the feet towards the head.

Whichever of these methods of operating be selected, namely, the battery of muscles with the less delicate galvanometer, or a single muscle with the highly sensitive galvanometer, it will still be observed that the use of the laminæ of platina renders the experiment imperfect and sometimes erroneous. It will be found, in effect, especially in using a single frog and very sensitive galvanometer, that while the first deviation of impulsion is very great, the needle, after oscillating for a brief period, establishes the ultimate amount of deviation at a few degrees, and even then exhibits instability, promptly descending towards zero. We may, therefore, easily satisfy ourselves that these effects do not depend on the feebleness of the electricity of the frog, but on a physical phenomenon with which we are already acquainted—that is, on the development of secondary polarity in the laminæ of platina, by which there is created an electro-motive force contrary to that of the frog. In effect if we remove the frog and close the circuit by bringing the cushions into direct contact with one another, we immediately observe a deviation in a direction opposite to that of the frog. This secondary polarity, then, is an imperfection in these experiments, both because it rapidly weakens the animal currents, and because it creates heterogenity in the laminæ—effects which are not easily removed, and which may introduce errors into succeeding experiments. Fortunately we possess at present a method in which these imperfections are avoided. I have already shown how, in causing a current to pass by two electrodes of platina into a saline solution, these electrodes, when the current of the battery ceases, give to the galvanometer a strong secondary current in the opposite direction; I also remarked that this latter current was due to the products of electrization which collected on the electrodes of platina, and to their chemical action on recombining within the liquid. Observe now the experiment by which it is proved that a current may be transmitted in a liquid by means of metallic electrodes without generating secondary polarity. Instead of electrodes of platina I employ strips of zinc perfectly amalgamated and covered with mercury, immersed in a saturated solution of sulphate of neutral zinc. By presenting the experiment we easily obtain an explanation of the result. I cause the usual current to pass, and then close the circuit between these strips or laminæ alone; there is now no trace of a current. The sulphate of zinc is decomposed—that is, the oxygen and sulphuric acid go to the positive wire and form sulphate of zinc, which is immediately set free in the liquid, and keeps it saturated as at first; at the negative pole zinc is precipitated, which, finding an excess of mercury, becomes amalgamated, and leaves the wire in nearly the same state as before. There is, therefore, no alteration produced by the passage of the current either in the liquid or the electrodes, and this explains the absence of the secondary polarity.

Hence the practice is now generally adopted, in all electro-physiological experiments made with the galvanometer, of having two strips of amalgamated zinc, soldered to the extremities of the copper wire and immersed in a saturated solution of sulphate of neutral zinc which fills the two cups furnished with the cushions above described. I have recently introduced a useful modification, as follows: instead of strips of zinc I use an amalgam of zinc which half fills a glass tube bent in the shape of U, but of very different diameter in the two branches. Into the narrow tube filled with the amalgam I introduce the copper wires of the galvanometer; on the amalgam which is in the wider branch I pour a saturated solution of sulphate of zinc, so as to fill this branch almost entirely; each of these branches has a wide rostrum or beak, flattened like a duck's bill, in which the liquid spreads itself in a thin stratum and fulfils the office of the cushions. With this modification the metallic appendages continued to be homogeneous without renewing the amalgamation, and we avoid the soiling of the cushion with the liquids which moisten the pieces of muscle, an inconvenience which obliges us to wash frequently the actual cushions in the solution of the sulphate. An experiment will evince that by these means we avoid the secondary polarity. I place any small body, a piece of glass for instance, against the needle of the galvanometer to prevent any deviation occasioned by the current of a frog or battery of frogs. This obstacle does not hinder the needle from deviating in the opposite direction—in that, namely, in which it would be made to deviate by the secondary polarity on closing the circuit between the two cups alone, immediately after the removal of the frogs. I cause, in effect, the current of a battery of six or eight frogs to pass and notice what occurs, on seeing the needle approach and press against the obstacle. After some seconds I remove the muscular battery and immediately afterwards bring the cups into contact, and I obtain no sign of deviation. I conclude, then, from this experiment that the method I adopt renders these experiments of electro-physiology independent of the secondary polarity. Moreover, I propose to show that by causing the current of a frog to pass with these electrodes of zinc, the needle is promptly fixed, and indicates a deviation much greater than that which would be obtained by using electrodes of platina in water either distilled or slightly saline.

In terminating this exposition of the experimental method, I will state finally, that, for a comparison of the electro-motor power of two pieces of muscle or other animal tissue, I have always used with invariable success the principle of opposition which I have already brought to your notice. For this purpose the two electro-motors are placed on the customary surface of gutta-percha in communication with one another and in such a way that their currents are in an opposite direction to one another. On now touching the extremities of this double battery with the two cushions of the galvanometer, we shall have, if there be a difference of electro-motor power, a differential current in the direction of the stronger electro-motor, which will be independent of the internal resistance. Would we next measure in some manner the electro-motor power of the different muscular elements, we use, as was first done by Giulio Regnault, an *electro-motive unit,* which may be a thermo-electric pair, bismuth and copper, in which the union is maintained at a constant difference of temperature, being the one at 0° and the other at 100°. The electro-motor power of a muscle or other animal tissue may be expressed by the number of the electro-motive units required in order that there shall be no differential current.

Having detained you so long in describing how exact experiments on the muscular electro-motors are conducted, it will be more easy to explain with brevity and distinctness the laws of these electro-motors.

Any muscle selected in an animal alive or recently killed, may be submitted to experiment either by taking the animal entire and without alteration of the organic integrity, or after having had its fibres transversely divided. Let us begin with the muscles entire; and for these experiments the frog, whether from its structure or its tenacity of life, is better suited than any other animal. Hence it is on the muscles of the frog that we shall operate, regard being had, however, to the muscles of other animals in demonstrating the fundamental facts of muscular electricity. Whatever may be the entire muscle selected, it will be always terminated at its extremities by the tendinous appendages, which anatomists consider as a continuation of the same fibres and which therefore rest on the bases of the muscular fibres. A muscular mass would hence be a fasciculus of cylindrical fibres on whose bases or tranverse sections the tendinous fibres are established and form a continuation of them.

I will not stop to describe all the experiments which have been deemed necessary suitably to analyze the fact discovered by Nobili, and to prove that the animal electro-motor does not consist in the assemblage of muscles and of nerves, as has been supposed, nor in the whole mass of muscles of the entire animal, but that, on the other hand, each of the muscles is a distinct electro-motor, whence, by putting an entire frog or other animal in a circuit, all these muscles are made to act at once. The current which is obtained from the entire frog depends, therefore, on the intensity and direction of the various currents with which it is charged.

In order to make a simple experiment we detach from the entire frog a muscle as entire and intact as possible, such as the gastrocnemius or rather some small muscles of the upper members. With these muscles a battery is readily constructed by disposing them in series, and then, even with a galvanometer not very delicate, we have the indications of a current which increases with the number of the elements and which is directed like that of the entire frog. In using a battery of gastrocnemian or other muscles it will be seen that the current has always the same direction, either on directly touching with the laminæ of platina the extremities of the muscular battery or interposing wet layers of paper or flannel imbibed with different liquids. If, instead of using gastrocnemians alone, placed in contact with one another, we employ gastrocnemians to which the nervous filament is united we equally obtain indication of the gastrocnemian current, but they will be much more weak from the great resistance introduced into the circuit by the nerve. By making the experiment with greater attention, and especially by selecting muscles whose tendinous extremities are as equal as possible, we shall succeed finally in ascertaining what is the most simple form of the muscular electro-motor, and what the law of that electro-motor.

Suppose that we operate with the very delicate galvanometer, whose extremities are those above described—that is, laminæ of amalgamated zinc immersed in the solution of the sulphate of zinc—and that we use the rostra described, either with a simple beak or with cushions of flannel. We place the muscle, which ought to be the *great adductor muscle,* because the experiment succeeds better, upon gutta-percha, and bring it with its tendinous extremities into contact with the cushions. It rarely occurs that there is no sign of a current, but it is certain that this is much weaker than that which arises from touching first one and then the other tendinous extremity with one of the cushions, and the surface or median zone of the muscle with the other cushion. The same result is realized by touching two symmetrical points of the muscle equally distant from the extremities. This fact is constant and general—that is to say, it is verified in all entire muscles, whatever be the animal operated upon, so that the following proposition may be considered as perfectly established by experiment: "Whatever be the entire muscle operated upon, if this muscle is alive or belongs to an animal recently killed, a current is obtained in a homogeneous arc which touches with its extremities one of the tendinous extremities of the muscle and the surface of the muscle; this current is constantly directed in the external arc from the surface of the muscle to the tendon, and its intensity diminishes in proportion as both the extremities of the arc separate from one another by approaching the extremities of the muscle." This proposition, suitably applied, comprises all the cases of currents obtained in entire muscles, and is confirmed by operating on the muscles of frogs, insects, and animals of warm blood, but with difference of intensity and lapse of time after death, depending upon laws which will be considered in the sequel.

It has already been said that the current obtained by Nobili, in operating on the entire frog, was in some sense the algebraic expression of all the currents of the muscular fasciculi, which compose the animal, and which are included in the circuit at the same time. It will suffice to form a double battery, by opposing a single gastrocnemian to an entire half of the frog, in order to obtain in the galvanometer a strong differential current in the direction of the single gastrocnemian. I will merely add that, with a battery formed of ten or twelve gastrocnemians, I have been enabled to obtain a current so intense as to discharge the condenser and to give indications of electro-chemical decomposition.

The current with which we have been till now occupied, and which is found in entire muscles, is evidently the cause of the contraction which occurs in the experiment of Galvani on touching the muscles of the leg with the lumbar nerves. This current, proceeding from the feet to the head in the frog, is *inverse* as regards the lumbar nerves—that is, in the direction in which we have seen the contraction prevail at the opening of the circuit. We have observed, on the other hand, in repeating the experiment of Galvani, that the contraction is more frequently obtained when the nerves and muscles touch each other, and not in the act by which that contact ceases. To explain this result, which would seem contrary to the law which has been stated respecting the physiological action of the current according to its direction, we must observe that in making the experiment of Galvani it may not happen that the circuit is kept closed for a certain time, while this condition is indispensable for obtaining the contraction at the opening, when the nerve is traversed by the inverse current. And, in fact, if the experiment of Galvani be repeated by prolonging the contact, we then generally succeed in obtaining the contraction when the current ceases. Professor Cima has pointed out a certain method of obtaining, with the current of the frog, contraction at the opening of the circuit. He takes the galvanoscopic frog, rests it on an isolating support, and then immerses in water at the same time the fore paw and the end of the nervous fibre which hangs below; then, by keeping the circuit closed each time for several seconds, we obtain the contraction, first at the closing and opening of the circuit, and afterwards only at the opening.

Let us pass now to the electric current obtained in muscles in which the fibres are divided. The first experiment of this kind which seems to have been tried is that which I now exhibit, and which is performed by using the galvanoscopic frog rapidly prepared, and supported on an isolating handle of gutta-percha. I take another frog, a living fish, bird, or any animal whatever, and make an incision in a muscular mass; I then cause the nerve of the galvanoscopic frog to touch with two different points the interior of the incision and the surface of the muscle. In this way we never fail of obtaining a contraction in the frog, and, by making the experiment with due care, we may even succeed in determining in the usual manner the direction of the current, that is, the position of the poles of the muscular electro-motor.

If the not very delicate galvanometer be used, the effects of the muscular current obtained from divided muscles are augmented by forming the usual batteries. The muscular elements, which are most readily supplied, are the half thighs of frogs. With this view a certain number of frogs are prepared *a la Galvani*, the legs of which are cut at the articulations; then, by dividing transversely at the middle of the thigh, we obtain, especially with the inferior half, a perfect element. Muscular elements with divided fibres are also procured by removing the skin of an eel, and cutting it transversely into so many pieces; and in like manner from the legs of birds and mammifers stript of the skin. The heart of a bird or fish cut in half likewise furnishes a muscular element, and the same may be said of slices of the pectoral muscles. Whatever be the muscular element thus obtained, it is readily understood how a muscular battery may be formed of a certain number of these elements: it suffices to arrange all of them in the same direction, that is, by causing the interior or section of an element to touch the surface of the succeeding element. Similar batteries have been constructed even on the still living muscles; for this purpose the frogs were fixed upon a table, their lower members denuded of skin, and a thigh of each was cut in the middle. The battery was completed by establishing the contact between the section created by cutting in one frog and the leg of the succeeding frog. A similar experiment has been repeated with birds. Whichever of these batteries may be adopted, there occur at the galvanometer currents which increase with the number of elements, and with which we may obtain the discharge of the condenser and the signs of electro-chemical action on the ioduret of potassa. Even with a single piece of divided muscle and the very delicate galvanometer, we observe the indications of the same muscular current, which is constantly directed in the muscle from the internal or transverse section to the surface, and thence again to the transverse section in the circuit of the galvanometer.

As, while studying the current in entire muscles, we saw it always become weaker, and finally even null, when the tendinous extremities of the muscle, or two symmetrical points of it nearest to those extremities, were touched; so it will be seen even more generally that the muscular current is annulled in a muscle having two transverse sections equidistant from its middle, on applying the extremities of the galvanometer at the centre of those sections. The experiment may be made by cutting transversely, at two points equidistant from its middle, the thigh of a frog. By employing the delicate galvanometer, and using for its extremities cushions reduced to a point, we are enabled to operate on small pieces of muscular fibre, and to obtain in these that which is obtained with a muscular battery. We are led from the above considerations to generalize the experiments cited, and to conclude that every element of muscular fibre of an animal living or recently killed is an electro-motor.

Du-Bois Reymond regards the electro-motor power of entire and of divided muscles under a single point of view, and as having the same origin. In accordance with modern anatomical observations, it is now admitted that the extremities of the muscular fibres, the bases of those cylindrical fibres which compose the muscles, are in immediate communication with the tendinous fibres. For this reason the tendons may be considered in the muscular electro-motor as a natural transverse section. Hence Du-Bois Reymond calls the interior or the incision of a muscle the *artificial transverse section*, and the tendon the *natural transverse section*. To complete this definition, the same physiologist calls the surface of the muscle the *natural longitudinal section*, and maintains that we can also obtain, what is sufficiently difficult in practice, an *artificial longitudinal section*. These definitions being premised, every muscular electro-motor is embraced under this general formula: *any point of the longitudinal section of a muscle, whether natural or artificial, is positive with respect to every point of the transverse section, natural or artificial.*

It might be proper to proceed now to a consideration of the muscular electromotor in its analogy with the different electro-motors which we possess in physics, and endeavor to throw some light on the mode of action of this electromotor. But first, and in the interest indeed of this theoretical research, I must state the laws of muscular electricity, that is, its relations with the physical and physiological properties of the muscle.

The first proposition relative to the physical condition of the muscle is the following: *The electro-motor power of the muscle is independent of the size of the muscle itself, or of the magnitude of its transverse section, and is, on the other hand, proportional to the length of the muscle.*

This proposition, as well as those which follow, is founded on experiments very aptly performed upon the principle of opposition before mentioned. Thus, for the first part of the proposition, it is sufficient to prepare a certain number of half thighs of frogs, and to construct a double battery formed on one part of a single half thigh, and on the other, of five or six elements, which are all superposed in the same direction. From this double battery there is no differential current, or one very slight, and in the direction indifferently of a single element or of the combined elements. The same result is verified by collating the muscular elements of very different size with one another, unless it be found that in this case the differential current is almost always in the direction of the thicker muscle, the reason of which will be presently shown.

We proceed in like manner to show that the muscular current is proportional to the length of the muscle. A double and opposed battery formed of two half thighs being prepared, after having ascertained the equality of these two elements, we execute with each of them a new transverse division parallel to the first; with this difference, that in one of the elements the operation is restricted to the removal of a very thin stratum, while in the other it should reduce the element to not more than a third or fourth of the previous length. The battery is then reconstructed, and we have now a differential current in the direction of the longer muscle. This result has also been realized with the gastrocnemian and the long dorsal muscles of the rabbit. Hence we derive an explanation of the observation made by Du-Bois Reymond regarding the current obtained by touching two non-symmetrical points of the same transverse section. This current indicates that the points of this section nearest to the centre act as the transverse section, and stand in relation to points further from the centre as if these last pertained to the surface of the muscle. The fibres of the centre are, in effect, longer than the fibres situated near the edge of the transverse section, and the resulting current is in conformity with the proposition above stated.

Let us pass now to the physiological laws of the muscular current, and here also, for sake of brevity, I shall state such well-established facts as we possess in the form of propositions.

First proposition.—"The electro-motive force of the muscle in an animal living or recently dead is greater in the mammifers and in birds than in fishes and reptiles. This force rapidly diminishes after death, but in an inverse order as regards the animal series, and still with great differences. The muscular electricity varies, therefore, as the so-called muscular irritability."

As this irritability is extinguished very rapidly in mammifers and birds, particularly in a detached muscle of the animal, it is necessary to operate with great promptness in order to demonstrate the first part of this proposition. Having prepared, therefore, the half thigh of a frog, we rapidly detach a thigh from a living bird, cut the thigh in half, remove the skin, and promptly form the double battery with the opposed elements of the frog and the bird. In the greater number of cases the differential current is in the direction of the bird. If, after some moments, the battery be reconstructed, we shall see the differential current arise in the opposite direction, that is, in favor of the muscle of the frog. Giulio Regnault, in measuring with thermo-electric units the electro-motor powers of the muscle of the rabbit and of the frog, found that, in the first moments after death, the muscle of the rabbit predominated. I have myself made these comparisons by operating upon living animals, and with results still more decisive.

Second proposition.—"The nerve does not directly exercise any influence upon the electro-motor power of the muscle."

We have before seen that by leaving to the muscular elements the nervous filament, and introducing it into the circuit, we only produce a weakening of the muscular current. When the opposite battery is formed by two muscular elements, one without the nerve and the other with the nerve placed in circuit, since by this arrangement the internal resistance is excluded, there is no differential current—a fact which indicates that the electro-motive force of the muscle is independent of the presence of the nerve. We may also experiment on pieces of muscle in which have been left, now the nerve which ramifies therein, and now the nerve which issues from it. In both these cases it will be found that the nerve acts only as would a piece of wet thread, which is a bad conductor, in contact with a certain part of the muscle. Under the above proposition might be comprised the results obtained by a comparative study of healthy muscles and those affected with narcotics. The general result, is that the electro-motor power of the muscles of these animals, if they died after having undergone contractions more or less violent and prolonged, is diminished. The same occurs in the muscles of animals killed with the poison of *curare.* We shall see in the sequel, in speaking of the electrical phenomena of muscular contraction, how we have succeeded in interpreting the diminution of the electro motor power of the muscles in which, by the action of the poison, contractions have been excited.

Third proposition.—"All the physico-chemical actions which modify the muscular irritability act also on the electro-motor power of the muscles."

I shall content myself with adducing two or three precise experiments which, in different cases, verify this proposition. We have here two vessels containing frogs; in one are some taken but a few hours from the fens where they naturally live; in the other, some taken several weeks ago, and consequently much enfeebled. Whether we operate on the gastrocnemians or on the half thighs, there will constantly be realized, with the usual method of the double battery, a differential current which will evince that the electro-motor power of the muscles of the fresh frogs is greater than that of the muscles of the frogs which I denote as enfeebled. The effect of the enfeeblement is always more manifest in the gastrocnemian muscles than in the half thighs.

The influence of refrigeration on the muscles may be shown in a very distinct manner. I prepare a certain number of gastrocnemian muscles taken from different frogs, and introduce a portion of them into a glass tube, which I immerse in a frigorific mixture of ice and salt. After two or three minutes I form the usual double battery with a gastrocnemius which has not been in the mixture, and with one which has undergone refrigeration. There will constantly be observed a great diminution in the electro-motor power as the effect of the reduction of temperature. By prolonging the refrigeration for fifteen or twenty minutes, the muscles lose this property entirely, while, if the process of cooling has been of brief duration, they reacquire, on regaining the ordinary temperature, a portion of the electro-motive force. An effect analogous to that of a reduction of temperature is produced by its elevation. The fact may thus, at least in part, be explained, why it is that frogs, as well in the depth of winter as in the hottest weather, yield with their muscles much weaker electric currents than in the intermediate seasons.

I have sought to ascertain whether muscles, entire or divided, left for a certain time in hydrogen, oxygen, or carbonic acid gas, or within the vacuum of the air pump, underwent variations in their electro-motor power. I have discerned no difference, for even the effect produced by operating with hydrogen gas was found to result from the action of the gas on the secondary polarity of the platina extremities of the galvanometer. It is possible that the action of these gases on the nutrition and on the muscular respiration may eventually have a certain influence on the electro-motor power of the muscles; but this effect will be slow in manifesting itself. The action of nitrous gas and of sulphuretted hydrogen is, on the other hand, distinct. On causing frogs and even animals of warm blood to die by inhaling these gases, it is found that the electro-motor power of their muscles is enfeebled. The same may be said of frogs which have died after many hours, confinement in water boiled and deprived of air.

It has finally been inquired what effect ensued from the immersion of muscles in different liquids upon their muscular electro-motive power. The first effect realized from such immersion, if very brief, in saline water and in a solution of sulphate of soda or of magnesia, is the augmentation of the power in question. On using in effect the differential method, and comparing a muscle which has not been, with another which has been, immersed in these liquids, if a differential current has been observed, as is in fact often the case, this current has been in the direction of the muscles which had undergone immersion. It is impossible to explain this effect solely by the increase of conductibility due to the liquids; we are obliged to recur to the augmented electro-motive force. It is to be noticed also that these liquids maintain the red color of the globules of the blood, and must therefore increase the chemical action of nutrition. From prolonging for a greater time the immersion of a muscle in any liquid, the constant and general effect is the weakening, and finally the disappearance of the electro-motive power much sooner than would be the case if the muscles were left in air.

After having stated the laws of muscular electricity, it would remain to propound the theory of this animal electro-motor. What is its form? What its manner of acting? With which of the electro-motors known in physics has the living muscle an analogy? We are forced to confess that, notwithstanding the many investigations which have been devoted to muscular electricity, we are yet unable to give any satisfactory reply to these questions We know that a piece of muscular elementary fibre, however small, if taken from an animal living or recently dead, is an electro-motor, of which the current has a determinate direction in the muscular fibre under examination. We know that this electro-motor power increases with the length of the fibre, and with the number of the muscular elements organically combined. We know, finally, that this same power varies with the muscular irritability. We are thus led to conclude that the electro-motive power of the muscles has its origin in the chemical actions of the muscular respiration. But all this conveys no information respecting the form of the muscular electro-motor, nor whether the currents which we detect with the galvanometer are derived currents—that is, whether the muscular electro-motors being always in action, their currents are discharged through the liquids or the tissues which envelop the muscle. Certain it is that the currents obtained in the frog which still retains the skin are weaker than those which occur at the same points in the same frog when the skin is removed, and this is also the case on covering a muscle with any liquid stratum whatever.

We should not forget that there is in the muscular electro-motor a fundamental character which distinguishes it from the common electro-motors. The extremities of an entire muscle have the same electrical state, as two artificial transverse sections also have the same state, which can never be the case either in a battery, an electro-dynamic spiral, or in a Leyden jar. We cannot, in the actual state of the science, form any precise idea of this difference, nor is it more easy to understand why the half thigh, which is a muscle terminated by two transverse sections, one natural and the other artificial, which should have, it would seem, the same electrical state, is, notwithstanding, a powerful electro-motor. Nor do we better understand why, with the enfeeblement of the vital force, the half thigh loses so much more than the gastrocnemian. I repeat, that as long as the form of the electro-motor is not known to us we shall not succeed in clearing up these peculiarities which have at present the appearance of anomalies.

Du Bois Reymond has imagined a scheme or figure for the electro-motor element, comparing it to a cylinder of zinc whose bases are of copper. A series of these cylinders immersed in an acid liquid would produce, by dipping the extremities of the galvanometer in different points of the liquid, direct currents such as are obtained from different points of the muscle; but this analogy is wholly imaginary. The explanation of muscular electricity proposed some years since by Liebig, who surmised that there were in the muscles acid liquids and alkaline liquids which react, has not borne the test of experiment.

I will not conclude this lecture without very briefly noticing the results obtained in seeking whether other organic tissues have the electric properties of the muscles. By introducing the platina extremities of a good galvanometer into the fruit, leaves, or stalk of a plant, electric currents are obtained. Admitting that these currents are constant and independent of the polarity and heterogeneity of those extremities, the most probable explanation of the currents in question points at present to liquids of a different chemical nature which, with great probability, exist in the various parts of the plant and which react on one another. Investigations of this kind have been directed to the tendons, the tissues of the lungs and liver and kidneys, of animals living or recently killed, and careful experiment has shown that these tissues have no proper electro-motor power. This power, however, exists in the nerves, and since it is much weaker there than in the muscles, (perhaps about one-eighth or one-tenth,) Du Bois Reymond, who discovered it, was obliged, in order to succeed, to employ an extremely delicate galvanometer. The direction of the electric current in the nerves is the same as in the muscles, and is therefore obtained by establishing a homogeneous arc between the artificial transverse section of the nerve and its surface. A current in the galvanometer is thus obtained, directed in the nerve from the section to the surface.

In studying the electro-motor power of the nerves Du Bois Reymond has made an experiment which I must by no means omit. In a fowl or rabbit he takes a long nervous filament, such as a portion of the sciatic about eighty millimetres long. This nerve is placed on the two cushions of the galvanometer in such manner that a long piece of the nerve shall remain hanging down beyond one or both cushions. As the two cushions of the galvanometer touch two points of the surface of the nerve, there cannot be, and, in fact, there is no current. This being done, let the two electrodes of any battery be applied to the part of the nerve which hangs beyond the cushion, and in this part let a continuous current be made to pass. There now occurs a strong deviation in the galvanometer, which indicates that a constant current is circulating therein, a current which is directed in the interval of the nerve between the two cushions of the galvanometer, like the external current of the battery. If the nerve were still longer, so as to have a piece pendent beyond the other cushion, and if in this pendulous part the same current were made to pass, we should still obtain with the galvanometer the current always directed in the nerve like the current of the battery. Du Bois Reymond has given the name of *electro-tonic* to that state of the nerve into which the whole nerve is thrown by a current which traverses it, even in portions which are not permeated by the current. It is this supposed electro-tonic state of the nerve which occasions the electric current I have described, or, to speak more correctly, the electro-tonic state has been imagined in order to deduce from it the existence of that current.

I have latterly studied these phenomena with attention, and having found that they are produced more readily and endure much longer in the large nerves of mammifers than in those of reptiles, that they are equally obtained in nerves which have for several hours lost their excitability as in those just detached from the living animal, I have had no hesitation in concluding that the electro-tonic state is an effect independent of the electro-motor power, and in general of the life of the nerve. I spoke at length, in one of the former lectures, of the secondary electro-motor power discovered in the nerves; a phenomenon which, as I stated, the nerve presents in common with all porous solid bodies imbibed with a conducting liquid, and which is owing to the electric currents excited between the products of electrization deposited on the nerve in contact with the electrodes of the battery. I showed that in the spaces of a nerve beyond the electrodes—that is, in the portions which we will call neutral, from their not having been traversed by the current, there are generated nevertheless currents directed exactly as is the current of the battery between the electrodes. The cause of these secondary currents is also known. In contact with the positive electrode acids are separated, oxides in contact with the negative. Now, as between water and an acid liquid a current is produced, directed in the liquid from the water to the acid, as likewise a current is produced between water and an alkaline liquid directed in the liquid from the alkaline liquid to the water, there is little difficulty in understanding how, in the portions of the nerve beyond the electrodes, being those in which the electro-tonic state is developed, secondary currents should be generated directed exactly as those which characterize that state—that is, as the current of the battery. I will further recall that these secondary currents are generated after an instantaneous passage of the battery, whence there is no wonder if, with a very delicate galvanometer, the effects of the electro-tonic state should be manifested when the current barely begins to pass.

I will mention in conclusion that there is in the frog, as if that animal were destined by nature to a revelation of all the secrets of animal electricity, another tissue endowed with strong electro-motor power, namely, the skin. Let us take the skin of a frog, and, after cutting a long strip, form of this a roll or cylinder, and let us cut this roll transversely at its two extremities; if the cushions of the galvanometer be now brought into contact, one with the transverse section, the other with the natural surface of the skin, we shall obtain, as Budge has observed, a current directed in the galvanometer from the transverse section to the surface of the skin. It is scarcely probable, however, that this current is really a fact of animal electricity, and even its direction, in some sort contrary to that of the muscular current, strengthens this suspicion. Perhaps to this electro-motor power of the skin, whatever it may be, is due the opinion once entertained, that the contraction obtained by Galvani in folding the leg on the nerve succeeded better with the leg covered with skin that when denuded of it.

LECTURE VI.—Electrical fishes.—General phenomena of the discharge of electrical fishes.—Chemical action.—Spark.—Deviation of the needle of the galvanometer.—Direction of the discharge in various fishes.—Structure and chemical composition of the organ.—Fourth or electrical lobe.—Elementary electro-motor organ and laws of the electrical function.—Organ an electro-motor constantly charged.—Electrical phenomena of muscular contraction.—Inductive contraction.—Proofs that it is owing to a current or electric discharge which arises in the act of contraction.—Experiments which prove that the contraction consumes the electro-motor power of the muscles.—Comparison between the muscular electro-motor and the electric organ.

Arrived at the last lecture of this course, it gives me pleasure to be able to illustrate in a living torpedo one of the most singular and most instructive cases pertaining to the subject under consideration.

The fishes endowed with the electrical function are, according to the naturalists, five or six in number; but as it is our present object merely to give, as far as possible, a theory of the electrical function, we shall confine ourselves to the electrical species best known, and which have been the subject of extensive researches. These are the silurus of the Nile, the gymnotus of South America, and the torpedo, which is frequently met with in the ocean and in our own seas.

From the times of Aristotle and Pliny notice has been taken of the property possessed by the living torpedo of communicating a shock to those by whom it is touched, and thus benumbing the member which comes into contact with it; hence the names of *torpedo*, *tremolo*, *mago*, given to these fish in different countries. Our own Redi was the first to discover that what he called the *stupefying* or *dolorific* property of the torpedo has its seat in two special organs to which he gave the name of *falcated organs*, and which are now termed *electric* organs. In the torpedo, stripped of its skin, we find that on each side of the head there exist two flattened elliptical masses which are connected with the animal by integuments and ligaments, and by some large nerves which are ramified in the electric organ. After Redi the most complete study of the electric function of the torpedo and gymnotus is due to Walsh, who, in 1773, published in the *Philosophical Transactions* a series of experiments which, as far as was then practicable, demonstrated, through the employment of arcs now isolating and now conducting, that these fish owe the property in question to an organ which suddenly develops a great quantity of electricity, and that the two opposite faces of this organ are the two poles, the two extremities of this animal battery.

These notices being premised, I shall describe the electrical properties of the torpedo with the simplicity and in the order with which we should study any physical apparatus. To receive the electric shock of the fish it is necessary, if the animal is in water, to touch, or rather press, with the hands the electric organs. If it be the torpedo, one hand is applied on the back and the other on the belly; if the gymnotus, it is necessary to grasp with one hand the head, and with the other the tail. While these fishes are yet vivacious, having been just caught, the shocks are very strong, and out of the water even stronger than in it, because, as is readily understood, while the animal is still immersed a great part of the electricity passes into the liquid and not into the body of the experimenter. When just removed from their element, these fishes, perhaps through irritation or uneasiness, give rapidly a succession of shocks which, when the animal is vigorous, do not differ from those of a good electro-magnetic machine. Out of the water, however, the fish soon becomes wearied, and yields weaker and more intermittent shocks; naturally, because it is enfeebled, but in part, perhaps, because of an instinct which warns it that in the air the shocks may be partially expended on itself. It often occurs that, in irritating a torpedo out of water with arcs formed of isolating material, the animal gives no shocks, or but few, which is not the case when conducting arcs are used. The electrical function of these fishes is for them, evidently, a weapon of offence or defence; if irritated, especially in the neighborhood of the organs, they give a shock, as they do also in order to benumb the small fishes which serve them for food. The well-known description which Humboldt gives us of the fishery of the gymnotus in the lakes of Brazil, proves the vigor with which these fishes exert their electric faculty in combating their enemies. The natives use horses in this pursuit, which, being driven into the lakes, often fall victims to the electric discharges of the gymnoti. In the mean time many of the latter, exhausted in the contest, approach the shore half alive, and are drawn out with hooks.

When the gymnotus avails itself of the electric organ to kill the fishes it would devour, it goes to work like a physicist: as the poles of its organ are at the extremities of its body, it bends itself into a bow and seeks to bring those extremities into the closest proximity with the victim. The torpedo, being flattened in form and having the poles of its organ in contact with the belly and spine, cannot use this artifice; but it employs another: lying generally in the sand and covered with a thin stratum of it, when the small fishes pass over it unawares it launches its discharge upon them.

In order to study the discharge of the torpedo, and observe those points of its body in which the electric organ resides, it must be withdrawn from the water and wiped dry; prepared frogs should then be distributed on its surface. We shall see the frogs, especially if the fish be irritated, fall into contractions; and if the torpedo be allowed to weary itself, we shall observe that the contractions continue to retreat towards those parts of the body which correspond to the electric organs. As long as it continues alive it responds, if sharply irritated in any part of its body, with an electric discharge, and an experiment may be made to show that the electric function is put in play at the will of the animal. In effect, when the torpedo is irritated at any point of the tail or spine the discharge occurs, provided the point irritated and the brain are connected by means of the nerves of the spinal medulla. But if this medulla be divided, it will be seen that the discharge is no longer provoked by irritation applied below the point of division, though it still results from irritation above it.

The early observer, Walsh, and also Gay Lussac, thought that the shock might be elicited without forming an arc, that is, without touching at the same time the belly and back of the torpedo. If the fish be not well isolated from the ground, it is conceded that the shock will be experienced on touching one face only of its body, because the arc is in that case established by the ground and the entire person of the observer; and hence, when fishermen perceive that there are torpedoes among the fishes drawn in nets from the water, they are accustomed to throw buckets of water on them, in order thus to exhaust the shock. It is easy, however, to show by decisive experiment the necessity of forming the arc if we would have the shock. Let a living torpedo be wiped dry and placed upon a tablet well isolated, with feet of gutta-percha. After several galvanoscopic frogs have been spread over its body, let the organ be touched with the end of the nerve of a galvanoscopic frog supported by the usual isolating handle. At every discharge, whether spontaneous or provoked, given by the torpedo, all the frogs undergo contraction except that supported on the isolating handle. It is unnecessary to add, that if the tablet on which the torpedo is placed were perfectly isolated, a condition difficult to realize, it would not be necessary to support the galvanoscopic frog with such a handle. But it must be further observed that even the isolated galvanoscopic frog will contract when the torpedo gives the shock, if a portion of the nerve sufficiently long be stretched upon the organ. We shall presently see the explanation of this fact.

After what has been said, there will be no difficulty in comprehending how all the effects of the instantaneous electric discharge should be obtained from that of the torpedo, namely, the spark, the deviation of the galvanometer, the magnetization, the heating of the platina wire, the chemical effects. All these phenomena, which I made the subject of attentive study in 1837, were at that time an occasion of surprise; for it could not but be deemed strange that electrical effects so distinct should be realized from a fish, and these discoveries offered, moreover, a new field for the study of animal electricity. Nor should I omit to mention, as something surprising in its way, that only a few years earlier experimenters of much ability, and even so illustrious a cultivator of our science as Sir Humphrey Davy, had published that the discharge of the torpedo did not produce a deviation in the needle of the galvanometer. In order to obtain with facility the electrical effects in question, I use a single and very simple apparatus, which consists of two circular plates of copper, of which the lower has three feet of gutta-percha, that it may be isolated from the ground, and the upper one is provided with an isolating handle. To each of these plates is soldered or screwed a long copper wire. I take a living torpedo, dry it properly, place it on the lower plate, and cover it with the upper one. It is sufficient to press the fish lightly with the upper plate in order to have the discharge, and thence to obtain the electric effects in the circuit of the copper wire.

If it is proposed to produce the heating of the platina wire, I take a piece as fine as possible, and form of it a small spiral; with this I connect the two extremities of the copper wire united to the plates. On provoking the torpedo to give several discharges in succession, the platina wire will be found to have become perceptibly heated. Magnetization is obtained by closing the circuit with the usual small spiral of copper wire, in which is placed a needle of steel; after the discharge the needle is found to be magnetized, and the position of the poles indicates the direction of the discharge. To obtain the indications of electro-chemical action, we unite to the extremity of each of the copper wires two pieces of platina wire, and these are placed with their extremities on paper prepared with a mixture of starch and ioduret of potassa. Each time that the torpedo gives the discharge there is a blue spot formed under the extremity of the platina wire which is united to the plate resting in contact with the back of the animal.

It is rather more difficult to obtain the spark of the discharge, and the reason is clear, because it is necessary that at the moment the animal yields the discharge there should be between the extremities of the circuit not a perfect communication, but a slight interval of air, in which the spark is transmitted. Among different means which I have devised for the purpose, that which succeeds best is to connect one of the usual copper wires with a large iron file, and while the fish is lightly pressed with the upper plate with a view to induce the discharge, to pass the other copper wire over the file. By operating thus in the dark, the spark is not long in making its appearance on the file. It was in this way that I obtained the phenomenon in experimenting at Naples, on a living gymnotus, during the scientific congress of 1845.

In order to ascertain the effect of the discharge on the galvanometer, we should have two laminæ of platina united to the wires of the instrument, and touch with these laminæ the two faces of the organ of the torpedo on which are spread galvanoscopic frogs. Whenever the fish yields the discharge, the frogs contract and the galvanometer shows a prompt deviation. Any galvanometer of 500 or 600 coils, with a system judiciously astatic, suffices for the experiment. The constant result is, that in the circuit of the galvanometer a current passes directed from the back to the belly of the fish. It is worth noticing that on introducing into the circuit of the galvanometer a liquid stratum somewhat long and divided by diaphragms of platina, if the fish be quite vivacious, the current indicated by the galvanometer undergoes no variation from the resistance introduced into the circuit.

If the operation be conducted after the surface of the fish is wiped dry, and when some little enfeeblement has taken place, it will be soon apparent that the discharge is stronger at those points of the organ which are thickest, that is, in proximity with the median line of the animal. Hence it is that on touching with the extremities of the galvanometer two points of the same face of one of the organs, provided they correspond to different thicknesses, we have the discharge; if the back be touched, the discharge proceeds from the thicker points of the organ to those less thick; if the ventral face of the organ be touched, the discharge passes from the thinner points of the organ to those which are thicker. For the same reason and by the same law the discharge is procured on touching upon the same face two non-symmetrical points of the two organs. From this we may infer why it is that the galvanoscopic frog well isolated contracts if a long piece of its nervous filament is extended on the organ.

I will mention, finally, that on cutting perpendicularly one of the organs of the living torpedo, the discharge is obtained in the galvanometer by touching with the ends of platina two points of the incision, and that this discharge is proportionably stronger as the points touched are further from one another, the same direction being always observed—that is, from the point nearest the back to the point nearest the belly in the arc of the galvanometer. In a word, the poles or polar surfaces are the dorsal and the ventral surface of the organ.

In operating with the galvanometer on the gymnotus, a strong deviation is likewise obtained in the act of the discharge, by placing the extremities of the instrument in contact with those of the animal. All other circumstances being equal, the deviation increases by increasing the surface of the electrodes in contact with the fish, and this is distinctly seen in the torpedo. In proportion as the extremities of the galvanometer approach one another towards the middle of the gymnotus the discharge is diminished, and the same occurs when the discharge is received with the hands. With the gymnotus the discharge is directed in the galvanometer from the head to the tail of the fish.

Quite recently Ranzi, an eminent surgeon, whose loss Italy still deplores, received from me, on the occasion of a visit which he made to Egypt, instruments and instructions for studying the discharge of the silurus of the Nile, and to him we owe the discovery of the direction of the discharge in that fish, a discovery since verified at Berlin in a living silurus. In this fish, whose body is also elongated, the poles of the electrical organ are at the extremities of its body, but, strangely enough, in opposition to the gymnotus, the positive pole is here towards the tail and the negative pole towards the head.

After having thus shown the electrical phenomena of the discharge of the torpedo, phenomena which cannot differ from those of other electrical fishes, we should now proceed to state the principal facts on which the theory of these animal electro-motors is founded. But it is necessary first to give some account of the structure of the electric organs, which does not essentially differ in one fish and another. In general the organ in question is constituted by an albuminous liquid contained in certain cylindrical or prismatic cavities which have a membranaceous envelope and are separated transversely by partitions of very thin membrane. These masses or prismatic columns are disposed in the torpedo with their bases in contact with the skin of the back and belly, and hence when the fish is in its natural position the prisms are vertical. In the gymnotus, on the contrary, the prisms are horizontal and parallel to the axis of the body, and have their bases at the head and at the tail of the animal. Analogous to the structure of the organ of the gymnotus is that of the organ of the silurus.

The chemical composition of the substance of the organ is that of the liquid which fills the prisms and the cellules of which these are composed, and which is in great part a solution of albumina; a thousand parts of the substance of the organ of the torpedo contain 903.4 of water. The fresh substance of the organ is neutral, and it is only on leaving it for some time in the air that its consistency is found to diminish, when it becomes partially fluid, and then offers a slight alkaline reaction. A part of the composition of the organ is formed by the cellular matter and a considerable quantity of fatty matter containing phosphorus, which is probably due to the great number of nervous filaments distributed in the electric organ. Anatomists have found that the nerves of this organ have in different fishes a different origin. In the torpedo these nerves proceed from the fifth and eighth pair, while in the gymnotus the nerves of the organ are all spinal nerves. The brain of the torpedo is distinguished by a large mass which stretches behind the olfactory and optic lobes and the cerebellum, a mass which exists only in a rudimentary state in the brain of fishes of the same species. This mass, which is called the fourth or electric lobe, seems to consist of an expansion of the medulla elongata, and is composed in part of elementary fibres which furnish nerves to the organ, and especially of gray and ganglionic matter.

Hunter counted in an organ of the torpedo four hundred and seventy prisms, and succeeding anatomists have ascertained that each of these prisms contains about two thousand superposed cellules. The prisms of the gymnotus are considerably longer than those of the torpedo, for they extend almost from one extremity of the fish to the other. The number of prisms in each of the organs of the gymnotus is much less than in the torpedo, but in each of these prisms are contained about four thousand cellules or elementary organs. From these numbers it results that the cellules of the organ of the torpedo are smaller than those of the gymnotus, and accordingly ten diaphragms of a prism of the gymnotus form a height which is six times greater than that occupied by ten diaphragms in the prism of the torpedo. The surface of the cellules is also very different in the two organs: the cellule of the gymnotus has about fifty square millimetres of surface, and that of the torpedo has only from six to eight millimetres. In fine, the sum total of prisms in the organ of the torpedo is about ten times greater than that of the prisms in the gymnotus. It may be approximately assumed that one of the two organs of the torpedo contains nine hundred and forty thousand cellules, while the organ of the gymnotus, though larger, contains only one hundred and ninety-two thousand cellules; hence the volume of one of the cells of the gymnotus is seventy or eighty times greater than the volume of the cellule of the torpedo. The nerves distributed in the organ are ramified upon the diaphragms which constitute the cellules, and are therefore extended in planes transverse to the axes of the prisms.

Very recently Pacini and some of the anatomists of Germany have assured us that in every diaphragm there are found two laminæ, one of *connective* cellular tissue, and the other, which they have termed the *electrical lamina*, of nervous elements. There would seem to exist a relation between the position of this electrical lamina and the relative distribution of the poles in different electrical fishes. If this relation were well demonstrated and generalized it would furnish an important datum for the theory of the electric organ.

After these anatomical statements we will proceed to a consideration of the fundamental fact on which rests the theory of these electro-motors, and which leads to the conclusion that the cellule of the organ is the elementary electromotor of that organ. To show this, I detach from the organ of the living torpedo a portion as small as possible, and remembering the position which this portion occupied with reference to the faces of the fish, I apply the extremities of the galvanometer to the bases of the prisms of the portion in question. If I now irritate, in any manner, the nervous filaments of this detached piece, I am secure of obtaining the discharge, and consequently the deviation of the needle in the same direction it would have with the entire organ. This experiment may be made on a piece of the organ not larger than the head of a pin; for if we place upon this the nerve of a galvanoscopic frog and wound the piece with a very fine forfex, we at once observe the contraction of the frog.

Among the stimulants applied to the nerves of the organs, the electric current was naturally considered. For this purpose one of the organs of a living torpedo is rapidly detached, and by an incision the nerves distributed within it are exposed to view. It is best to keep each of these nerves suspended by a silk thread, and galvanoscopic frogs are distributed on the surface of the organ to indicate the discharges. An electric current is then made to pass now into one and now into another of these nerves. The discharge obtained is constantly in the usual direction in the galvanometer—that is, from the back to the belly. On separately irritating different nerves, the contractions of the frogs show that the discharge occurs each time in the portion of the organ in which the irritated nerve ramifies. Under the irritation produced by the current on the nerves the discharge of the electric organ pursues a like course with the muscular contraction. At first, when the organ is scarcely fatigued, there is a discharge as well at the opening as at the closing of the circuit; when the excitability of the nerves is enfeebled the discharge occurs at the commencement of the direct and the end of the inverse current.

I have sought to discover whether the function of the electric organ is accompanied by a variation of the volume or of the form of the organ. For this purpose I place the torpedo in a glass receptacle which is filled with water and exactly closed with a cork stopper; through the stopper pass two copper wires covered with gutta-percha and inserted in the skin of the fish in proximity to the nerves of the organ; a tube of glass open at both extremities also passes, in which a column of the liquid of the vessel remains suspended as an index. On irritating the torpedo with the current of an electro-magnetic machine we may be assured that the animal yields the shock by placing also prepared frogs in the vessel. It will thus be ascertained that the discharge occurs without any sensible variation in the volume of the fish. It might be that the volume of the organ is not altered, as is the case with muscles in the act of contraction, which yet become shortened. To ascertain whether this occurs in the torpedo, I take a lever of straw or very thin glass, with very unequal arms, and rest the extremity of the shorter arm on the face of the organ; it will be seen that in the act of the discharge the extremity of the long arm remains unmoved.

Let us now inquire what relation exists between the electric function of the torpedo and the nervous action. To evince this relation, the experiments which we have made to prove that the cellule of the electric organ constitutes the elementary electro-motor will suffice. It is known that when the torpedo or gymnotus has given a certain number of discharges it is necessary to allow some time to intervene that the organ may recover its faculty; the organ and its nervous system are exhausted by being brought into action, as are the nerves and muscles by contraction.

In this connexion it is proper to notice the special function exercised by the fourth lobe of the brain on the discharge of the organ. We have already seen that on irritating the torpedo at any point whatever of its body, the fish responds by an action which proceeds through the medium of the nerves of sense from the irritated point to the brain, and from the brain to the electric organ by means of the large nervous branches which are distributed therein, and which have no other function than that of determining the discharge. The fourth lobe is the centre, in which is collected and from which proceeds the nervous action of the organ. I take a living torpedo, in which I rapidly remove the covering of the brain. I distribute prepared frogs on the body of the torpedo, and then, from time to time, mechanically irritate the different lobes of the brain and the spinal medulla. I perceive, on performing the experiment carefully, that it is the irritation of the fourth lobe which specially determines the discharge, and that this proceeds now from the right and now from the left organ, according as I touch at one time the right, at another the left part of the fourth lobe. I remove entirely the three first lobes of the brain, and divide the medulla elongata immediately under the fourth lobe. The fish now neither renders the discharge voluntarily, nor when irritated in some part of its body; there is no longer anything but irritation of the fourth lobe which gives rise to strong discharges. I have proposed to try, by irritating a nervous fibre uncovered in the middle of the organ, whether the irritation of this fibre ever excited the discharge in that part of the organ corresponding to the centripetal portion of such nerve, but have in no case obtained any sign of this retrogressive excitation of the nerve of the electric organ. Strychnia and morphia kill the torpedo by exciting strong muscular contractions, and at the same time compelling the animal to yield frequent discharges. With strychnia the torpedo is thrown into a state of superexcitation, so that the slightest blow given to the surface on which the fish lies occasions the discharge. Poisoned with curare, a torpedo, which no longer yields muscular contractions when its nerves are irritated, does not therefore cease to give the electrical discharge.

It behooved also to study the influence of the sanguineous circulation and of respiration on the electrical function of the torpedo. Galvani was one of the first who proved that this fish, depleted of blood, continues to yield the discharge. The sanguineous circulation is, therefore, not immediately necessary to the function of the electric organ. The temperature of the water in which the fish is placed has great influence on that function. If the torpedo be placed in water at four or five degrees above zero, or still colder, it ceases at once to move or to give discharges. It is not, therefore, dead, and may be kept for a long time at this low temperature in a limited quantity of sea-water, since, respiring less, the air therein will for a longer time supply its requirements. After immersion for a few minutes in water at +15° to 20°, the torpedo resumes the electric function which had been suspended while it was in the cold water. Placed in water of +30° or 40°, the fish gives several discharges, and soon dies; the circulation and respiration, quickened by the higher temperature, probably increase the nervous action, but respiration presently ceases, because the air of the water is too rapidly consumed. By measuring and analyzing the air disengaged in the water in which torpedoes have been kept, I have found that the exercise of the electric function is attended with greater activity of respiration, whence, under equal conditions, there is considerably less oxygen in the air disengaged in water in which there has been a torpedo provoked to give many discharges, than in that in which a torpedo has been left in repose. It should be added that the coagulation of the albumina of the substance of the organ destroys the electric function.

The ultimate conclusion to which all the experiments made upon the torpedo and other electric fishes conduct us is the following:

"The irritation of the nerve of the electric organ transmitted in the direction of its ramification, when arrived at the extremity of the nerve, and consequently in the elementary cellule of the organ, electrically polarizes that cellule, and by an action analogous to electro-magnetism develops transversely to its direction, and upon the two faces of each diaphragm, opposite electrical states."

There can be no difficulty in understanding, in the light of this principle, the chief properties of the electric organ. Each prism of the organ resulting from a great number of superposed cellules, it is clear that the poles of the organ must always be at the extremities of the prisms, and that the electric tension will increase with the height of the prisms, and with the energy of the nervous action. It is not so easy to understand, at least by the law of the voltaic battery, the influence of the volume of the cellules, and especially of the number of the prisms. These two conditions should cause a variation of the internal resistance of the batteries and of the prisms, and not of the electro-motive force, and hence not of the intensity of the discharge, since this is generally transmitted by bodies which have a much greater resistance than the organ of the torpedo. That the tension depends on the number of cellules is proved by the influence of the height of the prisms; and is proved also by the discharge of the torpedo, which, at an equal height of organ, is stronger than that of the gymnotus, because the elementary cellules in the former are much the most delicate.

If we were acquainted with the intimate nature of the electro-motor element of these fishes, we should very probably know how the number of the prisms and their division act in the discharge. It is a fact that the discharge in the galvanometer increases with the extension of the electrodes which touch the organ of the torpedo. Since each of the prisms is polarized independently one of the other under the irritation of its proper nerve, it may be inferred that the total discharge is the effect of the sum of the simultaneous, though independent, discharges of the different prisms.

It remains, then, to know what is the intimate manner of action of the elementary electro-motor, and thence to discover the relation between the distribution of the nerves in the organ, its structure, and the distribution of the electrical states in the organ itself. It is only of late that we have succeeded in throwing some light on these problems, and I shall terminate the study of the electric function of these animals by briefly stating the principal results obtained on this subject.

It is now proved that the organ of the torpedo, and probably that of other electric fishes, is not an electro-motor instantaneously called into action, but an electro-motor which is constantly active. I take, therefore, from a torpedo, dead for several hours, a piece of the organ, which I reduce to a cubic form and render as small as conveniently may be, without forgetting the position of the bases of the prisms with reference to the back and belly of the fish. This piece, placed on the gutta-percha support, is brought into contact with the cushions of the most sensitive galvanometer. If I touch the bases corresponding to the belly and the back, I obtain a deviation equivalent to the entire quadrant, and which indicates a current directed in the galvanometer from the back to the belly, as in the discharge of the fish. The needle settles at 70 or 80 degrees, and the deviation endures for several hours, decreasing very slowly. By touching the longitudinal wall of the prisms, and now one, now the other of the bases, a deviation is obtained which is slighter, but still in the direction of the discharge. There is no deviation in the single case in which I touch at the same time any of the two lateral faces.

This constant electro-motor power of the organ of the torpedo persists long after the excitability of the nerve is completely extinct. I have left a torpedo for twenty-four hours in a tin vessel surrounded with a frigorific mixture, and after that lapse of time a portion of its organ and a portion of that of another torpedo left in the air have presented no difference. I have found the same result in a torpedo which had been for five days in ice. Even in a torpedo poisoned with curare the proper electro-motor power of the organ persists as in a torpedo untouched. I have also verified, by resorting to the so-called differential method, that this electro-motor power of the organ is independent of the size of the piece on which we operate—that is, of the number of prisms of which it is composed, and that, on the other hand, it is proportional to the length of the prisms. Nor does the power in question vary through the nature of the gas in which the portion of the organ is kept. This was verified by keeping it in air more or less rarified, in oxygen, carbonic acid and hydrogen. On the contrary, if the organ is kept immersed in any liquid for ten or twelve hours, the power is much weakened. Saline solutions act feebly, but acid or alkaline solutions even very much diluted, and in which the organ is immersed but a few hours, entirely destroy the electro-motor power. On comparing a piece of the organ taken from a living torpedo with the gastrocnemian of a frog of the same length, the electromotor power of the organ is found to prevail over the muscle; but two of the latter united overcome the organ.

The result which I consider most interesting as regards the theory of the electric function of the fish is the influence which the excitation of the nervous system of the organ exercises in permanently increasing its electro-motor power. The experiment is decisive and easily performed. I take from the same torpedo two equal pieces of organ, with the precaution that each of them shall have united with it a portion of the nervous trunk. I oppose these pieces to one another by bringing into contact either the two faces of the back or those of the belly, and assure myself that there is no differential current. Then, the double battery being decomposed, I irritate with the electric current or with an instrument the nerve of one of the pieces, which yields several successive discharges, as is indicated by the galvanoscopic frog. I now recompose the battery, and find a strong differential current in the piece of organ which has given the discharges and which has been in action. The differential current continues diminishing, till after a certain time it becomes null. The phenomenon may be then reproduced in the same piece several times in succession, a diminution, however, in the differential current being always observed, which is, of course, owing to the cessation of the discharges of the organ. The conclusion is, that the excitation of the nervous system increases the electro-motor power of the organ; in the living animal it is exalted to the point of yielding strong discharges; but this power still persists when the nerves of the organ have for a long time lost every trace of excitability. I will finally add that the action of the electric organ is not accompanied by the development of caloric, even when the most delicate instruments are used to detect it, and that a piece of organ enclosed in a limited space of air may give many successive discharges without producing any sensible modification in the air.

These conclusions, deduced from rigorous experiments, show the differences which exist between the function of the electric organ and that of the muscular contraction. Without pretending to give the theory of the mode of action of the electric organ, I will not withhold the opinion that these results would be readily explained on the hypothesis that in every cell there are substances of different nature which react chemically by generating electric currents, and that these materials are secreted in greater quantity by the excitation of the nervous system. I will merely intimate that these substances might be an acid and a base, and, in passing, notice the fact that if a piece of divided organ be left in a funnel there issues a liquid which after some time has an alkaline reaction; the aqueous infusion of the same divided organ being slowly evaporated leaves a residuum which yields a very distinct acid reaction. I do not from this fact, I repeat, propose to say in what consists the electro-motor power of the torpedo; but certainly the results which I have presented open a new path for investigation in the subject of animal electricity.

Electrical phenomena of muscular contraction.—In the beginning of 1842 I communicated to the Academy of Sciences of Paris an experiment which has been the origin of an interesting branch of electro-physiology. It was this: I prepare one or more galvanoscopic frogs which are placed as usual on the gutta-percha support, and I extend the nerves of these frogs upon the surface of the exposed muscles of any animal living or recently killed. To render the experiment more easy I take a frog prepared after Galvani's manner and stretch the nerves of the galvanoscopic frogs upon the muscles of its thighs. Whatever be the means used to make these muscles contract, the galvanoscopic frog at the same instant undergoes contraction. If the nerve of the galvanoscopic frog be placed upon the heart of a frog or other living animal, at every contraction of that muscle the galvanoscopic frog suffers contraction. This phenomenon, which for the sake of brevity I shall call *inducted contraction*, is realized whatever be the manner in which the nerve is spread upon the muscle, and if the frog, which I term inductive, is very vivacious, the inducted contraction is obtained even by placing the nerve on the extremity of the foot, or by detaching promptly a portion of muscle and forcing it to contract by incisions in different directions. In this respect the experiment of inducted contraction appears analogous to the discharge of the organ of the torpedo.

There exists no other organic tissue which, irritated in whatsoever manner, excites the inducted contraction; the muscle itself, if taken from a frog killed with curare, or with tendons so divided that contraction does not occur, will not, on irritating the nerve, give rise to the *inducted contraction.* On the other hand, by having several galvanoscopic frogs freshly prepared and very vivacious, and arranging them in series—that is, placing the nerve of one on the gastrocnemius of another—all will be seen to fall into contraction when one of the extreme ones is made to contract. This is what may be termed having *inducted contraction of the second, of the third order.* Attention has been directed to the influence of bodies interposed between the nerve of the galvanoscopic frog and the muscle in contraction, and it has been found that a good conducting stratum, leaf-gold for instance, or a solid isolating stratum, though extremely thin, alike hinders the inducted contraction. On the supposition that the phenomenon is analogous to the discharge of the torpedo, we can understand how the conducting metallic stratum, by allowing the discharge to pass entirely within itself, hinders the effect of it on the nerve. A stratum of oil or of turpentine was interposed and the inducted contraction was equally produced: the idea of the electric discharge was not on that account excluded, because it was proved by experiment that under like conditions a slight discharge of the jar made itself felt upon the nerve of the galvanoscopic frog.

I propose to show still another experiment with the galvanoscopic frog which will render clearer the nature of the inducted contraction. The inductive frog being prepared and placed upon an isolating plane, I place two wicks of cotton or strips of paper moistened with water in contact with the extremities of the frog—that is, one of them in contact with the upper thigh and the other with the leg—and I fold the two cushions in such a way as to leave an interval of some millimetres free between their extremities. Having then some galvanoscopic frogs, fresh and vivacious, the circuit is closed by placing their nerves between the two wicks or strips. We shall now see the galvanoscopic frogs contract each time that the entire frog undergoes contraction. If one of the wicks be detached, if at any point this circuit is opened, the contractions of the galvanoscopic frog are at an end. This result leads necessarily to the admission that in the act of contraction a homogeneous conducting arc applied on the extremity of the muscle is traversed by an electric current, or rather by a discharge, if we judge from its brief duration.

By varying these experiments with the galvanoscopic frog, placing some with the nerves in a certain direction, and others with the nerves in an opposite direction, it will be found generally that the contraction does not take place except in the galvanoscopic frogs whose nerves are extended from the inferior to the superior extremity of the inductive muscle. In a word, having two galvanoscopic frogs, whose two nerves close the circuit, arranged one against the other, the most constant inducted contraction takes place in that which has the leg turned towards the upper extremity of the inductive muscle. Similar experiments were tried upon animals of warm blood and with analogous results, although such experiments are sufficiently difficult from the brief duration of the muscular irritability in these animals. By supposing the moistened wicks, placed in contact with the inductive thigh, to be progressively smaller, we may pass from this case to that of the inducted contraction, and to the conclusion that the phenomenon is of the same nature, as well in placing the nerve directly on the muscle as in forming the arc in the manner I have shown. We thus arrive at the conclusion that the inducted contraction is really the effect of an electric discharge which occurs in the act of contraction, and the direction of which, in the exterior arc, is contrary to the current of the same muscle in a state of repose.

Inasmuch as the galvanoscopic frog is the most convenient instrument for the study of electric currents of brief duration, it was proper to have recourse to the galvanometer and to contemplate these electrical effects of contraction through the accurate indications of that instrument. This was done by Du Bois Reymond in using a very delicate galvanometer and causing the inductive muscle to contract several times in succession in order that the action might be prolonged. The following is the mode in which he makes the experiment: A galvanoscopic frog being placed on the cushions of the galvanometer, the nerve is kept isolated in order to cause a series of electric currents to pass, which may in some measure torpify the muscle, without giving room to the doubt that this current might pass into the galvanometer. At first, as is natural, the needle deviates through the muscular current. When the needle is distinctly fixed the muscle is made to contract, and thereupon, if the contractions are strong and sustained, the needle will be seen to move to zero, to pass beyond, and become fixed in the opposite quadrant with a certain deviation, which lasts as long as the contractions are strong. To remove every doubt of the exactness of the experiment, Du Bois Reymond has satisfied himself that the same thing occurs from irritating the nerve with heat or by mechanical means, or employing a frog poisoned with nux vomica. From all which he concludes: "that in the act of contraction the electro-motor power of a muscle is diminished, whence the opposite current due to the secondary polarity of the laminæ being enabled to prevail over the muscular current, not only does the needle descend to zero, but it is compelled to deviate in an opposite direction." When the experiment is made by using two opposed gastrocnemians, one of which alone is made to contract, the explanation is still more easy, according to Du Bois Reymond, for then it is the current of the muscle left in repose which is the prevalent one, and which compels the needle to deviate in the contrary direction.

As to the phenomenon of the inducted contraction, properly so called, Du Bois Raymond admits that when the nerve of the galvanoscopic frog is placed in contact with the muscle the muscular current circulates in it, and that the rapid diminution or negative variation, as he calls it, of the same current, is the cause of the contraction of the galvanoscopic frog. This supposes that in the nerve of this frog the circuit of the muscular current is constantly established: instead of which we have seen that the inducted contraction is obtained, whatever be the manner in which the nerve is disposed; that it occurs from only touching with the nerve the extremities of the leg of the frog, and that, following at the moment of the contraction, it results that the nerve is traversed by a discharge which is in an opposite direction to the muscular current.

It occurred to me that to remove every doubt on the interpretation of the electric phenomenon which accompanies the contraction, it would be sufficient to repeat the experiments with the galvanometer by the method which excludes the secondary polarity. This I have done after having satisfied myself that there was in fact no sensible trace of secondary polarity, even after having caused to pass in the laminæ currents much more intense than the muscular current. Now, even in this manner, when strong contractions are excited in the muscle placed in the circuit of the galvanometer, the needle immediately declines and continues to oscillate for some moments in the opposite quadrant. In a word, the result is not different when the secondary polarity of the laminæ enters into the experiment or when this polarity is entirely excluded. To succeed better with the experiment, instead of the gastrocnemian, the thigh of the frog may be used, by which we have from the first a muscular current which is small and often even null. Then, under the contractions the deviation of the needle immediately takes place in the same direction in which that deviation occurred when the muscular current circulated from the first in the galvanometer.

I propose also a simple arrangement with which the experiment succeeds more easily and certainly. In a piece of varnished wood I form two cavities in the shape of the two connected thighs of a frog. In the same wood is formed another cavity divided into two compartments which are filled with sulphate of zinc, and in which are immersed the amalgamated laminæ of the galvanometer. I take a lively frog and reduce it to the two thighs alone, leaving the lumbar nerve to a single thigh. The other thigh, though not meant to contract, is left only that there may not be a transverse incision in the inductive thigh, which would introduce a new cause of the muscular current. To establish the circuit I use two pieces of hempen string secured with sealing-wax to a plate of glass, which is so placed that on one part the two strings touch the extremities of the galvanometer, and on the other two points of the thigh at a distance of about ten millimetres apart. The thigh should be pressed with the lamina, that it may be certain that during the contraction the contact remains unaltered. Generally, when the circuit is closed, there is a slight deviation, first in one direction, then in the other, as is known to occur from touching in various points the muscle of the thigh. We now irritate the lumbar nerve with a current several times in succession, which nerve is kept suspended and isolated by a silk thread. The deviation which results, and which continues as long as the strong contractions of the thigh endure, always indicates a current which is directed in the galvanometer from the lower to the upper extremity of the thigh, and therefore in a contrary direction to that which is obtained from the gastrocnemian or from the entire frog in repose. I have further modified this experiment by forming with a slight incision a transverse section in the upper part of the thigh. In this way there circulates in the galvanometer from the first a muscular current in a contrary direction to the current of the gastrocnemian. The experiment is repeated on the thigh, which is made to contract, and as often as several violent contractions occur, the deviation in the galvanometer is seen also to increase. In this case the discharge which accompanies the contraction is in the same direction with the current which before circulated in the galvanometer.

To these experiments and conclusions there was opposed in Germany a mode of performing the experiment of inducted contraction with the galvanometer, which would have led to a consequence opposed to our own, and by which it was thought the secondary polarity might be avoided. This mode consists in causing the muscle to contract several times and then closing the circuit of the galvanometer. This experiment, which I have often repeated and varied, leads to a result which was to be foreseen from constant observation, and on which I have frequently insisted; that is, that the electric phenomenon of contraction decreases rapidly with the weakening of the contraction, and is in general not produced, except under the strongest contractions, which are the first, and which quickly cease, especially by using a strong inducted current to excite the nerve. The result is, that in such case the circuit is scarcely closed after the first contractions have occurred, when the needle makes a slight deviation in a direction opposed to the muscular current, sometimes seems undecided in its movements, and, if the circuit be closed after many contractions, deviates in the direction of the muscular current, but more feebly than with the muscle in repose.

From all the results which I have cited, as well in operating with the galvanometer as with the galvanoscopic frog, I find myself always led to the conclusion, at which Cima has also arrived by repeating these experiments upon the higher animals, viz: that in the act of contraction of certain muscles there is an instantaneous current, or rather discharge, which flows in the galvanometer in a contrary direction to the current circulating therein, when the muscles, touched by the extremities of the galvanometer, are in repose. It must be confessed, however, that properly to generalize this result, it would be necessary that we should be able to operate on the muscles of the higher animals in which the muscular irritability ceases with extreme rapidity.

From this conclusion we do not infer that the electric phenomena of the muscle in repose and that of the muscle in contraction have a different origin. Notwithstanding appearances, we have an opposite conviction; and if there could in experimental science be any advantage in framing hypotheses, we might, by recurring to analogy with well-known electrical apparatus, conceive how it would be practicable, from the same electro-motor, with a change of form in the various parts of the circuit, to obtain in the galvanometer currents in opposite directions. But this procedure we are not willing to adopt, and prefer rather to say that we must wait till a true knowledge of the form of the muscular electromotor is acquired, in order to see this species of anomaly disappear.

I will conclude this lecture by adducing a new fact of muscular electricity which clearly lends itself to the support of the theory we have formed respecting the production of this electricity. I prepare from the same frog two half thighs, oppose them to one another, and find by the galvanometer that they are equal. I then cause one of these half thighs to contract several times by irritating the nerve which is attached. I quickly thereupon recompose the double battery, and now find a strong differential current in the direction of the muscle left in repose. This current continues a certain time, but constantly decreases, and finally the needle returns to zero. The phenomenon is produced by new contractions, but is naturally less intense, because the contractions become continually weaker. The consequence of this fact, according to all analogy, is, that the chemical actions, on which depends the muscular electricity, are consumed in the contraction, and are gradually restored through repose. This fact, however, should not be confounded with the instantaneous electric phenomenon of the inducted contraction which is produced in the very act of the contraction.

It is proper that I should notice the difference which exists between the electric organ of the torpedo and the muscular electro-motor. The function of the former does not develop heat; does not produce movement; is not accompanied by the chemical action of combustion between the oxygen and the substance of the organ; the nervous irritation of the organ of the torpedo does not permanently increase the electro-motor power, as though that irritation acted upon an apparatus of secretion, and from the product of that secretion the electro-motor element of the organ had its origin. In the muscle, on the other hand, all proceeds from the chemical action which arises between the oxygen and the substance of the muscle; hence the heat, movement, electricity which accompany that chemical action. When an excess of contraction occurs, there is an excess of consumption of this chemical action which must necessarily be followed by a diminution of the electro-motor power of the muscle.

Arrived at the termination of this course, I cannot but be grateful for the attention manifested, and indulge the hope that, on some not distant occasion, I may again meet you in order to treat of some other of those branches of physics with which I have occupied and still occupy myself.

PALAFITTES, OR LACUSTRIAN CONSTRUCTIONS

OF THE LAKE OF NEUCHATEL.

BY E. DESOR:

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TRANSLATED, WITH THE AUTHOR'S RECENT ADDITIONS, FOR THE SMITHSONIAN INSTITUTION.

PREFACE.

The following essay does not profess to be a summary of the whole of our knowledge respecting lacustrian constructions, still less to present a picture of the civilization of central Europe during the three ages of stone, of bronze, and of iron, which are represented in our lake. We have neither the authority nor the resources necessary for embracing so vast a subject. What we propose is but a simple excursion into a domain which, though not our own, is by no means a foreign one. When, twelve years ago, the discoveries of prehistoric antiquities at the bottom of the lake of Zurich first awakened the interest and curiosity of all the friends of science in Switzerland, we were tempted, after the example of MM. Schwab, Troyon, Morlot, and Rochat, to seek what the lakes at the foot of Jura might contain. We were not long in being satisfied that these lakes, and especially that of Neuchâtel, were richly endowed; and the idea occurred to us of applying to lacustrian researches the methods employed in geology, hoping that by taking into account certain accessory circumstances, to which archæologists do not always accord the importance which they merit—such as the distribution of objects, their frequency, their association, their state of preservation at different stations—we might perhaps obtain a picture, if not more complete, at least more exact, of the conditions of the existence of our primitive populations.

Setting out from this point of view, we directed our inquiries chiefly to the stations which present a definite stamp, and which, like the characteristic repositories in paleontology, may be regarded as authentic for the age of stone, the age of bronze, or the age of iron. On the other hand, we have attached but a secondary importance to the stations which comprise the remains of several ages, even when very rich, like the station of Nidau at the lake of Bienne, that of Font at the lake of Neuchâtel, and that of Montillier at the lake of Morat. The antiquities which these stations disclose will always possess a real interest as objects of curiosity, or as serving as the complement of specimens collected elsewhere; but we should hesitate in conceding to them a conclusive value when the determination of the character of an epoch is in question.

Our first researches were given to the public in an article in the *Almanach de la Société d'Utilité Publique de Neuchâtel* in 1859; a second edition, considerably enlarged, appeared in the *Bibliothèque Universelle* of 1862. Meanwhile discoveries multiplied from day to day, the age of iron in particular furnishing a large contingent of rare and new objects, and we were thereby induced to publish a new edition, which appeared in the *Musée Neuchâtelois* of 1864, accompanied with several plates.

At the present hour the mine is far from being exhausted; new treasures have been still added to those which we already possessed. And as the interest of the scientific public has not ceased to encourage us, we have not thought proper to resist the temptation held out to us by our friend, Mr. Reinwald, librarian at Paris, of giving to the essay in question a wider publicity, as well as greater extent, accompanying it, on this occasion, with numerous engravings, which will allow a more thorough comparison with the antiquities of other localities, whether collected on firm land, in the tombs, the hypogeums, the dolmens, &c.

Figure A.

Figure B.

As the lacustrian constructions, to which the antiquities of our lakes appertain, constitute at the present day a definite type, there would seem to be room for designating them by a specific name, like other monuments, such as dolmens, pyramids, &c. The German name of *Pfahlbauten*, (constructions on pile-work,) proposed by M. Ferd. Keller, and now popularized in Germany and Switzerland, has been adopted by Italian archeologists under the form of *palafitta*. Hence the appellation of *palafitte* which we propose to introduce into our language.

In recommending the geological and paleontological methods for the study of our lacustrian antiquities, we must be understood as having imposed on ourselves the greatest reserve in determining the age of our different palafittes. There can be scarcely a question of dates, except for the epoch of iron. As to the ages of stone and bronze, we shall esteem ourselves fortunate if this essay shall furnish some terms of comparison to those who occupy themselves with the origin and affinities of the ancient races which have peopled Europe.

As the antiquities of the age of stone are widely distributed, and much better known than those of the subsequent ages, we have dispensed with entering into all the details which this phase of the prehistoric period would permit. On the other hand, we have given so much more attention to the palafittes of the age of iron, which are a speciality of our lake, at the same time that they serve as a link uniting historical with prehistoric times.

The principal implements employed in the search for antiquities in our lakes are a sort of hand-hoe, (figure A,) and of tongs, (figure B,) provided with a cord for retraction, by means of which quite small objects can be recovered, such as knives and pins, from a depth of four or five metres. The annexed drawing represents our operator, B. Kopp, searching with the tongs in the midst of a palafitte, (figure C.) To the skilful pencil of Professor A. Favre-Guillarmod we are indebted for all the designs with which this essay is embellished. Our types have been, as far as possible, selected from our own collection, with a view to facilitate the task of those who may feel an interest in comparing the originals.

Figure C.

INTRODUCTION.

It has been long known to the inhabitants upon the shores of the Swiss lakes that there existed in many of them ancient posts or piles, which, without reaching the surface, rose to a height of 30 or 60 centimetres above the bottom. On Lake Neuchâtel they were especially known to the fishermen, who dreaded them as a cause of injury to their nets. Doubtless, also, boatmen, in crossing the bay of Auvernier, or coasting along the southern shore when the weather was calm, have now and then stopped for a moment above them, wondering meanwhile to whom the strange idea could have occurred of driving piles at such a depth; and as no inhabitant, not even the oldest fishermen, could tell anything about their origin, the only conclusion arrived at was that "all this must be very ancient." More than once, also, from the ooze of the lake had been drawn, at low water, large horns of the deer, and strange utensils whose origin was unknown; among other occasions, at the lake of Zurich in 1829, and still later at the lake of Bienne. These things, however, remained a dead letter; the circumstance was thought to be curious, but nothing more. An idea has sufficed to restore life, in some sort, to these ancient remains, and draw from them a disclosure of surprising facts. A man of true science happens to pass in the neighborhood of the works which, during the low stages of water of the winter of 1853–1854, were in course of execution at Meilen, on Lake Zurich. To him are shown the half decomposed posts withdrawn from the black deposit on the strand, which the lake had temporarily abandoned, and here and there some fragments of rude pottery, evidently very ancient, but not Roman, for it is black, imperfectly baked, and fashioned by the hand without the help of the potter's wheel. The utensils, the arms, the posts, which accompany them, have a still more primitive aspect; they recall the analogous objects collected in the peat-mosses of Scandinavia, and must, consequently, be of very high antiquity. What had escaped all notice was the relation which these objects bear to one another, and especially to the piles imbedded in the ooze. Yet the arms and pottery are not dispersed at hazard; they are limited to a particular stratum, having a thickness of two feet, which has received the name of "archæological stratum," (*Cultur-Schicht.*) Moreover, they are accumulated around the piles, where they are found in large quantity, while they diminish and disappear in proportion as they retire from it. There was a connexion then between the piles and the antique objects.

It was this connexion which our friend, M. Ferdinand Keller, guided by his practiced eye, was enabled to detect, and which, once caught sight of, has become the torch to conduct us to the discovery of a whole unknown world.

In effect, such an association of arms and utensils indicated beyond a doubt traces of man. The piles, upright in the midst of these objects, had been placed there by design, evidently to support some construction. But as their foundation is below the mean water level, they must necessarily have been planted in the water. There had existed, therefore, habitations or storehouses built intentionally on the water at the place indicated by the piles. The number of scattered utensils, corresponding to the thickness of the bed which contains them, bore witness, in turn, to a prolonged sojourn. Consequently, there had been an epoch during which the inhabitants of our country constructed places of refuge on the water, if, indeed, they did not dwell there. It was the period of *lacustrian constructions.*

The history of the sciences does not afford many examples of so brilliant an acquisition by human sagacity; it recalls that which, in another province, we owe to the genius of Cuvier. Long before this great naturalist the bones of mammals had been collected in the plaster-quarries of Montmartre; but no one had perceived the relations of these remains to one another, and to the medium in which they are concealed. They were looked upon as vestiges of the deluge. Cuvier studies these ancient and petrified skeletons, and recognizes in their association and manner of deposition the traces of a whole creation anterior to man. To the French naturalist some fossil bones had thus sufficed for the reconstruction of a phase of the history of the earth: some fragments of pottery, buried under the gravel of the lake of Zurich, sufficed to our own learned antiquary for the revelation of a forgotten cycle of humanity beyond the bounds of history. The hint being once given by the first publication of M. Keller,* in the Memoirs of the Archæological Society of Zurich, the zeal and activity of our Swiss antiquaries might safely be relied upon to elaborate this new vein, which, indeed, did not long delay to furnish us with scientific treasures.

They began by seeking for piles in the other lakes of Switzerland. The fishermen could almost everywhere point them out, and these piles became, in turn, valuable guides in conducting to unexpected discoveries. At Meilen, with the exception of a single object in metal, only utensils of bone and stone had been brought to light. Elsewhere, and more especially in the lakes of eastern Switzerland, beside stations recalling those of Lake Zurich, were discovered other stations which, instead of objects in silex and bone, yielded numbers of utensils in bronze. These articles bore witness to a much more advanced civilization. The lacustrian period, therefore, embraced several distinct phases. It became an interesting problem to investigate, and fix, if possible, the peculiar character of these different phases or epochs. This work was everywhere pressed forward; discoveries rapidly multiplied, and gave rise to numerous publications devoted to the description of new stations, and the antiquities which they contained. Nor was the necessity of co-ordinating the facts obtained slow in making

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***Die keltischen Pfahlbauten in den Schweizerseen.*—Communications to the Antiquarian Society, Zurich, 1854.

itself felt, whence at present we are in possession of several systematic works on lacustrian constructions, which, while evincing the science and sagacity of their authors, have powerfully contributed to diffuse a taste for the study of this old world, scarcely resuscitated from the tomb of unrecorded centuries. Among the number of works which have obtained a well-merited success may be cited, as in the first rank, that of M. Troyon, (*Habitations lacustres des temps anciens et modernes*, Lausanne, 1860;) that of M. Morlot, (*Etudes Geologico-Archæologiques en Danemark et en Suisse*, Bulletins de la Societé Vaudoise des Sc. Nat., 1860;) to which may be now added a third, that of M. Schaub, (*Die Pfahlbauten in den Schweizerseen*, Zurich, 1864,) which is especially calculated to popularize throughout our country the study of lacustrian constructions.

The lake of Neuchâtel, thanks to the interest which the inhabitants of its shores cherish for the history of their native land, could not fail to stimulate interesting discoveries. There, pile-works were known to exist at many localities—at Bied, Cortaillod, Auvernier, Chez-le-Bart, St. Aubin, Concise, Corcelles, Corcelettes, Font, Estavayer, Forel, Chevroux, Port Alban, Cudrefin, which became so many fertile fields, in which inquiry was soon to be rewarded with abundant harvests. The low waters of 1858–1859 having facilitated the exploration of these several stations, collections were gradually formed at different points of the shore. It will suffice to mention those of the museum of Neuchâtel, of M. Troyon, of M. Portalés-Sandoz, at Lance; of Dr. Clement, at St. Aubin; of M. Rochat, at Yverdon; of MM. Rey and de Vevey, at Estavayer, without counting our own, and the most complete of all, that of Colonel Schwab, at Bienne.

But how can habitations be conceived of at places now covered with water to the depth of two and three metres? The first question usually asked is, whether the waters of our lakes might not have been lower at the period referred to than at present? This suggestion led to the inquiry whether there existed at the outlet of these lakes obstacles which, by obstructing the rivers, may have raised the level of their waters. Land-slips have in fact been distinguished at the mouth of the Thielle,* and it has been attempted to establish a relation between these and the ancient encroachments at Nidau, as well as the remains of Roman roads in the *grand marais*, which are at present covered with peat. We are far from pretending that certain lakes of Switzerland have not undergone changes; perhaps that of Neuchâtel is of the number. But the fact must not be lost sight of, that the phenomenon in question is a general one, and as pile-work exists in nearly all the lakes, it would follow that all had been obstructed at their outlet. Now, as this is not the case, we are forced to admit that the piles must have been sunk and secured under the water, and that consequently the constructions which they supported were really lacustrian. The diameter of the piles is in general too small to have supported constructions at all massive; there can be no question here, but of cabins of very frail character.†

At first glance, the idea may seem strange, if not absurd, that men should have established themselves on the water instead of pitching their tents or building their cabins on the *terra firma;* but closer reflection will enable us to comprehend that at the origin of the lacustrian period, at an epoch when the soil of Switzerland was covered with forests and the borders of the lakes probably occupied by marshes, these lacustrian abodes may have offered to their inhabitants a more secure asylum against the ambush of enemies and the attack

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* See the article of M. Culmann, in the *Schweizerische Polytechnische Zeitschrift*, 111, pp. 9 and 10, on the bar of the Pfeidwald, near Nidase.

† For the form and appearance of these constructions we refer to the descriptions and figures which have been published by MM. Keller, Troyon, and Lyell.

of savage animals. At a later period, the epoch of bronze, these stations, as will be seen, served probably for simple magazines or places of assemblage.*

The idea of comparing the antiquities of our lakes with those found in so great number in the islands of Denmark would naturally present itself to the minds of our antiquaries. As soon as it was recognized that there existed, with reference to their utensils, striking differences between the stations, some yielding only arms and objects of silex or bone, others containing utensils and arms of metal, especially of bronze, though sometimes also of iron, it could no longer be supposed that these stations were cotemporaneous; they could but correspond to successive periods of development, having each its distinctive character. Hence, as in the north, three epochs were distinguished: *the age of stone*, *the age of bronze*, and *the age of iron*.† The lake of Neuchâtel has the prerogative, among all the lakes of Switzerland, of comprising stations of the three ages, thus enabling us to follow, within a circumscribed space, the development of humanity during the remote epochs which preceded historic times.

I. AGE OF STONE.

The stations of the age of stone, though not as numerous as in eastern Switzerland, are not wanting in the lake of Neuchâtel. To the station of Concise, which has yielded since 1859 a considerable quantity of utensils of silex and bone, have been added several others, especially those of Neuchâtel, Hauterive, Cortaillod, Auvernier, Bevaix, Chez-le-Bart, Corcelles, Estavayer, Chevroux, Cudrefin, and at Lake Morat, Greng.

These stations have with us a particular stamp, which admits of their being easily recognized. They are, in general, less extensive than those of the age of bronze;‡ not so distant from the shore, nor so deep, being at a depth not exceeding two metres below the mean level of the water. But what chiefly distinguishes them is the quality of the piles, which are much larger than those of the stations of bronze, consisting frequently of entire trunks of 25 and 30 centimetres in diameter. Instead of forming a projection in the water, they are level with the bottom, so that, notwithstanding their size, some experience is requisite to distinguish them amidst the stones which surround them. These stones themselves constitute an important character of the epoch; it is evident, from a mere inspection, that they have been heaped up by the agency of man. This is attested as well by their distribution, which is always distinctly circumscribed, while sand or mud prevails around, as by their variety in form and appearance—some being rounded, others angular. The process employed was probably of

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* It should be remembered that the ancient occupants of our own soil are not alone in their preference of aquatic habitations. There are populations of the Pacific ocean among whom this usage exists at the present day. The Indians of Venezuela construct their cabins preferably on the water, as a refuge from the flies, and we are told by Herodotus that the ancient people of Thrace had the same custom. "The Peonians of Lake Prasias," says the father of history, "could not be subjugated by Megabysus. Their habitations are constructed in the following manner: Upon tall posts sunk in the lake they fix a scaffolding which communicates with the shore by a single bridge. Each has his cabin with a trap-door opening upon the lake, and lest their children should fall into the water through this aperture, they tie them by the feet with a cord." See also Hippocrates, Opera Omnia, Ed. Kuhn, I, p. 551: "The riparians of Pharos," says the father of medicine, "lived in marshes, where they inhabited houses of wood and of reeds above the water, traversing the river in pirogues; their health suffered much from this kind of life."

† This division into three distinct ages has been contested by M. de Hochstetter, who thinks that the difference should not be attributed to separate epochs, but to differences of class. To this pretended cotemporaneity M. Keller (6th Report, p. 7) properly objects that there is a series of lacustrian constructions in which there exists not the least trace of bronze. With us this is, indeed, most frequently the case.

‡ The *tenevière* of Auvernier measures 80 metres in diameter; that of Hauterive is greater and of less regular outline.

the simplest kind; the stones were collected on the shore and transported to the designated place by means of pirogues or hollowed trees.* There they were heaped around large posts which were placed upright, and were simply secured in place by this coacervation. The result is observed at all the stations of stone in our lake, in the formation of prominences or small hillocks, which are designated on the southern shores by the name of *tenevières*,† at Courtaillod by that of *pervous*‡, while at the lake of Bienne they are denominated *steinberge*, signifying mountains of stone. This mode of construction was the only one practicable wherever the soil is of rock, as is the case at a number of points on the southern shore of the lake, at Monruz, Hauterive, Neuchâtel, where the banks of urgonian limestone approach so near the surface that it is impossible to drive piles.§ Elsewhere, if the bottom was of ooze, as is especially the case in eastern Switzerland, it sufficed to sink the posts in the ground itself without the support of stones. In this case there is no *steinberg*, in the proper acceptation of the word; but the stations are not less distinguishable from their slight depth and proximity to the shore, which is the cause of their being sometimes left dry at low water, as was the case in 1863 at Markelfingen, on Lake Constance.

The two stations of the small lake of Moosseedorf, where M. Uhlmann has gathered so ample a harvest of curious objects of the age of stone, show the same state of things; as the soil was favorable, the piles, after being pointed, were simply driven into it. Recent explorations have just disclosed the same thing at the Lake of Morat. Count de Pourtalés having proposed to form an island in front of his park of Greng, it was quite natural to select, as a nucleus, the point where the water was of least depth. This was the summit of a spacious teneviere, which occupies, it seems, a surface of several *arpents*, since, in excavating around the space reserved for the isle, there are still found numbers of piles. These are cut to a point, and sunk in the bottom of the lake, which consists of very fine sand. Here also a passage of deeper water separates the place occupied by piles from the shore; while the piles which are withdrawn present distinct traces of blows made with the hatchet.

Here an objection will scarcely fail to occur; if the tenevieres trace their origin to the age of stone, when not only iron but even bronze was unknown, and only knives and hatchets of stone could be used, how was it possible, with such implements, to cut trees a foot in diameter, even supposing the concomitant action of fire to have been employed, as is done by many savage tribes. We will not deny that, at first, this difficulty occasioned us no little embarrassment. But having examined attentively and separately the tops of the piles in many tenevieres, we remarked a peculiarity, which seems to us capable of solving the difficulty. The piles are not cut squarely, but around their circumference (to a depth of 8 or 12 centimetres;) the centre, on the other hand, is often unequal, sometimes protruding, sometimes re-entering, having the appearance, therefore, of a post which had been notched circularly, and then broken. But as soon as

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* There are a number of these pirogues in the lake of Bienne. One of them, near isle Saint Pierre, is still loaded with stones, which has led to the supposition that it had foundered with its cargo. Another has been recently taken from the lake, and forms part of the collection of Neuchâtel. We have ourselves taken one from the station of Auvernier; it is formed from the trunk of an oak, the wood of which is decomposed to the depth of five or six inches; but the centre is only so much the harder and completely black, insomuch that it might serve for material to the carpenter.

† In the idiom of the fishermen of Estavayer, this word signifies a submerged hillock—a site where the water is quite shallow.

‡ This name is also given to heaps of stone on terra firma, when they are of large dimensions; the smaller are called *morgiers*.

§ Several of these steinbergs continued to be inhabited or used during the following ages—among others, the steinberg of Nidau, where are found at the same time remains of the age of stone, of bronze, and of iron. These results are corroborated by very recent researches which we have made in the lakes of Bavaria. The isle of Roses, in Lake Starnberg, appears to be nothing but an artificial island of those remote epochs, which has never ceased to be inhabited during all succeeding periods, and is at this day the site of a regal habitation.

the question is found to relate merely to incisions of some inches, there can be nothing improbable in their having been effected by means of sharpened silex, as, indeed, we have satisfied ourselves by experiment. By means of this same process, even bodies much harder were successfully dealt with, such as the large horns of the stag, which they cut into handles.* Indeed, hard stones are found to have been cut, while here and there occur other stones which have been attempted, but not completely divided.

Figure 1 represents a section of one of these tenevieres on a rocky bottom, that, namely, of Hauteville, near Neuchâtel. The space which separates it from the shore reaches a depth of nearly two metres, while the summit of the teneviere is at a depth of barely $0^{m}.50$ at mean water.

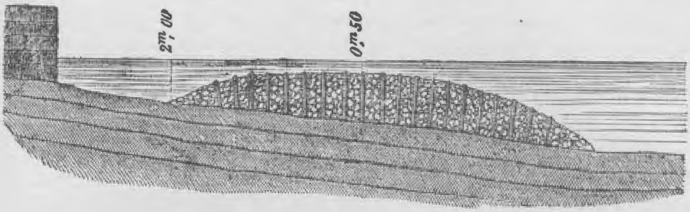

Figure 1.

The tenevieres, especially those of our lake, do not in themselves necessarily suppose habitations constructed on the water. Their proximity to the shore, their structure and inconsiderable depth, comport perhaps better with the idea that they were artificial islands, like the crannoges of Ireland,† which would also explain their nearly uniform depression. On this hypothesis it would be necessary to suppose that, at the epoch of their construction, the lake was lower than at present, by the distance which separates their level from that of high water—that is, about two metres. Ulterior researches will perhaps teach us whether the idea of a depression of the water within these limits can be justified in a geological point of view. Such islands would have afforded a secure retreat against all sorts of dangers. Perhaps, also, assemblages might have gathered there for certain festivals or entertainments, which would account for the prodigious quantity of bones accumulated at such stations, while they are more rare in those of the bronze period.‡

The station of Concise has furnished more remains of animal bones than all the stations of bronze united.§ The following have been identified up to this time at the different stations of the age of stone in Switzerland: the bear, the

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* Knives of silex are so efficacious for working in wood and bone that M. Lartet, our eminent paleontologist, has assured us that he preferred them to knives of metal for a multitude of uses.

† Ferd. Keller, *Deuxieme rapport sur les constructions lacustres*, in the *Mittheilungen der antiquarischen Gesellschaft*.

‡ This opinion, which is held also by M. de Hochstetter, has been recently controverted by our friend M. F. Keller, (Sixth Report, p. 4,) upon the ground of the regular distribution of the palafittes in eastern Switzerland, where each palafitte is divided into compartments containing the necessary space for a family, a fireplace, utensils for cooking, a frame for weaving, a store of thread. Further, it seems to result from the researches of M. Heer, that cattle also were kept there, as well as stores of provender for winter. If this be so, we should feel the less difficulty in doing justice to the arguments of our learned friend, as our exceptions bore rather on the palafittes of the age of bronze and of iron, than those of stone.

§ M. Schwab objects, in regard to the greater quantity of bones of the station of Concise and the palafittes of the age of stone in general, that it is in these alone that extensive researches have yet been made, and he surmises that if the stations of the age of bronze were adequately dredged, bones would present themselves in equal number. We shall await, therefore, the results of experience.

badger, the martin, the ermine, the otter, the polecat, the wolf, the fox, the dog, the cat, the hedgehog, the beaver, the squirrel, the horse, the hog, the wild boar, the elk, the stag, the roe, the fallow deer, the sheep, the bison, the urus, the goat, and a quantity of remains of the domestic ox. These, it will be seen, embrace for the most part the same animals which, where the chase has not destroyed them, still inhabit the forests of Europe. Among the cows M. Rutimeyer distinguishes two varieties, a very large and a small one, which is the stock of the domestic animal. The same author distinguishes, besides the domestic hog and the wild boar, a third variety, the hog of the fens (*Sus palustris,*) whose remains are especially found in great abundance in the palafittes of the age of stone in east Switzerland. This species, which was smaller than the two preceding, has shared the fate of the urus, (which should not be confounded with the bison of Lithuania,) having been lost in the course of ages. It is not probable that this animal was domesticated, nor, consequently, that it is the stock of our present hog, which descends rather from the wild boar.

The stations of stone in our lake have not yet furnished a human skeleton. We possess, in Switzerland, but a single skull of that epoch, derived from the station of Meilen on the lake of Zurich, and this, unfortunately, is not complete. It results, however, from the researches of MM. Rutimeyer and His,* that the skull in question occupies in some sort a mean between the long and short heads, (the ratio of breadth to length being as 83 to 100,) approximating in this respect to the type most common in Switzerland. It does not differ sensibly from the skulls of the station of bronze of Auvernier, of which notice will be taken further on; and being, like one of the latter, the skull of an infant, there is room to suppose that the characteristic traits of the race had not yet acquired their definitive expression.

It is difficult to form an idea of the state of culture of the tribes of the age of stone. To judge of them only by their arms and utensils, they could scarcely have been more advanced than the savages of the isles of Sunda or of the Pacific ocean, since only silex and bone were at their disposal; but it cannot be denied, on the other hand, that they had put these materials to singular profit. Such, at least, is the impression derived from an examination of the collections which have been made from Lake Neuchâtel; as, for example, that of the museum and of M. Schwab, but above all, that of Dr. Clement at St. Aubin. The distinction between arms and utensils was probably not very rigid, and it is possible, nay, probable, that some utensil which served to cultivate the soil or cut the posts for a cabin, were, on occasion, employed also as an offensive and defensive weapon, as hatchets, hammers, &c.

The arms properly so called are: lances of silex, which are sometimes several decimetres in length. Most of them are elaborated with extraordinary care, which evinces great dexterity in the art of cutting stone; all present on one side a slightly curved surface, which is the natural fracture as detached from the nucleus, and on the other a median longitudinal carina (Fig. 2;) some have a sort of tongue or neck (Fig. 3) which probably penetrated into the staff. In some rare cases an exceptional finish was bestowed on the lance by means of small transverse fractures of singular regularity. Of this Fig. 3*a* is a striking example.

The arrows are triangular, (Fig. 4,) frequently provided with barbs, which rendered them more formidable. Traces are sometimes observed of the cement which united them to the stock. There were also points of arrows of bone,

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* *Crania Helvetica*, p. 35. Some fragments of a skull have just been discovered in the midst of a quantity of animal remains, at the station of Greng, on Lake Morat; they are in the possession of Count G. de Portales.

but these are much rarer; the museum of Neuchâtel possesses one from Concise (Fig. 7) of very peculiar form; others were fitted to the stick and secured by means of pitch. In the specimen annexed (Fig. 7*a*) may be distinguished the impression of the stick and the thread which fastened it; others still are serrated, or furnished with a small lateral hook, similar to a diminutive harpoon.


Figure 5.

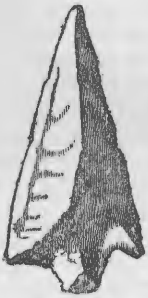
Figure 4.

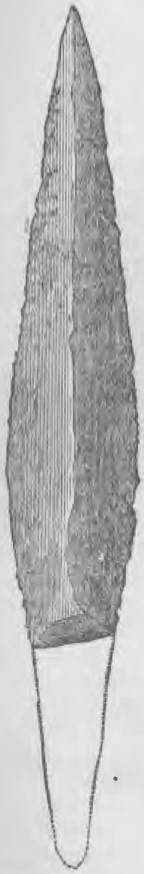
Figure 2.

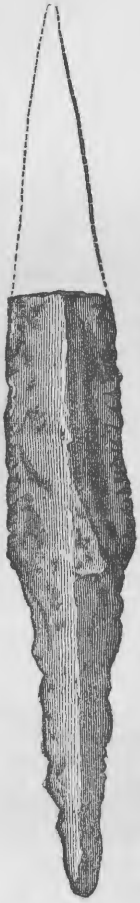
Figure 3.

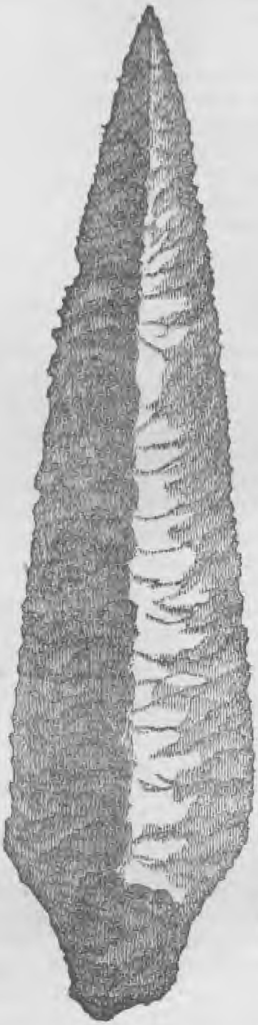
Figure 3*a*.

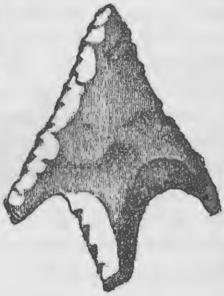
Figure 6.

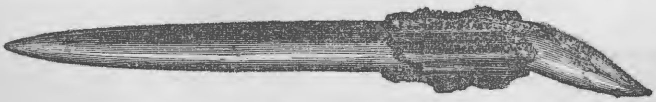
Figure 7*a*.

The bow for these arrows was of yew; they have been found in good preservation at the station of Kobenhausen and at Lake Constance. Our own lake has as yet yielded only fragments.

Another weapon, more primitive, and at the same time more formidable, is the horn of a stag converted into a club by retaining the brow antler, while the rest are removed. A club of this kind is in our possession, derived from the teneviere of Auvernier, and must have belonged to a truly colossal animal. That of which we here give a sketch, (Fig. 7*b*,) forms part of the collection of Dr. Clement, and measures fifty centimetres in length. With such an instrument we can conceive that terrible blows might be inflicted. We have in our collection a skull pierced with a round hole in the hinder part of the left parietal, which may well have been made with a club of this description.

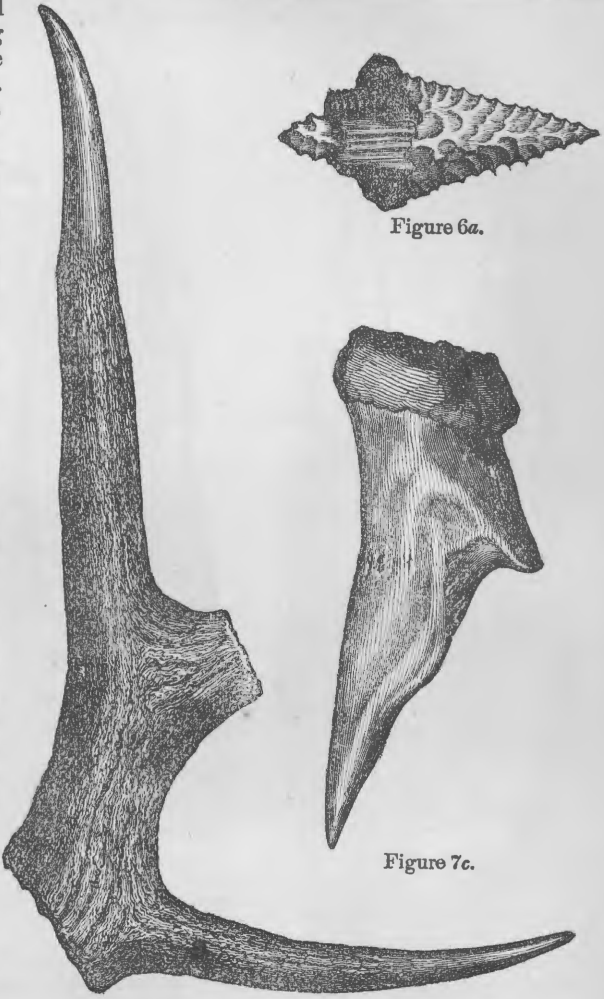
Figure 6*a*.

Figure 7*c*.

Figure 7*b*.

The horns of the stag were also employed for different utensils, as, for example, hammers, sixteen of which have been found in the single palafitte of Nussdorf at Lake Ueberlingen. There occur also hatchets of this material as well as clubs, pins, and even combs, (Keller, 6th Report, p. 280.) In the collection of Dr. Clement, we also meet with fish-hooks of stag's horn, composed of a straight stem with a lateral branch or barb, (Fig. 11*a*.) Sometimes again these instruments were formed of the tusk of the wild boar by crossing it, a remarkable specimen of which may be seen in the collection of Dr. Uhlmann. The olecrane of the stag was sometimes carved into a poinard.

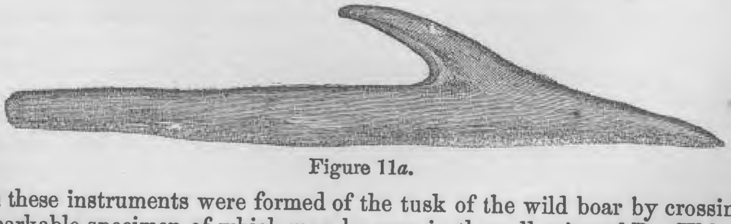
Figure 11*a*.

The long bones of certain animals (of the cow, the hog) are also often cut in the form of pointed instruments (Figs. 8, 9) which might have served for poniards, lances, or pikes; others are in the form of chisels (Fig. 10) or of pins for the hair, (Fig. 11.) The flakes of silex were employed by way of a knife or saw; we find many inserted in a fragment of horn, which were probably used in felling trees or cutting their branches, (Fig. 12.)


Figure 7.


Figure 8.


Figure 9.

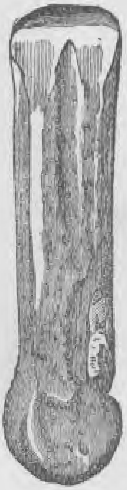
Figure 10.


Figure 11.

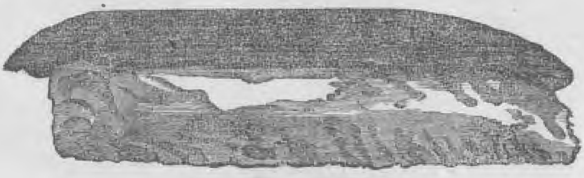
Figure 12.

If these utensils cannot rival those of the age of stone in the north of Europe, if we possess none of those poniards artistically wrought which occur in the collections of Denmark and Mecklenburg, nor those elegantly formed knives of silex which recall the finest produced by the age of bronze, it is not the less true that we can realize a degree of emulation as having existed at this remote epoch between the inhabitants of different stations. The objects collected in the lakes of western Switzerland display something of finish, of care in the details, which is not to be recognized to the same degree in the stations of eastern Switzerland. This observation is particularly suggested by the symmetry and pleasing forms of certain objects which would have been equally as efficient without being so finished. The hammers are the articles of most elegance; they are always formed of hard stone, generally of serpentine, enlarged in the middle in order that the hole destined to receive the handle may not render them too fragile; one of the extremities is rounded or plane—the other contracted more or less to an edge, and sometimes to a point. The hole itself is often irregular, being narrowed within, as an aperture would be if alternately pierced from both faces, (see disk of Fig. 21.) In every instance the hole is smooth throughout and very little contracted, nor is it unusual to meet with the core or lump which corresponds to the cavity. The patience, perseverance, and skill required to execute the perforation of such an object cannot but occasion surprise, especially when performed without the help of metal. It was perhaps effected by means of very thin flakes of silex fixed around a stock which was made to turn in such a way as to separate a portion of the stone which, when the perforation was accomplished, would fall to the ground.* Precaution was observed to enlarge the hammer at the place where the hole for receiving the handle was situated, yet accidents would still occur, as is testified by the fragments of those broken in the operation which are much more numerous than entire ones. It might be that the instrument of perforation was fixed, and the hammer itself made to revolve, as with our stationary graving implements. Figures 13 and 14 represent a specimen in our collection. Like others of the same type, it is perfectly unworn, which would seem to indicate that it was rather a symbol of command than a weapon. We have seen one, however, in the collection of Dr. Clement, which bears distinct traces of use.

Figure 13.

Figure 14.

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* It would be more simple to suppose that the piercing was effected by means of a cylinder or hollow tube. But this would imply the use of metal, and would assign our hammers to the age of bronze, while as yet we know them as only pertaining to the age of stone. A discovery has just been made of some very fine ones in the palafitte of Greng, on Lake Morat, where there exists not a trace of metal. M. Lachmann mentions not less than fifty of them at the station of Nussdorf, (see Keller, 6th Report, p. 217,) which pertains to the age of stone, though it is true that some have been found in the palafitte of Unteruhdingen which is of the age of bronze. From the researches of M. Ley we learn that at the station of Bodman (small lake of Constance) the hammer hatchets are limited to the upper archæological stratum, while they are wanting in the lower; whence the author feels authorized to claim for these primitive tribes a progress in civilization during the period of stone.

Neither are hatchets rare; we possess them by dozens. A hard stone was chosen, preferably an erratic pebble of diorite, serpentine, quartzite, or saussurite,* to which they managed to give an edge, (Fig. 15.) This was afterwards introduced into a socket made of buck-horn, which was itself cut in such a manner as to adopt itself to a handle of wood, (Fig. 16.) The sockets are quite frequently met with in certain localities, among others at Concise, but it is rare to find the two, (the hatchet in its socket,) and still more rare to possess the complete instrument, (hatchet, socket, and handle.) Dr. Clement has one of the most complete specimens in existence, (Fig. 17;) it was derived from Concise. At other times the stone was inserted at the extremity of a portion of buck-horn, which was pierced with a hole to receive a handle. The hatchet was supported against this handle, which prevented the wood from splintering. This instrument, which served at once as hatchet and hammer, (Fig. 18,) ranked, but a few months since, among the rare objects of our tenevieres. Now, thanks to the persistent researches of Dr. Clement, several dozens are known, representing the utensil in every state, from the rude outline to the complete instrument. At other times the hatchet is found simply attached to the extremity of a stag's antler; we know, however, but one specimen, being that in the collection of Dr. Clement, which is here represented. Along with the hatchets are found other

Figure 15.

Figure 16.

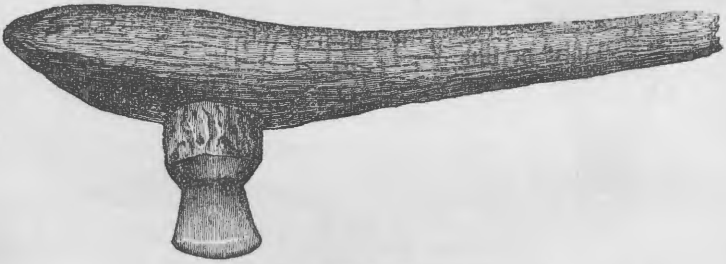

Figure 17.

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* As is justly remarked by M. Demour, it is impossible not to be struck by the sagacity with which these ancient people selected materials which, with the exception of the metals, unite in the highest degree the three properties of density, hardness, and tenacity, conditions essential to the use and duration of these implements. (*Comptes Rendus*, August, 1865.)

stones cut smaller, fixed in simple fragments of buck-horn, and serving rather for chisels or paring-knives than for hatchets, properly so called. That represented by figure 19 is of transparent nephrite of a fine leek-green color; it forms part of the collection of Dr. Clement, at Saint Aubin. Most of these singular stones are set in the same manner. *It is a characteristic distinction between our hatchets of the age of stone and those of anterior ages that the former are always worn down by sharpening on the grindstone, so as to present a very regular edge, which is never the case with the hatchets of Abbeville, of the caverns, nor those of the Kjoekkenmœdings of Denmark, which are simply cut by chopping.

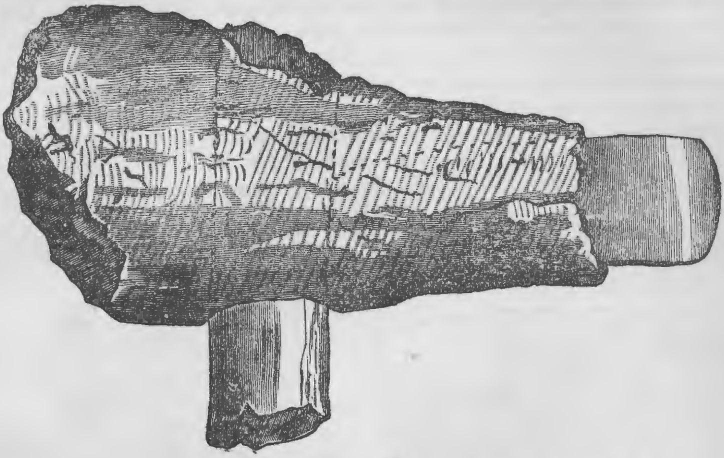

Figure 18.

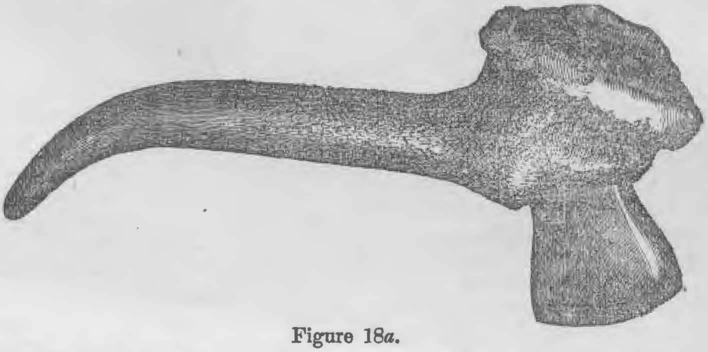

Figure 18*a*.

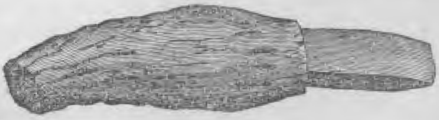

Figure 19.

Apart from these utensils, which are common to almost all savage tribes, we recognize, among our lacustrians of the age of stone, the beginnings of an art which attests the dawn of civilization. They manufactured pottery, somewhat shapeless and rude it is true, but which is not the less interesting, as well for its composition as its form and dimensions. It consists for the most part of large protuberant vessels, fashioned with the hand, the wheel of the potter being not yet known. The paste of which it is composed is but slightly homogeneous,

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*It has been recently remarked that, in general, none but small hatchets of stone are found provided with handles, which would seem to warrant the conclusion that the largest, some of which weigh as much as two pounds, were not used with handles, but immediately with the hand. (Lachmann, 6th report of Dr. Keller, p. 277.)

gray or black, never red, and always intermixed with small siliceous pebbles, doubtless to guard against the defects of unequal and imperfect baking.

In default of silex they also employed limestone, and sometimes fragments of shell, or even charcoal. The large vases are protuberant, the small ones cylindrical, more or less contracted or rounded towards the base, but without being conical like those of the age of bronze; hence the earthen supports or rings, which, in that period, were used to keep the vessels upright, do not as yet occur in this. It is not unusual to distinguish marks of the fingers, especially at the base, (Fig. 19*b*.) Others are furnished with a sort of handle or rude projection intended to facilitate the handling or carriage, (Fig. 19*a*;) this latter is rarer. In the specimen given the impression of the fingers of the potter may be recognized. With regard to the black color, it was obtained perhaps by smoking, or, as is more probable, by introducing grease into the paste, as is done, we are told, by the potters of Peru.

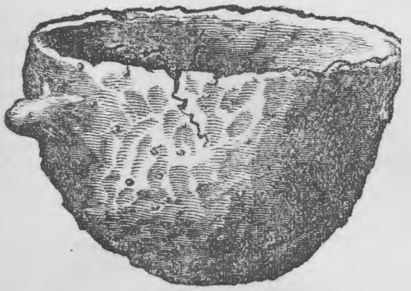

Figure 19*a*.

Figure 19*b*.

Figure 20.

The prints of the fingers are very small, which would seem to indicate a race of diminutive size, unless we are to suppose them to have been made by women, and we know that among certain tribes, (the Kabyles, for instance) it is the women who manufacture the pottery. From time to time vessels are met with composed of a paste less coarse and exhibiting rudiments of ornamentation, (Fig. 20.)

There is sufficient reason for supposing that these jars served, like those of the age of bronze, for the preservation of food. Along with those earthen vessels occur others, not less interesting and more characteristic, constructed out of the horns of the stag. They hollowed the horn at the place of its insertion, where it becomes enlarged, and formed of it small and not ungraceful vessels, usually pierced with a hole on one side; of these, several specimens, derived from the lakes of Neuchâtel and Moossedorf, are known to us; we select for representation that from the collection of Dr. Clement, (Fig. 20*a*.) This savant has discovered at Concise another small vase, made from a piece of stag's horn, but of different form, and thus far unique in its kind. It is cylindrical, with two small handles, doubtless intended for the passage of a strap for suspension.

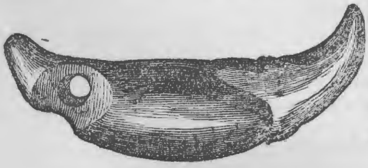

Figure 20*a*

A certain number of utensils are scattered on the surface of the tenevieres, such as hatchets, lances, hammers, sometimes also fragments of coarse pottery, which are then covered with a tufaceous incrustation formed by the perennial deposit of the lake.* Most of the objects, however, are buried in the soil, and to obtain them it is necessary to dig and dredge in the teneviere. It is thus that Dr. Clement proceeds, in order to collect the varied objects with which he every day enriches his admirable collection.

These objects are not dispersed at hazard in the interior of the teneviere. They are found preferably at a depth of from 1½ to 2 feet, intermingled with fragments of cut wood, bits of charcoal and bones of animals, which constitute a kind of repository, like those of fossils in geological formations. This is the archæological stratum. It is not unusual to encounter several of these strata or repositories in the same teneviere. Dr. Clement has recognized two very distinct ones in the teneviere of St. Aubin, while it appears that M. Messikommer has distinguished even three at Kobenhausen. (See 6th Report of M. Keller, p. 247, for a profile of this station.)

Stones for grinding, (commonly known as mills,) which occur in considerable numbers in the tenevieres, many having a diameter of 60 centimetres, indicate that the grain was triturated with the help of rounded pestles. These last, as well as the mill-stones, were of granite or grit, never of limestone. It was scarcely to have been expected that we should have discovered the products of this primitive contrivance; nor have the stations of our lake furnished anything of the kind. But it is otherwise in eastern Switzerland, where have been found the remains of the bread eaten by our predecessors, and which have been preserved by carbonization.

In all likelihood some traffic was carried on with the neighboring countries, especially with those situated on the borders of Jura, whence doubtless the silex was derived. In the stations of eastern Switzerland it is usual to cite, as proof of a local commerce, the presence at Kobenhausen of micaceous schists, of parti-colored grit of Rheinfelden, of crystals of the Alps, of asphaltum of Valde Travers, of white marble of Splügen, &c. M. Keller even describes and delineates a small vase of asphaltum found at Kobenhausen. (6th Report, p. 251.) But we cannot share the opinion which attributes extensive commercial relations to the tribes of the age of stone. In support of this opinion are cited the hatchets of nephrite, of which numbers are found at Concise and other stations of that epoch; and as this stone now comes to us from the east, it has been inferred that the tribes of the remote period in question trafficked with Asia. But it should be remembered that the greater part of the hatchets which are assumed to be nephrite may very well be only varieties of indigenous rocks, proceeding from siliceous veins in the serpentine, and whose depository might be found, according to M. de Mortillet, in the higher Maurienne.† It seems to us very difficult to admit that so distant a commerce should have been restricted to the exchange of certain stones which, after all, are not very superior to common silex, while the east might have furnished objects of far greater utility, particularly metals.

It is proper, however, to mention here a recent communication of M. de Fellenberg to the Society of Natural Sciences of Berne,‡ in which that accomplished chemist gives an account of a series of analyses which he has made of five fragments of nephrite from the lakes of Switzerland, three of which are

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[*] There is some interest in regard to authenticity in not removing this crust, even where it impairs the beauty of the object.

[†] *Materiaux pour l'histoire positive et philosophique de l'homme*, 1865, p. 231.—M. Nauman (*Elements de Mineralogie*, p. 305) mentions nephrite as occurring among the erratic blocks of Saxony, (Schwemsal near Duben,) which are known to have proceeded from Scandinavia.

[‡] *Mitteilungen der Bern. naturforschenden Gesellschaft*, 1865.

from Meilen, on Lake Zurich; one from Moosseedorf, in the canton of Berne; and one from Concise, on Lake Neuchâtel, with the following results:

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Silic acid	57.10	56.50	56.90	58.89	56.14
Alumine	....	....	....	22.40	0.48
Magnesia	20.60	20.09	20.37	1.28	22.68
Lime	12.76	13.27	12.94	3.12	11.12
Oxidulated iron	6.30	6.75	7.06	1.66	4.66
manganese	0.65	0.42	0.67	....	1.13
Oxide of zinc	....	....	....	0.73	....
Soda	....	....	....	12.86	....
Potassium	....	....	....	0.49	....
Water	3.25	3.50	2.80	0.20	3.72
	100.66	100.53	100.74	101.63	99.93

It follows that four of these fragments, Nos. 1, 2, 3, and 5, have nearly the same composition; while No. 4, from Moosseedorf, would correspond to the green jade or jadeite of M. Damour. It would represent a new bi-silicate in the group of feldspaths, or alkaline silicate of aluminum, resembling in many respects oligoclas. On the other hand, the four identical fragments agree in a striking manner with the analyses which M. Scheerer* has made of the true oriental nephrite, and especially with his analysis No. 7, which yielded him: Silic acid, 57.10; alumine, 0.72; magnesia, 23.29; lime, 13.48; oxide iron, 3.39; oxide mangan., 0; water, 2.50.

M. de Fellenberg thinks that, in view of this remarkable correspondence, and to the extent that chemical composition can be made subservient to ethnography, it must be admitted that the hatchets of Meilen and Concise are of the true nephrite, while that of Moosseedorf is the green oriental jade. "It might, indeed," adds M. de Fellenberg, "be asked whether the nephrites of our lacustrian stations be not of Alpine origin, like the serpentine hatchets which accompany them, since we find in the Grisons and in Valais the same masses of serpentine talcose and chloritic schists which in New Zealand accompany the true nephrites, and of which these last are but emanations, (by segregation.) But as yet the facts have not confirmed this hypothesis, so that, until proof of the contrary, the hypothesis of the oriental origin of the nephrite appears the most just and probable."

Without making pretensions to combat deductions drawn from investigations so precise and conscientious as those of M. de Fellenberg, we cannot help observing that if the Asiatic origin of nephrite in itself presents difficulties, by attributing to the tribes of the age of stone commercial relations with India, Persia, or Arabia, these difficulties appear to us still greater, if not, indeed, insurmountable, when the question concerns the pursuit of this nephrite in China, New Zealand, or New Caledonia.† As regards the fragments of white coral which have been found at Concise, they might well pertain, like the fragment of amber at Meilen, to the age of bronze, some vestiges of which exist in each of these stations.

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* Rammelsberg, *Mineralchemie*, p. 777, note. The green jade should not be confounded with saussurite, which, according to M. Damour, presents the following composition: Silex, 0.5069; alumine, 0.2565; oxide of iron, 0.0250; lime, 0.1061; manganese, 0.0576; soda, 0.0464. The chief difference consists in the much less considerable quantity of soda. (Letter of M. Damour to Dr. Clement.)

† It is from New Caledonia that proceed the beautiful translucid nephrites, of a delicate green color, which form an ornament of the museum of Montpelier. As we go to press, the *Comptes Rendus* of the Academy of Sciences of Paris (25th and 28th August, 1861) bring us, on this much-controverted question, a remarkable memoir of M. Damour "On the composition of the hatchets of stone found in Celtic monuments and among savage tribes," (*Comptes Rendus*, T. LXI,) which we cannot but commend to the attention of our readers.

Meanwhile, the tribes of the age of stone were not reduced to the sole care of providing for their existence. However mean might be their arms and utensils, the requirements of personal decoration were not wholly neglected, as is attested by certain ornaments made of stone or of bone. These could of course be but very simple, being the teeth of carniverous animals pierced with a hole, and worn, doubtless, in the manner of a collar, intermingled with disks or beads of bone or buckhorn similarly pierced, (Fig. 20.) Pins for the hair occur, not deficient in a certain degree of elegance, nor differing much from those worn at present, as is witnessed by that represented above, and which comes from the station of Concise, (Fig. 11.) At other times, a pin was cut from the rib of a stag or roe, which, besides the head, had a protuberance pierced with an eye. (Fig. 20*b*.)

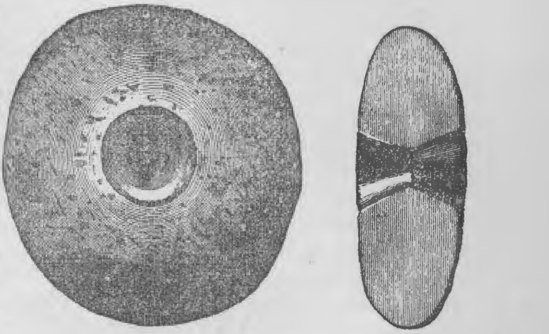

Figure 20*a*.

Lastly, domestic industry is attested by a quantity of small disks pierced with a hole, which we believe to have been spindle whirls rather than weights for fishing nets. (Fig. 21.) However this may be, it is not superfluous to remark that at the lake of Neuchâtel these disks are always of stone (preferably of molassic sandstone; sometimes of limestone) in the stations of the age of stone, while they are of baked earth in the palafittes of the age of bronze.* (Fig. 21.)

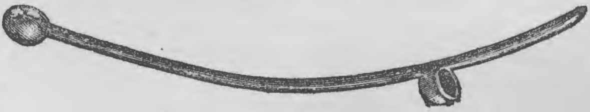

Figure 20*b*.

If the tenevieres of our lake and of western Switzerland have not yet afforded a specimen of the thread which was spun by means of these whirls, such is not the case with the canton of Zurich, where are found not only skeins of thread, but numerous remains of webs, tissues, and nets, all of flax. It does not appear that wool was ever spun.

Nor was the ground left uncultivated in the age of stone, as is attested by the remains of cereals which are found here and there in our tenevieres. M. Gillieron has collected in the archæological stratum of the Pont de Thielle very fine grains of wheat, carbonized like the peat which surrounds them. The stations of eastern Switzerland, and especially that of Robenhausen, on Lake Pfæfikon, are in this respect of the highest interest. The conditions are here so favorable to the preservation of vegetable products that it has been practicable to make ample collections of fruits of all kinds—apples, cherries, beech nuts, seeds of the strawberry and raspberry, and large quantities of the water chestnut, *(Trapa natans,)* which must have been common in the lakes, while at present it is found only at two points north of the Alps—near Langenthal and Elgg. In the bread found there the grain is but imperfectly crushed, as in the pumpernickel of Westphalia, so that it is possible to recognize the species of cereal of which it is composed. The bread of Robenhausen is of wheat. Of late that of millet has been also discovered. All these vegetable remains have been

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* Quite recently Dr. Ullersberger has collected some of these disks in baked earth at the station of stone, at Ueberlingen, on Lake Constance, but their shape is different from that of the age of bronze; they are true disks, slightly convex, while those of the age of bronze are conic.

recently described in a remarkable treatise by Professor O. Heer, (*Die Pflanzen der Pfahlbauten*, Zurich, 1865,) to which we would refer our readers, extracting from it only the annexed group, which represents different species of cereals cultivated in the age of stone. (Fig. 21*a*.)

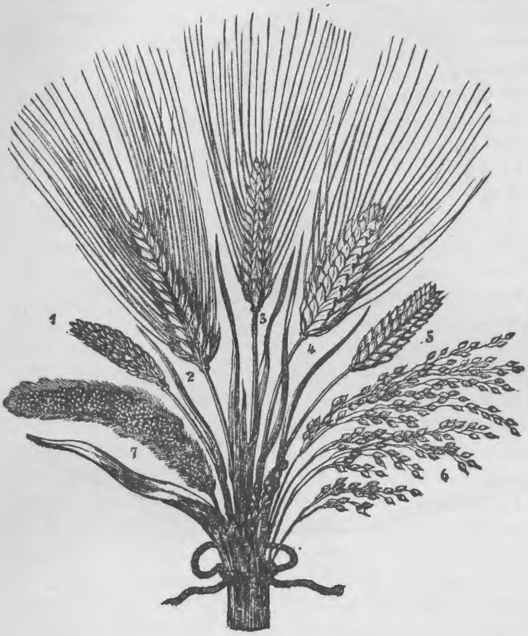

Figure 21*a*.

If thus skilled in the art of cultivating cereals, the possession by the inhabitants of implements of tillage follows by necessary implication; and it is from the station of Robenhausen again that the first revelation in this respect might have been anticipated. M. Keller, in effect, has just given us the description, accompanied by a design, of an instrument formed of a portion of a stag's horn, fixed in a handle of wood, and so cut as to serve for a mattock on one side and a hook on the other, (Ferd. Keller, 6th Report, page 249,) while the same teneviere has yielded other implements of husbandry, made of maple wood, and remarkable for their execution when we consider the tools of that epoch, (Keller, 6th Report, page 249.) All this implies conditions very different from those of the populations of the age of the reindeer, who were only hunters, or of those of the kökkenmödings of Denmark, who lived upon shell-fish collected on the sea shore.

The inhabitants of our tenevieres had fixed habitations and much cattle. They made provision for winter; they took thought for their raiment and had regard to their toilet; they were expert in the art of spinning and weaving. They were no longer, therefore, in the savage state.

Let us remember, in the last place, that, according to the latest researches, the tenevieres often comprise several archæological strata, superposed and separated by deposits of peat, &c., which attain even a metre in depth (at Robenhausen.) It may be possible, some day, through a close study of these deposits, to estimate the duration of such intervals. We know with certainty that it must have been very long. It was not time, then, which was wanting to the tribes of the age of stone in order to arrive at the degree of civilization, humble no doubt, but yet remarkable, which is attested by the remains of their industry and culture.

II.—AGE OF BRONZE.

There exists a notable difference between the palafittes of the age of stone and those of the age of bronze. The latter, which are at once more extensive* and more numerous, are found at a greater distance from the shore; their depth is consequently more considerable, generally from 3 to 5 metres below mean water. This is observable at the lake of Constance as well as our own. In a letter of M. A. Senoner to M. G. de Mortillet it is said: "There is a great difference between the stations of stone and those of metal; the former approach the shore more or less nearly, while the latter are distant from it about 330^m." Sometimes they are very near the tenevieres, being separated by a space of only a few metres, as for instance at Auvernier. The piles are more slender, frequently trees cleft in four parts, scarcely exceeding 12 to 15 centimetres in diameter; instead of being on a level with the bottom, they rise from 30 to 60 centimetres above it, which allows of their being easily recognized, notwithstanding their greater depth. As they are simply sunk in the ground, they may be occasionally withdrawn, when the wood is not too much decayed. Their number is so considerable that at some stations they may be counted by thousands, now grouped by six, ten, or twenty, now arranged in several rows which seem to tend towards the shore, thus affording a proof that the question really regards constructions on pile-work elevated above the water and communicating with the shore by avenues or foot-bridges, and that these are no artificial islands, as the tenevieres or steinbergs of the age of stone might possibly be.

It is in the intervals of the piles that we find the utensils, arms and habiliments of every sort which characterize this epoch, as well as the earthen vessels, of which there existed of old large deposits at certain stations, among others at Auvernier.† This pottery, although prepared in the same manner with that of the preceding age, without the help of the wheel, is distinguished by a much greater variety of form and outline. Like that, too, it is black, and it is only exceptionally that the surface is brown or red, tending to show that the baking was not conducted in furnaces but in the open air.‡ If the paste of the large vessels is still coarse and characterized by the same mixture of small siliceous pebbles, it is not so with that of the small ones, which is fine, homogeneous, and often coated with a glaze of graphite. We are struck at the same time with the elegance of form and fine proportions of these vessels, (Figs. 22, 23.) It is not unusual to meet with rudiments of design, described with a point, which represent sometimes chevrons or small triangles, sometimes simple rows or points traced around the neck or handle, (Figs. 24, 25, 30.) Most of the vases

Figure 22.

Figure 23.

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* It is not easy to assign the extent of these palafittes on account of their border being often very sinuous. Some comprise a surface of several *hectares*, especially those on the southern shore of Lake Neuchâtel. That of Auvernier is estimated to contain some 50 *ares*.

† An aged fisherman has told us that, when a child, "he sometimes amused himself by thrusting, with a long pole, at these old pots; that there were great heaps, *real mountains* of them."

‡ According to Brogniart, *Traité des Arts céramiques*, p. 487, this mode of fabrication still exists in certain parts of France. The pottery is fashioned by the hand, after which it is baked in the open air by means of the flame of heaps of fern which surround it.

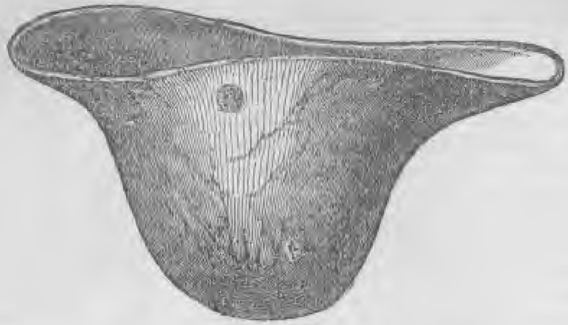

Figure 23 *a*.

Figure 24.

Figure 25.

Figure 25 *a*.

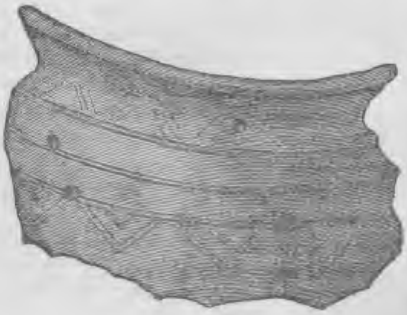

Figure 25 *b*.

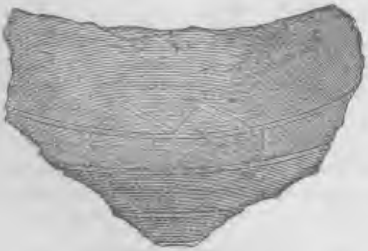

Figure 25 *c*.

Figure 30.

have a conical base, so that to keep them upright it would be necessary to sink them in the sand or earth, or to place them in circles or rings of baked earth de-

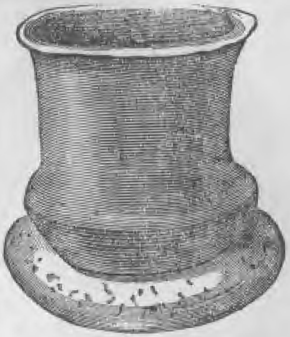

Figure 26.

signed for this use, (Fig. 26*) Even the large vessels of coarse paste are not destitute of ornament; they frequently have. the neck encircled by a kind of twisted belt which must have been adapted to the hand, for the trace of fingers is still discernible on it, (Fig. 27.†) Porringers are also found (Figs. 28, and 29,) as well as dishes, and vessels pierced with small holes, evidently intended for draining liquid substances and serving perhaps for the manufacture of cheese, (Fig. 31.) The spindle whirls are very numerous and often artistically fashioned with a hole in the middle; they are no longer of stone, as in the preceding epoch, but of baked earth, (Figs. 32 and 33.) It is not difficult, with a little practice, to distinguish the pottery of the age of bronze from that of the age of stone, which is always more shapeless. Hence it is that, from our first researches at the lake of Bourget in 1861, we did not hesitate to refer this station to the bronze period, although as yet no object in that metal had been met with. It is not rare to find vessels still containing provisions, for the preservation of which they were no doubt intended. From one of these we have obtained ap-

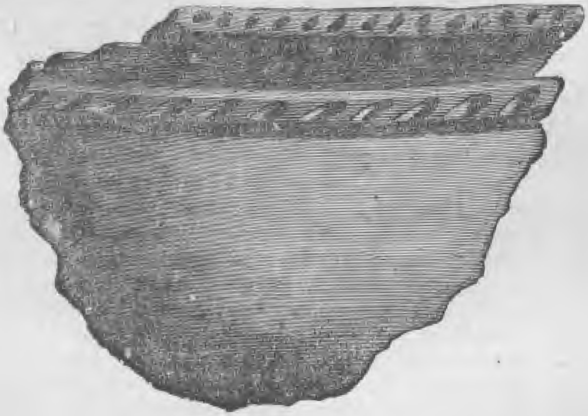

Figure 27.

Figure 28.

Figure 29.

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* It is probable, however, that this conic form was only applicable to vases of moderate or small dimensions. The large vessels, though protuberant and narrowed towards the base, have always a plane bottom; and, indeed, the earthen rings do not imply large vessels, none of them, to our knowledge, exceeding 20 centimetres in diameter.

† In respect to the designs, these vessels recall in several features those found in the tumular chambers of the dolmens of Bretagne, which have been described and figured by Dr. Closmadenc in the *Revue Archæologique*, 1864. In the dolmens, however, those conical forms so characteristic of the age of bronze are not met with, nor the rings or circles destined to support them.

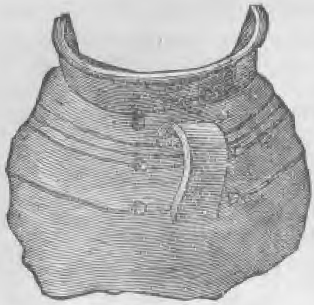
Figure 31.

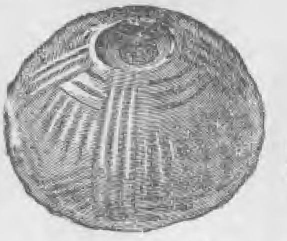
Figure 32.

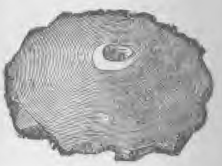
Figure 33.

ples, cherries, wild plums, and a quantity of hazel-nuts. At Auvernier these vessels do not form part of the great heap, but are found at other points of the station. We may also mention here, as proper to the age of bronze, the lacustrian crescents of baked earth, which will claim our notice hereafter.

UTENSILS OF METAL.

The utensils of bronze are remarkable for their fine state of preservation in all our palafittes. The hatchets are numerous, measuring from 12 to 20 centimetres, and weighing from 300 to 750 grammes. Most frequently they are perfectly undamaged, without any trace of wearing, as if they had never been in use, though marks of the hammering, by which the edge was widened, are often visible. They are of several types. Some have ears carved on each side, in such a way as to present a double socket intended to receive a forked handle, which had probably an elbow. These are the most frequent, and are often provided with a small metallic loop, which served, doubtless, to suspend them to the girdle, (Fig. 34.) Sometimes the two points of the upper extremity are bent round, so as to touch one another, and to form a lunule, destined, doubtless, to receive a rivet which passed through the handle; this peculiarity is presented only by hatchets of a large dimension,* (Fig. 35.) A second type has only the rudiments of ears, but the edge is considerably enlarged. These are rather paring-knives, managed with the hand, like those of our curriers, than hatchets; perhaps they had not even a handle. M. Morlot terms them knife-hatchets (*couteaux haches;*) with us they are quite rare, our lake, particularly, having as yet furnished but few samples, one of which is here represented, (Figs. 36 and 37.) M. Forel has collected many from the lake of Geneva, at a particular station near Morges. Others, of a somewhat different form, have been found in the fields and forests of our vicinage. The sample of figure 38 is borrowed from the work of M. Troyon, and represents a specimen found


Figure 34.


Figure 35.

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* It is scarcely necessary to remark that the ears as well as the points must have [illegible] straight at issuing from the mould, and been bent afterwards.

under the stone at Niton, towards the middle of the XVIIth century.* Others, again, have a perfect socket, sometimes circular, sometimes square, with a loop of suspension, (Fig. 39.) This form, very common in France, and to which is applied more specifically the name of *celt*, is rare in our palafittes, though, according to Nillson, one of the most common in Scandinavia. The specimen given comes from the lake of Geneva. Lastly, there occurs in the museum of Neuchâtel a fourth type, represented by a single specimen. It has the ears broad and recurvated, but arranged in the plane of the edge, instead of being perpendicular to it as in the true celts, (Figs. 40 *a* and 40 *b*.) This very rare form had not been observed hitherto except in Ireland and Germany.† Knives are nu-


Figure 36.


Figure 37.

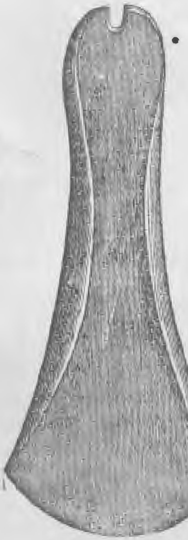
Figure 38.


Figure 39.


Figure 40 *a*.


Fig. 40 *b*.

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* The more rare these instruments with us, the more common do they appear to be in Hungary, where they are found associated with the hatchet, furnished with a socket, which is not less abundant, especially that with a round socket.

† Kemble, *Horæ ferales*, tab. 9; Lindenschmidt, *Alterthümer unserer heidnischen Vorzeit*. M. de Mortillet has lately noticed it at the museum of Vannes, derived, no doubt, from a dolmen. M. Lachmann has also obtained a specimen from the palafitte of Unteruhldingen, on Lake Constance. In view of this variety of forms, all well characterized, of the same utensil, there might be room. perhaps, to make an application of the method used in ineralogy, and designate each type of hatchet by a proper name. If this suggestion be adopted e would propose the following titles: The Keller hatchet for that of figure 34; the Schwab for that of figure 35; the Morlot for that of figure 36; the Troyon for that of figure 40; the ertrand for that of figure 39; the l'Haridon for the same with a square socket; the Mortillet for the small hatchet with a socket, which is very common in France, but not yet found in our palafittes. The same method might be applied with advantage to the different types f pins, vases, swords, &c. The small hatchet, which we have named after M. Mortillet, is njectured by that savant to have been a sort of votive offering. The Commission of Toography of Gaules has just published a proposed classification of bronze hatchets, in which ot less than twenty-two types are distinguished; the six forms, whose figures we have given, epresenting therein as many distinct types. (*Revue Archæologique*, p. 58, pl. 1.

merous, generally small, but always elegant, the blade measuring in length from 10 to 20 centimetres. Most are furnished with a tongue which entered a handle that has not been preserved, but must have been of wood or horn, (Fig. 41, 42;) others have a handle of metal, (Fig. 43;) others, again, are provided with a socket, (Fig. 44.) Two types are distinguishable in the blades of these knives: those which become gradually thicker from the edge to the back, (Fig. 42,) like our ordinary knives, and those which have the back abruptly enlarged. The latter served, doubtless, for a special use; both one and the other are frequently embellished with designs on the side and back.*


Figure 41.


Figure 42.


Figure 43.

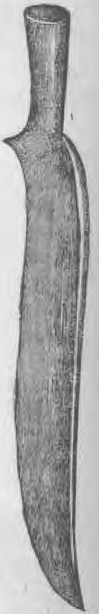
Figure 44.

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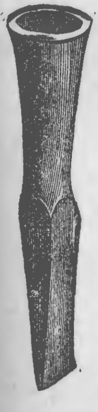
* From the fact that the original of figure 41 is covered with a fine patina, it might be inferred that this specimen was not derived from a palafitte, but had been preserved in the ground. In that case, it would pertain to the age of iron.

In many stations of our lake, particularly at Auvernier and Cortaillod, reaping hooks have been found in great number; they are small, the largest not exceeding fifteen centimetres, measured lengthwise. But they are skilfully elaborated, and usually strengthened by one or more concentric ribs. The stock is scarcely narrower than the blade, and so contrived as to adapt itself to a handle of wood or horn. Some evince artistic design, and are ornamented with dilatations in the middle of the curvature, as the specimen (Fig. 45) taken from Chevroux will testify.

Figure 45.

In connexion with the knives, a very recent discovery should be mentioned: that, namely, of small blades, wide and very thin, which remind us of the razors of the age of iron, to be noticed hereafter. The annexed specimen, (Fig. 44*a*,) taken from the palafitte of the Moulin de Bevais, (Lake Neuchâtel,) is from our own collection, and is provided with a notch on the back, which facilitated its management.

Figure 44*a*.



Real chisels are also found, destined, no doubt, to the same use with those of our own joiners, but with the difference that, instead of a shank, they are provided with a socket, like the hatchets of the third type. We possess a very complete one, derived from the station of Auvernier, which measures ten centimetres, (Fig. 46.) There occurs also in the collection of M. Schwab, at Bienne, a kind of hammer with six faces, having a length of six centimetres by a diameter of four. This instrument likewise has a socket, (Fig. 47.) It remains, lastly, to mention the fish-hooks of bronze which are found at many of our stations. All are barbed, and though usually small, like those now used with a line, there are larger and stronger ones for the capture of heavy fish. We possess one from Gauderon, which measures not less than twelve centimetres in length.

Figure 46.

Figure 47.

ARMS.

These, for the epoch under review, are swords, poniards, lances, and arrows. The first are not numerous in our lake. The most remarkable one we possess was discovered nearly forty years ago, in the midst of the station of Concise, by Captain Pillichody, and was deposited in the museum of Neuchâtel, where it attracted the attention of numbers of the curious, but without stimulating new investigations, until the day when a happy intution of the savant of Zurich kindled the torch which guides us to-day. The sword in question is not one of the largest; it measures fifty-nine centimetres, (Fig. 48.) The blade, but little contracted above the hilt,* is enlarged in the middle, and furnished with four grooves nearly parallel. The hilt, terminated by a double volute, is composed of a metal redder than the blade and softer. But what is most significant is the smallness of the hilt, which measures only seven centimetres, and supposes a hand much smaller than an ordinary one ; hilts of such dimensions are scarcely even found in the sabres of India.

Neither are the poniards numerous. One has been found at the lake of Bienne, the figure of which we borrow from the work of M. Keller, (Fig. 49.) The blade was fixed to the hilt by means of riveted nails. These arms appear to have been more abundant in the stations of the lakes of Italy, and might, probably enough, have pertained to the early age of iron. Of the lances, the points are skilfully wrought, (Fig. 50,) and measure from ten to seventeen centi-

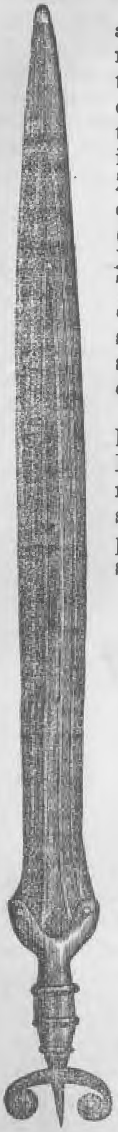


Figure 48.

Figure 49.

Figure 50.

Figure 51.

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* In other specimens this contraction is considerable. The swords and poniards of bronze have, like the hatchets, been made the subjects of classification by the Commission of Topography of Gaules. In this eleven types of the poniard are distinguished; that whose figure we give (Fig. 49) approaches a form quite common in Greece, Italy, and Gaul, and recalls the blade with which the priest of Mithra slaughters his victim in most of the known bas-reliefs. Of the sword the types are fourteen; the specimen here represented pertains to section L, being the sword shaped like a sage leaf, whose hilt is furnished with antennæ curved after the manner of the horns of Ammon. (*Revue Archæologique*, 1866, p. 180, pl. VI.)

metres. The wings are not very large, but the centre expands into a rounded prominence, which gives great solidity to the weapon. The socket, whose border is usually embellished by some parallel lines, is large, so as to be capable of receiving a stout handle. Arrows (Fig. 51) are thus far not numerous. They are small, scarcely measuring from three to four centimetres, triangular, and frequently furnished with barbs more or less divergent, being of the same form with those of the age of stone. Like the latter, most of them were attached to the wood by a shank, and it is only in one specimen, found at Estavayer, that a socket occurs. Those with a shank come from the station of Font, or from that of Nidau.


Fig. 52.

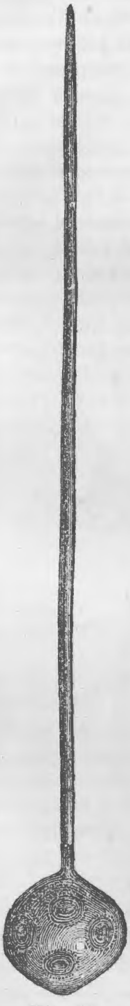
Fig. 53.


Fig. 54.

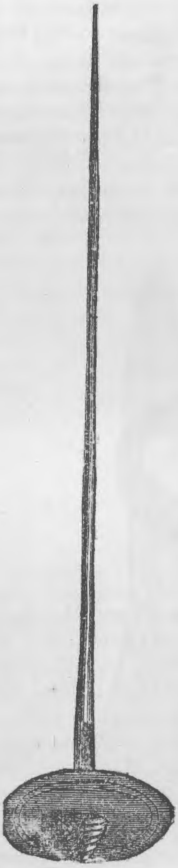
Fig. 55.


Fig. 56.


Fig. 57.


Fig. 58.

ARTICLES OF ATTIRE.

Objects of luxury or personal adornment are at least as numerous as utensils and arms, thus proving that the tribes of the age of bronze had arrived at a certain degree of ease and prosperity. In the collections attention is attracted by pins for the hair, bracelets, pendants for the ears, and certain engraved plates, serving, probably, as amulets. Clasps, on the other hand, which are so abundant in subsequent epochs, are as yet unknown at our stations of bronze.

Pins for the hair are very numerous. The lake of Neuchâtel has itself furnished many hundreds, and among them all we have not yet met with two which were exactly alike, and could therefore be referred to the same mould. Most of them are adorned with designs more or less complex, few being wholly destitute of ornamentation. In the number several types may be distinguished which are characterized by the form of the head, as may be judged of by a comparison of the annexed figures. Some have a round head (Figs. 52 and 53) and are sometimes of considerable size. We possess one 34 centimetres in length, while M. Troyon mentions specimens of 49 and even 57 centimetres. The head is usually open-worked with circular holes, into which were fitted studs of the metal in relief, perhaps also small plates of some other metal. In this case the pin traverses the head and is often detached from it. At other times the head is massive and without ornament, after the model of the pins of bone in the age of stone. Pins with a flat button are not less abundant, this button being sometimes very small, (Fig. 54,) sometimes of considerable size, (Fig. 55.) It is in some a mere enlargement of the stem, (Fig. 56,) and is occasionally repeated, so as to furnish pins with two and three buttons, (Fig. 57.) With these pins for the hair we would not confound certain very simple stylets, whose flattened extremity is merely convoluted, (Fig. 58.)* It is probable that these objects served for some special use. We are induced to think so from the fact that we have found them at different times combined with small rings of bronze similar to those of which notice will be taken further on, and which we have reasons for regarding as the money of the epoch.

The *bracelets* testify a cultivated taste; we find them of every model, from the simple bracelet, composed of a bronze stem with a semi-cylindrical button at each end, (Fig. 59,) to the large bracelet covered with elegant designs, (Fig. 60;) the latter are more rarely found, and the finest of them were taken from an urn obtained from the pile-work of Cortaillod. These were six in number, all uninjured, and the designs as perfect as if they had just issued from the workshop of the engraver. M. Otz possesses no less than eight of them in perfect preservation, derived from this same palafitte of Cortaillod, which appears to have enjoyed a speciality of this ornament; among them we find one embellished with designs in the form of concentric circles.† Others are composed of several twisted strands of bronze, artistically connected, (Fig. 61;) others still of large and massy cylinders, bent, so as to touch by their extremities, and

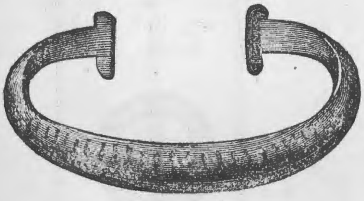

Figure 59.

Figure 60.

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* Better to evince the elegance of forms and beauty of the metal, we caused a number of hair-pins, derived from different stations of Lake Neuchâtel, to be polished, and exhibited them at the *exposition horlogere* of Chaux-de-Fond, 1863, under the title of *lacustrian jewelry*. An opportunity was thus afforded to those who take an interest in the subject of forming, by a comparison of the variety of models and perfection of designs, an idea of the care which the ladies of this epoch brought to their toilets.

† Judging from the slight depth of the engraving, these articles would seem to have been all cast. The last explorations have disclosed specimens of double rings, which were, perhaps, fastenings for the belt. We possess two specimens from the palafitte of Cortaillod (Fig. 62*a*.)

embellished with simple designs, (Fig. 62.) These were probably rings for the legs.* A considerable number of bracelets are so small that, unless we suppose them intended for children, it is difficult to conceive how they could have been passed over the hand, however small its dimensions. This, again, seems to confirm the idea that the race of the bronze epoch was a diminutive one.


Figure 61.


Figure 62.

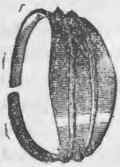
Fig. 63.

The *ear-rings* are variously fashioned; some are in the form of a thin plate, narrowing toward the point of suspension, (Fig. 63;) others are simple strands of bronze. We have specimens covered with a sort of enamel, the composition of which deserves an analysis. Some, again, are of slight threads, to which a small convolved appendage is suspended.


Figure 63*a*.


Figure 64.

The objects which we regard as *amulets* are small triangular metallic plates, furnished with an opening at the top, probably for suspension to the neck. Most of them are ornamented with designs, usually parallel to the sides of the triangle and in zigzag lines, (Fig. 64.) Others are composed of several branches, (Fig. 65;) while others are crescents borne upon a stem, (Fig. 66.)

The designs which adorn these several objects of dress, as well as the knives and other utensils, are generally much defaced, which leads to the supposition that they are only the reproduction of the figures of the mould which had been themselves engraved on the primitive model. But there are others which are of too sharp outline to have been a mere reproduction by moulding. These have been probably retouched by means of burins of bronze, of which a number have been collected.

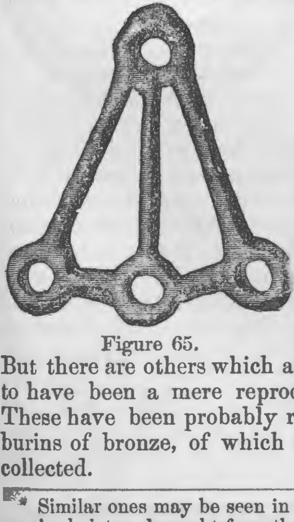
Figure 65.


Figure 66.

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* Similar ones may be seen in the museum of Wiesbaden, around the leg-bones of a woman's skeleton, brought from the environs of Hoechst.

It is not rare to meet with bronze buttons in the palafittes; we possess several from the stations of Cortaillod and Auvernier. They are convex, formed of a thin plate, and have a loop on the concave side. One, in the possession of M. Otz, from Cortaillod, measures not less than two inches in diameter, and is decorated on the border with designs characteristic of the age of bronze, similar to those on the vase represented by figure 25.


Figure 62 *a.*

The chemical composition is now an important element of the study of lacustrian bronzes. From numerous analyses made by M. Fellenberg, it results that the proportion of copper and tin is not so fixed as was at first thought, when, to a lacustrian bronze there was assigned 10 per cent. of tin and 90 of copper. The proportion of the tin, on the contrary, may vary from 4 to 20 per cent., according as the founders of the epoch experienced more or less facility in procuring that metal. These proportions are indicated more or less sensibly by the tint of the metal. The bronze into which enters a tenth part of tin (as in the metal of cannon) has the finest color; it is the most common in our lakes, and in its tint nearest approaches gold. When the tin is in less proportion, the metal is more red and soft; it takes, on the other hand, a light tint and becomes very hard when the proportion of tin sensibly exceeds the tenth. What is still more suggestive is the absence in objects of the age of bronze, of every other metal in any considerable proportion. If lead, iron, or nickel be occasionally found, it is in insignificant quantities, like impurities in the ores of copper. Hence M. Fellenberg concludes that when a bronze contains however inconsiderable a portion of lead or zinc, it cannot have proceeded from the age of bronze, but must date from a more recent epoch.* We will further add that, from the recent researches of Dr. F. Wibel† on the composition of ancient bronzes, the antehistoric artisans must have possessed the art of annealing bronze. In effect, bronze, to be malleable when cold, should not contain more than five per cent. of tin; when heated, it still yields under the hammer, though containing 15 per cent. of tin. To work bronzes which contain a greater proportion of tin, it is necessary to subject them to the process of d'Arcet, by cooling them suddenly. In this way, a malleability is given to them which they have not naturally, and which they do not acquire when they are cooled slowly. Now as, among the bronzes of the palafittes, there are found hammered specimens which must necessarily have been worked when cold, it follows that the art of annealing bronze must be nearly as ancient with us as the art of preparing it. It is surprising that, knowing this influence of cooling on the metals, the people of the age of iron should not have been led to the discovery of steel, which is but an inverse process.


Figure 66 *a.*

It remains to mention, in connexion with the stones for grinding grain which are common to the two ages, certain discoïd stones from 10 to 12 centimetres in diameter, furnished with a groove of more or less depth on their circumference, and respecting whose signification there is far from being an agreement,(Fig. 66*a.*) It has been asked if they were not pullies, especially as their two faces often present slight cavities, which would explain why they are always of hard stone, (quartzite, granite or diorite,) and never of limestone or molasse.

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* See Appendix at the end of this section.

† *Die Cultur der Bronzezeit*: Kiel, 1865, p. 24.

But if this were so, the groove should never be wanting; but this is not the case. Others have thought that they served for weights to support the warp in weaving; but whence, then, the necessity of choosing hard stones? Pebbles of limestone or molasse would have answered the same purpose. M. Troyon entertains the opinion that these discoïds were used in games, and relies upon the fact that in the collection of Pinelli (Rome, 1816, fol. 15,) a personage is represented as holding between his hands a similar disk, on the circumference of which is rolled a cord designed to assist in casting the stone. In this manner the advantage of employing hard stones might be explained, but we should scarcely understand the absence of the groove. Till now these stones appear to be especially characteristic of the palafittes of the age of bronze; they are not found in the ancient tombs, nor yet in the dolmens.

SKELETONS OF THE AGE OF BRONZE.

For a long time we possessed but a single authentic skull of the age of bronze, derived from the station of Auvernier. Though incomplete, for it wants the bones of the face, it is still sufficiently characterized to throw some light on the conformation of the race to which it belonged. It is at once small, thin, elongated and remarkably narrow, especially in the middle region, which begins to contract even from the middle of the parietal bones. These bones present, moreover, a very singular curvature, being, as it were, elbowed in the middle. The occiput, on the other hand, is extraordinarily developed. This, it will be seen, is not a favorable conformation. Unless an individual exception be supposed, we must conclude that the race was feeble and inferior. The diminutiveness of form is further corroborated by the smallness of the hilt of the swords, which has been noticed above.

In the course of last year, our skilful explorer, Benz Kopp, has withdrawn from beneath a beam partially carbonized, among the piles of the same station of Auvernier, a skeleton much more complete. The skull, particularly, is almost entire. It pertains, like that from Meilen before mentioned, to a child, as is testified not only by the loose sutures, but also and chiefly by the dentition. The molar next to the last has but come through, and the canines are seen at the bottom of the alveoli, which indicates an age of about eight years. The skull is small, elongated, the front very low and narrow, but, apart from that, well formed, without exaggerated prominences, which is to be attributed perhaps to the immature age. MM. Rutimeyer and His assign it to their type of skulls of Sion, the most widely disseminated in the ante-Roman epochs, (*Crania Helvetica*, p. 37.) The races of domestic animals do not appear to have varied from the epoch of stone to that of bronze.

INDUSTRY OF THE AGE OF BRONZE.

The men of this age, however diminutive in size, had not the less arrived at quite an advanced degree of civilization. From the age of stone to the age of bronze, there is a manifest progress. This progress is due, beyond all, to the introduction of the metal which, by endowing the lacustrian colonists with better arms and better utensils, had for its necessary result the augmentation of their security and comfort. Once in possession of arms of bronze, they must have sought completely to appropriate this element by preparing it for themselves. They did not delay to manufacture bronze at home, as is attested by the matrices of hatchets collected from Lake Geneva and now in the collection of M. Forel at Morges.* This was the commencement of the industrial arts. And no sooner, doubtless, was what is necessary provided for, than luxury made its appearance; and the ornaments and attire which have been preserved to us

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*These matrices are of bronze; there are others of clay, and among them fragments of moulds for bracelets may be seen in the collection of Dr. Clement, taken from the palafitteof Estavayer.

prove that the artists of the epoch were wanting neither in taste nor skill.* This taste is evinced even in common objects; witness the elegant forms of the earthen vessels and utensils, and the care which was bestowed on the decoration of such objects as knives and reaping-hooks, though with designs, it is true, very simple and monotonous. Strange that these tribes did not, like the aborigines of the caverns of Perigord, conceive the idea of imitating nature in their ornaments, but shut themselves up within certain arbitrary and traditional lines, as do still the Kabyles of our own day. If it were allowed to compare them with any modern people, we should say that their stereotyped manner somewhat reminds us of that of the Chinese.

DESTINATION OF THE PALAFITTES.

The distribution and state of preservation of the antiquities in the interior of the palafittes of the age of bronze are not without importance. It is evident, from a mere inspection of the objects collected at no matter what station, that we have before us no rubbish which might be lost, without being regarded. They have not fallen into the water at hazard, any more than the quantity of vessels which are accumulated at certain points, or the jars of provisions which are drawn up uninjured. It has been said that they were hoarded beneath the water by some violent cause, by a defeat, for instance, in which the inhabitants were overwhelmed with their most precious effects, their arms and provisions, under the burning ruins of their cottages. But in that case we ought to find their skeletons beside the bones of their animals. In view of this difficulty, and yet others which the idea of *habitation* involves, we would ask if, perhaps, we have not to deal with the question of simple magazines destined for utensils and provisions, and which have been destroyed by the flames, as seems to be indicated by the traces of fire frequently exhibited as well by the wood-work as the earthen vessels. It would thus be explained how it is that the objects in bronze are almost all new and the vessels accumulated at single points. This hypothesis seems to be corroborated by the opinion of some of our most experienced seekers of antiquities, who maintain that there is no chance of finding anything of value except where the timbers are charred, and that time is lost in exploring palafittes where the wood is not carbonized. It is, at the same time, no unusual thing to find in the palafittes utensils which have been deformed by the action of fire.

LAND-HABITATIONS OF THE AGE OF BRONZE IN SWITZERLAND.

There is every reason to believe that there existed simultaneously habitations on *terra firma,* nor could it be otherwise if the palafittes are admitted to have been simply magazines. The supposition of such habitations, based on the considerable number of celts and other bronze objects which are found not only in the woods and fields of our environs, but in a number of other localities both in Switzerland and foreign countries, has been confirmed by the recent discovery of genuine dwellings, containing the same utensils with our lacustrian stations, at Ebersberg in the canton of Zurich,† as well as at other places.

Dr. Clement having explored last year in the environs of Gorgier, canton of Neuchâtel, several mounds composed of erratic stones bearing traces of fire, found in one of them, intermingled with coals, different objects in bronze, among others a bracelet and some reaping hooks, the latter resembling in form those of Cartaillod and Auvernier, but differing from them by the presence of a quite

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* We have seen on the arm of a lady of our acquaintance a bracelet taken from one of our palafittes which would have reflected no disgrace on our own jewellers.

† *Mittheilungen der antiquar. Gesellschaft*, vol. vij, div. 7.—It has been supposed, on the ground of certain traditions respecting sacred lakes mentioned by ancient authors (Cicero, de nat. Deor., lib. iij, 30; Justin, xxx, 3; Strabo, Geog., vol. iv,) that the well-preserved objects of the palafittes of the age of bronze might be offerings which had been cast into the lake; but this is an hypothesis to which nothing yet known affords corroboration. (*Die Reste, &c.* 5th Report, 1862.)

prominent heel at the origin of the blade.* The chemical composition of the bronze is the same with that of the reaping-hooks of the lake.

Dr. Schild has recently discovered on the plateau of Granges, in the canton of Soleure, a series of these heeled reaping-hooks, accompanied by four very perfect paring-knives and a fragment of a sword, intermingled with calcined pebbles and earth, which leads him to conjecture that at this place there was once a workshop or foundry, more especially as the paring-knives are new, without the least trace of being worn. A not less characteristic specimen of knife-axe has just been found near Neuchâtel, in the defiles of Seyon. In general, this form seems more frequent on firm land than in the lacustrian stations. The same appears to be the case with the reaping-hooks with a heel, which have not yet been found in the palafittes, but which occur at Hallstadt. The question therefore is, whether they pertain to the same epoch.

M. Suess has lately published (*Bulletin de l'Académie des Sciences de Vienne*, tome li) a view of very important discoveries which he has just made in Lower Austria, where antiquities, analogous to those of our lakes, are found heaped together on the summits of hills, especially in the Vitur-Berg, not far from the small village of Eggenburg. There are found here, along with a prodigious quantity of flakes of silex, which seem to indicate manufactures of the epoch of stone, objects in bronze, such as brooches and poniards, some articles of iron, but chiefly utensils of stone and a vast amount of fragments of pottery, sometimes rude and mixed with small pebbles, sometimes of a fine homogeneous paste, which would seem to imply, not a people possessing simultaneously all these objects of stone, bronze, and pottery, but simply that these places have been inhabited during many consecutive ages. M. Forel has picked up in the environs of Morges a bracelet in all points similar to those of his rich lacustrian collection. M. Gerlach has discovered in the alluvion of the Sionne, near Sion in Valais, bracelets characteristic of the age of bronze, accompanied by calcined bones, which would tend to prove that the tribes of that epoch were accustomed to burn their dead, and again might serve to explain the rarity of human remains. M. Thioly,† last year, collected in one of the grottoes of the Grand-Saleve, (the cavern of Bossey,) near Geneva, a quantity of fragments of pottery, which, by their designs, altogether remind us of the age of bronze. Fragments of vessels not less curious, but of fine paste, accompanied, as at the Grand-Saleve, by numerous bones, were some time ago found by M. Otz, civil engineer, in a grotto on the banks of the Reuse, in the canton of Neuchâtel.

M. Quiquerez has just announced as existing in the Bernese Alps, in front of Vorbourg, and at a point which commands the entrance of the Val de Felémont, a remarkable series of prehistoric objects, which relate essentially to the age of bronze. They consist of several knives, an arrow point, part of a bracelet, all of bronze, besides a considerable collection of fragments of earthen vessels imperperfectly baked, and bearing figures which recall in all respects those of our palafittes.‡

RELIGIOUS EMBLEMS.

Hitherto we have discovered no idol nor anything having reference to a cult or worship, unless we consent to regard as religious emblems certain earthen objects, the so-called *lacustrian crescents*. These have, in effect, the crescent form, the curve and horns varying in different specimens; some are furnished

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*The same form with the heel is found also in Scandinavia and at many points of France and Germany, (Nillson, *Urcinwohner*, tab. iii, fig. 41.) It remains to be known whether the hillocks, which are found by thousands in the environs of Gorgier, were tombs, or places of incineration.

† *Debris de l'industrie humaine trouvés dans la caverne de Bossey*. Geneva, 1865.

‡ *Indicateur d'hist. et d'antiquités suisses.* March, 1866, page 16.—M. Quiquerez mentions, moreover, objects of iron and vessels fabricated with the wheel, which would indicate the age of iron; and, again, an arrow of silex, which would point to the age of stone; so that this station would seem to afford remains of three ages.

with a stem or foot, but too slender to support them in an upright position. The measurement in some cases amounts to 40 centimetres. Most of them are of argillaceous earth mixed with quartzose grains and small pebbles, like the rings for supporting the vases, but still ornamented with designs very rude, and only on one side. They were, at all events, objects of little value either for composition or form; and since it is impossible to assign to them any use, it has been asked whether they might not represent a species of talisman or religious symbol which was suspended within or perhaps at the doors of dwellings, which would explain why they are pierced with a hole intended evidently for the passage of a strap.


Figure 67.

The first lacustrian crescents were discovered by M. Schwab at the station of Nidau; but as this station comprises the relics of the three ages, while the true palafittes of the age of bronze had furnished nothing of the kind, M. Troyon (*Habit. lacustres*, page 185) concluded that they must have appertained to the first age of iron rather than to that of bronze. Since that time we have ascertained their presence in the two palafittes of Cortaillod and Auvernier; there is no doubt, therefore, that they ascend to the age of bronze. They have been found also at Ebersberg, in the canton of Zurich, though here, instead of being of baked earth, as with us, they are occasionally of stone. Such, among others, is the fine specimen which we borrow from the work of M. Keller, and which our learned friend has adopted as the frontispiece of his third report.* It is of reddish sandstone (Fig. 67.) M. Quiquerez announces a fragment of one in stone among the débris of Vorbourg, near Felémont.


Figure 67*a*.

Commercial relations.—If commerce there was none, or one very much restricted, during the age of stone, it is beyond doubt, on the other hand, that, from the commencement of the age of bronze, there must have existed very extensive commercial relations, which are attested by divers objects of foreign origin; among others, by the graphite which served as a coating to the vases, by the beads of amber and objects of glass which have been furnished by the palafittes of Cortaillod and Auvernier.

But the most conclusive proof in favor of an international commerce is supplied by the tin, which enters, to the amount of nearly a tenth, into the composition of the bronze. Now, as this metal is completely a stranger to our countries, (to the Alps as well as the Jura,) it must necessarily have been brought from abroad, and, as its consumption was considerable, judging from the quantity of objects collected within a few years, there was in this the material for an important traffic. We have no positive data as to what products the natives might offer in return for the metal which was im-


Figure 68.

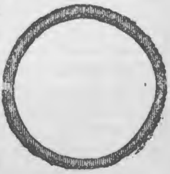
Figure 69.

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* *Mittheilungen der antiquarischen Gesellschaft*, 3d and 4th Report.

ported; but as, among all ancient populations, commerce was effected by simple exchange, it may be conjectured that furs supplied to our lacustrian tribes no inadequate medium for that purpose. Nothing has yet been found which recalls our coin bearing an effigy; possibly it may have been replaced by certain small rings of bronze similar to those now used for curtains, but with dentated edges; of these we have collected a large number of different calibres at the station of Auvernier. (Figs. 68 and 69.)

RESUMÉ ON THE AGE OF BRONZE.

The features which characterize the age of bronze in the palafittes of the lakes of East Switzerland, and which distinguish them from those of the preceding age, may be summed up as follows:

1. The presence of metal under the almost exclusive form of cast bronze, more or less pure, but with no intentional alloy of lead or zinc. The seams of the moulding are seen on most of the objects. The cutting instruments only have undergone hammering, and the articles of dress have sometimes been retouched with the graver.
2. A considerable improvement in the pottery, notwithstanding the absence of the wheel. The finer utensils are generally conical, and provided with a glaze of graphite.
3. The presence of rings of baked earth to support the conical vessels.
4. The appearance of discoïd stones and lacustrian crescents.
5. Spindle-whirls of baked earth, replacing the stone weights of the preceding age.
6. The greater depth of the palafittes, and hence their greater distance from the shore.
7. The piles are sunk in the ground, and to this end are always hewed to a point; the strokes of the axe are still easily recognized.

APPENDIX.

It will be acceptable, we doubt not, to archæolgists and to students of lacustrian antiquities if we here lay before them the general remarks with which M. de Fellenberg terminates, in the *Bulletin de la Societé des Sciences* of Berne, the series of his numerous analyses of ancient bronzes. (*Mittheilungen der Bern. naturforschenden Gesellschaft*, 1865.)

"The aspect of ancient bronzes is different according as they have been found—1, in peat; 2, in water; 3, in earth.

"1. The bronzes found in peat are covered with a black, earthy crust, which is easily removed by scouring in water; the alloy then appears with metallic brilliancy and with the color peculiar to bronzes. That the metal was imbedded in an organic ooze, beneath a stratum of water of several feet, which entirely excluded the access of atmospheric oxygen, sufficiently explains the perfect preservation of the bronzes, which present themselves in the state in which they existed at the moment of their submersion in the water.

"2. Those found in water at the bottom of lakes and rivers are less perfectly preserved; they are ordinarily covered with a calcareous coat, which still allows the lustre and color of the metal to appear at many points. When these bronzes have dark or greenish spots, the latter are of little depth, and disappear by treatment with acids, which re-establishes the color of the metal. The hatchets and knives have retained their edges unimpaired. When we find in water, bronzes covered with a thick coat of verdigris, it may be inferred that they have been a long time in earth before being covered with water, immersion therein not having availed to remove the strong oxidation already contracted.

"3. Bronzes found in the earth in tombs are very frequently distinguished by a fine green crust, more or less light or dark, having often a vitreous lustre, which is designated by the name of patina. This envelope has a very variable thickness—sometimes that of a sheet of paper, sometimes attaining several millimetres. When the file is applied to it, or, better still, when it is dissolved with nitric or sulphuric acid diluted, the bronze appears colored red; under the crust of carbonate of copper is a stratum of protoxide of copper, and only when this has been removed by means of ammonia does the metal appear with its proper color and lustre. This characterizes in a sure manner the slow oxidation of the bronze in a moist soil. The layer of protoxide of copper between the pure metal and the exterior layer of carbonate of copper is, according to the researches of Dr. Wibel, a product of the reduction of the carbonate of copper by the copper of the bronze. Bronzes of this category have often lost their previous metallic properties, and are found, when the objects have an inconsiderable section, to be transformed throughout into protoxide of copper covered on the exterior with a brilliant stratum, green or blue, of carbonate. When there remains in the interior a nucleus of metal this has become crystalline, and so fragile and incohesive that it shivers under the hammer. Figures, if a little delicate, as well as the edges and points of the objects, have disappeared, which is never the case with bronzes preserved in water.

"*Composition of bronzes.*—With reference to this subject, it is of importance to distinguish the principal elements of the composition from those which are only accidentally present. To the former pertain the copper, tin, zinc, and in some cases also the lead. The accidental elements are silver, lead, iron, antimony, nickel, and cobalt. As regards the two latter, I thought, when entering upon this inquiry, that their presence might lead to some conclusions on the origin of the copper used in the bronzes; but when I found that these metals appeared there, though in very small quantity, much more frequently than I had anticipated, I was forced to abandon that idea and ceased to pay attention to it.

"*Principal elements of the bronzes.*—1. Copper is incontestably the most important element of the bronzes, as well as that of which the proportion is greatest; yet its quantity varies from 67 to 95 per 100, and even more. It is necessary, moreover, to observe that after deduction of the tin, all the accidental elements, such as silver, lead, iron, antimony, nickel, and cobalt, should be added to the copper as forming part of its impurities, so that it is difficult to indicate, from the analyses, any constant and intentional proportion of its alloyage with tin. According as the copper proceeds from pure oxides or from very impure sulphurated ores mixed with different metallic sulphurs, its influence on the composition of bronzes is considerable, inasmuch as the greater or less quantities of accidental elements are in relation to its degree of purity, as the bronzes of Mecklenbourg show in a striking manner. 2. Tin. From historical tradition, tin appears to have been introduced into commerce by the Phenicians and dispersed by them through Europe, in the sense, no doubt, that this mercantile people conveyed the metal directly to the inhabitants of the coasts, whence it made its way into the interior by means of commercial exchange, which would explain why tin appears in the bronzes in proportions so variable—from 3 to 4 per cent. up to 20 per cent. and more, according as it was more or less abundant, and without regard to the properties which it might communicate to the bronze. As the tin coming from the tin islands was alluvial, its influence on the bronzes was, in view of its relative purity, but in the ratio of the quantity employed. 3. Zinc made its appearance late, in the bronzes of the age of iron. Although it was only recognized as a special metal towards the end of the fifteenth century, yet as early as the third century before our era it was added, under the form of natural calamine or cadmium, in the casting of copper or bronze, in order to obtain a yellow alloy. All the bronzes containing zinc pertain, consequently, to times posterior to the period of bronze, and were unknown during the age properly so called. 4. Lead is found, according to our analyses, in such small quantity in the bronzes of the lacustrian constructions and in the Celtic bronzes of Hallstadt and Mecklenbourg, that it must be there considered as an accidental element proceeding from the impurity of the copper. With this is associated the fact that silver has been found in none of these places, while gold is quite frequent. It must be inferred that the populations to whom silver was unknown had no more knowledge of lead as a particular metal. The case is wholly different with the bronzes of the Greeks, Egyptians, Etruscans, and Romans, in which lead appears as an intentional element in considerable proportions. It has been shown that all these nations were acquainted with silver from a remote period, and partially possessed it before iron. The appearance of lead as a special metal, applicable in large quantity to technic uses, can only be explained by the metallurgic elaboration of the ores of silver, since, in ancient times, silver was principally extracted from the argentiferous ores of lead, and, indeed, could be extracted from them only, for no other sources were known. This does not imply that it may not often have happened, during the age of bronze, that lead was produced in a metallic state by the Celtic smelters, but without being considered by them as other than an isolated result, which led to no other consequences. The question is not whether lead might have been known before silver, but whether lead was in general use among the ancient populations before silver. This question appears to have been resolved negatively, inasmuch as, even in the time of Pliny, the Romans only distinguished lead from tin by the names of *plumbum nigrum* and *plumbum candidum* or *album*, and possessed no particular appellation for tin. By *stannum* they only understood certain alloys of lead destined for the soldering or lining of vessels of copper. If this was so among the people of civilized antiquity, it can scarcely be admitted that the half savage tribes of the age of bronze were more advanced in this respect.

"The presence, therefore, of lead in bronzes, in such proportions as to denote that it has been designedly introduced, seems a sufficient criterion for recognizing these alloys as proceeding from civilized populations, and not from those of the age of bronze. The vase of Grœchwyl* affords, in this respect, an instructive example. The bronze of the group of lions does not differ only from that of the vase by the object represented, but also by its proportion of lead, which is 10 per 100. From the considerations here developed, then, I regard lead as a factor altogether as important as zinc in the estimate formed regarding bronzes, and I repeat that lead is not found in the bronzes of the age of bronze properly so called, in the quality of a principal element. The plumbiferous bronzes proceed from populations among whom that period was past, in consequence of their knowledge of iron and of silver, and who had acquired a superior degree of culture.

"*Origin of bronzes.*—Opinions on the origin of bronzes are contradictory. There are very competent authorities who maintain that it was the Phenicians who discovered and also diffused bronze over the European continent, and that the bronzes which come to us from the north, from the Celtic tombs and lacustrian constructions, are Phenician bronzes. They receive it as an ascertained fact that the Phenicians alone possessed the commerce of tin, because they alone knew the route to the tin islands, the Cassiterides, and likewise that they had penetrated to the Baltic, and while they sought there for the yellow amber, conveyed lead and a knowledge of the preparation of bronze to the inhabitants of the coasts. But it does not follow that the fabrication of bronze was confined to the Phenicians. This supposition is contradicted in a positive manner by the very different composition of the bronzes of different nations, by the very variable proportions between the copper and tin and the inequality of the accidental elements. Moreover, it seems surprising that the nearest neighbors of the Phenicians, the Greeks, the Egyptians, the Etruscans, and the Romans, should have manufactured plumbiferous bronzes, while the Phenicians carried to the

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* See A. Jahn, *Etruskische Alterthumer gefunden in der Schweiz*, (Mem. de la Soc. des Antiq. de Zurich, VIII, 1865.) A. Morlot, *Etudes geologico archæologiques*, (Soc. vaudoise Sc. nat., T. VI, p. 314.)

people of the north only pure bronzes, without the alloy of lead. If the civilized people of the Mediterranean added lead to their bronzes, it can scarcely be doubted that the calculating Phenicians would have done as much, and, at least in their commerce with distant and half-civilized tribes, have replaced the more costly tin by the cheaper metal. But this question cannot be decided with certainty until we shall possess analyses of well authenticated ancient Phenician bronzes, whose composition we can then compare with that of the bronzes of the north. This desideratum it has not been in my power to realize. In fine, the Phenician origin of the bronzes widely scattered over the European continent is further contradicted by the discovery of numerous foundries, which prove that the smelting of bronze was almost everywhere a domestic industry, the tin of commerce and the copper of the nearest excavations being employed, which would of itself explain the presence in the bronzes of such different accidental elements.

"On the whole, then, I consider that the first knowledge of bronze may have been conveyed to the populations of the period under review not only by the Phenicians, but by other civilized people dwelling more to the southeast. It became thenceforth a common resource, the type, in some sort, of a whole civilized epoch, and was maintained and developed of itself, until, by the discovery and diffusion of iron, the general and exclusive employment of bronze had ceased and an end was thus put to the period of bronze.

"I here terminate a work commenced five years ago, with the hope that the undertaking may not have been useless, but may contribute in some small degree to the advancement of our knowledge of the pre-historic times of our ancestry, as yet so obscure. Should my opinions not have been exempt from all prepossession, it is to be hoped that others, with greater means at their disposal, will resume the investigation, and, guided by better lights, conduct it to a successful end, by embracing within the scope of their researches the bronzes of the ancient Persians, Assyrians, Babylonians, Egyptians, Jews, and Phenicians."

III.—AGE OF IRON.

For a long time there have been collected at many points of the lake of Neuchâtel, articles of iron associated with others of a more ancient origin, as at Gletterens, Bevaix, Cortaillod, and Font.* In reality, however, there is but a single station of pile-work which is referable exclusively to the age of iron—that, namely, of the Tène, near Marin, on lake Neuchâtel. It consequently claims from us a moment's attention.

The shore of the lake, between the Maison Rouge and the Hospice de Prefargier, below a stretch of land called the *Heidenweg* or highway of the Pagans, is very flat and composed of a fine and turfy deposit, which extends under the neighboring peat-mosses. The waves of the lake, by wasting and undermining this formation, occasion frequent land-slips, which, viewed from the surface, have the appearance of large abrupt rocks conveying the idea of a jetty. A post which here and there shows itself at the edge of these fallen masses has been erroneously taken by the inhabitants of the coast for a relic of this ancient jetty, and hence the piles, though long known, attracted no attention. This extent of shoal, where the water is of little depth, (60 to 70 centimetres,) has received the name of *Tène*.† In sailing over this oozy floor of the Tène, there

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* Objects in iron are also found at many points of the lake of Bienne, as at the Steinberg of Nidau, at Sutz, Latrigen, Hageneck, de Neuville, Vigneules.

† Doubtless from the latin *tenuis*, in German dünn. In the patois of certain places, it would seem customary to say, "the water is *tene*," that is of little depth; the local word teneviére has probably the same origin. (This class of words, like the Greek Τεναγος, a *shoal* or *shallow*, would seem to be derived from the root Τεν of the verb Τεινω, Τενω, to *extend* and so to become thin. Tr.)

may be seen, at a number of places, groups of piles hewed for the most part to a point and rising from 10 to 30 centimetres above the bottom, without attaining the surface except at very low water. The piles are similar to those of the stations of bronze, of medium thickness, measuring from 12 to 20 centimetres in diameter. They are generally very soft, in so much that it is difficult to withdraw them entire. The beams which lie here and there on the bottom are for the most part less decomposed; some have been squared and even furnished with mortises carefully cut; while occasionally the cross-pieces are found still attached to the beams, being the remains of ancient walls or enclosures.

Here, as in the stations of the age of stone and of bronze, it is in the immediate neighborhood of the piles that antiquities are collected. At first some objects were found on the surface, but the greater part are buried at a depth of 1 m. to 1 m. 50 c., whence they are withdrawn by searching in the ooze. The objects obtained in this way are always best preserved; the arms and utensils of iron particularly have been protected from the contact of the air, and, favored also by the antiseptic properties of the peaty substratum, remain uninjured. It is possible that eventually antiquities will be found wherever piles make their appearance; if so, the Tène must have been a considerable establishment.* Hitherto the greater part of the objects has been collected at two or three points of a very limited extent, measuring in the whole less than a hectare. The numerous objects which the station of the Tène has furnished within a few years may be classified as follows, in the order of their frequency: Arms, utensils and vessels, objects of apparel, coins, skeletons.

ARMS.

The arms of the Tène possess a peculiar interest,† not only on account of their fine preservation as objects of art and curiosity, but also and chiefly as documents for the history of Gallic civilization. Through the munificence of his Majesty the Emperor of the French we have been enabled to compare these arms with a collection of casts representing the arms collected in the trenches of Alise, and we have there found the most vivid confirmation of our previous impressions that the inhabitants of the palafitte of Tène were Gauls.

Among these arms, those which strike us most are the large iron *heads of lances*, measuring as much as 40 centimetres in length by a breadth of from 4 to 6, of elaborate workmanship, strengthened by a central prominence running down each face, and very large lateral development or wings, which are not always symmetrical. Some are irregularly emarginated, doubtless to render the

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* The piles are not limited to the shoals, but extend also under the detritus of the shore to the distance of more than a hundred metres from the beach at mean water. It may be assumed that they advance at least fifty metres into the water; on the other hand, the space they occupy is in breadth at least 1,000 metres, which multiplied by 150 gives a surface of 150,000 square metres, or fifteen hectares.

† From a communication made by M. F. de Rougemont to the Society of History of Switzerland, at its last meeting at Neuchâtel, in 1864, it appears that the arms of the Tene correspond in a striking manner with the description of the arms of the Gauls given by Diodorus Siculus, (book 5, chap. 30:) "As a weapon of defence, he says, the Gauls have a large sword suspended to the right side by a long chain of iron or copper, and some of them fasten their tunics with belts ornamented with plates of gold and silver. For throwing, they have javelins, which they call lances; the iron, a cubit in length, (nearly a demi-metre,) the stock a cubit and something more, the breadth of the blade is nearly two palms, (about three inches.) Their swords are not less in length than the saunium or javelin of other nations, and their javelins have the iron longer than their swords. Of these arms some are forged straight, others in zigzag with the extremity bent backward, with a view that, in striking, they may not only pierce, but tear and lacerate the flesh when withdrawn." As M. de Rougemont suspects Diodorus of not having perfectly understood the author from whom he copied, we propose the following correction, which has been suggested to us by a view of the arms of the Tene: "The blades of these javelins, three inches in breadth, are very extraordinary, for they are broader than the long and large swords, and yet these swords do not yield in point of breadth to the javelins of any other nation."

weapon more formidable, (Fig. 70;) others are open-worked, with salient outline, (Fig. 71;) fragments of the staff are also found, which was remarkably slender, and shod at its extremity with an iron point. Although the socket is very small, and supposes consequently a slender staff, the blade was too carefully wrought to admit the idea of any purpose of exposing it to the hazard of being lost by launching it. It was a weapon for thrusting, not for throwing.*

The *swords* of the Tène merit particular attention. The blade (Fig. 72) has a length of from 80 to 90 centimetres, is very flat, being scarcely 3 millimetres in thickness, with two edges carried regularly to a point; it has no guard, and of the hilt there remains but the tongue, which, without being very large, is yet calculated for the hand of an ordinary man, (13 to 15 centimetres.) The transition from the tongue to the blade is formed by a graceful curve provided with an iron flange, which serves as a guard and is adapted to a corresponding projection of the scabbard. We have not yet discovered the square form, which is the most common at Alise. A part of the swords are in their sheaths, but as they have not been attacked by rust, (the qualities of the peat having preserved them from oxidation,) we have succeeded in withdrawing several of them. They are straight and two-edged, most of them so sharp and uninjured that they might very well be used to-day. On examination we discover on their surface undulating lines, which somewhat remind us of damasked blades, as if they were composed of strips and clippings which had been welded together; the borders only are perfectly smooth, like the blades found at Alise.† Several of them bear the token of the workshop near the hilt, (Fig. 74.‡) We may here remark that almost all the swords we possess have been collected within a very

Figure 70.

Figure 71.

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* We cannot concur in the opinion of M. Keller that these were only arms of parade; the median ridge, which is prolonged, while always diminishing to the extremity of the lance, is a well known and constant expedient in the art of forging. As the lance is hollow within, it has been asked whether it be not formed of two plates soldered together. If this were so, the soldering would have been effected with great skill, for there is no trace of it perceptible. M. Schwab has recently discovered at the Tene an iron lance 22 centimetres in length, whose edges alternately re-enter and project like the teeth of a saw, so as to present in profile an undulating line, the object of which, no doubt, was to aggravate the wound. M. Keller is confident that it was to this form that Diodorus Siculus refers, (V. chap. 30,) in the description he has left us of a formidable weapon of the Gauls.

† M. de Reffye remarks as follows in regard to this type of sword, which is very frequent at Alise, and which the Gauls may have borne from the time of Camillus: "In these weapons the edge is not of the same iron as the body of the blade. The workman after having forged that part of very tough iron, drawn out in the direction of its length, welded on each side small strips of a softer iron to form the edges; this iron was afterwards hardened by hammering. In this way, the soldier, after combat, might repair, by whetting, the gaps of his blade as the mower does those of his scythe when it requires sharpening." (*Revue Archæologique*, November, 1864, p. 347.)

‡ The mark of the sword here represented resembles slightly a leaf of trefoil. There are not less than ten of them in the collection of Col. Schwab, which we reproduce from the

Figure 72.

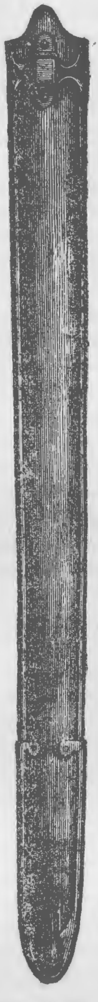

Figure 73.

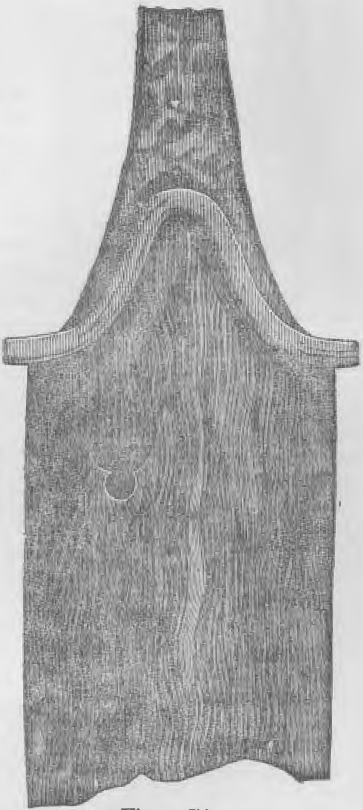

Figure 74.

limited space, of a few square metres, a circumstance which seems to countenance the idea which we have already advanced with reference to the constructions of the age of bronze, namely, that there were here magazines rather than habitations properly so called. It would be difficult to conceive how so many swords could be found in a single dwelling, except upon the supposition of a desperate defence. But, in that case, it would be little probable that they should be new and in the sheath.

The sheath is of wrought iron, composed of two very thin sheets, of which one covers the other, with the exception of the lower part, which is furnished

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6th Report of M. Ferd. Keller, p. 296, (Fig. 74*a*.) Our learned friend remarks, on this occasion, that with the exception of the wild boar and leaf of trefoil, all these figures recall the form of the crescent, which appears as a symbol on many pieces of the Gallic money. He asks if these different signs may not perhaps designate the manufacture of various clans.

with a rim of iron skilfully adapted, and embracing the two edges of the scabbard. It is furnished at top with a special plate, which bears the ring of suspension, and whose border serves as a frame for the very remarkable designs which characterize these sheaths. These designs had from the first attracted the attention of M. F. Keller, as being equally foreign to Roman art and to the age of bronze.* Most of them are engraved with the oscillating burin, *(tremulirstich,)* so that, on close examination, we recognize the reciprocating movement of the instrument by which they are traced. Some of the scabbards are ornamented with figures wrought with the punch; this is particularly the case with a unique specimen of our own collection which represents the characteristic emblem of the Gauls, (Fig. 75,) namely, the horned horse, such as occurred also on the coinage of the Tène. There is seen, moreover, on the face opposite to that which bears the clasp of suspension a sort of granulation, which sometimes reminds us of shagreen skin, and at other times of such damaskeened work as modern armorers obtain by the use of acids. These ornaments and designs have, in an ethnographic point of view, a much greater importance than the swords themselves, seeing that thus far they are exclusively peculiar to the age of iron, while the form of the blade has been preserved during the subsequent epochs.


Figure 75.


Figure 76.

Together with swords and lances, we find at the Tène considerable numbers of *javelins* of iron, of small dimensions, (10 to 12 centimetres,) and of much less finished workmanship, without the median ridge, but with a simple socket, (Fig. 76,) in which is sometimes found the nail which fastened it to the staff. These javelins are in all respects similar to those of the collection of Alise. From the trials which have been made at Saint-Germain, under the direction of the Emperor Napoleon, it is apparent that these javelins could have no efficiency but as missiles, which were launched by means of a thong known by the name of *amentum.*† These points are, in fact, too light to have pertained to javelins thrown by hand; while the experiments made by direction of the Emperor prove that a light shaft which the hand could project but twenty metres at most, might attain by the help of the amentum a distance of eighty metres.‡ On the other hand, these instruments are executed with so slight a degree of elaboration as to have rendered the losing a number of them a matter of small consideration. It would hence appear that there were among the

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* They are composed of very simple elements, namely, the undulating line, the circle, and the triangle, recalling at times, by their combinations, the paraphs of our ancient calligraphists. There is something about them which, according to the learned antiquary of Zurich, would remind one of the ornaments on the arms and utensils of the later Celtic period as these have been delineated by M. Franks. (Kemble, Horæ ferales, p. 122.)

† See the figure of a warrior launching a javelin with the amentum, published by M. Merimée, from a panathenaic amphora in the British Museum. *(Revue Archæologique,* 1860, p. 211.)

There have been taken at different times from the palafitte of Tène plates of iron with a median swelling, and having the sides furnished with nails which attached them to a piece of wood. M. Keller regards them, with much reason, as parts of a buckler. (Table XIII, figure 12.)

‡ Verchere de Reffye, *les Armes d'Alise.* *(Revue Archæologique.)*

Gauls troops exercised in launching the javelin by means of the amentum, as others launched stones or other projectiles by means of the sling. (Fig. 77.*)

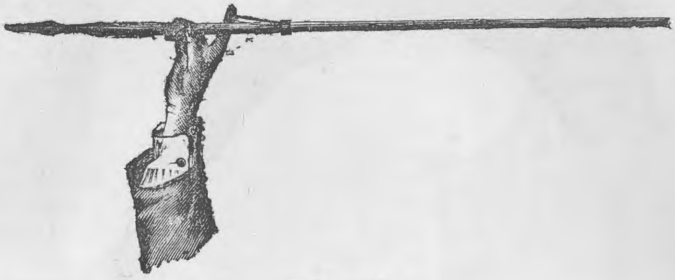

Figure 77.

Of the *pilum* no trace has been hitherto found at the Tène, which would tend to prove that this kind of arm was not a Gallic weapon, and that those which have been found intermixed with the Gallic javelins and swords in the trenches of Alise are derived from the Roman legions, as well as the slender lances bearing a cross-bar near the socket, and which are supposed to have been the lances of horsemen. Neither are the iron arrows, so abundant at Alise, found at the Tène,† nor the short swords terminating in a sharp point, with a median enlargement, like the blades of the age of bronze, and which were designed, doubtless, like these last, for thrusting. On the other hand, the large lances in form of halberts, which have been above described, are thus far peculiar to the Tène, and have as yet been nowhere found in the Gauls. What approaches them nearest are certain flame-formed lance-heads of the collection of Alise.

UTENSILS

of iron, without being very numerous, are yet deserving of attentive consideration. The most frequent are a sort of boat-hooks, (for pushing boats in shallow places,) which have been sometimes improperly taken for the heads of pikes. They are found at the Tène and at Bied, near Colombier. Most of them have the form represented by Fig. 78 ; some are simply cylindrical. It is not unusual to find in the socket the end of the wooden pole with the nail which secured it. The *sickle* of this epoch has the form of those of our own time, though rather less curved, while it is much larger than the same implement of the age of bronze, and is without ornament, (Fig. 79.) We also possess two scythes, with the collar for attaching the handle, and the curved heel, (Fig. 80,) a proof that it was adapted for a long handle, and for mowing. The curve is the same with that of our scythes, but the dimensions are a third less, (35 centimetres.) A particular interest attaches to these implements, for, as they are exclusively intended for mowing grass,‡ we are authorized to conclude that their owners

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* M. Vogt, the eminent anthropologist of Geneva, has just communicated to us the existence of a design altogether similar on the great mosaic of Pompeii, now one of the ornaments of the *Museo Reale* of Naples, and which represents one of the battles of Alexander. The javelin is there seen with other arms in the foreground of the tablet. The amentum may be distinctly recognized, forming a short, but quite wide, bi-colored loop nearly at the middle of the staff.

† The absence of all traces of the arrow would seem to indicate that the people from whom these remains are derived made no use of the bow. M. F. Keller remarks, with reference to this fact, that the bow is not mentioned by Diodorus among the arms of the Gauls. It was probably replaced by the javelin.

‡ It is only in modern times that the scythe has begun to be made use of for reaping grain.

were in a condition to require a provision of hay, and were consequently rearers of cattle. The *axe* of the epoch of iron is larger and stronger than the celt or hatchet of the age of bronze; it has no longer four pinions like the true celt, but the handle is adapted to a sort of socket formed by the junction, more or less complete, of two wings, (Fig. 81;) its edge is at the same time much wider. A specimen of somewhat different form, with a socket complete and circular, has

Figure 79.

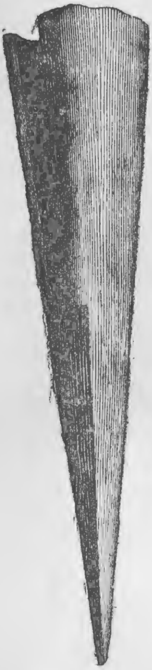

Figure 78.

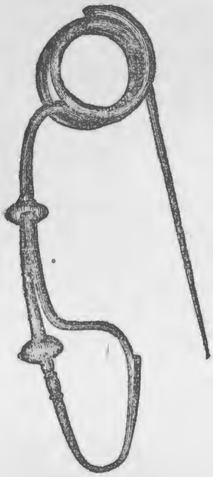

Figure 83.

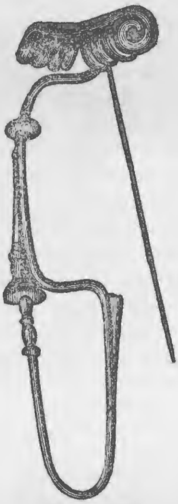

Figure 84.

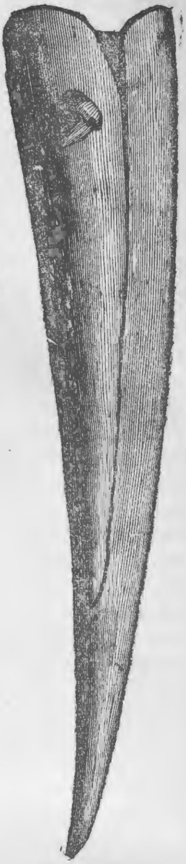

Figure 82.

Figure 80.

been found at the station of Font, and is in the collection of M. Otz, at Cortaillod. In the collection of M. Schwab is a specimen, thus far unique, of the ordinary hatchet with a circular hole; it was taken from the Tène, and does not differ from the Roman axe. Fragments of iron *bridle-bits* sometimes occur at the Tène, not differing much from those used in subsequent epochs. M. Schwab possesses a complete one. The lake has also furnished iron horseshoes, of which we possess a specimen remarkably slender, from the Tène; others, which are not, however, derived from that station, but from a locality on terra firma near the shore, (*the terriere de Marin,*) have an undulated edge, the undulations having proceeded from the thrust occasioned by the holes of the nails. In the same palafitte there has been lately discovered a sort of pike, slightly incurvated, which might have served as a ploughshare, (Fig. 82;) for it would seem to be too heavy to answer for a boat-hook, weighing, as it does, not less than four kilogrammes. The collection of M. Schwab, at Bienne, contains a pot or saucepan taken from the Tène, which is of wrought bronze. Is it authentic? *Knives* are not wanting, but they have not in general the elegance of those of the age of bronze. They are simple blades, usually straight and quite broad, with a tongue which entered into a handle of wood or horn. There is also in the collection of M. Schwab a pair of *scissors,* with an elastic handle like those now used in shearing sheep. In our own possession is a sort of iron *stewpan,* which presents numerous traces of reparation, a proof that some importance was attached to it.

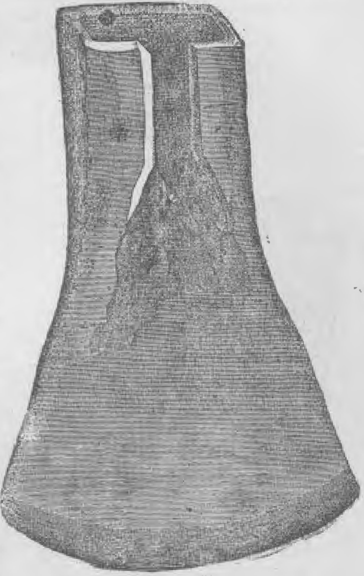

Figure 81.

ORNAMENTS AND OBJECTS OF APPAREL.

At the epoch which we are considering, iron had not only replaced bronze for arms and domestic utensils; it appears to have been so highly regarded, perhaps on account of its novelty, as to have been employed even where bronze would have been more appropriate, as for objects of the toilette. It is true that these objects are relatively less abundant than in the stations of the age of bronze, and comprise rather useful articles than those of mere fantasy. Thus, we find neither rings nor ornaments for the ears, and we have thus far recovered but one small hair-pin, which itself might have been derived from the preceding age, for it is of bronze. Ornaments of detail, it would seem, were not in request; on the other hand, elegance of form was affected, as is attested by a multitude of objects which have descended to us. Of this number are the *fibulæ* or *clasps* of mantles, (Figs. 83 and 84.) We are in possession of a numerous series of them, presenting variations of every kind, but all based on the same principle of the elastic spiral,* which is more or less complicated, according to the number of convolutions. They are of all dimensions, from six to twelve centimetres in length, and have sometimes the principal stem ornamented. Most of them are in a perfect

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* The so-called *epingles d'hopital,* which have been recently so much vaunted, depend upon the same principle. They are the Gallic fibula, without its elegance.

state, and might serve for use to-day. All are provided with a groove to receive the end of the pin and prevent pricking. This clasp is different both from that of the Etruscans and that of the Romans, but is perfectly similar to those of Alise. If, as seems probable, it served for the same purpose, we are justified in concluding that those who owned it wore also the toga or mantle. The same clasp, likewise, is found at Tiefenau and at the Wylerfeld, near Berne, accompanied, in this last locality, by glass bracelets, which make part of the collection at Berne. Colonel Schwab owns one of these clasps, the studs of which are of bronze. Analogous forms occur also in the tombs. We have seen in the collection of M. Troyon small clasps of bronze, very similar to those of our figure 83; the same form is found at Hallstadt.

RINGS.

We possess a great number of these, both plain and ornamented, but the use made of them is yet imperfectly known. Some probably served as buckles or clasps for the girdle, (Figs. 85 and 86;) others, especially the circular rings, still await interpretation. Most of them are too small to have been intended for bracelets; while others again are divided into sections, (Fig. 87,) conveying the idea that they constituted a sort of annular money like the small rings of the age of bronze. From the same locality have been taken nippers of very finished workmanship, in the shape of our tweezers, but longer, and destined, no doubt, for depilatory purposes, for which they might serve even now, (Fig. 88.) To these should be added certain very broad and flat plates, with a stem neatly fashioned; the purpose of which is unknown, but which, from their tenuity, we are disposed to regard as razors, (Fig. 89.) M.

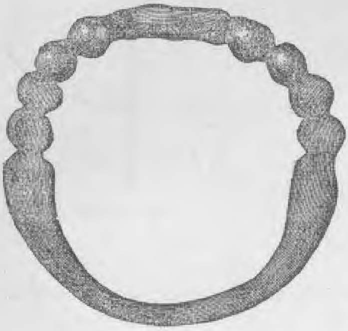
Figure 85.


Figure 86.


Figure 87.

Keller inclines to the opinion that they were tools of the currier. The thickness of the blade is represented by Fig. 89*a*.

Figure 88.

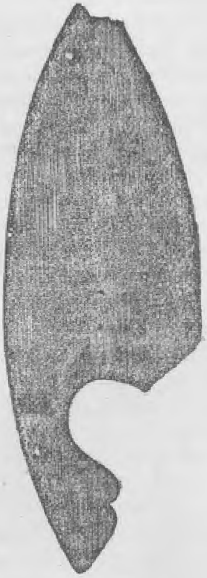

Figure 89.

Figure 89*a*.

OBJECTS IN BRONZE.

The station of the Téne has furnished us with some objects in bronze which at first glance may seem incongruous in the midst of all this assemblage of utensils and arms of iron, but these articles, though of the same metal, have nothing in common with those of the properly called age of bronze. It has been seen that the utensils of that age are characterized by having been run in moulds; those in question are wrought; they are garnitures for the helmet, the saddle, or some other object. The chemical composition of the bronze is much the same as that of the preceding age.[*]

PRECIOUS METALS.

It is quite certain that the Gauls were acquainted with gold and silver; but in this respect the tombs are richer than the palafittes, which have as yet furnished mere traces of those metals.

GLASS AND ENAMEL.

It is equally apparent, from explorations in the Gallic tombs, that glass was in extensive use in the age of iron. The palafittes leave, however, much to be desired in this respect, having hitherto yielded only some fragments of colored glass. At the palafitte of Nidau beads of an enamelled paste have been brought to light, and are supposed to have formed portions of necklaces, in which they alternated with beads of amber, as in the tombs of the epoch.

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[*] A fragment of a bronze plate (probably the ornament of a casque or helmet) is composed, according to an analysis made by M. Fellenberg, of the following: Copper, 86.30 per 100; tin, 13.03; lead, 0.34; iron, 0.18; nickel, 0.15.

COINS.

We were so fortunate, last year, (1864,) as to recover from the station of the Tène the first lacustrian money, (Fig. 90.) This consisted of genuine Gallic coins, bearing on the obverse the effigy of a man in profile, on the reverse the characteristic image of the horned horse, which has sometimes been regarded as a bull or he-goat, and which was probably only an allegory, a fantastic animal, serving perhaps as an ensign, as we still exhibit the unicorn and griffon in our escutcheons. These coins, to the number of five, (one of which has been deposited in the museum of Neuchâtel, and another in that of Saint-Germain,) are all of the same type, but with slight variations in the figure of the horse and effigy of the human head, which is different on each piece, representing probably five different chiefs. The coins, which bear no legend, are of bronze, simply run in moulds, united with one another by a neck, after the manner in which children cast their leaden playthings. The two seams of the neck, which united the corresponding pieces, are distinguishable in all. This type of Gallic money is to be met with quite frequently, not only in France, but in Switzerland, as will be apparent on comparing them with the collection of drawings by Dr. Meyer of Zurich.* Very similar ones exist from Tiefenau, near Berne, where they are associated with others bearing the effigy of Diana and Apollo, and the impress of Marseilles. Besides those coins in bronze, there have been taken from the palafitte of the Tène some of gold and silver; among others, a small gold piece which is quite frequent in Switzerland, being a bad imitation of the philippics of Macedon; on the obverse is exhibited a head of Apollo with a crown of laurel, on the reverse a *biga* with the head of a bird on the chariot, and some Greek letters referring to the name of Philip. Coins of silver, like those of Tiefenau, have also been announced. The Roman coins of the Tène are an *as*, a Tiberius, and a Claudius, the last being in excellent preservation; it is a copper piece of the size of a sous, and would indicate that the station had existed till the middle of the first century.

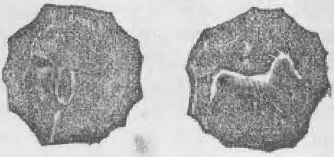
Figure 90.

On the other hand, there have been found neither in the palafitte of the Tène nor at Tiefenau those chains of iron or copper to which, according to Diodorus Siculus, were suspended the swords of the Gauls,† nor yet those plates of gold and silver which decorated their girdles.

Of *pottery* there was no deficiency in the age of iron. We have collected at the Tène a quantity of fragments of black or half baked pottery, which does not differ sensibly from that of the age of bronze. Together with this, which is eminently lacustrian in character, occur vessels made with the wheel, as well as red pottery or that baked in the kiln, such as amphoras, big-bellied vessels with handles, fragments of vases of *terra sigillata*, and a quantity of Roman tiles which, if they bear the number of no legion, attest no less the presence of Roman stations. According to M. Keller, the art of constructing kilns of brick, like the use of mortar, was unknown to the Helvetians and to the Gauls in general, who must have possessed only cottages of wood covered with shingles or thatch. It was the Romans who introduced the art on this side the Alps; so that the presence of tiles and vases of terra sigillata does but corroborate the indication afforded by the coin of Claudius, namely, that certain stations on pile-work have

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* *Mittheilungen der antiquarischen Gesellschaft*, vol. xv.

† It is possible, however, that the two rings of the collection of M. Schwab, which are represented by M. Troyon, (pl. xv., fig. 3,) and were taken from the Tene, may be relics of a Gallic chain.

continued to exist under the Roman domination.* One might be tempted, in view of the quantity of large tiles found among the piles of the Tène, to suppose that they covered the structures of that station, although a roof of tiles does not very well comport with huts of wood built upon simple piles.

M. Troyon does not hesitate to assign to the first age of iron certain very curious potteries, which form part of the collection of Col. Schwab. These are fragments of large dishes ornamented on the inside with red and black paintings representing sometimes concentric bands, sometimes triangles or squares, and reminding us of pottery of the same kind found with various objects of iron in the tumuli of eastern Switzerland. It should not be lost sight of, however, that the station of Nidau, whence these objects proceed, constitutes a repository of several epochs, among which that of iron of the Gallic period is perhaps least competently represented, since the large swords and most of the objects which elsewhere accompany the latter are there wanting, while, on the other hand, this painted pottery is at present a stranger to the palafitte of the Tène and to other repositories of authentic Gallic origin. From these considerations we cannot regard them as characteristic of the age with which we are at present occupied.

Skeletons of animals are less abundant than in the stations of the preceding ages; yet they are not absent. The bones of the horse particularly are numerous. Neither are other domestic animals wanting, but they have not yet been made the subject of special study, any more than the remains of the wild animals which accompany them.

It is but recently that we have been successful in procuring the first human relics of this epoch. They are the bones of the trunk, of the members, and, what is more important, a skull almost complete, which we propose to describe elsewhere in detail, and of which we shall give here but a sketch, (Fig. 91.)

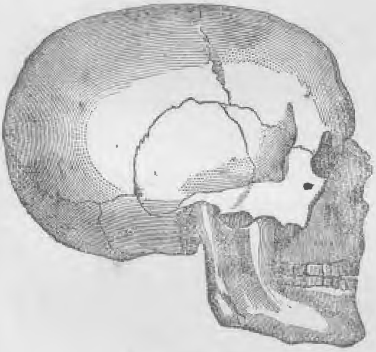

Figure 91*a*.

Figure 91*b*.

We content ourselves, then, with saying that in size it is quite large, but of a conformation far from advantageous, very long, extremely flattened on top, with an enormous occipital development, while the forehead is so low as to appear almost absent. In this respect it is not superior to the skulls of the two previous ages, if it be not even inferior to them. No skull is to be found in the work of Messrs. Rutimeyer and His so unfavorably formed. It pertains, however, to the group of Helvetian skulls, and is of the so-called type of Sion, to which it most nearly approximates.

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* An analogous consequence may be drawn from the discovery, recently made by M. Rabut, of a vase bearing a Roman inscription, in the midst of the station of Châtillon, at the lake of Bourget. (See Rabut, *Habitations lacustres de la Savoie*, page 21.)

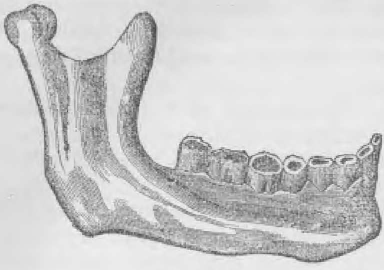
Figure 92.

The bones of the members, especially those of the thigh and hip, of which we possess a certain number, have been the subject of detailed study by our colleague, Dr. Guillaume; they indicate a race of men whose stature attained $1^{m}.90$, and who were consequently of more than average height. The teeth, which are all preserved, present a rather singular peculiarity, inasmuch as not only the incisors, but even the canines, are greatly worn, as if they too had served for mastication, (Fig. 92.) The same circumstance has been remarked in jaw-bones taken from ancient tombs.

COTEMPORANEOUS ANTIQUITIES OF THE PALAFITTES OF THE AGE OF IRON.

It is almost idle to insist on the existence of establishments on terra firma cotemporaneous with the palafittes of the age of iron, when it is once understood that the antiquities of the Tène are of Gallic origin, for history teaches us that the Helvetians inhabited cities which they burned when they emigrated into the Gauls; but nothing indicates that these cities were lacustrian constructions or settlements upon piles. It must be admitted, on the contrary, that the palafitte of the Tène, supposing that it existed at the epoch of the Helvetian emigration, was the exception, and not the rule, of the epoch of bronze.*

Unfortunately, we know neither the history nor even site of the twelve Helvetian cities. We are consequently reduced to the necessity of seeking in the tombs the tumuli, and the so-called battle-fields, equivalents for the arms and utensils which characterize the palafittes of our lakes at the epoch of iron. The number of the tombs which are referred to the age of iron or the Gallic epoch is considerable. But on examining their remains more closely, we cannot fail to be convinced that the identification is often determined in a very incomplete manner. From the fact that a tomb contains a weapon or object of ornament which somewhat approaches those of our palafittes, it does not follow that it is cotempóraneous. As in paleontology, it is not a single object which suffices to establish with certainty the age of a repository; it is necessary that there should be a conformity in the collective objects. In this respect, we scarcely know, in Switzerland, other antiquities than those of Tiefenau and Wylerfeld, which are identical with those of the Tène. On the other hand, the tombs too often contain objects unknown to our palafittes to make it prudent to refer them at present to the same epoch. The same doubts exist for us in regard to the great tumuli, (Cairns or Erdberger.) In return, a part of the arms found in the trenches of Alise-Sainte-Reine present, as we have seen above, too striking a resemblance to those of the Tène not to be referred to the same people, notwithstanding the contrary opinion of some eminent archæologists, who choose to see therein the relics of a later epoch.†

RECAPITULATION.

The age of iron, as it appears in our palafittes, is characterized by the following features: 1. The appearance of iron and its general use for arms,

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* The new station just discovered at Unteruhldingen, on Lake Constance, and which is formed of some 10,000 piles, contains objects in iron, (knives, lance-points, clasps, rings,) together with others in bronze and stone. This station will, perhaps, throw new light on the transition of one of these ages to another, and especially that of the age of bronze to the age of iron.

† J. Quicherat, *Examen des armes trouves a Alise-Sainte-Reine.*

utensils, and even objects of apparel. 2. The application of peculiar processes in the manufacture of swords of iron, similar to Damascus blades. 3. A particular system of ornamentation very different from that of the age of bronze, consisting especially of figures applied to the sheaths of swords. 4. The appearance of coins with an effigy. 5. The use of clasps of iron with a spiral spring. 6. Wrought bronze introduced into general use.*

Having thus indicated the prominent points which constitute the criterion of the age of iron, such as it appears in the palafittes of our lake, it will not, perhaps, be useless to signalize briefly the objects which are wanting, since these negative characters are not without their importance in a comparative study. We first recognize that the most characteristic designs, such as are observed on the sheaths of swords or on the vases, are much less complicated than those which decorate the objects taken from a considerable number of tombs, referred, wrongfully perhaps, to this epoch. We have not yet discovered in the palafittes of the Tène those armlets covered with fine engravings, nor those disks with concentric circles, still less those cinctures of bronze, presenting casts of small human figures and quadrupeds, (Troyon, *Habitations lacustres*, Figs. 23, 21, 35, and 36,) which exist at Hallstadt and in certain cairns of Switzerland; nothing, in a word, which approaches those overloaded ornaments so frequent in Helveto-Burgundian and Merovingian tombs. The palafittes of the age of iron are also much more frugal as regards objects of apparel than the tombs. Many of the objects which it has been agreed to term Etruscan, and which are very abundant in the north as well as at Hallstadt, are wholly absent, especially the clasps with a double spiral, the bracelets of bronze like collars, as well as that variety of ear-drops and chains which distinguish the ancient tombs of our environs. We may mention, further, the complete absence of that particular kind of design representing a circle with a point in the middle, which is frequently found at Hallstadt in the Etruscan necropoles, and even on the walls of the dolmens of Bretagne. Lastly, the palafitte of the Tène, equally with that of Tiefenau, has never furnished scramasax nor true spar.

IV.—RELATIVE AGE OF THE PALAFITTES, OR LACUSTRIAN CONSTRUCTIONS.

We should but imperfectly satisfy the curiosity of our readers if we did not attempt to answer a very natural question which must occur to every one: From what epoch do the constructions on piles take their date?

It is beyond doubt that the duration of each of the periods we have been reviewing was very long. They bear each their peculiar stamp, which can be impressed only by time, among populations which had a fixed residence, and whose prolonged sojourn in the different stations of our lake is attested by a considerable accumulation of ruins. It is equally certain that the lacustrian constructions ascend to a very remote epoch, since there exists no tradition, no legend which makes any allusion to them; since ancient chronicles are wholly silent with regard to them, and none of the authors of antiquity who have spoken of Helvetia make any mention of them. It is idle, therefore, to aim at assigning to them precise dates; it is as much as can be expected if the latter phases of

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* We might be tempted to add the potter's wheel, as well as red bricks baked in the kiln, the occurrence of which at the Tene is beyond a doubt. It was in fact, the opinion which we pronounced in the former edition of this work. But having reflected thereon, we are disposed to adopt the opinion of our friend, M. Keller, who regards these two branches of industry as of Roman importation, though probably anterior to the invasion.

this long period may be brought into some connexion with our historical epoch We know scarcely more than that with us, as in the north of Europe, the age of stone preceded the age of bronze, as this preceded the age of iron.*

AGE OF THE PALAFITTES OF STONE.

The teneviere or palafittes of the age of stone, from the very fact that they are the most ancient, are least susceptible of a chronological determination. As in geology, there can be no question here except of a relative chronology. If it is beyond doubt that the palafittes of the age of stone are anterior to those of the age of bronze, it is not less certain, on the other hand, that they are posterior to the first traces of man as revealed to us by modern geological researches, more especially: *a*, at the epoch of the hatchets of Moulin-Quignon and of Abbeville, when man was a cotemporary of the mammoth; *b*, at the epoch of the osseous breccias of the Pyrenees, when MM. Lartet and Christy show us man associated with the reindeer and carving on its horns the image of some domestic animals which he possessed, including the reindeer itself;† *c*, in the kökkenmödings of Denmark, which contain no trace of cereals or cultivated fruits, and in which we find, as regards domestic animals, only the dog; *d*, in the turf-pits of Iceland and of the mouth of the Somme, which contain the great-horned elk, (*cervus megaloceras.*) All these epochs, if it be that they are distinct, possessed only the hatchet rudely cut by blows, while that of our tenevieres is always ground and smooth.

If more precise data respecting the epoch of the tenevieres are ever obtained, it will be through the study of deposits, rather than from written documents. As an essay towards this geological chronology, we already possess some contributions. M. Morlot‡ has taken advantage of a section made in constructing the railroad across the cone of dejection of the Tinnière, near Villeneuve, to study the structure of the cone. He has recognized, we are assured, the traces of three epochs distinctly superposed—the Roman epoch, the epoch of bronze, and the epoch of stone, each represented by an ancient stratum. By comparing the depths of these different beds, he has been led to assign to the age of bronze an antiquity of from 29 to 42 centuries, and to the age of stone, one of from 47 to 70 centuries. M. Gillieron§ likewise, from a study of the lake of Bienne, has arrived, as has been already seen, at a result nearly analogous, since he has carried back the station of stone of the bridge of Thielle to at least 67½ centuries.

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* Some authors, relying on the fact that at Alise the arms of the three eras are found associated in the same foss, arrows of stone with those of bronze and iron, have thought themselves justified in calling into question the succession of ages above mentioned. But it must not be forgotten that Alise was the theatre of a conflict in which were engaged troops drawn from every part of Gaul, a portion of whom might well be greatly in arrear as regards their armament. Thus, in 1815, our fathers saw in the Russian army, Cossacks armed with the bow and arrows, beside troops better equipped. From the fact that there have been improvements in successive ages we cannot conclude that these improvements have been everywhere simultaneous in the ancient world. Hence we should not be surprised if it were shown that the lacustrian populations of Italy had already reached the age of bronze, when those of our lakes were still at the age of stone, just as it is probable that iron was known in Etruria earlier than in Helvetia. According to William of Poitiers, arms of stone were in use even in the eleventh century, at the battle of Hastings. (*Jactant Angli cuspides et diversorum generum tela, sævissimas quoque secures et lignis imposita saxa.*)

† According to the latest researches of M. Lartet, the mammoth is also found there.

‡ *Etudes geologico-archéologiques. Bulletin de la Soc. Vaudoise.* Tom. VI, p. 325.

§ *Notice sur les habitations lacustres du pont de Thielle. Actes de la Soc. jurassienne d'emulation,* 1860. M. Troyon, on the other hand, has arrived at a much lower number in estimating the age of the pile-works of Uttins, near Yverdon, namely, at fifteen centuries only before our era. But, according to a recent memoir of M. Jayet, this calculation is inadmissible, because the establishment of Uttins must have existed in a lagoon.

AGE OF THE PALAFITTES OF BRONZE.

The uncertainty would be nearly as great in regard to the age of the palafittes of bronze as of those of stone, if in this inquiry we were restricted to the stations of Switzerland. The antiquities of these stations had hitherto found their analogue only in the north of Europe, on the shores of the Baltic; but there, also, they are without any positive connexion with the written history of the country, which does not ascend very far. If, then, there exists anywhere a connexion between the age of bronze and history, it might be looked for rather to the south, in Italy, where we find the seat of the most ancient populations of Europe. But no lacustrian constructions had been indicated in Italy. Desiring to be enlightened on this subject, we visited, in 1860, the lakes of Lombardy, and were not long in verifying the existence of pile-works and antique objects, altogether similar to our own, in the peat-mosses of the Lago Maggiore. Since that time, these researches have been prosecuted with as much success as zeal by M. Moro, as well as by M. B. Gastaldi, who has given us an excellent work on the lacustrian antiquities of Italy.[*] We have ourselves more recently (1863) explored the lake of Varèse, in company with MM. Stoppani and G. de Mortillet, nor were we disappointed in discovering several stations of the age of stone, as well as manifest traces of that of bronze. One of these stations is the small isle (isoletta) on which the family Litta has reared a pleasure-house. Although larger than the isle of the little lake of Inkwyl, near Soleure, the isoletta is, like the other, artificial, so that to this day we are deriving benefit from labor performed by the people of the age of stone. Still later, a whole series of new stations has been discovered in this same lake of Varèse, as well as several in the small lakes of Brianza.[†] Lake Garda also contains well characterized palafittes, which were brought to light through works executed some years ago around the fortress of Peschiera. Among objects in metal, collected under the superintendence of M. de Silber, an Austrian officer, and which now form part of the museum of antiquities of Munich, some are of copper, the rest of bronze.[‡]

The researches of M. Paolo Lioy[§] have recently revealed not less curious palafittes of the age of stone in the peat-mosses of Lake Fimon, near Vicenza. Add to this, that in 1864 we succeeded in discovering lacustrian stations in all respects similar to our own in the lakes of Bavaria. Through the liberality of the Bavarian government, these researches are continued, and already constructions have been announced in six lakes,[‖] most of which ascend to the age of stone, though some are of the age of bronze, especially at Lake Starnberg, near the isle of Roses. This isle offers a counterpart of the isoletta of Lake Varèse, in being artificial like the latter. Our agent has there recovered, together with numerous relics of pottery, a fine bronze pin, which forms part of the public collection of Munich. There had previously been found, in digging the foundations of the royal chateau on the island, Roman as well as other more recent objects, which would seem to prove that this isle has not ceased to be inhabited since its formation by the first possessors of the soil, in the age of stone.

In consequence of these discoveries, the Academy of Sciences of Vienna judged that the time had come for undertaking researches also in the lakes of Austria. M. Hochstetter, the eminent traveller and geologist, having been charged with the exploration of the lakes of Carinthia, soon encountered traces of lacustrian constructions in several of them, especially those of Wörth, d'Ossiach, and the

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[*] *Nuovi cenni sugli oggetti di alta antichità*, Torino, 1862.

[†] *Stoppani, Atti della Società di Scienze naturali.*, vol. V. In Lake Varèse are the stations Keller, Desor, Bodio, Bardello, Cazzago, and Isolina.

[‡] *Mittheilungen der antiq. Gesellschaft in Zurich*, vol. XIV. Fifth report of M. Keller.

[§] *Le abitazioni della età della pietra nel lago di Fimon nel Vicentino :* Acts of the Venetian Institute, 1864, 1865.

[‖] Siebold. *Pfahlbauten in Baiern*, in the *Comptes Rendus* of the Academy of Munich.

small lake of Keutschach to the south of Lake Wörth. The relics collected, as yet not numerous, are associated with stony shallows, which recall our teneveires, and hence indicate the age of stone.* The marshes of Pomerania also, it would seem, are about to furnish their contingent; in the circle of Lubtow have just been discovered palafittes in all respects analogous to those of Robenhausen. Here two archæological strata are recognized. In the lower are found fragments of pottery, vases with figures, axes in serpentine, silex and amphibole, a chisel with circular socket of bronze, horns of the stag and roe, carbonized grains, especially of wheat, barley, and peas. In the upper stratum, which is very distinct from the former, are found also utensils of iron. We shall receive, no doubt, without delay, more detailed information respecting these interesting discoveries. The lakes of France, apart from those of Savoy,† have not yet been explored in a persistent manner. There is good reason to suppose, however, that when the peat beds and ponds, which are so numerous in many departments, shall have been attentively examined, traces will be eventually detected of our different lacustrian ages, for it is scarcely possible that Gaul should have been unoccupied while numerous tribes existed at the foot of the Alps.

The skilful explorations of MM. Strobel and Pignorini in regard to the palafittes of Italy are well known.‡ In the Parmesan are small hills, which bear the name of *Monti*, and from the sides of which is taken a sort of ammoniacal earth, mixed with cinders, called *terra mara*, which serves as a fertilizer for meadows. In excavating a gallery in one of these monti, M. Strobel, assisted by M. Pignorini, found posts supporting a sort of floor, and between the posts objects of bronze, in all points similar to those of our lakes, accompanied by earthen vessels of very careful workmanship, although fabricated without the help of the wheel.§ Consequently this artificial hill, with its fertilizing earth, which encloses the remains of an age evidently later, (Etruscan and Ligurian,) has for its nucleus and origin a construction on piles of the age of bronze, whence it must be concluded that the age of bronze is anterior to the establishment of all the other populations which have left their remains on the flanks of these hills.

The objects found at these different stations, those of Lago Maggiore, described by M. Gastaldi, those we have ourselves collected at the lake of Varèse, those of the hill of Castione in the Parmesan, of the Veronese, are sufficiently numerous and well characterized to leave no doubt about their perfect identity with those of the Swiss lakes. The lakes of Italy, the plain of the Po, and the Veronese have consequently been strewn, at a certain epoch, with constructions on piles, erected by populations having the same usages, the same customs, and to all appearance pertaining to the same stock. But can it be admitted that the Roman authors, so many of whom knew and appreciated the beautiful sites of the lakes of Italy, that Pliny, among others, who had his country seat on the banks of Lake Como, would have neglected to mention lacustrian constructions in the vicinage, and perhaps under the windows of his mansion, so litle sparing as he was of details respecting the men and things of his time? If, then, this celebrated writer had not a single word to bequeath to us upon lacustrian habitations, we feel authorized to conclude that these constructions existed no longer

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* Bulletin of the Academy of Sciences of Vienna, vol. LI, *Bericht über Nachforschungen nach Pfahlbauten in den Seen von Kärnthen und Krain*, by Professor Hochstetter.

† See on the palafittes of the lake of Bourget the excellent memoir by M. Laurent Rabut, *Habitations lacustres de la Savoie*, with an atlas of 16 plates: Chambery, 1864. M. Rabut still continues the sheets of this work, availing himself of the valuable assistance of M. Costa de Beauregard.

‡ Pignorini and Strobel, *Die Terramara-Lager der Emilia, Mittheilungen der antiq. Gesellschaft in Zürich*, vol. XIV, Fifth Report of M. Keller.

§ A similar mound formed of the débris of different ages has been lately discovered by M. Pignorini, at Basilicanova of Montechiarugolo, in the Parmesan. See M. G. de Mortellet, *Materiaux*, t. II, p. 68.

at that date, (seventy-nine years after Christ,) but that they had even perished from the memory of men. At the same time, the fact that we find the same constructions and the same utensils in the Milanese and Venetia sufficiently proclaims to us that the inhabitants of the lakes of Lombardy did not live in isolation at the foot of the Alps.

The same civilization could not be simultaneously spread over so wide a space as the plain of the Po and the steppes of the Alps, without also penetrating even to the coasts. The advantages of the sea are too numerous and obvious not to attract mankind when arrived at a certain degree of culture. This single fact would suffice at need to justify the opinion that the bays and havens of the Italian coast must have been inhabited at the epoch of bronze. And as the utensils and ornaments of that epoch attest a maritime commerce, we hazard nothing in supposing that this commerce was conducted through the ports of Liguria and Umbria. It was from these, among others, that the inhabitants of the stations of Lombardy derived the tin which entered into the composition of their arms and utensils of bronze, and which could scarcely have come except from the Cassiterides.* Once discharged on the coasts of Italy, the imported metal must have been spread abroad, especially among populations having the same manners and usages, as was the case on the lakes of the two slopes of the Alps. The Alpine chain could be no obstacle to these communications. There is no reason for supposing that the passages of the Alps, St. Bernard, and St. Gothard, were at that epoch more impracticable than in our day, at least for transport on the backs of men and horses, which was probably the only mode in use. Between this hypothesis and that which derives the bronze of the Swiss lakes from the shores of the Baltic, we do not think there is room for hesitation. In favor of these relations between Switzerland and Italy, may be further alleged the perfect identity of composition of the bronzes, which are composed only of copper and tin, the latter metal varying, as has been shown above, from four to twenty per cent.

It has been asked if the preparation of bronze was not an indigenous invention which had originated on the slopes of the Alps, suggested by the presence of the ore of copper, which is quite abundant on the south flank of the Piedmontese chain. In this idea we acquiesced for a moment. But we are met by the objection that, if this were so, the natives, like the ancient tribes of America, would have commenced by manufacturing utensils of copper. Yet thus far no utensils of this metal have been found except a few in the strand of Lake Garda. The great majority of metallic objects is of bronze, which necessitated the employment of tin, and this could not be obtained except by commerce, inasmuch as it is a stranger to the Alps. It would appear, therefore, more natural to admit that the art of combining tin with copper, in other words that the manufacture of bronze, was of foreign importation. On this hypothesis it would still remain to determine whether the principal element, the copper, was derived, like the tin, from abroad, or whether, as M. Wibel maintains,† the ores of the country were employed. The first analyses of M. de Fellenberg had flattered us with an interesting solution by means of the nickel contained in the bronzes of the Swiss and Italian palafittes, and which characterizes especially the ores of the Alpine copper. But it has been seen above that, from the later researches of this learned chemist, the nickel has not the importance which he was at first disposed to ascribe to it, seeing that is found also in the bronzes of the north. It may consequently well be that the copper also was of foreign

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* See on this point the supplement to the work of Nilsson, which has lately made its appearance, 1866, p. 7: also "The Cassiterides: an inquiry into the commercial operations of the Phenicians in western Europe, with particular reference to the British tin trade." by G. Smith, London, 1863.

† *Die Cultur der Bronze-Zeit*, 1861, p. 36 and following

importation. Now, in view of the prodigious quantity of bronze manufactured at that epoch, this single branch of commerce must itself have necessitated the most incessant commercial communications.

RELATIONS BETWEEN THE AGE OF BRONZE OF THE NORTH OF EUROPE AND THAT OF THE PALAFITTES.

In general no hesitation exists in comparing the bronze antiquities of our palafittes with those found in the tombs of Scandinavia. This parallelism was natural and, in some sort, inevitable in the inception of lacustrian studies; indeed, from the moment when for the north of Europe a prehistoric epoch characterized by bronze was admitted, the coincidence could not but impose upon the Swiss antiquaries, who had just detected in their own lakes palafittes which contained in point of metal nothing but bronze. It was the material much more than the form or structure of the objects which was considered. Nor can it be disputed that several of the more common articles—such as swords, axes, lances, reaping-hooks—have nearly the same form. But, on the other hand, it can as little be denied that among the Scandinavian bronzes there are found numerous objects which are entirely wanting in our palafittes. It is sufficient in this respect to compare the figures of the work of Worsaae* with the arms and utensils of our lakes.

To one who surveys the magnificent collections of the museum of Copenhagen or of Mecklenburg, it is evident that the so-called age of bronze there presents itself with an affluence and finish which would be sought in vain in our palafittes of the second age. On the other hand, it is impossible to overlook a striking resemblance between the bronzes of the north and those of Hallstadt. This resemblance applies not only to the form and physiognomy of the objects; it is equally apparent in certain types of the designs which are common to both groups, especially the complex helix which is found even on the arms and axes, (Worsaae, tab. XXVIII, fig. 113, and tab. XXXI, fig. 130,) and which are wholly unknown to our palafittes. We are at liberty, therefore, to ask whether the correspondence is not perhaps more complete between these two localities than between the palafittes of our lakes and the tombs of the north. But at Hallstadt, the bronzes are found to be associated with arms and utensils of iron; swords have even been found of which the blade was iron and the hilt of bronze. Nothing of this sort has yet been recognized in the north, whether they be really absent or have been overlooked by reason of the wasted condition in which iron is generally found in the tombs. If it be ever demonstrated that the bronzes of Hallstadt and those of Scandinavia are cotemporary, there could no longer be room for surprise at the superior perfection of the last, since it would thus be demonstrated that they pertain to a more recent epoch—one cotemporaneous, perhaps, with historic populations.

M. Nilsson sustains this opinion in his remarkable work on the primitive people of Scandinavia.† The learned Swedish archæologist and naturalist recog-

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* *Nordiske Oldsagen*, Copenhagen, 1859. In the number of remarkable objects which are entirely absent with us, may be cited, among others, the ornamented hatchets, (Tab. 28.,) curious knives or razors, (Tab. 36,) vessels in bronze of finished workmanship, (Tab. 61,) shields, (42 and 43,) and, finally, the *lurer* or trumpets of war, (39 and 40,) of which there exists no trace in the palafittes.

It is here, perhaps, that we should mention another figure not less frequent—the circle, with a point in the middle—which is found alike at Hallstadt, in the north of Europe, and in the dolmens of Brittany. Should we associate it with the sign of the sun, the *Ra* of the Egyptians, which occurs so frequently in the writings and on the monuments of Egypt, or as M. Nilsson thinks, with the sign of *Bal*, the sun-god of the Phenicians? This sign is not entirely a stranger to our palafittes; we have observed it on certain bracelets and on the blade of a knife of bronze, but derived from stations which might not have been completely unknown to the age of iron. Other antiquaries see in it only an ornament which was recommended by its simplicity and a certain natural elegance.

† *Die Ureinwohner Scandinaviens*, 1863.

nizes the trace of the Phenicians not only in ancient monuments, but also in the usages and superstitions of the country, as well as in the names of different localities which relate, for the most part, to the worship of Baal, the god of the sun or the Apollo of the Phenicians. The navigator Pythias would thus have been a Phenician of Marseilles, visiting the stations and colonies strung along the Scandinavian coasts.

This is not the place to discuss the weight of the arguments on which M. Nilsson relies. We are quite disposed to admit his conclusions respecting the part which the Phenicians have played in the north; but it is not necessary on that account to transport these Phenicians back to the age of bronze, especially if there is any probability that the bronze objects of Scandinavia, and the tombs which cover them, date from the age of iron, as is the case with the antiquities of Hallstadt and those of the Etruscan sepulchres of the Romagna, which form at this time objects of exploration and study to M. Gozzadini. The Phenicians certainly knew the use of iron, and it can scarcely be conceived why they should have excluded it from their commerce on the Scandinavian coasts.

Neither are the Etruscans to be passed by in silence in the present discussion. Occupying Tuscany and Umbria, they had there arrived at an advanced degree of civilization which could not fail to react on their neighbors, and to extend itself, at all events, to the inhabitants of the plain of the Po and foot of the Alps. We have inspected, with this idea in view, the different collections of Etruscan antiquities in Italy. It is impossible to mistake a certain general resemblance with many of our lacustrian objects; but this resemblance does not extend itself to details. The antiquities of Etruria attest a civilization much more advanced, and particularly processes in metallurgy, which were unknown to the people of our stations of bronze. The Etruscans, moreover, were acquainted with iron as well as the Phenicians, and it has already been seen that the composition of their bronzes is different, since it contains lead, which is entirely a stranger to our bronze epoch. Now, it can scarcely be admitted that, if the Etruscans were the purveyors of the lacustrian stations of the Lago Maggiore, no traces of that manufacture should be found at those stations, nor any objects in iron, while at that epoch iron was very widely in use.*

We must look, then, beyond both the Etruscans and Phenicians in attempting to identify the commerce of the bronze age of our palafittes. It will be the province of historians to inquire whether, exclusive of Phenicians and Carthagenians, there may not have been some maritime and commercial people who carried on a traffic through the ports of Liguria with the populations of the age of bronze of the lakes of Italy, before the discovery of iron. We may remark, in passing, that there is nothing to prove that the Phenicians were the first navigators. History, on the contrary, positively mentions prisoners, under the name of Tokhari, who were vanquished in a naval battle fought by Rhamses III, in the thirteenth century before our era,† and whose physiognomy, according to Morton, would indicate the Celtic type. Now, there is room to suppose that if these Tokhari were energetic enough to measure their strength on the sea with one of the powerful kings of Egypt, they must, with stronger reason, have been in a condition to carry on a commerce along the coasts of the Mediterranean, and perhaps of the Atlantic. If such a commerce really existed before the time of

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* According to Homer, Pseudomentes conducted a commerce of brass and iron, (Odyssy I, 184.) The Bible mentions iron in several places: Chariots of iron, in Judges I, v, 19; a bed of iron in Deuteronomy V, v, 11; the use of iron is even considered a profanation in monuments consecrated to worship, Exodus XX, v, 21; Deuteronomy, XXVII, v, 5; Joshua VI, v, 24, where it is said that after the taking of Jericho, utensils of brass and iron were carried away. Indeed, agreeably to the Mosaic tradition, the age of iron would ascend beyond the deluge, since Tubal-Cain even then worked in iron. According to this chronology, there remains but very little margin for the ages of bronze and of stone, which, nevertheless, by the acknowledgment of every one, embraced very long periods of time.

† Nott and Gliddon, *Types of mankind.*

the Phenicians, it would not be limited to the southern slope of the Alps; it would have extended also to the people of the age of bronze in Switzerland. The introduction of bronze would thus ascend to a very high antiquity, doubtless beyond the limits of the most ancient European history.

AGE OF THE PALAFITTES OF IRON.

The uncertainty is not so great in regard to the epoch of iron, which appears to have immediately succeeded the age of bronze. The arms and utensils collected among the piles of Marin have no longer the same exceptional character. Though still strange to positive history, they connect themselves, nevertheless, more or less directly, with other events the date of which may be fixed at least approximately. It is this which, with us, gives to the palafitte of the Tène near Marin its preponderant importance. It is, in Switzerland, the bond of union between the lacustrian ages and the commencement of history. In effect, the utensils and arms of the Tène, for not being Roman, are not therefore altogether strange to us. It is sufficient to compare them with those which are found near Berne, at a locality which it has been agreed to designate by the name of the "battle-field of Tiefenau," because remains of every kind are there heaped pell-mell as on the theatre of a conflict. Among these remains, which have been described by M. Jahn* and sketched by M. de Bonstetten,† are found, in point of arms, some hundred swords and large lance heads identical with those of the Tène; in point of utensils, rings, brooches, remains of coats of mail, fragments of iron bracelets, débris of chariots, all much impaired by oxidation, but bearing no less the same stamp with the objects of our lacustrian station. Coins are also found there; coins of silver and pinchbeck, which are not Roman, but Gallic and Marseillese. Consequently, if the station of the Tène is cotemporary, it must ascend to the epoch when the Helvetians, who are only a branch of the Gauls, (though coming from Germany,) inhabited Switzerland.

ORIGIN AND FILIATION OF THE LACUSTRIAN RACES.

The antiquity of the lascustrian races can scarcely be considered without bringing into view the question of their origin and filiation. Whence came these populations which had so great a predilection for the water, and to what stock do they pertain? However striking this propensity to establish themselves on the water rather than on *terra firma*, we still should not exaggerate its importance so far as to conclude from this that all those who constructed palafittes were necessarily of the same race. Here again there are grounds for a distinction between epochs. It has already been seen that, to all appearance, the populations of the age of iron pertain to the great Gallic stock, that they are the same Helvetians who, under Divicon, beat the Romans, and who, still later, emigrated under Orgetorix. They were not Autocthones, for ancient authors apprise us that they proceeded from the banks of the Rhine. On the other hand, the relics of their civilization, such as they occur in the palafittes of the Tène, bear in too distinct a manner the Gallic impress not to tempt us at once to identify them with the similar objects which are furnished by the tombs and battle-fields of Gaul. But what relation do these Gallic constructors or inhabitants of the palafittes of the Tène bear to the populations of the palafittes of bronze?

When, in a collection, somewhat complete, of lacustrian antiquities, we consider on one hand the objects collected in the palafittes of the age of bronze, and on the other those of the age of iron, we are struck with the disparity which

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* *Memoires de la Soc. Histor. du canton de Berne*, t. II, p. 350.

† *Supplement au Recueil d'Antiquites Suisses*, Lausanne, 1860.

prevails between these two series. Arms, utensils, objects of luxury, all are different; on one side, in the series of iron, the stamp of a people active, energetic, looking to practical results; on the other, the care of details, the love of display, but nothing which announces vigor, sustained action, progress. Inquirers have been thus led to think that the question here related to two different races—the one large and vigorous; the other small and feeble—which would seem to be further corroborated by the difference of size indicated by the arms and skeletons of the two ages. The tribes of the age of iron would appear to have arrived as conquerors, bearing with them new elements of the highest importance, among which were iron, bronze of easy purchase, and coined money.

But if, with us, there is a contrast between the antiquities of the age of bronze and those of iron, the case is not the same elsewhere. Thus, the tombs of Hallstadt, in Austria, seem to indicate an epoch of transition between the age of bronze and that of iron. Numbers of utensils which, with us, are characteristic of the age of bronze, are found here to be replaced by iron, among others the celts or axes with four pinions. Other objects bear designs which remind us of those found in the Etruscan sepulchres. The tombs of Hallstadt would hence seem to form a sort of transition between the age of iron and the age of bronze. It is not without reason, therefore, that M. Morlot discerns here a given point, a chronological horizon, to which recourse may be had for determining the relative age of certain isolated objects found in Germany, France, Switzerland, and even in Italy and the north of Europe. (*Materiaux*, January, 1866, p. 235.)

We are thus led to infer that iron was probably introduced from Italy to the northern slope of the Alps by the Tyrol. It was at a later period that the Helvetians, after having been completely familiarized with its uses and returning from Germany, carried it into Switzerland.* This invasion would not necessarily have involved the annihilation of the tribes of the age of bronze. It is possible, even probable, that these last continued to subsist by the side of the conquerors; for, as M. Troyon has shown, there are still found in the age of iron, bracelets of too small an opening to have passed over large hands, or too massive to have been closed upon the arm.

Nothing indicates that the Helvetians had crossed the Alps, but the relations which had existed from the age of bronze, and perhaps of stone, between the two slopes of the chain, did not on that account cease. We find in Switzerland, as well as in France, in Germany, and as far as the north of Europe, associated with arms of iron, objects in bronze of exact workmanship, much superior to all which has been furnished us by the palafittes of the age of bronze. The ornaments are no longer simply lines arbitrary and stereotyped; they are imitations of nature, figures of animals artistically engraved. Such, among others, is the bronze of Graechwyl, described by M. Jahn† and preserved in the Museum of Antiquities of Berne. It is impossible here to mistake the oriental type, (Assyrian or Etruscan,) which is corroborated by the chemical analysis made by M. R. de Fellenberg, and which indicates a considerable proportion of lead. Now this metal, as has been seen above, is a stranger to the age of bronze of the palafittes.

The Helvetians do not appear to have participated in the lacustrian constructions. We know, on the contrary, from the testimony of Cæsar, that they lived in villages which they burned when they emigrated into the Gauls. If these villages had been constructed on the water, it can scarcely be believed that the Romans would have passed in silence by such a peculiarity. The palafitte of the Tène, near Marin, is but the more interesting as an exception to the rule.

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*According to M. Keller, the introduction of iron took place on the arrival of the Helvetians, who came not as conquerors, but merely reoccupied their ancient dwelling-places after being chased by the Germans from the countries of the Rhine and the Maine.

†*Etruskische Alterthümer gefunden in der Schweiz*, in the *Mitthelbungen der antiq. Gesellschaft*, Zurich, t. VIII. See, also, Morlot, *Etudes Geologico-archæologiques*, p. 314.

It is as the last echo of an order of things which had no longer any reason for existing. We cannot agree with M. Troyon that this was a refuge of the populations of the age of bronze. The objects there accumulated bear in too high a degree the Gallic or Helvetic stamp. We should be rather disposed to recognize here a bazaar or an arsenal erected by the Helvetians after their return into the country, in consequence of the defeat of Bibracte. (See Keller, 6th Report, page 6.) They may have chosen this lonely corner, in the midst of marshes, making use of or restoring the ancient piles, there to continue the ancient usages. This establishment may have acquired much development* and have become sufficiently important to attract the Romans, who would introduce some of their own pursuits, especially the manufacture of bricks and tiles.

RECAPITULATION.

After having endeavored to determine the population of the palafittes of the iron age of our lakes, by referring it to the great Gallic stock, of which the Helvetians were but a branch, we are naturally led to inquire also the origin of the tribes who constructed the much more numerous palafittes of the two preceding ages. Who were they? Whence came they, and what connexion exists between them and the people of the age of iron? Here, however, positive data almost entirely fail us. The field of conjecture is unlimited, and those whom it interests may allow themselves free career. Habituated to other methods, we shall not essay to follow our fellow-laborers into this domain, which has but too many attractions for some archæologists. We could, indeed, give our readers a semblance of satisfaction through favor of certain names, by designating, for example, the populations of the age of bronze under the name of Celts,† and those of the age of stone under that of Iberians; but this would be without profit for science, leaving out of view the risk we should run of propagating uncertain, and, perhaps, erroneous ideas.

The following is all that we are authorized to conclude from the actual state of our knowledge: 1st, that the inhabitants of our palafittes of bronze were probably of a different race from that of the age of iron, and smaller; 2d, that they had commercial relations with maritime races, who conveyed to them at least a part of the elements of bronze, (the tin,) as well as amber and trinkets of glass; 3d, that the navigators in this traffic were seemingly neither Phenicians nor Etruscans, for we may presume that these would not have failed to impart also the arms and utensils of iron with which they were familiar, and which, when fallen to the bottom of the lake with other objects of the epoch, would have there been undoubtedly preserved, like those of the palafittes of the Tène.

The uncertainty is necessarily still greater in regard to the populations of the age of stone, who lived more or less isolated on the borders of our lakes, and whose commerce with the foreigner is at least very problematical, although they were already husbandmen and rearers of cattle. Having no theory to propose as to their origin, we confine ourselves to regarding them, for the moment, as the first inhabitants of our soil, reserving to ourselves a recurrence to this question, if there should ever be discovered in Switzerland traces of man cotemporary with the reindeer and the mammoth.

Guided by the instinct common to all men, the inhabitants of our tenevieres would fabricate utensils and arms with the only instruments within reach—silex and bone. In the course of time they would learn to cultivate the ground, to rear cattle, and still later, through their communications with Italy, would be initiated in the art of manufacturing bronze. In this manner the knowledge of

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* We shall shortly publish a memoir on the topography of the station of the Tene.

† The reader will remark that we have carefully avoided this name in the course of the present work.

metals would be gradually introduced, without there being need of having recourse to violent invasion. A little more time would have been required for this knowledge to penetrate into eastern Switzerland and Germany, but it would arrive there nevertheless, as certain objects of bronze found at Meilen and those of the lakes of Bavaria attest. Thanks to the more energetic means of defence afforded by the new arms, a residence on the water would be no longer so indispensable. The population would by degrees become established on *terra firma*, while preserving the lacustrian constructions only for magazines or places of assemblage.

On this hypothesis, the passage from the epoch of stone to that of bronze would be effected without overthrow or violence. It would be the expression of a slow and gradual progress, such as humanity is naturally inclined to realize when untoward circumstances do not intervene. This view is strongly corroborated by the new discoveries made in Italy, where we find the two ages intimately associated, especially in the sepultures of the Milanese. We may also invoke in its favor the similarity of form of certain customary objects, notwithstanding the fundamental difference of material. Thus the arrows of the age of bronze have remained the same with those of the age of stone; the large-headed pins of the former age are evidently an imitation of those of the latter; the pottery has not changed, any more than the manner of preserving fruits and provisions; finally, the lacustrian constructions have continued, although with some modifications, as above shown. It would result, then, that it was the same people who inhabited our soil during the ages of stone and of bronze, and up to the time of the invasion by the Helvetians.

EXPERIMENTAL AND THEORETICAL RESEARCHES

ON

THE FIGURES OF EQUILIBRIUM OF A LIQUID MASS

WITHDRAWN FROM THE ACTION OF GRAVITY, &c.

BY J. PLATEAU, PROFESSOR IN THE UNIVERSITY OF GHENT.

Translated for the Smithsonian Institution from the Memoirs of the Royal Academy of Brussels.

FIFTH SERIES.

CONTINUED FROM THE SMITHSONIAN REPORT FOR 1864, PAGE 369.

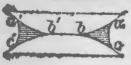
Theory and description of a new process for the realization of figures of equilibrium.

§ 1. In the second and fourth series of this treatise I have applied my process of the immersion of a liquid mass in another liquid of the same density, and with which it cannot mingle, to the realization of a part of the figures of equilibrium, infinite in number, which pertain to a liquid mass supposed to be without weight and in a state of repose. In the present series I shall indicate a wholly different process, much more simple and commodious, which enables us to attain the same end, and I shall state a portion of the numerous consequences which have been afforded by its employment and by the theoretical principles on which it rests.

§ 2. We will first notice some of the curious results which serve, so to speak, as a transition between these two processes. It will be remembered that oil immersed in the alcoholic liquid easily assumes a laminar form; our liquid polyhedrons, for example, (2d series, §§ 31 to 35,) became transformed into systems of films by the gradual exhaustion of almost the whole of the oil. It will be remembered also that these films in general appeared plane, but that the octahedron gave rise to a system of films evidently curved. Curved films were likewise developed in the experiment of § 22, 4th series.

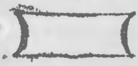
Let us recur to this last experiment and repeat in what it consists. After having formed in the midst of the alcoholic liquid a cylinder of oil between two equal rings of iron wire, parallel and placed opposite to one another at a distance considerably less than two-thirds of their diameter, we gradually withdraw liquid from the mass by means of the small syringe. The surface comprised between the rings, it will be remembered, becomes then more and more concave, while at the same time the bases of the figure sink, become plane, then concave, and all these curvatures continue to increase with the progress of the absorption. Ultimately we see three films produced, of which one has its origin at the centre of the figure at the time when the two concave bases approach so as nearly to touch one another with their summits, and the two others begin to show themselves when the bases become tangents, along their borders, to the portion of surface comprised between the rings; the first of these films is plane, the rest appear conic. Fig. 1 represents a meridian section of the system, in which the films have already acquired a certain development, the dotted lines being sections of the planes of the rings. The films afterwards become more and more extended by the cautious abstraction of new quantities of oil, and the whole tends, as a final result, to give a laminar figure approaching to two truncated cones united by their small bases; but one or other of the films always breaks before the little thickish mass which surrounds the plane film and attaches it to the curved films can be entirely absorbed.

Fig. 1



It is here that the experiment stops in the fourth series. Let us now proceed still further. Suppose it to be the plane film that breaks; then the oil of which it is constituted unites itself rapidly with that which formed the rest of the system, and the whole is reduced to a single film, somewhat thick, which remains attached to the circumference of the two rings, presenting, in the meridian direction, a curvature slightly concave, (Fig. 2.) In comparing figs. 1 and 2, it will be observed that in the first the meridian concavity between the two rings is much more considerable than in the second—a difference easily explained if we consider that the small thick mass which surrounds the plane film, and whose meridian section is represented in *a*, *b*, *c*, and *a' b' c'*, (Fig. 1,) has three surfaces, which, for the sake of equilibrium, must respectively exert the same pressure. In effect, the curvature in those which have for meridian lines the arcs *ab* and *cb*, is evidently concave in all directions around any one point, while the third, whose meridian arc is *ac*, presents in each point a convex curvature in the direction perpendicular to that arc; whence it is requisite that, in a state of equilibrium, the concavity of this same arc should be much more decided than that of the arcs *ab* and *cb;* but it is clear that this condition could not be fulfilled with a meridian curvature as weak as that in Fig. 2.

Fig. 2



§ 3. The liquid figure being thus reduced to a single film,* let us gradually raise the upper ring. The film will now be extended, and from being rather thick, as was above said, will become very thin; the meridian concavity will be more decided, and we shall thus obtain a laminar figure (Fig. 3) whose form recalls in every respect the surface to which I have given (4th series, § 14) the name of *catenoid.* If we continue to raise the upper ring, we shall arrive at a point where equilibrium is no longer possible, and we shall see the figure become spontaneously more and more constricted, till at last it separates into two parts which respectively proceed to form a plane film in each of the two rings. In repeating the experiment, I have attempted to arrest the upper ring precisely at the point in question, and have found that then the separation of the two rings was perceptibly two-thirds of their diameter; this ratio, it may be remembered, is that at which I arrived (4th series, § 21) as the limitary height of the partial catenoid.

Fig. 3

§ 4. It is a remarkable fact that the disunion of our laminar figure is effected in exactly the same manner with that of full figures (2d series, § 62:) at the moment which precedes this disunion, the constriction is converted into a cylindrical thread which is transformed into spherules of different diameters; only here the thread is itself laminar, as well as the large spherule; the other spherules

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*For this it is necessary that, in the preceding system, it should be the plane film which is broken; if it happened that the rupture took place in one of the curved films, it would remain to recommence the experiment, and, in order to operate with certainty, break the plane film with the point of the syringe a little before the spontaneous rupture is expected to take place.

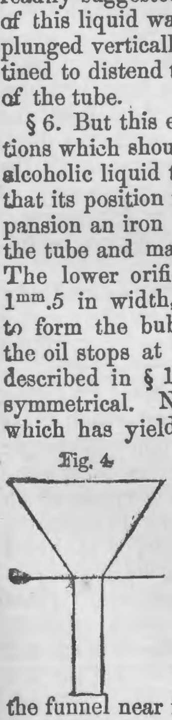
are too small to allow of easy verification; in other words, the thread may be said to constitute a slender tube, and the large spherule is a bubble whose interior is occupied by the alcoholic liquid.

Let us remark, in connexion with this experiment, that nothing is better adapted for the realization of that part of the nodoid (4th series, § 32) generated by a node of the meridian line, when this node approximates to a circumference of a circle (4th series, §35,) than the plane films which, after disunion, occupy the two rings. If we pierce one of these plane films in its middle, the liquid ring which the oil forms in retiring towards the metallic ring has, in relation to the radius of the latter, but little thickness, (4th series, § 36,) and, as far as can be judged by the eye, its meridian section is decidedly circular: with a metallic ring, for instance, whose radius is 35^{mm}, the width of the liquid ring, that is, the distance between its inner and outer circumference, is scarcely 3^{mm}.

§ 5. The facility with which, under the conditions of my experiments, the oil extends itself in thin films, connected with the fact above mentioned of the formation of a laminar spherule, naturally led to the belief that large spherical films of oil might be developed, or, in other words, that hollow bubbles of oil of considerable size might be obtained in the alcoholic liquid by distending them with the alcoholic liquid itself, as we obtain, in the air, bubbles of soap by inflating them with air. The experiment was completely successful. Analogy readily suggested the process: in order to form the bubble of oil a small mass of this liquid was first made to adhere to the lower extremity of an iron tube plunged vertically to a certain depth in the alcoholic liquid, and the liquid destined to distend the bubble was then slowly poured through the other extremity of the tube.

§ 6. But this experiment, so simple in principle, exacts a number of precautions which should be here pointed out. To facilitate the introduction of the alcoholic liquid the tube should be funnel-shaped at its upper end, and, in order that its position may be stable, it is necessary to adapt to the base of this expansion an iron disk of 7 to 8 centimetres in diameter, pierced in its centre by the tube and made to rest on the neck of the central aperture of the vessel. The lower orifice of the tube should be furnished with a thin rim of about $1^{\mathrm{mm}}.5$ in width, the object of which is to prevent the small mass of oil designed to form the bubble from partially rising along the exterior wall of the tube; the oil stops at the circumference of the little rim, conformably with the facts described in § 13 of the 2d series, and arranges itself in a manner perfectly symmetrical. Nor is the diameter of the tube a matter of indifference; that which has yielded me the best results is one of 16^{mm}. The section of the system is represented by figure 4.

Fig. 4.



It is evident that the alcoholic liquid with which the bubble is to be filled should have precisely the same density with the exterior liquid. This condition is satisfied without difficulty by previously withdrawing, through the faucet at the lower part of the vessel, a portion of the liquid contained in the latter, and using this portion for the distension of the bubble. It is necessary, of course, that the liquid should descend slowly and gradually into the bubble, especially at the commencement; it should, at first, fall drop by drop, then in a slender thread, and should enter the funnel near its upper edge, in order that, gliding along the inclined wall before descending into the tube, it may have less velocity. But if, to accomplish this operation, we are content to hold in the hand the flask containing the liquid in question, we shall never succeed, whatever pains be taken, in giving to the bubble the whole diameter which it might acquire, and this for two reasons: in the first place, it is impossible to graduate, in a sufficiently regular manner, the velocity of the flow, and the liquid sometimes arriving in too great abundance, produces in the interior of the bubble such movements as cause it to burst; in the second place, the heat of the hand slightly increases the temperature of the liquid of the flask, and hence diminishes the density, whence there results a tendency for the bubble to rise, which, occasioning it to shift to one side or another, and thus altering the symmetry of action, also leads to a rupture. To remove these two causes of miscarriage, I have had a flask constructed of brass, furnished with a tap and with feet, so that, when placed on the plate of glass which serves as a cover to the vessel, the orifice of the tap reaches a little higher than the edge of the funnel; into this flask I introduce the liquid necessary to form the bubble, and then let it flow by the tap into the funnel with a velocity which can be graduated at will, while any influence of the heat of the hand is at the same time avoided.*

§ 7. By means of the system of apparatus which I have just described we obtain without difficulty results greatly developed. By giving to the little mass of oil attached to the orifice of the tube a diameter of about three centimetres, I have often realized bubbles 12 centimetres in diameter, and might, doubtless, have realized still larger had the vessel been of greater capacity. When the dimensions just stated have been attained, if the funnel be raised by a quick and dexterous movement, the bubble remains behind, and the film of which it is formed being elongated by its adhesion to the orifice of the tube, a sort of train is constituted, which rapidly narrows and separates into two parts; of these the lower one serves to close and complete the bubble, which is thus left entirely isolated in the liquid of the vessel. In this state it continues for a longer or shorter time, amounting sometimes to more than an hour, and then bursts spontaneously. Experience will soon indicate the rate of quickness with which the funnel should be withdrawn: if this be too great, the bubble will burst; if too small, the bubble rises with the tube and bursts in like manner when the orifice of the latter quits the surface of the alcoholic liquid. Calculation gives, as the mean thickness of the film which forms the bubble in the above case, $0^{mm}.3$, that is, less than the third of a millimetre; I say the mean thickness, for this is not uniform, and must, at certain points, be much thinner than $0^{mm}.3$.

It may perhaps be asked why, when such a bubble is isolated from the tube, it does not last indefinitely; nor do we see in effect in the capillary action any cause which should induce its rupture. It is necessary, I think, to seek that cause in a remainder of chemical action exerted by the alcoholic liquid on the oil. This liquid, no doubt, dissolves the film little by little, so that at the point where it is thinnest it is eventually quite absorbed for a small part of its extent. As in the experiments here reported I was not able to carry the expansion of the bubbles to its absolute limit, I reduced the small initial mass to a diameter of 2 centimetres. The bubbles then usually broke between the diametres of 7 and 11 centimetres. Still I have sometimes succeeded in augmenting the diameter to as much as 12 centimetres, which would assign to the film a mean thickness of $0^{mm}.09$, that is to say, less than the tenth of a millimetre; but I have never been able to isolate the bubbles thus attenuated; some would burst spontaneously before the funnel had been withdrawn, others while it was being withdrawn.

§ 8. In § 12 of the first series of these researches, after having described the

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* I will here mention quite a curious circumstance. I had at first employed a flask of tin furnished with a tap of iron, but when the alcoholic liquid contained in this flask accidentally included small spheres of oil, these, on leaving the tap, sometimes brought over oxide of iron, and thus becoming heavy, fell with much rapidity to the bottom of the bubble of oil; now, when this happened, however minute the ferruginous spherule might be, the film of oil was seen, after some seconds, to become suddenly thin at the place where this spherule rested, the attenuation being propagated by a retreat of the oil to a small distance around the point of contact, and the bubble burst almost simultaneously at this place. The retreat of a part of the oil at the contact of the ferruginous spherule is doubtless, as we may say in passing, a phenomenon of the kind described by M. Dutrochet in his *Recherches physiques sur la force épipolique.*

formation of the liquid ring by the action of the centrifugal force, it was said that during the first moments this ring remains united to the metallic disk by a very thin pellicle or sheet of oil. We now see to what order of facts this pellicle pertains. It is evidently of the same kind with all the films which we have been considering in the preceding paragraphs of the present memoir. It may be remarked here that this pellicle further establishes a difference between the phenomena produced in my apparatus, phenomena which depend on molecular attraction, and those which belong properly to the domain of astronomy, and which depend on universal attraction. I shall show in another series that the liquid figures of my first series differ essentially, by the equation of their surfaces, from those which would be assumed by a planetary mass supposed to be fluid. I will repeat, then, what I have already said in note 2 of § 62 of the second series, namely, that from the experiment in question no induction can be drawn in favor of a cosmogonic hypothesis.

§ 9. It results from facts previously described that liquid films withdrawn from the action of gravity affect, like full masses, determinate figures of equilibrium. Now, it is easily demonstrable that these figures must be the same in both cases.

If, at a point of one of the two surfaces of such a film, we imagine a right line perpendicular to that surface, it is clear that, considering the slight thickness of the film, this line may be also regarded as perpendicular to the other surface. Further, if a plane be made to pass by this common perpendicular, it will cut the two surfaces by curves which may, without appreciable error, be taken as identical. Consequently, at the points where the perpendicular pierces the two surfaces, the curvatures of the two curves will be the same; only, in relation to the liquid which forms the film, one of these curvatures will be convex and the other concave. If, then, the radius of the first be designated by ρ, that of the second will be $-\rho$; and as this result is general, it is equally applicable to the principal curvatures, that is to say, to the greatest and to the smallest; so that if R and R′ represent the two principal radii of curvature at one of the two points considered, the two principal radii of curvature at the other point will be $-R$ and $-R'$. Hence the capillary pressures respectively corresponding to these two points, and referred to the unity of surface, are (2d series, § 4) for the first, $P+\frac{A}{2}\left(\frac{1}{R}+\frac{1}{R'}\right)$ and for the second, $P-\frac{A}{2}\left(\frac{1}{R}+\frac{1}{R'}\right)$, P being the pressure which a plane surface would occasion, and A a constant which depends on the nature of the liquid.

Now, these two pressures being opposed, they give a resultant equal to their difference, namely, to $A\left(\frac{1}{R}+\frac{1}{R'}\right)$. Then if the laminar figure is such that, in its whole extent, the above resultant is null, it is clear that equilibrium will exist. If this condition is not fulfilled, the resultants corresponding respectively to the different points of the figure will tend to drive these points in one direction or the other, but in this case again equilibrium will be possible if the laminar figure is closed, like the bubbles of § 7, and hence imprisons in its interior a limited mass of alcoholic liquid; for if the figure has then such a form that the resultants in question have everywhere the same intensity, these forces will evidently be destroyed by the resistance of the interior alcoholic mass. We shall express, therefore, the general equation of equilibrium of laminar figures by establishing the condition that the resultant is null or constant; and for this, as the co-efficient A is constant and finite, it will suffice to put $\frac{1}{R}+\frac{1}{R'}=C$, where the quantity C may be null or constant. Now, this general equation being also that of the equilibrium of full masses, it results that the films assume, as I said in advance, the same figures with those masses.

Thus, under the circumstances of my experiments, it must be possible to form with films of oil all the figures which I have obtained with full masses of oil, and it has, in effect, been seen (§ 7) that a film of oil which is not adherent to any solid system, takes a spherical figure, as does a full mass placed in the same conditions.

§ 10. I should here present an important remark in reference to the sign of the constant C and the signification of that sign. From the manner in which I have just arrived at the general equation of the equilibrium of laminar figures, it is clear that, in this equation, the quantity $\frac{1}{R}+\frac{1}{R'}$ may, as to its absolute value, be referred indifferently to one or to the other of the two surfaces of the film. If we agree to refer it to that one of the two which regards the exterior of the figure, then when this same quantity, or, what amounts to the same, the constant C is positive, the pressure corresponding to the surface in question will be superior to P—that is to say, to that of a plane surface, and the pressure corresponding to the other surface will be less than that of a plane, and consequently less than the former; wherefore the resultant, which necessarily acts in the direction of the greatest of the two forces, will be directed, like that, towards the interior of the figure. On the other hand, when the interior surface is considered and C is negative, the greatest of the two pressures will pertain to that surface, whence it follows that the resultant will be directed towards the exterior. When C, therefore, is positive, the laminar figure will exert a pressure on the alcoholic mass which it encloses; and when C is negative, the laminar figure will, on the contrary, exert a traction on the mass in question; in both cases the action will be destroyed by the resistance of this mass; finally, when C is null, the laminar figure will exert neither pressure nor traction.

§ 11. When the laminar figure is closed, the condition of equilibrium has consequently its entire generality, since C may be positive, negative, or null; but if the figure is not closed, equilibrium can evidently subsist only for $C = O$. Hence it follows, for example, that a single film in a solid ring will be in equilibrium if it is plane, and we have seen, in effect, in the experiment of § 3, the two separate films take respectively, in each of the rings, a plane form. For the same reason, a single film attached to two parallel and opposite rings, like that which is formed in the experiment just recalled, and which is represented, Fig. 3, must constitute a portion of a catenoid, as announced by its aspect, and as the value of the maximum separation of the rings substantiates. Finally, in the combination of films of Fig. 1 the two short films are necessarily also two portions of a catenoid, but taken sufficiently far from their respective *cercles de gorge* for their meridian curvature to be little perceptible, and for these films to seem to appertain to cones.

§ 12. It would be easy to imagine proper means for realizing with our films of oil all the other figures of equilibrium which have been considered in the second and fourth series; but this, we shall see, would be useless, inasmuch as we are directly led by what has been premised to a more simple mode of producing laminar figures of equilibrium. Let us suppose that we could form, in the air, liquid films without weight; these films would necessarily take the same figures with the films of oil formed in the alcoholic mixture. In effect, if the system is closed and exerts pressures directed towards its interior, the enclosed mass of air will be compressed until its elasticity neutralizes those pressures, and equilibrium will then evidently exist if these same pressures are all equal as regards one another; in other words, if the figure is such that the equation $\frac{1}{R}+\frac{1}{R'}=C$, an equation in which C is positive, be satisfied. If again the system be closed, but its action, on the contrary, be directed towards the exterior, the imprisoned mass of air will be dilated until the assemblage of forces which result from its elasticity thus diminished and from the action of the films be neutralized by the external atmospheric pressure, and then, it is clear, the figure will be still in equilibrium if all these actions are equal, or if the above equation, in which C will be negative, is satisfied. Lastly, if the system is not closed, the thing is evident of itself, and it will be remembered that, in this case, equilibrium requires that C should be null.

Now, the gravitating liquid films which we can develop in the air, soap-bubbles for instance, being extremely thin, their mass is very small, and consequently the action of gravitation may in general be here regarded as insensible in relation to that of the molecular forces; whence it follows that the figures which we would realize with these films should not differ in an appreciable manner from those which would be constituted by films without weight. We ought, therefore, to be able to obtain, in the air, with films of soap and water, or an analogous liquid, the same figures of equilibrium as with films of oil in the alcoholic mixture, and consequently the figures which would correspond to a liquid mass, full and withdrawn from the action of gravitation. It is in this that the new process which I announced at the commencement of the present series consists.

Thus we arrive at this curious consequence, that with a liquid exposed to the action of gravitation and in repose we can realize on a great scale all the forms of equilibrium which correspond to a liquid mass withdrawn from the action of gravitation and equally in repose. Soap-bubbles afford a first example of the employment of the process in question: isolated in air, they are spherical as would be a full liquid mass withdrawn from the action of gravitation and free from all adhesion. We shall now show that, in adopting as a more general example the figures of revolution, it is easy to procure by this same process a realization of all the figures of equilibrium.

§ 13. Let us first consider the liquid. Films obtained with a simple solution of soap have but a brief existence unless they are enclosed in a vessel: a soap-bubble one decimetre in diameter, and formed in the free air of an apartment, rarely subsists for two minutes; more frequently it bursts in one or even half a minute; it was, therefore, important to seek some better liquid, and, after several fruitless attempts, I was so fortunate as to discover one which furnished, in open air, films of remarkable persistence. This liquid is formed by mixing, in suitable proportions, glycerine, water, and soap. The glycerines which occur in commerce differ considerably in purity and concentration, and the preparation of the mixture consequently varies; but by procuring it from London, where it is manufactured by a particular process, we obtain, without great expense, a glycerine which appears to be very pure and concentrated; it is almost colorless, and of a weak odor. I shall confine myself at present to a description of the mixture prepared from this glycerine; in a note, at the end of the memoir, will be found an account of the means proper for obtaining satisfactory results with other qualities of glycerine.

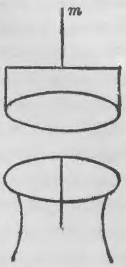
As far as possible we should operate in summer, and when the exterior temperature is at least 66° Fah. Let a portion, by weight, of Marseilles soap, previously reduced to small parings, be dissolved, at a gentle heat, in forty parts of distilled water, and, when the solution has cooled, let it be filtered. This having been done, we may choose between the two following modes of procedure: 1*st process.*—Carefully mix in a flask, by a brisk and protracted agitation, two volumes of glycerine with three volumes of the solution of soap, and then leave the mixture at rest; this, though limpid at first, after some hours begins to grow turbid: a light white precipitate is produced, which remains at first suspended throughout the mass, but which afterwards ascends with extreme slowness, and, in some days, forms a layer distinctly separated at the top of the liquid; the limpid portion is now to be drawn off by means of a siphon which is primed with a lateral tube, and the preparation is terminated. It is proper to add, however, that, when the short branch of the siphon is introduced into the liquid, a portion of the deposit is brought away and forms around the exterior surface of the tube a sort of reversed cone; it is necessary, therefore, before priming the siphon, to rid it of this envelope. For this, the whole should be first left at rest for a quarter of an hour, and then the immersed branch of the siphon should be slightly shaken right and left; the deposited cone will thus become detached in small clots which gradually reascend and unite with the upper layer. 2*d process.*—With the same care as in the former process, let one volume of glycerine be mixed with three volumes of the solution of soap; twenty-four hours afterwards filter, covering the funnel and renewing the filter when the drops succeed each other only at long intervals; finally, add to the filtered liquor the quantity of glycerine necessary to establish the proportion of two volumes of glycerine to three of the solution of soap, and the preparation is here likewise terminated. This second process has the advantage of being somewhat more expeditious than the first; but if the filtering paper be not of excellent quality, a sensible portion of the precipitate passes with the liquid, and the result is not so satisfactory.

The liquid thus prepared, and which I shall call *glyceric liquid,* yields films of very great persistence: for example, a bubble having a diameter of one decimetre, deposited, in the open air of an apartment, on a ring of iron-wire of four centimetres diameter, previously moistened with the same liquid, as in the experiments which I shall presently describe, is capable, when in complete repose, of lasting fully three hours. If the season be cold the second process is the only one that can be employed, and days for operating must be chosen in which the external temperature is several degrees above zero; it is, moreover, necessary to realize artificially, at least in an approximate degree, the conditions of summer with regard to the liquids. To this end, the apartment being heated to 66° or 68°, we must first, for an hour or two, keep the flasks which contain separately the glycerine and the solution of soap in water maintained at the above temperature; then the mixture is to be made; after which the flask containing it should be placed in water at 66° or 68°; and we provide, whether by fire in the apartment during the night or by enveloping the exterior vessel with thick folds of woollen cloth, that the temperature of the mixture shall descend but little below 19° for twenty-four hours. Filtration follows, the apartment being still kept warm, and the preparation is finished as above stated; the glycerine which is added should itself have been kept, for about an hour, at a temperature of 66° or 68°.

The glyceric liquid may be preserved for nearly a year; it then, in a day or two, becomes decomposed and completely loses its properties. This decomposition has seemed to me to be unattended by any gaseous disengagement; yet, as the liquid is of an organic nature, it would seem not improbable that it should sometimes be otherwise; and it will be prudent, in order to avoid a possible explosion of the flask, to close the latter not too tightly with cork. Just as the films formed with soap last much longer in a closed vessel than in the free air, the duration of those of the glyceric liquid becomes much more considerable when these films are enclosed in like manner, especially if certain precautions are used; of this we shall see some examples at the end of the present series, and I shall recur to this subject in a subsequent series.

§ 14. For the realization of figures of revolution the following instruments are required: 1st, a system of rings of iron wire, seven centimetres in diameter, similar to that used for the formation of the laminar catenoid of oil, (§§ 3 and 11,) a system of which I here reproduce (fig. 5) the design in perspective, although it has been already given in the plates of the second series; 2d, a system of rings of the same kind, but having a diameter of only three centimetres; 3d, a system of disks seven centimetres in diameter, the lower one of which is borne upon three feet more solid than those of the rings, and issuing from points situated between the border and the centre, while the upper disk is sustained by an iron wire fixed perpendicularly at its centre; 4th, a small table with adjusting screws; 5th, several common clay pipes, and a soft brush of middle size; 6th, a support consisting of a vertical rod, along which slides, with little friction, a horizontal arm: it is to the extremity of this arm that we attach either one of the upper rings by the end *m* of the handle of its fork, or the upper disk by the end of the iron wire which sustains it; for this support I have adopted a cathetometer; the ring or the disk is attached, by means of an intermediate piece, to the extremity of the eye-glass. We obtain thus, besides the other conditions, the faculty of reading on the scale of the instrument the quantity by which the ring or the disk is raised or lowered.

Fig. 5



With a view to give more stability to the lower rings, each of them is fixed by its feet with drops of sealingwax in a small porcelain saucer, and, that adhesion may be better secured, the glaze of the porcelain should be previously removed with emery at the places which are to receive the wax. When the rings and disks are new the glyceric liquid adheres imperfectly, and the laminar figures burst while one is trying to form them, or almost immediately after their formation; but this difficulty is removed in the following manner: the apparatus is immersed in nitric acid diluted with four times its volume of water, until the surface of the pieces in question is perceptibly oxidized, which requires not more than a minute; they should then be carefully washed with pure water, wiped with a strip of filtering paper, and left to dry; they are thus rendered fit for service indefinitely, and will always yield durable figures.

Let us observe now how the experiments are prepared. The cathetometer is to be first rendered quite vertical, and the upper ring or disk attached to it; if this ring or disk appear to be not exactly horizontal, the position must be corrected by cautiously bending with pincers the iron wire which sustains it. The lower piece, whether ring or disk, is then to be placed on the table in such manner that it shall be vertically below the other, when, by means of the adjusting screws and by slight displacements of the lower piece, it is easily managed that in lowering the upper one the two rings or disks shall exactly cover one another. Then, after having raised the upper one, we carefully wet each of them with the glyceric liquid. For the lower ring we use, for this purpose, the brush well saturated, and for the upper a capsule containing the same liquid, into which this ring is to be plunged. After the withdrawal of the capsule the ring will be found to be occupied by a plane film, but this we break. As to the disks, the liquid is spread with the brush over the whole of the two opposite faces, and then the liquid contained in the capsule is brought into contact with the moistened face of the upper disk, after which the capsule is removed. We should add that in the case of the rings it is necessary to pour a little water in the saucer which supports the lower one, without which the small portions of glyceric liquid which fall therein would attack the sealingwax and eventually detach it from the porcelain.

§ 15. Let us suppose now that it is the laminar catenoid which is to be realized. We take the system of rings of seven centimetres, and after having made the arrangements above indicated, lower the upper ring until it is separated from the other by not more than a fraction of a millimetre; then let the brush, well steeped with glyceric liquid, be several times applied along the whole circuit of the two rings, so as to fill the small space left between them. We now raise the upper ring and a laminar catenoid is seen extending from one to the other. It will be remembered that between two equal rings whose distance apart is less than the limitary separation, there are (4th series, § 16) two catenoids possible unequally curved, and that when we realize with oil, within the alcoholic liquid, a full catenoid, it is always (4, § 18) the one least concave which is produced, whence I drew the conclusion that the one most concave is unstable. Now, as might be expected, the laminar catenoid of the experiment of § 3, and of the present experiment, is always that which is least concave.

By continuing to raise the ring gradually, we attain the point where equilibrium ceases, and the catenoid is presently seen to contract in the middle and become converted into two plane films occupying respectively the two rings, like the laminar catenoid of oil, with this difference, that the phenomenon is accomplished in a much shorter time. As with the laminar catenoid of oil, likewise, there is a formation of a thread and of spherules; and although the thread cannot be observed because of the rapidity of the transformation, a spherule of some millimetres in diameter may be seen to fall, at the moment of disunion, on the lower film, and then rebound for some instants; this spherule then changes into a bi-convex lens, laminar like itself, engaged by its border in the film. At this time the cathetometer gives, as the interval between the two rings, about 46 millimetres, or very nearly two-thirds of the diameter of the rings, being the same state of things which presented itself with the laminar catenoid of oil.

A remark of importance should here be made in reference to this rupture of equilibrium. A full limitary catenoid, formed with oil in the alcoholic liquid, far from disuniting like our laminar catenoid, is, on the contrary, very stable, (4th series, §§ 18 and 21,) although it be at its limit of stability. I have given in the second of the paragraphs just cited the reason of this singular fact, and it may be concluded from the experiments of § 20 of the same series, that if the distance of the rings be a little increased, the figure will simply pass to the unduloid by a slight modification. But it cannot be so with regard to a laminar limitary catenoid without bases: for, as has been seen, (§§ 11 and 12 of the present series,) between two equal rings, parallel and placed opposite to one another, the only figure of equilibrium possible in a laminar state and unclosed is the catenoid.* Consequently, in these latter conditions, if the separation of the two rings exceed by the least quantity that which corresponds to the limitary catenoid, equilibrium can no longer exist, and the figure must necessarily separate into two.

To realize a laminar cylinder, the same system of rings is employed. After having raised the upper ring to a sufficient height, we inflate, by means of one of the pipes, a bubble of about 10 centimetres in diameter; we deposit it on the lower ring, to which it immediately attaches itself, and withdraw the pipe; we then lower the upper ring until it touches the bubble, which attaches itself thereto in like manner; finally, we again gradually raise this ring, and the bubble, which, thus vertically elongated, loses more and more its lateral meridian curvature, is converted, at a certain stage of separation of the rings, into a perfectly regular cylinder, presenting convex bases like the full cylinders of oil. We can give the bubble a diameter a little greater; but when it is too considerable, the cylindrical form is not attained, whether because the cylinder which we would realize exceeds its limit of stability, or because, if still within that

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*At least among figures of revolution; but as, between two rings thus arranged in the alcoholic liquid, full masses never take other than forms of revolution, it must be admitted *a priori* that the case is the same with films in that liquid or in the air, and experiment verifies it.

limit, it begins to approach it: in this latter case, in effect, the configurative forces becoming of very little intensity, the slight weight of the film exerts a sensible influence, and the figure shows itself more or less distended in its lower half and constricted in its upper half.* The tallest cylinder that can be realized in a regular manner with the rings indicated has a height of about 17 centimetres, and will be seen to be within the limit of stability, since this corresponds to a height a little greater than the triple of the diameter, (2d series, § 46.)

Do we propose to obtain a partial unduloid constricted in its middle, (4th series, § 13)? We deposit on the lower ring a bubble having but about nine centimetres diameter, lay hold of it, as before, with the upper ring, and then raise this latter, but we proceed beyond the point where the figure becomes cylindrical; this figure now becomes constricted at its middle, the more deeply in proportion as the ring rises, and thus constitutes the desired unduloid. This, like the cylinder, shows itself perfectly regular, and its bases are in like manner convex spherical caps. By still raising the upper ring, we reach a point where equilibrium can no longer exist, and then the figure rapidly narrows at its middle, where it disunites, becoming transformed into two spherical bubbles respectively attached to the two rings.

If it is a partial unduloid dilated in the middle, (4th series, § 10,) which we propose to realize, we make use of the system of rings of three centimetres. A bubble of about eight centimetres in diameter is to be formed and deposited on the lower ring; being then laid hold of with the upper ring, when this latter is elevated the bubble passes by degrees to a figure composed of a dilatation between two portions of constrictions, and again presenting convex spherical caps as bases; it is consequently the unduloid in question. In this experiment it is necessary to stop at a degree of separation of the rings, for which the tangents at the extreme points of the meridian line are still considerably inclined to the axis, and, with this condition, the figure appears regular like the preceding. If we proceed so far as to approach the point where these tangents would be vertical, the figure borders upon its limit of stability, (4th series, § 10,) and, as with regard to the cylinder, the diminution of the configurative forces leaves a sensible action to the weight of the film; the dilatation then shows itself a little lower than the middle of the figure.

The realization of the nodoid requires the employment of the system of disks. We begin by inflating a bubble of a diameter of three or four centimetres; we bring it into contact with the moistened face of the lower disk, to which, spreading more or less, it immediately adheres, and we continue to inflate it until it forms part of a sphere about ten centimetres in diameter; the pipe is then removed, while the film springs from the very edge of the disk. The upper disk is now lowered until it touches the summit of the bubble, which immediately opens at that point, and the film gaining likewise the edge of the latter disk, forms, from one edge to the other, a portion of the dilatation of an unduloid. While it is in this state we continue to lower the upper disk, and when the point is overpassed where the figure would constitute a spherical zone, we have the partial nodoid sought for, (4th series, §§ 31 and 32). If the disk be still further lowered, we attain, just as with the full nodoid of oil, (4, § 31,) a point beyond which the figure ceases to be one of revolution, and is directed more laterally in proportion as the disk continues to be lowered.

§ 16. These experiments are very curious; there is a peculiar charm in contemplating these slight figures almost reduced to mathematical surfaces, which

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* We shall see, in another series, a phenomenon of the same kind produced in regard to full cylinders of oil, when they are near their limit of stability, and there remains a minute difference between the densities of the oil and of the alcoholic liquid.

exhibit themselves in the most brilliant colors, and which, notwithstanding their extreme fragility, endure for so long a time. They are experiments, too, which may be executed promptly and in the most commodious manner. Here we have not the embarrassments which, in experiments with full masses of oil, result from the equalization of the two densities, from variations of temperature and from the mutual chemical action, however slight, of the two liquids. Still, there are certain experiments which indispensably require the employment of oil and the alcoholic liquid. Such are those of my first series; such too is that for the realization of the figure generated by an entire node of the meridian line of the nodoid, (4th series, § 27,) &c.

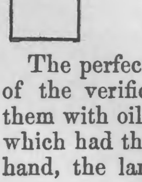
When a series of experiments with the glyceric liquid is terminated, the rings or disks should be washed by agitating them in rain-water; to dry them, the former are placed on filtering paper and the latter wiped. Two useful precautions may also be prescribed. When a considerable number of experiments are conducted in succession, it is well, from time to time, to remoisten the upper ring; it will be also proper sometimes to take a new pipe; when the same one has been used too frequently, the films seem less persistent, doubtless because a small portion of watery vapor introduced by the breath is condensed on the inner edge of the bowl of the pipe.

§ 17. It has been already remarked that, in these experiments, the disunion of the laminar cantenoid is preceded by the formation of a thread, which is converted into spherules; now the same is the case in the disunion of all the other laminar figures, just as in that of all the full figures of oil, (2d series, § 62.) If, after having realized a constricted or a dilated unduloid, we continue to raise the upper ring until rupture of equilibrium occurs, we shall see, at the instant of disunion, a spherule some millimetres in diameter escape from the figure and float in the air of the apartment or fall on the bubble which has formed in the lower ring, according to the greater or less tenuity of the film which constitutes the spherule. I shall discuss, in another series, the theory of the production of these threads, whether full or laminar.

§ 18. We also easily realize, and still by means of the glyceric liquid, the laminar systems which I obtained with oil, in the interior of the alcoholic liquid, while using polyhedral frames of iron wire, (2d series, §§ 31 and 35). For this, it is sufficient to plunge one of these frames into a vessel filled with the liquid in question, and, having left it therein for some seconds that it may be thoroughly wetted, withdraw it; it will be found to be occupied by the laminar system, and always in a complete state—that is, including no inspissated portions appreciable by the sight. It is to this process that I allude at the end of § 35 of the 2d series. The frames, like the rings and disks, must be once treated with diluted nitric acid, (§ 14).

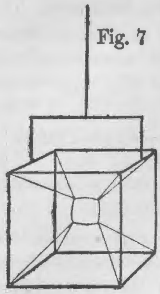
In order to observe conveniently, and without causing agitation, any one of the laminar systems thus produced, the frame containing it should be placed on a small structure of iron wire like that which is represented in perspective at figure 6; the structures which I use are four centimetres in width, twelve in length, and two and a half in height. After each series of experiments the frames should be washed by passing them through rain-water, and be left to dry on filtering paper.

Fig. 6



The perfection of the laminar systems realized by the present process admits of the verification of a peculiarity which had escaped me when I obtained them with oil in the alcoholic liquid: it was perceived that several of the films which had then appeared plane have in reality slight curvatures. On the other hand, the laminar system of the octahedron, which, with oil, was formed of

Fig. 7



films visibly curved, is always seen by the new process to be composed of plane films; this singular difference I shall attempt hereafter to explain. I have here presented as an example (figure 7) the laminar system of the cubic frame; it is composed of twelve films, proceeding respectively from the twelve edges, and all terminating at a single quandrangular *lamella* or little film, situated at the middle of the assemblage of films. The sides of this lamella are slightly curved, as well as all the other liquid edges, and consequently all the films, with the exception of the lamella, have feeble curvatures. The curvatures of the liquid edges which proceed from the summits of the frame are too slight to be indicated in the figure. By an error, for which I can scarcely account, this system is badly executed in the plates of the second series; the central lamella, besides that it is seen obliquely and not in front, is much too small. I will call to mind that my frame has a length of seven centimetres to the side. In order to plunge it in the glyceric liquid it is held by the extremity of the handle of the fork, which is soldered to it.

§ 19. The laminar systems thus developed in the air by means of the glyceric liquid have excited the admiration of all to whom I have shown them. They are of a perfect regularity, their liquid edges have an extreme delicacy, and their films display after some time the richest colors. The arrangement of these films is governed by simple and uniform laws, of which the following is a statement: 1. From each of the edges of the solid frame proceeds a film. 2. If care be taken that there be no bubbles of air on the surface of the liquid in the vessel before immersing the frame, the laminar system will present no space closed on all sides by films; in other words, each of the films of the system will be in contact by its two faces with the ambient air. 3. At the same liquid edge not more than three films ever terminate, and these make with one another equal angles. 4. When several liquid edges terminate at the same point in the interior of the system, the edges are always four in number and form between them equal angles. 5. When the conditions can be fulfilled by plane surfaces, the films take that form; when this is impossible, all the films, or several of them, are more or less curved, but always in such a manner as to constitute surfaces of mean curvature null; the first takes place, for example, in the systems of the tetrahedron, of the triangular prism, and the octahedron; the second in those of the cube, the hexagonal prism, &c.

The system of the quadrangular pyramid, such as it is represented in the plates of the second series, would form an exception to the fourth of the above laws; but, as I shall show in the next series, when I shall examine the laminar systems under a theoretical point of view, this is attributable to the fact that in realizing the system in question with oil within the alcoholic mixture, the exhaustion of the oil cannot be carried far enough, whence the liquid edges retain too great thickness. When the present process is employed, the figure is modified, and fully satisfies all the laws.

A frame being given as to its form, it might be proposed, as a geometric problem, to occupy the interior of it with an assemblage of surfaces subject to the preceding laws; but the solution would be, in general, very difficult. Now, if we have recourse to experiment, the liquid, in disposing itself in films, plays the geometer, and it is an extremely curious thing to see it always resolve the question in a simple and elegant manner, at least if the solution be possible. For these experiments we may, in strictness, employ, in place of the glyceric liquid, a simple solution of soap. In that case the figures will have little duration, but it is always in our power to renew the immersion of the frame, and thus pursue our observation.

§ 20. I have several times already, in the second and fourth series, insisted on the principle that, for every figure of equilibrium in relief there is an identical figure of equilibrium in concave. Now, these latter figures are evidently realized by the surfaces of the laminar figures of equilibrium which face towards the interior: the interior surface of a soap-bubble, for instance, is a spherical surface in concave; the interior surface of a laminar cylinder constitutes a cylindrical surface in concave, &c.

Pressure exerted by a spherical film on the air which it contains.—Application.

§ 21. The exterior surface of a laminar sphere being convex in every direction, the pressure which corresponds to it is greater than that of a plane surface, and consequently (§ 10) the resultant of the pressures exerted in any point of the bubble by the two surfaces of the latter is directed towards the interior; whence it results that the bubble presses on the air which it encloses. It is, indeed, well known that, when a soap-bubble has been inflated, and while it is still attached to the tube, if the other extremity of this last be left open, the bubble gradually collapses, expelling the air which it contained through the tube. We see now what is the precise cause of this expulsion.

§ 22. But we may go further, and determine according to what law it is that the pressure, exerted by such a bubble on the confined air, depends on the diameter of that bubble. We can compute, moreover, the exact value of the pressure in question for a bubble having a given diameter and formed of a given liquid. The pressure corresponding to a point of a laminar figure has (§ 9) for its expression $A\left(\frac{1}{R}+\frac{1}{R'}\right)$. Now, in the case of the spherical figure, we have $R = R' =$ the radius of the sphere. If, therefore, we designate by d the diameter of the bubble, the value of the pressure will simply become $\frac{4A}{d}$, always, be it understood, neglecting the slight thickness of the film; whence it follows that the intensity of the pressure exerted by a laminar spherical bubble on the air which it confines is in inverse ratio to the diameter of that bubble.

§ 23. This first result established, let us recur to the general expression of the pressure corresponding to any point of a liquid surface, an expression which is $P+\frac{A}{2}\left(\frac{1}{R}+\frac{1}{R'}\right)$. For a surface of convex spherical curvature, if we designate by d the diameter of the sphere to which this surface pertains, the above expression becomes $P+\frac{2A}{d}$, and for a spherical surface of concave curvature pertaining to a sphere of the same diameter, we shall have $P-\frac{2A}{d}$. Thus, in the case of the convex surface, the total pressure is the sum of two forces acting in the same direction—forces, of which one designated by P is the pressure which a plane surface would exert, and the other represented by $\frac{2A}{d}$ is the action which depends on the curvature. On the contrary, in the case of the concave surface the total pressure is the difference between two forces acting in opposite directions, and which are again, one the action P of a plane surface, and the other $\frac{2A}{d}$ which depends on the curvature. Whence it is seen that the quantity $\frac{4A}{d}$, which represents the pressure exerted by a spherical film on the air it encloses, is equal to double the action which proceeds from the curvature of one or the other surface of the film.

Now, when a liquid rises in a capillary tube, and the diameter of this is sufficiently small, we know that the surface which terminates the column raised does not differ sensibly from a concave hemisphere, whose diameter is consequently equal to that of the tube. Let us recall, moreover, a part of the reasoning by which we arrive, in the theory of capillary action, at the law which connects the height of the column raised with the diameter of the tube. Let us suppose a pipe excessively slender proceeding from the lowest point of the hemispheric surface in question, descending vertically to the lower orifice of the tube, then bending horizontally, and finally rising again so as to terminate vertically at a point of the plane surface of the liquid exterior to the tube. The pressures corresponding to the two orifices of this little pipe will be, on the one part, P, and, on the other, $P - \frac{2A}{\delta}$, if by δ be designated the diameter of the concave hemisphere, or, what amounts to the same thing, that of the tube. Now, the two forces P mutually destroying one another, there remains only the force $-\frac{2A}{\delta}$, which, having a sign contrary to that of P, acts consequently from below upwards at the lower point of the concave hemisphere, and it is this which sustains the weight of the molecular thread contained in the first branch of the little pipe between the point just mentioned and a point situated at the height of the exterior level. This premised, let us remark that the quantity $\frac{2A}{\delta}$ is the action which results from the curvature of the concave surface. The double of this quantity, or $\frac{4A}{\delta}$, will therefore express the pressure exerted on the enclosed air by a laminar sphere or hollow bubble of the diameter δ, and formed of the same liquid. It thence results that this pressure constitutes a force capable of sustaining the liquid at a height double that to which it rises in the capillary tube, and that consequently it would form an equilibrium to the pressure of a column of the same liquid having that double height. Let us suppose, for the sake of precision, δ equal to a millimetre, and designate by h the height at which the liquid stops in a tube of that diameter. We shall have this new result, that the pressure exerted on the enclosed air by a hollow bubble formed of a given liquid and having a diameter of 1^{mm}, would form an equilibrium to that exerted by a column of this liquid of a height equal to $2h$. Now, the pressure exerted by a bubble being in inverse ratio to the diameter thereof, (§ 22,) it follows that the liquid column which would form an equilibrium to the pressure exerted by a bubble of any diameter whatever, d, will have a height equal to $\frac{2h}{d}$.

It would seem at first that this last expression ought to apply equally well to liquids which sink in capillary tubes, h then designating this subsidence, the tube still being supposed 1^{mm} in diameter; but it is not altogether so, for that would require, as is readily seen by the reasonings which precede, that the surface which terminates the depressed column in the capillary tube should be sensibly a convex hemisphere; now we know that in the case of mercury this surface is less curved; according to the observations of M. Bède,* its height is but about the half of the radius of the tube; whence it follows that the valuation of the pressure yielded by our formula would be too small in regard to such liquids. It may be considered, however, as a first approximation.

§ 24. Let us take as a measure of the pressure exerted by a bubble the height of the column of water to which it would form an equilibrium. Then, if ρ designates the density of the liquid of which the bubble is formed, that of water being 1, the heights of the columns of water and of the liquid in question which would

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* *Memoires de l'Academie,* tome xxv *des Memoires couronnés et des Memoires des étrangers.*

form an equilibrium to the same pressure will be to one another in the inverse ratio of the densities, and, therefore, if the height of the second is $\frac{2h}{d}$, that of the first will be $\frac{2h\rho}{d}$. Hence, designating by p the pressure exerted by a laminar sphere on the air which it encloses, we obtain definitively $p=\frac{2h\rho}{d}$, ρ being, as we have seen, the density of the liquid which constitutes the film, h the height to which this liquid rises in a capillary tube 1^{mm} in diameter, and d the diameter of the bubble. If, for example, the bubble be formed of pure water, we have $\rho=1$, and, according to the measurements taken by physicists, we have, very exactly, $h=30^{mm}$; the above formula, therefore, will give, in this case, $p=\frac{60}{d}$. If we could form a bubble of pure water of one decimeter, or 100^{mm}, in diameter, the pressure which it would exert would consequently be equal to $0^{mm}.6$, or, in other terms, would form an equilibrium to the pressure of a column of water $0^{mm}.6$ in height; the pressure exerted by a bubble of the same liquid one centimetre, or 10^{mm}, in diameter, would form an equilibrium to that of a column of water of 6^{mm}. As regards soap-bubbles, their pressures, if the solution were as weak as possible, would differ very little from those exerted by bubbles of the same diameters formed of pure water.

For mercury we have $\rho=13.59$, and, according to M. Bède, h about equal to 10^{mm}; the formula would therefore give, for a bubble of mercury $p=\frac{271.8}{d}$, but, from the remark which closes the last paragraph, this value is too weak and can only be regarded as a first approximation. It only instructs us that, with an equality of diameter, the pressure of a bubble of mercury would exceed four times and a half that of a bubble of pure water. For sulphuric ether, we have $\rho=0.715$, and conclude from measurements taken by M. Frankenheim, (*Bibliothéque Universelle,* nouvelle serie, III, 1836,) h to be very closely equal to $10^{mm}.2$; whence results $p=\frac{14.6}{d}$, and thus, with an equal diameter, the pressure of a bubble of sulphuric ether would be but the fourth of that of a bubble of pure water.

We know that the product $h\rho$, being the product of the capillary height by the density, is proportional to the molecular attraction of the liquid for itself, or in other terms, to the cohesion of the liquid; it is, moreover, the result from a comparison of the values $\frac{4A}{d}$ and $\frac{2h\rho}{d}$, which have been successively found, in § 22 and in the present paragraph, to represent the pressure exerted by a laminar sphere on the air which it contains; hence we deduce $h\rho=2A$, and it will be remembered that A is the constant capillary; that is to say, a quantity proportional to the cohesion of the liquid. The formula $p=\frac{2h\rho}{d}$ indicates, therefore, as must be evident, that the pressure exerted by a laminary bubble on the included air is in the direct ratio of the cohesion of the liquid which constitutes the film and the inverse ratio of the diameter of the bubble.

§ 25. As early as 1830, a learned American, Dr. Hough, had sought to arrive at the measure of pressure exerted, whether on a bubble of air contained in an indefinite liquid or on the air enclosed in a bubble of soap.* He conceives

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* *Inquiries into the principles of liquid attraction.* (Silliman's Journal, 1st series, vol. xvii, page 86.)

quite a just idea of the cause of these pressures, which he does not, however, distinguish from one another, and, in order to appreciate them, sets out, as I have done, with a consideration of the concave surface which terminates a column of the same liquid raised in a capillary tube; but, although an ingenious observer, he was deficient in a knowledge of the theory of capillary action, and hence arrives, by reasoning of which the error is palpable, at values and a law which are necessarily false.

Professor Henry, in a very remarkable verbal communication on the cohesion of liquids, made in 1844 to the American Philosophical Society,* described experiments by means of which he had sought to measure the pressure exerted on the internal air by a bubble of soap of a given diameter. According to the account rendered of this communication, the mode of operation adopted by Mr. Henry was essentially as follows: he availed himself of a glass tube of U form, of small interior diameter, one of whose branches was bell-shaped at its extremity, and inflated a soap-bubble extending to the edge of this widened portion; he then introduced into the tube a certain quantity of water, and the difference of level in the two branches now gave him the measure of the pressure. Unfortunately the statement given does not make known the numbers obtained, nor does it appear that Mr. Henry has subsequently published them. This physicist refers the phenomenon to its real cause, and states the law which connects the pressure with the diameter of the bubble; the account does not say whether the experiments verified it. But Mr. Henry considers that a hollow bubble may be assimilated to a full sphere reduced to its compressing surface; that is to say, he attributes the phenomenon to the action of the exterior surface of the bubble, without taking into account that of the interior surface. Let us add that, in the same communication, Mr. Henry has mentioned several experiments which he had made on the films of soap and water, and which, from the statement given, would elucidate in a remarkable manner the principles of the capillary theory. It is much to be regretted that these experiments are not described.

In a memoir presented to the Philomathic Society in 1856, and printed in 1859 in the *Comptes Rendus*, (tome xlviii, p. 1405,) M. de Tessan maintains that if the vapor which forms clouds and fogs were composed of vesicles, the air enclosed in a vesicle of 0.02 millimetre diameter would be subjected, on the part of this vesicle, to a pressure equivalent to $\frac{1}{7}$ of an atmosphere. M. de Tessan does not say in what manner he obtained this valuation; but it is easily seen that he has fallen into an error analogous to that of Mr. Henry, in the sense that he pays no attention except to the exterior surface of the liquid pellicle. According to the formula of the preceding paragraph, the pressure exerted on the interior air by a bubble of water of 0.02 millimetre diameter would, in fact, be equivalent to that of a column of water 3 metres in height, which equals nearly $\frac{2}{7}$ of the atmospheric pressure; M. de Tessan has found then but half the real value, and we know (§ 23) that this half is the action due to the curvature of one only of the surfaces of the film.

§ 26. After having obtained the general expression of the pressure exerted by a laminar sphere on the air which it encloses, it remained for me to submit my formula to the control of experiment. I have employed, with that view, the process of Mr. Henry, which means that the pressure was directly measured by the height of the column of water to which it formed an equilibrium.

From our formula we deduce $pd = 2h\rho$; for the same liquid and at the same temperature, the product of the pressure by the diameter of the bubble must, therefore, be constant, since h and ρ are so. It is this constancy which I have

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* *Philosophical Magazine*, 1845, vol. xxvi, page 541.

first sought to verify for bubbles of glyceric liquid of very different diameters. The apparatus of which I made use is represented at Fig. 8, in vertical projection; *a b* is the upper part of a support, whose total height is 40 centimetres. On this support is fixed a tube of copper, *c d f*, to the extremity *c* of which is fixed with mastic a bent tube of glass, *c g h k*, designed to serve as a manometer, the interior diameter of which is about one centimetre; the length, *d g*, is 20 centimetres. The copper tube has, at *l*, a horizontal branch, which it was not possible to represent in the same figure, because it is directed towards the spectator, but which is seen apart at *l m*, (Fig. 9.) To the extremity *m* is cemented a glass tube, *m n*, whose interior diameter is but about 2 millimetres. Lastly, at the extremity *f* of the copper tube (Fig. 8) is soldered an adjutage of iron, *f p*, widened below into a small funnel, whose border has a diameter of 5 millimetres; this funnel has been slightly oxidized by diluted nitric acid, (§ 14.)

Fig. 8

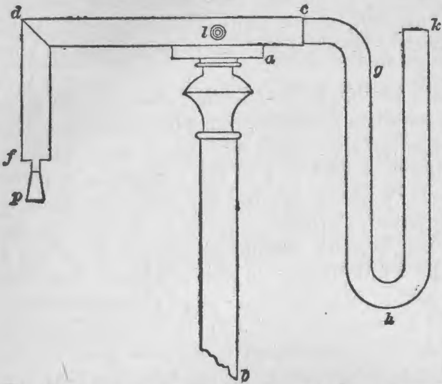
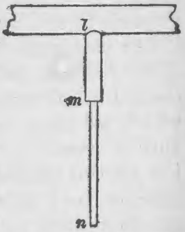


Fig. 9



To use this apparatus, we commence by introducing distilled water into the manometer *g h k* in sufficient quantity to occupy a height of some centimetres in the two branches; we then convey under the adjutage *f p* a capsule containing the liquid destined to form the bubbles; we plunge therein the border *p* of the small funnel, and lower the capsule; then, applying the mouth to the orifice *n* of the glass tube of the branch, (Fig. 9,) we blow with caution. A bubble quickly appears at the adjutage, to which. with the precautions to be presently indicated, is given such a diameter as may be deemed suitable, and when this is obtained, the orifice *n* is carefully closed with a small ball of wax. The water is now rather higher in the branch *h k* of the manometer (Fig. 8) than in the branch *h g*, on account of the pressure exerted by the bubble, and it only remains to measure the difference of the above level and the diameter of the bubble. For the first of these measurements, we avail ourselves of the cathetometer in the usual manner, and, for the second, we lay the same instrument in a horizontal position upon suitable supports.

§ 27. These experiments, though very simple in principle, present considerable difficulty in the execution. In the first place, the air which we breathe into the apparatus is warmer than the ambient air, so that the bubble, after its formation, becomes a little contracted by reason of the gradual cooling of the air contained within it and in the tubes of the instrument; whence it is necessary to wait some time before proceeding to measure the diameter. In the second place, bubbles of great diameter exerting but very feeble pressure, a small error in the measure of the latter has a considerable influence on the product *pd;* it is therefore requisite, in order that the results may not oscillate too greatly around the true value, to stop at a certain limit of the diameter. In the third place, very small bubbles have also their inconveniences; to bring them to the desired diameter, and to obviate, at the same time, the contraction by cooling, they are at first inflated much beyond the size they are intended ultimately to have, and are then left to diminish spontaneously by the expulsion of a part of the air which they contain; now, when this diminution has reached a certain point, it becomes very rapid, and much address is required to apply the ball of wax at the exactly proper moment. Moreover, these small bubbles seem to have much less persistence than the large one; they frequently break before the measurements can be accomplished. Lastly, although the manometer of my instrument has an interior diameter of one centimetre, equilibrium is established therein very slowly, and great errors would result if no regard were paid to this circumstance.

We will further remark that, when a bubble is just formed, there is, in general, a drop suspended at its base, the weight of which somewhat elongates the bubble in a vertical direction. To get rid of this small additional mass without causing the film to break, we lightly touch it with one of the corners of a piece of filtering paper; the drop is thus partially absorbed, and the same operation is repeated with the other corners of the paper, until the excess of liquid has entirely or apparently disappeared. A last remark should be added: if, when a bubble is to be formed, we plunged the whole funnel of the adjutage into the liquid, this would ascend, by capillary action, into the interior of the narrow tube which surmounts this funnel, and would be only, in part, expelled by the breath, so that, after the inflation of the bubble, it might collect in a small mass at the lower part of the tube in question, and thus interrupt the communication between the bubble and the manometer. To avoid this inconvenience, the whole exterior surface of this little funnel should, at the commencement, be moistened with glyceric liquid, and then the extreme border only of the funnel be immersed.

Let us see now by what steps we have proceeded. For the largest diameters of the table to be given in the next paragraph, we first inflated the bubble to about 6 centimetres, applied the ball of wax, and then waited five minutes; after which we again opened the tube of insufflation, allowed the bubble to diminish until it seemed to have the desired diameter, and then applied the wax anew. For all the smaller diameters we began by inflating the bubble to about 4 centimetres, and after having applied the wax, waited ten minutes before allowing the diminution to take place. Previous trials had shown that, with these precautions, the diameter afterwards remained invariable. The bubble on which it was proposed to operate having thus attained the desired dimension, we removed the drop suspended at its lower part, inclined the instrument to right and left with a view to moisten well the two branches of the manometer a little above the two levels, and we measured the diameter. We then allowed the instrument to remain at rest for ten minutes, in order to give time for the establishment of the equilibrium of the manometer, when we measured the pressure, and five minutes afterwards measured it again. If the results of these two measurements were not exactly the same, we made, after the lapse of another five minutes, a third measurement, and so on, until two identical results were obtained in succession, or the difference was in a contrary direction from those preceding; in the former case, the two last results were considered as giving the value of the pressure; in the latter, as their difference would be attributable to a slight error of observation, we assumed, for the value of the pressure, the mean of these two results.

§ 28. The following table contains the results of these experiments; I have arranged them, not in the order in which they were obtained, but in the ascending order of the diameters, and I have distributed them into groups of analogous diameters. During the continuance of the operations the temperature varied from 18⅓.5 to 20°.

Diameters, or values of *d*.	Pressures, or values of *p*.	Products, or values of *pd*.
mm.	*mm.*	
7.55	3.00	22.65
10.37	2.17	22.50
10.55	2.13	22.47
23.35	0.98	22.88
26.44	0.83	21.94
27.58	0.83	22.89
46.60	0.48	22.37
47.47	0.48	22.78
47.85	0.43	20.57
48.10	0.55	26.45

The general mean of the products is 22.75, and it will be seen that, except in the last two, the departures from this general mean are inconsiderable, and that, moreover, they are irregularly distributed. Further, as the first diameter is to those of the last group very nearly as 1 to 6, these results suffice, I think, to establish distinctly the constancy of the product *pd*, and consequently the law according to which the pressure is in the inverse ratio of the diameter. In the next series we shall see this law verified by experiments of a wholly different kind.

I should say here that, in the measurements relating to the smallest bubble, that, namely, of $7^{mm}.55$ diameter, I have been constrained to make a slight exception to the procedure indicated at the end of the preceding paragraph: the second measurement of the pressure exceeded the first by $0^{mm}.02$; it was decided therefore to take a third measurement after a new interval of five minutes, but during this time the bubble burst. The attempt was several times made to renew the experiment, and in each instance one or other of the causes which I have noticed in regard to very small bubbles interfered with the success. As the difference $0^{mm}.02$ was so minute that it might be attributed to an error of observation; as, moreover, by reason of this smallness, it was very improbable that a new excess would be manifested in a third measurement; as lastly, with a diameter of that order, such faint differences have no influence except on the decimal part of the product, I felt authorized to consider the second measurement as giving the value of the pressure, and to retain the result of the experiment. As to the general mean 22.75 of the results of the table, its decimal part is necessarily a little too high, on account of the excessive value 26.45 of the last product. As this product and that which precedes it are, as has been already remarked, those which alone deviate materially from 22 in their integral part, it will be admitted, I think, that a nearer approach to the true value will be made by neglecting these two products and taking the mean of the others, a mean which is 22.56, or more simply 22.6; we shall adopt, then, this last number for the value of the product *pd* in regard to the glyceric liquid.

§ 29. It remained to be verified whether this value satisfied our formula, according to which we have $pd=2h\rho$, the quantities ρ and h being respectively, as has been seen, the density of the liquid and the height which this liquid would attain in a capillary tube one millimetre in diameter. With this view, therefore, it was necessary to seek the values of these two quantities in reference to the glyceric liquid. The density was determined by means of the areometer of Fahrenheit, at the temperature of 17°, a temperature little inferior to that of the preceding experiments, and the result was $\rho=1.1065$. To determine the capillary height, the process of Gay-Lussac was employed, that is to say, the measurement by the cathetometer, all known precautions being taken to secure an exact result. The experiment was made at the temperature of 19°. I had procured a capillary tube having an interior diameter of but a fraction of a millimetre; for what reason will presently be seen. First, a slight mark was made with a file upon this tube at about 3½ centimetres from one of its extremities, a distance which had been found, by previous trial with another fragment of the same tube, to be a little greater than the height of the capillary column raised; next, the interior of the tube was thoroughly moistened by plunging it several times to the bottom of the vessel containing the glyceric liquid and shaking it each time on withdrawing it. Lastly, after having wiped it outside, it was put in place by immersing it in the liquid until the extremity of the column raised appeared to stop very near the mark, and the point of steel was lowered so as to be on a level with the exterior liquid. The horizontal thread of the eye-glass of the cathetometer was then brought into contact with the image of the lowest point of the concave meniscus, and at intervals of five minutes this contact was verified and re-established until the point in question appeared stationary; nor yet was measurement made until, by the lapse of half an hour, the perfect immobility of the summit of the column was ascertained. The movements had been very small, so that the column still terminated near the mark. The reading at the cathetometer gave, for the distance from the lowest point of the concave meniscus to the exterior level, $27^{mm}.35$.

This measurement having been taken, the tube was removed, cut at the mark, and its interior diameter at that point measured by means of a microscope furnished with a micrometer giving directly hundredths of a millimetre. It was found that the interior section of the tube was slightly elliptical, the greater diameter being $0^{mm}.374$, and the smaller $0^{mm}.357$; the mean was adopted, namely, $0^{mm}.3655$, to represent the interior diameter of the tube assumed to be cylindrical. To have the true height of the capillary column, it is necessary, we know, to add to the height of the lowest point of the meniscus the sixth part of the diameter of the tube, or, in the present case, $0^{mm}.06$; the true height of our column is consequently $27^{mm}.41$. Now, to obtain the height h to which the same liquid would rise in a tube having an interior diameter of exactly a millimetre, it is sufficient, in virtue of the known law, to multiply the above height by the diameter of the tube, and thus we find definitively $h=10^{mm}.018$.

I should here say for what reason I have chosen for the experiment a tube whose interior diameter is considerably less than a millimetre. The reasoning by which I arrived (§ 23) at the formula supposes that the surface which terminates the capillary column is hemispherical; now that is not strictly true, but in a tube so narrow as that which I have employed, the difference is wholly imperceptible, so that in afterwards calculating, by the law of the inverse ratio of the elevation to the diameter, the height for a tube one millimetre in diameter, we would have this height such as it would be if the upper surface were exactly hemispherical.

The values of ρ and h being thus determined, we deduce therefrom $2h\rho=22.17$, a number which differs but little from 22.56, obtained in the preceding paragraph as the value of the product pd. The formula $pd=2h\rho$ may therefore be regarded as verified by experiment, and the verification will appear still more complete if we consider that the two results are respectively deduced from elements altogether different. I hope hereafter to obtain new verifications with other liquids.

Investigation of a very small limit below which is found, in the glyceric liquid, the value of the radius of sensible activity of the molecular attraction.

§ 30. The exactness of the formula $p=\frac{2h\rho}{d}$ supposes, as we are about to show, that the film which constitutes the bubble has, at all points, no thickness less than double the radius of sensible activity of the molecular attraction.

We have seen (§ 23) that the pressure exerted by a bubble on the air which it encloses is the sum of the actions separately due to the curvatures of its two faces. On the other hand, we know that, in the case of a full liquid mass, the capillary pressure exerted by the liquid on itself emanates from all the points of a superficial stratum having as its thickness the radius of activity in question. Now, if the thickness of the film which constitutes a bubble is everywhere superior or equal to double that radius, each of the two faces of the film will have its superficial stratum unimpaired, and the pressure exerted on the enclosed air will have the value indicated by our formula. But if, at all its points, the film has a thickness inferior to or double this same radius, the two superficial strata have not their complete thickness, and the number of molecules comprised in each of them being thus lessened, these two strata must necessarily exert actions less strong, and consequently the sum of these, that is to say, the pressure on the interior air, must be smaller than the formula indicates it to be. Hence it follows that if, in the experiments of §§ 27 and 28, the thickness of the films which formed the bubbles had, through the whole extent of these last, descended below the limit in question, the results would have been too small, but in this case we should have remarked progressive and continued diminutions in the pressures; which, however, never happened, although the color of the bubbles evinced great tenuity. But all physicists admit that the radius of sensible activity of the molecular attraction is excessively minute.

But what precedes permits of our going further, and deducing from experiment a datum on the value of the radius of sensible activity, at least in the glyceric liquid. When, after having formed at the adjutage of the apparatus of § 26 a bubble of this liquid, we introduce the adjutage into the interior of a glass jar, (*bocal*,) the opening of which is then closed by an obturator through which passes the copper tube *df*, the bubble always manifests a remarkable phenomenon. If we observe it by placing the eye at the height of its centre, the colors are at first seen ranged in curved bands, whose order indicates a gradual increase of thickness from the adjutage to the base of the bubble: but after a longer or shorter time this arrangement is modified; we then observe a large central and circular space colored with a uniform tint and surrounded with narrow concentric rings presenting other colors. If we change our position around the bubble, with the eye still remaining at the height of the centre, the appearances turn with the spectator, and if we place ourselves higher the appearances still follow the movement of the eye. We must hence conclude that the film, arrived at this point, has a uniform thickness throughout the whole bubble, with the exception, always understood, of the very lowest part, where there is constantly a small accumulation of liquid; the colors of the rings which surround the central space are evidently referable to the obliquity of vision.* The bubble having once assumed this aspect, preserves it till bursting; only the respective tints of the central space and of the rings vary progressively, ascending in the succession of the colors of the rings of Newton, whence it follows that the film continues to grow thinner, but equally so everywhere, still excepting the small portion at the base.

Now, after the film has acquired a uniform thinness, if the pressure exerted on the air within underwent a diminution, this would be evinced by the mano-

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*This fact had been remarked by Newton, but only as accidental, in reference to hemispherical bubbles of soap-water.

meter, and it would be seen to progress in a continuous manner in proportion to the ulterior attenuation of the film. In this case, the thickness which the film had when the diminution of pressure commenced would be determined by the tinge which the central space presented at that moment, and the half of that thickness would be the value of the radius of sensible activity of the molecular attraction. If, on the contrary, the pressure remains constant until the disappearance of the bubble, we may infer from the tint of the central space the final thickness of the film, and the half of this thickness will constitute at least a limit, very little below which is to be found the radius in question.

§ 31. I have made, in this view, a great number of experiments, of which I proceed to give an account. At first a diameter of about four centimetres was given to the bubble; it was then left to shrink to nearly two centimetres, and sometimes even to one, before the wax ball was applied. In the first experiment, the drop was next removed and the adjutage with the bubble introduced into the interior of a small jar, the orifice of which was simply closed with a disk of pasteboard; the contact of the horizontal thread of the eye-glass of the cathetometer with the summit of the image of the surface of the water in one of the branches of the manometer was now established, and as equilibrium did not immediately take place (§ 27,) the contact was readjusted from time to time until it became stationary. Eight bubbles were observed under these circumstances until their disappearance: of these, seven burst before having passed the first colors of the second order; one only seemed to have attained the indigo of that order, but there was some uncertainty as regards this; the greatest duration was fourteen hours.

As to the contact of the thread of the eye-glass with the image of the surface of the water, it never, but in one instance, varied in the direction of a diminution of pressure, but, singularly enough, it sometimes varied by small quantities in the opposite direction. For one of the bubbles it was ascertained, by measurements taken before and after these variations, that the pressure had really somewhat increased. When such a variation was produced, it was with a certain rapidity, and the manometer afterwards remained stationary, either until the disappearance of the bubble or until a new variation in the same direction. These variations are not owing to changes in the temperature, for that of the apartment was very constant, nor do they proceed from an imperfect application of the wax, for in that case the augmentation of pressure would be continuous and accelerated.

§ 32. These experiments alone might have furnished me with a result; but I was desirous of knowing why the colors of the bubbles proceeded no further. Led to suspect that a slight chemical action between the iron of the adjutage and the liquid a little altered the constitution of the latter in the vicinity of the orifice, I fitted to this orifice, with sealingwax, a piece of glass tube of the same exterior diameter and with walls suitably thin, and inflated at the free extremity of the tube a bubble which was introduced, as before, into the small jar. Then, indeed, things took place after a different and rather curious manner: the colors proceeded at first even into the third order, after which they retrograded gradually to the red and bluish green of the last orders, then grew pale, and the bubble finally again became white as at the moment of its formation. The thickness of the film therefore had been first diminishing, and afterwards augmenting. The bubble lasted twenty-four hours.

This phenomenon would have seemed inexplicable, had not an experiment which I had made before equipping the adjutage with the piece of glass tube furnished me the key to its solution. For the experiment referred to, a little water had been poured into the jar, and the interior walls of this had been also moistened: now a bubble placed in this atmosphere saturated with watery vapor had in like manner lasted for twenty-four hours, and had burst without having emerged from the red and green of the last orders; it had therefore absorbed aqueous vapor, and this absorption, which is explained by the hygrometric property of the glycerine, had continually repaired the diminution of thickness due to the descent of the liquid. Now, in the present experiment, as there was no water in the jar, and hence the atmosphere which surrounded the bubble was less humid, it might be supposed that the effect of the descent of the liquid had at first prevailed over that of the absorption, and that afterwards it was the contrary which took place. Upon this assumption, I deposited in the bottom of the dry jar morsels of caustic potash, and contrived by the application of a little lard around the orifice of the jar and of the aperture through which passed the copper tube, that, after the introduction of the bubble, the pasteboard disk should close the opening hermetically. Further, as the small quantity of liquid which always accumulates by degrees at the base of the bubble must contribute by its weight to cause a rupture of the latter, a space of ten minutes was allowed to elapse before the removal of the drop; the film had thus already become thinner when it was introduced into the jar, and the accumulation resulting from the further descent of the liquid would be much less. Now, under these conditions, the diminution of thickness of the film was continuous, the bubble lasted for nearly three days, and when it burst, it had arrived at the transition from the yellow to the white of the first order; it then presented a central space of a pale yellow tint, surrounded by a white ring. The level of the water in the observed branch of the manometer underwent small oscillations, sometimes in one direction, sometimes in the other, but the last of them was in the direction of an augmentation of pressure. Although, during the long duration of this bubble, the temperature of the apartment had necessarily undergone slight changes, the oscillations just mentioned could not be entirely attributed to these, for, if that had been so, there would have been seen, after each of the three nights, a movement of the manometer in the direction of an augmentation of pressure; but the contrary was observed after the first two nights, and it was only after the third that there was a movement in that direction. From the progression of these movements it results that if the pressure varied, it was in an irregular manner, in both directions, and terminating not in a diminution, but an augmentation at least relative; we may therefore admit, I think, that the final thickness of the film was still superior to double the radius of sensible activity of the molecular attraction.

§ 33. Let us now see what we may deduce from this last experiment. According to the table given by Newton, the thickness of a film of pure water which reflects the yellow of the first order is, in millionths of an English inch, $5\frac{1}{3}$, or 5.333, and for the white of the same order $3\frac{7}{8}$, or 3.875. We may therefore take the mean, namely 4.064, as the closely approximative value of the thickness corresponding, at least in the case of pure water, to the transition between those colors and the English inch being equal to $25^{mm}.4$, this thickness is equivalent to $\frac{1}{8554}$ of a millimetre. Now we know that, for two different substances, the thickness of the films which reflect the same tint is in the inverse ratio of the indices of refraction of those substances. In order therefore to obtain the real thickness of our film of glyceric liquid, it suffices to multiply the denominator of the preceding fraction by the ratio of the index of the glyceric liquid to that of water. I have measured the former approximately by means of a hollow prism *a liquides*, and have found it equal to 1.377. That of water being 1.336, there results, for the thickness of the glyceric film, $\frac{1}{8811}$ of a millimetre. The half of this quantity, or $\frac{1}{17622}$ of a millimetre, constitutes therefore the limit furnished by the experiment in question. Hence we arrive at the very probable conclusion, that in the glyceric liquid the radius of sensible activity of the molecular attraction is less than $\frac{1}{17000}$ of a millimetre.

I had proposed to continue this investigation with a view to reach, if possible, the black tint, and to elucidate the variations of the manometer; but the cold season has intervened, diminishing the persistence of the bubbles, and I have been forced to postpone my attempts till a more favorable period.

Note on the preparation of the glyceric liquid with the impure glycerines of commerce.

The first glycerine which I tried was that sold by the apothecaries of Ghent. It is of an intense yellow color and disagreeable odor, and contains in large quantity a foreign substance, which I believe to be lime, and of which it is necessary to rid it. After many fruitless attempts, the following process occurred to me, and has been attended with considerable success: Mix in a flask a suitable quantity of distilled water and an equal quantity of the glycerine in question; then introduce into the flask a quantity of Marseilles soap cut in thin shavings, whose weight should be about a fifteenth of that of the water; these shavings remain floating on the liquid. The soap should have been kept in a moist place; if dry, the action is scarcely appreciable. After five minutes the flask is to be slowly turned three or four times, when small white particles will be seen to be detached from the soap and disseminated through the liquid, and which, when the flask is left at rest, gradually ascend. The slow turning is to be repeated at intervals of five minutes, for an hour and a half; the particles become more and more numerous, and eventually and permanently fill the whole mass. The greater part of the soap remains unattacked, but an excess was needed in order to present the more surface to the action of the liquid. This is disengaged from the particles and the excess of soap by passing it through a filter formed of cotton stuff of close texture; when a new supply of the soap-shavings equal to the first is introduced, and the turnings are to be repeated as before for the space of an hour, the particles again form and must be separated anew by filtering; the liquor now passes in a milky state, but is rendered limpid, or nearly so, by filtering it through paper; the preparation is then complete. All these operations should be conducted at a time when the outer temperature and that of the apartment are from 18° to 20°; if this last limit be greatly exceeded the liquid will dissolve too much soap. A bubble formed of this liquid and deposited on a ring, as indicated in §§ 13 and 15, may subsist for perhaps an hour and a half. This liquid, however, has one serious inconvenience; at a temperature below 18° it fails altogether to yield bubbles; hence, when we wish to use it in winter, it is necessary previously to keep the flask for about an hour in water maintained at 20°. It is needless to add that the apartment should be warmed.

I have tried, in the second place, a glycerine which came, as I was assured, from Paris. It has the same color and the same odor with that from Ghent. When mixed with the solution of soap the compound is at first slightly turbid, and after some hours becomes very milky. If this liquid be left at rest the precipitate, as in the case of the glycerine of London, already described, ascends gradually, and forms in a few days a distinct layer on the top. Here, also, the limpid liquid should be withdrawn by means of a siphon. The proportions which I have used with most success are five volumes of glycerine and four of the same solution of soap employed for the glycerine of London. The liquid thus obtained yielded bubbles which lasted five quarters of an hour. This trial was made in autumn, at a time when the external temperature did not rise above 7° or 8°, by operating in a warmed apartment, but without any other precaution. It is probable that the same liquid prepared in summer would furnish films of a still greater persistence.

I am led to believe, from what I know of the different processes by which the glycerines of commerce are obtained, that all that can be procured are analogous to one or other of the three which I have employed; hence it will be seen that the best course is to obtain the substance from London, provided none as pure can be found elsewhere.

OUTLINE

OF A

SYSTEMATIC REVIEW OF THE CLASS OF BIRDS.

BY PROFESSOR W. LILLJEBORG, OF UPSALA.

[From proceedings of Zoological Society of London, for January, 1866.]

[The following article is believed to present the later views of some of the best systematic ornithologists in reference to the classification and arrangement of the higher divisions of birds, such as have been adopted in the main by the highest authorities. It is essentially the same as has been followed in the recent rearrangement of the mounted birds in the museum of the Smithsonian Institution.

The article, as written in the original Swedish, was translated some years ago by Professor Jillson, then of the United States Patent Office, for publication in the annual report of the Institution, but the MSS. was mislaid in the hands of a gentleman to whom it was intrusted for the purpose of revision, and never found. The present paper is of later date, and presented in an English dress to the Zoological Society of London, early in 1866, by the author. The tables, originally in Latin in this communication, have been translated into English by Professor Gill.—J. HENRY.]

LITERATURE.

We may particularly mention Chr. L. Nitzsch,[*] C. J. Sundevall,[†] G. R. Gray,[‡] J. Cabanis,[§] and C. L. Bonaparte,[||] among those that of late years have devoted their attention to the classification of birds. John Müller[¶] has given an important contribution to this classification by his treatise on the apparatus of singing in the larynx inferior in a great number of Passeres.

The contribution given by Nitzsch certainly contains only a very short and incomplete review of the class of birds; but it has, notwithstanding, a particular scientific value from its attracting attention to the importance that the carotides communes of the birds have in their classification.

The ornithological system given by Sundevall has the merit of being based upon a careful and particular examination of the exterior characters of the birds, and of, for the first time, calling attention to the importance of the wing-coverts in classification, and exhibits a correct idea of the designating characters in the nature of the birds. The structure of the wings generally has been minutely described in the treatise on these organs, and its importance as regards

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[*] Observationes de Avium arteria carotide communi. Halæ, 1829. (Appendix to a programme by Prosector Fridericus Blumius Ictus.)

[†] Ornithologiskt system (Transactions of the Royal Academy of Science of Sweden, for the year 1835, (printed 1836,) p. 43.) Over foglarnas wingar (ibid. for the year 1843, (printed 1844,) p. 303.) Svenska foglarna, 1856.

[‡] A List of the Genera of Birds, (London, 1841.) The Genera of Birds, (London, 1844–49.)

[§] Ornithologische Notizen (Wiegmann's Archiv für Naturgeschichte, 1847, vol. i, pp. 186 and 308.) Museum Heineanum, (Halberstadt, 1850–63.)

[||] Conspectus generum avium (Leyden, 1850–57;) Conspectus systematis ornithologiæ (Annales des Science Naturelles, 1857? ;) Tableaux paralléliques des ordres Linnéens, Anseres, Grallæ, et Grallinæ, (Paris, 1856.) (Extract from Comptes Rendus des Séances de l'Académie des Sciences;) besides several other treatises in different magazines.

[¶] Abhandlungen der königl. Akad. der Wissenschaften zu Berlin, 1847, p. 321.

classification held forth. As the wings must be considered to be of the highest importance to a bird, being among those parts that indeed make him a bird, it is natural that a system in which the structure of the wings has been considered should be preferable to any other where the wings have been neglected, or this subject but slightly touched upon, without any minute examination of their structure. The above-mentioned author has, in his "Svenska Foglarna," observed the muscular structure of their feet as important in classification, after having previously, for the first time, called attention to the same at the meeting of naturalists in Stockholm, in 1851.

The new genera and species that have been added since Latham's "Index Ornithologicus" was edited had increased to such a number, and their literature had become so scattered, that such a work as G. R. Gray's systematic "List of the Genera of Birds," although only a list of names without characters, was very necessary to science, and the obtaining of the same also highly beneficial. The right of priority has generally been observed in this work. The same author has, in his "Genera of Birds," given descriptions of the orders, families, and genera, and even figures of the same. A single species of some genera is represented by a colored figure; and of others only certain parts, such as the head or the foot of some typical species, have been figured. This work is certainly of great value for the study of birds; and the very good figures often give a necessary explanation to the descriptions of the genera, which at times are but little distinguishable, and are not given in a diagnostic manner.

Cabanis has, in his ornithological system, given good characters for the arranged groups, taken partly from J. Müller's descriptions of the inferior larynx, partly from the nature of the horny covering on the tarsi, first studied by Keyersling and Blasius, and partly from the number of quills and tail-feathers. It is principally the order Passeres to which this author has devoted his attention, and which consequently has obtained an improved classification. It has been divided into two groups, (Oscines and Clamatores,) and the families have been carefully limited and arranged. This work, with that of Sundevall, may rightly be considered most important in the classification of birds.

The numerous contributions to this classification that have been made by Bonaparte are valuable as giving minute registers of families and species, showing an unusual knowledge of the species, and a sharp distinction between the genera, and often arranging these in a manner corresponding with the demands of the natural affinity; but they are generally only registers of names, often giving the characters for the species, but very seldom for the higher groups.

Bonaparte has published, in the "Transactions of the Linnean Society," xviii, p. 258, a systematic arrangement of the class of birds, together with the classes of the other vertebrated animals in general. The first class has been divided into two sub-classes—Insessores and Grallatores. The first of these corresponds fully with the one arranged by us under the same name, and the latter includes both Grallæ and Natatores. This classification corresponds also with the one given here, in the Longipennes having their place between the Steganopodes and the Pygopodes. Characters of the orders, families, and subfamilies are also given.

After this brief reference to the literature, we will proceed to a synoptic statement of the principles upon which the systematic arrangement here given rests.

PRINCIPLES.

We have preferred the progressive method, as it seems to us to be the most rational, from its correspondence with the physiological and geological development. We therefore commence the system with the lowest, and finish it with the highest forms.

Irritability seems to us to be the most distinguishing character for birds; and this should consequently be taken into consideration more than others with regard to their classification. The swimmers seem to us the lowest, from their showing a tendency to the lowest form of vertebrated animals—the fish form. In the *Aptenodytidæ,* where the wings resemble fins, and where they, as in all other diving birds, serve as such, we have this form most strongly designated. The heavy, clumsy structure, with small wings and short legs, also makes them generally less active than other birds, and shows a lower development of the type of bird. This, however, is not the case with all the swimmers; and the order Longipennes gives us instances where swimmers possess a high degree of activity.

The Passerine birds (Passeres) seem to us to possess the highest irritability, and to be those in which the nature of birds has reached its highest development. We do not by irritability mean the muscular strength alone, but vivacity and activity generally. Where this is most manifold, most changing and constant, it is the most developed. We find in the Passeres "the power to stay and move with ease as well on the ground as in the trees or in the air, and to make their presence known by characteristic melodious notes," (Sundevall;) we find them in a constant and manifold motion, and they let us constantly hear their notes either as song or as affectionate voices. The birds of prey have generally been placed highest, and been considered the most developed, in consequence of their muscular strength and strong flight, and their thereby supposed high degree of irritability; but by keeping them in captivity we find at once that the birds of prey are dull birds, and that they, as regards irritability, are far behind the Passeres. They remain for a long time silent and quiet, and do not generally show any activity, unless they are frightened or driven by appetite for food. The Passerine birds, on the contrary, are in captivity constantly in motion, and let us incessantly hear their lively song and affectionate voices. Besides, we cannot in a system place the birds of prey far from the lower groups of the Columbine and the Gallinaceous sections without violating natural affinities based upon important characters. They correspond with these lower groups as regards external characters in the nature of their wing-coverts, and, as regards interior anatomical characters, in the nature of their carotides communes. Some of them, for instance those of the Vulturine section, exhibit, with regard to their form, a near analogy with some of those of the two mentioned groups. We may, for instance, compare a Condor with a Turkey. A system that places the dirty Vultures highest does not seem to us to indicate a correct idea of the nature of the birds.

If we do not regard flight, which is common to almost all birds, but consider birds with regard to the various other ways of motion for which they especially are shaped, and for which their structure is also adapted, we find easily that these in general may be comprehended in three different modes, viz: 1st, swimming on the water; 2d, running on the ground; and 3d, climbing and jumping on the branches of trees.* The hinder extremities or the legs exhibit, in conformity with this, three different forms. This induces us to divide the class of birds into three primary groups or sub-classes: 1, Natatores; 2, Cursores; 3, Insessores. Those belonging to the third group generally move more with the assistance of their wings than the others, except some forms of the Natatores, and show generally a higher development of the bird-type. This group also furnishes the greatest variety of forms. The Natatores include about 550 species, the Cursores 900, and the Insessores 6,900, (Bonaparte.)

Nitzsch has, in the treatise referred to, divided the class of birds into three groups: Aves aëreæ, Aves terrestres, and Aves aquaticæ, which in a reverse order correspond with the three groups here arranged; but he differs from us in including the Columbine birds among the Terrestres, and the Grallatorial birds

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* The second mode appears an intermediate link between the first and third.

among the Aquaticæ, and in considering the Struthionine birds a distinct group from the other three.

There is, as far as experience yet extends, a very remarkable correspondence between the nature of the upper wing-coverts and of the carotides communes, which adds to the importance of both these characters, which have generally been but little observed. All those birds that have the large upper wing-coverts of the first row on the cubitus so short that they do not reach beyond the middle of the cubital quills, have only one carotis communis, viz., the sinistra. Those birds in which the above-mentioned wing-coverts form several rows and extend beyond the middle of the cubital quills, have, on the contrary, generally two carotides communes, viz., one dextra and one sinistra. The only exceptions to this rule are *Cypselus, Trochilus, Merops*, one or a few species of *Psittacus, Rhea, Phœnicopterus, Podiceps,* and *Pelecanus,* which, although belonging to the latter category in regard to the wing-coverts, yet have only one carotis communis. This is the dextra in *Phœnicopterus.* We do not, therefore, hesitate to consider these two characters to be among the most important in judging of the affinity of the birds; and they show with certainty that the birds of prey have not their place at the beginning or at the end of the system.

The Strisores, one of the twelve orders in which we have arranged the class of birds, includes several birds that we formerly considered should belong to the Passeres, from their near correspondence in form with the latter. But as they deviate from them in regard to the upper wing-coverts and the claw of the hind toe, and sometimes even in regard to the carotides communes, we are of opinion that they should be regarded as belonging to a different order. They have been separated from the Passeres by Sundevall† and by Nitzsch; and the former has arranged them under the order Coccyges, which, according to him, also includes the *Zygodactyli* and *Columbæ.* They are, however, distinct from the Zygodactyli in the nature of their feet, and cannot be arranged under this order without depriving it of its most distinguishing character. They seem also to cause confusion if they are arranged within either of the orders Passeres or Zygodactyli; and we have therefore considered it right to arrange them as a distinct order—Strisores, which name was given to them by Cabanis in 1847. However distinct they seem to be, as well from the Columbine section and the birds of prey as from the Zygodactyli, it is very difficult to find any character that sharply and distinctly distinguishes them from these three orders; and we have been compelled to use a character in the scheme that does not belong to all, although the majority of them possess it. They appear to be an intermediate group between Accipitres, Zygodactyli, and Passeres.

The order Longipennes has generally had a very changeable place in the system, sometimes the first among the swimmers, sometimes the last. When the swimmers are, as here, arranged in two groups according to the form of the beak, their place is, as will be seen from the scheme, unquestionable, as we of course must begin with the Pygopodes. The Longipennes approach these very nearly in the genera *Puffinus* and *Halodroma. Puffinus* has, together with *Colymbus* and *Podiceps,* a long pyramidal erect process at the upper end of the tibia, and the tarsi are compressed like theirs. The genera *Phalacrocorax* and *Mergus* form an intermediate link between the Steganopodes and the Lamellirostres.

First Division or Sub-class.

NATATORES, Illiger; Sundevall.

Upper part of the crus (tibia and fibula) not free, but drawn in within the skin that covers the body.* The basis of the hind toe above that of the ante-

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† Kongl. Vetensk. Acad. Handl. 1843, pp. 375 and 376.

* Some of the Longipennes are said to form an exception to this.

rior toes, the hind toe sometimes absent. Legs short; and the anterior toes, sometimes even the hind toes, united by web. The upper large wing-coverts of the first row on the lower arm (antibrachium) extend in all beyond the middle of the cubital quills. All, with the exception of ***Podiceps***, have, as far as is known, two carotides communes.

Group 1. SIMPLICIROSTRES.

The bill without laminæ. Doubly monogamous.* "Altrices;" that is, carry food to their young.

Order 1. PYGOPODES, Illiger.

The legs are placed far back; and the hind toe is, when it is present, free. The wings short, hardly extending to the base of the tail. The tail short, or none at all. Heavy, clumsy birds, that dive well, but walk badly.

Note.—This order contains the typical forms of Natatores.

Order 2. LONGIPENNES, Duméril.

The legs are not so far back; and the hind toe, when there is one, is free. The wings long, extending more or less beyond the base of the tail. They are generally light birds, and lie, when swimming, shallow in the water, and cannot, with a few exceptions, dive, unless they dart from the air into the water, which power a great many of them possess. They generally fly remarkably well.

Order 3. STEGANOPODES, Illiger.

The hind toe united to the inner anterior toe by a web, and its base but slightly raised above that of the anterior toes. The wings and tail rather large, the former sometimes pointed and sometimes obtuse. Some of these birds are pelagic, fly remarkably well, and are darting divers; some fly badly, but dive and swim well. The position of the hind toe enables some of them at times to sit on the branches of trees and to build their nests there.

Group 2. LAMELLIROSTRES.

The bill with laminæ. Generally singly monogamous.† "Præcoces;" that is, do not carry food to their young.

Order 4. LAMELLIROSTRES, Cuvier.

The point of the upper jaw with a so-called nail of the bill; the other part of the bill covered with a soft skin. The hind toe free. The body generally more or less thick and heavy. The power of flight sometimes moderate, sometimes rather inferior. Those that fly best dive badly, or cannot dive at all; the others lie, when swimming, deep in the water, and dive exceedingly well. Some of the former are rather fast walkers, and approach in this respect the next division.

Second Division or Sub-class.

CURSORES, Illiger; Sundevall.

The entire crus and the lower part of the femur free. The base of the hind toe above that of the anterior toes;‡ the hind toe sometimes missing. The anterior toes, when united by a web, are, with very few exceptions, so united only

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* Both the old ones sit on the eggs, take care of the young, and carry food to them.

† The female alone cares for the young.

‡ The majority of the *Ardeidæ* make an exception to this; and these live and build very often in trees.

at the base. The large upper wing-coverts of the first row on the lower arm extend beyond the middle of the cubital quills. They have, with the exception of *Rhea* and *Phœnicopterus*, as far as is known, two carotides communes.

Order 5. GRALLÆ, Linné.

The legs high, and the lower part of the crus without feathers.* The wings well adapted for flying. The pectoral bone with a crista. They generally walk and run with ease or very fast, and mostly live in damp places, near swamps or on the banks of water-courses. The majority fly fast and with ease; some fly badly. They live generally in the middle ("mittlere," Faber) monogamy.† Præcoces. A great number of the *Ardeidæ* are Altrices.

Order 6. BREVIPENNES, Duméril.

The wings more or less rudimentary, and not adapted to flight. Pectoral bone without crista. A small number of large birds that run fast, and may be considered typical of the whole group. Their structure exhibits a strong tendency towards the mammalian. Some are said to live in the middle monogamy, others in single monogamy, and others again in polygamy. Præcoces.

Order 7. GALLINÆ, Linné.

The legs of a mediocre height, and the entire crus feathered.‡ The wings adapted to flying, but generally rather short and obtuse, and more or less bent. They run fast; but are easily fatigued by flying, and then hide among rocks, bushes, grass, &c. Some live in polygamy, but the majority live in middle monogamy. Præcoces.

Third Division or Sub-class.

INSESSORES, Vigors; Bonaparte.

The entire crus and the lower part of femur free. The coat of feathers generally extends at least to the tarsal joint.§ The hind toe with its base on a level with that of the anterior toes,‖ and very seldom missing.

Order 8. PULLASTRÆ, Sundevall.

The bill not covered by a cere at the base, but generally naked there, and with an inflated skin at the nostrils. The point of the upper jaw rounded, but very seldom bent down in the form of a hook. Three toes directed forward, and not united together. The large upper wing-coverts of the first row on the lower arm extend beyond the middle of the cubital quills. Two carotides communes. The majority fly very fast; some do not fly so well, but these run fast. The majority live in double monogamy, a few in middle or single monogamy (*Penelope*,) and a few in polygamy (*Crax*.) The majority are Altrices, the others Præcoces.

Note.—This order is evidently an intermediate group between Cursores and Insessores. The *Talegallinæ*, *Penelopidæ*, and *Didunculidæ* exhibit some tendency towards the Accipitres.

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* The genus *Scolopax* deviates from this.

† Both the old ones attend to their young, but do not carry food to them, letting them, under their care, hunt for their own food.

‡ The genus *Ortyxelos*, Vieill., is an exception to this.

§ *Didunculus* (*Pleiodus*) deviates from this, and has the lower part of the crus naked.

‖ The *Cathartini* form an exception to this.

Order 9. ACCIPITRES, Linné.

The bill covered with a cere at the base, convex towards the point; and the point of the upper jaw bent down in the form of a hook. The legs strong, with three anterior toes, which are not united, and are, like the hind toe, armed with strong bent claws. The wings large, with the large upper wing-coverts of the first row on the lower arm extending beyond the middle of the cubital quills. Two carotides communes. They have a strong power of flying, but run badly,* and do not jump. Doubly monogamous. Altrices. Their food consists generally of vertebrated animals.

Order 10. STRISORES, Cabanis.

The bill without a cere, hard at the base, without any swollen skin at the nostrils, and of a variable form. Three anterior toes, which are generally united at the base, sometimes there united by a web, and seldom free. The hind toe is at times turned forwards. The claw of the hind toe is smaller than the claw on the middle anterior toe, (Sundevall.) The large upper wing-coverts of the first row on the lower arm extend beyond the middle of the cubital quills. Some of them (*Caprimulgus, Coracias, Alcedo*) have two carotides communes, and some (*Cypselus, Trochilus, Merops*) have only one. *Buceros* is unknown as regards its carotides. Some fly remarkably well, others not so well. The legs are short in most of them, and not well adapted for walking. Doubly monogamous. Altrices.

Note.—A polymorphic group, that shows a tendency as well towards the Accipitres and Zygodactyli as towards the Passeres.

Order 11. ZYGODACTYLI, Vieillot.

Two anterior and two hind toes, or sometimes two anterior and one hind toe,† or one hind toe and three anterior ones, the exterior one of which is turned backwards. The claws compressed. The large upper wing-coverts of the first row on the lower arm, except in the *Picidæ* and *Bucconidæ*, do not extend beyond the middle of the cubital quills. Some have two carotides communes, and others (*Picus, Ramphastos, Cacatua*) only one. The power of flying not very good. They generally walk badly on the ground; but a great many of them climb well on the trees, and cling skilfully to the branches. Doubly monogamous. Altrices.

Order 12. PASSERES, Linné; Sundevall.

Three anterior toes and one hind toe, and the exterior anterior toe generally at the base united with the middle one. The claw of the hind toe as large as that of the middle anterior toe; and its long flexor muscle separated from the muscle that bends the claw phalanx of the anterior toes, (Sundevall.) The large upper wing-coverts of the first row on the lower arm do not extend beyond the middle of the cubital quills, and we meet with only one row of greater upper wing-coverts. As far as known, only one carotis communis, or truncus caroticus impar, which arises from the left arteria subclavia. Lively and active birds, with a fast and excellent flight, which move easily as well on the ground as on the branches of the trees. They generally jump on the ground, and seldom run. Some of them have a separate muscular apparatus for singing in the larynx inferior and a more or less exquisite song. Doubly monogamous. Altrices.

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* *Gypogeranus* deviates from this.

† The thumb or the proper hind toe, which corresponds with the inner hind toe in the others, is in this case missing, except in the *Trogonidæ*.

Note.—This order embraces the typical forms of the group Insessores, and the birds that generally have the highest degree of development.

In the following tables I have tried to use the most important as well as the most positive and evident characters, but have in this, like others, met with much difficulty of finding such of the smaller groups, or families and genera, in the higher orders. A great many of the characters used are taken from Sundevall; and in the Passeres several from Cabanis. Their validity has first been fully tested. In consequence of the above-mentioned difficulty we find that the place in the system of a form in question cannot always be ascertained from similar tables, as a more minute description is often necessary. It must not, therefore, be expected that these tables should give an infallible ground for the determination of the forms belonging to the respective families and sub-families, but only that they should denote some of the most important characters that form the basis for the groups, and give an easy review of these groups. Such a table shows us most plainly what characters are common and what are not.

As a great many of the exotic generic forms are not well known to me, I do not insist that they can be all arranged under the 69 families and 144 sub-families here charcterized, and that the arrangement of other families or sub-families is unnecessary; but I believe that a great part of the genera have been considered. I may mention that the difficulties arising in limiting the families *Corvidæ*, *Paridæ*, and *Sylvidæ* among the Passeres have induced me to make these families more comprehensive than they have been.

It seems that the *Epimachini* and *Paradiseini* should together form a separate family; but I have not been able to find any distinguishing characters, common to both, that make them distinct from the *Corvidæ*. The family *Corvidæ* corresponds with "cohors *Corviformes*," of Sundevall (Sevenska Foglarna.) The *Troglodytini* include forms of both *Troglodytinæ* and *Timalinæ*, Cabanis, excepting some with emarginated bill. The other *Liotrichidæ*, Cabanis, are given to the *Sylvidæ*, partly to *Lanini* and partly to *Sylvini*. It seems that the family *Brachypodidæ* as arranged by Cabanis should at least partly be included in the last-mentioned sub-family (*Sylvini*,) which, as it also embraces the *Sylviadæ*, Cabanis, is very rich, and contains about 500 species or more. I even include the *Vireoninæ*, Cabanis, in the *Sylvicolini*.

TABLE I. CONSPECTUS ORDINUM.

Orders.

BIRDS. Upper part of crus (tibia and fibula) .

- not free, but inclosed within the skin of the trunk. Feet palmate .... FIRST SUB-CLASS. **NATATORES,** Illiger. Bill ..
  - without lamellæ.. FIRST SECTION. Simplicirostres. Hinder toe ....
    - free or absent. Wings
      - short, when closed scarcely reaching the tail. Legs posterior .... 1. PYGOPODES.
      - long, passing beyond base of tail. Legs at centre of equilibrium .. 2. LONGIPENNES.
    - connected by membrane with inner toe, and nearly on level with others .... 3. STEGANOPODES.
  - lamellate .... SECOND SECTION. Lamellirostres .... 4. LAMELLIROSTRES.
- free, as well as the lower part of the femur. Base of hinder toe or hallux..
  - elevated above base of anterior toes .. SECOND SUB-CLASS. **CURSORES,** Illiger. Inferior part of crus.
    - naked. Posterior toe sometimes wanting. Sternum .
      - provided with a crest .... 5. GRALLÆ.
      - without crest .... 6. BREVIPENNES.
    - feathered. Posterior toe rarely absent .... 7. GALLINÆ.
  - not elevated above the base of the anterior toes; the entire posterior toe insistent,* (rarely obsolete.) THIRD SUB-CLASS. **INSESSORES,** Ill. Coverts of secondaries of first series.
    - exceeding half the length of secondary quills. *Flexor longus* muscle of posterior toe connected with the *flexor communis perforans* of the anterior toes, (Sundevall.) Toes..............
      - three anterior and one posterior. Bill......
        - ..
          - without cere or hook, generally naked at base, or with large swollen skin above nostrils .... 8. PULLASTRÆ.
          - covered with a cere at base and with the point of the upper mandible hooked. 9. ACCIPITRES.
        - without cere, hard at the base. Claw of the hind toe smaller than the claw of the median anterior one. Anterior toes often connected at the base. 10. STRISORES.
      - two anterior and two posterior, or rarely one posterior and three anterior of which the external is versatile .... 11. ZYGODACTYLI.
    - not exceeding half the length of the secondary quills. *Musculus flexor longus* of the posterior toe not connected with the *m. flexor communis perforans* of the anterior toes. Claw of the posterior toe not smaller than the claw of the anterior median one .... 12. PASSERES.

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* By insistent is meant the application of the inferior surface to the ground or object on which the foot rests. As there is no single English word to express this character, the Latin term used by Lilljeborg is adopted, modified only by an English termination.—G.

TABLE II. CONSPECTUS FAMILIARUM.

						Family.
PYGOPODES. Wing with quills and thumb.	wanting					1. APTENODYTIDÆ, Sund.
	present. Feet with posterior toe	wanting				2. ALCIDÆ, G. Gray.
		present	palmate			3. COLYMBIDÆ, Sund.
			lobate			4. PODICIPIDÆ.
LONGIPENNES. Nares	prominent, tubular; posterior toe none, or represented only by an immovable nail					5. PROCELLARIDÆ, Sund.
	normally developed					6. LARIDÆ, Sund.
STEGANOPODES. Bill	not hooked; edge of mandibles serrated					7. DYSPORIDÆ, Sund.
	hooked; edge of mandibles entire					8. PELECANIDÆ, G. Gray.
LAMELLIROSTRES. Lamellæ of bill	like the teeth of a saw, and directed backwards, at least in the upper mandible; nail hook-shaped					9. MERGIDÆ, Bonap.
	slender and unlike the teeth of a saw; nail not hook-shaped. Bill broader					10. ANATIDÆ, G. Gray.
	Section *Anatiformes.*					
GRALLÆ. Bill	lamellate					11. PHŒNICOPTERIDÆ, Bonap.
	simple. Wing	Section *Amblypteræ.* obtuse; primaries from second to fifth longest; the secondaries long. Wings	more or less short, and the humerus turned backwards, not passing beyond the acetabulum. Toes	long, and hind toe inserted near the lower end of tarsus. Tarsi in front	transversely scutellate	12. RALLIDÆ, Sund.
					reticulate	13. PALAMEDEIDÆ, G. Gray.
				short; hind toe inserted more or less high		14. PSOPHIDÆ, Bonap.
			long, and humerus turned backwards, passing beyond the acetabulum. Nasal aperture between the maxillaries and intermaxillaries	small, and in basal half of bill. Base of hinder toe	on same plane as bases of anterior toes	15. ARDEIDÆ, Sund.
					elevated more or less above the bases of anterior toes	16. CICONIDÆ, Bonap.
				large, and beyond middle of bill. Base of hinder toe quite elevated		17. GRUIDÆ, Bonap.
		Section *Oxypteræ.* acute, with first or second primary longest, or second and third sub-equal. Bill	soft and flexible, long and slender, and without an angle at the lower margin of the lower mandible. Anterior toes rather long. Orbits below	open		18. TOTANIDÆ.
				closed		19. SCOLOPACIDÆ, Bonap.
			hard, generally short, thicker toward the apex, and with the lower margin of the lower mandible angulated. Toes rather short. Upper mandible behind the tip	entire		20. CHARADRIIDÆ, Bonap.
				notched		21. OTIDIDÆ.
BREVIPENNES. Nostrils	normally situated					22. STRUTHIONIDÆ, Sund.
	at the end of the long bill					23. APTERYGIDÆ, G. Gray.
GALLINÆ. Wings	short and obtuse. Anterior toes at base	separated				24. CRYPTURIDÆ, Sund.
		more or less connected by skin. Upper margin of bill before nostrils	forming a continuous arch. Bill with no fleshy skin at base			25. TETRAONIDÆ, Sund.
			impressed. Bill covered with fleshy skin at base			26. PHASIANIDÆ, Sund.
	long and pointed. First or second primary longest					27. PTEROCLIDÆ, Sund.
PULLASTRÆ. Anterior toes at base	connected by skin. Claws	but little arcuate; that of hinder toe not larger than the others				28. MEGAPODIDÆ, G. Gray.
		strongly arcuate; that of hinder toe largest				29. PENELOPIDÆ, Sund.
	separated. Lower mandible	not toothed in front				30. COLUMBIDÆ, Sund.
		toothed in front				31. DIDUNCULIDÆ, Bonap.
ACCIPITRES. Eyes	Section *Diurni.* lateral. Claw of posterior toe	not larger than claw of median anterior toe. Claws obtuse. Head more or less naked				32. VULTURIDÆ, Sund.
		larger than claw of median anterior toe. Claws acute. Head feathered				33. FALCONIDÆ, Sund.
	Section *Nocturni.* directed fowards					34. STRIGIDÆ, Sund.

TABLE II. CONSPECTUS FAMILIARUM—Continued.

Family.

STRISORES. Anterior toes
- connected by a movable skin. Gape very large. Secondaries long …… 35. CAPRIMULGIDÆ, Sund.
- not connected by movable skin, though sometimes more or less united. Secondaries
  - very short, not extending to base of tail. Wings long and arcuate. Bill
    - short and broad at base. Hinder toe generally versatile forwards …… 36. CYPSELIDÆ, Sund.
    - long and slender. Hinder toe not versatile …… 37. TROCHILIDÆ, Sund.
  - rather long and passing beyond base of tail. Anterior toes at base
    - unconnected. Bristles rigid …… 38. CORACIDÆ, Sund.
    - united. Feet
      - small, with the tarsi short. Bill
        - arcuate downwards …… 39. MEROPIDÆ, Sund.
        - straight, or little recurved …… 40. ALCEDINIDÆ, G. Gray.
      - large, with tarsi quite long or moderate; sometimes rather short …… 41. BUCEROTIDÆ, Sund.

ZYGODACTYLI. External or fourth toe
- versatile forwards …… 42. MUSOPHOGIDÆ, Sund.
- not versatile. Second toe
  - directed backwards …… 43. TROGONIDÆ, Sund.
  - directed forwards. Bill
    - without cere. Tongue
      - not extensible. Anterior toes
        - united as far as the outer end of the second phalanx …… 44. GALBULIDÆ, Sund.
        - not united to the outer end of the second phalanx, but only
          - to the outer end of the first phalanx. Bill
            - not twice as long as the head. Nostrils in their usual position. Bristles generally present …… 45. BUCCONIDÆ, Sund.
            - very large, generally twice or more than twice as long as head. Nostrils in the dorsal surface of bill, and not surrounded by skin. Bristles absent. 46. RAMPHASTIDÆ, Bonap.
          - near base. Bristles none …… 47. CUCULIDÆ, Sund.
      - lumbriciform and extensible …… 48. PICIDÆ, Bonap.
    - provided with a cere. Tip of upper mandible hooked …… 49. PSITTACIDÆ, Bonap.

PASSERES.* Posterior surface of tarsus covered
- with many small corneous scutellæ, or sometimes on the outer side with one lamina, and on the inner side with none. Apparatus for singing generally wanting …… First Section. *Clamatores*, A. Wagner; Cabanis.
- with two corneous entire laminæ, connected at the posterior margins. Provided with an apparatus for singing in the lower larynx …… Second Section. *Oscines*, Pallas; Cabanis.

CLAMATORES. Singing apparatus
- absent. Mandible within, towards tip.
  - excavated. Anterior scutellæ of tarsi
    - covering also the inner side of tarsus. Upper mandible generally entire behind tip …… 50. ANABATIDÆ, Sund.
    - not covering the inner side of tarsus. Upper mandible notched behind tip. Tarsus
      - reticulated. Anterior toes
        - only two connected at base. Margins of mandibles
          - entire …… 51. AMPELIDÆ, Sund.
          - serrated …… 52. PHYTOTOMIDÆ, Bonap.
        - all connected at base; the median and external in greater degree 53. PIPRIDÆ, Sund.
      - not reticulated, with the external side covered
        - with the anterior scutellæ which are brought round to the back. Tarsi
          - long and slender .. 54. PLATYRHYNCHIDÆ, Sund.
          - short or moderate . 55. TYRANNIDÆ, Sund.
        - with an entire or divided plate …… 56. ERIODORIDÆ, Cabanis.
  - almost plane …… 57. UPUPIDÆ, Bonap.
- present. Innermost secondaries
  - produced. Claw of hind toe less arched than the others …… 58. ALAUDIDÆ, Sund.
  - not produced. Claw of hind toe like the others …… 59. BOMBYCILLIDÆ.†

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* The order of Passeres is here classified on the same principles as by Sundevall in his Birds of Sweden.

† Although provided with a singing apparatus, the *Bombycillidæ*, nevertheless, seem to have a greater affinity with the *Ampelidæ*.

OSCINES. Anterior surface of tarsi.	covered with many scutellæ. Tongue..	tubular, long and extensible. Bill generally long, slender, arcuate, and acute						60. NECTARINIDÆ, G. Gray.
		neither tubular nor extensible. Angle of chin.....	extended far forward, and anterior to nostrils. Bill large, generally more or less conic. Feet stout					61. CORVIDÆ, Bonap.
			not far forward. Margins of mandible..	acute and inflexed. Bill stout, conic, rarely perceptibly notched behind the tip of the upper mandible				62. FRINGILLIDÆ, Bonap.
				not acute or inflexed. Number of primaries.	9. Bill notched behind tip. Wings....	moderate, when folded, not ten times as long as the tarsus. Inner secondaries	not produced	63. TANAGRIDÆ, Sund.
							produced, sometimes reaching as far as the ends of the wing.	64. MOTACILLIDÆ, Bonap.
						very long, when folded about ten times as long as the tarsus, which is short. Bill slender and depressed		65. HIRUNDINIDÆ, Bonap.
					10. Bill behind tip of the upper mandible.	entire		66. PARIDÆ, Bonap.
						notched		67. SYLVIDÆ, Bonap.
	chiefly covered with a single entire plate. Primaries 10. Nostrils...	more or less covered with feathers						68. REGULIDÆ.*
		naked						69. TURDIDÆ, Bonap.

* The *Regulidæ* seem to be related to the *Paridæ*, but differ in the structure of the bill and feet.

TABLE III. SYNOPSIS OF SUB-FAMILIES.

				Sub-families.
PYGOPODES.				
APTENODYTIDÆ				1. *Aptenodytini.*
ALCIDÆ				2. *Alcini*, Bonap.
COLYMBIDÆ				3. *Colymbini*, Bonap.
PODICIPIDÆ				4. *Podicipini*, G. Gray.
LONGIPENNES.				
PROCELLARIIDÆ				5. *Procellarini*, Bonap.
LARIDÆ.	Upper mandible..	hooked		6. *Larini*, Bonap.
		not hooked		7. *Sternini*, Bonap.
STEGANOPODES.				
DYSPORIDÆ				8. *Dysporini.*
PELECANIDÆ				9. *Pelecanini*, Bonap.
LAMELLIROSTRES.				
MERGIDÆ				10. *Mergini*, Bonap.
ANATIDÆ.	Hind toe.	lobate		11. *Fuligulini*, Swains.
		not lobate		12. *Anatini*, Swains.
GRALLÆ.				
PHŒNICOPTERIDÆ				13. *Phœnicopterinæ*, Bonap.
RALLIDÆ.	Anterior toes.	connected by skin at base, and lobate		14. *Fulicinæ.*
		unconnected. Claws..	moderate, arcuate	15. *Rallinæ*, Bonap.
			very long, straight, or little arcuate	16. *Parrinæ*, Bonap.
PALAMEDEIDÆ				17. *Palamedeinæ*, Bonap.
PSOPHIDÆ.	Hind toe.	moderate, partly insistent		18. *Psophinæ*, G. Gray.
		very short, scarcely insistent		19. *Dicholophinæ.*
ARDEIDÆ.	Bill.....	cultriform		20. *Ardeinæ*, Bonap.
		not cultriform, with the tip of the mandible more or less hooked		21. *Scopinæ*, Bonap.
CICONIDÆ.	Bill.....	cultriform		22. *Ciconinæ*, Bonap.
		not cultriform..	depressed, with the tip expanded and rounded	23. *Plataleinæ*, Bonap.
			sub-cylindrical, at least arcuate towards the tip	24. *Tantalinæ*, Bonap.
GRUIDÆ				25. *Gruinæ*, Bonap.

TABLE III. SYNOPSIS OF SUB-FAMILIES—Continued.

				Sub-families.
TOTANIDÆ. Feet	long. Tarsus generally* longer than a fourth of the folded wing. Anterior toes more or less connected by skin. Legs	very long. Tarsus longer than a third of the folded wing. Hind toe small or absent		26. *Recurvirostrinæ*, Bonap.
		long. Tarsus not or scarcely longer than a third of the folded wing. Hind toe insistent		27. *Totaninæ*, G. Gray.
	moderate. Tarsus shorter than a fourth of the folded wing. Anterior toes	lobate, and connected by skin at base		28. *Phalaropodinæ*, Bonap.
		not lobate		29. *Tringinæ*, Bonap.
SCOLOPACIDÆ				30. *Scolopacinæ*, Bonap.
CHARADRIIDÆ. Anterior toes	broad, widely margined, connected at base. Bill longer than tarsus			31. *Hæmatopodinæ*, Bonap.
	moderate, not, or little margined. Bill not longer than tarsus. Mouth	small, its angles not extended behind the base of bill		32. *Charadriinæ*, Gray.
		larger, and with the fissure extending behind the base of the bill. Hind toe	absent	33. *Cursoriinæ*, G. Gray.
			present	34. *Glareolinæ*, Bonap.
OTIDIDÆ				35. *Otidinæ*, Bonap.

BREVIPENNES.

STRUTHIONIDÆ	36. *Struthionini*, Bonap.
APTERYGIDÆ	37. *Apterygini*, G. Gray.

GALLINÆ.

CRYPTURIDÆ.	Bill	slightly depressed towards base, and scarcely compressed towards apex. Tail very short and covered, or none. Hallux or hind toe generally present			38. *Tinaminæ*, G. Gray.
		compressed, especially towards apex. Tail more or less distinct. Hallux absent			39 *Turnicinæ*, G. Gray.
TETRAONIDÆ.	Tarsi	naked. Skin above nostrils naked			40. *Perdicinæ*, Bonap.
		more or less feathered. Skin above nostrils feathered			41. *Tetraoninæ*, Bonap.
PHASIANIDÆ.	Tail	depressed	short, and mostly covered by the coverts		42. *Numidinæ*, Bonap.
			more or less long, and mostly visible. Head and neck	partly naked	43. *Meleagrinæ*, G. Gray.
				feathered	44. *Pavoninæ*, Bonap.
		more or less compressed and erect			45. *Phasianinæ*, Bonap.
PTEROCLIDÆ.	Tarsi	feathered			46. *Pteroclinæ*, Bonap.
		naked. Nostrils	not covered		47. *Thinocorinæ*, Bonap.
			covered		48. *Chionidinæ*, Bonap.

PULLASTRÆ.

MEGAPODIDÆ.	Head and neck	feathered		49. *Megapodinæ*, Swains.
		partly naked, and only beset with scattered piliform feathers		50. *Talegallinæ*, Bonap.
PENELOPIDÆ.	Bill	high, short, convex from the base		51. *Cracinæ*, G. Gray.
		oblong, only convex near the tip		52. *Penelopinæ*, Bonap.
COLUMBIDÆ.	Tarsus	as long as or longer than the middle toe		53. *Gourinæ*, G. Gray.
		shorter than the middle toe. Bill	slender	54. *Columbinæ*, Illig.
			short and stout	55. *Treroninæ*, G. Gray.
DIDUNCULIDÆ				50. *Didunculinæ*, Bonap.

ACCIPITRES.

VULTURIDÆ.	Hallux and its nail	less than anterior toes, and inserted above their base	57. *Cathartini*, De Lafr.
		about as long as inner toe	58. *Vulturini*, Illig.

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* The genera *Tringoides* and *Terekia*, as well as the species *Limosa rufa*, do not exhibit this character.

FALCONIDÆ. Chin ......
- bearded ...... 59. *Gypaëtini*, Bonap.
- not bearded. Behind ears.
  - no collar. Bill ....
    - not toothed. Wings ...
      - passing beyond middle of tail. Lore covered with .....
        - setæ. Tail..
          - truncated or rounded. Anterior surface of tarsi ......
            - more or less reticulate. Outer toe.
              - not versatile ...... 60. *Polyborini*, Bonap.
              - versatile.... 61. *Pandionini*, Bonap.
            - feathered, or more or less covered with transverse plates...... 62. *Aquilini*, Swains.
          - forked or emarginated ...... 63 *Milvini*, Bonap.
        - dense feathers ...... 64. *Pernini*.
      - not passing beyond middle of tail. Two median tail feathers .
        - elongated ...... 65. *Gypogeranini*, Bonap.
        - not elongated...... 66. *Asturini*, G. Gray.
    - toothed behind tip of upper mandible ...... 67. *Falconini*, Bonap.
  - a more or less distinct collar, formed of feathers broader and incurved towards their ends...... 68. *Circini*, Bonap.

STRIGIDÆ. Aperture of ear with cutaneous lobe or valve ......
- absent. Radiate facial area incomplete ...... 69. *Surnini*, Bonap.
- present. Radiate facial area complete......
  - rounded...... 70. *Strigini*, Bonap.
  - pointed below, subtriangular. Nail of middle toe pectinated on inner side...... 71. *Hybridiui*.*

STRISORES.

CAPRIMULGIDÆ. Nail of middle toe..
- pectinated inside. Bill weak...... 72. *Caprimulgini*, Bonap.
- not pectinated. Bill stout ...... 73. *Steatornithini*, G. Gray.

CYPSELIDÆ. Hallux...
- versatile forwards ...... 74. *Cypselini*, Bonap.
- not versatile ...... 75. *Collocalini*.

TROCHILIDÆ. Anterior toes at base..
- disconnected ...... 76. *Trochilini*, Bonap.
- connected ...... 77. *Phaëthornithinæ*, G. Gray.

CORACIDÆ ...... 78. *Coracini*, Bonap.

MEROPIDÆ. Bill with margins..
- serrated...... 79. *Prionitini*, Bonap.
- entire ...... 80. *Meropini*, Bonap.

ALCEDINIDÆ. Bill at base..
- compressed ...... 81. *Alcedinini*, Bonap.
- broad, not compressed ...... 82. *Halcyonini*, G. Gray.

BUCEROTIDÆ ...... 83. *Bucerotini*, Bonap.

ZYGODACTYLI.

MUSOPHAGIDÆ. Hallux..
- versatile...... 84. *Colini*, Bonap.
- not versatile ...... 85. *Musophagini*, Swains.

TROGONIDÆ ...... 86. *Trogonini*, Bonap.

GALBULIDÆ ...... 87. *Galbulini*, Bonap.

BUCCONIDÆ. Tarsus ..
- shorter than external toe ...... 88. *Bucconini*, Bonap.
- as long as or longer than outer toe ...... 89. *Capitonini*, Bonap.

RAMPHASTIDÆ ...... 90. *Ramphastini*, Bonap.

CUCULIDÆ. Nostrils ....
- not in a nasal fossa or aperture, more or less open and between the nasal bones and maxillaries. Bill large and stout...... 91. *Phœnicophæini*, G. Gray.
- in a nasal fossa or aperture, and more or less covered by a membrane or shield. Tail......
  - long, cuneate, rounded. Bill ......
    - long, straight, with mandible arched only at tip ...... 92. *Saurotherini*, Bonap.
    - short, with upper mandible curved almost from baso. ...... 93. *Cuculini*, Bonap.
  - moderate, emarginated. Bill short and stout...... 94. *Indicatorini*, Swains.

PICIDÆ. tail feathers ..
- soft, with the tip rounded...... 95. *Jyngini*, Bonap.
- rigid, with the tip more or less acute ...... 96. *Picini*, Swains.

PSITTACIDÆ. Tarsus ......
- not longer than the inner anterior toe without its nail. Eyes......
  - surrounded by an incomplete facial area...... 97. *Strigopini*, Bonap.
  - not surrounded by a facial area. Tongue......
    - small, slender, and cylindrical ...... 98. *Microglossini*, Bonap.
    - broad, depressed, and fleshy. Tail ......
      - short, rounded or even...... 99. *Psittacini*, Illiger.
      - more or less long and cuneate ...... 100. *Araini*, G. Gray.
- longer than the inner anterior toe without its nail. Claws.
  - short and strongly incurved ...... 101. *Platycercini*, Swains.
  - long and but little incurved ...... 102. *Pezoporini*, Bonap.

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* Named from the genus *Hybris* Nitzsch, of which the *Strix flammea* is the type.

TABLE III. SYNOPSIS OF SUB-FAMILIES.

PASSERES—Clamatores.

							Sub-families.
ANABATIDÆ.	Outer toe	nearly equal to the middle					103. *Dendrocolaptini*, G. Gray.
		shorter than the middle. Hallux longer than in the preceding					104. *Anabatini*, Swains.
AMPELIDÆ.	Bill	thick and convex, not compressed. Second primary abbreviated in the males					105. *Psarini*, Bonap.
		broad at base, compressed towards tip. Second primary not abbreviated					106. *Ampelini*, Swains.
PHYTOTOMIDÆ							107. *Phytotomini*, Swains.
PIPRIDÆ							108. *Piprini*, Swains.
PLATYRHYNCHIDÆ							109. *Platyrhynchini*.
TYRANNIDÆ.	Bill	large and thick, wider than high at base					110. *Tyrannini*, Swains.
		moderate, not wider than high					111. *Fluvicolini*, Swains.
ERIODORIDÆ.	Outer side of tarsus	covered with scutellæ. Bill	high and stout, like that of *Lanius*				112. *Thamnophilini*, Swains.
			weak, like that of *Turdus*				113. *Myiotherini*, Swains.
		covered with an entire plate					114. *Hypocnemidini*, Cabanis.
UPUPIDÆ.	Claw of hallux	strongly incurved. Head without crest					115. *Irrisorini*.
		almost straight. Head with crest					116. *Upupini*, Bonap.
ALAUDIDÆ							117. *Alaudini*, Bonap.
BOMBYCILLIDÆ							118. *Bombycillini*, Swains.

PASSERES—Oscines.

							Sub-families.
NECTARINIDÆ.	Number of primaries	10. Tongue	penicillate				119. *Melliphagini*, Bonap.
			not penicillate				120. *Nectarinini*, G. Gray.
		9					121. *Dacnidini*, Cabanis.
CORVIDÆ.	Bill	long, slender, and incurved					122. *Epimachini*, Cabanis.
		stout, rather short or moderate. Feet	large. Males with ornamental plumes variously formed and arranged				123. *Paradiseini*, Bonap.
			moderate. Fissure of mouth	straight. Mandible behind tip	very slightly or indistinctly notched		124. *Corvini*, Bonap.
					distinctly notched. Tarsus	longer than middle toe with claw	125. *Garrulini*, Swains.
						not longer than middle toe with claw	126. *Oriolini*, Swains.
				descending. Number of primaries	10		127. *Sturnini*, Bonap.
					9		128. *Icterini*, Swains.
FRINGILLIDÆ.	Number of primaries	10. Culmen of bill extending at base between frontal feathers					129. *Ploceini*, Bonap.
		9. Palate	without tubercle or ridge				130. *Fringillini*, Bonap.
			with tubercle or ridge. Lower mandible generally larger than upper				131. *Emberizini*, Bonap.
TANAGRIDÆ.	Bill	thick, more or less conic, and sometimes dilated at base					132. *Tanagrini*, Bonap.
		slender and subulate					133. *Sylvicolini*, Bonap.
MOTACILLIDÆ							134. *Motacillini*, Bonap.
HIRUNDINIDÆ							135. *Hirundinini*, Bonap.
PARIDÆ.	Outer toe	much longer than the inner. Hallux and its claw large					136. *Certhini*, Bonap.
		little or no longer than the inner. Bill	short, conic				137. *Parini*, Bonap.
			slender and more or less subulate. Wings short and rounded, with the first primary more than half as long as second				138. *Troglodytini*, Swains.
SYLVIDÆ.	Bill	broad and depressed at base. Feet very small					139. *Muscicapini*, Bonap.
		compressed. Feet moderate or large. Bill	large and strong, deeply notched behind the hooked tip of the mandible				140. *Lanini*, Bonap.
			moderate, subulate, emarginated behind the tip of the mandible				141. *Sylvini*, Bonap.
REGULIDÆ							142. *Regulini*, Bonap.
TURDIDÆ.	Wings	short, arcuate and concave					143. *Cinclini*, Bonap.
		moderate, not concave					144. *Turdini*, Bonap.

PRIZE QUESTIONS.

PRIZE QUESTIONS PROPOSED IN 1865 BY THE ROYAL DANISH SOCIETY OF SCIENCES.

Mathematical class.—The theory of rectilinear surfaces has been long since carried to the height of perfection and elegance, so that not only has the full and complete distribution of surfaces of this kind into species been divulged, but for each class also, if the partial differential equation, by means of which classes are distinguishable among themselves, be supplied, the geometric character of the several classes may be determined. On the other hand, the theory of those surfaces which are generated by a movable circle, and which are hence called "circular surfaces," has not been so thoroughly considered as is desirable; for, although certain surfaces of this kind have been adequately analyzed, and all are readily susceptible of distribution into species and classes, yet the general treatment of this branch of the subject has been hitherto neglected. Hence the Royal Danish Society of Sciences offers its gold medal, of the value of fifty Danish ducats, for a satisfactory discussion of the following theme:

"The analytical investigation of circular surfaces, with a view to the distribution of all such surfaces into proper species, each of which species again may be distinctly defined by means of its own partial differential equation."

Physical class.—It is proposed accurately to determine by actual experiments what time is required in order that the blood, greatly diminished and diluted through phlebotomy or arteriotomy, may be restored, both as to the whole quantity and the quantity of red corpuscles, to its normal condition in the animal. With this view a series of experiments on some species of mammalia, nourished with food of a certain quantity and quality, should be instituted; the alterations in the weight of the whole body should be noted, as well as the quantities of urea daily secreted.

In order to determine the quantity of blood which, remaining in the animal after the effusion, cannot be directly measured, it will be competent to apply the following method: The parts, after the spontaneous effusion of blood is exhausted, are to be dissected, macerated, and washed in water to complete discoloration, by which means the blood contained in the colored water may be determined by using as a means of comparison a portion of the blood which flowed at the beginning of the experiment, agitated with a certain quantity of water to a corresponding degree of coloration with the former. For comparing the red corpuscles also the different relative determinations may be employed which result from a comparison of the blood agitated with the serum: 1, with reference to the quantities of solid parts; 2, the quantities of albumen; 3, the specific weights; and 4, the determination which is obtained from a comparison of the quantities of blood necessary to color a certain quantity of water to a certain degree.

The prize will be the gold medal of the society, with the addition of one hundred *imperiales.*

Historical class.—The questions proposed for the preceding year (*de conventu Urnehoved*) are continued for solution till the next.

Classenian bequest.—As it is known that several metals which are deposited in the electrical way possess peculiar properties which are of no little importance in the technical arts, the society offers a premium of one hundred *imperiales* to the candidate who, upon accurate examination, shall furnish a comparison of the qualities of metals or compounds of metals deposited by electrolysis, with the qualities of the same metals produced in another way.

In the treatment of the above inquiries, the Latin, French, English, German, Swedish, or Danish languages may be used. The memoirs communicated must be anonymous; but the name, style and domicile of the author of each will be conveyed in a sealed note, which will bear a mark or epigraph corresponding with a similar one on the paper offered for competition. Communications will be made to G. Forchhammer, corresponding secretary, before the end of October, 1866.

PRIZE QUESTION IN PHYSICS, PROPOSED MAY 30, 1865, BY THE IMPERIAL ACADEMY OF SCIENCES OF VIENNA.

Since the time when the existence of two opposite electrical states or conditions was inferred by Grey from the attraction and repulsion of electrical bodies, up to the present day, when we have learned, by means of magnetic influence, to separate from one another the two opposite currents, the positive and negative, and to present each distinct in itself, there has been recognized a succession of other facts which authorize us to regard these conditions as different and opposed in other respects. The occasion would, therefore. seem to have arisen for a critical examination of the collective facts already discovered and bearing upon this subject, and for their discussion in connexion with the question: how far they lend support to one or other of the existing hypotheses on the nature of the electric principle.

The mathematico-physical class of the Imperial Academy of Sciences has therefore decided to propose a prize for a satisfactory solution of the following question in the department of physics: "To collate and critically examine the phenomena which, since the thirtieth year of the eighteenth century, have been recognized as distinguishing positive and negative electricity from one another, as well in their statical as active state, and to discuss those phenomena in connexion with the question: in what relation they stand to one or other of the hypotheses already advanced respecting the nature of the electrical principle."

The discovery of hitherto unknown and important criteria of the two electrical states, or the proposal and proof of a new hypothesis on the nature of electricity, more closely corresponding with the phenomena than those already adduced, though not made an express condition of success, will yet be regarded with peculiar favor in the assignment of the prize. The time limited for competition is the 31st of December, 1867; the adjudication of the prize of two hundred Austrian ducats will take place at the stated meeting of the year 1868. The memoir offered for competition must not contain the name of the author, but be inscribed, as usual, with a motto which will be repeated on a sealed note communicating his name and address. At the regular meeting, May 30, the note pertaining to the successful treatise will be opened, and the name announced by the president. There will be no division of a prize among several competitors, nor will members of the academy be allowed to enter the lists. The preferred memoir will remain the property of its author, but will be published, if he desires it, by the academy; as will other memoirs adjudged worthy of publication, should the authors signify their wishes to that effect.

PROGRAMME OF THE PONTIFICAL ACADEMY OF THE NUOVI LINCEI.

The academy, with the view of conferring the annual prize founded by the testamentary liberality of one of its members in ordinary, the late Chevalier PIERRE CARPI, proposes the discussion of the following theme: To discover a method by means of which may be determined *all* the rational values of x capable of reducing to a perfect square or cube the polynome $A+Bx+Cx^2+Dx^3+Ex^4$

by whole values of A, B, C, D, E, provided that one or more of these values of x really exist, and if not, that the impossibility of their existence be shown.

Explanation.—A method employed by the celebrated Pierre de Fermat to reduce to a square $A+Bx+Cx^2+Dx^3+Ex^4$, or to a cube the expression, $A+Bx+Cx^2+Dx^3$, is given by P. Jacques de Billy in his work entitled *Doctrinæ analyticæ inventum novum,* (p. 30 and 31 of the edition entitled *Diophanti Alexandrini libri sex, et de numeris multangulis liber unus, &c. Tolosæ, MDCLXX.*) This method is also explained by Leonard Euler in the eighth, ninth, and tenth chapters of the second volume of his work entitled *Einleitung der Algebra,* translated into French under the title of *Elemens d'Algebre.*

The XI volume of Memoirs of the Imperial Academy of Sciences of St. Petersburg (1830) contains several posthumous memoirs of Euler, relative to the analysis of Diophantus, one of which is entitled *Methodus nova et facilis formulas cubicas et biquadraticas ad quadratum reducendi.* This method, well considered, is no other, says Jacobi, than that of the *multiplication of elliptic integrals;* a method already proposed by Euler himself in his Institutions of the integral calculus and elsewhere, in order to resolve algebraically the transcendental equation $\pi(y)=n\pi(x)$, or $\pi(x)=\int_0^x \frac{dx}{\sqrt{f(x)}}$, $f(x)=a+bx+cx^2+dx^3+ex^4$. This observation of Jacobi is found in the XIII volume of the *Journal de Mathematiques* of M. A. L. Crelle (1835) at the article, *De usu theoria integralium ellipticorum et integralium Abelianorum in analysi Diophantea.* The method given by Fermat for reducing to a square $A+Bx+Cx^2+Dx^3+Ex^4$ is also stated in the volume entitled *Theorie de nombres, 3d edition; by Adrien Marie Legendre. Tome* ii, *Paris,* 1830, (p. 123–125.) In a memoir of Lagrange, entitled *Sur quelques problemes de l'analyse de Diophante,* and inserted in the *Nouveaux Memoires de l'Academie royale des Sciences et belles-lettres,* 1777*; á Berlin,* 1779, a method also is given for resolving into rational numbers the general equations of the third and fourth degree between two indeterminates x, y.

Nevertheless, these methods are imperfect: 1st, because they already suppose a known solution; 2d, because it is not demonstrated that they furnish all the solutions possible. It is therefore desirable that another should be found in which there should be no need of the knowledge of any solution, and it should be made to appear whether the problem be or be not possible, and, if possible, that all the solutions be given. This would be of great advantage in the theory of numbers, or indeterminate analysis, and would open the way to important progress, the question having as yet not been satisfied, except in very special cases treated by learned geometers under the above-noticed conditions. It would be conducive also to the progress of other parts of the mathematical sciences, as may readily be seen from the relation, indicated by Jacobi in the memoir already cited, between the problem propounded and the doctrine of elliptical functions.

Conditions.—Memoirs on the proposed theme should be rendered in Italian, Latin, or French; no other language is admissible. Each memoir will bear a motto, which shall be repeated on a sealed envelope containing the name and address of the author; and only that envelope will be opened corresponding to the memoir which shall have obtained the prize. If the authors who receive honorable mention desire that the academy should publish their names, it will be necessary for them to signify their wishes within three months from the day on which the prize is awarded; at the end of that term the envelopes will be burned without having been unsealed. The academy has decided that, with the exception of its own thirty members in ordinary, any one, whatever his nationality, may compete for the prize. The memoirs and envelopes must be transmitted free of postage before the 1st of October, 1866, when the competition will close, and the prize will be awarded in January, 1867. It will consist of a gold medal of the value of *one hundred Roman scudi.* The preferred memoir will be published entire or by extracts in the Acts of the Academy, and the author will receive fifty copies.

N. CAVALIERI SAN BERTOLO,
President.
P. VOLPICELLI, *Secretary.*

ROME, *June* 11, 1865.

ROYAL SCIENTIFIC AND LITERARY INSTITUTE OF LOMBARDY.—SUBJECTS ANNOUNCED FOR COMPETITION IN THE ANNUAL MEETING OF THE 7TH AUGUST, 1865.

Class of letters and of moral and political sciences.—Subject for the year 1866, proposed 7th August, 1864: "Of the principle of nationality in modern European society." Time for presenting the memoirs, the whole of February, 1866.

Class of mathematical and natural sciences.—Subject for the year 1867, proposed 7th August, 1865: "To give the genetic history of some species of intestinal worm pertaining to the family either of the Ascaridæ, or the Oxyuridæ, or the Strongylidæ, describing the entire cycle which it fulfils; prefaced by a succinct account of the actual state of this branch of science."

Recent investigations in Germany seem to favor the opinion that the nematoid worms undergo metamorphoses similar to those of the cestoids. It would greatly promote our knowledge of this department of science to verify the reality of these facts with new and conclusive experiments in addition to what has already been performed; but this cannot be done except by following the development of a considerable number of species belonging to the order of worms above indicated. For this academical body, however, it will be sufficient if an account be given of the mode of evolution of some species pertaining to one of the three families of nematoids, of which the ascaris, the oxyuris, and the strongylus are types. As these families comprise species by which man and many domestic animals are infested, inquiries such as these might prove also of advantage to medical practice. The memoir should be furnished with appropriate illustrations.

The time limited for competition is the whole month of February, 1867. The prize for each of the above consists of 1,200 lire. The author will retain his property in the memoir, but the institute reserves the right of publishing it in its Transactions.

Prizes of the Cagnola foundation.—Subject for the year 1866, proposed 7th August, 1864: "To show the evils and imperfections inherent in the military conscription in the different provinces of Italy, and to indicate means and arrangements adapted to their prevention."

The time for presenting the memoirs is the whole of February, 1866. A premium of 1,500 lire, and a medal of gold, of the value of 500 lire, will be awarded.

Subject for 1867, announced 7th August, 1865.—The opinion has obtained among many cultivators of the silk-worm of the province of Milan, that the rearing of the worms, conducted in such a way that in ordinary seasons the process shall be completed before the end of May, will usually yield good results, and the worst results when completed in June. It is desirable to collect scientific facts which shall serve to evince whether that opinion is confirmed by the demonstration of a difference in the proportion of nitrogenous substances at different stages of development of the leaves of the mulberry. Hence the following inquiry is submitted to competition: "To determine separately the chemical composition, or, at least, the proportion of nitrogenous principles in the leaves of three or four mulberry trees of the same species, cultivated in the same soil, gathered in the first stage of their development, and also after the leaves have reached an advanced state of maturity; and also the proportion of the same principles existing at a given epoch in the leaves of different species of the mulberry generally cultivated in upper Italy, not overlooking the wild variety." Competitors should furnish all the means possible for the verification of their work.

The memoirs must be presented within the month of February, 1867. The prize will consist of 1,000 lire, and a medal of gold of the value of 500 lire, subject to be adjudicated only in part. While the successful memoir will continue to be the property of the author, he should publish it *within a year*, upon consultation with the secretary of the institute as regards size and character of the publication, and fifty copies shall be consigned to that officer, after which only will the money be paid. The institute, as well as the requirements of the founder, reserve the right of publishing, at their own expense, such larger number of copies as may seem desirable in the interests of science.

Prizes of the Secco-Comneno foundation.—Subject for the year 1866, announced August 7, 1863. The importance of utilizing the greatest possible quantity of heat which can be developed by our combustibles, renders it desirable that investigation should be directed to this object, to the benefit of our national industry. We therefore propose a "manual which shall exhibit, in an elementary form, the phenomena and laws constituting the doctrine of the transformation of heat into mechanical labor, and *vice versa*, with application to thermo-dynamic machinery." Time for presenting the memoir, month of February, 1866.

Subject for the year 1867, announced in 1862 and again proposed in 1865: "Among the various forms of active credit, to determine which would be most beneficial and suitable to the actual state of the kingdom of Italy, and which would satisfy at once the three-fold object of disburdening the hypothecated debt, promoting the great meliorations of agriculture, and furnishing relief to the class of simple tenants and cultivators of land." For the solution of this question, the abstract and known theories of authors will not be sufficient; but what is required is, their immediate and practical application to the necessities and interests of the country, with proofs and illustrations, both statistical and economical, to be accompanied by the project of a law for a new funded credit for Italy, in the shape of an appendix or *resumé* of the whole treatise. The time for the presentation of memoirs is limited to the 31st December, 1866; and the prize for either of the above topics is 864 lire. Publication is to be made within a year after the award, when eight copies are to be consigned to the administration of the great hospital of Milan, and one to the institute for collation with the manuscript.

Special Castiglioni prize.—For the prize of 500 lire, offered by the Cavalier Cesar Castiglioni, director of the insane asylum of Senavra, the following subject is proposed: "A memoir upon meteorological studies and observations, conducted with reference to some circumscribed territory in the kingdom of Italy, and preferably in Lombardy; provided the conclusions arrived at be judged to be of real importance and practical utility." The month of April, 1867, is the term of the competition for this prize.

General regulations regarding competition.—Foreigners and natives, with the exception of active members of the institute, may compete for the prizes by memoirs in Italian, Latin, or French. These should be transmitted, free of postage, at the time assigned for each, to the secretary of the institute, at the palace of Brera, in Milan. They must be strictly anonymous, and bear only a motto, which is to be repeated on a sealed note, containing the name and domicile of the author. All the manuscripts will be preserved in the archives of the institute, but the authors of memoirs which obtain no prize will be at liberty to withdraw them within a year from the date of the award, which will take place in the stated meeting of the 7th of August following the close of the period of competition.

A. VERGA, *President.*
G. CURIONI, *Secretary.*

MILAN, *August* 7, 1865.

PROGRAMME OF THE IMPERIAL SOCIETY OF SCIENCE, AGRICULTURE, AND ARTS OF LILLE, 1866.

ANNUAL PRIZES.—The society will award medals of gold, silver gilt, silver, and bronze, to the authors of meritorious memoirs addressed to it upon the subjects here designated. The accepted memoirs may be published by the society, and will form a separate collection, the publication of which will date from the present time.

I. PHYSICAL SCIENCES.

Questions proposed for the competition of 1866.—1. A comparative critical examination of the numerous processes proposed for preventing incrustations in steam boilers. Indication of the most efficacious and economical process for each description of water of supply. 2. A study of the different kinds of coal of the north of France, under the two-fold relation of chemical composition and calorific properties. 3. A comparative study of the photometers hitherto proposed, and an indication of the instrument of this kind which may be regarded as most simple and most exact. 4. An elementary exposition adapted to employment in instruction of the mechanical theory of heat and its applications in machinery. 5. The meat furnished by the shambles is distributed, as is well known, under several categories or qualities, whose price by the kilogram is very different; but no comparative chemical analysis has been made of the different qualities of meat from the same animal. It is desirable to know what are the differences which these qualities present under the relation of immediate composition: Whether, under the alimentary relation, these qualities really offer marked differences in conformity with their market value? Why the inferior pieces, the quantities of flesh being equal, should afford less nutriment than the more choice pieces? Finally, is it possible for chemistry to give precise answers to these questions so interesting to public hygiene? 6. A comparative direct analysis of the principal kinds of cheeses in the state in which they serve for consumption, and a deduction, from the analytical results obtained, of the real value of cheeses in the comparative scale of aliments.

Question proposed for the competition of 1868.—Among the aliments or condiments borrowed from the vegetable kingdom there are a great number whose immediate composition is not known in an exact manner, and of which, consequently, it is difficult to appreciate the true alimentary value. Of this number are the small and the long red radishes *(raphanus sativus ;)* the black or gray radish *(raphanus niger ;)* the horse-radish *(cochlearia armoracia;)* the esculent gallingale *(cyperus esculentus ;)* the edible arum *(caladium esculentum ;)* the ground chestnut *(bunium bulbocastanum ;)* the tuberous vetch *(lathyrus tuberosus ;)* the root of rampion *(campanula rapunculus ;)* the bulbs of garlic *(allium sativum,)* of shallot *(allium ascalonicum,)* of onion *(allium cepa ;)* bulbs and leaves of the leek *(allium ponum,)* of the orchis *(orchis morio, mascula, &c.;)* leaves of lettuce *(lactuca sativa,)* of scorzonera *(scorzonera hispanica,)* of wild chicory *(cicorium intybus,)* of endive *(cicorium endivia,)* of dandelion *(taraxacum dens-leonis,)* of water-cress *(nasturtium officinale,)* of common garden cress *(lepidium sativum,)* of scallion *(allium fistulosum et* *schœnoprasum,)* of different cabbages *(brassica oleracea,)* of parsley *(petroselinum sativum,)* of chervil *(anthriscus cerefolium,)* of pimpernel *(poterium sanguisorba,)* of tarragon *(artemisia dracunculus,)* of sorrel *(rumex acetosa,)* of spinach *(spinacia oleracea,)* of herbaceous glasswort *(salicornia herbacea,)* of the white beet *(beta cicla,)* of purslane *(portulaca oleracea,)* of corn salad *(valeriana olitoria,)* of rampion *(campanula rapunculus ;)* the stalks of celery *(apium graveolens,* sweet variety,) of turnip celery, (a variety of the preceding,) of angelica *(angelica archangelica,)* of rhubarb *(rheum ribes,)* of cardoons *(cynara carduncului ;)* the flower tops of sarietta *(satureia hortensis ;)* the receptacles or bottoms of artichokes *(cynara scolymus ;)* the young shoots of asparagus *(asparagus officinalis,)* of the hop *(humulus lupulus ;)* the green pods of peas *(pisum sativum,)* of beans *(phaseolus vulgarus ;)* cucumbers *(cucumis sativus,)* gherkins (a variety of the preceding ;) the fruit of the egg-plant *(solanum melongena,)* of the tomato *(lycopersicum esculentum;)* the fig *(ficus carica ;)* the date *(phœnix dactylifera ;)* the carob *(ceratonia siliqua ;)* common and French chestnut *(castanea vesca;)* the sweet acorn *(quercus ballota ;)* water chesnut, or caltrop, *(trata natans.)*

It would be interesting to determine, in these different edible substances, the relative proportions of water, of organic matters, nitrogenous or otherwise, of succulent matter, of salts, (particularly phosphates and alkalies,) of the total nitrogen.

II. MEDICAL SCIENCES AND PHYSIOLOGY.

Questions proposed for the competition of 1866.—1. To determine, according to the present state of science, the chemical and mechanical influences which gases absorbed by the intestinal and pulmonary mucous surfaces exert on the circulatory current. To inquire what affections and effects are produced on the animal economy by the passage of the principal gaseous substances in the sanguineous system.

The Society of Sciences, while leaving full liberty to competitors in arriving at the solution of this important question, desires that the works of Nysten, of Vidal, of MM. Andral and Gavarret, &c., should be consulted, and that efforts should be made to ascend to the etiology of certain affections, the origin and nature of which are still unknown. 2. To inquire into the disturbance introduced into the functions of nutrition and relation by the use of tobacco; to determine, by recourse to numerous observations, what mode of smoking is most injurious to health. 3. The physiological and therapeutic action of quinine is known : to study and show by experiments the physiological effects of the other principles contained in the quinquinas. 4. To perform the same study as regards tobacco.

Question proposed for the competition of 1867.—The mode in which eels are reproduced is entirely unknown to naturalists ; it is not known what organs are productive of the elements which serve for generation, and we are ignorant whether eels produce eggs or young eels. Several kinds or varieties of eels are known, and some naturalists have thought that these different forms might be only sexual. It is proposed that these problems, important as regards physiology and pisciculture, should be examined and resolved. 2. To study the cadaveric phenomena which precede the period of putrefaction, to the effect of determining by positive researches at what epoch rigidity appears and ceases both in the adult and new-born infant. To draw from this study applications for the use of legal medicine.

III. SCIENCES APPLIED TO INDUSTRY.

Questions proposed for the competition of 1866.—1. To indicate an industrial means for the direct preparation of oxalic acid in aid of the mangel-wurzel in nature. 2. To compose a technological history of flax, and show the importance of its culture and employment in the north of France and in Belgium. 3. To prepare a practical *guide for the construction and employment of steam generators,* recapitulating, as briefly as possible and in simple and unscientific language, the rules and numerical data furnished by the most certain and recent researches and experiments with reference to the construction of boilers, furnaces, chimneys, and in regard to the management of the fire. 4. To indicate a simple process, industrially practical and economic, for rendering saponifiable the oily matter extracted from the washings of wool. To consider the subject also in its commercial application.

IV. AGRICULTURE.

Questions proposed for the competition of 1866.—1. To make a comparative analysis of all or part of the kinds of calcareous substances which are made use of in the north of France, whether for the liming or marling of land. To indicate the repositories and physical characters of these substances. 2. To show the different modes of liming and marling practiced in the north of France, specifying for each several soil the quantities of lime or marl adopted in different localities, as well as the duration of the liming or marling; also, the net cost of these two operations in each locality. 3. To give the statistics, with proofs and illustrations, of the agricultural state of the arrondissement of Lille, from 1850 to 1864.

V. SOCIAL AND STATISTICAL ECONOMY.

Questions proposed for the competition of 1866.—1. A view of the societies for mutual succor among workingmen (called *Societies of the Sick*) which existed at Lille previous to 1789, their organization, and results. 2. To determine, by means of administrative acts, public documents or incontestable private records, the variations which the price of a day's work has undergone within a century at Lille and in the arrondissement; comparing therewith the price of the hectolitre of wheat as well as other objects of prime necessity during the same period, drawn from similar sources of information. 3. An historical account of one of the chief industries of the department of the north, (manufacture of sugar, of potash of the beet, of soap, distilling, rotting of flax, spinning and weaving, &c.,) stating the different phases of its development, and indicating its probable career in the future. The present state of the industry selected for consideration should be established by statistics, whose elements, derived from official sources, shall be susceptible of verification.

VI. LEGISLATION.

Questions proposed for the competition of 1866.—1. On the legislation of *prebends* before and since the revolutionary period; the advantages and inconveniences of these sorts of foundations. 2. Researches respecting the legislation for annoying or unhealthful establishments in the city of Lille previous to the decree of 1810.

VII. HISTORY.

Questions for the competition of 1866.—1. To indicate the physical topography of maritime Flanders from the Roman conquest, embracing a discussion, under a critically scientific point of view and based upon geological, geographical, and archæological documents, of the different opinions heretofore maintained upon this subject; also, an inquiry whether there exist in the department remains of human industry which may be referred to the age of stone. 2. History of some rural commune of the department of the north. 3. History of the judicial organization of the different provinces which now constitute the department of the north from the invasion of the barbarians to 1789. 4. Notice on the life and writings of Jacques Meyer, author of the Annals of Flanders. 5. History of the charitable and hospital establishments of the arrondissement of Lille situated outside of the ancient city. 6. Biographical study on the botanist, Desmazières. 7. Biographical study on the naturalist, Macquart.

VIII. LITERATURE AND POETRY.

Each year there will be opened a competition in poetry, and medals will be awarded to the authors of the best pieces of verse; the subject will be left to the choice of competitors. The first gold medal for the most distinguished production in the two lines of literature and poetry will be replaced by an object of art. *Questions for competition in* 1866.—1. History of literature in the provinces which now form the department of the north from its incorporation by France (1667) to our own time. 2. A dramatic scene comprising personages and choruses proper to be set to music. 3. Eulogium upon one of the benefactors of the poor at Lille (the Countess Jeanne, Gantois, Masurel, Stappart, &c.)

IX. FINE ARTS.

Questions for competition in 1866.—1. The project of a monument to be erected on one of the new public spaces of Lille, and which might serve on occasion for expositions of art or industry, for public solemnities, such, for instance, as the distribution of prizes, for concerts, or even for balls. 2. A design for a statue to be erected to one of the benefactors of the poor at Lille, (Countess Jeanne, Gantois, Masurel, Stappart, &c.) The model should be of plaster, and one-fourth of the intended size. 3. History of the arts of design at Lille from the foundation of the city to the nineteenth century, inclusive. By arts of design are to be understood painting, sculpture, engraving, architecture, as well as the industrial arts in their relations to the former. 4. A study of the life and works of Arnould de Vuer. 5. A study, principally with a view to exterior decoration, of the architectural conditions of edifices built of ordinary bricks or of bricks and stones. An examination of the special difficulties which attend ornamentation when ordinary bricks are exclusively employed, together with an indication of the most suitable arrangements. 6. A medal will be awarded to the author of a remarkable musical composition, such as a symphony, overture, chorus, with or without accompaniment. For a composition for singing without accompaniment or with the accompaniment of the piano, the medal may, at the choice of the candidate, be replaced by publication at the cost of the society. 7. *Photography:* The indication of a mode of preparation furnishing a collodion, comprising in itself the photogenic elements, so as to dispense with the operations which are necessary to give sensibility to the common collodion. The collodion must be sufficiently sensitive for obtaining portraits or animated landscapes.

X. VARIOUS ENCOURAGEMENTS.

The society reserves to itself the compensation and encouragement, by premiums and medals, of the authors of productions or labors, whether scientific, literary, artistic, agricultural, or industrial, not mentioned in the present programme. It may even recompense the importation into the arrondissement of Lille of a new industry or of new industrial processes, and, in general, every kind of work capable of exerting a happy influence on the situation of the country.

XI. RECOMPENSES TO INDUSTRIAL AGENTS.

Since 1831 the society recompenses, by checks on the Savings Bank, premiums and medals, the fidelity and attachment of servants to their masters; it will each year award similar distinctions to the old servants of industry. The certificates delivered in favor of industrial agents must be recognized and certified as true by the patrons.

General conditions of competition.—Each year the memoirs and other labors will be addressed, free of charge, to the secretary general of the society at the *Hôtel de Ville*, before the 15th of October. Every remittance will bear an epigraph, reproduced in form of address upon a sealed note, which contains the name and denotes the domicile of the author, together with an attestation signed by him, certifying that which sent has not been made public nor presented before for competition. This note will not be opened unless the candidate shall have merited a recompense. Every manuscript, design, plan, or model offered in competition remains the property of the society, which may authorize the author to take a copy at his own expense; the preceding disposition, however, is not applicable to objects of art. The certificates given in favor of workmen and industrial agents who prefer a claim to the medals and premiums offered for good and long service must be addressed before the 15th of October to the secretary general.

COMPTE DE MELUN,
President.
P. GUIRAUDET,
Secretary General.

WICAR PRIZE, INSTITUTED BY THE IMPERIAL SOCIETY OF SCIENCES, AGRICULTURE, AND ARTS OF LILLE.

An annual prize is founded by a decree of the society, which shall bear the name of the Wicar prize, and, in the present state of the resources, shall consist of 1,000 francs. This prize will be annually awarded, in succession, to different branches of study, which, with that view, will be divided into three sections: Section of literature and of the fine arts, comprising literature, poetry, architecture, painting, sculpture, &c.; section of sciences, physics, chemistry, mechanics, industrial sciences, &c.; section of historical, moral, and economical sciences. In the event that a prize assigned to one section be not awarded the first year, competition will remain open for following years, until the prize shall be awarded or triennial rotation restore it to the same section. As in the latter case the society must open anew a competition in the same section, the sum appropriated to the new prize will be added to that of the prize which has remained unemployed, when there may be two prizes proposed or a single one of double value.

Competition for 1866.—*Section of sciences.*—Geology: to show the distribution of fossil vegetables in the coal basin of the north of France, and to indicate the conclusions which may be drawn from this distribution in regard to the geological constitution of the basin and its mode of formation. It should be ascertained whether special floras, analogous to those which M. Geinitz has recognized in Saxony, can be distinguished in the different beds of our coal basin. Such a discovery would be of much importance, since it would suffice to collect a certain number of vegetables in a bed of coal in order immediately to know the place which this bed occupies in the coal formation. It would enable us also to ascertain whether our coal basin is complete, or is only a remnant, of which the portions elevated by a cataclysm are to be sought elsewhere. It should be inquired, at the same time, whether the nature of the vegetables constituting these different floras be such that we can recognize them as having lived under different conditions, and an attempt should be made to determine these conditions, as has been done by M. Ludwig for the tertiary combustibles of the banks of the Rhine. It would be well also to consider the influence of the different floras on the composition of the coal.

Competition for 1867.—*Section of historical, moral, and economical sciences.*—History.—The prize will be awarded to the best monograph of an establishment, whether civil or ecclesiastical, such as an abbey, chapter, or city of the department of the north. The proposed work should have as its basis authentic unpublished documents, literally reported in the form of a cartulary or body of proofs. It should be followed by an index containing the names of places and persons.

Section of literature and the fine arts.—Painting.—On account of the exhibition of painting which is to take place exceptionally at Lille during the present year, the society has decided that the competition for 1868 pertaining to the section last named shall be a competition in painting, and be assigned by anticipation to 1866. Consequently the prize which would have been awarded in 1868 will be conferred at the close of the exhibition of the present year on the author of the painting which shall be judged most worthy by a jury taken from the society or designated by it,

Architecture—Designs for habitations.*—Of these, three kinds are proposed: 1. A hotel of the first class. 2. A private habitation or domicile for a family. 3. A house for rent by apartments. Conformity with the following conditions is required: 1. Models or well-executed outlines (plans and elevations) of the three designated kinds of buildings. The principal façade of the hotel should be rendered with the greatest care; a particular study of any important detail should be added. 2. Sketches, plans, and elevations, expressing in a clear and exact manner a system of arrangement of the three types of habitations contemplated. It will be allowable, therefore, to unite or to separate them in such arrangement as may be chosen, to divide the plats comprised between the public ways by new walks or free spaces, &c.

Hotel of the first class.—The approximate expense, (not including painting, glazing, and furnishing,) 300,000 francs; ground-plat, 2,500 square metres; front on the street, 40 metres. It has not been thought expedient to give a designation of the apartments; it is not proposed to insist on any disposition, form, or dimension, except the length of front on the street, with a limitation of depth. Even the cost is not fixed in a rigorous manner, although it is indicated in order to engage competitors to take into account the merit of relative economy. The liberty allowed should not be deemed, however, an abandonment of the fundamental principles of architectural art. The society, on the contrary, recommends the strictest practical observance of it; but it will also regard with favor new ideas and forms, in so far as they shall correspond to the well-considered requirements of a habitation, at once rich, comfortable, and of superior taste.

Private dwelling.—By this title must be understood a house suitable for a numerous family, the head of which might be engaged in a liberal profession, and have extensive relations both in business and society. The length of the front on the street may be from 10 to 13 metres, the depth not being prescribed; the expense may be fixed approximately at 300 francs per square metre of the surface covered, (ground-floor and two stories.) As in the preceding case, an indication of the general intention only is submitted, leaving to each competitor the entire merit of originality in his ideas. The modern dwelling should comprise, without great expenditure, much tastefulness and a comfort but too little sought after by architects. Art should ally itself with science to give the stamp of distinction to our homes, without forgetting, however, that a modest

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* The prize which was offered for 1865 having not then been awarded, will be conferred in 1866, if deserved; if not, competition for it will be continued till 1867, but not later.

reserve in ornamentation should be considered a necessary economy, and at the same time a proof of taste.

House for renting by apartments with shops on the ground-floor.—This species of habitation, so generally adopted at Paris, is still but little in use at Lille, where the inhabitants prefer houses reserved for a single family; this is a defect, however; strangers do not find means of lodging conveniently; the system of groups of rooms in one house would satisfy a real necessity. Here, it will be seen, the question of expense is predominant, and it is necessary to renounce the advantages of the habitation entirely private, with a view to the admission of partial communism. The highest rate of renting should not exceed 2,500 francs.

The society would especially call the attention of competitors to the difficult problem of the establishment of lodgings at a reduced price for the working class. Thinking it useful, in the interest of society, not too far to separate from one another the different classes of the population, that object will be understood to form an essential part of the present programme. The difficulty, therefore, of lodging different classes of society under the same roof must not be evaded, but in proposing a special solution for lodges of workmen arranging themselves in the general plan required. In this lodge the rent should not exceed a mean of seventy-five francs by the apartment and year. The conditions of hygiene, of cleanliness, of morality, and, as far as possible, of commodiousness, must be met by means of an expenditure proportioned to the revenue. Whatever combination be adopted, the price of the ground, even in the centre of a square, cannot be expected to fall below fifteen francs per square metre.

The length of the street front is fixed at twenty-four metres, the depth of the ground space being undetermined—that is to say, it is left to the discretion of the architect whether one or several blocks of buildings be proposed. The number and extent of the apartments is not fixed, depending, as they must do, upon conditions which cannot be prescribed with exactness, without being prejudicial to the conception of the types which the society wishes to obtain. It may be added that the conveniences sought to be realized in the present case must involve no neglect of the prescriptions of hygiene. The provisions relative to sewerage are the same at Lille as at Paris.

General conditions for the competition in architecture.—To encourage the extensive and complex science which is applied to the art of building habitations corresponding to all the present wants of society, and at the same time to elevate the public taste by the view of better types of modern civil and domestic architecture, such is the special object contemplated in the proposed competition; competitors are therefore apprised that the society will accord the same value to the qualities of economy, convenience, and health as to the artistic merit of the architectural form. It will not consider its intentions well fulfilled except by the simultaneous application of science and of art. To competitors the initiative of ideas is left both as to substance and form, as well as the mode of their realization; yet, without excluding the employment of materials transported at much expense, it would seem judicious to prefer materials drawn from the country or of no remote origin: for the walls, bricks, red or glazed; for the basement, the sandstone of Soignies, Belgium; for the roof, slate, violet or green. The designs for the whole should be given on a scale of 0.0025; plans and sections on one of 0.025; façades 0.05; a detail of the façade should be represented of the size to be executed. Independently of the required indications, competitors will be at liberty to send all drawings and notes explanatory or descriptive which they deem necessary.

The Society of Sciences will appoint a jury of adjudication, of which a majority shall be architects, and the greatest publicity will be given to the result of the competition. An exhibition will precede the reading, in public session, of the report, and after the judgment thus rendered a second exhibition will complete the guarantee of impartiality offered to the competitors; the names of the latter may, at their own request, be affixed to their respective plans during this second exhibition, which will continue but for twenty days, during which no piece can be withdrawn.

The general conditions for the Wicar prize [are substantially the same with those previously given.] Each memoir transmitted remains the property of the society, with liberty to the authors to have copies made at their own expense, but this does not apply to paintings, designs, plans, and models intended for competition in the fine arts; in that for architecture the work to which the prize has been adjudged will be the property of the society, and may be published by the latter. For all further information recourse should be had to the secretary-general of the society, P. Guiraudet.

DUNKIRK SOCIETY (SOCIÉTÉ DUNKERQUOISE) FOR THE ENCOURAGEMENT OF SCIENCES, LETTERS, AND ARTS.—PROGRAMME OF SUBJECTS FOR COMPETITION—1866.

In the regular meeting of 1866 the society will award, if occasion be afforded, a gold medal for the best memoir on each of the following subjects:

SCIENCES.

I. *Study upon naval constructions.*—To treat of the history of naval constructions, and of the progress successively made therein at Dunkirk; to examine particularly the influence of the modes of building on the sailing of vessels propelled by wind.

II. *Study on the fauna of maritime Flanders.*—The author may, at his choice, treat of the ornithological fauna, or of the entomological fauna, or of the conchological description; the parts not treated of will remain for competition in following years.

LETTERS.

III. *A history of Dunkirk for the use of the young.*—A sum of one hundred francs has, in this case, been added as a donation to the medal of the society by an anonymous contributor. The object proposed is not a long and elaborate work, but a series of detached lectures, wherein the most important facts of the history of Dunkirk may be appropriately placed in relief. In the opinion of the society, such a work should not, at its greatest extent, exceed 250 pages 12mo.

IV. An unpublished memoir on a subject relative to the history or archæology of maritime Flanders.

ARTS.

V. *Architecture.*—Design of a monument commemorative of the battle of Dunes, (1658,) to be erected on the site of that battle.

COMPETITION FOR 1867.

Sciences.—A succinct history of cotton manufactures in the north of France, from the origin of that branch of industry to the present day, under the twofold relation of the labor in cotton and its hygiene; with an appreciation of the progress achieved and an indication of the ameliorations desirable, especially in a hygienic point of view. The sojourn in the workshops, through the absorption of the dust, having been found to generate certain affections of the respiratory organs, the proposed treatise should be adapted to serve as a sort of manual for the use of manufacturers and workmen.

The society proposes to offer successively for competition analogous investigations respecting hemp, wool, and other substances employed in industry; to be followed by memoirs on weaving and its incidents, on the manufactures which interest at once the industry and agriculture of our country, such as sugar, oils, alcohols, beer, &c. Persons who may have prepared works on these topics are requested to address them to the society, which, in case of merit, will award recompenses.

The answers will be directed *free* to the perpetual secretary of the Dunkirk Society before the 1st of July of the year of competition. They must not be signed, but will bear an epigraph or motto, repeated in a sealed note communicating the name, profession and residence of the author, who will certify that *his memoir is unpublished and has never been offered in competition.* This note will not be opened unless the work should merit a prize or honorable mention; otherwise it will be burned. Authors who make themselves known in advance, by whatever means, will be excluded from competing. The works sent become the property of the society, though authors may have a copy taken at their own expense. The candidate who, having been successful at one of the five preceding awards, shall obtain the first rank, will be entitled only to a commemorative notice of the medal. In this case honorable mention, inscribed on a silver medal, may be accorded to the memoir rated as second in point of merit. The author who, for one of the subjects proposed for competition, may obtain several recompenses, will be entitled only to the higher medal.

The society reserves the right of awarding medals to persons who shall have sent presents or memoirs which, although not invited by the programme, shall appear to merit distinction. For all further information reference may be had to the perpetual secretary of the society.

TERQUEM, *President.*
VOR. DERODE, *Perpetual Secretary.*

DUNKIRK, *November* 10, 1865.

THE METRIC SYSTEM OF WEIGHTS AND MEASURES,

WITH TABLES INTENDED ESPECIALLY FOR THE USE OF TEACHERS AND AUTHORS OF ARITHMETICS.

PREPARED BY PROF. H. A. NEWTON, OF YALE COLLEGE.

While this part of the appendix to the Annual Report of the Smithsonian Institution was passing through the press the following resolutions, pertaining to the French system of weights and measures, were adopted by both houses:

AN ACT to authorize the use of the metric system of weights and measures.

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That from and after the passage of this act, it shall be lawful throughout the United States of America to employ the weights and measures of the metric system; and no contract, or dealing, or pleading in any court, shall be deemed invalid, or liable to objection, because the weights or measures expressed or referred to therein are weights or measures of the metric system.

SEC. 2. *And be it further enacted,* That the tables in the schedule hereto annexed shall be recognized, in the construction of contracts, and in all legal proceedings, as establishing, in terms of the weights and measures now in use in the United States, the equivalents of the weights and measures expressed therein in terms of the metric system; and said tables may be lawfully used for computing, determining, and expressing in customary weights and measures the weights and measures of the metric system.

AN ACT to enable the Secretary of the Treasury to furnish to each State one set of the standard weights and measures of the metric system.

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 furnish to each State, to be delivered to the governor thereof, one set of the standard weights and measures of the metric system, for the use of the States respectively.

AN ACT to authorize the use in post offices of weights of the denomination of grams.

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That the Postmaster General be, and he is hereby, authorized and directed to furnish to the post offices exchanging mails with foreign countries, and to such other offices as he shall think expedient, postal balances denominated in grams, of the metric system, and until otherwise provided by law, one-half ounce avoirdupois shall be deemed and taken for postal purposes as the equivalent of fifteen grams of the metric weights, and so adopted in progression; and the rates of postage shall be applied accordingly.

The metric system is so called from the metre, which is its principal and only arbitrary unit. It is in use, to the exclusion of other weights and measures, in several countries of Europe, and is in partial use in almost all the nations of christendom.

UNITS OF THE SYSTEM.

The metre is a measure of length. It is intended to be, and is very nearly, one ten-millionth part of the distance from the equator to the pole, measured on the earth's surface. It is 39.37 inches, very nearly. Five metres are a little less than a rod.

The arc is a measure of surface, and is equal to a square whose side is 10 metres. It contains 100 square metres, or a little less than four square rods.

The litre is the unit of dry measure, and also of liquid measure. It is equal to the volume of a cube whose edge is one tenth of a metre. A cubic metre, therefore, contains 1,000 litres. The litre is a little more than a wine quart, being equal to about $1\frac{1}{18}$ quart.

The gram is the unit of weight, and is the weight of a cube of water, each edge of the cube being $\frac{1}{100}$ of a metre. A litre of water weighs, therefore, 1,000 grams, and a cubic metre of water weighs 1,000,000 grams. A gram is 15.432 + grains.

The stere is a cubic metre, and is about 1.308 cubic yards.

Each of these units is divided decimally, and also larger units are formed from multiples by 10, 100, 1,000, &c. The successive multiples are designated by the prefixes *deka, hecto, kilo* and *myria;* the successive parts by *deci, centi,* and *milli.*

The following schedules of equivalents of the several units of the system represent their values in denominations in use. The numbers are not carried to the highest degree of accuracy, but the amount of the error in them is generally (except in some of the smaller denominations) less than the change due to a difference of two or three degrees of temperature of the standard metre bar.

Measures of length.

METRIC DENOMINATIONS AND VALUES.		EQUIVALENTS IN DENOMINATIONS IN USE.
Myriametre	10,000 metres	6.2137 miles.
Kilometre	1,000 metres	0.62137 mile, or 3,280 feet and 10 inches.
Hectometre	100 metres	328 feet and one inch.
Decametre	10 metres	393.7 inches.
Metre	1 metre	39.37 inches.
Decimetre	$\frac{1}{10}$th of a metre	3.937 inches.
Centimetre	$\frac{1}{100}$th of a metre	0.3937 inch.
Millimetre	$\frac{1}{1000}$th of a metre	0.0394 inch.

Measures of surface.

METRIC DENOMINATIONS AND VALUES.		EQUIVALENTS IN DENOMINATIONS IN USE.
Hectare	10,000 square metres	2.471 acres.
Are	100 square metres	119.6 square yards.
Centare	1 square metre	1,550 square inches.

Measures of capacity.

METRIC DENOMINATIONS AND VALUES.			EQUIVALENTS IN DENOMINATIONS IN USE.	
Names.	No. of litres.	Cubic measure.	Dry measure.	Liquid or wine measure.
Kilolitre or stere	1000	1 cubic metre	1.308 cubic yard	264.17 gallons.
Hectolitre	100	$\frac{1}{10}$ of a cubic metre	2 bus. and 3.35 pecks.	26.417 gallons.
Decalitre	10	10 cubic decimetres	9.08 quarts	2.6417 gallons.
Litre	1	1 cubic decimetre	0.908 quart	1.0567 quart.
Decilitre	$\frac{1}{10}$	$\frac{1}{10}$ of a cubic decimetre	6.1022 cubic inches	0.845 gill.
Centilitre	$\frac{1}{100}$	10 cubic centimetres	0.6102 cubic inch	0.338 fluid ounce.
Millilitre	$\frac{1}{1000}$	1 cubic centimetre	0.061 cubic inch	0.27 fluid drachm.

Weights.

METRIC DENOMINATIONS AND VALUES.			EQUIVALENTS IN DENOMINATIONS IN USE.
Names.	Number of grams.	Weight of what quantity of water at maximum density.	Avoirdupois weight.
Millier or tonneau	1000000	1 cubic metre	2204.6 pounds.
Quintal	100000	1 hectolitre	220.46 pounds.
Myriagram	10000	10 litres	22.046 pounds.
Kilogram, or kilo	1000	1 litre	2.2046 pounds.
Hectogram	100	1 decilitre	3.5274 ounces
Decagram	10	10 cubic centimetres	0.3527 ounce.
Gram	1	1 cubic centimetre	15.432 grains.
Decigram	$\frac{1}{10}$	$\frac{1}{10}$ of a cubic centimetre	0.5432 grain.
Centigram	$\frac{1}{100}$	10 cubic millimetres	0.1543 grain.
Milligram	$\frac{1}{1000}$	1 cubic millimetre	0.0154 grain.

For convenience in converting metric weights and measures into denominations in use, and *vice versa*, the following tables have been prepared. For the sake of uniformity in them, the metre is regarded as 39.37 inches, and the kilogram as 2.2046 avoirdupois pounds. The contents of the bushel and gallon are regarded, severally, as 2,150.42 and 231 cubic inches. The tables are computed from these several numbers and from the commonly recognized relations of weights and measures.

The use of the tables may be seen by a few examples:

Example 1—to find the number of miles in 74 kilometres: In Table I, in the line for 70 and in the column under 4 we find 45.981, which is the number of miles required.

Example 2—to find the number of miles in 63,000 kilometres: In the same table, in the line for 60 and in the column under 3 is found 39.146. Removing the decimal point three places to the right for the three ciphers in 63,000, we have the answer—39,146 miles.

Example 3—to find the number of miles in 63,740 kilometres:

For 63,000 kilometres we have.................... 39,146 miles.
For 740 kilometres we have.................... 459.81 "

Hence, for 63,740 kilometres we have.................... 39,605.81 "

which is the number of miles required.

Example 4—to find the number of miles in 2,746 meters—that is, in 2.746 kilometres: In 27 kilometres by the table are 16.777 miles, and in 46 kilometres by the table are 28.583 miles. Hence, in 2.7 kilometres are 1.6777 miles,
and in .046 kilometres are .028583 "

and therefore, in 2.746 kilometres are 1.706283 "

Or about 1.7063 miles, which is the number required.

Scheme of the metric system.

Ratios.	Lengths.	Surfaces.	Volumes.	Weights.
1000000	. . . .	. . . .	. . . . . .	Millier, or Tonneau.
100000	. . . .	. . . .	. . . . . .	Quintal.
10000	Myriametre.	. . . .	Dekastere.	Myriagram.
1000	Kilometre.	. . . .	Kilolitra, or Stere.	Kilogram, 2.2046 pounds av.
100	Hectometre.	Hectare. (2.471 acres.)	Hectolitre.	Hectogram.
10	Dekametre.	. . . .	Dekalitre.	Deckagram.
1	METRE, (39.37 inches.)	ARE.	LITRE.	GRAM, (15.4322 grains.)
$\frac{1}{10}$	Decimetre.	. . . .	Decilitre.	Decigram.
$\frac{1}{100}$	Centimetre.	Centare.	Centilitre.	Centigram.
	Millimetre.	. . . .	Millilitre.	Milligram.

The following scales exhibit the relative magnitude of the divisions of the metre and inches. The upper one represents a six-inch rule, divided to eighths, and the lower one represents 15 centimetres, or 150 millimetres. One inch is a little more than 25 (more exactly 25.4) milimetres.

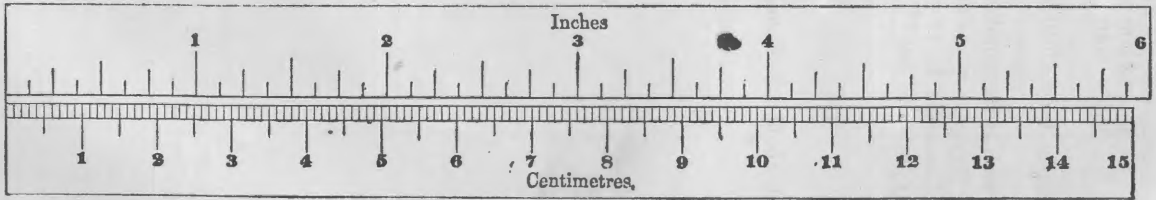

TABLE I.—*For converting kilometres into miles.*

Kilometres.	**0.**	**1.**	**2.**	**3.**	**4.**	**5.**	**6.**	**7.**	**8.**	**9.**
0	0	.621	1.243	1.864	2.485	3.107	3.728	4.350	4.971	5.592
10	6.214	6.835	7.456	8.078	8.699	9.321	9.942	10.563	11.185	11.806
20	12.427	13.049	13.670	14.292	14.913	15.534	16.156	16.777	17.398	18.020
30	18.641	19.262	19.884	20.505	21.127	21.748	22.369	22.991	23.612	24.233
40	24.855	25.476	26.098	26.719	27.340	27.962	28.583	29.204	29.826	30.447
50	31.068	31.690	32.311	32.933	33.554	34.175	34.797	35.418	36.039	36.661
60	37.282	37.904	38.525	39.146	39.768	40.389	41.010	41.632	42.253	42.875
70	43.496	44.117	44.739	45.360	45.981	46.603	47.224	47.845	48.467	49.088
80	49.710	50.331	50.952	51.574	52.195	52.816	53.438	54.059	54.681	55.302
90	55.923	56.545	57.166	57.787	58.409	59.030	59.652	60.273	60.894	61.516

TABLE II.—*For converting miles (of 5,280 feet) into metres.*

(To convert miles into kilometres use this table and divide the result by 1,000.)

Miles.	**0.**	**1.**	**2.**	**3.**	**4.**	**5.**	**6.**	**7.**	**8.**	**9.**
0	00	1609	3219	4828	6437	8047	9656	11265	12875	14484
10	16093	17703	19312	20922	22531	24140	25750	27359	28968	30578
20	32187	33796	35406	37015	38624	40234	41843	43452	45062	46671
30	48280	49890	51499	53108	54718	56327	57936	59546	61155	62765
40	64374	65983	67593	69202	70811	72421	74030	75639	77249	78858
50	80467	82077	83686	85295	86905	88514	90123	91733	93342	94951
60	96561	98170	99780	101389	102998	104608	106217	107826	109436	111045
70	112654	114264	115873	117482	119092	120701	122310	123920	125529	127138
80	128748	130357	131966	133576	135185	136795	138404	140013	141623	143232
90	144841	146451	148060	149669	151279	152888	154497	156107	157716	159325

TABLE III.—*For converting kilometres into feet.*

(To convert metres into feet use this table and divide the result by 1,000.)

Kilometres.	**0.**	**1.**	**2.**	**3.**	**4.**	**5.**	**6.**	**7.**	**8.**	**9.**
0	0	3281	6562	9842	13123	16404	19685	22966	26247	29527
10	32808	36089	39370	42651	45932	49212	52493	55774	59055	62336
20	65617	68897	72178	75459	78740	82021	85302	88582	91863	95144
30	98425	101706	104987	108267	111548	114829	118110	121391	124672	127952
40	131233	134514	137795	141076	144357	147637	150918	154199	157480	160761
50	164042	167322	170603	173884	177165	180446	183727	187007	190288	193569
60	196850	200131	203412	206692	209973	213254	216535	219816	223097	226377
70	229658	232939	236220	239501	242782	246062	249343	252624	255905	259186
80	262467	265747	269028	272309	275590	278871	282152	285432	288713	291994
90	295275	298556	301837	305117	308398	311679	314960	318241	321522	324802

TABLE IV.—*For converting feet into millimetres.*

(Divide the result by 1,000 to obtain metres.)

Feet.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	305	610	914	1219	1524	1829	2134	2438	2743
10	3048	3353	3658	3962	4267	4572	4877	5182	5486	5791
20	6096	6401	6706	7010	7315	7620	7925	8230	8534	8839
30	9144	9449	9754	10058	10363	10668	10973	11278	11582	11887
40	12192	12497	12802	13106	13411	13716	14021	14326	14630	14935
50	15240	15545	15850	16154	16459	16764	17069	17374	17678	17983
60	18288	18593	18898	19202	19507	19812	20117	20422	20726	21031
70	21336	21641	21946	22250	22555	22860	23165	23470	23774	24079
80	24384	24689	24994	25298	25603	25908	26213	26518	26822	27127
90	27432	27737	28042	28346	28651	28956	29261	29566	29870	30175

TABLE V.—*For converting metres into feet and inches.*

Metres.	0.		0.1.		0.2.		0.3.		0.4.		0.5.		0.6.		0.7.		0.8.		0.9.	
	Ft.	*In.*	*Ft.*	*In.*	*Ft.*	*In.*	*Ft.*	*In.*	*Ft.*	*In.*	*Ft.*	*In.*	*Ft.*	*In.*	*Ft.*	*In.*	*Ft.*	*In.*	*Ft.*	*In.*
0	0	0	0	3.9	0	7.9	0	11.8	1	3.7	1	7.7	1	11.6	2	3.6	2	7.5	2	11.4
1	3	3.4	3	7.3	3	11.2	4	3.2	4	7.1	4	11.1	5	3.0	5	6.9	5	10.9	6	2.8
2	6	6.7	6	10.7	7	2.6	7	6.6	7	10.5	8	2.4	8	6.4	8	10.3	9	2.2	9	6.2
3	9	9.1	10	2.0	10	6.0	10	9.9	11	1.9	11	5.8	11	9.7	12	1.7	12	5.6	12	9.5
4	13	1.5	13	5.4	13	9.4	14	1.3	14	5.2	14	9.2	15	1.1	15	5.0	15	9.0	16	0.9
5	16	4.8	16	8.8	17	0.7	17	4.7	17	8.6	18	0.5	18	4.5	18	9.4	19	0.3	19	4.3
6	19	8.2	20	0.2	20	4.1	20	8.0	21	0.0	21	3.9	21	7.8	21	11.8	22	3.7	22	7.7
7	22	11.6	23	3.5	23	7.5	23	11.4	24	3.3	24	7.3	24	11.2	25	3.1	25	7.1	25	11.0
8	26	3.0	26	6.9	26	10.8	27	2.8	27	6.7	27	10.6	28	2.6	28	6.5	28	10.5	29	2.4
9	29	6.3	29	10.3	30	2.2	30	6.1	30	10.1	31	2.0	31	6.0	31	9.9	32	1.8	32	5.8

TABLE VI.—*For converting inches and fractions into millimetres.*

Inches.	0	$\frac{1}{12}$	$\frac{1}{8}$	$\frac{1}{6}$	$\frac{1}{4}$	$\frac{1}{3}$	$\frac{3}{8}$	$\frac{5}{12}$	$\frac{1}{2}$	$\frac{7}{12}$	$\frac{5}{8}$	$\frac{2}{3}$	$\frac{3}{4}$	$\frac{5}{6}$	$\frac{7}{8}$	$\frac{11}{12}$
0	0	2.1	3.2	4.2	6.4	8.5	9.5	10.6	12.7	14.8	15.9	16.9	19.1	21.2	22.2	23.3
1	25.4	27.5	28.6	29.6	31.8	33.9	34.9	36.0	38.1	40.2	41.3	42.3	44.5	46.6	47.6	48.7
2	50.8	52.9	54.0	55.0	57.3	59.3	60.3	61.4	63.5	65.6	66.7	67.7	69.9	72.0	73.0	74.1
3	76.2	78.3	79.4	80.4	82.6	84.7	85.7	86.8	88.9	91.0	92.1	93.1	95.3	97.4	98.4	99.5
4	101.6	103.7	104.8	105.8	108.0	110.1	111.1	112.2	114.3	116.4	117.5	118.5	120.7	122.8	123.8	124.9
5	127.0	129.1	130.2	131.2	133.4	135.5	136.5	137.6	139.7	141.8	142.9	143.9	146.1	148.2	149.2	150.3
6	152.4	154.5	155.6	156.6	158.8	160.9	161.9	163.0	165.1	167.2	168.3	169.3	171.5	173.6	174.6	175.7
7	177.8	179.9	181.0	182.0	184.2	186.3	187.3	188.4	190.5	192.6	193.7	194.7	196.9	199.0	200.0	201.1
8	203.2	205.3	206.4	207.4	209.6	211.7	212.7	213.8	215.9	218.0	219.1	220.1	222.3	224.4	225.4	226.5
9	228.6	230.7	231.8	232.8	235.0	237.1	238.1	239.2	241.3	243.4	244.5	245.5	247.7	249.8	250.8	251.9
10	254.0	256.1	257.2	258.2	260.4	262.5	263.5	264.6	266.7	268.8	269.9	270.9	273.1	275.2	276.2	277.3
11	279.4	281.5	282.6	283.6	285.8	287.9	288.9	290.0	292.1	294.2	295.3	296.3	298.5	300.6	301.6	302.7

TABLE VII.—*For converting metres into inches.*

Metres.	**0.**	**1.**	**2.**	**3.**	**4.**	**5.**	**6.**	**7.**	**8.**	**9.**
0	0	39.4	78.7	118.1	157.5	196.8	236.2	275.6	315.0	354.3
10	393.7	433.1	472.4	511.8	551.2	590.5	629.9	669.3	708.7	748.0
20	787.4	826.8	866.1	905.5	944.9	984.2	1023.6	1063.0	1102.4	1141.7
30	1181.1	1220.5	1259.8	1299.2	1338.6	1377.9	1417.3	1456.7	1496.1	1535.4
40	1574.8	1614.2	1653.5	1692.9	1732.3	1771.6	1811.0	1850.4	1889.8	1929.1
50	1968.5	2007.9	2047.2	2086.6	2126.0	2165.3	2204.7	2244.1	2283.5	2322.8
60	2362.2	2401.6	2440.9	2480.3	2519.7	2559.0	2598.4	2637.8	2677.2	2716.5
70	2755.9	2795.3	2834.6	2874.0	2913.4	2952.7	2992.1	3031.5	3070.9	3110.2
80	3149.6	3189.0	3228.3	3267.7	3307.1	3346.4	3385.8	3425.2	3464.6	3503.9
90	3543.3	3582.7	3622.0	3661.4	3700.8	3740.1	3779.5	3818.9	3858.3	3897.6

TABLE VIII.—*For converting inches into millimetres.*

Inches.	**0.**	**1.**	**2.**	**3.**	**4.**	**5.**	**6.**	**7.**	**8.**	**9.**
0	0	25.4	50.8	76.2	101.6	127.0	152.4	177.8	203.2	228.6
10	254.0	279.4	304.8	330.2	355.6	381.0	406.4	431.8	457.2	482.6
20	508.0	533.4	558.8	584.2	609.6	635.0	660.4	685.8	711.2	736.6
30	762.0	787.4	812.8	838.2	863.6	889.0	914.4	939.8	965.2	990.6
40	1016.0	1041.4	1066.8	1092.2	1117.6	1143.0	1168.4	1193.8	1219.2	1244.6
50	1270.0	1295.4	1320.8	1346.2	1371.6	1397.0	1422.4	1447.8	1473.2	1498.6
60	1524.0	1549.4	1574.8	1600.2	1625.6	1651.0	1676.4	1701.8	1727.2	1752.6
70	1778.0	1803.4	1828.8	1854.2	1879.6	1905.0	1930.4	1955.8	1981.2	2006.6
80	2032.0	2057.4	2082.8	2108.2	2133.6	2159.0	2184.4	2209.8	2235.2	2260.6
90	2286.0	2311.4	2336.8	2362.2	2387.6	2413.0	2438.4	2463.8	2489.2	2514.6

TABLE IX.—*For converting hectares into acres.*

Hectares.	**0.**	**1.**	**2.**	**3.**	**4.**	**5.**	**6.**	**7.**	**8.**	**9.**
0	0	2.47	4.94	7.41	9.88	12.36	14.83	17.30	19.77	22.24
10	24.71	27.18	29.65	32.12	34.59	37.07	39.54	42.01	44.48	46.95
20	49.42	51.89	54.36	56.83	59.30	61.78	64.25	66.72	69.19	71.66
30	74.13	76.60	79.07	81.54	84.01	86.49	88.96	91.43	93.90	96.37
40	98.84	101.31	103.78	106.25	108.72	111.20	113.67	116.14	118.61	121.08
50	123.55	126.02	128.49	130.96	133.43	135.91	138.38	140.85	143.32	145.79
60	148.26	151.73	154.20	156.67	159.14	161.62	163.09	165.56	168.03	170.50
70	172.97	175.44	177.91	180.38	182.85	185.33	187.80	190.27	192.74	195.21
80	197.68	200.15	202.62	205.09	207.56	210.04	212.51	214.98	217.45	219.92
90	222.39	224.86	227.33	229.80	232.27	235.75	238.22	240.69	243.16	245.63

TABLE X.—*For converting acres into hectares.*

Acres.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	.405	.809	1.214	1.619	2.023	2.428	2.833	3.238	3.642
10	4.047	4.452	4.856	5.261	5.666	6.070	6.475	6.880	7.284	7.689
20	8.094	8.499	8.903	9.308	9.713	10.117	10.522	10.927	11.331	11.736
30	12.141	12.546	12.950	13.355	13.760	14.164	14.569	14.974	15.378	15.783
40	16.188	16.592	16.997	17.402	17.807	18.211	18.616	19.021	19.425	19.830
50	20.235	20.639	21.044	21.449	21.854	22.258	22.663	23.068	23.472	23.877
60	24.282	24.686	25.091	25.496	25.900	26.305	26.710	27.115	27.519	27.924
70	28.329	28.733	29.138	29.543	29.947	30.352	30.757	31.161	31.566	31.971
80	32.376	32.780	33.185	33.590	33.994	35.399	35.804	36.208	36.713	37.018
90	36.423	36.827	37.232	37.637	38.041	38.446	38.851	39.256	39.660	40.065

TABLE XI—*For converting square kilometres into square miles.*

Square kilometres.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	0.386	0.772	1.158	1.544	1.931	2.317	2.703	3.089	3.475
10	3.861	4.247	4.633	5.019	5.405	5.792	6.178	6.564	6.950	7.336
20	7.722	8.108	8.494	8.880	9.266	9.653	10.039	10.425	10.811	11.197
30	11.583	11.969	12.355	12.741	13.127	13.514	13.900	14.286	14.672	15.058
40	15.444	15.830	16.216	16.602	16.988	17.375	17.761	18.147	18.533	18.919
50	19.305	19.691	20.077	20.463	20.849	21.236	21.622	22.008	22.394	22.780
60	23.166	23.552	23.938	24.324	24.710	25.097	25.483	25.869	26.255	26.641
70	27.027	27.413	27.799	28.185	28.571	28.958	29.344	29.730	30.116	30.502
80	30.888	31.274	31.660	32.046	32.432	32.819	33.205	33.591	33.977	34.363
90	34.749	35.135	35.521	35.907	36.293	36.680	37.066	37.452	37.838	38.224

TABLE XII.—*For converting square miles into hectares.*

(To convert square miles into square kilometres use this table and divide the result by 100.)

Square miles.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	259	518	777	1036	1295	1554	1813	2072	2331
10	2590	2849	3108	3367	3626	3885	4144	4403	4662	4921
20	5180	5439	5698	5957	6216	6475	6734	6993	7252	7511
30	7770	8029	8288	8547	8806	9065	9324	9583	9842	10101
40	10360	10619	10878	11137	11396	11655	11914	12173	12432	12691
50	12950	13209	13468	13727	13986	14245	14504	14763	15022	15281
60	15540	15799	16058	16317	16576	16835	17094	17353	17612	17871
70	18130	18389	18648	18907	19166	19425	19684	19943	20202	20461
80	20720	20979	21238	21497	21756	22015	22274	22533	22792	23051
90	23310	23569	23828	24087	24346	24605	24864	25123	25382	25641

TABLE XIII.—*For converting centares or square metres into square yards.*

Centares.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	1. 196	2. 392	3. 588	4. 784	5. 980	7. 176	8. 372	9. 568	10. 764
10	11. 960	13. 156	14. 352	15. 548	16. 744	17. 940	19. 136	20. 332	21. 528	22. 724
20	23. 920	25. 116	26. 312	27. 508	28. 704	29. 900	31. 096	32. 292	33. 488	34. 684
30	35. 880	37. 076	38. 272	39. 468	40. 663	41. 859	43. 055	44. 251	45. 447	46. 643
40	47. 839	49. 035	50. 231	51. 427	52. 623	53. 819	55. 015	56. 211	57. 407	58. 603
50	59. 799	60. 995	62. 191	63. 387	64. 583	65. 779	66. 975	68. 171	69. 367	70. 563
60	71. 759	72. 955	74. 151	75. 347	76. 543	77. 739	78. 935	80. 131	81. 327	82. 523
70	83. 719	84. 915	86. 111	77. 307	88. 503	89. 699	90. 895	92. 091	93. 287	94. 483
80	95. 679	96. 875	98. 071	99. 267	100. 463	101. 659	102. 855	104. 051	105. 247	106. 443
90	107. 639	108. 835	110. 031	111. 227	112. 423	113. 619	114. 815	116. 011	117. 207	118. 403

TABLE XIV.—*For converting square yards into centares or square metres.*

Square yards.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	0. 836	1. 672	2. 508	3. 345	4. 181	5. 017	5. 853	6. 689	7. 525
10	8. 361	9. 197	10. 034	10. 870	11. 706	12. 542	13. 378	14. 214	15. 050	15. 886
20	16. 723	17. 559	18. 395	19. 231	20. 067	20. 903	21. 739	22. 576	23. 412	24. 248
30	25. 084	25. 920	26. 756	27. 592	28. 428	29. 265	30. 101	30. 937	31. 773	32. 609
40	33. 445	34. 281	35. 117	35. 954	36. 790	37. 626	38. 462	39. 298	40. 134	40. 970
50	41. 807	42. 643	43. 479	44. 315	45. 151	45. 987	46. 823	47. 659	48. 496	49. 332
60	50. 168	51. 004	51. 840	52. 676	53. 512	54. 348	55. 185	56. 021	56. 857	57. 693
70	58. 529	59. 365	60. 201	61. 038	61. 874	62. 710	63. 546	64. 382	65. 218	66. 054
80	66. 890	67. 727	68. 563	69. 399	70. 235	71. 071	71. 907	72. 743	73. 580	74. 416
90	75. 252	76. 088	76. 924	77. 760	78. 596	79. 432	80. 269	81. 105	81. 941	82. 777

TABLE XV.—*For converting square metres or centares into square feet.*

Square metres.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	10. 76	21. 53	32. 29	43. 06	53. 82	64. 58	75. 35	86. 11	96. 87
10	107. 64	118. 40	129. 17	139. 93	150. 69	161. 46	172. 22	182. 99	193. 75	204. 51
20	215. 28	226. 04	236. 81	247. 57	258. 33	269. 10	279. 86	290. 62	301. 39	312. 15
30	322. 92	333. 68	344. 44	355. 21	365. 97	376. 74	387. 50	398. 26	409. 03	419. 79
40	430. 55	441. 32	452. 08	462. 85	473. 61	484. 37	495. 14	505. 90	516. 67	527. 43
50	538. 19	548. 96	559. 72	570. 48	581. 25	592. 01	602. 78	613. 54	624. 30	635. 07
60	645. 83	656. 60	667. 36	678. 12	688. 89	699. 65	710. 42	721. 18	731. 94	742. 71
70	753. 47	764. 23	775. 00	785. 76	796. 53	807. 29	818. 05	828. 82	839. 58	850. 35
80	861. 11	871. 87	882. 64	893. 40	904. 16	914. 93	925. 69	936. 46	947. 22	957. 98
90	968. 75	979. 51	990. 28	1001. 04	1011. 80	1022. 57	1033. 33	1044. 10	1054. 86	1065. 62

TABLE XVI.—*For converting square feet into square decimetres.*

(Divide the result by 100 to obtain square metres.)

Square feet.	**0.**	**1.**	**2.**	**3.**	**4.**	**5.**	**6.**	**7.**	**8.**	**9.**
0	0	9.29	18.58	27.87	37.16	46.45	55.74	65.03	74.32	83.61
10	92.90	102.19	111.48	120.77	130.06	139.36	148.65	157.94	167.23	176.52
20	185.81	195.10	204.39	213.68	222.97	232.26	241.55	250.84	260.13	269.42
30	278.71	288.00	297.29	306.58	315.87	325.16	334.45	343.74	353.03	362.32
40	371.61	380.90	390.19	399.48	408.78	418.07	427.36	436.65	445.94	455.23
50	464.52	473.81	483.10	492.39	501.68	510.97	520.26	529.55	538.84	548.13
60	557.42	566.71	576.00	585.29	594.58	603.87	613.16	622.45	631.74	641.03
70	650.32	659.61	668.90	678.19	687.49	696.78	706.07	715.36	724.65	733.94
80	743.23	752.52	761.81	771.10	780.39	789.68	798.97	808.26	817.55	826.84
90	836.13	845.42	854.71	864.00	873.29	882.58	891.87	901.16	910.45	919.74

TABLE XVII.—*For converting square metres or centares into square inches.*

Square metres.	**0.**	**1.**	**2.**	**3.**	**4.**	**5.**	**6.**	**7.**	**8.**	**9.**
0	0	1550	3100	4650	6200	7750	9300	10850	12400	13950
10	15500	17050	18600	20150	21700	23250	24800	26350	27900	29450
20	31000	32550	34100	35650	37200	38750	40300	41850	43400	44950
30	46500	48050	49600	51150	52700	54250	55800	57350	58900	60450
40	62000	63550	65100	66650	68200	69750	71300	72850	74400	75950
50	77500	79050	80600	82150	83700	85250	86800	88350	89900	91450
60	93000	94550	96100	97650	99200	100750	102300	103850	105400	106950
70	108500	110050	111600	113150	114700	116250	117800	119350	120900	122450
80	124000	125550	127100	128650	130200	131750	133300	134850	136400	137950
90	139500	141050	142600	144150	145700	147250	148800	150350	151900	153450

TABLE XVIII.—*For converting square inches into square centimetres.*

(Divide the number of square centimetres by 10,000 to obtain square metres.)

Square inches.	**0.**	**1.**	**2.**	**3.**	**4.**	**5.**	**6.**	**7.**	**8.**	**9.**
0	0	6.45	12.90	19.35	25.81	32.26	38.71	45.16	51.61	58.06
10	64.52	70.97	77.42	83.87	90.32	96.77	103.23	109.68	116.13	122.58
20	129.03	135.48	141.94	148.39	154.84	161.29	167.74	174.19	180.65	187.10
30	193.55	200.00	206.45	212.90	219.36	225.81	232.26	238.71	245.16	251.61
40	258.07	264.52	270.97	277.42	283.87	290.32	296.77	303.23	309.68	316.13
50	322.58	329.03	335.48	341.94	348.39	354.84	361.29	367.74	374.19	380.65
60	387.10	393.55	400.00	406.45	412.90	419.36	425.81	432.26	438.71	445.16
70	451.61	458.07	464.52	470.97	477.42	483.87	490.32	496.78	503.23	509.68
80	516.13	522.58	529.03	535.48	541.94	548.39	554.84	561.29	567.74	574.19
90	580.65	587.10	593.55	600.00	606.45	612.90	619.36	625.81	632.26	638.71

TABLE XIX.—*For converting cubic metres or steres into cubic yards.*

Cubic metres.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	1.308	2.616	3.924	5.232	6.540	7.848	9.156	10.464	11.771
10	13.079	14.387	15.695	17.003	18.311	19.619	20.927	22.235	23.543	24.851
20	26.159	27.467	28.775	30.083	31.391	32.699	34.007	35.314	36.622	37.930
30	39.238	40.546	41.854	43.162	44.470	45.778	47.086	48.394	49.702	51.010
40	52.318	53.626	54.934	56.242	57.549	58.857	60.165	61.473	62.781	64.089
50	65.397	66.705	68.013	69.321	70.629	71.937	73.245	74.553	75.861	77.169
60	78.477	79.785	81.092	82.400	83.708	85.016	86.324	87.632	88.940	90.248
70	91.556	92.864	94.172	95.480	96.788	98.096	99.404	100.712	102.020	103.327
80	104.635	105.943	107.251	108.559	109.867	111.175	112.483	113.791	115.099	116.407
90	117.715	119.023	120.331	121.639	122.947	124.255	125.563	126.870	128.178	129.486

TABLE XX.—*For converting cubic yards into litres.*

(To convert cubic yards into steres or cubic metres use this table and divide the result by 1,000.)

Cubic yards.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	765	1529	2294	3058	3823	4587	5352	6116	6881
10	7646	8410	9175	9939	10704	11468	12233	12998	13962	14527
20	15291	16056	16820	17585	18349	19114	19879	20643	21408	22172
30	22937	23701	24466	25230	25995	26760	27524	28289	29053	29818
40	30582	31347	32111	32876	33641	34405	35170	35934	36699	37463
50	38228	38993	39757	40522	41286	42051	42815	43580	44344	45109
60	45874	46638	47403	48167	48932	49696	50461	51225	51990	52755
70	53519	54284	55048	55813	56577	57342	58107	58871	59636	60400
80	61165	61929	62694	63458	64223	64988	65752	66517	67281	68046
90	68810	69575	70339	71104	71869	72633	73398	74162	74927	75691

TABLE XXI.—*For converting cubic feet into litres or cubic decimetres.*

(Divide the number of litres by 1,000 for cubic metres.)

Cubic feet.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	28.3	56.6	85.0	113.3	141.6	169.9	198.2	226.5	254.9
10	283.2	311.5	339.8	368.1	396.4	424.8	453.1	481.4	509.7	538.0
20	566.3	594.7	623.0	651.3	679.6	707.9	736.2	764.6	792.9	821.2
30	849.5	877.8	906.1	934.5	962.8	991.1	1019.4	1047.7	1076.0	1104.4
40	1132.7	1161.0	1189.3	1217.6	1245.9	1274.3	1302.6	1330.9	1359.2	1387.5
50	1415.9	1444.2	1472.5	1500.8	1529.1	1557.4	1585.8	1614.1	1642.4	1670.7
60	1699.0	1727.3	1755.7	1784.0	1812.3	1840.6	1868.9	1897.2	1925.6	1953.9
70	1982.2	2010.5	2038.8	2067.1	2095.5	2123.8	2152.1	2180.4	2208.7	2237.0
80	2265.4	2293.7	2322.0	2350.3	2378.6	2406.9	2435.3	2463.6	2491.9	2520.2
90	2548.5	2576.8	2605.2	2633.5	2661.8	2690.1	2718.4	2746.7	2775.1	2803.4

TABLE XXII.—*For converting cubic metres into cubic feet.*

(To convert litres into cubic feet use this table and divide by 1,000.)

Cubic metres.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	35.3	70.6	105.9	141.3	176.6	211.9	247.2	282.5	317.8
10	353.1	388.5	423.8	459.1	494.4	529.7	565.0	600.3	635.7	671.0
20	706.3	741.6	776.9	812.2	847.5	882.9	918.2	953.5	988.8	1024.1
30	1059.4	1094.7	1130.1	1165.4	1200.7	1236.0	1271.3	1306.6	1341.9	1377.3
40	1412.6	1447.9	1483.2	1518.5	1553.8	1589.2	1624.5	1659.8	1695.1	1730.4
50	1765.7	1801.0	1836.4	1871.7	1907.0	1942.3	1977.6	2012.9	2048.2	2083.6
60	2118.9	2154.2	2189.5	2224.8	2260.1	2295.4	2330.8	2366.1	2401.4	2436.7
70	2472.0	2507.3	2542.6	2578.0	2613.3	2648.6	2683.9	2719.2	2754.5	2789.8
80	2825.2	2860.5	2895.8	2931.1	2966.4	3001.7	3037.0	3072.4	3107.7	3143.0
90	3178.3	3213.6	3248.9	3284.2	3319.6	3354.9	3390.2	3425.5	3460.8	3496.1

TABLE XXIII.—*For converting litres into cubic inches.*

Litres.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	61.0	122.0	183.1	244.1	305.1	366.1	427.2	488.2	549.2
10	610.2	671.3	732.3	793.3	854.3	915.4	976.4	1037.4	1098.4	1159.4
20	1220.5	1281.5	1342.5	1403.5	1464.6	1525.6	1586.6	1647.6	1708.7	1769.7
30	1830.7	1891.7	1952.8	2013.8	2074.8	2135.8	2196.8	2257.9	2318.9	2379.9
40	2440.9	2502.0	2563.0	2624.0	2685.0	2746.1	2807.1	2868.1	2929.1	2990.1
50	3051.2	3112.2	3173.2	3234.2	3295.3	3356.3	3417.3	3478.3	3539.4	3600.4
60	3661.4	3722.4	3783.5	3844.5	3905.5	3966.5	4027.5	4088.6	4149 6	4210.6
70	4271.6	4332.7	4393.7	4454.7	4515.7	4576.8	4637.8	4698.8	4759.8	4820.8
80	4881.9	4942.9	5003.9	5064.9	5126.0	5187.0	5248.0	5309.0	5370.1	5431.1
90	5492.1	5553.1	5614.2	5675.2	5736.2	5797.2	5858.2	5919.3	5980.3	6041.3

TABLE XXIV.—*For converting cubic inches into cubic centimetres.*

(To convert cubic inches into litres use this table and divide the result by 1,000.)

Cubic inches.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	16.4	32.8	49.2	65.5	81.9	98.3	114.7	131.1	147.5
10	163.9	180.3	196.6	213.0	229.4	245.8	262.2	278.6	295.0	311.4
20	327.7	344.1	360.5	376.9	393.3	409.7	426.1	442.5	458.8	475.2
30	491.6	508.0	524.4	540.8	557.2	573.6	589.9	606.3	622.7	639.1
40	655.5	671.9	688.3	704.6	721.0	737.4	753.8	770.2	786.6	703.0
50	819.4	835.7	852.1	868.5	884.9	901.3	917.7	934.1	950.5	966.8
60	983.2	999.6	1016.0	1032.4	1048.8	1065.2	1081.6	1097.9	1114.3	1130.7
70	1147.1	1163.5	1179.9	1196.3	1212.6	1229.0	1245.4	1261.8	1278.2	1294.6
80	1311.0	1327.4	1343.7	1350.1	1376.5	1392.9	1409.3	1425.7	1442.1	1458.5
90	1474.8	1491.2	1507.6	1524.0	1530.4	1556.8	1573.2	1589.6	1605.9	1622.3

TABLE XXV.—*For converting hectolitres into bushels.*

(To convert cubic metres to bushels use this table and multiply the result by 10.)

Hectolitres.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	2.84	5.68	8.51	11.35	14.19	17.03	19.86	22.70	25.54
10	28.38	31.22	34.05	36.89	39.73	42.57	45.40	48.24	51.08	53.92
20	56.75	59.59	62.43	65.27	68.11	70.94	73.78	76.62	79.46	82.29
30	85.13	87.97	90.81	93.65	96.48	99.32	102.16	105.00	107.83	110.67
40	113.51	116.35	119.19	121.02	124.86	127.70	130.54	133.37	136.21	139.05
50	141.89	144.72	147.56	150.40	153.24	156.08	158.91	161.75	164.59	167.43
60	170.26	173.10	175.94	178.78	181.62	184.45	187.29	190.13	192.97	195.80
70	198.64	201.48	204.32	207.16	209.99	212.83	215.67	218.51	221.34	224.18
80	227.02	229.86	232.69	235.53	238.37	241.21	244.05	246.88	249.72	252.56
90	255.40	258.23	261.07	263.91	266.75	269.59	272.42	275.26	278.10	280.94

TABLE XXVI.—*For converting bushels into litres.*

Bushels.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	35.2	70.5	105.7	141.0	176.2	211.4	246.7	281.9	317.2
10	352.4	387.6	422.9	458.1	493.3	528.6	563.8	599.1	634.3	669.5
20	704.8	740.0	775.3	810.5	845.7	881.0	916.2	951.5	986.7	1021.9
30	1057.2	1092.4	1127.7	1162.9	1198.1	1233.4	1268.6	1303.9	1339.1	1374.3
40	1409.6	1444.8	1480.0	1515.3	1550.5	1585.8	1621.0	1656.2	1691.5	1726.7
50	1762.0	1797.2	1832.4	1867.7	1902.9	1938.2	1973.3	2008.6	2043.9	2079.1
60	2114.4	2149.6	2184.8	2220.1	2255.3	2290.6	2325.8	2361.0	2396.3	2431.5
70	2466.7	2502.0	2537.2	2572.5	2607.7	2642.9	2678.2	2713.4	2748.7	2783.9
80	2819.1	2854.4	2889.6	2924.9	2960.0	2995.3	3030.6	3065.8	3101.1	3136.3
90	3171.5	3206.8	3242.0	3277.3	3312.5	3347.7	3383.0	3418.2	3453.4	3488.7

TABLE XXVII.—*For converting hectolitres into wine gallons.*

Hectolitres.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	26.42	52.83	79.25	105.67	132.09	158.50	184.92	211.34	237.75
10	264.17	290.59	317.00	343.42	369.84	396.26	422.67	449.09	475.51	501.92
20	528.34	554.76	581.18	607.59	634.01	660.43	686.84	713.26	739.68	766.09
30	792.51	818.93	845.35	871.76	898.18	924.60	951.01	977.43	1003.85	1030.26
40	1056.68	1083.10	1109.52	1185.93	1162.35	1188.77	1215.18	1241.60	1268.02	1294.44
50	1320.85	1347.27	1373.69	1400.10	1426.52	1452.94	1479.35	1505.77	1532.19	1558.61
60	1585.02	1611.44	1637.86	1664.27	1690.69	1717.11	1743.53	1769.94	1796.36	1822.78
70	1849.19	1875.61	1902.03	1928.44	1954.86	1981.28	2007.70	2034.11	2060.53	2086.95
80	2113.36	2139.78	2166.10	2192.61	2219.03	2245.45	2271.87	2298.28	2324.70	2351.12
90	2377.53	2403.95	2430.37	2456.79	2483.10	2509.62	2536.04	2592.45	2618.87	2645.29

TABLE XXVIII.—*For converting wine gallons into litres.*

Wine gallons.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	3.79	7.57	11.36	15.14	18.93	22.71	26.50	30.28	34.07
10	37.85	41.64	45.43	49.21	53.00	56.78	60.57	64.35	68.14	71.92
20	75.71	79.49	83.28	87.06	90.85	94.64	98.42	102.21	105.99	109.78
30	113.56	117.35	121.13	124.92	128.70	132.49	136.28	140.06	143.85	147.63
40	151.42	155.20	158.99	162.77	166.56	170.34	174.13	177.92	181.70	185.49
50	189.27	193.06	197.84	200.63	204.41	208.20	211.98	215.77	219.56	223.34
60	227.13	230.91	234.70	238.48	242.27	246.05	249.84	253.62	257.41	261.19
70	264.98	268.77	272.55	276.34	280.12	283.91	287.69	291.48	295.26	299.05
80	302.83	306.62	310.41	314.19	317.98	321.76	325.55	329.33	333.12	336.90
90	340.69	344.47	348.26	352.05	355.83	359.62	363.40	367.19	370.97	374.76

TABLE XXIX.—*For converting wine quarts into millilitres.*

(To convert quarts into litres use this table and divide the result by 1,000.)

Wine quarts.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	946	1893	2839	3785	4732	5678	6625	7571	8517
10	9464	10410	11356	12303	13249	14195	15142	16088	17034	17981
20	18927	19874	20820	21766	22713	23659	24605	25552	26498	27444
30	28391	29337	30283	31230	32176	33123	34069	35015	35962	36908
40	37854	38801	39747	40693	41640	42586	43532	44479	45425	46373
50	47318	48264	49211	50157	51103	52050	52996	53942	54889	55835
60	56782	57728	58674	59621	60567	61513	62460	63406	64352	65299
70	66245	67191	68138	69084	70031	70977	71923	72870	73816	74762
80	75709	76655	77601	78548	79494	80440	81387	82333	83280	84226
90	85172	86119	87065	88011	88958	89904	90850	91797	92743	93690

TABLE XXX.—*For converting hectolitres into wine quarts.*

(To convert litres into quarts use this table and divide the result by 100.)

Hectolitres.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	105.7	211.3	317.0	422.7	528.3	634.0	739.7	845.3	951.0
10	1056.7	1162.3	1268.0	1373.7	1479.4	1585.0	1690.7	1796.4	1902.0	2007.7
20	2113.4	2219.0	2324.7	2430.4	2536.0	2641.7	2747.4	2853.0	2958.7	3064.4
30	3170.0	3275.7	3381.4	3487.1	3592.7	3698.4	3804.1	3909.7	4015.4	4121.1
40	4226.7	4332.4	4438.1	4543.7	4649.4	4755.1	4860.7	4966.4	5072.1	5177.7
50	5283.4	5389.1	5494.7	5600.4	5706.1	5811.8	5917.4	6023.1	6128.8	6234.4
60	6340.1	6445.8	6551.4	6657.1	6762.8	6868.4	6974.1	7079.8	7185.4	7291.1
70	7396.8	7502.4	7608.1	7713.8	7819.4	7925.1	8030.8	8136.5	8242.1	8347.8
80	8453.5	8559.1	8664.8	8770.5	8876.1	8981.8	9087.5	9193.1	9298.8	9404.5
90	9510.1	9615.8	9721.5	9827.1	9932.8	10038.5	10144.1	10249.8	10355.5	10461.2

TABLE XXXI.—*For converting litres into fluid ounces.*

Litres.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	33.8	67.6	101.4	135.3	169.1	202.9	236.7	270.5	304.3
10	338.1	372.0	405.8	439.6	473.4	507.2	541.0	574.8	608.6	642.5
20	676.3	710.1	743.9	777.7	811.5	845.3	879.2	913.0	946.8	980.6
30	1014.4	1048.2	1082.0	1115.9	1149.7	1183.5	1217.3	1251.1	1284.9	1318.7
40	1352.6	1386.4	1420.2	1454.0	1487.8	1521.6	1555.4	1589.2	1623.1	1656.9
50	1690.7	1724.5	1758.3	1792.1	1825.9	1859.8	1893.6	1927.4	1961.2	1995.0
60	2028.8	2062.6	2096.5	2130.3	2164.1	2197.9	2231.7	2265.5	2299.3	2333.2
70	2367.0	2400.8	2434.6	2468.4	2502.2	2536.0	2569.8	2603.7	2637.5	2671.3
80	2705.1	2738.9	2772.7	2806.5	2840.4	2874.2	2908.0	2941.8	2975.6	3009.4
90	3043.2	3077.1	3110.9	3144.7	3178.5	3212.3	3246.1	3279.9	3313.8	3347.6

TABLE XXXII.—*For converting fluid ounces into centilitres.*

Fuid ounces.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	2.96	5.91	8.87	11.83	14.79	17.74	20.70	23.66	26.62
10	29.57	32.53	35.49	38.45	41.40	44.36	47.32	50.28	53.23	56.19
20	59.15	62.10	65.06	68.02	70.98	73.93	76.89	79.85	82.81	85.76
30	88.72	91.68	94.64	97.59	100.55	103.51	106.47	109.42	112.38	115.34
40	118.29	121.25	124.21	127.17	130.12	133.08	136.04	139.00	141.95	144.91
50	147.87	150.83	153.78	156.74	159.70	162.66	165.61	168.57	171.53	174.48
60	177.44	180.40	183.36	186.31	189.27	192.23	195.19	198.14	201.10	204.06
70	207.02	209.97	212.93	215.89	218.85	221.80	224.76	227.72	230.67	233.63
80	236.59	239.55	242.50	245.46	248.42	251.37	254.33	257.29	260.25	263.21
90	266.16	269.12	272.08	275.04	277.99	280.95	283.91	286.86	289.82	292.78

TABLE XXXIII.—*For converting tonneaux or milliers into long tons, (of 2,240 pounds.)*

(To convert kilograms into long tons use this table and divide the result by 1,000.)

Tonneaux or milliers.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	0.984	1.968	2.953	3.937	4.921	5.905	6.889	7.874	8.858
10	9.842	10.826	11.810	12.795	13.779	14.763	15.747	16.731	17.716	18.700
20	19.684	20.668	21.652	22.637	23.621	24.605	25.589	26.573	27.557	28.542
30	29.526	30.510	31.494	32.478	33.463	34.447	35.431	36.415	37.399	38.384
40	39.368	40.352	41.336	42.320	43.305	44.289	45.273	46.257	47.241	48.226
50	49.210	50.194	51.178	52.162	53.147	54.131	55.115	56.099	57.083	58.068
60	59.052	60.036	61.020	62.004	62.989	63.973	64.957	65.941	66.925	67.910
70	68.894	69.878	70.862	71.846	72.831	73.815	74.799	75.783	76.767	77.752
80	78.736	79.720	80.704	81.688	82.672	83.657	84.641	85.625	86.609	87.593
90	88.578	89.562	90.546	91.530	92.514	93.499	94.483	95.467	96.451	97.435

TABLE XXXIV.—*For converting long tons (of 2,240 lbs. each) into kilograms.*

(Divide the result by 1,000 for tonneaux or milliers.)

Long tons.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	1016	2032	3048	4064	5080	6096	7112	8128	9145
10	10161	11177	12193	13209	14225	15241	16257	17273	18289	19305
20	20321	21337	22353	23369	24385	25401	26417	27434	28450	29466
30	30482	31498	32514	33530	34546	35562	36578	37594	38610	39626
40	40642	41658	42674	43690	44707	45723	46739	47755	48771	49787
50	50803	51819	52835	53851	54867	55883	56899	57915	58931	59947
60	60963	61979	62996	64012	65028	66044	67060	68076	69092	70108
70	71124	72140	73156	74172	75188	76204	77220	78236	79252	80269
80	81285	82301	83317	84333	85349	86365	87381	88397	89413	90429
90	91445	92461	93477	94493	95509	96525	97542	98558	99574	100590

TABLE XXXV.—*For converting tonneaux or milliers into short tons, (of 2,000. pounds.)*

(To convert kilograms into short tons use this table and divide the result by 1,000.)

Tonneaux or milliers.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	1.102	2.205	3.307	4.409	5.512	6.614	7.716	8.818	9.921
10	11.023	12.125	13.228	14.330	15.432	16.535	17.637	18.739	19.841	20.944
20	22.046	23.148	24.251	25.353	26.455	27.558	28.660	29.762	30.864	31.967
30	33.069	34.171	35.274	36.376	37.478	38.581	39.683	40.785	41.887	42.990
40	44.092	45.194	46.297	47.399	48.501	49.604	50.708	51.808	52.910	54.013
50	55.115	56.217	57.320	58.422	59.524	60.627	61.729	62.831	63.933	65.036
60	66.138	67.240	68.343	69.445	70.547	71.650	72.752	73.854	74.956	76.059
70	77.161	78.263	79.366	80.468	81.570	82.673	83.775	84.877	85.979	87.082
80	88.184	89.286	90.389	91.491	92.593	93.696	94.798	95.900	97.002	98.105
90	99.207	100.309	101.412	102.514	103.616	104.719	105.821	106.923	108.025	109.128

TABLE XXXVI.—*For converting short tons (of 2,000 lbs. each) into kilograms.*

(Divide the result by 1,000 for tonneaux or milliers.)

Short tons.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	907	1814	2722	3629	4536	5443	6350	7258	8165
10	9072	9979	10886	11794	12701	13608	14515	15422	16329	17237
20	18144	19051	19958	20865	21773	22680	23587	24494	25401	26309
30	27216	28123	29030	29937	30845	31752	32659	33566	34473	35381
40	36288	37195	38102	39009	39917	40824	41731	42638	43545	44453
50	45360	46267	47174	48081	48988	49896	50803	51710	52617	53524
60	54432	55339	56246	57153	58060	58968	59875	60782	61689	62596
70	63504	64411	65318	66225	67132	68040	68947	69854	70761	71668
80	72576	73483	74390	75297	76204	77111	78019	78926	79833	80740
90	81647	82555	83462	84369	85276	86183	87091	87998	88905	89812

TABLE XXXVII.—*For converting kilograms into avoirdupois pounds.*

Kilograms.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	2.205	4.409	6.614	8.818	11.023	13.228	15.432	17.637	19.841
10	22.046	24.251	26.455	28.660	30.864	33.069	35.274	37.478	39.683	41.887
20	44.092	46.297	48.501	50.706	52.910	55.115	57.320	59.524	61.729	63.933
30	66.138	68.343	70.547	72.752	94.956	77.161	79.366	81.570	83.775	85.979
40	88.184	90.389	92.593	94.798	97.002	99.207	101.412	103.616	105.821	107.025
50	110.230	112.435	114.639	116.844	199.048	121.253	123.458	125.662	127.867	130.071
60	132.276	134.481	136.685	138.890	141.094	143.299	145.504	147.808	149.913	152.117
70	154.322	156.527	158.731	160.936	163.140	165.345	167.550	169.754	171.959	174.163
80	176.368	178.573	180.777	182.982	185.186	187.391	189.596	191.800	194.005	196.209
90	198.414	200.619	202.823	205.028	207.232	209.437	211.642	213.846	216.051	218.255

TABLE XXXVIII.—*For converting avoirdupois pounds into grams.*

Pounds.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	454	907	1361	1814	2268	2722	3175	3629	4082
10	4536	4990	5443	5897	6350	6804	7258	7711	8165	8618
20	9072	9526	9979	10433	10886	11340	11794	12247	12701	13154
30	13608	14062	14515	14969	15422	15876	16329	16783	17237	17690
40	18144	18597	19051	19505	19958	20412	20865	21319	21773	22226
50	22680	23133	23587	24041	24494	24948	25401	25855	26309	26762
60	27216	27669	28123	28577	29030	29484	29937	30391	30845	31298
70	31752	32205	32659	33113	33566	34020	34473	34927	35381	35834
80	36288	36741	37195	37649	38102	38556	39009	39463	39917	40370
90	40824	41277	41731	42185	42638	43092	43545	43999	44453	44906

TABLE XXXIX.—*For converting kilograms into avoirdupois pounds and ounces.*

Kilograms.	0.	0.1.	0.2.	0.3.	0.4.	0.5.	0.6.	0.7.	0.8.	0.9.
	lbs. oz.	*lbs. oz.*	*lbs. oz.*	*lbs. oz.*	*lbs. oz.*	*lbs. oz.*	*lbs. oz.*	*lbs. oz.*	*lbs. oz.*	*lbs. oz.*
0	0 0.0	0 3.5	0 7.1	0 10.6	0 14.1	1 1.6	1 5.2	1 8.7	1 12.2	1 15.7
1	2 3.3	2 6.8	2 10.3	2 13.9	3 1.4	3 4.9	3 8.4	3 12.0	3 15.5	4 3.0
2	4 6.5	4 10.1	4 13.6	5 1.1	5 4.7	5 8.2	5 11.7	5 15.2	6 2.8	6 6.3
3	6 9.8	6 13.3	7 0.9	7 4.4	7 7.9	7 11.5	7 15.0	8 2.5	8 6.0	8 9.6
4	8 13.1	9 0.6	9 4.1	9 7.7	9 11.2	9 14.7	10 2.3	10 5.8	10 9.3	10 12.8
5	11 0.4	11 3.9	11 7.4	11 11.0	11 14.5	12 2.0	12 5.5	12 9.1	12 12.6	13 0.1
6	13 3.6	13 7.2	13 10.7	13 14.2	14 1.8	14 5.3	14 8.8	14 12.3	14 15.9	15 3.4
7	15 6.9	15 10.4	15 14.0	16 1.5	16 5.0	16 8.6	16 12.1	16 15.6	17 3.1	17 6.7
8	17 10.2	17 13.7	18 1.3	18 4.8	18 8.3	18 11.8	18 15.4	19 2.9	19 6.4	19 9.9
9	19 13.5	20 1.0	20 4.5	20 8.0	20 11.6	20 15.1	21 2.6	21 6.2	21 9.7	21 13.2
10	22 0.7	22 4.3	22 7.8	22 11.3	22 14.8	23 2.4	23 5.9	23 9.4	23 13.0	24 0.5
11	24 4.0	24 7.5	24 11.1	24 14.6	25 2.1	25 5.6	25 9.2	25 12.7	26 0.2	26 3.8
12	26 7.3	26 10.8	26 14.3	27 1.9	27 5.4	27 8.9	27 12.4	28 0.0	28 3.5	28 7.0
13	28 10.6	28 14.1	29 1.6	29 5.1	29 8.7	29 12.2	29 15.7	30 3.2	30 6.8	30 10.3
14	30 13.8	31 1.4	31 4.9	31 8.4	31 11.9	31 15.5	32 3.0	32 6.5	32 10.0	32 13.6
15	33 1.1	33 4.6	33 8.2	33 11.7	33 15.2	34 2.7	34 6.3	34 9.8	34 13.3	35 0.9
16	35 4.4	35 7.9	35 11.4	35 15.0	36 2.5	36 6.0	36 9.6	36 13.1	37 0.6	37 4.1
17	37 7.7	37 11.2	37 14.7	38 2.2	38 5.8	38 9.3	38 12.8	39 6.3	39 3.9	39 7.4
18	39 10.9	39 14.5	40 2.0	40 5.5	40 9.0	40 12.6	41 0.1	41 3.6	41 7.1	41 10.7
19	41 14.2	42 1.7	42 5.3	42 8.8	42 12.3	42 15.8	43 3.4	43 6.9	43 10.4	43 13.9
20	44 1.5	44 5.0	44 8.5	44 12.1	44 15.6	45 3.1	45 6.6	45 10.2	45 13.7	46 1.2

Table XL.—*For converting avoirdupois pounds and ounces into grams.*

(Divide the result by 1,000 for kilograms.)

Avoir. pounds.	0.	1 oz.	2 oz.	3 oz.	4 oz.	5 oz.	6 oz.	7 oz.	8 oz.	9 oz.	10 oz.	11 oz.	12 oz.	13 oz.	14 oz.	15 oz.
0	0	28	57	85	113	142	170	198	227	255	283	312	340	369	397	425
1	454	482	510	539	567	595	624	652	680	709	737	765	794	822	850	879
2	907	936	964	992	1021	1049	1077	1106	1134	1162	1191	1219	1247	1276	1304	1332
3	1361	1389	1417	1446	1474	1503	1531	1559	1588	1616	1644	1673	1701	1729	1758	1786
4	1814	1843	1871	1899	1928	1956	1984	2013	2041	2070	2098	2126	2155	2183	2211	2240
5	2268	2296	2325	2353	2381	2410	2438	2466	2495	2523	2551	2580	2608	2637	2665	2693
6	2722	2750	2778	2807	2835	2863	2892	2920	2948	2977	3005	3033	3062	3090	3118	3147
7	3175	3204	3232	3260	3289	3317	3345	3374	3402	3430	3459	3487	3515	3544	3572	3600
8	3629	3657	3685	3714	3742	3771	3799	3827	3856	3884	3912	3941	3969	3997	4026	4054
9	4082	4111	4139	4167	4196	4224	4252	4281	4309	4338	4366	4394	4423	4451	4479	4508
10	4536	4564	4593	4621	4649	4678	4706	4734	4763	4791	4819	4848	4876	4905	4933	4961
11	4990	5018	5046	5075	5103	5131	5160	5188	5216	5245	5273	5301	5330	5358	5386	5415
12	5443	5472	5500	5528	5557	5585	5613	5642	5670	5698	5727	5755	5783	5812	5840	5868
13	5897	5925	5953	5982	6010	6039	6067	6095	6124	6152	6180	6209	6237	6265	6294	6322
14	6350	6379	6407	6435	6464	6492	6520	6549	6577	6606	6634	6662	6691	6719	6747	6776
15	6804	6832	6861	6889	6917	6945	6974	7002	7031	7059	7087	7116	7144	7173	7201	7229
16	7258	7286	7314	7343	7371	7399	7428	7456	7484	7512	7541	7569	7598	7626	7555	7583
17	7711	7740	7768	7796	7825	7853	7881	7910	7938	7966	7995	8023	8051	8079	8108	8136
18	8165	8193	8222	8250	8278	8307	8335	8363	8392	8420	8448	8477	8505	8533	8562	8590
19	8618	8646	8675	8703	8732	8750	8789	8817	8845	8874	8902	8930	8959	8987	9015	9044
20	9072	9100	9129	9157	9185	9213	9242	9270	9299	9327	9356	9384	9412	9441	9469	9497

Table XLI.—*For converting kilograms into avoirdupois ounces.*

Kilograms.	**0.**	**1.**	**2.**	**3.**	**4.**	**5.**	**6.**	**7.**	**8.**	**9.**
0	0	35.3	70.5	105.8	141.1	176.4	211.6	246.9	282.2	317.5
10	352.7	388.0	423.3	458.6	493.8	529.1	564.4	599.7	634.9	670.2
20	705.5	740.7	776.0	811.3	846.6	881.8	917.1	952.4	987.7	1022.9
30	1058.2	1093.5	1128.8	1164.0	1199.3	1234.6	1269.8	1305.1	1340.4	1375.7
40	1410.9	1446.2	1481.5	1516.8	1552.0	1587.3	1622.6	1657.9	1693.1	1728.4
50	1763.7	1799.0	1834.2	1869.5	1904.8	1940.0	1975.3	2010.6	2045.9	2081.1
60	2116.4	2151.7	2187.0	2222.2	2257.5	2292.8	2328.1	2363.3	2398.6	2433.9
70	2469.2	2504.4	2539.7	2575.0	2610.2	2645.5	2680.8	2716.1	2751.3	2786.1
80	2821.9	2857.2	2892.4	2927.7	2963.0	2998.3	3033.5	3068.8	3104.1	3139.4
90	3174.6	3209.9	3245.2	3280.4	3315.7	3351.0	3386.3	3421.5	3456.8	3492.1

Table XLII.—*For converting avoirdupois ounces into grams.*

Av. ounces.	**0.**	**1.**	**2.**	**3.**	**4.**	**5.**	**6.**	**7.**	**8.**	**9.**
0	0	28.3	56.7	85.0	113.4	141.7	170.1	198.4	226.8	255.1
10	283.5	311.8	340.2	368.5	396.9	425.2	453.6	481.9	510.3	538.6
20	567.0	595.3	623.7	652.0	680.4	708.7	737.1	765.4	793.8	822.1
30	850.5	878.8	907.2	935.5	963.9	992.2	1020.6	1048.9	1077.3	1105.6
40	1134.0	1162.3	1190.7	1219.0	1247.4	1275.7	1304.1	1332.4	1360.8	1389.1
50	1417.5	1445.8	1474.2	1502.5	1530.9	1559.2	1587.6	1615.9	1644.3	1672.6
60	1701.0	1729.3	1757.7	1786.0	1814.4	1842.7	1871.1	1899.4	1927.8	1956.1
70	1984.5	2012.8	2041.2	2069.5	2097.9	2126.2	2154.6	2182.9	2211.3	2239.6
80	2268.0	2296.3	2324.7	2353.0	2381.4	2409.7	2438.1	2466.4	2494.8	2523.1
90	2551.5	2579.8	2608.2	2636.5	2664.9	2693.2	2721.6	2749.9	2778.3	28066.

TABLE XLIII.—*For converting kilograms into troy ounces.*

Kilograms.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	32.15	64.30	96.45	128.60	160.75	192.90	225.05	257.20	289.35
10	321.50	353.65	385.81	417.96	450.11	482.26	514.41	546.56	578.71	610.86
20	643.01	675.16	707.31	739.46	771.61	803.76	835.91	868.06	900.21	932.36
30	964.51	996.66	1028.81	1060.96	1093.11	1125.26	1157.42	1189.57	1221.72	1253.87
40	1286.02	1318.17	1350.32	1382.47	1414.62	1446.77	1478.92	1511.07	1543.22	1575.37
50	1607.52	1639.67	1671.82	1703.97	1736.12	1768.27	1800.42	1832.57	1864.72	1896.87
60	1929.03	1961.18	1993.33	2025.48	2057.63	2089.78	2121.93	2154.08	2186.23	2218.38
70	2250.53	2282.68	2314.83	2346.98	2379.13	2411.28	2443.43	2475.58	2507.73	2539.88
80	2572.03	2604.18	2636.33	2668.48	2700.64	2732.79	2764.94	2797.09	2829.24	2861.39
90	2893.54	2925.69	2957.84	2989.99	3022.14	3054.29	3086.44	3118.59	3150.74	3182.89

TABLE XLIV.—*For converting troy ounces into grams.*

Troy ounces.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	31.10	62.21	93.31	124.42	155.52	186.62	217.73	248.83	279.93
10	311.04	342.14	373.25	404.35	435.45	466.56	497.66	528.76	559.87	590.97
20	622.08	653.18	684.28	715.39	746.49	777.59	808.70	839.80	870.91	902.01
30	933.11	964.22	995.32	1026.43	1057.53	1088.63	1119.74	1150.84	1181.94	1213.05
40	1244.15	1275.26	1306.36	1337.46	1368.57	1399.67	1430.77	1461.88	1492.98	1524.09
50	1555.19	1586.29	1617.40	1648.50	1679.60	1710.71	1741.81	1772.92	1804.02	1835.12
60	1866.23	1897.33	1928.44	1959.54	1990.64	2021.75	2052.85	2083.95	2115.06	2146.16
70	2177.27	2208.37	2239.47	2270.58	2301.68	2332.78	2363.89	2394.99	2426.10	2457.20
80	2488.30	2519.41	2550.51	2581.62	2612.72	2643.82	2674.93	2706.03	2737.13	2768.24
90	2799.34	2830.45	2861.55	2892.65	2923.76	2954.86	2985.96	3017.07	3048.17	3079.28

TABLE XLV.—*For converting grams into grains.*

Grams.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	15.43	30.86	46.30	61.73	77.16	92.59	108.03	123.46	130.89
10	154.32	169.75	185.19	200.62	216.05	231.48	246.92	262.35	277.78	293.21
20	308.64	324.08	339.51	354.94	370.37	385.81	401.24	416.67	432.10	447.53
30	462.97	478.40	493.83	509.26	524.69	540.13	555.56	570.99	586.42	601.86
40	617.29	632.72	648.15	663.58	679.02	694.45	709.88	725.31	740.75	756.18
50	771.61	787.04	802.47	817.91	833.34	848.77	864.20	879.64	895.07	910.50
60	925.93	941.36	956.80	972.23	987.66	1003.09	1018.53	1033.96	1049.39	1064.82
70	1080.25	1095.69	1111.12	1126.55	1141.98	1157.42	1172.85	1188.28	1203.71	1219.14
80	1234.58	1250.01	1265.44	1280.87	1296.30	1311.74	1327.17	1342.60	1358.03	1373.47
90	1388.90	1404.33	1419.76	1435.19	1450.63	1466.06	1481.49	1496.92	1512.36	1527.79

TABLE XLVI.—*For converting grains into grams.*

Grains.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
0	0	0.0648	0.1296	0.1944	0.2592	0.3240	0.3888	0.4536	0.5184	0.5832
10	0.6480	0.7128	0.7776	0.8424	0.9072	0.9720	1.0368	1.1016	1.1664	1.2312
20	1.2960	1.3608	1.4256	1.4904	1.5552	1.6200	1.6848	1.7496	1.8144	1.8892
30	1.9440	2.0088	2.0736	2.1384	2.2032	2.2680	2.3328	2.3976	2.4624	2.5272
40	2.5920	2.6568	2.7216	2.7864	2.8512	2.9160	2.9808	3.0456	3.1104	3.1752
50	3.2400	3.3048	3.3696	3.4344	3.4992	3.5640	3.6288	3.6936	3.7584	3.8232
60	3.8880	3.9528	4.0176	4.0824	4.1472	4.2120	4.2768	4.3416	4.4064	4.4712
70	4.5360	4.6008	4.6656	4.7304	4.7952	4.8600	4.9248	4.9896	5.0544	5.1192
80	5.1840	5.2488	5.3136	5.3784	5.4432	5.5080	5.5728	5.6376	5.7024	5.7672
90	5.8320	5.8968	5.9616	6.0264	6.0912	6.1560	6.2208	6.2856	6.3504	6.4152

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