FOR THE INCREASE AND DIFFUSION OF KNOWLEDGE AMONG MEN

PER ORBEM

SMITHSONIAN INSTITUTION WASHINGTON 1846

ANNUAL REPORT

OF THE

BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING

THE OPERATIONS, EXPENDITURES, AND CONDITION
OF THE INSTITUTION

TO

JULY, 1892.

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1893.

Fifty-second Congress, Second Session.

Concurrent resolution adopted by the Senate February 9, 1893, and by the House of Representatives February 15, 1893.

Resolved by the Senate (the House of Representatives concurring), That there be printed of the Reports of the Smithsonian Institution and of the National Museum for the year ending June 30, 1892, in two octavo volumes, 10,000 extra copies; of which 1,000 copies shall be for the use of the Senate, 2,000 copies for the use of the House of Representatives, 5,000 copies for the use of the Smithsonian Institution, and 2,000 copies for the use of the National Museum.

LETTER

FROM THE

SECRETARY OF THE SMITHSONIAN INSTITUTION,

ACCOMPANYING

The annual report of the Board of Regents of the Institution to the end of June, 1892.

SMITHSONIAN INSTITUTION,
Washington, D. C., July 1, 1892.

To the Congress of the United States:

In accordance with section 5593 of the Revised Statutes of the United States, I have the honor, in behalf of the Board of Regents, to submit to Congress the annual report of the operations, expenditures, and condition of the Smithsonian Institution for the year ending June 30, 1892.

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

S. P. LANGLEY,
Secretary of Smithsonian Institution.

Hon. LEVI P. MORTON,
President of the Senate.

Hon. CHARLES F. CRISP,
Speaker of the House of Representatives.

ANNUAL REPORT OF THE SMITHSONIAN INSTITUTION TO THE END OF JUNE, 1892.

SUBJECTS.

1. Proceedings of the Board of Regents for the session of January, 1892.

2. Report of the Executive Committee, exhibiting the financial affairs of the Institution, including a statement of the Smithson fund, and receipts and expenditures for the year 1891–'92.

3. Annual report of the Secretary, giving an account of the operations and condition of the Institution for the year 1891–'92, with statistics of exchanges, etc.

4. General appendix, comprising a selection of miscellaneous memoirs of interest to collaborators and correspondents of the Institution, teachers, and others engaged in the promotion of knowledge.

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THE SMITHSONIAN INSTITUTION.

MEMBERS EX OFFICIO OF THE "ESTABLISHMENT."

(January, 1892.)

BENJAMIN HARRISON, President of the United States.
LEVI P. MORTON, Vice-President of the United States.
MELVILLE W. FULLER, Chief-Justice of the United States.
JOHN W. FOSTER, Secretary of State.
CHARLES FOSTER, Secretary of the Treasury.
STEPHEN B. ELKINS, Secretary of War.
BENJAMIN F. TRACY, Secretary of the Navy.
JOHN WANAMAKER, Postmaster-General.
WILLIAM H. H. MILLER, Attorney-General.
WILLIAM E. SIMONDS, Commissioner of Patents.

REGENTS OF THE INSTITUTION.

(List given on the following page.)

OFFICERS OF THE INSTITUTION.

SAMUEL P. LANGLEY, *Secretary.*
Director of the Institution and of the U. S. National Museum

G. BROWN GOODE, *Assistant Secretary.*

REGENTS OF THE SMITHSONIAN INSTITUTION.

By the organizing act approved August 10, 1846 (Revised Statutes, Title LXXIII, section 5580), "The business of the Institution shall be conducted at the city of Washington by a Board of Regents, named the Regents of the Smithsonian Institution, to be composed of the Vice-President, the Chief-Justice of the United States [and the Governor of the District of Columbia], three members of the Senate, and three members of the House of Representatives, together with six other persons, other than members of Congress, two of whom shall be resident in the city of Washington and the other four shall be inhabitants of some State, but no two of the same State."

REGENTS FOR THE YEAR 1892.

The Chief-Justice of the United States:

MELVILLE W. FULLER, elected Chancellor and President of the Board January 9, 1889.

The Vice-President of the United States:

LEVI P. MORTON.

United States Senators: Term Expires

JUSTIN S. MORRILL (appointed Feb. 21, 1883, and Dec. 15, 1891). Mar. 3, 1897.
SHELBY M. CULLOM (appointed Mar. 23, 1885, and Mar. 28, 1889). Mar. 3, 1895.
RANDALL L. GIBSON (appointed Dec. 19, 1887, and Mar. 28, 1889). Mar. 3, 1895.

Members of the House of Representatives:

JOSEPH WHEELER (appointed Jan. 5, 1888, and Jan. 15, 1892)..Dec. 27, 1893.
HENRY CABOT LODGE (appointed January 15, 1892)...........Dec. 27, 1893.
W. C. P. BRECKINRIDGE (appointed January 15, 1892)...........Dec. 27, 1893.

Citizens of a State:

HENRY COPPÉE, of Pennsylvania (first appointed Jan. 19, 1874)..Jan. 26, 1898.
JAMES B. ANGELL, of Michigan (first appointed Jan. 19, 1887)...Jan. 19, 1893.
ANDREW D. WHITE, of New York (first appointed Feb. 15, 1888). Feb. 15, 1894.
WILLIAM P. JOHNSTON, of Louisiana (appointed Jan. 26, 1892)..Jan. 26, 1898.

Citizens of Washington:

JAMES C. WELLING (first appointed May 13, 1884)...............May 22, 1896.
JOHN B. HENDERSON (appointed January 26, 1892)...............Jan. 26, 1898.

Executive Committee of the Board of Regents.

JAMES C. WELLING, *Chairman.* HENRY COPPÉE. J. B. HENDERSON.

JOURNAL OF PROCEEDINGS OF THE BOARD OF REGENTS OF THE SMITHSONIAN INSTITUTION.

SPECIAL MEETING OF THE BOARD OF REGENTS.

OCTOBER 21, 1891.

Pursuant to a call by the Secretary, a special meeting of the Board of Regents was held at the Institution to-day at 10:30 A. M. Present: the Honorable Levi P. Morton, Vice-President of the United States; the Honorable S. M. Cullom, the Honorable R. L. Gibson, the Honorable B. Butterworth, Dr. A. D. White, Dr. J. C. Welling, Dr. Henry Coppée, Gen. M. C. Meigs, and the Secretary.

The Vice-President took the chair and called the meeting to order, and on Dr. Welling's suggestion, there being no objection, the reading of the minutes of the last annual meeting was dispensed with.

The Secretary then stated that he had, some months since, entered on a correspondence with Mr. Thomas G. Hodgkins, of Setauket, Long Island, and that Mr. Hodgkins had intimated his desire to give a considerable sum to the fund of the Smithsonian Institution "for the increase and diffusion of knowledge among men." Further correspondence led to visits to Mr. Hodgkins by the Secretary and by the Assistant Secretary, and to prolonged conferences with him, the result of which was that Mr. Hodgkins offered a donation of $200,000, concerning which the Secretary had telegraphed the Regents June 22, and upon receiving the individual approval of most of the Regents to the acceptance of the sum named, Mr. Hodgkins had later, on September 22, at his home on Long Island, given this amount in cash to the Secretary, who, in company with the Assistant Secretary, had brought it to Washington and deposited it in the Treasury of the United States, with the understanding that an early meeting of the Regents would be called as a body to consider as to its acceptance.

The exact terms in which Mr. Hodgkins made this gift would, the Secretary said, be stated later: but he gives $200,000 to the Smithsonian Institution to be added to the Smithson fund proper "for the increase and diffusion of knowledge among men," with the condition that the income of $100,000 of the gift shall be used, under this general purpose, for the especial one of the increase and diffusion of knowledge by investigating and spreading knowledge concerning all the phenomena of atmospheric air.

This meeting was, therefore, called in pursuance of this understanding, and also with regard to some matters concerning the Zoölogical Park.

Dr. Welling said that he had been instructed by his colleagues on the Executive Committee to bring the matter of this donation before the Regents in such a way that they can accept or reject the munificent gift made by Mr. Hodgkins. He then read the following preamble and resolutions:

Whereas, Thomas G. Hodgkins, of Setauket, Long Island, has placed in the hands of the Secretary of the Smithsonian Institution, the sum of two hundred thousand dollars, for the purpose declared by him in a formal statement, as follows:

SEPTEMBER 22, 1891.

I, Thomas G. Hodgkins, of Setauket, New York, desiring to increase the endowment of the Smithsonian Institution, founded in the city of Washington, for the increase and diffusion of knowledge among men, have transferred to Samuel Pierpont Langley, Secretary of the Smithsonian Institution, the sum of two hundred thousand dollars, the same to be delivered to the Board of Regents of the Smithsonian Institution, to whom I give it in trust, to be invested permanently in the Treasury of the United States, as a part of the Smithson fund, and its interest to be applied to the increase and diffuson of knowledge among men; this fund to be called the Hodgkins Fund, and all premiums, prizes, grants, or publications made at its cost, to be designated by this name; the interest of one hundred thousand dollars of this fund to be permanently devoted to the increase and diffusion of more exact knowledge in regard to the nature and properties of atmospheric air, in connection with the welfare of man in his daily life and in his relations to his Creator, the same to be effected by the offering of prizes, for which competition shall be open to the world, for essays in which important truths regarding the phenomena on which life, health, and human happiness depend shall be embodied, or by such other means as in years to come may appear to the Regents of the Smithsonian Institution calculated to produce the most beneficent results. * * *

(Signed) THOMAS G. HODGKINS.

Witness:

(Signed) M. L. CHAMBERS.

Therefore, be it

Resolved, That the Regents hereby accept the sum in question, subject to the conditions thus stated by the donor, and that the Secretary is instructed to carry into effect these conditions, and to administer the income as in the case of the income from other funds belonging to the Institution.

Resolved, That the Secretary is instructed to place the sum of $200,000 in the U. S. Treasury, at six per centum interest, under the terms of section 5591, of Title LXXIII, of the Revised Statutes of the United States.

* * * * * * *

Resolved, That the thanks of the Board of Regents are tendered to Mr. Hodgkins for his generous and public-spirited donation, and that an engrossed copy of the above preamble and resolutions be transmitted to him by the Secretary.

In answer to a question as to whether this was an absolute gift to the Institution, the Secretary said that Mr. Hodgkins thoroughly understood that this gift was subordinate to the general title of the Smithsonian fund, though it was to bear his own name as a sub-title.

Senator Cullom addressed the meeting at length, quoting frequently from the Revised Statutes, arguing in favor of accepting the gift with its conditions, and concluding his remarks with a motion that the resolutions be adopted.

The Chairman having put the question, the resolutions were unanimously adopted.

The Secretary then brought before the Regents the difficulties under which he was laboring from the insufficient appropriations for the National Zoölogical Park, and after a full discussion of the special difficulties of the situation belonging to a novel undertaking, where no one could say beforehand what appropriation would certainly be required under each item, but where limited appropriations are nevertheless made in unchangeable specific items, unsupplemented by discretionary power, the following preamble and resolution were adopted:

Whereas, the National Zoölogical Park has been placed under the direction of this Board, under legislative conditions quite other than those contemplated at the time that the responsibility of its administration was accepted by it:

Resolved, That the Secretary is authorized and instructed to represent to the proper committees of Congress the difficulties which these conditions impose upon the administration of the Institution, and to advise such legislation as may do away with the present system by which half of the expense of said park is paid from the revenues of the District of Columbia; and also to advise such changes in the form of future appropriation bills as may be requisite to do away with the especially imposed difficulties which are now encountered in carrying on the work.

Adjourned.

ANNUAL MEETING OF THE BOARD OF REGENTS.

JANUARY 27, 1892.

The annual meeting of the Board of Regents of the Smithsonian Institution was held to-day at 10 A. M. Present: Mr. Chief Justice Fuller, Vice-President Morton, the Hon. J. S. Morrill, the Hon. S. M. Cullom, the Hon. R. L. Gibson, the Hon. Joseph Wheeler, the Hon. W. C. P. Breckinridge, Dr. Henry Coppée, Dr. J. B. Angell, Dr. William Preston Johnston, the Hon. J. B. Henderson, and the Secretary.

Excuses for non-attendance were read from Dr. J. C. Welling, caused by illness, and from Dr. A. D. White, by important engagements.

The Chancellor stated that the minutes of the annual meeting of January 28, 1891, and of the special meeting of October 21, 1891, were of considerable length, and the Secretary was requested to read them in abstract, which was done.

The Secretary announced that the Vice-President on December 15, 1891, re-appointed as Regent the Hon. J. S. Morrill, a United States Senator: that the Speaker of the House had re-appointed Representatives Joseph Wheeler, of Alabama; Henry Cabot Lodge, of Massachusetts, and appointed Representative W. C. P. Breckinridge, of Kentucky, and that further vacancies in the Board had been filled by the re-appointment, by joint resolution approved by the President, January 26, 1892, of Henry Coppée, of Pennsylvania, and by the appointment of William Preston Johnston, of Louisiana, and John B. Henderson, of the District of Columbia.

The Secretary announced the death of Gen. M. C. Meigs, a Regent at large, on January 2, 1892.

Dr. Coppée moved that a committee, to consist of one member of the Board and the Secretary, be appointed to present to this meeting an obituary notice of the late Gen. Meigs. The motion was carried, and the Chancellor nominated Dr. Coppée to act with the Secretary. Dr. Coppée, after expressing his regret at the illness of the chairman of the Executive Committee, and his personal sorrow at the death of his colleague on the committee, Gen. Meigs, read the following memorial resolution:

MEMORIAL RECORD OF GEN. M. C. MEIGS.

The Board of Regents of the Smithsonian Institution desires to place on record the expression of its sincere sorrow and its sense of the great loss it has suffered in the death of Gen. Montgomery Cunningham Meigs, a member of the Board and one of its Executive Committee. His valuable services to the Institution began indeed before he was officially connected with it as a regent and continued until his death.

While Gen. Meigs was prominently associated with many useful undertakings, his record as a soldier and as a citizen is marked by unswerving fidelity and extraordinary capability. The principal events of his life can only be briefly mentioned, as showing what varied experience he placed at the service of the Institution.

He was born on the 3d of May, 1816, at Augusta, Ga., where his father, Charles D. Meigs, afterwards the eminent physician and author of Philadelphia, was then practicing medicine. After preliminary studies at the University of Pennsylvania, he entered the Military Academy at West Point on the 1st of July, 1832, and was graduated with distinction in 1836. He was at once appointed to a position in the artillery service, and in the following year was transferred to the Corps of Engineers. In 1849 he was engaged in the Engineer Bureau at Washington, and from that time until the outbreak of the civil war his activity was principally directed to the construction of Government works. Toward the close of 1852 he made a survey at Washington to determine the best plan for supplying the city with water. He was eventually placed in charge of the work, which included the designing and construction of the Potomac acqueduct. This remarkable work contains a single arch of 220 feet span, which still remains the largest stone arch hitherto constructed.

He also had charge, as supervising engineer, of the north and south extensions of the National Capitol and of the construction of the iron dome, as well as of the northward extension of the General Post-Office building.

When the war broke out he was appointed colonel of the Eleventh Infantry (May 14, 1861) and afterwards quartermaster-general of the U. S. Army, with the rank of brigadier-general. This post required unusual administrative ability, with a probity which commanded general recognition, and it was because of his high integrity and the strength of his personal character, as well as his acknowledged capacity for business, that he was entrusted with the handling and use of hundreds of millions of dollars in the greatest war ever waged.

This is not the place to recount his military services. They were numerous and admirably discharged. His duties took him to all parts of the country, connected him with many fields of labor, and engaged him on the most varied commissions. Suffice it to say that he fully justified the confidence imposed in him by President Lincoln, performing with signal ability the duties entrusted to him. In 1864 he received the well earned title of brevet major-general in the Army.

Even during the period of his service in the Army he was engaged in other occupations; rendering the Smithsonian Institution most important service in 1876 by devising the new building for the National Museum, a marvel of economic design.

While still full of vigor Gen. Meigs was retired from active service on the 6th of February, 1882, by the inexorable law which makes the grand climacteric the period when military inaction begins. But he was by no means idle. He signalized his talent as an architect by the construction of the Pension-Office building at Washington between the years 1882 and 1887.

He was elected a fellow of the National Academy of Sciences in 1865, and a regent of the Smithsonian Institution, as a "citizen of Washington," and directly upon his entrance into the board, December 26, 1885, became an active member of its Executive Committee. He was always present, extremely painstaking, and eminently judicious in his counsel and judgment on important points of business and policy. He had just been nominated as regent for another term of six years when he was taken away from us by sudden illness (January 2, 1892).

He was eminent as a soldier, as a scientific investigator, as a public-spirited citizen, and as a man. Industrious and exact in business, he knew no idle time. He was a busy man even when he spent a year in Europe for his health in 1867 and 1868, as well as on his visit there in 1875 on Government service.

Few regents have been of such importance to the Institution as Gen. Meigs, and it is fitting that we should record our tribute of thankfulness for his eminent services and our great sorrow at his loss. He was a man faithful in all things, who has left behind him an enduring reputation.

Senator Gibson moved the adoption of the memorial and that a copy thereof should be sent to the family of Gen. Meigs, which was carried.

three items: For buildings, improvements, and maintenance. While all were insufficient, that for maintenance (which was essentially for the care and food of living animals) was peculiarly inadequate, since it left him unable to care for creatures who could not care for themselves, and ought not to be allowed to suffer. This item, then, was notably different in kind from those providing for buildings or roads, which might be left incomplete with less immediate damage or only pecuniary loss.

Senator Morrill expressed his regret at the deplorable insufficiency of the appropriations for the park, and at the necessity of contemplating the sundering of the park from the Institution, but he was of the opinion that such a separation would become desirable unless some change was made. He thought it out of the question that the matter should continue on the present footing, and the Smithsonian ought not again to be put under the necessity of caring for any part of the park out of its private funds, even temporarily and indirectly.

Further remarks were made by Mr. Breckinridge and Mr. Wheeler.

With reference to the administration of the Institution, the Secretary recalled that the Assistant Secretary has, as such, no power to act in the Secretary's place, such as the Assistant Secretary in any Executive Department possesses, and that he can not even execute such routine signatures of necessary vouchers and like papers as in Executive Departments the law authorizes, not only him, but his subordinates to do.

Apart from the important administrative duties assigned to the Secretary, there present themselves daily a great many vouchers and like routine papers for the Treasury from the different bureaus under his charge—papers which, as has just been stated, would in every bureau of any Executive Department of the Government be signed by a subordinate officer; while here the Secretary or Acting Secretary must personally sign such routine money papers, under a custom which has grown step by step from small beginnings to be a hardly tolerable burden in the illness or absence either of the Secretary or of the Acting Secretary, while for their joint illness no provision is made whatever. To meet in part the difficulties arising from the necessity of delegating authority for signing vouchers and like Treasury papers, it was stated that by proper action of the Board of Regents all requirements of the Treasury Department might be met.

No similar difficulty exists in any Executive Department, because in all such the law provides not only for the Secretary and Acting Secretary, but for a line of succession of subordinate officers authorized to execute such acts as the daily conduct of their respective bureaus renders necessary.

The Secretary pointed out that, owing to the established principles of conduct in the Smithsonian Institution (which there was no intention here of departing from), the Secretary's power had never been diffused and delegated as was the case in the Executive Departments of the Government, where there were several persons in every separate bureau who had a right, in case of the absence not only of the Secretary and Acting Secretary, but of the head of the bureau itself, to carry on its affairs, and especially to sign such money papers as were required for its current business with the Treasury. There was no time, however, in the past twelve years, when, in the joint event of the illness of the Secretary and the Acting Secretary, there was any such provision for carrying on the current business of the Institution. The Secretary further pointed out that since the provision for an Acting Secretary was first made in 1879, he had made a computation of the amount of business coming before the Secretary then and now, which shows that the work is at present from eight to ten times that when the first legislation for an Acting Secretary was asked for.

Dr. Coppée said that owing to his long connection with the Institution—perhaps the longest of any member present, with the possible exception of Senator Morrill—he felt particularly in a position to corroborate the statements made by the Secretary as to the growth of the business of the Institution since the passage of the act relating to the appointment of an Acting Secretary, and he thought the best manner of effecting this immediate relief to the Secretary was covered by the following resolution:

Resolved, That the Secretary be empowered to appoint some suitable person who, in case of need, may sign such requisitions, vouchers, abstracts of vouchers, accounts current, and indorsements of checks and drafts as are needed in the current business of the Institution or of any of its bureaus, and are customarily signed in the bureaus of other departments of the Government.

He added that as this came before the Board at a late hour he would move, in order to give time for its consideration, that the whole matter be put in the hands of a committee appointed by the Chancellor with power to act.

The Chancellor stated that undoubtedly the increased growth of the Institution had introduced new demands, and that it was desirable that the action in reference to them should be carefully studied.

After further remarks by Mr. Breckinridge and Mr. Henderson and other members of the Board, Dr. Coppée said that he thought the action of the Executive Committee could cover the ground of the resolution, and, on motion of the Vice-President, the whole subject was referred to the Executive Committee with power to act on the resolution.

The Secretary said, in connection with what had just been done, that the increased burdens of extraneous duties imposed by Congress were accompanied by special expenses for administering appropriations for which no legislative provision was made, and which necessarily fell on the limited Smithsonian fund, partly in indirect ways. There was no provision, for instance, for a disbursing officer, or private secretary, or stenographers, or clerks, or messengers to attend to the administrative duties common to all the bureaus under the Regents' care.

Dr. Coppée offered the following resolution, at the same time calling the attention of the Board that it referred to public funds only:

Resolved, That the Secretary be instructed to ask for an appropriation by Congress to meet the miscellaneous expenses incident to the administration of the public funds with which the Regents are intrusted.

On motion the resolution was adopted.

There being no further business before the meeting the Board adjourned.

SPECIAL MEETING OF THE BOARD OF REGENTS.

MARCH 29, 1892.

A special meeting of the Board of Regents was held to-day at a quarter before 10 o'clock A. M. Present: The Chancellor—Mr. Chief-Justice Fuller, in the Chair; the Hon. Levi P. Morton, Vice-President; the Hon. S. M. Cullom; the Hon. R. L. Gibson; the Hon. Joseph Wheeler; the Hon. H. C. Lodge; the Hon. W. C. P. Breckinridge; Dr. J. C. Welling, and the Secretary.

The reading of the minutes of the last meeting was dispensed with, and the Secretary read a telegram from Dr. Coppée, expressing his regret at his inability to be present.

The Secretary stated that the meeting had been called at the request of three of the Regents chiefly on account of the action of the Appropriations Committee of the House of Representatives—a matter in which the good name of the Institution was in some measure involved, —whereby the appropriations for various Government interests under the charge of the Regents had been reduced to such an extent that the prosperity of all these departments would receive a blow from which they could not hope to recover for years to come.

Especial stress was laid upon the inadequacy of the appropriations for the National Zoölogical Park and attention was also called to the fact that the park is already visited on fair days by thousands not only of adults but of children, while dangerous animals are there without sufficient buildings or cages or inclosures, and without means to provide them, and that the only protection of the public and especially of children must be from incessant guardianship, which the present small and overworked force is unable to properly render.

The Secretary stated that he was unable to carry on the park with less expenditure for maintenance than $26,000, or with a less total appropriation than $50,000, in case it were made in one item.

The following resolutions were introduced by Mr. Wheeler:

Resolved, That the Board of Regents of the Smithsonian Institution would respectfully represent to Congress the impossibility of maintaining the administration of the United States National Zoölogical Park, required by the act of Congress of April 30, 1890, with a less appropriation for maintenance than $26,000, or with a less total appropriation than $50,000.

Resolved, That the Secretary of the Institution be requested to communicate this resolution to the President of the Senate and Speaker of the House of Representatives, with a preliminary statement of the reasons and considerations on which it is based.

After some further discussion, the resolutions were adopted, with the understanding that such limited modification of the wording might be made as to meet any technicality suggested by the Treasury Department.

There was a general expression of opinion among the Regents that the condition of the affairs of the park should be brought to the attention of Congress by explanation on the floor of the House and Senate from Regents and friends of the Institution.

Further remarks on the matter were made by Mr. Lodge and Dr. Welling.

The Secretary then read a communication from Mr. Thomas G. Hodgkins, dated March 10, 1892, in which Mr. Hodgkins stated that he desired to relinquish the option of contributing the further sum of $100,000 to the Smithsonian fund.

There being no further business before the Board, the meeting adjourned.

REPORT OF THE EXECUTIVE COMMITTEE OF THE BOARD OF REGENTS OF THE SMITHSONIAN INSTITUTION.

FOR THE YEAR ENDING 30TH OF JUNE, 1892.

To the Board of Regents of the Smithsonian Institution:

Your Executive Committee respectfully submits the following report in relation to the funds of the Institution, the appropriations by Congress, and the receipts and expenditures for the Smithsonian Institution, the U. S. National Museum, the International Exchanges, the Bureau of Ethnology, the National Zoölogical Park, and the Astro-Physical Observatory, for the year ending 30th June, 1892, and balances of former years:

SMITHSONIAN INSTITUTION.

Condition of the fund July 1, 1892.

The amount of the bequest of James Smithson deposited in the Treasury of the United States, according to act of Congress of August 10, 1846, was $515,169. To this was added by authority of Congress, February 8, 1867, the residuary legacy of Smithson, savings from income and other sources, to the amount of $134,831.

To this also have been added—a bequest from James Hamilton, of Pennsylvania, of $1,000; a bequest of Dr. Simeon Habel, of New York, of $500; the proceeds of the sale of Virginia bonds, $51,500; and a gift from Thomas G. Hodgkins, of New York, of $200,000, making in all, as the permanent fund, $903,000.

Statement of the receipts and expenditures from July 1, 1891, to June 30, 1892.

RECEIPTS.

Cash on hand July 1, 1891		$10,062.11	
Interest on fund July 1, 1891	$21,090.00		
Interest on fund January 1, 1892	23,391.36		
		44,481.36	
Cash from Thomas G. Hodgkins		200,000.00	
			$284,543.47
Cash from sales of publications		378.24	
Cash from repayment of freight, etc		2,595.99	
			2,974.23
Total receipts			287,517.70

EXPENDITURES.

Building:			
Repairs, care, and improvements	$1,892.23		
Furniture and fixtures	855.89		
		$2,748.12	
General expenses:			
Meetings	558.50		
Postage and telegraph	243.50		
Stationery	486.37		
General printing	284.55		
Incidentals (fuel, gas, etc.)	2,209.19		
Library (books, periodicals, etc.)	1,234,52		
Salaries *	16,276.85		
		21,293.48	
Publications and researches:			
Smithsonian contributions	6,067.43		
Miscellaneous collections	855.55		
Reports	429.04		
Researches	2,031.90		
Apparatus	1,625.61		
Explorations	10.75		
Museum	1,270.00		
		12,290.28	
Literary and scientific exchanges		3,310.49	
Increase of fund		200,000.00	
Total expenditures (including $200,000 deposited in the U. S. Treasury October 22, 1891, to the credit of the permanent fund)			$239,642.37
Balance unexpended June 30, 1892			47,875.33

The cash received from sales of publications, repayments for freights, etc., is to be credited on items of expenditures as follows:

Postage and telegraph		$2.02
Stationery		4.25
General printing		2.10
Incidentals		3.57
Smithsonian contributions	$126.41	
Miscellaneous collections	196.18	
Reports	55.65	
		378.24
Apparatus		4.00
Museum		320.00
Researches		120.86
Exchanges		2,139.19
Total		2,974.23

The net expenditures of the Institution for the year ending June 30, 1892, was therefore $236,688.14, or $2,974.23 less than the gross expenditures, $239,642.37, above given. From the net expenditures, $236,688.14, there should be deducted $200,000, the amount deposited in the U. S. Treasury to the credit of the permanent fund, making the

* In addition to the above $16.276.85 paid for salaries under general expenses, $4,874.44 were paid for services, viz: $236.34 from apparatus account, $1,500 from building account, $306.28 from library account, $1,431.90 from researches account, and $1,399.92 from Smithsonian contributions account.

net expenditures for the expenses and operations of the Institution for the year ending June 30, 1892, $36,688.14.

All moneys received by the Smithsonian Institution from interest, sales, refunding of moneys temporarily advanced, or otherwise are deposited with the Treasurer of the United States to the credit of the Secretary of the Institution, and all payments are made by his checks on the Treasurer of the United States.

Your committee also presents the following statements in regard to appropriations and expenditures for objects intrusted by Congress to the care of the Smithsonian Institution:

INTERNATIONAL EXCHANGES.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1892, "for expenses of the system of international exchanges between the United States and foreign countries, under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employés" (sundry civil act, March 3, 1891) $17,000.00

Expenditures from July 1, 1891, to June 30, 1892.

Salaries or compensation:*		
1 curator, 3 months, at $208.33, $624.99; 9 months, at $225, $2,025	$2,619.99	
1 clerk, 12 months, at $160	1,920.00	
1 clerk, 12 months, at $120	1,440.00	
1 clerk, 12 months, at $85	1,020.00	
1 clerk, 12 months, at $80	960.00	
1 clerk, 12 months, at $75	900.00	
1 clerk, 12 months, at $75	900.00	
1 clerk, 12 months, at $65	780.00	
1 stenographer, 12 months, at $45	540.00	
1 clerk, 7 months, at $45, $315; 1 copyist, 5 months, at $40, $200	515.00	
1 copyist, 8 days, at $40, $10.32; 11 months, at $40, $440	450.32	
1 packer, 12 months, at $75	900.00	
1 packer, 12 months, at $50	600.00	
1 packer, 3 days, at $1.50	4.50	
1 laborer, 5 months, at $50	250.00	
1 laborer, 7 months, at $35	245.00	
Total salaries or compensation		14,074.81
General expenses:		
Freight	$1,772.01	
Packing boxes	563.05	
Printing and binding	103.00	
Postage	164.17	
Stationery and supplies	322.96	
		2,925.19
Total expenditures for international exchanges		17,000.00

*Note.—The payments of salaries for parts of months in January, March, July, August, October, and December are made on the basis of 31 days, and for the other months (except February) at 30 days.

NORTH AMERICAN ETHNOLOGY.

Appropriation by Congress for the fiscal year ending June 30, 1892, "for continuing ethnological researches among the American Indians under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employés" sundry civil act, March 3, 1891 ..	$50,000.00
Balance July 1, 1891, as per last annual report	12,774.24
Total	62,774.24

The actual conduct of these investigations has been continued by the Secretary in the hands of Maj. J. W. Powell, Director of the U. S. Geological Survey.

Expenditures July 1, 1891, to June 30, 1892.

Salaries or compensation:		
1 ethnologist, at $3,000 per annum, 11 months	$2,750.00	
1 ethnologist, at $3,000 per annum	3,000.00	
2 ethnologists, at $2,400 per annum	4,800.00	
1 ethnologist, at $2,000 per annum	1,999.92	
1 ethnologist, at $1,800 per annum	1,800.00	
1 archaeologist, at $2,600 per annum	2,599.92	
1 assistant archaeologist, at $1,200 per annum	1,200.00	
1 assistant archaeologist, at $1,500 per annum	1,500.00	
1 assistant ethnologist, at $1,800 per annum, 1 month	150.00	
1 assistant ethnologist, at $1,800 per annum, 2 months	300.00	
1 assistant ethnologist, at $1,600 per annum, 10 months	1,333.30	
1 assistant ethnologist, at $1,800 per annum, 5 months	750.00	
1 assistant ethnologist, at $1,400 per annum, 9 months	1,049.94	
1 assistant ethnologist, at $1,200 per annum, 11 months	1,100.00	
1 assistant ethnologist, at $900 per annum	900.00	
1 assistant ethnologist, at $900 per annum, 2 months and 15 days	187.50	
1 assistant ethnologist, at $600 per annum, 2 months and 7½ days	115.00	
1 stenographer, at $1,500 per annum	1,500.00	
1 clerk, at $1,200 per annum, 11 months	1,100.00	
1 clerk, at $1,200 per annum	1,200.00	
2 clerks, at $720 per annum	1,440.00	
1 copyist, at $1,000 per annum	999.96	
1 copyist, at $840 per annum	840.00	
1 copyist, at $600 per annum, 1 month	50.00	
1 modeller, at $720 per annum	720.00	
1 modeller, at $720 per annum, 5 months and 21 days	340.65	
1 messenger, at $600 per annum, 9 months	450.00	
1 messenger, at $600 per annum, 2 months and 23 days	138.33	
1 laborer, at $600 per annum, 7 months	350.00	
1 laborer, at $600 per annum, 5 months and 28 days	295.16	
Unclassified or special jobs, etc	1,600.65	
Total salaries or compensation	36,560.33	
Miscellaneous:		
Travelling expenses	$3,660.05	
Transportation	963.69	
Field subsistence	719.20	
Field expenses	1,675.25	

Miscellaneous—Continued.			
Field material	$166.19		
Freight	380.55		
Supplies	1,867.98		
Stationery	80.38		
Office furniture	138.25		
Publications	566.63		
Drawings	908.77		
Laboratory supplies	27.80		
Repairs	51.11		
		$11,205.85	
			$47,766.18
Balance July 1, 1892			15,008.06

Expenditures re-classified by subject-matter:	
Sign language and picture-writing	4,732.40
Explorations of mounds	4,342.13
Researches in archæology	14,561.15
Researches, language of North American Indians	14,660.21
Salaries in office of Director	3,678.29
Illustrations for reports	1,388.21
Researches among Pueblos	2,560.20
Contingent expenses	1,673.52
	47,596.11
Bonded railroad accounts settled by Treasury	170.07
Total expenditures, North American ethnology	47,766.18
Balance July 1, 1892	15,008.06

Summary.

July 1, 1891. Balance on hand	12,774.21
Appropriation for North American ethnology	50,000.00
	62,774.21
Expended	47,766.18
Balance July 1, 1892	15,008.06

NATIONAL MUSEUM.

Preservation of collections, July 1, 1891, to June 30, 1892.

Receipts:

Appropriation by Congress for the fiscal year ending June 30, 1892, "for continuing the preservation, exhibition, and increase of the collections from the surveying and exploring expeditions of the Government, and from other sources, including salaries or compensation of all necessary employés" (sundry civil act, March 3, 1891) $145,000.00

Expenditures.

Salaries or compensation:

DIRECTION.

1 Assistant Secretary of the Smithsonian Institution, in charge of U. S. National Museum, 12 months, at	$333.33	3,999.96

Salaries or compensation—Continued.

SCIENTIFIC STAFF.

1 curator, 6 months, at $225; 6 months, at $200	$2,550.00	
1 curator, 12 months, at $200	2,400.00	
1 curator, 12 months, at $200	2,400.00	
1 curator, 7 months, at $200; 5 months, at $175	2,275.00	
1 curator, 12 months, at $175	2,100.00	
1 curator, 12 months, at $150	1,800.00	
1 curator, 11 months, at $100	1,100.00	
		$14,625.00
1 acting curator, 6 months, at $140; 6 months, at $125		1,590.00
1 assistant curator, 11 months, at $166.66; 1 month, at 181.66	2,014.92	
1 assistant curator, 12 months, at $140	1,680.00	
1 assistant curator, 12 months, at $133.33	1,599.96	
1 assistant curator, 12 months, at $100	1,200.00	
1 assistant curator, 1 month, at $125; 3 months, at $50	275.00	
		6,769.88
1 assistant, 1 month, at $100	100.00	
1 assistant, 1 month and 20 days, at $85	141.67	
1 assistant, 5 months, at $65	325.00	
1 assistant, 2 months, at $65	130.00	
1 assistant, 11 months, at $80	880.00	
		1,576.67
1 aid, 12 months, at $100	1,200.00	
1 aid, 12 months, at $80	960.00	
1 aid, 11 months 15 days, at 83.33	958.00	
1 aid, 3 months, at $80	240.00	
1 aid, 12 months, at $60	720.00	
1 aid, 8 months, at $50	400.00	
1 aid, 10 months at $60; 1 month, at $40	640.00	
1 aid, 2 months, at $50	100.00	
1 aid, 12 months, at $40	480.00	
1 aid, 4 months, at $46; 1 month, at 44.50; 7 months, at $40	508.50	
		6,206.80
1 special agent, 29 days, at $6		171.00
1 collector, 9 months, at $140	1,260.00	
1 collector, 9 months, at $50	450.00	
		1,710.00
		32,652.35

CLERICAL STAFF.

1 chief clerk, 12 months, at $187.50	2,250.00
1 corresponding clerk, 12 months, at $175	2,100.00
1 registrar, 12 months, at $158.33	1,899.96
1 disbursing clerk, 12 months, at $100	1,200.00
1 assistant librarian, 12 months, at $100	1,200.00
1 stenographer, 11 months, at $60; 1 month, at $85	745.00
1 draftsman, 7 months, 15 days, at $83.33	626.41
1 assistant draftsman, 5 months, at $40	200.00
1 clerk, 12 months, at $125	1,500.00
1 clerk, 4 months and 15 days, at $125	665.32
1 clerk, 12 months, at $115	1,380.00
1 clerk, 12 months, at $115	1,380.00
1 clerk, 12 months, at $100	1,200.00
1 clerk, 12 months, at $100	1,200.00

Salaries or compensation—Continued.

1 clerk, 12 months, at $90	$1,080.00	
1 clerk, 12 months, at $83.33	999.96	
1 clerk, 6 months 15 days, at $80	520.00	
1 clerk, 12 months, at $75	900.00	
1 clerk, 12 months, at $70	840.00	
1 clerk, 12 months, at $60	720.00	
1 clerk, 11 months 24 days, at $60	706.45	
1 clerk, 11 months 1 day, at $60	661.91	
1 clerk, 12 months, at $60	720.00	
1 clerk, 12 months, at $60	720.00	
1 clerk, 11 months, at $60	660.00	
1 clerk, 12 months, at $55	660.00	
1 clerk, 12 months, at $55	660.00	
1 clerk, 12 months, at $55	660.00	
1 clerk, 12 months, at $55	660.00	
1 clerk, 12 months, at $50	600.00	
1 clerk, 12 months, at $50	600.00	
1 clerk, 11 months 7 days, at $50	561.29	
1 clerk, 3 months 9 days, at $50	165.00	
1 copyist, 12 months, at $55	660.00	
1 copyist, 12 months, at $50	600.00	
1 copyist, 12 months, at $50	600.00	
1 copyist, 12 months, at $50	600.00	
1 copyist, 3 months, at $50	150.00	
1 copyist, 8 months 15 days, at $45	381.77	
1 copyist, 12 months, at $40	480.00	
1 copyist, 12 months, at $40	480.00	
1 copyist, 12 months, at $40	480.00	
1 copyist, 12 months, at $40	480.00	
1 copyist, 12 months, at $40	480.00	
1 copyist, 12 months, at $35	420.00	
1 copyist, 3 months 16 days, at $35	123.06	
1 copyist, 12 months, at $35	420.00	
1 copyist, 6 months, at $30	180.00	
1 copyist, 12 months, at $30	360.00	
1 copyist, 12 months, at $30	360.00	
1 copyist, 56 days, at $1.50 per day	84.00	
1 type-writer, 12 months, at $50	600.00	
		$38,580.16

PREPARATORS.

1 preparator, 10 months, at $100	1,000.00	
1 preparator, 12 months, at $80	960.00	
1 preparator, 12 months, at $60	720.00	
1 preparator, 6 months, at $60, $360; 30 days, at $60 per month, $58.06; 28½ days, at $60 per month, $58.97; 30 days, at $60 per month, $58.06	535.00	
1 preparator, 1 month	75.00	
1 preparator, 24 days, at $3.20	76.80	
1 artist, 12 months, at $110	1,320.00	
1 photographer, 9 months, at $158.33, $1,424.97; 15 days, at $158.33 per month, $76.61; 16 days, at $158.33 per month, $81.72	1,583.30	
1 taxidermist, 12 months, at $60	720.00	
1 taxidermist, 12 months, at $125	1,500.00	

Salaries or compensation—Continued.

1 taxidermist, 12 months, at $120	$1,440.00	
1 taxidermist, 1 month, $80; 2,280 hours, at 45 cents	1,106.00	
1 assistant taxidermist, 19 days, at $60 per month	36.77	
		$11,072.96

BUILDINGS AND LABOR.

1 superintendent, 12 months, at $137.50	1,650.00	
1 assistant superintendent, 12 months, at $90	1,080.00	
1 chief of watch, 12 months, at $65	780.00	
1 chief of watch, 12 months, at $65	780.00	
1 chief of watch, 7 months, at $65	450.00	
1 watchman, 12 months, at $65	780.00	
12 watchmen, 12 months, at $50	7,200.00	
1 watchman, 9 months, at $50, $450; 29 days, at $50 per month, $48.33; 29 days, at $50 per month, $48.34; 30 days, at $50 per month, $48.39	595.06	
1 watchman, 3 months 26 days, at $50	191.94	
1 watchman, 9 months 24 days, at $45	441.00	
1 watchman, 8 months 17 days, at $45	384.68	
1 watchman, 3 months, at $45	135.00	
1 watchman, 10 months 29 days, at $45	492.10	
1 watchman, 10 months, at $45, $450; 30 days, at $45 per month, $43.55; 30 days, at $45 per month, $43.55	537.10	
1 skilled laborer, 12 months, at $52	624.00	
1 skilled laborer, 12 months, at $50	600.00	
1 skilled laborer, 1 month 16 days, at $50	75.81	
1 skilled laborer, 19 days, at $45 per month	27.58	
1 skilled laborer, 27 days, at $2	54.00	
1 skilled laborer, 21 days, at $1.50	31.50	
1 laborer, 8 months, at $46; 3 months, at $47.50; 1 month, at $44.50	555.00	
1 laborer, 2 months, at $51; 1 month, at $52.50; 1 month, at $49.50; 5 months, at $45; 3 months, at $48	573.00	
1 laborer, 4 days, at $1.29; 39 days, at $1.25; 240 days, at $1.50	413.91	
1 laborer, 10 months, at $40; 1 month, at $43; 1 month, at $41.50	484.50	
1 laborer, 12 months, at $40	480.00	
1 laborer, 12 months, at $40	480.00	
1 laborer, 12 months, at $40	480.00	
1 laborer, 12 months, at $40	480.00	
1 laborer, 19 days, at $40 per month	24.52	
1 laborer, 298 days, at $1.50	447.00	
1 laborer, 303½ days, at $1.50	455.25	
1 laborer, 51 days, at $1.50	76.50	
1 laborer, 281 days, at $1.50	421.50	
1 laborer, 297 days, at $1.50	445.50	
1 laborer, 287½ days, at $1.50	431.25	
1 laborer, 11 days, at $1.50	16.50	
1 laborer, 32 days, at $1.50	48.00	
1 laborer, 78 days, at $1.50	117.00	
1 laborer, 321 days, at $1.50	481.50	
1 laborer, 32 days, at $1.50	48.00	
1 laborer, 305¾ days, at $1.50	460.38	
1 laborer, 114 days, at $1.50	171.00	

Salaries or compensation—Continued.

1 laborer, 324½ days, at $1.50	$536.25	
1 laborer, 327 days, at $1.50	190.50	
1 laborer, 283 days, at $1.50	121.50	
1 laborer, 152 days, at $1.50	228.00	
1 laborer, 298 days, at $1.50	117.00	
1 laborer, 13 days, at $1.50	19.50	
1 laborer, 65 days, at $1.50	97.50	
1 laborer, 11 months 28 days, at $1.50	476.13	
1 laborer, 291½ days, at $1.50	183.75	
1 laborer, 5 days, at $1.50	7.50	
1 laborer, 24 days, at $1.25	30.00	
1 laborer, 2 months, at $20; 24 days, at $1	64.00	
1 attendant, 10 months, at $40, $400; 30 days, at $40 per month, $38.71; 30 days, at $40 per month, $38.71	477.42	
1 attendant, 10 months, at $40, $400; 28 days, at $40 per month, $37.33; 29 days, at $40 per month, $38.67	476.00	
1 cleaner, 9 months, at $30, $270; 30 days, at $30 per month, $29.03; 29½ days, at $30 per month, $29.48; 30½ days, at $30 per month, $29.52	358.03	
1 cleaner, 12 months, at $30	360.00	
1 cleaner, 10 months, at $30, $300; 30½ days, at $30 per month, $29.52; 30 days, at $30 per month, $29.03	358.55	
1 cleaner, 12 months, at $30	360.00	
1 cleaner, 314 days, at $1	314.00	
1 cleaner, 314 days, at $1	314.00	
1 messenger, 12 months, at $45	540.00	
1 messenger, 6 months, at $45; 6 months, at $50	570.00	
1 messenger, 12 months, at $30	360.00	
1 messenger, 11 months, at $30; 1 month, at $31.50	361.50	
1 messenger, 6 months, at $25	150.00	
1 messenger, 12 months, at $20	240.00	
1 messenger, 11 months 26 days, at $20	236.77	
1 messenger, 8 months, at $25, $200; 26 days, at $25 per month, $20.16; 20 days, at $25 per month, $16.67	236.83	
1 messenger, 5 months, at $15	88.55	
		$33,606.36
Special services by job or contract		2,839.64
Total services		122,751.43

Summary—Preservation of collections, 1892.

Direction	$3,999.96
Scientific staff	32,652.35
Clerical staff	38,580.16
Preparators	11,072.96
Building and labor	33,606.36
Special or contract work	2,839.64
Total salaries or compensation	122,751.43

Miscellaneous:

Supplies	$2,038.76
Stationery	812.79
Specimens	6,340.12
Books and periodicals	153.00

Miscellaneous—Continued.

Travel	$1,574.81	
Freight and cartage	2,180.95	
		$13,430.43
Total expenditure to June 30, 1892, for preservation of collections, 1892		136,181.86
Balance July 1, 1892, to meet outstanding liabilities		8,818.14

National Museum—Furniture and fixtures, July 1, 1891, to June 30, 1892.

RECEIPTS.

Appropriation by Congress for the fiscal year ending June 30, 1892, "for cases, furniture, fixtures, and appliances required for the exhibition and safe-keeping of the collections of the National Museum, including salaries or compensation of all necessary employés" (sundry civil act, March 3, 1891)	$25,000.00

EXPENDITURES.

Salaries or compensation:

1 engineer of property, 9 months, at $175		$1,575.00
1 carpenter, 127 days, at $3		381.00
1 carpenter, 299¾ days, at $3		899.25
1 carpenter, 308½ days, at $3		925.50
1 carpenter, 296 days, at $3		888.00
1 carpenter, 13 days, at $3		39.00
1 carpenter, 58 days, at $3		174.00
1 carpenter, 10 months, at $91		910.00
1 carpenter, 301 days, at $3		903.00
1 carpenter, 14 days, at $3		42.00
1 skilled laborer, 314 days, at $2		628.00
1 skilled laborer, 318½ days, at $2		637.00
1 skilled laborer, 1 month 19½ days, at $50		81.45
1 skilled laborer, 19 days, at $50		30.65
1 skilled laborer, 11½ months, at $50		575.00
1 skilled laborer, 275 days, at $2		550.00
1 skilled laborer, 315 days, at $1.75		551.25
1 cabinet-maker, 314 days, at $3		942.00
1 painter, 12 months, at $65		780.00
1 storekeeper, 12 months, at $70		840.00
1 property clerk, 12 months, at $90 per month		1,080.00
1 laborer, 8½ months, at $40 per month	$340.00	
1 laborer, 19 days, at $40 per month	26.21	
1 laborer, 1 month, at $46 per month	46.00	
1 laborer, 1 month, at $41.50 per month	41.50	
		453.71
		13,885.81
Special or contract service		87.96
Total expenditures for salaries or compensation		13,973.77

Miscellaneous, materials, etc.:

Exhibition cases	$350.00
Drawings for cases	15.00
Drawers, trays, boxes	543.72
Frames, stands, etc	169.50

Miscellaneous, materials, etc.—Continued.

Glass	$281.75	
Hardware	1,016.95	
Tools	15.59	
Cloth, cotton, etc	63.05	
Glass jars	1,062.97	
Lumber	1,660.21	
Paints, oil, brushes	499.70	
Office furniture	765.00	
Metals	367.14	
Rubber and leather	122.28	
Apparatus	129.00	
Travel	2.00	
Plumbing	632.00	
		$7,725.86
Total expenditure July 1, 1891, to June 30, 1892, for furniture and fixtures, 1892		21,699.63
Balance July 1, 1892, to meet outstanding liabilities		3,300.37

Heating, lighting, electric, and telephonic service, July 1, 1891, to June 30, 1892.

RECEIPTS.

Appropriation by Congress for the fiscal year ending 30th June, 1892, "for expenses of heating, lighting, electrical, telegraphic, and telephonic service for the National Museum"	$12,000.00
"For removing old boilers under Museum hall in Smithsonian building, replacing them with new ones, and for necessary alterations and connections of steam-heating apparatus and for covering pipes with fireproof material" (sundry civil act, March 3, 1891)	3,000.00
	15,000.00

EXPENDITURES.

Salaries or compensation:			
1 engineer, 12 months, at $115		$1,380.00	
1 fireman, 6 months, at $50 per month, $300; 30½ days, at $50 per month, $49.18; 19½ days, at $50 per month, $31.45; 28 days, at $50 per month, $48.27; 9 days, at $50 per month, $15	$443.91		
1 fireman, 12 months, at $50	600.00		
1 fireman, 12 months, at $50	600.00		
1 fireman, 11 months and 9 days, at $50	561.52		
		2,208.13	
1 telephone clerk, 12 months, at $60	720.00	1,110.00	
1 telephone clerk, 12 months, at $35	420.00		
1 laborer, 327 days, at $1.50 per day		190.50	
Special service		20.00	
Expenditures for salaries or compensation			5,238.93
General expenses:			
Coal and wood		$3,365.85	
Gas		1,360.51	
Telephones		622.65	
Electric work		37.00	
Electric supplies		87.53	
Rental of call boxes		100.00	

General expenses—Continued.

Heating repairs	$329.00	
Heating supplies	433.62	
New boilers (special appropriation)	2,938.47	
		$9,274.63
Total expenditures July 1, 1892, to June 30, 1892, for heating, lighting, etc		14,513.56
Balance July 1, 1892, to meet outstanding liabilities		486.44

Postage, July 1, 1891, to June 30, 1892.

RECEIPTS.

Appropriation by Congress for the fiscal year ending 30th June, 1892, "for postage stamps and foreign postal cards for the National Museum" (sundry civil act, March 3, 1891)	$500.00

EXPENDITURES.

City post-office for postage and postal cards	500.00

Appropriation all expended July 1, 1892.

Printing, July 1, 1891, to June 30, 1892.

RECEIPTS.

Appropriation by Congress for the fiscal year ending June 30, 1892, "for the Smithsonian Institution, for printing labels and blanks, and for the 'Bulletins' and volumes of the 'Proceedings' of the National Museum" (sundry civil act, March 3, 1891)	$15,000.00	
For the Smithsonian Institution, for printing for the use of the National Museum (deficiency act, March 3, 1891), not exceeding	1,000.00	
		16,000.00

EXPENDITURES.

Bulletins Nos. 39, 40, 41, 42	$3,639.03		
Bulletin, special, No. 1 (in part)	1,819.75		
Bulletin, special, No. 2 (in part)	427.95		
		5,886.73	
Proceedings, Vols. XIII, XIV, XV		2,317.96	
Extras from reports		310.87	
Lists, etc		74.46	
Labels for specimens		2,023.66	
Letter heads, memorandum pads, and envelopes		125.14	
Blanks		360.05	
Record books		37.70	
Congressional Records		24.00	
Total expenditure, July 1, 1891, to June 30, 1892, for printing, National Museum			11,160.57
Balance July 1, 1892			4,839.43

Building.

RECEIPTS.

Appropriation by Congress "for removing the decayed wooden floors in the Museum building, substituting granolithic or artificial stone therefor, and for slate for covering trenches containing heating and electric apparatus, including all necessary material and labor, to be immediately available."	$5,000.00

EXPENDITURES.

From March 3, 1891, to June 30, 1892	4,474.64
Balance July 1, 1892, to meet outstanding liabilities	525.36

Duties on Articles Imported for National Museum.

Appropriation by Congress "to meet custom duties on glass, tin, and other dutiable articles and supplies imported for the United States National Museum"		1,000.00
Paid direct by Treasury Department:		
Duty on glass	$642.75	
Duty on glass-top boxes	7.25	
Duty on glass	291.75	
		941.75
Balance July 1, 1892		58.25

Preservation of Collections, 1890.

Balance July 1, 1891, as per last annual report	14.92
Expenditures from July 1, 1891, to June 30, 1892, freight	14.40
Balance July 1, 1892	.52

Preservation of Collections, 1891.

Balance July 1, 1891, as per last annual report			7,979.99
Expenditures from July 1, 1891, to June 30, 1892:			
Salaries or compensation:			
1 assistant, 1 month, at $80	$80.00		
1 assistant, 1 month, at $65	65.00		
1 clerk, 2 mo., at $60	120.00		
		$265.00	
Special or contract work		224.93	
		489.93	
Supplies		1,079.37	
Stationery		122.54	
Specimens		4,191.54	
Books		768.15	
Travel		273.01	
Freight		465.95	
Expenditure to June 30, 1892			7,690.49
			289.50
Cr., by disallowance on stationery			2.08
Balance, July 1, 1892			291.58

Statement of Total Expenditures of the Appropriation for Preservation of Collections, 1891.

	Expenditures.		
	From July 1, 1890, to June 30, 1891.	From July 1, 1891, to June 30, 1892.	Total to June 30, 1892.
For salaries	$117,300.52	$489.93	$117,790.45
For supplies	3,052.32	1,079.37	4,131.69
For stationery	1,653.02	422.54	2,075.56
For specimens	6,211.40	4,191.51	10,402.91
For travel	1,114.78	273.04	1,387.82
For freight	1,862.57	465.95	2,328.52
For books	825.40	768.15	1,593.55
Total	132,020.01	7,690.49	139,710.50
Balance	7,979.99	289.50	289.50
Cr			2.08
			291.58

Furniture and Fixtures, 1890.

Balance July 1, 1891, as per last annual report $0.28

Carried under the action of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1892.

Furniture and fixtures, 1891.

Balance July 1, 1891, as per last annual report $3,690.54

Expenditures from July 1, 1891, to June 30, 1892:

Exhibition cases	$1,118.00	
Drawers, trays, etc	13.50	
Glass	397.91	
Hardware	212.42	
Tools	23.85	
Cloth, etc	4.50	
Glass jars	723.76	
Lumber	737.65	
Paints, oil, and brushes	52.77	
Office furniture	316.70	
Tin, lead, etc	42.40	
Rubber goods	11.88	
Travelling expenses	2.85	
Total expenditure		3,688.19
Balance July 1, 1892		2.35

Statement of total expenditure of appropriation for furniture and fixtures, 1891.

	From July 1, 1890, to June 30, 1891.	From July 1, 1891, to June 30, 1892.	Total to June 30, 1892.
Salaries	$14,212.52		$14,212.52
Exhibition cases	1,295.00	$1,118.00	2,413.00
Designs and drawings	36.00		36.00
Drawers, trays, boxes	448.08	43.50	491.58
Frames, stands, etc	330.52		330.52
Glass	954.56	397.91	1,352.47
Hardware	707.13	212.42	919.55
Tools	73.67	23.85	97.52
Cloth, cotton, etc	108.03	4.50	112.53
Glass jars	61.92	723.76	785.68
Lumber	1,364.05	737.65	2,101.70
Paints, oil, and brushes	565.40	52.77	618.17
Office furniture	588.22	316.70	904.92
Metals	268.48	42.40	310.88
Rubber goods	105.04	11.88	116.92
Iron brackets	87.10		87.10
Apparatus	84.50		84.50
Travelling expenses	5.00	2.85	7.85
Plumbing	14.24		14.24
Total	21,309.46	3,688.19	24,997.65
Balance	3,690.54	2.35	2.35

Heating and lighting, etc., 1890.

Balance July 1, 1891, as per last annual report $1.85

Carried under the action of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1892.

Heating, lighting, electric, and telephonic service, 1891.

Balance July 1, 1891, as per last annual report		$842.34
Expenditures from July 1, 1891, to June 30, 1892:		
Coal and wood	$16.20	
Gas	71.75	
Telephones	200.25	
Electric work	32.75	
Electric supplies	384.95	
Rental of call boxes	20.00	
Heating supplies	81.79	
Total expenditure		810.69
Balance July 1, 1892		1.65

Statement of total expenditure of appropriation for heating, lighting, etc., 1891, $12,000.

	From July 1, 1890, to June 30, 1891.	From July 1, 1891, to June 30, 1892.	Total to June 30, 1892.
Salaries	$5,084.91	....	$5,084.91
Coal and wood	2,766.96	$46.20	2,813.16
Gas	1,233.84	74.75	1,308.59
Telephones	604.40	200.25	804.65
Electric work	7.50	32.75	40.25
Electric supplies	905.68	384.95	1,290.63
Rental of call boxes	100.00	20.00	120.00
Heating repairs } Heating supplies }	448.95	81.79	530.74
Travelling expenses	5.42	....	5.42
Total	11,157.66	840.69	11,998.35
Balance	842.34	1.65	1.65

Postage—National Museum, 1889–'90.

Balance July 1, 1891 .... $500.00

Carried under the action of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1892.

NATIONAL ZOÖLOGICAL PARK.

Organization, improvement, maintenance.

RECEIPTS.

Balance July 1, 1891 .... $23,441.84

EXPENDITURES FROM JULY 1, 1891, TO JUNE 30, 1892.

Shelter of animals	$1,249.96	
Shelter-barns, cages, fences, etc	312.73	
Artificial ponds, etc	1,032.98	
Water supply, sewerage, and drainage	6,342.86	
Roads, walks, and bridges	4,755.81	
Miscellaneous supplies	867.53	
Current expenses	7,101.63	
		21,663.50
Balance July 1, 1892		1,778.34

Statement of the total expenditure of the appropriation for the Zoölogical Park, act of April 30, 1890.

	From April 30, 1890, to June 30, 1891	From July 1, 1891, to June 30, 1892.	Total to June 30, 1892.
Shelter of animals	$13,675.25	$1,249.96	$14,925.21
Shelter-barns, cages, fences, etc	8,643.33	312.73	8,956.06
Repairs to Holt mansion, etc	2,000.00	....	2,000.00
Artificial ponds, etc	56.16	1,032.98	1,089.14
Water supply, sewerage, and drainage.	657.14	6,342.86	7,000.00
Roads, walks, and bridges	10,244.19	4,755.81	15,000.00
Miscellaneous supplies	4,132.47	867.53	5,000.00
Current expenses	29,149.62	7,101.63	36,251.25
Total	68,558.16	21,663.50	$90,221.66

Buildings, 1892.

Appropriation by Congress "for erecting and repairing buildings and inclosures for animals and for administrative purposes in the National Zoölogical Park, including salaries or compensation of all necessary employés, eighteen thousand dollars" (sundry civil act, March 3, 1891)		$18,000.00
Expenditures from July 1, 1891, to June 30, 1892:		
Fencing	$107.50	
Fuel	1.20	
Glass, paints, oils, etc	227.10	
Hardware, tools, etc	1,219.98	
Heating apparatus	3,515.00	
Lumber	3,023.11	
Miscellaneous	83.10	
Plans, drawings, etc	575.00	
Salaries or compensation	8,621.32	
Stone, brick, lime, cement	328.55	
		17,768.16
Balance July 1, 1892		231.51

Improvements, 1892.

Appropriation by Congress "for continuing the construction of roads, walks, bridges, water supply, sewerage, and drainage, and for grading, planting, and otherwise improving the grounds of the National Zoölogical Park, including salaries or compensation of all necessary employés, fifteen thousand dollars" (sundry civil act, March 3, 1891.)		15,000.00
Expenditures from July 1, 1891, to June 30, 1892:		
Building bridge (contract)	$1,712.50	
Building material	511.17	
Freight	71.00	
Hardware	17.20	
Lumber	333.09	
Salaries or compensation	8,181.84	
Settees, etc	120.00	
Supplies	81.95	
Surveying, plans, and drawings	2,961.79	
Tools and implements	173.77	
Travelling expenses, etc	68.50	
Trees, plants, and fertilizers	279.85	
		11,878.66
Balance July 1, 1892		121.34

Maintenance, 1892.

Appropriation by Congress "for care, subsistence, and transportation of animals for the National Zoölogical Park, and for the purchase of rare specimens not otherwise obtainable, including salaries or compensation of all necessary employés, and general incidental expenses not otherwise provided for, seventeen thousand five hundred dollars, one-half of which sum shall be paid from the revenues of the District of Columbia and the other half from the Treasury of the United States" (sundry civil act, March 3, 1891.)	$17,500.00

For care and subsistence of animals for the National Zoological Park, fiscal year eighteen hundred and ninety-two, one thousand dollars, one-half of which sum shall be paid from the revenues of the District of Columbia and the other half from the Treasury of the United States (deficiency act, March 8, 1892)	$1,000.00	
		$18,500.00
Expenditures from July 1, 1891, to June 30, 1892:		
Coal and wood	263.12	
Food for animals	3,738.12	
Freight and hauling	222.91	
Hardware, etc	76.05	
Horseshoeing	33.14	
Lumber	27.50	
Miscellaneous expenses	394.22	
Salaries or compensation	10,984.12	
Stationery and printing	72.77	
Specimens	1,301.55	
		17,113.50
Balance, July 1, 1892		1,386.50

SMITHSONIAN INSTITUTION BUILDING.

Repairs.

RECEIPTS.

Balance, July 1, 1891	$22,585.77

EXPENDITURES FROM JULY 1, 1891, TO JUNE 30, 1892.

Building material, lime, cement, etc	$230.47	
Copper gutters, flashing, etc	545.65	
Glass	305.71	
Hardware	136.66	
Iron roof and ceiling (contract)	4,663.12	
Lumber	230.67	
Miscellaneous	19.25	
Plastering (contract)	2,176.50	
Services, carpenters, painters, laborers, etc	1,490.01	
Slate-work (contract)	603.65	
Window sash, etc	323.00	
		10,724.69
Balance July 1, 1892		11,861.08

ASTRO-PHYSICAL OBSERVATORY—SMITHSONIAN INSTITUTION, 1892.

Receipts.

Appropriation by Congress "for maintenance of Astro-Physical Observatory under the direction of the Smithsonian Institution, including salaries of assistants and the purchase of additional apparatus" (sundry civil act, March 3, 1891)	$10,000.00

Expenditures from July 1, 1891, to June 30, 1892.

Salaries or compensation:	
1 senior assistant, 7½ months, at $200	$1,500.00
1 astronomer, 1 month, at $180, $180; 11½ days, at $180 per month, $66.77	246.77

Salaries or compensation—Continued.			
1 assistant, 10 days, at $166.66 per month	$55.55		
1 assistant, 1 month, at $60, $60; 17 days, at $60 per month, $32.90; 1½ months, at $83.33 per month, $124.99; 9 days, at $83.33 per month, $25	242.89		
1 aid, 3 months, at $45, $135; 30 days, at $45 per month, $13.55	178.55		
1 photographer, 1 month, at $158.33, $158.33; 16 days, at $158.33 per month, $81.72; 15 days, at $158.33 per month, $76.61	316.66		
1 photographer, 1 month, at $150	150.00		
1 instrument-maker, 9½ days, at $60 per month, $47.42; 157½ hours, at 25 cents per hour, $39.38	56.80		
1 draftsman, 246 hours, at 60 cents per hour	147.60		
1 carpenter, 3½ days, at $3 per day	10.50		
1 carpenter, 1½ months, at $91 per month, $136.50; 11½ days, at $91 per month, $33.76	170.26		
1 laborer, 3 months, at $50 per month, $150; 4 months, at $60 per month, $240	390.00		
Total salaries or compensation		$3,465.58	
General expenses:			
Apparatus and appliances	3,841.42		
Electric work	280.50		
Freight	11.57		
Miscellaneous supplies	480.80		
Office furniture	29.75		
Painting	8.00		
Plumbing and gas-fitting	26.70		
Printing blanks, etc	18.00		
Reference books and binding	119.19		
Skylight	145.00		
Travelling expenses	87.10		
		5,378.03	
Total expenditures, Astro-Physical Observatory			$8,843.61
Balance July 1, 1892			1,156.39

RECAPITULATION.

The total amount of funds administered by the Institution during the year ending June 30, 1892, appears from the foregoing statements and the account books to have been as follows:

Smithsonian Institution.

From balance of last year, July 1, 1891		$40,062.1	
(Including cash from executors of Dr. J. H. Kidder	$5,000.00		
Including cash from gift of Dr. Alex. Graham Bell)	5,000.00		
	10,000.00		
From interest on Smithsonian fund for the year		14,481.36	
From sales of publications		378.24	
From re-payments for freight, etc		2,595.99	
From Thomas G. Hodgkins		200,000.00	
Total			$287,517.70

Appropriations committed by Congress to the care of the Institution.

International exchanges—Smithsonian Institution:		
From appropriation for 1891–'92		$17,000.00
North American Ethnology:		
From balance of last year (1890–'91), July 1, 1891	$12,774.24	
From appropriation for 1891–'92	50,000.00	
		62,774.24
Preservation of collections—Museum:		
From balance of 1889–'90	11.92	
From balance of 1890–'91, July 1, 1891	7,979.99	
From appropriation for 1891–'92	145,000.00	
		152,994.91
Printing—Museum:		
From balance of 1889–'90	64.55	
From balance of 1890, July 1, 1891	1,064.65	
From appropriation for 1891–'92	16,000.00	
		17,129.20
Furniture and fixtures—Museum:		
From balance of 1889–'90	.28	
From balance of 1890–'91, July 1, 1891	3,690.54	
From appropriation for 1891–'92	25,000.00	
		28,690.82
Heating, lighting, etc.—Museum:		
From balance of 1889–'90	1.85	
From balance of 1890–'91, July 1, 1891	842.34	
From appropriation for 1891–'92	15,000.00	
		15,844.19
Postage—Museum:		
From balance of 1889–'90	500.00	
From appropriation for 1891–'92	500.00	
		1,000.00
Building—National Museum:		
From appropriation for 1891–'92		5,000.00
Duties on articles imported for National Museum:		
From appropriation for 1891–'92		1,000.00
National Zoölogical Park:		
From balance of 1889–'90, July 1, 1891	23,441.84	
From appropriation for 1891–'92	50,500.00	
		73,941.84
Smithsonian Institution building, repairs:		
From balance of appropriation, July 1, 1891		22,585.77
Astro-Physical Observatory—Smithsonian Institution:		
From appropriation 1891–'92		10,000.00

SUMMARY.

Smithsonian Institution	$287,517.70
Exchanges	17,000.00
Ethnology	62,774.24
Preservation of collections	152,994.91
Furniture and fixtures	28,690.82
Heating and lighting	15,844.19
Postage	1,000.00
Printing	17,129.20
Building, National Museum	5,000.00
Duties on articles for National Museum	1,000.00
Zoölogical park	73,941.84
Smithsonian building, repairs	22,585.77
Astro-Physical Observatory	10,000.00
	695,478.67

The committee has examined the vouchers for payment from the Smithsonian income during the year ending June 30, 1892, each of which bears the approval of the Secretary or, in his absence, of the Acting Secretary, and a certificate that the materials and services charged were applied to the purposes of the Institution.

The committee has also examined the accounts of the several appropriations committed by Congress to the Institution, and finds that the balances hereinbefore given correspond with the certificates of the disbursing clerk of the Smithsonian Institution, whose appointment as such disbursing officer was accepted and his bonds approved by the Secretary of the Treasury.

The quarterly accounts-current, the vouchers, and journals have been examined and found correct.

As stated in the last annual report of the committee, the balance of the appropriation for last year for ethnological researches (Bureau of Ethnology) was continued in the hands of the disbursing clerk of the Bureau (Mr. J. D. McChesney). This balance having since been fully disbursed by him and the appropriation for ethnological researches for the present year as well as the appropriation for the Astro-Physical Observatory having been added to those already in the hands of the disbursing clerk of the Institution (Mr. William W. Karr), all the appropriations committed by Congress to the care of the Institution are now disbursed by the disbursing clerk of the Smithsonian Institution, excepting the appropriation for "printing, National Museum," which is executed under the direction of the Public Printer (Revised Statutes, section 3661).

Statement of regular income from the Smithsonian fund available for use in the year ending June 30, 1893.

Balance on hand June 30, 1892		$47,875.33
(Including the cash from executors of Dr. J. H. Kidder	$5,000.00	
Including the cash from Dr. Alex. Graham Bell)	5,000.00	
	10,000.00	
Interest due and receivable July 1, 1892	27,090.00	
Interest due and receivable January 1, 1893	27,090.00	
		54,180.00
Total available for year ending June 30, 1893		102,055.33

Respectfully submitted.

JAMES C. WELLING,
HENRY COPPÉE,
J. B. HENDERSON,
Executive Committee.

WASHINGTON, D. C., *November 18, 1892.*

ACTS AND RESOLUTIONS OF CONGRESS RELATIVE TO THE SMITHSONIAN INSTITUTION, NATIONAL MUSEUM, ETC.

(In continuation from previous Reports.)

[Fifty-second Congress, first session, 1891, 1892.]

SMITHSONIAN INSTITUTION.

JOINT RESOLUTION [No. 2] to fill vacancies in the Board of Regents of the Smithsonian Institution.

Resolved by the Senate and House of Representatives of the United States of America in Congress assembled, That the vacancies in the Board of Regents of the Smithsonian Institution, of the class other than members of Congress shall be filled by the appointment of William Preston Johnston, of Louisiana, in place of Noah Porter, of Connecticut, resigned, and the appointment of John B. Henderson, a citizen of the District of Columbia, in place of Montgomery C. Meigs, deceased, and by the reappointment of Henry Coppee, of Pennsylvania, whose term of office expired on December twenty-sixth, eighteen hundred and ninety-one. (Approved January 26, 1892.)

War Department.—Buildings and Grounds: For improvement, maintenance and care of Smithsonian Grounds, including construction of asphalt roads and paths, five thousand dollars. (Sundry civil appropriation act. Chap. 380. Statutes, p. 375. Approved August 5, 1892.)

INTERNATIONAL EXCHANGES.

For expenses of the system of international exchanges between the United States and foreign countries, under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employees, twelve thousand dollars. (Sundry civil appropriation act. chap. 380. Statutes p. 360. Approved August 5, 1892.)

Treasury Department: To pay amounts found due by the accounting officers of the Treasury on account of international exchange, Smithsonian Institution, being for the service of the fiscal year eighteen hundred and ninety, as follows:

To pay the Baltimore and Ohio Railroad Company, sixty-seven cents. (Deficiencies appropriation act, chap. 311. Statutes, p. 283. Approved July 28, 1892.)

Library of Congress: For compensation of Librarian, [etc.] * * eight assistant librarians, at one thousand four hundred dollars each, one of whom shall be in charge of international exchanges.

* * * For expenses of exchanging public documents for the publications of foreign governments, one thousand five hundred dollars. (Legislative, executive, and judicial act. Chap. 196, Statutes, p. 189. Approved July 16, 1892.)

Department of the Interior—United States Patent Office: For purchase of professional and scientific books, and expenses of transporting publications of patents issued by the Patent Office to foreign governments, two thousand five hundred dollars. (Legislative, executive, and judicial act. Chap. 196, Statutes, p. 215. Approved July 16, 1892.)

War Department: For the transportation of reports and maps to foreign countries through the Smithsonian Institution, one hundred dollars. (Sundry civil appropriation act, Chap. 380. Statutes, p. 378, Approved August 5, 1892.)

Naval Observatory: For repairs [etc.,] * * * freight, including transmission of public documents through the Smithsonian exchange. [etc.] two thousand five hundred dollars, (Legislative, executive and judicial act, Chap. 196, Statutes, p. 211. Approved July 16, 1892.)

U. S. Geological Survey: For the purchase of necessary books, for the library, and the payment for the transmission of public documents through the Smithsonian exchange, two thousand dollars. (Sundry Civil appropriation act, chap. 380. Statutes, p. 371. Approved August 5, 1892.)

NATIONAL MUSEUM.

For continuing the preservation, exhibition, and increase of the collection from the surveying and exploring expeditions of the Government, and from other sources, including salaries or compensation of all necessary employees, one hundred and thirty-two thousand five hundred dollars.

For cases, furniture, fixtures, and appliances required for the exhibition and safe-keeping of the collections of the National Museum, including salaries or compensation of all necessary employees, fifteen thousand dollars.

For expense of heating, lighting, electrical, telegraphic and telephonic service for the National Museum, eleven thousand dollars.

For postage stamps and foreign postal cards for the National Museum, five hundred dollars. (Sundry civil appropriation act, chap. 380. Statutes, p. 360. Approved August 5, 1892.)

Treasury Department.—To pay amounts found due by the accounting officers of the Treasury on account of preservation of collections, National Museum, being for the services of the fiscal year eighteen hundred and ninety, as follows:

To pay the Baltimore and Ohio Railroad Company, four dollars and forty-seven cents; to pay the Atlantic and Pacific Railroad Company, two dollars and fifty cents; in all, six dollars and ninety-seven cents. (Deficiencies appropriation act. Chap. 311. Statutes, p. 283. Approved July 28, 1892.)

Public Printing and Binding.—For the Smithsonian Institution, for printing labels and blanks and for the "Bulletins" and annual volumes of the "Proceedings" of the National Museum, twelve thousand dollars. (Sundry civil appropriation act. Chap. 380. Statutes, p. 388. Approved August 5, 1892.)

JOINT RESOLUTION [No. 8,] to encourage the establishment and endowment of institutions of learning at the national capital by defining the policy of the Government with reference to the use of its literary and scientific collections by students.

Whereas, large collections illustrative of the various arts and sciences and facilitating literary and scientific research have been accumulated by the action of Congress through a series of years at the national capital; and

Whereas it was the original purpose of the Government thereby to promote research and the diffusion of knowledge, and is now the settled policy and present practice of those charged with the care of these collections specially to encourage students who devote their time to the investigation and study of any branch of knowledge by allowing to them all proper use thereof; and

Whereas it is represented that the enumeration of these facilities and the formal statement of this policy will encourage the establishment and endowment of institutions of learning at the seat of Government, and promote the work of education by attracting students to avail themselves of the advantages aforesaid under the direction of competent instructors: Therefore,

Resolved by the Senate and House of Representatives of the United States of America, in Congress assembled, That the facilities for research and illustration in the following and any other Governmental collections now existing or hereafter to be established in the city of Washington for the promotion of knowledge shall be accessible, under such rules and restrictions as the officers in charge of each collection may prescribe, subject to such authority as is now or may hereafter be permitted by law, to the scientific investigators and to students of any institution of higher education now incorporated or hereafter to be incorporated under the laws of Congress or of the District of Columbia, to wit:

One. Of the Library of Congress.
Two. Of the National Museum.
Three. Of the Patent Office.
Four. Of the Bureau of Education.
Five. Of the Bureau of Ethnology.
Six. Of the Army Medical Museum.
Seven. Of the Department of Agriculture.
Eight. Of the Fish Commission.
Nine. Of the Botanic Gardens.
Ten. Of the Coast and Geodetic Survey.
Eleven. Of the Geological Survey.
Twelve. Of the Naval Observatory.
(Approved, April 12, 1892.)

NORTH AMERICAN ETHNOLOGY.

For continuing ethnological researches among the American Indians, under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employees, forty thousand dollars. (Sundry civil appropriation act. Chap. 380. Statutes, p. 360. Approved August 5, 1892.)

ASTRO-PHYSICAL OBSERVATORY.

For maintenance of astro-physical observatory, under the direction of the Smithsonian Institution, including salaries of assistants, apparatus, and miscellaneous expenses, ten thousand dollars. (Sundry civil appropriations act, Chap. 380, Statutes, p. 360. Approved August 5, 1892.)

NATIONAL ZOOLOGICAL PARK.

For continuing the construction of roads, walks, bridges, water supply, sewerage, and drainage; and for grading, planting, and otherwise improving the grounds; erecting, and repairing buildings and inclosures for animals; and for administrative purposes, care, subsistence, and transportation of animals, including salaries or compensation of all necessary employees, and general incidental expenses not otherwise provided for, fifty thousand dollars, one-half of which sum shall be paid from the revenues of the District of Columbia and the other half from the Treasury of the United States; and a report in detail of the expenses on account of the National Zoological Park shall be made to Congress at the beginning of each regular session. (Sundry civil appropriation act. Chap. 380, Statutes, p. 360. Approved August 5, 1892.)

For care and subsistence of animals for the National Zoological Park, fiscal year eighteen hundred and ninety-two, one thousand dollars, one-half of which sum shall be paid from the revenues of the District of Columbia, and the other half from the treasury of the United States. (Deficiency appropriation act, Chap. 12, Statutes, p. 6. Approved March 8, 1892.)

To pay Melville Lindsay for rubber boots furnished to employees engaged to work in water in the National Zoological Park, being a deficiency for the fiscal year eighteen hundred and ninety-one, thirty-eight dollars. (Deficiencies appropriation act, Chap. 311. Statutes, p. 284. Approved July 28, 1892.)

WORLD'S COLUMBIAN EXPOSITION AT CHICAGO.

Treasury Department: For the selection, purchase, preparation, transportation, installation, care and custody, and arrangement of such articles and materials as the heads of the several Executive Departments, the Smithsonian Institution, and National Museum, and the United States Fish Commission may decide shall be embraced in the Government exhibit, and such additional articles as the President may designate for said Exposition, and for the employment of proper persons as officers and assistants to the Board of Control and Management of the Government exhibit, appointed by the President, of which not exceeding five thousand dollars may be expended by said Board for clerical services, four hundred and eight thousand two hundred and fifty dollars: *Provided* That all expenditures for the purposes and from the appropriation specified herein shall be subject to the approval of the said Board of Control and Management and of the Secretary of the Treasury, as now provided by law. (Sundry civil appropriation act. Chap. 380. Statutes, p. 362. Approved August 5, 1892.)

COLUMBIAN HISTORICAL EXPOSITION AT MADRID.

State Department: For the expense of representation of the United States at the Columbian Historical Exposition to be held in Madrid in eighteen hundred and ninety-two in commemoration of the four hundredth aniversary of the discovery of America, fifteen thousand dollars, or so much thereof as may be necessary, to be expended under the direction and in the discretion of the Secretary of State: and the President is hereby authorized to appoint a commissioner-general and two assistant commissioners, who may, in his discretion, be selected from the active or retired list of the Army or Navy, and shall serve without other compensation than that to which they are now entitled by law, to represent the United States at said exposition; that it shall be the duty of such commissioners to select from the archives of the United States, from the National Museum, and from the various executive departments of the Government such pictures, books, papers, documents, and other articles as may relate to the discovery and early settlement of America and the aboriginal inhabitants thereof; and they shall be authorized to secure the loan of similar articles from other museums and private collections, and arrange, classify, and install them as the exhibit of the United States at the said exposition; that the President is authorized to cause the detail of officers from the active or retired list of the Army and Navy, to serve without compensation other than that to which they are now entitled by law, as assistants to said commissioners; and the said commissioners shall be authorized to employ such clerical and other assistance as may be necessary, subject to the approval of the Secretary of State. (Deficiencies appropriation act, Chap. 72, Statutes, p. 34. Approved May 13, 1892.)

State Department: For expenses of representation of the United States at said exposition, ten thousand dollars. (Sundry civil appropriation act, Chap. 380, Statutes, p. 350. Approved August 5, 1892.)

REPORT OF S. P. LANGLEY,

SECRETARY OF THE SMITHSONIAN INSTITUTION,

FOR THE YEAR ENDING JUNE 30, 1892.

To the Board of Regents of the Smithsonian Institution:

GENTLEMEN: I have the honor to submit herewith my report for the year ending June 30, 1892, of the operations of the Smithsonian Institution, including the work placed by Congress under its charge in the National Museum, the Bureau of Ethnology, the International Exchanges, the National Zoological Park, and the Astro-Physical Observatory.

Matters of general interest have been treated of in the body of the report, while in the Appendix will be found detailed reports on the more important subdivisions of the work of the Institution, namely: the Bureau of Ethnology, the Bureau of International Exchanges, the Library, the National Zoological Park, the Astro-Physical Observatory, and the Editor in charge of Publications.

The work of the National Museum is reported on at length in a separate volume by the Assistant Secretary in charge.

THE SMITHSONIAN INSTITUTION.

THE ESTABLISHMENT.

I have to record the following changes in the Establishment during the year: The resignation of the Hon. James G. Blaine, Secretary of State, on June 4, 1892, and the appointment of his successor to the Secretaryship, the Hon. John W. Foster; the resignation of the Hon. Redfield Proctor, Secretary of War, on December 6, 1891, and the appointment of his successor, the Hon. Stephen B. Elkins; and the resignation of the Hon. Charles E. Mitchell, Commissioner of Patents, on July 31, 1891, and the appointment of the Hon. William E. Simonds as his successor.

THE BOARD OF REGENTS.

In accordance with a resolution of the Board of Regents fixing the time of the stated annual meeting of the Board on the fourth Wednesday of January in each year, the Board met on January 27, 1892, at 10 o'clock A. M. The journal of its proceedings will be found as usual in its annual report to Congress, but for convenience reference is also here made later to a portion of its action.

A special meeting of the Board of Regents was held on October 21, 1891, at which a gift of $200,000 from Mr. Thomas G. Hodgkins, of Setauket, Long Island, was formally accepted; and another was held on March 29, 1892, to take action regarding certain Congressional appropriations.

The following changes in the *personnel* of the Board of Regents are to be noted: The appointment of the Hon. W. C. P. Breckinridge, of the House of Representatives, by the Speaker of the House (*pro tempore*), January 15, 1892, to succeed the Hon. Benjamin Butterworth, whose term expired December 23, 1891; the appointment, by joint resolution of Congress, approved January 26, 1892, of President William Preston Johnston, of Tulane University, Louisiana, to succeed Dr. Noah Porter, who resigned December 31, 1889; and the appointment, by the same resolution, of the Hon. John B. Henderson, of the District of Columbia, to succeed Gen. M. C. Meigs, who died January 2, 1892.

The following have been re-appointed to fill vacancies caused by the expiration of their own terms: The Hon. Justin S. Morrill, of the United States Senate, by the Vice-President of the United States, December 15, 1891, the Hon. Joseph Wheeler, and the Hon. Henry Cabot Lodge, of the House of Representatives, by the Speaker (*pro tempore*) of the House, January 15, 1892, and Dr. Henry Coppée, by joint resolution of Congress approved January 26, 1892.

The Board has suffered the loss by death of Gen. Montgomery C. Meigs on January 2, 1892. Dr. Noah Porter, an ex-member of the Board, died on March 4, 1892. Reference is made to them elsewhere in the necrologic notices.

ADMINISTRATION.

I beg to repeat the recommendation contained in my report of last year, that Congress be requested to make some provision for meeting the actual expenses of the administration of the affairs of the General Government confided to the Institution. There is no such provision now for the considerable and increasing clerical expenses, which belong not to any single Government bureau under its care, but to the charge of their common administration, and these expenses all fall ultimately upon the Institution.

Another difficulty arising out of the great extension of the interests under the care of the Regents, which makes the duties of the Secretary and the Assistant Secretary altogether different from what they were in its early history, has been calling for relief for some time, and has finally been met by appropriate action of the Board; for, apart from the need of a Congressional appropriation which shall provide for the increased expenses of the clerical force in the Secretary's office engaged in transacting purely Government business, I have directed the attention of the Regents to the fact that the Chancellor of the Institution (in whom alone the power of appointing an Acting Secretary is vested by law) may be absent when the Acting Secretary is ill and when there is no one to relieve him. Such a case has actually presented itself, directing attention to the necessity of authorizing the Secretary to delegate authority for performing certain subordinate but indispensable functions, such as signing a certain class of papers.

Owing to the established principles of conduct in the Smithsonian Institution (which there has been no intention of departing from) the Secretary's power has never been diffused or delegated even as far as is usual in the case of Executive Departments of the Government, where there are several persons in every separate bureau constituting a line of succession of those who are authorized, in case of the absence of its head, to carry on ordinary business and especially to sign all such routine papers as are required for its current business with the Treasury. There has been no time however in the past twelve years, when, in the joint event of the illness or absence of the Secretary and the Acting Secretary of the Smithsonian Institution, any such provision has existed for carrying on even the routine business.

At the meeting of the Regents on January 27, 1892, the following resolution was introduced, and was duly given effect through the Executive Committee in the appointment of an officer of the Institution to act according to its provisions:

"*Resolved*, That the Secretary be empowered to appoint some suitable person, who, in case of need, may sign such requisitions, vouchers, abstracts of vouchers, accounts current, and indorsements of checks and drafts, as are needed in the current business of the Institution, or of any of its bureaus, and are customarily signed in the bureaus of other Departments of the Government."

FINANCES.

I have recalled the fact that the gift of $200,000 to the Institution by Mr. Thomas G. Hodgkins, of Setauket, Long Island, to which I briefly referred in my report of last year, was formally accepted by the Board of Regents at a special meeting held October 21, 1891.

At this meeting I stated that I had entered on a correspondence with Mr. Hodgkins in which he had intimated his desire to give a considerable sum to the fund of the Smithsonian Institution for the "increase and diffusion of knowledge among men." The correspondence was followed by personal visits both by the Secretary and by the Assistant Secretary, the result of which was that Mr. Hodgkins offered a donation of $200,000, concerning which the Secretary telegraphed the Regents on June 22. Upon being advised of the individual approval of most of the Regents to the acceptance of the sum named, Mr. Hodgkins later, on September 22, at his home on Long Island, gave the amount in cash to the Secretary, who deposited it in the United States Treasury at Washington, with the understanding that an early meeting of the Board would be called as a body to consider its acceptance.

The essential conditions are that the income of $100,000 of this gift shall be permanently devoted to the increase and diffusion of more exact knowledge in regard to the nature and properties of atmospheric air in connection with the welfare of man; the income of the remaining $100,000 being for the general purposes of the Institution.

In view of the importance of the subject I have referred to it again later in the report, under a distinct heading.

I may call attention in this place to the fact that the Smithsonian Institution is, by reason of its far-reaching connection with the scientific world, enabled to make specially effective use of sums given for immediate employment in specific purposes or investigations. A few such special trusts (distinct from those for adding to the permanent endowment) have been committed to the Institution in the past, through the Secretary, and yet I feel assured that, were the intentions of the Regents better understood in this regard, the Institution would much more frequently be made the medium for giving effect to the plans of those interested in promoting specific researches, as well as in making permanent endowments.

The permanent funds of the Institution are as follows:

Bequest of Smithson, 1846	$515,169.00
Residuary legacy of Smithson, 1867	26,210.63
Deposit from savings of income, etc., 1867	108,620.37
Bequest of James Hamilton, 1875	1,000.00
Bequest of Simon Habel, 1880	500.00
Deposit from proceeds of sale of bonds, 1881	51,500.00
Hodgkins' gift, 1891	200,000.00
Total permanent Smithsonian fund in the Treasury of the United States, bearing interest at 6 per cent. per annum	$903,000.00

At the beginning of the fiscal year the balance on hand was $40,062.11. Interest on the invested fund, amounting to $44,481.36, has been received from the Treasury of the United States during the year, and from sales of publications and miscellaneous sources, including repayments on account of international exchanges, $2,974.23, making a total of $87,517.70.

The total expenditures, as shown in detail in the report of the Executive Committee, have been $39,642.37, leaving an unexpended balance on June 30, 1892, of $47,875.33. This includes a sum of $10,000, the amount of a bequest of $5,000 from the late Dr. J. H. Kidder and a donation of a like amount from Dr. Alexander Graham Bell personally to the Secretary for physical investigations, which was, with the donor's consent, deposited by the Secretary to the credit of the funds of the Institution, subject to order. Neither of these sums, then, forms a portion of the invested funds, and both have been held in the hope that Congress would later provide a site for a permanent building for the Astro-Physical Observatory. The balance available for the general purposes of the Institution on July 1, 1892, was $37,875.33, but this is in large part held against various liabilities, for scientific purposes.

The Institution has been charged by Congress with the disbursement during the year of the following appropriations:

For International Exchanges	$17,000
For Ethnological Researches	50,000
For National Museum:	
Preservation of collections	115,000
Furniture and fixtures	25,000
Heating and lighting	15,000
Postage	500
Flooring for Museum building	5,000
Duties on articles imported	1,000
Purchase of Capron Collection of Japanese Works of Art	10,000
Printing	16,000
For National Zoological Park:	
Improvements	15,000
Buildings	18,000
Maintenance	17,500
For Astro-Physical Observatory	10,000

To these should be added the small unexpended balance of the special appropriation of $92,000 made April 30, 1890, for the National Zoological Park.

The vouchers for the disbursement of these appropriations have been examined by the Executive Committee, and the various items of expenditure are set forth in a letter addressed to the Speaker of the House of Representatives, in accordance with a provision of the Sundry Civil Act of October 2, 1888; while the expenditures from the Smithsonian fund have likewise been examined and approved by the Executive Committee, and are shown in their report.

I may call attention to the fact that the Secretary has been desirous to see a change in the phraseology of the Sundry Civil Act making appropriation for ethnological researches, which would relieve him from the personal responsibility imposed by the language of former bills. Such a change has now been made, whereby the appropriation is placed "under the direction of the Smithsonian Institution," instead of in the charge of the "Secretary of the Smithsonian Institution," as heretofore. The vouchers from the Bureau of Ethnology are therefore now scrutinized by the Executive Committee, as are all other expenditures of the Institution.

The estimates for the fiscal year ending June 30, 1893, forwarded to the Secretary of the Treasury, under date of October 7, 1891, were as follows:

Building, Smithsonian Institution	$5,000
International Exchanges	17,000
North American Ethnology	50,000

National Museum:	
Preservation of collections	$145,000
Heating and lighting	12,000
Furniture and fixtures	25,000
Printing and binding	18,000
Postage	500
Duties on articles imported	1,000
Addition to electric-light plant	5,000
Galleries	8,000
National Zoological Park:	
Improvements	15,000
Buildings	18,000
Maintenance	17,500
Astro-Physical Observatory	10,000

BUILDINGS.

I have repeatedly urged upon your attention the necessity for more ample accommodations for the rapidly increasing collections of the National Museum, a necessity that has been emphasized by the difficulties attending the preparation for the Museum exhibit at the World's Columbian Exposition in Chicago and the Columbian Historical Exposition in Madrid.

In the light of past experience, it is not unreasonable to anticipate a large increase in the collections of the Museum in the shape of donations from exhibitors at these expositions, if any assurance can be given that such material will eventually be properly installed. If no such assurance can be given a great amount of material will be lost to the Institution, the value of which would, in my opinion, nearly equal the estimated cost of a new building for the Museum.

The present Museum building was finished and occupied in 1881. The collections increased so rapidly that as early as 1883 the Regents, at their meeting of January 17, recommended to Congress the erection of a new building.

Since 1883 the collections have again increased to such an extent that a new building as large as the present one could now be advantageously filled with material held in storage, and I can only repeat with increased emphasis the closing sentence of my letter of January 21, 1890, to the Hon. Leland Stanford, chairman of the Senate Committee on Public Buildings and Grounds, "That unless more space is provided, the development of the Government collection, which is already partly arrested, will be almost completely stopped."

The Museum collections have overflowed into every part of the Smithsonian building, and special provisions have been made for them, beginning with the galleries long since erected in the main hall, not contemplated in the original plans of the building, and which seriously interfere with lighting the exhibition open to the public. The storage space of the Institution building needed for other purposes, is now also almost exclusively occupied by Museum specimens, and some relief must be found.

A bill providing for the erection of a fire-proof building for the National Museum was introduced in the Senate by the Hon. J. S. Morrill, and passed the Senate on April 15, 1892, but failed to secure favorable action in the House.

The work of fire-proofing the so-called "chapel" of the west wing of the Smithsonian building has been practically completed, and I would especially urge that the balance of this appropriation, unexpended, by reason of a limiting clause introduced in the act, on account of which the money is not available for certain repairs originally contemplated, should be now made available by Congress for increasing the storage room in the east wing of the building, and at the same time that certain rooms be fitted for the special needs of the Government Exchange Bureau, now occupying rooms in the Main Building, urgently needed for other purposes.

The new buildings erected or in progress of erection for the collection of living animals, being all in the Zoological Park, are mentioned in the report upon the park.

RESEARCH.

In pursuance of the long established policy of the Institution, financial aid has, during the past year, been extended to original investigators in the domain of science, and considering the modest sum that it has been found possible to devote to this purpose, the results are gratifying.

The subscription for twenty copies of the Astronomical Journal, which are distributed abroad as exchanges of the Institution, has been continued.

To the Lick Observatory, through its director, Prof. Holden, an additional grant has been made for the continuance of experiments in lunar photography.

Prof. E. W. Morley is still engaged in his determinations of the density of oxygen and hydrogen, for which some special apparatus has been provided by the Institution.

Mention has been made in previous reports of the aid extended to Prof. A. A. Michelson, of Clark University, in his experiments with the refractometer, and in the determination of a universal standard of length founded on the wave length of light. In furtherance of the latter project, the Institution will, during the coming summer, send one of its scientific staff to assist Prof. Michelson in his investigations under the auspices of the International Bureau of Weights and Measures in the laboratory of the Bureau at Sevres, France.

Both these latter investigations refer to fundamental constants of nature, and their results promise to be of wide and lasting importance.

Allusion was made in my last report to aid extended to Dr. Wolcott Gibbs in his investigations of the physiological action of chemical compounds. These investigations are now completed, and have resulted in a substantial contribution to this branch of science.

Astro-physical Observatory.—The Smithsonian Astro-physical Observatory still occupies the temporary wooden shelter upon the grounds just south of the Smithsonian building, and the money given to the Institution for the erection of a more permanent building is still held while awaiting the action of Congress in providing a site. The observatory has received much of my personal attention during the year.

In statements to Congress and elsewhere some brief official explanation has been given of the object of this observatory, which, as it has not been explicitly given in previous reports, I repeat here in the most succinct manner before entering on any description of the special work.

The general object of astronomy, the oldest of the sciences, was, until a very late period, to study the places and motions of the heavenly bodies, with little special reference to the wants of man in his daily life, other than in the application of the study to the purposes of navigation.

Within the past generation, and almost coincidentally with the discovery of the spectroscope, a new branch of astronomy has arisen, which is sometimes called astro-physics, and whose purpose is distinctly different from that of finding the places of the stars, or the moon, or the sun; which is the principal end in view at such an observatory as that, for instance, at Greenwich.

The distinct object of astro-physics is, in the case of the sun, for example, not to mark its exact place in the sky, but to find out how it affects the earth and the wants of man on it; how its heat is distributed, and how it in fact affects not only the seasons and the farmer's crops, but the whole system of living things on the earth, for it has lately been proven that in a physical sense, it, and almost it alone, literally first creates and then modifies them in almost every possible way.

We have however arrived at a knowledge that it does so, without yet knowing in most cases how it does so, and we are sure of the great importance of this last acquisition, while still largely in ignorance how to obtain it. We are, for example, sure that the latter knowledge would form among other things a scientific basis for meteorology and enable us to predict the years of good or bad harvests, so far as these depend on natural causes, independent of man, and yet we are still very far from being able to make such a prediction, and we cannot do so till we have learned more by such studies as those in question.

Knowledge of the nature of the certain, but still imperfectly understood dependence of terrestrial events on solar causes, is, then, of the greatest practical consequence, and it is with these large aims of ultimate utility in view, as well as for the abstract interest of scientific investigation, that the Government is asked to recognize such researches as of national importance; for it is to such a knowledge of causes with such practical consequences that this class of investigation aims and tends.

Astro-physics by no means confines its investigation to the sun, though that is the most important subject of its study and one which has been undertaken by nearly every leading government of the civilized world but the United States. France has a great astro-physical observatory at Meudon, and Germany one on an equal scale at Potsdam, while England, Italy, and other countries have also, at the national expense, maintained for many years institutions for the prosecution of astro-physical science.

It has been observed that this recent science itself was almost coeval with the discovery of the spectroscope, and that instrument has everywhere been largely employed in most of its work. Of the heat which the sun sends, however, and which, in its terrestrial manifestations, is the principal object of our study, it has long been well known that the spectroscope could recognize only about one-quarter—three-quarters of all this solar heat being in a form which the ordinary spectroscope can not see nor analyze, lying as it does in the almost unknown "infra-red" end of the spectrum, where neither the eye nor the photograph can examine it. It has been known for many years that it was there, and we have had a rough idea of its amount, with an almost total incapacity to exhibit it in detail. Our imperfect knowledge of this region is at present represented by a few inadequate types of parts of it given in drawings made by hand, where the attempts to depict it at all are even to-day more crude than the very earliest charts of the visible spectrum, made in the infancy of spectroscopic science.

One of the first pieces of work which this observatory has undertaken is to explore and describe what may be properly called "this great unknown region," by a method which the writer has recently been able to bring to such a degree of success as to give good grounds for its continued prosecution and for the hope that a complete map of this whole region will shortly be produced by an automatic and therefore trustworthy process, showing the lines corresponding to the so-called Fraunhofer lines in the upper spectrum.

The first year's work of any such observatory must ordinarily consist largely in perfecting its apparatus and determining its constants, but a portion of this necessary labor has been deferred in favor of this principal task, of which it is hoped that another year will see the essential completion. In this, the present principal scientific work here, all resources of the observatory are, then, for the time being engaged.

I have acknowledged in a previous report the valuable assistance of Prof. C. C. Hutchins, of Bowdoin College, who efficiently aided in installing the apparatus. Prof. Hutchins was obliged to leave in August. On the 16th of November Dr. William Hallock was appointed senior assistant.

At different times during the year, there have been employed as assistants Mr. C. A. Saunders, Mr. C. T. Child, and Mr. F. L. O. Wadsworth. A photographer and a laborer complete the present force of the Observatory.

In the latter part of the year, Dr. Hallock, to my regret, advised me of his proximate call to another duty, and the work was later left temporarily in the charge of Mr. Wadsworth, who had joined the staff in June, but who was sent to Europe in July, for the purpose, elsewhere referred to, of contributing to the work of establishing a wave-length standard under Professor Michelson. The labor has been carried on under the disadvantages of these interruptions, and also under others of another kind, due to the fact that the extremely delicate apparatus, which is used in a perpetually darkened room, is, by reason of the location of the temporary observatory shed, in proximity to traffic-laden streets, where there is danger that the passing vehicles affect the accuracy of the observations both by earth tremors and by magnetic disturbances. Notwithstanding these latter drawbacks, much better results have been obtained than it was supposed could be reached in such a situation, and these, as I have said, I trust, another year will enable the Institution to make public.

EXPLORATIONS.

Several explorations have been carried on during the year by the U. S. Fish Commission, resulting in the transfer to the Museum of many large and varied collections of zoölogical, botanical, and geological material. Dr. W. L. Abbott has continued his work in Asia and has contributed collections made in Kashmir. Dr. Edgar A. Mearns, of the International Boundary Commission, has sent several large collections of natural-history specimens obtained near the border line between the United States and Mexico. Mr. P. L. Jouy has made important collections in Arizona and New Mexico. Collections of the fishes of Nicaragua have been received from Mr. C. W. Richmond.

Mr. W. W. Rockhill, the distinguished traveler, whose previous explorations have been mentioned in my reports and who has already deposited in the Museum very valuable collections which he made illustrating the religious practices, occupations, and amusements of various peoples in different parts of China, Thibet, and Turkestan, has started upon a second journey to hitherto almost unknown parts of Thibet, with such aid (much more limited than I could wish) as it was possible for me to afford him. From his known qualities as an explorer it may be confidently expected that his journey will result in important contributions to our knowledge of this country.

PUBLICATIONS.

The number of publications during the year has been about the same as in preceding years.

As has been frequently stated, the publications of the Institution proper are of three classes: First, the series of "Smithsonian Contributions to Knowledge," in quarto form, comprising original memoirs of researches believed to present new truths, and which, as required, are liberally illustrated with figures or plates; secondly, the series of "Smithsonian Miscellaneous Collections," in octavo size, containing special reports, systematic lists of synopses of species, etc., whether from the organic or the inorganic world, instructions to naturalists for collecting and preserving specimens, special bibliographies, tabulated results, and other aids to scientific investigation not generally requiring illustrations; and lastly, the series of "Smithsonian Annual Reports," presenting to Congress, through the Secretary, the condition of the Institution, accompanied, under the early plan of Professor Henry, by scientific articles from competent writers, either original or selected, but as a rule in untechnical terms, representing the advances made in various departments of research and frequently admitting of illustration by plates or figures. These articles are intended to be of interest not alone to the correspondents and collaborators of the Institution, but to that large number of the educated public who follow such statements with profit when they are presented in popularly intelligible form.

Smithsonian Contributions to Knowledge.—The only publication of the year in this series is a memoir detailing the results of original experiments in aërodynamics by the Secretary,* and occupying 115 quarto pages, illustrated with 11 figures and 10 plates.

Smithsonian Miscellaneous Collections.—The number of titles in this series during the year is 47, of which none seem to call for any particular comment.

Smithsonian Annual Report.—I have referred in my report for 1889 to a modification of the plan on which the Appendix was prepared. From 1880 to 1888 the Appendix was chiefly devoted to an annual summary of progress in various branches of science. The growing inefficiency of this summary, due to causes elsewhere mentioned, led me to return in the report for 1889 to the earlier plan of Prof. Henry, which was to present a selection of papers by eminent, or at least competent, expositors, chosen from the scientific literature of the year. This modification, or rather this return to the method of the earlier reports, has been continued, and seems to meet with general appreciation at the hands of the correspondents of the Institution and others to whom the reports are sent. The report for 1890 issued during the year embraces a considerable range of scientific investigation and discussion. Many of the papers are the work of distinguished investigators, and all are presented in untechnical language so as to interest the largest number.

Lunar photographs.—I have devoted considerable thought to a plan for publishing a work on the moon, which shall represent the present knowledge of the physical features of our satellite. A study of the

* *Resolved,* That the Secretary of the Smithsonian Institution be requested to continue his researches in physical science, and to present such facts and principles as may be developed for publication in the Smithsonian Contributions. (Journal of Proceedings of Board of Regents, January 26, 1847.)

surface of the moon is of special and growing interest to geologists, who have rarely access to the largest class of telescopes, and what we know of it is derived very largely from maps made from eye-studies by astronomers.

Within a few years photography has been used with such increasing advantage in this interesting field, that it is believed by those competent to express an opinion, that photographs can shortly be produced which will exhibit in a permanent form everything that a trained eye can recognize at the most powerful telescope. If this surprising result be not actually obtained, I am of opinion that it is attainable; and I have proposed to procure, through the association of the Smithsonian Institution with some of the leading observatories of the world, a series of photographic representations of hitherto unequaled size and definition, which shall represent the moon's surface as far as possible on a definite scale, and entirely without the intervention of the draftsman. Photographs of the moon made at the Harvard, Lick, and Paris observatories have been placed at the disposition of the Smithsonian Institution for publication, and it is intended to issue a series of them accompanied by explanatory text. Whether this considerable work shall appear as one of the regular series of "Contributions to Knowledge," or as a special publication in a more limited edition, has not yet been decided.

Smithsonian Tables.—The meteorological and physical tables, originally prepared by Dr. Guyot and first published in 1851, have been in such demand that they have already passed through four editions. The last edition was exhausted several years ago, and in considering the advisability of issuing a fifth edition, it was determined in 1887 to revise the tables to conform to the present state of our knowledge. The work has been divided into three parts, meteorological, geographical, and physical, each one being independent of the others, but the three capable of forming a homogeneous volume.

In carrying out this plan I was able to secure the assistance of Prof. William Libbey, jr., of Princeton, under whose editorship the last edition was issued in 1884, and Prof. Libbey, devoting gratuitously such time to the work as he could command from his engrossing college duties, prepared the first volume of the series, the "Meteorological Tables." The plan of the work was then somewhat modified and a further revision was made by Mr. G. E. Curtis, who was at the time employed upon other work at the Smithsonian Institution, and by the end of December, 1891, the manuscript was essentially ready for the printer. Since that time it has been passing through the press, and it is hoped that the volume will be entirely finished by the close of the present calendar year.

SMITHSONIAN INTERNATIONAL EXCHANGE SERVICE.

The international exchange service, through which the Smithsonian Institution is known to most of the large libraries and to a vast number of scientific men throughout the world, has received such attention in my recent reports that it seems unnecessary to dwell upon it at length here.

The work of the bureau continues to increase, and in spite of many labor-saving devices in the clerical work suggested by experience, it will be impossible to meet all the demands made for transportation of documents unless some considerable increase is also made in the amount appropriated by the General Government in the near future.

The United States Government has undertaken, by a treaty formulated at Brussels in 1886, and finally proclaimed by the President in 1889, to carry on a system of international exchanges. These various countries adhering to the treaty have formally agreed each to establish a bureau charged with the duty of attending to the exchange of official documents, parliamentary and administrative, which are published in the country of their origin, and the bureaus of exchange will furthermore serve as intermediaries between the learned bodies and literary and scientific societies of the contracting States for the reception and transmission of their publications.

In transmitting abroad each State assumes the expenses of packing and transportation to the place of destination, but when the transmissions are made by sea, special arrangements regulate the share of each State in the expense of transportation.

The Smithsonian Institution, having since 1850 conducted an exchange service with means of communication over the entire world, has been charged by the United States Government with the conduct of its own exchange business, and appropriations for the purpose have accordingly been made of late years to the Institution, covering at present the greater part of the expense. The deficiency arising each year has been met from the Smithsonian fund, and the Institution has continued its paid agents in England and in Germany, as these two countries have not signified their adherence to the treaty in question, but maintain exchange relations with the United States independently of other countries concerned in the treaty. By referring to the curator's statistical report contained in the Appendix, it will be seen that over 100 tons of books passed through the exchange office during the fiscal year, representing 97,027 packages—an increase of 6,361 packages over the number handled during the preceding year—while upon the exchange books, accounts of publications received and transmitted are kept with 20,682 societies or institutions and individuals. The expenditures upon this account have amounted to $20,310.49, of which $17,000 were appropriated by Congress, $2,108.11 were repaid by Government bureaus, and $30.75 by State institutions and others, leaving a deficiency of $1,171.30 to be met by the Smithsonian Institution.

The expenses, it will be noted, take no account of the rent value of the rooms in the Institution occupied in this manner by the General Government for exchange purposes, or that portion of the service of the regular officers of the Institution occupied with exchange business, and the sum appropriated by Congress would be entirely inadequate were it not that the chief ocean steamship companies have, since the early days of the Institution, granted the privilege of free freight for its exchange boxes. I have repeatedly called attention to the impropriety of further trespassing upon the generosity of these companies, the privilege having been originally intended as a direct encouragement of the philanthropic aims of the Institution, whereas now a very large proportion of the freight thus carried is Government property and the service is conducted under an international treaty.

I may further call attention in this place to the fact that an additional treaty made at Brussels in 1886 and proclaimed by the President of the United States on January 15, 1889, wherein provision is made for the immediate exchange of official journals, parliamentary annals and documents, has never been executed. A bill making an appropriation of $2,000 for this purpose passed the Senate in 1891, but no final action thereon has been taken.

The amount estimated for the conduct of the exchange service for the year 1892-'93 was $23,000, a sum which was expected to cover the present expense of the Exchange Bureau in a single item, including the $2,000 just mentioned. At the close of the fiscal year the Sundry Civil Appropriation bill, of which this was an item, had not become a law.

I desire to mention again here the increasing difficulty of making provision for the storage of Government publications not needed for immediate transmission abroad. A portion of the building is now devoted to this purpose which is needed more and more each year for the more legitimate purposes of the Institution.

The exchange offices are also needed for the growing reference library of scientific books belonging to the Institution, and with a view to relieving the overcrowded condition of the library by removing these offices to the basement, I have urged upon Congress the desirability of making available for the purpose, the balance of an appropriation originally intended for repairs and alterations to the western part of the building, which, by reason of a restricting clause in the appropriation act, can not be used for the work first proposed. By the expenditure of about $10,000 the basement of the east wing, now damp and sometimes flooded with water, can be made thoroughly healthy and well adapted to the needs of the exchange work.

In my report for 1890 I stated that there had been expended from the Smithsonian fund for the support of the international exchange system, in the interests and by the authority of the National Government, $38,141.01 in excess of appropriations, advanced from January 1, 1868, to June 30, 1886, for the exchange of official Government documents, and $7,034.81 in excess of appropriations from July 1, 1886, to June 30, 1889, advanced for the purpose of carrying out a convention entered into by the United States, or an aggregate of $45,175.82.

A memorandum in regard to this matter was duly transmitted to the Hon. Benjamin Butterworth, a member of the Board of Regents, in the House of Representatives, for the purpose of taking the necessary steps to procure a return by Congress to the Smithsonian fund of this last mentioned sum, namely, $45,175.82, but I am not aware that action has been taken on it.

LIBRARY.

The accessions to the library have been recorded as in the previous year, the entry numbers in the accession book extending from 225,586 to 246,109.

The following statement shows the number of volumes, parts of volumes, pamphlets, and charts received from July 1, 1891, to June 30, 1892.

	Octavo or smaller.	Quarto or larger.	Total.
Volumes	1,320	669	1,989
Parts of volumes	7,631	16,098	23,729
Pamphlets	3,087	502	3,589
Charts			621
Total			29,928

Of these accessions, 297 volumes, 6,363 parts of volumes, and 774 pamphlets—7,434 in all—were retained for use in the Institution and Museum; and 857 medical dissertations were deposited in the library of the Surgeon-General, U. S. Army; the remaining publications were sent to the Library of Congress on the Monday following their receipt.

The reading room continues to be well used; it has only been possible to provide room upon the shelves for new periodicals by removing to the special libraries under the charge of curators or to the Library of Congress such technical periodicals as experience has shown are seldom called for by general readers.

The plan detailed in my report for 1887–'88 for increasing the accessions to the library and for completing the series of scientific journals already in possession of the Institution has been continued; the supplementary work involved by the issue of new scientific journals within the last few years has added somewhat to the work originally planned.

The small collection of books forming what is called "the Secretary's library" has been added to this year, but is already encroaching upon the limited space available for library purposes. These books, as I have stated in my previous reports, are mostly, if not exclusively, books of scientific reference, and are, under certain restrictions, available to all connected with the Institution.

I regret to state that Mr. John Murdoch, who has been the efficient librarian of the Institution since 1887, resigned his position on May 15, 1892. At the close of the year his successor had not been appointed.

MISCELLANEOUS.

Tomb of Smithson.—During the summer of 1891, upon the occasion of a visit to Europe, I made a special journey to Genoa for the purpose of seeing if the place of sepulture of the founder of the Institution was properly cared for. The tomb of Smithson is on the hill of San Benigno, high above the Gulf of Genoa, in a small obscure cemetery, whose existence is unknown to most of the people of the city. It is the property of the English Government and in the immediate charge of the British consul. Smithson's tomb is a substantial structure, but it appears to have had no attention during the sixty years of its existence, though other tombs in the small inclosure give evidence of continued care. A small sum of money, the interest of which is sufficient to defray the expense of the care of the inclosure and tomb, was placed to the credit of the United States consul at Genoa, who kindly consented to take charge of the matter.

Statue of Prof. Baird.—A bill to provide for the erection of a bronze statue of Prof. Baird in the grounds of the Institution was introduced in the Senate by Mr. Morrill, but failed to pass. This was a renewal of previous efforts in this direction and the result is particularly disappointing to the friends of the Institution.

Statue of Robert Dale Owen.—A bill to appropriate $20,000 for a statue to the Hon. Robert Dale Owen, of Indiana, first chairman of the Board of Regents of the Institution and one of its staunchest friends, was introduced in the Senate by Mr. Voorhees and passed, but failed to secure favorable action in the House.

Perkins collection of copper implements.—An amendment to the Sundry Civil Bill providing for the purchase by the Institution of a further collection of prehistoric copper implements belonging to Mr. Frederick S. Perkins, was proposed, but failed to secure favorable action in the House.

Stereotype plates.—The Institution is possessed of a large collection of stereotype plates and engravers' blocks. An effort has been made to arrange these in a systematic manner to facilitate reference, but owing to the pressure of routine work, much yet remains to be done in this direction. It is the policy of the Institution to permit the use of these plates by publishers under reasonable conditions.

Government collections at Washington.—There was passed during the first session of the Fifty-second Congress a joint resolution (H. Res. 92) defining the policy of the Government with reference to the scientific and literary collections, designed to facilitate the use of such collections by students, and to encourage the establishment of institutions of learning at the national capital.

Assignment of rooms.—Pendulum observations by officers of the U. S. Coast and Geodetic Survey have been continued in a basement room specially fitted for such work.

The use of the "chapel" of the Smithsonian building was granted to the American Oriental Society as a place of assembly in April, 1892, and later to the Art Congress for a loan exhibition of works of American artists, held during the session of the Congress in May, 1892.

The Hodgkins gift.—In May, 1891, a letter received from Mr. Thomas George Hodgkins, of Setauket, N. Y., led to a correspondence in which he was advised by the Secretary of the objects of the Institution. At Mr. Hodgkins's request, the Secretary, and subsequently, the Assistant Secretary, made several visits to him at his home, and in conversation with him learned more in detail his wishes with reference to a proposed gift.

Mr. Hodgkins wished to present to the Smithsonian Institution the sum of $200,000, the interest of $100,000 of which was to be used for the general purposes of the Institution in the "increase and diffusion of knowledge among men," provided that the interest of the other $100,000 should be used in the investigation of the properties of atmospheric air considered in its very widest relationship to all branches of science.

Before taking any steps with regard to this offer, a telegram was sent on June 22, 1891, to each Regent who could be reached in this country, requesting his individual opinion of the propriety of accepting Mr. Hodgkins' proposition. Favorable opinions having been received in answer to this from nearly all the Regents, Mr. Hodgkins later, on September 22, at his home on Long Island, placed his gift of $200,000 in cash in the hands of the Secretary, with the understanding that an early meeting of the Regents would be called to consider its formal acceptance under the terms which Mr. Hodgkins proposed.

A meeting of the Regents was therefore called at the earliest day practicable (October 21, 1891), and the matter having been laid before them in detail, the gift was accepted in the terms of the donor.

It seems appropriate at this time to make a statement in elucidation of Mr. Hodgkins's wishes as they have been expressed in various conferences with the Secretary and the Assistant Secretary. It is not his intention that his fund should be applied to special investigation in sanitary science, but he desires rather that the standard of work should be primarily in relation to the demands of pure science, believing that application in many directions would follow. He has spoken of the experiments of Franklin upon atmospheric electricity as one of the investigations which, if carried on at the present day, would be germane to his foundation; and has, in further illustration of his meaning, also referred to the prize awarded by the French Academy of Sciences to Paul Bert for his discovery in regard to the influences of oxygen on the phenomena of vitality, as appropriate to his own proposed foundation. His great interest in the diffusion of knowledge concerning air grows out of his belief that the air is of the highest importance to man in every aspect of his physical and mental condition, and he hopes that his gift will stimulate scientific investigation of the highest order by the best minds, believing that by this means the attention of mankind may best be concentrated and kept concentrated on the importance of the subject. He has expressed a hope that it might be thought advisable to offer some very considerable prize, which, being published to the entire world, would by its magnitude call attention to the subject in which he was so much interested.

Much consideration has been given to the question as to how the donor's wishes may best be carried into effect, for no small difficulty arises from the universality of the application of his foundation, since manifestly there is no branch of natural science which is not affected by it. Meteorology, hygiene and related subjects are most obviously concerned, while others, though less obviously, are no less immediately connected, such as geology, for instance, which has for its field the crust of the earth, now recognized as being largely formed of atmospheric deposits and molded by atmospheric influences. This is only an instance of what we find in the case of nearly every one of the whole circle of sciences, biological and physical, all of which appear on examination to be affected by our knowledge of the atmosphere in a very real and important sense.

In order to secure the advice and co-operation of scientific men throughout the world, letters were addressed to a number of eminent specialists, stating the circumstances of Mr. Hodgkins's gift to the Institution, and explaining his wishes. The following letter is an example:

SIR: I have the honor to inform you that a bequest has been made to the Smithsonian Institution by Mr. Thomas G. Hodgkins, the income of a portion of which is to be devoted to the increase and diffusion of more exact knowledge of the nature and properties of atmospheric air.

In carrying out the donor's wishes, it is proposed to offer a number of prizes for scientific investigations of a high order of merit bearing upon the properties of the atmosphere, to be awarded without regard to the nationality of the author.

While hygiene will occupy a prominent place, it is not intended to limit these prizes to any single class of investigations, however important, but to extend them over the whole field of the natural sciences, as far as these may be regarded as related to each other through the atmosphere as a common bond.

In illustration of my meaning, I may instance as proper subjects for investigation—

1. Anthropology, considering man himself as modified by climate, and his arts as affected by the atmosphere;
2. Biology, in connection with the atmosphere as a fountain of life;
3. Chemistry, in its many obvious relationships to the subject;
4. Electricity, considered in connection with atmospheric electricity;
5. Geology, considered in connection with the action of the atmosphere in its formation and deformation of the surface of the planet;

and so on through almost the whole circle of the sciences.

I now write to ask if you will kindly suggest the nature of the principal relationships existing between physics and the atmosphere, and indicate one or two subjects arising out ot these relations which you consider to be proper tor prize essays.

I shall also be glad to know if you will consent to be a member of a committee to award such a prize, if given, and to learn from you in the same connection of any important research, germane to your own studies, that would be materially advanced by a grant from the funds now available under this liberal construction.

In further illustration of my meaning, I take the liberty of inclosing a copy of a reply made to me in answer to a similar inquiry concerning the science of anthropology, which I do merely to show more clearly the character of the information I desire.

The following was the inclosure. It is an answer by a distinguished anthropologist to a similar question, and was inclosed as an illustration of the fact that the terms of the Hodgkins donation apply even to scientific matters which may appear at first sight disconnected with the subject (*i. e.* to anthropology), but which upon consideration are seen to be intimately related to it:

Dear Sir: In reply to your inquiry concerning the relations existing between anthropology and the study of the atmosphere I beg leave to say that the natural history of man takes into consideration:—

(1) Man, as modified by climate.

(2) His arts as occasioned and affected by the atmosphere.

As to the first, the atmosphere, through climate, elevation, etc., upon man considered as an animal, is believed to have affected his bodily form and stature, the color of his eyes, hair, and skin; his longevity, fecundity, and vigor, and therefore to have been the most potent factor of all in producing those varieties of our species called races, and to be at the foundation of these problems whose discussion constitutes the science of ethnology.

As to the second, most of the arts and activities of man depend upon the atmosphere for their suggestion and methods. For example, his habitations, clothing, and the common occupations of his daily life are most obviously controlled by his atmospheric surroundings, which make him in the Arctic regions a hunter of furs, dwelling underground; in the temperate zone a farmer, dwelling in houses; in the tropics a hunter of ivory, dwelling in open shelters from the sun.

Permit me to observe further, that the study of the air can not be omitted in connection with the science of sociology. Even philology draws its material and perhaps derives it forms largely from the atmosphere, and the primitive philosophies and mythologies of the world are filled with imagery and theories derived therefrom. Therefore in selecting, at your request, from the relationships of the atmosphere to the science of anthropology in general, two or more subjects for prize essays, I have only too much scope.

After much consideration I would propose to suggest that a prize of not less than $1,000 should be offered for an essay upon one of the following topics:

1. The relation of atmospheric phenomena to the cosmogenies, creeds, and cults of all peoples.
2. Atmospheric changes as determining the forms of primitive society, family and tribal organizations, etc.
3. As between the monogenistic and the polygenistic theory of the origin of man, what light is thrown upon the question by a study of atmospheric influences upon man's physical constitution.
4. Atmospheric influences and phenomena as affecting constructive and decorative architecture.

These essays should be presented within a specified time and submitted to the judgment of a committee, of which I should be willing to be a member. Notice of this prize could advantageously be made public through the following special journals: L'Anthropologie, Paris; Archiv für Anthropologie, Braunschweig.

In regard to your inquiry as to any important research germane to the subject in which I am personally interested, which would be advanced by a grant of money, I beg leave to say that I am at present hindered from pursuing my investigations into the influence of climate and other atmospheric phenomena in bringing about the distribution of tribes and stocks of North American aborigines at the time of the discovery, by the need of a small sum of money which might be placed at my disposal. If I had $500 unfettered by conditions, I could within a year's time undertake to bring together the elements for the solution of this problem, which has puzzled for so many years students of ethnology and philology.

I am, very respectfully yours,

* * *

S. P. Langley, Esq.,
Secretary Smithsonian Institution,
Washington, D. C.

As soon as the attention of the public had been directed to Mr. Hodgkins's gift, numerous applications for assistance from the fund were made, and I deemed it advantageous to appoint a special advisory committee, to which might be referred matters pertaining to the Hodgkins fund. This committee was composed of Surgeon John S. Billings, U. S. Army, Director of the Army Medical Museum, in behalf of hygiene and the related sciences; Prof. F. W. Clarke, chemist of the U. S. Geological Survey; Mr. William H. Dall of the U. S. Geological Survey, well known for his biological and anthropological studies; Prof. William C. Winlock, in behalf of astronomy and physics, and the Assistant Secretary of the Institution, Dr. G. Brown Goode, who acted as chairman. The committee has held several meetings, and I desire at this time to express my high appreciation of the value of the work which they have already done, both as a committee and individually. At the close of the year, the committee had under consideration, at my request, a form of circular to be issued to learned institutions and investigators throughout the world, calling attention to the establishment of the Hodgkins fund, and announcing certain prizes which it is intended to offer for essays upon specified subjects.

THE NATIONAL MUSEUM.

I took occasion in my last report to invite your attention to the fact that the very rapid growth of the collections of the Museum was becoming, under existing circumstances, a source of great embarrassment. The difficulties of the situation have increased during the past year, since, while the influx of specimens has continued, no additional space has been provided for their reception and only an insignificant additional sum of money for their maintenance.

This unsolicited increase of the collections should properly be a source of gratification rather than of embarrassment. Growth is essential to the welfare of a museum, and to check it is sure to produce unfortunate results. It seems undesirable to say to the friends of the Museum that their valuable donations can not be received. Such a course would alienate their sympathy, and the Museum would lose the advantage of their good offices. Under existing conditions, however, the necessity of resorting to so undesirable a measure is perilously near. The increase of the collections from certain other sources can not even thus be checked.

Large collections are made every year by the Department of Agriculture, the Geological Survey, the Fish Commission, and other Departments and Bureaus of the Government, either as an essential part of their work or incidentally. By provision of law the Museum is made the custodian of these collections, and it can not, therefore, do otherwise than to receive and preserve them.

Many valuable objects are exposed to dust and vandalism from the lack of sufficient money to procure the necessary cases for their protection. Series of objects, such as the great Lacoe collection of fossil plants, recently acquired, are frequently offered with the condition that suitable cases be provided. For the safe-keeping of the objects already in the possession of the Museum and for the reception of those offered, numerous storage and exhibition cases are a necessity.

The number of specimens of all kinds in the Museum at the close of the year, as shown by the following table, nearly equalled three and a quarter millions. The increase for the year was about 260,000 specimens, or nearly double that of 1891.

Table showing the annual increase in the departments of the National Museum.

Name of department.	1882.	1883.	1884.	[1] 1885–'86.	1886–'87.
Art and industries:					
Materia medica		4,000	4,142	4,850	5,516
Foods		1,244	1,580	822	877
Textiles			2,000	3,063	3,144
Fisheries			5,000	9,870	10,078
Animal products			1,000	2,792	2,822
Graphic arts					
Transportation and engineering					
Naval architecture			600		
Historical relics				1,002	13,634
Coins, medals, paper money, etc				1,005	
Musical instruments				400	417
Modern pottery, porcelain, and bronzes				2,278	2,238
Paints and dyes				77	100
"The Catlin Gallery"				500	500
Physical apparatus				250	251
Oils and gums				197	198
Chemical products				659	661
Domestic animals					

[1] No census of the collection taken.

Table showing the annual increase in the departments of the National Museum—Continued.

Name of department.	1882.	1883.	1884.	[1] 1885–'86.	1886–'87.
Ethnology	....	....	200,000	500,000	503,764
American aboriginal pottery	....	....	12,000	25,000	26,022
Oriental antiquities	....	....	....	....	....
Prehistoric anthropology	35,512	40,491	45,252	65,314	101,659
Mammals (skins and alcoholics)	4,660	4,920	5,694	7,451	7,811
Birds	44,354	47,246	50,350	55,945	54,987
Birds' eggs and nests	....	....	40,072	44,163	48,173
Reptiles and batrachians	....	....	23,495	25,344	27,542
Fishes	50,000	65,000	68,000	75,000	100,000
Vertebrate fossils	....	....	....	....	....
Mollusks	33,375	....	400,000	460,000	425,000
Insects	1,000	....	151,000	500,000	585,000
Marine invertebrates	11,781	14,825	200,000	350,000	450,000
Comparative anatomy:					
Osteology	3,535	3,640	4,214	} 16,210	} 11,022
Anatomy	70	103	3,000		
Paleozoic fossils	....	20,000	73,000	80,482	84,491
Mesozoic fossils	....	....	100,000	69,742	70,775
Cenozoic fossils	(Included with mollusks.)				
Fossil plants	....	4,624	7,291	7,429	8,462
Recent plants ([2])	....	....	....	30,000	32,000
Minerals	....	14,550	16,610	18,401	18,601
Lithology and physical geology	9,075	12,500	18,000	20,647	21,500
Metallurgy and economic geology	....	30,000	40,000	48,000	49,000
Living animals	....	....	....	....	....
Total	193,362	263,143	1,472,600	2,420,944	2,666,335

Name of department.	1887–'88.	1888–'89.	[3] 1889–'90.	1890–'91.	1891–'92.
Arts and industries:					
Materia medica	5,762	5,942	[4] 5,915	6,083	6,290
Foods	877	911	1,111	1,111	1,111
Textiles	3,144	3,222	3,288	3,288	3,288
Fisheries	10,078	10,078	10,080	10,080	10,080
Animal products	2,822	2,948	2,949	2,994	2,994
Graphic arts	....	....	[5] 600	974	1,174
Transportation and engineering	....	....	[5] 1,250	1,472	1,737
Naval architecture	....	600	[6] 600	[6] 600	600
Historical relics	} 14,640	14,990	20,890	23,890	28,390
Coins, medals, paper money, etc					
Musical instruments	427	427	447	542	636
Modern pottery, porcelain, and bronzes	3,011	3,011	3,132	3,144	3,232

[1] No census of the collection taken.

[2] Up to 1890 the numbers have reference only to specimens received through the Museum, and do not include specimens received for the National Herbarium through the Department of Agriculture. The figures given for 1890–'91 include, for the first time, the number of specimens received both at the National Museum and at the Department of Agriculture for the National Herbarium.

[3] The actual increase in the collections during the year 1889–'90 is much greater than appears from a comparison of the totals for 1889 and for 1890. This is explained by the apparent absence of any increase in the department of lithology and metallurgy; the total for 1890 in both of these departments combined, showing a decrease of 46,311 specimens, owing to the rejection of worthless material.

[4] Although about 200 specimens have been received during the year, the total number of specimens in the collection is now less than that estimated for 1889, owing to the rejection of worthless material.

[5] The collection now contains between 3,000 and 4,000 specimens.

[6] No estimate of increase made in 1890 or 1891.

Table showing the annual increase in the departments of the National Museum—Continued.

Name of department.	1887–'88.	1888–'89.	([1])1889–'90.	1890–'91.	1891–'92.
Arts and industries—Continued.					
Paints and dyes	100	109	197	197	197
"The Catlin Gallery"	500	500	([2])	([2])	
Physical apparatus	251	251	263	273	273
Oils and gums	198	213	1,112	1,112	1,112
Chemical products	661	688			
Domestic animals			66	97	103
Ethnology	505,464	506,321	508,830	510,630	512,871
American aboriginal pottery	27,122	28,222	29,269	30,188	32,305
Oriental antiquities		850	3,485	3,187	3,487
Prehistoric anthropology	108,631	116,472	123,677	127,761	137,087
Mammals (skins and alcoholics)	8,058	8,275	8,836	9,301	10,387
Birds	56,484	57,974	60,219	([3])62,601	68,416
Birds' eggs and nests	50,055	50,173	51,241	52,166	58,260
Reptiles and batrachians	27,664	28,405	29,050	29,935	30,939
Fishes	101,350	107,350	122,575	127,312	129,218
Vertebrate fossils			([4]) 512	521	1,582
Mollusks	455,000	468,000	471,500	476,500	482,725
Insects	595,000	603,000	618,000	630,000	646,500
Marine invertebrates	515,000	515,300	520,000	526,750	533,870
Comparative anatomy:					
Osteology } Anatomy }	11,558	11,753	12,326	12,981	([5]) 12,555
Paleozoic fossils	84,649	91,126	92,355	92,970	93,839
Mesozoic fossils	70,925	71,236	71,305	79,754	82,853
Cenozoic fossils	(Included with mollusks.)				
Fossil plants	10,000	10,178	10,507	10,685	110,685
Recent plants ([6])	38,000	38,459	39,654	80,617	134,001
Minerals	21,896	27,690	37,101	44,236	48,357
Lithology and physical geology	22,500	27,000	32,762	64,162	([7]) 35,787
Metallurgy and economic geology	51,412	52,076			
Living animals	220	([8]) 491			
Total	2,803,459	2,864,244	2,895,104	3,028,714	3,226,941

[1] The actual increase in the collections during the year 1889–'90 is much greater than appears from a comparison of the totals for 1889 and for 1890. This is explained by the apparent absence of any increase in the department of lithology and metallurgy; the total for 1890 in both of these departments combined, showing a decrease of 46,311 specimens, owing to the rejection of worthless material.

[2] Included in the historical collection.

[3] The total number of specimens in the department of Birds in 1890–'91 was 62,806 instead of 62,601.

[4] Only a small portion of the collection represented by this number was received during the year 1889–'90.

[5] The decrease in this department for the year 1891–'92 was occasioned by the transfer of 1,000 skeletons to the department of vertebrate fossils.

[6] Up to 1890 the numbers have reference only to specimens received through the Museum, and do not include specimens received for the National Herbarium through the Department of Agriculture. The figures given for 1890–'91 include, for the first time, the number of specimens received both at the National Museum and at the Department of Agriculture for the National Herbarium.

[7] Collections combined in October, 1889, under Department of Geology. The apparent decrease of more than 50 per cent of the estimated total for 1889 is accounted for (1) by the rejection of several thousands of specimens from the collection, and (2) by the fact that no estimate of the specimens in the reserve and duplicate series is included.

[8] Transferred to the National Zoölogical Park.

NOTE.—The fact that the figures for two successive years relating to the same collection are unchanged, does not necessarily imply that there has been no increase in the collection, but that for some special reason it has not been possible to obtain the figures showing the increase.

Condition of the exhibition halls.—The results of over-crowding are evident everywhere in the exhibition halls. The installation of the collections and the comfort of visitors are interfered with. It has become necessary to narrow the aisles in many halls to such a degree that they are almost impassable, and on occasions when unusual numbers of visitors are in the city, many objects of interest have to be withdrawn from exhibition. The unavoidable crowding of the cases interferes with the lighting, so that many objects are practically hidden from view.

To relieve the present pressure, as regards space, I have, in addressing Congress, brought forward two propositions. For immediate and temporary relief I have recommended the erection of light galleries in two of the halls, with the intention of hereafter asking for others of the same character. Such galleries, unlike those in the main Smithsonian hall, were provided for in the original plans of the building, and can be erected without detracting from the appearance of the halls.

While these galleries would add materially to the available exhibition space, we must look to the erection of a new museum building for more permanent relief from the present overcrowded condition. A bill providing for the construction of a new building has twice received favorable action by the Senate, but has failed to pass the House.

It is greatly to be hoped that both the galleries and also an additional building may be provided without further delay.

Curatorships.—There are now in the Museum thirty-three organized departments and sections, under the care of eight curators, paid by the Museum, and twenty honorary curators, detailed for special duty from different bureaus of the Government. While the latter render very important and highly appreciated services, they are, of course, more especially occupied with their own peculiar duties, and can not devote more than a small portion of their time to the interests of the Museum. The technical character of the duties of the curators renders highly desirable the employment of a larger paid staff of men who have had special training for museum work. In order to secure the services of such persons, however, and to obtain the best results for the Museum, greater inducements should be offered in the way of compensation. There are few professors in our colleges who do not receive larger salaries than it is now possible to pay the curators of the Museum, who, nevertheless, in addition to their onerous executive duties as custodians of the collections, are expected to furnish scientific information for replies to the thousand of inquiries received every year.

It may be added that the proper preservation of the collections often entails much manual labor, and in many instances immediate and strenuous efforts are needed to save from entire loss large collections of a perishable nature. Urgent work of this kind is not unfrequently performed by the curators.

It is most desirable that the scientific staff of the Museum should be permanently identified with it, and this condition can hardly be reached unless a majority at least of the curators are paid from its appropriations. The permanent assignment of the curators to their respective departments, with adequate compensation and the absence of extraneous duties, would materially advance the work of the Museum on its scientific side.

The lack of means to employ a sufficient number of assistants in the lower grades causes a large amount of minor routine work to fall on the curators, who are capable of rendering services of a higher character. On account of this condition of affairs many plans of the greatest importance to the Museum are held in abeyance from year to year, or are never consummated.

Clerical force.—Allusion has been made in my former reports to the need of additional clerical assistance in the Museum. This need becomes greater every year as the collections increase in magnitude. The salaries paid for clerical work are less than in the executive departments of the Government and elsewhere, and the Museum on many occasions has lost the services of competent clerks, trained in their special work, who have been attracted to other fields of labor by higher compensation. Some of the departments in the Museum are entirely without clerical assistance, and the curators are obliged to devote time which could be much better employed, to the simple but necessary work of cataloguing and labelling specimens, preparing invoices, and unpacking boxes.

For the safekeeping of the collections, which have greatly increased in intrinsic value as well as extent, a larger number of watchmen is necessary. The force is now so small that it is difficult to grant the usual leaves of absence without exposing the collections to danger. It is also with difficulty that the cleanliness of the floors and cases is maintained, on account of the limited number of laborers and cleaners which the present appropriation will permit the Museum to employ.

Distribution of specimens.—The distribution of duplicate material to educational institutions has been continued as far as practicable. This means of diffusing knowledge is one of the most popular features of the Museum work, and has been carried on unceasingly for nearly half a century, during which time nearly half a million specimens, embracing mammals, fishes, marine invertebrates, birds, shells, rocks, ores, minerals, and ethnological objects, have been given to Museums and other educational institutions in the United States, while important exchanges with similar institutions abroad have resulted in much good to the Museum. This work, too, is now being seriously hindered, owing to lack of space for the proper handling and separation of the duplicate material, and its classification and arrangement into series for distribution.

The material distributed during the year consisted chiefly of minerals, marine invertebrates, and casts of prehistoric stone implements, and amounted to 32,098 specimens.

Publications.—There has been unusual activityin the work of this department of the Museum during the year. The report for 1889 has been published, and the report for 1890 has been put in type. The manuscript of the report for 1891 was sent to the Public Printer and is now going through the press. Vol. XIII of the "Proceedings" of the National Museum has been published. Of the "Bulletin," Nos. 39 (Parts A to G), 41 and 42 have been issued.

The Proceedings and Bulletins of the National Museum are not "public documents," hence no part of the edition is regularly apportioned for distribution by the Senate and House, or to the legal depositories. The edition of 3,000 copies, now printed, is only sufficient to supply in limited measure the very urgent requests from public libraries, educational institutions, and scientific investigators in the United States and throughout the world. A larger appropriation for printing is needed, so as to enable the Museum to place a full series of its publications in representative libraries in different parts of each State. It is not the intention that the annual number of issues of the Proceedings and Bulletins should be increased, but that a larger edition of each should be printed. On account of the small edition, the Museum fails to receive in exchange the valuable publications of many scientific institutions.

The amounts hitherto appropriated, though expended with strict economy, have been found inadequate.

Visitors.—The total number of visitors to the Smithsonian building during the past year was 114,817, and to the Museum 269,825; total, 384,642. This is an increase of 13,453 over the previous year.

Heating and lighting.—The larger part of this appropriation is expended for fuel and gas. As has been explained in connection with the estimates for previous years, it is necessary for the safety of the collection that the buildings should be kept at a nearly even temperature day and night throughout the winter. The reduction of this appriation below the minimum of $12,000 will make a deficiency estimate necessary. From lack of fuel, required to maintain the proper temperature, some of the offices had to be abandoned on several occasions during the winter of 1892. The longer the heating apparatus is used the less efficient it becomes, and of late it has been necessary each successive year to expend a larger sum for replacing worn-out parts. The wires of the burglar alarms, watchman's call boxes, and other electrical apparatus, have deteriorated from long use, and need immediate attention.

There are at present in use in the Museum building twenty-five arc lights, and this number is not sufficient to illuminate the entire building, there being no lights in the courts and an insufficient number in the halls. It is thought that with an additional plant, costing about $5,000, the building may be so lighted that it can be thrown open occasionally to the public at night, to the advantage of those persons who are unable to avail themselves of the regular hours of exhibition.

The purchase of an additional engine will also render it possible to provide against the contingency of total darkness in case of damage to dynamo, line, or motor.

By the appropriation of $5,000 for the removal of decayed wooden floors, and the substituting of granolithic or artificial stone pavement therefor, it has been possible to complete a much needed improvement in several of the halls and courts of the Museum.

With a view of securing the best pavement possible, as well as for the purpose of obtaining for future guidance a practical knowledge of the merits of the artificial stone flooring made by different bidders, three proposals which did not vary materially in amount, were accepted. It will require a greater length of time than has yet elapsed to pronounce upon the relative merits of these pavements, but they have already proved themselves far more satisfactory than the wooden floors for which they were substituted, and it is hoped that it will soon be possible to put down the same, or some equally durable form of pavement, in the parts of the museum which still lack this improvement.

The World's Columbian Exposition.—The work of preparing an exhibit for the World's Fair in Chicago has been continued during the year. A full report of the participation of the Smithsonian Institution and the National Museum in this exhibition will be deferred until such time as a complete statement can be made.

BUREAU OF ETHNOLOGY.

Ethnological researches among the North American Indians have been continued by the Smithsonian Institution, in compliance with acts of Congress, during the year 1891–'92, under the direction of Maj. J. W. Powell, who is also Director of the U. S. Geological Survey.

The work of the Bureau of Ethnology during the year has been conducted on the same systematic plan before explained as in successful operation. The authors of the publications of the Bureau prepare them from material personally gathered by themselves in the field, which is supplemented by study of all the information attainable from other sources.

In addition to the issue during the year of the Seventh Annual Report and of six other volumes of publications, mentioned under that heading in the report of the Director hereunto appended, at the close of the fiscal year the Eighth and Ninth Annual Reports were in type, the tenth had been delivered to the Public Printer, and the eleventh and twelfth were on file ready for delivery to that official as soon as there should be any prospect that their printing could be commenced. Other reports and papers not intended to form parts of the series of annual reports were also filed as ready for printing.

Another feature of the year's work consisted in the collection by officers of the Bureau, under the authority of law, of ethnologic objects for the exhibit at the World's Columbian Exposition. This authority was opportune, as objects of that character are becoming scarce and costly and probably could not, after a few months, be secured for preservation in a permanent collection. A similar work of preservation, also authorized by law, was executed in the restoration of the ruin of Casa Grande, in Arizona.

Mention of these special operations does not imply that the researches into the religions, customs, history and other ethnologic data of the Indian tribes were omitted during the year. Details respecting all the work of the Bureau will be found in the report of its director, given in the Appendix.

NATIONAL ZOOLOGICAL PARK.

The insufficiency of the appropriations for the maintenance of the National Zoological Park was pointed out in the report for the year ending June 30, 1891, and experience amply supports the opinions there expressed. It does not seem superfluous to repeat the following passage from this last report:

"The primary object for which Congress was asked to establish a National Zoological Park was to secure the preservation of those American animals that are already nearly extinct, and this object it was thought would be best secured by the establishment of a large inclosure in which such animals could be kept in a seclusion as nearly as possible like that of their native haunts. It was believed that, except for initial expenses for buildings and roads for the public, this could be done with an outlay comparatively small, probably not exceeding $50,000 a year; for, after the necessary land was once acquired and fenced in, smaller inclosures and paddocks could be set off and inexpensive barns erected at about this yearly charge.

It was, in the nature of things, inevitable that some provision should be made for the convenience of a curious and interested public, as well as for the care and well being of animals unaccustomed to the presence of man. For the first of these it was intended to set aside a considerable area, on which the principal buildings should be placed and to which should be taken, as was expedient, such of the animals as might interest the public, the larger portion of the park being still considered as a natural preserve where animals need be disturbed by no unusual surroundings, and where it was hoped they might, after the time necessary for their acclimation, breed their young.

The maintenance of a park devoted to these purposes, that is, primarily to useful and scientific ends, and secondly to recreation, seemed to those interested in its success a legitimate tax upon national resources, but when Congress decided that one-half of the necessary expenses should be raised by local taxation it seemed only fit that the tax-payers should be heard in their wish to have prominence given to the feature that principally interested them, and their chief interest was naturally in the park as a place of recreation. That this was recognized by a considerable body in Congress became evident from the subsequent debates.

The moral right of the people of the District to ask consideration of their wishes for entertainment in return for the outlay which falls upon them can not be questioned, and so far as this could be recognized it introduced a tendency to provide an establishment more like an ordinary zoological garden, or permanent menagerie, than the comparatively inexpensive scheme at first contemplated.

In view of the circumstances an appropriation was asked of Congress, which was believed to be smaller than was consistent with the proper ultimate development of the park, but on an estimate which proposed to begin on the most economical scale. Thus, for the general maintenance of the collection, $35,000 was asked, which is about the same as the annual sum spent in the Central Park menagerie, New York, having an area of about 10 acres, and at least $10,000 less than is spent either at the zoological garden in Cincinnati or Philadelphia, each having an area of about 40 acres. When it is reflected that these latter enterprises are conducted for business purposes by business men, that they have their collections already nearly complete and purchase but few new animals, it will be seen that the sum asked for the maintenance of the 67 acres of the National Zoological Park with all the expensive animals yet to be procured was certainly not extravagant. Congress reduced this estimate to $17,500, a sum for which as a year's experience has now shown the Park can not be maintained.

For buildings, an appropriation of $36,850 was asked. In this connection it may be recalled that in the Philadelphia gardens the buildings and inclosures cost $194,705. The sum estimated was intended to cover all inclosures and structures of every character indispensable on the modest scale proposed. Congress reduced this to $18,000.

The average expense of preparing such uncultivated grounds in city parks elsewhere has proved to be at least $2,900 per acre. The sum of $29,500 was asked for that purpose, as no more than sufficient to fit such portions of the park as were necessary for the immediate accommodation of the public. Congress reduced this to $15,000.

These reductions have not only obliged me to retard the development on the lines that had been laid down, but have increased the ultimate cost; for where living creatures are in question it is plain that they have not only to be fed and guarded but to be housed; and all this at once, under penalty of their loss. Congress has plainly intended that they should be preserved, and that some sort of roads and access for the public should be provided this year.

The result has necessarily been, that with every effort to obtain permanent results there has been a partial expenditure of the absolutely insufficient grant on enforced expedients of a temporary character, which are not in the interest of economy.

It is extremely desirable that a sum for emergencies be secured in the next appropriation. In carrying forward, from the beginning, novel and untried work of such varied character, unforeseen difficulties must inevitably arise, but no provision has been made for these, nor even for such readily anticipated emergencies as are caused, for instance, by floods in grounds traversed by a stream which has been known to rise 6 feet in less than half an hour.

The difficulties which these conditions have imposed on the administration of the park may be fairly called extreme, and the amount and character of what has been effected must be considered in this connection. In spite of these the result, I think, may be said to be, that at least as a source of interest and amusement to the people the park has exceeded the most sanguine expectations."

It will be observed that, of the $101,350 asked, two-thirds were for buildings and grounds which, if not provided for, could wait with comparatively little inconvenience, while the remaining third, or $35,000, was for the care and food of living animals, for policing of the park and for the safety of the public, matters which, when the garden was once opened, could not wait, and could not be materially diminished, but constituted a comparatively fixed sum without which the park could not go on, and which should therefore be given nearly as it stood or withheld altogether. Congress, however, it will be seen, reduced all these items nearly in the same proportion, that is, to about one-half.

Two-thirds of the desired appropriation was of a nature that could perhaps be reduced one year and made good later; the other third (that for food of living animals and maintenance), as painful experience has shown, could not be materially reduced and could not be made good later; and it is the deficiency on this item that has been the special cause of the difficulties of the administration.

Inadequacy of appropriations.—Embarrassment also arose from the fact that the small amount appropriated was specified and allotted under three separate subordinate heads and in three nearly equal amounts, although the needs were not equal. As the bounds of these allotments could not be overstepped, it occurred that, while there were relatively sufficient funds under one item (the care of grounds), there was entire inadequacy under the much more essential head which provided for the maintenance and care of living animals. No matter how great the emergency or serious the need, it was, of course, impossible to change this allotment, and while the total appropriated by Congress might, by close economy, have been sufficient, yet there was danger that the animals would be unfed and that the force of watchmen and keepers, although overworked, would be inadequate for their proper protection; and as there existed no authority to give away or sell the animals, disaster of some kind would have ensued but for the aid indirectly given by the Smithsonian Institution.

It may here be mentioned that it was expected that a large number of animals would be obtained from the Yellowstone National Park, that being the principal great preserve for wild game controlled by the Government of the United States. With the consent of the honorable the Secretary of the Interior, a hunter was employed to capture large wild animals in considerable numbers, which were to be forwarded to the park at Washington. When a number of bears, deer, and elk were thus obtained, the reduced appropriations were insufficient to continue his employment or to transport the animals already captured. A still more regrettable consequence was the necessity of refusing absolutely all gifts made by the public, as there were no means of paying for the transportation of animals or for their subsistence when received. This has been a serious disadvantage to the collection, not only at present, but as regards its future, for it need hardly be said that it has discouraged and rebuffed many public-spirited citizens who would have been glad to present animals to the park, and who will now cease to have any further interest in the enterprise.

Dangers by freshet.—On the 5th of September, 1891, a freshet of unusual violence invaded the valley of Rock Creek. Such was the rapidity of the increase of water that in less than half an hour the little stream had risen 6 feet and had become a torrent of considerable magnitude and power. The piers for the bridge had just been completed, but the banks above and below were not yet protected from the abrasion of a flood. In consequence of this the water formed an eddy near one of the piers, causing it to break, and cracked one of the abutments. It is believed that this unfortunate accident was not due to any defect in the design of the pier (which was constructed under the competent supervision of the late Gen. Meigs), but rather to the fact that the freshet occurred before the neighboring banks were properly prepared.

The damage to the pier was by no means the total extent of that caused by the flood. The bear-yards, then nearly completed and ready for occupation, were very seriously injured by the precipitation into them of many tons of rock and earth. This made it evident that the bank of earth and decomposed rock on the cliff above the yards could not be depended on without some additional safeguard. The heavy fall of water seriously injured and cut away the new roads, gutters, and drains that were yet fresh and unsettled, removed whole banks of earth from fresh slopes and washed out trees and bushes. The creek changed the level of its banks, cutting out a new channel for itself in several places, and covered the slopes with hundreds of tons of gravel and sand, and occasionally even deposited considerable stones, which were lifted by the rushing water and left upon the grass as a striking evidence of the violence of the flood. Immediate steps were commenced to repair the damage, but this work was not completed within the fiscal year on account of the insufficiency of the appropriation.

The bear-yards are in an abandoned quarry, adjoining a precipice whose summit is upon the extreme boundary line of the park. For this reason no permanent protection can be provided until the Government secures the few contiguous roods needed at the top. With this the summit of the precipice, formed of the original rock, would constitute the cheap and natural barrier. For protection under the actual, existing conditions, the only measure (and it is both incomplete and expensive) is to build a retaining wall reaching from the solid rock of the cliff high enough to hold any detritus that might be displaced by the action of rain or frost.* This has been commenced, but left incompleted owing to lack of funds.

*See illustration, Plate II, page 41.

A considerable force of men was employed in repairing the roads, gutters, and drains, and in diverting the course of the stream so as to prevent further erosion of the banks. The amount expended in partially repairing the damage caused by the freshet was nearly $5,000. This unexpected demand upon already insufficient appropriations was another cause of embarrassment.

Influx of visitors.—Public interest in the park has steadily increased from the beginning, and even in its present unfinished state the number of visitors in a single day sometimes reaches from five to ten thousand or more. It was supposed that when the collection should be of notable size, when the buildings were completed, the grounds improved, and the means of access ample, that a large number of visitors would frequent the park, but so very large and so immediate an attendance could hardly have been anticipated. It was found that the force of watchmen was quite insufficient properly to direct and control the throngs of people that on holidays passed through the unfinished houses and along the roads and paths. There are five entrances to guard, and eight separate houses and inclosures where animals and property are kept, so far distant from one another that a watchman or keeper should be stationed at each, while in the larger houses, like the general animal house and the elephant house, it is desirable to have more than one keeper on hand, during the presence of great crowds, both for the purpose of protecting children as well as to prevent mischievous individuals from injuring the animals. The services of the keepers are required chiefly in the day, but there must be watchmen to relieve each other during the whole twenty-four hours. Under these circumstances, the appropriation allowed—for the guarding of the animals, the public, and the policing of the 167 acres by day and by night—but six men including both watchmen and keepers.

Deficiency appropriation.—In view of these and other circumstances it seemed proper to ask a measure of relief from Congress. The following estimates were accordingly framed and a deficiency appropriation asked to meet them:

National Zoölogical Park: Improvements—

For continuing the construction of roads, walks, and bridges, and for grading, planting, and otherwise improving the grounds of the National Zoölogical Park, being a deficiency for the fiscal year 1892 .... $4,870.81

NOTE.—This appropriation is rendered necessary because of the storm of September 5, 1891, which greatly damaged the works of improvement in the park. The sum asked is for the purpose of reimbursing the appropriation for the amount actually expended in repairing those damages and preventing similar occurrences for the future.

National Zoölogical Park: Maintenance, etc.—

For care, subsistence, and transportation of animals for the National Zoölogical Park, and for the purchase of rare specimens not otherwise obtainable, including salaries or compensation of all necessary employés and general incidental expenses not otherwise provided for, being a deficiency for the fiscal year 1892 .... $4,434.00

National Zoölogical Park: Maintenance, etc.—Continued.

NOTE.—This sum includes:

Payment of extra watchmen on Sundays and holidays, necessary because of the great influx of visitors, 18 men, 19 days each, at $2 ..	$684.00
Transportation of specimens already offered to and purchased by the park, viz.:	
From Yellowstone Park	350.00
From South America	500.00
From Australia	500.00
Care and maintenance of the above animals	900.00
Care and maintenance of the elephants presented and lent to the park.	1,500.00

National Zoölogical Park: Organization, improvement, maintenance—

For repairs to the Holt mansion to make the same suitable for occupancy, and for office furniture:

To pay Devereux & Gaghan, plumbing and gas fitting	$320.47	
To pay Julius Lansburgh, chairs	14.00	
To pay Barber & Ross, grates	46.00	
To pay George Breitbarth, chairs	25.75	
To pay A. Eberly's Sons, stoves	20.35	
Total		$426.57

NOTE.—The above liabilities were incurred under the supposition that they could properly be charged against other items of this appropriation. The First Comptroller is of the opinion that they should be charged against this item.

To reimburse the Smithsonian fund for assuming the expenses of labor and materials for repairs urgently necessary for the preservation of the Holt mansion, including the following:

C. Burlew, concreting and pitching	$60.48	
Belt & Dyer, doors and moldings	37.11	
H. C. Mounie, lathing and plastering	173.64	
C. W. Dawes, carpentry	24.00	
W. O. Stricker, carpentry	33.00	
Church & Stephenson, lumber	116.22	
O. L. Wolfsteiner & Co., skylight	55.00	
Total		$499.45

NOTE.—The amount appropriated by Congress for repairs to the Holt mansion was expended before the roof was covered in, and upon the decision of the Comptroller that it could not be covered in from the item for "expenditures not otherwise provided for," the Smithsonian Institution advanced this sum from its private funds to prevent the destruction by the weather of what had already been done.

For current expenses—

To pay Melville Lindsay for rubber boots furnished to employés engaged to work in water in the National Zoölogical Park 38.00

961.02

NOTE.—These boots were issued to the men each morning and taken from them at night, being worn only while on duty. The First Comptroller holds that the sum can not properly be paid without special legislation.

(All being for the service of the fiscal year 1891.)

The following deficiency appropriation was made by Congress under date of March 8, 1892:

For care and subsistence of animals for the National Zoölogical Park, fiscal year eighteen hundred and ninety-two, one thousand dollars, one-half of which sum shall be paid from the revenues of the District of Columbia and the other half from the Treasury of the United States.

Damages occasioned by the undue reduction of force.—As the season advanced and no additional appropriation was made by Congress, it became necessary to reduce the expenses of the park still further. This was done by stopping all work upon the buildings and grounds, and reducing the force till one watchman only could be on duty at a time, and much danger to the public and many accidents to the animals ensued in consequence. The deer and antelope were annoyed and injured by dogs, the flock of valuable Angora goats was nearly destroyed by being poisoned by visitors with laurel (*Kalmia latifolia*), and many other injuries were inflicted on the animals, while the administration was in anxiety lest some grave accidents, such as were almost to be expected under these circumstances, should occur among the crowds of visitors, embracing not only adults, but children, of the latter of whom there were often many hundreds present and unprotected.

That this anxiety was not unwarranted was shown on the night of May 24, when a grizzly bear, during the absence of the single watchman, scrambled up the almost perpendicular cliff in the rear of the yards and escaped from the park. After fruitless attempts to capture him, and the injury of one of the employés whom he wounded, orders were reluctantly given to shoot him.

The following letter, setting forth the urgent needs of the park, was addressed to the Secretary of the Treasury on January 23:

SMITHSONIAN INSTITUTION,
Washington, D. C., January 23, 1892.

SIR: I beg leave to invite your attention to the estimates under the Smithsonian Institution for the fiscal year ending June 30, 1893, duly submitted to you October 7, 1891, and to the modified form in which these estimates were transmitted to Congress, whereby it would seem to be recommended that no increase be made over the amounts appropriated for the current year.

While feeling that all the amounts asked for by the Institution have been only such as are adequate with the strictest economy, I have to ask your especial attention to the three items for the National Zoölogical Park, *i. e.*, Improvements, Buildings, and Maintenance. Disasters from floods and like contingencies for which no provision was made by Congress in the appropriations for the present year emphasize the necessity of securing the full amount estimated under the headings Improvements and Buildings, while there exists exceptional necessity in the item for Maintenance, which is essentially for the food and care of living animals.

The appropriations made by the act of March 3, 1891, for "maintenance" during the present fiscal year (for which $35,000 was asked), was $17,000, but the sum of $5,122.71 from the appropriation of April 30, 1890, was available and has been used for this purpose; and even with this addition it has been necessary to ask for a deficiency appropriation of $4,434, chiefly to cover expenditures which were found to be absolutely necessary to prevent loss to the Government.

The minimum expenditures for the present year under this item will therefore be $22,622.71; the expenses for the first six months being $14,269.73, or at the rate of $28,539.46 per annum. I trust, therefore, that it is made sufficiently clear that with an appropriation of $17,500 it will be impossible to properly care for and feed the animals now on hand.

The past expenditures would have been still larger but that the work on the accounts for the Treasury has in part been done gratuitously by the Institution, which has also supplied free of cost office rooms, as well as the aid and supervision of unpaid naturalists. This can not be reckoned upon for the future, but has been sanctioned by the Regents as a means to meet the exigency until the need of a larger appropriation can be represented to Congress, and in the meantime the working force has been reduced to an extreme degree, the policing, for instance, being now done by one watchman, aided by two employés who are largely engaged with other duties; and these three men are required to maintain order over an area of 167 acres, visited during each day by thousands of people. These details are mentioned in connection with the fact that (unless some small purchases of animals made at the outset be excepted) it is under like stringencies of economy in every branch of the administration that the expenses have already amounted, as shown above, to more than $14,000 in six months.

I can not too emphatically represent the peculiar difficulties that must arise in administering an insufficient appropriation for the care of living wild animals, unable to care for themselves where they are, if no provision has been made by Congress for disposing of them elsewhere.

In view of increased expenses since the estimates were prepared, due directly to the unexpectedly great popular interest manifested in the park, and to the extraordinary increase of visitors, I now feel compelled to either increase the estimate for maintenance to $30,000, to cover further contingencies, or to ask that the total appropriation requested for the park be made in such form as to allow a certain discretionary power to meet them. If under the circumstances stated, the latter would, in your judgment, be the more advisable course, I would respectfully ask that you recommend to Congress that the three items of improvements ($20,000), buildings ($27,000), and maintenance ($26,000) be appropriated in one sum of $73,000, as follows:

National Zoölogical Park, Smithsonian Institution:

Continuing the construction of roads, walks, bridges, water supply, sewerage, and drainage, and for grading, planting, and otherwise improving the grounds, erecting, and repairing buildings and inclosures for animals and for administrative purposes, care, subsistence, and transportation of animals and for the purchase or exchange of specimens not otherwise obtainable, including salaries or compensation of all necessary employés, and general incidental expenses not otherwise provided for, $73,000.

I have the honor to be, very respectfully, yours,

S. P. LANGLEY,
Secretary.

The SECRETARY OF THE TREASURY,
Washington, D. C.

Notwithstanding this urgent appeal, it was found, when the sundry civil appropriation bill was reported to the House, that but $29,000 was recommended to be appropriated for the National Zoölogical Park. This was divided into the following heads:

Improvements	$9,000
Buildings	10,000
Maintenance	10,000

The matter seemed to me so urgent and serious as to demand the immediate attention of the Regents. I therefore called a special meeting of the Board and laid the matter before them. The result of that meeting will be seen in the following letter addressed to the President of the United States Senate.

SMITHSONIAN INSTITUTION,
Washington, April 2, 1892.

SIR: In accordance with the instruction of the Regents of the Smithsonian Institution, I have the honor to transmit a resolution passed by them on the 29th of March, 1892, together with the following preliminary statement of the considerations on which it is based:

The National Zoölogical Park was placed under the Regents of the Smithsonian Institution by the act of April 30, 1890, to be administered by them, first "for the advancement of science" and, second, "for the instruction and recreation of people."

The necessity of protecting the unexpectedly large crowd of people that have been attracted to the Park and of providing for their access to the animals, as well as for the protection of the latter, has made it necessary to assign to this secondary object a disproportionate share of the appropriations, and it seems unavoidable that this subordinate feature should thus claim the larger portion of the expenses, as long as the collections are open to the public, as in ordinary zoölogical gardens.

The appropriations for the fiscal year 1891–'92 were made under three heads: Improvements and care of grounds, $15,000; buildings, $18,000, and maintenance, $17,500, these amounts being about one-half those that were submitted to Congress as necessary to make preliminary provision for the security and accessibility of the collections and to administer their trust with safety to the public.

The Regents recognized the impossibility of doing this with such means; but, considering that the animals were already in the Park, in view of this public safety, and regarding the act as mandatory upon them, they, with the aid of a balance, economized, in anticipation from the original appropriation made for the organization of the Park, and a deficiency item of $1,000, to meet urgent needs, have endeavored to get through the year until relief could be had from Congress. In doing so they have been obliged to reduce the number of watchmen and employés of the Park in every grade till the public safety threatens to be endangered, while yet a considerable part of these watchmen have been called on to labor continuously through Sundays and holidays ten to twelve hours a day without extra compensation, and have in other respects felt obliged to carry economy to a degree which would have been unjustifiable, except upon compulsion under such circumstances.

They would, in their opinion, have been unable to administer the Park to the close of the fiscal year, even under these conditions, had they not, in view of the emergency, also given without charge the services of officials and employés paid from the private Smithsonian fund. The total expenditure for maintenance during the current year may, under these conditions, be expected to be $23,600. These facts were represented by them through the Secretary of the Institution in a letter dated January 23, 1892, to the Secretary of the Treasury (a copy of which is appended) and by him transmitted to Congress.

For the year 1892–'93 the following estimates were sent to the Treasury Department: Improvements, $20,000; buildings, $27,000, and maintenance, $26,000.

In the sundry civil bill (H. R. 7520) as now reported to the House of Representatives, there is appropriated for improvements $9,000, for buildings $10,000, and for maintenance, $10,000; in all $29,000. If the Regents considered, as they must, $9,000 as inadequate for a year's expenditure in laying out the roads and grounds in a new park of 167 acres, they yet would not have felt compelled to make this present representation, since such improvements may await the action of a future

Congress; but, under the appropriation for "buildings," the security of the animals must be provided for without delay, while under "maintenance" come not only their food and warmth, but the protection of the public: and that in the case of animals, which are helpless to provide for themselves and dangerous if not guarded, can not wait future action, has been a pressing consideration to them.

The Regents think it proper to remark that the roads of the park in the vicinity of the cages have been crowded with visitors, to the number of as many as 10,000 in a day, before there was time to make any means for the permanent care of the animals, or provide proper roads to get to them, even had the means for these been appropriated, and that there is, in their judgment, every reason to expect during the coming summer the visit of still larger throngs, composed not only of adults, but of children.

The Regents feel desirous to represent that they can not be held responsible for the imminent danger which must result, under the contemplated withdrawal even of these means for protection which experience has already shown to be absolutely insufficient. They would also ask attention to the fact that small as the appropriation is, it is in several items, and that under no emergency is any discretion allowed them as to their relative amounts, although the whole matter of expenditure is here for a novel purpose, on which only experience could decide the relative exigency of each part.

If Congress intended that the park must be maintained on the appropriation under which the Regents have been unable to administer it the last year (improvement, $15,000; buildings, $18,000; maintenance, $17,500), they deem it reasonable to bring the attention of Congress to the fact that a discretion might properly be exercised by them as to what proportion they should apply to the imminent needs of the public safety and what to matters of less urgency, and that they should either be allowed to expend on the part upon which the safety of the public and the existence of the animals especially depends, that which their experience has shown to be indispensable, or that they should be relieved of responsibility for the consequences.

They desire to add in further explanation that they do not suppose that with the total appropriation of $50,000, of which $26,000 is for "maintenance" (mentioned in the resolution), the park can be properly conducted, and that they believe this sum to be in fact inadequate for such conduct, their intent being to state to Congress the sum below, which, according to their experience, it is impossible to undertake that the park shall be carried on another year, though not creditably, yet without most probable danger.

The resolutions are as follows:

MARCH 29, 1892.

Resolved, That the Board of Regents of the Smithsonian Institution would respectfully represent to Congress the impossibility of maintaining the United States National Zoölogical Park, required by the act of Congress of April 30, 1890, with a less total appropriation than $50,000, of which at least $26,000 will be required for maintenance.

Resolved, That the Secretary of the Institution be requested to communicate this resolution to the President of the Senate and Speaker of the House, with a preliminary statement of the reasons and considerations on which it is based.

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

S. P. LANGLEY,
Secretary.

HON. LEVI P. MORTON,
President of the Senate.

[House Ex. Doc. No. 102, Fifty-second Congress, first session.]

TREASURY DEPARTMENT, *January 25, 1892.*

SIR: I have the honor to transmit herewith, for the consideration of Congress, a communication from the Secretary of the Smithsonian Institution of the 23d instant, in relation to the estimates on page 231 of the Book of Estimates, for the fiscal year 1893, submitted for the improvement, maintenance, etc., of the National Zoölogical Park, District of Columbia, for the fiscal year ending June 30, 1893.

Respectfully, yours,

O. L. SPAULDING,
Acting Secretary.

The SPEAKER OF THE HOUSE OF REPRESENTATIVES.

SMITHSONIAN INSTITUTION,
Washington, D. C., January 25, 1892.

SIR: I beg leave to invite your attention to the estimates under the Smithsonian Institution for the fiscal year ending June 30, 1893, duly submitted to you October 7, 1891, and to the modified form in which these estimates were transmitted to Congress, whereby it would seem to be recommended that no increase be made over the amounts appropriated for the current year.

While feeling that all the amounts asked for by the Institution have been only such as are adequate with the strictest economy, I have to ask your special attention to the three items for the National Zoölogical Park, *i. e.*, improvements, buildings, and maintenance. Disasters from floods and like contingencies, for which no provision was made by Congress in the appropriations for the present year, emphasize the necessity of securing the full amount estimated under the headings Improvements and Buildings, while there exists exceptional necessity in the item for maintenance, which is essentially for the food and care of living animals.

The appropriations made by the act of March 3, 1891, for "maintenance" during the present fiscal year (for which $35,000 was asked), was $17,500, but the sum of $5,122.71 from the appropriation of April 30, 1890, was available and has been used for this purpose; and even with this addition it has been necessary to ask for a deficiency appropriation of $4,434, chiefly to cover expenditures which were found to be absolutely necessary to prevent loss to the Government.

The minimum expenditures for the present year under this item will therefore be $22,622.71; the expenses for the first six months being $14,269.73, or at the rate of $28,539.46 per annum. I trust, therefore, that it is made sufficiently clear that with an appropriation of $17,500 it will be impossible to properly care for and feed the animals now on hand.

The past expenditures would have been still larger but that the work on the accounts for the Treasury has in part been done gratuitously by the Institution, which has also supplied free of cost office rooms, as well as the aid and supervision of unpaid naturalists. This can not be reckoned upon for the future, but has been sanctioned by the Regents as a means to meet the exigency until the need of a larger appropriation can be represented to Congress, and in the meantime the working force has been reduced to an extreme degree, the policing, for instance, being now done by one watchman, aided by two employés who are largely engaged with other duties; and these three men are required to maintain order over an area of 167 acres, visited during each day by thousands of people. These details are mentioned in connection with the fact that (unless some small purchases of animals made at the outset be excepted) it is under like stringencies of economy in every branch of the administration, that the expenses have already amounted, as shown above, to more than $14,000 in six months.

I can not too emphatically represent the peculiar difficulties that must arise in administering an insufficient appropriation for the care of living wild animals, unable to care for themselves where they are, if no provision has been made by Congress for disposing of them elsewhere.

In view of increased expenses since the estimates were prepared, due directly to the unexpectedly great popular interest manifested in the park, and to the extraordinary increase of visitors, I now feel compelled either to increase the estimate for maintenance to $30,000, to cover further contingencies, or to ask that the total appropriation requested for the park be made in such form as to allow a certain discretionary power to meet them. If, under the circumstances stated, the latter would, in your judgment, be the more advisable course, I would respectfully ask that you recommend to Congress that the three items of improvements ($20,000), building ($27,000), and maintenance ($26,000) be appropriated in one sum of $73,000, as follows:

"National Zoölogical Park, Smithsonian Institution: Continuing the construction of roads, walks, bridges, water supply, sewerage, and drainage, and for grading, planting, and otherwise improving the grounds, erecting and repairing buildings and inclosures for animals, and for administrative purposes, care, subsistence, and transportation of animals, and for the purchase or exchange of specimens not otherwise obtainable, including salaries or compensation of all necessary employes, and general incidental expenses not otherwise provided for, $73,000."

I have the honor to be, very respectfully, yours,

S. P. LANGLEY,
Secretary.

The SECRETARY OF THE TREASURY,
Washington, D. C.

The Committee on Appropriations of the United States Senate finally recommended that the sum allowed by the House for the Park be raised to $73,000, and that the amount be appropriated in one item, that is to say, without assigning special sums to special subordinate heads.

In the conference committee upon the sundry civil bill the amount recommended by the Senate was reduced to $50,000, but the embarrassment of special subheads of appropriation was removed. The bill was finally passed* in the following terms.

National Zoölogical Park: For continuing the construction of roads-walks, bridges, water supply, sewerage, and drainage; and for grading, planting, and otherwise improving the grounds; erecting and repair, ing buildings and inclosures for animals; and for administrative purposes, care, subsistence, and transportation of animals, including salaries or compensation of all necessary employes, and general incidental expenses not otherwise provided for, fifty thousand dollars, one-half of which sum shall be paid from the revenues of the District of Columbia and the other half from the Treasury of the United States; and a report in detail of the expenses on account of the National Zoölogical Park shall be made to Congress at the beginning of each regular session.

Work already done.—Notwithstanding a compulsory waste of means caused by the fact that insufficient appropriations made it necessary to do certain urgent work provisionally and imperfectly, it is believed that results have been attained at a smaller expense than is usual in establishments of the same nature elsewhere. The following table shows the cost of the principal works projected up to June 30, 1892.

In elucidation of these statements, the plans and drawings of a portion of the work given (on a necessarily small scale in the text) may be referred to (see Plates II, III, IV and V.)

IMPROVEMENTS.†

Ponds (still incomplete)	$1,945.00
Bear yards and stone retaining wall above them (Plate II)	1,501.00
Water supply	4,190.00
Sewerage and drainage	2,691.15
Roads and walks	13,995.00
Bridge over Rock Creek, including repairs	8,186.00
Cultivating, grading, planting etc	3,350.00
Services of engineers and landscape architects	1,088.30

*Owing to the long session of Congress the bill did not become a law until August 5, 1892. Although the scope of this report is confined to the fiscal year ending June 30, 1892, it seems desirable to conclude here the history of this session's operation.

† See Plate I.

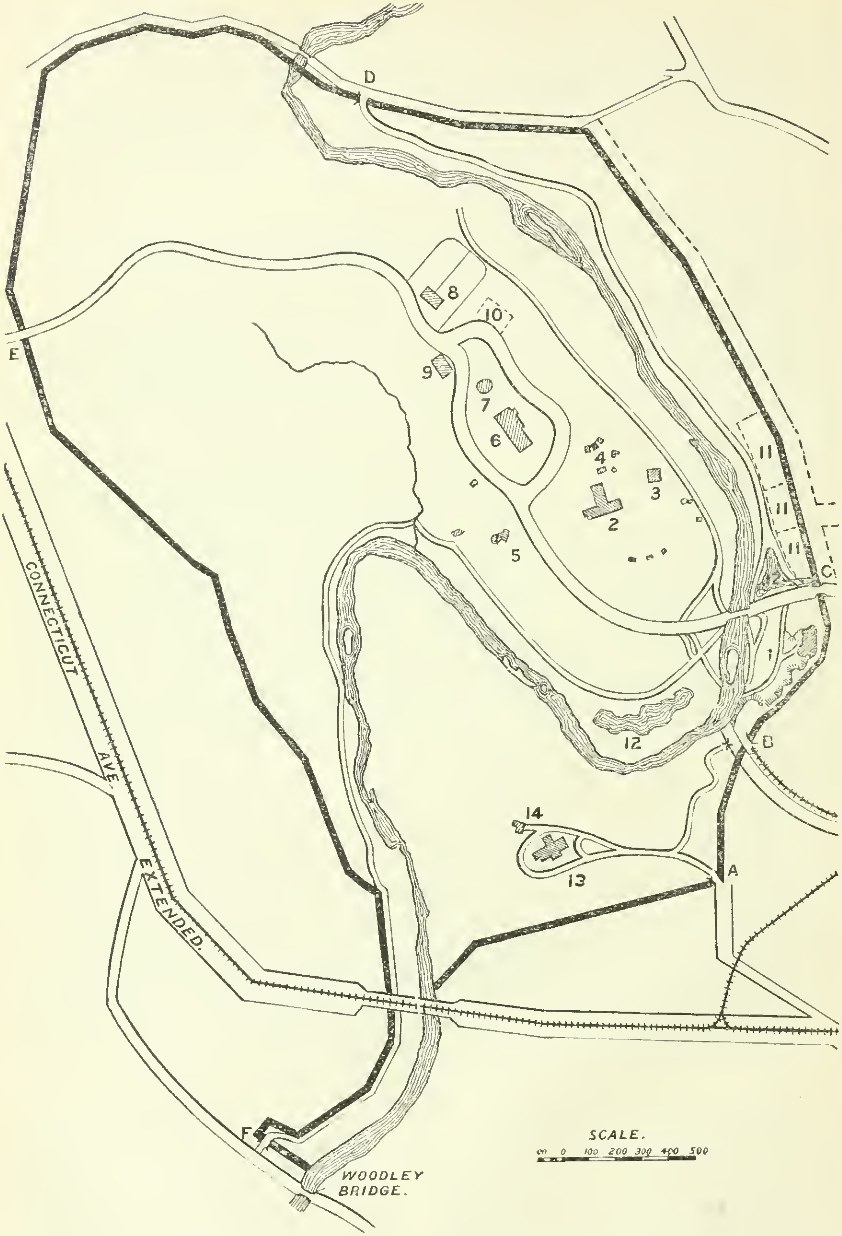

PLATE I.—General Plan of the National Zoölogical Park.

A. Entrance to offices (Holt house).
B. Ontario avenue entrance.
C. Principal temporary entrance at Quarry road.
D. Klingle road entrance, communicating with Quarry road by bridle path along left bank of creek.
E. Connecticut avenue entrance.
F. Entrance for foot passengers at Woodley bridge.

No. 1. Bear yards in abandoned quarry.
2. Animal house.
No. 3. Bird inclosure.
4. Inclosure for wolves and foxes.
5. Prairie-dog town.
6. Property house.
7. Temporary shed for elephants.
8. Buffalo house and paddock.
9. Restaurant.
10. Goat paddock.
11. Deer paddocks.
12. Ponds for aquatic animals.
13. Offices (Holt house).
14. Stables.

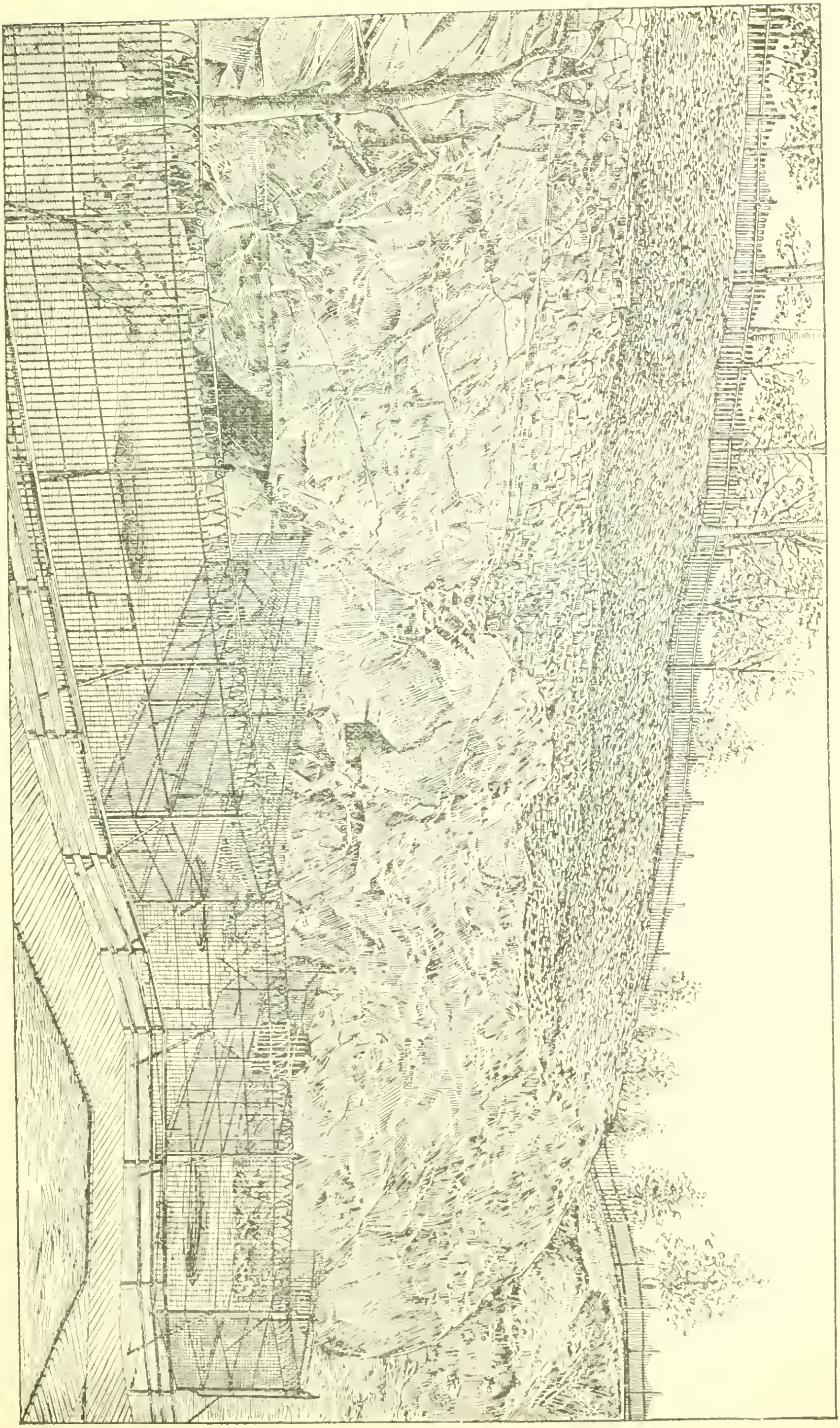

Plate II. Bear Dens and Yards.

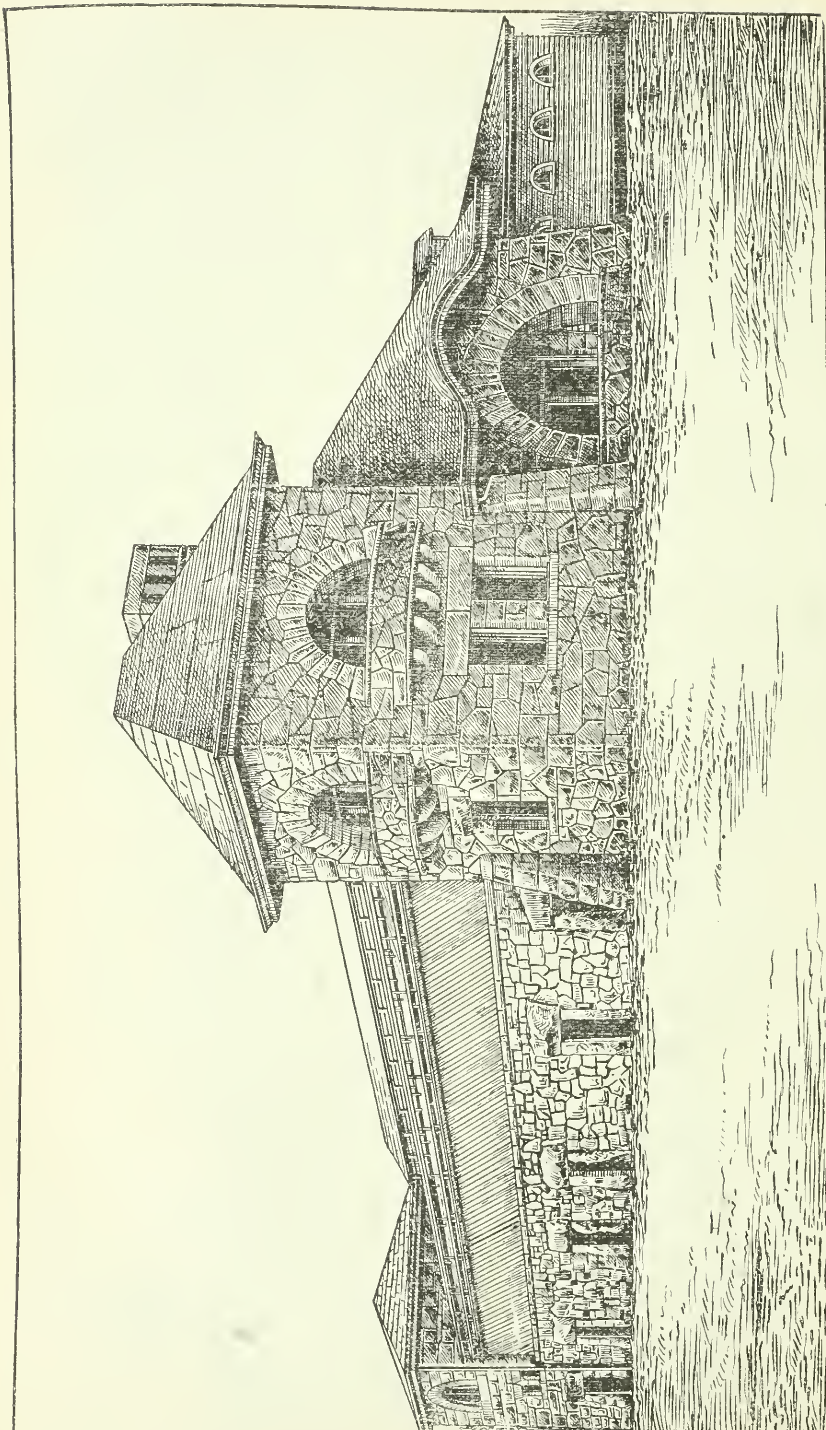

PLATE III.—View of the Principal Animal House, National Zoölogical Park.

BUILDINGS AND INCLOSURES.

Large animal house (See Plates III and IV), including extension, heating apparatus, and engine for running the same	$1,580.00
Buffalo and elk house (See Plate V)	3,537.00
Fences for ruminants and for small inclosures (including small shelters)	2,957.00
Boundary fence for Park	1,355.00
Holt house (office of the Park), repairing and office furniture	2,000.00
Stable and shed	2,600.00
Tool house, shops, and sheds	1,275.00
Elephant house (temporary)	1,099.00

In the appendix to the "estimates"* for the year ending June 30, 1892, on page 299, will be found a statement of the original estimate for the Park, made, it will be remembered, with the expectation that

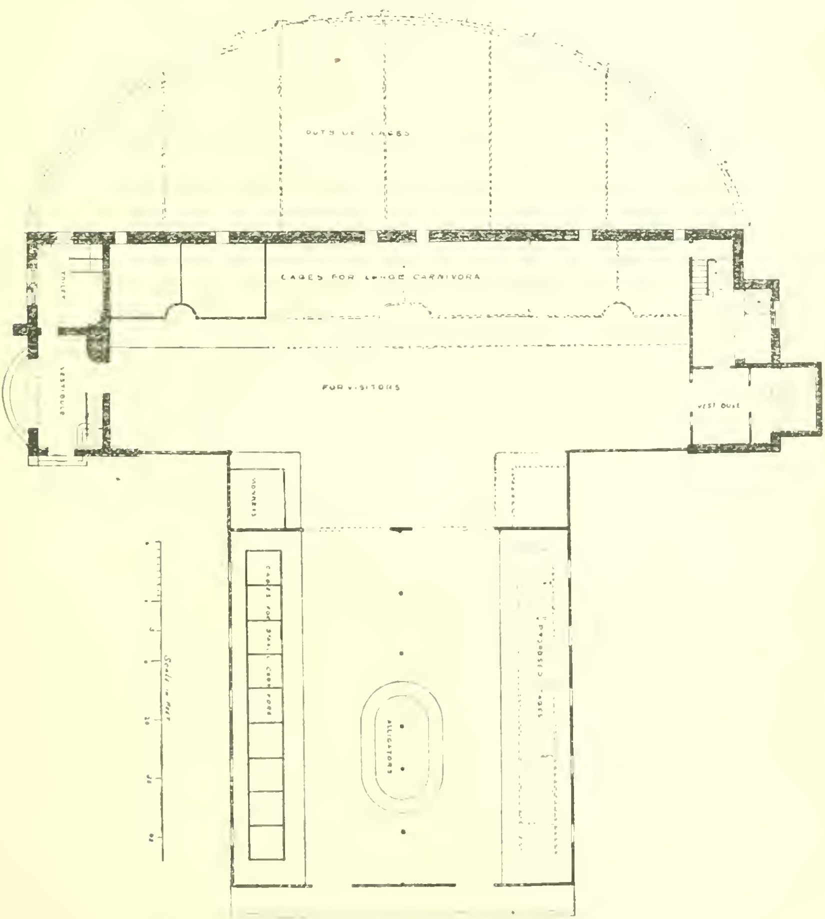

PLATE IV. Plan of Principal Animal House, National Zoölogical Park.

the grounds were to be used essentially in large preserves for the preservation of the national large game.

* Fifty-first Congress, 2d session; H. R. Ex. Doc. No. 5.

The area occupied by the buildings and inclosures for animals in the Park is not far from 40 acres.

Work for the next fiscal year.—The limited amount appropriated by Congress will not permit a rapid development of the Park. Nearly $30,000 of the sum allowed will be required for the feeding and care of the animals and the maintenance of the necessary staff of employés. The remainder will be applied to carrying out the plans already devised; that is to say, to completing the structures yet unfinished, making secure the inclosures and building additional quarters where necessary, in strengthening the banks of the creek against erosion, in completing the ponds for aquatic animals, in extending somewhat the water supply, sewerage, and drainage, and in grading and dressing the roads and slopes.

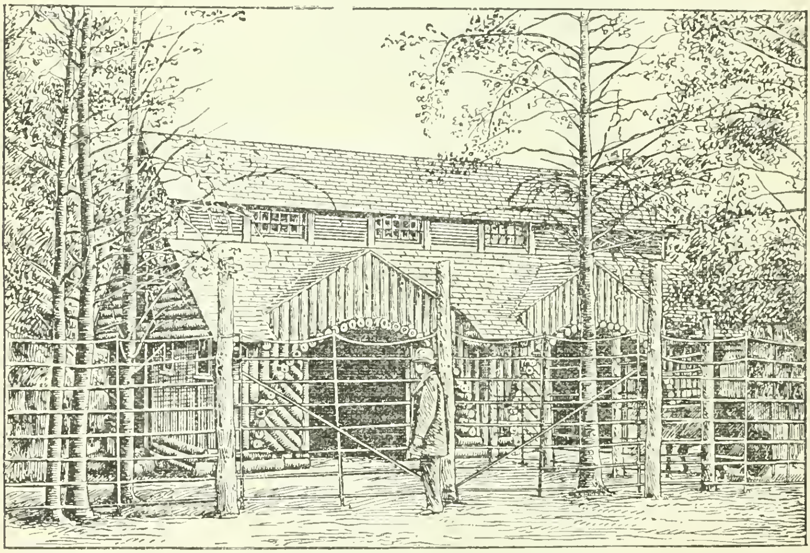

PLATE V.—House for Bison and Elk.

There is an urgent need for a better structure for the elephants than the temporary barn which was hastily erected for their accommodation and which they have ever since occupied. It is calculated that a complete building suitable for the accommodation of elephants and other animals of the same general habits and needs would cost about $15,000. Only a portion of this sum need be appropriated for the present, as the house could be built upon a plan that admits of enlargement.

Access to the park.—The Rock Creek Railway Company intend, as I understand, to carry a branch of their road directly to the Ontario avenue entrance to the park and to transfer passengers from all the city street-car lines without extra charge. In that case there is no doubt that the number of visitors will be greatly increased. The widening and improvement of Quarry road and the extension of Kenesaw avenue to the park lines on the eastward will also tend to increase the ease of access to the park.

Additions to the collections.—The collection is now added to only through gifts made by public-spirited citizens and the somewhat limited accessions derived from the Yellowstone Park. As before mentioned, lack of funds has prevented any notable increase from these sources. It is decreased by the inevitable deaths which the experience of older zoölogical gardens shows will not be less than from 20 to 30 per cent annually.

Accessions from the Yellowstone Park are usually limited to a very narrow range of specimens and can not be depended on for producing a really valuable and characteristic collection of the North American fauna. Under the terms of the first appropriation act it was allowable to procure by purchase "rare specimens not otherwise attainable." This provision was nearly ineffective owing to the inadequate fund given, and it was omitted from the act passed at the recent session of Congress.

The total number of animals in the park is 448 of which 340 are indigenous to North America. Fifty-five of the animals were obtained by purchase.

NECROLOGY.

MONTGOMERY CUNNINGHAM MEIGS.

Montgomery Cunningham Meigs was a son of the eminent physician and medical author, Charles D. Meigs. He was born May 3, 1816, in Augusta, Ga., and graduated from the United States Military Academy at West Point in 1836. He was assigned first to the artillery service, and subsequently to the Corps of Engineers. In 1852 he was directed to make a survey at Washington, D. C., with the view of determining the best plan for supplying the city with water. The plan proposed by him received the approval of the War Department and was adopted by Congress, General Meigs, then a captain of engineers, being charged with its execution—a task that occupied his attention for a period of ten years, and which he completed with signal success. During the prosecution of this important work Capt. Meigs was also placed in charge, as supervising engineer, of the north and south extensions of the Capitol, and of the construction of its crowning iron dome, as well as of the northern extension of the General Post-Office Building.

He served with eminent distinction throughout the war of the rebellion, reaching the rank of brigadier-general in charge of the Quartermaster's Department. The place sought him not only for his high integrity and acknowledged capacity for business, but on account of the strength of his personal character. In 1864 he received the well earned title of brevet major general in the Army.

With the close of the war ends the most active period of his life, and begins the gentle course of an honored old age, devoted among other occupations to the advancement of the best interests of this Institution. He was a member of the National Academy of Sciences and one of the founders of the Philosophical Society of Washington. He was appointed by a joint resolution of Congress in 1885 a Regent of the Smithsonian Institution, and from his entrance into the Board became an active member of its executive committee, which positions he filled until his death, which occurred at his residence in this city on the 2d of January, 1892.

Of Gen. Meigs as a man alike in external or in moral aspects, one can only speak in terms of respect. Personally, he will be remembered by all, even until his very last days, as erect in carriage, with a soldierly bearing which did not recognize the lapse of years, and a manner both dignified and engaging. In character he was not only conscientious and sagacious, but firm at a time when firmness tried every quality of a man. What more can be added when we have said that he was a man faithful in all things, who has left behind him a reputation both high and enduring?

NOAH PORTER.

Noah Porter was born in Farmington, Conn., December 14, 1811, and graduated at Yale College in 1831. During a tutorship in Yale in 1833–1835, he devoted himself to the study of theology. He was appointed professor of moral philosophy and metaphysics at Yale in 1846. In 1871 he was called to the presidency of Yale, which post he resigned in 1886. During President Porter's administration the progress of the college was marked. As a teacher, and in his personal relations with the students, he was one of its most popular presidents. He received the degree of D. D. from the University of the city of New York in 1858, and that of LL. D. from Edinburgh in 1886. His writings cover a wide range of subjects, but are mainly philosophical. He was one of the most scholarly metaphysicians this country has produced. His connection with the Smithsonian Institution began January 26, 1878, when he was elected to the Board of Regents by a joint resolution of Congress, as a citizen of Connecticut, and this connection terminated with his resignation December 31, 1889, on account of failing health.

His death occurred on March 4, 1892, in the eightieth year of his age. President Dwight said of him in an address delivered at his funeral:

He was strong in the native force of his mind, quick in his mental action, keen in his insight, firm in his grasp of truth, rich in his thinking, but most of all, wide in his reach. His eye kindled with enthusiasm as he saw the first opening of new ideas. His face beamed with joy as he gained new measures of knowledge. The field of truth was full of attractiveness for him, and he was glad to enter it by any pathway. He has been in the brotherhood of scholars a man of mark and of influence. He has commanded respect for his learning. He has commended learning by his own possession of it. He has stimulated those nearest to him by his many thoughts, by his wide interest in many departments of knowledge, and by his free and liberal spirit. He has kept an open mind for truth and an open heart for his friends. He has stood among us as one of the ablest men in intellectual power whom we have known in these past years.

Respectfully submitted.

S. P. Langley,
Secretary of the Smithsonian Institution.

APPENDIX TO SECRETARY'S REPORT.

APPENDIX 1.

REPORT OF THE DIRECTOR OF THE BUREAU OF ETHNOLOGY FOR THE YEAR ENDING JUNE 30, 1892.

SIR: Ethnologic researches among the North American Indians were continued under the Secretary of the Smithsonian Institution, in compliance with acts of Congress, during the year 1891–'92.

A report upon the work of the year is most conveniently presented under two general heads, viz, field work and office work.

FIELD WORK.

The field work of the year is divided into (1) archaeology and (2) general field studies, the latter being directed chiefly to religion, technology, and linguistics.

Archæological field work.—The explorations of the Bureau for the last fiscal year were continued under the personal supervision of Mr. W. H. Holmes, with Messrs. Cosmos Mindeleff, Gerard Fowke, and William Dinwiddie as assistants.

The work begun in the tide-water regions of Maryland and Virginia in the spring of 1891 was continued throughout the present year. Careful attention was given to the examination and mapping of the shell deposits of the Lower Potomac and the Chesapeake, and many of the historic village sites visited by John Smith and his associates were identified and examined. The remains upon these sites are identical with those of the many other village sites of the region. Mr. Holmes studied the archæology of South, West, and Rhode rivers and of the shores of the bay above and below Annapolis. The middle Patuxent was visited and the site of the ancient village of Mattpament identified and examined. The valley of the Rappahannock in the vicinity of Fredericksburg and a number of the other western tributaries of the Potomac received attention. Ancient soapstone quarries, one in Fairfax County, Va., three in Montgomery County, Md., and one in Howard County, Md., were studied, and collections of the quarry rejects and implements used in quarrying and cutting the stone were obtained.

In July Mr. Holmes made a trip to Ohio to assist in the resurvey of several geometric earthworks at Newark and near Chillicothe. A visit was made to the great flint quarries in Licking County, between Newark and Zanesville. This well-known quarry is one of the most extraordinary pieces of aboriginal work in the country, and the evidence of pitting and trenching and of the removal and working up of great bodies of the flint are visible on all sides, the work having extended over many square miles. Numerous hammer stones and large bodies of the refuse of manufacture are seen. The chief product of the work upon the site here as elsewhere was a thin blade, the blank from which various implements were to be specialized. The countless handsomely shaped and tinted arrow and spear points and knives scattered over Ohio and the neighboring States are derived chiefly from this site.

When the work of re-surveying the earthworks at Newark and Chillicothe was finished, Mr. Holmes made a journey into Indian Territory to examine an ancient quarry formerly supposed to be a Spanish silver mine. It was reported by Mr. Walter P. Jenney, of the Geological Survey, that this was really an Indian flint quarry, and the visit of Mr. Holmes confirmed this conclusion. Seven miles northwest of Seneca, Mo., and 2 or 3 miles west of the Indian Territory line there are numerous outcrops of massive whitish chert, and in places this rock has been extensively worked for the purpose of securing flakable material for the manufacture of implements. The pits and trenches cover an area of about 10 acres. They are neither as deep nor as numerous as the Flint Ridge quarries. The product of this quarry was also the leaf-shaped blades of the usual type, the size being greater than in the other similar quarries of the country as a result of the massive unflawed character of the stone.

In May Mr. Holmes visited and examined a number of extensive quarries of novaculite in Arkansas, one of which had been visited during the previous year. A great quarry situated upon the summit of a long mountainous ridge at the head of Cove Creek is the most extensive yet discovered in this country. The ancient excavations extend along the crest of the ridge for several miles. The largest pits are still 25 feet deep and upwards of 100 feet in diameter. The product of this quarry was also leaf-shaped blades of the type obtained from the other quarries, and closely analogous in size, shape, and appearance to those of Flint Ridge, Ohio. Mr. Holmes next passed north into Stone County, Mo., to visit a very large cave situated about 20 miles southeast of Helena, the county seat. Neither human remains nor works of native art were found within the cave. The manufacture of chert implements had been carried on extensively in the surrounding region. From Stone County he went to southwest Minnesota, and spent ten days in the study of the red pipestone quarry so famous in the history of the Coteau des Prairies. Evidence of the prehistoric operation of this quarry was found in the series of ancient pits extending across the prairie for nearly a mile in a narrow belt and following the outcrop of the thin layer of pipestone.

The ancient copper mines of Isle Royale, Lake Superior, were next visited and mapped, and extensive collections of stone hammers were obtained from the very numerous pits and trenches.

Mr. Holmes then went west to Little Falls, Minn., to examine the locality from which certain flaked quartz objects, supposed to be of paleolithic age had been obtained. It was found that these bits of quartz were the refuse of the manufacture of blades of quartz by the aborigines and at a period not necessarily more remote than the period of quarry working already described.

Mr. Cosmos Mindeleff, early in July, 1891, closed the field work on the Rio Verde, in Arizona, an account of which was given in the last annual report, and returned to Washington, after which time he was engaged for the remainder of the fiscal year in office work.

Mr. Gerard Fowke completed the exploration of the James River and its northern tributaries, making interesting discoveries in Botetourt, Bath, Alleghany, and Highland counties. He then began an examination of the prehistoric remains of the Shenandoah Valley, remaining in the field until December. Later he examined the islands and coast between the Savannah and St. Johns rivers, locating mounds and shell heaps. In the spring he resumed work in the Shenandoah Valley, making a careful and thorough investigation of every county. The results show that this region was not the seat of any permanent occupation by the aborigines, though it seems to have been a place of resort for hunters in large numbers.

Mr. William Dinwiddie was engaged during the year in mapping and examining the shell banks and other aboriginal remains of the Potomac-Chesapeake region.

As Prof. Cyrus Thomas was engaged most of his time during the year in necessary office work, his field work was limited. Finding it desirable that more accurate information in reference to certain ancient works in Vanderburg County, Ind., should be obtained, he engaged Mr. F. W. Wright to make a careful survey and measurement of them. As the result showed that they were of unusual importance on account of their peculiar character as compared with other ancient works of the same section, Prof. Thomas thought it necessary to make a personal examination of them, and did so. During the same trip he examined certain important mounds in Illinois, among which was the noted "Cahokia" or "Monk's Mound," of Madison County. His object in this case was to ascertain the present condition of this remarkable monument, and to investigate certain other points in relation to which satisfactory conclusions could be reached only by personal inspection.

He also made during the summer another examination of the Newark works and Fort Ancient, in Ohio, in order to settle some points which previous reports had overlooked. At his suggestion the director had a resurvey made, under Mr. Gannett's direction, of the four most noted circles of the Ohio works, the plane table being used to show their exact form as they at present appear.

Mr. F. H. Cushing, during the summer and autumn months of 1891, made some examinations on the shores of Lake Erie, near Buffalo, and of Lake Ontario in Orleans County, N. Y., where he discovered pottery of the well-known net-impressed lacustrine or littoral type, and also, at the former point, some pits or slightly indurated cavities in the sand, which he considered to be connected with the manufacture of that pottery. By experiments made without the aid of modern appliances of any kind he duplicated the ancient specimens found in the vicinity and showed that these pits, lined with ordinary fishing nets, had actually been used simply and effectively for shaping pottery. He has prepared an illustrated report giving the details on the subject.

General field studies.—In August, 1891, Mrs. Matilda C. Stevenson resumed her investigations into the mythology, religion, and sociology of the Zuñi Indians, making careful study of the shrine worship, which constitutes such an important feature in the religion of those people. She added to the already valuable collection of photographs and sketches of their sanctuaries, made in previous years by Mr. James Stevenson, and by the aid of the war priest of Zuñi, secured from the tribe some interesting objects.

Through the influence of the war priest, the priest of the Kâ-kâ and theurgists of the "medicine societies," Mrs. Stevenson was able to be present at Zuñi ceremonials almost continuously from the time of her arrivalto her departure in March.

Dr. W. J. Hoffman proceeded early in August to the Menomonee Reservation, Wisconsin, in response to an invitation from the Mitawok or chiefs of the Mitawit (or "Grand Medicine Society") of the Menomonee Indians, to observe the ritualistic ceremonies and order of initiation of a new candidate for membership, for comparison with similar ceremonials of other Algonquian tribes. In addition to the mythologic material collected at this attendance, he also secured much valuable information relating to the primitive customs and usages of the Menomonee for use in the preparation of a monograph upon that people. Specimens of their workmanship were also collected.

As he had been appointed a special agent for making ethnologic collections for the exhibit to be made by the Bureau of Ethnology at the World's Columbian Exposition, he secured a collection of Menomonee material, as well as a number of desired objects at White Earth Reservation, Minnesota. In May, 1892, he visited the Crow Agency, Mont., to procure a collection of articles to illustrate the industries and workmanship of the Crow Indians. It was specially desirable to obtain some of the elaborate clothing for which the tribe is remarkable. A unique series of articles was obtained, after which a visit was made to the isolated band of Ojibwa at Leech Lake, Minn, to collect various specimens desired to complete the collection illustrating early Ojibwa history.

On his return, Dr. Hoffman again stopped at the Menomonee Reservation to make final collections of ethnologic material and to complete his studies of the ritual and initiatory ceremonies of the Grand Medicine Society, a meeting of which body had been called for this special purpose. He returned to Washington in June, 1892.

Mr. James Mooney, during the field months of the fiscal year, continued the collection for an exhibit at the World's Columbian Exposition, of objects to illustrate the daily life, arts, dress, and ceremonies of the Kiowa in the southeastern part of the Indian Territory. That tribe was selected as continuing in its primitive condition more perfectly than any other which could be examined with profit. He succeeded in making a tribal collection which is practically complete, including almost every article in use among the Kiowas for domestic uses, and for war, ceremony, amusement, or dress. A number of illustrating photographs were also obtained. On his return in August this collection was labeled and arranged in cases ready for transportation to Chicago on the opening of the Exposition, and by means of the photographs and costumes obtained several groups of life-size figures were prepared to show characteristic scenes in Indian life.

In November he again set forth to obtain additional information upon the ghost dance, especially among the principal tribes not before visited. After a short stay in Nebraska with the Omahas and Winnebagoes, neither of whom, as it was found, had taken any prominent part in the dance, he went to the Sioux at Pine Ridge Agency, S. Dak., the chief seat of the late outbreak, where he collected a large number of songs of the dance and much miscellaneous information on the subject. From there he went to the Piutes in Nevada, among whom the Messiah and originator of the ghost dance resides. Here he obtained the statement of the doctrine from the lips of the Messiah himself, took his portrait, the only one ever taken, and obtained a number of dance songs in the Piute language. He then returned to the Cheyennes and Arapahoes in the Indian Territory, among whom he had begun the study of the dance, and obtained from them the original letter which the Messiah had given them, containing the authentic statement of his doctrine and the manner in which they were to observe the ceremonial. He returned to Washington in February.

In May he again started out to gather additional ethnologic material, especially with regard to the Kiowas, and to obtain further collections for the World's Columbian Exposition. Going first to the Sioux, he proceeded next to the Shoshones and Northern Arapahoes, in Wyoming, and then turned south to the Kiowas, in the Indian Territory, where he was still at work at the close of the fiscal year.

Mr. H. W. Henshaw, on May 14, 1892, proceeded to New Mexico and California for the purpose of pursuing certain linguistic investigations and to make collections for the World's Columbian Exposition. This duty was continued until the close of the fiscal year.

Rev. J. Owen Dorsey, from January 14 to February 21, 1892, made a trip to Lecompte, Rapides Parish, La., for the purpose of gaining information from the survivors of the Biloxi tribe. He found only one person, an aged woman, who spoke the language in its purity, and two others, a man and his wife (the latter the daughter of the old woman), whose dialect contains numerous modifications of the ancient language. From these three persons he obtained several myths and other texts in the Biloxi language, material for a Biloxi-English dictionary, local names, personal names, names of clans, kinship terms, lists of flora and fauna, with their Biloxi names, and grammatical notes. He filled many of the schedules of a copy of the second edition of "Powell's Introduction to the Study of Indian Languages" (English-Biloxi in this instance). He brought to Washington a few botanical specimens, for which he had gained the Biloxi names, in order to obtain their scientific names from the botanists of the Smithsonian Institution. He photographed three Biloxi men and two women, all that could be found. There were about seven other Biloxi residing in the pine forest 6 or 7 miles from Lecompte, but they would not be interviewed. The Biloxi language contains many words which resemble their equivalents in other Siouan languages, some being identical in sound with the corresponding words in Dakota, Winnebago, etc. The Biloxi has more classifiers than are found in the other languages of this family, and while it uses adverbs and conjunctions, it often expresses a succession of actions by mere juxtaposition of two, three, or more verbs. In the paucity of model prefixes it may be compared with the Hidatsa and Tutelo, and in the use of d^{dh} and t^{th} it may be classed with the Kwapa and Hidatsa. The information now gained permits a tabular comparison of the Biloxi with the Hidatsa, Winnebago, Catawba, and Tutelo, those five being regarded as the archaic languages of the Siouan family.

Mr. Albert S. Gatschet, having met with little success in his previous attempt, in 1881, to study the Wichita language in the field, continued to watch for better opportunities. In 1892 he met twelve young men of that tribe in the Educational Home (branch of the Lincoln Institute) at Philadelphia, and selected four of the brightest of their number, who seemed to be the most promising through their advanced knowledge of English. With their help he gathered about three thousand terms of Wichita, which is a Caddoan dialect, also a large number of paradigms, sentences, and a few mythological texts. A thorough interchangeability of the consonants makes the study peculiarly difficult.

Maria Antonia, a young Costa Rica woman residing in Philadelphia, was questioned concerning what she remembered of her native tongue, the Guatuso. About one hundred and twenty vocables were recorded as the result of the inquiry. Mr. Gatschet's field work extended from the beginning of March to the beginning of June, 1892.

OFFICE WORK.

The Director took special pains in the revision and correction of his work on the "Indian Linguistic Families of America north of Mexico," as it passed through the press. The scope and importance of this linguistic classification has before been explained, and the paper forms part of the Seventh Annual Report of the Bureau, now issued.

Col. Garrick Mallery, U. S. Army, was chiefly occupied in writing in final form, for publication in the Tenth Annual Report of the Bureau, a comprehensive paper on the "Picture Writing of the American Indians," which presents the result of several years of personal exploration and study of all accessible material on that subject. At the close of the year, the manuscript and the drawings for the large number of necessary illustrations had been transmitted through the Secretary of the Smithsonian Institution to the Public Printer. Col. Mallery was also, during the greater part of the year, charged with administrative duties and with the execution of a variety of special works under the instructions of the Director.

Mr. H. W. Henshaw, in addition to his usual administrative duties, when not in the field as before mentioned, was employed in preparing the exhibit of the Bureau for the World's Columbian Exposition at Chicago and also in the preparation of the forthcoming volume on "Tribal Synonymy," the purport and utility of which have been explained in former reports. In this work Mr. Henshaw has had the assistance of Mr. F. Webb Hodge, who has devoted special attention to the Piman, Yuman, and the several Pueblo linguistic stocks.

The office work of Mr. W. H. Holmes consisted in the completion of papers upon the pottery and shellwork of the aborigines of the United States. A third paper was written upon the textile fabrics obtained from the mound region, and a fourth upon the stone implements of the tide-water country. A fifth paper upon the general archæology of the region was commenced.

At the commencement of the official year Prof. Cyrus Thomas was engaged in examining and correcting the proof of his "Catalogue of Prehistoric Works East of the Rocky Mountains," which was published in the latter part of 1891, as a Bulletin of the Bureau. This examination involved in many cases the necessity of a reference to the authorities quoted.

Much of his time during the year was employed in writing the final pages of the report on the field work and explorations which for several years had been in his charge, and in adapting it to a change in the form and manner of its publication which had been made necessary. This involved the re-writing of many pages and a material condensation of the introductory portion relating to the distribution of types of mounds. It was completed by the close of the fiscal year and filed for publication, nearly all the illustrations having been drawn and prepared for engraving.

He devoted all his spare time to the study of the Maya Codices and in the preparation of a report of the discoveries he made therein. One of these, which is deemed of much interest and importance, is that, when the Dresden Codex, which is considered the most ancient of those known, was written, the year consisted of 365 days, and that the calendar was arranged precisely as it was found to be by the Spanish conquerors. But his most important discovery was made during the closing days of the year. This consisted in what may be termed the discovery of the key to the signification of the hieroglyphic characters of the Codices by which it is probable that the inscriptions may ultimately be read. This discovery, which the tests so far applied appear to confirm, consists, first, in the evidence that the characters as a rule are phonetic and, second, in ascertaining the signification of a sufficient number to form a basis for the interpretation of the rest. If this discovery proves to be what, from the evidence presented, it appears to be, it will be of incalculable importance to American archæology.

Early in the year the work of Mr. Cosmos Mindeleff commenced in repairing and securing the preservation of the Casa Grande ruin. This work was ordered by act of Congress and plans had been prepared by Mr. Mindeleff while in Arizona during the previous year. These plans provided for the excavation of the interior of the ruin, the underpinning of the walls with brick and cement, the use of tie-beams to hold the walls in place and render them more solid, the restoration of the lintels over door and window openings, and the filling of the cavities above the lintels with brick and cement. The work was completed in November and was inspected and accepted. Although all that was deemed necessary to preserve the ruin could not be done with the appropriation provided, still it is believed that enough was done to preserve it in its present condition for many years. All the work done was directed to the preservation of the ruin, no attempt at restoration being made. In June, 1892, the President, in accordance with the authority vested in him by Congress, reserved from settlement twelve quarter sections about the ruin, comprising an area of about 480 acres. A number of specimens obtained during the excavation were shipped to Washington and deposited in the National Museum.

During a part of the year Mr. Mindeleff was engaged in the preparation of a report upon his field work of the previous year. This report, entitled "Aboriginal Remains in the Valley of the Rio Verde, Arizona," was completed and will appear in the Twelfth Annual Report of the Bureau. Aside from a comprehensive treatment of the ruins in the valley of the Verde the report will contain the first illustrations published of ancient irrigating ditches, and the first comprehensive data, including illustrations of cavate lodges. It is fully illustrated from photographs, plans, and surveys made by the author. Subsequently he commenced a scientific report on the Casa Grande ruin of Arizona.

No new work was undertaken in the modelling room during the year, as the entire force was occupied upon the preparation of duplicates of models previously made, for use at the World's Columbian Exposition and elsewhere. Six models, in addition to other material, were sent to Spain, to be exhibited at the historical exposition at Madrid. The series comprised models of the Pueblo of Zuñi, New Mexico, the Pueblo of Walpi, Arizona, Mummy Cave cliff ruin, Arizona, all of large size, and three smaller models of ruins.

An indefinite leave of absence without pay was granted to Mr. Frank H. Cushing in December, 1886, in order that he might organize and conduct the important explorations in southern Arizona and the Zuñi country of the Southwestern Archaeological Expedition established by Mrs. Mary Hemenway, of Boston. His successful fulfillment of this work was suddenly interrupted in the winter of 1889 by a severe and prostrating illness, which disabled him until the summer of 1891. He was therefore unable to resume promised work on his older Zuñi material for the Bureau until August, 1891, when he began the preparation of a contribution intended to appear in the Twelfth Annual Report of the Bureau, on the Zuñi myths of creation and migration as related to the mythic drama-dance organization, or *Kâkâ* of the Zuñis, and the so-called Cachina ceremonials of all Pueblo and other southwestern tribes. Mr. Cushing's discoveries as set forth in this essay confirm and substantiate the opinion held by the Director that all primitive so-called dance ceremonials are essentially dramatic, and they go so far as to indicate also that all primitive ceremonials, of whatever nature, are essentially dramaturgic, thus making his contribution of general as well as of special significance.

In January, 1892, Mr. Cushing again reported at Washington and was regularly engaged as an ethnologist of the Bureau on the 1st of February, and has since been occupied in elaborating his paper on the myths of the drama dances and on a study of manual concepts or the influence of primitive hand-usages on mental development in the culture growth of mankind.

Mrs. Stevenson returned from the field work before mentioned in March, 1892, and was employed for the remainder of the fiscal year in preparing her field notes for publication.

Mr. Gerard Fowke was engaged during December and January in preparing a report of the season's work by him in archaeology, arranging and classifying the specimens procured, and embodying in reports, previously prepared, the results of recent discoveries.

The office work of Dr. Hoffman consisted in arranging the material gathered during the preceding field season and in preparing for publication an account of the Midēwiwin, or so-called "Grand Medicine Society," of the Ojibwa Indians of White Earth, Minnesota. This work, which forms one of the papers accompanying the Seventh Annual Report, embraces new material and consists of the traditions of the Indian cosmogony and genesis of mankind, the "materia medica" of the shamans, and the ritual of initiation, together with the musical notation of the chants and songs used.

During the winter and spring months a delegation of Menomonee Indians from Wisconsin visited Washington and Dr. Hoffman frequently conversed with them to obtain information explanatory of the less known practices of the Menomonee ceremony of the Mitawit, or their "Grand Medicine Society," for the purpose of comparison with the ritual as observed by the Ojibwa. In addition a large mass of mythologic material was obtained, as well as texts in the Menomonee language.

On returning from the field in August 1891, Mr. James Mooney spent about ten weeks in arranging his Kiowa collection for the World's Columbian Exposition, writing out a series of descriptive labels, and in copying all the more important documents relating to the "ghost dance" from the files of the Indian Office and the War Department. He then again went out into the field, as above stated, returning to Washington in February 1892. About three months were then occupied in arranging the material thus obtained and in writing the preliminary chapters of his report on the ghost dance. He also superintended the preparation, at the National Museum, of a number of groups of life-size figures to accompany the Kiowa collection at the World's Fair.

Rev. J. Owen Dorsey continued the arrangement of Kwapa texts with interlinear and free translations and critical notes. He revised the proof of "Omaha and Ponka

Letters," a bulletin prepared from Çegiha texts collected by himself. He finished the collation of all the Tutelo words recorded by Dr. Hale, Mr. J. N. B. Hewitt, and himself, with the result that he had 775 words in the Tutelo-English dictionary. He furnished a list of several hundred linguistic and sociologic questions to be used among Indian tribes. These questions were in addition to those contained in the second edition of Powell's Introduction to the Study of Indian Languages and were based on original investigations made by Mr. Dorsey among the Siouan tribes. He prepared for publication the following articles: Siouan Onomatopes (sound-roots), illustrated by charts; The Social Organization of Siouan Tribes, illustrated by figures consisting chiefly of material gained by himself from the Dakota tribes, the Omaha, Ponka, Kwapa, Osage, Kansa, Iowa, Oto, Missouri, Winnebago, and Tutelo; Nanibozhu in Siouan Mythology; Games of Teton Dakota Children (translated and arranged from the original Teton manuscript in the Bushotter collection of the Bureau of Ethnology).

After his return from Louisiana he devoted most of his time to the arrangement of the material collected in his Biloxi note-books. He prepared a Biloxi-English dictionary of 3,183 words on about 7,000 slips in alphabetic order. He arranged the Biloxi texts for publication, adding to the myths (with their interlinear and free English translations and critical notes) a list of several hundred Biloxi phrases. In his article on the Biloxi kinship system, he gave 53 kinship groups, of which number only 27 have their counterparts in the Dakota, Çegiha, and other Siouan languages of the Missouri valley. The elaboration of all the Biloxi material could not be completed by June 30, 1892.

Mr. Albert S. Gatschet assisted in augmenting and improving the data for the Tribal Synonymy now in preparation, by extracting material from a number of books and original reports especially referring to southern and southwestern Indians. His main work during the year was directed towards extracting and arranging some of the more extensive vocabularies made by him previously in the field. After completing the Tonkawe of Texas, he carded each word of the Shawano and Creek languages obtained by him, copied the historical and legendary texts of both, and extracted the lexical and grammatic elements from them to serve as the groundwork for future grammars. The remains of the Virginia or Powhatan languages that are known were also made accessible by carding the terms.

During the fiscal year Mr. J. N. B. Hewitt was a part of the time engaged in careful study of the grammatic forms of the Iroquoian languages, especially in the ascertainment of the number and order in which the affixes may be used with one and the same stem or base. He was also engaged in translating, extracting, and transferring to library cards, from the "Découvertes et Établissements des Français dans l'Amérique septentrionale," by Pierre Margry, matter relating to the manners, customs, beliefs, rights, ceremonies, and history of the Iroquois, which matter is now placed on about 20,000 cards.

He continued his work on the Tuscarora Dictionary and directed attention to developing the full number of ordinary sentences in which every generic noun may be employed, which affords a measure of the capacity of the vocabulary for the expression of thought.

Mr. James C. Pilling continued his bibliographic work throughout the year, with special attention to the Athapascan family. Work upon this family was begun early in the fiscal year, on October 13 the manuscript was sent to the printer, and at the close of the year but a few pages of the final proofs remained unread. The Bibliography of the Athapascan Languages forms a pamphlet of xiii+125 pages. While this volume was being put in type Mr. Pilling began the collection of material for a number of bibliographies relating to the languages of the northwest coast of America—the Chinookan, Salishan, and Wakashan, and satisfactory progress has been made. Probably one or more of them will be ready to send to the printer during the coming autumn. During the month of May, 1892, Mr. Pilling made a brief visit to libraries in Boston and Cambridge in connection with the compilation of material relating to these northwest languages.

Mr. De Lancey W. Gill continued in charge of the work of preparing and editing the illustrations for the publications of the Bureau.

The total number of illustrations prepared during the year was 980. These drawings may be classified as follows:

Landscapes	6
Maps	6
Objects	300
Diagrams	31
Miscellaneous	637

The number of illustration proofs handled during the year was as follows: Eighth Annual Report, 308; Ninth Annual Report, 459; 678 illustrations for the Tenth Annual Report were transmitted to the Public Printer.

The photographic laboratory remains under the able management of Mr. J. K. Hillers. A small but valuable collection of portraits of North American Indians was secured by him during the year from sittings; twenty-six negatives were obtained. The following table shows the size and number of photographic prints made:

Size.	Number.
20 by 24	45
11 by 14	274
8 by 10	546
5 by 8	875
4 by 5	1,187

PUBLICATIONS.

The publications issued during the year are as follows:

(1) "Seventh Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution, 1885–'86, by J. W. Powell, Director." This report contains an introductory report by the Director, 27 pages, with accompanying papers, as follows:

"Indian Linguistic Families of America north of Mexico," by J. W. Powell; "The Midéwiwin or 'Grand Medicine Society' of the Ojibwa," by W. J. Hoffman; "The sacred formulas of the Cherokees," by James Mooney. The report forms a royal octavo volume of lxi+409 pages, illustrated with 39 figures and 27 plates, one of which is a folding plate in a pocket at the end of the volume.

(2) "Contributions to North American Ethnology, Vol. II, part II." This part contains the Klamath-English and English-Klamath Dictionary, by Albert Samuel Gatschet, and concludes his work relating to "The Klamath Indians of Southwestern Oregon." The volume is a quarto of 711 pages.

(3) "Contributions to North American Ethnology, Vol. VI," containing the following papers by James Owen Dorsey: "The Çegiha Language, part I, Myths, Stories, and Letters," and "The Çegiha Language, part II, Additional Myths, Stories, and Letters." The report forms a quarto volume of xviii+794 pages.

(4) "Contributions to North American Ethnology, Vol. VII. "A Dakota-English Dictionary, by Stephen Return Riggs, edited by James Owen Dorsey." This is a quarto volume of x + 665 pages.

(5) Bulletin of the Bureau of Ethnology. This work consists of a paper entitled "Omaha and Ponka Letters," by James Owen Dorsey, and forms an octavo volume of 127 pages.

(6) Bulletin of the Bureau of Ethnology. The work is a "Catalogue of Prehistoric Works East of the Rocky Mountains," by Cyrus Thomas. It forms an octavo volume of 246 pages, with 17 maps, one of which is in a pocket at the end of the volume.

(7) Bulletin of the Bureau of Ethnology. The work is "Bibliography of the Algonquian Languages," by James Constantine Pilling. It forms an octavo volume of 614 pages, with 82 plates of facsimiles of title-pages of rare works.

Very respectfully,

J. W. POWELL,
Director.

Mr. S. P. LANGLEY,
Secretary Smithsonian Institution.

APPENDIX II.

REPORT OF THE CURATOR OF EXCHANGES FOR THE YEAR ENDING JUNE 30, 1892.

SIR: I have the honor to present the following brief report of the operations of the Bureau of International Exchanges for the fiscal year ending June 30, 1892:

TABULAR STATEMENT OF THE WORK OF THE BUREAU.

The statistics of work done by the Bureau during the year are succinctly given in the annexed table, prepared in a form adopted in the preceding reports:

Transactions of the Bureau of International Exchanges during the fiscal year 1891–'92.

Date.	Number of packages received.	Weight of packages received.	Ledger accounts.				Domestic packages sent.	Invoices written.	Cases shipped abroad.	Letters received.	Letters written.
			Foreign societies.	Domestic societies.	Foreign individuals.	Domestic individuals.					
1891.		Lbs.									
July	6,550	12,578							21	230	177
August	11,316	21,718							62	179	226
September	7,271	15,590							17	179	146
October	9,275	25,508							116	232	222
November	5,638	11,416							99	210	302
December	5,334	24,224							117	189	224
1892.											
January	5,161	13,116							60	183	262
February	12,058	23,671							96	188	226
March	12,869	32,375							127	217	301
April	7,435	18,315							95	179	204
May	8,118	15,957							90	169	236
June	6,002	11,052							112	168	226
Total	97,027	226,517	6,204	2,044	7,910	4,521	26,000	23,136	1,015	2,323	2,752
Increase over 1890-'91	6,361	11,095	223	456	838	317	3,047	1,213	53	116	335

For comparison with previous years I add a tabular statement from 1886 to 1892, inclusive, by which the rapid growth of the service clearly appears:

	1886-'87	1887-'88	1888-'89	1889-'90	1890-'91	1891-'92
Number of packages received	61,940	75,107	75,966	82,572	90,666	97,027
Weight of packages received	141,263	149,630	179,928	202,657	237,612	226,517
Ledger accounts:						
Foreign societies	7,396	4,194	4,466	5,131	5,981	6,204
Foreign individuals		4,153	4,699	6,340	7,072	7,910
Domestic societies	2,165	1,070	1,355	1,431	1,588	2,044
Domestic individuals		1,556	2,610	3,100	4,207	4,524
Domestic packages sent	10,294	12,301	17,218	13,216	29,047	26,000
Invoices written	15,288	13,525	14,095	16,948	21,923	23,136
Cases shipped abroad	692	663	693	873	962	1,015
Letters received	1,131	1,062	1,214	1,509	2,207	2,323
Letters written	1,217	1,804	2,050	1,625	2,417	2,752

EXPENSES.

The expenses of the exchange bureau are met in part by direct appropriation by Congress and in part by appropriations made to Government Departments or Bureaus, either in their contingent funds or in specified terms for repayment to the Smithsonian Institution of a portion of the cost of transportation. In 1878 the Board of Regents established a charge of 5 cents per pound weight for the publications sent out or received by the various Government bureaus, this charge being necessary to prevent an undue tax upon the resources of the Institution, as the appropriations made by Congress have never been sufficient to meet the entire cost of the service. For similar reasons it has been found necessary to make a charge of the same amount to State institutions, and from these a further small sum has been received.

The appropriation made by Congress for the fiscal year 1891-'92 was in the following terms:

For expenses of the system of international exchanges between the United States and foreign countries, under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employees, seventeen thousand dollars.

The receipts and disbursements by the accounting officer of the Smithsonian Institution on account of international exchanges, under date of July 1, 1892, and covering the fiscal year immediately preceding, were as follows:

RECEIPTS.

Direct appropriation by Congress	$17,000.00
Repayments to the Smithsonian Institution from United States Government Departments	2,108.44
State institutions	30.75
Total	19,139.19

DISBURSEMENTS.

For—	From specific Congressional appropriations.	From other sources.
Salaries and compensations	$14,074.81	....
Freight	1,792.83	....
Packing boxes	561.40	....
Printing	98.50	....
Postage	145.00	....
Stationery	327.46	....
	17,000.00	$3,310.49

The foregoing table shows that the entire amount received from Government bureau and others was $2,139.19, making the sum practically available for the specific purpose of exchanges $19,139.19, while the expenses have amounted to $20,310.49, the deficiency of $1,171.30 being made up from the Smithsonian fund.

The advantages have been pointed out in previous reports of combining in a single item the various appropriations for the exchange service, now divided into comparatively small sums among the several larger appropriation bills of the Government, but the matter seems to be of sufficient importance to call attention to it again in this place.

For the year 1891-'92 an estimate for the entire expense of the service of $23,000 was submitted, this sum being intended to include these smaller amounts alluded to, and also an item of $2,000 to cover the expense of an immediate exchange of parliamentary documents with the countries entering into the treaty of Brussels in 1886. The amount appropriated was $17,000, the same as that for the preceding year.

CORRESPONDENCE.

The name of each person or institution sending or receiving publications through the exchange bureau was heretofore entered upon a large ledger card, showing all such packages received or sent. This system has proved itself of great convenience, but with the large increase in the number of cards the space occupied has become of serious moment, and it was therefore found desirable to begin a new series of cards, of smaller size, entering in an abbreviated form the receipts from correspondents upon a blue card, while the packages forwarded to these correspondents are entered upon a white card. This system was put in operation January 1, 1892, and during the six months succeeding, 9,808 cards of the new form have been prepared, representing the number of correspondents with whom communication has been had during that period. There have been added to the list of correspondents during the year 1,831 names.

Additions to list of correspondents.	Foreign.	Domestic.
Societies and institutions	6,204	2,044
Individuals	7,910	4,524
Total	14,114	6,568

INTERNATIONAL EXCHANGE OF OFFICIAL DOCUMENTS.

Under the treaty alluded to in the Secretary's report for 1887-'88, the exchange of the official publications of the United States Government with other governments has been continued by the Smithsonian Institution, and it now forms a very large proportion of the bureau's work.

The entire number of publications sent abroad during the year under the provision of the act of Congress of March 2, 1867, and of the treaty above referred to, was 27,873, and there have been received in return 1,911 packages or volumes. The United States Government Departments have forwarded to their correspondents abroad 20,373 packages, and have received in return 13,000 packages. The total number of exchanges on Government account has been 14,911 received and 52,783 packages sent abroad. There have, therefore, been a total of 67,721 packages, or about 70 per cent of the total number handled.

This exchange on account of Government bureaus is shown in detail in the following table:

Statement of Government exchanges during the year 1891-'92.

Name of bureau.	Packages— Received for.	Packages— Sent by.	Name of bureau.	Packages— Received for.	Packages— Sent by.
American Ephemeris	1	....	Navy Department library	18	....
Astro-physical Observatory	53	1	Office of Indian Affairs	2	....
Bureau of Education	85	1	Office of Naval Intelligence	1	....
Bureau of Ethnology	104	1,809	Office of the Chief of Engineers	37	52
Bureau of International Exchanges	3	....	Ordnance Bureau (War Department)	3	70
Bureau of Medicine and Surgery	2	....	Post-Office Department	1	....
Bureau of Navigation	4	....	Public Printer	....	32,410
Bureau of Ordnance (Navy Department)	1	....	Smithsonian Institution	9,975	6,535
Bureau of Statistics	19	....	Surgeon-General's Office (U. S. Army)	156	555
Bureau of Steam Engineering	2	....	Surgeon-General (U. S. Navy)	10	....
Bureau of the Mint	3	....	U. S. Board on Geographic Names	1	....
Census Office	8	1	U. S. Coast Survey	69	221
Commissioner of Patents	1	....	U. S. Commissioner of Weights and Measures	2	....
Comptroller of the Currency	2	....	U. S. Entomological Commission	6	....
Department of Agriculture	143	893	U. S. Fish Commission	55	605
Department of Justice	10	....	U. S. Geographical Survey	3	....
Department of Labor	26	532	U. S. Geological Survey	434	3,260
Department of State	22	....	U. S. National Museum	1,294	3,566
Department of the Interior	29	45	U. S. Naval Observatory	112	951
Department of War	13	225	U. S. Patent Office	45	491
General Land Office	4	....	U. S. Senate	2	....
House of Representatives	1	....	U. S. Signal Office	41	16
Hydrographic Office	68	....	U. S. Treasury Department	9	....
Index Medicus	1	....	U. S. Weather Bureau	50	181
Library of Congress	1,941	....		14,941	52,783
Light-House Board	2	1	Total	67,724	
Marine Hospital Service	5	94			
National Academy of Sciences	48	91			
Nautical Almanac Office	14	183			

Adding this to the number of miscellaneous packages the total of 97,027 packages, weighing 113 tons, has passed through the bureau.

EFFICIENCY OF THE SERVICE.

It is, perhaps, desirable to rehearse briefly here the method of receiving and handling exchange packages received by the Institution.

Scientific societies and individuals in the United States desiring to forward their publications abroad send them to the Smithsonian Institution, where a record is kept of the number of packages received, under the sender's name, and also a record showing each person or institution to which a copy of the work in question is transmitted. The books are then packed, with invoices from other senders, and forwarded by freight to the Government bureau abroad which has undertaken the task of distributing all exchange packages in that country. The books are forwarded direct to the paid agents of the Institution, if in Great Britain or Germany, by whom they are distributed by mail or express, the Institution assuming the cost of transportation to the distributing agents, and in the case of its special agent the cost is further defrayed to the recipient when practicable. Because of the lack of sufficient funds transportation is effected by slow freight, and the governmental bureau of the United States or other correspondents of the Institution can not expect their publications to be delivered with the same promptness with which they may be sent by mail or by express. The transmissions are especially slow to foreign countries with which we have comparatively infrequent communication. To England and Germany, where, as before stated, the agencies are under control and pay of the Smithsonian Institution, and to France, cases are dispatched on the average about three times a month.

Special care is taken to insure the safe delivery of the package to the person addressed, and the cases of failure constitute but a small percentage of the entire number of packages handled. Some errors are inevitable unless the greatest care is exercised by the sender in securing the proper address of his correspondents.

With each package sent out a receipt card is forwarded requesting acknowledgment thereon of the package in question, and when this receipt is placed in the files of the exchange office the record of that particular package is complete.

Transmissions from abroad received by freight in large cases are distributed in the United States by registered mail, a record first having been made of the name of the sender and of the recipient of each package. A receipt card, returnable by mail without postage, is sent with each of these packages, and should be returned at once by the recipient in acknowledgment of the package, otherwise further transmissions to that address can not be made. It should be borne in mind that as no record is made of the title of the book contained in each package, it is not always possible to trace a given work unless the date of its dispatch to the exchange office is known.

I give this account of the working of the Exchange Service, as I am led to believe from a number of inquiries with reference to this Bureau that it is not thoroughly understood by all who have had occasion to make use of it.

I am gratified to state that, the recommendation for additional assistance contained in previous reports having been approved, it has been possible to bring the records and files into a much more satisfactory state than heretofore, as, owing to the insufficient clerical force, the work has been for several years past somewhat in arrears. By the adoption of new and abbreviated forms for records the clerical work has been materially decreased without the sacrifice of accuracy, though in spite of this reduction in the work and the increase of the force it is only now possible to keep the work of the Bureau well up to date. This, I think, will readily be understood if it is remembered that since 1886 the number of packages accounted for has been nearly doubled.

Six thousand four hundred and sixty-one more packages were handled in 1891-'92 than in the previous year, and on June 30, 1892, there were but 102 packages on hand to be disposed of.

The increased number of shipments to the principal foreign countries has been maintained, as shown in the tables appended as Exhibit A. A further improvement in this direction can be looked for only when the appropriations made by Congress become sufficient to enable the Institution to pay for fast freight. As it is now, free freight is granted by a majority of the ocean steamship companies to the Smithsonian Institution in its endeavor to increase and diffuse knowledge among men, while full rates would be charged to the United States Government for a similar service; and where the privilege of free freight has not been secured the exchange boxes are sent by slow steamers or by sailing vessels offering low rates.

I take pleasure in bearing witness to the conscientious efficiency of the employés of the Exchange Office, and I beg leave to express my appreciation of the careful attention to the interests of the Bureau on the part of its special agents abroad, Doctor Felix Flügel, in Leipzig, and Messrs. William Wesley & Son, in London.

Grateful acknowledgments are also due to the following transportation companies and firms for their continued liberality in granting free freight, or in otherwise assisting in the transmission of exchange parcels and boxes, while to other firms thanks are due for reduced rates of transportation in consideration of the disinterested services of the Institution in the diffusion of knowledge.

LIST OF SHIPPING AGENTS GIVING FREE FREIGHT.

Allen Steamship Company (A. Schumacher & Co., agents), Baltimore.
d'Almeirim, Baron, Royal Portuguese consul-general, New York.
American Board of Commissioners for Foreign Missions, Boston.
American Colonization Society, Washington, District of Columbia.
Anchor Steamship Line (Henderson & Bro., agents), New York.
Atlas Steamship Company (Pim, Forwood & Co.), New York.
Bailey, H. B., & Co., New York.
Börs, C., consul-general for Sweden and Norway, New York.
Botassi, D. W., consul-general for Greece, New York.
Boulton, Bliss and Dallett, New York.
Calderon, Climaco, consul-general for Colombia, New York.
Caldo, A. G., consul-general for Argentine Republic, New York.
Cameron, R. W. & Co., New York.
Baltazzi, X., consul-general for Turkey, New York.
Compagnie Générale Transatlantique (A. Forget, agent), New York.
Cunard Royal Mail Steamship Company (Vernon H. Brown & Co., agents), New York.
Espriella, Justo R. de la, consul-general for Chile, New York.
Florio Rubattino Line—Navigazione Generale Italiana (Phelps Bros. & Co.), New York.
Hamburg American Packet Company (R. J. Cortis, manager), New York.
Hensel, Bruckmann & Lorbacher, New York.
Inman Steamship Company (Henderson & Bro., agents), New York.
Mantez, Jose, consul-general for Uraguay, New York.
Muñoz y Espriella, New York.
Navarro, J. N., consul-general for Mexico, New York.
Netherlands American Steam Navigation Company (W. H. Vanden Toorn, agent), New York.
New York and Brazil Mail Steamship Company, New York.
New York and Mexico Steamship Company, New York.
North German Lloyd (agents: Oelrichs & Co., New York; A. Schumacher & Co., Baltimore).
Obarrio, Melchor, consul-general for Bolivia, New York.
Pacific Mail Steamship Company (H. J. Bullay, superintendent), New York.
Panama Railroad Company, New York.
Pioneer Line (R. W. Cameron & Co.), New York.
Perry, Ed., & Co., New York.
Pomares, Mariano, consul-general for Salvador, New York.
Red Star Line (Peter Wright & Sons, agents), New York and Philadelphia.
Royal Danish consul, New York.
Ruiz, Domingo L., consul-general for Ecuador.
Stewart, Alexander, consul-general for Paraguay, Washington, District of Columbia.
Toriello, Enrique, consul-general for Guatemala, New York.
White Cross Line of Antwerp (Funch, Edye & Co.), New York.

LIST OF THE CORRESPONDENTS OF THE SMITHSONIAN ACTING AS ITS AGENTS FOR THE INTERNATIONAL EXCHANGES.

Algeria: Bureau Français des Échanges Internationaux, Paris, France.
Argentine Republic: Museo Nacional, Buenos Aires.
Austria-Hungary: Dr. Felix Flügel, No. 1, Robert Schumann Strasse, Leipzig, Germany.
Brazil: Bibliotheca Nacional, Rio Janeiro.
Belgium: Commission des Échanges Internationaux, Rue du Musée, No. 5, Brussels.
Bolivia: University, Chuquisaca.
British America: McGill College, Montreal, or Geological Survey Office, Ottawa.
British Colonies: Crown Agents for the Colonies, London, England.
British Guiana: The Observatory, Georgetown.
Cape Colony: Colonial Secretary, Cape Town.
China: Dr. B. W. Doberck, Government Astronomer, Hong-Kong; for Shanghai: Zi-ka-wei Observatory, Shanghai.
Chili: Museo Nacional, Santiago.
Colombia (U. S. of): National Library, Bogota.
Costa Rica: Instituto Fisico-geográfico Nacional, San José.
Cuba: Dr. Frederico Poey, Calle del Rayo, 19, Habana, Cuba.
Denmark: Kongelige Danske Videnskabernes Selskab, Copenhagen.
Dutch Guiana: Surinaamsche Koloniaale Bibliotheek, Paramaribo.
East India: Director-General of Stores, India Office, London.
Ecuador: Observatorio del Colegio Nacional, Quito.
Egypt: Institut Egyptien, Cairo.
France: Bureau Français des Échanges Internationaux, Paris.
Germany: Dr. Felix Flügel, No. 1, Robert Schumann Strasse, Leipzig.
Great Britain and Ireland: William Wesley & Son, 28, Essex street, Strand, London.
Greece: National Library, Athens.
Guatemala: Instituto Nacional de Guatemala, Guatemala.
Guadeloupe (Same as France.)
Haiti: Secrétaire d'État des Rélations Extérieures, Port au Prince.
Honduras: Bibliotheca Nacional, Tegucigalpa.
Iceland: Icelands Stiptisbokasáfn, Reykjavik.
Italy: Biblioteca Nazionale Vittorio Emanuele, Rome.
Japan: Minister of Foreign Affairs, Tokio.
Java: (Same as Holland.)
Liberia: Liberia College, Monrovia.
Madeira: Director-General, Army Medical Department, London, England.
Malta: (Same as Madeira.)
Mauritius: Royal Society of Arts and Sciences, Port Louis.
Mozambique: Sociedad de Geografia, Mozambique.
Mexico: Packages sent by mail.
New Caledonia: Gordon & Gotch, London, England.
Newfoundland: Postmaster General, St. Johns.
New South Wales: Government Board for International Exchanges, Sydney.
Netherlands: Bureau Scientifique Central Néerlandais, Den Helder.
New Zealand: Colonial Museum, Wellington.
Nicaragua: care Captain J. M. Dow, Panama.
Norway: Kongelige Norske Frederiks Universitet, Christiania.
Paraguay: Government, Asuncion.
Peru: Biblioteca Nacional, Lima.
Philippine Islands: Royal Economical Society, Manila.
Polynesia: Department of Foreign Affairs, Honolulu
Portugal: Bibliotheca Nacional, Lisbon.

Queensland: Government Meteorological Observatory, Brisbane.
Roumania: (Same as Germany).
Russia: Commission Russe des Échanges Internationaux, Bibliothèque Impériale Publique, St. Petersburg.
St. Helena: Director General, Army Medical Department, London, England.
San Salvador: Museo Nacional, San Salvador.
Servia: (Same as Germany.)
South Australia: General Post-Office, Adelaide.
Spain: R. Academia de Ciencias, Madrid.
Sweden: Kongliga Svenska Vetenskaps Akademien, Stockholm.
Switzerland: Central Library, Bern.
Tasmania: Royal Society of Tasmania, Hobarton.
Turkey: American Board of Commissioners for Foreign Missions, Boston.
Uruguay: Oficina de Deposito, Reparto y Canje Internacional, Montevideo.
Venezuela: University Library, Caracas.
Victoria: Public Library, Museum, and National Gallery, Melbourne.

Appendix A.

Transmission of exchanges to foreign countries.

Country.	Date of transmission, etc.
Argentine Republic	October 13, November 10, December 16, 1891; January 19, April 6, June 14, 1892.
Austria-Hungary	July 14, 25, August 19, September 3, October 3, 14, 27, November 6, 19, 23, December 8, 17, 23, 1891; January 13, 25, February 5, 10, 17, March 1, 4, 11, 12, 25, 29, April 11, 19, 30, May 3, 23, 28, June 20, 30, 1892.
Belgium	October 20, November 12, 23, 1891; January 15, February 1, 19, March 21, April 2, May 12. June 6, 30, 1892.
Bolivia	December 16, 1891.
Brazil	October 13, November 10, 1891; January 19, April 16. June 14, 30, 1892.
British Colonies	October 29, 1891, January 26, June 17, 30, 1892.
Cape Colony	August 1, November 16, 1891; January 26, June 16, 1892.
China	January 18, April 4, June 10, 1892.
Chile	October 13, 1891; February 11, April 6, June 14, 30, 1882.
Colombia	October 13, 1891; January 19, April 6, June 14, 1892.
Costa Rica	November 13, 1891; January 23, June 18, 1892.
Cuba	November 12, 1881; January 23, June 18, 1892.
Denmark	August 15, November 4, December 11, 1891; January 22, February 3, March 23, April 2, June 6, 1892.
Dutch Guiana	
East India	October 31, December 4, 1891; January 16, April 5, June 11, 1892.
Ecuador	January 19, April 6, 1892.
Egypt	June 16, 1892.
France and Colonies	July 17, August 1, 18, September 5, October 7, 13, 23, November 9, 16, 20, December 10, 19, 23, 28, 1891; January 15, 28, February 10, 29, March 5, 12, 17, 28, April 2, 19, 30, May 6, 27, June 16, 21, 29, 1892.
Germany	July 14, 25, August 19, September 3, October 3, 14, 27; November 6, 19, 23, December 8, 17, 23, 1891; January 13, 25, February 5, 10, 17, March 1, 4, 11, 12, 25, 29, April 11, 19, 30, May 3, 23, 28, June 20, 28, 1892.
Great Britain, etc	July 23, August 1, 19, 31, September 19, October 9, 24, 29, November 6, 10, 21, December 4, 14, 26, 1891; January 15, 26, February 4, 15, 24 March 3, 10, 14, 19, 26, 29, April 12, 19, 30, May 4, 17, 26, 31, June 20, 29, 1892.

Transmission of exchanges to foreign countries—Continued.

Country.	Date of transmission, etc.
Greece	January 23, February 3, 1892.
Guatamala	November 13, 1891; June 18, 1892.
Haiti	November 12, 1891; June 18, 1892.
Honduras	November 13, 1891; March 4, 1892.
Italy	July 31, August 20, October 13, 16, November 4, 20, December 8, 28, 1891; January 20, February 1, 19, March 14, 17, April 1, 26, May 10, June 2, 30, 1892.
Japan	August 3, November 4, 24, 1891; January 18, February 1, April 4, June 10, 30, 1892.
Liberia	November 16, 1891; June —, 1892.
Mexico	(By registered mail.)
New South Wales	October 31, December 4, 1891; January 16, April 5, June 11, 1892.
Netherlands and Colonies	October 22, November 24, December 28, 1891; January 22, March 22, April 11, May 3, June 3, 30, 1892.
New Zealand	October 31, December 4, 1891; January 16, April 5, June 11, 1892.
Nicaragua	November 13, 1891; June 18, 1892.
Norway	August 15, October 23, December 14, 28, 1891; April 9, June 9, 30, 1892.
Peru	October 13, 1891; January 19, April 6, June 14, 1892.
Polynesia	October 31, 1891; January 16, April 5, June 11, 1892.
Portugal	October 23, December 12, 28, 1891; February 3, April 11, June 9, 1892.
Queensland	October 31, December 4, 1891; January 16, February 1, April 5, June 11, 30, 1892.
Roumania	(Included in Germany.)
Russia	July 28, October 14, 29, November 11, 24, December 28, 1891; January 21, 29, February 20, March 14, 15, April 7, 26, May 6, June 4, 30, 1892.
San Domingo	October 16, 1891.
San Salvador	November 13, 1891.
Servia	(Included in Germany.)
South Australia	October 31, December 4, 1891; January 16, April 5, June 11, 1892.
Spain	October 22, November 23, December 28, 1891; January 21, February 3, April 9, June 4, 1892.
Sweden	July 28, October 20, November 11, 24, December 28, 1891; January 21, 29, March 16, April 7, May 6, June 4, 30, 1892.
Switzerland	October 22, November 16, December 11, 28, 1891; January 21, February 1, 23, March 21, April 8, June 6, 30, 1892.
Tasmania	January 16, April—, 1892.
Turkey	November 19, 1891; January 23, June 17, 1892.
Uruguay	October 13, December 16, 1891; June 14, 1892.
Venezuela	October 13, 1891; January 19, April 6, June 14, 1892.
Victoria	October 31, December 4, 1891; January 16, February 1, April 5, June 11, 1892.
West Africa	October 16, 1891.

Shipments to India, New South Wales, Victoria, New Zealand, Tasmania, and Crown agents were made in bundles inclosed in case number 838, June 30, 1892.

Shipments of United States Congressional publications were made on August 26, November 24, 1891; February 29, May 17, 1892, to the Governments of the following named countries:

Argentine Republic,	Colombia,	Japan,	Saxony,
Austria,	Denmark,	Netherlands,	South Australia,
Baden,	France,	New South Wales,	Spain,
Bavaria,	Germany,	New Zealand,	Sweden,
Belgium,	England,	Norway,	Switzerland,
Buenos Aires,	Greece,†	Peru,	Tasmania,
Brazil,	Haiti,	Portugal,	Turkey,
Canada (Ottawa),	Hungary,	Prussia,	Venezuela,
Canada (Toronto),	India,	Queensland,	Victoria,
Chili,*	Italy,	Russia,	Würtemberg,

The distribution to foreign countries was made in 846 cases, representing 283 transmissions, as follows:

Argentine Republic	15	Mexico (by mail).	
Austria-Hungary	53	New South Wales	10
Belgium	25	Netherlands	20
Bolivia	1	New Zealand	6
Brazil	11	Nicaragua	2
British Colonies	5	Norway	15
Cape Colony	4	Peru	4
China	3	Polynesia	4
Chile	7	Portugal	6
Colombia	4	Queensland	11
Costa Rica	3	Roumania (included in Germany).	
Cuba	3	Russia	37
Denmark	12	San Domingo	1
East India	9	San Salvador	1
Ecuador	2	Servia (included in Germany.)	
Egypt	2	South Australia	5
France and Colonies	109	Spain	14
Germany	139	Sweden	22
Great Britain	149	Switzerland	26
Greece	2	Tasmania	2
Guatemala	2	Turkey	3
Haiti	2	Uruguay	2
Honduras	4	Venezuela	4
Italy	56	Victoria	9
Japan	16	West Africa	1
Liberia	2		

RECAPITULATION.

Total Government shipments	169
Total miscellaneous shipments	846
Total shipments	1,015
Total shipments last year	962
Increase over last year	53

Very respectfully, yours,

W. C. WINLOCK,
Curator of Exchanges.

Mr. S. P. LANGLEY,
Secretary of the Smithsonian Institution.

*No shipment made to Greece on May 17, 1892.
†Two special Government cases were sent to Chili on February 11, 1892.

Appendix III.

REPORT OF THE ACTING MANAGER OF THE NATIONAL ZOÖLOGICAL PARK.

Sir: I have the honor to submit the following report of the operations of the National Zoölogical Park for the fiscal year ending June 30, 1892:

At the close of the last year the park had but just been occupied by the animals of the collection. The experience of the present season has been valuable as indicating the lines along which development should proceed.

The main road having been laid out, the permanent locations for the animals were established at convenient distances near it. The bridge across the creek was contracted to be built, and a temporary bridge established until such time as the permanent structure should be completed. The work of completing the bear yards was also continued.

On September 5, a disastrous rain storm occurred, during which Rock Creek, the small stream that flows through the park rose to a height nearly equalling that which it reached at the time of the famous Johnstown flood in 1890. The rise was extraordinarily rapid, being, according to the watchmen of the park, at the rate of 6 feet within two hours. Within a short time a cavity 10 feet deep was excavated by the stream alongside of one of the bridge piers, undermining one of its corners, the temporary bridge was swept away, a large quantity of earth and rock was precipitated from the cliff above into the bear pits, the banks of the creek were eroded and a considerable amount of filling washed away, and the roads and gutters of the park recently laid were cut out and injured to a very great extent. The cost of repairing the damages thus occasioned was nearly $5,000, a sum that could not well be spared from the scanty appropriation, and the loss embarrassed the park very seriously during the entire season.

The bridge pier damaged by the storm was rebuilt and this delayed the final completion of the bridge, which was not finally opened for travel until about October 1.

For the same reason the occupation of the bear yards was postponed until a retaining wall much larger and stronger than had been anticipated could be built, it being considered dangerous to place the animals in yards where some tons of rock and earth might fall after any serious storm.

The main animal house, although far from complete, was hastily prepared for the reception of animals by closing it up with temporary work and substituting for the metal roof designed by the architect a felt roof of cheap construction. The completion of the tower at the eastern end was deferred until more funds should be available.

As soon as the cooler autumn weather set in the number of visitors to the park greatly increased. There was during each Sunday of October and until nearly the last of November an average attendance of about 7,000 people each Sunday, the number reaching over 10,000 on some particularly fine days. The daily attendance during the week was considerably less.

This large influx of visitors tested the arrangements which had been made, and they were found wanting in several respects. The bridge was found to be too narrow and dangerous for foot passengers. The road was in some localities so narrow that it became inconveniently and dangerously crowded. The number of watchmen was found to be entirely inadequate, and the crowd was so great in the principal animal house as to be extremely uncomfortable.

To remedy this state of affairs it seemed necessary to place footways upon the bridge, to relieve the main roadway by making side roads and walks, to enlarge the ground plan of the animal house, and to provide more ample means of exit. It seemed best also to remove from the house as many animals as could be properly accommodated in quarters outside, both for the convenience of the public and the health of the animals.

The limited means at the disposal of the park did not permit the full completion of this plan. New roadways were cut out and new sidewalks built, an addition to the animal house was commenced in the shape of a large wooden shed situated on the north side. None of these were entirely completed at the close of the fiscal year.

A grading plan for a portion of the park was furnished by Mr. F. L. Olmsted. This contemplated the excavation of a large pond for aquatic animals upon the meadow west of the bridge, the shaping up of the banks of the creek and their protection from erosion by means of riprap and the formation of a smaller pond to the north of the road near the main entrance. But little of this could be done during the year, the expenses of preparing the winter quarters of the animals being such that all surplus funds were exhausted. It was indeed found necessary to limit the expenditures to the barest necessities, and although an additional appropriation of $1,000 was made by Congress it was with difficulty that the park was maintained until the end of the year. The force of employés was reduced to the lowest possible number, and every device was used to insure the strictest and most parsimonious economy.

The scantiness of the resources of the park made it necessary to postpone any purchases of animals, and the collection has therefore increased but slowly. There were on June 30, 448 living animals in the collection, of which 320 were mammals, 63 birds, and 65 reptiles. A catalogue of the additions made is appended hereto. Ninety-six animals have been presented to the park during the year, of which 43 were mammals, 26 birds, and 27 reptiles.

The insufficient nature of the temporary quarters in which it has been necessary to keep the animals has led to a considerable mortality. Besides this, it is found that many specimens do not survive the fatigue and excitement of the journey necessary to reach the park, and they succumb shortly after their arrival here. The most alarming mortality has been that of the bears, two of which died from injuries received, two others from pulmonary trouble. While the bear yards are certainly picturesque and effective from the landscape architect's point of view, it is believed that they are not now proper sanitary dwellings for the animals, as they are constantly damp, are too cold in winter and too hot in summer. It is intended to take measures to remedy their defects.

Owing to the small number of watchmen necessarily employed, one of the bears escaped by climbing up a nearly perpendicular wall over 50 feet high. He was pursued and an attempt was made to capture him. This was, however, unsuccessful and it was found necessary, finally, to shoot him.

The elephants have continued constantly to gain in weight since arriving at the park. "Dunk" now weighs 7,260 pounds, having gained 1,110 pounds. "Golddust" weighs 4,920 pounds, having gained 860 pounds.

List of animals presented.

Name.	Donor.	Number of specimens.
Capuchin monkey	Mrs. Hendrickson, Alexandria, Va. (loaned)	2
Teetee	Ensign Roger Welles jr., U. S. Navy	1
Coyote	Capt. G. C. Doane, San Carlos, Ariz	2
Do	R. M. Lee, Buckland, Va	1
Red fox	George Fox, New Lisbon, Ohio	2
Do		1
Do		1
Swift fox	H. Petersen, Washington, D. C	4
Coati-mundi	C. O. Chenault, Jersey City, N. J	1
Raccoon	Prof. H. A. Ward, Rochester, N. Y	2
Do	Miss Margaret Kiewit, Nokesville, Va	1
Do	Miss Bessie Elliot, Washington, D. C	1
Black bear	J. R. Thomas, Whittier, N. C	1
Do	C. S. Hayes, Anacostia, D. C	1
Cinnamon bear	Lieut. G. P. Ahern, Fort Missoula, Mont	1
Grizzly bear	Capt. G. S. Anderson, Mammoth Hot Springs, Wyo	1
Peccary	Hon. R. M. Bartleman, Caracas, Venezuela	1
Zebu	Hon. J. H. Starin, New York City	1
Virginia deer	Thaddeus Surber, White Sulphur Springs, Va	1
Do	D. W. Maxfield, Bangor, Me	1
South American deer	Hon. R. M. Bartleman, Caracas, Venezuela	2
American beaver	U. S. Agricultural Experiment Station, Nebraska, through Prof. C. V. Riley, Washington, D. C	1
Paca	Ensign Roger Welles, jr., U. S. Navy	1
Agouti	Hon. R. M. Bartlemen, Caracas, Venezuela	1
Do	Mr. Greaves, Port of Spain, Trinidad	1
Do	Ensign Roger Welles, jr., U. S. Navy	2
Arizona gray squirrel	W. H. Volandt, Washington, D. C	2
Flying squirrel	Ralph Saers, Mount Pleasant, D. C	1
Prairie dog	Miss Edith A. Barnes, Seabrook, Maryland	1
Woodchuck	J. A. Rubert, Washington, D. C	1
White rat	C. C. Zahn, Washington, D. C	2
Armadillo	Wm. Taylor, San Diego, Texas	1
Do	A. C. Downs, Realitos, Texas	1
Do	Prof. R. T. Hill, U. S. Geological Survey	1
Do	Hon. R. M. Bartlemen, Caracas, Venezuela	1
Opossum	W. H. Babcock, Washington, D. C	1
Do	S. D. Caldwell, Bethesda, Md	1
Do	J. W. Howlett, Washington, D. C	1
Do	President Harrison, Washington, D. C	2
Golden eagle	Capt. H. S. Barbour, Washington, D. C	1
Bald eagle	C. O. Chenault, Jersey City, N. J	1
Sparrow hawk	G. Boegeholz, Washington, D. C	2
Pigeon hawk	E. C. Call, Laurel, Md	1
Marsh hawk	E. B. Clark, Washington, D. C	1
Do	G. B. Coleman, Washington, D. C	1
Do	G. W. Simpson, Washington, D. C	1
Do	H. W. McGeorge, Washington, D. C	1
Red-tailed hawk	J. L. Davison, Lockport, N. Y	1
Hawk	Dr. T. E. Butler, Glen Allan, Miss	1
Barred owl	E. Johnson, Washington, D. C	1
Do	W. L. Bishop, Washington, D. C	1

List of animals presented—Continued.

Name.	Donor.	Number of specimens.
Great horned owl	Employés of H. E. Burgess, Washington, D. C	1
Do	P. T. Bell, Conowingo Md	1
Screech owl	Mrs. Babcock, Washington, D. C	1
Do	J. J. Mahoney, Washington, D. C	5
Common crow	Mrs. Wheeler, Washington, D. C	1
American magpie	E. S. Schmid, Washington, D. C. (loaned)	8
Golden-winged woodpecker	Cortez Daniel, Leesburg, Va	1
Blue and yellow macaw	D. M. Cranford, Washington, D. C. (loaned)	1
Green parrot	Mrs. Williams, Washington, D. C. (loaned)	1
Guan	Hon. C. I. Croft, Cartagena, Colombia, South America	2
Curassow	W. C. Butler, Washington, D. C	1
White ibis	A. M. Nicholson, Orlando, Fla	2
Black-crowned night heron	E. Lyons, Washington, D. C	1
Sandhill crane	Mrs. M. C. Rerdell, Orlando, Fla	2
Alligator	W. E. Wilson, Orange Point, Fla	1
Do	Latta Griswold, Washington, D. C	1
Do	Sterrett Bros., Washington, D. C	2
Do	Sir Julian Pauncefote, Washington, D. C	1
Do	U. S. Fish Commission (loaned)	1
Do	S. C. Williams, Washington, D. C	2
Caiman	Ensign Roger Welles, jr., U. S. N	1
Soft-shelled turtle		1
Snapping turtle	R. T. Leipold and A. M. Rock, Brookside, W. Va	1
Do		1
Turtle	C. O. Chenault, Jersey City, N. J	1
Chuck molly lizard	Dr. M. M. Crocker, Fort Mojave, Ariz	7
Horned toad	A. T. Gage, Washington, D. C	1
Do	Dr. R. E. C. Stearns, San Diego, Cal	4
Diamond rattlesnake		1
Tiger rattlesnake	Dr. M. M. Crocker, Fort Mojave, Ariz	2
Ground rattlesnake	Capt. Henry Romeyn, Mount Vernon Barracks, Ala	2
Water moccasin	do	2
Copperhead	L. H. Brilton, ———, Ohio	1
King snake	Dr. Z. T. Daniels, Cheyenne Agency, Dakota	1
Do		1
Do	Capt. Henry Romeyn, Mount Vernon Barracks, Ala	1
Black snake	do	1
Do	L. P. Weeden, Washington, D. C	1
Do	J. P. Stabler	2
Whip snake	Capt. Henry Romeyn, Mount Vernon Barracks, Ala	1
Hog-nosed snake	do	1
Boa	Ensign Roger Welles, jr., U. S. N	1

List of accessions.

Name.	Specimens.
Diana monkey (*Cercopithecus diana*)	1
Barrigudo (*Lagothrix humboldtii*)	1
Capuchin monkey (*Cebus capucinus.*)	2
Sapajous (*Cebus hypoleucus*)	2
Durukuli (*Nyctipithecus trivirgatus*)	1
Teetee (*Chrysothrix sciureus*)	3
Marmoset (*Hapale jacchus*)	5
Lion (*Felis leo*)	1
Ocelot (*Felis pardalis*)	4
American Wildcat (*Lynx rufus*)	3
Coyote (*Canis latrans*)	4
Red fox (*Vulpes fulvus*)	7
Arctic fox (*Vulpes lagopus*)	1
Gray fox (*Vulpes virginianus*)	2
Swift fox (*Vulpes velox*)	8
American badger (*Taxidea americana*)	3
Common skunk (*Mephitis mephitica*)	3
American otter (*Lutra canadensis*)	1
Grizzly bear (*Ursus horribilis*)	1
Black bear (*Ursus americanus*)	2
"Cinnamon" bear (*Ursus americanus*)	1
Polar bear (*Thalassarctos maritimus*)	2
Raccoon (*Procyon lotor*)	7
Coati-mundi (*Nasua rufa*)	1
Coati-mundi (*Nasua narica*)	1
Kinkajou (*Cercoleptes caudivolvulus*)	2
Zebu (*Bos indicus*)	1
American bison (*Bison americanus*)	1
American antelope (*Antilocapra americana*)	3
Angora goat (*Capra hircus angorensis*)	1
Virginia deer (*Cariacus virginianus*)	5
South American deer (*Coassus* sp.)	3
Moose (*Alces machlis*)	1
Collared peccary (*Dicotyles tajacu*)	5
Flying squirrel (*Sciuropterus volucella*)	1
Arizona gray squirrel	2
Striped gopher (*Spermophilus tridecimlineatus*)	33
Prairie dog (*Cynomys ludovicianus*)	52
Woodchuck (*Arctomys monax*)	1
American beaver (*Castor canadensis*)	1
Muskrat (*Fiber zibethicus*)	14
White rat (*Mus rattus*)	2
Coypu (*Myopotamus coypu*)	1
White rabbit (*Lepus cuniculus*)	6
Tree porcupine (*Synetheres prehensilis*)	1
Capybara (*Hydrochœrus capybara*)	1
Paca (*Cœlogenys paca*)	1
Agouti (*Dasyprocta aguti*)	10
Acouchy (*Dasyprocta acouchy*)	3
Guinea pig (*Cavia aperea*)	11
Great anteater (*Myrmecophaga jubata*)	1
Peba armadillo (*Tatusia novemcincta*)	4
Opossum (*Didelphys virginiana*)	5
Bald eagle (*Haliaetus leucocephalus*)	1
Golden eagle (*Aquila chrysaëtos*)	1
Sparrow hawk (*Falco sparverius*)	2
Pigeon hawk (*Falco columbarius*)	1
Red-tailed hawk (*Buteo borealis*)	2
Marsh hawk (*Circus hudsonius*)	4
Great horned owl (*Bubo virginianus*)	2
Barred owl (*Syrnium nebulosum*)	2
Screech owl (*Megascops asio*)	6
American magpie (*Pica pica hudsonica*)	8
American crow (*Corvus americanus*)	1
Golden-winged woodpecker (*Colaptes auratus*)	1
Yellow and blue macaw (*Ara ararauna*)	1
Green parrot	9
Parrakeet	1
Crested curassow (*Crax alector*)	4
Sandhill crane (*Grus mexicana*)	2
Black-crowned night heron (*Nycticorax nycticorax nævius*)	1
Scarlet ibis (*Guara rubra*)	3
White ibis (*Guara alba*)	2
Gannet (*Sula bassana*)	1
Alligator (*Alligator mississippiensis*)	8
Caiman (*Jacare sclerops*)	1
Soft-shelled turtle (*Aspidonectes ferox*)	1
Snapping-turtle (*Chelydra serpentina*)	2
Painted turtle (*Chrysemys picta*)	3
Chuck-molly (*Sauromalus ater*)	7
Marbled polychrus (*Polychrus marmoratus*)	2
Horned toad (*Phrynosoma douglassii*)	5
South American lizards (un-named)	23
Banded rattlesnake (*Crotalus horridus*)	1
Diamond rattlesnake (*Crotalus adamanteus*)	3
Ground rattlesnake (*Caudisona miliaris*)	3
Water moccasin (*Ancistrodon piscivorus*)	1
Copperhead (*Ancistrodon contortrix*)	2
King snake (*Ophibolus getulus*)	1
Ring-necked snake (*Diadophis punctatus*)	1
Black snake (*Bascanium constrictor*)	5
Coach-whip snake (*Bascanium flagelliforme*)	2
Common boa (Boa constrictor)	3
Anaconda (*Eunectes murinus*)	1
Garter snake (*Eutænia sirtalis*)	1
Hog nose snake (*Heterodon platyrhinus*)	1
Small South American snakes (unnamed)	19
South American batrachians (unnamed)	16

Respectfully submitted,

FRANK BAKER, *Acting Manager.*

Secretary S. P. LANGLEY, *Smithsonian Institution.*

APPENDIX IV.

REPORT OF THE LIBRARIAN FOR THE YEAR ENDING JUNE 30, 1892.

SIR: I have the honor to submit herewith the report on the library of the Smithsonian Institution during the year ending June 30, 1892.

The operations of the library have been conducted as in the two preceding years. The entry numbers in the accession book extend from 225,586 to 246,109.

Following is a statement of the volumes, parts of volumes, pamphlets, and charts received during the year:

Publications received between July 1, 1891, and June 30, 1892.

	Octavo or smaller.	Quarto or larger.	Total.
Volumes	1,320	669	1,989
Parts of volumes	7,631	16,098	23,729
Pamphlets	3,087	502	3,589
Charts			621
Total			29,928

Of these publications, 297 volumes, 6,363 parts of volumes, and 774 pamphlets—7,434 in all—were retained for use in the National Museum.

Eight hundred and fifty-seven medical dissertations were deposited in the library of the Surgeon-General, U. S. Army; the remaining publications were sent to the Library of Congress on the Monday after their receipt.

In carrying out the plans formulated by the Secretary for increasing the library by exchanges, 803 letters asking for publications not on our list, or asking for numbers to complete the series already in the library, have been written. As a result of this correspondence it gives me pleasure to report that 444 new exchanges were acquired by the Institution, while 220 defective series were completed either wholly or as far as the publishers were able to supply missing parts.

Below is a comparative statement of the operations of the library since June 30, 1889:

Number of publications received.

	1889–'90.	1890–'91.	1891–'92.
Volumes	1,763	2,681	1,989
Parts of volumes	13,458	20,525	23,729
Pamphlets	4,330	3,769	3,589
Charts	636	319	621
Total	20,187	27,294	29,928

The following universities have sent complete lists of all their academic publications:

Basel,	Halle a S.,	Lund,
Bern,	Heidelberg,	Marburg,
Bonn,	Helsingfors,	Strassburg,
Christiania,	Jena,	Tübingen,
Dorpat,	Kazan,	Vienna,
Erlangen,	Kiel,	Würzburg,
Freiberg, Br.,	Leipsic,	Utrecht,
Giessen,	Louvain,	Zürich.
Göttingen,		

The following publications have been added to the list of regular serials:

A A Notes (Archit's' Assoc.), London.
Acts of the Parliament of South Australia, Adelaide.
Actes Société Simico-Japonaise, Paris.
Agricultural Science, State College, Pa.
Amateur Sportsman, New York.
American Amateur Photographer, New York.
American Anthropologist, Washington, D. C.
American Cyclist, Hartford, Conn.
American Florist, Chicago.
American Gardening, New York.
American Jeweler, Chicago.
American Journal of Philately, New York.
American Naturalist, Philadelphia.
American Notes and Queries, Philadelphia.
Analele Academa Romana, Bucharest.
Anales de la Universidad Central del Ecuador, Quito.
Anales de la Universidad de Montevideo.
Annaes Biblioteca Nacional, Rio Janeiro.
Annalen der Physik und Chemie, Leipsic.
Annales de Chimie et de Physique, Paris.
Annals of Scottish Natural History, Edinburgh.
Annuaire, Societé des études juives, Paris.
Annuaire israélite, Société des études juives, Paris.
Annuaire Statistique des Pays-Bas, Amsterdam.
Annual Report Agricultural Bureau, Adelaide.
Annual Report Chiswick Free Public Library.
Annual Report Department of Agriculture, Brisbane.
Annual Report Dep't of Mines, Sydney.
Annual Report Gordon Technical College, Geelong, Australia.
Annual Report and Prospectus School of Mines, Stawell, Australia.
Annuario Scolastico Regia Università, Parma.
Annuario Società Reale Academia di Archeologia, Naples.
Antiquitäten-Zeitschrift, Strassburg.
Anuario, Asociacion de Ingenieros Industriales, Barcelona.
Archief Zeeuwsch Genootschap der Wetenschappen, Middelburg.
Archives des Sciences Biologiques, St. Petersburg.
Argus Annual, Cape Town.
Army and Navy Journal, New York.
L'Art et l'idée, Paris.
Artist Printer, Chicago.
Ateneo Italiano, Rome.
Atti Società Reale Accademia di Archeologia, etc., Naples.
Babylonian and Oriental Record, London.
Bacteriological World, Battle Creek, Michigan.
Baptist Quarterly Review, New York.
Beiblätter zu den Annalen der Physik und Chemie, Leipsic.
Bergmanns Kalender, Saarbrücken.
Bericht des akademischen Vereins deutscher Historie, Vienna.
Berichte der bayerischen botanischen Gesellschaft, Munich.
Berichte der deutschen chemischen Gesellschaft, Berlin.
Bible Advocate, Birmingham.
Bible Society Record, New York.
Bibliographie des Travaux Historiques et Archéologiques, Paris.
Bibliotheca Philologica Classica, Berlin.
Bicycling World, Boston.
Blacksmith and Wheelwright, N. Y.
Blackwood's Edinburgh Magazine.
Body and Soul, Cardiff.
Boletim de la Sociedade Broteriana, Coimbra.
Boletim Sociedade de Geographia, Rio Janeiro.
Boletim de Agricultura, Mineria e Industrias, Mexico.
Boletin Bibliographico y Escola, Mexico.
Boletin de la Institucion Libre de Enseñanza, Madrid.
Boletin de la Real Academia de Ciencias y Artes, Barcelona.
Boletin de la Sociedad Geográfica, Lima.
Bollettino Mensile della Situazione dei Conti, etc., Rome.
Bollettino delle Pubblicazioni Italiane, Florence.
Bollettino della Reale Accademia Medica, Genoa.

Bollettino della Società Adriatica di Scienze Naturali, Trieste.
Bollettino della Società di Naturalisti, Naples.
Bollettino della Società Romana per gli Studi Zoologica, Rome.
Book Buyer and Seller, Cincinnati.
Book Shop, New York.
Books, Denver.
Brazilian Missions, Brooklyn.
Breeder and Sportsman, San Francisco.
British Naturalist, Hartlepool.
Buletin Societatea Geografica Romàna, Bukarest.
Bulletin Aéronautique, Paris.
Bulletin Agricultural Experiment Station, Reno, Nevada.
Bulletin Association Polytechnique, Paris.
Bulletin Astronomique, Paris.
Bulletin of the Botanical Department, Kingston, Jamaica.
Bulletin Commission Archéologique de Narbonne.
Bulletin Cornell University Experiment Station, Ithaca.
Bulletin Department of Agriculture, Toronto.
Bulletin of the Geological Society of America.
Bulletin of the Library and Museum of Laurent College, Montreal.
Bulletin Mensuel des Publications Étrangères, Paris.
Bulletin Mensuel Statistique Municipale, Buenos Aires.
Bulletin du Ministère de l'Instruction Publique, Brussels.
Bulletin New York Mathematical Society, New York.
Bulletin Ontario Agricultural Experiment Farm, Toronto.
Bulletin Pennsylvania State College Agricultural Experiment Station.
Bulletin Société d'Agriculture du Dépt. du Cher, Bourges.
Bulletin de la Société Française de Physique, Paris.
Bulletin de la Société d'Histoire et d'Archéologie, Geneva.
Bulletin de la Société d'Horticulture du Doubs, Besançon.
Bulletin Société Royale Linnéenne, Brussels.
Bulletin Société de Statistique des Sciences Naturelles, Grenoble.
Buletinul Observatinnilov Meteorologici din Romania, Bucharest.
Bye-Gones, Oswestry, England
Calabria, Monteleone, Italy.
Cambridge University Reporter.
Canadian Bee Journal, Beeton, Ontario.
Canadian Entomologist, London, Ontario.
Canadian Patent Office Record, Ottawa.
Canadian Poultry Journal, Beeton, Ontario.
Canadiana, Montreal.
Cape Times, Cape Town.
Capitale (now L'Universelle), Paris.
Carpet and Upholstery Trade Review, New York.
Carpentery and Building, New York.
Carrier Dove, San Francisco.
Casopis pro prumysl chemicky, Prague.
Cassier's Magazine, New York.
Cesky Lid, Prague.
Chinese-American Advocate, Philadelphia.
Christian Recorder, Philadelphia.
Christian Worker, Manchester, England.
Chronique Industrielle, Paris.
Church and Home Magazine, London.
Church Union, New York.
Circular System, Oakland, California.
Circular Leland Stanford, Jr., University, Palo Alto, California.
Civics, New York.
Civil Service Record, Boston
Clay Record, Chicago.
Clay Worker, Indianapolis.
Collector (monthly), New York.
Collector (semimonthly), New York.
Collector's Monthly, Danielsonville, Conn.
College Echo, Austin, Tex.
Compass, New York.
Comptes Rendus des Séances de la Société Américaine, Paris.
Comptes Rendus de L'Athénée Louisianais, New Orleans.
Conchologist, St. Andrews, Scotland.
Congo Illustré (Le), Brussels.
Contemporary Review, London.
Contributions Historical Society, Helena, Mont.
Cornhill Magazine, London.
Crank, Ithaca.
Culture, Bombay.
Current Review, New York.
Darkest Russia, London.
Dedham Historical Register, Dedham, Mass.
Deutsche Zuckerindustrie, Berlin.
Discovery, London.
Documente privitor la Istoria Romanilor culese de Eudoxiu, Academia Romàna, Bucharest.
Droit d'Auteur (Le), Berne.
Ecclesiastical Chronicle, London.
Echo Polyglotte (L'), Paris.
Economista Español (El), Barcelona.
Edinburgh Review, Edinburgh.
Electric Power, New York.
Electrical Enterprise, Boston.
Electricity, New York.
Elektrichestvo Zhurnal, St. Petersburg.
Electrotechnische Rundschau, Frankfort O. M.
Entomologist's Record, London.
Erdészeti Lapok Közlonye, Budapest.
Esoteric, Applegate, Cal.
Experiment Station Bulletin (U. S. Dept. of Agriculture).
Experiment Station Record (U. S. Dept. of Agriculture).
Fanciers' Journal, Philadelphia.
Farben-Industrie, Berlin.
Farm, Field, and Stockman, Chicago.

Farmers' Bulletin (U. S. Dept. of Agriculture).
Fauna, Luxemburg.
Federal Reporter, St. Paul.
Fernschau, Mitteischweizerische Geographische Commercielle Gesellschaft Aarau.
Financial World, Boston.
Fortnightly Review, London.
Fortschritte der Physik, Berlin.
Franco-Gallia, Cassel.
French and German Echoes, London.
Geelong Naturalist, Geelong, Australia.
Gewerbehalle, New York.
Gewerbeschau, Dresden.
Great Divide, Denver.
Guide, Glasgow.
Hapisgoh, Baltimore.
Harness Gazette, Rome, N. Y.
Helios, Frankfort o. O.
Hide and Leather, Chicago.
Hintz's Moderne Häuser, Berlin.
Hoisting, Stamford, Conn.
Home and Country, New York.
Home Cheer, New York.
Ice and Refrigeration, Chicago.
Illustrirte Welt, Stuttgart.
Insekten-Börse, Leipsic.
Instructor (El), Aguas Calientes, Mexico.
Internationale Patent-Zeitung, Berlin.
Inventors' Review, London.
Inzhjenjer, Kiev.
Iowa School Journal, Des Moines.
Irish Naturalist, Dublin.
Irrigation Age, Denver, Colo.
Iron Belt, Roanoke, Va.
Jahresbericht Geographische Gesellschaft, Bern.
Jahresberichte Verein für Erdkunde, Cassel.
Jewish Messenger, New York.
Journal of Comparative Neurology, Granville, Ohio.
Journal de l'Éclairage au Gaz, Paris.
Journal of the Engineering Society of the Lehigh University, Bethlehem, Pa.
Journal of the Institute of Jamaica, Kingston.
Journal of Medical Philosophy and Practice, Philadelphia.
Journal of Philately, New York.
Journal of Philology, Cambridge, Eng.
Journal of the Polynesian Society, Wellington.
Journal of the Society of Dyers and Colorists, Bradford, England.
Journal of the U. S. Artillery, Fort Monroe.
Juvenile Magazine for the Young, London.
Kansas University Quarterly, Lawrence.
Knowledge, New York.
K. T. S. News, Mount Sterling, Ky.
Landwirthschaftliche Jahrbuch der Schweiz, Bern.
Library Record, Jersey City.
Light, London.
Lithographer, London.
Lithographers' Journal, Philadelphia.
Littell's Living Age, Boston.
Litterarischer Merkur, Weimar.
Locomotive Engineering, New York.
London Quarterly Review, London.
Longman's Magazine, London.
Manufacturer and Builder, New York.
Manufacturers' Engineering and Export Journal, London.
Marine Rundschau, Berlin.
Marine Verordnungsblatt, Berlin.
Matériaux et Documents d'Architecture et de Sculpture, Paris.
Meddelelser fra Carlberg Laboratoriet, Copenhagen.
Mémoires Société Royale de Géographie, Antwerp.
Memoirs British Astronomical Association, London.
Memorias Sociedad Científica, Mexico.
Memorie Società degli Spettroscopisti Italiani, Rome.
Mercurio Occidental, Guadalajara.
Meteorologicheskija Nobljudenija, Odessa.
Methodist Review, New York.
Milling, Indianapolis.
Mineralogists' Magazine, Jersey City.
Minerals, New York.
Minerva, Rome.
Minutes of the Managing Committee, Provincial Museum, Lucknow.
Mitteilungen aus dem gesammten Gebiete der englischen Sprache und Litteratur, Leipsic.
Mitteilungen der Vereinigung von Freunden der Astronomie und kosmischen Physik, Berlin.
Mitteilungen Vereins für Kunst und Alterthum, Ulm.
Mittheilungen von F. A. Brockhaus, Leipsic.
Mittheilungen aus dem Gebiete der angewandten Naturwissenschaften, Schönberg, Moravia.
Mittheilungen des ornithologischen Vereins, Vienna.
Mittheilungen der Section für Naturkunde, Oesterreichischen Touristen-Club, Vienna.
Mittheilungen der statistischen Amtes, Dresden.
Mittheilungen des Verbandes deutscher Architekten und Ingenieure, Berlin.
Modern Language Monthly, London.
Modern Miller, Kansas City.
Monatsblatt der numismatischen Gesellschaft, Vienna.
Monitor de la Educacion Comun, Buenos Aires.
Monthly Bulletin Colorado State Weather Service, Denver.
Monthly Bulletin Texas Weather Service, Galveston.
Monthly Chronicle of North Country Lore and Legend, Newcastle u. Tyne.
Monthly Weather Review, Calcutta.
Mouvement Antiesclavagiste, Brussels.
Nabytki Biblioteki, Cracow.
Narragansett Historical Register, Providence.
Nasha Pistsha, St. Petersburg.

National Coopers' Journal, Buffalo, N. Y.
National Educator, Allentown, Pa.
National Monitor of Poultry and Pets, Fort Wayne, Ind.
Natural Science, London.
Nature (La), Paris.
Neptunia, Venice.
Neue Mitteilungen aus dem Gebiete der historisch-antiquarischen Forschungen, Halle a. S.
Neue Philologische Rundshau, Gotha.
New Jerusalem Magazine, Boston.
New Nation, Boston.
New York State Library Bulletin, Albany, N. Y.
Nineteenth Century, London.
North American Review, New York.
Nouvelles Géographiques, Paris.
Observations faites à l'Observatoire Météorologique, Kiev.
Observations Finska Vetenskaps-Societetens meteorologiska Centralanstalt, Helsingfors.
Observations Institut Météorologique Central, Helsingfors.
Oesterreichische Zeitschrift für Verwaltung, Vienna.
Onderzokningen Physiologische Laboratoriet, Utrecht.
One and All, Birmingham.
Onward and Upward, Aberdeen.
Operele principelier Dimileantermiru, Bucharest.
Ornithologist and Botanist, Birmingham.
Our Day, Boston.
Painswick Annual Register.
Painting and Decorating, Philadelphia.
P. C. P. Alumni Report, Philadelphia.
Pedagogical Seminary, Worcester.
Pestalozziblätter, Zurich.
Peterborough Diocesan Magazine, Leicester.
Petit Étranger (Le), Paris.
Pharmaceutical Record, New York.
Phonographic Magazine, Cincinnati.
Phosphate (Le), Amiens.
Photographic Work, London.
Photographischer Correspondenz, Vienna.
Postal Record, New York.
Power and Transmission, Mishawaka.
Proceedings of the Cotteswold Naturalists' Field Club, Cheltenham.
Proceedings of the Society of Antiquaries, Newcastle u. Tyne.
Protokoly Zasiedanij oidjelenija, Khimii, St. Petersburg.
Public Library Bulletin, Los Angeles.
Publications Alfred University, Alfred Center, N. Y.
Publications of the Architectural Association, London.
Publications from Dr. C. U. S. Aurivillius, Upsala.
Publications Guille-Allès Library, Guernsey.
Publications by Jardin, M. Ed.
Publications K. K. orientalische Akademie, Vienna.
Publicationen des Königlichen Museum für Naturkunde, Berlin.
Publications of Dr. Olsen.
Publications Section de Moscou de la Société Impériale Technique, Moscow.
Publications University, Vienna.
Quarterly Bulletin American Catholic Hist. Society, Philadelphia.
Quarterly Review, London.
Raportŭ asupra activitatei, Academia Româna, Bukarest.
Rapport École Polytechnique Suisse, Bern.
Records and Papers of the New London County Historical Society, New London, Conn.
Reformed Church Messenger, Philadelphia.
Reformed Quarterly Review, Philadelphia.
Regents' Bulletin, N. Y. State Library, Albany, N. Y.
Religio-Philosophical Journal, Chicago.
Rendiconti Società Reale Accademia di Archeologia, etc., Naples.
Répertoire des Travaux de la Société de Statistique de Marseille.
Repertorium für Meteorologie, St. Petersburg.
Repertorium der technischen Journal-Litteratur, Berlin.
Report Rotherhite Public Library.
Report Society for Promoting Christian Knowledge, London.
Review of Reviews, New York.
Revista General de Marina, Madrid.
Revista Italiana di Scienze Naturali, Naples.
Revista Militar de Chili, Santiago.
Revista del Museo de la Plata, La Plata.
Revue du Bas-Poitou, Fontenay-le-Comte.
Revue de Botanique, Auch.
Revue Botanique, Paris.
Revue de Botanique, Toulouse.
Revue de l'École d'Anthropologie, Paris.
Revue des Etudes Juives, Paris.
Revue d'Horticole, Marseilles.
Revue Internationale Scientifique et Populaire des Falsifications, Amsterdam.
Revue des Livres et de la Presse, Paris.
Revue Mensuelle de l'École d'Anthropologie, Paris.
Revue des Questions Historiques, Paris.
Revue des Questions Scientifiques, Brussels.
Revue des Sciences Naturelles de l'Ouest, Paris.
Revue Universelle des Inventions Nouvelles, A, B, C, D, Paris.
Richmond College Magazine, Galle.
River-Plate Sport and Pastime, Buenos Aires.
Romens's Journal, Charlottenberg.
Rosario, La Nuova Pompei (II), Ville di Pompei.
Rural Californian, Los Angeles.
Rutland County Historical Society, Newport, Vt.

Safety Valve, New York.
St. Joseph's Advocate, Baltimore.
Salem Press Historical and Genealogical Record, Salem.
Scottish Notes and Queries, Aberdeen.
Scottish Review, London.
Séances de la Société Française de Physique, Paris.
Seifensieder Zeitung, Augsburg.
Selmi (Il), Pavia.
Shendun News, Shendun, Va.
Sitzungsberichte der Gesellschaft für Morphologie und Physiologie, Munich.
Socialpolitisches Correspondenzblatt, Berlin.
Sociologic News, Brooklyn.
South Eastern Naturalist, Canterbury, England.
Southern Farm, Atlanta, Ga.
Southern Historical Magazine, Charleston, W. Va.
Sozialpolitisches Centralblatt, Berlin.
Speaker, London.
Sportsman's Review, Chicago.
Strand's Magazine, London.
Sugar, London.
Sugar Beet, Philadelphia.
Supplemento Annuale alla Enciclopedia di chimica scientifica, etc., Turin.
Svensk Kemisk Tidskrift, Stockholm.
Technics, Stawell, Australia.
Temple Bar, London.
Tennessee Journal of Meteorology, Nashville.
Textile Record of America, Philadelphia.
Theosophist, Madras.
Tidsskrift för Folkundervisning, Stockholm.
Tidskrift Jämtlands Läns Forumminnesförening, Ostersund.
Tidsskrif tfor Physik og Chemie, Copenhagen.
To-day, Boston.
Torch, London.
Tradition, La, Paris.
Transactions of the Academy of Science, St. Louis.
Transactions of the Canadian Institute, Toronto.
Transactions Manchester Statistical Society.
Transactions Mining Association of Cornwall, Camborne.
Transactions of the Yorkshire Naturalists, Union, Leeds.
Travaux et Mémoires des Facultés de Lille.
Travaux de la Section de Physico-Chimique de la Société des Sciences Experimentales, Kharkov.
Treasury of Religious Thought, New York.
Uebersicht der Ein und Ausfuhr der wichtigsten Waarenartikel, Bern.
Uchenijja Zapiski, Kazan.
Union Postale (L'), Bern.
Union Signal (The), Chicago.
U. S. Catholic Historical Magazine, New York.
U. S. Miller, Milwaukee.
Universal Market, Berlin.
University Extension Bulletin, Albany.
University Magazine, New York.
University Star, Omaha, Nebr.
Verhandlungen Gelehrte Estnische Gesellschaft, Dorpat.
Veröffentlichungen des Rechen-Instituts der Königlichen Sternwarte, Berlin.
Vierteljahreshefte zur Statistik des deutschen Reiches, Berlin.
Voleur Illustré, Paris.
Volkskunde, Ghent.
Vom Fels zum Meer, Stuttgart.
Vremennik Tsentralnije, St. Petersburg.
Weather crop Bulletin, Crete, Nebr.
Wee Willie Winkie, Aberdeen.
Weekly Bulletin, Boston.
Weekly Stationary Engineer, Chicago.
Western Electrician, Chicago.
Worcester Commercial and Board of Trade Bulletin, Worcester, Mass.
Workshop, New York.
World's Progress, Cincinnati.
Württembergisch-Franken, Hall am/Kocher.
Year Book of Australia, Sydney.
Yorkshire County Magazine, Bradford, England.
Yorkshire Notes and Queries, Bradford, England.
Zdrowie miesieczrisk poswieconij, etc., Warsaw.
Zeitschrift für anorganische Chemie, Munich.
Zeitschrift für deutsche Philologie Halle a/S.
Zeitschrift für Oologie, Berlin.
Zeitschrift des Vereins deutscher Ingenieure, Berlin.
Zeitschrift Verein für Volkskunde, Berlin.
Zeitschrift für Volkskunde, Halle a/S.
Zeitschrift Westpreussicher Geschichts-Verein, Danzig.
Zeitschrift für wissenschaftliche Geographie Weimar.
Zeitschrift für wissenschaftliche Mikroskopie und für mikroskopische Technik, Braunschweig.

Very respectfully submitted.

N. P. SCUDDER,
Acting Librarian.

Mr. S. P. LANGLEY,
Secretary of the Smithsonian Institution.

APPENDIX V.

PUBLICATIONS FOR THE YEAR ENDING JUNE 30, 1892.

SIR: I have the honor to submit the following report upon the publications of the Smithsonian Institution for the year ending June 30, 1892.

SMITHSONIAN CONTRIBUTIONS TO KNOWLEDGE.

Among the issues in quarto size a fragmentary publication, referred to and partly described in the last annual report as nearly ready, has been completed and distributed during the present fiscal year. This fragment, as explained in the preceding report, is not included in the collected volumes of the "Contributions to Knowledge," though produced in same form and style. It forms in the Smithsonian series:

No. 800. "Plates prepared between the years 1849 and 1859, to accompany a report on the Forest Trees of North America, by Asa Gray." This is a quarto brochure, comprising all the plates (23 in number) prepared for Dr. Gray's long contemplated work on forest trees. Though nearly forty years old, these plates, carefully engraved and skillfully colored by hand, are here for the first time collected and issued, without any descriptive text, no accounts or descriptions having been found among the lamented Dr. Gray's papers.

No. 801. "Experiments in Aerodynamics." By S. P. Langley. Quarto volume of IV + 115 pages; illustrated with 11 figures in the text, and 10 plates.

SMITHSONIAN MISCELLANEOUS COLLECTIONS.

No. 787. "Lists of Institutions and Foreign and Domestic Libraries, to which it is desired to send future publications of the National Museum." (From the Report of the National Museum for 1889.) Octavo pamphlet of 78 pages.

No. 788. "Memoir of Heinrich Leberecht Fleischer." By Prof. A. Müller. (From the Smithsonian Report for 1889.) Octavo pamphlet of 20 pages.

No. 789. "On Aerial Locomotion." By F. W. Wenham. (From the Smithsonian Report for 1889.) Octavo pamphlet of 20 pages; illustrated with 6 figures.

No. 790. "Photography in the service of Astronomy." By R. Radau. Translated from the French, by A. N. Skinner. (From the Smithsonian Report for 1889.) Octavo pamphlet of 22 pages.

No. 791. "A Memoir of Gustav Robert Kirchhoff." By Robert von Helmholtz. Translated from the German, by Joseph de Perott. (From the Smithsonian Report for 1889.) Octavo pamphlet of 14 pages.

No. 792. "The Museums of the Future." By G. Brown Goode, Assistant Secretary of the Smithsonian Institution. (From the Report of the National Museum for 1889.) Octavo pamphlet of 19 pages.

No. 793. "*Te Pito te Henua*, or Easter Island." By William J. Thomson. (From the Report of the National Museum for 1889.) Octavo pamphlet of 106 pages; illustrated with 20 figures and 49 plates.

No. 794. "Aboriginal Skin Dressing. A study based on material in the U. S. National Museum." By Otis T. Mason. (From the Report of the National Museum for 1889.) Octavo pamphlet of 62 pages; illustrated with 32 plates.

No. 795. "The Puma or American Lion (*Felis concolor* of Linnaeus). By Frederick W. True. (From the Report of the National Museum for 1889.) Octavo pamphlet of 18 pages; illustrated with 1 plate.

No. 796. "Animals recently extinct, or threatened with extermination, as represented in the collections of the U. S. National Museum." By Frederick A. Lucas. (From the Report of the National Museum for 1889.) Octavo pamphlet of 11 pages; illustrated with 9 figures and 11 plates.

No. 797. "The development of the American Rail and Track, as illustrated by the collection in the U. S. National Museum." By J. Elfreth Watkins. (From the Report of the National Museum for 1889.) Octavo pamphlet of 58 pages; illustrated with 115 figures.

No. 798. "Explorations in Newfoundland and Labrador in 1887, made in connection with the cruise of the U. S. Fish Commission schooner *Grampus*." By Frederick A. Lucas. (From the Report of the National Museum for 1889.) Octavo pamphlet of 20 pages; illustrated with 1 plate or sketch map.

No. 799. "Preliminary Handbook of the Department of Geology of the U. S. National Museum." By George P. Merrill. (From the Report of the National Museum; Appendix.) Octavo pamphlet of 50 pages.

No. 803. "The Squaring of the Circle." By Herman Shubert. (From the Smithsonian Report for 1890.) Octavo pamphlet of 24 pages.

No. 804. "An Account of the Progress in Astronomy for the years 1889, 1890." By William C. Winlock. (From the Smithsonian Report for 1890.) Octavo pamphlet of 62 pages.

No. 805. "Mathematical Theories of the Earth." By Robert S. Woodward. (From the Smithsonian Report for 1890.) Octavo pamphlet of 18 pages.

No. 806. "On the Physical Structure of the Earth." By Henry Hennessy. (From the Smithsonian Report for 1890.) Octavo pamphlet of 19 pages.

No. 807. "Glacial Geology." By James Geikie. (From the Smithsonian Report for 1890.) Octavo pamphlet of 10 pages.

No. 808. "The History of the Niagara River." By G. K. Gilbert. (From the Smithsonian Report for 1890.) Octavo pamphlet of 16 pages; illustrated with 8 plates.

No. 809. "The Mediterranean Physical and Historical." By Sir R. L. Playfair. (From the Smithsonian Report for 1890.) Octavo pamphlet of 18 pages.

No. 810. "Stanley and the map of Africa." By J. Scott Keltie. (From the Smithsonian Report for 1890.) Octavo pamphlet of 15 pages; illustrated with 2 maps.

No. 811. "Antarctic Explorations." By G. S. Griffiths. (From the Smithsonian Report for 1890.) Octavo pamphlet of 12 pages.

No. 812. "The History of Geodetic Operations in Russia." By B. Witskowski and J. Howard Gore. (From the Smithsonian Report for 1890.) Octavo pamphlet of 10 pages.

No. 813. "Quartz Fibers." By C. V. Boys. (From the Smithsonian Report for 1890.) Octavo pamphlet of 20 pages; illustrated with 9 figures.

No. 814. "Dr. Kœnig's Researches on the Physical Basis of Musical Harmony and Timbre." By Sylvanus P. Thompson. (From the Smithsonian Report for 1890.) Octavo pamphlet of 25 pages; illustrated with 8 figures.

No. 815. "The Chemical Problems of To-day." By Victor Meyer. (From the Smithsonian Report for 1890.) Octavo pamphlet of 15 pages.

No. 816. "The Photographic Image." By Raphael Meldola. (From the Smithsonian Report for 1890.) Octavo pamphlet of 11 pages.

No. 817. "A Tropical Botanic Garden." By M. Treub. (From the Smithsonian Report for 1890.) Octavo pamphlet of 18 pages.

No. 818. "Temperature and Life." By Henry de Varigny. (From the Smithsonian Report for 1890.) Octavo pamphlet of 18 pages.

No. 819. "Morphology of the Blood Corpuscles." By Charles-Sedgwick Minot. (From the Smithsonian Report for 1890.) Octavo pamphlet of 3 pages; illustrated with 1 plate.

No. 820. "Weismann's Theory of Heredity." By George J. Romanes. (From the Smithsonian Report for 1890.) Octavo pamphlet of 11 pages.

No. 821. "The Ascent of Man." By Frank Baker. (From the Smithsonian Report for 1890.) Octavo pamphlet of 20 pages.

No. 822. "Antiquity of Man." By John Evans. (From the Smithsonian Report for 1890.) Octavo pamphlet of 8 pages.

No. 823. "The Primitive Home of the Aryans." By A. H. Sayce. (From the Smithsonian Report for 1890.) Octavo pamphlet of 13 pages.

No. 824. "The Prehistoric Races of Italy." By Isaac Taylor. (From the Smithsonian Report for 1890.) Octavo pamphlet of 10 pages.

No. 825. "The Age of Bronze in Egypt." By Oscar Montelius. (From the Smithsonian Report for 1890.) Octavo pamphlet of 23 pages; illustrated with 6 plates.

No. 826. "An Account of the Progress of Anthropology in the year 1890." By Otis T. Mason. (From the Smithsonian Report for 1890.) Octavo pamphlet of 82 pages; illustrated with 8 figures and 4 plates.

No. 827. "A Primitive Urn Burial." By Dr. J. F. Snyder. (From the Smithsonian Report for 1890.) Octavo pamphlet of 5 pages; illustrated with 2 plates.

No. 828. "Manners and Customs of the Mohaves." By George A. Allen. (From the Smithsonian Report for 1890.) One sheet of 2 octavo pages.

No. 829. "Criminal Anthropology." By Thomas Wilson. (From the Smithsonian Report for 1890.) Octavo pamphlet of 70 pages.

No. 830. "Color-vision and Color-blindness." By R. Brudenell Carter. (From the Smithsonian Report for 1890.) Octavo pamphlet of 18 pages.

No. 831. "Technology and Civilization." By F. Reuleaux. (From the Smithsonian Report for 1890.) Octavo pamphlet of 15 pages; illustrated with 2 figures.

No. 832. "The Ramsden Dividing Engine." By J. Elfreth Watkins. (From the Smithsonian Report for 1890.) Octavo pamphlet of 19 pages; illustrated with 1 figure and 3 plates.

No. 833. "A Memoir of Elias Loomis." By H. A. Newton. (From the Smithsonian Report for 1890.) Octavo pamphlet of 30 pages.

No. 834. "A Memoir of William Kitchen Parker." (From the Smithsonian Report for 1890.) Octavo pamphlet of 4 pages.

No. 835. "Sale List of Publications of the Smithsonian Institution, January, 1892." Octavo pamphlet of 27 pages.

No. 838. "Report on the International Congress of Orientalists." Held at Stockholm, Sweden, and Christiania, Norway, in September, 1889. By Paul Haupt. (From the Smithsonian Report for 1890.) Octavo pamphlet of 8 pages.

SMITHSONIAN ANNUAL REPORTS.

No. 770. "Report of the National Museum. Annual Report of the Board of Regents of the Smithsonian Institution, showing the operations, expenditures, and condition of the Institution for the year ending June 30, 1889." This volume comprises five sections: I. Report of the Assistant Secretary of the Smithsonian Institution, G. Brown Goode, in charge of the National Museum, upon the condition and prospects of the Museum; II. Reports of the Curators of the Museum upon the progress of work during the year; III. Papers describing and illustrating the collections in the Museum; IV. Bibliography of publications and papers relating to the Museum during the year; and V. List of accessions to the Museum during the year. The whole accompanied with an index of 39 pages, and Appendix E.—Preliminary Handbook of the Department of Geology in the U. S. National Museum, of 50 pages, by George P. Merrill, Curator. This Report forms an octavo volume of xvii+933 pages; illustrated with 144 cuts or figures in the text, and 107 plates.

No. 786. "Report upon the condition and progress of the U. S. National Museum during the year ending June 30, 1889." By G. Brown Goode, Assistant Secretary of the Smithsonian Institution, in charge of the National Museum. (From the Report of the National Museum for 1889.) Octavo pamphlet of 277 pages; illustrated with four plates.

No. 802. "Proceedings of the Regents, and Report of the Executive Committee for the year 1889-'90, together with acts of Congress for the year. (From the Smithsonian Report for 1890.) Octavo pamphlet of 32 pages.

No. 386. "Report of S. P. Langley, Secretary of the Smithsonian Institution, for the year ending June 30, 1891." Octavo pamphlet of 63 pages.

No. 837. "Annual Report of the Board of Regents of the Smithsonian Institution, showing the operations, expenditures, and condition of the Institution to July, 1890." This volume contains the Journal of Proceedings of the Board of Regents at the annual meeting held January 8, 1890; the report of the Executive Committee of the Board; acts and resolutions of Congress relative to the Institution, for the year; and the Report of the Secretary of the Institution: followed by the "General Appendix," in which are given the following papers: "The Squaring of the Circle," by Herman Schubert; "The Progress of Astronomy for the years 1889, 1890," by William C. Winlock; "Mathematical Theories of the Earth," by Robert S. Woodward; "Physical Structure of the Earth," by Henry Hennessy; "Glacial Geology," by James Geikie; "History of the Niagara River," by G. K. Gilbert; "The Mediterranean, Physical and Historical," by Sir R. L. Playfair; "Stanley and the Map of Africa," by J. Scott Keltie; "Antarctic Exploration," by G. S. Griffiths; "History of Geodetic Operations in Russia," by B. Witskowski and J. Howard Gore; "Quartz Fibers," by C. V. Boys; "Kœnigs's Researches on Musical Harmony and Timbre," by Sylvanus P. Thompson; "The Chemical Problems of To-day," by Victor Meyer; "The Photographic Image," by Raphael Meldola; "A Tropical Botanic Garden," by M. Treub; "Temperature and Life," by Henry de Varigny; "Morphology of the Blood Corpuscles," by Charles S. Minot; "Weismann's Theory of Heredity," by George J. Romanes; "The Ascent of Man," by Frank Baker; "The Antiquity of Man," by John Evans; "Primitive Home of the Aryans," by A. H. Sayce; "The Prehistoric Races of Italy," by Isaac Taylor; "The Age of Bronze, in Egypt," by Oscar Montelius; "Progress of Anthropology in 1890," by Otis T. Mason; "A Primitive Urn Burial," by J. F. Snyder; "Manners and Customs of the Mohaves," by George A. Allen; "Criminal Anthropology," by Thomas Wilson; "Color-vision and Color-blindness," by R. Brudenell Carter; "Technology and Civilization," by F. Reuleaux; "The Ramsden Dividing Engine," by J. E. Watkins; "Memoir of Elias Loomis," by H. A. Newton; and "Memoir of William Kitchen Parker;" the whole forming an octavo volume of xli+808 pages, illustrated with 29 figures and 26 plates.

Very respectfully,

WM. B. TAYLOR,
Editor.

Mr. S. P. LANGLEY,
Secretary Smithsonian Institution.

GENERAL APPENDIX

TO THE

SMITHSONIAN REPORT FOR 1892.

ADVERTISEMENT.

The object of the GENERAL APPENDIX to the Annual report of the Smithsonian Institution is to furnish brief accounts of scientific discovery in particular directions; occasional reports of the investigations made by collaborators of the Institution; memoirs of a general character or on special topics, whether original and prepared expressly for the purpose, or selected from foreign journals and proceedings; and briefly to present (as fully as space will permit) such papers not published in the Smithsonian Contributions or in the Miscellaneous Collections as may be supposed to be of interest or value to the numerous correspondents of the Institution.

It has been a prominent object of the Board of Regents of the Smithsonian Institution, from a very early date, to enrich the annual report required of them by law with memoirs illustrating the more remarkable and important developments in physical and biological discovery, as well as showing the general character of the operations of the Institution; and this purpose has, during the greater part of its history, been carried out largely by the publication of such papers as would possess an interest to all attracted by scientific progress.

In 1880, the Secretary, induced in part by the discontinuance of an annual summary of progress which for thirty years previous had been issued by well-known private publishing firms, had prepared by competent collaborators a series of abstracts, showing concisely the prominent features of recent scientific progress in astronomy, geology, meteorology, physics, chemistry, mineralogy, botany, zoölogy, and anthropology. This latter plan was continued, though not altogether satisfactorily, down to and including the year 1888.

In the report for 1889, a return was made to the earlier method of presenting a miscellaneous selection of papers (some of them original) embracing a considerable range of scientific investigation and discussion. This method has been continued in the present report, for 1892.

THE METEOROLOGICAL WORK OF THE SMITHSONIAN INSTITUTION.*

The Smithsonian Institution has always made it a rule of action to undertake such lines of work as point the way to great public utilities, and these have subsequently been made the function of useful government bureaus of applied science.

This is notably true in the case of meteorology, which was developed by the Institution in both its scientific and its popular aspects, until its importance became so well understood, and its utility so widely appreciated, that in 1870, Congress made it the duty of the Chief Signal Officer of the U. S. Army to observe and report storms for the benefit of commerce and agriculture.

The interest of the Smithsonian Institution in meteorology began with the organization of its work by its first secretary, Prof. Joseph Henry, in 1847, and from that time to the present—nearly half a century—meteorological science has been granted an important share of its labors and expenditure.

In his "programme of organization," submitted on the 8th of December, 1847, in giving examples of objects for which appropriations might properly be made, the Secretary mentioned first, and urged upon the immediate attention of the Institution, a "system of extended meteorological observations for solving the problem of American storms." This clear appreciation of the existing state of knowledge, and of the utilities to be gained, are set forth in the following words, with which he commends this undertaking:

> Of late years, in our country, more additions have been made to meteorology than to any other branch of physical science. Several important generalizations have been arrived at, and definite theories proposed, which now enable us to direct our attention, with scientific precision, to such points of observation as can not fail to reward us with new and interesting results. It is proposed to organize a system of observations which shall extend as far as possible over the North American continent. The present time appears to be peculiarly auspicious for commencing an enterprise of the proposed kind. The citizens of the United States are now scattered over every part of the southern and western portions of North America, and the extended lines of the telegraph will furnish a ready means of warning the more northern and eastern observers to be on the watch for the first appearance of an advancing storm.

In the inauguration of this system of observations, Prof. Henry solicited the suggestions of the most experienced American meteorologists—Espy, Loomis, and Guyot—who extended their cordial co-operation.

*Summary prepared for the section of history, World's Congress of Meteorology, Chicago, 1893.

Accompanying the above-quoted presentation of his programme the Secretary published a valuable, and now historic, report by Prof. Loomis upon the meteorology of the United States, in which he showed what advantage society might expect from the study of storms, what had been already done in this country toward making the necessary observations, and, finally, what encouragement there was to a further prosecution of the same researches. He then presented in detail a plan for unifying all the work done by existing observers, and for supplementing it by that of new observers at needed points, for a systematic supervision, and, finally, for a thorough discussion of the observations collected.

On the 13th of December, 1847, the Board of Regents adopted the "programme of organization," and on the 15th inaugurated the system of meteorological observations by an appropriation of $1,000 for the purchase of instruments and other related expenses.

In the following year (1848) Prof. Espy, who was then the official meteorologist of the Navy Department, was assigned to duty under the direction of the Secretary of the Smithsonian Institution. In connection with Espy, the Secretary (Henry) addressed a circular letter to all persons who would probably be disposed to take part in the contemplated systems of observations, and co-operation was solicited from the existing systems under the direction of the Surgeon-General, and of the States of New York and Pennsylvania. As a result of these efforts the Institution at the close of 1849, already had one hundred and fifty daily observers, and the number continued to increase.

In order to unify the methods adopted by observers, Prof. Guyot was requested to prepare a pamphlet of Directions for Meteorological Observations,* which was published in 1850, and to compile a collection of Meteorological Tables, which was published as a volume of the Miscellaneous Collections in 1852. In 1857, after careful revision by the author, a second and much enlarged edition of the Tables was published, and in 1859, a third, with further amendments. Although designed primarily for the meteorological observers reporting to the Smithsonian Institution, the Tables obtained a much wider circulation, and were extensively used by meteorologists and physicists in Europe and the United States. An important step taken at the inception of the Smithsonian system was the introduction of accurate instruments. Standard barometers and thermometers were imported from Paris and London, with which those made for the use of the Institution were compared, and sets of such apparatus were furnished to observers.

In 1849, Prof. Henry personally requested the telegraph companies to

*Smithsonian Institution. Directions for Meteorological Observations, intended for the first class of observers. Washington City, 1850. Reprinted with additions in Annual Report for 1855, and again as a part of the Smithsonian Miscellaneous Collections in 1870.

direct their operators to replace in their regular morning dispatches the signal, "O. K.," by which they were accustomed to announce that their lines were in order, by such words as "fair," "cloudy," etc., thus giving, without additional trouble, and as concisely as possible, a summary of the condition of the weather at the different stations, which should be communicated to him. This request was complied with, and such elementary telegraphic weather reports were thus furnished the Institution daily, without charge. This action of Prof. Henry, which has sometimes been erroneously ascribed to Prof. Espy, was the beginning of telegraphic weather service, nothing of the kind having been attempted in Europe until a later date, and by means of these reports predictions of coming storms, with all the now recognized advantage to the country at large, were made possible. With the material thus obtained the Institution was enabled in 1850, to construct the first current weather map, giving daily, from "live data," the meteorological conditions over the whole country. This map was hung where the public could have general access to it to observe the changes, and its indications were first published at large by signals displayed from the high tower of the Institution. This method was followed, and further extended, by publications in the Washington *Evening Star* in 1857, and such general interest was manifested in the subject that telegraphic weather reports were thereafter furnished to the *Star* for daily publication. The systematic notification of the general public by the press and otherwise of weather observations, appears, then, to have been undoubtedly due to Henry, and unquestionably to have preceded by a year a similar publication in 1858, of Leverrier, to whom this pioneer step has been erroneously attributed.

In 1858, the meteorological map already in use was improved by the adoption of circular disks of different colors, which were attached to it by pins at each station of observation, and indicated by their color the state of the atmosphere—white signifying clear weather; gray, cloudy; black, rain, etc. The disks had an arrow stamped upon them, and as they were so arranged that they could be attached to the map in any direction, the motion of the wind at each station was shown by them, and the "probabilities" thus more accurately forecast.

The study of the meteorological data, begun in 1849, continued under the direction of the Institution for twenty-five years, during which time numerous publications were issued relative to temperature, rainfall, hygrometry, and casual phenomena, while popular information was continuously disseminated by publishing telegraphic weather reports, maps, etc. Among the associates of the Institution in this branch of investigation may be mentioned Prof. Espy, and later, Prof. J. H. Coffin, Mr. C. A. Schott, and others. Their work may be concisely described as follows: Prof. Espy utilized the already collected data in the preparation of his Third and Fourth Meteorological Reports. After the

Smithsonian observations were practically completed, Mr. Schott* took the data and prepared elaborate tables of temperature and precipitation, which were published in the Smithsonian Contributions to Knowledge.

Prof. Coffin † compiled his great work on the laws of the winds, and contributed various lesser works to the bibliography of the Institution on meteorological subjects.

The first collection of meteorological tables compiled by Dr. Guyot, at the request of the Institution, was published in 1852, as a volume of the Smithsonian Miscellaneous Collections, and new editions were published in 1857, and 1859. Twenty-five years later the work was again revised, and a fourth edition was published (1884). The demand for these tables exhausted this edition in a few years; it was then decided to re-cast the work entirely, and publish it in three parts, one of meteorological, one of geographical, and one of physical tables, each representative of the latest knowledge in its field, and independent of the others, but the three forming a homogeneous series.

The desirability of establishing a meteorological department under one comprehensive system, with an adequate appropriation of funds, was frequently urged by the Smithsonian Institution, and in 1869 an appropriation of $25,000 was made by Congress for the adoption and maintenance of a code of weather signals on the northern lakes, under the direction of the Chief of the Signal Corps of the United States Army. The Government having thus evinced a willingness to take charge of the meteorological system of the country, and it being the policy of the Institution to do nothing which could be accomplished as well by other means, the work of the Smithsonian in this direction was freely relinquished by the Institution, although its formal transfer to the War Department did not take place until 1874.

During the period when the Smithsonian was directly in charge of meteorological researches in the United States, its expenditures in this connection, which had been voluntarily assumed, were over $60,000. In addition to this the Institution made a contribution of incalculable value in the stimulus given to investigations of this class by the active personal interest of its first Secretary, who always devoted much time

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*Schott, C. A.: Base chart of the United States. Discussion of Caswell's meteorological observations at Providence, R. I.; Cleaveland's meteorological observations at Brunswick, Me.; Hayes's physical observations in the Arctic Seas; Hildreth and Wood's meteorological observations at Marietta, Ohio; Kane's astronomical observations in the Arctic Seas; Kane's magnetic observations in the Arctic Seas; Kane's meteorological observations in the Arctic Seas; Kane's physical observations in the Arctic Seas; Kane's tidal observations in the Arctic Seas; McClintock's meteorological observations in the Arctic Seas; Smith's meteorologcal observations made near Washington, Ark.; Tables, distribution, and variation of atmospheric temperature; Tables of rain and snow in the United States.

†Coffin, J. H.: Orbit and phenomena of meteoric fire ball; Psychrometrical tables; Storms of 1859; Winds of the globe; Winds of the northern hemisphere.

and thought to this subject, while even after the transfer of the Smithsonian system to the War Department, the discussion and publication of the material already accumulated was continued by the Institution.

The Smithsonian Institution may, then, be termed the parent of the present Weather Bureau.

In 1891, the present Secretary (Mr. S. P. Langley) deposited in the United States Signal Office all the voluminous monthly records of the Institution, and all the manuscript and printed observations and contributions relating to meteorology, subject to recall, but with the understanding that the entire official record of research and progress in this connection should be preserved intact by the Bureau which now has these investigations in charge.

THE HISTORY OF THE TELESCOPE.*

By Prof. C. S. Hastings, *Yale University.*

There is no instrument which has done so much to widen the scope of human knowledge, to extend our notions of the universe, and to stimulate intellectual activity as has the telescope, unless the microscope be regarded as a successful rival. But even admitting a parity in scientific importance, the former instrument is incomparably more interesting in its history, in the same degree that its history is more simple and more comprehensible. To trace its development from a curious toy in the hands of its discoverer, for we shall see that this term is more appropriate than inventor, to the middle of this century, is to be brought into contact with most of the great philosophers, from the time of the Renaissance, who have achieved greatness in physical science, Galileo, Torricelli, Huyghens, Cassini, Newton, Halley, Kepler, Euler, Calaiault, the Herschels, father and son, Fraunhofer, Gauss—from only a portion of the list of great names. Its growth toward perfection has constantly carried with it increased precision in the applied sciences of navigation and of all branches of engineering. It would be easy to show that even pure mathematics would be in a far less forward state had there been no problems of astronomy and physics which were first suggested by the employment of the telescope. It is to this history that I venture to invite your attention this evening. I purpose to review succinctly the origin and development of this potent aid in the study of nature, to name some of the more important achievements depending upon it, and to trace its gradual improvement to the magnificent and complicated instrument which constitutes the modern equatorial. After this sketch I shall try to give an idea of the imperfections which the conscientious artisan has to contend with in attaining perfection, and to make clear the methods which have been employed in reducing these imperfections in the noble instrument now erected at this institution,† and explain why its possessors are so hopeful of gratifying success.

* Address delivered at the dedication of the Goodsell Observatory of Carleton College, Northfield, Minn., June 11, 1891. (From the *Sidereal Messenger*, August, 1891, vol. x, pp. 335–351.)

† Carleton College.

Galileo learned in 1609, while visiting Venice, that a marvellous instrument had been invented the preceding year in Holland, which would enable an observer to see a distant object with the same distinctness as if it were only at a small fraction of its real distance. It required but little time for the greatest physicist of his age to master the problem thus suggested to his mind, and after his return to Padua, where he held the position of professor of mathematics in the famous university of that city, he set himself earnestly to work making telescopes. Such was his success that in August of the same year he sent to the Venetian senate a more perfect instrument than they had been able to procure from Holland; and in January of the next year, by means of a telescope magnifying thirty times, he discovered the four satellites of Jupiter. This brilliant discovery was followed by that of the mountains in the moon; of the variable phases of Venus, which established the Copernican theory of the solar system as incontestible, and of the true nature of the Milky Way, together with many others of less philosophical importance. Though Galileo did not change the character of the telescope as it was known to its discoverer in Holland, he made it much more perfect; and above all, made the first and most fertile application of the instrument to increase the bounds of human knowledge, so that it is inevitable that his name should be indissolubly connected with the instrument. Thus the form which he used is to this day known as the Galilean telescope.

Considering the enormous interest excited throughout intellectual Europe by the invention of the telescope, it seems surprising that its early history is so confused. Less than two years after it was first heard of, a discovery, perhaps the greatest of a thousand years in the domain of natural philosophy, had been made by its means. Notwithstanding these facts, the three contemporary, or nearly contemporary, investigators assign the honor to three different persons, and if we should write out the names of all those to whom more modern writers have attributed the invention, the list would be a long one. The surprise will not be boundless, however, if we consider the task before a historian in the next century who undertakes to justly apportion the honor of the invention of the telephone among its numerous claimants. The analogy, though suggested in the obvious fact that the telephone is to hearing just what the telescope is to sight, may be made much closer if we could imagine the future historian deprived of all but verbal description, that contemporary diagrams and models were wholly wanting. Under such conditions it is difficult to believe that the historian would easily escape ante-dating the discovery of the telephone proper on account of descriptions, generally imperfect, of the acoustic telephone. But this would fairly represent the condition of the material at the command of an investigator of the present day into a question of science of the early part of the seventeenth century. No wonder, then, that the invention has been attributed to Archimedes, to Roger

Bacon, to Porta, and to many others who have written on optics; but to find the name of Satan in the list is certainly surprising. Still we read that a very learned man of the seventeenth century, named Arias Montanus, finds in the fourth chapter of Matthew, eighth verse, evidence that Satan possessed, and probably invented, a telescope; otherwise, how could he have "shown him all the kingdoms of the world and the glory of them"?* It seems to be well established now, however, that Franz Lippershey, or Lippersheim, a spectacle maker at Middleburg, was the real inventor of the telescope, and that Galileo's first telescope, avowedly suggested by news of the Hollander's achievement, was an independent invention.

That this discovery was really an accident we may be quite sure, for not only was there no developed theory of optics at that time, but even the law of refraction, which lies at the basis of such theory, was quite unknown. So, too, it seems to me quite certain that Galileo's invention must have been empirical and guided by somewhat precise information, such as that the instrument consisted essentially of two lenses, of which one was a magnifying and the other a diminishing lens. At least, that Galileo's telescope was like that of the Hollander; that, theoretically considered, it is not so simple as that made of two magnifying lenses, as is evinced by the fact that Kepler, the first philosopher to establish an approximate theory of optical instruments, only two years later invented the latter and prevailing form; and finally, that Galileo published no contributions to the theory of optics, seem quite sufficient reasons for such a belief. But, in any case, Galileo's merit is in no wise lessened by having failed to do what could not be done at that time, and the value of his discoveries in emancipating men's minds from authority in matters of pure reason is incalculable.

No other discoveries of great moment were made until over a generation after Galileo proved the existence of spots on the sun in 1611. This cessation of activity was doubtless owing to the difficulty of securing telescopes of greater efficiency than that possessed by Galileo, and which he would hardly have left until its powers of discovery had been fully exhausted in his own hands. By the middle of the seventeenth century, however, several makers of lenses had so far improved the methods of grinding and polishing, that telescopes notably superior in power to that of Galileo were procurable. Of these Torricelli, Divini, and Campani, all Italians,—Auzout, who constructed a telescope 600 feet in length, though no means was ever found for directing such an enormous instrument towards the heavens,—but above all, Huyghens, have won distinction as telescope-makers. The last named philosopher discovered, by means of a telescope of his construction, the largest satellite of Saturn in 1655, thus adding a fifth member to the list

*The history of the telescope is admirably treated in Poggendorff's *Geschichte der Physik*, from which the statements above are taken.

of planetary bodies unknown to the ancients. But his most important astronomical discovery, made also in 1655, was the nature of the rings of Saturn. This object had greatly puzzled Galileo, to whose small telescope the planet appeared to consist of a larger sphere flanked on either side by a smaller one; but when in the course of the orbital motion of Saturn the rings entirely disappeared he was wholly unable to suggest an explanation. This planet had thus presented a remarkable problem to all astronomical observers for more than forty years, and the records of the efforts to solve it during that interval afford us a most excellent means of judging the progress in practical optics. Huyghens announced these discoveries early in 1656, but that relating to the ring was given in the form of an anagram, the solution of which was first published in 1659. This discovery was contested in Italy by Divini, but was finally confirmed by members of the Florentine Academy with one of Divini's own telescopes.

A few years later the famous astronomer Cassini, having come to Paris from Italy as royal astronomer, commenced a series of brilliant discoveries with telescopes made by Campani, of Rome. With these, varying in length from 35 feet to 136 feet, he discovered four satellites to Saturn in addition to the one discovered by Huyghens. The whole number was increased by Herschel's discovery of two smaller ones in 1789, a hundred and five years after Cassini's last discovery, and again by Bond's discovery of an eighth in 1848. The Saturnian system, to which the telescope has doubtless been directed more frequently than to anything else, thus serves as a record of the successive improvements of the telescope. Highly significant is the fact that the discoveries of the eighteenth century were made with a reflecting telescope, the others all being with refracting instruments.

Cassini's discovery in 1684 of the two satellites now known as Tethys and Dione, was not accepted as conclusive until long afterwards, when Pound, in 1718, with a telescope 123 feet in length, which Huyghens had made and presented to the Royal Society, saw all five. This particular instrument is of especial interest, because it is the only one of those of the last half of the seventeenth century which has been carefully compared with modern instruments. Moreover, it is without doubt quite equal in merit to any of that period. But we find that, although it had a diameter of 6 inches, its performance was hardly better than that of a perfect modern telescope of 4 inches in diameter, and, perhaps, 4½ feet in length, while in regard to convenience in use the modern compact instrument is incomparably superior.

Another notable discovery of this period was that of the duplicity of the rings of Saturn by the Ball brothers in 1665, though its independent discovery by Cassini ten years later first attracted the attention of astronomers. The earlier discovery was made by means of a telescope 38 feet long which seems to have been of English manufacture. We must regard Cassini's discovery of the third and fourth satellites of Saturn, however, as marking the very farthest reach of the old form of telescope; a century was to elapse and an entirely new form of telescope was to be developed before another considerable addition to our knowledge of the aspect of the heavenly bodies was to be made. It is true larger telescopes were made, and Huyghens invented a means by which they could be used without tubes, but notwithstanding this improvement they proved so cumbersome as to be impracticable.

The older opticians had found that if they attempted to increase the diameter of a telescope they were obliged to increase its length in a much more rapid ratio to secure distinct vision. The reason of this was not clearly understood, but it was supposed to be owing to the fact that a wave front, changed in curvature by passing through a spherical surface, is no longer strictly spherical. This deviation in shape of the refracted wave from a true sphere is called spherical aberration. When the refracting surfaces are large and of considerable curvature this soon becomes very serious, but by using small curvature, which, in a telescope, obviously corresponds to great length, the effects of the error can be made insensible. Newton's discovery of the composite nature of light and of the phenomenon of dispersion enabled him to explain the true cause of indistinctness in short telescopes; namely, that the refraction by the objective varies for different colors; consequently, if the ocular is placed for one particular color, it will not be in the right position for any of the others, whence the image of a star or planet will seem to be surrounded by a fringe of colored light. Newton found this source of indistinctness in the image, which is now known as chromatic aberration, many hundred times as serious as the spherical aberrations. As he was persuaded by his experiments that this obstacle to further improvement in the refracting telescope was insuperable, he turned his attention to a form of telescope which had been suggested a number of years earlier in which the image was to be formed by reflection from a concave mirror, and constructed a small one with his own hands which is still in the possession of the Royal Society. This little instrument seems to have been of about the same power as Galileo's instrument with which he discovered the satellites of Jupiter, but is was hardly more than 6 inches in length.

Since that time the reflecting telescope has had a remarkable history of development in the hands of a number of most skillful mechanicians, who have also for the most part been distinguished by their discoveries in physical astronomy; we may therefore advantageously depart from the chronological treatment and follow the history of this type of instrument. This course is the more natural because we may probably regard the supremacy of the reflector (undisputed a century ago) as passed away forever.

Even after Newton's invention was made public, little was done towards the improvement of telescopes for half a century, until Hadley presented a reflector of his own construction to the Royal Society in

1723, which was found to be equal to the Huyghens refractor of 123 feet in length. From this time we may date the beginning of the superiority of reflectors. A few years later Short commenced his career as a practical optician, and for thirty years he was unapproached in the excellence of his instruments. During this time many telescopes, more powerful than the best of the previous century and infinitely more convenient in use, had been made and scattered throughout Europe, but during this period also there was a singular dearth of telescopic discovery. Perhaps men thought that the harvest had already been gathered; or, perhaps, we may find the explanation in that the great cost of telescopes so restricted their use that the impulse to discovery by their means was confined to a very small class. In view of the remarkable manner in which the standstill in this branch of science was finally followed by a brilliant period of discovery, rivalled alone by that of Galileo, we might well regard the latter cause as the chief one.

William Herschel was born in 1738 in Hanover. In 1755 he left his native country, and going to England, secured a position as organist in Octagon Chapel, Bath, where we find him in 1766. Here he became so profoundly interested in the views of the heavens which a borrowed telescope of moderate power yielded, that he tried to purchase one in London. The cost of a satisfactory instrument proving beyond his command, he determined to construct one with his own hands. Thus he entered upon a course which was to reflect honor upon himself, his country, and his age, and which was to add more to physical astronomy than any other one man has added before or since. With almost inconceivable industry and perseverance he cast, ground, and polished more than four hundred mirrors for telescopes, varying in diameter from 6 to 48 inches. This in itself would imply a busy life in any artisan, but when we remember that all this was merely subsidiary to his main work of astronomical discovery, we can not withhold our admiration.

Fortunately for science as well as for himself, he made early in his career a discovery of the very first importance which attached the attention of all Christendom. On the night of March 13, 1781, Herschel was examining small stars in the constellation of Gemini with one of his telescopes of a little more than 6 inches in diameter, when he perceived one that appeared "visibly larger than the rest." This proved to be a new world, now known as Uranus. The discovery led in the following year to his appointment as astronomer to the king, George III, with a salary sufficient to enable him to devote his whole time to astronomy.

One of the fruits of this increased leisure was the construction of a telescope far more powerful than had been dreamed of by his predecessors, namely, a telescope 4 feet in diameter and 40 feet in length. Commenced in 1785, Herschel dated its completion as August 28, 1789, when he discovered by its means a sixth satellite of Saturn and, less than a month later, a seventh, even closer to the planet and smaller than the sixth. We may regard this achievement as marking the limit of progress in the reflecting telescope, for, although at least one as large is now in use, and one even half as large again has been constructed, it is more than doubtful whether they were ever as perfect as Herschel's at its best.

There has been one improvement however in the reflecting telescope since the time of Herschel which ought not to be left unnoticed here, namely, that of replacing the heavy metal mirror by one of glass, made even more highly reflective than the old mirrors by a thin coating of silver deposited by chemical methods upon the polished glass. The great advantage of this modern form of reflector lies, not so much in the greater lightness and rigidity of the material as in that the surface when tarnished can be renewed by the simple process of replacing the old silver film by a new one; whereas in the metal reflectors a tarnished surface required a repetition of the most difficult and critical portion of the whole process of construction. The construction is also so comparatively simple that an efficient reflector is far less expensive than are refracting telescopes of like power, so that this may be regarded as particularly the amateur's telescope. On the other hand, such telescopes are, like their predecessors, extremely inconstant, and they require much more careful attention to keep them in working order. It is for these reasons, doubtless, that silver-on-glass reflectors have done so little for the advancement of astronomical discovery. In astronomical photography, however, they promise to do much; and indeed, at the present date by far the best photographs we have of any nebulæ have been made by Mr. Common's magnificent reflector of 3 feet in diameter, and by the 20-inch reflector of Mr. Roberts.

We must go back now to a quarter of a century before Herschel discovered the new planet,—to the very year indeed when that great astronomer first set foot on English soil,—in order to trace the history of another form of telescope which has remained unrivalled for the last half century in the more difficult fields of astronomical research, and which to-day finds its most perfect development in the instruments at Mount Hamilton, at Pulkowa, at Vienna, and at Washington.

Newton had declared that, as a result from his experiments, separation of white light into its constituent colors was an inevitable accompaniment of deviation by refraction, and consequently the shortening of the unwieldy refractors was impracticable. The correctness of the experiments remained unquestioned for nearly a century; but a famous German mathematician, Euler, did question his conclusion. His argument was that since the eye does produce colorless images of white objects it might be possible by the proper selection of curves to so combine lenses of glass and of water as to produce a telescope free from the color defect. Although Euler's premise was an error, since the eye is not free from dispersion, his efforts had the effect of leading to much more critical study of the phenomena involved. In this John Dolland, an English optician, met with brilliant success. Repeating an experiment of Newton's with a prism of water opposed by a prism of glass he found that deviation of light could be produced without accompanying dispersion into prismatic colors. More than this, he found that the two varieties of glass, then as now common in England—crown or common window glass, and flint glass, which is characterized by the presence of a greater or less quantity of lead oxide—possessed very different powers in respect to dispersion; thus, of two prisms of these two varieties of glass which would deflect the light by the same angle, that made of flint glass would form a spectrum nearly twice as long as the other; hence, if a prism of crown glass deflecting a transmitted beam of light, say 10 degrees, were combined with one of flint glass which would deflect the beam of light 5 degrees in the opposite direction there would remain a deflection of 5 degrees without division into color. It also follows that a positive lens of crown combined with a negative lens of flint of half the power would yield a colorless image. Such combinations of two different substances are called achromatic systems.

It is a singular fact, worth noting in passing, that more than twenty years before Dolland's success, Mr. Chester More Hall had invented and made achromatic telescopes, but this remained unknown to the world of science until after Dolland's telescopes became famous.

For a long time this ingenious invention remained fruitless for astronomical discovery, (though they were early applied to meridian instruments,) on account of the impossibility of securing sufficiently large and perfect pieces of glass, more particularly of flint glass. Not until after the beginning of this century was any real advance in this branch of the arts exhibited. Even then success appeared, not in England or France, where most strenuous efforts had been made to improve the quality of optical glass, but in Switzerland. There a humble mechanic, a watchmaker named Guinaud, spent many years in efforts, long unfruitful, to make large pieces of optical glass. What degree of success he attained there during twenty years of experiment we do not know, though from the fact that during that period good achromatic telescopes of more than 5 inches in diameter were unknown we must conclude that his success was limited. In 1805 he joined the optical establishment of Fraunhofer and Utzschneiden in Munich. Here he remained nine years, and with the increased means at his disposal, and the aid of Fraunhofer, he perfected his methods so far that the production of large disks of homogeneous glass became only a matter of time and cost; that is to say, all of the large pieces of optical glass which have since been produced, whether in Germany, France, or England, have been made by direct heirs of the practical secrets of this Swiss watchmaker.

Fraunhofer was a genius of a high order. Although he died at the early age of 39, he had not only brought the achromatic telescope to a degree of optical perfection which made it a rival of the most powerful of the reflector type, and so far improved its method of mounting, that his system has replaced all others; but he also made some capital discoveries in the domain of physical optics. His great achievement was the construction of an achromatic telescope 9.6 inches in diameter, with which the elder Struve made at Dorpat his remarkable series of discoveries and measurement of double stars. The character of Struve's work demonstrates the excellence of the telescope, and shows us that it is to be ranked as the equal of all but the very best of its predecessors. Indeed, it may fairly be concluded that not more than one or two telescopes, and those made and used by Herschel, had ever been of greater power, while in convenience for use the new refractor was vastly superior.

For a long time Fraunhofer and his successors, Merz and Mahler, from whom the great telescopes of Pulkowa and of the Harvard Observatory were procured, remained unrivalled in this field of optics. But they have been followed by a number of skillful constructors whose products have, since the middle of the century, been scattered all over the world. In Germany, Steinheil and Schröder; in France, Canchois, Martin, and the Henry brothers; in England, Cook and Grubb; and in this country the Clarks and Brashear, have each produced one or more great telescopes which has rendered his name familiar to all readers of astronomical history. Of these the Clarks, father and son, have beyond a doubt won the first place, whether determined by the character of the discoveries made by means of their instruments or by the fact that the two most powerful telescopes in existence were made by them, namely, the new refractor of 30 inches in diameter, at Pulkowa, and the great refractor of 3 feet diameter, of the Lick Observatory in California. The most notable discoveries made with their telescopes are the satellites of Mars and the companion to Sirius; but besides these there is a long list of double stars of the most difficult character discovered by the makers themselves, by Dawes, in England, by Burnham, in our own country, and by a number of other observers.

We ought not to terminate our review of the development of the telescope without a reference to the parallel development of the mounting of great telescopes. Indeed, did this not lead us too far from the immediate aim in view, we might find a great deal of interest and be brought into agreeable contact with some of the cleverest mechanicians and engineers of two centuries by tracing its course. We should meet with Huyghens, as the inventor of the aerial telescope, and perhaps consider the claims of his contemporary, Robert Hook, as a rival inventor, for we may be sure that nothing which brings us to a study of that curious and able philosopher would fail to possess interest. We should find Herschel confronted with the problem as to how he should use his great 40-foot telescope, and the study of his solution would guide us in valuing the results of the subsequent efforts of Lassel and Rosse. The same line of study would bring us to Grubb's clever and interesting equatorial mounting of that anachronism, the 4-foot Melbourne reflector. But we should find nothing of very notable interest in the mounting of refractors, after the time of Huyghens and Hook, until Fraunhofer invented a type of mounting for the famous Dorpat equatorial, which still remains in its essential features as the type in universal use. With the increase in size of the telescopes to be directed towards the heavens, however, the number and complexity of the mechanical problems to be solved has been vastly increased, so that they have taxed the best powers of some of the ablest mechanicians. The Repsolds, of Germany, and Sir Howard Grubb, of Dublin, have specially distinguished themselves in this field of activity. But it seems to me that none have shown greater fertility of resources, greater skill in the solution of every problem affecting the comfort and efficacy of the observer, and greater taste, combined with accurate workmanship, than have the celebrated firm which has mounted the telescope at Mount Hamilton and that at Carleton College.

We come now to a consideration of the present state of the art of lens-making. We ask why such a very large proportion of the telescopes in existence are bad; why there was a time, brief it is true, during which the glass-maker was certainly in advance of the demands of telescope-makers; and why, finally, the first of the great modern objectives was in the hands of the most skillful optician in Great Britain for seven years, and even then this maker asserted that it was incomplete.

These questions can not be answered in a word, but we can, at least, gain much in perspicuity by recognizing that the reasons are of two distinct kinds, namely, purely technical, and theoretical; and by regarding them briefly in succession.

The art of lens-making can be certainly traced back to the 13th century, though the methods at a much later day than that were so rude that, as we have seen, Galileo had the utmost difficulty in making a lens good enough to bear a magnifying power of 30 times. At the present day there is little difficulty in selecting a spectacle glass which would rival that most famous of all telescopes. Not until after another generation of effort was there such notable improvement in the technique of lens-making that further astronomical discovery was possible. The reasons for this slow progress are to be found in the extremely critical requirements for a good lens. A departure by a fraction of a hundred-thousandth part of an inch from a correct geometrical surface will greatly impair the performance of an objective. But even at this day the limit of accurate measurement may be set at about a one-hundred-thousandth of an inch, while it is quite probable that ten times that value was vanishingly small to the artisans of a century or more ago. It was necessary therefore to devise a method of polishing—for it is a comparatively simple matter to grind a surface accurately—

which should keep the surface true within a limit far transcending the range of measurements. Huyghens is the first who seems to have done this, by polishing upon a paste which was formed to the glass and then dried, and by using only the central portion of a large lens. In Italy Campani developed a system which he most jealously guarded as a secret until his death, consisting of polishing with a dry powder on paper cemented to the grinding tools. This method still survives in Paris to the exclusion of almost all others, and it is probably the best for work which does not demand the highest scientific precision.

Newton however was the first to introduce a method which has since been developed to a state of surprising delicacy. Casting about for a means which should be sufficiently "tender," to use his own expression, for polishing the soft speculum metal, he fixed upon pitch, shaped to the mirror while warm, as a bed to hold the polishing powder. But the enormous value of this substance lies not so much in the comparative immunity which it gives from scratching, but in the fact that under slowly changing forces it is a liquid, but under those of short duration it behaves like a hard and brittle solid. Thus it is possible to slowly alter the shape of a lens while polishing, in any desired direction. It was only after the practical recognition of this fact that really excellent lenses were much more than a question of good fortune. The perfecting of this method belongs without doubt to the English of the last century and the early part of this. In the Philosophical Transactions, we find many long papers relating to this art, contributed by skillful and successful amateurs. We may therefore regard the technique of the art of lens-making as practically complete at the middle of this century and as common property, so that success no longer depends upon the holding of some special or secret method.

We are now (after this, I fear, somewhat dry discussion of a necessary point) in a condition to explain the differences between the processes pursued by most telescope-makers and that of the maker of the Carleton College telescope.

This is the ordinary method: After securing perfect pieces of glass, crown and flint, as like as possible to those generally used, and having fixed upon the general shape of the lenses, a guess is made as to the proper radii of the four surfaces to determine the desired focal length and corrections both for color and spherical aberration. The success of this guess has much to do with the necessary outlay of labor, and therefore past experience is of great value as a guide. After working the four surfaces to the dimensions provisionally adopted so far as to admit of fairly good seeing through the objective, an examination of the errors is made. Should the errors of color be so small that their final correction will not make the telescope more than from 3 to 10 per cent greater or less than the desired focal length, the crown lens will probably be completed in accordance with the provisional figures. Then the flint lens will be modified in such a direction as will tend to correct the observed errors of color and figure, until, by a purely tentative process, the color error is practically negligible and the error of figure is small. Then follows a process when the qualities of skill, conscientiousness, and perseverance have full scope. This process first introduced, or at least made public by Foucault, is known as local correcting. It consists in slowly polishing away portions of the lens surfaces so that errors in the focal image become so small, not that they can not be detected, but that one can not determine whether they are on the one side of truth or the other. Local correcting has always seemed to me to be eminently unscientific and unnecessary. It is a process of making errors small which ought not to exist.

Mr. Brashear's method is essentially different from this. Before the glasses are touched every dimension and constant of the finished objective is known with great accuracy. His whole aim is to make the surfaces geometrically perfect; and by ingenious polishing machinery, which embodies twelve years of his thought and experience, he is enabled to do this with truly astonishing exactness. All the surfaces which admit of investigation—usually three in his ordinary construction—are made rigidly true without regard to the character of the focal image. This leaves only one surface which is known to be very nearly a sphere, but probably deviating slightly within in the direction of a prolate or oblate spheroid. A glance at the character of the focal image will determine this point. Then the polishing machine is adapted to bring about a change in the proper direction, and after action during a measured interval of time, the image is again examined, and from the observed change in character the necessary time for complete correction by the same or contrary action may be deduced. It will be observed that by this means it is quite possible to correct errors which are much too small to betray their nature, since a step in the wrong direction carries with it no consequences of the slightest moment, since any step may be retraced.

When we learn that Mr. Brashear's telescope objectives have always had a focal length differing only from one-tenth to one one-hundred-and-eightieth of 1 per cent of the value prescribed, we have a suggestion of the success of his efforts. But adding to that the fact that he is absolutely untrammelled by purely mechanical considerations. either as to the shape of his lenses or the character of his materials, leaving these questions to be decided alone by the requirements of the astronomer, it seems to me that we may fairly accord to him the merit of the most important improvements introduced into his art for a very long period.

I shall not venture to demand much of your time in considering the purely theoretical difficulties in telescope construction, not merely because the subject has already taxed our patience, but because it would be of almost too technical a character did we allow ourselves to regard anything but the most general features.

The obvious requirements are that in a good objective the light coming from a point in the object should be concentrated at a point in the image; but this, combined with a prescribed focal length, may be reduced to three conditions: First, a fixed focal length; second, freedom from color error; third, freedom from spherical aberration for a particular color or wave length of light. Now let us catalogue what provisions we have for satisfying these conditions. They are, four surfaces, which must be spherical but may have any radii we please, the two thicknesses of the two lenses, and the distance which separates the lenses—that is, seven elements which may be varied to suit our requirements. As a matter of fact, however, on account of the cost of the material and the fact that glass is perfectly transparent, for powerful telescopes we must make the lenses as thin as possible; and we shall find also that separating the lenses introduces errors away from the axis which are, to say the least, undesirable. We have left, therefore, only the four radii as arbitrary constants. These, however, are more than enough to meet the three requirements. To make the problem determinate we must add another condition. The suggestion of this fourth condition and carrying the problem to its solution is the work of the great mathematicians who have directed their thought to it. Clairault proposed to make the fourth condition that the two adjacent surfaces should fit together and the lenses be cemented. This condition would be, doubtless, of great value were it possible to cement large lenses without changing their shapes to a degree which would quite spoil their performance. Sir John Herschel published a very important paper in 1821, in which he made the fourth condition that the spherical aberration should vanish, not only for objects at a very great distance, but also for those at a moderate distance. In this paper he computed a table, afterwards greatly extended by Prof. Baden Powell, for the avowed purpose of aiding the practical optician. It was this feature undoubtedly which brought his construction, not at all a good one as we shall see, into more general use than any other for some time. But, as all Herschel's tables were derived from calculations which wholly disregarded the thickness of the lenses, I am quite unable to see how they could have been of any material aid, and am inclined to suspect that the discredit with which opticians have received the dicta of mathematicians concerning their instruments may have been due in part to this very fact. It is a singular fact, for which I have in vain sought the explanation, that Fraunhofer's objectives are of just such a form as to comply with the Herschelian solution, although they must have been made quite independently.

Gauss made the fourth condition that another color or wave-length of light should be also free from spherical aberration. This seems to have been a *tour de force* as a mathematician, not as a sober suggestion of an improvement in construction, for in a point of fact the construction is very bad. It was generally believed that this condition could not be fulfilled; therefore Gauss, who was particularly fond of doing what all the rest of the world believed impossible, straightway did it. There has been only one effort to carry out this suggestion of Gauss, and that forty years later by Steinheil, but it proved a disappointment. A much larger objective made by Clark a few years ago, of the general form of Gauss's objective, probably does not meet the Gaussian condition,—at least this condition is extremely critical, and I believe it is not asserted that the objective was ever thoroughly investigated. It has been the father of no others.

It is hardly surprising, since none of these forms have any real merit, that the practical optician has, following the line of least resistance, adopted a form which costs him less labor than those heretofore mentioned and is quite as good. By making the curve equi-convex the trouble and expense of making one pair of tools is saved, although this would hardly appear a satisfactory reason for choice of a particular form to the astronomer, who simply demands the best possible instrument of research.

The reason for so much futile work on the theory of the telescope objective is not far to seek. It had always been tacitly assumed that the condition of color correction, one of those which serves to determine the values of the arbitrary constants, was readily determinable—in fact, one of the *donne* of the problem, whereas it is just this datum which has offered peculiar difficulties. Fraunhofer brought all the resources at the command of his genius to bear upon this point, and frankly failed, although in the effort he made a splendid discovery, which has assured a permanence to his fame no less than that of the history of science itself—the discovery of the dark, or Fraunhofer, lines in solar and stellar spectra. Gauss proposed the condition that the best objective is that which produces the most perfect concentration of light about the place of the geometrical image of a point, just as the best rifle practice is that which produces the maximum concentration of hits about the center of the target. That this is a false guide appears at once from the consideration that if we take even as much as 10 per cent of the light from an object and diverted from the image so far that it can not be found, the telescope may still be practically perfect; all of Herschel's did much worse than this. But if you take that same 10 per cent and concentrate it very close about the image, the telescope will be absolutely worthless.

The true difficulty with most of the theorists is this: There is no recognition of the relative weight or importance of unavoidable errors. The optician is confronted at the very outset by the fact that absolute elimination of color error is impossible, for certain physical reasons which we have not time for considering farther. He can reduce the color error of the old single-lens type of telescopes hundreds of times, and hence the length of the telescope tens of times. It is this fact which prevents the still farther shortening of telescopes, which keeps the ratio of length to diameter not less than fifteen to one in large telescopes. This restriction being recognized, let us revise our limiting conditions. They now become, first, fixed focal length; second, best color correction; third, freedom from spherical aberration for a particular wave-length of light. We therefore have still one arbitrary constant undetermined. How shall we fix its value, and thus solve the problem completely? Surely there is only one rational guide. Consider the residual errors and make the fourth condition such as to reduce these errors as far as possible. Now the only remaining errors are secondary color error and spherical aberration for colors other than that for which it is eliminated, or more scientifically, chromatic difference of spherical aberration. Which of these is the gravest defect? Our answer must depend upon the use to which the objective is to be put. If it is a high-power microscope objective, it is certainly the second. If it is an objective to be used for photographing at considerable angular distances from the axis, our question loses its physical significance, since we have excluded the consideration of eccentric refraction. But if the objective is to be for a visual telescope, there is no question that the defect of secondary color error is incomparably the most serious. Our fourth and determining condition must, therefore, be better color correction.

These are therefore the considerations which have served as guides in the construction of the Carleton College objective. First, the selection of the materials which, in the present condition of the art of optical glass making, possess in the highest degree the desired physical properties; second, a general discussion of every possible combination of these two pieces of glass and a selection of the forms which yield the best attainable results. This conscientious strife after scientific perfection, the unexcelled skill with which the results of analysis have been interpreted into the reality of substance, the gratifying identity of predicted and realized values of physical characteristics—all of these have led some of those who have watched the growth of this new instrument of research with the most solicitous attention to the belief that although not the most powerful in existence it may well be the most perfect great telescope yet made. Let us therefore congratulate the possessors of this noble instrument, wish them God speed in their search after knowledge, while we remind them that although no astronomer can ever make another discovery which will rival that made by the insignificant tube first directed toward the heavens by the Paduan philosopher, yet no mind can weigh the importance of any truth, however trivial in appearance, which may be added to that store which we call "science."

GEOLOGICAL CHANGE, AND TIME.*

By Sir ARCHIBALD GEIKIE,

Director-General of the Geological Survey of Great Britain.

In its beneficent progress through these islands the British Association for the Advancement of Science now for the fourth time receives a welcome in this ancient capital. Once again, under the shadow of these antique towers, crowded memories of a romantic past fill our thoughts. The stormy annals of Scotland seem to move in possession before our eyes as we walk these streets, whose names and traditions have been made familiar to the civilized world by the genius of literature. At every turn, too, we are reminded, by the monuments which a grateful city has erected, that for many generations the pursuits which we are now assembled to foster have had here their congenial home. Literature, philosophy, science, have each in turn been guided by the influence of the great masters who have lived here, and whose renown is the brightest gem in the chaplet around the brow of this Queen of the North.

Lingering for a moment over these local associations, we shall find a peculiar appropriateness in the time of this renewed visit of the Association to Edinburgh. A hundred years ago a remarkable group of men was discussing here the great problem of the history of the earth. James Hutton, after many years of travel and reflection, had communicated to the Royal Society of this city, in the year 1785, the first outlines of his famous Theory of the Earth. Among those with whom he took counsel in the elaboration of his doctrines were Black, the illustrious discoverer of fixed air and latent heat; Clerk, the sagacious inventor of the system of breaking the enemy's line in naval tactics; Hall, whose fertile ingenuity devised the first system of experiments in illustration of the structure and origin of rocks; and Playfair, through whose sympathetic enthusiasm and literary skill Hutton's views came ultimately to be understood and appreciated by the world at large. With these friends, so well able to comprehend and criticise his efforts to pierce the veil that shrouded the history of this globe, he

* Presidential Address before the British Association for the Advancement of Science; at Edinburgh, August 3, 1892. (*Report Brit. Assoc. A. S.* 1892, vol. LXII, pp. 3–26.)

paced the streets amid which we are now gathered together; with them he sought the crags and ravines around us, wherein Nature has laid open so many impressive records of her past; with them he sallied forth on those memorable expeditions to distant parts of Scotland, whence he returned laden with treasures from a field of observation which, though now so familiar, was then almost untrodden. The centenary of Hutton's Theory of the Earth is an event in the annals of science which seems most fittingly celebrated by a meeting of the British Association in Edinburgh.

In choosing from among the many subjects which might properly engage your attention on the present occasion, I have thought that it would not be inappropriate nor uninteresting to consider the more salient features of that Theory, and to mark how much in certain departments of inquiry has sprung from the fruitful teaching of its author and his associates.

It was a fundamental doctrine of Hutton and his school that this globe has not always worn the aspect which it bears at present; that on the contrary, proofs may everywhere be culled that the land which we now see has been formed out of the wreck of an older land. Among these proofs, the most obvious are supplied by some of the more familiar kinds of rocks, which teach us that, though they are now portions of the dry land, they were originally sheets of gravel, sand, and mud, which had been worn from the face of long-vanished continents, and after being spread out over the floor of the sea were consolidated into compact stone, and were finally broken up and raised once more to form part of the dry land. This cycle of change involved two great systems of natural processes. On the one hand, men were taught that by the action of running water the materials of the solid land are in a state of continual decay and transport to the ocean. On the other hand, the ocean floor is liable from time to time to be upheaved by some stupendous internal force akin to that which gives rise to the volcano and the earthquake. Hutton further perceived that not only had the consolidated materials been disrupted and elevated, but that masses of molten rock had been thrust upward among them, and had cooled and crystallized in large bodies of granite and other eruptive rocks which form so prominent a feature on the earth's surface.

It was a special characteristic of this philosophical system that it sought in the changes now in progress on the earth's surface an explanation of those which occurred in older times. Its founder refused to invent causes or modes of operation, for those with which he was familiar seemed to him adequate to solve the problems with which he attempted to deal. Nowhere was the profoundness of his insight more astonishing than in the clear, definite way in which he proclaimed and reiterated his doctrine, that every part of the surface of the continents, from mountain top to seashore, is continually undergoing decay, and is thus slowly travelling to the sea. He saw that no sooner will the sea floor be elevated into new land than it must necessarily become a prey to this universal and unceasing degradation. He perceived that as the transport of disintegrated material is carried on chiefly by running water, rivers must slowly dig out for themselves the channels in which they flow, and thus that a system of valleys, radiating from the water parting of a country, must necessarily result from the descent of the streams from the mountain crests to the sea. He discerned that this ceaseless and wide-spread decay would eventually lead to the entire demolition of the dry land, but he contended that from time to time this catastrophe is prevented by the operation of the under-ground forces, whereby new continents are upheaved from the bed of the ocean. And thus in his system a due proportion is maintained between land and water, and the condition of the earth as a habitable globe is preserved.

A theory of the earth so simple in outline, so bold in conception, so full of suggestion, and resting on so broad a base of observation and reflection, ought (we might think) to have commanded at once the attention of men of science, even if it did not immediately awaken the interest of the outside world; but, as Playfair sorrowfully admitted, it attracted notice only very slowly, and several years elapsed before any one showed himself publicly concerned about it, either as an enemy or a friend. Some of its earliest critics assailed it for what they asserted to be its irreligious tendency,—an accusation which Hutton repudiated with much warmth. The sneer levelled by Cowper a few years earlier at all inquiries into the history of the universe was perfectly natural and intelligible from that poet's point of view. There was then a wide-spread belief that this world came into existence some six thousand years ago, and that any attempt greatly to increase that antiquity was meant as a blow to the authority of Holy Writ. So far however from aiming at the overthrow of orthodox beliefs, Hutton evidently regarded his "Theory" as an important contribution in aid of natural religion. He dwelt with unfeigned pleasure on the multitude of proofs which he was able to accumulate of an orderly design in the operations of nature, decay and renovation being so nicely balanced as to maintain the habitable condition of the planet. But as he refused to admit the predominance of violent action in terrestrial changes, and on the contrary contended for the efficacy of the quiet, continuous processes which we can even now see at work around us, he was constrained to require an unlimited duration of past time for the production of those revolutions of which he perceived such clear and abundant proofs in the crust of the earth. The general public, however, failed to comprehend that the doctrine of the high antiquity of the globe was not inconsistent with the comparatively recent appearance of man,—a distinction which seems so obvious now.

Hutton died in 1797, beloved and regretted by the circle of friends who had learned to appreciate his estimable character and to admire his genius, but with little recognition from the world at large. Men knew not then that a great master had passed away from their midst, who had laid broad and deep the foundations of a new science; that his name would become a household word in after generations, and that pilgrims would come from distant lands to visit the scenes from which he drew his inspiration.

Many years might have elapsed before Hutton's teaching met with wide acceptance, had its recognition depended solely on the writings of the philosopher himself. For, despite his firm grasp of general principles and his mastery of the minutest details, he had acquired a literary style which, it must be admitted, was singularly unattractive. Fortunately for his fame, as well as for the cause of science, his devoted friend and disciple, Playfair, at once set himself to draw up an exposition of Hutton's views. After five years of labor on this task there appeared the classic "Illustrations of the Huttonian Theory," a work which for luminous treatment and graceful diction stands still without a rival in English geological literature. Though professing merely to set forth his friend's doctrines, Playfair's treatise was in many respects an original contribution to science of the highest value. It placed for the first time in the clearest light the whole philosophy of Hutton regarding the history of the earth, and enforced it with a wealth of reasoning and copiousness of illustration which obtained for it a wide appreciation. From long converse with Hutton, and from profound reflection himself, Playfair gained such a comprehension of the whole subject, that discarding the non-essential parts of his master's teaching, he was able to give so lucid and accurate an exposition of the general scheme of Nature's operations on the surface of the globe, that with only slight corrections and expansions his treatise may serve as a text-book to-day. In some respects, indeed, his volume was long in advance of its time. Only, for example, within the present generation has the truth of his teaching in regard to the origin of valleys been generally admitted.

Various causes contributed to retard the progress of the Huttonian doctrines. Especially potent was the influence of the teaching of Werner, who, though he perceived that a definite order of sequence could be recognized among the materials of the earth's crust, had formed singularly narrow conceptions of the great processes whereby that crust has been built up. His enthusiam, however, fired his disciples with the zeal of proselytes, and they spread themselves over Europe to preach everywhere the artificial system which they had learned in Saxony. By a curious fate Edinburgh became one of the great headquarters of Wernerism. The friends and followers of Hutton found themselves attacked in their own city by zealots, who proud of superior mineralogical acquirements, turned their most cherished ideas upside down and assailed them in the uncouth jargon of Freiberg. Inasmuch as subterranean heat had been invoked by Hutton as a force largely instrumental in consolidating and upheaving the ancient sediments that now form so great a part of the dry land, his followers were nicknamed Plutonists. On the other hand, as the agency of water was almost alone admitted by Werner, who believed the rocks of the earth's crust to have been chiefly chemical precipitates from a primeval universal ocean, those who adopted his views received the equally descriptive name of Neptunists. The battle of these two contending schools raged fiercely here for some years, and though mainly from the youth, zeal, and energy of Jameson, and the influence which his position as professor in the university gave him, the Wernerian doctrines continued to hold their place they were eventually abandoned even by Jameson himself, and the debt due to the memory of Hutton and Playfair was tardily acknowledged.

The pursuits and the quarrels of philosphers have from early times been a favorite subject of merriment to the outside world. Such a feud as that between the Plutonists and Neptunists would be sure to furnish abundant matter for the gratification of this propensity. Turning over the pages of Kay's "Portraits," where so much that was distinctive of Edinburgh society a hundred years ago is embalmed, we find Hutton's personal peculiarities and pursuits touched off in good-humored caricature. In one plate he stands with arms folded and hammer in hand, meditating on the face of a cliff, from which rocky prominences in shape of human faces, perhaps grotesque likenesses of his scientific opponents, grin at him. In another engraving he sits in conclave with his friend Black, possibly arranging for that famous banquet of garden snails which the two worthies had persuaded themselves to look upon as a strangely neglected form of human food. More than a generation later, when the Huttonists and Wernerists were at the height of their antagonism, the humorous side of the controversy did not escape the notice of the author of "Waverley," who, you will remember, when he makes Meg Dods recount the various kinds of wise folk brought by Lady Penelope Pennfeather from Edinburgh to St. Ronan's Well, does not forget to include those who "rin uphill and down dale, knapping the chucky-stanes to pieces wi' hammers (like sae mony road-makers run daft), to see how the warld was made."

Among the names of the friends and followers of Hutton there is one which on this occasion deserves to be held in especial honor, that of Sir James Hall, of Dunglass. Having accompanied Hutton in some of his excursions, and having discussed with him the problems presented by the rocks of Scotland, Hall was familiar with the views of his master, and was able to supply him with fresh illustrations of them from different parts of the country. Gifted with remarkable originality and ingenuity, he soon perceived that some of the questions involved in the theory of the earth could probably be solved by direct physical experiment. Hutton however mistrusted any attempt "to judge of the great operations of nature by merely kindling a fire and looking into the bottom of a little crucible." Out of deference to this prejudice Hall delayed to carry out his intention during Hutton's lifetime. But afterwards he instituted a remarkable series of researches which are memorable in the history of science as the first methodical endeavor to test the value of geological speculation by an appeal to actual experiment. The Neptunists, in ridiculing the Huttonian doctrine that basalt and similar rocks had once been molten, asserted that, had such been their origin, these masses would now be found in the condition of glass or slag. Hall however triumphantly vindicated his friend's view by proving that basalt could be fused, and thereafter by slow cooling could be made to resume a stony texture. Again, Hutton had asserted that under the vast pressures which must be effective deep within the earth's crust, chemical reactions must be powerfully influenced, and that under such conditions even limestone may conceivably be melted without losing its carbonic acid. Various specious arguments had been adduced against this proposition, but by an ingeniously devised series of experiments Hall succeeded in converting limestone under great pressure into a kind of marble, and even fused it, and found that it then acted vigorously on other rocks. These admirable researches, which laid the foundations of experimental geology, constitute not the least memorable of the services rendered by the Huttonian school to the progress of science.

Clear as was the insight and sagacious the inferences of these great masters in regard to the history of the globe, their vision was necessarily limited by the comparatively narrow range of ascertained fact which up to their time had been established. They taught men to recognize that the present world is built of the ruins of an earlier one, and they explained with admirable perspicacity the operation of the processes whereby the degradation and renovation of land are brought about. But they never dreamed that a long and orderly series of such successive destructions and renewals had taken place and had left their records in the crust of the earth. They never imagined that from these records it would be possible to establish a determinate chronology that could be read everywhere and applied to the elucidation of the remotest quarter of the globe. It was by the memorable observations and generalizations of William Smith that this vast extension of our knowledge of the past history of the earth became possible. While the Scottish philosophers were building up their theory here, Smith was quietly ascertaining by extended journeys that the stratified rocks of the west of England occur in a definite sequence, and that each well-marked group of them can be discriminated from the others and identified across the country by means of its inclosed organic remains. It is nearly a hundred years since he made known his views, so that by a curious coincidence we may fitly celebrate on this occasion the centenary of William Smith as well as that of James Hutton. No single discovery has ever had a more momentous and far-reaching influence on the progress of a science than that law of organic succession which Smith established. At first it served merely to determine the order of the stratified rocks of England. But it soon proved to possess a world-wide value, for it was found to furnish the key to the structure of the whole stratified crust of the earth. It showed that within that crust lie the chronicles of a long history of plant and animal life upon this planet, it supplied the means of arranging the materials for this history in true chronological sequence, and it thus opened out a magnificent vista through a vast series of ages, each marked by its own distinctive types of organic life, which, in proportion to their antiquity, departed more and more from the aspect of the living world.

Thus a hundred years ago, by the brilliant theory of Hutton and the fruitful generalization of Smith, the study of the earth received in our country the impetus which has given birth to the modern science of geology.

To review the marvellous progress which this science has made during the first century of its existence would require not one, but many, hours for adequate treatment. The march of discovery has advanced along a multitude of different paths, and the domains of nature which have been included within the growing territories of human knowledge have been many and ample. Nevertheless, there are certain departments of investigation to which we may profitably restrict our attention on the present occasion, and wherein we may see how the leading principles that were proclaimed in this city a hundred years ago have germinated and borne fruit all over the world.

From the earliest times the natural features of the earth's surface have arrested the attention of mankind. The rugged mountain, the cleft ravine, the scarped cliff, the solitary bowlder, have stimulated curiosity and prompted many a speculation as to their origin. The shells embedded by millions in the solid rocks of hills far removed from the seas have still further pressed home these "obstinate questionings." But for many long centuries the advance of inquiry into such matters was arrested by the paramount influence of orthodox theology. It was not merely that the church opposed itself to the simple and obvious interpretation of these natural phenomena. So implicit had faith become in the accepted views of the earth's age and of the history of creation, that even laymen of intelligence and learning set themselves unbidden and in perfect good faith to explain away the difficulties which nature so persistently raised up, and to reconcile her teachings with those of the theologians. In the various theories thus originating, the amount of knowledge of natural law usually stood in inverse ratio to the share played in them by an uncontrolled imagination. The speculations, for example, of Burnet, Whiston, Whitehurst, and others in this country, can not be read now without a smile. In no sense were they scientific researches; they can only be looked upon as exercitations of learned ignorance. Springing mainly out of a laudable desire to promote what was believed to be the cause of true religion, they helped to retard inquiry, and exercised in that respect a baneful influence on intellectual progress.

It is the special glory of the Edinburgh school of geology to have cast aside all this fanciful trifling. Hutton boldly proclaimed that it was no part of his philosophy to account for the beginning of things. His concern lay only with the evidence furnished by the earth itself as to its origin. With the intuition of true genius he early perceived that the only solid basis from which to explore what has taken place in bygone time is a knowledge of what is taking place to-day. He thus founded his system upon a careful study of the processes whereby geological changes are now brought about. He felt assured that Nature must be consistent and uniform in her working, and that only in proportion as her operations at the present time are watched and understood will the ancient history of the earth become intelligible. Thus, in his hands, the investigation of the Present became the key to the interpretation of the Past. The establishment of this great truth was the first step towards the inauguration of a true science of the earth. The doctrine of the uniformity of causation in Nature became the fruitful principle on which the structure of modern geology could be built up.

Fresh life was now breathed into the study of the earth. A new spirit seemed to animate the advance along every pathway of inquiry. Facts that had long been familiar came to possess a wider and deeper meaning when their connection with each other was recognized as parts of one great harmonious system of continuous change. In no department of Nature, for example, was this broader vision more remarkably displayed than in that wherein the circulation of water between land and sea plays the most conspicuous part. From the earliest times men had watched the coming of clouds, the fall of rain, the flow of rivers, and had recognized that on this nicely adjusted machinery the beauty and fertility of the land depend. But they now learned that this beauty and fertility involve a continual decay of the terrestrial surface; that the soil is a measure of this decay, and would cease to afford us maintenance were it not continually removed and renewed; that through the ceaseless transport of soil by rivers to the sea the face of the land is slowly lowered in level and carved into mountain and valley, and that the materials thus borne outwards to the floor of the ocean are not lost, but accumulate there to form rocks, which in the end will be upraised into new lands. Decay and renovation, in well-balanced proportions, were thus shown to be the system on which the existence of the earth as a habitable globe had been established. It was impossible to conceive that the economy of the planet could be maintained on any other basis. Without the circulation of water the life of plants and animals would be impossible, and with that circulation the decay of the surface of the land and the renovation of its disintegrated materials are necessarily involved.

As it is now, so must it have been in past time. Hutton and Playfair pointed to the stratified rocks of the earth's crust as demonstrations that the same processes which are at work to-day have been in operation from a remote antiquity. By thus placing their theory on a basis of actual observation, and providing in the study of existing operations a guide to the interpretation of those in past times, they rescued the investigation of the history of the earth from the speculations of theologians and cosmologists, and established a place for it among the recognized inductive sciences. To the guiding influence of their philosophical system the prodigious strides made by modern geology are in large measure to be attributed. And here in their own city, after the lapse of a hundred years, let us offer to their memory the grateful homage of all who have profited by their labors.

But while we recognize with admiration the far-reaching influence of the doctrine of uniformity of causation in the investigation of the history of the earth, we must upon reflection admit that the doctrine has been pushed to an extreme perhaps not contemplated by its original founders. To take the existing conditions of Nature as a platform of actual knowledge from which to start in, an inquiry into former conditions was logical and prudent. Obviously, however, human experience, in the few centuries during which attention has been turned to such subjects, has been too brief to warrant any dogmatic assumption that the various natural processes must have been carried on in the past with the same energy and at the same rate as they are carried on now. Variations in energy might have been legitimately conceded as possible, though not to be allowed without reasonable proof in their favor. It was right to refuse to admit the operation of speculative causes of change when the phenomena were capable of natural and adequate explanation by reference to causes that can be watched and investigated. But it was an error to take for granted that no other kind of process or influence, nor any variation in the rate of activity save those of which man has had actual cognizance, has played a part in the terrestrial economy. The uniformitarian writers laid themselves open to the charge of maintaining a kind of perpetual motion in the machinery of Nature. They could find in the records of the earth's history no evidence of a beginning, no prospect of an end. They saw that many successive renovations and destructions had been effected on the earth's surface, and that this long line of vicissitudes formed a series of which the earliest were lost in antiquity, while the latest were still in progress towards an apparently illimitable future.

The discoveries of William Smith, had they been adequately understood, would have been seen to offer a corrective to this rigidly uniformitarian conception, for they revealed that the crust of the earth contains the long record of an unmistakable order of progression in organic types. They proved that plants and animals have varied widely in successive periods of the earth's history; the present condition of organic life being only the latest phase of a long preceding series, each stage of which recedes further from the existing aspect of things as we trace it backward into the past. And though no relic had yet been found, or indeed was ever likely to be found, of the first living things that appeared upon the earth's surface, the manifest simplification of types in the older formations pointed irresistibly to some beginning from which the long procession has taken its start. If then it could thus be demonstrated that there had been upon the globe an orderly march of living forms from the lowliest grades in early times to man himself to-day, and thus that in one department of her domain, extending through the greater portion of the records of the earth's history, Nature had not been uniform, but had followed a vast and noble plan of evolution, surely it might have been expected that those who discovered and made known this plan would seek to ascertain whether some analogous physical progression from a definite beginning might not be discernible in the framework of the globe itself.

But the early masters of the science labored under two great disadvantages. In the first place, they found the oldest records of the earth's history so broken up and effaced as to be no longer legible. And in the second place, they lived under the spell of that strong reaction against speculation which followed the bitter controversy between the Neptunists and Plutonists in the earlier decades of the century. They considered themselves bound to search for facts, not to build up theories; and as in the crust of the earth they could find no facts which threw any light upon the primeval constitution and subsequent development of our planet, they shut their ears to any theoretical interpretations that might be offered from other departments of science. It was enough for them to maintain, as Hutton had done, that in the visible structure of the earth itself no trace can be found of the beginning of things, and that the oldest terrestrial records reveal no physical conditions essentially different from those in which we still live. They doubtless listened with interest to the speculations of Kant, Laplace, and Herschel on the probable evolution of nebulæ, suns, and planets, but it was with the languid interest attaching to ideas that lay outside of their own domain of research. They recognized no practical connection between such speculations and the data furnished by the earth itself as to its own history and progress.

This curious lethargy with respect to theory on the part of men who were popularly regarded as among the most speculative followers of science would probably not have been speedily dispelled by any discovery made within their own field of observation. Even now, after many years of the most diligent research, the first chapters of our planet's history remain undiscovered or undecipherable. On the great terrestrial palimpsest the earliest inscriptions seem to have been hopelessly effaced by those of later ages. But the question of the primeval condition and subsequent history of the planet might be considered from the side of astronomy and physics. And it was by investigations of this nature that the geological torpor was eventually dissipated. To our illustrious former president, Lord Kelvin, who occupied this chair when the association last met in Edinburgh, is mainly due the rousing of attention to this subject. By the most convincing arguments he showed how impossible it was to believe in the extreme doctrine of uniformitarianism. And though, owing to uncertainty in regard to some of the data, wide limits of time were postulated by him, he insisted that within these limits the whole evolution of the earth and its inhabitants must have been comprised. While therefore the geological doctrine that the present order of Nature must be our guide to the interpretation of the past remained as true and fruitful as ever, it had now to be widened by the reception of evidence furnished by a study of the earth as a planetary body. The secular loss of heat, which demonstrably takes place both from the earth and the sun, made it quite certain that the present could not have been the original condition of the system. This diminution of temperature with all its consequences is not a mere matter of speculation, but a physical fact of the present time as much as any of the familiar physical agencies that affect the surface of the globe. It points with unmistakable directness to that beginning of things of which Hutton and his followers could find no sign.

Another modification or enlargement of the uniformitarian doctrine was brought about by continued investigation of the terrestrial crust and consequent increase of knowledge respecting the history of the earth. Though Hutton and Playfair believed in periodical catastrophes, and indeed required these to recur in order to renew and preserve the habitable condition of our planet, their successors gradually came to view with repugnance any appeal to abnormal, and especially to violent manifestations of terrestrial vigor, and even persuaded themselves that such slow and comparatively feeble action as had been witnessed by man could alone be recognized in the evidence from which geological history must be compiled. Well do I remember in my own boyhood what a cardinal article of faith this prepossession had become. We were taught by our great and honored master, Lyell, to believe implicitly in gentle and uniform operations, extended over indefinite periods of time, though possibly some, with the zeal of partisans, carried this belief to an extreme which Lyell himself did not approve. The most stupendous marks of terrestrial disturbance, such as the structure of great mountain chains, were deemed to be more satisfactorily accounted for by slow movements prolonged through indefinite ages than by any sudden convulsion.

What the more extreme members of the uniformitarian school failed to perceive was the absence of all evidence that terrestrial catastrophes even on a colossal scale might not be a part of the present economy of this globe. Such occurrences might never seriously affect the whole earth at one time, and might return at such wide intervals that no example of them has yet been chronicled by man. But that they have occurred again and again, and even within comparatively recent geological times, hardly admits of serious doubt. How far at different epochs and in various degrees they may have included the operation of cosmical influences lying wholly outside the planet, and how far they have resulted from movements within the body of the planet itself, must remain for further inquiry. Yet the admission that they have played a part in geological history may be freely made without impairing the real value of the Huttonian doctrine, that in the interpretation of this history our main guide must be a knowledge of the existing processes of terrestrial change.

As the most recent and best known of these great transformations, the Ice Age stands out conspicuously before us. If any one sixty years ago had ventured to affirm that at no very distant date the snows and glaciers of the Arctic regions stretched southwards into France, he would have been treated as a mere visionary theorist. Many of the facts to which he would have appealed in support of his statement were already well known, but they had received various other interpretations. By some observers, notably by Hutton's friend, Sir James Hall, they were believed to be due to violent debacles of water that swept over the face of the land. By others they were attributed to the strong tides and currents of the sea when the land stood at a lower level. The uniformitarian school of Lyell had no difficulty in elevating or depressing land to any required extent. Indeed, when we consider how averse these philosophers were to admit any kind or degree of natural operation other than those of which there was some human experience, we may well wonder at the boldness with which, on sometimes the slenderest evidence, they made land and sea change places, on the one hand submerging mountain ranges and on the other placing great barriers of land where a deep ocean rolls. They took such liberties with geography because only well-established processes of change were invoked in the operations. Knowing that during the passage of an earthquake a territory bordering the sea may be upraised or sunk a few feet, they drew the sweeping inference that any amount of upheaval or depression of any part of the earth's surface might be claimed in explanation of geological problems. The progress of inquiry, while it has somewhat curtailed this geographical license, has now made known in great detail the strange story of the Ice Age.

There can not be any doubt that after man had become a denizen of the earth, a great physical change came over the Northern hemisphere. The climate, which had previously been so mild that evergreen trees flourished within ten or twelve degrees of the north pole, now became so severe that vast sheets of snow and ice covered the north of Europe and crept southward beyond the south coast of Ireland, almost as far as the southern shores of England, and across the Baltic into France and Germany. This Arctic transformation was not an episode that lasted merely a few seasons, and left the land to resume thereafter its ancient aspect. With various successive fluctuations it must have endured for many thousands of years. When it began to disappear it probably faded away as slowly and imperceptibly as it had advanced, and when it finally vanished it left Europe and North America profoundly changed in the character alike of their scenery and of their inhabitants. The rugged rocky contours of earlier times were ground smooth and polished by the march of the ice across them, while the lower grounds were buried under wide and thick sheets of clay, gravel, and sand, left behind by the melting ice. The varied and abundant flora which had spread so far within the Arctic circle was driven away into more southern and less ungenial climes. But most memorable of all was the extirpation of the prominent large animals which, before the advent of the ice, had roamed over Europe. The lions, hyenas, wild horses, hippopotamuses, and other creatures either became entirely extinct or were driven into the Mediterranean basin and into Africa. In their place came northern forms—the reindeer, glutton, musk ox, woolly rhinoceros, and mammoth.

Such a marvellous transformation in climate, in scenery, in vegetation and in inhabitants, within what was after all but a brief portion of geological time, though it may have involved no sudden or violent convulsion, is surely entitled to rank as a catastrophe in the history of the globe. It was probably brought about mainly if not entirely by the operation of forces external to the earth. No similar calamity having befallen the continents within the time during which man has been recording his experience, the Ice Age might be cited as a contradiction to the doctrine of uniformity. And yet it manifestly arrived as part of the established order of Nature. Whether or not we grant that other ice ages preceded the last great one, we must admit that the conditions under which it arose, so far as we know them, might conceivably have occurred before and may occur again. The various agencies called into play by the extensive refrigeration of the northern hemisphere were not different from those with which we are familiar. Snow fell and glaciers crept as they do to-day. Ice scored and polished rocks exactly as it still does among the Alps and in Norway. There was nothing abnormal in the phenomena, save the scale on which they were manifested. And thus, taking a broad view of the whole subject, we recognize the catastrophe, while at the same time we see in its progress the operation of those same natural processes which we know to be integral parts of the machinery whereby the surface of the earth is continually transformed.

Among the debts which science owes to the Huttonian school, not the least memorable is the promulgation of the first well-founded conceptions of the high antiquity of the globe. Some six thousand years had previously been believed to comprise the whole life of the planet, and indeed of the entire universe. When the curtain was then first raised that had veiled the history of the earth, and men, looking beyond the brief span within which they had supposed that history to have been transacted, beheld the records of a long vista of ages stretching far away into a dim illimitable past, the prospect vividly impressed their imagination. Astronomy had made known the immeasurable fields of space; the new science of geology seemed now to reveal boundless distances of time. The more the terrestrial chronicles were studied the farther could the eye range into an antiquity so vast as to defy all attempts to measure or define it. The progress of research continually furnished additional evidence of the enormous duration of the ages that preceded the coming of man, while, as knowledge increased, periods that were thought to have followed each other consecutively were found to have been separated by prolonged intervals of time. Thus the idea arose and gained universal acceptance that, just as no boundary could be set to the astronomer in his free range through space, so the whole of bygone eternity lay open to the requirements of the geologist. Playfair, re-echoing and expanding Hutton's language, had declared that neither among the records of the earth, nor in the planetary motions, can any trace be discovered of the beginning or of the end of the present order of things; that no symptom of infancy or of old age has been allowed to appear on the face of nature, nor any sign by which either the past or the future duration of the universe can be estimated; and that although the Creator may put an end, as he no doubt gave a beginning, to the present system, such a catastrophe will not be brought about by any of the laws now existing, and is not indicated by anything which we perceive. This doctrine was naturally espoused with warmth by the extreme uniformitarian school, which required an unlimited duration of time for the accomplishment of such slow and quiet cycles of change as they conceived to be alone recognizable in the records of the earth's past history.

It was Lord Kelvin, who, in the writings to which I have already referred, first called attention to the fundamentally erroneous nature of these conceptions. He pointed out that from the high internal temperature of our globe, increasing inwards as it does, and from the rate of loss of its heat, a limit may be fixed to the planet's antiquity. He showed that so far from there being no sign of a beginning, and no prospect of an end, to the present economy, every lineament of the solar system bears witness to a gradual dissipation of energy from some definite starting point. No very precise data were then, or indeed are now, available for computing the interval which has elapsed since that remote commencement, but he estimated that the surface of the globe could not have consolidated less than twenty millions of years ago, for the rate of increase of temperature inwards would in that case have been higher than it actually is; nor more than four hundred millions of years ago, for then there would have been no sensible increase at all. He was inclined, when first dealing with the subject, to believe that from a review of all the evidence then available, some such period as one hundred millions of years would embrace the whole geological history of the globe.

It is not a pleasant experience to discover that a fortune which one has unconcernedly believed to be ample has somehow taken to itself wings and disappeared. When the geologist was suddenly awakened by the energetic warning of the physicist, who assured him that he had enormously overdrawn his account with past time, it was but natural under the circumstances that he should think the accountant to be mistaken, who thus returned to him dishonored the large drafts he had made on eternity. He saw how wide were the limits of time deducible from physical considerations, how vague the data from which they had been calculated. And though he could not help admitting that a limit must be fixed beyond which his chronology could not be extended, he consoled himself with the reflection that after all a hundred millions of years was a tolerably ample period of time, and might possibly have been quite sufficient for the transaction of all the prolonged sequence of events recorded in the crust of the earth. He was therefore disposed to acquiesce in the limitation thus imposed upon geological history.

But physical inquiry continued to be pushed forward with regard to the early history and antiquity of the earth. Further consideration of the influence of tidal friction in retarding the earth's rotation, and of the sun's rate of cooling, led to sweeping reductions of the time allowable for the evolution of the planet. The geologist found himself in the plight of Lear when his bodyguard of 100 knights was cut down. "What need you five-and-twenty, ten or five?" demands the inexorable physicist, as he remorselessly strikes slice after slice from his allowance of geological time. Lord Kelvin is willing, I believe, to grant us some twenty millions of years, but Professor Tait would have us content with less than ten millions.

In scientific as in other mundane questions there may often be two sides, and the truth may ultimately be found not to lie wholly with either. I frankly confess that the demands of the early geologists for an unlimited series of ages were extravagant, and even, for their own purposes, unnecessary, and that the physicist did good service in reducing them. It may also be freely admitted that the latest conclusions from physical considerations of the extent of geological time require that the interpretation given to the record of the rocks should be rigorously revised, with the view of ascertaining how far that interpretation may be capable of modification or amendment. But we must also remember that the geological record constitutes a voluminous body of evidence regarding the earth's history which can not be ignored, and must be explained in accordance with ascertained natural laws. If the conclusions derived from the most careful study of this record can not be reconciled with those drawn from physical considerations, it is surely not too much to ask that the latter should be also revised. It has been well said that the mathematical mill is an admirable piece of machinery, but that the value of what it yields depends upon the quality of what is put into it. That there must be some flaw in the physical argument I can, for my own part, hardly doubt, though I do not pretend to be able to say where it is to be found. Some assumption, it seems to me, has been made, or some consideration has been left out of sight, which will eventually be seen to vitiate the conclusions, and which when duly taken into account will allow time enough for any reasonable interpretation of the geological record.

In problems of this nature, where geological data capable of numerical statement are so needful, it is hardly possible to obtain trustworthy computations of time. We can only measure the rate of changes in progress now, and infer from these changes the length of time required for the completion of results achieved by the same processes in the past. There is fortunately one great cycle of movement which admits of careful investigation, and which has been made to furnish valuable materials for estimates of this kind. The universal degradation of the land, so notable a characteristic of the earth's surface, has been regarded as an extremely slow process. Though it goes on without ceasing, yet from century to century it seems to leave hardly any perceptible trace on the landscapes of a country. Mountains and plains, hills and valleys appear to wear the same familiar aspect which is indicated in the oldest pages of history. This obvious slowness in one of the most important departments of geological activity doubtless contributed in large measure to form and foster a vague belief in the vastness of the antiquity required for the evolution of the earth.

But, as geologists eventually came to perceive, the rate of degradation of the land is capable of actual measurement. The amount of material worn away from the surface of any drainage basin and carried in the form of mud, sand, or gravel, by the main river into the sea represents the extent to which that surface has been lowered by waste in any given period of time. But denudation and deposition must be equivalent to each other. As much material must be laid down in sedimentary accumulations as has been mechanically removed, so that in measuring the annual bulk of sediment borne into the sea by a river, we obtain a clue not only to the rate of denudation of the land, but also to the rate at which the deposition of new sedimentary formations takes place.

As might be expected, the activities involved in the lowering of the surface of the land are not everywhere equally energetic. They are naturally more vigorous where the rainfall is heavy, where the daily range of temperature is large, and where frosts are severe. Hence they are obviously much more effective in mountainous regions than on plains; and their results must constantly vary, not only in different basins of drainage, but even, and sometimes widely, within the same basin. Actual measurement of the proportion of sediment in river water shows that while in some cases the lowering of the surface of the land may be as much as $\frac{1}{730}$ of a foot in a year, in others it falls as low as $\frac{1}{6800}$. In other words, the rate of deposition of new sedimentary formations, over an area of sea floor equivalent to that which has yielded the sediment, may vary from one foot in seven hundred and thirty years to one foot in six thousand eight hundred years.

If now we take these results and apply them as measures of the length of time required for the deposition of the various sedimentary masses that form the outer part of the earth's crust, we obtain some indication of the duration of geological history. On a reasonable computation these stratified masses, where most fully developed, attain a united thickness of not less than 100,000 feet. If they were all laid down at the most rapid recorded rate of denudation, they would require a period of seventy-three millions of years for their completion. If they were laid down at the slowest rate they would demand a period of not less than six hundred and eighty millions.

But it may be argued that all kinds of terrestrial energy are growing feeble, that the most active denudation now in progress is much less vigorous than that of bygone ages, and hence that the stratified part of the earth's crust may have been put together in a much briefer space of time than modern events might lead us to suppose. Such arguments are easily adduced and look sufficiently specious, but no confirmation of them can be gathered from the rocks. On the contrary, no one can thoughtfully study the various systems of stratified formations without being impressed by the fullness of their evidence that, on the whole, the accumulation of sediment has been extremely slow. Again and again we encounter groups of strata composed of thin paper-like laminæ of the finest silt, which evidently settled down quietly and at intervals on the sea bottom. We find successive layers covered with ripple-marks and sun-cracks, and we recognize in them memorials of ancient shores where sand and mud tranquilly gathered as they do in sheltered estuaries at the present day. We can see no proof whatever—nor even any evidence which suggests—that on the whole the rate of waste and sedimentation was more rapid during Mesozoic and Palaeozoic time than it is to-day. Had there been any marked difference in this rate from ancient to modern times, it would be incredible that no clear proof of it should have been recorded in the crust of the earth.

But in actual fact the testimony in favor of the slow accumulation and high antiquity of the geological record is much stronger than might be inferred from the mere thickness of the stratified formations. These sedimentary deposits have not been laid down in one unbroken sequence, but have had their continuity interrupted again and again by upheaval and depression. So fragmentary are they in some regions that we can easily demonstrate the length of time represented there by still existing sedimentary strata to be vastly less than the time indicated by the gaps in the series.

There is yet a further and impressive body of evidence furnished by the successive races of plants and animals which have lived upon the earth and have left their remains sealed up within its rocky crust. No one now believes in the exploded doctrine that successive creations and universal destructions of organic life are chronicled in the stratified rocks. It is everywhere admitted that, from the remotest times up to the present day, there has been an onward march of development, type succeeding type in one long continuous progression. As to the rate of this evolution precise data are wanting. There is however the important negative argument furnished by the absence of evidence of recognizable specific variations of organic forms since man began to observe and record. We know that within human experience a few species have become extinct, but there is no conclusive proof that a single new species have come into existence, nor are appreciable variations readily apparent in forms that live in a wild state. The seeds and plants found with Egyptian mummies, and the flowers and fruits depicted on Egyptian tombs, are easily identified with the vegetation of modern Egypt. The embalmed bodies of animals found in that country show no sensible divergence from the structure or proportions of the same animals at the present day. The human races of Northern Africa and Western Asia were already as distinct when portrayed by the ancient Egyptian artists as they are now, and they do not seem to have undergone any perceptible change since then. Thus a lapse of four or five thousand years has not been accompanied by any recognizable variation in such forms of plant and animal life as can be tendered in evidence. Absence of sensible change in these instances is, of course, no proof that considerable alteration may not have been accomplished in other forms more exposed to vicissitudes of climate and other external influences. But it furnishes at least a presumption in favor of the extremely tardy progress of organic variation.

If however we extend our vision beyond the narrow range of human history, and look at the remains of the plants and animals preserved in those younger formations which, though recent when regarded as parts of the whole geological record, must be many thousands of years older than the very oldest of human monuments, we encounter the most impressive proofs of the persistence of specific forms. Shells which lived in our seas before the coming of the Ice age present the very same peculiarities of form, structure, and ornament which their descendants still possess. The lapse of so enormous an interval of time has not sufficed seriously to modify them. So too with the plants and the higher animals which still survive. Some forms have become extinct, but few or none which remain display any transitional gradations into new species. We must admit that such transitions have occurred, that indeed they have been in progress ever since organized existence began upon our planet, and are doubtless taking place now. But we can not detect them on the way, and we feel constrained to believe that their march must be excessively slow.

There is no reason to think that the rate of organic evolution has ever seriously varied; at least no proof has been adduced of such variation. Taken in connection with the testimony of the sedimentary rocks, the inferences deducible from fossils entirely bear out the opinion that the building up of the stratified crust of the earth has been extremely gradual. If the many thousands of years which have elapsed since the Ice age have produced no appreciable modification of surviving plants and animals, how vast a period must have been required for that marvellous scheme of organic development which is chronicled in the rocks!

After careful reflection on the subject, I affirm that the geological record furnishes a mass of evidence which no arguments drawn from other departments of nature can explain away, and which, it seems to me, can not be satisfactorily interpreted save with an allowance of time much beyond the narrow limits which recent physical speculation would concede.

I have reserved for final consideration a branch of the history of the earth which, while it has become, within the lifetime of the present generation, one of the most interesting and fascinating departments of geological inquiry, owed its first impulse to the far-seeing intellects of Hutton and Playfair. With the penetration of genius these illustrious teachers perceived that if the broad masses of land and the great chains of mountains owe their origin to stupendous movements which from time to time have convulsed the earth, their details of contour must be mainly due to the eroding power of running water. They recognized that as the surface of the land is continually worn down, it is essentially by a process of sculpture that the physiognomy of every country has been developed, valleys being hollowed out and hills left standing, and that these inequalities in topographical detail are only varying and local accidents in the progress of the one great process of the degradation of the land.

From the broad and guiding outlines of theory thus sketched we have now advanced amid ever-widening multiplicity of detail into a fuller and nobler conception of the origin of scenery. The law of evolution is written as legibly on the landscapes of the earth as on any other page of the book of nature. Not only do we recognize that the existing topography of the continents, instead of being primeval in origin, has gradually been developed after many precedent mutations, but we are enabled to trace these earlier revolutions in the structure of every hill and glen. Each mountain chain is thus found to be a memorial of many successive stages in geographical evolution. Within certain limits land and sea have changed places again and again. Volcanoes have broken out and have become extinct in many countries long before the advent of man. Whole tribes of plants and animals have meanwhile come and gone, and in leaving their remains behind them as monuments at once of the slow development of organic types, and of the prolonged vicissitudes of the terrestrial surface, have furnished materials for a chronological arrangement of the earth's topographical features. Nor is it only from the organisms of former epochs that broad generalizations may be drawn regarding revolutions in geography. The living plants and animals of to-day have been discovered to be eloquent of ancient geographical features that have long since vanished. In their distribution they tell us that climates have changed; that islands have been disjoined from continents; that oceans once united have been divided from each other, or once separate have now been joined; that some tracts of land have disappeared, while others for prolonged periods of time have remained in isolation. The present and the past are thus linked together, not merely by dead matter, but by the world of living things, into one vast system of continuous progression.

In this marvellous increase of knowledge regarding the transformations of the earth's surface, one of the most impressive features, to my mind, is the power now given to us of perceiving the many striking contrasts between the present and former aspects of topography and scenery. We seem to be endowed with a new sense. What is seen by the bodily eye—mountain, valley, or plain—serves but as a veil, beyond which, as we raise it, visions of long-lost lands and seas rise before us in a far-retreating vista. Pictures of the most diverse and opposite character are beheld, as it were, through each other, their lineaments subtly interwoven, and even their most vivid contrasts subdued into one blended harmony. Like the poet, "we see, but not by sight alone;" and the "ray of fancy" which, as a sunbeam, lightened up his landscape, is for us broadened and brightened by that play of the imagination which science can so vividly excite and prolong.

Admirable illustrations of this modern interpretation of scenery are supplied by the district wherein we are now assembled. On every side of us rise the most convincing proofs of the reality and potency of that ceaseless sculpture by which the elements of landscape have been carved into their present shapes. Turn where we may, our eyes rest on hills that project above the lowland, not because they have been upheaved into these positions, but because their stubborn materials have enabled them better to withstand the degradation which has worn down the softer strata into the plains around them. Inch by inch the surface of the land has been lowered, and each hard rock successively laid bare has communicated its own characteristics of form and color to the scenery.

If, standing on the Castle Rock, the central and oldest site in Edinburgh, we allow the bodily eye to wander over the fair landscape, and the mental vision to range through the long vista of earlier landscapes which science here reveals to us, what a strange series of pictures passes before our gaze! The busy streets of to-day seem to fade away into the mingled copsewood and forest of pre-historic time. Lakes that have long since vanished gleam through the woodlands, and a rude canoe pushing from the shore startles the red deer that had come to drink. While we look, the picture changes to a polar scene, with bushes of stunted Arctic willow and birch, among which herds of reindeer browse and the huge mammoth makes his home. Thick sheets of snow are draped all over the hills around, and far to the northwest the distant gleam of glaciers and snowfields marks the line of the Highland mountains. As we muse on this strange contrast to the living world of to-day the scene appears to grow more Arctic in aspect, until every hill is buried under one vast sheet of ice, 2,000 feet or more in thickness, which fills up the whole midland valley of Scotland and creeps slowly eastward into the basin of the North Sea. Here the curtain drops upon our moving pageant, for in the geological record of this part of the country an enormous gap occurs before the coming of the Ice Age.

When once more the spectacle resumes its movement the scene is found to have utterly changed. The familiar hills and valleys of the Lothians have disappeared. Dense jungles of a strange vegetation—tall reeds, club-mosses, and tree-ferns—spread over the streaming swamps that stretch for leagues in all directions. Broad lagoons and open seas are dotted with little volcanic cones which throw out their streams of lava and showers of ashes. Beyond these, in dimmer outline and older in date, we descry a wide lake or inland sea, covering the whole midland valley and marked with long lines of active volcanoes, some of them several thousand feet in height. And still further and fainter over the same region, we may catch a glimpse of that still earlier expanse of sea which in Silurian times overspread most of Britian. But beyond this scene our vision fails. We have reached the limit across which no geological evidence exists to lead the imagination into the primeval darkness beyond.

Such in briefest outline is the succession of mental pictures which modern science enables us to frame out of the landscapes around Edinburgh. They may be taken as illustrations of what may be drawn, and sometimes with even greater fulness and vividness, from any district in these islands. But I cite them especially because of their local interest in connection with the present meeting of the Association, and because the rocks that yield them gave inspiration to those great masters whose claims on our recollection, not least for their explanation of the origin of scenery, I have tried to recount this evening.

GEOLOGICAL HISTORY OF THE YELLOWSTONE NATIONAL PARK.*

By Arnold Hague,
U. S. Geological Survey.

In the short time allotted to me I can only hope to present a brief sketch of the main geological features of the country which you are about to visit. My remarks must, of necessity, be more or less incomplete, as my desire is not so much to elucidate any special problem connected with the many interesting geological questions to be found here, but rather to offer such a general view of the region as will enable you, during your five days' trip through the Park, to understand clearly something of its physical geography and geology.

The Yellowstone Park is situated in the extreme northwestern portion of the Territory of Wyoming. Its boundaries, as determined by the original act of Congress setting apart the Park, are very ill-defined. At the time of the enactment of the law establishing this national reservation, the region had been but little explored, and its relation to the physical features of the adjacent country was but little understood. Since that time, surveys have shown that only a narrow strip, about 2 miles in width, was situated in the Territory of Montana, but it was also found that a still narrower strip extended westward into the Territory of Idaho. The question of properly establishing the boundaries, based upon our present knowledge of the country, is now before Congress, and an act has already passed the Senate, proposing to make the northern boundary coincide with the boundary between Wyoming and Montana, and the western boundary coincide with the Wyoming and Idaho line. The act under consideration extends the southern boundary of the Park to the 44th parallel of latitude, carrying the area of the reservation southward 9½ miles. The eastern boundary is made to coincide with the meridian of 109° 30′, adding a strip of country about 24½ miles in width along the entire eastern side of the Park.

The area of the Park, as at present defined, is somewhat more than 3,300 square miles, and the proposed addition increases the reservation

* An address at a special session of the American Institute of Mining Engineers, at Mammoth Hot Springs, Wyoming, on the borders of the National Park, July, 1887. (From *Trans. Am. Inst. Mining Engineers.*)

by nearly 2,000 square miles. The Park plateau, with the adjacent mountains, presents a sharply defined region, in strong contrast with the rest of the northern Rocky Mountains. It stands out boldly by itself, unique in topographical structure, and complete as a geological problem.

The central portion of the Yellowstone Park is, essentially, a broad, elevated, volcanic plateau, between 7,000 and 8,500 feet above sea-level, and with an average elevation of about 8,000 feet. Surrounding it on the south, east, north, and northwest, are mountain ranges with culminating peaks and ridges rising from 2,000 to 4,000 feet above the general level of the inclosed table-land.

For present purposes it is needless to confine ourselves strictly to legal boundaries, but rather to consider the entire region in its broader physical features. It is worthy of note, however, that by the proposed enlargement the protected area will agree closely with the geographical province.

South of the Park, the Tetons stand out prominently above the surrounding country, the highest, grandest peaks in the northern Rocky Mountains. The eastern face of this mountain mass rises with unrivalled boldness for nearly 7,000 feet above Jackson Lake. Northward, the ridges fall away abruptly beneath the lavas of the Park, only the outlying spurs coming within the limits of the reservation. For the most part the mountains are made up of coarse crystalline gneisses and schists, probably of Archean age, flanked on the northern spurs by upturned Palæozoic strata.

To the east, across the broad valley of the Upper Snake, generally known as Jackson Basin, lies the well-known Wind River Range, famous from the earliest days of the Rocky Mountain trappers. The Northern end of this range is largely composed of Mesozoic strata, single ridges of Cretaceous sandstone penetrating still farther northward into the regions of the Park, and protruding above the great flows of lava.

Along the entire eastern side of the Park stretches the Absaroka Range—so-called from the Indian name of the Crow Nation. The Absaroka Range is intimately connected with the Wind River, the two being so closely related that any line of separation must be drawn more or less arbitrarily, based more upon geological structures and forms of erosion than upon physical limitations.

The Absarokas offer, for more than 80 miles, a bold, unbroken barrier to all western progress; a rough, rugged country, dominated by high peaks and crags from 10,000 to 11,000 feet in height. Only a few adventurous hunters and mountaineers cross the range by one or two dangerous, precipitous trails known to but few. The early trappers found it a forbidding land; prospectors who followed them, a barren one.

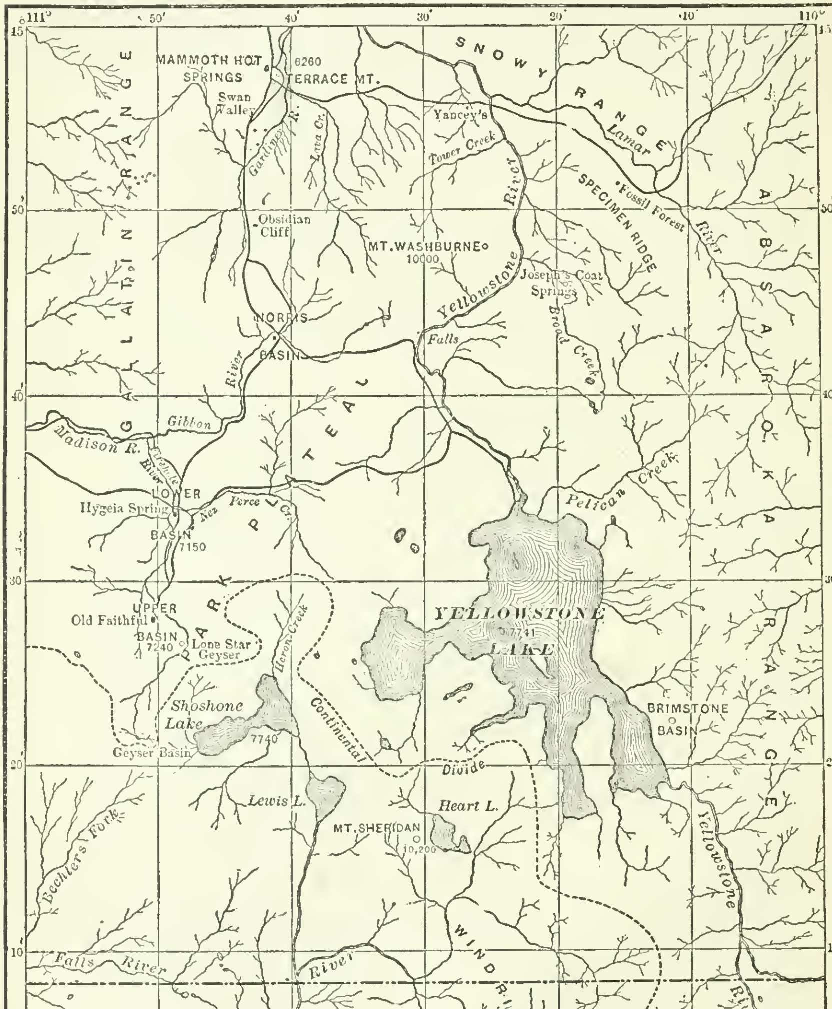
At the northeast corner of the Park a confused mass of mountains connects the Absarokas with the Snowy Range. This Snowy Range shuts in the Park on the north, and is an equally rough region of country, with elevated mountain masses covered with snow the greater part of the year, as the name would indicate. Only the southern slopes, which rim in the Park region, come within the limit of our investigation. Here the rocks are mainly granites, gneisses, and schists, the sedimentary beds, for the most part, referable to the pre-Cambrian series.

The Gallatin Range incloses the Park on the north and northwest. It lies directly west of the Snowy, only separated by the broad valley of the Yellowstone River. It is a range of great beauty, of diversified forms, and varied geological problems. Electric Peak, in the extreme northwestern corner of the Park, is the culminating point in the range, and affords one of the most extended views to be found in this part of the country. Archean gneisses form a prominent mass in the range, over which occur a series of sandstones, limestones, and shales, of Paleozoic and Mesozoic age, representing Cambrian, Silurian, Devonian, Carboniferous, Trias, Jura, and Cretaceous. Immediately associated with these sedimentary beds, are large masses of intrusive rocks, which have played an important part in bringing about the present structural features of the range. They are all of the andesitic type, but showing considerable range in mineral composition, including pyroxene, hornblende, and hornblende mica varieties. These intrusive masses are found in narrow dikes, in immense interbedded sheets forced between the different strata, and as laccolites, a mode of occurrence first described from the Henry Mountains in Utah, by Mr. G. K. Gilbert, but now well recognized elsewhere in the northern Cordillera.

We see then that the Absarokas rise as a formidable barrier on the eastern side of the Park, the Gallatins as a steep mural face on the west side, while the other ranges terminate abruptly, rimming in the Park on the north and south, and leaving a depressed region not unlike the parks of Colorado, only covering a more extended area with a relatively deeper basin. The region has been one of profound dynamic action, and the center of mountain building on a grand scale. On the accompanying map of the Yellowstone Park, which shows the position of the principal objects of interest, the relations of the ranges to the plateau are clearly indicated.

It is not my purpose at the present time to enter upon the details of geological structure of these ranges, each offering its own special study and field of investigation. My desire is simply to call your attention to their general features and mutual relations. So far as their age is concerned, evidence goes to show that the action of upheaval was contemporaneous in all of them, and coincident with the powerful dynamic movements which uplifted the north and south ranges, stretching across Colorado, Wyoming, and Montana. This dynamic movement blocked out, for the most part, the Rocky Mountains, near the close of the Cretaceous, although there is good reason to believe that in this region profound faulting and displacement continued the work of mountain building well into the Middle Tertiary period.

Throughout Tertiary time in the Park area, geological history was char-



Scale: 1 inch=12 miles. **YELLOWSTONE NATIONAL PARK**

acterized by great volcanic activity, enormous volumes of erupted material being poured out in the Eocene and Middle Tertiary, continuing with less force through the Pliocene, and extending into Quaternary time. Within very recent times there is no evidence of any considerable outburst: indeed the region may be considered long since extinct. These volcanic rocks present a wide range in chemical and mineral composition and physical structure. They may all however be classed under three great groups—andesites, rhyolites, and basalts—following each other in the order named. In some instances, eruptions of basalt occurred before the complete extinction of rhyolite, but in general, the relative age of each group is clearly and sharply defined, the distribution and mode of occurrence of each presenting characteristics and salient features frequently marked by periods of erosion.

Andesites are the only volcanic rocks which have played an important part in producing the present structural features of the mountains surrounding the Park. As already mentioned, they occur in large masses in the Gallatin range, while most of the culminating peaks in the Absarokas are composed of compact andesites and andesitic breccias. On the other hand, the andesites are not confined to the mountains, but played an active role in filling up the interior basin. That the duration of the andesitic eruptions was long continued, is made evident by the plant-remains found in ash and lava beds through 2,000 feet of volcanic material. The plants have as yet been too little studied to define positively their geological horizons. It is quite possible that they may indicate marked differences of climate between the lower and upper beds.

In early Tertiary times, a volcano burst forth in the northeast corner of the depressed area encircled by the Park Mountains, not far from the junction of the Absaroka and Snowy ranges. While not to to be compared in size and grandeur with the volcanoes of California and the Cascade Range, it is, for the Rocky Mountains, one of no mean proportions. It rises from a base about 6,500 feet above sea-level, the culminating peak attaining an elevation of 10,000 feet. This gives a height to the volcano of 3,500 feet from base to summit, measuring from the Archæan rocks of the Yellowstone Valley to the top of Mount Washburne. The average height of the crater rim is about 9,000 feet above sea level, the volcano measuring 15 miles across the base. The eruptive origin of Mount Washburne has long been recognized, and it is frequently referred to as a volcano. It is however simply the highest peak among several others, and represents a later outburst which destroyed in a measure the original rim and form of the older crater. The eruptions for the most part were basic andesites. Erosion has so worn away the earlier rocks, and enormous masses of more recent lavas have so obscured the original form of lava-flows, that it is not easy for an inexperienced eye to recognize a volcano and the surrounding peaks as the more elevated points in a grand crater wall. By following around on the ancient andesitic rim, and studying the outline of the old crater, together with the composition of its lavas, its true origin and history may readily be made out. This older crater has as yet received no special designation, but when our maps and reports are finally published, this ancient geological ruin will receive an appropriate designation. This old volcano of early Tertiary time occupies a prominent place in the geological development of the Park, and dates back to the earliest outbursts of lava which have in this region changed a depressed basin into an elevated plateau. We have here a volcano situated far inland, in an elevated region, in the heart of the Rocky Mountains. It lies on the eastern side of the continent, only a few miles from the great continental divide which sends its waters to both the Atlantic and Pacific.

After the dying out of the andesitic lavas, followed by a period of erosion, immense volumes of rhyolite were erupted, which not only threatened to fill up the crater but to bury the outer walls of the volcano. On all sides the andesitic slopes were submerged beneath the rhyolite to a height of from 8,000 to 8,500 feet. This enormous mass of rhyolite, poured out after the close of the andesitic period, did more than anything else to bring about the present physical features of the Park table-land. A tourist making the customary trip through the Park, visiting all the prominent geyser basins, hot springs, and the Grand Cañon and Falls of the Yellowstone, is not likely to come upon any other rock than rhyolite, excepting, of course, deposits from the hot springs. If he extended his journey to the lake region, taking in Shoshone, Lewis, and Yellowstone lakes, and spending a week or ten days going over the beaten routes of travel, he will not, unless he ascends Mount Washburne, leave the rhyolite lavas. A description of the rhyolite region is essentially one of the Park plateau. Taking the bottom of the basin at 6,500 feet above sea level, these acidic lavas were piled up until the accumulated mass measured 2,000 feet in thickness. It completely encircled the Gallatin Range, burying its lower slopes on both the east and west sides; it banked up all along the west flanks of the Absarokas, and buried the outlying spurs of the Teton and Wind River ranges.

The Park Plateau covers an area approximately 50 by 40 miles, with a mean altitude of 8,000 feet. It is accidented by undulating basins of varied outline and scored by deep canyons and gorges. Strictly speaking it is not a plateau; at least it is by no means a level area, but a rugged country, characterized by bold escarpments and abrupt edges of mesa-like ridges. But few large vents or centers of volcanic activity for the rhyolite have been recognized, the two principal sources being the volcano to which reference has already been made, and Mount Sheridan in the southern end of the Park. Mount Sheridan is the most commanding peak on the plateau, with an elevation 10,200 feet above sea level and 2,600 feet above Heart Lake. From the summit of the peak on a clear day one may overlook the entire plateau country and the mountains which shut it in, while almost at the base of the peak lie the magnificent lakes which add so much to the quiet beauty of the region, in contrast with the rugged scenery of the mountains. From no point is the magnitude and grandeur of the volcanic region so impressive. The lava-flows—bounded on the east by the Absarokas—extend westward not only across the park, but across the Madison Plateau, and out on to the great plains Snake of River, stretching far westward almost without a break in the continuity of the eruptive flows. Over the central portion of the park, whe. e the ryholites are thickest, erosion has failed to penetrate to the underlying rock. Even such deep gorges as the Yellowstone, Gibbon, and Madison Cañons have nowhere worn through these rhyolite flows. In the Grand Cañon of the Yellowstone the andesitic breccias are found beneath the rhyolites, but the deepest cuts fail to reveal the underlying sedimentary beds. Although the rocks of the plateau for the most part belong to one group of acidic lavas, they by no means present the great uniformity and monotony in field appearance that might be expected. These 2,000 square miles offer as grand a field for the study of structural forms, development of crystallization, and mode of occurrence of acidic lavas as could well be found anywhere in the world. They vary from a nearly holocrystalline rock to one of pure volcanic glass. Obsidian, pumice, pitchstone, ash, breccia, and an endless development of transition forms alternate with the more compact lithoidal lavas which make up the great mass of the rhyolite, and which in colors, texture, and structural developments present an equally varied aspect. In mineral composition these rock are simple enough. The essential minerals are orthoclase and quartz, with more or less plagioclase. Sanidine is the prevailing feldspar, although in many cases plagioclase forms occur nearly as abundantly as orthoclase. Chemical analyses, whether we consider the rocks from the crater of Mount Sheridan, the summit of the plateau, or the volcanic glass of the world-renowned Obsidian Cliff, present comparatively slight differences in ultimate composition.

The following analyses of two rocks, representing extreme forms in physical habit, show how closely they approach each other in composition of the original magma:

	No. 1. Madison Plateau.	No. 2. Obsidian Cliff.
Silica	75.19	75.52
Titanic acid	None.	None.
Phosphoric acid	None.	None.
Alumina	13.77	14.11
Ferric oxide	0.61	1.74
Ferrous oxide	1.37	0.08
Ferric sulphide		0.11
Manganese oxide	Trace.	None.
Lime	0.68	0.78
Magnesia	0.09	0.10
Lithia	0.02	
Potash	3.33	3.62
Soda	3.83	3.93
Sulphuric acid	0.29	
Water	0.65	
Ignition		0.39
Total	99.83	100.38

The rock from Madison Plateau was collected on the north side of Madison Canyon and was selected as a typical rock covering large areas of the Park. It is purplish-gray in color, rough in texture, porphyritic in structure, and characterized by well-developed sanidin and quartz. The obsidian, from Obsidian Cliff, is an excellent example of pure volcanic glass, wholly devoid of porphyritic crystals. In general the investigations of the laboratory confirm the observations of the field geologist, that the differences exhibited by the volcanic product are not of chemical or mineral composition, but rather of physical conditions under which the magma has cooled.

I have dwelt somewhat in detail upon the nature of these rocks for two reasons: First, because of the difficulty met with by the scientific traveller in recognizing the uniformity and simplicity of chemical composition of the rhyolite magma over the entire plateau, owing to its great diversity in superficial habit; second, on account of their geological importance in connection with the unrivalled display of the geysers and hot springs. That the energy of the steam and thermal waters dates well back into the period of volcanic action, there is in my opinion very little reason to doubt. As the energy of this underground heat is to-day one of the most impressive features of the country, I will defer commenting upon the geysers and hot springs until speaking of the present condition of the Park.

Although the rhyolite eruptions were probably of long duration and died out slowly, there is, I think, evidence to show that they occupied a clearly and sharply defined period between the andesites and basalt eruptions. Since the outpouring of this enormous body of rhyolite and building up of the plateau the region has undergone profound faulting and displacement, lifting up bodily immense blocks of lava and modifying the surface features of the country. Following the rhyolite came the period of basalt eruptions, which, in comparison with the andesite and rhyolite eras, was, so far as the Park was concerned, insignificant, both as regards the area covered by the basalt and its influence in modifying the physical aspect of the region. The basalt occurs as thin sheets overlying the rhyolite and in some instances as dikes cutting the more acidic rocks. It has broken out near the outer edge of the rhyolite body and occurs most frequently along the Yellowstone Valley, along the western foothills of the Gallatin Range and Madison Plateau, and again to the southward of the Falls River basin.

After the greater part of the basalt had been poured out came the glacial ice, which widened and deepened the pre-existing drainage channels, cut profound gorges through the rhyolite lavas and modelled the two volcanos into their present form. Over the greater part of the Cordillera of the central and northern Rocky Mountains wherever the peaks attain a sufficiently high altitude to attract the moisture-laden clouds evidences of the former existence of local glaciers are to be found. In the Teton Range several well-defined characteristic glaciers still exist upon the abrupt slopes of Mount Hayden and Mount Moran. They are the remnants of a much larger system of glaciers. The Park region presents so broad a mass of elevated country that the entire plateau was, in glacial times, covered with a heavy capping of ice. Evidences of glacial action are everywhere to be seen.

Over the Absaroka Range glaciers were forced down into the Lamar and Yellowstone valleys, thence westward over the top of Mount Evarts to the Mammoth Hot Springs Basin. On the opposite side of the Park the ice from the summit of the Gallatin Range moved eastward across Swan Valley and passing over the top of Terrace Mountain joined the ice field coming from the east. The united ice sheet plowed its way northward down the valley of the Gardiner to the Lower Yellowstone, where the broad valley may be seen strewn with the material transported from both the east and west rims of the Park.

Since the dying out of the rhyolite eruptions erosion has greatly modified the entire surface features of the Park. Some idea of the extent of this action may be realized when it is recalled that the deep cañons of the Yellowstone, Gibbon and Madison rivers—cañons in the strictest use of the word—have all been carved out since that time. To-day these gorges measure several miles in length and from 1,000 to 1,500 feet in depth.

To the geologist one of the most impressive objects on the Park plateau is a transported bowlder of granite which rests directly upon the rhyolite near the brink of the Grand Cañon, about 3 miles below the Falls of the Yellowstone. It stands alone in the forest, miles from the nearest glacial bowlder. Glacial detritus carrying granitic material may be traced upon both sides of the cañon wall, but not a fragment of rock more than a few inches in diameter, older than the recent lavas, has been recognized within a radius of many miles. This massive block, although irregular in shape and somewhat pointed toward the top, measures 24 feet in length by 20 feet in breadth and stands 18 feet above the base. The nearest point from which it could have been transported is distant 30 or 40 miles. Coming upon it in the solitude of the forest with all its strange surroundings it tells a most impressive story. In no place are the evidences of frost and fire brought so forcibly together as in the Yellowstone National Park.

Since the close of the ice period no geological events of any moment have brought about any changes in the physical history of the region other than those produced by the direct action of steam and thermal waters. A few insignificant eruptions have probably occurred, but they failed to modify the broad outlines of topographical structure and present but little of general interest beyond the evidence of the continuance of volcanic action into quaternary times. Volcanic activity in the Park may be considered as long since extinct. At all events indications of fresh lava-flows within historical times are wholly wanting. This is not without interest, as evidence of under-ground heat may be observed everywhere throughout the Park in the waters of the geysers and hot springs. All our observations point in one direction and lead to the theory that the cause of the high temperatures of these waters must be found in the heated rocks below, and that the origin of the heat is in some way associated with the source of volcanic energy. It by no means follows that the waters themselves are derived from any deep-seated source; on the contrary, investigation tends to show that the waters brought up by the geysers and hot springs are mainly surface waters which have percolated downward a sufficient distance to become heated by large volumes of steam ascending through fissures and vents from much greater depths. If this theory is the correct one it is but fair to demand that evidence of long-continued action of hot waters and super-heated steam should be apparent upon the rocks through which they passed on their way to the surface. This is precisely what one sees in innumerable places on the Park plateau. Indeed, the decomposition of the lavas of the rhyolite plateau have proceeded on a most gigantic scale, and could only have taken place after the lapse of an enormous period of time and the giving off of vast quantities of heat, if we are to judge at all by what we see going on around us to-day. The ascending currents of steam and hot water have been powerful geological agents, and have left an indelible impression upon the surface of the county. The most striking example of this action is found in the Grand Cañon of the Yellowstone. From the lower falls for 3 miles down the river abrupt walls upon both sides of the cañon, a thousand feet in depth, present a brilliancy and mingling of color beyond the power of description. From the brink of the cañon to the water's edge the walls are sheer bodies of decomposed rhyolite. Varied hues of orange, red, purple, and sulphur-yellow are irregularly blended in one confused mass. There is scarcely a piece of unaltered rock in place. Much of it is changed into kaolin; but from rhyolite, still easily recognized, occur transition products of every possible kind to good procelain clay. This is the result of the long-continued action of steam and vapors upon the rhyolite lavas. Through this mass of decomposed rhyolite the course of ancient steam vents in their upward passage may still be traced, while at the bottom of the cañon hot springs, fumaroles, and steam vents are still more or less active, but probably with diminished power.

It is needless to weary you with the details of this decomposition, but I may add that investigations in the laboratory upon these transition products fully substantiate field observations.

Still other areas are quite as convincing, if not on so grand a scale, as the Yellowstone Cañon. Joseph's Coat Basin, on the east side of the cañon, and Brimstone Hills, on the east side of the Yellowstone Lake, an extensive area on the slopes of the Absaroka Range, both present evidences of the same chemical processes brought about in the same manner. It is not stating it too strongly to say that the plateau on the east side of the Grand Cañon, from Broad Creek to Pelican Creek, is completely undermined by the action of super-heated steam and alkaline waters on the rhyolite lava. Similar processes may be seen going on to-day in all the geyser basins. To accomplish these changes a long period of time must have been required. The study of comparatively fresh vents shows almost no change from year to year, although careful scrutiny during a period of five years detects a certain amount of disintegration, but infinitely small in comparison with the great bodies of altered rock. This is well shown in a locality like the Monarch Geyser in the Norris Geyser Basin, where the water is thrown out at regular intervals through a narrow fissure in the rock.

The Grand Cañon of the Yellowstone offers one of the most impressive examples of erosion on a grand scale within recent geological times. It is self-evident that the deep cañon must be of much later origin than the rock through which it has been worn, and it seems quite clear that the course and outlines of the cañon were in great part determined by the easily eroded decomposed material forming the cañon walls, and this in turn was brought about by the slow processes just described.

The evidence of the antiquity of the hot spring deposits is, perhaps, shown in an equally striking manner and by a wholly different process of geological reasoning. Terrace Mountain is an outlying ridge of the rhyolite plateau just west of the Mammoth Hot Springs. It is covered on the summit with thick beds of travertine, among the oldest portions of the Mammoth Hot Springs deposits. It is the mode of occurrence of these calcareous deposits from the hot waters which has given the name to the mountain. Lying upon the surface of this travertine on the top of the mountain are found glacial bowlders brought from the summit of the Gallatin Range, fifteen miles away, and transported on the ice sheet across Swan Valley and deposited on the top of the mountain, 700 feet above the intervening valley. It offers the strongest possible evidence that the travertine is older than the glacier which has strewn the country with transported material. How much travertine was eroded by the ice is, of course, impossible to say, but so friable a material would yield readily to glacial movement.

Still another method of arriving at the great antiquity of the thermal energy and the age of the hot spring formation is by determining the rate of deposition and measuring the thickness of the accumulated sinter. This method, although the one which would perhaps first suggest itself, is in my opinion by no means as satisfactory as the geological reasoning already given. It is unsatisfactory because no uniform rate of deposition can be ascertained for even a single area, like the Upper Geyser Basin, and it is still more difficult to arrive at any conclusion as to the growth of the sinter in the past. Moreover, it is quite possible that heavy deposits may have suffered erosion before the present sinter was laid down. It however corroborates other methods and possesses the advantage of being a direct way.

It may be well to add here that there exists the greatest contrast between the deposits of the Mammoth Hot Springs and those found upon the plateau. At the Mammoth Springs they are nearly pure travertine, with only a trace of silica, analyses showing from 95 to 99 per cent of calcium carbonate. On the plateau, the deposits consist for the most part of siliceous sinter, locally termed "geyserite." The reason for the difference is this: At the Mammoth Hot Springs the steam, although ascending from fissures in the igneous rock, comes in contact with the waters found in the Mesozoic strata, which here form the surface rocks. The Jura or Cretaceous limestones have furnished the lime held in solution and precipitated on the surface as travertine. On the other hand, the mineral constituents of the plateau waters are derived almost exclusively from the highly acidic lavas, which, as it will be seen by reference to the analyses, carry but a small amount of lime.

Deposition of sinter from the hot waters of the geyser basins depends in a great measure on the amount of silica held in solution, which varies considerably at the different localities and may have varied still more in past time. The silica, as determined by analyses, ranges from .22 to .60 grammes per kilogramme of water, the former being the amount found in the water of the caldron of the Excelsior Geyser and the latter at the Coral Spring in the Norris Basin. Analysis shows that from one-fifth to one-third of the mineral matter held in solution consists of silica, the remaining constituents being readily soluble salts carried off by surface drainage. A few springs highly charged with silica, like the Coral, deposit it on the cooling of the waters; but such springs however are exceptional, and I do not recall a single instance of a spring in the Upper Geyser Basin precipitating silica in this way. At most springs and geysers it results only after evaporation, and not from mere cooling of the water. It seems probable that the nature and amount of alkaline chlorides and carbonates present influence the separation of silica. Temperature also may in some degree influence the deposition. My friend, Mr. Elwood Hofer, one of the best guides to this region and a keen observer of nature, has called my attention to an observation of his made in mid-winter, while on one of his snow-shoe trips through the Park. He noticed that certain overflow pools of spring water, upon being frozen, deposited a considerable amount of mineral matter. He has sent me specimens of this material, which, upon examination, proved to be identical with the silica deposited from the Coral Spring upon the cooling of the water. Demijohns of geyser water which have been standing for one or two years have failed to precipitate any silica. Quite recently, in experimenting upon these waters in the laboratory, it was noticed that on reducing them nearly to the freezing point no change took place, but upon freezing the waters there was an abundant separation of free silica. The waters frozen in this way were collected from the Coral Spring, Norris Basin, and the Taurus Geyser, Shoshone Basin.

Again, there is no doubt that the algous growths found flourishing in the hot waters of the Park favor the secretion of silica and exert an influence in building up the geyserite far greater than one would at first be led to suppose. These low forms of vegetable life occur in nearly all pools, springs, and running waters, up to a temperature of 185° F. (only 13° below the boiling point), at the Upper Geyser Basin. If time permitted, much might be said on this subject. I will only add that Mr. Walter H. Weed, in connection with his other duties on the Geological Survey, has devoted much time to a study of these algous growths, and the results of his investigations will form an important chapter in the final publications.

Several methods have been devised for ascertaining the growth of deposition of the geyserite. One way is by allowing the water to trickle over twigs, dried grasses, or almost anything exposing considerable surface, and noting the amount of incrustation. This way gives the most rapid results, but is far from satisfactory and by no means reproduces the conditions existing in nature. Other methods employed are placing objects on the surface of the water or, still better, partially submerging them in the hot pools, or again by allowing the water to run down an inclined plane with frequent intervals for evaporation and concentration.

The vandals who delight to inscribe their names in public places have invaded the geyser basins in large numbers and left their addresses upon the geyserite in various places. It is interesting to note how quickly these inscriptions become indelible by the deposition of the merest film of silica upon the lead-pencil marks, and, at the same time, how slowly they build up. Names and dates known to be six and eight years old remain perfectly legible, and still retain the color and luster of the graphite. That there is some increase in the thickness of the incrustation is evident, although it grows with incredible slowness. Mr. Weed tells me that he has been able, in at least one instance, to chip off this siliceous film and reproduce the writing with all its original distinctness, showing conclusively that a slow deposition has taken place. Pencil inscriptions upon the siliceous sinter at Rotomahana Lake, in New Zealand, are said to be legible after the lapse of twenty or thirty years. It is easy to see that various ingenious devices might be planned to estimate the rate of deposition, but in my opinion none of them equal a close study of the conditions found in nature, especially where investigations of this kind can be watched from year to year. All observations show an exceedingly slow building up of the geyserite formation. This is well seen in the repair going on where the rims surrounding the hot pools have been broken down, and where it might be supposed that the building-up process was under the most favorable conditions; yet, in a number of instances, I can see no appreciable change in three or four years. Re-visiting hot springs in out-of-the-way places after several years' absence, I am surprised to see that objects that I had noted carefully at the time remain unchanged. Taking the entire area of the Upper Geyser Basin covered by sinter, I believe that the development of the deposit does not exceed one-thirtieth of an inch a year, and this estimate I believe to be much nearer the maximum than the minimum rate of growth. The thickness of the geyserite has never been ascertained; the greatest thickness measured is 70 feet, the depth reached in the conduit of Old Faithful geyser, without meeting any obstruction. Supposing the deposit around the Castle geyser to have been built up with the same slowness as observed to-day, and assuming it to grow at the rate given—one-thirtieth of an inch a year—it would require over twenty-five thousand years to reach its present development. This gives us a great antiquity for the geyserite, but I believe that the deposition of the siliceous sinter in the Park has been going on for a still longer period of time. It is certain that the decomposition of the rhyolite of the plateau dates still further back.

From a geological point of view, there is abundant evidence that thermal energy is gradually becoming extinct. Tourists re-visiting the Park after an absence of two or three years occasionally allude to the springs and geysers as being less active than formerly and as showing indications of rapidly dying out. It is true that slight changes are constantly taking place, that certain springs become extinct or discharge less water, but this action is fully counterbalanced by increased activity in other localities. Close examination of the source of the thermal waters fails to detect any diminution in the supply. Moreover, it stands to reason that if the flow of these waters dated—geologically speaking—far back into the past, the few years embraced within the historical records of the Park would be unable to indicate any perceptible change based upon a gradual diminution of the heat. Accurate descriptions of the region go back only to 1871, the year of the first exploration by Dr. F. V. Hayden.

The number of geysers, hot springs, mud-pots, and paint-pots scattered over the Park exceeds 3,500, and if to these be added the fumaroles and solfataras from which issue in the aggregate enormous volumes of steam and acid and sulphur vapors, the number of active vents would in all probability be doubled. Each one of these vents is a center of decomposition of the acid lavas. In the four principal geyser basins the geysers in action—or known to have been active within the past sixteen years—numbered 84. The following list comprises all the geysers that are known in the Norris, Lower, Midway, and Upper Geyser Basins.

NORRIS GEYSER BASIN.—14.

Arsenic,	Fissure,	Monarch,	Veteran,
Constant,	Growler,	Pearl,	Vixen.
Echinus,	Hurricane,	Pebble,	
Fearless,	Minute,	Schlammkessel,	

LOWER GEYSER BASIN.—17.

Bead,	Great Fountain,	Pink Cone,	Surprise,
Clepsydra,	Impulsive,	Rosette,	White Dome.
Conch,	Jet,	Spasm,	
Fountain,	Mound,	Spray,	
Fitful,	Narcissus,	Steady,	

MIDWAY GEYSER BASIN.—4.

Excelsior,	Flood,	Rabbitt,	Tromp.

UPPER GEYSER BASIN.—49.

Artemesia,	Daisy,	Model,	Splendid,
Bee Hive,	Fan,	Mortar,	Sponge,
Bijou,	Giant,	Oblong,	Spouter,
Bonita,	Giantess,	Old Faithful,	Sprinkler,
Brilliant,	Grand,	Restless,	Sprite,
Bulger,	Grotto,	Riverside,	Tardy,
Cascade,	Infant,	Rocket,	Three Sisters,
Castle,	Jewel,	Saw Mill,	Triplets,
Catfish,	Liberty,	Sentinel,	Turban,
Churn,	Lion,	Shell,	White.
Cliff,	Lioness,	Spasmodic,	
Comet,	Mastiff,	Spanker,	
Cubs,	Midget,	Spiteful,	

A comparative study of the analyses of the fresh rhyolite, the various transition-products, and the thermal waters points clearly to the fact that the solid contents of these waters are derived for the most part from the volcanic rocks of the plateau. During the progress of the work of the Geological Survey in the Yellowstone Park there have been collected from nearly all the more important localities samples of the waters, which have been subjected to searching chemical analyses in the laboratory of the Survey, by Messrs. F. A. Gooch and J. E. Whitfield; the results of whose work will be published at an early date.

The following analyses of hot waters from the three principal geyser basins serve to show their chemical composition:

	Constant Geyser.		Hygeia Spring.		Old Faithful.	
	Grams per kilo of water.	Per cent of total matter in solution.	Grams per kilo of water.	Per cent of total matter in solution.	Grams per kilo of water.	Per cent of total matter in solution.
Silica	0·4685	28·88	0·2477	20·98	0·3828	27·52
Sulph. acid	0·0923	5·69	0·0196	1·65	0·0152	1·09
Carbonic acid	0·0155	0·95	0·2907	24·62	0·0894	6·43
Boracic acid	0·0317	1·95	0·0239	2·02	0·0148	1·07
Arsenious acid	0·0018	0·11	0·0034	0·29	0·0021	0·15
Chlorine	0·5740	35·39	0·2457	21·06	0·4391	31·57
Bromine	Trace	....	Trace	....	0·0034	0·25
Hydr. sulph	None	....	None	....	0·0002	0·01
Oxygen (basic)	0·0048	1·14	0·0504	4·27	0·0419	3·02
Iron	Trace	....	None	....	Trace	....
Manganese	None	....	None	....	Trace	....
Aluminium	0·0185	0·29	0·0036	0·31	0·0009	0·06
Calcium	0·0146	0·90	0·0064	0·54	0·0015	0·11
Magnesium	0·0018	0·11	0·0022	0·19	0·0006	0·04
Potassium	0·0745	4·60	0·0154	1·30	0·0267	1·92
Sodium	0·3190	19·67	0·2654	22·48	0·3666	26·36
Lithium	0·0030	0·19	0·0032	0·27	0·0056	0·40
Ammonium	0·00127	0·08	0·00021	0·02	0·00001	....
Hydr. (HCl)	0·0008	0·05	....	....	....	....
Caesium	....	....	....	....	Trace	....
Rubidium	....	....	....	....	Trace	....
Total	1·62207	100·00	1·18081	100·00	1·39081	100·00

Constant Geyser, Norris Geyser Basin. Date of collection, September 13, 1885; temperature, 198° F.; reaction, slightly acid; specific gravity, 1·0011.

Hygeia Spring, Lower Geyser Basin. Date of collection, September 11, 1885; temperature, 109° F.; reaction, alkaline; specific gravity, 1·0010.

Old Faithful Geyser, Upper Geyser Basin. Date of collection, September 1, 1884; temperature, 190° F.; reaction, alkaline; specific gravity, 1·00096.

They are all siliceous alkaline waters holding the same mineral constituents, but in varying quantities. Silica forms the principal deposit not only immediately around the springs, but over the entire floor of the basins. The carbonates, sulphates, chlorides, and traces of other easily soluble salts are carried off in the waters. Oxides of iron and manganese and occasionally some calcite occur under certain conditions in the cauldrons of the hot springs or immediately around their vents. Concentrations from large quantities of these waters fail to show the presence of even a trace of copper, silver, tin or other metal. Nearly all the waters carry arsenic, the amount present, according to Messrs. Gooch and Whitfield, varying from .02 to .25 per cent of the mineral matter in solution.

Among the incrustations found at several of the hot springs and geysers is a leek-green amorphous mineral, which proves on investigation to be scorodite, a hydrous arseniate of iron. The best occurrence observed is at Joseph's Coat Springs, on the east side of the Grand Cañon of the Yellowstone, where it occurs as a coating upon the siliceous sinter lining the cauldron of a boiling spring. Analysis shows a nearly pure scorodite, agreeing closely with the theoretical composition:

Ferric oxide	34.94
Arsenic acid	48.79
Water	16.27
	100.00

Alteration of the scorodite into limonite takes place readily, which in turn undergoes disintegration by the wearing of the water, and is mechanically carried away. So far as I know this is the only occurrence where scorodite has been recognized as deposited from the waters of thermal springs. Although pure scorodite is only sparingly preserved at a few localities in the Yellowstone Park, it is easily recognized by its characteristic green color, in strong contrast with the white geyserite and yellow and red oxides of iron. After a little practice the mineral green of scorodite is not easily mistaken for the vegetable green of the algeous growths. The latter is associated everywhere with the hot waters, while the former, an exceedingly rare mineral, is obtained only in small quantities after diligent search. In America traces of arsenic have been reported from several springs in Virginia, and quite recently sodium arseniate has been detected in the hot springs of Ashe County, N. C. Arsenical waters of sufficient strength to be beneficial for remedial purposes and not otherwise deleterious are of rare occurrence. In France the curative properties of arsenical waters have long been recognized, and the famous sanitarium of La Bourboule in the volcanic district of the Auvergne has achieved a wide reputation for the efficacy of its waters in certain forms of nervous diseases. Hygeia Springs, supplying the bath houses at the hotel in the Lower Geyser Basin, carries .3 of a grain of sodium arseniate to the gallon. The Yellowstone Park waters, while they carry somewhat less arsenic than those of La Bourboule, greatly excel the latter in their enormous overflow. It is stated that the entire discharge from the springs of La Bourboule, amounts to 1,500 gallons per minute. The amount of hot water brought to the surface by the hot springs throughout the park is by no means easily determined, although during the progress of our investigations we hope to make an approximate estimate. Some idea of the amount of hot water brought to the surface and carried off by the great drainage channels may be formed at the Midway Basin. According to the most accurate measurements which could be made, the discharge from the caldron of the Excelsior Geyser into the Firehole River during the past season amounted to 4,400 gallons of boiling water per minute, and there is no evidence that this amount has varied within the last two or three years. The sample of the Excelsior Geyser water collected August 25, 1884, yielded .19 grains of sodium arseniate to the gallon. It is impossible to say as yet what curative properties these park waters may possess in alleviating the ills of mankind. Nothing but an extended experience under proper medical supervision can determine. I may say that no hot springs with which I am acquainted prove so delightful for bathing purposes and so agreeable in their action upon the skin.

Changes modifying the surface features of the park in recent times are mainly those brought about by the filling up with detrital material from the mountains, the valleys and depressions worn out by glacial ice, and those produced by the prevailing climatic conditions. Between the park country and what is known as the arid regions of the West there is the greatest possible contrast. Across the park plateau and the Absaroka range the country presents a continuous mountain mass 75 miles in width, with an average elevation unsurpassed by any area of equal extent in the northern Rocky Mountains. It is exceptionally situated to collect the moisture-laden clouds, which coming from the southwest precipitate immense quantities of snow and rain upon the cooled table-land and neighboring mountains. The climate in many respects is quite unlike that found in the adjacent country, as is shown by the meteorological records, the amount of snow and rainfall being higher, and the mean annual temperature lower. Rain storms occur frequently throughout the summer, while snow is quite likely to fall any time between September and May. Protected by the forests the deep snows of winter lie upon the plateau well into mid-summer, while at still higher altitudes, in sheltered places, it remains throughout the year. By its topographical structure the park is designed by nature as a reservoir for receiving, storing, and distributing an exceptional water supply, unexcelled by any area near the head-waters of the great continental rivers. The Continental Divide, separating the waters of the Atlantic from those of the Pacific, crosses the park plateau from southeast to northwest. On both sides of this divide lie several large bodies of water which form so marked a future in the scenery of the plateau that the region has been designated the lake country of the park. Yellowstone Lake, the largest lake in North America at this altitude (7,740 feet) and one of the largest in the world at so high an elevation above sea-level, presents a superficial area of 139 square miles, and a shore-line of nearly 100 miles. From measurements made near the outlet of the lake in September, 1886, the driest period of the year, the discharge was found to be 1,525 cubic feet per second, or about 34,000,000 imperial gallons per hour.

At the same time all the principal lakes and streams in the park were carefully gauged. Dr. William Hallock, who undertook this work, estimated that the amount of water running into the park and leaving it by the Yellowstone, Gallatin, Madison, Snake, and Falls rivers, the five main drainage channels, would be equivalent to a stream 5 feet deep, 190 feet wide, with a current of 3 miles per hour, and that over an area of 4,000 square miles the minimum discharge was equal to 1 cubic foot per second per square mile. For the preservation and regulation of this water supply the forest, which covers the mountains, valleys, and table-lands, and everywhere borders upon the lake shores, is of inestimable value. Of the present park area about 84 per cent is forest clad, almost wholly made up of coniferous trees. The timber is by no means of the finest quality, but for the purpose of water protection it meets every possible requirement. Much has been said of late years by scientific and experienced persons of the great necessity of preserving the forests near the sources of our great rivers. It is mainly for the forest protection that the proposed enlargement is demanded by the public welfare. In my opinion no region in the Rocky Mountains is so admirably adapted for a forest reservation as the Yellowstone National Park.

SOAPING GEYSERS.*

By Arnold Hague,

U. S. Geological Survey.

At the Buffalo meeting, October, 1888, Dr. Raymond presented a paper entitled "Soaping Geysers," in which he called attention to the use of soap by tourists to cause eruptions of several of the well-known geysers in the Yellowstone Park. Incorporated in this paper appears a communication received from me, written from camp in the park, in reply to some inquiries on the subject. The letter discussed somewhat briefly the means employed by visitors to the park to hasten the eruptions from hot springs and reservoirs of hot water, which remain dormant for days, or even weeks or months, at a temperature near the boiling-point, without any display of geyser-action. As the paper has called forth considerable comment, I desire to elucidate one or two points in relation to the temperature of the springs, and to answer some inquiries about the composition of the thermal waters.

In the summer of 1885, a Chinaman, employed as a laundryman for the accommodation of the tourists at the Upper Geyser Basin, accidentally discovered, much to his amazement, that soap thrown into the spring from which he was accustomed to draw his supply of water produced an eruption in every way similar to the actual workings of a geyser. Tourists, with limited time at their command, who had travelled thousands of miles to look upon the wonders of the Yellowstone, soon fell into the way of coaxing the laundryman's spring into action, to partly compensate them for their sore disappointment in witnessing only the periodical eruptions of Old Faithful. Successful attempts upon this spring soon led to various endeavors to accelerate action in the dormant and more famous geysers. In a short time, so popular became the desire to stimulate geysers in this way, that the park authorities were compelled to enforce rigidly the rule against throwing objects of any kind into the springs.

In connection with a thorough investigation of the thermal waters of the Yellowstone Park and the phenomena of the geyers, I under-

* Read at New York meeting of the American Institute of Mining Engineers, February, 1889. (From *Trans. Am. Inst. Mining Engineers.*)

took a number of experiments to ascertain the action of soap upon the waters and to determine, if possible, those physical conditions of various pools and reservoirs which permitted the hastening of an eruption by the employment of any artificial methods. This investigation, conducted from time to time, as opportunity offered, throughout the field-season of 1885, included experiments upon the geysers and hot springs of the Upper, Lower, and Norris geyser basins. The results proved, beyond all question, that geyser-action could be forced in a number of ways, but most conveniently by the application of soap. The greater part of the more powerful geysers undergo no perceptible change with a moderate use of soap, although several of them may, under favorable physical conditions, be thrown at times into violent agitation. In most of the experiments, Lewis's concentrated lye, put up in one-half pound cans for laundry purposes, was employed. Each package furnished a strong alkali, equivalent to several bars of soap. In this form alkali is more easily handled than in bars of soap, more especially where it is required to produce a viscous fluid in the larger reservoirs; and, in conducting a series of experiments for comparative purposes, it seemed best, in most instances, to employ the same agent to bring about the desired results.

Old Faithful, the model geyser of the park, exhibits such marked regularity in its workings that attempts to hasten its action appear futile. The interval between eruptions is about sixty-five minutes, and rarely exceeds the extreme limits of fifty-seven and seventy-two minutes. After an eruption of Old Faithful, the reservoir fills up gradually; the water steadily increases in temperature; and conditions favorable to another eruption are produced under circumstances precisely similar to those which have brought about the displays for the past eighteen years, or as far back as we have authentic records. The few experiments which have been made upon Old Faithful are insufficient to afford any results bearing on the question; but it seems probable that soon after the water attains the necessary temperature an eruption takes place.

Of all the powerful geysers in the park, the Bee-Hive offers the most favorable conditions for producing an eruption by artificial means, all the more striking because the natural displays are so fitful that they can not be predicted with any degree of certainty. Observations, extending over a period of several years, have failed to determine any established law of periodicity for the Bee-Hive, even for three or four consecutive months, although they indicate that some relationship may exist between its display and those of the famous Giantess. Frequently the Bee-Hive will play several times a day and then become dormant, showing no signs of activity for weeks and months, although the water may stand above the boiling-point the greater part of the time. The name Bee-Hive was suggested by the symmetry of the cone built around the vent. It rises about 4 feet above the sloping mound of geyserite, and in cross-section measures about 3 feet at the top, while at the bottom of the cone the vent is less than 10 inches in width. From the top of this narrow vent it is only possible to sink a weight 17 feet before striking a projecting ledge, which interferes with all examination of the ground below. The constant boiling and bubbling of the water, the irregularity of its action, and the convenient location of the geyser, within an easy walk from the hotel, make attempts to accelerate the eruptions of the Bee-Hive most attractive to tourists.

In most instances such efforts are futile; yet success does so frequently reward the astonished traveller that, unless the geyser were carefully watched by the authorities, attempts would be made daily throughout the season. If the conditions are favorable to an eruption, it usually takes place in from ten to twenty-five minutes after the addition of laundry soap or lye. It is doubtful if more than two eruptions of the Bee-Hive have ever been produced on the same day by artificial means, although I know of no reason, based upon the structure of the geyser, why more displays might not be obtained, for the reservoir and vent fill up with boiling water very rapidly after each eruption.

Although the Giantess is situated only 400 feet from the Bee-Hive, these two differ in surface and under-ground structure and mode of action as widely as any two of the more prominent geysers of the Park. Around the Giantess no cone or mound has formed. The broad basin is only partially rimmed in by a narrow fringe of siliceous sinter, rising above and extending out over the deep blue water. At the surface this basin measures about 15 to 20 feet in width by 20 to 30 feet in length. It has a funnel-shaped caldron, 30 feet in depth, ending in a vertical vent or neck, 12 feet deep, through which a sounding-lead may be dropped into a second reservoir, meeting a projecting ledge or obstruction of some kind, 61 feet below the surface. After an outburst of the Giantess, the basin, which has been completely emptied of its water, gradually fills again to the top; and, for days before another eruption, a steady stream of hot water overflows the brim. The intervals between the eruptions of the Giantess vary from twelve to twenty days, and the displays last several hours, being unsurpassed for violence and grandeur by any geyser in the Upper Basin. Artificial means have never been successful in bringing this geyser into action, although, for days before an eruption, it is an easy matter to cause an agitation of the water by throwing into the basin small pieces of sinter, or to produce a boiling on the surface, lasting several minutes, by simply stirring the water with a stick.

The Giant, one of the most violent of the geysers in the Upper Basin, more closely resembles the Bee-Hive than any other of those along the Firehole River. It has built up a cone 10 feet in height, one side of which has been partly broken down by some eruption more violent than any witnessed at the present day. Through this notched side, steam and broken jets of water are constantly emitted; and on this account but little examination has been made of the underground reservoirs and vents. The Giant is fitful in its action, at times playing with considerable regularity every fourteen days, and at other times lying dormant for nearly a year. I have no positive knowledge that an eruption of the Giant has ever been produced by any other than natural causes. At the time of my experiments no eruption of the Giant had taken place for several months, although the water was constantly agitated, so much so that it was quite impossible to examine the vent with any satisfactory results. The only effect produced by the application of lye was additional height to the column of water thrown out and a decided increase in the thumping and violence of the boiling.

In the Lower Basin, the Fountain has been more carefully studied than the other geysers; and, its action and periodicity of eruptions having been fairly well ascertained, it afforded the most favorable conditions for observing the action of soap and lye upon the waters. In its general structure the Fountain belongs to the type of the Giantess, having a funnel-shaped caldron which, long before an eruption, overflows into an adjoining basin. At the time of my experiments upon the Fountain, the intervals between eruptions lasted about four hours. This interval allowed sufficient time to note any changes which might take place. My own experiments with lye yielded no positive results; although it seemed highly probable that action might be hastened by the application of soap or lye just before the time for an eruption, or when, for some cause, the eruption was overdue. I preferred to make the attempt to bring about an explosion before the usual time, only waiting until the water in the pool had nearly reached the boiling-point. All experiments failed. The previous year, when wishing to produce action for the purpose of photography, I was enabled to accomplish the desired result by vigorously stirring with a slender pole, the water near the top of the vent connecting with the lower reservoir. In this instance, it should be said, the usual interval of time between eruptions had long since passed; the geyser was, so far as time was concerned, a half-hour overdue. My opinion now is that the experiments with lye failed because the temperature had scarcely reached the boiling-point.

The Monarch, in the Norris Basin, is quite unlike those already described, and affords evidence of being a much newer geyser. It is formed by two convergent fissures, on the line of a narrow seam in the rhyolite, probably coming together below the surface. The main vent measures about 20 feet in length and, at the surface, 3 feet in width. But slight incrustation is found around the vent, the conditions not being favorable to deposition. In this narrow fissure the water, which ordinarily stands about 15 feet below the surface, constantly surges and boils, except immediately after an eruption. The intervals between eruptions vary somewhat from year to year; but at the time of these experiments the action was fairly regular, the geyser playing every four hours. I was successful in obtaining an eruption quite equal to the natural displays, which throw a column of water 50 feet into the air. Here at the Monarch there is no surface reservoir, and the narrow fissure, filled with loose blocks of rocks around which the water is in constant agitation, prevents all measurements of depth.

The results of the many experiments, not only upon active geysers but upon a large number of hot springs, determine fairly well the essential conditions which render it possible to bring about geyser action by artificial means. Negative results are frequently as valuable for this inquiry as experiments yielding imposing displays.

Outside of a few exceptional instances, which could not be repeated, and in which action was probably only anticipated by a few minutes in time, geyser eruptions produced by soap or alkali appear to demand two essential requirements: First, the surface caldron or reservoir should hold but a small amount of water, exposing only a limited area to the atmosphere; second, the water should stand at or above the boiling point of water for the altitude of the geyser basin above sea level. The principal factor which makes it possible to cause an eruption artificially is, I think, the super-heated and unstable condition of the surface waters. Many of the geysers and hot springs present singular phenomena of pools of water heated above the theoretical boiling point, and, unless disturbed, frequently remain so for many days without exhibiting any signs of ebullition. It may not be easy to describe accurately these super-heated waters; but any one who has studied the hot springs and pools in the Park and carefully noted the temperatures, quickly learns to recognize the peculiar appearance of these basins when heated above the boiling point. They look as if they were "ready to boil," except that the surface remains placid, only interrupted by numerous steam-bubbles, rising through the water from below and bursting quietly upon reaching the surface.

Marcet, the French physicist, has specially investigated the phenomena of super-heated waters, and has succeeded in attaining a temperature of 105° C. before ebullition. Super-heated waters in nature, however, appear to have been scarcely recognized, except during the progress of the work in the Yellowstone Park, in connection with a study of the geysers. The altitudes of the geyser basins above sea-level have been ascertained by long series of barometric readings, continued through several seasons. In conducting a series of observations upon the boiling points of the thermal waters in the Park, Dr. William Hallock, who had charge of this special investigation, determined the theoretical boiling-point by noting the mean daily readings of the mercurial column. The exact boiling-point of a pure surface-water, obtained from a neighboring mountain stream and the boiling-point of the thermal waters from the springs, were determined from actual experiments by heating over a fire, employing every possible precaution to avoid sources of error. Surface waters and deep-seated mineral waters gave the same results, and coincided with the calculated boiling-point at this altitude. Hundreds of observations have been carefully taken where the waters in the active and running springs boiled at temperatures between 198° and 199° F.

As will be shown later in this paper, the thermal waters are solutions of mineral matter too dilute to be affected to any appreciable extent as regards their boiling-point by their dissolved contents. The theoretical boiling-point for the springs and pools in the Upper Geyser Basin may be taken at 92.5° C. (198.5 F.). In many of the large caldrons, where the water remains quiet, a temperature has been recorded of 94° C. (201.2° F.) without the usual phenomena of boiling. This gives a body of super-heated water, with a temperature at the surface 1.5° C. (2.7° F.) above the point necessary to produce explosive action. Thermometers plunged into the basins show slightly varying temperatures, dependent upon their position in the basin. They indicate the existence of numerous currents, and a very unstable equilibrium of the heated waters, which are liable, under slight changes, to burst forth with more or less violence. It is under these conditions that geyser action can be accelerated by artificial means. If into one of these super-heated basins a handful of sinter pebbles be thrown, or the surface of the water be agitated by the rapid motion of a stick or cane, or even by lashing with a rope, a liberation of steam ensues. This is liable to be followed by a long boiling of water in the pool, which in turn may lead to geyser action. There is some reason to believe that, at least in one instance, an eruption has been brought about by a violent but temporary gust of wind, which either ruffled the water or disturbed the equilibrium of the pool, and changed momentarily the atmospheric pressure.

In Iceland travellers have long been accustomed to throw into the geysers turf and soft earth from the bogs and meadows which abound in the neighborhood, the effect produced being much the same as that of sinter pebbles and gravel upon the geysers in the National Park. So well was this understood that at one time a peasant living near the Iceland locality kept a shovel solely for the accommodation of those visiting the geysers.

In my letter to Dr. Raymond I mention the curious fact that the Laundryman's Spring, now known as the Chinaman, in which geyser action may most easily be produced by artificial means, has never been regarded by the Geological Survey as anything but a hot spring, and no one has ever seen it in action without the application of soap, except in one instance, when it was made to play to a height of 20 feet after stirring it vigorously with a pine bough for nearly ten minutes. In our records it is simply known as a spring.

If soap or lye is thrown into most of the small pools, a viscous fluid is formed; and viscosity is, I think, the principal cause in hastening geyser action. Viscosity must tend to the retention of steam within the basin, and, as in the case of the super-heated waters, where the temperature stands at or above the boiling point, explosive liberation must follow. All alkaline solutions, whether in the laboratory or in nature, exhibit, by reason of this viscosity, a tendency to bump and boil irregularly. Viscosity in these hot springs must also tend to the formation of bubbles and foam when the steam rises to the surface, and this in turn aids to bring about the explosive action, followed by a relief of pressure, and thus to hasten the final and more powerful display. Of course relief of pressure of the superincumbent waters upon the column of water below the surface basin is essential to all eruptive action. These conditions, it seems to me, are purely physical. Undoubtedly the fatty substances contained in soap aid the alkali in rendering the water viscous. On the other hand, when concentrated lye is used it acts with greater energy and furnishes a viscous fluid, where soap would yield only surface suds insufficient to accomplish any phenomenal display.

It is well known that saturated solutions of mineral substances raise the boiling-point very considerably, the temperature having been determined for many of the alkaline salts. In general, I believe the boiling-point increases in proportion to the amount of salt held in solution. Actual tests have shown that the normal boiling-point of siliceous waters in the Park does not differ appreciably from the ordinary surface waters, mainly, I suppose, because they are extremely dilute solutions.

The amount of lye required to produce a sufficiently viscous condition of the waters increases but slightly the percentage of mineral matter held in solution.

All the waters of the principal geyser basins present the closest resemblance in chemical composition, and, for the purposes of this paper, may be considered as identical in their constituents. They have a common origin, being, for the most part, surface waters which have percolated downward for a sufficient distance to come in contact with large volumes of steam ascending from still greater depths. The mineral contents of the hot springs are mainly derived from the acid lavas of the Park plateau, as the result of the action of the ascending steam and super-heated waters upon the rocks below. These thermal waters are essentially siliceous alkaline water, carrying the same constituents in somewhat varying quantities, but always dilute solutions, never exceeding two grams of mineral matter per kilogram of water. When cold they are potable waters, for the most part slightly alkaline to the taste, and probably wholesome enough, unless taken daily for a long period of time.

The following analyses of three geyser waters, selected from the

Upper, Lower, and Norris geyser basins, may serve to show the composition of all of them, the differences which exist being equally well marked in the analyses of any two waters from the same geyser basin.

	Bee-Hive Geyser.		Fountain Geyser.		Fearless Geyser.	
	Grams per kilo of water.	Per cent of total matter in solution.	Grams per kilo of water.	Per cent of total matter in solution.	Grams per kilo of water.	Per cent of total matter in solution.
Silica	0.3042	25.12	0.3315	23.69	0.4180	25.60
Sulphuric acid	.0271	2.24	.0195	1.39	.0367	2.25
Carbonic acid	.0920	7.60	.2307	16.48	.0046	.28
Phosphoric acid	....	....	.00004	....	....	....
Boracic acid	.0145	1.20	.0138	.99	.0223	1.36
Arsenious acid	.0011	.09	.0027	.19	.0022	.14
Chlorine	.3894	32.15	.3337	23.84	.6705	41.06
Bromine	Trace	....	.0004	.03	.0026	.16
Iodine	....	....	....	....	....	....
Fluorine	....	....	....	....	....	....
Hydr. sulph	....	....	Trace	....	Trace	....
Oxygen (basic)	.0364	3.00	.0654	4.67	.0113	.70
Iron	Trace	....	.0002	.01	.0006	.04
Manganese	....	....	Trace	....	....	....
Aluminium	.0029	.24	.0057	.41	.0002	.01
Calcium	.0039	.32	.0014	.10	.0092	.56
Magnesium	.0002	.02	.0010	.07	.0001	.01
Potassium	.0213	1.76	.0379	2.71	.0415	2.54
Sodium	.3118	25.74	.3522	25.16	.4046	24.77
Lithium	.0061	.50	.0035	.25	.0081	.50
Ammonium	.00021	.02	.00015	.01	.00025	.02
Cæsium	....	....	....	....	Trace	....
Rubidium	....	....	....	....	Trace	....
	1.21111	100.00	1.39979	100.00	1.63275	100.00

Bee Hive Geyser, Upper Geyser Basin. Date of collection September 1, 1884; temperature 199.4° F.; reaction, alkaline; specific gravity, 1.0009.

Fountain Geyser, Lower Geyser Basin. Date of collection, August 24, 1884; temperature 179.6° F.; reaction, alkaline; specific gravity, 1.0010.

Fearless Geyser, Norris Geyser Basin. Date of collection, August 18, 1884; temperature 190.4° F.; reaction neutral, specific gravity, 1.0011.

The differences of temperature shown in these three waters are simply due to the varying interval between the time of collection and the last preceding eruption of the geyser. In the case of the Fountain, the water rises in a large open basin, which slowly fills up, increasing in temperature until the time of the eruption, the form of the basin permitting the collection of the water two or three hours before the next outburst. In the case of the Fearless the surface reservoir is a shallow, saucer-shaped basin, into which the water seldom rises before attaining a temperature near the boiling-point. At the Bee-Hive the water only reaches a sufficiently high level to permit of its collection without difficulty when the temperature stands at or near the boiling-point.

Dr. Raymond has made the suggestion that the addition of caustic alkali would possibly precipitate some of the mineral ingredients found in these waters, thereby changing their chemical composition sufficiently to affect the point of ebullition. At the same time he remarks that the geyser waters are probably too dilute solutions to be much influenced by such additions. Anyone who glances at the analyses of the waters of the Bee Hive, Fountain, and Fearless must see, I think, that they are not only too dilute to undergo any marked change of temperature, but that the mineral constituents consist mainly of the carbonates and chlorides of the alkalis, associated with a relatively large amount of free silica which would remain unacted upon by caustic alkali. There is nothing in the waters to be thrown down by the addition of alkali or permit any chemical combinations to be formed by the addition of a small amount of soap. The desire of tourists to "soap a geyser" during their trip through the Park grows annually with the increase of travel, so much so that there is a steady demand for the toilet soap of the hotels. If visitors could have their way, the beautiful blue springs and basins of the geysers would be "in the suds" constantly throughout the season. Throwing anything into the hot springs is now prohibited by the Government authorities. It is certainly detrimental to the preservation of the geysers, and the practice can not be too strongly condemned by all interested in the National Reservation.

CONTINENTAL PROBLEMS OF GEOLOGY.*

BY G. K. GILBERT.

Introduction.—For a decade attention has been turned to the continents. Through the distribution of animals and plants Wallace has studied the history of the former connection and disconnection of land areas. Theories of interchange of land and water have been propounded by Suess and Blytt. By means of geodetic data Helmert has discussed the broad relations of the geoid to the theoretic spheroid. Darwin has computed the strength of terrestrial material necessary to sustain the continental domes. James Geikie, treating nominally of coast lines, has considered the shifting relations of land and sea, and a half score of able writers have debated the question of continental permanence. The American Society of Naturalists, now holding its annual meeting at Princeton, N. J., devoted yesterday's session to the consideration of such evidences of change in the geography of the American continent as are contained in the distribution of animals and plants. The intercontinental congresses auxiliary to the World's Fair next summer are to be devoted to the discussion of continental and inter-continental themes; and a committee, at the head of which stands one of our vice-presidents, invites the geologists of the world to assemble for the consideration of those broader questions of earth structure and earth history which affect more than one hemisphere. This occasion, too, in which, after three years' sojourn in the land of the raccoon and the opossum, we return to the land of the sable and the beaver, brings forcibly to mind the continental extent of our society and its continental field. It is not strange, then, that the continents have seemed to me a fitting theme of which to speak to you to-day. Realizing not only the breadth and grandeur, but the inherent difficulty of the subject, I do not hope to enlarge the contribution the decade has made, nor shall I attempt to summarize it; neither is it my desire to anticipate the discussions of the World's Fair congress. It is my purpose rather to state, as clearly as I may, some of the great unsolved problems which the continents propound to the coming inter-continental congress of geologists.

*Presidential address before the Geological Society of America; delivered December 30, 1892. (From the *Bulletin Geolog. Soc. of Am.*, vol. IV, pp. 179–190.)

Differentiation of continental and oceanic plateaus.—It is one of the paradoxes of the subject that our ideas as to the essential character of the continents have been greatly modified and clarified by the recent exploration of the sea. The work, especially, of the *Challenger* and the *Blake* in delineating and sampling the bottom of the ocean has given new definitions, not only to the term "deep sea," but also to the term "continent," as they are employed by students of terrestrial mechanics and of physical geography. To the continental lands are now added the continental shoals, and the *depth* of the deep sea is no longer its sole characteristic. Look for a moment at this generalized profile of the earth's surface. It expresses in a concise way the relations of area to altitude and of both to the level of the sea. Murray, to whose generalizations from the *Challenger* dredgings and soundings the student of continents owes so much, has computed, with the aid of the great body of modern data, the areas of land and ocean bed contained between certain contours, fourteen in number,* and from his

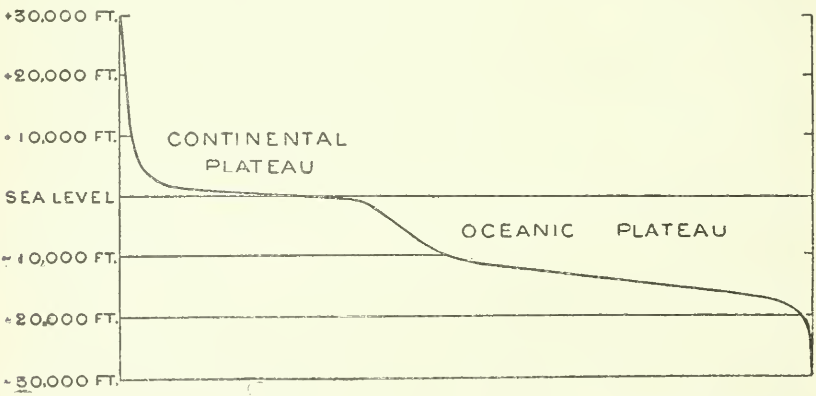


FIGURE 1.—*Generalized Profile, showing relative Areas of the Earth's Surface at different Heights and Depths.*

figures I have constructed the profile. Vertical distances represent heights and horizontal distances represent terrestrial areas. The full width of the diagram from side to side stands for the entire surface of the earth. The striking features of the profile are its two terraces or horizontal elements. Two-fifths of the earth's area lies between 11,000 and 16,000 feet beneath the ocean, constituting a vast submerged plateau, whose mean altitude is −14,000 feet. This is the plateau of the deep sea. One-fourth of the earth's area falls between the contour 5,000 feet above the ocean and the contour 1,000 feet below, and has a mean altitude of +1,000 feet. This is the continental plateau. The two plateaus together comprise two-thirds of the earth's surface, the remaining third including the intermediate slopes, the areas of extreme and exceptional depth, and the areas of extreme and exceptional height.

* John Murray: "On the height of the land and the depth of the ocean." *Scottish Geographical Mag.*, vol. IV, 1888, p. 1.

Thus in the broadest possible way, and in a manner practically independent of the distribution of land and water, we have the ocean floor clearly differentiated from the continental plateau. It is at once evident that for the discussion of the greater terrestrial problems connected with the configuration of the surface, and especially of the problems of terrestrial mechanics, we must substitute for the continents, as limited by coasts, the continental plateau, as limited by the margins of the continental shoals.

It does not follow from the profile, which, as I have said, represents only the relation of extent to altitude, that all districts of continental plateau are united in a single body, and in point of fact they are not completely united; but the greater bodies are brought together, and the only outlying district is that of the Antarctic continent. Running a line along the edge of the continental shelf where a gentle slope is exchanged for a steep one, and passing freely, as occasion may require, from the coast down to the line of 1,000 fathoms, a continental outline is produced in which North America and Eurasia are united through the shoals of the Arctic ocean, and in which Australia and the greater islands of the East Indies are joined to southwestern Asia. Antarctica alone stands separate, being parted from South America by a broad ocean channel, imperfectly surveyed as yet, but believed to have a depth of between 1,000 and 2,000 fathoms. The lower plateau, or the floor of the deep ocean, is less continuous, being separated by tracts of moderate depth into three great bodies, coinciding approximately with the Pacific, Atlantic, and Indian oceans.

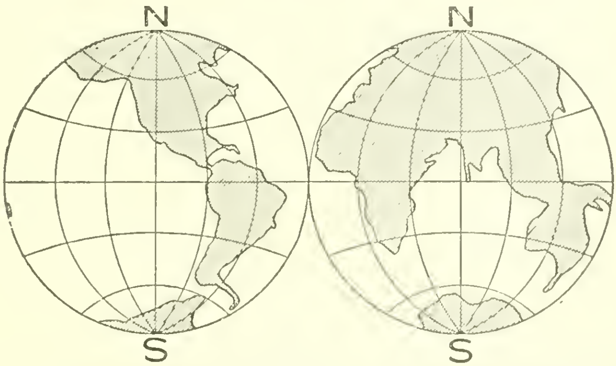


FIGURE 2.—*The continental plateau as related to the Western and Eastern Hemispheres.*

Rigidity versus isostasy.—The first of our continental problems refers to the conditions under which the differentiation of the earth's surface into oceanic and continental plateaus is possible. How are the continents supported? Every part of the oceanic plateau sustains the weight of the superjacent column of water. At the same level beneath the continental plateau each unit of the lithosphere sustains a column of rock both taller and denser than the column of water and weighing about three times as much. The difference between the two pressures, or the differential pressure, is about 12,000 pounds to the square inch, and this force, applied to the entire area of the continental plateau, urges it downward and urges the oceanic plateau upward. Referring again to the diagram in Figure 1, the entire weight of the continental plateau, pressing on the track beneath it, tends to produce a transfer of material in the direction from left to right, resulting in the lowering of the higher plateau and the raising of the lower. To the question, how this tendency is counteracted, two general answers have been made: first, that the earth, being solid, by its rigidity maintains its form; second, that the materials of which consist the continental plateau and the underlying portions of the lithosphere are. on the whole, lighter than the materials underlying the ocean floor, and that the difference in density is the complement of the difference in volume, so that at some level horizon far below the surface the weights of the superincumbent columns of matter are equal. The first answer regards the horizontal variations of density in the earth's crust as unimportant; the second regards them as important. The first may be called the doctrine of terrestrial rigidity; the second has been called the doctrine of isostasy. At the present time the weight of opinion and, in my judgment, the weight of evidence lie with the doctrine of isostacy. The differential pressure of 12,000 pounds per square inch suffices to crush nearly all rocks, and it may fairly be questioned whether there are any rock masses which in their natural condition near the surface of the earth are able to resist it. The samples of rock to which the pressures of the testing machine are applied have been indurated by drying; but it is a fact familiar to quarrymen that rocks in general are softer as they lie in the quarry below the water-line than after they have been exposed to the air and thoroughly dried. It is probable therefore that rocks lying within a few hundred or a few thousand feet of the surface are unable to resist such stresses as are imposed by continents. At greater depths we pass beyond the range of conditions which we can reproduce in our laboratories, and our inferences as to physical conditions are less confident. The tendency of subterranean high temperatures is surely to soften all rocks, and the tendency of subterranean high pressures is probably to harden them. It is not known which tendency dominates; but if the tendencies due to pressure are the more powerful, we are at least assured by the phenomena of volcanism that their supremacy admits of local exception.

Nature of density differences.—If we accept the doctrine of isostasy and regard the material under the continents as less dense than that under the ocean floors, the question then arises whether the difference in density is due merely to a difference in temperature or whether it arises primarily from differences in composition. This, which may be called the second problem of the continents, is so intimately related to the one which follows that we may pass it by without fuller statement.

What caused the continental plateau?—The problem of the origin of the continents remains almost untouched. Those who have propounded theories for the formation of mountain ranges have sometimes included continents also, but as a rule without adequate adaptation to the special conditions of the continental problem. So far as I am aware, the subject has been seriously attacked only by our second president, Prof. Dana. He postulates a globe with solid nucleus and molten exterior, and postulates, further, local differences of condition, in consequence of which the formation of solid crust on the liquid envelope was for a long period confined to certain districts. In those districts successive crusts were formed, which sunk through the liquid envelope to the solid nucleus and by their accumulation built up the continental masses. The remaining areas were afterward consolidated, and subsequent cooling shrunk the ocean beds more than it shrunk the continental masses, because their initial temperatures (at the beginning of that process) were higher.* That the philosophic mind may find satisfaction in this explanation, it appears necessary to go behind the second postulate and discover what were the conditions which determined congelation in certain districts long before it began in others. Can it be shown that the localization of congelation, having been initiated by an otherwise unimportant inequality, would be perpetuated by any of those cumulative processes which are of such importance in various departments of physics? And can it be shown that such a process of continent-building would segregate in the continental tract certain kinds of matter and thus institute the conditions essential to isostatic equilibrium? To the first of these questions no answer is apparent, but I incline to the opinion that the second may be answered in the affirmative. If we assume the liquid envelope to consist of various molten rocks arranged in the order of their densities and if we assume, further, that their order of densities in the liquid condition corresponds to their order of densities in the solid condition, then the successive crusts whose heaping built up the continents would all be formed from the lightest material, and the isostatic condition would be satisfied.

It was the fashion of the last generation of physical geographers to study the forms of continents as delimited by coasts, seeking analogies of continental forms with one another and also with various geometric figures, especially the triangle. The generalizations resulting from these studies have not yielded valuable ideas, and the modern student is apt to smile at the effort of his predecessor to discover the ideal geometric figure where the unbiased eye sees only irregularity. But barren as were those studies I am not satisfied that their method was faulty, and as a physiographer I have such appreciation of the ideas that sometimes grow from studies of form that I have attempted to apply the old method to the new conception of the continental plateau. Con-

*James D. Dana: *Manual of Geology*, 2d edition, New York, 1874, p. 738.

fessing in advance that my only result has been negative, I nevertheless recite what I have done, partly because negative contributions to an obscure subject are not entirely valueless and partly with the thought that the forms whose meanings I failed to discover may nevertheless prove significant to some other eyes.

What I did was to draw upon a globe the outline of the continental plateau and then view it from every direction. Afterwards I developed the figure upon a plane surface, employing for that purpose a mode of projection which is probably novel. As this mode is not susceptible of mathematical formulation, and therefore will not find place in the literature of cartography, I may be pardoned for applying a trivial name and calling it the *orange-peel* projection. The name almost explains it. Conceive the continental plateau to be outlined upon a spherical orange and the rind of the orange to be divided by a sharp knife along the sinuosities of the outline; conceive then that the portion of the rind

FIGURE 3.– *The continental plateau developed on a plane surface.*

thus circumscribed is peeled from the orange and is spread upon a flat surface, the different parts being stretched and compressed so as to pass

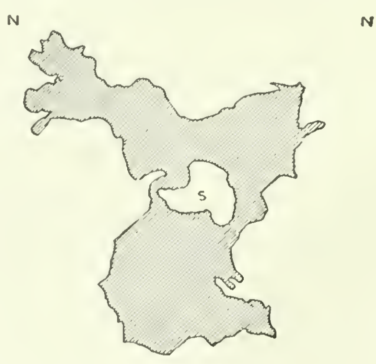


FIGURE 4.—*Oceanic area complementary to the continental plateau developed on a plane surface.*

from spherical form to plane with the least strain of the rind. The resulting shape is delineated in Figure 3. Figure 4 shows the form assumed by the complementary part of the orange peel, which represents, of course, that portion of the ocean outside the continental shoals. In each diagram the positions of the poles north and south are represented by the letters N and S. From the study of these figures, and especially from their study as delineated on the globe, it appeared possible that a portion of the continental plateau might belt the earth as a great circle. The discovery of such a belt would be important, for by assuming that it was originally equatorial we might be led to new hypotheses of continental development. In a rotating liquid sphere the only differentiation of surface condition we can readily conceive is that between equatorial and polar regions, and if such differentiation were sufficient to cause or localize continental elevations, then these elevations would constitute either two polar tracts or else an equatorial belt. Moreover, I have been induced by recent studies of the physical history of the moon to suspect that the earth may at one time have received considerable accessions from without and that these accessions were made to the equatorial tract. If these suspicions are well founded, peculiar characters may have been given to a tract having the form of a belt. So for a double reason I was led to compare the outline of the continental plateau with a great circle. To this end a great circle was chosen, coinciding as nearly as possible with the line of greatest continental extension, and the projection was so modified as to render the

FIGURE 5.—*Area of continental plateau, developed with reference to a great circle.*

locus of that great circle a straight line. The result appears in Figure 5, where the straight line is the projection of the hypothetic ancient equator; and you will probably agree with me that it gives little support to the suggestion that the principal line of continental elevation was originally equatorial.

Why do continental areas rise and fall?—A fourth problem refers to continental oscillations. The geologic history of every district of the land includes alternate submergence under and emergence from the sea. To what extent are these changes due, on one hand, to movements of the sea and, on the other, to movements of the land, and what are their causes? With American geologists the idea, recently advocated, that the chief movements are those of the ocean finds little favor, because some of the most important of the changes of which we are directly cognizant are manifestly differential. Our palaeozoic map pictures a sea where now are Appalachian uplands and uplands where now are low coastal plains and oceanic waters. In Cretaceous time the two margins of what are now the Great Plains had the same height, or at least the western margin was no higher than the eastern; but now the western margin lies from four thousand to six thousand feet above the eastern, and the intervening rock mass appears to have been gently tilted without important internal distortion. Such geographic revolutions are not to be explained by the shifting of the hydro-sphere nor by its dilatation and contraction. Neither can they be ascribed to isostatic restoration of an equilibrium deranged through the transfer of masses by erosion and sedimentation, for that hypothetic process is essentially conservative. Neither is it easy to believe that the two margins of the plains have differed, since the Cretaceous, to the extent of one mile in their radial contraction due to secular cooling of the globe; nor is it easy, at least for the disciple of isostasy, to believe that such a change can have resulted from the localization of deformation consequent on the slowing of the earth's rotation. Each of these processes may have been concerned, but I conceive that the essential factor still awaits suggestion. Our knowledge of surface processes, as compared to subterranean, is so full that the field of plausible epigene hypotheses may be exhausted, but the vista of hypogene possibility still opens broadly.

Are continents permanent?—The doctrine of the permanence of the continental plateau, enunciated long ago by Dana and more recently advocated, with a powerful array of new data, by Murray and Wallace, has made rapid progress toward general acceptance. Nevertheless its course is not entirely clear, and among the obstacles still to be overcome is one whose magnitude is perhaps magnified for the American student by proximity. All who have studied broadly the stratigraphy of the Appalachian district have concluded that the sediments came chiefly from the east, and the detailed Appalachian work of the past decade is disclosing a complicated history, in which all chapters tell of an eastern palæozoic land and some chapters seem to testify to its wide extent. At some times the western shore of this land lay east of the sight of the Blue Ridge, and there is serious doubt whether the existing belts of coastal plain and submerged continental shelf afford it sufficient space. For the present, at least, the subject of continental permanence must be classed with the continental problems.

Do continents grow?—According to my own view there is yet another, a sixth, continental problem deserving the attention of the World's Fair inter-continental congress. We have been told by the masters of our science (and their teaching has been echoed in every text-book and in every classroom), that through the whole period of the geologic record the continents have grown; not that the continental plateaus have been materially extended, not that the pendulum has moved always in one direction, but that the land area has on the whole steadily increased. From this doctrine there has been no dissent—

and possibly there should be no dissent—but the evidence on which it is founded appears to me so far from conclusive that I venture to doubt.

The evidence employed consists partly in the general distribution of formations as shown by the geologic map and partly in inferences drawn from certain formations which contain internal evidence that they originated on coasts. With the aid of such data are drawn the outlines of ancient ocean and land at various geologic dates, and from the comparison of these outlines continental growth is inferred. In passing from the formation boundaries of the geologic map to the oceanic limits of the charts of ancient geography, allowance is made for the former extent of non-littoral formations beyond their present boundaries. This allowance is largely conjectural and the range of possible error is confessedly great. In passing from the observed limits of littoral formations to the coast lines of ancient geography little or no allowance is usually made for the former extent of the formations, and I conceive that great possibility of error is also thus admitted. During a period of oceanic transgression over the land, all portions of the transgressed surface are successively coastal, and the coastal deposits they receive are subsequently buried by off-shore deposits. When therefore littoral beds are found in remnants of strata surviving the processes of degradation, it is indeed proper to infer the proximity of ancient coasts during their formation, but the inference that they represent the limit of transgression for that epoch may be far from the truth. For these reasons it appears to me that the specific conclusions which have been reached with reference to the original extent of various formations are subject to wide uncertainties; and, if this be granted, then but brief attention to a simple law of denudation is necessary to show that the general conclusion may be illusory. The process of degradation by aqueous agencies is chiefly regulated, not by the thickness of formations, but by the height to which they are uplifted. Thus the present extent of most formations is determined in large part by crustal oscillations subsequent to their deposition. As formations are progressively eroded, the under-lying and older can not be attacked until the over-lying and younger have been carried away, and so the outcrops of the older of necessity project beyond the boundaries of the younger. The progress of vague inference, making indefinite allowance for the unknown quantity of eroded strata, nearly always assigns to the older formation, which projects visibly beyond the newer, a greater original extent. It appears to me thus possible that the greater part of the data from which continental growth is inferred may be factitious and misleading.

Furthermore, inference, such as it is, deals with only one phase of the problem. It is applied to the incursions of the sea upon the land, but it is not applied to the excursions of the land upon the sea. Just as we infer from stratified rocks the presence of the sea, so also we infer from unconformities the sea's absence; and to the student of ancient geography the two classes of evidence are equally important. But the strata, spread widely over the surface of the land, are conspicuous phenomena, while unconformities are visible only here and there and are usually difficult of determination. For this reason the data derived from unconformity have never been assembled. Essays toward ancient geography have dealt only with the minima of ancient land, never with its maxima, and the question of continental growth can not be adequately treated while half of the history is ignored.

We may borrow a figure from the strand of a lake. As the waves roll inward, each records its farthest limit by a line upon the sand and each obliterates all previous wave lines which it overpasses. The observer who studies the transient record at any point may find a series of lines, of which the highest is the oldest and the lowest is the newest, and he may infer that the lake level was higher when the first wave left its trace and that the water is receding from the land. But, if he continue his observations through many days and fix monuments to record from time to time the lowest land laid bare between the waves, he may discover that the highest wave line and the lowest record of ebb correspond in time with the play of the largest waves, and that the lowest wave line and the highest record of ebb correspond to the play of smaller waves, and thus reach the conclusion that the lake level has remained unchanged. In the study of Time's great continental strand we are not even able to observe directly the wave lines of rhythmic transgression, but infer their positions from data often ambiguous; and of the lower wave limits, the lines of maximum regression, we are absolutely ignorant.

It may be true that *a priori* considerations afford a presumption in favor of continental growth, but such presumption should not be permitted to give color to evidence otherwise neutral; and moreover it is not impossible to discover an *a priori* presumption in favor of continental diminution. Assuming that hypogene agencies cause continental areas to rise above the ocean, the work of epigene agencies constantly tends to remove the projecting eminences and deposit their material about their margins, so as to extend the area of the continental plateau. Thus we have a strong *a priori* presumption in favor of continental growth. On the other hand, if we admit the principle of isostatic equilibrium, then the continental eminences have low density; and as they are worn away by epigene processes the material which rises from below to restore them has greater density and maintains a somewhat less altitude. The process of isostatic restoration tends thus toward the permanent levelling of continents, and if the hypogene initiative should cease the continents would ultimately be reduced to ocean level, and finally, through processes of solution, to a level below the ocean; so, assuming the initiative processes of the under earth to be of finite duration, the work of terrestrial degradation, combined with isostatic restoration, should afford a continental history characterized in an earlier stage by growth and in a later stage by decadence. In our ignorance of subterranean forces we should use such *a priori* considerations only as a means for the suggestion of hypotheses. As they have doubtless served to promote the theory of continental growth, they should also be permitted to indicate the possibility of continental retrogradation.

Summary.—The problems of the continents have been touched to-day so briefly that a summary is almost superfluous. The doctrine of isostasy, though holding a leading position, has not fully supplanted the doctrine of rigidity. If it be accepted, there remains the question whether heat or composition determines the gravity of the ocean beds and the levity of continents. For the origin of continents we have a single hypothesis, which deserves to be more fully compared with the body of modern data. The newly determined configuration of the continental mass has yielded no suggestion as to its origin. The cause of differential elevation and subsidence within the continental plateau is unknown and has probably not been suggested. The permanence of the continental plateau, though highly probable, is not yet fully established; and the doctrine of continental growth, though generally accepted, has not been placed beyond the field of profitable discussion. Thus the subject of continents affords no less than a half dozen of great problems, whose complete solution belongs to the future. It is not altogether pleasant to deal with a subject in regard to which the domain of our ignorance is so broad; but if we are optimists we may be comforted by the reflection that the geologists of this generation, at least, will have no occasion, like Alexander, to lament a dearth of worlds to conquer.

PRE-COLUMBIAN COPPER-MINING IN NORTH AMERICA.

By R. L. Packard.

The broad classification of the successive stages of culture of the prehistoric peoples of Europe into the stone, bronze, and iron "ages" was based upon prehistoric finds, and is an induction derived from observation similar to that relating to the succession of the different orders of animals and plants in geological history. It is also confirmed, as far as bronze and iron are concerned, by ancient tradition, for in early historical times it was known among the Greeks that bronze had preceded iron at an earlier period, and this knowledge, passing to the Romans in a later age, was expressed in the line of Lucretius which has been often quoted in this connection, "*sed prior æris erat quam ferri cognitus usus.*"

But there is evidence to show that the use of copper was independent of, if it did not precede, that of bronze, particularly in places where the metal was indigenous. This evidence consists in the discovery of copper implements and weapons instead of or sometimes accompanying bronze, mingled with numerous stone articles of the same character, in various places in Europe and the East. The prehistoric people had learned the art of extracting copper from its ore, and in some cases practiced it near the places where the metal was used for implements and weapons. Prehistoric copper mines have been reported from the Urals and elsewhere, and a circumstantial account of such a mine, which was discovered in 1827 near Bischofshofen in Salzburg, in Germany, has been published by M. Much, an archaeologist who examined it in 1879.* The traces of the old workings, nearly obliterated after so long a time, had led to the establishment of a flourishing modern copper mine on the same vein, just as the trenches on the outcrops of the copper-bearing rocks in the Lake Superior district served as guides to modern miners in sinking shafts there. The Salzburg mine, however, was in copper ore and not native copper, and was a mine in the proper sense of the term, with extensive underground workings. The remains of small smelting furnaces, with slag heaps and other rubbish, were

* Die Kupferzeit in Europa und ihr Verhältniss zur Cultur der Indogermanen. Wien, 1886.

found in the neighborhood, in the midst of which were a few pieces of the copper produced from the ore on the spot by the prehistoric smelters.* No iron tools or signs of their use were found in this mine, which was assigned by the archæologist who examined it to the time of the neighboring lake-dwellers, who used its copper for weapons and tools. Another mine in the Tyrol, referred to by the same author, was also apparently worked to supply a colony of lake dwellers situated near by.

It might be expected on both mineralogical and metallurgical grounds that copper would be used before bronze, and even before smelting was discovered, because copper, like gold and silver, is found in the native state in many places, while considerable metallurgical skill is necessary for the production of bronze. Moreover, bronze is an alloy of copper and tin, and, except in the comparatively rare cases where copper and tin ores occur together, tin would have to be transported to the copper-smelters to produce the alloy. In North America,† while copper was known to the natives, bronze had not appeared at the epoch of discovery by Europeans, and neither smelting nor even melting was necessary for the production of the copper articles found in use by the discoverers.

The first comers to the northern part of this continent were struck with the absence of metals in the native weapons and implements, and found their place supplied by stone and bone. The inhabitants were in the neolithic stage of culture. They were, indeed, in possession of copper, but, as far as the discoverers observed, it was almost exclusively used for ornamental purposes, and formed, apparently, no part of the native equipment in the arts of life. Exclusive of the Spaniards, the earliest voyagers who left records or reports of their explorations sailed along the coast, or visited different parts of it, from Labrador to Florida, and the inhabitants of the whole seaboard were found sparingly in possession of the "red metal." Thus, in the account of Cabot's voyage in 1497, given in Hakluyt, there is this brief statement: "Hee (Cabot) declareth further that in many places of these Regions he saw great plentie of copper among the inhabitants." The account is a translation from Peter Martyr, and the words "great plentie of" are not warranted by the original.‡ Cabot's observations were made on the northern coast of the continent, and he went as far as 60° north latitude. A similar brief statement is given in the account of the voyage of Cortereal in 1500, who is said to have gone as far north as 56°. The account (in Ramusio) describes the painted inhabitants, their clothing of skins, and other particulars, and states that they had bracelets of silver and copper. The mention of silver is unfortunate. Verrazano's

* A piece of this copper gave, on analysis: Copper, 98·46 per cent; sulphur, 0·09 per cent; slag, 0·44 per cent, while a copper tool found in the workings gave copper, 97·78 per cent; nickel, 0·88 per cent; iron, a trace; lead, 0·05 per cent; sulphur, 0·24 per cent; slag, 0·07 per cent.

† By North America is meant only the non-Spanish portion of the country.

‡ *Orichalcum in plerisque locis se vidisse apud incolas prædicat.*

report goes more into particulars. He coasted from 34° to beyond 41° north latitude in the year 1524, and made several landings. He says of the natives, at a point on the coast apparently in the neighborhood of New York, that they had "many plates of wrought copper, which they esteeme more than golde." On sailing along the coast to the eastward he saw certain hills and concluded that they had some "minerall matter in them, because," he says, "we saw many of them [the natives] have beadstones of copper hanging at their eares." On the southern and eastern coast, therefore, according to these accounts, the copper was used for ornaments. Neither of the observers quoted speaks of copper weapons in that part of the country, which they would have been likely to notice, as they naturally paid special attention to the arms they might have to encounter. Nor did later explorers who described the equipment of the natives in detail have occasion to give greater prominence to copper.

In Cartier's second voyage to the St. Lawrence, in 1535, he kidnaped the principal chief of a local tribe to take with him to France, following the common practice of the time, and this chief was visited on shipboard by condoling members of his tribe, who were assured that he would return the next year, "which, when they heard," says the account in Hakluyt, "they greatly thanked our captain and gave their lord three bundles of beaver and sea wolves skinnes, with a great knife of red copper that commeth from Saguenay." Here is an instance of a copper weapon or implement. The quantity of copper which the North American Indians possessed at the epoch of discovery, although the metal was diffused over a very wide territory, was very small compared with stone. A glance at collections of aboriginal articles, like that of the Smithsonian Institution in Washington or the Peabody Museum in Cambridge, will at once show how relatively insignificant it was. The Smithsonian has between six and seven hundred copper articles from mounds, graves, and other sources within the territory of the United States, while there are thousands of stone arrow and spear heads and implements in its collection. The Peabody and other copper collections are very much smaller. A closer examination of the Smithsonian exhibit will show that the copper articles from the south and east are mainly of an ornamental character and few in number compared with those found toward the northwest. As Wisconsin is approached the copper articles not only increase in number, but the proportion of arrow and spear heads and implements far exceeds that of the ornaments. Among the Wisconsin specimens are pieces of "float" copper, varying in size from those weighing several pounds down to nuggets, which indicate the convenient material of which some of the manufactured articles were probably made.

If one were to prepare a map showing by shading or colors, as is now the practice, the relative number of aboriginal copper finds in the United States, the deepest shade or darkest color would at present be in

Wisconsin. This condition is no doubt largely due to the indefatigable zeal of Mr. F. S. Perkins, of Wisconsin, who has devoted himself for many years to collecting copper articles of Indian origin from all parts of the State, about four hundred of which are in the Smithsonian cases. But the phenomenon can be explained in another way when one reflects that Keweenaw Point is directly north of the State and was the seat of the ancient copper mines which have attracted the attention of archæologists, and was the center of distribution of the native copper which was the object of the desultory mining carried on there. Wisconsin is also in a very favorable situation for receiving the drift which brought "float" copper from the copper-bearing rocks of Keweenaw, which "float" was apparently often manufactured into implements. The State covers a district which was near the mines and is in a direct course for people leaving them going south. It may be found that that district was the seat of the ancient miners themselves.

The yield of mounds, graves, and fields, as shown in the collections, confirms in a general way the observations of the first discoverers. In the eastern and southern parts of the country the majority of the copper articles which have been found are breast-plates, bracelets, beads, bobbin-like objects and other ornaments, while in the north and west, and especially in Wisconsin, implements and weapons prevail. The Wisconsin specimens are like those figured by Whittlesey (*Smithsonian Contributions*, vol. XIII), which were found in the mining district itself, and those found at Brockville, Canada, and shown in Wilson's "Prehistoric Man." Others, apparently of the same character, are mentioned by Wilson as being found near Marquette, Mich., east of the copper district.

The present evidence, therefore, shows that copper had not passed its ornamental or precious stage on the seaboard and in the south at the time this continent was brought to the attention of Europe. It was not a part of the general native equipment, either for war, or hunting, or other useful purposes, and its position in the native economy was not like the noticeable part it played in the armament of the Mexicans and Central Americans of the same period.

At the advent of Europeans copper was eagerly sought for in trade with the whites. An official present of copper articles is particularly mentioned in the account of Cartier's voyage before referred to, and Ralph Lane writes from Roanoke, in 1585, to his company in England that they could not do better than send over copper articles of all kinds to trade with: "copper carryeth the price of all, so it be made red," he explains. The copper obtained from the whites was very soon, with other imported things, disseminated by barter among the different tribes. In Frobisher's third voyage to the Labrador coast (lat. 58°), in 1578, he noticed the evidence of this aboriginal trade, and says "the natives have traffic with other people, and have barres of iron, arrowe, and speare heads and certain buttons of copper which they use to weare upon their foreheads for ornament, as our ladies in the court of England doe use great pearle." This trade with the natives must have been considerable. The fishing fleets which swarmed in the northern waters carried on trade, and copper and iron articles formed a part of their outward cargoes. According to Anthony Parkhurst, who had been in the business and on the fishing grounds, trade to Newfoundland from England was brisk in 1548, and an estimate which he made for Hakluyt shows that in 1578 there were 100 Spanish vessels engaged in cod-fishing, 20 to 30 whalers from Biscay, 50 Portuguese, and 150 French and Breton vessels. The English contingent was then much smaller than in former years.

After the arrival of Europeans, bringing an assortment of "novelties" of all kinds, there was no reason why the Indians should trouble themselves further to obtain domestic copper by the toilsome process of searching and digging for it, because they now had not only a ready and sufficient supply of that metal for ornamental purposes, but were introduced to many other things of superior attractiveness, especially iron, in the form of knives, hatchets, etc., which at once superseded copper for practical use. "The Chippewa chief, Kontika, asserted in 1824 that but seven generations of men had passed since the French brought them brass kettles; at which time their people at once laid aside their own manufactures and adopted those of the French." * The testimony of the earliest voyagers to the possession of copper ornaments by the natives is therefore of importance, because there was very soon enough of the imported article in the country to make a show. Incidentally, also, archaeologists have to keep this fact of foreign importation in mind in deciding upon the origin of copper articles in "finds." Lake Superior copper, of which pre-Columbian Indian articles were made, occurs in the native state, and is free from the impurities which are found in copper that has been smelted, so that chemical analysis could often decide whether a given specimen was of native origin or imported. On some copper articles found in the north, specks of silver have been noticed. This is a sure token of Lake Superior copper which has never been melted.

In the absence of evidence that the Indians of the United States had any knowledge of smelting, it must be inferred that all the copper they possessed was found in the metallic or native state. There is nothing to show that they were aware of the existence of copper ore as a source of metal. No remains of smelting places, or slag, or other indications of metallurgical operations have yet been found. If they had known smelting they could have had an ample supply of the metal, because ores of copper are comparatively abundant in the United States, while, as a matter of fact, copper was a rarity with them. Native copper occurs in small quantities in many places in the United States, but there is no evidence at present that the northern Indians had knowledge of any

* Schoolcraft, vol. IV, p. 142.

but two localities where it could be obtained in any quantity. These were the Coppermine River in the British Possessions, and the Lake Superior copper district. The latter affords the most remarkable occurrence of native copper in the world, and the present mines on Keweenaw Peninsula—including the famous Calumet and Hecla, the Tamarack, Quincy, and others—are of world-wide fame. The same deposits were worked superficially over their whole extent long before the advent of Europeans to these shores.

By referring to the map of Michigan it will be seen that Keweenaw Peninsula is a prominent geographical feature and extends a considerable distance into Lake Superior. Its northwestern shore and the continuation thereof through Ontonagon County is practically parallel to the opposite or north shore of the lake. Through the middle of Keweenaw Point runs a belt of elevated land which is several hundred feet above the lake in some places, and extends from the extreme point through the peninsula and Ontonagon County into Wisconsin. This elevated belt, which is known as the "mineral range," sometimes rises into bluffs which are abrupt on the southeastern or shoreward side, but sloping in the opposite direction or toward the lake. The dip of the formation composing this range (sandstone, and sheets of igneous rock, including conglomerates) is in a general northwesterly direction, or towards the lake and the north shore. On Isle Royale, near the north shore of the lake, the same formation occurs, but dipping in the opposite direction, viz, to the southeast or towards Keweenaw. "Trap" rock carrying copper is also found on the north and east shores of the lake at St. Ignace and Michipicoten Island. The copper-bearing series of the "mineral range" consists of sheets of igneous rocks—diabase, diabase-amygdaloid, and melaphyr—which include beds of conglomerate all carrying native copper. Both of these classes of rocks are mined. The famous Calumet and Hecla mine is in the conglomerate, as is also the Tamarack, while the Quincy, Atlantic, and others are in the amygdaloid rocks.

The product of the mines is divided by the miners into three classes, stamp rock, "barrel work," and mass copper. By stamp rock is meant that which contains the copper in fine particles and is sent to the powerful steam stamps to be crushed, in order to separate the grains of copper by washing (jigging), just as gold-bearing quartz is stamped. "Barrel work" means the pieces of copper which are large enough to be detached from the rock without stamping and are packed in barrels and sent directly to the smelters. They vary in size from pieces about as large as the hand to those not too large to be conveniently packed in barrels. Pieces too large for this constitute the third class, "mass copper," which includes the huge pieces of many tons' weight which are occasionally met with. All this copper shows as such in the rock, and the ancient miners had only to follow down a promising outcrop showing "barrel work" for a few feet and hammer away the rock from the copper to secure the latter. When they came upon mass copper they were compelled to abandon it, after hammering off projecting pieces, because they had no tools for cutting it up and removing it. Several instances of this sort have been found.

The ancient mines were not mines in the strict sense of the word, because they were not underground workings. As described by Whittlesey, who examined them at an early date,* they were shallow pits or trenches, and sometimes excavations in the faces of the cliffs, scattered along the mineral range from Ontonagon to near the end of the peninsula. At the time modern mining began they had become mere depressions in the ground, owing to the accumulations of earth, leaves, and decayed vegetable matter within them. Forest trees were growing in them and upon the waste thrown out of them, so that it was difficult to distinguish them from natural depressions due to the weathering of the rock beneath the soil, or, in some cases, from the hollows left by the upturned roots of fallen trees. After their character was discovered, however, they served as guides to the modern miners, who often sank shafts upon the copper-bearing rocks, which were revealed by clearing them out. No mine has been opened on the lake that was not thus "prospected" by the old miners. Trenches like those on Keweenaw Point and Ontonagon, but, if anything, more elaborate, were found on Isle Royale, and Sir William Logan mentioned similar workings on the east shore of the lake near Maimanse. All of these workings contained stone hammers or mauls, amounting in all to a countless number.

A few wooden shovels, strongly resembling canoe paddles, were found in some of the diggings, together with the remains of wooden bowls for baling, birch-bark baskets, and some spear or lance heads and other articles of copper. In Ontonagon County the old workings were for the most part shallow depressions only a few feet deep. Some of them in the bluff which showed outcroppings of copper rock were hardly large enough to shelter a bear, while others were larger. In Houghton County (i. e., on the Keweenaw promontory) on the Quincy location, there were broad and deep pits in the gravel, probably dug for the float copper, lumps of which are still met with in the neighborhood. At the Central mine, further out on the point, there was a pit filled in with rubbish, which was at first supposed to be natural. It was 5 feet deep and 30 long. On examination, a "flat piece of copper, 5 to 9 inches thick and 9 feet long, was found, which formed part of a piece still in the vein. Broken stone mauls were all about it, showing that the miners could do nothing with it. Its upper edge had been beaten by the stone mauls so severely that a lip or projecting rim had been formed, which was bent downwards." Other localities toward the end of the peninsula and at the Copper Falls location are described by Mr. Whittlesey, and as late as 1890 depressions in the ground, of

* *Smithsonian Contributions,* vol. XIII, 1862.

small dimensions, were pointed out to the writer at the latter place as the work of the old miners. Modern miners would regard the whole system as nothing more than prospecting work and not mining proper, as there were no shafts or tunnels or underground workings of any kind. As Mr. Whittlesey expressed it, "the old miners performed the part of the surface explorers."

I am fortunate in being able to add to the foregoing the testimony of an eye-witness of some other discoveries in this district, viz, that of Mr. J. H. Forster, a well-known mining engineer who lived in the district many years. He was at one time superintendent of one of the mines, and was engaged on the Portage Lake Ship Canal as State engineer when the canal was opened, when he discovered some copper articles in an ancient grave at that point. He writes in regard to the discovery of old operations: "The largest mass of float copper found in modern times - - - weighed 18 tons, and contained very little rocky matter. When found in the woods, on the Mesnard location, it was covered with moss and resembled a flat trap bowlder. It had been manipulated by the 'ancient miner,' and much charcoal was found around it. Its top and sides were pounded smooth, and marks of stone hammers were apparent. All projections—every bit of copper that could be detached—had been carried away. - - - Subsequent explorations disclosed the epidote lode whence the mass came—torn from its matrix doubtless by the ice. The mass had been transported only about 50 feet and dropped on a ridge. When the lode was stripped of the drift the jagged edges of a mass in place were exposed. It was of the same length, thickness, and structure of the 'float.' It was observed at the time that if the 'float' could be set up on edge on the piece in place it would fit in exactly. A beautiful illustration of the power and direction of the glacier was thus afforded." Mr. Forster was present when the famous Calumet conglomerate lode was opened. At that point a small mound was found in the woods, while explorations were in progress, upon which large pine, maple, and birch trees were growing. Roots of trees still more ancient were found in the drift. After stripping off the timber a pit was sunk, which reached the solid conglomerate at the depth of 15 feet. "But it was a hard rock filled with stamp copper only, and could not be mined by the ancient miners."

Numerous stone hammers and birch-bark baskets were found in the workings. Mr. Forster thinks the dirt was carried out of the pit in these baskets. On the north side of Portage Lake, on the extension of the Isle Royale lode (opposite Houghton), the drift being shallow, "long trenches were dug on the back of the lode 3 feet wide and deep. There was much small mass or nugget copper (barrel work) released by the disintegration of the soft epidote vein stone." This was thrown out, while the earth was thrown behind the miner as he advanced, and the work resembled that of an expert "navvy." No evidence of deep mining could be found. As usual, stone hammers and charcoal were found in the trenches. A remarkably deep trench which was filled with earth and leaves was discovered at the South Pewabie (now Atlantic) mine, several miles west of the last locality, which extended 2 or 3 feet into the solid rock. At the bottom "was a well-defined transverse fissure vein of quartz, about 2 feet wide, containing here and there chunks of solid copper. By the several pits sunk on the course of the vein, proof was had that it had been worked superficially several hundred feet in length. I walked through it a long distance. The surface of the formation was shattered and decomposed, hence the old miners could come at the quartz handily. They did not carry the rock out to the surface to dump it, but piled it up neatly on each side of the drift. At one point I found a handsome specimen of quartz and copper laid up carefully in a niche. It weighed several pounds. - - - As in other cases, we had proof that the ancient miner did not sink any shafts and do real mining; he was only a surface gleaner." Of the ancient workings on Isle Royale, on the north shore of the lake, which were very extensive and have been described as extending 20 feet and more in the solid rock, Mr. Forster says: "As I understand it, these extensive works were upon a high outcrop, promising natural drainage. And I should infer from what I heard from Mr. A. C. Davis, the agent, and others who opened the Minong mine* that the ancient workings were among disturbed shattered rocks, among which were found much mass copper and barrel work. The ancients were after these pieces of copper. Mr. Davis found many considerable masses, handled and beaten by the ancient men, which were too large for them to carry away."†

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* On Isle Royale.

† From a letter to the writer. Mr. Forster refers to the views of another mining man on the old copper workings on Keweenaw, who was the agent (or superintendent) of the Mesnard mine, and his opinions as an expert are valuable. Mr. Forster's letter continues as follows:

"Mr. Jacob Houghton, in a paper entitled 'The Ancient Copper Mines of Lake Superior,' says, speaking of the so-called ancient mines:

"'Their mining operations were crude and primitive. The process was to heat the embedding rocks by building fires on the outcrops of the veins or belts, to partially disintegrate the rocks by contraction produced by the sudden throwing on of water, and to complete the removal of the pieces of native copper by mauling off the adhering particles of rock with stone hammers. This is attested by the presence in all ancient pits of large quantities of charcoal and numberless hammers, the latter showing marks of long usage. The miners had not advanced to any knowledge of the artificial elevation of water, as is shown by the fact that apparently, in all cases, the pits have only been sunk to a depth where the limit in man power in baling out the water is reached.

"'The pits, the charcoal, the stone hammers, and the implements and tools made of copper are the only relics left of the races that wrought these mines. Neither a grave, vestige of a habitation, skeleton, or bone has been found.'

"In connection with these last remarks by Mr. Houghton, I beg to state that while I was State engineer on the Portage Lake and Lake Superior Ship Canal, the super-

At the Minnesota mine, in Ontonagon County, was found a large piece of mass copper which had been raised some distance in the excavation and abandoned by the old workers. As this was the first large mass discovered, and gave rise to considerable speculation, it deserves special mention. The account is taken from Forster and Whitney's report on the geology of the Lake Superior copper region, and is as follows: In the winter of 1847–'48, Mr. Knapp, the agent of the Minnesota, found an artificial cavern on the mine location containing stone hammers, and at the bottom was a vein with jagged projections of copper. After the snow had left in the spring he found other excavations, and particularly one 26 feet deep, filled with clay and a matted mass of moldering vegetable matter. On digging 18 feet he came to a mass of native copper 10 feet long, 3 feet wide, and nearly 2 feet thick, weighing over 6 tons. "On digging around it the mass was found to rest on billets of oak supported by sleepers of the same material. This wood, by its long exposure to dampness, is dark-colored and has lost all of its consistency. A knife blade may be thrust into it as easily as into a peat bog. The earth was so packed around the copper as to give it a firm support. The ancient miners had evidently raised it about 5 feet and then abandoned the work as too laborious. They had taken off every projecting point which was accessible, so that the exposed surface was smooth. Below this the vein was subsequently found filled with a sheet of copper 5 feet thick and of an undetermined extent vertically and longitudinally. - - - The vein was wrought in the form of an open trench, and where the copper was most abundant,

intendent in laying water pipes opened a very old grave. The grave was in the yellow sand, in a grove of Norway pines, near Lake Superior. At the bottom there was an exceedingly thin layer of mold, darker than the sand. Some human teeth were found and a string of copper beads strung on sinews. The sinews, much decayed, still held the beads in place. The copper bead was a small thin piece of copper about one-fourth of an inch long. It was rudely bent into a cylinder for the string to pass through, but was not welded; the edges were in contact, but not fastened together. This grave was at the Grand Portage or carrying place.

"In dredging, the dipper brought up from the bed of the ship canal where the sand drift had originally been at least 25 feet deep, several perfect stone hammers and a copper implement which I pronounced to have been the head and ferule of a pike pole. It was about 18 inches long, tapering, sharp, and solid for two-thirds the distance from the small or lower end. At the upper or pole end the copper had been flattened out and then bent round to form a socket for the pole. There was a slight opening between the two edges of the curved copper; it was not joined or welded. The pike was bright and shining like a clean copper kettle.

"I saw it, and it was dredged from the bottom of the canal, and its position, as regards strata, was under the drift or dune sand and on the hard gravel and clay underlying. I know of no other finds in that section. The gravel and hard pan found in the bottom of the dredged canal I regarded as the bed of the ancient stream or estuary, now filled up with drift sand blown in from Lake Superior. How much the glacial drift had to do in filling up the ancient gorge in which the present canal is only a line, I can not say. In some of the marshes cut by the canal were found three distinct forests, one growing on top of the other, to a depth of 14 feet."

there the excavations extended deepest. The trench is generally filled to within a foot of the surface with the wash from the surrounding surface, intermingled with leaves nearly decayed." Whittlesey says of this mass: "Its upper surface and edges were beaten and pounded smooth, all the irregularities taken off, and around the outside a rim or lip was formed, bending downwards. - - - Such copper as could be separated by their tools was thus broken off; the beaten surface was smooth and polished.

"On the edge of the excavation in which the mass was found there stood an ancient hemlock, the roots of which extended across the ditch. I counted the rings of annual growth on its stump and found them to be 290." Mr. Knapp felled another tree growing in a similar position, which had 395 rings. "The fallen and decayed trunks of trees of a previous generation were seen lying across the pits." A shaft was subsequently sunk on the lode revealed by this trench, which was in rich ground, to a great depth. The abandonment of this mass of copper formerly gave rise to conjectures. It was supposed that the ancient miners were interrupted in their work "by some terrible pestilence - - - or by the breaking out of war; or, as seems not less probable, by the invasion of the mineral region by a barbarian race, ignorant of all the arts of the ancient Mound-builders of the Mississippi and of Lake Superior."* But from a consideration of the evidence of the character and scope of the old workings which we now possess it will be seen that it is unnecessary to go so far for an explanation. As was clearly the case at the Central and Mesnard mines and on Isle Royale, the mass at the Minnesota was abandoned by the old miners because they found it impossible to get any more pieces from it. They had no tools which could cut it, and even at the present time mass copper is the least desirable form in which the metal presents itself in the mines, on account of the labor and expense of cutting it up, although there are steel tools especially invented for the purpose. The practice of hammering off pieces from mass copper is mentioned by visitors to the lake from the French missionaries down to Schoolcraft. There was a large mass on the Ontonagon, which has been in the Smithsonian Institution for many years, which was considerably reduced in size in this way in the course of a hundred and fifty years by casual visits.

A great antiquity has been assigned to these workings by some writers, and it used to be supposed that a busy industry was suddenly interrupted in them at some time over five hundred years ago. The tree with three hundred and ninety-five rings of growth has been used to support an argument that the workings must have been abandoned at least as long ago as the middle of the fifteenth century, or, to be exact, reckoning from 1847, before the year 1452. This would be at least forty years before the voyage of Columbus and eighty-four years before Cartier visited Montreal. Although it may be true that work

* Wilson; *Prehistoric Man*, vol. I, p. 278.

ceased at the particular trench where that tree was felled at the date indicated, it does not necessarily follow that all the workings were abandoned at the same time. Indeed, the tree which grew on the dump of the pit where the Minnesota mass was found did not begin its growth until over a hundred years later, or after the French had been up the St. Lawrence and there had been considerable traffic with Europeans on the seacoast. How long *a parte ante* the whole system had been worked can only be a matter of conjecture. When one reflects that many hundreds of men were busily engaged for several consecutive seasons, with all the feverish energy born of the modern thirst for gold, in the diggings of any one of the placer camps which are now seen abandoned in Idaho, Oregon, and California, it will be apparent that the old miners on Lake Superior must have taken a long time for their leisurely work. Their tools were primitive, their work was desultory, and they knew nothing about the desire of wealth. Primitive peoples are supposed not to have prosecuted any industry persistently and assiduously, like modern civilized men. Where there are no wages, no expenditures, no companies and employés, no stocks or fluctuations of the market, nothing even which can be called a demand, there is no need of pushing a laborious work. It was also, probably, only in the summer, and it may have been only at considerable intervals, that Keweenaw, Ontonagon, and Isle Royale were visited for copper. It must also not be forgotten that the ancient miners only carried away "barrel work." They were forced to abandon mass copper. Barrel work from the excavations and float copper from the neighboring and remote drift would furnish the material necessary for all the tools, weapons, and ornaments that have been found, and although the quantity of copper from these sources was small when reckoned in tons, yet the desultory and selective kind of mining which produced it, especially if carried on by a comparatively small number of persons over such an extensive territory as the mineral range of Keweenaw, would naturally require an indefinite length of time.

From the historical references which will be presently considered, it will appear that Keweenaw and Ontonagon were known as a copper district at the time the French arrived in Canada. But as it has been imagined that an extinct race superior in culture to Indians opened the trenches and mined copper there, it may be well to give a comparatively modern instance of a similar search for copper by Indians before taking up the historical argument. Such an instance is afforded in Hearne's narrative of his journey from Prince of Wales's Fort in the Hudson's Bay Company's territory to the Coppermine River in 1771. Hearne was an employé of the Hudson's Bay Company, and undertook the expedition in the interest of the company. His party was composed of Indians who were not very far removed in point of culture from their savage stone-using ancestors of three or four generations previous, and no better idea could be gained of the character and life of neolithic man as he was in that part of the world, of his methods of obtaining subsistence, his general degree of development, and, incidentally, his stealth and ferocity in attack on his neolithic fellow-men, than is contained in this book. After a journey of several months through barren wastes, during which he endured the greatest hardships and was in danger of starvation, Hearne reached the Coppermine River, and, after his savages had surprised and murdered some unsuspecting Esquimaux, he visited the copper "mine," which he thus describes: "This mine, if it deserve that appellation, is no more than an entire jumble of rocks and gravel, which has been rent many ways by an earthquake. Through these ruins there runs a small river. The Indians who were the occasion of my undertaking this journey represented this mine to be so rich and valuable that if a factory were built at the river a ship might be ballasted with the ore instead of stone. - - - By their account the hills were entirely composed of that metal, all in handy lumps like a heap of pebbles. But their account differed so much from the truth that I and almost all my companions expended near four hours in search of some of this metal, with such poor success that among us all only one piece of any size could be found. This, however, was remarkably good, and weighed above 4 pounds. I believe the copper has formerly been in much greater plenty; for in many places, both on the surface and in the cavities and crevices of the rocks, the stones are much tinged with verdigris." They afterwards found smaller pieces of the metal.

He goes on to remark that the Indians imagined that every bit of copper they found resembled some object in nature, but hardly any two could agree what animal or part of an animal a given piece was like. He also says that by the help of fire and two stones the Indians could beat a piece of copper into any shape they wished. The Indians were really living in a copper age of their own. Hearne says: "Before Churchill River was settled by the Hudson's Bay Company, which was not more than fifty years previous to this journey being undertaken, the northern Indians had no other metal but copper among them, except a small quantity of ironwork, which a party of them who visited York Fort about the year 1713 or 1714 purchased, and a few pieces of old iron found at Churchill River, which had undoubtedly been left there by Capt. Monk. This being the case, numbers of them from all quarters used every summer to resort to these hills in search of copper, of which they made hatchets, ice-chisels, bayonets, knives, awls, arrow heads, etc. The many paths that had been beaten by the Indians on these occasions and which are yet in many places very perfect, especially on the dry ridges and hills, is surprising. The Copper Indians set a great value on their native metal even to this day, and prefer it to iron for almost every use except that of a hatchet, a knife, and an awl; for these three necessary implements copper makes but a very poor substitute." The Esquimaux tents were plundered of their copper by

Hearne's Indians. They found arrows "shod with a triangular piece of black stone, like slate, or a piece of copper." "Their [the Esquimaux] hatchets are made of a thick lump of copper, about 5 or 6 inches long and from 1½ to 2 inches square. They are bevelled away at one end like a mortise chisel. This is lashed into the end of a piece of wood about 12 or 14 inches long, in such a manner as to act like an adze; in general they are applied to the wood like a chisel and driven in with a heavy club instead of a mallet. Neither the weight of the tool nor the sharpness of the metal will admit of their being handled either as adze or ax with any degree of success."

This testimony of a modern eye-witness to the working and use of copper by aborigines is very instructive, and it requires little imagination to see that we have here a reproduction of the conditions that prevailed on Keweenaw Point two and three hundred years before. The summer visits of the miners, the manufacture of the copper into tools and weapons, some to be used in the neighborhood and others to be carried away for barter—for Hearne gives the rate of exchange between copper and iron from tribe to tribe—were doubtless the same in both cases; even the mythical or "medicine" feature of the subject, which was noticed by early writers in the stories of the Indians of Lake Superior, is not wanting here. The Coppermine story was that a woman (who was a magician) was the discoverer of the mine and used to conduct the Indians there every year. Becoming offended, she refused to accompany the men on one occasion when they left the place, after loading themselves with copper, but declared that she would sit on the mine until it sank with her into the ground. The next year when the men returned (women did not go on these expeditions) she had sunk to the waist and the quantity of copper had much decreased. On the next visit she had disappeared and the principal part of the copper with her, leaving only pieces here and there on the surface. Before this untoward event the copper was so plentiful that the Indians had only to turn it over and pick out such pieces as would best suit the different uses for which they intended it.

From this account it will be seen that it is not necessary to imagine a mysterious and extinct race more advanced in industrial arts than Indians to account for the ancient mines on Lake Superior. Besides, other workings requiring as much labor have been carried on by Indians. The catlinite or pipestone quarry in Minnesota was worked far into the present century. The mica mines in North Carolina, which are now worked, were operated in a way and to an extent suggestive of the Lake Superior copper mines, and were abandoned, according to Prof. Kerr, the geologist who examined them, a little over three hundred years ago, or after the arrival of the whites. There are also novaculite mines in Arkansas, obsidian workings in the Yellowstone Park, soapstone pottery quarries in several places in the Eastern States and in California, and especially the astonishingly extensive workings at Flint

Ridge, Licking County, Ohio, where chert was mined and manufactured into various articles at "workshops" on the grounds. Some of these various diggings were undoubtedly the work of "Indians;" what the others were must be left to archaeologists to decide. All give evidence that the natives of the country were close observers and possessed a considerable degree of skill in detecting and obtaining the various minerals which pleased their taste or were of use in their simple lives.

The reason which has been given for supposing that the ancient miners on Lake Superior had disappeared before the arrival of the whites is that the Indians made no mention of the mines to the French and had no tradition about them. But the first French explorers of the St. Lawrence, who left a record of their voyage, were informed by the Indians even of the Gulf—over 1,500 miles away—that copper came from a distant country in the west, and this statement was confirmed as they proceeded up the river. The same story was repeated a hundred years later, after settlements had been made, and it persisted until the source of the copper was found.

In the account of Cartier's second voyage, in 1535, given in Hakluyt, it is stated that the natives of the south shore of the Gulf of St. Lawrence informed him that the way to Canada was toward the west, and that the north shore before Canada was reached was the beginning of Saguenay, "and that thence commeth the red copper of them named Caignetdage." Subsequently, at Hochelaga (Montreal), the natives described to the French the voyage up the St. Lawrence and the Ottawa to Saguenay. "Moreover, they showed us with signs that the said three fals being past, a man might sayle the space of three moneths more alongst that river, and that along the hills that are on the north side there is a great river which (even as the other) commeth from the west, we thought it to be the river that runneth through the countrey of Saguenay; and without any sign or question mooved or asked of them, they tooke the chayne of our Captaines whistle which was of silver, and the dagger-haft of one of our fellow Mariners, hanging on his side being of yellow copper gilt, and shewed us that such stuffe came from the said River." "Our Captaine shewed them redde copper, which in their language they call Caignetadze, and looking towards that countrey [in a different direction from Saguenay], with signs asked them if any came from thence, they shaking their heads answered no; but they shewed us that it came from Saguenay." "But the right and ready way to go to Saguenay is up that way to Hochelaga [Montreal], and then into another [river] that commeth from Saguenay [the Ottawa] and then entereth into the foresaid river [the St. Lawrence] and that there is yet one moneths sayling thither. Moreover they told us and gave us to understand that there are people - - - and many inhabited towns and that they have great store of gold and red copper - - - and that beyond Saguenay the said river entereth into two or three great lakes, and that there is a sea of fresh water found, and as they have heard say of those of Saguenay, there was never man heard of that found out the end thereof, for as they told us they themselves were never there."

Allowing for the difficulty of communicating by signs and the many chances of misunderstanding, of which the interpretation of the Indian signs to mean gold is doubtless an instance, this is a geographical description which can almost be followed on the map, and the account shows that the St. Lawrence Indians knew that the copper they had came from a place in the west where there were great lakes and a "sea of fresh water." This was all hearsay with them, as they had never visited the distant country, which was inhabited by other tribes. But it seems evident enough that there was at that time a widely diffused knowledge of the source of the copper, which would hardly have been the case if the supply had ceased two or three generations before. When, over a hundred years later, French settlements had been established and traders and missionaries began to push forward to the great "sea of fresh water," they continually encountered the statement that copper could be found on its shores, and Indian guides finally took them to the precise localities where the metal had formerly been mined, and whence it was still occasionally obtained. Copper specimens, sometimes of large size, all reported as coming from Lake Superior, were not uncommon, at this time, as the following extracts show, and it seems evident that Indians still visited the old diggings and carried away such pieces of copper as they could find.

The Abbé Sagard, who was a missionary to New France about the year 1630, gave an account of the resources of the country in his "Grand Voyage du pays des Hurons," published at Paris in 1632. He did not penetrate as far as the upper lakes, but says that there were copper mines in that distant country which might prove profitable if there was a white population to support them and miners to work them, which would be the case if colonies were established. He saw a specimen of copper from the mines, which, he says, were 80 or 100 leagues distant from the country of the Hurons. In Margry's *Découvertes et établissements des Francais, Première partie, voyages des Francais sur les grands lacs, 1614-1684*, p. 81, is an extract from a letter relating to an exploration for copper written by Sieur Patoulet in Canada to Colbert in Paris. It is dated at Quebec, November 11, 1669, and is as follows: "Messrs. Joliet and Péré, to whom M. Talon paid 100 and 400 livres respectively, to explore for the copper deposit which is above Lake Ontario, specimens from which you have seen, and ascertain if it is abundant, easy to work, and if there is easy transportation hither, have not yet returned. The first named should have been here in September, but there is no news of him yet, so that a report of what may be expected of the mine must be postponed until next year." On page 95 of the same volume is a letter from Jean Talon to the king, dated Quebec, November 2, 1671, in which occurs the following reference to copper, one locality of which had then become known: "The copper specimen from Lake Superior and the Nantaonagon (Ontonagon) River which I send, indicates that there is some deposit or some river bank which yields this substance in as pure a state as could be wished, and more than 20 Frenchmen have seen a mass of it in the lake which they estimate at eight hundred weight. The Jesuit fathers among the Ottawas use an anvil of this metal which weighs about 100 pounds. It only remains to find the source of these detached pieces." He then gives some description of the Ontonagon River, in which he attempts to account for the formation in situ of the copper specimens found in its neighborhood (*galets de ce mestail*, evidently float copper), and goes on to say: "It is to be hoped that the frequent journeys of the Indians and French, who are beginning to make expeditions in that direction, will result in the discovery of the place which furnishes such pure metal, and that without expense to the king."

The passages from the Jesuit Relations, which have been often quoted in this connection, show that the mining districts were well known to the Indians. Father Dablon, in the Relations for 1669–'70, describes these places, of which he was informed by the Indians. The first was Michipicoten Island, on the east shore of the lake; then came St. Ignace, on the north shore, and then Isle Royale, "celebrated for its copper, where could be seen in the cliffs several beds of red copper separated from each other by layers of earth." The other principal locality was the Ontonagon river, from which place the French had received a copper specimen three years previously which weighed 100 pounds. The Indian (Ottawa) women of this region, the father says, while digging holes for corn, used to find pieces of copper (float copper) weighing 10 and 20 pounds. A hundred years later Alexander Henry mentions the same thing of this locality, and adds that the Indians beat the pieces of copper into bracelets and spoons. Father Dablon goes on to say that opinions differed as to the place the Ontonagon copper came from some thinking it was near the forks of the river and along the eastern branch (near the old workings), while other guessers placed it elsewhere.

The information the Indians gave was not spontaneous, for Father Dablon says that it required some address to induce them to reveal the mineralogical secrets which they wished to conceal from the whites. This reluctance to give information about mineral localities has survived down to a very recent period, and stories are known to the older residents of the copper district, some of them amusing enough, illustrating this trait. At all events, Father Dablon's Indians knew precisely where the old mining localities were. He says he was assured that in the land to the south there were deposits (*mines* is the French word) of the metal in various places. He had just been speaking of Keweenaw Point, but the connection is not close enough to warrant the inference that he meant immediately to the south of the point. If that could be shown, there would be a direct reference to the "diggings" on the peninsula.

But most of the misapprehension in this matter has arisen from the use of the misleading term "mine" in connection with this district. We associate with that term shafts or tunnels and under-ground workings, none of which ever existed on the lake. The ancient miners were not miners in the proper sense of the word as were those prehistoric men who mined copper ore in the Tyrol, or those other prehistoric miners who sank shafts and ran drifts in the chert deposits of Belgium. On the contrary, they were, as has been abundantly shown, only surface prospectors, and appear to have dug for copper wherever they happened to find it. If the pieces were loose float in the gravel, as at the Quincy location, and as the Ottawa squaws found them at Ontonagon, in 1670, and the later Indians in Henry's and Schoolcraft's time, well and good, they "mined" them and beat them into shape. If the copper was in huge masses on the surface as at the Mesnard they "mined" it in that shape by working off pieces with their stone hammers. If the copper was fast in the rock they broke it out by hammering the rock away from it, and if the rock extended into the ground they dug down around it, broke away what "barrel work" they could, and treated the "mass" as they did that already dug for them on the surface. They had no idea corresponding to the word mine. Hence there is no apparent reason why there should have been much of a distinction in the minds of people who were not miners between places where they dug copper out of the gravel, as in the trenches at Quincy, and places where they were obliged to dig around rocks to obtain it.

It is largely the undue emphasis upon the idea of mining that has led writers to create another race than the Indians to practice that skilled art on Keweenaw Point, Isle Royale, and the Canadian shore. The false or exaggerated idea has led to an equally exaggerated inference. All this is well illustrated in a passage in Wilson's "Prehistoric Man," describing an interview with an old Chippewa chief some fifty years ago. He was asked about the ancient copper miners, and declared that he knew nothing about them. The Indians, he said, used to have copper axes, but until the French came and blasted the rocks with powder they had no traditions of the copper mines being worked. His forefathers used to build big canoes and cross the lake to Isle Royale, where they found more copper than anywhere else. This is a distinct tradition enough of one famous copper locality—Isle Royale—although it may be unreliable from its late date, but the story shows how the belief that the Indians had no tradition of the old mines could originate. The old chief very properly denied knowing about a thing that never existed. His ancestors never carried on mining, but only digging. Deep mines, where blasting is done, which very likely he had seen, were, of course, unknown to them.

Like this old chief, Father Dablon's Indians showed full traditional knowledge when they told him of the mineral localities where, several generations before, copper had been extensively dug. The ancient trenches in the woods had long been covered and contained no visible copper. They possessed only an antiquarian interest to which the Indians were strangers, and also, as Father Dablon relates, his Indian friends were not disposed to give more information than they could help.

The first systematic exploring or "prospecting" party to search for the Ontonagon lode was sent out from Quebec about the same year that Father Dablon described the place, viz, 1669. The expedition returned without accomplishing its object for want of time, and was met on Lake Erie by La Salle's party going to the Mississippi. No mining was done there until a hundred years later under Alexander Henry.

The foregoing extracts from the account of Cartier's voyage, the Abbé Sagard, the Jesuit Relations, and Margry, show the continuity of the ancient or pre-Columbian mining on Lake Superior and the modern. As soon as the French arrived at the St. Lawrence in 1535, they found the natives knowing proportionately as much about the distant source of the copper they possessed as the ordinary eastern citizen does now. Over a hundred years later, after settlements had been made, there was still living knowledge that copper came from Lake Superior, and especially the Ontonagon River, where it was easy to find float copper. But during this long period active importation of European articles had been going on so that, as the Chippewa chief explained, native industries, including the search for copper, had been interrupted. Iron articles, knives, hatchets, weapons, and innumerable other desirable things made it unnecessary for the Indians to exert themselves in exploiting the old source of supply. But when the French began to inquire for copper they were taken to the precise localities where the metal had formerly been obtained which, like all mining districts, were full of abandoned and forgotten workings, and they were shown the metal in place.

Native copper, as has been said, occurs sparingly in several places in the eastern part of the country. In the Appalachian region ores of copper occur and have been extensively mined, but native copper does not occur there except as a mineralogical rarity. Nevertheless it has been suggested that copper was produced in that part of the country in pre-Columbian times. If this were so there should be evidences of old mines and of smelting operations of some kind, because copper ore must be smelted to produce the metal. No old workings in that region have, however, yet been identified as pre-Columbian copper mines, and no traces of aboriginal smelting have been discovered to support the suggestion. Ancient mica mines have, indeed, been discovered in North Carolina which are now worked, but if the Indians mined for copper at all in that mineral district the fact remains to be proved. Moreover, the Smithsonian collection, so far from showing a comparative abundance of copper articles from the Appalachian region, as would be expected if it had been a center of distribution like Keweenaw and Ontonagon in the North, has remarkably few copper relics from the Carolinas, Georgia, Alabama, and Tennessee. The idea doubtless arose from the statements in the accounts of the Spanish explorers of this region and of the French and English colonies on the coast. De Soto's march was a continuous pursuit of an *ignis fatuus.* He was told that gold or copper and other riches were in the Appalachians, and was kept perpetually on the move after them, while they eluded him in the most tantalizing manner. He did find pearls, and probably in large quantities; the contents of graves show that that form of wealth really existed. But that other form of wealth—"a melting of gold or copper"—which he coveted, kept moving before him from town to town and tribe to tribe all through his weary journey, and he never found it. The Spaniards on the Florida coast in the following years were persuaded that there was great mineral wealth of some kind in the Appalachians, and told of a town in the region where the minerals were supposed to be, which they called La Grand Copal. This town was said to be 60 leagues northwest of St. Helena, on the South Carolina coast.

De Soto's march was undertaken in 1539. In 1562 the French established a short-lived colony at Port Royal, S. C., under Capt. Ribault, which was succeeded two years later by another at the river of May (the St. John's), in charge of René Laudonnière, the history of which, with its tragic end, was brought prominently to notice by Parkman some years ago. Laudonnière wrote a full description of the resources of the country, in the course of which he says (Hakluyt's translation), "there is found amongst the savages good quantitie of gold and silver which is gotten out of the shippes that are lost upon the coast, as I have understood by the Savages themselves. They use traffique thereof one with another. And that which maketh me the rather believe it, is that on the coast towards the cape, where commonly the shippes are cast away, there is more store of silver than towards the north. Neverthe-less, they say that in the mountains of Appalatcy there are mines of copper, which I thinke to be golde." From these mountains came "two stones of fine christal," which were presented to the French, together with a number of pearls, and they learned from the Indians that there was "an infinite quantity of slate stone, wherewith they made wedges to cleave their wood," in the same mountains. A "king" of the country lying near these mountains sent Laudonnière "a plate of a minerall that came out of this mountaine, out of the foot whereof there runneth a streame of golde or copper, as the savages thinke, out of which they dig up the sand with an hollow and drie cane of reed until the cane be full; afterward they shake it, and finde that there are many small graines of copper and silver among this sand; which giveth them to understand that some rich mine must needs be in the mountaine."

If the Spaniards had not been "prospecting" through this part of the country twenty years before, this would be a most interesting account of primitive panning, an operation familiar to all gold prospectors and known in many parts of the world. But the suspicion arises that the Indians had watched the Spaniards operating in this way in the streams in their search for gold and were describing their method. The description, moreover, could not apply to copper, although it is true of gold, which is found in the sands of the streams, and is "panned out" in the manner described. The effort to find copper from this mineral region was unavailing. On Ribault's arrival to succor Laudonnière's party, the Indians offered to conduct him, in a few days' journey, to the mountains of Apalatey. "In those mountaines, as they sayd, is found redde copper, which they call in their language Sieroa Pira, which is as much to say as redde mettall, whereof I had a piece, which at the very instant I showed to Captaine Ribault, which caused his gold finer to make an assay thereof, which reported unto him that it was perfect golde." This assay confirms, or perhaps was the cause of, Laudonnière's surmise that the copper of Apalatey was gold. It is not easy to understand at this distance why there should have been any difficulty in recognizing the metal at once. There was evidently some misunderstanding or misinterpretation of the questions and the answers between the French and Indians in reference to the red metal, so that while the French meant copper the Indians understood gold. At any rate, the French saw no copper from the Appalachians.

Sir Walter Raleigh planted a colony at Roanoke Island in 1585, of which Ralph Lane was superintendent. He, also, soon heard of mineral wealth in the mountains to the west, and was eager to find copper there. It must be remembered that it was a great disappointment in Europe to find that the land which Columbus and his successors had discovered was a continent, and incessant attempts were made to find a way through or around it to the south seas and Cathay, which were continued into the present century. Therefore Ralph Lane wrote that "the discoverie of a good mine by the goodnesse of God, or a passage to the south sea, or some way to it, and nothing els can bring this countrey in request to be inhabited by our nation." And particularly with reference to the rumored mine to the west, he says: "And that which made me most desirous to have some doings with the Mangoaks,* either in friendship or otherwise to have had one or two of them prisoners, was, for that it is a thing most notorious to all the countrey, that there is a Province to the which the said Mangoaks have recourse and trafique up that river of Monatoc (Roanoke) which hath a marveilous and most strange Minerall. This mine is so notorious amongst them as not only to the savages dwelling up the said river and also to the savages of the Chawanook, and all them to the Westward, but also to all them of the maine; the countrey's name is of fame and is called Chaunis Temoatan.

* Indians who lived in Virginia, near the North Carolina line.

"The minerall they say is Wassader which is copper, but they call by the name of wassader every mettall whatsoever: they say it is of the colour of our copper, but our copper is better than theirs, and the reason is for that it is redder and harder, whereas, that of Chaunis Temoatan is very soft and pale. - - - Of this mettall the Mangoaks have so great store, by report of all the savages adjoining, that they beautify their houses with great plates of the same." Chaunis Temoatan, or the mineral country, was said to be twenty days' journey from the Mangoaks.

This account contains a variation of the description given the French twenty years before, of washing or panning out, but in the English account there is a distinct reference to melting or smelting. The Indians told Lane that after the material from the stream was caught in a bowl it was "cast into a fire, and forthwith it melteth, and doeth yield in five parts at the first melting, two parts of mettall for three parts of oare." It is impossible to understand this statement as it stands. It may possibly have referred to the use of fire in getting out the mica, or may have been a tradition of some Spanish operations obscured by time and confused by interpretation. The story survived into the next century. The English, however, did not see this operation, nor did they see any "greate plates" of copper. The only things of the kind were small, probably like those found in graves and mounds. "An hundred and fifty miles into the maine," Lane continues, "in two towns we saw divers small plates of copper, that had been made, as we understood, by the inhabitants that dwell further into the country, where, as they say, are mountains and rivers that yield also white grains of mettall which is to be deemed silver." If the Indians had possessed large plates the English would doubtless have seen them as well as the small, and some of them would have turned up before now, as the smaller ones have, in graves.

That extensive mines really existed in the region indicated by the Indians, which produced a peculiar mineral in abundance, will appear when we put together the Spanish, French, and English accounts of the rumored mineral wealth and the region from which it came, and compare them with the results of modern discovery. The Spaniards were after gold, and learned, as they believed, that it was to be found in the Appalachians, because when they asked after a country rich in mineral they were referred there. Laudonnière speaks of a singular mineral which was sent to him, which occurred in plates and was found in the Appalachians, together with "christal" and slate stone; and Ralph Lane hears of a "marveilous and strange" mineral which occurred in large plates with which the Indians adorned their houses. The mine, he says, was "notorious" in the whole country, and was in the mountains to the west of Roanoke. This mineral, which was not copper or any ore of copper, occurring in large plates which were paler and softer than copper, was undoubtedly mica, and the ancient mines which were the cause of the early mining excitement were re-discovered in the mountains of North Carolina in 1868. Prof. Kerr, who was State geologist of North Carolina, thus describes them:* "There is one point of great interest connected with the history of mica-mining in this State which it is worth while to refer to in this connection. This industry is not really new here; it is only revived. The present shafts and tunnels are continually cutting into ancient shafts and tunnels, and hundreds of the spurs and ridges of the mountains, (all over Mitchell County especially), are found to be honeycombed with ancient workings of great extent, of which no one knows the date or history. In 1868 my attention was first called to the existence of old mine holes, as they are called in the region. Being invited to visit some *old Spanish silver mines* a few miles south of Bakersville, I found a dozen or more open pits, 40 to 50 feet wide by 75 to 100 feet long, filled up to 15 or 20 of depth, disposed along the sloping crest of a long terminal ridge or spur of a neighboring mountain. The excavated earth was piled in huge heaps about the margins of the pits, and the whole overgrown with the heaviest forest trees, oaks, and chestnuts, some of them 3 feet or more in diameter and some of the largest belonging to a former generation of forest growth, fallen and decayed, facts which indicate a minimum of not less than three hundred years. There is no appearance of a mineral vein and no clew to the object of these extensive works, unless it was to obtain the large plates of mica, or crystals of kyanite, both of which abound in the coarse granite rock. - - - Since the development of mica-mining on a large scale in Mitchell and the adjoining counties it has been ascertained that there are hundreds of old pits and connecting tunnels among the spurs and knobs and ridges of this rugged region, and there remains no doubt that mining was carried on here for ages and in a very systematic and skillful way; for among all the scores of mines recently opened, I am informed that scarcely one has turned out profitably which did not follow the old workings and strike the ledges wrought by those ancient miners. The pits are always open 'diggings,' never regular shafts; and the earth and debris often amount to enormous heaps."

This description would apply almost word for word to the Lake Superior copper diggings. The mineral is taken out in large lumps, 30 or 50 up to several hundred pounds in weight, which split readily into plates or sheets, sometimes 3 feet in diameter, and would cut 16 by 20 inches. The common forms are 2 or 3 by 4 or 6 inches. All this confirms and explains very fully the statements of the Spanish, French, and English explorers and colonists of the sixteenth century. Now that we know what the mineral or "mettall" was, we understand and can explain away the confusion which arose in the inquiry after copper. The thing which was valuable to the Indians, so valuable that they adorned their dwellings with it and placed it, with other valuables, in

*Report of the Geological Survey of North Carolina, vol. I, 1875, p. 300.

their graves, was naturally prominent in their minds when the strangers were inquisitive about riches, and they answered according to their light. It does not appear that copper was known to the Southern Indians except as an article of barter, as it was all along the coast, but mica held the place with them in point of production that copper occupied with the Northern Indians.

Reviewing, now, the whole evidence—historical, mineralogical, and, to a slight extent, archaeological—it appears that when this continent was revealed to Europeans the natives of the country were in the full neolithic period, but were using copper to a slight extent. They were probably mining it in a desultory way in the Keweenaw workings just as they were mining mica in the mountains of North Carolina. How long this had been going on it is impossible to say. The metal was principally used for ornamental purposes in the South, where it was scarce, but where it was plentiful, in the North, and particularly toward the center of production, it was put to a practical use. There is at present no evidence that the Indians had any knowledge of smelting, which art is necessary to a real metal age. The progress from stone, through copper, to bronze could hardly be expected on the northern and eastern parts of this continent, because there was no tin available in the northern and eastern parts of the country with which to make bronze. To be sure the Indians had distant neighbors in Mexico and Central and Southern America, some of whom possessed the rudiments of smelting and were in an incipient bronze "age," from whom a knowledge of smelting, whereby copper could be obtained from its ores, might possibly have been acquired in the course of centuries by the slow process of aboriginal intercourse, if all native industrial development had not been interrupted by the intervention of Europeans. As it was, however, it seems clear that metallurgy was not known among the North American Indians when this continent was discovered.

THE POLYNESIAN BOW.*

By E. Tregear.

Perhaps one of the most puzzling problems known to anthropologists is to account for the apparent dislike shown by the fair Polynesians for the use of the bow and arrow. They found the mighty weapon of the archer in the hands of almost every Melanesian or Papuan inhabitant of the neighboring islands; they had experience of its fatal powers, and yet, except in the case of the Tongans, the weapons appeared to be viewed with disfavor and neglect.

The bows used by the Tongans in the days of Cook were slight and by no means powerful instruments. Each bow was fitted with a single arrow of reed, which was carried in a groove cut for that purpose along the side of the bow itself. By the time that mariner arrived among these islanders, in 1806, they had possessed themselves of more powerful bows and arrows, probably procured from Fiji or imitated from Fijian weapons, as constant intercourse of either warlike or pacific character was then going on between the Friendly and Fijian islands. Moreover, they had also procured guns at that epoch.

The Hawaiian weapons were spears, javelins, clubs, stone axes, knives, and slings; the use of the bow being confined to rat shooting. The Tahitians used the bow only as a sacred plaything; the bows, arrows, quiver, etc., being kept in a certain place in charge of appointed persons and brought out on stated occasions. The arrow was not aimed at a mark, but merely shot off as a test of strength and skill, one archer trying to shoot farther than another. The Samoans did not use the bow, but fought with the club and spear, the sling being the missile weapon, as it also was in the Marquesas.

In regard to New Zealand, the subject has been handled at any length only by two writers. The first was Mr. C. Phillips, whose paper appeared in the *Transactions of the New Zealand Institute*, vol. x, p. 97. The article did not deal with the bow proper so much as with the weapon known to the Maoris as *kotaha*, which consists of a stick and whip with which a spear is thrown. Mr. Phillips made some incidental remarks on this paper, which provoked Mr. Colenso to reply in an article published in the *Transactions of the New Zealand Institute*, vol. xi, p. 106.

*From *The Journal of the Polynesian Society* (Wellington, New Zealand), for April, 1892; Vol. i, pp. 56–59.

Mr. Colenso's argument, briefly summarized, refers to the subject as follows. He considers—

First. That the bows and arrows found in the hands of Maori children were probably imitated from models shown to them by Tupaea, the Tahitian interpreter brought to New Zealand by Capt. Cook, or, perhaps, from models shown by foreigners, some of whom—notably a Hindoo, a Marquesan, and a Tahitian—were resident among the Maoris when the Rev. Mr. Marsden arrived in 1814.

Second. That neither Tasman, Cook, Parkinson, Forster, Crozet, Polack, Cruise, Nicholas, Marsden, nor any other of the early visitors to New Zealand mention seeing the bow or hearing of its use. That Mr. Colenso himself, in his frequent journeys about the country (in 1834) and continual listenings to stories of war, never heard of the bow being used in combat.

Third. That there is no mention in old legends of the bow being used as a weapon, either in the stories of the destruction of monsters, the deaths of chiefs in battle, or in the lists of arms, although these lists are given with great fidelity and attention to detail.

Of these three divisions, the first is not scientifically decisive. It is possible, and even probable, that the Maoris were taught the use of the bow by early visitors, but it can not now be proven. The bow might have been kept as a childish toy, although not used as a weapon; exactly, for example, as with the modern English, with whom bows and arrows are playthings, although but a few years ago (ethnologically speaking) they were the national weapons.

The second argument is from negative evidence. There may have been bows and arrows in New Zealand, and yet they may not have been produced or spoken of in the presence of new-comers; but that such a reticence occurred is most improbable, and, although the evidence is negative, it is of great value. Few impartial people will belive that the bow was a weapon of the New Zealander during the last century if no explorer or missionary saw or heard of it.*

The third argument is an exceedingly important one. If in the lists of weapons mentioned in New Zealand tradition the bow has no place, the conviction left in the minds of most Maori scholars will be that the omission marks the absence of the bow itself from Maori knowledge.†

Time, however, has a modifying effect on opinion, and the one thing certain to come to the interested student of anthropology is a wondering faith in the power of Time to dissolve and form and redissolve not

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*In the *Auckland Weekly News* of April 16, 1892, is an account of an old Pakeha-Maori named John Harmon, who came to New Zealand a child in 1805, and is now dead. "He told a tale of a battle between the Ngati-whatua and the Ngati-maru in the Thames Valley which was fought out with bows and arrows." It would perhaps be well if some member of this Society resident among either of these tribes would make inquiries among the old men as to what circumstance gave rise to Harmon's story.

†On the other hand, I do not know of any list of weapons or legend of monster-killing which includes the *kotaha* as a weapon. Yet I am informed by Mr. Percy Smith that not only was he shown an old ruined *pa* which was conquered by spears or darts thrown more than a quarter of a mile by means of the "whip," but that he knows that they were in use at least two hundred years ago.

only the tribes of the earth, but our knowledge concerning them. I received lately a letter from a friend in the north of the North Island of New Zealand, who informed me that in digging a drain upon his property at Maungapai he came upon a bow in a perfect state of preservation. It was lying in a bed of sandy clay, the surface of which was apparently undisturbed and virgin. The finder proceeded (in the usual fashion which horrifies archaeologists) to clean his treasure trove; but, luckily, before he had finished his work of scraping and oiling the bow, a friend interfered, and the original soil adheres to a portion of the weapon.

I have deposited the bow in the Museum for safe-keeping. It is 6 feet 4¾ inches in length; in shape resembling the bows of Fiji, the New Hebrides, and other Melanesian islands. It is almost certainly a war-bow, and it would try the strength of an athletic man to draw an arrow to the head upon so stiff an arc. It was unaccompanied by any relics whatever.

Several methods of accounting for the deposit of the bow in the locality might be suggested. It might have been buried in modern times by a European or by a visiting native of the South Sea islands. This is improbable, as the weapon must have been of some value to its owner, and is too large to have been easily lost. Again, the bow, if not a Maori weapon, might have belonged to some prehistoric inhabitant. There seems to be a concensus of tradition that the Polynesian and Malayan islands were once peopled by races exterminated or driven inland by the present occupiers of the seaward positions. In New Zealand many scholars believe that the Maori immigration dispossessed a people then in occupation.* If, on further testing, the bow should be found to be of Melanesian pattern, but of New Zealand wood, it would strengthen the theory that a people of Melanesian origin once occupied this country.

The evidence brought forward by Mr. Colenso in his paper makes it almost certain that no Maori within historical times has used the bow as a weapon. But did the *ancient* Maori use the bow? If we turn to comparative philology the answer is probably in the affirmative. The evidence stands thus:

MALAYSIA.

†Malay, *panah*, a bow.
Java, *panah*, a bow.
Bouton, *opana*, a bow.
Salayer, *panah*, a bow.
Cajeli, *panah*, a bow.
Massaratty, *panat*, a bow.
Ahtiago, *banah*, a bow.
Baju, *panah*, a bow.
Magindano, *pana*, an arrow.

*Much of interest on this subject can be found in Major Gudgeon's articles in the *Monthly Review* (Wellington, New Zealand, Lyon and Blair), vol. II, pp. 585 and 517. See also the article on flint arrowheads found near Wellington, by Mr. T. W. Kirk, *Transactions of the New Zealand Institute*, XIII, 436.

†It is said by Malay scholars that the Malay word *panah*, "a bow," is connected with the Sanscrit word *vana* or *bana*, "arrow." This variation as to "bow" and "arrow" may be found in the islands; but, if connected with Sanscrit, the word "goes ashore" into Asia.

PHILIPPINES.

Tagal, *pana*, a bow.
Bisaya, *pana*, a bow.

MELANESIAN ISLANDS.

Nengone, *pehna*, a bow.
Aneityum, *fana*, a bow.
Rotuma, *fan*, a bow.
Fiji, *fana*, to shoot with a bow.
Fiji, *vana*, to shoot.
Eddystone Island, *umbana*, an arrow.
New Britain, *panah*, a bow.
Santa Cruz, *nepna*, an arrow.
Florida, *vanahi*, to shoot.

POLYNESIAN PROPER.

Tahiti, *fana*, a bow; *fa'a-fana*, to guard property.
*Tongan, *fana*, to shoot; the act of shooting.
Samoan, *fana*, to shoot; *fanau*, a bow; *aufana*, a bow; *uāfana*, a volley of arrows.
Hawaiian, *pana*, a bow; to shoot as an arrow; *panapua*, an archer.
Rarotongan, *ana*, a bow (dialect drops *f* and *wh*).
Marquesan, *pana*, a bow.
Futuna, *fana*, a bow; to hunt.

In these comparatives we have evidence in a direct chain through the Malay, Melanesian, and Polynesian islands of a clearly marked word *fana* or *pana*, as "bow," the probable root being √ FAN or √ PHAN. In New Zealand the equivalent for the Polynesian *F* is *WH* (as *fare*, "a house," becomes *whare*, etc.); consequently we must expect to find the word as *whana*. The Maori word *whana* means "to recoil or spring back as a bow;" "a spring made of a bent stick, as a trap." When we compare the compound words, *tawhana*, bent like a bow; *kowhana*, bent, bowed; *korowhana*, bent, bowed, etc., there can be little doubt but that *whana* originally with the Maori meant what it did with all other Pacific islanders, viz, "a bow," and that they knew its use as a weapon. Just as the Maori words *amatiatia*, *taurua*, etc., for the double canoe or outriggered canoe prove former use, even though the modern Maori knows nothing of such vessel. The other Maori forms, *pana*, "to thrust away," and *panga*, "to throw," have taken slightly divergent meanings.

The Maori word *pewa*, meaning "arched, bow-shaped," and "the eyebrows" (with its compound, *koropewa*, "a loop or bow") also probably signified a weapon. *Pewa* has been preserved as "bow" by the Motu people of New Guinea (a Polynesian colony among Papuans), but may be a foreign word, since it has no universality in the Pacific as *fana* has.

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*On page 61 of Mr. Codrington's "Malanesian Languages" appears a note by Mr. Fison as to the Tongans having got the word *fana* with the bow from Fiji. No authority is greater with regard to Melanesian speech than is the opinion of Mr. Fison, but I believe in this matter that he had been misled by his native informant. In the first place, the bow had been in use long before the lifetime of the native in question began, and this makes the etymology of the name beyond his knowledge except as a guess; and, in the second, the wide distribution of the word among Polynesians makes it probable that the Tongans used the same word as the rest of their nation, and did not need to borrow from Fiji.

HERTZ'S EXPERIMENTS.*

I.

"Oh! yes; I understand it all now. Electricity is the æther;" or, "Yes; it's just like everything else: electricity is a vibration." These are the remarks one hears made by those who think that a few scattered words picked up at a popular lecture make things quite clear. It is no doubt unfortunate that repeating a form of words is a different matter from understanding them, and still more different from understanding the subject they are intended to explain. In this case there is the added misfortune that the form of words is not accurately repeated, and in its inaccurate form does not mean what is true. It is often hardly worth while remarking this to those who make these statements, because the words convey to them little or no signification, and are to them as true as any other unmeaning sentence. The connection between electricity and the æther is certainly not, as far as is known, well described by saying that "electricity is the æther," and we can not say with any certainty that electricity is or is not a vibration. Hertz's experiments have given an experimental proof of Maxwell's theory that electrical phenomena are due to the æther, and Herzt's experiments deal with vibrations. One can not however say, because the pressure of 15 pounds per square inch exerted by the atmosphere is due to the air, that therefore "pressure is the air"; nor even, because a person who studied the properties of the air had studied them by means of sounds propagated through it, can one assert that "pressure is a vibration." It is to be hoped no one will now assert that "electricity is pressure." The example is given to illustrate the absurdity of the statements made as deductions from recent experiments, and not to teach any new theory. And yet one comes across people who, after listening to an interesting lecture Lord Rayleigh might give, illustrated by Mr. Boys's sound-pressure meter, would make the above statements, and really think they understood them.

The subject is very difficult; one that has engaged the attention of thoughtful and clever men for many years, and is still in many parts, even to the most acute, shrouded with difficulties, uncertainties, and things unknown, so that nobody need be the least ashamed of not fol-

* From *Nature*, April 9, 1891; vol. XLIII, pp. 536–538; and May 7 and 14, 1891; vol. XLIV, pp. 12–14, and 31–35.

lowing even as far as others can go into this wonderful region. If the present articles can give to most who read them glimpses which unfold intelligible ideas of even the outskirts of this region, it is all that any writer can reasonably expect who is not one of those masters of exposition who combine the highest scientific and literary abilities.

Consider for a minute the question at issue. That electric and magnetic phenomena are due to the same medium by which light is propagated—that all-pervading medium by whose assistance we receive all the energy on this earth that makes life here possible, by which we learn the existence of other worlds and suns, and analyze their structures and read their histories; that medium which certainly pervades all transparent bodies, and probably all matter, and extends as far as we know of anything existing: this wonderful all-pervading medium is the one we use to push and pull with when we act by means of electric and magnetic forces; and remember that we can pull molecules asunder by this means as well as propel trains and light our houses. The forces between atoms are controlled by this all-pervading-medium, which directs the compass of the mariners, signals around the globe in times that shame e'en Shakespeare's fancy, rends the oak, and terrifies creation's lords in the lightning flash. It was a great discovery that proved all concord of sweet sounds was due to the medium that supplies the means of growth to animals and plants, and deals destruction in the whirlwind; and yet the 80 miles depth of our air is but an infinitesimal film compared with the all-pervading illimitable æther.

That there is a medium by which light is transmitted in a manner somewhat analogous to that by which the air transmits sound has been long held proved. Even those who held that light was due to little particles shot out by luminous bodies were yet constrained to superpose a medium to account for the many strange actions of these particles. Now, no one thinks that light is due to such particles, and only a very few of those who have really considered the matter think that it can be due to air, or other matter such as we know. How does light exist for those eight minutes after it has left the sun and before it reaches the earth? Between the sun and earth there is some matter, no doubt, but it is in far-separated parts. There are Mercury and Venus, and some meteors and some dust no doubt, and wandering molecules of various gases, many yards apart, that meet one another every few days, perhaps, but no matter that could pass on an action from point to point at a rate of thousands of miles each second. Some other medium must be there than ordinary gross matter. Something so subtle that the planets, meteors, and even comets—those wondrous fleecy fiery clouds rushing a hundred times more quickly than a cannon-ball around the sun—are imperceptibly impeded by its presence, and yet so constituted as to take up the vibrations of the atoms in these fiery clouds and send them on to us a thousand times more rapidly again than the comet moves, to tell us there is a comet, and teach us what kinds of atoms vibrate in its tail. How can a medium have these contrary properties? How can it offer an imperceptible resistance to the comet, and yet take up the vibrations of the atoms? These are hard questions, and science has as yet but dim answers to them, hardly to be dignified by the name of answers—rather dim analogies to show that the properties supposed to co exist, though seeming contradictory, are not so in reality.

One of the most beautiful experiments man knows—one fraught with more suggestions than almost any hundred others—is that by which a ring of air may be thrown through the air for many yards, and two such rings may hit, and shivering, rebound. These rings move in curved paths past one another with almost no resistance to their motion, urged by an action not transmitted in time from one ring to another, but, like gravitation, acting wherever a ring may be, and yet the air through which they move *can* take up vibrations from the rings showing thus that there is no real contradiction between the properties of things moving through a medium unresistedly in certain paths round one another, and yet transmitting other motions to the medium. This same air can push and pull, as when it sucks up waterspouts and deals destruction in tornadoes. Hence there seems no real contradiction between a medium that can push and pull and transmit vibrations, and yet offer no resistance to such fragile, light, and large-extended things as rings of air.

It is important to understand something about the properties that this medium must have in order to explain light, electricity, and magnetism, because there is no use expecting a medium to possess contradictory properties. It is also well to recollect that for about two hundred years the existence of a medium by which light is propagated has been considered as certain, and that it would be very remarkable if this medium, which can be set in vibration by material atoms, acted on matter in no other way. It seems almost impossible but that a medium which is moved by atoms, and which sets them into motion, should be able to move such armies of atoms as we deal with in material bodies. Even if we knew nothing of electricity and magnetism, it would be natural to look for some important phenomena due to the action of this medium on masses of matter. The medium is a *vera causa*, and if it can be shown that the same set of properties by which electric and magnetic forces are explained will also enable it to transmit vibrations that have all the properties of light, it will surely be beyond a doubt but that these electric and magnetic actions are those very ones we would naturally expect from the medium that propagates light.

Clerk Maxwell some years ago showed that this was so, but as far as any facts known at that time could prove, there were other theories of electric and magnetic actions which explained their known phenomena without the intervention of a medium. The matter stood somewhat thus: The older theories of electric and magnetic force explained all phenomena then known. These older theories assumed that electric and magnetic forces were propagated instantaneously throughout space; that if the sun became electrified it would instantaneously begin to induce electricity on the earth; that there would be no delay of eight minutes, such as occurs between a light occurring on the sun and its acting on the earth. Similarly in the case of magnetic actions, they were supposed to be propagated instantaneously throughout space. It was, no doubt, known that it took time for an electric signal to be transmitted along a conducting cable. This is however a very much more complicated problem than the simple one of supposing a body surrounded by a non-conductor to be electrified. Will it or will it not instantaneously act on all conductors in space, and begin to induce electrification on them? As far as was known such actions as this, actions through non-conducting space, were instantaneous. Such an instantaneous action could not be transmitted by the air. Air can not send on from point to point any effect more rapidly than a molecule of air can moving carry it forward, and that is only a little faster than the velocity of sound; and there was every reason to know that electric induction through air was propagated much more rapidly than that. There was every reason to believe that electric and magnetic forces acted without any material intervention.

In fact, in these older theories there was no thought of any medium to transmit the actions; it was supposed that electricity acted across any intervening space instantaneously. There is no real difficulty in such a supposition. As far as we know gravitation is just such an action, and as far as was then known there was no experiment that disproved the supposition in the case of electric and magnetic actions. It was known that no experiment had ever been devised that could test whether this action was instantaneous or whether it was propagated at a rate such as that of light. It was known that this action was enormously more rapid than sound, but as light goes about 300,000 times as fast as sound there was plenty of spare velocity. These older theories explained all that was known, and they supposed nothing as to the existence of an intervening medium. Any theory that assumed that induction was not instantaneous, but that energy having been spent on electrification at one place work would be done at another after some time, as in the case of light generated on the sun not reaching the earth for eight minutes, any theory that assumed such a disappearance of energy at one place and its re-appearance at another after the lapse of some time must assume some medium in which the energy exists after leaving the one place and before it reaches the other. A theory that only supposes instantaneous action throughout space need not assume the existence of a medium to transmit the action, but any theory that supposes an action to take time in being transmitted from one place to another must assume the existence of a medium. Now, Maxwell's theory assumed the existence of a medium, and along with that led to the conclusion that electric and magnetic actions were not propagated instantaneously, but were propagated with the velocity of light. According to his theory an electric disturbance occurring on the sun would not produce any effect on the earth for about eight minutes after its occurrence on the sun. No experiments were known to test the truth of this deduction until the genius of Hertz brought some of the most beautifully conceived, ingeniously devised, and laboriously executed of experiments to a brilliantly successful conclusion, and demonstrated the propagation of electric and magnetic actions with the velocity of light, and thereby proved experimentally that they are due to that same wonderful, all-pervading medium by means of which we get all the energy that makes life here possible.

The problem to be solved was, are electric and magnetic actions propagated from place to place in a finite time, or are they simultaneous everywhere? How can experiments be made to decide this? Consider the corresponding problem in sound. What methods are there for determining the rate at which sound is propagated? An experiment that measures the rate can tell whether that rate is finite or whether it is infinitely great. There are two important methods employed for measuring the velocity of sound. The second is really only a modification of the first direct method, as will be seen. The direct method is to make a sudden sound at a place and to find how long afterward it reaches a distant place. In this method there is required some practically instantaneous way of communicating between the two places, so that the distant observer may know when the sound started on its journey. A modification of the method does not require this. It depends on the use of reflection. If a sound be made at a distance from a reflecting surface, the interval of time between when the sudden sound is made and when the reflected sound (the echo) returns, is the time the sound took to travel to the reflector and back again. A well-known modification of this method can be applied if we can secure a succession of sudden sounds, such as taps, at accurately equal intervals of time. We originate such a regular succession of taps, and alter the distance from the reflector until each reflected tap occurs simultaneously with the succeeding incident tap. Or if the distance at which we can put the reflector be sufficiently great, we may arrange it to be such that a reflected tap is heard simultaneously with the second, third, fourth, or any desired succeeding tap. The coincidence of the taps with their reflections can be fairly accurately observed, and a fairly accurate estimate formed of the velocity of sound, *i. e.*, the velocity at which a compressing or rarefying of the air is propagated by the air. Instead of altering the distance of the source of sound from the reflector, we may ourselves move about between the source and the reflector, and we can find some places where the reflected taps occur simultaneously with the incident taps, and some places where they occur between the incident ones. This is pretty evident, for if we start from the source toward the reflector, as we approach it we get the reflected taps earlier and the incident ones later than when we were at the source. How far must we go toward the reflector in order that the original and reflected taps may again appear simultaneous? We must go half the distance that a tap is propagated during the interval between two taps—half the distance, because in going away from the source we are approaching the reflector and so make a double change—we not only get the original ones later, but we also get the reflected ones earlier, and so coincidence will have again been reached when we have gone half the distance between any pair of compressions travelling in the air. Now, if the taps succeed one another slowly, the distance in the air between any two of them travelling through it will be considerable; any one of them will go a considerable distance from the source before its successor is started after it. If, on the contrary, they succeed one another rapidly, the distance between the travelling taps will be small.

In general, if v be the velocity with which a tap travels, and t be the interval of time between successive taps, the distance apart of the taps travelling in the air will be $\lambda = vt$. By arranging, then, that the taps shall succeed one another very rapidly, *i. e.*, by making t small, we can arrange that λ may be small, and that consequently the distance between our source of sound and the reflecting wall may be small too, and yet large enough to contain several places at distances of $\frac{1}{2}\lambda$ apart between the source and the reflector where the incident and reflected taps occur simultaneously. Now, a very rapid succession of taps is to us a continuous sound, and where the incident and reflected taps coincide we hear simply an increased sound, while at the intermediate places where the incident taps occur in the intervals between the reflected taps we do not hear this effect at all. In the case of a succession of sharp taps we would hear in this latter place the octave of the original note, but if the original series be, instead of taps, a simple vibration of the air into and out from the reflector, the in and out motions of the incident waves will in some places coincide with the in and out motions of the reflected wave, and then there will be an increased motion, while at intermediate places the in and out motions of the incident wave will coincide with the out and in motions of the reflected wave, and no motion, or silence, will result, so that at some places the sound will be great and at intermediate places small.

This whole effect of having an incident and reflected wave travelling simultaneously along a medium can be simply and beautifully illustrated to the eye by sending a succession of waves along a chain or heavy limp rope or an India-rubber tube fixed at the far end so as to reflect the waves back again. It will then be found that the chain divides up into a series of places where the motion is very great, called loops, separated by points where the motion is very small, called nodes. The former are the places where the incident and reflected motions reinforce, while the latter are where these motions are opposed. If we measure the distance between two nodes, we know that it is half the distance a wave travels during a single vibration of the string, and so can calculate the velocity of the wave if we know the rate of vibration of the string. This is the second method mentioned above for finding the velocity of sound. There are so many things illustrated by this vibrating chain that it may be well to dwell on it for a few moments. We can make a wave travel up it, either rapidly or slowly, by stressing it much or little. If a wave travels rapidly, we must give it a very rapid vibration if we wish to have many loops and nodes between our source and the reflector; for the distance from node to node is half the distance a wave travels during a vibration, and if the wave goes fast the vibration must be rapid, or the distance from node to node will be too great for there to be many of them within the length of the chain.

Another point to be observed is the way in which the chain moves when transmitting a single wave and when in this condition of loops and nodes, *i. e.*, transmitting two sets of waves in opposite directions. There are two different motions of the parts of the chain it is worth considering separately. There is in the first place the displacement of any link up or down, and in the second place there is the rotation of a link on an axis which is at right angles to this up and down motion. Now, when waves are going up the chain those links are rotating most rapidly which are at any time most displaced: it is the links on the tops and bottoms of waves that are rotating most rapidly. On the other hand, in the case of loops and nodes the links in the middle of loops never rotate at all; they are much displaced up and down, but they keep parallel to their original direction all the time, while it is the links at the nodes where there is no displacement up and down that rotate first in one direction and then back again; there is, in the loops and nodes condition, a separation of the most rotating and the most displaced links which does not occur in the simple wave. There is a corresponding relation between the most rotated and the most rapidly moving links. These are the same links halfway up the simple waves, but in the loops and nodes the most rapidly moving links never rotate at all, while those at the nodes that get most rotated are not displaced at all. These remarks will be seen hereafter to throw light on some of the phenomena observed in connection with Hertz's experiments; hence their importance.

It will be observed that the method of measuring the velocity at which a disturbance is propagated along a string, and which depends on measuring the distance between two nodes, is really only a modification of the direct method of finding out how long a disturbance takes to go from one place to another: it is one in which we make the waves register upon themselves how long they took, and so does not require us to have at our disposal any method of sending a message from one place to another more quickly than the waves travel, and that is very important when we want to measure the rate at which disturbances travel that go as fast as light. If the wave travels very fast, we must have a very rapid vibration, unless we have a great deal of space at our disposal; for the distance between two nodes is half the distance the wave travels during one vibration, and so will be very long if the wave travels fast, unless the time of a vibration be very short. Hence, if we wish to make experiments in this way, in a moderate sized room, on a wave that travels very fast, we must have a very rapid vibration to start the waves.

II.

In the preceding article a general method of measuring the velocity at which a disturbance is propagated was described. It depended on being able to produce a regular succession of disturbances at equal intervals of time. These were made to measure their own velocity by reflecting them at an obstacle. Then, by the interference of the incident and reflected waves, a succession of loops and nodes are produced at intervals of half the distance a disturbance is propagated during the time between two disturbances. It is a general method applicable to any sort of disturbance that takes time to get from one place to another. It has been applied over and over again to measure the rate at which various kinds of disturbance are propagated in solids, liquids, and gases; it was applied in a modified form years ago, to measure the length of a wave of light; and, within the last year, some of the most beautiful experiments on photography ever described are applications of this principle by Herr Wiener and M. Lippman.

There are three things essential to this experiment: (1) Some method of originating waves; (2) some method of reflecting them; (3) some method of telling where there are loops and where there are nodes. We will take them in this order:

(1) How can we expect to originate electric waves? If, when a body is electrified positively, the electric force due to it exists simultaneously everywhere, of course we can not expect to produce anything like a wave of electric force travelling out from the body; but if, when a body is suddenly electrified, the electric force takes time to reach a place, we must suppose that it is propagated in some way as a wave of electric force from the body to the distant place. This of course assumes that there is a medium which is in some peculiar state when electric force exists in it, and that it is this peculiar state of the medium which we call electric force, existing in it, that is propagated from one place to another. It must be carefully borne in mind what sort of a thing this is that we call the electric force at any place. It is not a good name,—electric intensity would be a better one; but electric force has come so much into use it is hardly to be expected that it can be eradicated now. Electric force at any place is measured by the

mechanical force that would be exerted at the place if a unit quantity of electricity were there. It is not a force itself at all; it is only a description of the condition of the medium at the place which makes electricity there tend to move. The air near the earth is in such a condition that everything immersed in it tends to move away from the earth with a force of about 1.26 dynes for each cubic centimeter of the body, *i. e.* each cubic centimeter tends to move with a force of 1.26 dynes. Now, the condition of the air that causes this is never described as volume force existing at the place, though we do describe the corresponding condition of the æther as electric force existing there; and as volume force existing would be a very objectionable description of the condition of the air, when being at different pressures at various levels, it tends to make bodies move with a force proportional to their volume, so electric force existing is a very objectionable description of the condition of the æther, whatever it is, that tends to make bodies move with a force in proportion to their electric charges. We know more about the structure of the air than we do about the æther. We know that the structure of the air that causes it to act in this way is that there are more molecules jumping about in each cubic centimeter near the earth than there are at a distance, and we do not know yet what the structure of the æther is that causes it to act in this remarkable way; but even though we do not know the nature of the structure, we know some of its effects, by means of which we can measure it, and we can give it a name. Although we know very little indeed about the structure of a piece of stressed india rubber, yet we can measure the amount of its stress at any place, and can call the india rubber in this peculiar condition "stressed india rubber." As a matter of fact, we know a great deal more about the peculiar condition of the æther that we describe as "electric force" existing, than we do about the "stressed india rubber;" and there is every reason to suppose that the structure of the æther is, out of all comparison, more simple than that of india rubber.

When sound-waves travel through the air, they consist of compressions followed by rarefactions, and between them the pressure varies from point to point, so that here we have travelling forward a structure the same as that of the air near the earth, and waves of sound might be described as consisting of a succession of positive and negative "volume forces" travelling forward in the air; this form of expression would no doubt be objectionable, but still if all we knew about the properties of the air near the earth was that it tended to make bodies move away from the earth with a force proportional to their volume, it is quite likely that this condition of affairs near the earth might have been described as the existence of a "volume force" near the earth, and when it was discovered that this action was due to a medium, the air, it would have been quite natural to describe this state of the air as "volume force" existing in it; and then when waves of sound were observed it would be quite natural that they should be described as waves of "volume force," especially if the only way in which we could detect the presence of these waves was by observing the force exerted on bodies immersed in it, which was proportional to their volumes, and which we happen to know is really due to differences of pressure at neighboring points in the air. We do not know what is the structure of the æther that causes it to exert force on electrified bodies, but we know of the existence of this property, and when it is in this state we say that "electric force" exists in it, and we have certain ways by which we can detect the existence of "electric force," one of which is the production of an electric current in a conductor, and the consequent electrification of the conductor, and if this is strong enough we can produce an electric spark between it and a neighboring conductor. When a conductor is suddenly electrified, the structure of the æther which is described as electric force existing in it travels from its neighborhood through the surrounding æther, and this is described as a wave of electric force travelling through the surrounding æther. It is desirable to be quite clear as to what is meant by the term a wave of electric force and what we know about it. We know that it is a region of æther where its structure is the same as in the neighborhood of electrified and some other bodies, and owing to which force is exerted on electrified bodies, and electric currents are produced in conductors.

We may then reasonably expect that, if it is possible to electrify a body alternately positively and negatively in rapid succession, there will be produced all round it waves of electric force—that is, if the electric force is propagated by, and is due to, a medium surrounding the electrified body, if electrification is a special state of the medium that fills the space between bodies.

(2) The next question is: How can we reflect these waves? In order to reflect a wave, we must interpose in its way some body that stops it. What sort of bodies stop electric force? Conductors are known to act as complete screens of electric force, so that a large conducting sheet would naturally be suggested as the best way to reflect waves of electric force. Reflection always occurs when there is a change in the nature of the medium, even though the change is not so great as to stop the wave, and it has long been known that, besides the action of conductors as screens of electric force, different non-conductors act differently in reference to electric force by differing in specific inductive capacity. Hence we might expect non-conductors to reflect these waves, although the reflection would probably not be so intense from them as from conductors. Hence this question of how to reflect the waves is pretty easily solved. All this is on the supposition that there really are waves. If electric force exist everywhere simultaneously, of course there will be no waves to reflect, and consequently no loops and nodes produced by the interference of the incident and reflected waves.

(3) The third problem is: How can we expect to detect where there are loops and where there are nodes? Recall the effects of electric force. It tends to move electrified bodies. If then an electrified body were placed in a loop it would tend to vibrate up and down. This method may possibly be employed at some future time, and it may be part of the cause of photographic actions, for these have recently been conclusively proved to be due to electric force; but the alternations of electric force from positive to negative that have to be employed are so rapid that no body large enough to be easily visible and electrified to a reasonable extent could be expected to move sufficiently to be visibly disturbed. It is possible that we may find some way of detecting the vibrations hereby given to the electrified ions in an electrolyte; and it has recently been stated that waves originated electrically shake the elements in sensitive photographic films sufficiently to cause changes that can be developed. The other action of electric force is to produce an electric current in a conductor and a resultant electrification of the conductor. Two effects due to this action have actually been used to detect the existence of the wave of electric force sent out by a body alternately electrified positively and negatively. One of these is the heating of the conductor by the current. Several experimenters have directly or indirectly used this way of detecting the electric force. The other way, which has proved so far the most sensitive of all, has been to use the electrification of the conductor to cause a spark across an air space. This is the method Hertz originally employed. *A priori*, one would not have expected it to be a delicate method at all. It takes very considerable electric forces to produce visible sparks. On the other hand, the time the force need last in order to produce a spark is something very small indeed, and hitherto it has not been possible to keep up the alternate electrifications for more than a minute fraction of a second, and this is the reason why other apparently more promising methods have failed to be as sensitive as the method of producing sparks. If two conductors be placed very close to one another in such a direction that the electric force is in the line joining them, their near surfaces will be oppositely electrified when the electric force acts on them, and we may expect that, if the force be great enough and the surfaces near enough, an electric spark will pass from one to the other. This is roughly the arrangement used by Hertz to detect whether there are loops and nodes between the originator of the waves and the reflector.

Now arises the problem of how to electrify the body alternately positively and negatively with sufficient rapidity. How rapid is "with sufficient rapidity?" To answer this we must form some estimate of how rapidly we may expect the waves to be propagated. According to Maxwell's theory they should go at the same rate as light, some 300,000,000 of meters per second, and it is evident that if we are going to test Maxwell's theory we must make provision for sufficiently rapid electric vibrations to give some result if the waves are propagated at this enormous rate. The distance from a node to a node is half the distance a wave travels during a vibration. If we can produce vibrations at the rate of 300,000,000 per second, a wave would go 1 metre during a vibration, so that, with this enormous rate of alternation, the distance from node to node would be 50cm. We might expect to be able to work on this scale very well, or even on ten times this scale, *i. e.*, with alternations at the rate of 30,000,000 per second, and 5 metres from node to node, but hardly on a much larger scale than this. It almost takes one's breath away to contemplate the production of vibrations of this enormous rapidity. Of course they are very much slower than those of light; these latter are more than a million times as rapid; but 300,000,000 per second is enormously more rapid than any audible sound, about a thousand times as fast as the highest audible note. A short bar of metal vibrates longitudinally very fast, but it would have to be about the thousandth of a centimeter long in order to vibrate at the required rate. It would be almost hopeless by mechanical means to produce electric alternations of this frequency. Fortunately there is an electric method of producing very rapid alternate electrifications. When a Leyden jar is discharged through a wire of small resistance, the self-induction of the current in this wire keeps the current running after the jar is discharged, and re-charges it in the opposite direction, to immediately discharge back again, and so on through a series of alternations. This action is quite intelligible on the hypothesis that electrifications consists in a strained condition of the æther, which relieves itself by means of the conductor. Just as a bent spring or other strained body, when allowed suddenly to relieve itself, relieves itself in a series of vibrations that gradually subside, similarly the strain of the æther relieves itself in a series of gradually subsiding vibrations. If the spring while relieving itself has to overcome frictional resistance, its vibrations will rapidly subside; and if the friction be sufficiently great, it will not vibrate at all, but will gradually subside into its position of equilibrium. In the same manner, if the resistance to the relief of the strain of the medium, which is offered by the conducting wire, be great, the vibrations will subside rapidly, and if the resistance of the wire be too great, there will not be any vibrations at all.

Of course, quite independently of all frictional and viscous resistances, a vibrating spring, such as a tuning-fork that is producing sound-waves in the air, which carry the energy of the fork away from it into the surrounding medium, will gradually vibrate less and less. In the same way, quite independently o. the resistance of the conducting wire, we must expect that, if a discharging conductor produces electric waves, its vibrations must gradually subside owing to its energy being gradually transferred to the surrounding medium. As a consequence of this the time that a Leyden jar takes to discharge itself in this way may be very short indeed. It may perform a good many oscillations in this very short time, but then each oscillation takes an exceedingly short time. To get some idea of what quantities we are dealing with, consider the rates of oscillation which would give wave-lengths that were short enough to be conveniently dealt with in laboratories. Three hundred million per second would give us waves 1 meter long; consider what is meant by 100,000,000 per second. We may get some conception of it by calculating the time corresponding to one hundred million seconds. It is more than three years and two months. The pendulum of a clock would have to oscillate three years and two months before it would have performed as many oscillations as we require to be performed in one second. The pendulum of a clock left to itself without weights or springs to drive it, and only given a single impulse, would practically cease to vibrate after it had performed 40 or 50 vibrations, unless it were very heavy, *i. e.*, had a great store of energy or were very delicately suspended, and exposed only a small resistance to the air. A light pendulum would be stopped by communicating motion to the air after a very few vibrations. The case of a Leyden jar discharge is more like the case of a mass on a spring than the case of a pendulum, because in the cases of the Leyden jar there is nothing quite analogous to the way in which the earth pulls the pendulum: it is the elasticity of the æther that causes the electric currents in the Leyden jar discharge, just as it is the elasticity of the spring that causes the motion of the matter attached to it in the case of a mass vibrating on a spring.

It is possible to push this analogy still further. Under what conditions would the spring vibrate most rapidly? When the spring was stiff and the mass small. What is meant by a spring being stiff? When a considerable force only bends it a little. This corresponds to a considerable electric force only electrifying the Leyden jar coatings a little, *i. e.* to the Leyden jar having a small capacity. We would consequently expect that the discharge of a Leyden jar with a small capacity would vibrate more rapidly than that of one with a large capacity, and this is the case. In order to make a Leyden jar of very small capacity we must have small conducting surfaces as far apart as possible, and two separate plates or knobs do very well. The second condition for rapid vibration was that the mass moved should be small. In the case of electric currents what keeps the current running after the plates have become discharged and re-charges them again, is the so-called self-induction of the current. It would be well to look upon it as magnetic energy stored up in the æther around the current, but whatever view is taken of it, it evidently corresponds to the mass moved, whose energy keeps its moving after the spring is unbent, and re-bends the spring again. Hence we may conclude that a small self-induction will favor rapidity of oscillation, and this is the case. To attain this we must make the distance the current has to run from plate to plate as short as possible. The smaller the plates and the shorter the connecting wire the more rapid the vibrations; in fact, the rapidity of vibration is directly proportional to the linear dimensions of the system, and for the most rapid vibrations two spherical knobs, one charged positively and the other negatively, and discharging directly from one to the other, have been used.

Hertz in his original investigations used two plates about 40^{cm} square, forming parts of the same plane, and separated by an interval of about 60^{cm}. Each plate was connected at the center of the edge next the other plate with a wire about 30^{cm} long, and terminating in a small brass knob. These knobs were within 2 or 3^{mm} of one another, so that when one plate was charged positively and the other negatively they discharged to one another in a spark across this gap. An apparatus about this size would produce waves 10 or 12 meters long, and its rate of oscillation would be about 30,000,000 per second. As the vibration actually produced by these oscillators seems to be very complex, the rate of oscillation can only be described as "about" so and so. In a subsequent investigation Hertz employed two elongated cylinders about 15^{cm} long and about 3^{cm} in diameter, terminated by knobs about 4^{cm} in diameter, and discharging directly into one another. Such an oscillator produces waves from 60 to 70^{cm} long, and consequently vibrations at the rate of between 400,000,000 and 500,000,000 per second. Most other experimenters have used oscillators about the same dimensions as Hertz's larger apparatus, as the effects produced are more energetic; but many experiments, especially on refraction, require a smaller wave to be dealt with, unless all the apparatus used be on an enormous scale, such as could not be accommodated in any ordinary laboratory. When we are thus aiming at rapid rates of vibration, it must be recollected that we can not at the same time expect many vibrations after each impulse. If we have a stiff spring with a small weight arranged so as to give a lot of its energy to the surrounding medium, we can not expect to have very much energy to deal with, nor many vibrations, and, as a matter of fact, we find that this is the case. The total duration of a spark of even a large Leyden jar is very small. Lord Rayleigh has recently illustrated this very beautifully by his photographs of falling drops and breaking bubbles.

We can not reasonably expect each spark to have more than from ten to twenty effective oscillations, so that, even in the case of the slower oscillator, the total duration of the spark is not above a millionth of a second. It is very remarkable that the incandescent air (heated to incandescence by the spark) should cool as rapidly as it does, but there is conclusive evidence that it remains incandescent after the spark proper has ceased, and consequently lasts incandescent longer than the millionth of a second. What is seen as the white core of the spark may not last longer than the electric discharge itself, and certainly does not do so in the case of the comparatively very slowly oscillating sparks that have been analyzed into their component vibrations by photographing them on a moving plate. The incandescent air remaining in the path of such discharge is probably the conducting path through which the oscillating current rushes backward and forward. Once the air gap has been broken through, the character of the air gap as an opponent of the passage of electricity is completely changed. Before the air gap breaks down it requires a considerable initial difference of electric pressure to start a current. Once it has been broken down, the electric current oscillates backward and forward across the incandescent air gap until the whole difference of electric pressure has subsided, showing that the broken air-gap has become a conductor in which even the feeblest electric pressure is able to produce an electric current. If this were not so, Leyden jars would not be discharged by a single spark.

All this is quite in accordance with what we know of air that is—or even has lately been—incandescent; such air conducts under the feeblest electric force. All this is most essential to the success of our oscillator. Only for this valuable property of air, that it gives way suddenly, and thence forward offers but a feeble opposition to the rapidly alternating discharge, it would have been almost impossible to start these rapid oscillations. If we wish to start a tuning fork vibrating we must give it a sharp blow; it will not do to press its prongs together and then let them go slowly; we must apply a force which is short-lived in comparison with the period of vibration of the fork. It is necessary then that the air-gap must break down in a time short compared with the rate of oscillation of the discharge; and when this is required to be at the rate of 400,000,000 per second, it is evident how very remarkably suddenly the air-gap breaks down. From the experiments themselves it seems as if any even minute roughness, dust, etc., on the discharging surface interfered with this rapidity of break-down; it seems as if the points spluttered out electricity and gradually broke down the air-gap, for the vibrations originated are very feeble unless the discharging surfaces are kept highly polished; gilt brass knobs act admirably if kept polished up every ten minutes or so. One of the greatest desiderata in these experiments is some method of making sure that all the sparks should have the same character and be all good ones.

III.

In the foregoing, the principles upon which a rapidly vibrating electric oscillator should be constructed have been considered, and how the sudden break-down of the air-gap enabled these rapid vibrations to be started. It is probable that this break-down occurs in a time smaller than the thousand-millionth of a second. How very rapid the interatomic motions must be!

Consider now the principles on which an apparatus is to be constructed to receive the vibrations produced by this oscillator. We may observe in the first place that as we are dealing with a succession of impulses at equal intervals of time we can utilize resonance to accumulate the effect of a single impulse. Resonance is used in an immense variety of circumstances to accumulate the effect of a series of impulses, and is avoided in another immense variety of circumstances to prevent accumulating the effect of a series of impulses. We see, we hear, we photograph by using it; we use it to make musical sounds, to keep clocks and watches going, to work telegraphs. By avoiding it carriages drive safely over rough roads, ships navigate the seas, the tides do not now overwhelm the land, the earth and planets preserve their courses round the sun, and the solar system is saved from destruction. Resonance may be thus described: If a system is able to vibrate by itself in any way, and if we give it a series of impulses, each tending to increase the vibration, the effect will be cumulative, and the vibration will increase. To do this the impulses must be well timed, at intervals the same as the period of vibration of the system itself. Otherwise some of the impulses will tend to stop the the vibration, and only some to increase it, and on the whole the effect will be small.

In order to use resonance in the construction of the detector of waves of electric force, we must make our detector so as to be capable of an electric vibration of the same period as the generator of the waves. If we do this we may expect the currents produced in it to be increased by each wave, and thus the electrification at its ends to increase, and so increase the chance of our being able to produce a visible spark. Two ways of using a detector have been mentioned. One is to observe the heating of a conductor by the current in it, and the other to observe a spark due to the electrification at the end of the conductor. The latter is the most sensitive and has been most frequently employed, and is the method first employed by Hertz. Two forms of detector may be used for observing sparks. One form consists of a single conductor bent into a circle with its two extremities very close together. An electric charge can oscillate from one end of this to the other round the circle and back again. If the circle be the proper size, about 70^{cm} in diameter for the large-sized oscillator and about 8^{cm} in diameter for the smaller-sized one described in the last article, the period of oscillation of this charge will be the same as that of the charge on the generator of the waves, and its oscillation will be increased by resonance until, if the ends of the circular wire be close enough together, the opposite electrification of the ends will become great enough to cause a spark across the gap. The other form of detector depends on using two conductors, each of which has the same period of electric oscillation as the oscillations we wish to detect. These are placed in such a position that an end of one is near that end of the other which will at any time be oppositely electrified. For example, if the electric force in our waves be in vertical lines, then if we place two elongated conductors, one vertically above the other and separated by a very small air space, the electric force alternating up and down will cause currents to run up and down the conductors simultaneously, and the upper ends of both will be similarly electrified at any instant, while the lower end of the upper one will always be oppositely electrified to the upper end of the low conductor, and if these two points, or two short wires connected with them, be close enough together, a spark will pass from one to the other whenever the electric force sets up these electric oscillations in the conductor. Thus this apparatus is a detector of the electric force. Whenever there is a spark we may be sure that there is electric force, and whenever we can not get a spark we may be sure that there is either no electric force or at any rate too little to produce sparks. The apparatus will be more sensitive for electric forces that oscillate at the same rate as the natural vibration of the electric charge on the conductor, because the effect of each impulse will then add to that of the last; resonance will help to make the electrifications great, and so there will be a better chance of our being able to produce a spark.

We may weaken the strength of this air-gap by reducing the pressure of the air in it. To do this the ends of the conductors, or wires connected with them, must lead into an exhausted air vessel, such as a Geissler's tube. There is no doubt that much longer sparks may thus be produced, but they are so dim and diffused that when dealing with very minute quantities of electricity those sparks in a vacuum are not more easily seen than the smaller and intenser sparks in air at atmospheric pressure. The additional complication and difficulty of manipulation from having the terminals in a vacuum are not compensated for by any advantages. This whole detecting apparatus works on somewhat the same principle as a resonator of definite size connected with one's ear when used to detect a feeble note of the same pitch as the resonator. Such a resonator might very well be used to find out where this note existed and where it did not. It would detect where there were compressions and rarefactions of the air producing currents of air into and out of your ear. In the same way the conductor sparking tells where there are alternating electric forces making currents alternately up and down the conductor, and ultimately electrifying the end enough to make it spark. In the sound resonator there is nothing exactly like this last phenomenon. We have much more delicate ways of detecting the currents of air than by making them break anything. If anybody would allow the electric currents from a Hertzian detector to be led directly into the retina of his eye, it would probably be a very delicate way of observing, though even in this direct application of the current to an organ of sense it is possible that these very rapidly alternating currents might fail to produce any sensible effect, for they are not rapid enough to produce the photo-chemical effects by which we see.

To recapitulate the arrangements proposed in order to detect whether electric force is propagated with a finite velocity, and if possible to measure it if finite, it is proposed to create electric oscillations of very great rapidity, oscillating some four or five hunded million times per second, and it is expected thereby to produce waves of electric force whose length will be less than a meter if they are propagated with the velocity of light. It is proposed to do this by causing an electric charge to oscillate backwards and forwards between two conductors, and across an air gap between them. This oscillating charge is to be started by charging the conductors, one positively and the other negatively, until they discharge by a spark across this air gap. By making the conductors small, and the distance the charge has to go from one to the other small, the rate of oscillation of the charge can be made as great as we require. If waves are produced by this arrangement, we can reflect them at the surface of a large conducting sheet, and then loops and nodes will be produced where the incident and reflected waves co-exist. The loops will be places where the alternating electric forces are great, while at the nodes there will be no electric forces at all. In order to detect where there are these alternating electric forces and where there are none, it is proposed to use either a single wire bent nearly into a circle, with a very minute air-gap between its ends, or else two conductors placed end to end, with a minute air gap between their ends. In either case, if the natural period of vibration of a charge on the single conductor, or on each of the conductors in the second arrangement, is the same as the rate of alternation of the electric force we wish to detect, there may be sufficient electrification of the neighboring ends to cause a spark across the minute air-gap. We are thus in possession of a complete apparatus for determining whether electric waves are produced, and what their wave length is.

The experiment is conducted as follows:

The two conductors which are to generate the waves are placed—say, one above the other, so that the electric charge will run up and down in a vertical line across the spark gap between them. They might be placed horizontally or in any other line, but for definiteness of description it is well to suppose some definite position. We may call them *A* and *B*. They are terminated in polished knobs, between which the spark passes. *A* and *B* are connected with the terminals of a Ruhmkorff coil, or a Wimshurst or other apparatus by which a succession of sparks may be conveniently made to pass from *A* to *B*. Before the spark passes, *A* and *B* are being electrified, and when the spark occurs the electricity on *A* rushes over to *B*, and part of it charges *B*, while the electricity on *B* rushes across the spark and partly charges *A*, this taking place alternately up and down. Each time there is less electricity, for some is neutralized during each oscillation by the opposite charge; for energy is being spent, some in overcoming the resistance of the spark gap, *i. e.*, in producing the heat developed there, and some in producing electric waves in the surrounding medium. Thus the electric energy of the two oppositely charged bodies *A* and *B* is gradually dissipated, and one way of describing this is to say that the two opposite electric charges combine and neutralize one another. This whole language of talking of electric charges on bodies, and electric currents from one to the other, of electric charges neutralizing one another, and so forth, is not in accordance with the most recent developments of electro-magnetic theory. At the same time, those for whom these articles are written are familiar with this language and with the view of the subject that it is framed to suit, while they are unfamiliar with æther electrically and magnetically strained and thereby the seat of electric and magnetic energy, and consequently it would have added very much to their difficulty in grasping the details of a complicated question if it had been described in unfamiliar terms and from an unfamiliar point of view.

The electric force in the neighborhood of the vertical generator will lie in vertical planes through it, and as *A* and *B* are alternately positive and negative, the electric force will alternately be from above downwards, and from below upwards. If then this force is propagated outwards in a series of waves, we may expect that all round our generator waves of electric force will be diverging; waves in which the force will be alternately down and up. The state of affairs might be roughly illustrated by elastic strings stretched out in every direction from our generator. If their ends at the generator be moved alternately down and up, waves will be propagated along the strings, waves of alternate motion down and up.

In order to reflect these waves we require a metallic sheet of considerable area some two or three wave lengths away from the generator; so far away in order that we may have room for our detector to find the loops and nodes formed every half wave-length where the outgoing waves meet those reflected from the screen; not too far away or our waves will be too feeble even at the loops to affect our detector. The waves are thrown off all round, but are most intense in the horizontal plane through the spark, so that our detector had better be placed as near to this plane as possible. The detector may be either a very nearly closed circle of wire or two conductors, each somewhat longer and thinner than the combined lengths of the generating conductors, and placed vertically over one another, and separated by a minute air gap. As the theory of this latter form of detector is simpler than that of the circle, it will simplify matters to consider it alone. The two conductors should each have a period of electrical oscillation up and down it, the same as that of the charges on the generator. The generator consists of two conductors certainly, but then during the time the spark lasts they are virtually one conductor, being connected by the spark across which the electric charges are rushing alternately up and down. Hence the period of oscillation of the charges on the generator corresponds to that on a single conductor of the same size as its two parts combined.

Various experiments have been made as to the best form for these conductors that form the detector They might be made identical with the generator, only that the spark gap in the generator should be represented by a connecting wire. They may be longer and thinner. If longer, they should be thinner, or they will not have the same period of vibration. On the whole, the best results have been got with conductors somewhat longer and thinner than the generator. It is not generally convenient that the spark between the two conductors that form the detector should take place directly from one to the other. It is not easy to make arrangements by which the distance apart of these conductors can be regulated with sufficient accuracy. The most convenient way is to connect the lower end of the upper conductor and the upper end of the lower one each with a short thin wire leading, one to a fixed small knob and the other to a very fine screw impinging on the knob. The screw may then be used to adjust the spark gap between it and the small knob with great accuracy. This spark gap must be very small indeed, if delicate work be desired. A thousandth of a centimeter would be a fair-sized spark gap. The minute sparks that are formed in these gaps when doing delicate work are too faint to be seen, except in a darkened room. Having placed the detector in position between the generator and the screen, the difficult part of the observation begins. It is heart-rending work at first. A bright spark now and then arouses hope, and long periods of darkness crush it again. The knobs of the generator require re-polishing; the spark gap of the detector gets closed up; dust destroys all working, and not without much patience can the art be attained of making sure of getting sparks whenever the conditions are favorable, though it is easy enough not to get sparks when the conditions are unfavorable.

Before making any measurements all this practice must be gone through. It is hard enough with the success of others before us to encourage us, with their advice to lead us, with a clear knowledge of what is to be expected to guide us. How much credit then is due to Hertz, who groped his way to these wonderful experiments from step to step, without the success of others to encourage him, without the advice of others to lead him, without any certainty as to what was to be expected to guide him. Patiently, carefully, through many by-paths, with constant watchfulness, and checking every advance by repeated and varied experiments, Hertz worked up to the grand simplicity of the fundamental experiment in electricity that is engaging our attention.

Having gained command over the apparatus we may look about for places where sparks occur easily and for others where they can not be produced. Two or three places may be found where no sparks can be observed. These places will be found to be nearly equi-distant. They are the nodes we are in search of. The distance between any pair is half the distance an electric wave is propagated during the period of an oscillation. Their presence proves that the electric force is not propagated instantaneously, but takes time to get from place to place. If the electric force were propagated instantaneously there might be one place where the action of the currents induced in our reflecting sheet neutralized the direct action of our generator, but there could not be a series of two or more such places between the generator and the reflecting sheet. That there are more than one proves that electric force is propagated from place to place, and does not occur simultaneously everywhere. It sets the crowning stone on Maxwell's theory that electric force is due to a medium. Without a medium there can be no propagation from place to place in time. It only remains to confirm by calculation that the rate of propagation is the same as that of light. This is a complicated matter. It involves the question of how fast should, on any theory, the charge oscillate up and down a conductor. The problem has only been accurately solved in a few special cases, such as that of a sphere by itself. The conductors that have been employed are not this shape, are not by themselves, and so only rough approximations are possible as to the rate at which these oscillations occur. Knowing the wave length will not determine the velocity of propagation unless we know the period of vibration; and consequently this direct measure of the velocity has only been roughly made; but it agrees as accurately as could be expected with Maxwell's theory that it must be the same as the velocity of light if electrical phenomena are due to the same medium as light. The conviction that more accurate determinations will confirm this agreement is founded upon safe ground.

It was pointed out that the æther that transmits light and is set in vibration by the molecules of matter can hardly avoid moving them itself. This æther can hardly help having other properties than merely transmitting a comparatively small range of vibrations. It can hardly help producing other phenomena. When it has been shown that, if there is a medium concerned in conveying electric and magnetic actions, it must possess properties which would enable it to transmit waves like light; and when it has been shown that there is a medium concerned in conveying electric and magnetic actions, and that the rate at which they are conveyed is approximately the same as the rate at which light is propagated; the conclusion is almost unavoidable that we are dealing with the same medium in both cases, and that future experiments, capable of accurate calculation and observation, will confirm the conclusion that electric force is propagated through, and by means of, the luminiferous æther with the velocity of light. We really know very little about the nature of a wave of light. We know a great deal more about electric and magnetic forces, and much may be learnt as to the nature of a wave of light by studying it under the form of a wave of electric force. The waves produced by the Hertzian generator may be a meter long or more. The difficulty is to get them short enough. We know a good deal about how they are produced, and from this, and also by means of suitable detectors, we can study a great deal about their structure. They are truly very long waves of light. Atoms are Hertzian generators whose period of vibration is hundreds of millions of millions per second. A Hertzian generator may vibrate rapidly, but it is miserably slow compared with atoms. And yet the wonder is that atoms vibrate so slowly. If a Hertzian generator were, say, 10^{-7} cm long, about the size of a good big atom, its period of vibration would be some hundreds of times too rapid to produce ordinary light. Atoms are probably complicated Hertzian generators. By making a complicated shape, as, for example, a Leyden jar, a small object may have a slow period of vibration. All that is required is that the capacity and self-induction may be large in comparison with the size of the conductor. We saw that these rapidly vibrating generators have but little energy in them: they rapidly give out their energy to the æther near them. This is also the case with atoms. These, when free to radiate, give up their energy with wonderful rapidity. How short a time a flash of lightning lasts! It is hardly there but it is gone: the heated air molecules have so suddenly radiated off their energy. The reason why atoms in the air, for instance, do not radiate away their energy like this is because all their neighbors are sending them waves. Each molecule is a generator, but it is a detector as well. It is kept vibrating by its neighbors: it occupies a part of the æther that is in continual vibration, and so the atom itself vibrates. As each atom can radiate so rapidly, it must be a good detector; its own vibrations must be very much controlled by the neighborhood it finds itself in: and as the waves of light are very long compared with the distances apart of molecules, those in any neighborhood are probably, independently of their motions to and fro, each vibrating in the same way.

It is interesting to calculate how much of the energy in the air is in the form of vibrations of the æther between the molecules of air. A rough calculation shows that in air at the ordinary density and temperature only a minute fraction of the total energy in a cubic centimeter is in the æther; but when we deal with high temperatures, such as exist in lightning flashes, and near the sun, and with very small densities, there may be more energy in the æther than in the matter within each cubic centimeter. All this shows how wide-reaching are the results of Hertz's experiments. They teach us the nature of waves of light. We can learn much by considering how the waves are generated. Let us consider what goes on near the generator, consisting of two conductors, A and B, sparking into one another. Before each spark, and while A and B are being comparatively slowly what is called charged with electricity, the æther around and between them is being strained. The lines of strain are the familiar tubes of electric force. If A be positive, these tubes diverge from all points of A, and most from the knob between it and B, and converge on B. Where they are narrow, the æther is much strained; where wide, the æther is but little strained. Each tube must be looked upon as a tube of unit strain.

The nature of the strain of the æther is not known; it is, most probably, some increased motion in a perfect liquid. We must not be surprised at the nature of the strain being unknown. We do not know the nature of the change in a piece of India rubber when it is strained, nor indeed in any solid, and though the æther is much simpler in structure than india rubber, it can hardly be wondered at that we have not yet discovered its structure, for it is only within the present century that the existence of the æther was demonstrated, while men have known solids and studied their properties and structure for thousands of years. Any way, there is no doubt that the æther is strained in these tubes of force when A and B are oppositely charged, and that the energy per cubic centimeter of unstrained æther is less than that of strained æther, and that the work done in what is called charging A and B is really done in straining the æther all round them. When the air-gap breaks down, and an electric spark takes its place, there is quite a new series of phenomena produced. Suddenly, the strained æther relieves itself, and in doing so, sets up new motions in itself. The strained state was probably a peculiar state of motion, and in changing back to ordinary æther a new and quite distinct state of motion is set up. This new state of motion all round the conductors is most intense near the spark, and is usually described as an electric current in the conductors and across the spark, or as a rushing of the electric charge from one conductor to the other. The electric current is accompanied by magnetic force in circles round it, and the tubes of magnetic force define the nature of the new movement in the æther as far as we know it.

Hitherto, for the sake of simplicity, the existence of this magnetic force has been unnoticed. It is due to a peculiar motion in the æther all round what are called electric currents. The current in fact consists of little else than a line, all round which this movement is going on; like the movement surrounding an electrified body, but also unlike it. Whenever electric forces are changing, or electrified bodies moving, or electric currents running, there this other peculiar motion exists. We have every reason for thinking that this, which may be called the magnetic strain in the æther, as the movement all round electrified bodies was called the electric strain—that this magnetic strain only exists in these three cases: (1) When the electric strain is changing; (2) when electrified bodies are moving, and (3) when electric currents are running. These three may be all cases of one action; certainly the magnetic strain that accompanies each is the same, and it seems most likely that the electric change is only another aspect of the magnetic strain. There are analogies to this in the motion of matter that partly help and partly annoy, because they partly agree and partly will not agree with the ætherial phenomena. Take the case described in a former article of a chain transmitting waves. Attention was drawn to the displacement of a link and to its rotation. Now for the analogy: To seem at all satisfactory the first thing that would strike one would be to pay attention to two motions, to the velocity of displacement of the link and to its rotation. This would lead to interminable difficulties in carrying out the analogy. We can not liken electric strain to a velocity in this direct and simple way, because what are we to do with a change in the strain which produces the same effects as a continuous current? A change in the strain is all very well, it would be like a change in the velocity, but what about a continuous change in the velocity: We can hardly suppose a velocity continually increasing forever; we are evidently landed in immediate difficulties. It is better therefore to be content to liken the electric strain to a displacement of the chain link. It seems most likely that it really is a peculiar motion in the æther, but we must be content for the present with the analogy. If we want to drive it further, we must suppose stress in the chain that draws the link back to be due to a motion in the chain or of things fastened to it, and then the changed motions produced by a displacement of the chain might be analogous to the peculiar motions accompanying electric strain. It would lead us too far to work out this analogy.

Returning to the simpler case of the diplsacement of the link representing electric strain, and the velocity of its rotation representing magnetic strain, see how the actions near a Hertzian generator may be likened to what takes place when a wave is being sent along a chain. While the conductors are being slowly charged we must suppose electric strain to be produced in all the surrounding space. This is a comparatively slow action, and as the rate of propagation is very rapid, the electric strain will rise practically simultaneously in the whole neighborhood, and that it does so is a most important fact to be taken account of in all our deductions from these experiments. This slow charging must be represented by a slow raising of one end of the chain, which raises the rest of it to a great distance apparently simultaneously if the raising be done slowly. Suddenly the air-gap breaks. This might be represented by lifting the chain with a weak thread, and by having the end of the chain fastened to a pretty strong spring. When the thread broke the spring would pull the chain back quickly, would pass its position of equilibrium, and thus commence a series of rapid vibrations on each side of this position; the vibrations would gradually die away owing to the energy of the spring being gradually spent, partly on friction in itself, and partly in sending waves along the chain. In actually performing the experiment, an india-rubber tube or limp thin rope is better than a chain when hung horizontally, as the chain is so heavy; when it can be hung vertically, a chain does very well. In the description it simplifies matters to describe a chain, because it is easier to talk of a link than of a bit of the rope; a link has an individuality that identifies it, while a bit of the rope is so indefinite that it is not so easy to keep in mind any particular bit.

Consider now what these waves are, what sort of motion originates them. When the spring first starts, the near parts of the chain moves first. What happens to any link? One end of it moves down before the other. What sort of motion then has the link? It must be rotating. Thus it is that change in the displacement is generally accompanied by rotation of the links. Thus it is that change in the electric strain is accompanied by magnetic strain. The analogy goes farther than this. Each wave thrown off may be described as a wave of displaced—or as a wave of rotating—links, and the most displaced are at any time the most rapidly rotating links. Just in the same way, what have hitherto been called waves of electric force may also be looked upon as waves of magnetic force. Because there are two aspects in which the motion of the chain may be viewed does not diminish from the essential unity of character of the wave motion in its waves; and similarly the fact that these Hertzian waves have an electric and a magnetic aspect does not diminish from the essential unity of character of the wave motion in them. At the same time the two elements, the displacement of a link and the rotation of a link, are quite distinct things; either might exist without the other; it is only in wave propagation that they essentially co-exist. In the same way electric strain and magnetic strain are quite different things; though in wave motion, and indeed whenever energy is transmitted from one place to another by means of the æther, they essentially co-exist.

ON THE DISCHARGE OF ELECTRICITY THROUGH EXHAUSTED TUBES WITHOUT ELECTRODES.*

By J. J. Thomson, F. R. S.

The following experiments, of which a short account was read before the Cambridge Philosophical Society last February, were originally undertaken to investigate the phenomena attending the discharge of electricity through gases when the conditions are simplified by confining the discharge throughout the whole of its course to the gas, instead of, as in ordinary discharge-tubes, making it pass from metallic or glass electrodes into the gas, and then out again from the gas into the electrodes.

In order to get a closed discharge of this kind we must produce a finite electromotive force round a closed circuit, and since we can not do this by the forces arising from a distribution of electricity at rest, we must make use of the electromotive forces produced by induction. To break down the electric strength of the gas such forces must be very intense while they last, though they need not last for more than a short time. Forces satisfying these conditions occur in the neighborhood of a wire through which a Leyden jar is discharged. During the short time during which the oscillations of the jar are maintained enormous currents pass through the wire, and as with a moderate-sized jar these currents change their direction millions of times in a second, the electromotive force in the neighborhood of the wire is exceedingly large. To make these forces available for producing an electrodeless discharge, all we have to do is to make the wire connecting the coatings of the jar the primary of an induction-coil of which the discharge-tube itself forms the secondary. The arrangements which I have employed for this purpose are represented in the accompanying diagram.

In (α) A is the inside coating of a Leyden jar: this is connected to E, one of the poles of a Wimshurst electrical machine, or an induction-coil, the other pole F of the machine being connected to B, the outer coating of the jar. A C D is a wire connected to the inner coating of the jar, a few turns C (which we shall call the primary coil) are made in this wire; these turns are square if the discharge-tube is square, circular if the discharge-tube is a spherical bulb. The wire at D is attached to an air-break, the other side of which is connected with the outer

* From the *L. E. D., Phil. Mag.*, October and November, 1891; vol. XXXII, pp. 321–336, and 445–464.

coating of the Leyden jar. The knobs of this air-break ought to be kept brightly polished. The loop C is connected to earth. The discharge-tubes, which were in general either rectangular tubes or spherical bulbs, where placed close to the turns of C. When the difference of potential between A and B is sufficiently large, a spark passes across the air-break, and the electrical oscillations set up produce a large electromotive force in the neighborhood of the coil, sufficient under favorable circumstances to cause a bright discharge to pass through the vacuum-tubes. In some experiments the jars, at the suggestion of Prof. Oliver Lodge, were connected up differently, and are represented

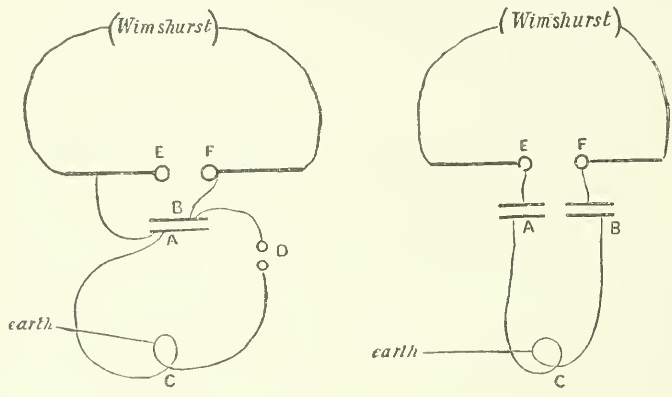

FIG. 1.

by (β) in Fig. 1. Two jars were used, the outside coatings of which, A and B, were connected by the wire containing the primary coil C, the inside coating of the first jar was connected to one pole of the Wimshurst, that of the second to the other. With this method of arranging the jars no air-space is required, as the sparks pass between the terminals of the machine, and the polishing of these terminals is not nearly so important as that of the knobs of the air-break in the arrangement (α).

Before proceeding to describe the appearance presented by the discharge, I will mention one or two points which may prove useful to any one who wishes to repeat the experiments. According to my experience the discharge is more easily obtained in bulbs than in square tubes, and with a Wimshurst machine than with an induction-coil. If an induction-coil is used a break which will transmit a large current ought to be substituted for the ordinary vibrating one supplied with such instruments. It is essential to success that the gas in the bulbs or tubes should be quite dry and at a suitable pressure; there is a pressure at which the brilliancy of the discharge is a maximum, and as in endeavoring to get at this pressure the exhaustion may be carried too far, it is convenient to use a form of mercury pump which will allow of the easy admission of a little gas; the pattern which I have used and found to answer very well is called the Lane-Fox pattern. When any gas is introduced it should be sent through sulphuric acid to get rid of any moisture that may be in it. Owing, I think, to the pressure in ordinary incandescent lamps being very different from that at which the discharge has its maximum brilliancy, I have met with very poor success in attempts to produce these discharges in already exhausted tubes such as incandescent lamps, though I have tried a considerable number by different makers; on the other hand, the radiometers which I have tried allow the discharge to pass pretty readily, though it is interfered with by the vanes, and is not comparable in brilliancy with that obtained in home-made tubes and bulbs. I have obtained sparks easily with apparatus of the following dimensions: two gallon jars, the outside coatings connected by a wire about 2 yards long, the coil consisting of three or four turns, each about 3 inches in diameter. I have some bulbs which with this apparatus will give a bright discharge when the distance between the terminals of the Wimshurst is only ¼ inch; these are, however, exceptionally good; it more frequently takes a spark an inch or an inch and a half long to produce the discharge.

I find that Hittorf, in Wiedemann's *Annalen*, XXI, p. 138, describes the light produced in a tube round which the wire connecting the coatings of a Leyden jar is twisted; the luminosity in Hittorf's experiments seems to have filled the tube, and not, as in the experiments described in this paper, been confined to a ring. It seems possible that the difference in the appearance in the tubes may have been due to the existence of an electrostatic action in Hittorf's experiments, the primary coil getting raised to a high potential before the discharge of the jar, and inducing a distribution of electricity over the inside of the glass of the tube; on the passage of the spark the potential of the primary coil will fall, and the electricity on the glass re-distribute itself; to effect this re-distribution it may pass through the rarified gas in the discharge tube and produce luminosity.

In my experiments I took two precautions against this effect. In the first place I connected the primary coil to earth, so that its potential before discharge took place was unaltered, and as an additional precaution I separated the discharge tube from the primary by a cage made of blotting paper moistened with dilute acid; the wet blotting paper is a sufficiently good conductor to screen off any purely electrostatic effects, but not a good enough one to interfere to an appreciable extent with the electro-motive forces arising from rapidly alternating currents. In this way we can screen off any electrostatic effects due to causes which operate before the electrical oscillations in the jars begin. When once these have commenced, there ought not, I think, to be any separation of the electro-motive forces into two parts, one being called electro-static, the other electro-dynamic. As this is a point on which it is desirable to avoid any misunderstanding, I hope to be excused if I treat it at some length.

In the mathematical treatment of the phenomena of the "Electro-magnetic Field," it is customary and not inconvenient to regard the electro-motive force as derived from two sources, or rather as consisting of two parts, one part being calculated by the ordinary rules of electro-statics from the distribution of electricity in the field, the other part being the differential coefficient of the vector potential with respect to the time. From a mathematical point of view, there is a good deal to be said for this division; the two forces have very distinct and sharply contrasted analytical properties. Thus the electrostatic force possesses the property that its line integral taken round any closed curve vanishes, while the surface integral of its normal component taken over a closed surface does not in general vanish. The "vector potential force," on the other hand, does not in general vanish when integrated round a closed curve; the surface integral of its normal component taken over any closed surface however vanishes. When however our object is not so much mathematical calculation as the formation of a mental picture of the processes going on in the field, this division does not seem nearly so satisfactory, as the fundamental quantities concerned, the electrostatic and vector potentials, are both of considerable complexity from a physical point of view. We might judge that this division of the electro-motive force into two parts, the one derivable from an electrostatic, the other from a vector, potential, is rather a mathematical device than a physical reality, from the fact which I pointed out in a report on electrical theories (*B. A. Report*, 1886), that though the electrostatic potential satisfies the mathematical condition of being propagated with an infinite velocity, the total electro-motive force in the electro-magnetic field travels with the velocity of light, and nothing physical is propagated at a greater velocity.

In an experimental investigation such as that described in this paper, it is not so important that our method of regarding the phenomena should lead to the shortest analysis as that it should enable us to picture to ourselves the processes at work in the field, and to decide without much calculation how to arrange the experiments so as to bring any effect which may have been observed into greater prominence.

The method which I have adopted for this purpose is the one described by me in the *Philosophical Magazine*, March, 1891, and which consists in referring everything to the disposition and motion of the tubes of electrostatic induction in the field. These tubes are either endless, or have their ends on places where free electricity exists, every unit of positive electricity (the unit being the quantity of electricity on the atom of a univalent element) being connected by a unit tube to a unit of negative electricity, the tube starting from the positive electricity and ending on the negative. At any point in the field the electro-motive intensity varies as the density of the tubes of electrostatic induction at that point. When the electricity and the tubes in the field are at rest, the tubes distribute themselves so that the electro-motive intensity at any point is derivable from a potential function; as soon, however, as the equilibrium is disturbed, the tubes move about and get displaced from their original positions, the disposition of tubes and therefore the electro-motive intensity are changed, and the latter will no longer be derivable from a potential function, and according to the mathematical theory would be said to include forces due to electrostatic and electro-magnetic induction. According to our view, however, the cause of the electro-motive intensity is the same in both cases, viz. the presence of tubes of electrostatic induction, and the electro-motive intensity ceases to be derived from a potential, merely because the distribution of these tubes is not necessarily the same when they are moving about as when they are in equilibrium. It is shown, in the paper already referred to, that these tubes when in motion produce a magnetic force at right angles, both to their own direction and to that in which they are moving, the magnitude of the force being 4π times the product of the strength of the tube, the velocity with which it is moving, and the sine of the angle between the direction of the tube and its direction of motion. In an electric field in which the matter is at rest, these tubes when in motion move at right angles to themselves with the velocity "v," that at which electro-dynamic disturbances are propagated through the medium. We can easily show that, K being the specific inductive capacity of the medium, the line integral of 4π K times the density of these tubes taken round a closed circuit is equal to the rate of diminution of the number of lines of magnetic induction passing through the circuit. Thus, since the fundamental laws of electro-dynamic action, viz. Faraday's law of induction and Ampère's law of magnetic force, follow from this conception of the field as produced by tubes of electrostatic induction moving at right angles to themselves with the velocity "v," and producing a magnetic force at right angles both to their own direction and to that in which they are moving, and proportional to the product of the strength of the tube and its velocity, it is a conception which will account for all the known phenomena of the field. It furnishes, in fine, a geometrical instead of an analytical theory of the field. It will also be seen that from this point of view the magnetic force, when introduced to calculate the electro-motive forces arising from induction, logically comes in as an intellectual middle-man wasting mental effort.

We may thus regard the distinction between electrostatic and electro-magnetic electro-motive forces as one introduced for convenience of analysis rather than as having any physical reality. The only difference which I think could from made from a physical point of view would be to define those effects as electrostatic which are due to tubes of electrostatic induction having free ends, and to confine the term electro-magnetic to the effects produced by closed endless tubes. It is only however when the electro-motive forces are produced exclusively by the motion of magnets that all the tubes are closed; whenever batteries or condensers are used, open tubes are present in the field.

It will be useful to consider here the disposition and motion of the tubes of electrostatic induction in the arrangement used to produce these electrodeless discharges. We shall take the case where two jars are used, as in β, Fig. 1, as being the more symmetrical.

Just before the discharge of the jar, the tubes of electrostatic induction will be arranged somewhat as follows: There will be some tubes stretching from one terminal of the electric machine to the other; others will go from the terminals to the neighboring conductors, the table on which the machine is placed, the floor and walls of the room, etc. The great majority of the tubes will, however, be short tubes passing through the glass between the coatings of the jars. Let us now consider the behavior of two of these tubes, one from the jar A, the other from B, when a spark passes between the terminals of the machine. Whilst the spark is passing these may be regarded as connected by a conductor; the tubes which originally stretched between them now contract, the repulsion they exerted on the surrounding tubes is destroyed so that these now crowd into the space between the terminals, the two short tubes under consideration now taking somewhat the form shown in Fig. 2. These tubes, being of opposite sign, tend to run together; they do

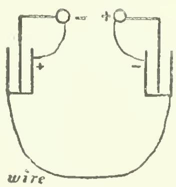

Fig. 2.

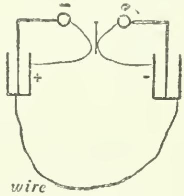

Fig. 3

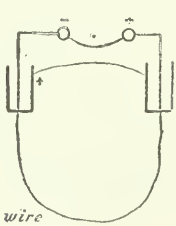

Fig. 4.

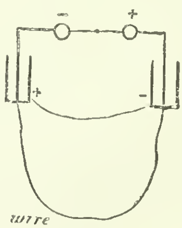

Fig. 5.

so until they meet as in Fig. 3, when the tubes break up as in Fig. 4, the upper portion running into the spark gap, where it contracts, while the lower portion rushes through the dielectric to discharge itself into the wire connecting the coatings of the jars, an intermediate position being shown in Fig. 5. These tubes while rushing through the dielectric produce, as already stated, magnetic forces; some of them on their way to the discharging wire will pass through the discharge tube; if they congregate there in sufficient density, discharge will take place through the rarefied gas.

The discharge of the jar is oscillatory, and we have only followed the motion of the tubes during a part of the oscillation: when, however, this tube enters the wire between the jars a tube of opposite kind emerges from it; the same thing happens when the other portion enters the spark gap. These go through the same processes as the tubes we have followed, but in the reverse order, until we get again two short tubes in the jars, but opposite in sign to the original ones; the process is then repeated, and so on as long as the vibrations last.

In order to see what are the most advantageous dimensions to give to our apparatus, let us consider on what the maximum electromotive force in the secondary depends. Let us take the case of a condenser of capacity C discharging through a circuit whose coefficient of self-induction is L; then, if the potential difference between the plates of the condenser is initially V_0, the current γ at the time t is (supposing as a very rough approximation that there is no decay in the vibrations) given by the equation

$$\gamma = \frac{CV_0}{\sqrt{LC}} \sin \frac{t}{\sqrt{LC}}.$$

The rate of variation of this, $\dot{\gamma}$, is therefore

$$\frac{V_0}{L} \cos \frac{t}{\sqrt{LC}}.$$

So that if M is the coefficient of self-induction between the primary and a secondary circuit, the maximum electro-motive force around the secondary is MV_0/L, which for a given spark-length is independent of the capacity of the condenser. In practice it is advisable, however, to have as much energy in the jars to start with as possible, and better results are got with large jars than with small ones. Using a six-plate Wimshurst machine I got very good results with two "gallon jars;" with a large induction coil the best results were got with two "pint-and-a-half jars."

The best number of turns to use in the primary coil C depends upon the size of the leads; if all the circuit were available for this coil one turn would give the largest electro-motive force, because, though for a given rate of change of the current in the primary the effect on the secondary increases with the number of turns, the rate of change of the current varies inversely as the self-induction of the primary, so that if all the circuit is in the coil C, since an increase in the number of turns will increase the self-induction of the circuit faster than the mutual induction, it will diminish the electro-motive force round the secondary. In practice however it is not possible to have the whole of the wire connecting the coatings of the jar in the coil C; and in this case an increase in the number of turns may increase the mutual induction more than the self-induction, and so be advantageous. The best result will be obtained when the self-induction in the coil C is equal to that of the remainder of the circuit. It is very easy to find by actual trial whether the addition of an extra turn of wire is beneficial or the reverse. The brightness of the discharge depends upon the time of the electrical oscillations as well as upon the magnitude of the electro-motive force. Thus, in an experiment to be described later, the brilliancy of the discharge was increased by putting self-induction in the leads, which, though it diminished the intensity of the electromotive force, increased the time constant of the system. When the discharge tube was square and the coil C had also to be square it was found most convenient to make it of glass tubing bent into the required form and filled with mercury. When however the discharge was required in a bulb, the primary coil was made of thick gutta-percha-covered copper wire wound round a beaker just large enough to receive the exhausted bulb. There is sometimes considerable difficulty in getting the first discharge to pass through the bulb, though when it has once been started other discharges follow with much less difficulty. The same effect occurs with ordinary sparks. It seems to be due to the splitting up of the molecules by the first discharge; some of the atoms are left uncombined, and so ready to conduct the discharge, or else when they re-combine they form compounds of smaller electric strength than the original gas. When the discharge was loath to start, I found the most effectual way of inducing it to do so was to pull the terminals of the Wimshurst far apart and then, after the jars had got fully charged, to push the terminals suddenly together. In this way a long spark is obtained, which, if the pressure of the gas is such that any discharge is possible, with the means at our disposal will generally start the discharge.

Appearance of the discharge.—Let us suppose that we have either a square tube placed outside a square primary or a bulb placed inside a circular coil of wire, and that we gradually exhaust the discharge tube, the jars sparking all the time. At first nothing at all is to be seen in the secondary, but when the exhaustion has proceeded until the pressure has fallen to a millimeter or thereabouts, a thin thread of reddish light is seen to go round the tube situated near to but not touching the side of the tube turned towards the primary. As the exhaustion proceeds still further, the brightness of this thread rapidly increases, as well as its thickness; it also changes its color, losing its red tinge and becoming white. On continuing the exhaustion the luminosity attains a maximum, and the discharge passes as an exceedingly bright and well-defined ring. On continuing the exhaustion, the luminosity begins to diminish until, when an exceedingly good vacuum is reached, no discharge at all passes. The pressure at which the luminosity is a maximum is very much less than that at which the electric strength of the gas is a minimum in a tube provided with electrodes and comparable in size to the bulb. The pressure at which the discharge stops is exceedingly low, and it requires long-continued pumping to reach this stage. We see from these results that the difficulty which is experienced in getting the dischage to pass through an ordinary vacuum-tube when the pressure is very low is not altogether due to the difficulty of getting the electricity from the electrodes into the gas, but that it also occurs in tubes without electrodes, though in this case the critical pressure is very much lower than when there are electrodes. In other words, we see that as the state of the bulb approaches that of a perfect vacuum its insulating power becomes stronger and stronger. This result is confirmed by several other experiments of a different kind, which will be described later.

The discharge presents a perfectly continuous appearance, with no sign of striation, of which I have never observed any trace on any of these discharges, though I must have observed many thousands of them under widely different conditions.

Action of a magnet on the discharge.—The discharges which take place in these tubes and bulbs are produced by periodic currents, so that the discharges themselves are periodic, and the luminosity is produced by currents passing in opposite directions. As this is the case, it seemed possible that the uniformity of the luminosity seen in the discharge was due to the super-position of two stratified discharges in opposite directions, the places of maximum luminosity in the one fitting into those of minimum luminosity in the other. Since these discharges are in opposite directions, they will be pushed opposite ways when a magnetic force acts at right angles to them, the discharges in opposite directions can thus be separated by the application of a magnetic force and examined separately. In the experiment which was tried with this object, a square tube was used placed outside the primary, the tube at one or two places being blown out into a bulb so as to allow of the wider separation of the constituent discharges. When one of these bulbs was placed in a magnetic field where the force was at right angles to the discharge, the luminous discharge through the bulb was divided into two portions which were driven to opposite sides of the bulb; each of these portions was of uniform luminosity and exhibited no trace of striation. It was noticed, however, in making this experiment that the discharge seemed to have much greater difficulty in passing through the tube when the electro-magnet was on than when it was off. This observation was followed up by several other experiments, and it was found that the discharge is retarded in a most remarkable way by a magnetic force acting at right angles to the line of discharge. This effect is most strikingly shown when the discharge passes as a ring through a spherical bulb. If such a bulb is placed near a strong electro-magnet, it is easy to adjust the length of spark so that when the magnet is off a brilliant discharge passes through the bulb, while when the magnet is on no discharge at all can be detected. The action is very striking, and the explanation of it which seems to fit in best with the phenomena I have observed, is that the discharge through the rarefied gas does not rise to its full intensity suddenly, but as it were feels its way. The gas first breaks down along the line where the electro-motive intensity is a maximum, and a small discharge takes place along this line. This discharge produces a supply of dissociated molecules along which subsequent discharges can pass with greater ease. Thus under the action of these electric forces the gas is in a state of unstable equilibrium, since as soon as any small discharge passes through it the gas becomes electrically weaker and less able to resist subsequent discharges. When the gas is in a magnetic field, the magnetic force acting on the discharge produces a mechanical force which displaces the molecules taking part in the discharge from the line of maximum electric intensity, and thus subsequent discharges do not find it any easier to pass along this line in consequence of the passage of the previous one. There will not therefore be the same instability in this case as in the one where no magnetic force acted upon the gas. A confirmation of this view is, I think, afforded by the appearance presented by the discharge when the intensity of the magnetic field is reduced, so that the discharge just—but only just—passes when the magnetic field is on. In this case the discharge, instead of passing as a steady fixed ring, flickers about the tube in a very undecided way.

If the strength of the magnetic field is reduced still further, so that the discharge passes with some ease, the bright ring which, when no magnetic force is acting, is in one plane, is changed into a luminous band situated between two planes which intersect along a diameter of the bulb at right angles to the magnetic force. These planes are inclined at a considerable angle, one being above and the other below the plane of the undisturbed ring. This displacement of the ring by the magnetic force shows that it consists of currents circulating tangentially round the ring.

This action of a magnet on a discharge flowing at right angles to its lines of force is not, however, the only remarkable effect produced by a magnet on the discharge. When the lines of magnetic force are along the line of discharge, the action of the magnet is to facilitate the discharge and not to retard it as in the former case. The first indication of this was observed when the jars were connected, as in (a) Fig. 1. The earth connection being removed, in this case there is a glow from the glass into the bulb, due to the re-distribution of the electricity induced on the glass by the primary when it is at a high potential before the spark passes. If the primary is connected to earth by a circuit with an air break in it, the intensity of the glow may be altered at will by adjusting the length of the air break; when the air-space is very small there is no glow; when it is long the glow is bright. The bulb in which the discharge was to take place was placed on a piece of ebonite over the pole of an electro-magnet, and the air-space in the earth connection of the primary was adjusted so that when the magnet was off no glow was observed in the tube. When the magnet was on, however, a glow radiating in the direction of the lines of magnetic force was produced, which lasted as long as the magnet was on, and died away rapidly, but not instantaneously, when the magnet was taken off. In this case the discharge seems to be much easier along the lines of magnetic force.

The following experiment shows that this effect is not confined to the glow discharge, but is also operative when the discharge passes entirely through the gas. A square tube ABCD (Fig. 6) is placed outside the primary EFGH, the lower part of the discharge tube CD being situated between the poles L M of an electro-magnet. By altering the length of spark of the Wimshurst machine, the electro-motive intensity

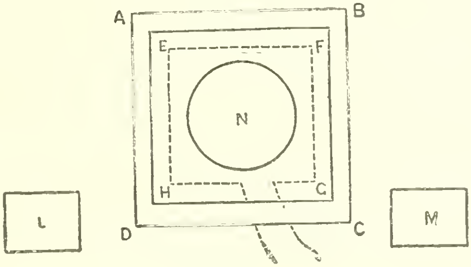

Fig. 6.

acting on the secondary can be so adjusted that no discharge passes round the tube ABCD when the magnet is off, whilst a bright discharge occurs as long as the magnet is on. The two effects of the magnet on the discharge, viz, the stoppage of the discharge across the lines of magnetic force, and its acceleration along them, may be prettily illustrated by placing in this experiment an exhausted bulb N inside the primary; then the spark length can be adjusted so that when the magnet is off the discharge passes in the bulb, and not in the square tube, while when the magnet is on the discharge passes in the square tube, and not in the bulb.

The experiments on the effect of the magnetic field on the discharge were tried with air, carbonic acid, and oxygen, but I could not detect any difference in the behavior of the gases.

The explanation of the longitudinal effect of magnetic force seems more obscure than that of the transverse effect; it is possible however that both may be due to the same cause, for if the feeble discharge which we suppose precedes the main discharge branches away at all from the line of main discharge, the action of the magnetic force when it is along the discharge will tend to bring these branches into the main line of discharge; and thus there will be a greater supply of dissociated molecules along the main line of discharge, and therefore an easier path for the subsequent discharges when the magnetic force is acting than when it is absent.

It is perhaps not necessary to assume that the mechanical action of the magnetic force is on a small discharge preceding the main one; the action of the magnetic force on the chain of polarized molecules which are formed before the discharge passes might produce an effect equivalent to that which we have supposed was produced on an actual discharge.

The chain of polarized molecules would be affected in the following way: The magnetic field due to the electro-magnet consists of tubes of electrostatic induction moving about these tubes, as well as the direction in which they are moving, are at right angles to the lines of magnetic force. The short tubes of electrostatic induction which join the atoms in the molecules of the gas will, under the influence of the electric forces, set themselves parallel to the direction of the electro-motive intensity at each point.

Thus, when the magnetic force is at right angles to the line of discharge, tubes of electrostatic induction parallel to those in the molecules will be moving about in the field: and since parallel tubes exert attraction and repulsion on each other, the molecular tubes will be knocked about and their efforts to form closed chains made much more difficult by the action of the magnet. On the other hand, when the lines of magnetic force are parallel to the discharge, the moving tubes are at right angles to those in the molecules, and will not disturb them in the attempt to form chains along the line of magnetic force; they will in fact assist them in doing so by preventing all attempts in directions across the lines of force.

Prof. G. F. Fitzgerald has suggested to me in conversation that this action of a magnet on the discharge might be the cause of the "streamers" which are observed in the aurora; the rare air being electrically weaker along the lines of magnetic force than at right angles to them will cause the discharge in the direction of those lines to be the brightest.

Discharge through different gases.—I have examined the discharge through air, carbonic acid, hydrogen, oxygen, coal gas, and acetylene. As I have already mentioned, at the highest pressures at which the discharge passes through air, the discharge is reddish, and gets brighter and whiter at lower pressures. If the discharge is examined through a spectroscope, the lines in the spectrum coincide with those obtained by sparking through air in the ordinary way with a jar in the circuit. The relative brightness of the lines in the spectrum of the discharge without electrodes varies very much with the pressure of the gas and the length of spark in the jar circuit. With a long spark in this circuit, and the pressure such as to give a bright white discharge, the spectrum is very much like that of the ordinary jar discharge in air. When however the pressure is so low that the discharge passes with difficulty, a few lines in the spectrum shine out very brightly, whilst others become faint, so faint indeed sometimes that if the air spectrum were not thrown into the field of view of the spectroscope at the same time, they might pass unnoticed. Three lines which are very persistent, the first a citron green, the second a more refrangible green, and the third a blue, I am inclined to think must be due to mercury vapor from the pump.

I am indebted to Prof. Liveing for the loan of a very fine direct-vision spectroscope, and to him and Mr. Robinson, of the Cambridge Chemical Laboratory, for valuable advice in the attempts which I made to photograph the spectra of some phosphorescent glows mentioned below.

I should like to call attention to the advantages for spectroscopic purposes which attend this method of producing the discharge; it is easily done either by an ordinary electrical machine or an induction coil. An intensely bright discharge is got, and there is no danger of complication arising from the spectrum of the gas getting mixed with that of the electrodes.

Discharge in oxygen.—By far the most remarkable appearance is presented when the discharge passes through oxygen, for in this gas the bright discharge is succeeded by a phosphorescent glow which lasts for a considerable time; indeed, with a strong discharge it may remain visible for more than a minute. When the discharges succeed one another pretty rapidly, the phosphorescence is so strong that it hides the successive bright discharges, and the tube seems permanently full of a bright yellow fog. We can thus, by the use of this gas, convert the intermittent light given by the bright discharge into a continuous one.

Perhaps the most striking way of showing this phosphorescence is to use a long tube, about a meter long and 6 or 7 centimeters in diameter, with a bulb blown in the middle, the primary coil being twisted round this bulb. Then, when the sparks pass between the jars, a bright ring discharge passes through the bulb, from which, as if shot out from the ring, the phosphorescent glow travels in both directions along the tube, moving slowly enough for its motion to be followed by the eye. It can not, therefore, be produced by the direct action of the light from the spark on the gas in the tube, for if it were, the glow would travel with the velocity of light. It is necessary to mention this point, for the light from these discharges has great powers of producing phosphorescence.

The glow seems to consist of gas which has been in the path of the discharge, and whose molecules have been split up by it and projected from the line of discharge. This gas which, when projected, is in a peculiar state, by a process of chemical combination gradually returns to its original condition, and it is while it is in this state of transition from its new condition to the old that it phosphoresces. If this is the case we should expect that the period of phosphorescence would be shortened by raising the temperature. On trying the experiment I found that this took place to a very marked extent. A discharge bulb filled with oxygen at a low pressure was placed over a Bunsen burner;

before the bulb got hot each bright discharge was succeeded by a bright after-glow, but as the bulb got hotter and hotter the glow became fainter and fainter, and at last ceased to be visible, though the bright ring was still produced at each discharge of the jar. When the Bunsen was taken away and the bulb allowed to cool the glow re-appeared.

The spectrum of the after-glow is a continuous spectrum, in which I could not detect the super-position of any bright lines. The only gas beside oxygen in which I have been able to detect any after-glow is air, though in this case the range of pressure within which it is exhibited is exceedingly small; indeed it is often by no means an easy matter to get a bulb filled with air into the state in which it shows the glow. The spectrum of the air-glow showed bright lines; I thought myself that I could see a very faint continuous spectrum as well. Some friends however who were kind enough to examine the spectrum, though they could see the bright lines clearly enough, were of opinion that there was nothing else visible. I endeavored to photograph it, but without success, so that the existence of a continuous spectrum for this glow must be considered doubtful.

When the discharge passes through acetylene, the first two or three discharges are a bright apple-green; the subsequent ones, however, are white, and as the green discharge does not reappear, we must conclude that the acetylene is decomposed by the discharge.

Phosphorescence produced by the discharge.—The discharge without electrodes produces a very vivid phosphorescence in the glass of the vessel in which the discharge takes place; the phosphorescence is green when the bulb is made of German glass, blue when it is made of lead glass. Not only does the bulb itself phorphoresce, but a piece of ordinary glass tubing held outside the bulb and about a foot from it phosphoresces brightly; while uranium glass will phosphoresce at a distance of several feet from the discharge. Similar effects, but to a smaller extent, are produced by the ordinary spark between the poles of an electrical machine.

The vessel in which the discharge takes place may be regarded as the secondary of an induction coil, and the discharge in it shows similar properties to those exhibited by currents in a metallic secondary. Thus no discharge is produced unless there is a free way all round the tube; the discharge is stopped if the tube is fused up at any point. In order that the discharge may take place, it is necessary that the molecules of the gas shall be able to form a closed chain without the interposition of any non-conducting substance; indeed, the discharge seems to be hindered by the presence in such a chain of any second body, even though it may be a good conductor of electricity. Thus, when a tube such as that in Fig. 7 is used, which has a barometer tube attached to it, so that by raising or lowering the vessel into which the tube dips a mercury pellet may be introduced into the discharge circuit, the spark length in the primary circuit may be so adjusted that a discharge passes when there is a clear way round the tube, but stops when a pellet of mercury is forced up so as to close the gangway. I noticed a similar effect in my experiments with a long vacuum tube described in the *Proceedings of the Royal Society* for January, 1891.

FIG. 7.

I had another discharge tube prepared, of which a section is shown in Fig. 8, α, in which a diaphragm (AB) of thin copper plate was placed across the tube; the diaphragm happened to catch at the bottom of the tube, so that it divided the latter rather unequally, and left a narrow passage round its edge. As much of the discharge as there was room for went round the edge of the plate; the remainder was not able to get through the copper, but formed a closed circuit by itself in the larger segment of the tube. In another tube, which is represented in section in Fig. 8, β, the copper diaphragm was attached to the walls of the tube by sealing-wax, so that there was no free way; in this case the discharge again refused to go through the copper, and split up into two separate discharges, as in the figure. When the tube was divided by copper diaphragms into six segments, as in Fig 8, γ, no discharge at all would pass through. When the primary was slipped up the tube above the diaphragm, a brilliant discharge was obtained. These four experiments all illustrate the difficulty which the electricity has in getting transferred from a gas to another conductor.

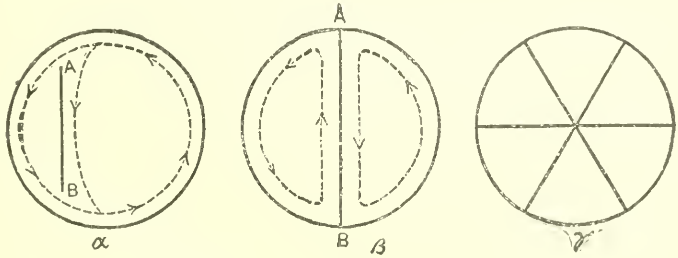

FIG. 8.

There is no discharge through the secondary, if it is of such a kind that, considering a closed curve drawn in it, the electro-motive intensity as we travel along the curve tends to polarize the particles in one half of the chain in one direction, and in the other half in the opposite direction, the direction being reckoned relative to the direction we are traveling round the curve. Thus for example if we take a tube whose axis is bent back on itself, as in the figure, the electro-motive intensity will tend to polarize the particles in one part of the chain in the direction of the arrow, and those in the other in the opposite direction; it is impossible to get a discharge through a tube of this kind.

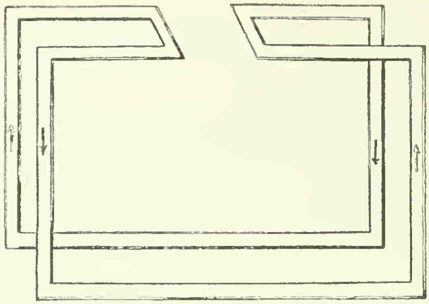

Fig. 9.

On the other hand, the molecules exhibit remarkable powers of making closed chains for themselves when not actually prevented by the action of the electro-motive intensity. Thus the discharge will pass through a great length of tubing in the secondary, even if it is bent up as in Fig. 10, where the vertical piece in the upper part of the secondary is at right angles to the direction of the electric force, and where the molecules will receive no help in forming closed chains from the action of the external electro-motive forces. I have c-ceeded in sending discharges through tubes of this kind 12 to 14 feet in length.

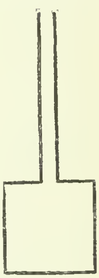

Fig. 10.

Screening effects due to the currents in the tubes.—One very noticeable feature of these discharges is the well-defined character of the ring, if the pressure is not too low. If a large bulb is used for the secondary with the primary just outside it, when the sparks pass between the jars a bright, well-defined ring passes through the bulb near to the surface of the glass, the gas inside this ring being, as far as can be judged, quite free from any discharge. If now a bulb whose diameter is less than that of the luminous ring is inserted in the primary in place of the larger bulb, a bright ring will start in this, though at this distance from the primary there was no discharge in the larger bulb. Thus when the large bulb was in the primary, the discharge through its outer portions screened the interior from electro-motive forces to an extent sufficient to stop a discharge which would otherwise take place.

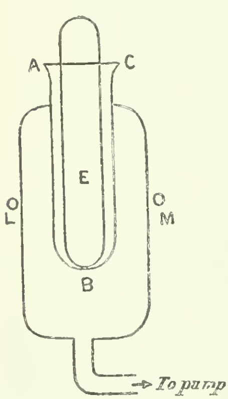

Fig. 11.

The screening action of these discharges is also shown by the following experiment: A, B, C, Fig. 11, is the section of a glass vessel shaped like a Bunsen's calorimeter; in the inner portion A, B, C of this vessel an exhausted tube is placed, while a pipe from the outer vessel leads to a mercury pump and enables us to alter the pressure at will. The primary coil, L, M, is wound round the outer tube. When the air in the outer tube is at atmospheric pressure, the discharge caused by the action of the primary passes in the tube E inserted in A, B, C; but when the pressure in the outer tube is reduced until a discharge passes through it, the discharge in the inner one stops; the discharge in the outer tube has thus shielded the inner tube from the action of the primary. If the exhaustion of the outer tube is carried so far that the discharge through it ceases, that in the inner tube begins again. It requires very high exhaustion to do this, and as on account of the joints it is unsafe to make the vessel very hot during the pumping, I have found it impossible to keep a vacuum good enough to show this effect for more than from half to three-quarters of an hour; in that time sufficient gas seems to have escaped from the sides of the vessel to make the pressure too high to show this effect, and it then takes from two to three hours' pumping to get the tube back again into its former state. An interesting feature of this experiment is that for a small range of pressure, just greater than that at which the discharge first appears in the outer tube, there is no discharge in either of the tubes; thus the action of the primary is screened off from the inner tube, though there is no luminosity visible in the outer one: this shows that a discharge equivalent in its effects to a current can exist in the gas without sufficient luminosity to be visible even in a darkened room. We shall have occasion to mention other cases in which the existence of a discharge non-luminous throughout the whole of its course is rendered evident in a similar way.

Another experiment by which the screening can be effectively shown is to place the primary coil inside a bell-jar which is connected with a mercury pump, the electrical connexions with the primary being led through mercury joints. An exhausted bulb is placed inside the primary, the bulb being considerably smaller than the primary, so that there is an air-space between the two. Before the bell-jar is exhausted the discharge passes through the bulb, but when the bell-jar is exhausted sufficiently to allow of the discharge passing through the gas outside the bulb the discharge in the bulb ceases, and the only discharge is that outside. I have never been able to exhaust the bulb sufficiently well to get the discharge outside the bell-jar to cease, and that in the bulb to appear again, as in the preceding experiment. In this experiment, as in the preceding one, there was a range of pressure when neither the bulb nor the bell-jar was luminous, showing again the existence of currents in the gas which are not accompanied by any appreciable luminosity.

A curious bending-in of the discharge which takes place in a square tube provided with a bulb can, I think, be explained by the principle of shielding. The discharge in the bulb does not, unless very long sparks are used, take as its course through the bulb the prolongation of the direction of the tube, but is bent-in towards the primary. In Fig. 12 the dotted line represents the course the discharge would have taken if there had been no bulb, the continuous line the course actually taken. This bending-in can be explained by supposing the currents started near the primary to shield off from the outlying space the action of the primary, and thus make the electro-motive intensity along the axis of the tube smaller than it would have been if no discharge had been possible between the axis and the primary circuit.

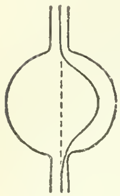
Fig. 12.

Before describing some further experiments on this shielding effect, it will be useful to consider the means by which it is brought about. Let us suppose we have a vertical plate made of conducting material, and to the right of the plate a region A which it shields. This region has to be shielded from tubes of electrostatic induction coming from the left, which have to pass through the shield before reaching A, and from tubes coming from the right which have to pass through A before reaching the field. The action of the shield in the first case is very simple, for when a tube gets inside a conductor it at once attempts to contract to molecular dimensions, and after a time proportional to the specific resistance of the conductor it succeeds in doing so. Thus if the shield is made of a good conductor the tubes of electrostatic induction will be transformed into molecular tubes before they have time to get through; so that the shield will protect A from all tubes which have to go through it. The way the shield destroys or rather neutralizes the effect of the tubes coming from the right is somewhat different: when a positive tube reaches the shield a negative one emerges from it, travelling at right angles to itself in the opposite direction to the incident tube; thus, when the first few tubes reach the shield from the right they will produce a supply of negative ones, and the presence of these negative tubes at A concurrently with the positive ones which continue to arrive there will weaken the field to a greater and greater extent as A approaches the shield. At the surface of the shield itself the neutralization will be complete. A dielectric whose specific inductive capacity is greater than usual will behave in a similar way to a metal plate, but to a smaller extent. It will emit tubes of the opposite sign, but not so numerous as those incident upon it. Thus a metal plate, or even one made of a dielectric of considerable specific inductive capacity, will reduce very considerably the tangential electromotive force on either side of it.

I have made several experiments in which this effect was very strikingly shown. In one of these, two square discharge-tubes of equal cross section placed near and parallel to each other were connected by a cross tube, so that the pressure was the same in both tubes; a fine wire passed round the inside of one of the tubes, its ends being connected together so that it formed a closed circuit, the other tube contained nothing but air at a low pressure. When this double tube was placed outside the primary the discharge went, at the passage of each spark, through the tube without a wire, while the tube containing the wire remained quite dark. A similar experiment was tried by taking a cylindrical tube and suspending in it a metal ring co-axial with the tube; in this case it was easy to adjust the spark-length so that no discharge passed through the tube when the primary was placed round it at the level of the ring, while a discharge passed as soon as the primary was moved above or below the ring.

Another very convenient tube for showing this effect is the one with the hollow down the middle, Fig. 11; when this is pumped so that discharges can pass through the outer tube the spark-length can be adjusted so that the discharge stops immediately when a metal tube, a test-tube containing a strong solution of an electrolyte, or a tube containing air at a pressure at which it is electrically very weak, is placed in the central opening. The discharge is renewed again as soon as the tubes are removed. On one occasion, when the large tube was in a peculiarly sensitive state, I was able to see distinctly the diminution produced by a dielectric in the electro-motive intensity parallel to its surface. The discharge stopped as soon as a stick of sulphur or a glass rod sufficiently large almost to fill the opening was inserted, and was renewed again as soon as these were withdrawn. It requires however the large tube to be in an extremely sensitive state for the effect produced by a dielectric to be apparent, and I have only on one occasion succeeded in getting the tube into this condition. The effect on that occasion however was so definite and regular that I have no doubt as to the existence of the screening effect due to the dielectric.

When the tube is of average sensitiveness dielectrics do not produce any appreciable effect, nor can the influence of even comparatively so good a conductor as distilled water be detected, and it is not until after the addition of a considerable quantity, 10 to 20 per cent of sulphuric acid or ammonium chloride that the insertion or withdrawal of the tube stops or starts the discharge.

A tube containing air at a low pressure is very efficacious in stopping the discharge, and the result of the comparison of the relative effects of an exhausted tube and a tube of the same size and shape containing a solution of an electrolyte are very remarkable. I found that an exhausted tube which contained air at a very low pressure (less than $\frac{1}{10}$ of a millimetre) produced as much effect on the discharge in the outer tube as a tube containing at least 50,000 times as many molecules of ammonium chloride. This would be expressed in the language of electrolysis by saying that under the electro-motive intensity to which it was exposed in this experiment the molecular conductivity of the gas is 50,000 times that of the electrolyte. The proportion between the number of air molecules and the number of molecules of an electrolyte, which produces an equal effect in stopping the discharge, depends upon the length of spark in the primary current, and so upon the electro-motive force acting upon the air. The longer the spark the greater is the molecular conductivity of the air in comparison with that of the electrolyte. This indicates that the conduction through the air does not follow Ohm's law. This is what we should expect, as under large electro-motive forces more molecules are split up and take part in the conduction of the electricity. This great conductivity of rarefied gases in those cases where the electricity has not to pass from metal, etc., into the gas are in striking contrast with the infinitesimally small values for the same property which are deduced from experiments on tubes with electrodes.

I was first led to suspect this high conductivity for rarefied gases by observing the appearance presented by the ring-discharge in bulbs; the ring, unless the pressure is exceedingly low, ceases at a distance of little more than 1 centim from the surface of the bulb, this thickness of conducting gas being sufficient to screen off the electromotive intensity from the interior. From experiments which I had made on the screening effect of electrolytes (*Proc. Roy. Soc.* XLV. p. 269), I knew that it would require a very strong solution of an electrolyte to produce screening comparable with this. To compare the screening effects more directly than by the method just described I tried the following experiment. The discharge-tube, Fig. 11, was pumped until the discharge passed through it very freely; an exhausted tube was then pushed down the central opening, it remained quite free from any visible discharge; the primary was now wound round a cylinder of the same diameter as the discharge-tube of Fig. 11, and this cylinder was filled with distilled water. When the tube, which had previously remained dark when placed in the exhausted discharge-tube, was immersed in the water, a brilliant discharge took place in it; and it was necessary to add about 25 per cent of sulphuric acid to the water before the shielding effect of the mixture was sufficient to keep the tube dark. This experiment shows perhaps even more directly than the other the great conductivity of a rarefied gas under large electro-motive forces when nothing but the gas is in the way of the passage of the current.

An experiment made in this connection illustrates the remark made before as to the large effects produced by discharges through the gas which are not accompanied by luminosity. A bulb A was fused into a tube B which was surrounded by the primary coil C, D. B was exhausted and then sealed off, while A was left connected to the pump. When A was at atmospheric pressure a bright discharge took place in B outside A; on pumping A a stage was reached in which no discharge could be seen in either A or B. On letting air into A the discharge appeared again in B; on pumping A still further a discharge appeared in A, but not in B. The appearance presented by the discharge round the bulb A (filled in this case with air at high pressure) is very remarkable. At the highest pressure at which the discharge passed it took the form of a thin ring round the middle of A; as the pressure got lower and lower the discharge broadened out, and at very low pressures formed for the greater part of its course two separate rings which ran together in the space between one side of the sphere and the tube.

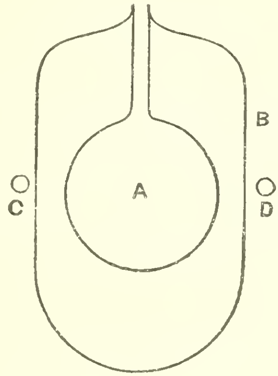

FIG. 13.

On the effect produced by conductors near the discharge tube.—The intensity of the discharge is very much affected by the presence of conductors in the neighborhood of the discharge tube, especially conductors which have large capacity or which are connected to earth. Let us take, for example, a very simple case, that of a bulb surrounded by a primary which is connected to earth; in this case the approach of the hand, or any conductor connected to earth, will make the discharge brighter and at the same time less well-defined at the edges; touching the tube, though this is already connected to earth, produces a very marked effect in increasing the facility of the discharge. We can, I think, understand the reason of this if we consider the behavior of the tubes of electrostatic induction. When the spark passes, these tubes (see Fig. 2, p. 234) rush out from the jars and make for the primary; in their journey to the primary they pass through the bulb and produce the discharge. Let us suppose now that there is a large conductor situated somewhere near the bulb; the tubes, as before, rush from the jar to the primary, but in doing so some of them strike against the conductor; the tubes which do so lose the portion inside the conductor, acquire two ends each on the surface of the conductor, and swing round until they are at right angles to its surface; they remain momentarily anchored, as it were, to the conductor, and if the conductor is in the neighborhood of the bulb, they will in general help to increase the maximum density of the tubes passing through the bulb. Though these tubes may not approximate to closed curves, and so directly produce a ring discharge, they may readily facilitate this discharge indirectly; for even those tubes which go radially through the bulb may produce a glow discharge from the glass into the bulb, and may thus furnish a supply of dissociated molecules through which the ordinary ring discharge can pass with much greater readiness. For nothing is more striking than the enormous difference produced in the electric strength of these rarefied gases by the passage of a spark. It is sometimes difficult to get the discharge to pass at first, but when once a spark has passed through the gas, a spark length one-quarter the length of that necessary to originate the discharge will be found sufficient to maintain it.

It is sometimes convenient, in cases where difficulty is found in starting the discharge, to avail ourselves of this property by connecting the mercury of the pump to which the tube is attached with one terminal of an induction coil, the other terminal of which is put to earth. When the induction coil is in action, a glow-discharge fills the pump and tube, and while this glow exists the electrodeless discharge can easily be started; once having been started, it will continue after the induction-coil is stopped. An experiment of this kind, which I had occasion to make, gave very clear evidence of the way in which dissociated molecules are projected in all directions from the negative electrode in an ordinary discharge tube, but not from the positive. The discharge tube was fused onto the pump, and at an elbow two terminals, *c* and *d*, Fig. 14, were fused into the glass; these terminals were connected with an induction coil, and the pressure in the tube was such that the electrodeless discharge would not start of itself. When the coil was turned on so that *c* was the negative electrode the electrodeless discharge in the tube at once took place, but no effect at all was produced when *c* was positive and *d* negative. We may thus regard the effect produced by the presence of a conductor as due to the conductor catching the tubes of electrostatic induction and concentrating them on the discharge tubes; these tubes in many cases acting indirectly by producing a glow discharge through the tube, which, by diminishing the electric strength of the gas, makes discharges of any other kind very much easier.

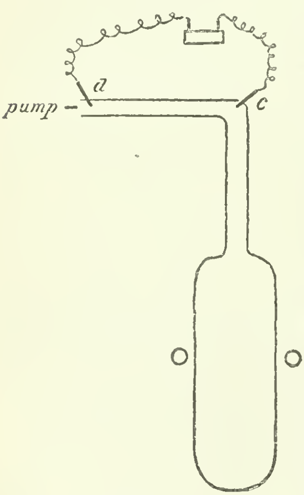

Fig. 14.

Though the presence of a conductor near the discharge tube will, in general, concentrate the tubes of electrostatic induction on the discharge tube more than would otherwise be the case, yet this does not always happen. When in some positions the conductor may hold back for a time from the discharge tube tubes of electrostatic induction which would otherwise pass through it, and thus diminish the maximum density of the tubes of electrostatic induction in the discharge tube, and hence tend to stop the discharge. I have frequently met with cases where the presence of a conductor diminishes the intensity of the discharge. One of the most striking of these is when the two jars are insulated, and a square discharge tube used. The spark was adjusted so that the discharge just, but only just, went round the tube. A sphere connected to earth was then moved round the discharge tube; in some positions it increased the brilliancy of the discharge, and the tube became quite bright, while in other positions it stopped the discharge altogether.

The observation of the behavior of the discharges through these tubes is a very convenient method of studying the effect of conductors in deflecting the flow of the tubes of electrostatic induction which fall upon them; for the appearance of the discharge is affected not merely by the average, but also by the maximum value of the electro-motive intensity which produces it. Thus a high maximum value, lasting only for a short time, might produce a discharge, while a more equable distribution of electro-motive intensity having the same average value might leave the tube quite dark.

I have employed these discharges to study the behavior of bodies under the action of very rapid electrical oscillations in the following way: In the primary circuit connecting the outside coatings of the jar two loops, A and B (Fig. 15), were made, in one of which, A, an exhausted bulb was placed, the spark-length and the pressure of the gas on it being adjusted until the discharge was sensitive, *i. e.*, until a small alteration in the electro-motive intensity acting on the bulb produced a considerable effect upon the appearance presented by the discharge. The substance whose behavior under rapid electrical vibrations was to be examined was placed in the loop B. The results got at first were very perplexing, and at first sight contradictory, and it was some time before I could see their explanation. The following are some of these results: When a highly exhausted bulb was placed in B a brilliant discharge passed through it, while the discharge in A stopped. A bulb of the same size, filled with a dilute solution of electrolyte, produced no appreciable effect; when filled with a strong solution it dimmed the discharge in A, but not to the same extent as the exhausted bulb. A piece of brass rod or tube increased the brightness of the discharge in A; on the other hand, a similar piece of iron rod or tube stopped the discharge in A at once. The most decided effect, however, was produced by a small crucible made of plumbago and clay; this, when put in the loop B, stopped the discharge in A completely. I found however that by considering the work spent on the substance placed in B, the preceding results could be explained. When a large amount of work is spent in B, the discharge in A will be dimmed, while no appreciable effect will be produced on A when the work spent in B is small. Now

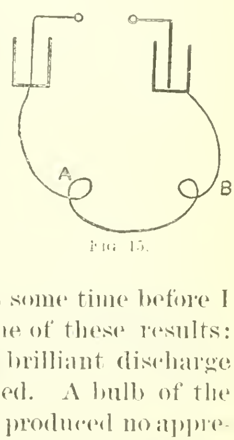

FIG. 15.

let us consider the work done in a secondary circuit whose resistance is R, whose coefficient of self-induction is L, and which has a coefficient of mutual induction, M, with the primary circuit. If the frequency of the current circulating in the primary is p, we can easily prove that the rate of absorption of work by the secondary is proportional to

$$\frac{R\,M^2 p^2}{L^2 p^2 + R^2}$$

Thus the work given to the secondary vanishes when $R = 0$ and when $R =$ infinity, and has a maximum value when $R = Lp$. Thus the condition that the secondary should absorb a considerable amount of work is that the resistance should not differ much from a value depending on the shape of the circuit and the frequency of the current in the primary. No appreciable amount of work is consumed when the resistance is very much greater or very much less than this value. I tested this result by placing inside B a coil of copper wire. When the ends were free, so that no current could pass through it, it produced no effect upon the bulb in A; when the ends were joined so that there was only a very small resistance in the circuit, the effect was, if anything, to increase the brightness of the discharge in A. When however the ends were connected through a carbon resistance which could be adjusted at will, the discharge in A became very distinctly duller when there was a very considerable resistance in the circuit. This experiment confirms the conclusion that to absorb energy the resistance must lie within certain limits, and be neither too large nor too small.

We can now see the cause of the differences observed when the substances mentioned above were put into B. The brass rod and tube did not dim the discharge in A, because their resistance was too low; the weak solution of electrolyte, because the resistance was too great; while the resistances of the crucible and the strong solution of electrolyte which obliterated the discharge from A were near the value for which the absorption of energy by the system was a maximum.

The case of iron is very interesting because it shows that even under these very rapidly alternating forces, iron still retains its magnetic properties. A striking illustration of the difference between iron and other metals is shown when we take an iron rod and place it in B, the discharge in A immediately stops; if we now slip a brass tube over the iron rod the discharge in A is at once restored. If on the other hand we use a brass rod and an iron tube, when the rod is put in B without the tube the discharge in A is bright; if we slip the iron tube over the rod, the discharge stops.

To compare the amount of heat produced in the brass and iron secondaries [calculations are introduced by which the author estimates that] for iron and copper cylinders of the same dimensions it would be about seventy times as large in the iron as in the copper, assuming that the iron retains its magnetic properties under these very rapidly alternating forces. The result explains the effect of the iron in stopping the discharge. As I am not aware that any magnetic properties of iron under such rapidly alternating forces have been observed, I was anxious to make quite sure that the difference between iron and brass was not due solely to the differences between their specific resistances. The first experiment I tried with this object was to cover the iron rod with thin sheet platinum, such as is used for Grove cells. As the resistance of platinum is not very different from that of iron, if the effect depended merely upon the resistance, slipping a thin tube of platinum over the iron ought to make very little difference. I found however that when the platinum was placed over the iron, all the peculiar effects produced by the latter were absent, thus showing that the effect is not due to the resistance of the iron. It then occurred to me that I might test the same thing in another way by magnetizing the iron to saturation, for in this state μ is nearly unity; thus if the result depended mainly on the magnetic properties of the iron it ought to diminish when the latter is strongly magnetized. I accordingly tried an experiment in which the iron in the coil B was placed between the poles of a powerful electro-magnet. When the magnet was "off" the iron almost stopped the discharge in A; when it was "on" the discharge became brighter, not indeed so bright as if the iron were away altogether, but still unmistakably brighter than when it was unmagnetized. This experiment, I think, proves that iron retains its magnetic properties when exposed to these rapidly alternating forces.

Another result worthy of remark is that though a brass rod or tube inserted in B does not stop the discharge in A, yet if a piece of glass tubing of the same dimensions is coated with Dutch metal, or if it has a thin film of silver deposited upon it, it will stop the discharge very decidedly. We are thus led to the somewhat unexpected result that a thin layer of metal when exposed to these very rapid electrical vibrations may absorb more heat than a thick one. I find, on calculating the heating effect in slabs of various thicknesses, that there is a thickness for which the heat absorbed is a maximum. - - -

The slight increase in the brightness of the discharge in A when a brass rod is placed in B is due, I think, to the diminution in the self-induction in the primary circuit produced by this rod whose conductivity is so good that it absorbs practically no heat.

We will now return to the case of bad conductors, where na is small; here the absorption of energy is proportional to the conductivity, and we might use this method to compare the conductivity of electrolytes for very rapidly alternating currents. I tried a few experiments of this kind and found, as I did in the experiments described in the *Proceedings of the Royal Society*, XLV, p. 269, that the ratio of the conductivities of two electrolytes was the same for rapidly alternating as for steady currents. I was anxious, however, to see whether these rapidly alternating currents could pass with the same facility as steady currents from an electrolite to a metal. To try this two equal beakers were filled with the same electrolyte made of such strength that when inserted in B they put out the discharge in A. I then placed in one beaker six ebonite diaphragms arranged so as to stop the eddy currents, and a similar metallic diaphragm in the other. The ebonite diaphragm made the beaker in which it was placed cease to have any effect upon the discharge in A. I could not detect however that the effect of the beaker in which the metal diaphragm was placed on the discharge in A was at all diminished by the introduction of the diaphragm. I conclude therefore that very rapidly alternating currents can pass with facility from electrolytes to metals and *vice versa*. In this respect electrolytes differ from gases, the currents in which, as we have seen, are stopped by a metallic diaphragm in the same way as they would be by an ebonite one.

It may be useful to observe in passing that a somewhat minute division of the electrolyte by the non-conducting diaphragm is necessary to stop the effect of the eddy currents; a division of the electrolytes into two or three portions seemed to produce very little effect.

Another point which is brought out by these experiments is the great conductivity of rarified gases when no electrodes are used as compared with that of electrolytes. An exhausted bulb will produce as much effect on the discharge in A as the same bulb filled with a solution of an electrolyte containing about a hundred thousand times as many molecules of electrolyte. The molecular conductivity of rarified gases when the electro-motive intensity is very great and when no electrodes are used must be thus enormously greater than that of electrolytes.

Bulbs filled with rarified gas used in the way I have described serve as galvanometers, by which we can estimate roughly the relative intensity of the current flowing through the primary coils which encircle them. Used for this purpose I have found them very useful in some experiments on which I am at present engaged, on the distribution of very rapidly alternating currents among a net-work of conductors.

THE MOLECULAR PROCESS IN MAGNETIC INDUCTION.*

By Prof. J. A. Ewing, F. R. S.

Magnetic induction is the name given by Faraday to the act of becoming magnetized, which certain substances perform when they are placed in a magnetic field. A magnetic field is the region near a magnet, or near a conductor conveying an electric current. Throughout such a region there is what is called magnetic force, and when certain substances are placed in the magnetic field the magnetic force causes them to become magnetized by magnetic induction. An effective way of producing a magnetic field is to wind a conducting wire into a coil, and pass a current through the wire. Within the coil we have a region of comparatively strong magnetic force, and when a piece of iron is placed there it may be strongly magnetized. Not all substances possess this property. Put a piece of wood or stone or copper or silver into the field, and nothing noteworthy happens; but put a piece of iron or nickel or cobalt and at once you find that the piece has become a magnet. These three metals, with some of their alloys and compounds, stand out from all other substances in this respect. Not only are they capable of magnetic induction—of becoming magnets while exposed to the action of the magnetic field, but when withdrawn from the field they are found to retain a part of the magnetism they acquired. They all show this property of retentiveness, more or less. In some of them this residual magnetism is feebly held, and may be shaken out or otherwise removed without difficulty. In others, notably in some steels, it is very persistent, and the fact is taken advantage of in the manufacture of permanent magnets, which are simply bars of steel, of proper quality, which have been subjected to the action of a strong magnetic field. Of all substances, soft iron is the most susceptible to the action of the field. It can also under favorable conditions, retain—when taken out of the field—a very large fraction of the magnetism that has been induced—more than nine-tenths,—more indeed than is retained by steel; but its hold of this residual magnetism is not firm, and for that reason it will not serve as a material for permanent magnets. My purpose to-night is to give some account of the molecular process through which we may conceive magnetic induction to take place, and of the structure which makes residual magnetism possible.

* Abstract of a Friday evening discourse delivered at the Royal Institution on May 22, 1891. (From *Nature*, Oct. 15, 1891; vol. XLIV, pp. 566–572.)

When a piece of iron or nickel or cobalt is magnetized by induction, the magnetic state permeates the whole piece. It is not a superficial change of state. Break the piece into as many fragments as you please, and you will find that every one of these is a magnet. In seeking an explanation of magnetic quality we must penetrate the innermost framework of the substance—we must go to the molecules.

Now, in a molecular theory of magnetism there are two possible beginnings. We might suppose, with Poisson, that each molecule becomes magnetized when the field begins to act. Or we may adopt the theory of Weber, which says that the molecules of iron are always magnets and that what the field does is to turn them so that they face more or less one way. According to this view, a virgin piece of iron shows no magnetic polarity, not because its molecules are not magnets, but because they lie so thoroughly "higgledy-piggledy" as regards direction that no greater number point one way than another. But when the magnetic force of the field begins to act, the molecules turn in response to it, and so a preponderating number come to face in the direction in which the magnetic force is applied, the result of which is that the piece as a whole shows magnetic polarity. All the facts go to confirm Weber's view. One fact in particular I may mention at once—it is almost conclusive in itself. When the molecular magnets are all turned to face one way, the piece has clearly received as much magnetization as it is capable of. Accordingly, if Weber's theory be true, we must expect to find that in a very strong magnetic field a piece of iron or other magnetizable metal becomes *saturated*, so that it can not take up any more magnetism, however much the field be strengthened. This is just what happens. Experiments were published a few years ago which put the fact of saturation beyond a doubt, and gave values of the limit to which the intensity of magnetization may be forced.

When a piece of iron is put in a magnetic field, we do not find that it becomes saturated unless the field is exceedingly strong. A weak field induces but little magnetism; and if the field be strengthened, more and more magnetism is acquired. This shows that the molecules do not turn with perfect readiness in response to the deflecting magnetic force of the field. Their turning is in some way resisted, and this resistance is overcome as the field is strengthened, so that the magnetism of the piece increases step by step. What is the directing force which prevents the molecules from at once yielding to the deflecting influence of the field, and to what is that force due? And again, how comes it after they have been deflected they return partially, but by no means wholly, to their original places when the field ceases to act?

I think these questions receive a complete and satisfactory answer when we take account of the forces which the molecules necessarily exert on one another in consequence of the fact that they are magnets. We shall study the matter by examining the behavior of groups of little magnets, pivoted like compass needles, so that each is free to turn except for the constraint which each one suffers on account of the presence of its neighbors.

But first let us see more particularly what happens when a piece of iron or steel or nickel or cobalt is magnetized by means of a field the strength of which is gradually augmented from nothing. We may make the experiment by placing a piece of iron in a coil, and making a current flow in the coil with gradually increased strength, noting at each stage the relation of the induced magnetism to the strength of the field. This relation is observed to be by no means a simple one: it may be represented by a curve (Fig. 1), and an inspection of the curve will show that the process is divisible, broadly, into three tolerably distinct stages. In the first stage (*a*) the magnetism is being acquired but slowly:

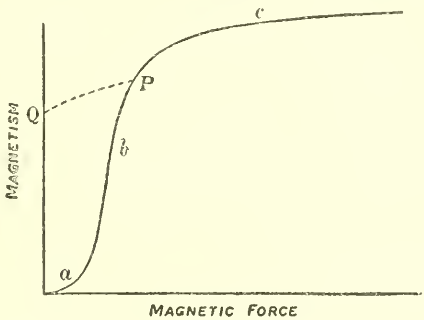

FIG. 1.

the molecules, if we accept Weber's theory, are not responding readily—they are rather hard to turn. In the second stage (*b*) their resistance to turning has to a great extent broken down, and the piece is gaining magnetism fast. In the third stage (*c*) the rate of increment of magnetism falls off: we are there approaching the condition of saturation, though the process is still a good way from being completed.

Further, if we stop at any point of the process, such as *P*, and gradually reduce the current in the coil until there is no current, and therefore no magnetic field, we shall get a curve like the dotted line *PQ*, the height of *Q* showing the amount of the residual magnetism.

If we make this experiment at a point in the first stage (*a*), we shall find, as Lord Rayleigh has shown, little or no residual magnetism; if we make it at any point in the second stage (*b*), we shall find very much residual magnetism; and if we make it at any point in the third stage (*c*), we shall find only a little more residual magnetism than we should have found by making the experiment at the end of stage *b*. That part of the turning of the molecules which goes on in stage *a* contributes nothing to the residual magnetism. That part which goes on in stage *c* contributes little. But that part of the turning which goes on in stage *b* contributes very much.

In some specimens of magnetic metal we find a much sharper separation of the three stages than in others. By applying strain in certain ways it is possible to get the stages very clearly separated. Fig. 2, a beautiful instance of that, is taken from a paper by Mr. Nagaoka—one of an able band of Japanese workers who are bidding fair to repay the debt that Japan owes for its learning to the West. It shows how a piece of nickel which is under the joint action of pull and twist becomes magnetized in a growing magnetic field. There the first stage is exceptionally prolonged, and the second stage is extraordinarily abrupt.

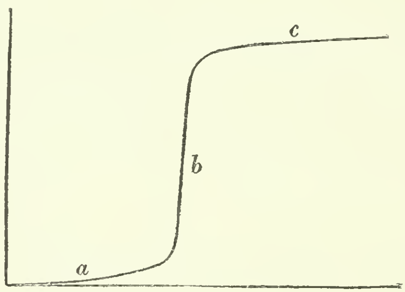

FIG. 2.

The bearing of all this on the molecular theory will be evident when we turn to these models, consisting of an assemblage of little pivoted magnets, which may be taken to represent, no doubt in a very crude way, the molecular structure of a magnetizable metal. I have here some large models, where the pivoted magnets are pieces of sheet steel, some cut into short flat bars, others into diamond shapes with pointed ends, others into shapes resembling mushrooms or umbrellas, and in these the magnetic field is produced by means of a coil of insulated wire wound on a large wooden frame below the magnets. Some of these are arranged with the pivots on a gridiron or lazy-tongs of jointed wooden bars, so that we may readily distort them, and vary the distances of the pivots from one another, to imitate some of the effects of strain in the actual solid. But to display the experiments to a large audience a lantern model will serve best. In this one the magnets are got by taking to pieces numbers of little pocket compasses. The pivots are cemented to a glass plate, through which the light passes in such a way as to project the shadows of the magnets on the screen. The magnetic force is applied by means of two coils, one on either side of the assemblage of magnets and out of the way of the light, which together produce a nearly uniform magnetic field throughout the whole group. You see this when I make manifest the field in a well-known fashion, by dropping iron filings on the plate.

We shall first put a single pivoted magnet on the plate. So long as no field acts it is free to point anyhow—there is no direction it prefers to any other. As soon as I apply even a very weak field it responds, turning at once into the exact direction of the applied force, for there was nothing (beyond a trifling friction at the pivot) to prevent it from turning.

Now try two magnets. I have cut off the current, so that there is at present no field, but you see at once that the pair has, so to speak, a will of its own. I may shake or disturb them as I please, but they insist on taking up a position (Fig. 3) with the north end of one as close as pos-

Fig. 3.

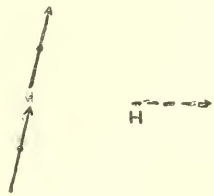

Fig. 4.

sible to the south end of the other. If disturbed they return to it; this configuration is highly stable. Watch what happens when the magnetic field acts with gradually growing strength. At first, so long as the field is weak (Fig. 4), there is but little deflection; but as the deflection increases it is evident that the stability is being lost, the state is getting more and more critical, until (Fig. 5) the tie that holds them

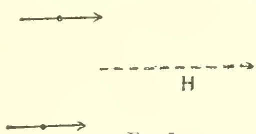

Fig. 5.

together seems to break, and they suddenly turn, with violent swinging, into almost perfect alignment with the magnetic force H. Now I gradually remove the force, and you see that they are slow to return, but a stage comes when they swing back, and a complete removal of the force brings them into the condition with which we began (Fig. 3).

If we were to picture a piece of iron as formed of a vast number of such pairs of molecular magnets, each pair far enough from its neighbors to be practically out of reach of their magnetic influence, we might deduce many of the observed magnetic properties, but not all. In particular, we should not be able to account for so much residual

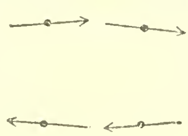

Fig. 6.

magnetism as is actually found. To get that, the molecules must make new connections when the old ones are broken; their relations are of a kind more complex than the quasi-matrimonial one which the experiment exhibits. Each molecule is a member of a larger community, and has probably many neighbors close enough to affect its conduct.

We get a better idea of what happens by considering four magnets (Fig 6). At first, in the absence of deflecting magnetic force, they group themselves in stable pairs—in one of a number of possible combinations. Then—as in the former case—when magnetic force is applied, they are at first slightly deflected, in a manner that exactly tallies with what I have called the stage *a* of the magnetizing process.

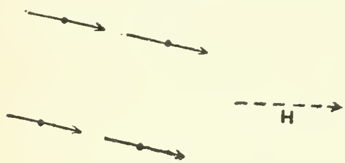

Fig. 7.

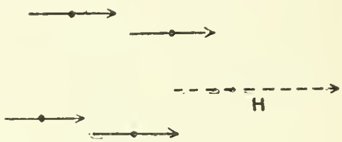

Fig. 8.

Next comes instability. The original ties break up, and the magnets swing violently round; but finding a new possibility of combining (Fig. 7), they take to that. Finally, as the field is further strengthened they are drawn into perfect alignment with the applied magnetic force. (Fig. 8).

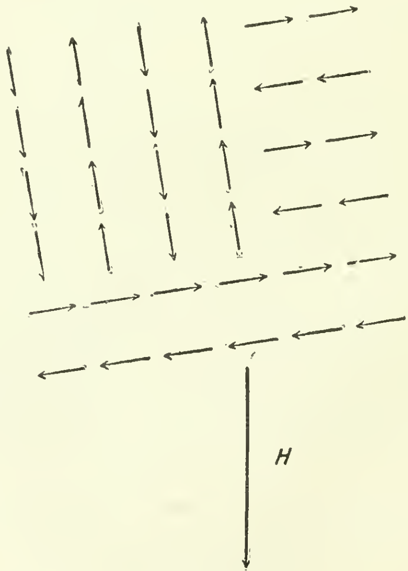

Fig. 9.

We see the same three stages in a multiform group (Figs. 9, 10, 11). At first, the group, if it is shuffled by any casual disturbance, arranges itself at random in lines that give no resultant polarity (Fig.

9). A weak force produces no more than slight quasi-elastic deflections; a stronger force breaks up the old lines, and forms new ones more favorably inclined to the direction of the force (Fig. 10). A very strong force brings about saturation (Fig. 11).

In an actual piece of iron there are multitudes of groups lying differently directed to begin with—perhaps also different as regards the spacing of their members. Some enter the second stage while others are still in the first, and so on. Hence, the curve of magnetization does not consist of perfectly sharp steps, but has the rounded outlines of Fig. 1.

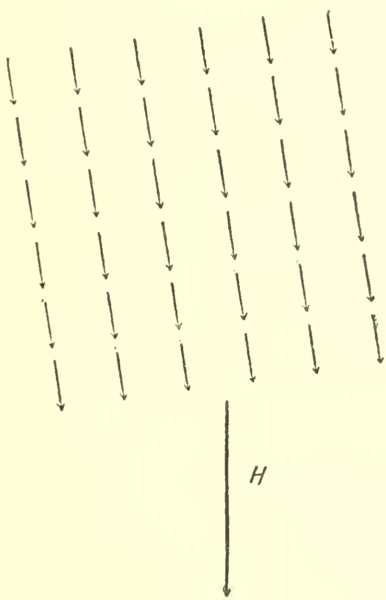

FIG. 10.

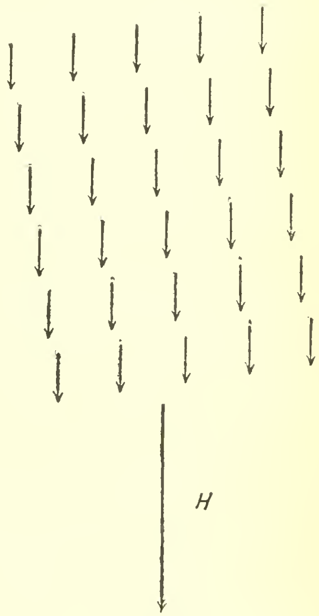

FIG. 11.

Notice, again, how the behavior of these assemblages of elementary magnets agrees with what I have said about residual magnetism. If we stop strengthening the field before the first stage is passed—before any of the magnets have become unstable and have tumbled round into new places—the small deflection simply disappears, and there is no residual effect on the configuration of the group. But if we carry the process far enough to have unstable deflections, the effects of these persist when the force is removed, for the magnets then retain the new grouping into which they have fallen (Fig. 10). And again, the quasi-elastic deflections which go on during the third stage do not add to the residual magnetism.

Notice, further, what happens to the group if after applying a magnetic force in one direction and removing it, I begin to apply force in the opposite direction. At first there is little reduction of the residual polarity, till a stage is reached when instability begins, and then reversal occurs with a rush. We thus find a close imitation of all the features that are actually observed when iron or any of the other magnetic metals is carried through a cyclic magnetizing process (Fig 12). The effect of any such process is to form a *loop* in the curve which expresses the relation of the magnetism to the magnetizing force. The changes of magnetism always lag behind the changes of magnetizing force. This tendency to lag behind is called magnetic *hysteresis*.

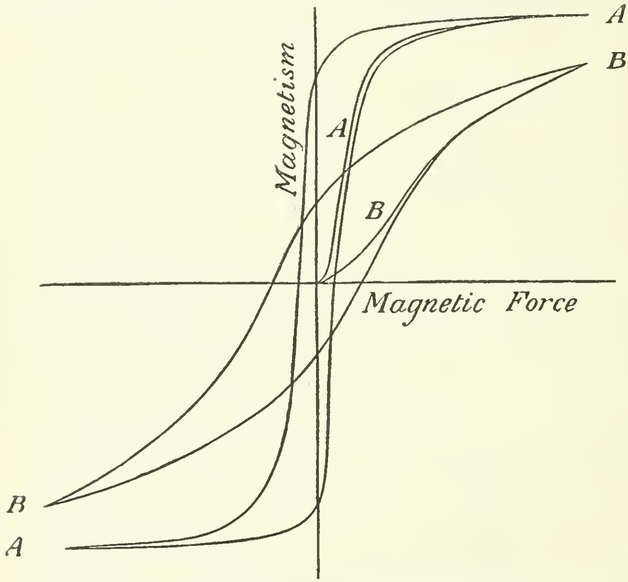

FIG. 12.—Cyclic reversal of magnetization in soft iron (AA), and in the same iron when hardened by stretching (BB).

We have a manifestation of hysteresis whenever a magnetic metal has its magnetism changed in any manner through changes in the magnetizing force, unless indeed the changes are so minute as to be confined to what I have called the first stage (*a*, Fig. 1). Residual magnetism is only a particular case of *hysteresis*.

Hysteresis comes in whatever be the character or cause of the magnetic change, provided it involves such deflections on the part of the molecules as make them become unstable. The unstable movements are not reversible with respect to the agent which produces them; that is to say, they are not simply undone step by step as the agent is removed.

We know, on quite independent grounds, that when the magnetism of a piece of iron or steel is reversed, or indeed cyclically altered in any way, some work is spent in performing the operation—energy is being given to the iron at one stage, and is being recovered from it at another; but when the cycle is taken as a whole there is a net loss, or rather a waste of energy. It may be shown that this waste is proportional to the area of the loop in our diagrams. This energy is dissipated: that is to say, it is scattered and rendered useless; it takes the form of heat. The iron core of a transformer, for instance, which is having its magnetism reversed with every pulsation of the alternating current, tends to become hot for this very reason: indeed, the loss of energy which happens in it, in consequence of magnetic *hysteresis*, is a serious drawback to the efficiency of alternating-current systems of distributing electricity. It is the chief reason why they require much more coal to be burnt, for every unit of electricity sold, than direct-current systems require.

The molecular theory shows how this waste of energy occurs. When the molecule becomes unstable and tumbles violently over, it oscillates and sets its neighbors oscillating, until the oscillations are damped out by the eddy currents of electricity which they generate in the surrounding conducting mass. The useful work that can be got from the molecule as it falls over is less than the work that is done in replacing it during the return portion of the cycle. This is a simple mechanical deduction from the fact that the movement has unstable phases.

I can not attempt, in a single lecture, to do more than glance at several places where the molecular theory seems to throw a flood of light on obscure and complicated facts, as soon as we recognize that the constraint of the molecules is due to their mutual action as magnets.

It has been known since the time of Gilbert that vibration greatly facilitates the process of magnectic induction. Let a piece of iron be briskly tapped while it lies in the magnetic field, and it is found to take up a large addition to its induced magnetism. Indeed, if we examine the successive stages of the process while the iron is kept vibrating by being tapped, we find that the first stage (a) has practically disappeared, and there is a steady and rapid growth of magnetism almost from the very first. This is intelligible enough. Vibration sets the molecular magnets oscillating, and allows them to break their primitive mutual ties and to respond to weak deflecting forces. For a similar reason, vibration should tend to reduce the residue of magnetism which is left when the magnetizing force is removed, and this, too, agrees with the results of observation.

Perhaps the most effective way to show the influence of vibration is to apply a weak magnetizing force first, before tapping. If the force is adjusted so that it nearly but not quite reaches the limit of stage (a), a great number of the molecular magnets are, so to speak, hovering on the verge of instability, and when the piece is tapped they go over like a house of cards, and magnetism is acquired with a rush. Tapping always has some effect of the same kind, even though there has been no special adjustment of the field.

And other things besides vibration will act in a similar way, precipitating the break-up of molecular groups when the ties are already strained. Change of temperature will sometimes do it, or the application or change of mechanical strain. Suppose, for instance, that we apply pull to an iron wire while it hangs in a weak magnetic field, by making it carry a weight. The first time that we put on the weight, the magnetism of the wire at once increases, often very greatly, in consequence of the action I have just described (Fig. 13). The molecules have been on the verge of turning, and the slight strain caused by the weight is enough to make them go. Remove the weight, and there is only a comparatively small change in the magnetism, for the greater part of the molecular turning that was done when the weight was put on is not undone when it is taken off. Re-apply the weight, and you find again but little change, though there are still traces of the kind of action which the first application brought about. That is to say, there are some groups of molecules which, though they were not broken up in the first application of the weight, yield now, because they have lost the support they then obtained from neighbors that have now entered into new combinations. Indeed, this kind of action may often be traced, always diminishing in amount, during several successive applications and removals of the load (see Fig. 13), and it is only when the process of loading has been many times repeated that the magnetic change brought about by loading is just opposite to the magnetic change brought about by unloading.

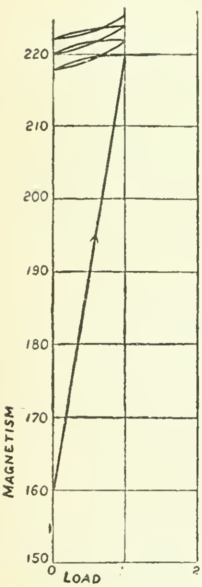

FIG. 13.—Effects of loading a soft iron wire in a constant field.

Whenever indeed we are observing the effects of an alteration of physical condition on the magnetism of iron, we have to distinguish between the primitive effect, which is often very great and is not reversible, and the ultimate effect, which is seen only after the molecular structure has become somewhat settled through many repetitions of the process. Experiments on the effects of temperature, of strain, etc., have long ago shown this distinction to be exceedingly important; the molecular theory makes it perfectly intelligible.

Further, the theory makes plain another curious result of experiment. When we have loaded and unloaded the iron wire many times over, so that the effect is no longer complicated by the primitive action I have just described, we still find that the magnetic changes which occur while the load is being put on are not simply undone, step by step, while the load is being taken off. Let the whole load be divided into several parts, and you will see that the magnetism has two different values, in going up and in coming down, for one and the same intermediate value of the load. The changes of magnetism lag behind the changes of load; in other words, there is *hysteresis* in the relation of the magnetism to the load (Fig. 14). This is because some of the molecular groups are every time being broken up during the loading, and re-established during the unloading, and that, as we saw already, involves *hysteresis*. Consequently, too, each loading and unloading requires the expenditure of a small quantity of energy, which goes to heat the metal.

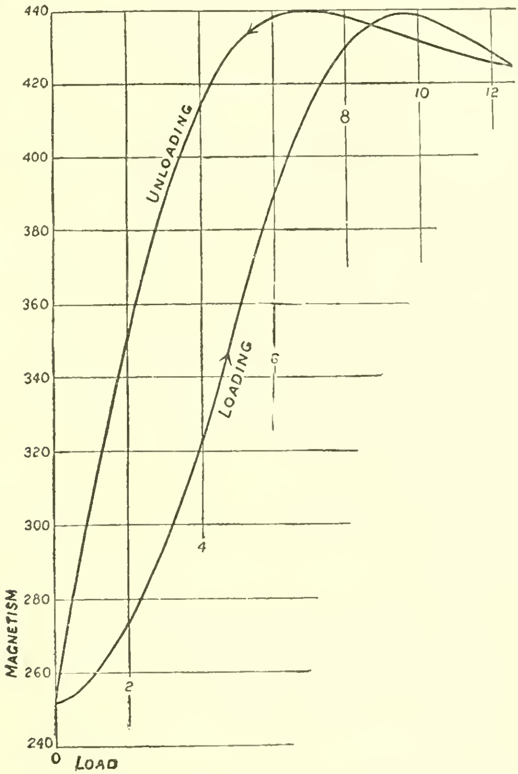

FIG. 14.—Cycle of loading and unloading.

Moreover, a remarkably interesting conclusion follows. This *hysteresis*, and consequent dissipation of energy, will also happen though there be no magnetization of the piece as a whole; it depends on the fact that the molecules are magnets. Accordingly, we should expect to find, and experiment confirms this (see *Phil. Trans.*, 1885, p. 614), that if the wire is loaded and unloaded, even when no magnetic field acts and there is no magnetism, its physical qualities which are changed by the load will change in a manner involving *hysteresis*. In particular, the length will be less for the same load during loading than during unloading, so that work may be wasted in every cycle of loads. There can be no such thing as perfect elasticity in a magnetizable metal, unless, indeed, the range of the strain is so very narrow that none of the molecules tumble through unstable states. This may have something to do with the fact, well known to engineers, that numerous repetitions of a straining action, so slight as to be safe enough in itself, have a dangerous effect on the structure of iron or steel.

Another thing on which the theory throws light is the phenomenon of time-lag in magnetization. When a piece of iron is put into a steady magnetic field, it does not take instantly all the magnetism that it will take if time be allowed. There is a gradual creeping up of the magnetism, which is most noticeable when the field is weak and when the iron is thick. If you will watch the manner in which a group of little magnets breaks up when a magnetic force is applied to it, you will see that the process is one that takes time. The first molecule to yield is some outlying one which is comparatively unattached—as we may take the surface molecules in the piece of iron to be. It falls over, and then its neighbors, weakened by the loss of its support, follow suit, and gradually the disturbance propagates itself from molecule to molecule throughout the group. In a very thin piece of iron—a fine wire, for instance—there are so many surface molecules, in comparison with the whole number, and consequently so many points which may become origins of disturbance, that the breaking up of the molecular communities is too soon over to allow much of this kind of lagging to be noticed.

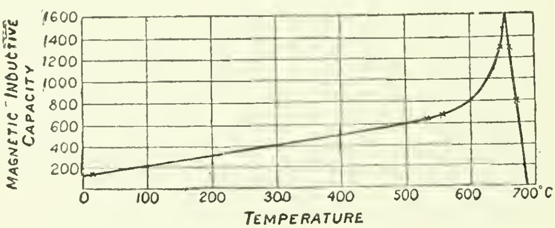

FIG. 15.—Relation of magnetic inductive capacity to temperature in hard steel (Hopkinson).

Effects of temperature, again, may be interpreted by help of the molecular theory. When iron or nickel or cobalt is heated in a weak magnetic field, its susceptibility to magnetic induction is observed to increase, until a stage is reached, at a rather high temperature, when the magnetic quality vanishes almost suddenly and almost completely. Fig. 15, from one of Hopkinson's papers, shows what is observed as the temperature of a piece of steel is gradually raised. The sudden loss of magnetic quality occurs when the metal has become red hot; the magnetic quality is recovered when it cools again sufficiently to cease to glow. Now, as regards the first effect—the increase of susceptibility with increase of temperature—I think that is a consequence of two independent effects of heating. The structure is expanded so that the molecular centers lie further apart. But the freedom with which the molecules obey the direction of any applied magnetic force is increased not by that only, but perhaps even more by their being thrown into vibration. When the field is weak, heating consequently assists magnetization, sometimes very greatly, by hastening the passage from stage *a* to stage *b* of the magnetizing process. And it is at least a conjecture worth consideration whether the sudden loss of magnetic quality at a higher temperature is not due to the vibrations becoming so violent as to set the molecules spinning, when, of course, their polarity would be of no avail to produce magnetization. We know at all events that when the change from the magnetic to the non-magnetic state occurs, there is a profound molecular change, and heat is absorbed which is given out again when the reverse change takes place. In cooling from a red heat, the iron actually extends at the moment when this change takes place (as was shown by Gore), and so much heat is given out that (as Barrett observed) it re-glows, becoming brightly red, though just before the change it had cooled so far as to be quite dull. [Experiment, exhibiting retraction and re-glow in cooling, shown by means of a long iron wire, heated to redness by an electric current.] The changes which occur in iron and steel about the temperature of redness are very complex, and I refer to this as only one possible direction in which a key to them may be sought. Perhaps the full explanation belongs as much to chemistry as to physics.

An interesting illustration of the use of these models has reached me, only to-day, from New York. In a paper just published in the *Electrical World* (reprinted in the *Electrician* for May 29, 1891), Mr. Arthur Hoopes supports the theory I have laid before you by giving curves which show the connection experimentally found by him between the result polarity of a group of little pivoted magnets and the strength of the magnetic field, when the field is applied, removed, reversed, and so on. I shall draw these curves on the screen, and rough as they are, in consequence of the limited number of magnets, you see that they succeed remarkably well in reproducing the features which we know the curves for solid iron to possess.

It may, perhaps, be fairly claimed that the models whose behavior we have been considering have a wider application in physics than to merely elucidate magnetic processes. The molecules of bodies may have polarity which is not magnetic at all—polarity, for instance, due to static electrification—under which they group themselves in stable forms, so that energy is dissipated whenever these are broken up and re-arranged. When we strain a solid body beyond its limit of elasticity, we expend work irrecoverably in overcoming, as it were, internal friction. What is this internal friction due to but the breaking and making of molecular ties? And if internal friction, why not also the surface friction which causes work to be spent when one body rubs upon another. In a highly suggestive passage of one of his writings,*

* *Encyclopædia Brit.*, Ninth Ed., 1877, art., "Constitution of Bodies," vol. VI, p. 313.

Clerk Maxwell threw out the hint that many of the irreversible processes of physics are due to the breaking up and re-construction of molecular groups. The models help us to realize Maxwell's notion, and in studying them to-night, I think we may claim to have been going a step or two forward where that great leader pointed the way.

CRYSTALLIZATION.*

By G. D. Liveing, F. R. S.

There is something very fascinating about crystals. It is not merely the intrinsic beauty of their forms, their picturesque grouping, and the play of light upon their faces, but there is a feeling of wonder at the power of nature, which causes substances, in passing from the fluid to the solid state, to assume regular shapes bounded by plane faces, each substance with its own set of forms, and its faces arranged with characteristic symmetry; some, like alum, in perfect octahedra; others, like blue vitriol, in shapes which are regularly oblique. It is this power of nature which is the subject of this discourse. I hope to show that crystalline forms, with all their regularity and symmetry, are the outcome of the accepted principles of mechanics. I shall invoke no peculiar force, but only such as we are already familiar with in other facts of nature. I shall call in only the same force that produces the rise of a liquid in a capillary tube and the surface-tension at the boundary of two substances which do not mix. Whether this force be different from gravity I need not stop to inquire, for any attractive force which for small masses, such as we suppose the molecules of matter to be, is only sensible at insensible distances is sufficient for my purpose.

We know that the external forms of crystals are intimately connected with their internal structure. This is betrayed by the cleavages with which in mica and selenite everybody is familiar, and which extend to the minutest parts, as is seen in the tiny rhombs which form the dust of crushed calcite. It is better marked by the optical properties, single and double refraction, and the effects of crystals on polarized light. These familiar facts lead up to the thought that it is really the internal structure which determines the external form. As a starting-point for considering that structure, I assume that crystalline matter is made up of molecules, and that, whereas in the fluid state the molecules move about amongst themselves, in the solid state they have little freedom. They are always within the range of each other's influence and do not change their relative places. Nevertheless, these molecules are in constant and very rapid motion. Not only will they communicate heat to colder bodies in contact with them, but they are

*A discourse delivered at the Royal Institution of Great Britain on Friday, May 15, 1891.—From *Nature*, June 18, 1891; vol. XLIV, pp. 156-160.

always radiating, which means producing waves in the æther at the rate of many billions in a second. We are sure that they have a great deal of energy, and, if they can not move far, they must have very rapid vibratory motions. It is reasonable to suppose that the parts of each molecule swing, backwards and forwards, through, or about, the center of mass of the molecule. The average distances to which the parts swing will determine the average dimensions of the molecule, the average space it occupies.

Dalton fancied he had proved that the atoms of the chemical elements must be spherical, because there was no assignable cause why they should be longer in one dimension than another. I rather invert his argument. I see no reason why the excursions of the parts of a molecule from the centre of mass should be equal in all directions, and therefore assume, as the most general case, that these excursions are unequal in different directions. And, since the movements must be symmetrical with reference to the centre of mass of the molecule, they will in general be included within an ellipsoid, of which the center is the centre of mass.

Here I may perhaps guard against a misconception. We chemists are familiar with the notion of complex molecules; and most of us figure to ourselves a molecule of common salt as consisting of an atom of sodium and one of chlorine held together by some sort of force, and it may be imagined that these atoms are the parts of the molecules which I have in mind. That however is not my notion. I am paradoxical enough to disbelieve altogether in the existence of either sodium or chlorine in common salt. Were my audience a less philosophical one I could imagine I heard the retort from many a lip: "Why, you can get sodium and chlorine out of it, and you can make it out of sodium and chlorine!" But no, you can not get either sodium or chlorine out of common salt without first adding something which seems to me of the essence of the matter. You can get neither sodium nor chlorine from it without adding energy; nor can you make it out of these elements without subtracting energy. My point is that energy is of the essence of the molecule. Each kind of molecule has its own motion; and in this I think most physicists will agree with me. Chemists will agree with me in thinking that all the molecules of the same element, or compound, are alike in mass, and in the space they occupy at a given temperature and pressure. The only remaining assumption I make is that the form of the ellipsoid—the relative lengths of its axes—is *on the average* the same for all the molecule sof the same substance. This implies that the distances of the excursions of the parts of the molecule depend on its constitution, and are, on the average, the same in similarly constituted molecules under similar circumstances.

I have come to the end of my postulates. I hope they are such as you will readily concede. I want you to conceive of each molecule as having its parts in extremely rapid vibration, so that it occupies a larger space than it would occupy if its parts were at rest; and that the excursions of the parts about the center of mass are on the average, at a given temperature and pressure, comprised within a certain ellipsoid; that the dimensions of this ellipsoid are the same for all molecules of the same chemical constitution, but different for molecules of different kinds.

We have now to consider how these molecules will pack themselves on passing from the fluid state, in which they can and do move about amongst themselves, into the solid state, in which they have no sensible freedom. If they attract one another, according to any law, and for my purpose gravity will suffice, then the laws of energy require that for stable equilibrium the potential energy of a system shall be a minimum. This is the same, in the case we are considering, as saying that the molecules shall be packed in such a way that the distances between their centers of mass shall on the whole be the least possible; or, that as many of them as possible shall be packed into unit space. In order to see how this packing will take place, it will be easiest to consider first the particular case in which the axes of the ellipsoids are all equal—that is, when the ellipsoids happen to be spheres. The problem is then reduced to finding how to pack the greatest number of equal spherical balls into a given space. It is easy to reduce this to the problem of finding how the spheres can be arranged so that each one shall be touched by as many others as possible. In this way the cornered spaces between the balls, the unoccupied room, is reduced to a minimum. You can stack balls so that each is touched by twelve others, but not by more. At first sight it seems as if this might be done in two ways.

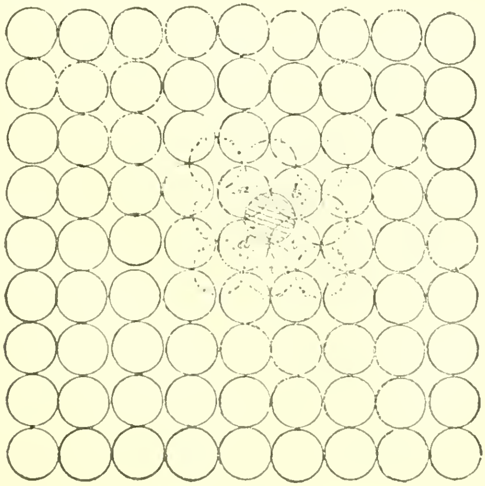

FIG. 1.

In the first place we may start with a square of balls, as in Fig. 1, where each is touched by four others. We may then place another (shaded in the figure) so as to rest on four, and place four more in adjacent holes to touch it, as indicated by the dotted circles. Above these four more may be placed in the openings *a b c d*, so as to touch it—making twelve in all. If the pile be completed, we shall get a four-sided pyramid, of which each side is an equilateral triangle, as

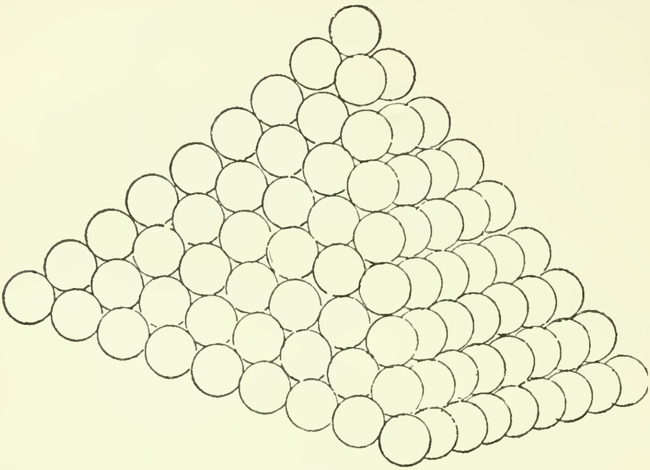

Fig. 2.

represented in Fig. 2. It will be seen that, in these triangular faces, each ball (except, of course, those forming the edges) is touched by six others. Again, if we start with such a triangle, as in Fig. 3, where each ball is touched by six others, we can place one ball—the shaded one—so as

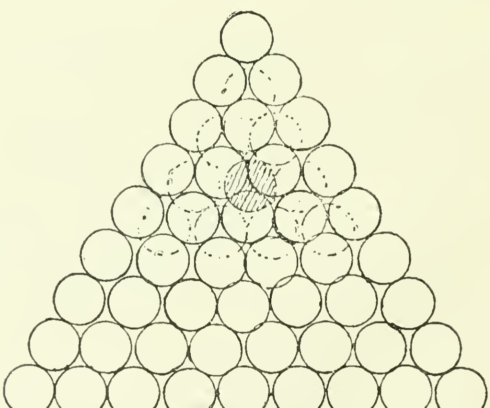

Fig. 3.

to rest on three others, and can then place six more round it and touching it, as indicated by the dotted circles. In three of the triangular holes between the shaded ball and the dotted balls touching it we can place three more, so as to touch the shaded ball—again twelve touching it in all. If we complete the pile, we shall get the triangular pyramid represented by Fig. 4, where each of the three sides is a right-angled triangle, while the base is an equilateral triangle. It will be seen that in the faces of this pyramid each ball (except those outside) is touched by four others. In fact, the arrangement in these faces is the same as in the base of the former pyramid; and the two arrangements are really identical in the interior, only one has to be turned over in order to bring it into parallelism with the other. Fig. 2 represents half a regular octahedron; Fig 4 the corner of a cube. Ellipsoids if they are all equal and similiar to one another can be packed in precisely the same way, so that each is touched by twelve others, provided their axes are kept parallel to each other—that is, if they are all oriented alike. This, then, by the laws of energy, will be the arrangement which the molecules will assume in consequence of mutual attraction, in passing from a fluid to a solid state.

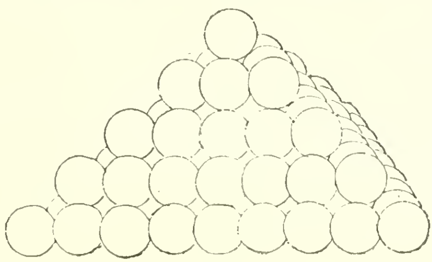

FIG. 4.

Next, let us see how the packing of the molecules will affect the external form. And here I bring in the surface tension. We are familiar with the effects of this force in the case of liquids, and if we adopt the usually received theory of it, we must have a surface tension at the boundary of a solid, as well as at the surface of a liquid. I know of no actual measures of the surface tension of solids; but Quincke has given us the surface tensions of a number of substances at temperatures near their points of solidification, in dynes per lineal centimeter, as follows:

Platinum	648	Antimony	244
Gold	983	Borax	212
Zinc	860	Sodium carbonate	206
Tin	587	Sodium chloride	114
Mercury	577	Water	86.2
Lead	418	Selenium	70.1
Silver	419	Sulphur	41.3
Bismuth	382	Phosphorus	41.1
Potassium	364	Wax	33.1
Sodium	253		

The surface tensions of most of the solids are probably greater than these, for the surface tension generally diminishes with increase of temperature; and you see that they amount to very considerable forces.

We have to do, then, with an agency which we can not neglect In all these cases the tension measured is at a surface bounded by air, and is such as tends to contract the surface. We have, then, at the boundary between a crystallizing solid and the fluid, be it gas or liquid, out of which it is solidifying, a certain amount of potential energy; and by the laws of energy the condition of equilibrium is, that this potential energy shall be a minimum. The accepted theory of surface tension is that it arises from the mutual attraction of the molecules. The energy will, therefore, be a minimum for a surface in which the molecules are as closely set as possible.

Now, if you draw a surface through a heap of balls packed so that each is touched by twelve others, you will find that the surfaces which have the greatest number of centers of balls per unit area are all plane surfaces. That in which the concentration is greatest is the surface of a regular octahedron, next come that of a cube, then that of a rhombic dodecahedron, and so on according to the law of indices of crystallographers. The relative numerical values of these concentrations are as follows, taking that of the faces of the cube as unity:

Octahedron	1.1547	Tetrakishexahedron	0.4472
Cube	1.0000	Eikositessarahedron	0.4083
Dodecahedron	0.7071	Triakisoctahedron	0.3333

We do know that the surface tension is exactly in the inverse proportion to the concentration; all that we can at present say is that it increases as the concentration diminishes.

If, then, the molecules occupy spherical spaces, the bounding surface will tend to be a regular octahedron.

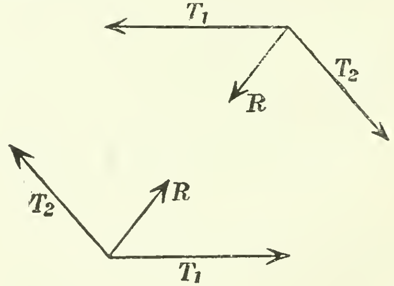

FIG. 5.

But we have another point to consider. If a solid is bounded by plane surfaces, there must be edges where the planes meet. At such an edge the surface tensions will have a resultant (see Fig. 5) tending to compress the mass, which must be met by a corresponding opposite pressure, and unless there is some internal strain there must be a correspondent resultant of the tensions on the opposite side of the crystal. Hence, if one face of a form is developed the opposite face will also be developed; and generally, if one face of a form be developed all the faces will be developed; and if one edge, or angle, be truncated, all the corresponding edges, or angles, will be truncated. Were it otherwise, there would not be a balance between the surface tensions in the several faces. But there is another point to be taken into account. The surface energy may become less in two ways—either by reducing the tension per unit surface, or by reducing the total surface. When a liquid separates from another fluid, as chloroform from a solution of chloral hydrate on adding an alkali, or a cloud from moist air, the liquid assumes the form which, for a given mass, has the least surface—that is, the drops are spherical. If you cut off the projecting corners and plane away the projecting edges of a cube or an octahedron, you bring it nearer to a sphere, and if you suppose the volume to remain constant, you still diminish the surface. And if the diminution of the total surface is not compensated by the increased energy on the truncations, there will be a tendency for the crystals to grow with such truncations. The like will be true in more complicated combinations. There will be a tendency for such combinations to form, provided the surface energy of the new faces is not too great as compared with that of the first simple form.

But it does not always happen that an octahedron of alum develops truncated angles. This leads to another point. To produce a surface in a continuous mass requires a supply of energy, and to generate a surface in the interior of any fluid is not easy. Air may be super-saturated with aqueous vapor, or a solution with a salt, and no cloud or crystals be formed, unless there is some discontinuity in the mass, specks of dust, or something of the kind. In like manner, if we have a surface already, as when a super-saturated solution meets the air or the sides of the vessel containing it, and if the energy of either of these surfaces is less than that of a crystal of the salt, some energy will have to be supplied in order to produce the new surface, but not so much as if there were no surface there to begin with. Hence, crystals usually form on the sides of the vessel or at the top of the liquid. When a solid separates from a solution there is generally some energy available from the change of state, which supplies the energy for the new surface. But at first when the mass deposited is very small, the energy available will be correspondingly small, and since the mass varies as the cube of the diameter of the solid, whereas the surface varies as the square of the diameter, the first separated mass is liable to be squeezed into liquid again by its own surface tension. This explains the usual phenomena of super-saturated solutions. A deposit occurs most easily on a surface of the same energy as that of the deposit, because the additional energy required is only for the increased extent of surface. It explains, too, the tendency of large crystals to grow more rapidly than small ones, because the ratio of the increase of surface to that of volume diminishes as the crystal grows.

While speaking of the difficulty of creating a new surface in the interior of a mass, the question of cleavage suggests itself. In dividing a crystal we create two new surfaces—one on each piece, and each with its own energy. The division must therefore take place most readily when that surface energy is a minimum. Hence the principal cleavage of a crystal made up of molecules having their motions comprised within spherical spaces will be octahedral. As a fact, we find that the greater part of substances which crystallize in the octahedral, or regular system, have octahedral cleavage. But not all; there are some, like rock salt and galena, which cleave into cubes, and a very few, like blende, have their easiest cleavage dodecahedral. These I have to explain. I may however first observe that some substances—as, for instance, fluor-spar—which have a very distinct octahedral cleavage are rarely met with in the form of octahedra, but usually in cubes. In regard to this, we must remember that the surface energy depends upon the nature of both the substances in contact at the surface, as well as on their electrical condition, their temperature, and other circumstances. The closeness of the molecules in the surface of the solid determines the energy, so far as the solid alone is concerned; but that is not the only —though it may be the most important—factor conducing to the result It is therefore quite possible that, under the circumstances in which the natural crystals of fluor were formed, the surface energy of the cubical faces was less than that of the octahedral, although when we experiment on them in the air, it is the other way. This supposition is confirmed by the well-known fact that the form assumed by many salts in crystallizing is affected by the character of the solution. Thus alum, which from a solution in pure water always assumes the octahedral form, takes the cubic form when the solution has been neutralized with potash.

To return to the cubic and dodecahedral cleavages. If we suppose the excursions of the parts of the molecule to be greater in one direction than in the others, the figure within which the molecule is comprised will be a prolate spheroid; if less, an oblate spheroid. Now, as already explained, the spheroids will be packed as closely as possible if the axes are all paralled and each is touched by twelve others. Now suppose the spheroids arranged as in Fig. 6, with their axes perpendicular to

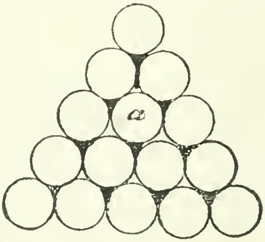

FIG. 6.

the plane of the figure; place the next layer in the black triangular spaces, and complete the pyramid. The three faces of the pyramid will be equal isosceles trangles; and if the spheroids be oblate, and the axis half the greatest diameter, the three angles at the apex of the pyramid will be right angles. The crystal will have cubic symmetry, but the relative condensation in the faces of the cube, octahedron, and dodecahedron, will be as 1:0·5774:0·7071. The easiest cleavage would therefore be cubic, as in rock salt and galena.

Again, if the spheroids have their axes and greatest diameters in the ratio of 1 : $\sqrt{2}$, and we place four, as in Fig. 7, with their axes perpen-

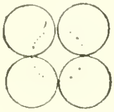

FIG. 7.

dicular to the plane of the figure, then place one upon them in the middle, and then four more upon it, in positions corresponding to those of the first four, we get a cubical arrangement, the center of a spheroid in each angle of a cube, and one in the center of the cube. Crystals so formed will have cubic symmetry, but the concentration of molecules will be greatest in the faces of the dodecahedron, and their easiest cleavage will be, like that of blende, dodecahedral.

If spheroids of any other dimensions be arranged, as in Figs. 1 and 2, with their axes perpendicular to the plane of Fig. 1, we shall get a crystal with the symmetry of the pyramidal system. If the spheroids be prolate, the fundamental octahedron will be elongated in the direction of the axis, and if sufficiently elongated, the greatest condensation will be in planes perpendicular to the axis, and the easiest cleavage, as in prussiate of potash, in those planes. On the other hand, if the spheroids be sufficiently oblate, the easiest cleavage will be parallel to the axis.

If spheroids be arranged, as in Fig. 6, with their axes perpendicular to the plane of the figure, they will, in general, produce rhombohedral symmetry, with the rhombs acute or obtuse, according to the length or shortness of the axes of the spheroids. The cubical form already described is only a particular case of the rhombohedral. If the ratio between the axis of the spheroids and their greatest diameters be only a little greater or a little less than 1 : 2, the condensation will be greatest in the faces of the rhombohedron, and the easiest cleavage will be rhombohedral, as in calcite. If the spheroids be prolate, the easiest cleavage will be perpendicular to the axis of symmetry, as in beryl and many other crystals. Such crystals have a tendency to assume hexagonal forms—equiangular six-sided prisms and pyramids. To explain this, it may be seen in Fig. 6 that, in placing the next layer upon the spheroids represented in the figure, the three spheroids which touch that marked *a* may occupy either the three adjacent white triangles or the three black ones. Either position is equally probable. The layer occupying the white triangles is in the position of a twin to that occupying the black triangles. So far as the central parts of the layer are concerned, it will make no difference in which of these ways the molecules are packed. It is only at the edges that the surface tension will be affected. If the form growing be a rhombohedron, a succession of alternating twins will produce a series of alternating ridges and furrows in the rhombohedral faces, which will give rise to increased surface tension, which will tend to prevent the twinning. On the other hand, a hexagonal form and its twin, formed in the way indicated, are identical, and we have in this fact a cause tending to the production of hexagonal forms. This tendency is increased by the fact that, for a given volume, the total surface of the hexagonal forms is in general less than that of the rhombohedral. Indeed, such forms lend themselves to the formation of almost globular crystals, as is well seen in pyromorphite and mimetite.

If the spheroids be arranged with their axes in other positions than those we have been discussing, or if the molecules occupy ellipsoidal spaces, they will, when packed so that each is touched by twelve others, give figures of less symmetry. The results may be worked out on the lines indicated in the foregoing discussion, and will be found to correspond throughout to the observed facts.

Bravais long ago proposed various arrangements of molecules to account for crystalline forms, and Sohncke has extended them to further degrees of complication in order to account for additional facts in crystallography. But neither of them has given any reason why the molecules should assume such arrangements. To me it seems that only one arrangement can be spontaneously assumed by the molecules, and that the varieties of crystalline form depend on the dimensions of the ellipsoids and the orientation of their axes. Curie also has indicated that the development of combined forms, as those of cube and octahedron, will depend on the surface tensions in the faces of these forms, but he has not indicated how the surface tension is connected with the crystalline arrangement, or why the energy of a cubic face should be greater or less than that of an octahedral face.

We are now in a position to understand the interesting facts brought forward by Prof. Judd in a discourse delivered at the Royal Institution early this year. However long a crystal has been out of the solution or vapor from which it was formed, its surface tension will remain unaltered, and when it is replaced it will grow exactly as if it had not been removed. Also, if any part be broken off it, the tension of the broken surface will, if it be not a cleavage face, be greater than on a face of the crystal, and in growing, the laws of energy necessarily cause it to grow in such a way as to reduce the potential energy—that is, to replace the broken surface by the regular planes of less surface energy. The formation of "negative crystals" by fusing a portion in the interior of a crystalline mass is due to the same principle. Surfaces of least energy will be most easily produced inside as well as outside, and in a crystalline mass of course they will be parallel to the external faces of the crystal. We see the same thing in the action of solvents. Most metals assume a crystalline texture on cooling from fusion, and when slowly acted on by dilute acids the surfaces of greater energy are most easily attacked, in accordance with the laws of energy, and the undissolved metal is left with surfaces of least energy which are the faces of crystals. This is easily seen on treating a piece of tin plate or of galvanized iron with very dilute aqua regia. In fact, solution is closely connected with surface energy. It is probably the low surface energy of one form of crystals of sulphur which makes them insoluble in carbon disulphide, and this low surface energy may be an electrical effect.

I pointed out that the development of all the faces of a form and the similar modification of all corresponding edges and angles of a crystal are in general necessary in order to produce equilibrium under the surface tensions. But we sometimes find crystals with only half the modifications required for symmetry. In such cases the surface tensions must produce a stress in the interior tending to deform the molecules. When the crystal was growing there must have been equilibrium, and therefore a pressure equal and opposite to this effect of the surface tension. There are various ways in which we may suppose that such a force would arise. The electric field might give rise to a stress in opposition to the aggregation of the molecules in the closest possible way, and then the crystal would grow such faces as would produce an equal and opposite stress. Inequalities of temperature or the presence of molecules of other kinds amongst those of the crystal might produce similar results. When the stress due to electricity or to temperature was removed by change of circumstances, that due to the surface tensions would persist, and the crystal would be left with an internal strain. Crystals of this sort, with unsymmetric faces, generally betray the internal strain either by developing electricity of opposite kinds at the two ends when heated or cooled, or they affect polarized light, rotating the plane of polarization. That these effects are due to the internal strain is shown by the fact that tourmalines and other crystals which are pyro-electric when unsymmetrical show no such property when symmetrically grown. Also sodium chlorate in solution, quartz when fused, and so on, lose their rotatory power. Substances which in solution show rotatory power as a rule develop unsymmetric crystals. This is well seen in the tartrates. The constitution of the molecules must be such that they will not without some strain form crystals; and equilibrium when the crystal is growing is attained by means of the opposing stress due to want of symmetry in the surface tensions. In all such crystals the rotatory power of the solution disappears in whole or in part. We can not test this in biaxial crystals, but, according to Des Cloiseaux, sulphate of strychnine is the only substance which shows rotation both in the solution and in the crystalline form, and in it the rotatory power is much increased by the crystallization. Effects comparable with these may be produced by mechanical means. A cube of rock salt, which has no effect on plane-polarized light in its ordinary state, changes the plane of polarization when it is compressed in a vise. And a cleavage slice of prussiate of potash, which is uniaxial, may by compression be distorted so as to give in a convergent beam of polarized light elliptical rings and two eyes like a biaxial crystal.

THE REJUVENESCENCE OF CRYSTALS.*

By Prof. John W. Judd, F. R. S.

Very soon after the invention of the microscope the value of that instrument in investigating the phenomena of crystallization began to be recognized.

The study of crystal morphology and crystallo-genesis was initiated in this country by the observations of Robert Boyle; and since his day a host of investigators—among whom may be especially mentioned Leeuwenhoek and Vogelsang in Holland, Link and Frankenheim in Germany, and Pasteur and Senarmont in France—have added largely to our knowledge of the origin and development of crystalline structures. Nor can it be said with justice that this field of investigation, opened up by English pioneers, has been ignobly abandoned to others, for the credit of British science has been fully maintained by the numerous and brilliant discoveries in this department of knowledge by Brewster and Sorby.

There is no branch of science which is more dependent for its progress on a knowledge of the phenomena of crystallization than geology. In seeking to explain the complicated phenomena exhibited by the crystalline masses composing the earth's crust, the geologist is constantly compelled to appeal to the physicist and chemist; from them alone can he hope to obtain the light of experiment and the leading of analogy whereby he may hope to solve the problems which confront him.

But if geology owes much to the researches of those physicists and chemists who have devoted their studies to the phenomena of crystallization, the debt has been more than repaid through the new light which has been thrown on these questions by the investigations of naturally formed crystals by mineralogists and geologists.

In no class of physical operations is time such an important factor as in crystallization, and Nature, in producing her inimitable examples of crystalline bodies, has been unsparing in her expenditure of time. Hence it is not surprising to find that some of the most wonderful phenomena of crystallization can best be studied—some indeed can only be studied—in those exquisite specimens of Nature's handiwork which

*The Friday evening discourse, delivered at the Royal Institution on January 30, 1891. (From *Nature*, May 28, 1891; vol. XLIV, pp. 83–86.)

have been slowly elaborated by her during periods which must be measured in millions of years.

I propose to-night to direct your attention to a very curious case in which a strikingly complicated group of phenomena is presented in a crystalline mass, and these phenomena, which have been revealed to the student of natural crystals, are of such a kind that we can scarcely hope to re-produce them in our test-tubes and crucibles.

But if we can not expect to imitate all the effects which have in this case been slowly wrought out in Nature's laboratory, we can at least investigate and analyze them, and, in this way, it may be possible to show that phenomena like those in question must result from the possession by crystals of certain definite properties. Each of these properties, we shall see, may be severally illustrated and experimentally investigated, not only in natural products, but in the artificially formed crystals of our laboratories.

In order to lead up to the explanation of the curious phenomena exhibited by the rock-mass in question, the first property of crystals to which I have to refer may be enunciated as follows:

Crystals possess the power of resuming their growth after interruption, and there appears to be no limit to the time after which this resumption of growth may take place.

It is a familiar observation that if a crystal be taken from a solution and put aside it will, if restored after a longer or shorter interval to the same or a similar solution, continue to increase as before. But geology affords innumerable instances in which this renewal of growth in crystals has taken place after millions of years must have elapsed. Still more curious is the fact, of which abundant proof can be given, that a crystal formed by one method may, after a prolonged interval, continue its growth under totally different conditions and by a very different method. Thus, crystals of quartz, which have clearly been formed in a molten magma and certain inclosures of glass, may continue their growth when brought in contact with solutions of silica at ordinary temperatures. In the same way, crystals of feldspar, which have been formed in a mass of incandescent lava, may increase in size when solvent agents bring to them the necessary materials from an enveloping mass of glass, even after the whole mass has become cold and solid.

It is this power of resuming growth after interruption which leads to the formation of zoned crystals, like the fine specimen of amethyst nclosed in colorless quartz, which was presented to the Royal Institution seventy years ago by Mr. Snodgrass.

The growth of crystals, like that of plants and animals, is determined by their environment, the chief conditions affecting their development being temperature, rate of growth, the supply of materials (which may vary in quality as well as in quantity), and the presence of certain foreign bodies.

It is a very curious circumstance that the form assumed by a crystal may be completely altered by the presence of infinitesimal traces of certain foreign substances—foreign substances, be it remarked, which do not enter in any way into the composition of the crystallizing mass. Thus there are certain crystals which can only be formed in the presence of water, fluorides, or other salts. Such foreign bodies, which exercise an influence on a crystallizing substance without entering into its composition, have been called by the French geologists "mineralizers." Their action seems to curiously resemble that of diastase and of the bodies known to chemists as "ferments," so many of which are now proved to be of organic origin.

Studied according to their mode of formation, zoned crystals fall naturally into several different classes.

In the first place, we have the cases in which the successive shells or zones differ only in color or some other accidental character. Sometimes such differently colored shells of the crystal are sharply cut off from one another, while in other instances they graduate imperceptibly one into the other.

A second class of zoned crystals includes those in which we find clear evidence that there have been pauses, or at all events changes in the rate of their growth. The interruption in growth may be indicated in several different ways. One of the commonest of these is the formation of cavities filled with gaseous, liquid, or vitreous material, according to the way the crystal has been formed, by volatilization, by solution, or by fusion, the production of these cavities indicating rapid or irregular growth. Not unfrequently is it clear that the crystal, after growing to a certain size, has been corroded or partially resorbed in the mass in which it is being formed, before its increase was resumed. In other cases, a pause in the growth of the crystal is indicated by the formation of minute foreign crystals or the deposition of uncrystallized material along certain zonal planes in the crystal.

Some very interesting varieties of minerals, like the Cotterite of Ireland, the red quartz of Cumberland, and the spotted amethyst of Lake Superior, can be shown to owe their peculiarities to thin bands of foreign matter zonally included in them during their growth.

A curious class of zoned crystals arises when there is a change in the habit of the crystal during its growth. Thus, as Levalle showed in 1851 (*Bull. Géol. Soc. Paris*, 2^{me}. sér., vol. VIII, pp. 610–13), if an octahedron of alum be allowed to grow to a certain size in a solution of that substance, and then a quantity of alkaline carbonate be added to the liquid, the octahedral crystal, without change in the length of its axes, will be gradually transformed into a cube. In the same way, a scalenohedron of calcite may be found inclosed in a prismatic crystal of the same mineral, the lengths of the vertical axes being the same in both crystals.

By far the most numerous and important class of zoned crystals is that which includes the forms where the successive zones are of different, though analogous, chemical composition. In the case of the alums and garnets, we may have various isomorphous compounds forming the successive zones in the same crystal; while, in substances crystallizing in other systems than the cubic, we find plesiomorphous compounds forming the different inclosing shells.

Such cases are illustrated by many artificial crystals and by the tourmalines, the epidotes, and the feldspars among minerals. The zones, consisting of different materials, are sometimes separated by well marked planes, but in other cases they shade imperceptibly into one another.

In connection with this subject it may be well to point out that zoned crystals may be formed of two substances which do not crystallize in the same system. Thus, crystals of the monoclinic augite may be found surrounded by a zone of the rhombic enstatite and crystals of a triclinic felspar may be found enlarged by a monoclinic feldspar.

Still more curious is the fact that, where there is a similarity in crystalline form and an approximation in the dominant angles (plesiomorphism), we may have zoning and intergrowth in the crystals of substances which possess no chemical analogy whatever. Thus, as Senarmont showed in 1856, a cleavage-rhomb of the natural calcic carbonate (calcite), when placed in a solution of the sodic nitrate, becomes enveloped in a zone of this latter substance, and Tschermak has proved that the compound crystal thus formed behaves like a homogeneous one, if tested by its cleavage, by its susceptibility to twin lamellation, or by the figures produced by etching. In the same way, zircons, which are composed of the two oxides of silicon and zirconium, are found grown in composite crystals with xenotime, a phosphate of the metals of the cerium and yttrium groups.

These facts, and many similar ones which might be adduced, point to the conclusion that the beautiful theory of isomorphism, as originally propounded by Mitscherlich, stands in need of much revision as to many important details, if not indeed of complete reconstruction, in the light of modern observation and experiment.

The second property of crystals to which I must direct your attention is the following:

If a crystal be broken or mutilated in any way whatever, it possesses the power of repairing its injuries during subsequent growth.

As long ago as 1836, Frankenheim showed that, if a drop of a saturated solution be allowed to evaporate on the stage of a microscope, the following interesting observations may be made upon the growing crystals. When they are broken up by a rod, each fragment tends to re-form as a perfect crystal; and if the crystals be caused to be partially re-dissolved by the addition of a minute drop of the mother liquor, further exaporation causes them to resume their original development (*Pogg. Ann.*, 1836, Bd. XXXVII).

In 1842, Hermann Jordan showed that crystals taken from a solution and mutilated gradually became repaired or healed when replaced in the solution (*Müller Archiv. für* 1842, pp. 46–56). Jordan's observations, which were published in a medical journal, do not however seem to have attracted much attention from the physicists and chemists of the day.

Lavalle, between the years 1850 and 1853,* and Kopp, in the year 1855, made a number of valuable observations bearing on this interesting property of crystals (*Liebig Ann.*, 1855, XCIV., pp. 118–25). In 1856 the subject was more thoroughly studied by three investigators who published their results almost simultaneously; these were Marbach (*Compt. rend.*, 1856, XLIII, pp. 705–706, 800–802), Pasteur (*ibid.*, pp. 795–800), and Senarmont (*ibid.*, p. 799). They showed that crystals, taken from a solution and mutilated in various ways, upon being restored to the liquid became completely repaired during subsequent growth.

As long ago as 1851, Lavalle had asserted that, when one solid angle of an octahedron of alum is removed, the crystal tends to reproduce the same mutilation on the opposite angle when its growth is resumed! This remarkable and anomalous result has however by some subsequent writers been explained in another way to that suggested by the author of the experiment.

In the same way the curious experiments performed at a subsequent date by Karl von Hauer, experiments which led him to conclude that hemihedrism and other peculiarities in crystal growth might be induced by mutilation,† have been asserted by other physicists and chemists not to justify the startling conclusions drawn from them at the time. It must be admitted that new experiments bearing on this interesting question are at the present time greatly needed.

In 1881, Loir demonstrated two very important facts with regard to growing crystals of alum (*Compt. rend.*, Bd. XCII, p. 1166). First, that if the injuries in such a crystal be not too deep, it does not resume growth over its general surface until those injuries have been repaired. Secondly, that the injured surfaces of crystals grow more rapidly than natural faces. This was proved by placing artificially cut octahedra and natural crystals of the same size in a solution and comparing their weight after a certain time had elapsed.

The important results of this capacity of crystals for undergoing healing and enlargement and their application to the explanation of interesting geological phenomena have been pointed out by many au-

**Bull. Géol. Soc. Paris*, 2me sér., vol. VIII, pp. 610–13. 1851; Moigno, *Cosmos*, II, 1853, pp. 151–56; *Compt. rend.*, XXXVI., 1853, pp. 193–95.

†*Wien, Sitz. Ber.*, XXXIX., 1860, pp. 611–22; Erdmann, *Journ. prakt. Chem.*, LXXXI. pp. 356–62; *Wien, Geol. Verhandl.*, XII. pp. 212–13, etc.; Frankenheim, *Pogg. Ann.*, CXIII, 1861. Compare Fr. Scharff, *Pogg. Ann.*, CIX, 1860, pp. 529–38; *Neues Jahrb. für Min.*, etc., 1876, p. 24; and W. Sauber. *Liebig Ann.*, CXXIV., 1862, pp. 78–82; also W. Ostwald, "Lehrbuch d. Allg. Chem.," 1885, Bd. I, p. 738, and O. Lehmann, "Molekular Physik," 1888, Bd. I, p. 312.

thors. Sorby has shown that, in the so-called crystalline sand grains, we have broken and worn crystals of quartz, which, after many vicissitudes and the lapse of millions of years, have grown again and been enveloped in a newly formed quartz crystal. Bonney has shown how the same phenomena are exhibited in the case of mica, Becke and Whitman Cross in the case of hornblende, and Merrill in the case of augite. In the feldspars of certain rocks it has been proved that crystals that have been rounded, cracked, corroded, and internally altered—which have, in short, suffered both mechanical and chemical injuries—may be repaired and enlarged with material that differs considerably in chemical composition from the original crystal.

It is impossible to avoid a comparison between these phenomena of the inorganic world and those so familiar to the biologist. It is only in the lowest forms of animal life that we find an unlimited power of repairing injuries: in the Rhizopods and some other groups a small fragment may grow into a perfect organism. In plants the same phenomenon is exhibited much more commonly, and in forms belonging to groups high up in the vegetable series. Thus, parts of a plant, such as buds, bulbs, slips, and grafts, may—sometimes after a long interval—be made to grow up into new and perfect individuals. But in the mineral kingdom we find the same principle carried to a much further extent. We know in fact no limit to the minuteness of fragments which may, under favourable conditions, grow into perfect crystals, no bounds as to the time during which the crystalline growth may be suspended in the case of any particular individual.

The next property of crystals which I must illustrate, in order to explain the particular case to which I am calling your attention to night, is the following:

Two crystals of totally different substances may be developed within the space bounded by certain planes, becoming almost inextricably inter-grown, though each retains its distinct individuality.

This property is a consequence of the fact that the substance of a crystal is not necessarily continuous within the space inclosed by its bounding planes. Crystals often exhibit cavities filled with air and other foreign substances. In the calcite crystals found in the Fontainebleau sandstone, less than 40 per cent of their mass consists of calcic carbonate, while more than 60 per cent is made up of grains of quartz sand, caught up during crystallization.

In the rock called "graphic granite," we have the minerals orthoclase and quartz intergrown in such a way that the more or less isolated parts of each can be shown, by their optical characters, to be parts of great mutually interpenetrant crystals. Similar relations are shown in the so-called micrographic or micro-pegmatic intergrowths of the same minerals which are so beautifully exhibited in the rock under our consideration this evening.

There is still another property of crystals that must be kept in mind if we would explain the phenomena exhibited by this interesting rock:

A crystal may undergo the most profound internal changes, and these may lead to great modifications of the optical and other physical properties of the mineral; yet, so long as a small—often a very small—proportion of its molecules remain intact, the crystal may retain, not only its outward form, but its capacity for growing and repairing injuries.

Crystals, like ourselves, grow old. Not only do they suffer from external injuries, mechanical fractures, and chemical corrosion, but from actions which affect the whole of their internal structure. Under the influence of the great pressures in the earth's crust, the minerals of deep-seated rocks are completely permeated by fluids which chemically react upon them. In this way, negative crystals are formed in their substance (similar to the beautiful "ice-flowers" which are formed when a block of ice is traversed by a beam from the sun or an electric lamp), and these become filled with secondary products. As the result of this action, minerals, once perfectly clear and translucent, have acquired cloudy, opalescent, iridescent, avanturine, and "schiller" characters; and minerals, thus modified, abounded in the rocks that have at any period of their history been deep-seated. As the destruction of their internal structure goes on, the crystals gradually lose more and more of their distinctive optical and their physical properties, retaining however their external form, till at last, when the last of the original molecules is transformed or replaced by others, they pass into those mineral corpses known to us as "pseudomorphs."

But while crystals resemble ourselves in "growing old," and, at last, undergoing dissolution, they exhibit the remarkable power of growing young again, which we, alas! never do. This is in consequence of the following remarkable attribute of crystalline structures.

It does not matter how far internal change and disintegration may have gone on in a crystal; if only a certain small proportion of the unaltered molecules remain, the crystal may renew its youth and resume its growth.

When old and much altered crystals begin to grow again, the newly formed material exhibits none of those marks of "senility" to which I have referred. The sand grains that have been battered and worn into microscopic pebbles and have been rendered cloudy by the development of millions of secondary fluid cavities may have clear and fresh quartz deposited upon them to form crystals with exquisitely perfect faces and angles. The white, clouded, and altered feldspar crystals may be enveloped by a zone of clear and transparent material, which has been added millions of years after the first formation and the subsequent alteration of the original crystal.

We are now in position to explain the particular case which I have thought of sufficient interest to claim your attention to-night.

In the Island of Mull, in the Inner Hebrides, there exist masses of granite of Tertiary age, which are of very great interest to the geologist and mineralogist. In many places this granite exhibits beautiful illustrations of the curious inter-growths of quartz and feldspar, of which I have already spoken. Such parts of the rock often abound with cavities (druses), which I believe are not of original, but of secondary origin. At all events, it can be shown that these cavities have been localities in which crystal growth has gone on; they constitute indeed veritable laboratories of synthetic mineralogy.

Now, in such cavities the inter-penetrant crystals of quartz and feldspar in this rock have found a space where they may grow and complete their outward form; and it is curious to see how sometimes the quartz has prevailed over the feldspar and a pure quartz crystal has been produced, while at other times the opposite effect has resulted and a pure feldspar individual has grown up. In these last cases, however much the original feldspar may have been altered (kaolinized and rendered opaque), it is found to be completed by a zone of absolutely clear and unaltered feldspar substance. The result is that the cavities of the granite are lined with a series of projecting crystals of fresh quartz and clear feldspar, the relations of which to the older materials in an altered condition, composing the substance of the solid rock, are worthy of the most careful observation and reflection.

Those relations can be fully made out when thin sections of the rock are examined under the microscope by the aid of polarized light, and they speak eloquently of the possession by the crystals of all those curious peculiarities of which I have reminded you this evening.

By problems such as those which we have endeavoured to solve to-night, the geologist is beset at every step. The crust of our globe is built up of crystals and crystal fragments—of crystals in every stage of development, of growth, and of variation—of crystals undergoing change, decay, and dissolution. Hence the study of the natural history of crystals must always constitute one of the main foundations of geological science, and the future progress of that science must depend on how far the experiments carried on in laboratories can be made to illustrate and explain our observations in the field.

DEDUCTION FROM THE GASEOUS THEORY OF SOLUTION.*

BY PROF. ORME MASSON.

Before passing on, let me briefly recapitulate the chief points in Van't Hoff's gaseous theory of solution and the experimental laws on which it is based.

(1) In every simple solution the dissolved substance may be regarded as distributed throughout the whole bulk of the solution. Its total volume is therefore that of the solution, the solvent playing the part of so much space; and its specific volume is the volume of that quantity of the solution which contains 1 gram of the substance. To avoid confusion, it is best to speak of this as the specific solution volume (v) of the substance. It is obviously in inverse ratio to the concentration.

(2) In every simple solution the dissolved substance exerts a definite osmotic pressure (p). This is normally independent of the nature of the solvent. It varies inversely as the specific solution volume (or directly as the concentration), and directly as the absolute temperature (T). We may then write for solutions, as we do for gases, the equation $p. v = r. T$, where p and v have their specialized meanings, and r is a constant for each soluble substance.

(3) The molecular solution volume of all dissolved substances is the same if they are compared at the same temperature and osmotic pressure. If m be the molecular weight, $m . v = V$ is the molecular solution volume; and we can now write, as we do for gases, $p V. = R . T$, where R is the same constant for all substances.

(4) This constant R has the same value when the formula is applied to the dissolved state as when it is applied to the gaseous state itself.

(5) The gaseous laws, as I have stated them, are not absolutely true for dissolved matter in all circumstances. Dissociation often occurs, as it may occur in the process of vaporization, thus causing apparent exceptions, but apart from this there are and must be variations from

* Part of an address delivered by the President of Section B of the Australian Asociation for the Advancement of Science, January, 1891. (From *Nature*, Feb. 12, 1891; vol. XLIII., pp. 315-319.)

the laws in the case of solutions of great concentration, just as there are in the case of gases and vapors of great concentration—for instance, in the neighborhood of the critical point.

I wish now to ask your attention more particularly to the actual process of dissolving, and then to lay before you a hypothesis, which, as it seems to me, is a logical consequence of the general theory.

Imagine, then, a soluble solid in contact with water at a fixed temperature. The substance exercises a certain pressure, in right of which it proceeds to dissolve. This pressure is analogous to the vapor pressure of a volatile body in space, the space being represented by the solvent; and the process of solution is analogous to that of vaporization. As the concentration increases, the osmotic pressure of the dissolved portion increases, and tends to become equal to that of the undissolved portion; just as, during vaporization in a closed space, the pressure of the accumulating vapor tends to become equal to the vapor pressure of the liquid. But if there be enough water present, the whole of the solid will go into solution, just as the whole of a volatile body will volatilize if the available space be sufficient. Such a solution may be exactly saturated or unsaturated. With excess of the solvent it will be unsaturated, and the dissolved matter will then be in a state comparable to that of an unsaturated vapor, for its osmotic pressure will be less than the possible maximum corresponding to the temperature. On the other hand, if there be not excess of water present during the process of solution, a condition of equilibrium will be arrived at when the osmotic pressure of the dissolved portion becomes equal to the pressure of the undissolved portion, just as equilibrium will be established between the volatile substance and its vapor if the space be insufficient for complete volatilization. In such a case we get a saturated solution in presence of undissolved solid, just as we may have a saturated vapor in presence of its own liquid or solid.

So far we have supposed the temperature to be stationary, but it may be raised. Now, a rise of temperature will disturb equilibrium in either case alike, for osmotic pressure and vapor pressure are both increased by this means, and a re-establishment of equilibrium necessitates increased solution or vaporization, as the case may be.

Now, what will this constantly increasing solubility with rise of temperature eventually lead to? Will it lead to a maximum of solubility at some definite temperature beyond which increase becomes impossible? Or will it go on in the way it has begun, so that there will always be a definite, though it may be a very great, solubility for every definite temperature? Or will it lead to infinite solubility before infinite temperature is obtained? One or other of these things must happen, provided of course that chemical change does not intervene.

Well, let us be guided by the analogy that has hitherto held good. Let us see what this leads us to, and afterwards examine the available experimental evidence. We know that a volatile liquid will at last reach a temperature at which it becomes infinitely volatile—a temperature above which the liquid can not possibly exist in the presence of its own vapor, no matter how great the pressure may be. At this temperature, equilibrium of pressure between the liquid and its vapor becomes impossible, and above this point the substance can exist only as a gas. This is the critical temperature. And so it seems to me that if we carry our analogy to its logical conclusion, we may expect for every substance and its solvent a definite temperature above which equilibrium of osmotic pressure between undissolved and dissolved substance is impossible—a temperature above which the substance can not exist in presence of its own solution, or in other words a temperature of infinite solubility. This may be spoken of as the critical solution temperature.

But a little consideration shows that in one particular we have been somewhat inexact in the pursuance of our analogy, for we have compared the solution of a solid body to the vaporization of a volatile liquid. We can however do better than this, for volatile solid bodies are not wanting. It is to these, then, that we must look in the first instance. Now, a volatile solid (such as camphor or iodine) will not reach its critical point without having first melted at some lower temperature, and a similar change should be exhibited in the solution process. At some definite temperature, below that of infinite solubility, we may expect the solid to melt. This solution melting point will not be identical with, but lower than, the true melting point of the solid, and for the following reason: No case is known, and probably no case exists, of two liquids one of which dissolves in the other and yet can not dissolve any of it in return. Therefore there will be formed by melting, not the pure liquid substance, but a solution of the solvent in the liquid substance. Hence the actual melting or freezing point must be lower than the true one, in right of the laws of which I have spoken when discussing Raoult's methods in the earlier part of this address.

From this solution melting-point upwards we shall then have to deal with two liquid layers, each containing both substance *A* and solvent *B*, but the one being mostly substance *A* and the other mostly solvent *B*. These may be spoken of as the *A* layer and the *B* layer. As temperature rises, the proportion of *A* will decrease in the *A* layer and increase in the *B* layer; and every gram of *A* will occupy an increasing solution volume in the *A* layer (*B* being absorbed there) and a decreasing solution volume in the *B* layer. At each temperature the osmotic pressures of *A* in the two layers must be equal. The whole course of affairs, as thus conceived, now admits of the closest comparison with the changes which accompany gradual rise of temperature in the case of a volatile liquid and its saturated vapor. The liquid is like the substance *A* in the *A* layer: the vapor (which is the same matter in another state) is like the same substance *A* in the *B* layer. As temperature rises the liquid diminishes in total quantity, the vapor increasing; but the specific volume of the liquid increases, while that of the vapor decreases. The residual liquid is, in fact, constantly encroaching on the space of its vapor, just as the residual substance *A* in the *A* layer is constantly absorbing the solvent *B* from the *B* layer. Finally, in either case, the specific volume of the substance will become identical in both layers, which means that the layers themselves will become homogeneous and indistinguishable. Our system will then have reached its critical temperature—the temperature of infinite volatility in the one case and of infinite solubility in the other.

So much for hypothesis. Are there any facts in support of it? Well, in the first place the hypothesis demands that (in the absence of chemical change) increase of solubility with rise of temperature shall be as general a law as increase of vapor pressure, and we find that this agrees with the known facts, more especially since Tilden and Shenstone (*Phil. Trans.*, 1884) cleared up certain doubtful cases. Secondly, the hypothesis seems to demand some connection between the true melting points of salts and the rates of their increase of solubility; and such a relation has in a general way been established by the same observers. Thirdly, we have the fact, in complete accordance with the hypothesis, that while no case is known of a solid body having, as such, infinite solubility in any simple solvent, several cases are known of liquids of infinite solubility, and also of solids which, after they have melted in presence of their own solution, become at some higher temperature infinitely soluble. This last statement refers to the cases described by Alexéeff (*Wiedemann's Annalen*, 1886), of which I must say a good deal more directly. It would seem to apply also to the case of silver nitrate, which Tilden and Shenstone described as dissolving in water to the extent of 18.25 parts to one at so low a temperature as 130° C. The true melting-point of the salt is 217°, and I have seen it stated (but have been unable to find the published account) that Shenstone has himself shown it to be fusible in water, and of infinite solubility at quite reachable temperatures.

With regard to substances that are liquid under ordinary conditions, we have the well-known fact that some pairs are infinitely soluble in one another, while others exhibit the phenomenon of only partial solubility. The hypothesis would draw no hard and fast distinction between these cases, except the practically important one that such a mixture as that of ether and alcohol, which belongs to the first class, is usually above its critical solution point, while such a one as ether and water, which belongs to the second class, is usually below it. It should be possible, according to the hypothesis, to cool mixtures of ether and alcohol sufficiently to cause separation into two layers, similar to those observed at the ordinary temperature in the case of ether and water; but I do not know that this has yet been put to the test of experiment.

Alexeeff's experiments appear to me to be of the very highest importance, and to merit the closest attention in any inquiry into the nature of solution. As already stated, they afford the strongest support to the hypothesis which I have been discussing; indeed, had it not been for this support, I should hardly have ventured to discuss it at all. They refer to solutions in water, below and above 100°, of phenol, salicylic acid, benzoic acid, aniline phenylate, and aniline, and to solutions in molten sulphur of chlorobenzene, benzene, toluene, aniline, and mustard oil. All these afford instances of reciprocal partial solution throughout a considerable range of temperature, leading eventually at a definite temperature to infinite solubility. Several of them afford instances also of solid substances with solution melting-points below their true melting-points.

Alexéeff experimentally determined the temperatures at which different mixtures of the same two liquids are just converted into clear solutions; or, in other words, he ascertained the strengths of the saturated solutions corresponding to different temperatures. For each pair of liquids he found that when a particular strength of mixture is reached, the temperature of saturation is lowered by further addition of either liquid. Thus a mixture of about 37 parts aniline to 63 parts water requires a temperature of 164°·5 to convert it into a homogeneous solution; but one of 21 of aniline to 79 of water assumes this condition at 156°, and one of 74 of aniline to 56 of water does so at 157°·5. He plotted his results in the form of curves, with temperature and percentage strength as the two coördinates. The curve for aniline and water is shown in Fig. 1, and this may be taken as a fair representative, the general form of all being similar. It is at once apparent that for every temperature up to a certain limit there are two possible saturated solutions, one of water in aniline and one of aniline in water. The limiting temperature at which there is but one possible saturated solution, and above which saturation becomes impossible, is called by Alexéeff the Mischungs Temperatur. It is what I have called the critical solution temperature. It is in the case of aniline and water about 167°, as nearly as one can judge from the curve without a greater number of experimental points than we have in this part; and the corresponding saturation strength is about 50 per cent. It is hardly necessary to say that this equality

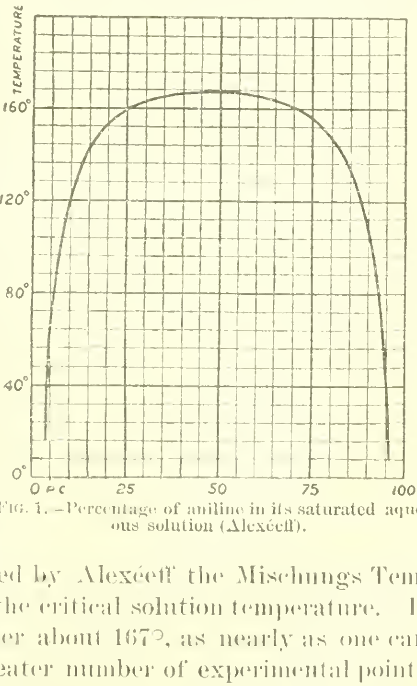

FIG. 1.—Percentage of aniline in its saturated aqueous solution (Alexéeff).

of the two ingredients is an accident which does not characterize all cases.

Now imagine a 50 per cent mixture of aniline and water sealed up in a tube, shaken, and gradually heated. Let us assume that the tube is only large enough to contain the mixture and allow of expansion by heat, so that evaporation may be neglected as too small to materially complicate the result. The course of events will be exactly what I have already described with reference to the hypothetical A layer and B layer. There will be formed a saturated solution of water in aniline, which we may call the *aniline layer*, and a saturated solution of aniline in water—the *water layer*. Given the temperature, the percentage strength of each layer may be read off from the curve. As the temperature rises, the two layers will effect exchanges in such a way that the aniline layer will become poorer and the water layer richer in aniline, and at about 167° the two layers will have attained equal strength and become merged into one. Were we to start with the aniline and water in any other proportions by weight, there would still be formed the two saturated solutions, but their relative amounts would be different, and one or other would be used up and disappear at a lower temperature than 167°. To attain the maximum temperature of complete solution, you must start with the exact proportions which correspond to that temperature.

But it is possible to learn even more from Alexéeff's work than he himself has made evident. Let me call your attention to the curve shown in Fig. 2*, the data for which I have calculated in the following manner.

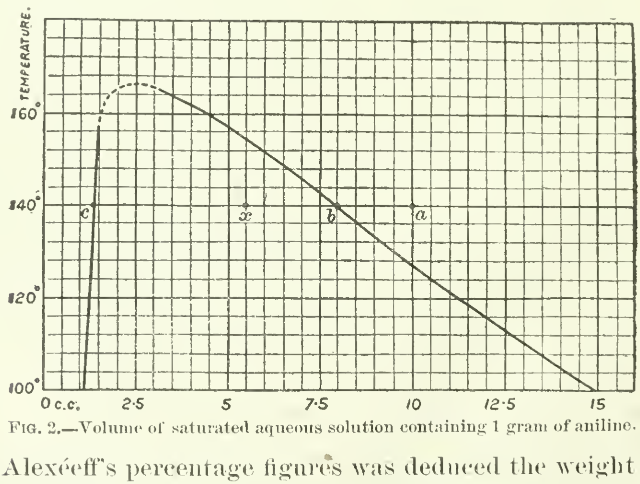

FIG. 2.—Volume of saturated aqueous solution containing 1 gram of aniline.

From Alexéeff's percentage figures was deduced the weight of water capable of dissolving, or being dissolved by 1 gram of aniline at

* In order to save space, only the upper portion of the curve is here represented, as it shows all that is essential to the argument. Of the twelve experimental points one appears to be somewhat misplaced; but this does not affect that part of the curve shown in the figure.

each of his experimental temperatures, so as to form a saturated solution. Then from curves showing the expansion of pure water and pure aniline (the latter drawn from Thorpe's data, *Trans. Chem. Soc.*, 1880) there were read the specific volumes of these substances at each of Alexéeff's temperatures; and from the combined information thus obtained, there was calculated the total volume of that quantity of the saturated solution at each temperature which contains 1 gram of aniline. This is what I have already called the specific solution volume. A slight error is involved by the fact that the volume of a solution is not exactly the sum of the volumes of its ingredients; but this error is necessarily small—too small to affect the general character of the curve or the nature of the lesson to be learned from it.

The specific solution volumes of the aniline, calculated in this manner, were found to be as follows:

Temperature.	Specific solution volumes of aniline.	
	In aniline layer.	In water layer.
8	1.015	
16		32.16
25	1.036	
39	1.053	
55		28.27
68	1.087	
77		19.55
137	1.297	
142		7.696
156		5.248
157.5	1.498	
164.5		3.412

These specific solution volumes are represented as abscissæ in Fig. 2, with the temperatures as ordinates. For the sake of comparison, I

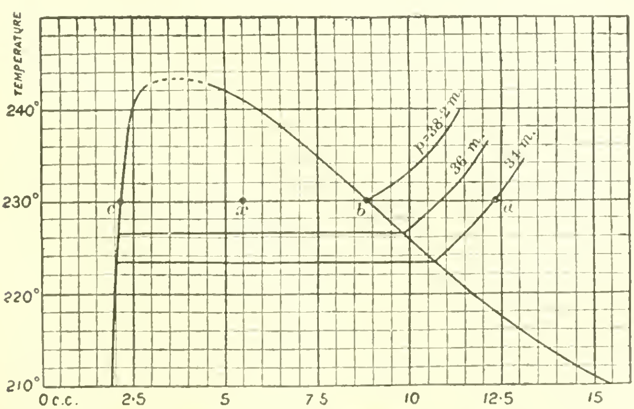

FIG. 3.—Volume of alcohol (liquid and saturated vapor) weighing 1 gram.

have placed side by side with it a specific volume and temperature curve (Fig. 3) for pure alcohol and its saturated vapor, plotted from the experimental data of Ramsay and Young (*Phil. Trans.*, 1886). The reason that alcohol was chosen is simply that the data were convenient to my hand.

The two curves are strikingly similar in form and significance. In Fig. 3 we see the specific volume of liquid alcohol increasing slowly with rise of temperature, while that of the saturated vapor rather rapidly decreases. In Fig. 2 we see the specific solution volume of the aniline in the aniline layer slowly increasing, while that of the aniline in the water layer decreases more rapidly, with rise of temperature. In Fig. 3 we see that above the critical point the existence of liquid alcohol in presence of its vapor is impossible. In Fig. 2 we see that above the critical solution point the existence of an analine layer in presence of a water layer is impossible. In Fig. 3 we see an inclosed area which represents those temperatures and specific volumes which are mutually incompatible. In Fig. 2 we see an inclosed area which represents those temperatures and specific solution volumes which are mutually incompatible. In Fig. 3 we see that any two points on the curve which correspond to equal temperature must also, from the nature of the case, correspond to equal osmotic pressure. In Fig. 3 some of the pressures are indicated, as this can be done from Ramsay and Young's data. In Fig. 2 the value of the osmotic pressures can not be given, as they have not been experimentally determined. In Fig. 3 any point outside of the curve and to the right, as at *a*, corresponds to the state of unsaturated alcohol vapor, whose temperature, specific volume, and pressure are indicated—the last by the isobaric line which passes through the point. In Fig. 2 any point outside the curve and to the right, as at *a*, must correspond to the state of an unsaturated aqueous solution of aniline, whose temperature and specific solution volume can be read, and whose osmotic pressure could be indicated by an isobaric line, had we the data for plotting it. A little thought makes it evident, too, that such isobaric lines would follow the same general course as those shown in the alcohol diagram.

Now, consider what must be the effect of gradually decreasing the volume of the unsaturated vapor in the one case and the solution volume of the aniline in the unsaturated solution in the other, while temperature is kept constant. In the case of the vapor (Fig. 3) the point *a* will pass to the left across lines of increasing pressure until the vapor becomes saturated at *b*. Then, if the diminution of volume continue, a portion of the vapor will condense to the liquid state, or be transferred to *c*, while the rest remains saturated vapor at *b*. With continued decrease of volume, the proportion condensed will constantly increase, but there can be no alteration of pressure till all is condensed; and after that nothing but a very slight diminution of volume is possible without a lowering of temperature. Well, how are we to diminish the solution volume of the aniline in the unsaturated aqueous solution? Clearly by depriving the solution of some of its water, so as to leave the same quantity of aniline distributed throughout a smaller space. And what will be the result of doing this while temperature is kept constant? Evidently, as in the other case, the point a (Fig. 2) will travel to the left, across lines of increasing osmotic pressure, until it reaches b—that is, until the solution is a saturated one; and after that, if more water be abstracted, some of the aniline will be thrown out or condensed, not as pure aniline but as a saturated solution of water in aniline, so that two layers will now coexist—the aniline in one having the specific solution volume represented at b, and the aniline in the other having that represented at c. This transference from b to c will continue, as water is abstracted, until the ratio of residual water to aniline is just enough to give the whole of the latter the specific solution volume shown at c. At this stage the water layer will disappear, and only a saturated solution of water in aniline will be left; and after that only a very small volume change can possibly result from further abstraction of water, as the specific solution volume is already not far from the specific volume of pure aniline itself at the same temperature.

To complete the comparison of the two curves, let me point out that, just as we can from Fig. 3 calculate the distribution of alcohol between its liquid and its vapor layers under given conditions, so can we calculate from Fig. 2 the distribution of the aniline between the aniline layer and the water layer under given conditions. In the former case, if the total volume of a tube containing n grams of alcohol, at, say, 230°, be $n \times x$, and if x be marked off (Fig. 3) between b and c on the line of that temperature, then (x, b, and c standing for the volumes which can be read off on the horizontal base line) $n \cdot \frac{x - c}{b - c}$ is the weight of the alcohol in the vapor layer, and $n \cdot \frac{b - x}{b - c}$ is its weight in the liquid layer, and the volumes of the two layers in cubic centimeters are $n \cdot b \cdot \frac{x - c}{b - c}$ and $n \cdot c \cdot \frac{b - x}{b - c}$ respectively, which are together equal to $n \cdot x$. Just also with the aniline and water mixture (Fig. 2). If $n \times x$ be the total volume of the mixture (both layers together) containing n grams of aniline, at, say, 140°, and if x be marked off as it was in the other case, then $n \cdot \frac{x - c}{b - c}$ is the weight of aniline in the water layer, and $n \cdot \frac{b - x}{b - c}$ is its weight in the aniline layer, and the total volumes of the two layers are $n \cdot b \cdot \frac{x - c}{b - c}$ and $n \cdot c \cdot \frac{b - x}{b - c}$ respectively, together equal $n \cdot x$.

If the actual weights of aniline and water in the mixture be given, the value of x can be calculated with a very fair approach to accuracy by the method adopted in plotting the curve; and thus all the facts with regard to the distribution at any temperature can be obtained.

Now, if it be remembered that this case of aniline and water is not an isolated one, but typical of many cases experimented on by Alexéeff, and if it be remembered also that there exists no direct experimental evidence to show that the law which governs these cases is not the general law regulating all simple solutions it must I think be granted that the facts do somewhat strongly support the hypothesis of a critical solution point which I deduced in the first instance from the general theory of solution. It may be summed up as follows:

(1) In every system of solution which starts with a solid and its simple solvent, the solid has a solution melting point which is lower than its true melting point. Above this temperature the system consists of two separate liquids, each of which is a saturated solution.

(2) These two liquids become one homogeneous solution at a temperature which depends on the ratio of the original ingredients. There is one ratio which demands a higher temperature than any other. This is the critical solution temperature, above which either ingredient is infinitely soluble in the other.

SOME SUGGESTIONS REGARDING SOLUTIONS.*

By Prof. William Ramsay, F. R. S.

The brilliant presidential address of Prof. Orme Masson at the Chemical Section of the Australasian Association for the Advancement of Science marks a distinct advance in our ideas of solution. The analogy between the behavior of a liquid and its vapor in presence of each other and of a pair of solvents capable of mutual solution is so striking as to carry conviction. The resemblance of the liquid-vapor curve, with its apex at the critical point, to the solubility curve, with its apex at the critical solution point, appears to me to prove beyond cavil that the two phenomena are essentially of the same nature.

There are two other phenomena, which, it appears to me, are made clear by the ideas of Prof. Masson. The first of these has reference to super-saturated solutions. The curves (published in *Nature*, February 12, p. 348) showing the analogy between liquid-gas and solution curves, are isobaric curves, or, more correctly, they represent the terminations of isobaric curves in the region of mixtures, where, on the one hand, a liquid exists in presence of its vapor, and on the other, one solvent in the presence of another (for both solvents play the part of dissolved substances, as well as of solvents). M. Alexéeff's data are not sufficient to permit of the construction of a curve representing a similar region mapped out by the termination of isothermal lines. But it is obvious that it would be possible to determine osmotic pressures of various mixtures by the freezing-point method, and so to construct isothermal curves for such mixtures of solvents. And there can be no reasonable doubt that, as the isobaric curves of liquid-gas and of solvent-solvent display so close an analogy, the isothermal curves would also closely resemble each other.

Granting then that this is the case, we may construct an imaginary isothermal curve on the model of the curve for alcohol published in the *Phil. Trans.* by Dr. Sydney Young and myself. Now, in one series of papers on the liquid-gas relations, we showed that with constant volume pressure is a linear function of temperature; and we were thus able to calculate approximately the pressures and volumes for any isothermal

*Read before the Royal Society on Thursday, March 5, 1891. (From *Nature*, April 23, 1891; vol. XLIII, pp. 589, 590.)

representing the continuous transition from the gaseous to the liquid state (see *Phil. Mag.*, 1887, vol. XXIII, p. 435). It would be interesting to ascertain whether, if concentration be kept constant, osmotic pressure would also show itself to be a linear function of temperature. But this apart, it appears in the highest degree probable that there should also exist, in theory at least, a continuous transition from solvent to solvent, the representation of which would be a continuous curve. In such a case, on increasing the concentration of the solution by eliminating one solvent, the other solvent should not separate visibly, but the two should remain mixed until one solvent has been entirely removed. The accompanying diagram (Fig. 1) will make this clear. The

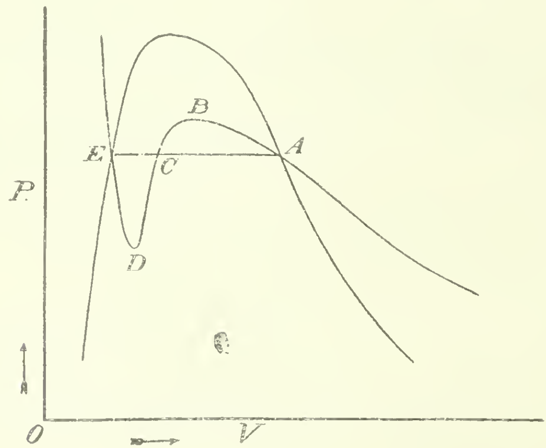

Fig. 1.

sinuous curve *A B C D E* may represent either continuous change from gas to liquid along an isothermal on decrease of volume, or it may represent a similar continuous change from saturated solution to dissolved substance on increase of concentration.

Mr. Aitken's experiments on the cooling of air containing water-vapor have shown us that it is possible to realize a portion of the curve *A B*; the phenomenon of "boiling with bumping" constitutes a practical realization of a portion of the curve *D E;* and we may profitably inquire what conditions determine such unstable states with solvent and solvent.

Regarding the portion of the curve *A B*, I think that no reasonable doubt can be entertained. It precisely corresponds to the condition of super-saturation. In the liquid-gas curve the volume is decreased at constant temperature without separation of liquid: in the solvent-solvent curve the concentration is increased without separation of the solvents. Dr. Nicol has shown that it is possible to dissolve dry sodium sulphate in a saturated solution of sodium sulphate to a very considerable extent without inducing crystallization; and here we have a realization of the unstable portion of the curve $A\ B$. In the gas-liquid curve pressure falls with formation of a shower of drops; in the solvent-solvent curve crystallization ensues and the solvents separate. The phenomena are however not completely analogous; the complete analogy would be if the temperature were so low that the substance in the liquid-gas couple were to separate in the solid, not in the liquid, state. This, so far as I am aware, has not been experimentally realized, but one sees no reason why it should not be possible.

I have some hesitation in offering speculations as to the state of matter at the portion of the continuous curve $D\ E$. It may be that it corresponds to a syrupy or viscous state. Cane sugar at the moderate temperature dissolves water; indeed it is possible to obtain a solution of 1 per cent of water in molten cane sugar. And such a solution, if quickly cooled, remains a syrup. But it can be induced to crystallize by the presence of crystals. Thus, in such a mixture of sugar and water a few grains of crystalline sugar cause the whole mass to crystallize, and water saturated with sugar, and sugar, separate into two layers. Here again a complete analogy fails us, for it is a solid which separates. As we know nothing of the osmotic pressure of a syrup, the analogy is a defective one; but it is probable that a dilute solution of sugar would pass continuously into a syrup of pure sugar by evaporation of the solvent, and analogy would lead to the supposition that the syrup coincides with the unstable state of the liquid. I would therefore offer the analogy between the syruppy and the super-cooled states as a tentative one; it lacks foundation in both cases.

One point remains to be mentioned. I have for the past nine months, in conjunction with Mr. Edgar Perman, been determining the adiabatic relations for liquid and gaseous ether: the rise of pressure and temperature when volume is decreased without the escape of heat. It is obvious that similar relations are determinable for solutions, and probably with much greater facility. M. Alexéeff has made some measurements which might be utilized for this purpose, but they are far too few in number, and moreover, the necessary data as regards osmotic pressure are wholly wanting. It would be possible, by a series of different experiments, to ascertain the evolution of heat on increasing concentration, and so to arrive at a knowledge of the specific heats of the solution at constant osmotic pressure, corresponding to the idea of specific heats at constant pressure; and also of specific heats at constant concentration, corresponding to specific heats at constant volume. I do not know whether such researches would yield as accurate results as those we are at present carrying out, but they are at least well worthy of attention.

LIQUIDS AND GASES.*

By Prof. WILLIAM RAMSAY, F. R. S.

Almost exactly twenty years ago, on June 2, 1871, Dr. Andrews, of Belfast, delivered a lecture to the members of the Royal Institution in this hall, on "The Continuity of the Gaseous and the Liquid States of Matter." He showed in that lecture an experiment which I had best describe in his own words:

"Take, for example, a given volume of carbonic acid at 50° C., or at a higher temperature, and expose it to increasing pressure till 150 atmospheres have been reached. In the process, its volume will steadily diminish as the pressure augments; and no sudden diminution of volume, without the application of external pressure, will occur at any stage of it. When the full pressure has been applied, let the temperature be allowed to fall, until the carbonic acid has reached the ordinary temperature of the atmosphere. During the whole of this operation, no break of continuity has occurred. It begins with a gas, and by a series of gradual changes, presenting nowhere any abrupt alterations of volume, or sudden evolution of heat, it ends with a liquid. For convenience, the process has been devided into two stages—the compression of the carbonic acid, and its subsequent cooling. But these operations might have been performed simultaneously, if care were taken so to arrange the application of the pressure and the rate of cooling that the pressure should not be less than 76 atmospheres when the carbonic acid had cooled to 31° C."

I am able, through the kindness of Dr. Letts, Dr. Andrews's successor at Belfast, to show you this experiment, with the identical piece of apparatus used on the occasion of the lecture twenty years ago.

I must ask you to spend some time to-night in considering this remarkable behavior; and, in order to obtain a correct idea of what occurs, it is well to begin with the study of gases, not, as in the case you have just seen, exposed to high pressures, but under pressures not differing greatly from that of the atmosphere, and at temperatures which can be exactly regulated and measured. To many here to-night, such a study is unnecessary, owing to its familiarity; but I will ask such of my audience to excuse me, in order that I may tell my story from the beginning.

* Lecture delivered at the Royal Institution, on Friday, May 8. (From *Nature*, July 23, 1891; vol. XLIV, pp. 274–277.)

Generally speaking, a gas, when compressed, decreases in volume to an amount equal to that by which its pressure is raised, provided its temperature be kept constant. This was discovered by Robert Boyle in 1660; in 1661 he presented to the Royal Society a Latin translation of his book, "Touching the Spring of the Air and its Effects." His words are:

"It is evident, that as common air, when reduced to half its natural extent, obtained a spring about twice as forcible as it had before; so the air, being thus compressed, being further crowded into half this narrow room, obtained a spring as strong again as that it last had, and consequently four times as strong as that of common air."

To illustrate this, and to show how such relations may be expressed by a curve, I will ask your attention to this model. We have a piston, fitting a long horizontal glass tube. It confines air under the pressure of the atmosphere—that is, some 15 pounds on each square inch of area of the piston. The pressure is supposed to be registered by the height of the liquid in the vertical tube. On increasing the volume of the air, so as to double it, the pressure is decreased to half its original amount. On decreasing the volume to half its original amount, the pressure is doubled. On again halving, the pressure is again doubled. Thus you see a curve may be traced, in which the relation of volume to pressure is exhibited. Such a curve, it may be remarked incidentally, is termed an hyperbola.

We can repeat Boyle's experiment by pouring mercury into the open limb of this tube containing a measured amount of air; on causing the level of the mercury in the open limb to stand 30 inches (that is, the height of the barometer) higher in the open limb than the closed limb, the pressure of the atmosphere is doubled, and the volume is halved. And on trebling the pressure of the atmosphere the volume is reduced to one-third of its original amount; and on adding another 30 inches of mercury, the volume of the air is now one-quarter of that which it originally occupied.

It must be remembered that here the temperature is kept constant; that it is the temperature of the surrounding atmosphere.

Let us next examine the behavior of a gas when its temperature is altered, when it becomes hotter. This tube contains a gas—air—confined at atmospheric pressure by mercury, in a tube surrounded by a jacket or mantle of glass, and the vapor of boiling water can be blown into the space between the mantle and the tube containing the air, so as to heat the tube to 100° C, the temperature of the steam. The temperature of the room is 17° C., and the gas occupies 290 divisions of the scale. On blowing in steam, the gas expands, and on again equalizing pressure, it stands at 373 divisions of the scale. The gas has thus expanded from 290 to 373 divisions, *i. e.*, its volume has increased by 83 divisions; and the temperature has risen from 17° to 100°, *i. e.*, through 83° C. This law of the expansion of gases was discovered almost simultaneously by Dalton and Gay-Lussac in 1801; it usually goes by the name of Gay-Lussac's law. Now, if we do not allow the volume of the gas to increase, we shall find that the pressure will increase in the same proportion that the volume would have increased had the gas been allowed to expand, the pressure having been kept constant. To decrease the volume of the gas, then, according to Boyle's law, will require a higher initial pressure; and if we were to represent the results by a curve, we should get an hyperbola, as before, but one lying higher as regards pressures. And so we should get a set of hyperbolas for higher and higher temperatures.

We have experimented up to the present with air—a mixture of two gases, oxygen and nitrogen; and the boiling points of both of these elements lie at very low temperatures: −184° C. and −193°.1 C., respectively. The ordinary atmospheric temperature lies a long way above the boiling points of liquid oxygen and liquid nitrogen at the ordinary atmospheric pressure. But it is open to us to study a gas, which, at the ordinary atmospheric temperature and pressure, exists in the liquid state; and for this purpose I shall choose water gas. In order that it may be a gas at ordinary atmospheric pressure, however, we must heat it to a temperature above 100° C., its boiling point. This tube contains water gas at a temperature of 105° C.; it is under ordinary pressure, for the mercury columns are at the same level in both the tubes and in this reservoir, which communicates with the lower end of the tube by means of the india-rubber tubing. The temperature 105° is maintained by the vapor of chloro-benzene, boiling in the bulb sealed to the jacket, at a pressure lower than that of the atmosphere.

Let us now examine the effect of increasing pressure. On raising the reservoir the volume of the gas is diminished, as usual, and nearly in the ratio given by Boyle's law; that is, the volume decreases in the same proportion as the pressure increases. But a change is soon observed; the pressure soon ceases to rise; the distance between the mercury in the reservoir and that in the tube remains constant, and the gas is now condensing to liquid. The pressure continues constant during this change, and it is only when all the water gas has condensed to liquid water that the pressure again rises. After all the gas is condensed an enormous increase of pressure is necessary to cause any measurable decrease in volume, for liquid water scarcely yields to pressure, and in such a tube as this no measurements could be attempted with success.

Representing this diagrammatically, the right-hand part of the curve represents the compression of the gas, and the curve is, as before, nearly a hyperbola. Then comes a break, and great decrease in volume occurs without rise of pressure, represented by a horizontal line; the substance in the tube here consists of water gas in presence of water; the vertical, or nearly vertical line represents the sudden and great rise of pressure, where liquid water is being slightly compressed. The pressure registered by the horizontal line is termed the "vapor-pressure" of water. If now the temperature were raised to 110° C., we should have a greater initial volume for the water gas; it is compressible by rise of the mercury as before, the relation of pressure to volume being, as before, represented on the diagram as an approximate hyperbola; and as before, condensation occurs when volume is sufficiently reduced, but this time at a higher pressure. We have again a horizontal portion, representing the pressure of water gas at 110° C. in contact with liquid water; again, a sharp angle where all gaseous water is condensed, and again a very steep curve, almost a straight line, representing the slight decrease of volume of water produced by a great increase of pressure. And we should have similar lines for 120°, 130°, 140°, 150° C., and for all temperatures within certain limits. Such lines are called isothermal lines, or shortly "isothermals," or lines of equal temperature, and represent the relations of pressure to volume for different temperatures.

Dr. Andrews made similar measurements of the relations between the pressures and volumes of carbon dioxide, at pressures much higher than those I have shown you for water. But I prefer to speak to you about similar results obtained by Prof. Sydney Young and myself with ether, because Dr. Andrews was unable to work with carbon dioxide free from air, and that influenced his results. For example, you see that the meeting points of his hyperbolic curves with the straight lines of vapor pressures are curves, and not angles; that is caused by the presence of about 1 part of air in 500 parts of carbon dioxide; also the condensation of gas was not perfect, for he obtained curves at the points of change from a mixture of liquid and gas to liquid. We however were more easily able to fill a tube with ether free from air, and you will notice that the points I have referred to are angles, not curves.

Let me first direct your attention to the shapes of the curves in the diagram. As the temperature rises the vapor-pressure lines lie at higher and higher pressures, and the lines themselves become shorter and shorter. And finally, at the temperature of 31° C. for carbon dioxide, and at 195° C. for ether, there ceases to be a horizontal portion at all; or rather the curve touches the horizontal at one point in its course. That point corresponds to a definite temperature, 195° C. for ether; to a definite pressure, 27 meters of mercury, or 35.6 atmospheres; and to a definite volume, 4.06 cubic centimeters per gram of ether. At that point the ether is not liquid, and it is not gas; it is a homogeneous substance. At that temperature ether has the appearance of a blue mist; the striæ mentioned by Dr. Andrews and by other observers are the result of unequal heating, one portion of the substance being liquid and another gas. You see the appearance of this state on the screen.

When a gas is compressed it is heated. Work is done on the gas, and its temperature rises. If I compress the air in this syringe forcibly its temperature rises so high that I can set a piece of tinder on fire and by its help explode a little gunpowder. If the ether at its critical point be compressed by screwing in the screw, it is somewhat warmed and the blue cloud disappears. Conversely, if it is expanded a little by unscrewing the screw and increasing its volume, it is cooled and a dense mist is seen, accompanied by a shower of ether rain. This is seen as a black fog on the screen.

I wish also to direct your attention to what happens if the volume given to the ether is greater than the critical volume—on increasing the volume you see that it boils away and evaporates completely; and also what happens if the volume be somewhat less than the critical volume—it then expands as liquid and completely fills the tube. It is only at the critical volume and temperature that the ether exists in the state of blue cloud, and has its critical pressure. If the volume be too great, the pressure is below the critical pressure; if too small, the pressure is higher than the critical pressure.

Still one more point before we dismiss this experiment. At a temperature some degrees below the critical temperature, the meniscus, *i. e.*, the surface of the liquid, is curved. It has a skin on its surface; its molecules, as Lord Rayleigh has recently explained in this room, attract one another, and it exhibits surface tension. Raise the temperature and the meniscus grows flatter; raise it further, and it is nearly flat and almost invisible; at the critical temperature it disappears, having first become quite flat. Surface tension therefore disappears at the critical point. A liquid would no longer rise in a narrow capillary tube; it would stand at the same level outside and inside.

It was suggested by Prof. James Thomson, and by Prof. Clausius about the same time, that if the ideal state of things were to exist, the passage from the liquid to the gaseous state should be a continuous one, not merely at and above the critical point, but below that temperature. And it was suggested that the curves, shown in the figure, instead of breaking into the straight line of vapor pressure, should continue sinuously. Let us see what this conception would involve.

On decreasing the volume of a gas, it should not liquefy at the point marked *B* on the diagram (Fig. 2), but should still decrease in volume on increase of pressure. This decrease should continue until the point *E* is reached. The anomalous state of matters should then occur, that a decrease in volume should be accompanied by a decrease of pressure. In order to lessen volume, the gas must be exposed to a continually diminishing pressure. But such a condition of matter is of its nature unstable, and has never been realized. After volume has been decreased to a certain point, *F*, decrease of volume is again attended by increase of pressure, and the last part of the curve is continuous with the realizable curve representing the compression of the liquid, above *D*.

Dr. Sydney Young and I succeeded, by a method which I shall briefly describe, in mapping the actual position of the unrealizable portions of the curve. They have the form pictured in this figure. The rise from the gaseous state is a gradual one, but the fall from the liquid state is abrupt.

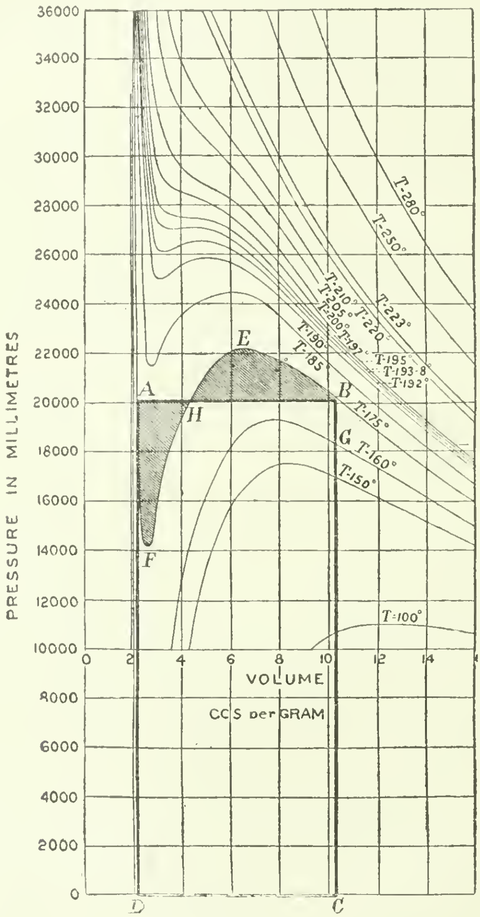

Fig. 2.

Consider the volume 14 cubic centimeters per gram on the figure. The equi-volume vertical line cuts the isothermal lines for the temperatures 175°, 180°, 185°, 190°, and so on, at certain definite pressures, which may be read from a properly-constructed diagram. We can map the course of lines of equal volume, of which the instance given is one, using temperatures as ordinates and pressures as abscissæ. We can thus find the relations of temperature to pressure for certain definite volumes, which we may select to suit our convenience—say 2 c.c. per gram; 3, 4, 5, 6, and so on. Now, all such lines are straight—that is, the relation of pressure to temperature, at constant volume, is one of the simplest; pressure is a linear function of temperature. Expressed mathematically—

$$p=bt-a,$$

where b and a are constants, depending on the volume chosen, and varying with each volume. But a straight line may be extrapolated without error; and so, having found values for a and b for such a volume as 6 c.c. per gram, by help of experiments at temperatures higher than 195°, it is possible by extrapolation to obtain the pressures corresponding to temperatures below the critical point 195° by simple means. But below that temperature the substance at volume 6 is in practice partly liquid and partly gas. Yet it is possible by such means to ascertain the relations of pressure to temperature for the *unrealizable portion* of the state of a liquid—that is, we can deduce the pressure and temperature corresponding to a continous change from liquid to gas. And in this manner the sinuous lines on the figure have been constructed.

It is possible to realize experimentally certain portions of such continuous curves. If we condense all gaseous ether and, when the tube is completely filled with liquid, carefully reduce pressure, the pressure may be lowered considerably below the vapor pressure corresponding to the temperature of ebullition, without any change further than the slight expansion of the liquid resulting from the reduction of pressure—an expansion too small to be seen with this apparatus. But on still further reducing pressure, sudden ebullition occurs, and a portion of the liquid suddenly changes into gas, while the pressure rises quickly to the vapor pressure corresponding to the temperature. If we are successful in expelling all air or gas from the ether in filling the tube, a considerable portion of this curve can be experimentally realized.

The first notice of this appearance, or rather of one owing its existence to a precisely similar cause, is due to Hooke, the celebrated contemporary of Boyle. It is noted in the account of the *Proceedings of the Royal Society* on November 6, 1672, that "Mr. Hooke read a discourse of his, containing his thoughts of the experiment of the quicksilver's standing top full, and far above the height of 29 inches, together with some experiments made by him, in order to determine the cause of this strange phenomenon. He was ordered to prepare those experiments for the view of the Society." And on November 13 "the experiment for the high suspension of quicksilver being called for, it was found that it had failed. It was ordered that thicker glasses should be provided for the next meeting."

There can be no doubt that this behavior is caused by the attraction of the molecules of the liquid for each other. And if the temperature be sufficiently low, the pressure may be so reduced that it becomes negative—that is, until the liquid is exposed to a strain or pull, as is the mercury. This has been experimentally realized by M. Berthelot and by Mr. Worthington, the latter of whom has succeeded in straining alcohol at the ordinary temperature with a pull equivalent to a negative pressure of 25 atmospheres, by completely filling a bulb with alcohol, and then cooling it. The alcohol in contracting strains the bulb inwards; and finally, when the tension becomes very great, parts from the glass with a sharp "click."

To realize a portion of the other bend of the curve, an experiment has been devised by Mr. John Aitken. It is as follows: If air—that is space, for the air plays a secondary part—saturated with moisture be cooled, the moisture will not deposit unless there are dust particles on which condensation can take place. It is not at first evident how this corresponds to the compressing of a gas without condensation. But a glance at the figure will render the matter plain. Consider the isothermal 175° C. for ether at the point marked *A*. If it were possible to lower the temperature to 160° C. without condensation, keeping volume constant, the pressure would fall, and the gas would then be in the state represented on the isothermal line 160° at *G*,—that is, it would be in the same condition as if it had been compressed without condensation.

You saw that a gas, or a liquid, is heated by compression; a piece of tinder was set on fire by the heat evolved on compressing air. You saw that condensation of ether was brought about by diminution of pressure—that is, it was cooled. Now, if air be suddenly expanded it will do work against atmospheric pressure and will cool itself. This globe contains air; but the air has been filtered carefully through cotton-wool, with the object of excluding dust particles. It is saturated with moisture. On taking a stroke of the pump, so as to exhaust the air in the globe, no change is evident; no condensation has occurred, although the air has been so cooled that the moisture should condense were it possible. On repeating the operation with the same globe, after admitting dusty air—ordinary air from this room—a slight fog is produced, and, owing to the light behind, a circular rainbow is seen; a slight shower of rain has taken place. There are comparatively few dust particles, because only a little dusty air has been admitted. On again repeating the fog is denser; there are more particles on which moisture may condense.

One point more and I have done. Work is measured by the distance or height through which a weight can be raised against the force of gravity. The British unit of work is a foot-pound—that is, a pound raised through 1 foot; that of the metric system is 1 gram raised through 1 centimeter. If a pound be raised through 2 feet twice as much work is done as that of raising a pound through 1 foot, and an amount equal to that of raising 2 pounds through 1 foot. The measure of work is therefore the weight multiplied by the distance through which it is raised. When a gas expands against pressure it does work. The gas may be supposed to be confined in a vertical tube and to propel a piston upward against the pressure of the atmosphere. If such a tube has a sectional area of 1 square centimeter, the gas in expanding a centimeter up the tube lifts a weight of nearly 1,000 grams through 1 centimeter, for the pressure of the atmosphere on a square centimeter of surface is nearly 1,000 grams—that is, it does 1,000 units of work, or ergs. So the work done by a gas in expanding is measured by the change of volume multiplied by the pressure. On the figure, the change of volume is measured horizontally, the change of pressure vertically. Hence the work done is equivalent to the area *A B C D* on the figure.

If liquid as it exists at *A* change to gas as it exists at *B*, the substance changes its volume and may be made to do work. This is familiar in the steam engine, where work is done by water expanding to steam and so increasing its volume. The pressure does not alter during this change of volume if sufficient heat be supplied; hence the work done during such a change is given by the rectangular area.

Suppose that a man is conveying a trunk up to the first story of a house; he may do it in two (or, perhaps, a greater number of) ways. He may put a ladder up to the drawing-room window, shoulder his trunk, and deposit it directly on the first floor; or he may go down the area stairs, pass through the kitchen, up the kitchen stairs, up the first flight, up the second flight, and down again to the first story. The end result is the same; and he does the same amount of work in both cases so far as conveying the weight to a given height is concerned, because in going down stairs he has actually allowed work to be done on him by the descent of the weight.

Now, the liquid in expanding to gas begins at a definite volume; it evaporates gradually to gas without altering pressure, heat being, of course, communicated to it during the change, else it would cool itself; and it finally ends as gas. It increases its volume by a definite amount at a definite pressure, and so does a definite amount of work. This work might be utilized in driving an engine.

But if it pass continuously from liquid to gas, the starting point and the end point are both the same as before. An equal amount of work has been done: but it has been done by going down the area stair (as it were), and over the round I described before.

It is clear that a less amount of work has been done on the left-hand side of the figure than was done before, and a greater amount on the right-hand side; and if I have made my meaning clear you will see that as much less has been done on the one side as more has been done on the other—that is, that the area of the figure *B E H* must be equal to that of the figure *A F H*. Dr. Young and I have tried this experimentally—that is, by measuring the calculated areas—and we found them to be equal.

This can be shown to you easily by a simple device, namely, taking them out and weighing them. As this diagram is an exact representation of the results of our experiments with ether the device can be put in practice. We can detach these areas, which are cut out in tin, and place one in each of this pair of scales and they balance. The fact that a number of areas thus measured gave the theoretical results of itself furnishes a strong support of the justice of the conclusions we drew as regards the forms of these curves.

To attempt to explain the reasons of this behavior would take more time than can be given to-night; moreover, to tell the truth, we do not know them. But we have at least partial knowledge and we may hope that investigations at present being carried out by Prof. Tait may give us a clear idea of the nature of the matter and of the forces which act on it, and with which it acts, during the continuous change from gas to liquid.

PRESENT PROBLEMS IN EVOLUTION AND HEREDITY.*

By Henry Fairfield Osborn.

In the past decade of practical research and speculation in biology, two subjects have outstripped in interest and importance the rapid progress all along the line. These are, first, the life history of the reproductive cell from its infancy in the ovum onward, and second, the associated problem of heredity, which passes insensibly from the field of direct observation into the region of pure speculation.

As regards the cell it was generally believed that the nucleus was an arcanum into the mysteries of which we could not far penetrate; but this belief has long been dispelled by the eager specialist, and it is no exaggeration to say that we now know more about the meaning of the nucleus than we did about the entire cell a few years ago. At that time the current solution of the heredity problem was a purely formal one: it came to the main barrier, namely, the relation of heredity and evolution to the reproductive cells, and leapt over it by the postulate of Pangenesis. The germ-cell studies of Balfour, Van Beneden, the Hertwig brothers, Weismann, Boveri, and others, have gradually led us to hope that we shall some day trace the connection between the intricate metamorphoses in these cells and the external phenomena of heredity, and more than this, to realize that the heredity theory of the future must rest upon a far more exact knowledge than we enjoy at present of the history of the reproductive cell both in itself and in the influence which the surrounding body cells have upon it.

These advances affect the problem of life and protoplasm, whether studied by the physician, the anthropologist, or the zoölogist, thus concentrating into one focus opinions which have been formed by the observation of widely different classes of facts. As each class of facts bears to the observer a different aspect and gives him a personal bias, the discusson is by no means irenical, and it is our privilege to live through one of those heated periods which mark the course of every revolution in the world of ideas. Such a crisis was brought about by

*The Cartwright Lectures for 1892; delivered before Alumni of the College of Physicians and Surgeons, February 12, 19, and 26, 1892. (From the *Medical Record* for February 20, March 5, April 23, and May 14, 1892.)

the publication of the theory of Darwin, in 1858, and, after subsiding, has again been aroused by Weismann's theory of heredity, published in 1883.

This is the situation I have ventured to present to you as Cartwright lecturer, not, of course, without introducing some conclusions of my own, which have been derived from vertebrate palæontology, but which I shall direct mainly upon human evolution.

So far as theories need come before us now, remember that Lamarck (1792) attributed evolution to the hereditary transmission to offspring of changes (acquired variations) caused by environment and habit in the parent. Darwin's latest view was that evolution is due to the natural selection of such congenital variations as favored survival, supplemented by the transmission of acquired variations. Weismann denies the transmission of acquired variations, or characters, entirely, and attributes evolution solely to the natural selection of the individuals which bear the most favorable variations of the germ or reproductive cells. We must therefore clearly distinguish between "congenital variations" which are part of our inheritance and "acquired variations" which are due to our life habits; the question is, are the latter transmitted?

At the outset I would emphasize the extreme complexity of evolution by a few words upon variation, or in terms of medical science, upon anomalies.

When we speak of a part as "anomalous" we mean that it varies at birth from the ordinary or typical form; it may be minute, as the small slip of a tendon, or large, as the addition of a complete vertebra to the spinal column. Wood has found that in the muscular system alone there are nine anomalies in the average individual. It is clear that the evolution of a new type, so far as the muscular system is concerned, must consist in the accumulation of anomalies in a certain definite direction by heredity. Thus the anomalous condition of one generation may become the typical condition of a very much later generation, and we observe the paradox of a typical structure becoming an anomaly and an anomalous structure becoming typical; for example, the supracondylar foramen of the humerus was once typical, it is now anomalous; the retardation in development of the wisdom tooth was once anomalous, it is now typical.

The same principle applies to races which are in different stages of evolution; an anomaly in the white, such as the early closure of the cranial sutures, is normal in the black. Now the deductions of the Weismann school of evolutionists seem to be founded upon the principle "*de minimis non curat lex;*" that we need only regard such major variations as can, *ex hypothesi*, weigh in the scale of survival. Against this I urge that we must regard the evolution of particular structures, the components of larger organs, the separate muscles and bones for example, for the very reason that while in some cases they play a most humble rôle in our economy we can prove beyond a doubt that they are in course of evolution. Minor variations in foot structure, which are possibly of vital importance to a quadruped whose very existence may depend upon speed, sink into obscurity as factors in the survival of the modern American.

The evolution of man in the most unimportant details of his structure promises, therefore, to afford a far more crucial test of the Lamarckian *vs.* the pure natural selection theory, than in the domain of his higher faculties, for the reason that selection may operate upon variations in mind, while it taxes our credulity to believe it can operate upon variations in muscle and bone. This is my ground for selecting the skeleton and muscles for the subject of the introductory lecture. Nevertheless, let us review variation in all its forms in human anatomy before forming an opinion. Let us remember, too, that congenital and acquired variations are universal as necessities of birth and life; they are exhibited in the body as a whole—in its proportions, in the components of each limb, finally in the separate parts of each component, as in the divisions of a complex muscle. Thus the possibilities of transformism are everywhere. What is the nature and origin of congenital variations? Their causes? Do they follow certain directions? Do they spring from acquired variations by heredity? These are some of the questions which are still unsettled.

But striking as are the anomalies from type, the repetitions of type as exhibited in atavism and normal inheritance are still more so, and equally difficult to explain. Therefore our theory must provide both for the observed laws of repetition of ancestral form and the laws of variation from ancestral form, as the pasture-land of evolution. Add to these, that for a period in each generation this entire legislation of nature is compressed into the tiny nucleus of the fertilized ovum, and the whole problem rises before us in its apparent impregnability which only intensifies our ardor of research.

LECTURE I.—THE CONTEMPORARY EVOLUTION OF MAN.

The anthropologists and anatomists have enjoyed a certain monopoly of *Homo sapiens*, while the biologists have directed their energies mainly upon the lower creation. But under the inspiring influences of the Darwinian theory these originally distinct branches have converged, and as man takes his place in the zoölogical system, comparative anatomy is recognized as the infallible key to human anatomy.

For our present purpose we must suppress our sentiment at the outset and state plainly that the only interpretation of our bodily structure lies in the theory of our descent from some early member of the primates, such as may have given rise also to the living Anthropoidea. This is also the only tenable teleological view, for many of our inherited organs are at present non-purposive, in some cases even harmful,—as the appendix vermiformis.

From the typical mammalian standpoint man is a degenerate animal; his senses are inferior in acuteness; his upright position, while giving him a superior aspect, entails many disadvantages, as recently enumerated by Clevenger,* for the body is not fully adapted to it; his feet are not superior to those of many lower Eocene plantigrades; his teeth are mechanically far inferior to those of the domestic cat. In fact, if an unbiassed comparative anatomist should reach this planet from Mars he could only pass favorable comment upon the perfection of the hand and the massive brain. Holding these trumps, man has been and now is discarding many useful structures. I refer especially to civilized man, who is more prodigal with his inheritance than the savage. By virtue of the hand and the brain he is nevertheless the best adapted and most cosmopolitan vertebrate. The man of Néanderthal or Spy, with retreating forehead and brain of small cubic capacity† was limited both in his ideas and his powers of travel; yet he was our superior in some points of osteological structure. But the period of Nêanderthal was recent compared with that in which some of our rudimentary organs were serviceable, such as the vermiform appendix or the panniculus carnosus‡ muscle. These rudiments in turn are neogenetic when we consider the age of the two antique sense organs in the optic thalamus, the remnants of the median or pineal eye and the pituitary body, both of which were undoubtedly present, and probably useful, in the recently discovered Silurian fishes.

I mention these vestiges of some of the first steps in creation to illustrate the extraordinary conservative power of heredity (which is even more forcibly seen in our embryological development), partly also to show how widely our organs differ in age. Galton has compared the human frame to a new building built up of fragments of old ones; extend this back into the ages and the comparison is complete.

Development, balance, degeneration.—It is probable that none of our organs are absolutely static and that the apparent halt in the development of some is merely relative, as where a fast train passes a slow one. The numerous cases of arrested evolution in nature are always connected with fixity of environment, an exceptional condition with man, and we have ample evidence that some organs are changing more rapidly than others.

Adaptation to our changing circumstances is mainly effected by the simultaneous development and degeneration of organs which lie side by side, as in the muscles of the foot or hand; in terms of physiology,

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*Disadvantages of the Upright Position, article in *American Naturalist*, January, 1884, vol. XVIII. p. 1.

†The remarkable skulls and skeletons which have recently been discovered at Spy remove all doubts as to the normal, *i.e.*, racial character of the famous Néanderthal skull, which were entertained by Quatrefages and others. See Fraipont and Lohest, *Archives de Biologie*, 1887, p. 697.

‡This is an epidermal or twitching muscle in the quadrupeds.

we observe the hypertrophy of adaptive organs and atrophy of inadaptive or useless organs. This compensating re-adjustment, whereby the sum of nutrition to any region remains the same during re-distribution to its parts, may be called metatrophism. It is the "gerrymander" principle in nature.

In practical investigation it is very difficult in many cases to determine whether an organ is actually developing or degenerating at the present time, although its variability or tendency to present individual anomalies indicates that some change is in progress. I may instance the highly variable peroneus tertius muscle (Wood). The rise or fall of organs is so constantly associated with their degree of utility that in each case the doubt can be removed by a careful analysis of the greater or less actual service rendered by the part in question. Apart from the question of causation, it is a fixed principle that a part degenerating by disuse in each individual will also be found degenerating in the race.

Degeneration is an extremely slow process; both in the muscular and skeletal systems we find organs so far on the down grade that they are mere pensioners of the body, drawing pay (*i. e.*, nutrition) for past honorable services without performing any corresponding work—the plantaris and palmaris muscles for example. Of course an organ without a function is a disadvantage, so that the final duty of degeneration is to restore the balance between structure and function, by placing it *hors de combat* entirely. One symptom of decline is variability, in which the organ seems to be demonstrating its own uselessness by occasional absence. As Humphrey remarks: "The muscles which are most frequently absent by anomalies are in fact those which can disappear with least inconvenience, either because they can be replaced by others or because they play an altogether secondary rôle in the organism." The stages downward are gradual: the rudiment becomes variable as an adult structure, then as a fœtal structure; the percentage of absence slowly increases until it re-appears only as a reversion; finally the part ceases even to revert and all record of it is lost. This long struggle of the destructive power of degeneration, which you see is essentially an adaptive factor, against the protective power of heredity is the most striking feature of the law of repetition. (See Galton's similar principle of regression in anthropology.)

A careful study of our developing, degenerating, rudimental, and reversional organs amply demonstrates that man is now in a state of evolution hardly less rapid, I believe, than that which has produced the modern horse from his small five-toed ancestor. As far as I can see, the only reason why our evolution should be slower than that of the ancient horse is the frequent intermingling of races, which always tends to resolve types which have specialized into more generalized types. Wherever the human species has been isolated for a long period of time, divergence of character is very marked, as will be seen in some of the races I refer to below.

To lighten the long catalogue of facts, gathered from many authors, I shall frequently allude to habit, but will ask you to consider it for the time as associational rather than causal. Pouchet says: "Man is a creature of the writing table, and could only have been invented in a country in which covering of the feet is universal;" he should have added the "eating table." From the average man our fashions and occupations demand the play of the forearm and hand, the independent and complex movements of the thumb and finger; the outward turning of the foot in walking. These are some of the most conspicuous features of modern habit.

*The skeletal variations.**—In a most valuable essay by Arthur Thomson upon "The Influence of Posture on the Form of the Articular Surfaces of the Tibia and Astragalus in the Different Races of Man and the Higher Apes,"† we find clearly brought out the distinction between congenital variations and those which may be acquired by prolonged habits of life. It is perfectly clear from this investigation that certain racial characters, such as "platycnemism" or flattened tibia, which have been considered of great importance in anthropology, may prove to be merely individual modifications due to certain local and temporary customs. Thomson's conclusions are that the tibia is the most variable in length and form of any long bone in the body. Platycnemia is most frequent in tribes living by hunting and climbing in hilly countries, and is associated with the strong development of the tibialis posticus. The great convexity of the external condyloid surface of the tibia in savage races appears to be developed during life by the frequent or habitual knee flexure in squatting; it is less developed where the tibia has a backward curve, and is independent of platycnemia. Another product of the squatting habit is a facet formed upon the neck of the astragalus by the tibia. This is very rare in Europeans; it is found in the gorilla and orang, but rarely in the chimpanzee. We must therefore be on our guard to distinguish between congenital or hereditary skeletal characters which are fundamental, and "acquired" skeletal variations which may not be hereditary. The latter are of questionable value in tracing lines of descent, if not actually misleading; on the other hand, the teeth, as shown by Cope in his essay on "Lemurine reversion in human dentition," have distinct racial patterns and are reliable indices of consanguinity, because their form can not be modified during life.

The main features of present evolution in the backbone are the elaboration of the spines of the cervical vertebræ, the increase of the spinal curvatures, the shortening of the centra of the lumbar vertebræ and

* For recent general articles, see Blanchard, "L'Atavisme chez l'Homme," *Rev. de Anthrop.*, 1885, p. 425; and Baker, "The Ascent of Man," *Proceedings of the American Association for the Advancement of Science*, 1890. Also, *Smithsonian Report* for 1890, p. 447.

† *Journal of Anatomy and Physiology*, 1889, p. 617.

shifting of the pelvis upward, whereby a lumbar vertebra is added to the sacrum and subtracted from the dorso-lumbar series.

Cunningham has found that the division of the neural spines in the upper cervical vertebræ distinguishes the higher races from the lower.* The spine of the axis is always bifid, but the spines of the cervicals three, four, and five, are also, as a rule, bifid in the European, while they are single in the lower races. The same author shows that the bodies of the lumbar vertebræ are altering, by widening and shortening, to form a firmer pillar of support, with a compensating increase in the length of the intervertebral cartilages.† In the child, the vertebræ present more nearly their primitive elongate compressed form. With this is associated an increase of the forward lumbar curvature (Turner);‡ the primitive (*i. e.*, Simian) curve was backward: even in the negroes the collective measurement of the posterior faces of the five lumbars is greater than the anterior, in the proportion of 106 to 100; whereas in the white the collective anterior faces exceed the posterior in nearly the same proportion—100 to 96.

The lower region of the back is also the seat of one of the most interesting and important of the changes in the body, namely, the correlated evolution of the inferior ribs, the lumbar vertebræ, and the pelvis,—to which embryology, adult and comparative anatomy, and reversion all contribute their quota of proof. In most of the anthropoid apes, and therefore presumably in the pro-anthropos, there were thirteen complete ribs and four lumbar vertebræ, while man has twelve ribs and five lumbars. Thus we may consider the superior lumbar of adult man as a ribless dorsal; not so in the human embryo, however, for Rosenberg§ has found a cartilaginous rudiment of the missing thirteenth rib upon the so-called first lumbar. Atavism contributes an earlier chapter in the history of this region, for Birmingham‖ reports, out of fifty cases examined in one year, two in which there were six lumbars, and in each the thirteenth rib was well developed; this is an interesting example of "correlated reversion," for as the pelvis shifted downward to its ancestral position upon the twenty-sixth vertebra, the thirteenth rib was also restored. The other ribs are in what the ancients styled a "state of flux;" our eighth rib has been so recently floated from the sternum that, and according to Cunningham,¶ it reverts as a true rib in twenty cases out of a hundred, showing a decided preference for the right side. Regarding also the occasional fusion of the fifth lumbar with the sacrum and the unstable condition of the twelfth rib, which is by variation rudimentary or absent, Rosenberg makes bold to predict that in the man of the future the pelvis will shift another step upward to the twenty-fourth vertebræ, and we shall then

* *Journal of Anatomy and Physiology*, 1886, p. 636.

† *Ibid.*, 1890, p. 117.

‡ *Ibid*, 1887, p. 473.

§ *Morph. Jahrb.*, 1876.

‖ *Journal of Anatomy and Physiology*, 1891, p. 526.

¶ *Ibid.*, 1890, p. 127.

lose our twelfth rib. The upright position, and consequent transfer of the weight of the abdominal viscera to the pelvis, may be considered the habit associated with this reduction of the chest; at all events, in the evolution of quadrupeds there is a constant relation of increase between the size of the posterior ribs and the weight of the viscera, until the rib-bearing vertebræ rise to twenty and the lumbars are reduced to three.* It would be interesting to note the condition of the ribs in some of the large-bellied tribes of Africans in reference to this point.

The coccyx has naturally been the center of active search for the missing flexible caudals. As is well known, the adult coccyx contains but from three to five centers, while the embryo contains from five to six. Dr. Max Bartels has made "Die geschwänzten Menschen" the subject of an exhaustive memoir upon cases of the reversion of the tail, while Testut records all the primitive tail muscles in various stages of reversion. Watson reports that the curvatores coccygia (depressores caudæ) occur only in 1 in 1,000 cases.

This suggests a moment's digression to consider the different phases of reversion. The thirteenth rib recurs by what Gegenbaur calls "neogenetic reversion,"† for it is simply the anomalous adult development of an embryonic rudiment. Under neogenetic reversions many authors also include cases of the "arrested development," or persistence of an embryonic condition to adult life, such as the disunited odontoid process of the axis vertebra, which happens to repeat a very remote ancestral condition. I think such cases may illustrate a reversional tendency, although many cases of arrested development, such as anencephaly, have no atavistic significance whatever.‡ More rare and far more difficult to explain are the "palæogenetic reversions," in which the anomaly, such as the supracondylar foramen, reverts to an atavus so remote that the rudiment is not even represented in the embryo.

The features of skull development are primarily the increase of the cranium and the late closure of the cranial sutures, in contrast with the more complete and earlier closure of the facial sutures.

So far as I can gather, this seems to be another region where the white and colored races present reversed conditions; the early closure and arrest of brain development in the negroes is well known; the later closure among the whites is undoubtedly an adaptation to brain growth. In his valuable statistics upon the Cambridge students, Galton says: "Although it is pretty well ascertained that in the masses of the population the brain ceases to grow after the age of nineteen, or even earlier, it is by no means the case with university students. In high honor men head growth is precocious, their heads predominate over the average more at nineteen than at twenty-five."

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* In the elephant and rhinoceros.

† *Morph. Jahrb.*, Bd. VI, p. 585.

‡ Anencephaly, it should be said, is frequently associated with numerous reversions.

Many of the cases of arrested closure of facial sutures are reversional, as they correspond with the adult condition of other races, such as the divided malar, or as Japonicum. The human premaxillary, a discovery with which Goethe's name will always be associated, is sometimes partially, more rarely wholly, isolated; it is late to unite with the maxillary in the Australians, and has been reported entirely separate in a new Caledonian child (Deslongchamps) and in two Greenlanders (Carus). The orbito-maxillary frontal suture, cited by Turner as a reversion to the pithecoid condition, is believed by Thomson, after the examination of 1,037 skulls, to be merely an accidental variation, without any deeper significance.* The development of the temporal bone from two centers, observed by Meckel, Gruber, and many others, is considered by Albrecht a reversion to the separate quadrate of the sauro-mammalia. This I think is in the highest degree improbable (see "Limits of Reversion"). The open cranial and closed facial sutures are apparently associated with our increasing brain action and decreasing jaw action; in one case the growth is prolonged and the sutures are left open, in the other, the growth is arrested and the sutures are closed.

Is the lower jaw developing or degenerating? This question has recently been the subject of a spirited controversy between Mr. W. Platt Ball,† representing the Weisman school, and Mr. F. Howard Collins,‡ supporting Herbert Spencer's view that a diminishing jaw is one of the features of our evolution which can only be explained by disuse. Mr. Collins find that, relatively to the skull, the mass of the recent English jaw is one-ninth less than that of the ancient British and, roughly speaking, half that of the Australian. He appears to establish the view that the jaw is diminishing.

Closely connected with this is the evolution of the teeth; how are they tending?

Flower§ has shown, as regards the length of our molar series, that we, together with the ancient British and Egyptians, belong to a small toothed or "microdont" race; the Chinese, Indians (North American), Malayans, and negroes in part, are intermediate or "mesodont," while the Andamanese, Melanasians, Australians, and Tasmanians are "macrodont." While undersize marks the molars as a whole, the wisdom tooth is certainly in process of elimination; it has the symptoms of decline; it is very variable in size, form, and in the date of its appearance, is often misplaced, and is not uncommonly quite rudimentary (Tomes‖). Here is another instance where the knife-and-forkless races reverse our degeneracy, for in them not only is the last normal molar

**Journal of Anatomy and Physiology*, 1890, p. 348.

† Are the Effects of Use and Disuse Inherited? *Nature Series*, 1890.

‡ The Lower Jaw in Civilized Races, 1891.

§ *Journal of the Anthropological Institute*, 1880.

‖ *Dental Anatomy*, p. 116.

(m. 3) large and cut long before the traditional years of discretion, but in the first two lower molars are found two intermediate cusps (Tomes)* which are variable or absent in us (Abbott); moreover, in the macrodont races a surplus molar † (m. 4) is sometimes developed. Mummery reports nine such cases among 328 West Africans (Ashantis). As an instance of associated habit I may here mention that Dr. Lumholtz, the Australian explorer, informs me that in adult natives the teeth are worn to the gum; in the absence of tools they are used in every occupation, from eviscerating a snake to cutting a root. A tour of inspection through any large collection of skulls brings out the contrast between the sound and hard-worn molars of the savage, and the decayed and little-worn molars of the white.

Upon the descent theory, the reduction of teeth in the progenitor of man began as far back as the Eocene period, for not later than that remote age do we find the full complement of three incisors and four premolars in each jaw; now there are but two remaining of each. Baume, a high authority, believes he has discovered eleven cases of a rudimental reversion of one of these lost premolars ‡ not cutting the jaw. Not infrequently both these missing teeth occur by reversion. It is difficult to conceive of reversion to such a remote period, yet it is supported by other evidence. An embryonic third incisor has, I believe, been discovered. As long ago as 1863 Sedgwick § recorded a case of six upper and lower incisors in both jaws, and appearing in both the milk and permanent dentitions; this anomaly was inherited from a grandparent, a striking instance of hereditary reversional tendency. We might consider that these cases of supernumerary teeth belong in the same category as polydactylism, or additional fingers, which are not atavistic, but for the fact that they do not exceed the typical ancestral number, whereas the fingers do.

We owe to Windle ‖ a careful review of the incisor reversions, in which he shows that the lost incisors re-appear more frequently in the upper than the lower jaw (coinciding with the fact that the lower teeth were the first to disappear in the race); he considers that the lost tooth was the one originally next the canine, and concludes by adding our present upper outer incisor to the long list of degenerating organs.¶ He supports this statement by measurements and by citing cases in which it has been found absent. Yet the reduction of the jaws is apparently outstripping that of the teeth, if we can judge from the

* *Dental Anatomy*, p. 416.

† This tooth has been found in several other macrodont tribes (Australians, Tasmanians, Neo-Caledonians), Fontan.

‡ *Odontologische Forschungen*, p. 268. This rudiment is found between the first and second normal premolars.

§ *British and Foreign Medico Chirurgical Review*, 1863.

‖ *Journal of Anatomy and Physiology*, 1887. p. 85.

¶ Baume believes that the missing incisor is the primitive median one, while Turner believes it is the second. The fossil record supports Windle.

frequent practice among American dentists of relieving the crowded jaw by extraction.

We now turn to the arches and limbs. Flower has pointed out that the base of the scapula is widening in the higher races, so that the "index," or ratio of length to breadth, is quite distinctive. Gegenbaur associates this with the development of the scapulo-humeral muscles and the greater play of the humerus as a prehensile organ.

In general, the arm increases in interest as we descend toward the hand, both in the skeleton and musculature, because here we meet with the first glimpses of facts which enable us to form some estimate of the rate of human evolution. The well-known humeral torsion (connected with increased rotation) ascends from 152° in the polished stone age to 164° in the modern European. The intercondylar foramen, or perforation of the olecranon fossa, is exceptionally well recorded;* it is found in 30 per cent of skeletons of the reindeer period; in the dolmen period it fell to 24 per cent; in Parisian cemeteries between the fourth and tenth centuries it is found in 5.5 per cent; it has now fallen to 3.5 per cent. The condylar foramen, occasionally forming a complete bridge of bone above the inner condyle and transmitting the median nerve and brachial artery, is known as the "entepicondylar" foramen in comparative anatomy, and is one of the most ancient characters of the mammalia; it reverts palaeogenetically in 1 per cent of recent skeletons, but much more frequently in inferior races (Lamb). In the wrist bone is sometimes developed another extremely old structure—the os centrale. Gruber† reported its recurrence at 0.25 per cent approximately. This is a case of neogenetic reversion, for Leboucq‡ shows that there is a distinct centrale in every human carpus in the first part of the second month, which normally fuses with the scaphoid by the middle of the third month.

The divergence of the female from the male pelvis is an important feature of our progressive development; it is proved by the fact that, as we descend among the lower races it becomes increasingly difficult to distinguish the female skeleton from the male, for the pelves of the two sexes are nearly uniform. Here it seems to me is a most interesting problem for investigation. Arbuthnot Lane's§ views of the mechanical causes of this divergence, which are strongly Lamarckian, may be weighed with the theory of survival of the fittest, for the large female pelvis is perhaps the best example that can be adduced of a skeletal variation which would be preserved by natural selection, for reasons which are self-evident. The third trochanter of the femur is believed by Prof. Dwight,‖ of the Harvard Medical School, to be a true re-

* See Blanchard, op. cit., p. 150.

† Virchow's *Archiv*, 1885, p. 353.

‡ *Ann. de la Soc. de Méd. de Gand*, *1884*.

§ *Journal of Anatomy and Physiology*, 1888, p. 214.

‖ *Ibid.*, 1890, p. 61.

version (1 per cent) in our race and not an acquired variation, as it is very frequently found among the Sioux, 50 per cent, Laplanders, 64 per cent, and Swedes, 37 per cent; like the condylar foramen it is an ancient mammalian character.

The foot is full of interest in its association of degeneration and development with our present habits of walking; the great toe is increasing and the little toe diminishing, causing the oblique slope from within outward which is in wide contrast with the square toes in the infant or in the lower races. In many races the second toe is as long as the first, and the feet are carried parallel instead of the large toe turning out. If anyone will analyze his sensations in walking, even in his shoes, he will be conscious that the great toe is taking active part in progression while the little toe is passive and insensitive. We are not surprised, therefore, to learn from Pfitzner* that we are losing a phalanx, that in many human skeletons (41.5 per cent in women and 31 per cent in men) the two end joints of the little toe are fused. The fusion occurs not only in adults, but between birth and the seventh year, and in embryos of between the fifth and seventh month. The author does not attribute this to the mechanical pressure of tight shoes because it is found in the poorer classes. He considers it the first act of a total degeneration of the fifth toe.

Variations in the muscles.—The evolution of the muscles of the foot looks in the same direction. As you know, the large toe in many of the apes is set at an angle to the foot and is used in climbing. It is still employed in a variety of occupations by different races. According to Tremlett,† the celebrated great toe of the Annamese, which normally projects at a wide angle from the foot, is contemptuously mentioned in Chinese annals of 2285 B. C., the race being then described as the "cross-toes." The long flexor of the hallux is apparently degenerating, showing a tendency to fuse with the flexor communis; the abductors and adductors of this toe are also degenerating, the latter being proportionately large in children (Ruge). The little toe exhibits only by reversion its primitive share of the flexor brevis (Gegenbaur); more frequently it varies in the direction of its future decline by losing its flexor brevis tendon entirely. Two atavistic muscles, the abductor metatarsi quinti‡ (always present in the apes), and the peroneus parvus (Bischoff), also point to the former mobility of the outer side of the foot. In general the bones of the foot are developing on the inner and degenerating on the outer side, with loss of the lateral movements of the hallux and of all independent movements in the little toe. The associated habit is that the main axis of pressure and strain now connects the heel and great toe, leaving the outer side of the foot comparatively functionless.

* See Humboldt, 1890; also *Nature*, 1890, p. 301.

† *Journal of the Anthropological Institute*, 1880, p. 461.

‡ Darwin: *Descent of Man*, p. 42.

The variations in the muscular system mark off more clearly the regions of contemporary evolution, and therefore are even more instructive than those in the skeleton. Muscular anomalies have however never been adequately analyzed. Even the remarkable memoir of M. Testut, "Sur les Anomalies musculaires," is defective in not clearly distinguishing between variations which look to the future, those which revert to the past, and those which are fortuitous, for the author is strongly inclined to refer all anomalies to reversion.

The law of muscular evolution is specialization by the successive separation of new independent contractile bands from the large fundamental muscles, while the law of skeletal evolution is reduction of primitive parts and the specialization of articular surfaces. The number of muscles in the primates as a whole has therefore been steadily increasing, while the number of bones has been diminishing. In man the number of muscles is probably increasing in the regions of the lower arm, and diminishing in every other region. The analysis is rendered very difficult by the fact that some muscles (*e. g.*, those connecting the shoulder with the neck and back) revert to a former condition of greater specialization when they were employed in swinging the body by the arms, and in quadrupedal locomotion; while other muscles (*e. g.*, those connecting the forearm and fingers) revert to a former simpler arrangement when the hand was mainly a grasping organ, and the thumb was not opposable.

As in the skeleton, we find that muscular anomalies include (1) palaeogenetic reversions, or complete restorations of lost muscles; (2) neogenetic reversions, or revivals of former types in the relations of existing muscles; (3) progressive variations, which either by degeneration or specialization point to future types; (4) fortuitous variations, which cannot be referred to either of the above.

Duval observes that the flexor longus policis repeats in reversion all the stages of its evolution between man and the apes, in which it is a division of the flexor profundus. Gruber and others have even observed the absence of the thumb tendon. This is true of all the new muscles. Of this Testut writes:

"Ne dirait-on pas, en le voyant s'éloigner si souvent de son état normal, que la nature voudrait le remener à sa disposition primitive, luttant ainsi sans cesse contre l'adaptation, et ne lui abandonnant qu' à regret l'une de ses plus belles conquêtes."

Speaking of the hand, Baker says:

"On comparing the human hand with that of the anthropoids, it may be seen that this efficiency is produced in two ways—first increasing the mobility and variety of action of the thumb and fingers; second, reducing the muscles used mainly to assist prolonged grasp, they being no longer necessary to an organ for delicate work requiring constant re-adjustment."*

*"The Ascent of Man," *Proceedings Am. Assoc. Adv. Sci.* 1890, vol. XXXIX, p. 353. Also, *Smithsonian Report*, 1890, p. 119.

You have noticed the recent discovery that the grasping power of infants is so great that the reflex contraction of the fingers upon a slender crossbar sustains their weight; this power and the decided inward rotation of the sole of the foot and mobility of the toes are persistent adaptations. Our grasping muscle, the palmaris longus, is highly variable and often absent; like the plantaris of the calf, it has been replaced by other muscles, and its insertion has been withdrawn from the metapodium to the palmar fascia. In negroes we frequently find the palmaris reverting to its former function of flexing the fingers by insertion in the metacarpals.

The rise of muscular specialization by degeneration is beautifully shown in the extensor indicis, which, while normally supplying the index only, reverts by sending its former slips to the thumb, middle, and even to the ring finger. Testut* believes that the extension power of the middle and ring fingers has declined, as the cases of reversion point to greater mobility; the extensor minimi digiti is distinct and highly variable (Wood), often sending a slip to the ring finger.

The entire flexor group of the hand, excepting the palmaris, is apparently specializing. The demonstration by Windle† and Bland Sutton, that the origin of the flexors and extensors is shifting downward from their original position, is evidence of an adaptation to the short special contractions required of them.

The abductor pollicis‡ is also progressive and variable (Wood); the reduplication of its inferior tendon, which is sometimes provided with a distinct muscle, apparently points to the birth of a second abductor. The opponens of the thumb is well established and constant. Variability seems to characterize both the developing and degenerating muscles; the latter are apt to be absent; it is rare that an important muscle, such as the extensor indicis, is absent, but such cases are reported.

It is interesting to note that the lost muscles of the body are almost exclusively in the trunk or shoulder, and pelvic arches, and not in the limbs. It will be remembered that the human shoulder joint is exceptionally rigid, whereas in the quadrupedal state it was a factor in progression. Some of the muscular reversions in this quadrupedal region are the levator claviculæ (1 to 60, Macalister), trachelo-clavicularis, scalenus intermedius, acromio-basilaris (Champneys), transversus nuchæ (Gegenbaur). Apparently associated with the former swinging of the body by the fore limb in the arboreal life are the atavistic coraco-brachialis-brevis (Testut), the epitrochleo-dorsalis (Testut), and pectoralis tertius (Testut).§

Centers of variability.—As the literature is so readily accessible I will not multiply illustrations of the innumerable congenital variations

* *Sur les Anomalies Musculaires*, p. 564.

† *Journal of Anatomy and Physiology*, 1890, p. 72.

‡ Or extensor ossis metacarpi pollicis. See Testut, p. 553.

§ Quain describes seventy anomalous muscles (*Ant.*, vol. I.). Testut describes a still larger number.

related to human evolution. I call attention to several important inductions. First, there are several centers in which both the skeletal and muscular systems are highly variable. Second, that the most conspicuous variations, and therefore the most frequently recorded, are reversions. Third, that structure lags far behind function in evolution.

The conclusions of Wood, and of Testut,* are that variability is independent of age or sex, of general muscularity, and of abnormal mental development. Wood found 981 anomalies in 102 subjects; of these, 623 were developed upon both sides of the body, while 358 were unilateral. Of still greater interest are the statistics collected by Wood between 1867–'68 in the dissecting room of King's College, upon 36 subjects (18 of each sex). These show that there are more anomalies in the limbs than in the trunk; that anomalies are rare in the pelvis; that there were 292 anomalies in the anterior limbs to 119 in the posterior; that in both limbs the anomalies increase toward the distal segments, culminating in the muscles of the thumb, where they rise to 90 per cent. (mainly flexor longus pollicis, and abductor longus pollicis). These facts seem to prove conclusively that while variation is universal it rises to a maximum in the centers where human evolution is most rapid; here are Herbert Spencer's conditions of unstable equilibrium. This has a direct bearing, as I shall show, upon our theory of heredity.

Fortuitous congenital variations.—I have thus far considered only those variations which apparently have a definite relation to the course of human evolution. There is an entirely different class of congenital variations which may be described as fortuitous or indefinite because they do not occur in any fixed percentage of cases; they are liable to take any direction; they can not be considered reversional because they are not found in the hypothetical atavus, and there is not sufficient evidence to cause us to consider them as incipient features of our future structure.

Some may not be truly congenital (*i. e.* springing direct from the germ cells) but may be merely deviations from the normal course of development. I may instance the variations in the carpus recorded by Turner† in which the trapezium and scaphoid unite, or the trapezoid and semilunar divide, or the astragalus and navicular unite (Anderson).

The best example of fortuitous congenital variations are seen in supernumerary fingers and vertebræ. The eighth cervical vetebra, bearing a rudimentary rib,‡ is not a reversion because the most remote ancestors of man have but seven cervicals. In cases where a rib is developed upon the seventh cervical, however, the reversion theory is perhaps applicable because rib-bearing cervicals are relatively less re-

* *Sur les Anomalies Musculaires*, p. 760.

† *Journal of Anatomy and Physiology*, 1881, p. 215.

‡ Arb. Lane: *Journal of Anatomy and Physiology*, 1885, p. 266.

mote. The same distinction applies to polydactylism. How absurd it is to consider a sixth finger atavistic, when we remember that even our Permian ancestors had but five fingers.

We can not however class, as purely fortuitous a variation which occurs in a definite percentage of cases presenting twenty four different varieties, but occurring in the same region. Such is the much-discussed* musculus sternalis, a muscle extending vertically over the origin of the pectoralis from the region of the sterno-mastoid to that of the obliquus externus. Testut lightly applies his universal reversion theory, and as this muscle is not found in any mammal considers it a regression to the reptilian presternal (Ophidia)! Turner also considered it as reversional in connection with the panniculus carnosus, the old twitching muscle of the skin, which plays so many freaks of reversion in the scalp and neck: this view is negatived by the fact that this muscle is innervated by the anterior thoracic (Cunningham, Shepherd) which would connect it with the pectorial system, or by the intercostal nerves (Bardeleben). Although the high percentage of recurrence in the sternalis in anencephalous monsters (90 per cent according to Shepherd) supports the reversion view, it is offset by the high percentage (4 per cent.) in normal subjects, for this is far too high for a structure of such age as the reptilian presternal. Cunningham has advanced another hypothesis, first suggested by the frequency of this anomaly in women, that this is a new inspiratory muscle, having its origin in reversion, but serving a useful purpose when it recurs, and therefore likely to be perpetuated.

These fortuitous variations, as well as variations in the proportions of organs, play an important part in the present discussion upon heredity, for it is believed by the Weismann school that such variations, if they chance to be useful, will be accumulated by selection and thus become race characters.

The limits of reversion.—There is such a wide difference of opinion upon the subject of reversions that it is important to determine what are some of the tests of genuine reversions. How shall we distinguish them from indefinite variations or from anomalies like the sternalis muscle, which strain the reversion theory to the breaking point?

Testut,† Duval, and Blanchard take the extreme position that almost all anomalies reproduce earlier normal structures, and that the exceptions may be attributed to the incompleteness of our knowledge of comparative anatomy. I may here observe that popular as the descent theory has recently become in France, neither these anthropologists nor the palæontologists show a very clear conception of the phyletic or branching element in evolution. If they do not find a muscle in the primates they look for it in other orders of mammals. Now, since these other branches diverged from that which gave rise to man at a

* See Turner, Shepherd, and Cunningham: *Journal of Anatomy and Physiology.*
† *Sur les Anomalies Musculaires*, p. 4.

most remote period, the discovery of a similar muscle may be merely a coincidence: it is by no means a proof of reversion.

The first test of reversion is therefore the anatomy of the atavus and this is derived partly from the palæontological record of the primates, partly from the law of divergence, viz., that features which are common to all the living primates were probably also found in the stem form which gave rise to man; finally, from the comparative anatomy of the living anthropoidea.

The second test is whether a structure passes the limits of reversion as determined by cases of atavism in which there can be no reasonable doubt. Two of these phenomena have recently been discussed, which seem to extend the possibilities of reversion back to structures which were lost at a very remote period. I refer to papers by Williams and Howes. Williams * has analyzed 166 recorded cases of polymastism; he finds that supernumerary nipples of some form occur in two per cent, and that in all except four of the cases examined the anomalies, tested by position, etc., support the reversion hypothesis. In the living lemurs, which form a persistent primitive group of monkeys, we find that the transition from polymastism to bimastism is now in progress by the degeneration of the abdominal and inguinal nipples; it is fair to assume that the higher monkeys also lost their abdominal nipples at a primitive stage of development, and therefore that cases of multiple nipples indicate reversion to a Lower Eocene condition! Howes † has recently completed a most interesting study of the "intranarial epiglottis," or cases in which the epiglottis is carried up into the posterior nares, as in young marsupials and some cetacea, to subserve direct narial respiration. This has now been observed to occur by reversion in all orders of mammals, including the monkeys and lemurs. One case has also been reported by Sutton of its occurrence in a human fetus. This is apparently a human reversion to a structure much older than the age of the lemurs.

The third test is the inverse ratio to time. It would seem, *a priori*, that the percentage of recurrence of atavistic structures should decrease as the extent of time elapsing since the structure disappeared increases. This law is apparently established in the case of the condylar and intercondylar foramina, and if we examine all the percentages which have been established we see at once that they bear a ratio to time: compare the relative frequency of the ischio-pubic (50 per cent), dorso-epitrochlearis (5 per cent), and levator-claviculæ (1.66 per cent) muscles with the periods which have elapsed since their past service. This is why it is so important to establish percentages for all our atavistic organs; fuller statistics will not only bear upon heredity, but I can conceive of their application to the extremely difficult problem of estimating geological time. We must, of course, establish as a stand

* *Journal of Anatomy and Physiology*, 1891, p. 224.
† *Ibid*, 1889, p. 587.

ard cases of congenital variation in which the frequency of recurrence has been steadily declining in the same race between two known periods of time—an available structure is the intercondylar foramen or supratrochlear foramen, as recorded by Blanchard, Shepherd, and others.

The reversional tendency is hereditary. There are many cases, both of reversions (as in the teeth) and indefinite variations being hereditary, that is, re-appearing in several generations, or skipping a generation and recurring in the second.

Summary.—There are clearly marked out several regions in the human body in which evolution is relatively most rapid, such as the lower portion of the chest, the upper cervicals, the shoulder girdle in its relation to the trunk, the lower portion of the arm and hand, the outer portion of the foot. We notice that these regions especially are centers of adaptation to new habits of life in which new organs and new relations of parts are being acquired and old organs abandoned.

We observe also that all parts of the body are not equally variable, but these centers of evolution are also the chief centers of variability. The variations here are not exclusively, but mainly, of one kind; they rise from the constant struggle between adaptation and the force of heredity. Here is a muscle like the extensor indicis attempting to give up an old function and establish a new one; it maintains its new function for several generations, and then goes back without any warning to a function which it had thousands of years ago. Thus the force of reversion strikes us as a universal factor.

Now the singular fact about reversion is the frequent proof it affords of what Galton has called "particulate inheritance." When the extensor indicis reverts, all the muscles around it may be normal; therefore we are obliged to consider each of these muscles as a structure by itself, with its own particular history and its own tendencies to develop or degenerate. Thus it is misleading to base our theory of evolution and heredity solely upon entire organs; in the hand and foot we have numerous cases of muscles in close contiguity, one steadily developing, the other steadily degenerating. Reversion very rarely acts upon many structures at once; when it does, we have a case of diffused anomaly, some repetition in the epidermis, or in the entire organism of a lower type.

Yet in spite of reversion and the strong force of repetition in inheritance, the human race is steadily evolving into a new type. We must, it seems to me, admit that an active principle is constantly operating upon these particular structures, guiding them into new lines of adaptation, acting upon widely separate minor parts, or causing two parts, side by side, to evolve in opposite directions, one toward degeneration, the other toward development.

I may now recall the two opposed theories as to what this active principle is:

The first, and oldest, is that individual adaptation, or the tendencies

Partial table of characters in evolution.

General regions of evolution.	Developing organs.	Degenerating organs.				
		Reduction.	Variable.	Rudiments (occasionally absent).	Reversions (from embryo).	Reversions (from germ cells).
Skull and jaw.	Cranium.	Facial sutures.			Premaxillary.	
Spinal curvatures.		Infer. maxilla.				
		Hyoids.	Lumbars and pelvis.	Coracoid.		
Cervicals lumbars, and coccyx.	Female pelvis.			4th and 5th caudals.	5th and 6th caudals.	Caudals [1].
Lower ribs.		8th rib.	12th rib.		13th rib.	7th cerv. ribs.
Scapula and lower humerus.	Scapula.					Condylar for.
	Clavicle.					S. trochlear. for.
Outer side of pes.	Hallux.	4th and 5th digits of pes.	Terminal phalanx of 5th dig. of pes.			3d trochanter.
	Tibia.				Centrale-manus.	
					Intermedium. pes	
Teeth		Canines			? Lateral incisor.	
		Incisors. lateral sup			? Third premolar	4th premolar.
		3d molar.	3d molar.			
Flexors and extensors of arms	Flex. prof. and perf.			Panniculus carnosus		Trans. nuchæ.
	Extensor indicis.					Epitrochl. dors.
	Flex. long. pollicis.					Acromio-basilar.
	Abd. long. pollicis.					Levator claviculæ.
						Pect. tertius.
						Cor. brach. brev.
		Pyramidalis.				Ischio-pubic.
Shoulder to trunk.	Triceps extensor suræ.	Psoas parvus.				Depressores caudæ.
						Scansorius.
Trunk to femur.	Glutæal group	Abd. and add. hallucis.	Muscles, 5th toe.	Plantaris.		Abd. metars. 5th.
	Facial group.			Palmaris.		Peroneus parvus.

[1] It is probable that some of these muscles are represented in the fœtus.

established by use and disuse upon particular structures in the parent are in some degree transmitted to the offspring and thus guide the main course of variation and adaptation.

The second is that all parts of the body are variable, and that wherever variations take a direction favorable (that is, adaptive) to the survival of the parent they tend to be preserved; where they take the opposite direction they tend to be eliminated. Thus, in the long run, adaptive variations are accumulated and a new type is evolved.

It is evident at once, from a glance over the facts brought forward in this lecture, that the first theory is the simplest explanation of these facts; that use and disuse characterizes all the centers of evolution; that changes of structure are slowly following our changes of function or habit.

But while the first explanation is the simplest it by no means follows that it is the true one. In fact, it lands us in many difficulties, so that I shall reserve the pros and cons for my second lecture upon heredity. The Lamarckian theory is a suspiciously simple explanation of such complex processes.

LECTURE II.—THE DIFFICULTIES IN THE HEREDITARY THEORY.

Nur muss ich nochmals betonen, dass nach meiner Auffassung der Anfang einer neuen Reihe erblicher Abweichungen, also auch der Eintritt einer neuen Art ohne eine vorausgegangene erworbene Abweichung undenkbar ist.—VIRCHOW.

State of opinion.—The above quotation from one of the most eminent authorities of our times represents the unshaken conviction of a very large class upon one side of the question of transmission of acquired characters, which is met by equally firm conviction upon the other side.

Herbert Spencer, whose entire system of biology, psychology, and ethics is based upon such transmission, says: "I will only add that, considering the width and depth of the effects which acceptance of one or other of these hypotheses must have on our views of life, mind, morals, and politics, the question which of them is true demands, beyond all other questions whatever, the attention of scientific men."* This shows that Spencer considers the matter still *sub judice*, and lest you may think I am bringing before you an issue in which learning and experience are ranged against ignorance and prejudice, I have taken some pains by correspondence with a number of friends abroad to learn the present state of opinion. The two leading English and French authorities upon this subject express themselves doubtfully.

Galton's mind is still wavering, as in his work of 1889 he says:

"I am unprepared to say more than a few words on the obscure, unsettled, and much-discussed subject of the possibility of transmitting acquired faculties. - - - There is very little direct evidence of its influence in the course of a single generation, if the phrase of 'acquired faculties' is used in perfect strictness and all inheritance is excluded that could be referred to some form of natural selection, or of infection before birth, or of peculiarities of nurture and rearing."†

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* *Nineteenth Century*, 1889.

† *Natural Inheritance*, 1889, p. 14.

Ribot, although in the center of the French Lamarckians, says: "Notwithstanding these facts the transmission of acquired modifications appears to be very limited, even when occuring in both of the parents."

Excepting from Kölliker; His, the Leipsic anatomist; Pflüger, the physiologist; Ziegler, in pathology; and De Vries, in botany, Weismann has not found much sympathy from his own countrymen in his opinion "that acquired characters can not be transmitted; - - - that there are no proofs of such transmission, that its occurrence is theoretically improbable, and that we must attempt to explain the transformation of species without its aid."* Besides Virchow † and Eimer,‡ Haeckel has expressed himself strongly against Weismann. My colleague, Prof. Wilson, writes me (Munich, December 31, 1891) that, while Weismann's modified theories as to the phenomena in the reproductive cells are pretty generally accepted, Hertwig, Hofer, Paully, Boveri, and others are pronounced advocates of the acquired-character-transmission theory.

In Paris Brown-Séquard, who was among the first to test this problem experimentally by observing the inheritance of the effects of nerve lesions, his assistant, Dupuy, Giard, Duval, Blanchard, and others are on the affirmative, or Lamarckian side.

Physiologists generally have fought shy of the question, although I think in the end they will be forced to take it up with the morphologists, and give us the physio-morphological theory of heredity of the future. Prof. Michael Foster, of Cambridge, and Prof. Burdon-Sanderson, of Oxford, both write me that the question has hardly come into the physiological stage of inquiry at all. Yet in England Weismann has found his strongest supporters among some of the naturalists: Wallace, Lankester, Thiselton Dyer, Meldola, Poulton, Howes, and others; while, excepting Windle, the anatomists, including Mivart and Lawson Tait, with Sir William Turner as the most prominent, are all Lamarckians. Huxley, Romanes, and Flower are said to be doubtful. In this country the opinion of naturalists is directly the outgrowth of the class of studies in which each happens to be engaged. So far as I know every vertebrate and invertebrate palæontologist is a Lamarckian,§ for in this field all evolution seems to follow the lines of inherited use and disuse; most of those engaged upon invertebrate zoölogy incline to follow Weismann. I have conversed upon this subject with many physicians, and find that without exception the transmission of acquired characters is an accepted fact among the profession.

Exact statement of the problem.—It is important at the outset to

* *Biologisches Centralblatt*, 1888, pp. 65 and 97.

† Ueber den Transformismus, *Archiv f. Anthropologie*, 1889, p. 1.

‡ Organic Evolution, upon the Law of Inheritance of Acquired Characters. Tübingen, 1888. Trans.

§ See the writings of Hyatt, Cope, Ryder, Dall, Scott, and others.

state most clearly what is and what is not involved in this discussion. Weismann* does not claim that the reproduction or germ cells are uninfluenced by habit; on the other hand, he admits that most important modifications in these cells may and do result from changes of food, climate, from healthy or unhealthy conditions of the body; also from infectious disease, where it is quite as possible that the microbes may enter the reproductive cells as any other cells of the body; from alcoholism, where the normal molecular action of the protoplasm of the germ cells may be disturbed, resulting in abnormal development, and there are some very interesting experiments which I shall cite on this point; from some nervous disorders which profoundly modify cell-function in all the tissues; in other words, *ovum sanum in corpore sano.* But to accept all this, and even to include all our rapidly increasing knowledge of the direct relation between such phenomena as production of deformities and determination of sex, and the influences of environment upon the ovum; or the influences of the mother upon the fœtus—this is all aside from the real question at issue.

It may be stated thus: Given *G*, the ova and spermatozoa, the germ cells or material vehicles of hereditary characters; *S*, the body or somatic cells of all the other tissues conveying the hereditary characters of nerve, muscle, and bone; *V*, the variations in these body cells "acquired" during lifetime; given these factors, the real question is: Do influences at work producing variations in certain body cells of the parent so affect the germ cells of the parent that they re-appear in corresponding body cells of the offspring? To take a concrete case, will the increased use of the cells of the extensor indicis muscle in the parent so stimulate that portion of the germ cells which represents this muscle that the increment of growth will in any degree re-appear in the offspring?

This is what is required of heredity upon the Lamarckian hypothesis, and I think you will see at once that while this hypothesis simplifies the problem of evolution it in a corresponding degree renders more difficult the problem of heredity—for we have not the first ray of knowledge of what such a process involves. There is no quality more essential to scientific progress than common honesty; if we take a position let us face all its consequences; the more we reflect upon it the more serious the Lamarckian position becomes.

In the present lecture let us first briefly review the progress of the science of heredity which has led up to the present discussion. Second, let us examine the evidence for and against the Lamarckian theory, and inquire how far natural selection can explain all the facts of evolution. Third, let us examine the evidence for such a continuous relation between the body cells and germ cells as must exist if the Lamarckian theory is the true one.

History of the heredity theory.—In a valuable summary of the past

* See Essays upon Heredity and Kindred Biological Problems, 1889. Trans.

theories of heredity* J. A. Thompson distinguishes three general problems, which are often confused: 1st. What characters distinguish the germ cells from other cells of the body? 2d. How do the germ cells derive these distinguishing characters? 3d. How shall we interpret "particulate" inheritance, or the re-appearance of single peculiarities in the offspring?

The various theories may be grouped under two heads, "Pangenesis of Germ cells" and "Continuity of Germ cells" according to the dominating idea in each.

1. Pangenesis.—The idea pervading pangenesis was first expressed by Democritus that the "seed" of animals was derived by contributions of material particles from all parts of the bodies of both sexes, and that like parts produced like. Two thousand years later, Buffon revived this conception of heredity in his "Molecules organiques." In 1864 Herbert Spencer suggested the existence of "physiological units," derived from the body cells of the parent, forming the germ cells and then developing into the body cells of the offspring.

It is interesting to note the course of Darwin's thought upon this matter in his published works and in his "Life and Letters." He was at first strongly opposed to the views upon evolution advanced by Buffon, by Erasmus Darwin, his grandfather, expanded by Lamarck, and now known as Lamarckian. But gradually becoming convinced that his own theory of natural selection could not account for all the facts of evolution, he unconsciously became a strong advocate of Lamarck's theory, and contributed to it a feature which Lamarck had entirely omitted, namely, a theory of heredity expressly designed to explain the transmission of acquired characters. Darwin's "provisional hypothesis of pangenesis" † postulated a material connection between the body cells and germ cells by the circulation of minute buds from each cell; each body cell throws off a "gemmule" containing its characteristics, these gemmules multiply and become especially concentrated in the germ cells; in the latter they unite with others like themselves; in course of development they grow into cells like those from which they were originally given off. (See Fig 1, Diagram II.)

Galton, who has always been doubtful in regard to use inheritance, while advancing a theory of "continuity," partly approved Darwin's pangenesis idea in the cautious statement: "Each cell may throw off a few germs that find their way into the circulation and thereby have a chance of entering the germ cells." ‡ At the same time Galton contributed very important experimental disproof of the existence of "gemmules," and in fact—of the popular idea of the circulation of hereditary characters in the blood, by a series of careful experiments upon the

*See *Proc. Roy. Soc. Edin.*, 1888, p. 93.

† See *Animals and Plants under Domestication*, 1875, vol. II, p. 349.

‡ *Contemporary Review*, vol. XXVII, p. 80–95.

transfusion of blood in rabbits; he found that the blood did not convey with it even the slightest tendency to transfer normal characteristics from one variety to another.

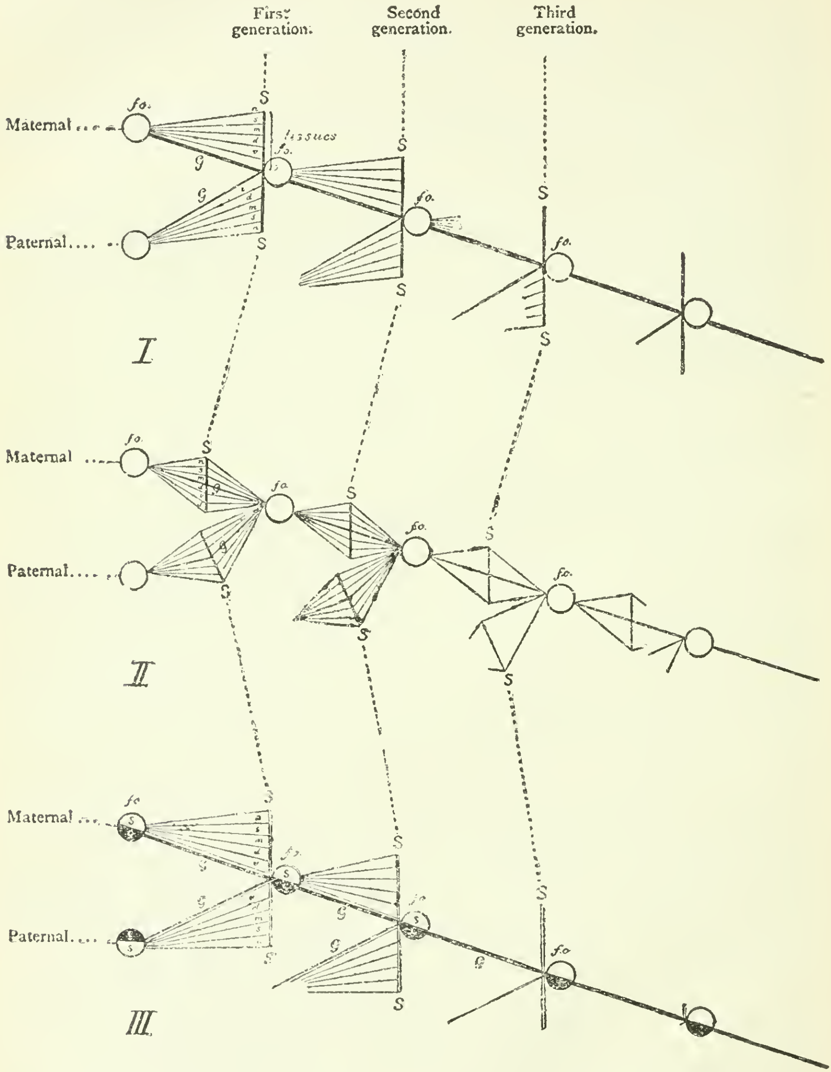

FIG. 1.

f. o., fertilized ovum or *embryo*, containing maternal and paternal characteristics; *S*, soma, or adult body, containing *n, s, m, d, v, somatic cells* of the various tissues; and *G*, *germ cells* of the reproductive glands.

I. HISTOGENESIS.—Showing the successive rise *G*, and union *f. o.* of the maternal and paternal germ cells by direct histogenesis.

II. PANGENESIS.—Showing the tissues of the body *S*, contributing to the germ cells *G*, so that each *f. o.* is composed of elements from both the somatic and germ cells.

III. CONTINUITY.—Showing the division of the embryo *f. o.*, into somatoplasm, *s* (from which arise the body cells), and germ plasm, *G* (which passes direct to the germ cells), establishing a direct continuity.

Prof. Brooks, of the Johns Hopkins University, then contribute and original modification of pangenesis in which the functions of the ova and spermatozoa were sharply differentiated.* (1) He regarded the ovum as a cell especially designed as a storehouse of hereditary characteristics, each characteristic being represented by material particles of some kind; thus hereditary characters were handed down by simple cell division, each fertilized ovum giving rise to the body cells in which its hereditary characters were manifested and to new ova in which these characters were conserved for the next generation (this portion of Brooks's theory is very similar to Galton's and Weismann's). (2) The body cells have the power of throwing off "gemmules," but this is exercised mainly or exclusively when its normal functions are disturbed, as in metatrophic exercise or under change of environment. (3) These gemmules may enter the ovum, but the spermatozoan is their main center. According to this view the female cell is rather conservative and the male cell progressive; the union of these cells produces variability in the offspring, exhibited especially in the regions of the offspring corresponding to the regions of functional disturbance in the parent. This hypothesis was well considered, and while that feature of it which distinguishes the male and female germ cells as different in kind has been disproved, and the whole conception of gemmules is now abandoned, the fact still remains that we shall nevertheless be obliged to offer some hypothesis to explain the facts disregarded by Weismann for which Brooks provides in his theory of the causes of variation.

2. Continuity of germ cells.—The central idea here is an outgrowth of our more modern knowledge of embryogenesis and histogenesis, and is therefore comparatively recent; it is that of a fundamental distinction between the "germ cells," as continuous and belonging to the race, and the "body cells," as belonging to the individual. Weismann has refined and elaborated this idea, but it was not original with him.

Richard Owen,† in 1849, Haeckel,‡ in 1866, Rauber,§ in 1879, in turn dwelt upon the distinction which Dr. Jaeger, now of manufacturing fame, first clearly stated:

"Through a great series of generations the germinal protoplasm retains its specific properties, dividing in every reproduction into an ontogenetic portion, out of which the individual is built up, and a phylogenetic portion, which is reserved to form the reproductive material of the mature offspring. This reservation of the phylogenetic material I described as the continuity of the germ protoplasm. - - - Encapsuled in the ontogenetic material the phylogenetic protoplasm is sheltered from external influences, and retains its specific and embryonic characters." The latter idea has, under Weismann, been expanded into the theory of isolation of the germ cells.

Galton introduced the term "stirp" to express the sum total of

* *The Law of Heredity*, 1883.

† See Parthenogenesis, in his *Anatomy of Vertebrates*.

‡ *Generelle Morphologie*, vol. II, p. 170.

§ *Zool. Anz.*, vol. IX, p. 166.

hereditary organic units contained in the fertilized ovum. His conception of heredity was derived from the study of man, and he supported the idea of continuity in the germ cells in order to account for the law of transmission of "latent" characters; it is evident from this law that only a part of the organic units of the "stirp" become "patent" in the individual body; some are retained latent in the germ cells, and become patent only in the next or some succeeding generation. For example, the genius for natural science was "patent" in Erasmus Darwin, grandfather of the great naturalist, it was "latent" in his son, and re-appeared intensified in his grandson, Charles Darwin. I have elsewhere* summed up as follows Galton's general results, which so remarkably strengthen the "continuity" idea: We are made up, bit by bit, of inherited structures, like a new building, composed of fragments of an old one, one element from this progenitor, another from that, although such elements are usually transmitted in groups. The hereditary congenital constitution thus made up is far stronger than the influences of environment and habit upon it. A large portion of our heritage is unused, for we transmit peculiarities we ourselves do not exhibit. The contributions from each ancestor can be estimated in numerical proportions, which have been exactly determined from statistics of stature in the English race; thus the contributions from the "patent" stature of the two parents together constitute one-half while the contributions by "latent" heritage from the grandparents constitute one-sixteenth, etc. One of the most important demonstrations by Galton is the law of regression; this is the factor of stability in race type which acts as gravitation does upon the pendulum; if an individual or a family swing far from the average characteristics of their race, and display exceptional physical or mental qualities, the principle or regression in heredity tends to draw their offspring back to the average.

Now how shall we distinguish regression from reversion? Very clearly, I think; regression is the short pull which tends to draw every variation and the individual as a whole back to the contemporary typical form, while reversion is the long pull which draws the typical form of one generation back to the typical form of a very much earlier generation. These forces are evidently akin, and in the shades of transition from one type to another we would undoubtedly find a constant diminution numerically in the recurrence of characters of the older type, and thus "regression" would pass insensibly into "reversion."

Weismann has carried the idea of continuity to its extreme in his simple and beautiful theory of heredity, which is founded upon the postulate that there is a distinct form of protoplasm, with definite chemical and molecular properties, set apart as the vehicle of inheritance; this is the germ plasm, *G*, quite separate from the protoplasm of the body cells or somatoplasm, *S*. Congenital characters arising in

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* *Atlantic Monthly*, March, 1891, p. 359.

the germ-cells are called blastogenetic, while acquired characters arising in the body cells are somatogenetic.

To clearly understand this view, let us follow the history of the fertilized ovum in the formation of the embryo. It first divides into somato-plasm and germ-plasm (see Fig. 1, Diagram III), the former supplies all the tissues of the body—*n*, *s*, *m*, *d*, *v*, nervous, muscular, vascular, digestive, etc.—with their quota of hereditary structure; the residual germ-plasm is kept distinct throughout the early process of embryonic cell division until it enters into the formation of the nuclei of the reproductive cells, the ova or spermatozoa. Here it is isolated from changes of function in the somato-plasm, and in common with all other protoplasm is capable of unlimited growth by cell division without loss or deterioration of its past store of hereditary properties; these properties are lodged in the nucleus of each ovum and spermatozoan, and these two cells, although widely different in external accessory structure (because they have to play an active and passive part in the act of conjugation), are exactly the same in their essential molecular structure, and the ancestral characters they convey differ only because they come along two different lines of descent. When these cells unite they carry the germ-plasm into the body of another individual. Thus the somato-plasm of each individual dies, while the germ-plasm is immortal; it simply shifts its abode from one generation to another; it constitutes the chain from which the individuals are mere offshoots. Thus the germ-plasm of man is continuous with that of all ancestors in his line of descent, and we have an explanation of the early stages observed in development in which the human embryo passes through a succession of metamorphoses resembling the adult forms of lower types.

In order to emphasize, as it were, the passage of the germ-plasm from one generation to another without deterioration in its marvellous hereditary powers, Weismann added the idea of its isolation. Not only does he repudiate the pangenesis notion of increment of germ-plasm by addition of gemmules, but he believes that it is unaffected by any of the normal changes in the somatic or body cells. As this continuity and isolation would render impossible the transmission of characters acquired by the somato-plasm, Weismann began to examine the evidence for such transmission, and coming to the conclusion that it was insufficient, in his notable essay on "Heredity," in 1883, he boldly atteсked the whole Lamarckian theory and has continued to do so in all his subsequent essays.

Being forced to explain evolution without this factor, he claimed that variation in the germ plasm was constanly arising by the union of plasmata from different lines of descent in fertilization, and that these variations are constantly being acted upon by natural selection to produce new types. He thus revived Darwin's earlier views of evolution, and this in part expiains his strong support by English naturalists.

It will be seen at once that there are a number of distinct questions involved.

The matter of first importance in life is the repetition and preservation of type, the principle which insures the unerring accuracy and precision with which complex organs are built up from the germ cells; the force of regression and the more remote forces of reversion all work in this conservative direction; the theory of the preservation of these forces in a specific and continuous form of protoplasm is by far the most plausible we can offer at present.

The matter of second importance, but equally vital to the preservation of races, in the long run, is the formation of new types adapted to new circumstances of life. I shall now attempt to show that the facts of evolution, while not inconsistent with the idea of continuity of the germ plasm, are wholly at variance with the idea of its independence, separation, or isolation from the functions of the body. This can be done by proving, first, that the theory of evolution solely by natural selection of chance favorable variations in the germ plasm is inadequate; second, that the inheritance of definite changes in the somatic cells is also necessary to explain evolution, and therefore there must exist some form of force or matter which connects the activities of the somatoplasm with those of the germ plasm.

In the following table are placed some of the facts of human evolution which we have observed in the first lecture, and as they are part of inheritance, they also constitute the main external phenomena of heredity:

Phenomena of heredity.

Conservative (toward past type).	Natural.	Progressive (toward future type).
a. Repetition of parental type. *b*. Regression (in many characters) to contemporary race type.	Fortuitous and indefinite.	*a*. Definite variation in single characters, by accumulation =. *b*. Definite variation in many characters (from contemporary race type).
c. Reversion (mainly in single characters) to past race type.	Variability.	

What are causes of these various phenomena?

Factors of evolution.—The term "kinetogenesis" has been applied to the modern form of the Lamarckian theory, for it is an application of kinetic or mechanical principles to the origin of all structures such as teeth, bone, and muscle. It would be fatal to this theory if it could be shown that the changes taking place in course of a normal individual life, under the laws of use and disuse, are inadaptive, or do not correspond to those observed in the evolution of the race.

The relative growth of Organs.—Ball,* in his long argument against Lamarckianism, claims that such is the case, and that use

* *Op. cit.*, p. 129.

inheritance would be an actual evil: "Bones would often be modified disastrously. Thus the condyle of the human jaw would become larger than the body of the jaw, because as the fulcrum of the lever it receives more pressure. Some organs (like the heart, which is always at work) would become inconveniently or unnecessarily large. Other absolutely indispensable organs which are comparatively passive or are very seldom used would dwindle until their weakness caused the ruin of the individual or the extinction of the species." He later cites from Darwin* the "Report of the United States Commission upon the Soldiers and Sailors of the Late War," that the longer legs and shorter arms of the sailors are the reverse of what should result from the decreased use of the legs in walking and increased use of the arms in pulling. A little reflection on Mr. Ball's part would have spared us this crude objection, for whatever difficulties may arise from theoretical speculation as to the laws of growth, or from statistics, the fact remains that activity must increase adaptation in every part of the organism; otherwise the runner and the trotting horse should be kept off the track to increase their speed, the pianist should employ as little finger exercise as possible. If the growth tendencies in single organs are transmitted, it is evident that the adaptive adjustments between these tendencies will also be transmitted.

The Feet.—In point of mechanical adaptation, man, with the single exception of his thumb and forearm, has not progressed beyond the most primitive eocene quadruped. The laws of evolution of the foot in the ungulate or hoofed animals, which have been especially studied by Kowalevsky, Ryder, Cope, and myself, affords a conclusive demonstration that the skeletal changes in the individual coincide with those which will mark the evolution of the race. In the earliest ungulates the carpals and tarsals are disposed, as in man, directly above each other, with serial joints, as in diagram *A*, Fig. 2; in the course of evolution all these joints became interlocking, as in diagram *B*, Fig. 3; thus producing an alternation of joints and surfaces similar to those which give strength to masonry. In studying these facts Cope† reached a certain theory as to the motion of the foot and leg in locomotion. In trying to apply this, I found it could not be harmonized with all the facts, and I worked out an entirely different theory.‡ This I found subsequently coincided exactly with the results previously obtained by Muybridge, by the aid of instantaneous photographs, and summarized by Prof. Harrison Allen, of the University of Pennsylvania.§

The monodactylism of the horse was attained by the atrophy of the

* *Descent of Man*, p. 32.

† *American Naturalist*, 1887, p. 986.

‡ See *Trans. of American Philosophical Society*, 1889, p. 561. Philadelphia.

§ The Muybridge Work at the University of Pennsylvania. Philadelphia, 1888.

lateral toes, and concentration of the major axis of body weight and strain upon the middle finger and toe. Man is also tending toward monodactylism in the foot by the establishment of the major axis through the large toe and atrophy of the outer toes. The present atrophy of our small toe is as good a parallel as we can find of the changes which were occurring in the eocene period among the ancestors of the horse.

The Teeth.—But how about the teeth, in which there is an absolute loss of tissue in consequence of use? This is another objection raised by Ball, Poulton, and others, which disappears upon examination.

The dental tissues, while the hardest in the body, and, unlike bone, incapable of self-repair, are not only both living and sensitive, but, to

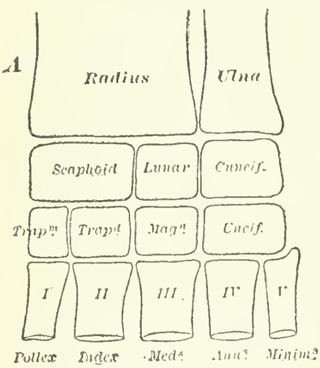


FIG. 2.

PRIMITIVE UNGULATE FOOT.—Lines of vertical cleavage on either side of the middle toe, III. Spreading of toes would cause separation of the carpals.

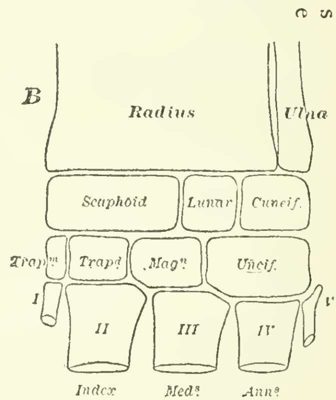


FIG. 3.

RECENT UNGULATE FOOT.—No lines of vertical cleavage. All joints broken by enlargement of scaphoid, unciform, and radius, the bones receiving greatest impact in walking. Lateral toes, I, V, degenerate.

a very limited degree, plastic and capable of change of form. *Ex hypothesi*, it is not the growth, but the reaction tendency which produces the growth, which is transmitted. The evolution of the teeth, therefore, falls into the same category as bone.* In the accompanying figures I have epitomized the slow transformation of the single-fanged conical reptilian tooth, such as we see in the serpents, into the low-crowned human grinder. We now know all the transition forms, so that we can homologize each of the cusps of the human molar with its varied ancestral forms in the line of descent. For example, the anterior lingual or inner cusp of the upper true molars traces its pedigree back to the reptilian cone. The anterior triangle of cusps, or trigon, seen in the mesozoic mammalia and persisting in the first inferior true molar of the modern dog, is still seen as the main portion of the crown of the human upper molars (*pr*, *pa*, *me*). To this was added, ages ago, the

* See especially the papers of Ryder, Cope, and the writer, "Evolution of Mammalian Molars to and from the Tritubercular Type," *American Naturalist*, 1889.

posterior lingual cusp, or hypocone, which, as Cope has shown, is exhibited in various degrees of development in different races and is an

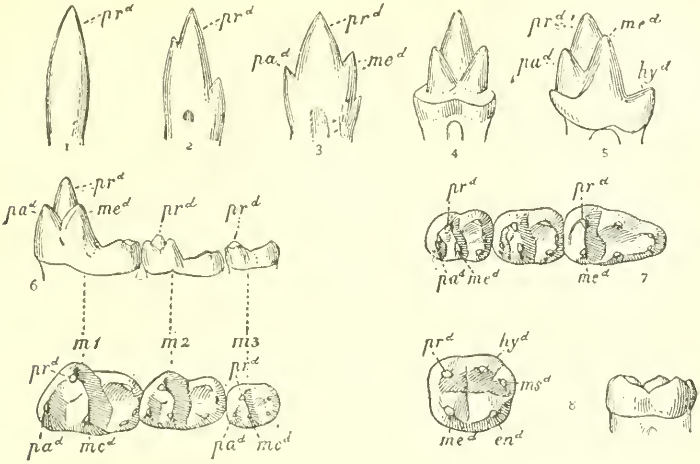
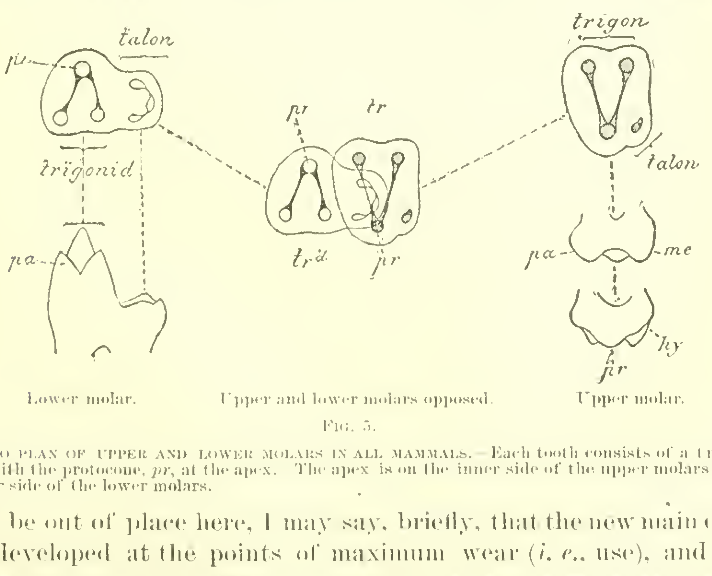


FIG. 4.

EVOLUTION OF THE CUSPS OF THE HUMAN LOWER MOLAR.—pr^d, protoconid (anterior buccal cusp); pa^d, paraconid; me^d, metaconid (anterior lingual cusp); hy^d, hypoconid (posterior buccal); en^d, entoconid (posterior lingual cusp); ms^d, mesoconulid (intermediate cusp). Diagram 1.—Reptilian stage. Diagrams 2–5.—Mesozoic mammals, first lower molars showing rise of ancestral cusps. Diagram 6.—Eocene carnivore (*miacis*), showing how the low tubercular crown *m3* is derived from the high crown *m1*. Diagram 7.—Eocene monkey (*Anaptomorphus*), showing how the primitive anterior lingual cusp pa^d disappears. Diagram 8.—Human first molar with its ancestral cusps.

important race index.* A glance through the diagrams shows that the development of the crown has been by the successive addition of new cusps. Without entering upon the details of evidence, which



Lower molar. Upper and lower molars opposed. Upper molar.

FIG. 5.

KEY TO PLAN OF UPPER AND LOWER MOLARS IN ALL MAMMALS.—Each tooth consists of a triangle, *trigon*, with the protocone, *pr*, at the apex. The apex is on the inner side of the upper molars and on the outer side of the lower molars.

would be out of place here, I may say, briefly, that the new main cusps have developed at the points of maximum wear (*i. e.*, use), and con-

* The upper molars in many Esquimaux are triangular (as in Fig. 6, diagram 11); in most negroes they are square (diagram 12). In our race they are intermediate.

versely in the degeneration of the crown, disuse foreshadows atrophy and disappearance.

Upon the whole, with some exceptions which we do not at present understand, the course of evolution of the teeth supports the evidence derived from the skeleton that, whether any true causal relation has existed or not, the lines of individual transformation in the whole fossil series preceded those of race transformation.

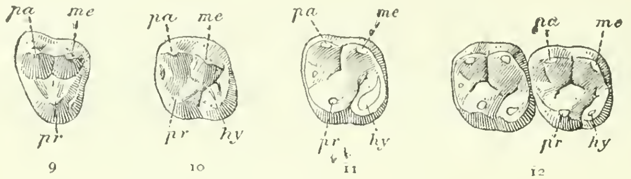


FIG. 6.

EVOLUTION OF THE HUMAN UPPER MOLARS.—Diagram 9.—*Anaptomorphus*, a Lower Eocene monkey. Diagram 10.—An Upper Eocene monkey. Diagrams 11 and 12.—Human: 11, Esquimaux; 12, negro. See addition of "talon," *hy*, to "trigon" composed of *pa. pr. me.*

The rise of new organs.—We owe to Dr. Arbuthnot Lane a most interesting series of studies upon the influences of various occupations upon the human body. He proves conclusively that individual adaptation not only produces profound modifications in the proportions of the various parts, but gives rise to entirely new structures.

His anatomy and physiology of a shoemaker* shows that the life-long habits of this laborious trade produce a distinct type, which if examined by any zoölogical standard would be unhesitatingly pronounced a new species—*homo sartorius*. The psychological analysis which a Dickens or Balzac would draw, showing the influences of the struggle for existence upon the spirit of this little tailor, could not be more pathetic than Dr. Lane's analysis of his body. The bent form, crossed legs, thumb and forefinger action, and peculiar jerk of the head while drawing the thread, are the main features of sartorial habit. The following are only a few of the results: The muscles tended to recede into tendons, and the bony surfaces into which they were inserted tended to grow in the direction of the traction which the muscle exerted upon them. The articulation between the sternum and the clavicle was converted into a very complex arthrodial joint, constituting almost a ginglymoid articulation. The sixth pair of ribs were anchylosed to the bodies of the vertebræ, indicating that they had ceased to rise and fall with sternal breathing, and that respiration was almost exclusively diaphragmatic. The region of the head and first two vertebræ of the neck was still more striking: the transverse process of the right side of the atlas, toward which the head was bent, formed a new articulation with the under surface of the jugular process of the occipital bone, "a small synovial cavity surrounded this acquired articulation, but there was no appearance of a

Journal of Anatomy and Physiology, 1888, p. 595.

capsular ligament;" the left half of the axis was united by bone to the corresponding portion of the third cervical; there was found a new upward prolongation of the odontoid peg of the axis, and a new accessory transverse ligament to keep it from pressing upon the cord. In short, "the anatomy of the shoemaker represents the fixation and subsequent exaggeration of the position and tendencies to change which were present in his body when he assumed the position for a short period of time.

Rate of inheritance.—This illustration serves also to emphasize the great contrast between the rapidity of individual transformation and the slowness of race transformation. No one would expect the son of this shoemaker to exhibit any of these acquired malformations. Yet Dr. Lane thinks he has observed such effects in the third generation by the summation of similar influences.

All palæontological evidence goes to show that the effects of normal habits, if transmitted at all, would be entirely imperceptible in one generation. The horse, for example, has not yet completely lost the lateral toes which became useless at the end of the Upper Eocene period. This objection as to rate of evolution may be urged with equal force against the natural selection theory. It is obvious that the active progressive principle in evolution (whatever it is), must contend with the enormous conservative power of inheritance, and this, to my mind, is one of the strongest arguments against the possibilities of the rise of adaptive organs by the selection of chance favorable variations in the germ plasm.

Application to human evolution.—Principles underlying these illustrations may now be applied to some of the facts in human evolution brought out in the first lecture. They show that if functional tendencies are transmitted we can comprehend the distinct evolution history of each organ; the rise and fall of two organs side by side; the definite and purposive character of some anomalies; the increase of variability in the regions of most rapid evolution; the correlation of development balance, and degeneration in the separate organs of the shoulder, hand, and foot.

Yet even granting this theory there still remain difficulties. The relation of use and disuse to some of the contemporary changes in the human backbone is rather obscure. I would hesitate to pronounce an opinion as to whether our present habits of life are tending to shorten the lumbars, increase the spinal curvatures, and shift the pelvis without making an exhaustive study of human motion. Among the influences which Dr. Lane has suggested* as operative here are the wearing of heeled shoes and the increase of the cranium. He considers the additional or sixth lumbar vertebra as a new element rather than as a reversion, and works out in some detail the mechanical effects of the

**Journal of Anatomy and Physiology*, 1888, p. 219.

presence of the fetus upon female respiration (*i. e.*, in the sternal region) and upon the pelvis. Now, if it be true that the pelvis is larger in the higher races than in the lower, I do not think that Dr. Lane can sustain his point, because in the lower races the fetus is carried for an equally long period, during a much more active life, and in a more continuously erect position. Therefore, if these mechanical principles were operating, the pelvis in the modern lower races should be larger than in the higher. On the other hand, the form of the female pelvis in the higher races is one of the best established selecting or eliminating factors, a large pelvis favoring frequent births and the preservation of those family stirps in which it occurs. I mention this to show how cautious we must be in jumping to conclusions as to kinetogenesis.

The transformism in all the external features of the skull, jaws, and teeth may be attributed to inherited tendencies toward hypertrophy or atrophy; but how about the convolutions of the turbinal bones or the complex development of the semicircular canals and cochlea of the internal ear and the many centers of evolution which are beyond the influences of use and disuse? These are examples of structures which fortify Weismann's contention, for if complex organs of this character can only be accounted for by natural selection, why consider selection inadequate to account for all the changes in the body?

Difficulties in the natural-selection theory.—The answer, I think, is readily given: We do not know whether use and disuse are operating upon the mechanical construction of the ear; we do know that the organ can be rendered far more acute by exercise; but even if it were true that habit can exert no formative influence, the ear is one of those structures which since its first origin has been an important factor in survival and may therefore have been evolved by natural selection. Now, the very fact that selection may have to care for variations in such prime factors in survival as the ear, renders it the more difficult to conceive that it also is nursing the minutiæ of variation in remote, obscure, and uncorrelated organs.

Even in the brief review of human evolution in the first lecture I have pointed out eight independent regions of evolution, upward of twenty developing organs, upward of thirty degenerating organs. A more exhaustive analysis would increase this list tenfold. Now, where chance variation should produce an increase in size in all the developing organs, and a decrease in size of all the degenerating organs, and an average size in all the static organs, we would have all the conditions favoring survival. But the chances are infinity to one against such a combination occurring unless the tendencies of variation are regulated and determined, as Lamarckians suppose, by the inheritance of individual tendencies. But may not the favorable variations in the body be grouped to either out-weigh or under-weigh the unfavorable variations? This would be possible if combinations occurred; but we can readily see that combinations, such as we observe in the separate elements of the foot alone, completely neutralize each other so far as "survival" is concerned; how the foot would neutralize the hand, or the foot and hand would neutralize the lumbar region.*

It is this consideration of single organs, the observation of their independent history, the rise of new compound organs by steady growth from infinitesimal beginnings of their separate elements, the combined testimony of anatomy and palaeontology which force us to regard the theory of evolution by the natural selection of chance variations as wholly untenable. With the utmost desire to regard the discussion in as fair a spirit as possible, the explanations offered by the adherents of Weismann's doctrine strike me as strained, evasive, and illogical.†

We can however by no means undervalue or dispense with natural selection, which must be in continuous operation upon every character of sufficient importance to weigh in the scale of survival. I need hardly remind you that this selecting principle was first discovered in 1813 by Dr. W. C. Wells, of Charleston, in connection with the immunity from certain tropical diseases enjoyed by negroes and mulattoes.‡

The eliminating factor in selection is illustrated almost daily in cases of *appendicitis*. I regret I have not had time to ascertain whether or not this disease is considered due purely to accident or to congenital variation in the aperture of the appendix, which favors the admission of hard objects. If so, modern surgery is only benefiting the individual to the detriment of the race by its efficient preventive operations; and every individual who succumbs to this disease can reflect with melancholy satisfaction that he does so *pro bono publico*.

Conclusions as to the factors of evolution.—The conclusions we reach from the study of the muscular and skeletal systems are therefore as follows: 1. That individual transformism in the body is the main determinant of variations in the germ cells, and is therefore the main cause of definite progressive or retrogressive variations in single organs. 2. That evolution in these organs is hastened where all members of the race are subject to the same individual transformism. The contrast between the rate of individual transformism and race transformism is due to the strong conservative forces of the germ plasma. 3. That evolution is most rapid where variations are of sufficient rank to become factors in survival. Then selection and use inheritance unite forces as active progressive principles opposing the conservative principle in the germ plasma. 4. That fortuitous and chance variations also arise from disturbances in the body or germ cells; they may be perpetuated, or disappear in succeeding generations.

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*I have expanded this idea fully in recent papers upon the theory of evolution of the horse. See "Are Acquired Variations Inherited?" *American Naturalist*, February, 1891.

†See Weismann's last essay, "Retrogressive Development," in *Nature*, Biol. Mem., trans., in press.

‡See Introduction of Darwin's *Origin of Species*.

Applying these views to variation there should theoretically appear to be just those two distinct classes of anomalies in the human body which we have seen actually occurring: First, those in the path of evolution, arising from perfectly normal changes in the somato-plasm and germ-plasm: second, those wholly unconnected with the course of evolution, arising fortuitously or from abnormal changes in the somato-plasm or germ-plasm; to this head may be attributed the whole scale of deformities. Thus tranformism and deformism should be kept distinct in our minds. Nevertheless the facts of deformism contribute the strongest body of evidence which we can muster at present to prove that there does exist a relation between the somato-plasm and germ-plasm which renders transformism possible.

The relations between the somato-plasm and germ-plasm.—We have seen reasons to take a middle ground as to the distinct specific nature of the body cells and germ cells, and this position is, I think, strengthened the more broadly we extend our inquiry into all the fields of protoplasmic activity.

There are three questions before us.

1. What is the evidence that the germ-plasm and somato-plasm are distinct?

2. What is the specific nature of the germ-plasm?

3. What is the nature of the relations which exist between the two?

1. The separation of the germ-plasm is in the regular order of evolution upon the principles of physiological division of labor. The unicellular organisms combine all the functions of life in a single mass of protoplasm, that is, in one cell. In the rise of the multi-cellular organisms the various functions are distributed into groups of cells, which specialize in the perfecting of a single function. Thus the reproductive cells fall into the natural order of histogenesis, and the theory of their entire separation is more consistent with the laws governing the other tissues than the theory which we find ourselves obliged to adopt, that while separate they are still united by some unknown threads with the other cells.

The morphological separation of what we may call the race protoplasm becomes more and more sharply defined in the ascending scale of organisms. Weismann's contention as to the absolutely distinct specific nature of the germ-plasm and somato-plasm has however to meet the apparently insuperable difficulty that in many multi-cellular organisms, even of a high order, the potential capacity of repeating complex hereditary characters, and even of producing perfect germ cells, is widely distributed through the tissues.

For example, cuttings from the leaves of the well-known hot-house plant, the begonia, or portions of the stems of the common willow tree, are capable of reproducing complete new individuals. This would indicate either that portions of the germ plasm are distributed through the tissus of these organisms, or that each body cell has retained its potential quota of hereditary characters.

Among the lower animals we find the same power; if we cut a hydra or bell animalcule into a dozen pieces, each may reproduce a perfect new individual. As we ascend in the animal scale the power is confined to the reproduction of a lost part in the process known as recrescence. As you well know, in the group to which the frog and salamander belong, a limb or tail, or even a lower jaw, may be reproduced. The only logical interpretation of these phenomena is that the hereditary powers are distributed in the entire protoplasm of the organism, and the capacity of reproduction is not exhausted in the original formation of the limb, but is capable of being repeated. There has been considerable discussion of late as to the seat of this power of recrescence. It seems to me not impossible that in the vertebrates it may be stored in the germ cells, and it would be very interesting to ascertain experimentally whether removal of these cells would in any way limit or affect this power: we know that such removal in castration or ovariotomy sometimes profoundly modifies the entire nature of the organism, causing male characters to appear in the female, and female characters to develop in the male.

So far as man is concerned it has been claimed by surgeons that genuine recrescence sometimes occurs; for example, that a new head is formed upon the femur after exsection; but my friend Dr. V. P. Gibney informs me that this is an exaggeration, that there is no tendency to reproduce a true head, but that a pseudo-head is formed, which may be explained upon the principle of regeneration and individual transformism by use of the limb.

Pflüger's opinion is that recrescence does not indicate a storage of hereditary power, that there is no pre-existing germ of the member, but that the re-growth is due to the organizing and distributing power of the cells at the exposed surface, so that, as new formative matter arrives, it is built up gradually into the limb. This view would reduce re-crescence to the level of the regeneration process which unites two cut sections of the elements of a limb in their former order. It is partly opposed to the facts above referred to, which seem to prove the distribution of the hereditary power. Yet it seems to me quite consistent to consider these three processes—*a*, reproduction of a new individual from every part; *b*, recrescence of a new member from any part; *c*, regeneration of lost tissues—as three steps indicating the gradual, but not entire withdrawal of the reproductive power into the germ cells.

I have not space to consider all the grounds which support the view of the separation of the germ cells in man. Some of the more prominent are: the very early differentiation of these cells in the embryo, observed with a few exceptions in all the lower orders of animals, and advancing so rapidly in the human female that several months before birth the number of primordial ova is estimated at seventy thousand, and is not believed to be increased after the age of two and a half years. The most patent practical proof is that we may remove every portion of the body which is not essential to life and yet the power of complete reproduction of a new individual from the germ cells are unimpaired. Among the many reasons advanced for pensioning the crippled soldiers of our late war you never hear it urged that their children are incapacitated by inheritance of injuries. The strongest proof however rests in the evidence I have already cited from heredity of the extraordinary stability of the germ cells, which is the safeguard of the race.

2. The specific nature of the germ-plasm must be considered before we consider its relations. Wherein lies the conservative power of the germ-plasm, and in what direction shall we look for its transforming forces? You see at once that marvellous as is the growth of cells in other tissues, the growth of the germ cell is still more so.

We find it utterly impossible to form any conception of the contents of the microcosmic nucleus of the human fertilized ovum, which is less than one twenty-five-hundredths of an inch in diameter, but which is nevertheless capable of producing hundreds of thousands of cells like itself, as well as all the unlike cells of the adult organism. We can only translate our ideas as to the possible contents of this nucleus in the terms of chemistry and physics.*

Spencer† assumed an order of molecules or units of protoplasm lower in degree than the visible cell units, to the internal or polar forces of which, and their modification by external agencies and inter-action, he ascribes the ultimate responsibility in reproduction, heredity, and adaptation. This idea of biological units seems to me an essential part of any theory; it is embodied in Darwin's "gemmules," in Haeckel's "plastidules;" yet, as Lankester says the rapid accumulation of bulk is a theoretical difficulty in the material conception of units. In the direction of establishing some analogy between the repetition power of heredity and known function of protoplasm, Haeckel‡ and Hering§ have likened heredity to memory, and advanced the hypothesis of persistence of certain undulatory movements; the undulations being susceptible of change. and therefore of producing variability, while their tendency to persist in their established harmony is the basis of heredity. Berthold, Gautier, and Geddes‖ have speculated in the elaboration of the idea of metabolism: the former holding the view that "inheritance is possible only upon the basis of the fundamental fact that in the chemical processes of the organism the same substances and mixtures of substances are reproduced in quantity and quality with regular periodicity."¶

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* See Ray Lankester, *Nature*, July 15, 1876.

† *Principles of Biology*, vol. i., p. 256.

‡ *Perigenesis der Plastidule oder die Wellenzeugung der Lebenstheilchen.* Jena, 1875.

§ *Ueber d. Gedächtniss als ein vallgemeine Function d. organischen Materie.* Vienna, 1870.

‖ See also Thomson, *op. cit.*, p. 102.

¶ Berthold: *Studien über Protoplasma-Mechanik.* Leipsic, 1886.

I have merely touched upon these speculations to show that the unknown factors in heredity are also the unknown factors in operation in living matter. All we can study is the external form, and conjecture that this form represents matter arranged in a certain way by forces peculiar to the organism. These forces are exhibited or patent in the somatic cells; they are potential or latent in the germ cells.

The last stage of our inquiry is as to the mode in which the action of habit or environment upon the somatic cells can be brought to bear upon the germ cells.

The nature of the relation between the body cells and germ cells.—I have already shown that we are forced to infer that such a relation exists by the facts of evolution, although these facts show that the transmission of normal tendencies from the body to the germ cells is ordinarily an extremely slow process.

Virchow* says every variation in race character is to be traced back to the pathological condition of the originator. All that is pathological is not diseased, and inheritance of a variation is not from the influence upon one individual necessarily, but upon a row of individuals. This is in the normal condition of things. In the abnormal condition the rate of transmission may be accelerated.

Does this transmission depend upon an interchange of material particles, or upon an interchange of forces, or both?

There are three phenomena about which there is much skepticism, to say the least, which bear upon the question of a possible interchange of forces between the body and the germ-cells. These are the inheritance of mutilations, the influence of previous fertilization, and the influence of maternal impressions. They are all in the quasi-scientific realm, which embraces such mental phenomena as telepathy. That is, we incline to deny them simply because we can not explain them.

Mutilations.—Since the publication of Weismann's essays the subject of inherited mutilations has attracted renewed interest. I would first call attention to the fact that this matter has only an indirect bearing, for a mutilation is something impressed upon the organism from without: it is not truly "acquired;" the loss of a part by accident produces a sudden but a less profound internal modification of the organism than the loss of a part by degeneration. Most of the results are negative; many of the so-called "certain" cases prove upon investigation to be mere coincidences. Weismann† himself experimented upon white mice, and showed that 901 young were produced by five generations of artificially mutilated parents, and yet there was not a single example of a rudimentary tail or of any other abnormality in this organ. The cases of cleft ear lobule have recently been summed up.‡ Israel reports two cases of clefts in which the parent's ears were normal. Schmidt and

* "Ueber den Transformismus," *Archiv f. Anthropologie*, 1888, p. 1.

† *Biological Memoirs*, p. 432.

‡ *Journal of Anatomy and Physiology*, 1891, p. 135.

Ornstein report affirmative cases. His shows that an affirmative case, cited by V. Zwieciki, is merely an inherited peculiarity. The entire evidence is unsatisfactory, and upon the whole, is decidedly negative.

Not so however in cases where the mutilation results in a general disturbance of the normal functions of different organs, as in the experiments conducted by Brown-Séquard* upon guinea-pigs, in which we see "acquired variation" intensified. In these, abnormal degeneration of the toes, muscular atrophy of the thigh, epilepsy, exophthalmia, etc., appeared in the descendants of animals in which the spinal cord or sciatic nerve had been severed, or portions of the brain removed. It was also shown that the female is more apt to transmit morbid states than the male; that the inheritance of these injuries may pass over one generation and re-appear in the second; that the transmission by heredity of these pathological results may continue for five or six generations, when the normal structure of the organs re-appears. These cases, which are incontestable, at first sight appear to establish firmly the transmission of acquired characters; they were so regarded by Brown-Séquard. These lesions act directly upon the organs, and the abnormal growth of these organs appears to be transmitted. But can they not be interpreted in another way, namely, that the pathological condition of the nerve centers has induced a direct disturbance in those portions of the germ cells which represent and will develop into the corresponding organs of the future offspring?

Previous fertilization.—Consider next the influence exerted upon the female germ cell by the mere proximity of the male germ cell, as exhibited in the transmission of the characteristics of one sire to the offspring of a succeeding sire, observed in animals, including the human species, also in plants. The best example is the oft-quoted case of Lord Morton's mare, which reproduced in the foal of a pure Arab sire the zebra markings of a previous quagga sire.

Some physiologists† have attempted to account for these remarkable indirect results from the previous fertilization or impregnation, by the imagination of the mother having been strongly affected, or from interchange between the freely inter-communicating circulation of the embryo and mother, but the analogy from the action in plants (in which there is no gestation but early detachment and development of the fertilized cells) strongly supports the belief that the proximity of male germ cells acts directly upon the female cells in the ovary. All that we can deduce from these facts is that in some manner the normal characteristics and tendencies of the ova are modified by the foreign male germ cells without either contact or fertilization.

Maternal impression.—The influence of maternal impressions in the

* *Comptes-Rendus*, March 13, 1882. These experiments have been confirmed by Obersteiner.

† See the cases cited by Ribot, and Darwin: *Animals and Plants Under Domestication*, vol. I, p. 437.

causation of definite anomalies in the fœtus is largely a matter of individual opinion.

It is denied by some high authorities, led by Bergman and Leuckhart.* Most practitioners, however, believe in it, and I need hardly add that it is a universal, popular belief,† supported by numerous cases. I myself am a firm believer in it. The bearing which the subject has upon this discussion is this: If a deviation in the development of a child is produced by maternal impression, we have a proof that a deviation from normal hereditary tendencies can be produced without either direct vascular or nervous continuity.

We see an analogy between the experiments of Brown-Séquard, the influence of the previous sire, and the maternal influence. Neither, in my opinion, directly supports the theory of transmission of acquired characters, for they do not prove that normal changes in the body cells directly react upon the germ cells; they all show that the typical hereditary development of single organs may be diverted by living forces which have no direct connection with them according to our present knowledge.

What the nature of these forces is I will not undertake to say, but I believe we must admit the existence of some unknown force, or rather of some unknown relations between the body cells and germ cells.

A year ago, recognizing fully the difficulty of advancing any theory of heredity which would explain the transmission of acquired characters, I came to the following result: "It follows as an unprejudiced conclusion from our present evidence that upon Weismann's principle we can explain inheritance but not evolution, while with Lamarck's principle and Darwin's selection principle we can explain evolution, but not, at present, inheritance. Disprove Lamarck's principle and we must assume that there is some third factor in evolution of which we are now ignorant."

In this connection it is interesting to quote again from my colleague, Prof. E. B. Wilson. He writes that the tendency in Germany at present is to turn from speculation to empiricism, and this is due partly "to the feeling that the recent wonderful advances in our knowledge of cell phenomena have enormously increased the difficulties of a purely mechanico physical explanation of vital phenomena. In fact, it seems that the tendency is to turn back in the direction of the vital-force conception. - - - As Boveri said to me recently, "Es gibt zu viel vorstand in der Natur um eine rein mechanische Erklärung der Sache zu ermöglichen."

In the final lecture we turn to the forces exhibited in the germ cells.

Handerwörterbuch der Physiologie, Wagner, Artikel "Zeugung," Leuckhart.

†See *Medical Record*, October 31, 1891, an article by Joseph Drzewiecki, M. D.

LECTURE III.—HEREDITY AND THE GERM CELLS.

According to the general law* the germ cell was considered as matter potentially alive and having within itself the tendency to assume a definite living form in course of individual development. The nucleus must be extraordinarily complex, for it contains within itself not only the tendencies of the present type, but of past types far distant. The supposition of a vast number of germs of structure is required by the phenomena of heredity; Nägeli has demonstrated that even in so minute a space as one one-thousandth cubic millimeter, 400,000,000 micellæ must be present.

The study of heredity will ultimately center around the structure and functions of the germ cells. The precise researches of Galton show that the external facts of heredity, questions of average and of probabilities, of paternal and maternal contributions to the offsprings, are capable of being reduced to an exact science in which mathematical calculations will enable us to forecast the characteristics of the coming generation.

There will still remain however a large residuum of facts which will present themselves to a mathematician like Galton, as fortuitous, or inexact, such as the physiological conditions of reversion; the course of pre-potency, by which the maternal or the paternal characteristics prevail in parts or in the entire structure of the offspring; the material basis of latent heritage upon which reversion depends, and which compels us to hypothecate either an unused hereditary substance or a return to an older disposition of the forces in this substance; the nature and determination of sex. These apparently chance phenomena must also be due to certain fixed laws, and by far the most promising routes to discovery have already been taken by Van Beneden, the Hertwig brothers, Boveri, Maupas, and others.

They have attacked the problem of the relation of the germ cells to the heredity on every side, and by the most ingenious and novel methods, which are familiar enough in various branches of gross anatomical and physiological research, but seem almost out of the limits of application to minute microscopic objects. For example, the Hertwig brothers have ascertained the influence of various solutions of morphine and other drugs of the alcohols, and of the various degrees of temperature upon the ovum and spermatozoon during the conjugation period, with results which are highly suggestive of the causes of congenital malformations, anomalies, and double births. The Hertwigs and Boveri have succeeded in robbing ova of their nuclei and watching the results of the subsequent entrance of spermatozoa. In order to further test the relations of the nucleus to the remainder of the cell, Verworn has experimented along the same line with extirpations of every kind from the single cells of Infusoria. Of equal novelty are the recent studies of

*See Huxley, Article "Evolution," *Enc. Britannica*, vol. VIII, p. 746.

Maupas upon the multiplication and conjugation of the Infusoria, giving us a host of new ideas as to the cycle of life, the meaning of sex, and the origin of the sexual relation.

In all this research and in the future outlook there are two main questions:

1. What is the hereditary substance? What is the material basis of heredity, which spreads from the fertilized ovums to every cell in the body, conveying its ancestral characteristics? Is there any substance corresponding to the hypothetical idioplasm of Nägeli?

2. What are its regulating and distributing forces? How is the hereditary substance divided and distributed? How far is it active or passive?

I may say at the outset that the idioplasm of Nägeli, a purely ideal element of protoplasm which he conceived of as permeating all the tissues of the body as the vehicle of heredity, has been apparently materialized in the chromatin or highly coloring materials in the center of the nucleus. This rests upon the demonstration by Van Beneden and others that chromatin is found not only in all active cells, but is a conspicuous element in both the ovum and spermatozoon during all the phenomena attending conjugation.

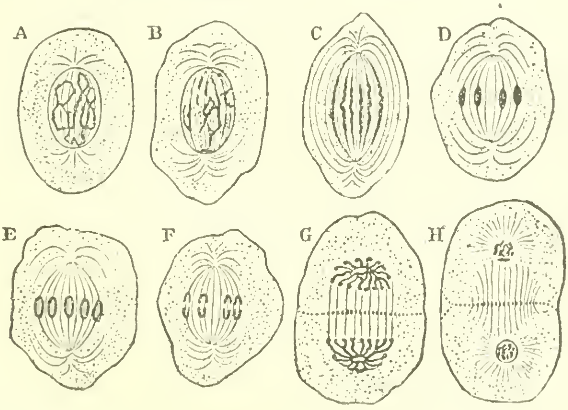


FIG. 7.—TYPICAL CELL DIVISION, SHOWING THE DISTRIBUTION OF CHROMATIN.—(From Parker after Carnoy.) A–C, arrangement of the chromatin in threads; D–E, formation of the chromatin rods and loops; F, splitting of the loops; G–H, retraction of the chromatin into the two daughter cells.

Secondly, that while the chromatin is apparently passive, it is played upon by forces resident in the clear surrounding protoplasm of the nucleus, but chiefly by the extra nuclear archoplasm, which seems to constitute the dynamic and mechanical factor in each cell. This, unlike the chromatin, only comes into view when there is unusual activity, as during cell-division, and is not evident (with our present histological technique, at least), when the cell is arrested by reagents in any of the ordinary stages of metabolism.

The distribution of hereditary substance.—I may first review some of the well-known phenomena attending the distribution of the chromatin substance to the tissues.

I have borrowed from Parker, figures by Carnoy, to illustrate the resting and active stages of the cell, and from Watase, a Japanese student of Clark University, figures representing the high differentiation of the cell contents during division (Figs. 8, 9). They bring out the active and passive elements of the typical cell.

The phenomena of karyokinesis which attend the division and distribution of the hereditary substance throughout the whole course of embryonic and adult development are well illustrated in Carnoy's figures (Fig. 7). First we have the quiescent period, in which the chro-

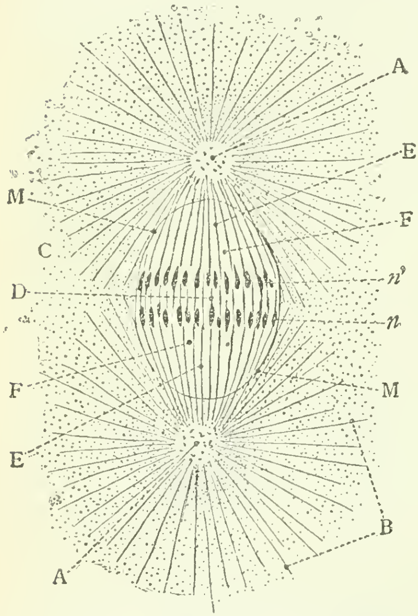


FIG. 8.—BEFORE DIVISION. DIFFERENTIATION OF THE CYTOPLASM AND NUCLEUS DURING CELL DIVISION OF A SQUID EMBRYO. LOLIGO. (After Watase). M, The nuclear membrane; F, Achromatin or nucleoplasm; C, Cytoplasm, or protoplasm outside of the nucleus; A-A, The two centrosomes of archoplasm; B, Extra nuclear archoplasmic filaments; E, Intra-nuclear archoplasmic filaments attached to *n*, *n'*, the chromatin rods.

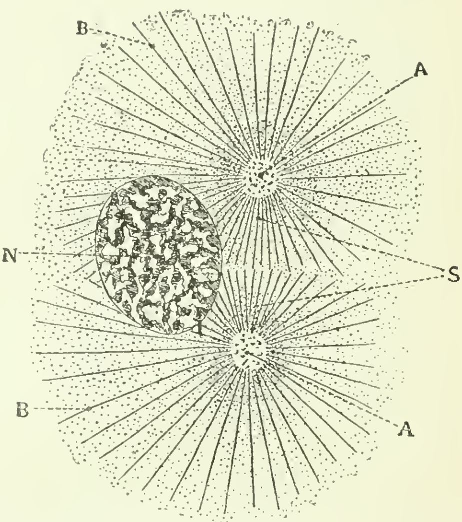


FIG. 9.—AFTER DIVISION. INTERIOR OF A DAUGHTER-CELL IN THE SQUID. (After Watase.) Division has just taken place and the daughter nucleus, N, shows the chromatin coil. The daughter centrosome is just forming two new centrosomes, A-A. by direct division.

matin presents the appearance of a coiled, tangled thread; surrounding this is the clear nucleo-plasm (or achromatin) bounded by the nuclear membrane; the extra-nuclear substance, or cyto-plasm, is apparently undifferentiated. As soon as cell division sets in, however, radiating lines are seen in the cyto plasm above and below the nucleus; these are called the archo-plasmic filaments by Boveri, since they proceed from what is now believed to be the dynamic element, the archoplasm (Fig. 8). As the activity becomes more intense the filaments are seen to diverge from a center—the archo-plasmic *centrosome*—which lies just without the nucleus at either pole; this radial display of cell forces suggested the term "asters" to Fol, and "spheres attractive" to Van Beneden. The behavior of the chromatin, or hereditary substance, under these archo-plasmic forces, is beautifully shown in Carnoy's diagrams (Fig. 7). First, the nuclear wall breaks up, then the chromatin coil unfolds into lines of vertical striation which become thread like, hence the term mitosis, and then more compact, until finally a number of distinct vertical rods, chromatin rods, or *chromasomes* are found.

A remarkable and significant fact may be noted here, that the number of chromasomes varies in the cells of different species, and even in the cells of different varieties (as in the thread-worm of the horse—*Ascaris megalocephala*), but is constant in all the cells of the same variety through all stages; thus the same number of chromasomes appear in the first segmentation of the fertilized ovum as in the subsequent cell division in the tissues.

Carnoy next indicates the vertical splitting of each rod into a loop or link preceding the horizontal splitting: thus we may conceive of a thorough re-distribution of the chromatin before it passes into the daughter-cells. The split loops are each retracted toward a centrosome, suggesting to some authors a contractile power in the archo-plasmic filaments, each chromasome being apparently withdrawn by a single filament. But as the chromasomes separate, the filaments also appear between them, and are variously termed "interzonal," "verbindungs fäden," "filaments réunissant;" there is therefore some difference of opinion as to what the mechanics of the chromasome divisions really are. The chromatin is now retracted into two coiled threads, each the center of the daughter nucleus with a single centrosome beside it. But as the line of cleavage is drawn between the two cells (Fig. 9), the single centrosome in each cell divides so that each daughter-cell is now complete with its chromatin coil and two archoplasmic centrosomes. This process has been beautifully described by Watase.*

It thus appears that both the chromatin and archo-plasm are permanent elements of the cell, such as we formerly considered the nucleus: the apparently passive chromatin is divided with great precision by the active archo-plasm, then the archo-plasm simply splits in two to resume the cleavage function.

Fertilization—the union of hereditary substances.—Before looking at the host of questions which fertilization suggests, let us review a few of the well-known phenomena preparatory to the union of the germ cells in order to give greater emphasis to the importance of recent discoveries.

First, the ovum is a single cell, the typical structure of which, with its nucleus and cytoplasm, is generally obscured by a quantity of food-material, surrounded by a rather dense cell wall. The ovum is said to be ripened or "matured" for the reception of the spermatozoön, by the extrusion of two small "polar bodies," containing both chromatin and

*See Marine Biological Laboratory Lectures, 1889. Boston: Ginn & Co.

hyaline protoplasm, and separating off by karyokinetic division. After maturation is complete, a single spermatozoön normally penetrates: then a reaction immediately sets in in the cell wall of the ovum which prevents other spermatozoa from entering. The head of the spermatozoön and the nucleus of the ovum now fuse together to form a single nucleus, which it is obvious contains the hereditary substance of two individuals. This is the starting point of the segmentation or distribution process above described, and it follows that the fertilized ovum at this stage must contain its typical complement of chromatin, archoplasm, etc., for the whole course of growth to the adult.

How shall we connect these phenomena of fertilization with the facts of heredity? The most suggestive enigma in connection with the fertilization process has been *the meaning of the two polar bodies*, especially since Van Beneden demonstrated that they contained chromatin? For twenty-five years, speculation has been rife as to why the ovum should extrude a portion of its substance in two small cells; why not in one cell? why not in a larger number? Thanks to the intense curiosity which these polar bodies have aroused, and to the great variety of explanations which have been offered for them, we have arrived to-day at a solution which links the higher animals with the lower, breaks down the supposed barrier between the sexes, and accords with the main external facts of heredity.

It seems to me best to disregard the order of discovery, and to state the facts in the most direct way. First, a few words as to the speculations upon the meaning of the polar bodies.

The early views of fertilization* were naturally based upon the apparent significance of this process in the human species, in which the sexes are sharply distinguished from each other in their entire structure, and the reproductive cells are also widely differentiated in form, the ovum large and passive, the spermatozoon small and active. The readiest induction was to regard these elements as representing distinct physiological principles, corresponding to the essential sexual characteristics—in short, as male and female cells, the former vitalizing and rejuvenating the latter. Thus one of the earliest definite "polar-body" theories was that the ovum was hermaphrodite, containing both male and female principles, and that it was necessary to get rid of the male substance before the spermatozoon could enter.

As Von Siebold and Leuckart had demonstrated that some ova reproduce parthenogenetically, that is without fertilization by spermatozoa, Weismann turned to such forms for the solution of this problem, and was surprised to find that parthenogetic ova only extrude one polar body. This led him to attach one meaning to the first polar body, and another meaning to the second, which he viewed as designed to reduce the heredity substance in the ovum without regard to sex. Thus both this and the older theory conveyed alike the idea of *reduc-*

*See also the introduction of Weismann's last essay, "Amphimixis."

sion, but with an entirely different supposition as to the nature of the material reduced or eliminated.

Maupas on Conjugation among the Infusoria.*—Among the newer researches which throw light upon this old problem, those of Maupas are certainly the most brilliant. After a most exact and arduous research, extending over several years, he collected his results in two memoirs, published in 1889 and 1890.

His experiments were first directed upon the laws of direct multiplication by fission, which revealed a complete cycle of life in the single-celled Infusoria and showed that after a long period this mode of reproduction becomes less vigorous, then declines, and finally ceases altogether unless the stock is rejuvenated by conjugation of individuals from different broods. In other words, these broods of minute organisms grow old and die unless they are enabled to fertilize each other by an exchange of hereditary substance altogether analagous to that observed in the higher multicellular organisms.

The cultures were made in a drop of water upon a slide, and feeding was adapted either to the herbivorous or carnivorous habits of the species. Under these conditions it was found that the rate of fission or direct multiplication varied directly with the temperature and food, rising in some species (*Glaucoma scintillans*) to five bipartitions daily. With the optimum of conditions this rate, if sustained for thirty-eight days, would produce from a single individual a mass of protoplasm equivalent to the volume of the sun. This rate is however found to be steady for a time, and then the offspring decline into "senescence," in which they appear at times only one fourth the original size, with reduced buccal wreaths and degenerate nuclear apparatus. This is reached sooner in some species than in others; *Stylonichia pustulata* survives three hundred and sixteen generations or fissions, while *Leucophrys patula* persists to six hundred and sixty generations. Finally, even under the most favorable conditions of environment, death ensues.

Not so where conjugation is brought about by mingling the offspring of different broods in the same fluid, as in the natural state. Maupas soon discovered that exhaustion of food would induce conjunction between members of mixed broods. He thus could watch every feature of the conjugation process, and determine all the phases in the cycle of life. These differed, as in the longevity of the species. In *Stylonichia*, for example, "immaturity" extended over the first one hundred bipartitions: "puberty," or the earliest phase favorable to conjugation, set in with the one hundred and thirtieth bipartition; "eugamy," or the most favorable conjugation phase, extended to the one hundred and seventieth; then "senescence" set in, characterized by a sexual hyperaesthesia in which conjugation was void of result or rejuvenesence, owing apparently to the destruction of the essential nuclear apparatus.

* Sur la multiplication des Infusoires Ciliés, *Archiv. de Zoölogie experimentata*, Ser. 2, vol. VI., pp. 165-273; *Le Rajeunissement Karyogamique chez les Ciliés*, vol. VII, pp. 149-517. See also Hartog, *Quart. Jour. Microscop. Science*, December, 1891.

Conjugation begins with the approach of two individuals, and adhesion by their oral surfaces. There is no fusion, but an immediate transformation in the cell contents of each individual sets in, concluding

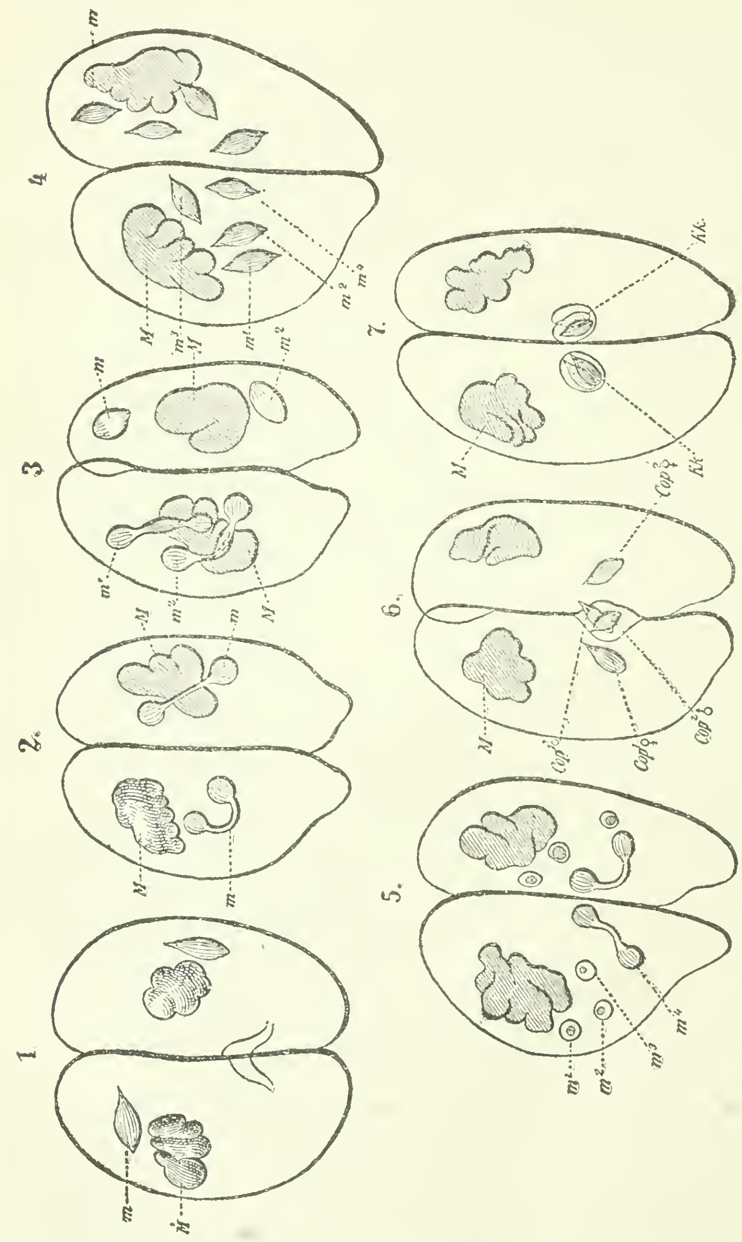


FIG. 10.—THE CONJUGATION OF INFUSORIA. (From Weismann, after Maupas.) 1, Two infusoria copulating; *M*, meganucleus; *m*, micronucleus; 2–5, Successive divisions of micronuclei; 6, The migration of one of the persisting micronuclei from each infusorian into the other; 7, Union of the interchanged micronuclei.

with an interchange of nuclear substance. In each cell Maupas distinguishes between the (*M*) *meganucleus* (Fig. 10, the macronucleus, nucleus, endoplast of authors), which presides over nutrition and growth and divides by constriction, and the (*m*) *micronucleus* (paranucleus, nucleolus, of authors), which presides over the preservation of the species. The latter contains chromatin; it is the seat of rejuvenescence, the basis of heredity, it divides by mitosis, showing all the typical stages of karyokinesis excepting the loss of the cell membrane.

The transformation in each of these copulating cells first affects the centers of hereditary substance, viz, the micro-nuclei; they divide three times; thus the micronuclear substance is reduced to one-fourth of its original bulk. It is contained in two surviving micronuclei (the others being absorbed or eliminated), one of which migrates into the adjoining cell: the other remains stationary. This migration is followed by a fusion of the migrant and stationary micronuclei; this fusion effects a complete interchange of hereditary substance, after which the two infusoria separate and enter upon a new life cycle. Meanwhile the meganucleus breaks up and is reconstituted in each fertilized cell.

Maupas gathers from these interesting phenomena additional proof that the chromatin of all cells bears the inherited characteristics and that the cyto-plasm and nucleo-plasm, or achromatin, is the dynamic agent, because the micronuclei bearing the chromatin are the only structures which are permanent and persistent, all the other structures—nucleo-plasm, archo-plasm, etc.—being replaced and renewed. The reduction of the chromatin is purely quantitative, the eliminated and fertilizing micronuclei being exactly equivalent; after the chromatin has been quartered the cell becomes incapable of further activity until it is reinforced by chromatin from the copulating cell.

No distinction between the sexes in heredity.—The three laws which underlie these phenomena are: (1) That fertilization consists in the union of the hereditary substance of two individuals. (2) That before the union the hereditary substance in each is greatly reduced. (3) That there is no line between male and female, the conjugating cells are simply in a similar physiological condition wherein a mingling of hereditary characteristics affords a new lease of life. As Maupas says:

"Les différences appelées sexuelles portent sur des faits et des phénomènes purement accessoires de la fécondation. La fécondation consiste uniquement dans la réunion et la copulation de deux noyaux semblables et équivalents, mais provenus de deux cellules distinctes."

In this conclusion as to the secondary and superficial, rather than fundamental, difference between the two sexes, Maupas simply confirms the views of Strassburger, the botanist, Hensen, R. and O. Hertwig, Weismann, and others, namely, that sex has evolved from the necessity of cell conjugation; that even in the higher forms the cells born by the two sexes are absolutely neutral so far as sex is concerned, the wide difference of form of the germ cells is a result of physiological division of labor—the mass and yolk of the ovum having been differentiated to support the early stages of development while the spermatozoon has dispensed with all these accessories and acquired an active vibratile form for its function of reaching and penetrating the ovum. The evidence of the Infusoria is paralleled among some of the plants, in which conjugation between entirely similar cells is observed.

The causes finally determining sex may come surprisingly late in development, and according to the investigations of Düsing and the experiments of Yung* and of Giron are directly related to nutrition. High feeding favors an increase of the percentage of females, while, conversely, low feeding increases the males. In Yung's experiments with tadpoles the following results were obtained:

	Females.	Males.
Normal percentage	57	43
High nutrition	92	8

Geddes expresses this principle in physiological terms of metabolism, that anabolic (constructive) conditions produce females, while katabolic (destructive) conditions produce males.

I think we may now safely eliminate the factor of sex from our calculations upon the problem of heredity, and thus rid ourselves of one of the oldest and most widespread fallacies. We shall thus, in using the terms "paternal" and "maternal" imply merely the distinction between two lines of family descent.

The theory of reduction.—This leads us back to the significance of the polar bodies. Van Beneden's discovery that these bodies contained chromatin led gradually to the view that they were not fragments of the ova, but represented minute, morphologically complete cells. Bütschli showed that they were given off independently of, and prior to, the contact of the spermatozoon, and, finding in the leeches that the first polar body subdivides to form two bodies, he considered them as formed by true cell division, and containing both nucleoplasm and chromatin. Giard independently reached a similar opinion, assigning an atavistic meaning to the polar cells. Whitman, in 1878, advanced the idea that they represented vestiges of the primitive mode of reproduction by fission, while Mark described them as "abortive ova."

At this point speculation subsided until it was revived by Weismann's attempt to connect these bodies with his theory of heredity,† already referred to. The whole history is clearly given in R. Hertwig's masterly memoir upon Ovo and Spermatogenesis in the Nematodes.‡ Taking advantage of Boveri's discoveries in staining tech-

*See Geddes and Thomson: *The Evolution of Sex*, 1891; also, Düsing: Die Regulierung des Geschlechtsverhältnisses bei d. Vermehrung der Menschen, Tiere und Pflanzen, *Jen. Zeit. f. Natur.*, Bd. 17, 1884.

† On the Number of Polar Bodies and their Significance in Heredity, 1887.

‡ Ei und Samenbildung bei Nematoden, *Archiv. f. Mikr. Anat.*, Bd. 26, 1890.

nique, and stimulated by Weismann's prediction that spermatozoa would also be found to extrude polar bodies, this author examined all stages in the peculiarly favorable germ cells of the thread-worm of the horse (*Ascaris megalocephala*).

He made the surprising discovery that ova and spermatozoa are formed in a substantially similar manner by repeated divisions, the single difference being that the last products of division among the sperm cells are effective spermatozoa, capable of development in fertilization, while the last products of division in the ovary are, first

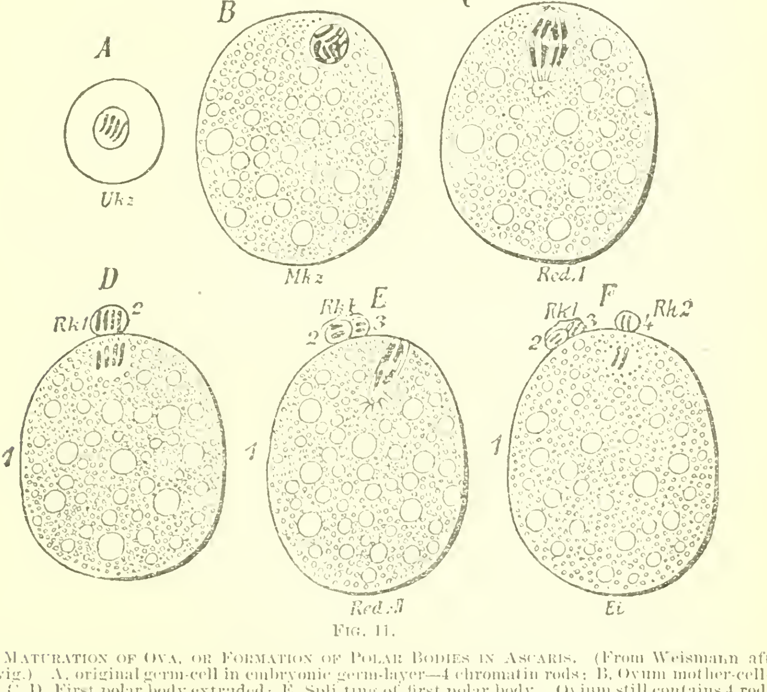


FIG. 11.

THE MATURATION OF OVA, OR FORMATION OF POLAR BODIES IN ASCARIS. (From Weismann afte Hertzwig.) A, original germ-cell in embryonic germ-layer—4 chromatin rods; B, Ovum mother-cell—8 rods; C D, First polar body extraded; E, Splitting of first polar body. Ovum still contains 4 rods; F, Second polar body extruded; Ovum mature with 2 rods.

the true ova, and, second, the abortive ova (polar cells), incapable of development. In both ova and spermatozoa the nucleus contains but one-half the chromatin which a typical nucleus contains; in the case of *A. megalocephala* each of the germ cells contains but two chromasomes while the normal body cells contain four. The manner in which this maturation of the germ cells for conjugation is brought about is beautifully shown in these diagrams, taken from Weismann's essay, "Amphimixis." You observe that the number of chromasomes in the primary germ cells is four (Figs. 11 and 12, *A*). Then are formed by subdivision the ovum and sperm "mother cells," in which the chromatin substance is doubled, so that we observe eight chromasomes. The mother cells then divide and the chromatin is reduced to four rods, a second division rapidly follows whereby the chromatin is reduced to two rods, or half the original quantity. These last divisions take place by karyokinesis, but, as Hertwig points out, they differ from typical karyokinesis in the fact that the divisions follow so rapidly upon each other that the vesicular resting period of the nucleus is omitted. Thus, he suggests, is prevented an overaccumulation of chromatin substance prior to the fusion of the ovum and sperm.

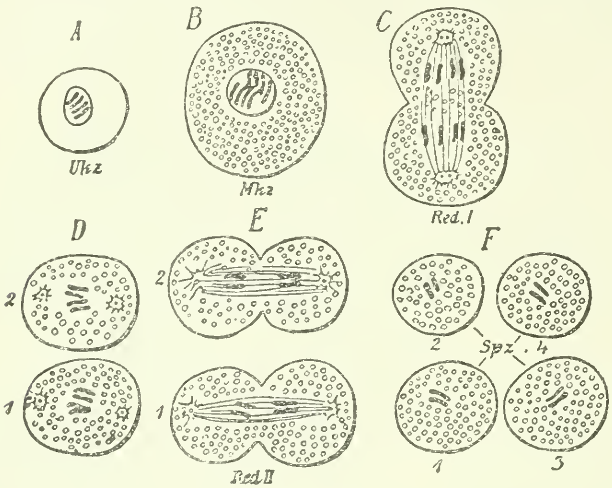


FIG. 12.

SPERMATOGENESIS IN ASCARIS.—(From Weismann after Hertwig.) A, original germ cell—4 chromatin rods; B, sperm mother cell—8 rods; C–D, first daughter cells with 4 rods each; E–F, formation of second daughter cells, or mature spermatozoa, with 2 rods each.

It is evident that the polar cells are rudimentary ova, which do not possess the yolk mass, etc., essential to development, and are divided off at a very late stage, sometimes after the egg has left the ovary, but are in other respects analogous to the spermatozoa. The reason these polar-cells have not disappeared altogether in either plants or animals is that they originally possessed a deep physiological importance. As the first polar cell subdivides and forms two, it follows that from both ovum and sperm mother cells four daughter cells are formed, each containing half the chromatin substance of a normal nucleus. In the ovary three of these daughter cells abort and the fourth forms a true ovum; in the sperm gland, however, all four daughter cells form spermatozoa.

We may thus consider the polar-cell problem as in all probability settled; the whole process is probably an inheritance or survival of a primitive condition in which all four ova, like the four spermatozoa, were fully functional.

The relation between the chromatin and heredity.—We have just seen that the last stages in the preparation of the ova and spermatozoa for conjugation result in halving the number of rods in the original germ cells. Now, as Hertwig and Weismann point out, one point is still left in doubt. Why is the chromatin substance doubled in the mother-cells so that two successive sub-divisions are necessary to reduce it to half the original quantity? Hertwig has not attempted to answer this question, as he prefers to wait for further research. Weismann, however, who is unfortunately cut off from research by failing eyesight, has offered a speculative solution to this problem which he trusts may guide future investigation.

This leads me to say a few words in regard to his conception of the relation of the chromatin to heredity. (1) His first premise is that in fertilization there is not a fusion of chromatin, but that a certain independence is preserved between the maternal and paternal elements, based upon the observed fact that the two pairs of rods do not fuse but lie side by side, and upon the assumption that these pairs are kept distinct in each cell through all the subsequent stages of embryonic and adult development. If this is the case, the hereditary substance contributed by the father would remain separate from that contributed by the mother, throughout. (2) "Each of these pairs would be made up of the collective predispositions which are indispensable for the building up of an individual, but each possesses an individual character, for they are not entirely alike. I have called such units 'ancestral plasms,' and I conceive that they are contained in numbers in the chromatin of the mature germ cells of living organisms, also that the older nuclear rods are made up of a certain number of these. - - - Obviously these units can not become infinitely minute; however small they may be they must always retain a certain size. This follows from the extremely complicated structure which we must without any doubt ascribe to them." These units are not, however, ultimate, they are in turn extremely complex, and are composed of countless biological units of the kind conceived of by Nägeli and others. (3) The reduction of the chromatin only acquires a meaning when taken in connection with the above supposition of distinct ancestral plasms, and has no meaning if we accept Hertwig's view that there is a complete fusion of maternal and paternal germ plasm. This meaning is that reduction in the maturation of germ cells is *sui generis*, it does not divide the ancestral plasms into two similar groups, but one daughter-cell receives one set of germ plasms or hereditary predispositions, and another daughter cell receives another; reduction is thus differential. According to this view the four sperm and ovum daughter-cells would each contain a different set of ancestral plasms. (4) The fact that the chromatin substance is doubled in the sperm and ovum mother cells, so that we observe double the number of rods characteristic of the species, is to be explained as an adaptation to the requirements of natural selection, for this doubling and subsequent double division render possible an infinite number of combinations (as many, in fact, as there are individuals) for selection to operate upon.

This explanation of Weismann's is an example of his apotheosis of the theory of natural selection. Every process is made to suit this theory, which, as we have seen in the first and second lectures, is, in his opinion, the exclusive factor of evolution. But this very high degree of mingling and re-mingling of ancestral pre-dispositions would be fatal to evolution, for after a combination favorable to survival had been established in one generation it would be broken up into a new combination, perhaps unfavorable. to survival, in the next generation. This entire essay upon "Amphimixis," or the theory of mingling of reduced hereditary substance, will, I believe, mark a turning point to decline in Weismann's influence as a biologist. His whole reasoning is now in a circle around the natural selection theory.

The meaning of conjugation.—Weismann looks upon sexual reproduction as designed to mingle hereditary tendencies and to create individual differences whereby natural selection may form new species. It is evident that these combinations must be mainly fortuitous and productive of indefinite variation; but we have seen that evolution advances largely by the accumulation of definite variations, or those in which each successive generation exhibits the same tendencies to depart from the typical ancestral form in certain parts of the body, and that these tendencies stand out in relief among the diffused kaleidoscopic or fortuitous anomalies.

The fact moreover that variability and evolution by the accumulation of certain variations in successive generations is also observed in organizisms which reproduce *asexually*, both among plants and animals, shows that we must look in another direction for the underlying cause or purpose of sexual reproduction. Weismann rightly combats the old idea of "vitalization" of the ovum by the spermatozoon, and it is perfectly evident from the researches of Maupas and Hertwig that the ovum may as accurately be said to vitalize the spermatozoon as the reverse. Fecundation is simply the approximation of two hereditary substances of distinct origin and their incorporation into a single nucleus. The action and re-action of these substances may be considered equal and mutual, so far as we now know.

The remarkably ingenious experiments of Hertwig and Boveri, above alluded to, strengthen this idea. Some years ago Weismann wrote: "If it were possible to introduce the female pronucleus of an egg into another egg of the same species, immediately after the transformation of the nucleus of the latter into the female pronucleus, it is very probable that the two nuclei would conjugate just as if a fertilizing sperm nucleus had penetrated. If this were so, the direct proof that egg nucleus and sperm nucleus are identical would be furnished." Boveri succeeded in accomplishing a similar feat by depriving an ovum of its nucleus and subsequently causing it to develop by admitting a spermatozoan which fertilized the denucleated ovum and produced a complete individual.

In opposing the vitalizing properties of the sperm, Weismann however went further, and advocated the view that there is nothing in the nature of vitalization or "rejuvenescence" in conjugation—that, given proper environment, protoplasm is immortal, and runs upon a course of undiminished activity. This we have seen is not the case in the infusoria, and, as recently remarked by Hartog, there is only one class of organisms which, according to our present knowledge, are completely agamous and immortal—namely, the Monadina. It may in future appear that even in the monads there is a cycle for the development in which conjugation plays its part.

Maupas' experiments seem to establish the primitive, and therefore the true, interpretation of the purpose of conjugation as well as of sex, the latter being a consequence of the former, namely, that after a long period of direct subdivision of hereditary material from a single individual, a limit is reached beyond which the forces of heredity are not reproduced in their original intensity unless combined with another set of similar forces of different origin. This combination restores the original intensity. It is objected to this that two sets of feeble forces can not constitute one vigorous force, but this is met by the observed fact that such union does start a new life cycle, and is therefore rejuvenescent. We may regard this as the fundamental meaning of conjugation, and the production of variations as entirely secondary.

The distribution of the chromatin.—We have now reviewed some of the main phenomena of fertilization; there still remains the relation of the hereditary substance to the future development of the individual. There is, first, the astonishing fact that, as the chromatin goes on dividing, its mass or volume remains apparently undiminished; that is, there is apparently as much chromatin in one of the many million active cells of the body as in the original fertilized ovum, and there is still an enigma as to the nature of this chromatin and its functions. Secondly, there is the problem of the maternal and paternal elements in each cell; do they lie side by side or are they fused?

1. In plants De Vries* and others believed that all or by far the greater number of cells in the plant body contain the total hereditary characters of the species in a latent condition. Kölliker† has fully discussed this question and called attention to Müller's early views that, in spite of the physiological division of labor producing the tissues, the properties of all the tissues can be derived from the nuclear substance of a single tissue, as proved by experiments upon the lower animals. Weismann, on the other hand, has held that the course of development is marked by a constant qualitative distribution of his germ-plasm or hereditary substance, so that, so far as nuclear content is concerned, there are three forms of cells: (1) with nucleo-plasm; (2) with nucleo-plasm and germ-plasm; (3) with germ-plasm only.

* Hugo de Vries: Intracellulare Pangenesis. Jena, 1889.

† Die Bedeutung der Zellkerne für die Vorgänge der Vererbung. Zeit. f. Wiss. Zool., 1885. And, Das Karyoplasma und die Vererbung, op. cit., 1886.

Kölliker opposes this idea and maintains that the "idioplasma" passes into all cells, in which it divides in course of development. Step by step from the embryonic layers to the tissues, the constructive processes are under the direction of the nuclei containing this hereditary substance. It remains in every nucleus for a long period unaltered, in order to finally, here earlier, there later, impress its constructive forces. In certain elements, as in blood corpuscles, epidermal scales, etc., it disappears, as the last product of division.

R. Hertwig takes a similar view. Since embryonic and adult cell division is differential, there must be a form of differentiation in the nucleus; but this does not consist in the total elimination of some qualities and survival of others, nor of a reduction in mass. The mass and the properties remain the same in every cell; the differentiation consists in the activity of certain elements in certain tissues. Thus we may say with De Vries that different "pangene" may leave the nucleus and enter the cell in different tissues; or with Nägeli, that special "micellæ" come into activity at certain points; in other words, the potential of the nucleus is differently exerted. Here again we have the idea of patent and latent hereditary elements, such as appear in the entire individual upon a larger scale.

This is one of the most interesting problems for future investigation, but the direction of research will, I imagine, cover a larger area of cell content than the nucleus, as we are now swinging back to regard the extra-nuclear archoplasm as an important factor in the process.

In the following paragraph Hertwig expressed his view of nuclear control and cyto-plasmic differentiation:

"As I saw in the transformation of the nucleus during fertilization proof that it is the bearer of hereditary substance, I recognized a great advance in the fact that the nucleus leaves in the same form in every cell, and in its vesicular capsule is somewhat removed from the metamorphoses of the cells. As Nägeli spreads his idio-plasm as a net-work throughout the whole body, so, according to my theory, every body-cell contained in its nucleus its quota of hereditary substance, while its specific histological peculiarities were to be regarded as its plasma products."

2d. The next question is the fate of the maternal and paternal contributions to the embryo. Here there is a wide difference of opinion. On the one side Van Beneden is the leader of those who regard each cell of the body as in a sense hermaphrodite; as we have seen, his views of maturation and the significance of the extension of the polar bodies were colored by this theory, for he regarded the germ cells as hermaphrodite until one sex was eliminated. But now that the researches of Hertwig have given the last blow to Van Beneden's theory, and it follows that there can be no male and female chromasomes, there still remains room for the analogous view that the maternal and paternal chromasomes remain distinct throughout the course of development, not as sexual elements, but as substances with the same racial and specific but different individual tendencies. Rabl, an eminent embryologist, shares this view, and it is supported by Boveri upon the observation that in each division the paternal and maternal elements are kept distinct, and in *Ascaris*, for example, two of the chromasomes of each division figure are paternal and two are maternal.

In favor of this hypothesis we may place the following facts: 1st, that there are an even number of chromasome rods in all cells; 2d, that the number is constant throughout all the subsequent changes in the tissues; 3d, that the number is fixed for each species or variety; 4th, that the number is the same in each sex.

Against this replacement hypothesis we must consider the extreme complexity of the division process, and the long-resting, or thread stage, in which the chromatin lies in a confused coil. Further, Hertwig argues that if the elements are distinct we should find some evidence that the maternal or paternal part is atrophied or replaced, or excluded from the nucleus, for both parts can not share alike in the control of the cell. These are Hertwig's grounds for supporting the "verschmelsungstheorie," or fusion theory, also advocated by Waldeyer, to the effect that by the complete union of the maternal and paternal substance a new product is formed: in this fusion the law of pre-potency may come into play, causing one or other of the parental tendencies to predominate, or there may be an even re-distribution, whereby, as expressed by Hensen, "the hereditary substance of the son is not that of the father plus that of the mother, but is his own, with a new hereditary form resulting from the combination."

While suspending judgment between these two views as to the separation or fusion of the chromatin, we may appeal to the external phenomena of heredity for light upon the probabilities in the question First, I refer to the very decided opinion of Francis Galton in regard to particulate inheritance; he is so impressed with the fact that we are made up bit by bit of separate structures derived from different ancestors that he has even suggested that the skin of the mulatto may represent not a fusion of white and black, but an excessively fine mosaic in which the colors are so distributed as to give the appearance of blending. We do sometimes observe patches of color as evidence of uneven distribution. As Galton distinguishes two types of structures with reference to inheritance, viz, those which blend and those which do not blend, we might correlate these types with pre-potency, replacement, and fusion. Where characteristics do not blend, as in eye-color, it is evident that, while the offspring must receive from both parents the material basis for the formation of the complete color of the eye, either the maternal or paternal material must be prepotent and exclude the development of the other; the logical inference is that the former activity replaces the latter; but it is not necessary that exclusion from the cell chromatin should follow. Now, while some blends seem to support the theory of fusion, the sum total of facts of heredity are strongly against this as a universal principle, for many maternal and paternal structures are preserved in their absolute integrity for generations without the least indication of mixture.

Cell forces and heredity.—We have thus far been considering only the chromatin as the heredity substance *par excellence*, and have disregarded for the time the archo-plasm or dynamic material of the cell. If we advance upon the hypothesis that a typical cell contains the more or less passive chromatin, and the archoplasm playing upon this chromatin in course of every phase of re-distribution, it seems *à priori* improbable that elements which are associated with every vital change should be dissociated in the phenomena of heredity. We might suppose that the mechanics of karyokinesis are exactly similar in every cell of one individual, but it is highly improbable that they should be exactly similar in two individuals. We should therefore anticipate the joint transmission of the chromatin and archoplasm, implying by the latter the dynamic centers especially connected with hereditary function as distinguished from the general functions of metabolism.

This leads us to look for evidence from the life of the cell in its totality. We owe to Dr. Max Verworn* a fresh treatment of this subject, based upon experimental researches among the Infusoria, mainly by the extirpation method. As his experiments included only the phenomena of living cells in which the chromatin substance was of course undifferentiated to the eye, he treats of the nucleus as a whole without distinction as to chromatin and achromatin. He concludes that the physiological importance of the nucleus is exhibited in its constant interchange of materials with the remainder of the cell body; only through this interchange does it influence the cell and control its life processes. The interchange is in triple currents, *a*, from outside of cell to cyto-plasm; *b*, from cyto-plasm to nucleus; *c*, from nucleus to cyto-plasm. These movements of interchange are the expression of life phenomena. He compares the *rôle* of the nucleus to that of a cell organoid, like chlorophyll, as not constantly present but as invaribly necessary to activity. Thus he believes even the most lowly organized cells have nuclear centers, and that even bacteria are differentiated into nuclear and extra-nuclear areas. Coupled with this idea of nuclear control is the somewhat paradoxical statement that the nucleus is not a dynamic center, either automatic or regulating, and the conclusion that the nucleus alone can not be the seat of fertilization and heredity, but both the nucleus and extra-nuclear protoplasm must constitute the material basis of heredity. This conclusion is in the direction of the general reaction of opinion which is now taking place against the centralization of cell-government in the nucleus.

Vague as they must necessarily be, our ideas of cell forces are somewhat further defined by the brilliant experiments of the Hertwig

* "Die Physiologische Bedeutung des Zellkerns," *Archiv für Physiologie*, 1891, pp. 113–115.

brothers upon germ cell physiology and pathology, which are full of suggestion as to the causation of abnormalities in inheritance. These were begun in 1884, and were first directed to the influence of gravitation upon the planes of embryonic cell division, following up the experiments of Pflüger and Rauber. In 1885 the conditions of bastard fertilization were studied; in 1887 the causes of polyspermy or multiple fertilization; and in 1890 the effects of extreme heat and cold upon germ-cell functions.* In general the conclusions reached were that in the normal state there exist regulating forces in the ovum which prevent multiple fertilization or bastard fertilizations (*i. e.*, by spermatozoa of other varieties), but these forces are neutralized where the life energy of the cell is diminished by reagents or by extremes of temperature.

For example, in the normal state the entrance of a single spermatozoon produces a reaction in the ovum wall preventing the entrance of other spermatozoa, but when the ovum is weakened by chloroform solution two or more spermatozoa enter before the reaction appears; in fact that degree of polyspermy is directly proportional to the intensity of the chemical, thermic, or mechanical disturbance of the ovum. Double fertilization or over-fertilization has not in a single case resulted in the production of twins, so that Fol's supposition is negatived, although other forms may behave differently. The cell function may be arrested at any stage by thermic influences; thus two pronuclei, paternal and maternal, about to unite, can be held apart by lowering the temperature. Polyspermy also results from a lowered temperature. It is noteworthy that the conditions of bastard fertilization and polyspermy are different; chloroform produces the latter but not the former. Kupffer has, I believe, succeeded in producing twins, or rather two-headed monsters, by abnormal fertilization in fishes.

These researches, although made with a different object, re-establish the older views as to the inter-dependence of nuclear and extra-nuclear activities, and show that no sharp line of demarcation of function can be drawn between the nucleus as a center of reproduction and heredity and the cyto-plasm, as the seat of tissue building and nutrition. In Boveri's discovery of the archo-plasmic centers, or centrosomes, we find positive ground for this broader view. It is connected with the cell phenomena of heredity in the following manner:

While the union of the nuclei in fertilization is the most obvious feature, this union is dependent upon the archoplasm, which re-arranges the nuclear elements. If the spermatozoon contains no archoplasm, this power can not come from the parental side; but Boveri shows that this is probably not the case and that the spermatozoon brings its centrosome with it, thus entering the ovum with both the parental chromatin substance and dynamic material. It is certain from this and

* Experimentelle Untersuchungen über die Bedingungen der Bastardbefruchtung. Jena, 1885. See series of papers in *Jenaische Zeitschrift.*

from the observations of Roux that the sperm cell is now to be regarded as more than a mere nucleus, that it contains both nuclein and para-nuclein.

Intercellular forces.—The forces within the different portions of the cell lead us to consider those which must exist between different cells. This is an obscure question at present; but, as I have observed in the close of the second lecture, it is an extremely important one in connection with the problem of heredity.

As Prof. Wilson writes: "My own conviction steadily grows that the cell is not a self-regulating mechanism in itself, that no cell is isolated, and that Weisman's fundamental proposition is false."

It is a long step between an *à priori* conviction and the demonstration by experiment of a correlation of forces between the cells. This seems to me a most important field of experiment. We have seen in Maupas's work that the contact of two infusoria initiates a rapid series of internal changes; we have only to conceive of analogous changes taking place when two cells are not in actual contact, as in the phenomena of previous fertilization referred to in my second lecture. Hertwig and others have shown how gravitation is related to cell activity. Roux has destroyed half an embryo with a hot needle in the first stages of segmentation and followed the other half through the stages of subsequent development. Another clever experimenter has turned fertilized ova upside down during the early stages of development, and shown how the protoplasmic pole and yolk pole forcibly change places. Driesch has traced the connection and meaning of the first plane of cleavage in the embryos of echinoderms, and has succeeded in raising a small adult from half an embryo artificially separated during the first cleavage stage. Wilson, in the larva of Nereis, has shown how a certain stage of division in one group of cells affects all the other groups. All these experiments are in the line of determining the relations which exist between internal cell forces and other natural forces. What we must now seek to determine is the relation of cell to cell throughout the body, in connection with the phenomena of heredity.

Conclusions.—Perhaps the most impressive result of our review of recent researches in evolution and heredity is the uniformity of life processes throughout the whole scale of life from the infusoria to man. The most striking analogy is that seen in the laws of fertilization and conjugation, which are shown by Maupas's researches to have been established substantially in their present form at a very early period in the evolution of living organisms. Such uniformity furnishes a powerful argument for the advocates of the study of biology as an introduction to the applied science of medicine. Much that is now entirely omitted from medical education, because it is considered too remote, is in reality at the very roots of the science. To understand the disorders of life we should first thoroughly understand the essential phenomena of normal life. Of course we shall never see life as it really is, because there is always something beyond our highest magnifying powers; but we come nearest to this invisible form of energy when, with such investigators as Hertwig and Maupas, we strip the life processes of all their accessories and view them in their simplest external form.

The problems of evolution are found to be inseparably connected with those of heredity. No theory is at all adequate which does not explain both classes of facts, and we have seen that the explanations offered by the two opposed schools—those who believe in the transmission of acquired characters and those who do not—are directly exclusive of each other. We should suspend judgment entirely rather than cease to gather from every quarter facts which bear upon the most important and central problem of the transmission of acquired characters. I have endeavored to point out the opportunities which medical practitioners enjoy of contributing evidence upon this mooted question. It must not be forgotten that while the inheritance of individual adaptation to environment is the simplest method of explaining race adaptation such as we observe in the evolution of man, we know absolutely nothing of how such inheritance can be effected through the germ cells. We can not at present construct even any form of working hypothesis for such a process. On the other hand, we have found how untenable is the alternative theory offered to us by Weismann, that it is solely natural selection or the survival of the fittest which

"- - - shapes our ends,
Rough hew them as we will."

At the same time Weismann's conception of a continuity of germinal protoplasm, which we have found to consist in chromatin plus archoplasm, helps us over many of the phenomena of heredity, especially on the retrogressive side, and if it were not that we must also account for progressive and definite transformation in heredity, we might credit the distinguished Freiburg naturalist with having loosened the Gordian knot.

In summing up, the order of treatment followed in the lectures may be reversed, and we can begin with the germ cells, and condense the more or less ascertained facts.

The germ cells:

(1) The material substance of hereditary transmission is the highly coloring protoplasm, or chromatin, in the nucleus of the germ cells, probably connected with a certain form of archoplasm, or dynamic protoplasm outside of the nucleus.

(2) Before conjugation and fertilization the hereditary substance of both the male and female cells is reduced to one-half that found in a typical cell. This substance is however first doubled and then quartered, the meaning of which process is not understood.

(3) There is a difference of opinion as to whether the paternal and maternal hereditary substances, during fertilization, are fused or lie side by side; also as to how the substance is distributed through the tissues, whether en masse or by qualitative distribution.

Heredity:

(4) No connection between the germ cells and body cells is known, but the facts of heredity seem to render such a connection theoretically necessary. Several classes of facts connected with reproduction seem to support this theory.

(5) The facts of heredity support the theory of a continuous and specific form of protoplasm as the basis of repetition of type.

Evolution:

(6) The facts of evolution, both in present and past time, point to transformism by definite progression toward new types of structure in succeeding generations, opposing the retrogressive forces of heredity.

(7) The theory (natural selection) of definite progression by the accumulation of fortuitous favorable variations is found to be not only theoretically improbable, but not to correspond with the observed laws of variation.

(8) The laws of variation (anomalies) lend support to the theory of hereditary transmission of individual acquired variations, but even this (Lamarckian) theory encounters many difficulties.

I think this is as fair a statement as can be made at the present time, and it rests upon a general survey of the whole field.

REPORT ON THE MIGRATION OF BIRDS.*

By Prof. Dr. J. A. Palmén.†

Helsingfors, Finland.

[Translated from the German by C. W. Shoemaker.]

The annual migration of birds has at all times attracted the attention of thinking men but in very different ways. Thus this phenomenon awakes in the simple peasant other thoughts than in the man of culture: the poet and the naturalist look upon the returning flocks with other feelings than the hunter. Natural philosophers finally take up the migration as a very complicated problem for investigation. They regard it however from different points of view, and accordingly the question is treated in dissimilar ways.

Linné admonished natural philosophers to observe the migration of birds. But this collecting of data could not be done methodically before the necessity for simultaneous observations over greater areas had made itself felt by the examination of geo-physical questions, and in this direction a program was practically carried out. Quetelet then in the year 1841 asked that the changes in living nature also be noted in a comprehensive and regular manner, and his countryman de Sélys Longchamps proposed to collect exact data specially on the migration of birds. This repeated warning was now obeyed with zeal.

* Submitted to the Second International Ornithological Congress in Budapest, 1891.

† When in the fall of 1890, the Hungarian committee for the Second International Ornithological Congress in Buda-Pesth charged me with the duty of preparing the report on the state of our knowledge regarding the migration of birds, a far deeper obligation was at the same time officially laid upon me, the fulfillment of which required great exertions. But the prospect of being able to say a word on such an occasion about the subject which had become dear to me was too enticing; and in the hope that I might be able to discharge my official duties by New Year, I promised to place my small abilities at the disposal of the committee.

Unfortunately I was mistaken, and those duties are not yet performed. I did not perceive the full scope of the work until after it was too late to turn it over to another specialist. And so nothing remained but to perform in the shortest possible time an almost impossible task. No one can be more conscious of the defects of the present report than the person making it. When he ventures, however, to appear with it before the Second International Ornithological Congress, he does it in the hope that in passing judgment upon it the above circumstances may be duly considered.

The writer hopes to be able to treat the subject more exhaustively on another occasion, and thus to remedy in a measure the defects of the present treatise.

Helsingfors, *May 6, 1891.*

When the new impetus on the side of physicists and climatologists became felt, the inquiry assumed adequate form. The arrival data were considered like other climatological observations, and treated in the same manner as the notices on temperature, air pressure, etc., with the view of finding an expression for the climate of the corresponding regions. The bird of passage was regarded as a complicated apparatus, by the observation of which the climatological problems could be explained in certain relations.

Zoölogists however could not permit the bird and its migrations to be regarded merely as a means to serve other purposes, but both constituted independent problems in themselves, dependent only in certain respects upon climate, etc. It was asked therefore how the question was to be examined in this regard; and, as may readily be conceived, methods were for the time being suggested which the climatologists had already perfected.

Consequently the "climatological" material of data about the arrival and departure of the birds assumed the character of an avi-phenological material. By its proper treatment, it was desired to fix the course of the migration of the species of birds; and with this view first of all an effort was made to determine its time, in order to judge first by that of the direction of the migration. Two means were now available:

(1) Either data of arrival from *one* year only were to be used, because only such can be compared with one another (Kessler, 1852, South Russia);

(2) Or from as many years as possible, in order that the errors of observation, unavoidable in any case, could be mutually corrected. (v. Middendorff, 1855, Russian Empire.)

Both conditions were very difficult to fulfill, the former however, easier than the latter.

At all events, both methods offered a good view of the gradual progress of the bird species, as heretofore of that of the increase of temperature and similar climatological phenomena, but hardly anything more. The conclusion that the migration takes place at right angles to the isopipteses seemed justified; but, with regard to the direction of the bird's migration to be ascertained, could not possibly prove more exact than the premises themselves, the methods of inquiry, and the material.

As a reminder of the "climatological" conception of the problem and of the corresponding methods, something medium, something of arithmetical mean values had to attach to the result, which however did not exist in the phenomenon itself.

For the migration of birds is by no means a meteorological but a biological phenomenon. The question is about the advance of living individuals, who move according to their own will. Their paths must be investigated, and the result of this inquiry must be given much more exactly than the advance of the spring warmth. For, since the indvidual can pass over only one line of migration, the question arises whether flocks of individals, and even the whole species, can not in the same manner follow fixed routes which suit them. Otherwise, how does it happen that certain species appear in numbers every year in fixed places without showing themselves in the neighborhood? How comes it that the single individuals at last find their way back to their old nesting places? The climatological methods do not suffice to answer these questions; others must be applied.

As in any complicated problem, the order in which the different sides of the question are taken up is not a matter of indifference; for the knowledge of some of its sides indisputably helps to an understanding of certain others. Which side of the phenomenon of migration is to form the scaffold? Which must be understood first, in a measure at least, before the other questions can be properly asked, much less answered.

In fact, a principal problem of the investigation falls within the domain of zoö-geography, for it must, first of all, be established *which route* the species takes before the time required to traverse this route and other circumstances connected with its passage can be considered.

Sunderall pointed out empirically the course which the investigation had to take when, in the year 1871, he made known the routes of migration of the crane in Europe.

With the conviction that only a systematically considered investigation could explain the migrations, the writer examined the question (Palmen, *über die Zugstrassen der Vögel*, 1874, 1876) with particular regard to only a small number of hyperborean species. It seems necessary to go into the question step by step, and first of all to fix the route of those species of birds whose migrations seemed proper, to place in a clear light the methods of investigation to be employed; for in this relation it is not possible to compare all species of birds nor all the regions penetrated with one another.

In the choice of the zoö-geographic, and therefore of the avifaunal material to be employed, two different groups of facts especially present themselves.

(1) Either the general migration of different species would have to be investigated, which is known to have awakened the most intense interest of observers in many places;

(2) Or every individual species is to be followed separately through all the regions which it visits in its migration.

The enormous masses of migratory birds which pass through certain narrow-bounded regions long since attracted attention, for all descriptions from those regions are highly picturesque and entertaining. Nevertheless, these rather touch our sentiment than our reason. They leave the questions whence? whither? why? un-answered, simply because each species can, properly speaking, answer these questions only in its own manner, but we ourselves can not separate and understand the individual voices. The whole thing must therefore appear to us as a complication, though a very interesting one.

Here also, then, an average answer is of no use, but the whole must be closely analyzed. This however can not be attained by study of the general migration. Just as a tangle of threads can only be loosened by getting out the single threads, so out of the net-work of migratory routes, that is out of the "highway," the separate road of each species of bird must be picked out and followed. And this work pre-supposes the knowledge of the last named, which however can only be acquired where the migration assumes a simpler aspect.

The existence of narrowly limited roads, ramified in a characteristic manner for each species, was felt beforehand by zoölogists who, in their investigations, depended upon the less productive climatological methods. Thus in admirable manner von Middendorff compared the routes of separate species of birds with the different forms of shrubs and trees which were drawn on the paper. The leaves correspond to the nesting places, the branches are to compare with the routes, and the roots with the winter quarters. On the map the shrubs would bear their foliage not far from the roots drawn somewhat more to the south; the trees—likewise ramified, but of lofty growth—would take a higher place; and, unramified, some gigantic stalks would jut out, which on the map take root in Egypt, but with their branches shade the coast regions of the frozen sea, beyond the tree limits.

With the indispensably necessary geographical determination of the routes of separate species of birds, all reliable local faunas, as well as smaller lists, must first of all be used as material for investigation, and moreover, all notices of isolated discoveries of a species of bird in places which the migration passes. Such isolated notices and minor works are indeed the extreme roots of the inductive zoö-geographic inquiry. Out of the entire ornithological literature therefore a comparison is to be made of all accounts of the appearance of a species of bird. Country after country must make its contribution, and for each district a separate conclusion must be drawn for the species in question, and the result added to that of the next district.

The writer's investigation of the migration of some birds breeding in the Arctic regions, carried out on these principles, yielded certain results which need not be here considered in detail, but it may perhaps be well to note some of the main points.

During the migration, these birds do not by any means take any direction they please, nor do they follow everywhere one and the same direction, perhaps a "general migratory route." They rather follow well defined, geographically bounded ways, whose bends depend primarily upon the topographical relations of the regions. Other not more accurately examined species of birds also act in the same or an analogous manner; since their migrations, taking place almost everywhere, are to be traced to extremely ramified routes. The routes of migration may be grouped into several categories according to their character.

The order of migration of the individuals may be reduced to two simple types: (1) where a number of individuals move from one place to another without changing their relative positions (nacheinander-zug); (2) where the relative positions are changed, some moving more rapidly than others (vorüber-zug): with all transitions between the two.

The irregular migrations also are to be distinguished from the regular, and the stray visitors are to be considered in harmony with this principle. These stray visitors can essentially contribute in the spring in different ways to the extension of the breeding district.

Theoretically the origin of the migration instinct becomes somewhat more comprehensible than before; and the explanations found might be fit to cause a new discussion about the phenomenon of migration. This conception of the particulars of the migrations met with approval on many sides. Nevertheless the voice of Mr. E. F. von Homeyer was soon raised against it, from whose work (*Die Wanderungen der Vögel*, 1881) I have brought together the following leading views:*

Birds usually migrate in a fixed direction,—in the greater part of Europe essentially from northeast to south-west. The maintenance of this direction depends on the birds' sense of direction.

The passage of a particular species extends over every single point within its migration district; for the birds fly so high in the air that the direction of their flight becomes independent of the topography of the ground. From this height they perceive suitable resting places, where they can repose or stop. By combining such places, the pretended migration routes are constructed, which by no means correspond to the facts. The birds do not move along linear routes, at least not along coast ways. They move with an extended front, everywhere.

Only in isolated places are the birds compelled by insurmountable obstacles, like high mountains, to deviate from the general direction, and to collect together in larger numbers at one point than at another.

The erroneously so-called stray visitors have by no means gone astray. They are simply rare birds, which, owing to the incompleteness of the observations, are overlooked in most places.

All theoretical speculations on the migration are to be rejected. They are not only useless, but directly injurious; because they only forestall the real observation of nature, and, instead of advancing knowledge, merely open the door to fancy. The tendency to construct hypothetical migration routes for the birds has sprung from the now popular endeavor to trace everything back to the Darwinian doctrine.

In later years Hartwig followed E. von Homeyer's conception of the course of the migration (*Journ. f. Ornith.*, 1885, p. 427; compare also Hartert, *J. f. Ornith.*, 1887, p. 251 and 1889, p. 231).

The writer had already in the year 1882 published his "answer" to

* This is done with all reservation, for there are still isolated places in the work where the above views, formerly subscribed by the author named, are still held.

the author of the "migrations". That conception has at all events its authority as the personal opinion of the renowned author. It could however be fully accepted only in case it were founded upon a material of communicated facts, and conclusions had been reached from these in a strictly logical and conscientious manner. Because, however, these conditions were wanting, a purely scientific examination of the views given could be made only with difficulty or not at all. The discussion could only take the form of a personal defense, which could at all events somewhat clear up the question, dulled by the manner of the attack, but could contribute little to the further elucidation of the problem itself.

A little attention will suffice to show that the writer had neither subscribed to narrow "linear" routes for "birds" in general, nor the existence alone of coast and river ways. He maintained above all that the individual species of birds do not move irregularly, and also that each species is not obliged to follow a fixed direction in the sky. They act according to the local and continental relations of the ground, and, besides, each species according to its own nature.

There are then also species whose routes go through narrow plains, and which spread out here as far as the space permits. The migration routes likewise often lead through regions where these species find ample scope and can choose suitable roads within wider bounds. The migration lines of individuals and bands group themselves then in bundles, which, according to circumstances, branch out and run together again, and the individuals do not here confine themselves so strictly to fixed places, as in the narrow bounded sections of the route. In such wider roads, as may readily be conceived, a wider front is also developed. Even our opponents seem to allow that here also certain obstacles situated on the flanks prevent the unlimited extension of the individual routes. How many species of birds exist, which are not at all dependent on the conformation of the ground and on the resting places, can not be decided beforehand.

Every species, every variety, and every form differing in the slightest respect must therefore, if possible, be treated separately. A complex of species are to be examined together, which in the migration agree somewhat geographically. Thus in certain respects a want of material in some species can be to some extent supplied by analogies in others. Accordingly an average treatment of heterogeneous species can not be justified in a special scientific investigation of migration, and in a region where the most dissimilar species are known to follow one and the same direction or road, this surely has its cause in topographical conditions which perhaps lie further and are only perceptible in the whole mass. The importance of such highways to the problem of migration was discussed above.

A "universal direction of migration," which perhaps all the species of birds in the expanse of whole continents should follow, does not, therefore, correspond to a well founded conception of the phenomenon of migration. It is likewise evident that in an exact scientific discussion a special declaration for "birds" in general can claim no scientific value. Such general expressions are to be avoided, because they may easily lead astray.

As already remarked, the view of the existence of geographically defined routes of migration began to prevail gradually, and almost simultaneously with the completion of the writer's work similar statements appeared from the most approved quarters, for example, from Wallace and A. von Middendorff. The writer can not omit to mention that in a personal interview he received the full approbation of the last-named venerable investigator of Siberia. The approval of Aug. Weismann may be regarded as of great importance with regard to zoölogy in general, and finally, Radde's well-known statement "that everywhere on earth the direction of the migration intimately depends, and is even dictatorially conditioned, on the relief of the land which the birds pass" (1884).

For the continued investigation of the phenomenon of migration in the sense above mentioned, it seemed absolutely necessary to obtain a large quantity of observations from the most different places. For indefinite accounts of the occurrence of the species of birds in greater districts permitted no reliable conclusions regarding the local extent of the migration routes. It rather seemed desirable to obtain numerous observations on the migration, repeated every year, just as is done in the study of meteorological phenomena.

For these reasons Mr. Reichenow, at the spring meeting of the Universal German Ornithological Society in Brunswick, on May 22, 1875 (*Journ. f. Ornith.*, 1875, p. 347), pointed to the necessity of collecting observations on the migration of birds according to a uniform plan in diverse regions. It was then referred to a committee to promote the inquiry.

The calling into existence of this committee for observation stations of the birds of Germany is known to the world; as also its annual reports, which have appeared since the year 1876 in ever increasing size, and whose editing has reflected the greatest credit on Prof. R. Blasius and Prof. A. Reichenow. It is also further known how this example of Germany found imitators in Austria-Hungary, the observations made there being first (1880, 1881) received in the German reports, then from the year 1882, appearing independently.

Every ornithologist is also acquainted with the progress of development of this question in Great Britain, where observations were begun from the year 1879, especially in the light-houses, and published annually.

Independently of these efforts, investigations of faunas of different small and large tracts, as well as of general migrations, were started. More will be said further on about these, as well as about the magnificent arrangements in North America.

So far had the question advanced when, in April, 1884, the First International Ornithological Congress met in Vienna and gave a powerful impetus to the furtherance of the investigations. Circulars were distributed in most countries and observations collected, as appears from the following short summary.

Regarding the observations from Germany (including Austria-Hungary, 1881–'82), the following notices are taken from the annual reports which appeared in the *Journal für Ornithologie:*

Report.	Year.	Number of observers.	Species observed.
I	1876	38	256
II	1877	41	264
III	1878	41	259
IV	1879	29	264
V	1880	36	280
VI	1881	39	245
VII	1882	29	204
VIII	1883	34	216
IX	1884	113	254
X	1885	305	274
XI	1886	238	262
XII*	1887	....	....

* Ready for the press.

The first annual reports (1876–'81) contain only the special observations. In accordance with Mr. E. F. von Homeyer's wish (*Journ. f. Ornith.*, 1880, p. 357), a general volume has since 1882 been sent in advance, wherein, by bringing forward some examples, a picture has been given of the time of migration of the year in question. To this general description of the migration was added a short review of the meteorological incidents of the year with reference to their influence upon the course of the migration.

The observations were at first made for each species, mainly with reference to the biological phenomena (arrival, breeding, departure, wintering). The new impetus of the year 1884 brought changes in this respect also. The necessity for editing in a simpler manner the increasing mass of observations became apparent. The geographic arrangement of the notices was chosen, by which at the same time local faunas were restored. To facilitate inquiries, the countries were arranged in alphabetical order, with all their greater political dependencies.

Since 1886 it has been the intention of the committee to construct maps showing the geographical distribution of the birds of Germany. Such maps have already appeared relating to three species (*Corvus cornix*, *C. corone*, *C. frugilegus*; prepared by Matschie). The committee also intends to establish the migration routes of certain birds in the German territory.

At the annual meeting in September, 1888 (*Journ. f. Ornith.*, 1889, p. 60), the instructions for observers were somewhat changed, and drawn up again as clearly and briefly as possible. The observations relating to this matter, especially in the Kingdom of Saxony, are edited by A. B. Meyer and Helm (*Zeitschr. f. d. ges Ornith.*). They embrace the following number:

	Year.	Observers.	Species.
I	1885	43	180
II	1886	60	199
III	1887	131	215
IV	1888	122	213

As stated above the observations from Austria-Hungary for the years 1880–'81 were incorporated in the corresponding German annual reports. Since the year 1882, under the editorship of Prof. K. von Dalla-Torre and V. von Tschusi zu Schmidhoffen, independent reports from the first named countries have appeared in *Ornis* as follows:

	Year.	Number of observers.	Species observed.
I	1882	46	328
II	1883	85	314
III	1884	60	322
IV	1885	67	309
V	1886	65	....
VI	1887	79	341
VII*	1888	....	......

* Not yet out.

In the same years appeared also reports on the recently published works on the avifauna of the monarchy. A map giving a view of the geographical distribution of the observation stations in the Austrian territory for the years 1882–'88 was also published (in *Ornis*, VI. 2, 3). The arrangement of the observations in the reports thus agrees pretty well with the publications of the German committee.

Furthermore, annual reports have been published in different European countries on the ornithological results, arranged in almost the same manner, and accompanied by general statements on the topography of the region and on the meteorological phenomena of the year.

A catalogue by Studer and Fatio of the birds observed in Switzerland is in preparation, and maps showing the distribution of twenty different species are added to the first edition.

Denmark (by Lütken, O. Winge, H. Winge).

Report.	Year.	Observers.		Species.
I	1883	9	....	211
II	1884	7	....	157
III	1885	6	....	175
IV	1886	5	*24	155
V	1887	7	*33	164
	1888	......	*31	60
	1889	......	31	59

* Light-houses.

From Iceland a report has appeared (1886; *Ornis*, II, Gröndal), wherein 28 species are designated.

Gätke has published three annual reports on the migration of birds on the island of Heligoland, in the form of continued diary-notes (January, 1884—December, 1886) on the weather and on the observed species (*Ornis*, I–III). Furthermore R. Blasius published Gätke's work "Die Vogelwarte Heligoland", wherein are preserved the results of fifty-three years' observations of the circumstances of the migration of the birds on this remarkable migration station (*Ornis*, VII, p. 132).

Some reports appeared on the migration of birds in Holland:

I	1885	By Albarda, in *Ornis*, I; in *Tijdschrift der nederl. dierk.*, Ver. 1887, 1888.
II	1886	
III	1887	
	1885–1888	By Coenradts, in *Ornis*, V, p. 333.

Dubois also published the following reports from Belgium:

1885, 1886	In *Bull. du Musée royale d'Hist. nat. de Belgique*, IV, V.
1887, 1888(–1889)	*Ornis*, VI.

Three numbers of communications of the ornithological committee of the Royal Swedish Academy of Sciences, prepared by Sundström and Smitt, contain the observations made by 107 correspondents in Sweden during recent years up to 1886.

From the Russian Baltic provinces, especially Livonia, E. von Middendorff has sent three annual reports, 1885–'87 (*Ornis*).

Abstracts of the phenological observations made in Finland have been published by Ad. Moberg (*Öfv. Finska Vet. Soc. Förh.*) for a number of years; and in the years 1878–'89, arrival data in tabular form for 12 species of birds at 34–68 stations have been published. The remainder is still being prepared for publication; also a comprehensive material of observations which were sent to the writer in reply to a summons of the year 1885. Out of this material, only a few local faunas have been published by way of preliminary.

We may here remark that in most of the other countries of Europe and in some isolated states out of Europe, it has been resolved in principle to join the above mentioned efforts, and some observations are at hand ready for printing, while in others the way has been prepared for this accession only by a few calls to the friends of the birds.

The investigations on the migration of birds in Great Britain have, as is known, taken a form consonant with the insular character of the territory. At the instance of the British Ornithologists' Union and with co-operation of the British Association for the advancement of Science, observations were commenced at most of the British light-houses and light-ships. Yearly notices have been sent in from 100 or 150 of these stations since the year 1879, and a committee named by the association (J. Cordeaux, J. A. Harvie-Brown, A. Newton, R. M. Barrington, A. G. More, W. Eagle Clarke, and others) has looked after the publication of individual observations in nine "Reports on the Migration" (1879–1887), as well as in a brief report which has been made annually to the association.

Already in 1882 these notices proved the wonderful constancy with which the birds of passage year after year follow the same lines or great routes when they approach or abandon the British coasts, and this constancy points to a definite law governing the whole phenomenon. Two separate migrations may be distinguished: the great periodical waves which sweep from the breeding places in the far northeast and return; and further, quite independent of these, a constant stream of immigrants, which moves from the continent toward the southeastern and eastern coasts of England, across the southern part of the North Sea in the direction of east to west or from southeast to northwest. On the other hand, the west coasts, and especially Ireland, are comparatively seldom visited by these birds of passage.

Nevertheless, the stream of migration (1886) does not strike equally all points on the east coast of England, but appears constantly to follow fixed lines. For example, the Farne islands on the coast of Northumberland, as also the mouth of the river Tee, appear to be principal stations for the passage over the North Sea; likewise certain parts of the coasts further to the south. To the north of Norfolk, the migratory birds appear to penetrate through the Wash into the interior of England toward Severn, Bristol Channel, and still further westward.

It is further pointed out that the vertical height at which the flights take place must be accurately noted*, and that the light-ships appear to yield a better series of observations than the light-houses. It was also ascertained that only certain species were attracted by the light, and those in an unequal manner. The special direction and force of the wind (1887) appears to exert only a small influence on the great autumn rushes; but the direction of the wind which prevails during the passage in general, seems to play a more important part,—the direction of the journey and the angle of the route to the coast being to a great extent dependent upon it.

A large part of the birds of passage are in autumn driven far out on the Atlantic Ocean. Observations on the subject are noted on the most frequented lines of communication, and also made over to the committee.

The more the published material accumulated, the more desirable it seemed to utilize the whole mass in forming conclusions. This was already referred to in the report for the year 1887. It was further pro-

* Compare also on this subject Albarda, *Ornis*, I, pp. 592–594.

posed, in the year 1888, that the collecting of observations should be stopped temporarily, that the immense mass of facts in the nine reports, arranged in concise form, statistically, and in strictly scientific manner, might be treated as briefly and clearly as possible, in order to attain practical scientific results. This proposal was accepted by the British Association in the year 1888, and a member of the committee, Mr. W. Eagle Clarke, charged with the duty of directing the comprehensive undertaking.

The British example exerted a deep influence on the First International Ornithological Congress in the year 1884. Through the efforts of the authorities of the permanent ornithological committee of the Congress, the wish was communicated to as many state governments as possible that the passage of the birds might be observed at the light-houses. The authorities of these institutions are also charged to advance the question as much as possible, from the White Sea to the Caspian. The light-house officers on wide tracts of the coasts of South Asia, certain parts of Africa, Australia, and South America are in the same manner called upon to make observations.

As yet, however, we have but few printed reports from the light-houses, except from Great Britain. These few are the ones above mentioned from Denmark, 1886–'89, and two reports on bird life at the German light-houses, published by R. Blasius (*Ornis*, VI, VII).

In North America the investigations relating to the migration of birds have assumed quite an independent form and reached considerable dimensions.

In the year 1882 Prof. W. W. Cooke took the initiative with a systematic observation of the migration of birds, at first in the State of Iowa, but later in the whole Mississippi Valley. Thirteen observers were at work the first year and twenty-six in the year 1883. But after the American Ornithologists' Union had been organized in September of the last-named year, it appointed a committee for the investigation of the geographical distribution of the birds of North America, as also another for the investigation of the migration of birds. The two were however consolidated later on. By the co-operation of this committee with Prof. Cooke, the investigations were continued systematically under the direction of Dr. C. H. Merriam. The whole territory was divided into fourteen, later sixteen, districts, each under a superintendent; and for the year 1884 thousands of question-sheets and instructions were communicated to members of the most different social groups, as well as to all light-houses and other public institutions.

As it was to be expected that the homogeneous territory of the Mississippi Valley, in consequence of its immense extent from north to south, and the absence of mountain range or great lakes, would afford a particularly favorable field for the investigation of migration, the observations coming from there for 1884 and 1885 were taken in special charge by Prof. Cooke.

In the meantime, the observations collected had reached such a quantity that it did not seem judicious to expect the Union alone to elaborate the material. At its instance, a special Division of Economic Ornithology was established by the Congress of the United States in the Department of Agriculture, at first under the Division of Entomology, and with an annual subsidy of $5,000. Since the year 1886 it has operated as a separate Division of Economic Ornithology and Mammalogy, with an annual appropriation of $10,000. Economically supported in this manner, and under direction of Dr. C. H. Merriam, the prosperity of the institution is assured.

In the year 1888 the Division issued a most excellent publication on the migration of birds: Report on Bird Migration in the Mississippi Valley in the years 1884 and 1885, by W. W. Cooke. In this work are submitted the data which 170 observers collected respecting 560 species of birds. The author also shows his method of investigating the relations of the phenomenon of migration to the prevailing meteorological phenomena, by which method he has extensively utilized the synoptically grouped observations of the meteorological stations existing in the territory

The old experience that weather exerts an important influence on migration was now definitely confirmed. Prof. Cooke proved that the atmospheric centers of depression, moving from west to east in the spring, according to the laws of meteorology, and which are characterized by corresponding phenomena of wind and temperature, produce in every region changing relations of temperature, namely, alternate warm and cold periods, which changes are accompanied by definite migration phenomena. A "warm wave" in the atmosphere of the region in question is also a necessary condition for the beginning of a "bird wave," "migration wave," whose further progress is checked by the occurrence of a cold period until a new warm wave again pushes it forward and gives rise to others.

Prof. Cooke specifies the time of these waves for the period chosen (1884) and for the territory in question. He expressly states that his investigation, on account of certain circumstances, is not a complete one, and that such a series of observations must be well prepared, and carried out besides under favorable conditions. "Under such adverse conditions no attempt would have been made to study the bird waves were it not for the extreme importance of the subject. It is during the nights of bird waves that the bulk of migration takes place. To study migration successfully it must be studied when most active. Moreover, it is on bird waves that the action of the weather is most apparent: hence these waves furnish the readiest means of studying the relation between meteorology and migration. The greatest drawback is met with in the difficulty of accurately observing and reporting bird waves. It is by far the hardest part of the field work in the study of migration, and requires more time and more constant presence in the field than most observers can give."

It is further pointed out by Prof. Cooke that the expression "bird wave" may be taken in a double sense, and consequently answers to two methods of investigation. First, a bird wave comprises a very large number of individuals of one or of several species, which extend at one time over a certain territory. In studying such a wave it is necessary to determine the species of which the moving mass is composed and the bounds of the territory over which the wave extends. Second, certain species of birds which are proved to move in company on the same day may also be regarded as a wave, whose progress from day to day and from week to week must be accurately observed.

By a critical study of these points of view and by a conscientious use of the meteorological and ornithological observations which embrace a precise time and a precise region, the rapidity of the migration, on which so much has heretofore been written, can be calculated. Only by attentive observation and multifarious labor can the migration be followed until the moving flocks have reached their resting places.

It is evident that Prof. Cooke's investigation of migration over a long continuous route, apart from all unavoidable shortcomings, must be particularly adapted to elucidate migration with respect to its course and its outward conditions; and that it is very desirable that similar investigations may henceforth be undertaken in suitable regions.

This course is opposed however by the difficulty of finding a sufficient number of capable observers. Prof. Cooke has been able to rely in essential degree upon one conscientious and expert observer, Mr. O. Widmann, of St. Louis, whose methodically arranged notices he submits in his work. In this connection the writer will only quote the words of a competent judge:

"A dozen observers like Mr. Widmann, scattered at proper intervals, would give a fairer basis for generalizations than hundreds of observers of the grade on whom Prof. Cooke was obliged to depend for many of his data. This should stimulate the more experienced and well qualified field ornithologists to contribute to the fullest degree possible to the furtherance of this important investigation.—J. A. Allen, *The Auk*, 1889. VI, p. 61."

The continued meteorological phenomena were rendered in the usual manner by synoptical maps, which alone made a view possible, and essentially facilitated the study of the influence of weather on the migration of birds. It may not therefore be injudicious to here refer to an attempt to represent graphically the migration of birds and the composition of the avi-fauna, changing with the season, as Mr. W. Stone has proposed (*Auk*, VI, p. 139).

Besides this material of observations, made chiefly with a view to explaining the problem of bird migration, numerous faunal works have appeared—minor local catalogues, and comprehensive works on the birds of larger definite geographical areas, prepared with conscientiousness and intimate knowledge. No one perhaps can value these avifaunal works more highly than the writer, with regard to their importance for the determination of the migration routes. External circumstances forbid us to notice all these works here, although they may be worth it, like those of Radde, Pleske, Olph-Galliard, Oustalet, Dresser, and others, and notwithstanding they contain many single results, many valuable thoughts on bird migration.

On the other hand, it seems proper to here call attention to some special investigations on bird migration in certain regions, founded on avifaunal material.

Some places have long been known in the western portion of our continent where migrating birds collect in crowds because obstacles situated on the side, like seas, allow a passage here only; or, like mountain regions, only here leave the door open. Such highways have been examined farther eastward in regions which are important for the migration between great districts.

From his comprehensive observations in the years 1857–'79 in the Aral-Thian-Shan region, N. Severtzow has given us (*Bull. des Nat. Moscow*, 1880,) a very brief view of the highways where the greatest numbers of birds of passage congregate. He designates cartographically three groups of such routes: A, through the Kirgheez steppes, from the river Ural to the Sir, characterized by the enormous number of merely passing birds; B, along the western border of the Thian-Shan mountains, distinguished by a tolerably large migration, and by the circumstance that the native summer birds give way to winter guests; and C, through the interior portions of the range mentioned, known chiefly by the annual change of species just alluded to. The wintering on the routes B and C is owing to the warm springs existing there.

Severtzow mentions further the connection of the routes named with the known migration routes along the Irtish and the Ob on the one side; on the other with their presumptive continuations in Persia, Afghanistan, Punjab, and in the region of the Indus. He designates the Altai as a region whence the migration routes diverge: in the southwest toward Russian Turkestan; in the south towards the desert; and in the southeast towards China. The movement of the birds of passage in a vertical direction from the highland of Thian-Shan and Pameer towards the low grounds is also suggested by him.

Referring to the details to be specified later, Severtzow here communicates some general points of view (pp. 282–284). The course of the steppe routes depends on the existence and situation of the waters in them. On the other hand, the general extent of the lofty Thian-Shan range compels the flocks moving at a short distance from it to follow a general direction from east northeast to west southwest; therefore, great masses of birds of passage are pressed together on the western border of the range (Tschemkend-Tashkend), while these masses can spread out again to the northward, as also to the southward.

"This divergence toward the north (more or less considerable) depends essentially upon the fact that each species has its particular routes, the direction of which is modified according to the nature of the localities which suit this species, and which it seeks also during its migrations, at least for stopping places; and for this reason the migration routes even of a single species, starting from different parts of the region which it inhabits in summer, are not parallel, but for many species convergent towards the south, for many others divergent. This applies much more forcibly to the routes of a number of species which, in the season of migration, concentrate in some locality particularly abundant in birds of passage."

The author therefore categorically maintains that the individual species, according to their peculiarities, are dependent on the topographical relations of the territory through which they migrate, and that the routes of each species accordingly assume a particular geographical form.

In the same spirit Prof. Menzbier also reports (1886, Bull. Soc. Nat. Moscow) on the results of his investigations on the migration routes of the birds in European Russia. He unconditionally joins those inquirers who maintain that each species moves along peculiar, strongly marked routes, because during the migration they are dependent upon the condition of the country through which they happen to be passing. He agrees in dividing the routes according to their topographical character into categories, which, however, are complicated by transitions.

He also lays stress upon the fact (pp. 333, 351) that the relations of the ground and the conditions of procuring food do not always suffice to explain the situation of these routes; but that the routes mark fairly well the ways along which a species has once migrated, and that about the same ways are still utilized as a result of inherited tendency. (Compare on this subject the writer's *Zugstr.*, cap. x, as also Weismann.)

Menzbier furthermore refers (p. 354) to the importance of the spring stragglers (compare Palmén, *Zugstr.*, ix, p. 238 *et seq.*), which under favorable circumstances may remain in a region where they have arrived by chance, and, nesting there, may change the bird fauna in a characteristic manner. Finally the author discusses the order of migration of the individuals, and appears to favor the belief that they change their relative positions (Vorüberzug). In conclusion, he declares that migration routes in the course of time, in consequence of geological changes in the topography, may pass from the compass of one group into that of another.

To these results, which agree with those of the writer, Prof. Menzbier adds corrections of some of my statements and conclusions, which are worthy of acknowledgment. The conclusions reached in the years 1874–'76 respecting the migration routes were founded on facts which certainly suffice in a measure for the western and central portions of Europe, but not for European and Asiatic Russia. The more abundant material of observations now available from the latter continental districts shows that the conclusions based mainly on the oro-hydrographic conditions in the West do not fully answer for the more eastern parts of the palæarctic region, where the ground takes another form. Many species of birds (for example, *Hæmatopus*, *Strepsilas*, *Totanus calidris*, *Limosa rufa*, *Phalacrocorax carbo*), which in western Europe migrate along the seashore, breed and migrate also in the interior of continental Russia, along rivers, salt lakes, and on the steppes. *Harelda glacialis* moves regularly along the Kama and the Volga, as well as behind the Ural, along the lakes—is even said to nest here—and winters on the Cespian Sea. These birds belong, therefore, in the East, to the group *submarino fluvio-lacustres*.

Accordingly, Prof. Menzbier distributes the birds of passage in somewhat different manner. He thinks that glacial littoral routes do not exist,* and groups the accepted routes also in another manner in the different categories. The following table shows the respective arrangements of the last-named writers:

Palmén, 1874–'76.	*Menzbier, 1886.*
A. *Viæ (aves) migr. aqr.*	
I. *pelagica.*	
II. *litorales.*	1. *Viæ (aves) marinæ litorales.*
a. glaciales-lit.	
b. pelagicæ-lit.	*a. pelagicæ-lit.*
c. marinæ-lit.	*b. marinæ-lit.*
	2. *Viæ (aves) continentales* and *submarinæ litorales.*
d. submarinæ-lit.	*c. submarinæ-lit.* *d. subm.-fluvio-lacustres.*
e. fluviatiles-lit.	*e. fluvio-lacustres.* *f. fluvio-litorales.*
III. *Palustres.*	*g. palustres.*
B. *Viæ (aves) migr. terrestres.*	
IV. Various groups not distinguished.	*h. continentales.*

After Prof. Menzbier has in this way considered and grouped his migration routes from the standpoint of their peculiar topographical character, he dicusses them with regard to their geographical position. The author designates on two maps the routes of European Russia found by him, basing his work on his considerably greater material from that country—greater because new observations were at his disposal, and above all because he has brought more southern species also within the scope of his investigation.

The question therefore is less about routes of individual species, for the author himself says (p. 320) he has paid little attention to such.

*His report of my division of the category "*viæ pelagicæ*" (p. 6) is not correct.

than about routes of communication (highways), which lead certain groups of migratory birds from their habitats in European Russia to their winter stations. This already appears from the names of the routes: *Via norwegica*, *Via baltica*, *Via pontica*, *Via caspica*. These all follow, at least partially, greater bodies of water, but receive their supplies also from the interior of the country. All the ways on the first map correspond more or less directly to the routes which are settled upon in the writer's work.

On the other hand, Prof. Menzbier's representation contains entirely new assertions about the continental routes (map 2). According to the writer's method—but without giving fully the material of observation, which is now indispensably necessary—he has studied the habitats of 13 eastern species of birds with reference to their geographical distribution, and finds this explicable only by the assumption of certain migration routes, which he designates upon the map without claiming to have thereby exhausted the question (p. 349). In the text, he completes their continuation towards the east. The routes are provided with names in the same sense as those before mentioned: (1) *Via sibirica* begins in the northern half of European Russia and passes in an easterly direction through the Siberian plains, on one side to the sources of the Ob and Irtish, on the other side to Lake Baikal, along the northern slope of the Ekta-Altai, out of Dauria in a straight direction towards Urga, and through Gobi to the Ala-Shan. A branch goes besides to the Kuku-nor. Individuals for example of *Emberiza aureola* from European Russia might winter in southern China, and those from southeastern Siberia might pass through southern China to winter in East India. (2) *Via turkestanica* also leads from the northern half of European Russia and from western Siberia, but towards the southeast, between the Caspian Sea and the Thian-Shan (therefore in part Severtzow's highways), to the winter quarters in northwestern and central India. (3) *Via transcaspia*, (partly coinciding with *V. caspia*), leads to winter quarters on the southern side of the Caspian Sea and the steppes lying to the east of it, as well as probably to the upper Oxus and the sources of the Indus. In eastern Asia this way might in part coincide with the *Via sibirica*. (4) *Via anatolica* leads out of the Kirgheez and Calmuck steppes to the Black Sea, through the Bosphorus to Asia Minor, Syria, Palestine, and northern Arabia.

As a matter of course, Prof. Menzbier has founded these assertions on the material which was at his disposal. The facts themselves were, however, as before stated, not laid before the reader in detail, but in short abstracts, accordingly all his conclusions relating to the position and the ramifications of these routes are withdrawn from all control, and from any improvement in consequence of newly discovered facts. It is therefore quite impossible for the writer to judge scientifically these routes of Prof. Menzbier. Only personal opinions, formed according to analogies, can be entertained on this subject; and the writer will by no means pass off his objections as critically weighed refutations.

These continental ways appear to me in a great part very doubtful. The routes from west to east especially are of an enormous length, like that from the Dwina to South China, lead entirely through the interior of a continent, without the guidence of a homogeneous well characterized conformation of soil, and touch alternately great forest, desert, and mountain regions. It is difficult to conceive how birds of passage could find their way on this route. It seems much more probable that we know too little just now about the occurrence of the species examined, some of which are difficult to distinguish, and that their winter stations are eventually to be sought much nearer, at the most in the regions whither Severtzow's routes lead. In the meantime, I will not venture upon a scientific judgment in this respect until the author submits the facts on which his opinions are based.

After this short statement of the development of the question of the migration of birds, the writer takes the liberty to cast a retrospective view upon it. In the study of the migration of birds, two kinds of material for investigation present themselves—the avi-phenological and the avi-faunal observations. Since the first-named were collected for this very purpose, it was at first thought that the investigation should be commenced on this side. The avi-phenological material explains the times of migration, and from these results an attempt was made to infer the directions of the migration. Nevertheless these results appeared too inexact to serve as a starting point for further investigations. An attempt was therefore made to take the opposite course, first to fix the migration routes from the avi-faunal material, and afterward to employ the method just mentioned with regard to the times of migration.

An attempt to fix geographically the routes of some species of birds proved that the question could be advanced in this way. The need for more abundant material now made itself felt, and new observations were zealously collected year after year in different countries.

It must be conceded that at present very considerable energy is devoted to the investigation of the distribution of birds and of the secrets of their migration. The annual reports, which contain observations from numerous stations, are multiplying. This gratifying increase in the material of facts which are to extend and deepen our knowledge of migration, is characteristic of the ornithological inquiry of the past years. It is to be desired that these efforts may continue and may be further completed, and that in consequence the quantity of material shall go on increasing. Nevertheless the condition of affairs may also examined and judged from another side.

In such investigations the quantity of observations will certainly not alone decide the question. The material is also to be treated scientifically. The inner connection of the facts and their fitness to form a basis for conclusions must be tested, in order that we may judge whether the method adopted is adequate or whether it can be completed in any way.

It seems high time to consider the matter from this point of view also, if the scientific character of the investigation, and with it its purpose also, is not to be laid at stake.

That this conception is shared by others we know from the fact that the result of nine years of British observations is now subjected to scientific treatment whose results will surely advance our question; further, from the fact that in North America, in a region where the conditions of the ground offer only small difficulties to the judgment of the direction of migration, an investigation of the time of migration and of the relation of the migration to the meteorological conditions has been undertaken, which has disclosed new points of view. The systematic observation of bird migration along certain lines, started in Hungary in the year 1890, and a statement of which is expected on occasion of the Second International Ornithological Congress in Buda-Pesth, also affords proof that we are not now satisfied with mere observations alone, but want these used scientifically, and that hereby new demands will surely be made of the observation in future.

Because therefore at present the existence of geographically fixed routes for the individual species is becoming more and more acknowledged, and these are to be determined from the material at hand, it seems timely to discuss the question according to the method to be employed.

Two different methods seem to present themselves, both of which have their advantages and disadvantages: (1) The migration routes of all the species of a certain district are examined by the investigators of that district and reproduced cartographically: (2) One species for itself is examined monographically in the largest possible geographical area and reproduced cartographically.

The first method offers many advantages. The workers concerned are masters of the language of the district, and the entire local literature is accessible to them, even to the most insignificant writings. They can judge of the reliability of the observers at each station, exercise a final control, and complete certain points by correspondence. At all events the native inquirers will thus be able to more completely group all the facts from a given country, as well as to watch over them critically. In the second case the monographer can study more closely the species treated by him, then special variations, their nature, as well as also the specific peculiarities of their flight. He will perceive more readily the difference in the flight during the successive sections of the route, or on different routes; in short, the route as a whole can be judged in a more exhaustive manner.

In the first method the following disadvantage becomes apparent: that in every country, unless it is a very large country, we get only fragments of migration routes, which perhaps can not be brought into continuity with those of the neighboring regions. On the other hand, difficulties will arise in the second case regarding the use of the literary sources of information. It would seem therefore the most practical, if by a combination of the two methods the advantage of each could be kept in view.

In a practical view, it would be advantageous if in every country all accessible data on the occurence of all the species met with in the region should be brought together into a national avifauna, in which details also could be accurately specified in concise form. Although the writer by no means under-rates the importance of such a work to the population of the country itself, yet attention must be called to the general advantage which science might derive from the translation of such a work into other modern languages. As a model work of this kind, I take the liberty to name Pleske's *Ornithographia Rossica*.

It would also be very much to the purpose if at the same time the habitat of the individual species could be indicated cartographically, as has already been done in Germany and Switzerland. By such work the investigation of migration routes would be greatly facilitated. The more complete the material at hand, the more suitable appears the method of determining the route of each species for itself. At least the writer would unconditionally give the preference to this method.

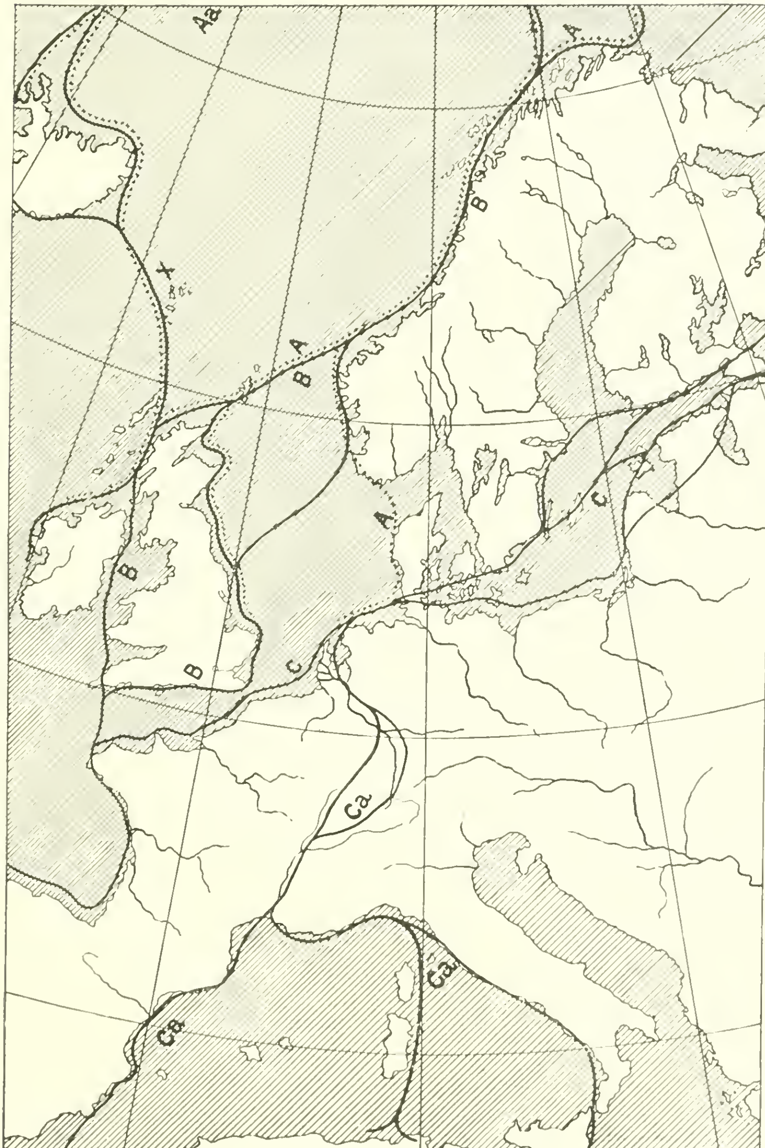
Finally we come to the question of the distribution of the work. The writer takes the liberty to urge once more that a choice be made of species to be investigated, because at present we are still at the threshold of the investigation, and it seems advantageous to first take up the less difficult species. Among all the categories of birds of passage, the littoral without doubt move along the routes easiest to be determined. Among the continental, on the other hand, those which avoid high mountains, like the swallows and their congeners, might be easier to study.

One more division of work appears to me advisable. Since every inquirer is specially interested in the species of his own country, it might be suitable for the northerners to investigate their species with regard to all their migration routes, the southerners in like manner theirs; further, that the eastern species of the palaearctic region should be taken up by those who are masters of the literature relating to them. That the chief interest of the Americans is directed towards their own species, they have already proved by the fact.

In ascertaining the migration routes of a species, it would be indispensably necessary to record all facts which contributed to the result; then only can the conclusion drawn from such premises, the migration route, claim real validity. The cartographic representation of the material, when at all possible, is highly to be recommended.

A model procedure for the investigation of the individual species can not be prescribed. It is rather to be expected that each inquirer will learn something from the practical methods of the others. The manner of representation will then develop of its own accord.

In conclusion, it is hardly necessary to call attention to the fact that a very inviting field of inquiry, in the same direction as that entered upon by Prof. Cooke in America, is open to those ornithologists who are sufficiently versed in practical meteorology. It is however to be foreseen that the phenomenon will be much more complicated in Europe, and that for this reason the investigation is to be commenced with those species whose routes have already been fixed geographically with some degree of certainty.



Section from Palmén's Map of the Main Migrating Routes of the Littoral, except Fluvio-littoral, Birds in Europe; in Ueber die Zugstrassen der Vögel, Leipzig, 1876.

THE EMPIRE OF THE AIR:

AN ORNITHOLOGICAL ESSAY ON THE FLIGHT OF BIRDS.*

By L. P. Mouillard.

INTRODUCTION.

If there be a domineering, tyrant thought, it is the conception that the problem of flight may be solved by man. When once this idea has invaded the brain, it possesses it exclusively. It is then a haunting thought, a walking nightmare, impossible to cast off.

If now we consider the pitying contempt with which such a line of research is appreciated, we may somewhat conceive the unhappy lot of the poor investigator whose soul is thus possessed.

Many of these searchers, either through pride or through timidity, have withdrawn themselves from human intercourse, and have found themselves paralyzed by attempting to carry on their experiments in secret. They quickly found themselves so cavalierly classed as dreamers or as lunatics that they were compelled, under pains of complete discredit, to conceal from others this so-considered flaw in their intellect.

It must however be acknowledged that this persecution has much diminished during the past decade. We are no longer classed with the seekers for the quadrature of the circle, or for perpetual motion. There has been progress since Charles, Janssen, Quatrefages, and other recognized scientific authorities, have been bold enough to affirm that they believed that the problem can be solved. We no longer risk the lunatic asylum, but the general public still considers us as mentally unsound.

The public understanding, moved by the assertions of some scientists, has made some progress. There were two roads to possible success, the one broad, beautiful, smooth, and bordered with flowers, but after all leading to no result; it was that of aërostation, of balloons lighter than the air. The other way was contrarywise, a rough, narrow, rugged path, bristling with difficulties, but still leading to something; it was that of aviation, of rapid transit by machines heavier than the air. Most of would-be inventors have taken the easy road, and from the height they have gained, pityingly look down upon the unfortunate

* Extracted and translated from a work entitled "L'Empire de l'Air; Essai d'Ornithologie appliquée a l'Aviation." Octavo · pp. 281. Paris, 1881.

aviators still floundering in the quagmire, with little thought that they may have to come down to this same quagmire in order to get somewhere.

O! blind humanity! open thine eyes and thou shalt see millions of birds and myriads of insects cleaving the atmosphere. All these creatures are whirling through the air without the slightest float: many of them are gliding therein, without losing height, hour after hour, on pulseless wings without fatigue; and after beholding this demonstration given by the source of all knowledge, thou wilt acknowledge that Aviation is the path to be followed.

It is therefore apparatus "heavier than the air" which I propose to study; and I mean to grasp the monster by the horns. I expect to have as a guide and as a support that potent creator of all prodigies, Nature herself.

She has wholly ignored the principle of "lighter than the air" in designing her creatures, and all her flying animals are heavier, much heavier, than the air which they displace. We can not err if we faithfully follow her teachings.

There are two methods of investigating such an arduous problem; one may be termed the "closet" and the other the "open air" method. The first calls in the aid of mathematics, it applies them to some few observations, more or less defective or irrelevant, and relying upon this fragile foundation, it expresses by a goodly show of equations all that the observations teach—and generally a good deal more.

Mathematics are doubtless useful, but they are less indsipensible than is generally believed towards the solution of this difficult problem. This arises from the fact that the basis of operation, the formula, is always erroneous.

Nothing seems more simple than to say: "Given, that we know that *V*, *R*, and *P*, are equal to some other compound factors, then it must follow,"—and then quadractic equations and calculus come in, and the student reaches a final result, which completely disagrees with the facts.

When we start from false premises, we arrive at some conclusion just the same, but it is not the object sought. But even if the formulae be correct, it is certain that for ninety-nine in one hundred intellects, including even the computer himself, a mathematical result will never be as convincing as a clear explanation of the phenomena, or what is much better a conclusive experiment.

Thus, I conceive mathematics to be an interesting instrument of research, but not a convincing argument. I will not resort to them as a means of persuading others of the probability of success, because I feel well convinced that I never will meet with anybody willing to hazard his life upon the bare dictum of a formula.

Historical.—There is nothing new under the sun; and for the problem of flight, as for many others, this old proverb is true.

In the farthest antiquity the problem is presented to us as having been solved by Icarus. What is there absolutely impossible in that assertion? With close observation, good sense, and inventive faculty success may be accomplished; Icarus, perhaps, had these marvellous good gifts. - - -

At a later period, balloons came with their enormous bulk athwart the question; for eighty years they obscured the way to success. They led men's minds estray into conceptions without issue, and inventors have all, one after the other, brought up against the impossible. - - -

Balloons.—At the first glance there seems to be a close connection between the power of ascending into the air, and that of progressing through it, and yet half a century's consideration has shown that there is a profound abyss between these two orders of ideas; they prove in point of fact to be directly opposed to each other.

THOUGHTS ON AVIATION.

I have already said that Nature, provident, infallible, always knowing far more than the most attentive study can teach us, points out to us the way to imitate her works.

Let us not seek to be wiser than she; let us in all simplicity follow where she leads; thus shall we arrive at a result easily, without fatiguing our brains with that Chinese puzzle,—that mathematical compounding of x and y and z, which at the present day invades all arduous questions.

By merely observing with close attention how the winged tribes perform their feats, by carefully reflecting upon what we have seen, and above all, by striving correctly to understand the *modus operandi* of what we do see, we are sure not to wander far from the path, which leads to eventual success.

Methods of Observation.—To be really fruitful, observation must possess several peculiarities and qualities. In the first place, we must see accurately and then we must understand what we have seen, and then again we must apply our acquired knowledge to the detailed investigation of the performances of the great masters in the art of flight.

To see accurately, it is not only necessary to have good eyes, to know how to keep in the field of the telescope a bird going at full speed, but still more, to know what to look at, what it is important to observe. For instance, when an amateur, little accustomed to this kind of observation, hears an expert affirm, peremptorily, that the little black dot just perceptible in the sky is a male kestrel falcon (*Faucon crécerelle*), he fancies the expert to be wool gathering, and yet the assertion is quite true.

Given the black dot perceived by the expert, who has acquired skill in such observations, a kestrel falcon is easily recognized in the air, whether in soaring or in flapping flight: its long tail is a sure index; there is no possibility of confounding it with a raven, a buzzard, a kite, or even with some other species of falcon; its peculiarities are too plain. Now, as to the determination of the sex, nothing is easier. One need only observe the bird for a few moments, the male discovers himself by the petulance and rapidity of his beats, by the energy of his movements; the female is more supple and less ardent in her mode of cleaving through the air.

As for the "Pharaoh's chicken" (*Percnoptère*), the case is again easy. Afar off it may be distinguished amid a flock of kites, which it often accompanies, by a slight peculiarity in its flight, a remarkable unsteadiness in its forward progress, also by the narrow width of its wings, and by their decided rectilinear set athwart the body, for they are partly folded or flexed only when the wind is very strong. As to the male bird, he may be distinguished from the female as far as the eye can reach by his color, for he is white and the female is dark brown.

The great tawny vultures (*Gyps fulvus*) are to be recognized by their steadiness in soaring, by the amplitude of their circling sweeps, and by the majestic deliberation of their movements.

The arrians (*Vultur monachus*) and the oricous (*Otogyps auricular*) are noticeable by the exaggeration of all these latter qualities and by a darker plumage.

As for the bearded griffin (*Gypaëtos*), its long tail, broad and rounded, easily discloses him afar off; there is no bird of similar outline among the large soarers.

Here then in all its simplicity, is the explanation of a feat of discernment which generally astonishes the inexpert. In order to determine accurately the kind of bird seen afar off in full flight, it is simply necessary to have observed it long and well. When eagles have started off within 50 yards, and the eyes have followed them many times, the evolutions have become photographed on the memory: and later, on other occasions, when the same rhythm of movement is perceived, there is no longer need to concentrate the attention on the shape of the claws to determine whether the bird in view be an eagle or a vulture. - - -

Close proximity is greatly to be desired in studying the manœuvres of birds. I have been enabled to observe at very close range several kinds: the crows, the kestrel falcons, the peregrine falcon, the kite, the Egyptian vulture, the pelicans, the tawny vultures, etc., have yielded many of their secrets to me.

I will not here amplify on what I say as to the crow and the kite in the chapter devoted to them: in Cairo it is easy to touch the latter bird in full flight, by going about it dexterously, but the most stirring, exciting sight (the word is not too energetic) is to stand in the vulture roost on the Mokatan ridge, near Cairo, and to look upon the *Gyps fulvus*, passing within five yards in full flight.

How useless to seek to describe this spectacle! When these enormous birds rush by so close to you, an astonishing rustling may be heard; the great primary feathers vibrate like tongues of steel, and flex upward to a quarter circle under the fifteen pounds of bird they bear.

There the great vultures gather in hundreds; the "Egyptian vultures" are no longer to be reckoned, they are but a garnishing, while the kites creep in among the lot and make themselves small, and the great raven (*Corvus corax*) incessantly croaks against the invasion of his domain. Beak blows are numerous; each smaller bird must keep his distance, for if he passes within neck length, a savage peck he gets from the vulture. The larger birds are scarcely more amiable to their own species; if interference threatens in alighting, a shrill warning cry is heard, a blow impends, and the weaker must dive away, to begin all over again the complicated evolutions required to check the motion, and to alight upon the perch in safety.

One of the maneuvres which always astonishes the observer is the alighting. The great vultures arise above the perch at the average height which they generally keep above the ground—that is to say, some 500 or 600 yards above it. Having reached the terminus, they sweep around for a few minutes to inspect the topography, and then they determine to descend. The eagle comes down like a meteor; he is so powerful that he can control his movements at 100 miles an hour; but the great vulture has no such strength of pectoral muscle. He drops perpendicularly like the eagle, but he seldom folds his wings to gain speed. He would come down too fast, and the descent is sometimes very great; for I have seen birds which were already in full descent when first they appeared at the zenith, say at a height of nearly 2 miles. If they had then folded their wings, and allowed acceleration to occur, they could no longer have controlled their velocity; they would have been disabled, for their power would then have been inadequate to a change of direction.

Next to close and accurate observation a proper understanding must be attained. This second stage is more difficult to reach than the first; and this results because we must discard many pre-conceived ideas which obscure the eyes of the mind. * * * Then the observations must also be accompanied by accurate data.

We can no longer accept the immense dimensions and the monstrous weights of guess-work. We must have exact measurements and accurate live weight of birds in full health and in normal condition. Above all, it is indispensable that the observer shall be enough of an ornithologist to determine at once the species and peculiarities of the bird he is looking at; not merely on the dissecting table, but also afar off, on a perch, and especially in full flight.

This knowledge is to be acquired neither from books nor from museums: it must be obtained by much conning of the great volume of nature, by taking account and thought of the various movements, operations, and evolutions of the birds, by becoming acquainted with all their manœuvers, and above all by understanding them correctly, the how and the wherefore. Without all this information success is not possible. If the man does not clearly understand what the bird does and intends in a given position and a certain conjuncture, how can he hope to imitate its flight?

The observer must constantly set problems for himself, in the hope that occasionally the bird will demonstrate a solution. Thus, I was convinced, *a priori*, that an expert soarer could, in a fresh breeze, rise directly into the air and advance against the wind at the same time. I felt sure that the feat was feasible. I waited for years before witnessing this evolution. At last one day in Africa, two eagles in love afforded me this spectacle. One of them launched from the top of the ash tree which served as a perch, descended against the wind 6 to 10 feet, was raised up by a gust of wind, and thus continued to rise, slowly, steadily, for a hundred yards into the air, while he also advanced some 50 yards against the wind, without a single beat or impulse of his mighty wings.

Such convincing demonstrations are not to be seen every day; they must be persistently awaited; the observer must burn with the sacred fire; he must be drawn to the study of flying creatures by that undefinable enthusiasm which shall cause his heart to throb when he witnesses certain evolutions. - - -

It is but rarely that a bird manœuver is absolutely incomprehensible; for peculiarities and motives not understood upon a first demonstration are explained by fresh observations made under happier conditions. In all cases, to learn the how and wherefore, the study must be a labor of love.

All my life shall I remember the first flight which I saw of the *Gyps fulvus*, the great tawny vultures of Africa. I was so impressed that all day long I could think of nothing else; and indeed there was good cause, for it was a practical, perfect demonstration of all my preconceived theories concerning the possibilities of artificial flight in a wind. Since then I have observed thousands of vultures. I have disturbed many of the vast flocks of these birds, and yet, even now, I can not see one individual passing through the air without following him with my eyes until he disappears in the distant horizon.

Fruitful observation requires that the model be well chosen. Ordinary observers are confined to the bad examples which are found in their locality. They can only study the flapping birds—the pigeons, the bats, the little insects even. What good is to be got from studying a model which can not be imitated on a larger scale? It is impossible to reproduce an insect, a sparrow, even a pigeon, upon proportions which will carry a man. No material will bear the strains of wing beats as energetic as those of the sparrow. Steel itself is too weak in proportion to weight. - - -

Common sense indicates that the weak can only aspire to light tasks. Which then are the birds that expend the least energy? They are clearly the soaring birds, sweeping over great distances, by the sole power of the wind.

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The vulture's needs are few, and his strength is moderate. To earn his living he but needs to sight the dead animal from afar. And so what does he know? He knows how to rise, how to float aloft, to sweep the field with keen vision, to sail upon the wind without effort, till the carcass is seen, and then to descend slowly, after careful reconnaissance and assurance that he may alight without danger, that he will not be surprised, and compelled to percipitous and painful departure. And so he has evolved a peculiar mode of flight; he sails and spends no force, he never hurries, he uses the wind instead of his muscles, and the wing flap occasionally seen is meant to limber up rather than to hasten through the air. And so the true model to study is the vulture—the great vulture. Beside him the stork is as a wren, the kite a mere butterfly, the falcon a pin feather.

Whoso has for five minutes had the fortune to see the Oricou vulture in full sail through the air, and has not perceived the possibility of his imitation by man, is—I will not say of dull understanding, but certainly inapt to analyze and to appreciate.

ORNITHOLOGY; SURVEYED IN FLIGHT.

And here I must deliver a little lecture upon ornithology, from a point of view vital to the question, that is to say, the acts of flight,—a point of view, queerly enough, which is generally ignored in books.

Flight is the bird's chief peculiarity; it is his one good gift, so let us rapidly review the acts of the creatures which travel on the air.

The lowest class is that of the insects. All of them progress by beating flight; they are rowers (*rameurs*), save perhaps some mid-day butterflies, which occasionally glide. Their wings are elastic, true planes, altering their shape and acting on the air through flexible torsion on the up and the down stroke.

Dr. Marey has given very interesting descriptions and graphic diagrams of insect flight. They are pictured motions, exactly re-produced. Nothing better is to be desired.

The reptiles—supposed to be the bird's original prototype—can nowadays only produce that little East Indian lizard, the *Draco volans*, who glides from tree to tree. He can compass but a few yards,—say, from one branch to another.

But past geologic times afford much more interesting specimens. At the epoch of the Lias, nature produced a whole family of reptiles whose lives must have been spent in the air. The pterodactyls must, in order to earn their living, have possessed the faculty of moving and sailing on the air, just as the large birds do to-day.

The class of fishes, as might be expected, presents few specimens of flying creatures; perhaps a dozen species can project themselves from the sea, glide a few air yards with great effort, and return to their liquid element.

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Flight progression is certainly the most elegant mode of motion given by nature to her creatures. But all birds are not equally gifted although each animal has modes of flight appropriate to his needs, for life depends on this.

Which of all the birds is best endowed for flight by nature? A question often put, and answered many ways.

Is it the eagle, with his majestic sweep? He is certainly great; the king of the air; but the humble pigeon outstrips him in the sky, as the greyhound flashing by the mastiff. Is it the frigate bird, with his great spread of wings? Assuredly no, I answer; there are circumstances when the frigate bird can not rise from the ground. Is it the group of the great vultures? These may be the best for man to imitate, but for speed, for endurance, for quick evolution, their vast wings require too much space to produce modes of flight entitling them to bird-life primacy. A condor can not get under way and rise like a sparrow.

May it not then be the charming swallow, so lively, so quick, so agile? Alas! no; her great proportion of wing surface is the sport of a gust of wind. Her small mass is insufficient in great currents of air. The sparrows are after all the best endowed for bird flight and bird life. Speed, quickness in action, difficult feats, constant readiness, all are compassed by them. And yet these birds during their whole year, do not flit as far as sea birds in a month.

From these remarks it is safe to conclude, and to say to ourselves, that each bird flies perfectly, according to his needs. Yet, from bird point of view, the sparrow approaches the type of perfection. As to speed, he may outstrip the pigeon; as to power, he can rise vertically to considerable heights; as to journeys, he equals other birds, for he also has his periodical migrations.

This selection may at first sight appear curious; but it will be remembered that it is only small birds which compass all the monstrous difficulties of flight. The warblers, the sylvias, the humming-birds, are constantly performing astonishing feats and gymnastics; they are athletes and acrobats. We may even at this point formulate an ornithological law, and here it is: The proportional power is in direct ratio to the smallness of the bird.

We do not generally appreciate this power; and yet let us observe the metallic elasticity of the warbler's muscles, in its zigzag flashings in pursuit of a fly; observe the wings' pulsation, vibrating like tongue of steel, and almost producing harmonic notes; and conceive of the energy spent in such rapid motions. A condor whose pectoral muscles could produce such lightning beats, needs have his wings of steel; their roar would be as thunder.

From the bird's point of view, the small are best endowed; but their power, skill, and life, man can not re-produce. If he is destined ever to cleave the azure, he must seek his model more nearly of his size.

Varieties in forms of wings.—Ornithologists have divided the different forms of wings into two groups; the acute wings and the obtuse wings; then again these are sub-divided into the super-acute and sub-acute, the super-obtuse and sub-obtuse.

This classification, however excellent it may be, is not sufficient. In order to explain satisfactorily the numerous facts observed concerning flight, we must have more data than are furnished by these divisions, which are too vague and general. We must take account of the amount of wing surface, in proportion to weight, of the length of the wing in relation to its width, and to the mass of the bird; in fact we must consider many circumstances, which render it necessary to study each family by itself, in order to reach satisfactory conclusions.

As a result of the study of all these conditions—a study to be found farther on,—we may now establish a series of principal divisions, which may be condensed under the remarks following.

It may be safely affirmed: that a bird with long and wide wings is well equipped for soaring flight. A gift which goes on increasing with the mass; that the bird with long and narrow wings is well equipped for gliding in great winds, and that this gift also increases with the mass; that short and wide wings (in proportion to body) indicate and produce a flight of small extent; finally that short and narrow wings denote great rectilinear speed. We may even lay down the law: Velocity is in inverse ratio to wing surface.

This, be it understood, applies to birds which fly, for else the ostrich and the apteryx would be the most rapid of birds; but we may say that among flying birds velocity, straitforward, increases as the proportional surface diminishes. It must be so to sustain the increased relative weight. Every sportsman knows the astonishing speed of ducks, teals, loons, etc., and the slowness of herons, lap-wings, and barn owls.

It is useless to enlarge upon these fundamental principles, for we shall find them constantly explained and applied in our studies of the flight of each feathered family.

The tail of birds.—The tail, as an apparatus, serves to sustain, to direct, and to preserve the equilibrium. - - - It is a useful organ, but not indispensable. A bird who has been deprived of its tail will fly, with its own particular mode of flight, after some days' practice, and without much variance or difficulty. - Many birds which are expert flyers have scarcely any tail; the herons, the albatross, the ducks, the teals, the pelicans, the gulls, etc., etc.

Again, the tail may be large or small without apparent reason. As witness the turtle-dove and the Egyptian dove, the magpie and the jay, the vulture and the tumbler eagle.

Great size of tail always indicates feeble flight, especially when the appendage becomes very large.

We may neglect to consider this organ, as giving but vague indications of its utility, yet if we must account for the final cause, for the necessity for this organ, particularly when it is well developed, we arrive at the following deduction:

The tail of birds serves as either an ornament or as an organ for flight. The ornamental consideration concerns us not, it may here be neglected. As an organ of flight, we may be enlightened as to its use by the following description of a maneuver witnessed.

A kestrel falcon was skimming close along a hedge, almost at ground level; its speed was moderate and its direction straight; when all at once,—as if moved by a released spring, it darted a right angle and pounced upon a lizard. The angle deviated was precise and the action of incredible swiftness. To perform this, the bird used its tail; it absolutely needed this rudder, so ample and powerful.

Here we see the use of a great development of this organ of locomotion; it permits surprising the prey by a sudden change of direction.

It seems probable that the powerful tail of the gypaëtus is destined for the same function; his mode of hunting among the rocks, delivering great body blows must be facilitated by its ample and powerful tail.

In fine, the tail best serves in pursuing the prey, but is not indispensable for long-continued flight, as indeed may be proved by removing it from the bird.

We then conclude that the tail's chief use is in producing rapid changes of direction; and curiously enough when the bird does not employ it, his flight is always straight. This may be formulated as follows: Aptitude for changing the direction of flight is in direct ratio with the amplitude and power of the tail.

It is only from the theoretical point of view, from its application to artificial flight, that the utility of this organ is here disregarded. The equilibrium may be maintained upon two points of support, as witness our legs, stilts, the velocipede, etc, yet we must acknowledge that in practice a third point of support becomes very useful; it introduces absolute stability, and minimizes that constant strain on the attention required to avoid falling.

Therefore a third point of support obtains even among birds with rudimentary tails. For example, the pelican dazzles not with the development of his caudal appendage, but we may note that the general form of his body supplies the deficiency. When in full flight he presents the following attitude: (See Fig. 1.)

It will be observed that his arm and his forearm form pronounced angles, like those of a flattened letter **M**, and that he may shift his center of gravity by playing these wings back and forth without compromising his equilibrium. This leads, incidentally, to another formula: Birds without tails all have the forearm very long.

The tail to be effective requires that the speed, or the wind, shall be great (these are equivalent in aviation), for if the bird had no other means of steering, his movements would be dreadfully hampered at

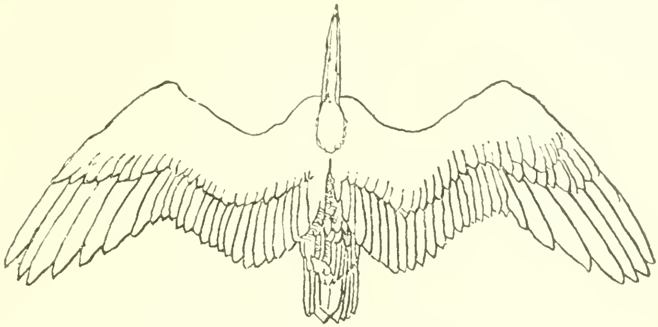

FIG. 1.—Pelican in flight.

low speeds. He substitutes, as we shall see, for this imperfect action other and more energetic means of changing his direction.

The flight of the flapping birds.—Let us view the bird when first he means to start. He is on the ground; he crouches to spring up, letting his wings hang down loosely.

Let us analyze this first movement. The wing is divided into three planes, one formed by the humerus, another by the radius and the ulna, and the third by the hand. The result produced by the position of these three planes is to offer no resistance to the air.

But this does not exhaust the decomposition of this attitude: all the feathers, particularly those at the tips of the wings, are so inclined that the air is met by their edges instead of their broadsides. Still further we see that the wing is never raised on the up stroke at full spread, but well folded on itself, so as to present the least possible surface and yet accomplish the movement with least effort and greatest celerity.

Now let us note the second movement: the bird's wing beating down the air. The action is simple; the wing is fully extended and stiff, the feathers close its whole surface, and it is concave on the under side. There is therefore a great difference in the result obtained between the up and down stroke of the wing. This difference produces the lifting effect in flapping flight.

Exactly to appreciate this difference, to feel it as it were, let the reader take the freshly severed wing of a large bird, grasp it by the humerus, and imitate the up and down stroke. This experiment will give a better understanding of beating flight than all possible descriptions and explanations. It is proximate and plain, one feels the efforts required by each movement, and passes judgment on them directly. The upward spring of the legs, and the first beat of the wings, have launched the bird in the air. He repeats the beats rapidly and rises, not vertically, but at an angle within 45°. To rise perpendicularly the bird is compelled to reverse himself, a difficult manœuver, sometimes performed by pigeons in the pigeon house to limber up their wings.

To pass from this inclination of some 45° into horizontal course, the bird brings the tail into play. He depresses it, and produces through the pressure corresponding to the velocity (sometimes aided by a particular light beat) a decomposition of forces which results in changing the course from 45° to the horizontal. If the tail is weak, he uses his deltoid muscles, which raise his body relatively and so produce the same result. In general, birds often employ both means simultaneously.

Horizontal motion being attained, the manner of flight is slightly modified with increasing speed. The wing no longer beats perpendicularly, but is slightly inclined in the direction of the course in order to increase the speed. - - -

Soaring flight.—Some naturalists have advanced most curious explanations of bird flight, especially of soaring or sailing flight. For their purpose lightness is the main requisite. They have pointed to the porous bones, to the spaces filled with air sometimes occurring under the skin of these creatures, as indispensable conditions for support in the air.

These are fallacies. Birds always exhibit corporal density, practically the same as land animals. Deprived of their feathers they sink in water; their specific gravity is about 1, as is that of man, of mammals, and of fishes.

To rightly explain the flight of birds, we must consider them as highly organized machines, which are sustained through the reactions produced by muscular effort; not as a balloon floating in the air, but as a stone glancing along the water, or a skater gliding over thin ice. All that apparatus described for distending the gannet, the pigeon—all those hollow bones of the pelican, the albatross, etc., serve flight in no degree. Their utility is different.

After all, experiment is easy. Strip a bird of his body feathers, leaving him only the wing and feathers, and his flight will be in no way changed; he will get chilled, he will not be able to swim if he is a water bird, but he will assuredly not fly the worse.

Let us now explain the flight of the soaring birds, or as I prefer to call them, the sailers. Birds soar perfectly in proportion to the magnitude of their sustaining surfaces and of the greatness of their mass. This is an unquestionable principle. A large bird, an average sized bird, and a small bird, all three having the same proportional surface relative to their weight, will soar the better the heavier they are.

Let us consider, then, only the larger birds, for these alone can effect the decompositions of wind-force which produce flight without flapping of wing. As the sailing bird first launches out with flaps, unless from a perch he plunges to get speed, we will suppose him in full action in air, and possessing initial velocity. He will then glide on rigid wings. If there be no wind he will glide to the ground, to a distance proportional always to his surface, and above all, to his mass; therefore an arrian vulture will glide farther than a tawny vulture, and this latter farther than an Egyptian vulture, yet they are all constructed very nearly in the same proportions.

When there is no wind, sailing birds must come down; flight is not possible unless they choose to flap. This sore necessity rarely brings early birds, for the morning is usually calm, particularly in the tropics. But let there be a current of air, a circumstance almost always present at a certain height in the atmosphere; at once the scene changes, the sailer sweeps in circles, he rises upon the air to great altitudes, and thence he glides downward where he wishes to go, even against the wind.

Let us try to explain this circling act. The bird glides in his sweep in the direction with the wind, losing as little height as possible, the wind imparting velocity almost equal to its own by impact in the rear. This push is effective: there is a good hold against upturned feathers, whereas when the bird turns again against the wind all feathers are smoothed down snug against each other and presenting surfaces of least resistance. This difference in action is akin to the revolving cups which serve as anemometers through the different resistances of the convex and concave surfaces.* As the bird further sweeps around he faces the wind, with freshly acquired velocity, and utilizes this in gaining fresh elevation.

In all soberness is this fraction of an explanation presented; for this circling action is little understood and is evidently of great use to the bird. When he faces the wind in the sweep he describes, the bird adjusts his wings and tail so as to rise slightly, his own acquired speed increasing the normal wind pressure, he rises more than he has dropped to develop his own speed.

To sum it up, there is a balance of benefit: the result, a lift produced by the force of the wind, which does not act with equal effect whether the bird's front or rear is presented thereto.

The soaring bird repeats this circling sweep, and gains elevation at every lap. The greater is the mass of bird and the more nearly concentric are the sweeps, especially when the breeze is light. Yet even among those birds best able to produce those decompositions of force approaching theory, the sweeps are only exactly concentric in the sole case when there is no wind; while he is awaiting the vivifying current the bird simulates the circling rise, he still sweeps around to sustain himself, but he gains no elevation. This action almost always deceives the observer, and leads him to believe that the bird is rising unless they are both placed on the same level.

It is not well however to ascribe undue importance to the varying effect of the wind on the feathers, front and rear, and to rely on this

* [M. Mouillard now considers this explanation erroneous.]

as a sole explanation; the problem contains many more elements. The variations in the amount of wing surfaces unfolded to the wind, in the different portions of the circles described, the variations in speed, and the shifting in position of the center of gravity of the bird are all of them factors to be taken into account.

The bird's elevation is gained by the skilfull utilization of all these elements, and by the happy use of a number of casual circumstances, beginning with those of ascending currents, which have been so much discussed of late, but which are not to be reckoned upon as a steady, sole cause, and ending with the utilization of the coming puff of wind, which the bird takes advantage of by breasting it at the best angle of incidence, just at the right moment; finally, and especially, by the difference in length of course while sweeping with the wind or against it, the latter being shorter, and the difference being more marked, as the rise becomes steeper.

The advantage obtained in rising on circling sweeps is easily observed and understood, yet it must be confessed that there is a weak point in the analysis, an insufficient explanation. This pertains to the stage when the bird is going with the wind. Is the acquired momentum, the velocity necessary to support the bird, sufficient to account for the subsequent phenomena? I scarcely think so, and I feel that the explanation is not absolutely correct, for observation shows that there is often complete arrest of motion. In any case, whether my analysis be good or inadequate, the circling sweep is much practiced by birds, and observation indicates that it is the manœuver which affords easiest ascent, for it is the process always employed by the sailing bird when there is a minimum of wind.

While still lacking a clear, convincing explanation, we may hold to that above given provisionally. Relying on the instinct of the birds, we may without risk accept the usefulness of the sweeping circle.

Instead, a manœuver which supports analysis, and which is easily understood, is that of direct ascension against the wind, either by drifting back, which is an easy feat, or vertically, which is more difficult, or even while advancing against the wind, the most difficult of all.

When we note the correct angle of incidence presented by the bird, the adjustment of his surfaces, and his skillful utilization of the varying velocities of the wind, advancing forward in the calm, and ascending on the increased velocity, we understand his manœuvers and find his solution of the problem easy to follow.

But we remark that this particular process of rising in the air requires that the wind shall possess such speed as to sustain at all times an areoplane with no velocity of its own, while, if in circling sweep, this same areoplane would have an initial velocity, thus enabling it to utilize breezes too feeble to serve in direct ascensions.

We must never think of the wind as a regular current of air—we would greatly err: attentive observation of bird flight demonstrates the constant recurrence of irregular gusts, not only near the ground, but even up to the limits of the visible atmosphere.

Birds certainly possess the gift, like good sailors, of seeing the coming gust of wind: the curling change of color on the water indicates to the seaman the approach of the squall. How is it that the bird perceives the coming gust? It may be difficult to conceive how it is done, but the fact is certain, for the irregular puff is often utilized; and yet here again is a basis it is not safe to build on overmuch, for the heavy soaring birds seem to disdain to use these puffs of wind: they accept them, they store up, as it were, the accruing momentum, but they never trouble themselves to profit fully by them.

In order to gain a sound idea of what is going on in sailing flight, to understand and to account for it, we must separate two conditions of wind which are usually confounded—the regular current of wind and the irregular gust.

It would seem at first consideration that when in a regular current of air the bird sweeps a circle, he must lose, against the current, just so much momentum as he has received in going with the wind, plus the frictional losses, etc. But we have observed that this is not the case, and as we say because the bird breasts the wind with his smooth cleaving shape, a shape more perfect as he excels as a sailer; which cleaving shape differs much from the rear, which latter is arranged in quite another form to catch the wind as a sail. Now to the difference in the coefficient of result upon these different shapes we must add the effect of the varying angle of the incidence of different spreads of wing as velocities change, the relative short course against the wind, and finally that mysterious first cause which we call life, which exhibits marvelous wonders of equilibrium of rest and of motion and governs the active part of existence.

Yet, as I said before, very large soaring birds do not seem to trouble themselves much to utilize all these little accessories; the experts in the art, having adjusted their surfaces at an average angle, judged sufficient from their experience, do not readily modify their attitude; they know there is small profit to them in small maneuvers, such as the furling and unfurling the wing, to modify the extent of sustaining surface in different portions of the circling sweep; one might say that they adjust their areoplane up to a fixed notch, which they know to be practically good, and trust to the wind gust for material uprise. There are probably minute changes in adjusting the equilibrium which the telescope does not disclose, such, for instance, as movement of the head, which is a precious balancing pendulum admirably located; there are even unconscious movements of the whole body; but as to intentional changes in the size and set of the sails (that is, the attitude of the bird's flight), they may remain for whole hours at the point fixed, with reasonably steady wind, just like the sails of ships. We must therefore examine the problem further, and seek a more satisfactory solution of the circling problem. This we shall find in studying the effects produced by irregular gusts of wind.

The wind gust is the very essence of the uprise; it is the magic wand which, striking the child's hoop, keeps it upright in rolling, drives it along, or raises it up to overleap elevations on its way. Suppose the toy to be placed on a steep inclined descent; gravity will cause it to roll to the bottom. If beyond this an ascending plane follows, the hoop, urged forward by momentum of acquired velocity, will rise to a height equal to that of fall, minus the losses by friction on the soil and by air resistance. But if, instead of utilizing gravity alone, we accelerate the hoop with the wand, it will run up much higher than the point it started from.

Let us suppose further, when the hoop is about to ascend, we can displace the ascending plane, in contrary direction to the toy's course, so that the plane shall glide under the hoop, then we would still more assist the ascension, by adding a supplementary force, independent of the others, and whose resultant would likewise be an uprise.

Let us now re-consider the action of the vivifying current of air upon the bird.

If the gust of wind occurs just where the bird is going with the wind, its effect is akin to the blow of the wand on the hoop; it stores up energy and economizes descent, hence the bird profits to that extent.

If the gust occurs when the bird faces the wind, then the sustaining plane is gliding beneath him, and the resulting pressure causes him to ascend: therefore again profit results in an uprise, nowise connected with antecedent fall.

If the wind gust occurs when the bird is on the quarter sweep, forward or back of the wind's course, it still exerts contributory rise; there is always, in each case, an impulse, a thrust from foreign source, which the bird profits by; or else a saving of acquired momentum, which the creature transforms into uprise.

But, after all, these explanations avail only for the public, curious to know why. They neither corroborate nor disprove the facts. Whether we understand and mathematically demonstrate the mechanics of sailing flight, or whether we fail in the attempt, the result is the same. There remains always the demonstration produced by the Great Master, who in His wisdom has implied: "If you understand it, it is well; if you do not understand, 'tis to be regretted; but in any case, look! that is the way 'tis done! I exhibit it all day long, not in a dark corner, but in the blue; and if you can not eventually profit by the lesson, you will really deserve never to join me in the skies."

Thus acts the bird; he demonstrates. We see the demonstration; and what more can we do with a formula which leads to no result? What testimony is an explanation more or less clear? Can there remain a doubt as to the fact of sailing flight, when the proof is evident and visible every day? The bird works no magic, he does not violate natural laws. We have not as yet rigorously explained these multifarious decompositions of forces because they are all complicated by movement and by life; but they are demonstrated each instant, and they constantly invite us to imitate a mode of motion which can not be beyond our attributes any more than the feats of equilibrium which we perform unconsciously every moment of our lives.

We appreciate well enough the acts of walking, of leaping, of gymnastics, of the velocipede: have the maneuvers which maintain their stability been calculated mathematically? No, they have not. Our vital instinct suffices for such action, not only accurately, but with all rapidity required by the need. Thus will it be assuredly for that remaining problem of equilibrium which resembles the others so greatly—the sailing on the wind: for man's life, that wondrous reservoir of unconscious science, will certainly prove equal to this new achievement.

The main requirement will be skill. The knowing how, and when, and why, each act is to be performed, to be expert in all possible maneuvers required to produce various results or to meet contingencies; in fine, the man must thoroughly know his business—as a bird, a soaring bird, just as he knows in time his business as a swimmer, a skater, a bicycle rider, an acrobat—and, in short, as an expert in any gymnastic exercise.

Speed of the wind.—For the sailing birds, the wind is the source of all good gained while sustained. No wind, no uprise, no sailing flight possible; therefore, in a dead calm, they are all on the perch. Now, what least velocity of wind can support and upraise the most expert soaring birds?

Observers may fancy they see kites and vultures ascending in dead calm. 'Tis an impossible feat. There must absolutely be, at a certain height, a current in the air, perhaps indiscernible to the eye, but nevertheless revealed to the experienced observer by the bird's maneuvers.

The sailing bird, rising during a calm, generally flaps his wings till he is up one hundred yards. At that elevation he begins to circle, partly gliding partly flapping; then he diminishes his beats as the elevation increases, and finally stops them altogether: this proves that the air is motionless only near the ground.

It is well known that there is almost always a strong current of air at prominent altitudes; we leave the valley where absolute stillness reigns, and on the mountain top we find a lively breeze. A light zephyr, fanning the spring-like day, which we can not miscall a wind, is blowing however a hundred yards above, some twenty-two miles per hour, as proved by accurate observations made by myself, by means of bursting fire-works, bombs, whose smoke is most sensitive.

When the wind is decidedly perceptible at the surface of the ground, it greatly exceeds twenty-two miles an hour at an altitude of 1,000 feet. A good wind, a fresh sea breeze, one in which the sailor takes in no reef, but keeps an eye on his sails, is found to blow forty-four miles an hour 1,500 feet up above the sea. The great North wind, measured by the transit of the cloud's shadow, blows from 67 to 89 miles per hour, while a violent "Kamsin" at a height of 500 yards shows incredible speed.

In this terrible wind, a tawny vulture moving with it, has a frightful velocity; in a moment it has traversed the field of vision, say (for a bird of that size) four or five miles.

These are the tempestuous winds which expatriate the birds, which cause the creatures, after a day's journey, to find themselves 3,000 miles from their own habitat.

These enormous velocities are proved by actual facts. The balloon "Ville d'Orleans," which left Paris during the siege at 11:45 P. M., arrived next day near Lifjeld (Norway) at 3:40 P. M. Say 900 miles in 15 hours, or 60 miles an hour. The balloon launched at the coronation of Napoleon I, travelled during seven consecutive hours at a speed of about 90 miles an hour. During a long summer's day, say 18 hours, a bird swept away by such a current of air, and rowing in the same direction, might travers 1,800 miles! - - -

What splendid journeys a powerful wind might enable man to make if he could navigate the air! But let us entertain no illusions; it will be the accident, not the rule. - - -

Let us now consider every day winds, those of moderate velocity. Observation indicates, by comparing birds' progress with that of railway trains, that the slow flyers go at most 25 miles an hour, and that birds well endowed, such as the turtle-doves and the large sailing birds, in full flight through space, get over some 37 miles in the hour. So that, for general use, we may assume a speed of a little over a mile in two minutes, as a probable achievement, if man sails on the wind.

If the problem can be worked out, if the skill be acquired, this is the rate of translation man may expect to compass, less perhaps, rather than more. But it is a fair promise. He may journey 300 to 400 miles in the day of 10 hours, with no expenditure of power whatever, for the wind will do the work.

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Velocity of the bird.—The speed of the bird's translation, considered generally, especially for sailing birds, is composed of the bird's velocity with that of the wind. With flapping birds the case is different, and the speed results from three factors; the speed of wind, the theoretical speed of the bird (which is to be estimated as if he were a sailer), and the additional speed produced by personal exertion. This latter speed, already object of many experiments within doors, and of reams of calculations, nowise interests the observer who watches the sailing bird in all the simplicity of its flight. We therefore will consider only the lesson to be learned from practical performances, To measure accurately the speed of translation of the bird, we cannot survey him in the air, for we have no reference points. It is to the bird's shadow upon the ground that we must direct our attention. This shadow is easily followed by the eye; it may be gauged by the speed of a horse, of a chance donkey, a dog, a carriage, or a railway train; and thus we get sure data and trustworthy points of comparison. For instructive observation it is well to study many models, and to live close to them.

For instance, as I write these lines two families of domesticated ravens are within a few yards, awaiting for the food I am about to throw to them. On the mosque, in front, my pet kites are perched, waiting my appearance to plunge towards me, at the least gesture I may simulate of casting meat to them. Thus I can closely view this expert at full speed, for there are two of them which snatch their pittance from my hand.

There are endless battles between ravens and kites and among kites themselves, and battle always brings the performance of feats; constantly does the kite turn over back downward, this being a favorite fighting posture with all the eagle tribe. I often see two kites lock claws up in the air, clutched fast, and thus locked spin down hundreds of yards.

When a great wind blows, the observation is wonderfully interesting. To try to explain these complicated movements with mathematical formulæ seems a farce. Their mere description is difficult enough; how then can we fasten within algebra's rigid rules the evolutions, the feats, the stratagems, which shift with each wind gust, with each fancy? It is like an attempt to calculate the foot pounds expended by a gymnast during his exercises, or the thermal units utilized in a struggle between two athletes.

What is most known concerning the speed of birds is generally vague, for they do not lend themselves to accurate experiment. The speed of flapping flight is pretty well indicated by the carrier pigeons, who, by a dead calm, cover from 33 to 50 miles an hour, according to species.

We know that the turtle-dove flies faster than the wood-pigeon, its velocity being about 50 miles an hour. Ducks and teals have greater swiftness still, but it is difficult to determine how much. Moreover, the effective speed is governed by the force and direction of the wind. It is therefore almost impossible to be exact as to the speed of flapping birds, and the question possesses small interest for him who thinks the soaring birds to be the true model to imitate.

The sailing birds afford an occasional chance of measuring their speed of flight, which naturally varies with the wind, but which always illustrates the advantage of mass. There often is a race between three birds of different size: the tawny vulture, the Egyptian vulture, and the kite.

Near the "Abbassieh" gate at Cairo, amid mountains of potsherds, dead animals are deposited by the scavengers. They are not buried; there is no need, for between vagrant dogs and rapacious birds, they are eaten up in a few hours. As soon as the carcass has been laid down and the knackers have finished skinning it, carniverous birds appear. They pass in the zenith of the observer and arrive at their destination, all the time visible through the telescope. The distance is known, and only the time need be noted which is consumed in the journey.

This trip is performed with the same velocity by the three birds named, but the force expended is evidently much less when the mass is great. The actual speed, of course, varies with the velocity and direction of the wind. - - -

As a final result, deduced from such daily observations, I think that I closely approximate the fact when I state that a kite, soaring to survey the hunting ground beneath him, has a mean proper speed of 11 miles an hour when the wind blows also 11 miles an hour; this is the sailing bird which seems able to obtain support with the feeblest current of air. The tawny vultures, in order to rise on such velocity of breeze, need to unfurl their entire possible wing surface. For them it seems that the wind velocity should be at least 17 miles an hour to be in full accord with their faculties.

Effect of speed.—Theorists frequently set themselves this problem: What is the power required to obtain support in the air?

The lifting force, (ascensional power if you please so to call it), is under many circumstances so slight that it may be neglected, and is reduced to the force necessary to sustain the apparatus.

In soaring flight this ascensional power is only indispensable when there is no wind. The problem would be better stated thus: What velocity must be imparted to an aeroplane, bird or machine, in order that it may be sustained on the air and may rise? Now for this, as for all aspects of this problem, we find a solution provided by nature.

Birds whose pectoral muscles have not power to raise them bodily are not rare. Sailing birds can, unaided, compass but little rise; especially the very large birds. A tawny vulture can not rise 20 yards on a start of 45°; he can not rise 10 yards vertically. So this king of soarers may be kept a prisoner in a roofless cage, provided the sides or walls are 20 yards high and 20 yards apart. Among birds with narrow wings this peculiarity is still more marked.

The Swift, this wild, darting, rustic inhabitant of the air, can not rise vertically 6 feet. It is perfectly caged in a large box without a cover, and yet if any creature is thoroughly equipped for flight it is he. The same is true of the large sea birds. A frigate bird is impotent with less than a certain space to perform its evolutions, whereas as soon as the two birds I have named have acquired speed, or are launched in a current of air (which amounts to the same thing), they become forthwith one the dashing swift, and the other the tireless frigate bird and this tells the whole story.

I may here formulate an axiom: *No speed, no flight.*

I once had a curious problem to solve, based on the above principle. It was years ago, and I was in Algeria; so far as I now remember, it was in 1864, in the spring. I already understood the problem pretty well, and with a li[illegible] help and better surroundings I might have succeeded in imitating the birds. I appreciated the possible results: perhaps the French collapse of 1870 would have been averted, the Russo-Turkish war might have remained *in limbo*, nations might have gained freedom, or Asia might have invaded Europe with countless throngs: who knows? But why speculate? Let us leave all that aside.

I was saying that one fine morning, in the port of Algiers, I had gone to the harbor to look over the fish caught over night. This was my way of studying the dogfish and the form of the great submarine swimmers, etc. There I ran across a peddler, who instead of fish had some sea birds for sale. There were some fifty of them. I did not at first know what kind of birds they were, but after a while I recognized them to be "procellariæ," variety *Puffinus kuhlii*, or stormy petrels, which I had already seen at sea, but only afar off. As they were cheap, I treated myself to four, and then I took the train at 8 o'clock, and at 10 I was at home on the plain of Mitidja.

It was my object to examine and measure these birds, and then to set them at liberty when I was tired of them. I therefore deposited them on the water, in a little duck-pond, near my farm. Here I think it may be well to give a short description of this bird, so that persons who are not acquainted with it may fully appreciate all my mishaps.

The petrel is a bird about the size of a small hen. By referring to my table, (The Gull type,) p. 433, it will be seen that it weighs 1.65 pounds, that its spread of wings is 4.10 feet, by a width of only 5 inches. Thus its equipment for flight consists, as it were, of two drumsticks, which only permit its launching forth under special conditions. We may form a fair idea by imagining a pullet equipped with two flat rules, such as draughtsmen use, in lieu of wings. The legs are long, delicate, and feeble: the feet are webbed. The bird can scarcely walk; it runs 8 or 10 steps, then stops as if fatigued.

My four birds, set on the pond, did nothing in particular: they seemed to have no notion of taking flight. I took up one, the weakest one, and I threw it into the air sufficiently high. It undertook to fly, crashed against a wall and knocked its brains out. I was vexed; I took a second bird and carried it upstairs; to the first floor. This second bird was sick; it fell so stupidly that I allowed the dog to strangle it. So I took a third, and I took my oath that day to see a stormy petrel in full flight. To bring this about, I went to the top of my observatory, which was several feet higher than the peak of the roof. Thence I launched the creature into the air. That poor devil of a bird had no better luck than the others; he flapped his wings vigorously, sinking downward, and just as I believed him to be fairly under way, he struck a post and broke a wing.

I must own that I was not pleased with my purchase, and there was good reason. To spend good money in conferring liberty on captives, to rack my brain about them, to carry them up to the roof of the house, and then to fail flat, this was hard luck.

There was still one bird, a forlorn hope. I had got it into my head that I must see this bird in full flight and I was determined not to miss my aim this time.

I reflected a long while; at last an idea struck me, and here was the result.

Less than a mile from my farm there was an open tract, naked, bare of grass, flat, smooth as a mirror. It struck me that these conditions were somewhat analogous to the surface of the sea in a calm.

I carried thither my No. 4, who appeared to be just as stupid as his three predecessors. I set him down on this extended area, and retired to a distance. The creature remained squatted down for a good while, then he turned, with his beak to the wind, and he stretched his wings. Then he showed me that I had reflected to some purpose.

He started running and beating his wings, which were not hampered by any herbage, and ran in this way about 100 yards, carrying his weight less and less upon his feet, and finally all on his wings, but all the time skimming the ground. At last, with a single bound, catching the wind, he rose some 60 feet, returned towards me, and as he glanced by on his way, I thought he said to me, "Remember hereafter, oh! my preserver, that *success in flight is all based upon speed.*"

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Effects of Mass.—Among birds of the same shape and same muscular power mere difference of weight produces effects quite varied, and yet all of a piece.

Let us note how a difficult exercise is performed by the eagle, by the falcon, and by the lark, all three being perfectly comparable as to their construction.

The eagle remains motionless in the air, on rigid wings, using only his tail to balance himself; he is as fixed in space as if spiked to the sky. The falcon also remains at a fixed point, but he beats his wings; while the lark cannot perform this maneuver, under the same atmospheric conditions, without painful effort, as it is constantly carried away by the wind.

This law of disproportional aptitude for flight between the smaller and the larger birds constantly deceives the eye and confuses the data gathered. An actual example will exhibit the law.

The quail, which everybody knows, is an especially heavy bird. It flies with great effort. It is a round ball with two small wings which barely sustain it. Yet, actually measured and weighed, it presents the following surprising results:

It weighs only 0.92 pound per square foot of sustaining surface, body and wings, and this is less than the flamingo, which weighs 1.56 pounds per square foot of surface, and yet this rosy wader is generally thought of as a mere pack of feathers. Or than the pelican, which weighs 1.36 pounds per square foot, and which flies very well. Or than the stormy petrel, which weighs 1.17 pounds per square foot, and must depend upon speed for a living. Or, it will not be believed, the quail weighs actually less than the tawny vulture, which weighs 1.47 pounds to the square foot of surface, and yet floats for whole days without a single flap. And yet how badly the poor quail flies! Its usual course is 200 yards, and it is then outflown, it pants for breath.

From the comparison of the rates existing between the weights and the surfaces of the birds which I have measured and inscribed in the tables of this work, I may deduce a general law: The amount of proportionate surface required by a bird for a given mode of flight diminishes as the weight of the bird increases.

The exact proportion is yet to be deduced from more complete tables than those which I present, and from experiments to be made upon the sustaining power of aëroplanes constructed upon similiar models, but of different sizes, and loaded with ballast until they produce similar satisfactory results.

The exact advantage obtained through large mass, while indicated by my tables in a decided and regular manner, is somewhat difficult to formulate. The manner of increase of the volume, in relation to the containing periphery, certainly cuts a figure; it is a factor to the credit of the larger mass, by diminishing the relative air resistance; this is unquestionable. Increasing sustaining surfaces, on the other hand, produces increased friction and resistance, but the gains and losses do not increase with the same factors, and hence result complications.

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We might reasonably expect that the area of surface required to sustain each pound of bird would be a fixed and definite ratio; any deviation should only be produced through gain in the ratio between the volume and its enveloping surface, or through loss by increased resistance and friction of the sustaining surfaces. And yet it is not so. The tables I have constructed show that things are very different: they indicate enormous discrepancies.

Thus the surface of 3.62 square feet required to sustain one pound of bank-swallow is reduced in the tawny vulture (which flies and is sustained at least as well), to 0.68 square feet per pound, a variation of five times and more.

If we seek an explanation for this phenomenon, we find that a disturbing cause always brings advantage to the larger mass; it is the variation in the coefficient of air resistances opposed to different masses.

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Continuing our study of the effects of mass upon flight, we shall notice in the tables that as the weights of birds vary between 15 grains and 3.5 ounces, the peculiarities of their flight become very different. From 3 ounces up to 10 ounces, from 1 pound up to 2 pounds, from 5 pounds up to 10 pounds, and up to 16 pounds, there are as many steps in the increased economy of power and support in the air. So it seems at least from information furnished by the experts, by the birds. - - -

It is probable that the benefits to be derived from large mass continue to make themselves felt beyond the weights comprised in the tables, and that up to some 200 pounds the law remains the same, or, to express it in another way, the curve of variation which may be constructed with the figures contained in these tables would continue in regular sweep beyond the points observed. - - -

When we observe the sailing flight of the Nubian vulture, whose weight varies from 17 to 22 pounds, the one astonishing thing which strikes us immediately is the great steadiness in advancing. When the bird has set his course, accidental wind-gusts do not seem to affect his great mass, which appears insensible to them, and which continues its majestic motion without disturbance. An aëroplane, therefore, weighing 200 pounds with its load, ought to move on the wind with even more persistence and regularity than the Nubian vulture. How will it be, then, when from 200 pounds we pass to 1,000 or 2,000 pounds in artificial flight? We may rest well assured that unsteadiness and accidents of the flight, under equal conditions of wind, will diminish in a still greater degree.

For weights up to 200 pounds, intuition furnishes us with a pretty sound idea of what manner of flight we can get on the wind; we have, as it were, bench marks to refer to; but when we shall come to weights of a ton or so we know scarcely anything, and as for 10 tons, we are in the dark unknown. However, until the contrary be proved, I shall believe that the advantages shall continue to grow in the direction of the heavier weights.

Effects produced by aggregation (in flocks).—Observation seems to show that birds (whether sailing or rowing birds), have more power to penetrate the air when they are congregated in flocks than when flying singly. - - -

It is a fact well known to sportsmen that a charge of shot so rammed as to scatter does not penetrate as far as when well rammed so as to act as a single ball. - - -

Birds must know this property of aggregated bodies, for they frequently avail of it. Thus the *passerines* make no long transits unless grouped in close order, beginning with the sparrows which have a well fixed order of flight. Pigeons, ducks, geese, swan, cranes, etc., never travel save in serried ranks, in always the same order for the same species. - - -

Pelicans on their travels arrange themselves in the form of a wedge, which from a distance simulates the point of an arrow. They move with a curious sluggishness, and with the regularity of iron rolling-mill machines. These enormous palmipedes sometimes afford a most engrossing spectacle. I remember, one day upon the Nile, to have seen a flock of them come sailing out of the sky, dropping from a height where first they seemed as swallows, alighting within 200 yards of my boat, upon one of those islands of gelatinous mud peculiar to that river. I followed all their evolutions, through a telescope, and the spectacle lasted half an hour. It was astonishing! How beautiful were these great birds in their wheelings among the clouds! From afar off one could hear the hissing of their wings cleaving the air, their hoarse cries, somewhat like the donkey's bray, and even the slap of their great feet as they struck the liquid mud.

The three supports.—Each family of flying creatures presents in the air a particular aspect, which it is interesting to study.

There are birds with long arms and others with short arms; some have long primary feathers and others have them short. Some birds have long narrow wings, others have them thin and short. Some wings are round, some are square, terminating in five feathers of equal length; others again run to a point in which either the third, the second, or even the first, primary feather is the longest. - - - For what particular kind of weather and for what special uses were these wing forms intended?

When nature had to provide a large bird (and we need only concern ourselves with such) with wings for rapid flight, she made these wings small and narrow, and provided powerful pectoral muscles, as witness the ducks. When the bird's success in life required it to move in high winds, like ocean storms, she invariably endowed it with long and narrow wings, to avoid friction, as witness the gull, the mew, the gannet, and the albatross. When she determined, as in the case of the eagle, to produce a powerful creature, to create a great hunter, she then endowed it with her best gifts; that is to say, with the aptitude of sailing indefinitely without fatigue while surveying the field. For this she gave it the wings of the sailing bird, and to these joined powerful motor muscles, so that it might flap vigorously in case of need. When she determined only to provide a bird with the faculty of remaining in the sky without fatigue, she endowed it with two things: a large mass and a large surface.

As for other birds, her unfavored children, the disinherited, she made rowers of them, and they drag their bodies through the air by main force, flapping and fatigue.

Let us now inquire as to what relation there is between the mode of flight of a bird and the length of the radius and of the ulna in proportion to the hand.

Before going further we should remark that the lengthening of the forearm coincides with that of the arm; there is an almost constant relation between these two parts of the wing, but there is a decided divergence in the relation of these two parts (taken together) with the hand.

Each flying family is proportioned somewhat differently; these peculiarities should be ascertained by detailed and minute measurements, but in their absence some general remarks may be made.

We may cover all genera of soaring birds by the two types we proceed to specify.

(1) Among soaring birds the general posture of their wings at an acute angle of about 100° (the summit of the angle being the beak) corresponds with the shortening of the arm and fore-arm, and the excessive lengthening of the hand. This covers rapid soarers, such as swallows, martins, and in lesser degree, the kites, fish-hawks, etc. Let us remark, by the way, that this kind of soaring necessitates a powerful tail.

(2) In the extreme opposite type, that of slow soaring birds with wide wings, the angle mostly affected is about 200°, and sometimes more, and here we notice a diminution in the length of the hand, and a great development of the arm and forearm. The best type of this genus is the vulture. In the latter genus the tail is generally small.

There is however a family of sailing birds with narrow wings, which has an exaggerated length of arm, fore-arm, and also, generally, of the hand. This is the family of the *Pelicanidæ*, comprising four or five genera which are constant, paradoxal sailing birds.

The tropical phæton soars wonderfully. The Egyptian pelican is a charming instance which can be closely studied, for it is easy to see him in full sail over the water. But the frigate-bird is the *ne plus ultra* of soaring creation, the *chef d'œuvre* of nature in that direction; in this bird the lengthening is general, the arm, the forearm, the hand, all are of enormous length; therefore the creature soars fast or slow just as it chooses. It is perfect of its kind but of no use as a model, for man can not imitate it. Let us therefore dismiss it in this essay.

Thus we have reached the fact that the two extreme types of soaring flight are the martin and the Nubian vulture. The first presents its wings at an acute angle, and this produces instability of equipoise requiring great velocity, just like the velocipede which can not remain upright unless it is in motion.

Vultures, on the contrary, can spread their wings at a re-entering angle. The two wings and the tail then furnish three points of support, upon which the system is balanced. The relative positions of these three moveable supports govern the motion and the speed. Therefore the variable positions of these three supports produce all the evolutions of those avian aerobats which have been termed soarers or sailers. The variation of the angles between these points of support covers all the types of sailing flight, and produces all the manœuvers, from that of absolute immobility in the sky to that of vertical fall; from great velocity to the actual stopping, from going forward to going backward; for, from a theoretical standpoint, backward flight or backward gliding may be considered as feasible.

Various manœuvers.—To write concerning the manœuvers performed by soaring birds is somewhat like attempting to describe a picture or a piece of music in words,—the best possible description will never be worth the roughest sketch, or a line of notes.

However, as we can not rest content with always preaching observation to those who can go where the birds are to be found, we shall try to say a few words concerning some evolutions of sailing winged creatures; for before man dare trust himself to any apparatus, however well designed, he ought to know, approximately at least, what may be done in the air with such an apparatus, for else he can only be sure of the descent to the ground.

Birds get their initial start in many ways. For most of them this is the easiest act. Those which have to vanquish the greatest difficulties are the large water birds, which in starting from water or ground are compelled to run a long distance, using both feet and wings, in order to gain the speed required for support. This applies to large sea birds with narrow wings, and in general to all water birds: such as the geese, the swans, the pelicans, etc.

The rowing birds simply jump into the air to take flight; their pectoral muscles are so powerful that they enable them to get support without much headway; moreover this leap possesses great energy; this may be realized by watching the leaps of a large passerine bird deprived of his wing feathers, such as a raven or a magpie: one single leap sends them up three feet.

The smaller the bird the greater is this initial leap in proportion; as witness the blackbird, the lark, and that life-spark, the tom-tit. Among the birds, the strength of the legs follows the same law as that of the pectoral muscles; it increases in proportion as the weight diminishes. In fact, a nightingale or a sylvia use their wings when in a thicket only as an aid to equilibrium and as directing power.

The plovers, certain scolopax, the tringas, etc., get into flight by previous running. The larger number of the great waders, the large vultures, etc., also get their start by running, if on the ground, but as soon as they can, they abandon the running steps for a series of leaps which continue as long as their feet can touch the ground.

Birds of prey in general have two methods of getting under way. When they start from the ground, with or without a prey (except vultures), they always enter into action by a leap measuring a yard. When they are on the perch, being always at great heights, they simply launch into space and spread their wings open to get under full motion.

Coming to a rest is always a serious business for a large bird, and seems to become more so the heavier they are. Generally they manage to face the wind, and thus to extinguish in part their velocity. A pigeon without experience, alighting with tail to the wind, is generally upset and tumbled over. In a state of nature, a wild bird knowing his business perfectly, never misses making a safe landing. When the wind is high, the heavier birds of large surface perform wonders in coming to a rest. An eagle alights with incredible lightness; the shock is no greater than that due to a 4-inch fall.

When there is no wind, the winged experts who dislike to be jarred adopt another way. They glide upward, the steeper the slant the better, and by thus opposing gravity to speed they completely extinguish the inertia of their motion, rising as high as may be required before coming to rest.

When man comes to experiment with an aërial apparatus the bird's mode of alighting will needs be studied *à outrance*. The man may add a lot of embellishments, such as elastic nets, beds of straw, suspended cords with elastic connections, rings for attachment, watery beds for floating machines, etc.

The act of alighting is the terror of all winged creatures. There is especially one class which dreads (and with reason) even the smallest fall; these are the waders. Therefore do they possess great proportional wing surface, which perhaps may be intended to allow them to come to rest without the risk of breaking their long legs. Happy are the birds which alight upon the water. The reader doubtless has seen a swan come down to his liquid bed; it is a striking spectacle; they plow deep furrows with their palmed feet; the jets of water and the foam which they raise with great fuss attracts attention forcibly. This is the mode of coming down, simple and practical; which man must ponder well, and try to imitate. - - -

OBSERVATIONS OF BIRDS.

I give in the following pages some data concerning birds, which, although scant in extent, have required from me many long years of hunting. I now have but the later specimens; two-thirds of those I had gathered have perished in numerous removals—they have been lost, forgotten or abandoned.

It is not enough to kill a bird: this must be done under favorable circumstances, that is to say, we must have two things which are not always at hand—scales to weigh the creature directly after death, and appliances for measuring and calculating its surface.

Birds sold on the market in the towns of Europe are generally unfit to gather data from, because they have been drawn or are dried up, and the exact weight can not be ascertained. Among fifty rare birds which have been sent to me, only three could be utilized; all the others lacked something or other and had to be rejected. One of the latter was a curious bird: it was brought from the Shoa country by the explorer Arnous: but what could I do with a bundle of primary feathers and a few large secondaries? I could only say, after examining them that if these were feathers of the griffon, that bird attains an extraordinary size in that region, for the longest feather was at least as large as that of the condor and was 29½ inches long. It may have belonged to a great unknown vulture of Central Africa, the existence of which I suspect from the accounts of Abyssinians. If so, it must be upon the old continent the analogue of the harpy of the Amazon River.

I was in possession for many years of the most beautiful eagle which I have ever seen; neither Paris nor Geneva possessed, to my knowledge, anything its equal in figure and beauty. Yet I can furnish no accurate data of eagle measurements. I have killed over a dozen, and I can not present one of them to the reader. However, as the proverb goes, we can not give what we have not got: so I give the best information I have.

All the birds I present were weighed when fresh killed. As to their surface, this is the way I proceed: I spread out the bird, back downward, upon a large sheet of paper, the wings being stretched out into the attitude of their flight when there is no wind; this being set down in the tables as "wind, 0 per second." Sometimes, when the wing happened to be stiff and could not be fully extended, the attitude resembled that which the bird assumes when there is a light breeze; in such cases it is marked on the table as "wind, 5 meters per second" (11 miles per hour). Finally, some measurements have been made with the birds' wings adjusted as when they sail on a good wind. In such cases they are headed, "wind, 10 meters per second" (22 miles per hour).

Once laid down on its back, well adjusted in proper attitude of sailing flight, the bird is made immovable by weights, these being plates of lead to flatten down unruly feathers, and two or three large masses of lead to hold the wings in position and to counteract the contraction of the muscles; then with a pencil it is easy to trace a precise silhouette. We thus get the total projected surface of the bird—wings, tail, body, head and feet. Now, if we only measure the surface of the wings, we would err; for when under way, all parts serve to suppor, weight, all goes to form an aëroplane, being more or less effective, according to its shape. Assuredly, we might neglect the feet of the waders, as being nothing but an impediment which the creature trails behind, but as to the body, there can be no thought of ignoring it, for it derives much support from the air.

We may now say, simply, that this gives us the surface of the shadow of the bird.

In calculating these surfaces we must proceed with patience, many figures, and much order. There are a dozen triangles to compute and four or five parallelograms. This is very tedious work. We pluck up courage with both hands, as the saying is, and when the operation is ended we may say that this is another stake planted.

When the weight and surface are ascertained, we next measure the spread of the wings (alar dimension) and the mean width of wing, which two latter indicate the proportions of the aëroplane of the bird. This relation is indicated in the tables by a simple proportional fraction, 5:1 for example, which indicates that the width being 1, the total spread is 5.

In addition, I set down the amount of surface necessary to carry 1 gram, also the weight sustained per square meter, and finally the aggregate surface required to support 80 kilograms (176 pounds). This weight of 80 kilograms corresponds to the weight of a man equipped with a light aëroplane. The figures therefore indicate the total surface required for an aëroplane of that particular type.

In order not to present at random (alphabetically) the different kinds of birds, they are grouped according to their mode of flight. This produces strange groupings; all ornithological rules are boldly violated; the charadrius (plovers) are classed without hesitation with the vanellus; even classing the accipiters (night birds) with the passerines (sparrows, etc.), which is an infinitely more grave departure. It will thus be seen that similitude in flight has alone been taken account of.

The Rail type.—Under this head are comprised all the birds which, in flying, hold their bodies at an inclination of about 45 degrees, instead of spreading themselves out horizontally, a position invariably assumed by the other birds. The marouettes (rails), the different rails, the waterfowls, and the domestic fowls, here compose this branch of flyers.

The turkeys, the guinea-hens, the canepetières, and the peacocks do not form part of this class, because they stretch themselves out horizontally when in full flight.

The above class of birds (rails), although they fly but rarely are sometimes compelled to make long journeys; they probably then utilize strong winds of good sustaining force. These great air currents have indeed a power which human instinct does not reveal.

During a strong sirocco, blowing at least 20 meters per second (45 miles per hour), I have compelled Kabyle poultry, which, to be sure, fly somewhat better than European fowls, to take extensive flights, during part of which they soared in a way surprising. The guinea-hens were by this violent wind sustained in the air with an ease one would never suspect in a gallinaceous fowl.

Here, methinks, the good housewife will ask, were many eggs laid on that day? I confess that this did not disquiet me. I would have sacrificed the whole barn-yard for such a beautiful demonstration; for after all, when knowledge is sought, we must do all requisite things to arrive at that knowledge, even to the sacking of the poultry yard, if need be.

My poor pigeons! what a time they had. They were adjusted in all

The Rail Type.

Common name. French	Common name. British or American.	Scientific name		Weight of bird.	Surface within contour.	Spread of wings. (b)	Mean width of wing. (a)	Proportion. b/a	One gram of bird's weight sustained by—	One square meter sustains—	Relative surface required to sustain 80 kilos or 176·4 pounds avoirdupois.
			Γ	*Grams.*	*Sq. meters.*	*Meters.*	*Meters.*		*Sq. meters.*	*Grams.*	*Sq. meters.*
Maronette de genêts		Ortygometra crex									
Râle de genets		Rallus crex									
Râle d'eau	Water rail	Rallus aquaticus	0″	192	0·0506				0·000263	3802	21·04
Poule d'eau	Water fowl	Gallinula chloropus	5″	548	0·086092				0·000157		12·56
Do	do.	do	0″	595	0·095422	0·696 2.28 ft.	0·158	4·40:1		6224	12·82
Poule sultane		Porphyrio veterum									
Foulque	Coot	Fulica									
Poule	Cock	Gallus domesticus									

sorts of ways. Their wings were clipped short: their wings were lengthened out. They were made semi-long, narrow and long, narrow and short. They were pieced out with primary feathers from birds of prey, thoroughly fastened on with glue, and pressed on in the vise.

This is what happens when a bird's wings are altered; when the active surfaces are changed in extent and form, the creature being accustomed to a particular mode of flight starts off in his habitual manner; but his organs are no longer adapted to produce their customary effects, they are shaped to produce different effects. Thus the bird finds itself with habitudes and instincts for one mode of flight, and with a new arrangement of feathers adapted to another mode. The latter constantly impresses him with the necessities of the moment, and these necessities compel him, willy-nilly, to fly as flies the other type of bird which has been forced upon him.

Thus, a kestrel falcon, a good soarer, who had his nest near my observatory, has just one-half in length of his primary feathers cut off; the result was forcibly to transform him into a rowing bird. I restored him to liberty, and as he remained on his hunting ground, I had every opportunity to observe him. Although much hampered the bird was not very unhappy; the prey sometimes escaped, but he made up for what he had lost by increased activity. He was easily singled out, for the mutilation gave him an unusual figure; his long tail seeming longer, now that it was not accompanied with two long wings, and it attracted attention from afar. And so he had to row constantly. Once in a while he would try gliding in the old fashion, but as he found himself dropping too rapidly, the instinct of necessity for support compelled him again to beat the air.

I have transformed kites into stormy petrels by clipping off half the width of the wing for its whole length, and abolishing the tail. The result was to compel them, notwithstanding their habits of soaring upon light winds, to await instead the brisk winds which alone could sustain them without fatigue.

As will be seen, to succeed in such observations we must absolutely control the bird and have him at command. Experiments with increased surfaces did not succeed as well; I tried them on pigeons, and on two kites who were old neighbors of mine. I only succeeded in producing birds which were exceedingly awkward in their movements. I ought to have practised upon a goss-hawk, but how could I have afterwards observed such a mere bird of passage, whereas the kites a have fixed habitat, they are well recognized, so that if there is an important feather missing in one of the kites in your neighborhood, you can recognize him, single him out, and consequently observe him.

* * * * * * *

The Hawk type.—These are here separated from the great family of birds of prey, because, although they are raptores, there are between the two divisions profound differences; one may be called raptores of long flight and the other of short flight.

Among the eagles, the falcons, the kites, and the buzzards the primary feathers are long, the second and third being the longest, the result being an acute-shaped wing. Among the hawks the fifth quill is the longest, and moreover, the whole combination of primary feathers is remarkable for its shortness. They are not birds of extensive flights, either as regards elevation or duration; they frequently are perched. They comprehend three species—the sparrow-hawk, the goss-hawk, and the harpy, which has the same conformation as the two preceding, and must fly in the same manner. I do not know the manner of movement of the harpy, never having seen it myself, but I am sure of being right in saying that its gait is in general the same as that of the goss-hawk.

Nature has meant this bird for a forest hunter. Its life is passed among the trees. Its forte is not the attack of a hare or a duck in open field; it would not succeed in that; but it is the chase after other birds or after mammals under the trees. The powerful tail of this fam-

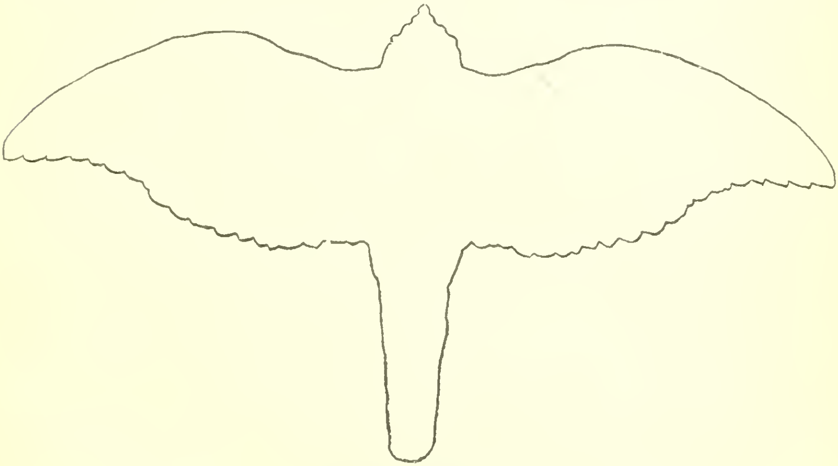

Fig. 2.—The Sparrow-hawk.

ily of birds has been given them to turn short corners; they needs must seize under the trees a pigeon or a turtle dove, and this vigorous rudder enables them to thread the innumerable branches and trunks of trees with all required velocity.

There is another bird which accomplishes the same results by other means. It is the great eared-owl (*Grand duc*).

Its life needs are the same; it is also in the verge of the forests among the trees that its hunting exploits are performed. It has no tail to speak of: it cannot afford it; for in the hole where the bird cowers in the day time a tail would be a nuisance. What, then, is to be done? The bird brings into play a peculiar aptitude, born of necessity; this is a mobility and an extraordinary power in the mode of presenting the planes of

THE HAWK TYPE.

Common name. French.	Common name. British or American.	Scientific name.		Weight of bird.	Surface within contour.	Spread of wings. (b)	Mean width of wing. (a)	Proportion. b/a	One gram of bird's weight sustained by—	One square meter sustains—	Relative surface required to sustain 80 kilos or 176·4 pounds avoirdupois.
			V.	*Grams.*	*Sq. meters.*	*Meters.*	*Meters.*		*Sq. meters.*	*Grams.*	*Sq. meters.*
Epervier du Caire (queue étalée).	....	Accipiter nisus..........	0″	152	0·093392	0·618 2·03 ft.	0·126	4·90:1	0·000614	1631	49·12
Autour	Goshawk	Astur palumbarius	0″	290	0·103008	0·718 2·36 ft.	0·16	4·48:1	0·000355	2816	28·40
Harpie	Harpy eagle	Thasaëtus harpyia	......	....	....	....	....	....	....	....	....

its wings. Changes of plane against the air which change the direction of its flight with incredible celerity. No other flying creature has this gift equally developed.

With what astonishment is the flight of this enormous bird beheld under the trees! No such dexterity can be re-called. The observer's habit being to see birds generally proceed in a straight line, it renders the flight of the great-eared owl fairly stupefying; it seems at every moment as if it were about to dash against a tree, and yet every obstacle is avoided. It flashes silently, horizontally, or even vertically through spaces where there would seem to be no passage; and this is done with absolute mechanical precision, without hesitation or slackening of speed.

This is truly the most extraordinary mode of flight, but it is a rare sight even to dwellers in the country.

The Gull type.—This genus teaches one thing, namely, that in order to hover or to penetrate in great currents of wind, the resistance of the

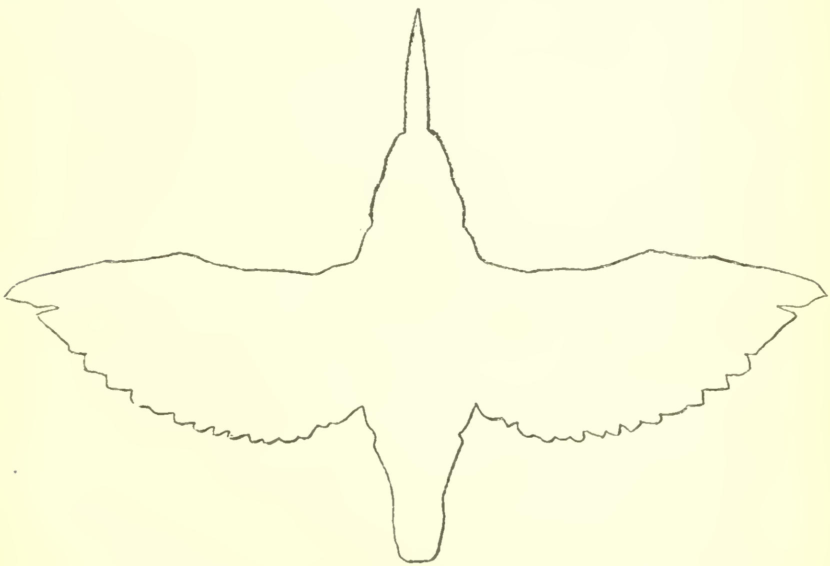

FIG. 3.—The Kingfisher.

aeroplane must be reduced to a minimum. The spread of wing is of less consequence than its width. When we think of it this is rational, but it must be confessed that nature did well to demonstrate it, for it was a difficult deduction to get at. The table shows that in this type the spreads of the wings is from 6 to 10 times their width.

The albatross, which is not included, must greatly exceed this proportion, for travelers mention some 16 or 17 feet as the extreme spread of wings, and as its width is about 10 inches we have for this bird a proportion of about 20 to 1.

This teaching is of great importance, and should be borne in mind in the design of æroplanes with adjustable surfaces.

THE GULL TYPE.

Common name. French.	Common name. British or American.	Scientific name.	T	Weight of bird.	Surface within contour.	Spread of wings. (b)	Mean width of wing. (a)	Proportion. $\frac{b}{a}$	One gram of bird's weight sustained by—	One square meter sustains—	Relative surface required to sustain 80 kilos or 176·4 pounds avoirdupois.
				Grams.	*Sq. meters.*	*Meters.*	*Meters.*		*Sq. meters.*	*Grams.*	*Sq. meters.*
Oiseau-mouche		Frochilus minimus									
Hirondelle grise	Bank swallow	Cotyle rupestris	0″	16	0·012384	0·31 1·02 ft.	0·05	6·20:1	0·000774	1291	61·92
Hirondelle de fenêtre	Martin	Hirundo urbica									
Hirondelle de cheminée	Barn swallow	Hirundo rustica									
Martinet	Swift	Cypselus apus	0″	33	0·015918	0·376 1·23 ft.	0·048	7·83:1	0·000482	2073	38·56
Sterne	Tern	Sterna	0″								
Glaréole		Glareola torquata	0″	67	0·031946	0·525 1·72 ft.	0·065	8·00:1	0·000476	2097	
Engoulevent	Night hawk	Caprimulgus	0″	62	0·040334	0·509 1·70 ft.	0·08	6·36:1	0·000650	1527	38·08
Mouette	Pewit gull	Larus	0″	232	0·109283	0·946 3·10 ft.	0·13	7·27:1	0·000471	2123	52·
Mouette		do	0″	280	0·134756	0·965 3·17 ft.	0·13	7·42:1	0·000481	2709	37·68
Goëland	Gull										38·48
Procellaria pelagica	Stormy petrel	Puffinus Kulhii	0″	500	0.14	1·17 3·84 ft.	0·124				
Procellaria	Petrel	Procellaria	0″	700		1·25 4·10 ft.	0·125	10·00:1	0·000175	5714	
Fou	Gannet	Sula									14·
Frégate	Man-of-war-bird, frigate bird.	Fregate aquila									
Flammant (mâle)	Flamingo										
Petrel géant		Procellaria gigantea	0″	2880	0·378130	1·75 5·76 ft.	0·21	8·33:1	0·000131	7616	10·48
Albatros	Albatross	Diomedia exalans									

Teal and Duck type.—These birds are the representatives of rapidity in flight. They flap to excess, supplied with carbon as they are by the heavy layer of oily fat which covers their pectoral muscles. This is the type to imitate for aviation with motors. These birds afford practical lessons as to methods of getting under way and of alighting, both of them points of capital importance.

These birds need, like the scolopax, to traverse over long distances, from one lake to the next, and as they are weaponless speed is their sole safeguard. It is enough for their safety, for they are never attacked in the air.

The eagle, their chief enemy, abandons pursuit as soon as they are well under way.

The Pelican.—Here is a wag and a philosopher, a swift sybarite, mounted on two great wings.

Where is his nest? Whence does he come to us? I confess I hardly know. I merely am aware that we see a good deal of him in Egypt. Great flocks of these birds are found on the inundated lands, upon the the Marcotis and the Manzaleh lakes. There are even some in the city, domesticated. I bought one for a dollar in the Mouski. Every year they are peddled in Cairo during November, December, and January.

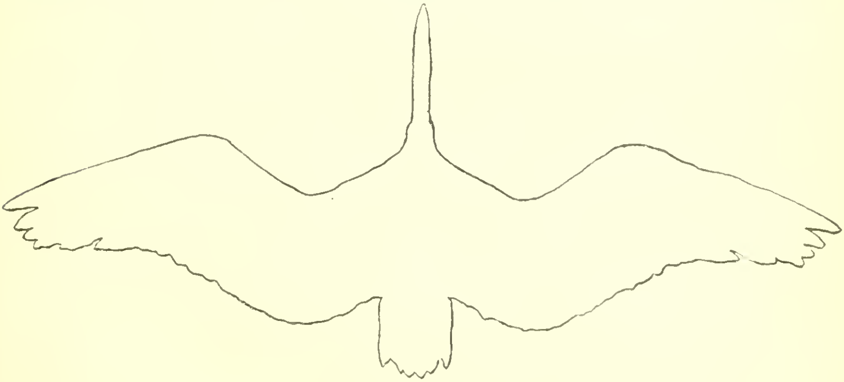

FIG. 1.—The Pelican.

What a droll creature! I had two of these for my personal friends. They were more ludicrous than will be credited. But I must abstain, for if I were to begin telling of waggish tricks of pelicans, I never would be done. Let humorists treat themselves to one, and have no fear of his bill, which is quite inoffensive, and they will have their money's worth of amusement. But, dropping the humoristic feature of this charming animal, let us attend to his flight.

The pelican possesses a peculiarity in his structure; it is an excessive length of arm and fore arm. He is without a tail. His center of oscillation, or the limits within which he can shift his center of gravity without compromising his equipoise, lies within the branches of that great flat ΛΛ which is outlined by his extended wings and body. This is the disposition of parts which gives him that equilibrium, lengthwise, which his rudimentary tail could not furnish.

His mode of flight is magnificent. He rows but seldom, for as soon as the wind permits he becomes a sailing bird.

The effect produced by great mass is always surprising. If a bird be large and has adequate surface, he practices sailing flight, as witness the pelican, who has less surface, proportionally, than the teal, and yet soars to perfection, while the other only rows. The teal is proportioned at 1 gram per 177 square millimetres (1 pound for each 0.86 square feet), and the pelican at the rate of 1 gram per 150 square millimetres (1 pound for each 0.73 square feet).

When in full flight he does not stretch out his neck a yard in front, like the goose, the stork, the swan, but he curves it back like the heron, and rests his head gently upon his shoulders, which attitude gives him a peculiarly graceful appearance. He then seems so much at his ease, he appears so comfortably posed upon his two immense wings, set at picturesque angles, that once well launched he seems to glide through space without the least fatigue.

Of all the large birds he is certainly the one which presents the most elegant silhouette. The great vulture is rigid, and looks as if cut out of tin plate: the swan and the goose have an attitude as if already spitted; the eagle is stiff and all of a piece, but the pelican, notwithstanding his awkward heaviness when on the ground, becomes as graceful as a gull once he has mounted in the air. The varied attitudes of his wings, the great length of their arm and forearm, offer every moment new aspects which the evolutions of other birds never present.

In point of intelligence the pelican is among birds what the elephant is among mammals. Like the latter animal, a boundless curiosity attracts him to man. The doings of the sovereign of creation interest him profoundly. The attention which he bestows upon all movements is a sure sign of great intelligence.

He will not, like the large birds of prey, morosely assume a gloomy state of sulks, beginning with his captivity and ending with his death; he will not go crouch in a corner and motionless ponder on his lost liberty—not a bit of it. After two or three days, if, without looking at him or apparently noticing him in any way, you are occupied in doing something, he will not let half an hour pass before he is between your legs, the better to observe your actions. Every now and then he will stretch out that frightful bill of his, but there is no need of extra guard; all there is to do is not to draw the hand back, because it might be cut against the saws of his mandibles. When he sees no reply to his overtures he will become almost troublesome in his familiarities; he will come into the house as if it were his own, he will pick fleas off the dogs, he will purloin a shoe, he will make way with a ball from the billiard table with an air of perfect innocence; and he will not even

Duck and Goose Type.

Common name. French.	Common name. British or American.	Scientific name.		Weight of bird.	Surface within contour.	Spread of wings. (b)	Mean width of wing. (a)	Proportion. b/a	One gram of bird's weight sustained by—	One square meter sustains—	Relative surface required to sustain 80 kilos or 176·4 pounds avoirdupois.
			V	*Grams.*	*Sq. meters.*	*Meters.*	*Meters.*		*Sq. meters.*	*Grams.*	*Sq. meters.*
Martin pêcheur (femelle)	Kingfisher	Alcedo hispida	0	27	0·011672	0·232 0·76 ft.	0·05	4·64:1	0·000432	2314	34·56
Martin pêcheur (mâle)	do	do	0′	31	0·013000	0·25 0·82 ft.	0·054	4·55:1	0·000433	2309	34·61
Do	do	do	0″	34	0·013080	0·262 0·86 ft.	0·052	5·03:1	0·000384	2604	30·72
Sarcelle	Teal	Anas querquedula	0	297	0·052624				0·000177	5619	14·16
Canard (femelle)	Wild duck	Anas clypeata	0″	727	0·074562	0·70 2·30 ft.	0·10	6·36:1	0·000102	9750	8·20
Canard (mâle)	do	do	0′	925	0·083709	0·72 2.36 ft.	0·11	6·54:1	0·000090	11050	7·23
Canard kasarka		Anas casarka									
Canard de Barbar											
Cormoran	Cormorant										
Oie sauvage	Wild goose	Anser sylvestris	0″	20.0	0·244317	1·37 4·49 ft.	0·19	7·26:1	0·000120	8333	9·60
Oie domestique	Goose										
Cygne	Swan										
Pelican gris	Pelican	Pelicanus onocrotalus	0″	6625	0·998608	2·80 9·19 ft.	0·39	7·17:1	0·000150	6634	12·
Do		do	10′	6625	0·926008	2·80	0·39	7·17:1	0·000132	7578	10·56
Do		do	20′	6625	0·778878	2·80	0·39	7·17:1	0·000111	9009	8·88

cease his rascally tricks at night, for if he is allowed he will stay up, like the human biped.

In the garden or yard, you must not expect him to fraternize with the other inmates; he has a profound contempt for these weak-headed winged creatures. He will not quit the neighborhood of man's social gathering, but he will squat down all rolled up in a ball, in the middle of the group, rest his bill on his back, and from this vantage ground his intelligent eye will follow every gesture and every word spoken.

He imposes himself upon man as his companion; he decides that his society will be accepted, and as after all he is not very troublesome, as—far from being repugnant—he is clean and stately, man gives in and becomes his friend.

We have yet to speak of the pelican when in possession of his full wings, when he is able to fly; for up to this we have only described the bird whose wings have been clipped; but the chapter would be endless. I will only add this, that his familiarity grows with his wings. Judge then what it becomes when his plumage is complete.

It might perhaps be possible to acclimate the pelican in Europe, in full liberty. He would find himself so expatriated that he might not attempt to escape. Should his wings be allowed to grow his first trials of flight would not permit him to think of undertaking a long journey; at most, he might visit the surrounding country the first year. Care being taken to keep him captive in September—the period of migration—we might in a country wherein hunting them was prohibited treat ourselves to the curious sight of the evolutions of these great water birds which are as amiable as the swans which we have are stupid and mischievous.

The Swan.—There are two cities where the swan is easily observed, Geneva and London. The foggy atmosphere of the Thames does not permit of keeping them in sight very long, moreover they look very melancholy on that foul stream.

To observe them thoroughly, there is but one spot—the lake of Geneva. There these beautiful birds are quite at home, and act as if they owned the whole lake.

They build their nests in the moats of the city, and make the rounds to beg, or rather to collect their daily rations of bread, as far as Villeneuve. They even do more, they follow the steamboats and dip down to gather food thrown over to them; then when the boat has gained a mile or two ahead, they resume their flight, catch up with the ship and settle in its wake.

Their flight is a composite of beats of small amplitude, alternated with rectilinear glidings. They do not wheel in circles like the pelicans and birds of prey; they always proceed in a straight line, like the ducks, the geese, and in fact like all birds which have but little proportional surface at their command.

* * * * * * *

The great eared-Owl.—The great eared owl is a curious creature, and a painter's brush would better describe him than a pen. Those great horn-like ears, those large yellow eyes, that plumage spotted with crosses and drops, the noise which he makes when snapping his bill, which might be mistaken for that of a crackling bone; everything in fact, even its attitudes, give it a satanic air, little like anything in this world. But let us disregard this infernal aspect, and examine him in broad day.

It is a large bird of prey; its talons are strong, its wings are powerful; its beak, while almost entirely hidden beneath the hairy feathers protecting its nostrils, is nevertheless strong and with a force not found in the bill of most diurnal birds of prey. Its ears are very large; we see at first sight, that in consequence of the enormous development of this organ, the sense of hearing must exist in great perfection. This group of brilliant qualities, joined to a reckless courage, render this bird an animal of extraordinary power.

The great eared-owl might dispute with the eagle the empire of air. The eagle is like the lion: he has the noble look of royalty, whilst the tiger, which has only a brilliant reputation for ferocity, might easily dispute for the prey should both meet on the same hunting grounds.

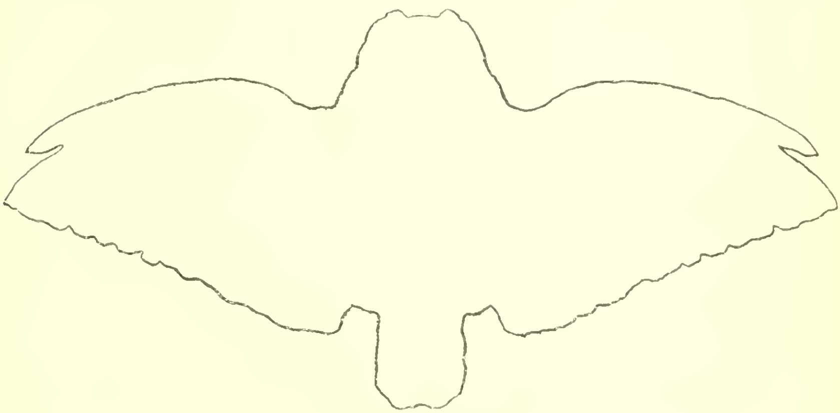

FIG. 5.—The great eared-Owl.

Let us consider the bird further, for we are confronted with a remarkable creature. His bony frame is robust, his feathers are like hose of all nocturnal birds, of velvet-like softness; but this soft down covers muscles with a different order of action from those of the eagle; they are shorter, quicker, and more rigid in their contraction; the lever arms are longer. The flight is a marvel; it is excessively complicated in action. He soars perfectly well but rarely does so; he rows like a pigeon, and possesses moreover the faculty of stopping suddenly when at full speed, and of glancing off in another direction. This he does to avoid collision with tree trunks at every moment. The flight is absolutely silent. We see these large creatures flash under the foliage, and we do not hear a murmur. This silence results from the

THE OWL TYPE.

Common name. French.	Common name. British or American.	Scientific name.		Weight of bird.	Surface within contour.	Spread of wings. (b)	Mean width of wing. (a)	Proportion. $\frac{b}{a}$	One gram of bird's weight sustained by—	One square meter sustains—	Relative surface required to sustain 80 kilos or 176·4 pounds avoirdupois.
				Grams.	*Sq. meters.*	*Meters*	*Meters.*		*Sq. meters.*	*Grams.*	*Sq. meters.*
Scops		Ephialtes zorca	1' 0''	150	0·058438	0·526 1.73 ft.	0·12	4·38:1	0·000389	2570	31·12
Hibou brachiote		Otus brachyotus	5'	305	0·141438	1·00 3.28 ft.	0·17	5·88:1	0·000463	2156	37·04
Effraie	Screech owl	Strix flammea	0'	305	0·141194	0·94 3.08 ft.	0·165	5·69:1	0·000462	2160	36·96
Ascalaphe		Bubo ascalaphus									
Grand-duc	Great eared-owl	Bubo maximus									

conformation of the feathers, which are not constructed like those of diurnal birds, and the shock of the quills against each other is deadened by an exceedingly soft down. It is only at dawn, in the spring, that the bird is to be seen high in air; then an occasional couple is seen wheeling up high, but as the day advances they rapidly come down and make for their gloomy dens, where they remain crouched till sunset. One evening I witnessed the setting out of two owls on their hunt. I had climbed with a young guide to a cavern high up, and we looked down upon the pine forest. The day was dying away. We were sheltered behind a large rocky shoulder which hid us perfectly, and there we waited. Five minutes after sun-set an owl appeared, as if by enchantment, perched on a rock in front of the cavern. We had neither seen nor heard it come. A few instants later a second bird appeared, taller and larger than the first; this was the female, and it was huge; its height was at least 31 inches. They slowly turned their horned faces from side to side, then one awakened the echoes of the valley with three piercing notes, and yet harmonious, in the fashion of screech-owl melody. The voice is strange and impresses much. Then the male descended to a rivulet flowing from the glacier; the female followed; they drank, bathed their faces a little and re-ascended to the rock where first we saw them. There they dried themselves and smoothed their feathers, and then they began to dance. I had been told beforehand of this performance and it had been described in terms so excessive that I had not believed; but now I witnessed the most grotesque scene which can be imagined. Fancy two huge creatures, by no means elegant, springing into the air alternately like jumping jacks, snapping their beaks by way of accompaniment.

At this extraordinary sight a wild guffaw escaped me; the shepherd put his hand upon my arm to beg for silence. I looked for the birds and they were gone, the rock was bare; they flitted away as silently as they came. I visited their eyrie; there was scattered about some 400 or 500 pounds of bones, chiefly of the hare, of tips of partridges' wings, and of the balls of hair rejected from the bird's stomach.

I possessed a couple of these birds in captivity. They were the young of the pair I have just described. During the ensuing year the little shepherd took them from the nest and brought them to me.

These birds, although their wing spread is nearly 6 feet across, flew perfectly well in a cage 16 by 40 feet. They went in different directions, making several whole rounds without coming to rest, while the large diurnal birds of prey in the same cage confined themselves to single journeys lengthways, passing over the space with three noisy beats of wing.

The Heron type.—This table comprises birds possessing large sustaining surfaces in proportion to their weight. Now what is nature's object in endowing them with such excess in that direction? It is probably to enable them to soar in calm weather when the wind is light, and above all to

THE HERON TYPE.

Common name. French.	Common name. British or American.	Scientific name.	♂	Weight of bird.	Surface within contour.	Spread of wings. (b)	Mean width of wing. (a)	Proportion. $\frac{b}{a}$	One gram of bird's weight sustained by—	One square meter sustains—	Relative surface required to sustain 80 kilos or 176·4 pounds avoirdupois.
				Grams.	*Sq. meters.*	*Meters.*	*Meters.*		*Sq. meters.*	*Grams.*	*Sq. meters.*
Huppe	Hoopoe	Upupa epops	V 0″	62	0·017993	0·430 1.41 ft.	0·115	3·73:1	0·000774	1291	61·92
Pluvier armé	....	Charadrias spinas	0″	170	0·070230	0·600 1·97 ft.	0·125	4·80:1	....	2420	33·04
Vanneau	....	Vanellus cristatus	0″	210	0·103770	0·755 2·48 ft.	0·138	5·47:1	0·000494	2024	39·52
Héron garde-bœuf	....	Buphus minutus	0″	318	0·131300	0·893 2·93 ft.	0·155	5·76:1	0·000413	2421	33·04
Ibis	Ibis	Ibis falcinellus	0″	365	0·123990	0·900 2·95 ft.	0·150	6·00:1	0·000339	2943	27·12
Bihoreau	Night heron	Ardea nycticorax	0″	615	0·180826	1·040 3.41 ft.	0.200	5·20:1	0·000294	3402	23·52
Héron rouge	....	Ardea purpurea	....	....	....	....	....	....	....	....	....
Héron gris	Grey heron	Ardea cinerea	0″	....	....	1·664 5·46 ft.	....	....	....	....	....
Spatule	Spoonbill	Platalea leucoradia	....	....	....	....	....	....	....	....	....
Cigogne	Stork	Ciconia alba	0″	2140	0·615240	2·080 6·82 ft.	0·300	6·93:1	0·000287	3536	22 62
Grue	Crane	Grus	....	....	....	....	....	....	....	....	....
Argula	....	Argala vulgaris	....	....	....	....	....	....	....	....	....
Jabiru	....	Mysteria senegalensis	....	....	....	....	....	....	....	....	....

alight without breaking their legs. It is clear however that they are hampered by this excess of surface; not one of them is remarkable as a flyer, neither as to velocity—which is easily explained, nor even as a permanent denizen of the air—which is more extraordinary.

They are in fact so well equipped for sailing on light winds that the surface resistance destroys all the other qualities when the breeze freshens. It is only when the weight becomes 4 pounds or more that the mass momentum succeeds in overcoming the friction of the air against these over-feathered wings.

The birds first named in the table fly as unsteadily as the butterfly; the hoopoe, the armed plover, and the lapwing can advance against strong winds only by completely folding their wings. This deficiency diminishes with the increase in weight. The ibis flies better than the small heron, and both are distanced by the storks.

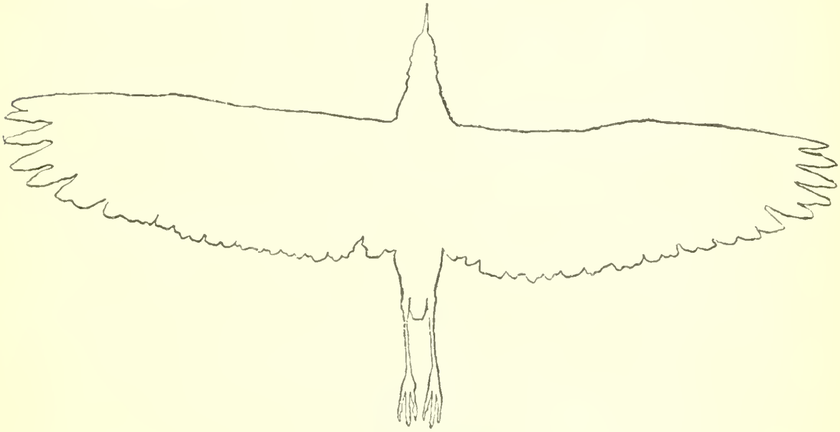

FIG. 6.—The grey Heron.

We find proof in this type that the useful, active surface of the wing lies in the hand and the forearm, and that the arm remains almost quiescent in flapping. The demonstration is palpable, as is ever the case in nature. The feathers of the humerus have been simply suppressed in most of the herons, and only those of the covert remain, which latter feathers are merely ornamental.

* * * * * * *

The kestrel Falcon.—The kestrel is common in France. It inhabits our large cities. It is known by all observers, and they doubtless have gathered their best knowledge from its evolutions. Its strength is great and it always rows when hunting; but when there comes a change of weather and the south wind sets in, then the creature climbs up soaring into the sky and exhibits its talents as a sailing bird, which talents are fully as great as might be expected from its mass.

The peregrine Falcon.—A rare bird, and therefore difficult to study. An astonishing rower, reaching at times a velocity almost unique; the pigeon, the duck are then greatly out-distanced. - - -

It soars well, but only when at leisure. It weighs 1.32 pounds, and its surface amounts to 1.72 square feet, being in the proportion of 1.3 square feet to the pound.

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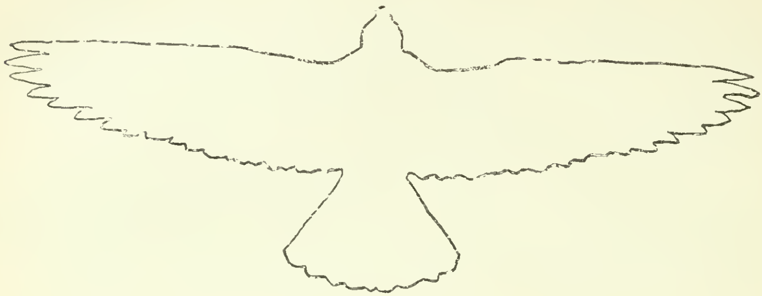

Fig. 7.—Kestrel Falcon.

Eagles.—With the fish-hawk (*Pandion fluvialis*) we reach the heavy birds. We still find a bird which beats the air, but already the results of inertia are manifest. When the mass is 2 pounds or more there appears a steadiness in flight not found among the 1-pound birds.

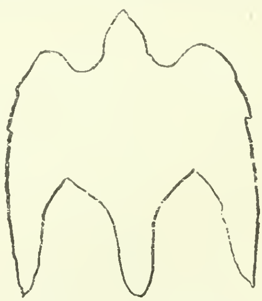

Fig 8.—Peregrine Falcon.

The small eagles of Europe and of Africa, the imperial eagle with white back (*Aquila heliaca*), and the great golden eagle (*Aquila chrysaëta*) found in the Alps, all have the same modes of flight. Their talents as sailing birds increase with their weight.

The necessities of their existence require many different qualities: First, they must be able to remain easily in the air, in order to survey the field, to watch a possible prey for whole hours; therefore they must be able to soar well, and this they do to perfection. Once the prey discovered, there needs be great speed to capture it, for often it is a duck, and a duck flies very fast; or it is a hare to be caught on the run, and this is not at all an easy enterprise. To gain this great speed the eagle utilizes gravity; he lets himself drop 200 or 300 yards, and employs the velocity so acquired with great dexterity to catch the game. These violent hunting exertions in catching other rapid animals require an enormous muscular power on the part of these birds.

A few eagles are to be found in Savoy: they are the finest, but they are rare. Some few are also to be seen in Egypt. From time to time a passing bird is seen with an unusual figure; when it is far distant one remembers that it is an eagle. In Algeria I was enabled to observe this creature close at hand. In winter there were always three or four stationed some 200 yards from my farmhouse. They were hunting for wild ducks on a drowned meadow. Sometimes they came to inspect my barnyard, but from a distance, as they were not well received. Upon the whole, they kept nearly as far away as one would desire for a chicken's sake, for it does not take long to pick up a fowl. There is a tremendous outcry from the roosters, then a terrible hissing and scuffle, and an unhappy egg producer is seen ascending into the air, strewing her feathers by the way during her dizzy rise.

The tales which are told of the eagle letting his prey drop when a gun is fired at him, even when beyond range, are perfectly true. Only in this, as in all else, it must be done at the proper moment. I once caused the experiment to be tried by a sportsman who doubted the fact. He made such haste that the eagle had not had time to kill the duck before letting him go; the result was that the eagle went one way and his victim another. As both by that time were three times beyond range, we had to be content with looking on.

Not far from there, grew two great ash trees, where often in the spring eagles were seen in pairs. Here it was that occurred that wondrous *tour de force* of rising in the air, advancing against wind, an observation of prime importance already described in a preceding chapter.

The great golden Eagle.—Here is undoubtedly the king of the birds. He possesses strength and courage. Having no enemy his equal, he peacefully passes long days in the beatitude of uncontested autocracy. The eagle fears none but man, and even him he fears but little. Brought to bay, he does not hesitate to hurl himself at his enemy. In captivity he is at first exceedingly dangerous; his ferocious temper renders him an untamable animal. It requires great skill to succeed in impressing him with fear, and moreover he must not be excited, for then he will fight to the death.

Nature has created him to keep down undue increase. In this he is like the *felis* (tigers, etc.), the *squales* (dog-fish, etc.), and the *esox* (pike etc.) This tyrant of the air is abundantly provided with all the weapons necessary for his murderous life. His arms consist in eight talons as long as a finger, curved and sharp pointed and moved by terrible muscles. His much-hooked beak serves him to carve the animal perforated by his talons.

His wings are large and exceedingly strong. They are preeminently adapted to sailing flight. He rarely beats the air unless there is no wind, or unless he is loaded with a prey. Pen is powerless to depict the majesty of his gait, the amplitude of the immense circles which he sweeps in the air. At times he is absolutely motionless. He is examining the field or watching a prey; then suddenly he drops hundreds of yards. He falls like a meteor with the velocity of bodies falling through space.

The speed is such that it produces a sound difficult to describe. It is not like that of the bullet or of the cannon ball, but must be heard to get a true conception. Then, when within a dozen feet of earth, his wings' great strength safely checks his descent; and this at once—in half a second—merely by expanding his wings to their full spread.

His skill is wonderful; never a miss makes he. His eyes are excellent; from high up in the air he spies out the rabit hiding in the thicket, or the inconspicuous duck swimming among the reeds. He uses his talons, the arms with which he kills, in a remarkably skillful manner. In captivity, when he is hungry, he catches on the fly the morsels of meat thrown to him with a single claw, and never misses them if they pass within his reach. His movements have all the precision of those of small birds. He is free, quick, sharp, and powerful in his movements; above all, his *coup d'œil*, the power of taking all in at a glance, is very remarkable. As the motor muscles of the eye-ball are but little developed, he is compelled to turn his head whenever he desires to see anything sharply. His head then assumes splendid poses; his eye, that brilliant gem set under a deep arch, darts out lightning glances; his curved beak, his savage air, his sharp head feathers bristling up and forming a diadem—all that ensemble of vehement sweeping outline—make the eagle a model of power and of audacity.

He lords it over a territory which he always selects of vast extent. All the smaller mammals dread him; the young of larger animals fear to be seen by him; the young chamois crouches up to its dam, the old bucks call the herd and stamp their feet with fury. Man himself—in infancy—has been attacked by him.

He is intelligent only from a hunting point of view. A very interesting spectacle it is, that of a family of eagles making a *battue* in order to furnish the nest with provisions.

The male is up 100 yards in air, quite motionless, the female is beating the thickets; her flight while doing this has an ease of great elegance; she follows the undulations of the ground without effort; glides from one hill to another, descends and re-ascends the mountain slope; then when a prey appears the two spouses are upon it almost at the same time. It often happens that a hare starting up 10 yards from the female is caught by the male, who was stationed 100 yards, in the air; he dives head foremost, is upon the prey in four or five seconds, and picks it up on the fly; then, if he is in the mountains, he first plunges in the valley with his load, and with great wing-beats re-ascends to his eyrie. There the spoils are divided, and this never takes place without much dispute, spite of the charms of matrimonial bonds.

Aside from this old leaven of ferocity, which constantly appears, the

THE FALCON AND EAGLE TYPE.

Common name: French.	Common name: British or American.	Scientific name.	V	Weight of bird.	Surface within contour.	Spread of wings. (b)	Mean width of wing. (a)	Proportion. b/a	One gram of bird's weight sustained by—	One square meter sustains—	Relative surface required to sustain 80 kilos or 176·4 pounds avoirdupois.
				Grams.	*Sq. meters.*	*Meters.*	*Meters.*		*Sq. meters.*	*Grams.*	*Sq. meters.*
Alouette cochevis		Galerida cristata	0	34	0·022000	0·305 1. 00 ft.	0·07	4·35:1	0·000628	1592	50·24
Alouette cochevis (male)		do	0	37	0·024130	0·331 1. 09 ft.	0·08	4·13:1	0·000652	1533	52·16
Faucon crecerelle	Kestrel	Falco tinnunculus	0	181	0·092010	0·74 2. 43 ft.	0·125	5·92:1	0·000508	1968	40·64
Faucon pèlerin (femelle)	Duck hawk, peregrine Falcon.	Falco peregrinus	0	580	0·154228	1·035 3. 40 ft.	0·15	6·90:1	0·000265	3773	21·20
Milan (male)	Kite	Milvus ægyptiacus	0	640	0·287400	1·33 4. 36 ft.	0·22	6·04:1	0·000449	226	35·92
Buse	Buzzard	Buteo									
Circaëte		Circaetus									
Balbuzard	Fish hawk	Pandion fluvialis	0	1270	0·329178	1·548 5. 08 ft.	0·24	6·45:1	0·000259	3797	20·72
Do		do	10	1270	0·279175				0·000162		12·96
Aigle criard		Aquila navia									
Aigle impérial		Aquila heliaca									
Pygargue	White-tailed eagle										
Aigle royal	Golden eagle	Aquila chrysaëtes									

family is perfectly well brought up, and above all abundantly supplied with nourishment. During the education of their young, families of eagles consume enormous quantities of game; the eaglets during this period require much food in order to furnish material for the growth of their great feathers. Nature then inspires the parents with an activity, which, happily for neighboring hares and rabbits, has nothing in common with their usual indolence. During this time, this period of activity, lasting a month, there is no respite; the vicinity of the eyrie is generally encumbered with putrefying carcasses; but luckily the crows have an eye out for everything which is spoiling, and possess the audacity to go for it even in the eagle's nest. All things considered, the crows run little risk; they are so adroit that even in a very confined space they manage to escape.

The great golden eagle, which I possessed for years, had always a magpie with him. There is no sort of malicious trick that this mischievous little creature did not play upon his terrible and taciturn companion, but he received no more attention than the tom-tits, which may be seen with the telescope roving among the branches among which the nest of the eagle is built.

The eaglets, after a month and a half, are as big as papa and mamma. Their first flights are timid enough and the parents then follow them with peculiar solicitude; then little by little, as skill in flight and hunting increases, the family affection wanes, the eyrie is abandoned, and each by degrees becomes a hunter upon his own account. - - -

Two eagles are rarely seen together when they have no young ones. Those shut up together in cages, all perish in the same manner, from a stroke of the talon, penetrating the brain.

Vultures.—Let us rest long in our studies of this type of flight, for it is full of useful instruction: this is the type which will lead man to navigate the immensity of space.

This great family of birds solves the problem of remaining in the air with the least expenditure of force; we may even say, in other words, that it is that which flies, or rather soars, with the utmost science. The life needs of the creature, here as everywhere, determine its kind of talent. The vulture, to make a living, must rise to a great height thence to gain a large field of observation, and he must there long remain without fatigue.

Now consider the construction of this bird. His weight is very great, his wings are immense, both in length and breadth; his large proportional surface sustains him, and his great mass stores up momentum. So we see him after a few beats of wing, at once begin to soar, climbing up in the air and floating there with no expenditure of force, save for the start and for guidance. Certain species of vultures, particularly the larger, absolutely can, upon a windy day, leave their perch in the morning, travel many leagues, spend the whole day in the air, and get back to their perch at night without one single beat of their wings.

In this family, velocity is only useful to the smaller species, which are, after a fashion, the purveyors of the larger; therefore the Egyptian vultures, the turkey-buzzards, the urubus, having to execute more varied movements expend much more force.

Now, here is the way a vulture spends his day upon one or the other continent. The larger species have spent the night perched among rugged and inaccessible rocks, where they gather for shelter from the wind, if they have neither eggs nor young ones. The turkey-buzzards and the Egyptian vultures have been roosting in the lower regions, they are less fierce and much more intelligent. The sun comes out and dries the dew collected on their feathers; the vultures stretch their wings, limber up the joints and trim the growing quills with all the care that the maintenance of an essential organ needs. About 7 o'clock there are many flappings of the great wings, but without quitting the perch; then they sink their heads between their shoulders and resume their sinister, forbidding look. Between 8 and 9 o'clock the breeze begins to rise; once in a while the vulture glances into the valley through those magnificent eyes, unique in creation for their power, then with four or five great beats of wing he launches into space. He descends some 50 yards on rigid wings, and is then in full sailing flight. The smaller species, who are earlier risers, are already at work and searching for food.

The large vultures sail at heights which vary with the species. The tawny vultures, the Sarcoramphes or *papa* of South America, generally keep at elevations of 500 or 600 yards in the air; they are scarcely visible from the ground. The arrian vultures, the otogyps, the condors usually float much higher; they become quite invisible.

The arrian vultures survey the movements of the tawny vultures, who in turn have an eye upon the Egyptian vultures, which latter again watch the doings of the kites, and especially of the crows.

In America the urubus is watched by the turkey-buzzard, the latter by the *papa*, and this last by the condor.

As all these great birds of prey thus establish a complete net-work of observation over the earth, from the mere fact of mutual surveillance, just as soon as a meal is found all the neighbors close by start in that direction, they are at once followed by the others, and thus they assemble very rapidly.

They scent the carcass, is the common expression. In point of fact this is quite incorrect, being impossible if the birds are to windward of the dead animal. Their olfactory apparatus is so little developed that it is quite insufficient to guide them to a carcass, even close at hand. This may be easily tested by hiding some tainted meat and attracting a vulture to the neighborhood. He will pass close to it without finding the meal; his sense of smell will not have revealed it.

The attitudes of these birds in the air are particularly worthy of attention. Their aspect when sailing upon a wind of about 11 miles per hour, which velocity is best adapted to exhibit their faculties, becomes a most interesting study of flight without beat of wing.

It is clear that to rise upon so feeble a current of air they must spread all their surface. At this speed of 11 miles per hour the Egyptian vulture holds his wings to an even straight line; the *Gypoierax cathartoide* slightly brings the tips of his pinions forward: the tawny vulture advances them so much that the angle produced in front is 165°; the oricou or Nubian vulture goes much further, to make a satisfactory sketch of his attitude in flight, would require an angle of 140°.

The great tawny Vulture.—We are now face to face with our *desideratum.* Look at his beak! We might disregard his talons, but the beak is terrible, of a force not to be imagined; garments are insufficient to protect a man from this beak.

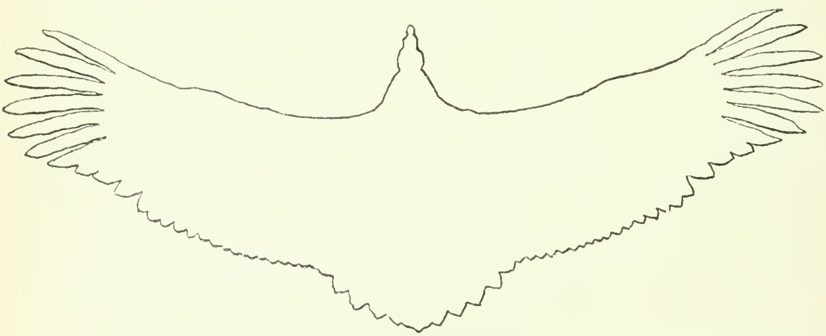

FIG. 9. The tawny Vulture.

Once he is dead, your gorge rises, for the smell is horrible. This odious perfume is in no way fugacious, for it persists worse than musk; the whole body of the animal is impregnated with it. The room in which it remains for only a few hours will not lose that nauseating odor for many months. Then look out for lice; they are of good figure. The first of the enormous parasites one sees, wandering over one's clothes, causes inexpressible astonishment. However, notwithstanding its size, it is not dangerous, for it does not become acclimated to man.

But, passing by these petty annoyances, what a magnificent animal we have before our eyes. Here we have alar spread of 8 feet or more of wing, a weight of 16 pounds for this admirable living aeroplane. Beyond him there are but three or four varieties which surpass him in size, but without causing us to forget him. In any case he is quite their brother in sailing flight.

Little need be said concerning the oricou (*otogyps*), save that the closest attention is required to recognize them in any group.

As to the condors, inasmuch as their conformation is the same as that of the vultures of the ancient continent, we may say, without risking a mistake, that their mode of performance in the air is practically the same.

We commend the special study of the ways of the tawny vulture. This will better—in five minutes—explain the action of sailing flight and the possibility of its imitation than long observations of all the other families of birds.

The dominant note in this flight, the remarkable feature, is the decided tendency to perform all required manœuvers and evolutions by gliding, by soaring flight, and to avoid all performances which involve flapping the wings. The oricou and the arrian are in the same category; they even exaggerate this tendency. All these large birds only beat the air when there is a dead calm, an atmospheric circumstance quite rare in Egypt, and as the slightest breeze suffices for their support it is rarely the tranquillity of the air which keeps them at rest. Rain troubles them much more than a calm; they seem

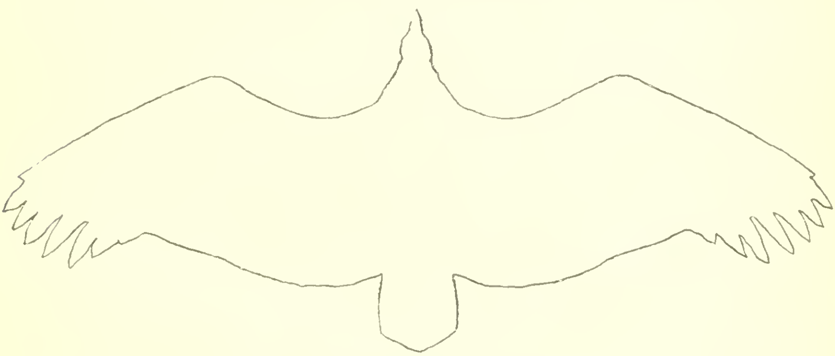

FIG. 10.—The tawny Vulture.

to dread having wet wings. Great winds also disorder the economy of their mode of flight; they are proportioned to sail well upon an average wind, so when the wind freshens much they begin to encounter difficulties; and when it blows a tempest they seek shelter and do not stir out. This results from the great breadth of their wings, a breadth which by the resistance it offers completely deranges their facilities for locomotion.

In order to encounter strong currents of air, there must be narrow wings; thus, observe the gulls, the stormy petrel, the albatross. In a wind where all aboard ship is clewed up you will see them in full activity, chasing ardently and moving with ease—they are in their element; there is no beating the air; they are then, as it were, set on two rigid supports, much curved downwards, skimming the wave with astonishing precision, fingering it with their wing tips, rising and descending with the billows without ever being overtaken. These same birds, in a wind of 11 miles per hour, a light breeze, are compelled to settle down on the water, to dabble around like common ducks: while in this same wind the great land-sailing birds sweep with ease those great circles which transport them, without fatigue, up to enormous heights.

Thus the vulture is the bird which can utilize the feeblest current of air in order to obtain sustaining power; he exaggerates the type of what we might call permanent rest in the air.

I have already said—and I repeat it, a large vulture can make long flights without once beating the air. I have seen the following performance, not once but a hundred times: At the abattoirs of oriental cities vultures are to be seen in great numbers, waiting a propitious moment to get at their food, and sustaining themselves in the air meanwhile without a single beat of wing. They mount up out of sight, they descend within 200 yards of earth, advance against the wind, glide with the wind, slide to the right or left, cruising in a single hour over all the surrounding country to see if there be not a dead animal more easy of access: and they perform these maneuvers the whole day long, making twenty ascensions of 1,000 yards each, gliding over 100 leagues, and all this without one single stroke upon the air.

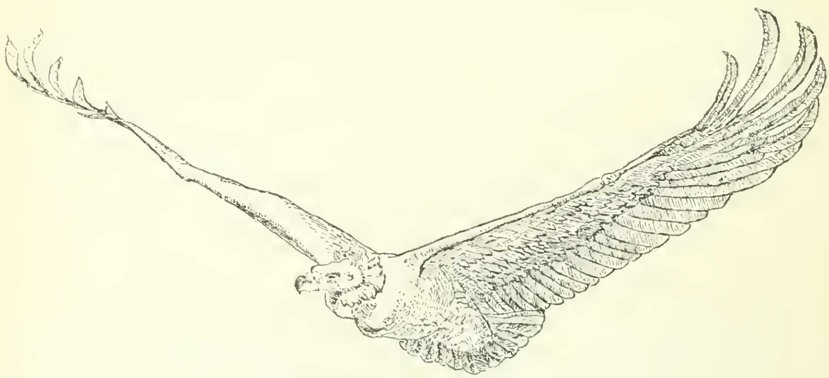

FIG. 11.—Oricou or Nubian Vulture.

When you go still-hunting for a tawny vulture, take notice how he first comes into sight; he does not then appear to be a large bird. At the altitude at which he habitually soars he appears of exactly the same size as the kites and the Egyptian vultures; he makes no more impression than they. You will however learn quickly to distinguish him by the angle to the front produced by his wings, by the absence of wing beats, and above all, by the slowness and steadiness with which he moves in space. This is an infallible sign by which to recognize him as far as the eye can reach. It is only much later that his true size will be understood, when he is only 200 or 300 yards off; and as he approaches within that distance he will grow in appearance much faster than other birds. You will further distinguish him by the peculiar spread of his wing tips. We may say that this is the bird who spreads his primary feathers most widely apart from each other. There is at the extremity an open space between each quill about five times the width of the feather.

Still another peculiarity: the primary feathers, instead of tapering towards the point, are constructed on the reverse plan: they seem to be implanted into the wing by the thin end: the outer tip being materially wider than the part which seems to be attached to the wing, and which precedes the main widening of the barbs. These large feathers, widest at their tip and spread asunder, present a curious outline which would please artists greatly if they could observe this bird in his native habitat.

To the peculiar construction described we must add the effects of the partial rotation of the quills within their sockets, which action is observed only in these large birds. These quills must be wonderfully strong and elastic, for the birds put them to severe proofs. During the efforts which he makes when starting up from the ground and when his pectoral muscles are doing their utmost, the tips of the feather point directly to the zenith. In short, from every point of view these great birds, when free, are exceedingly interesting to observe. There are altitudes, quite unknown to those who only see the bird in museums, which would confer success on an animal painter, if he reproduced them.

But there must be freedom: otherwise, we have only eagles motionless as milestones, or ill-smelling vultures worrying themselves to death, their heads smothered between their shoulders: two aspects which have nothing in common with that of these kings of air proudly traversing the immensity of the skies. The one circumstance which frequently deprives the observer of the chance of witnessing their interesting evolutions is the bird's alarm. At the slightest apprehension these great creatures resort to rowing flight, they desire to get rapidly beyond danger, so that developing all their powers with strokes of wing they quickly fly away.

Their power of vision must be great: we may safely assume this, because these birds, of all flying creatures, are those whose mode of life requires the most extensive views.

A sparrow needs a field of view of but a few hundred yards: a more powerful organ of sight would be needless, and therefore atrophied in a few generations: the sea birds need to observe the surface of the waves for only some dozen yards or so. It is not among these creatures that we must seek for those perfect lenses, capable of collecting all divergent rays of light.

The hunting birds of prey, such for instance as the falcons and the eagles, often scrutinize the surface of the ground from a great height; the latter birds, especially, sometimes maintain themselves at an elevation of 400 or 500 yards while hunting: but what is that distance when compared with the 3 or 4 miles required by the vultures to study their field of research?

We may safely conclude that the constant necessity for seeing further than other birds has caused them to acquire in the organ of vision a perfection not possessed by other birds. We must therefore be ourselves invisible to be able to witness their extraordinary evolutions when sailing; or better still. we must seek them in the primitive countries where they have not yet learned to be afraid of man. and even there we should wear a dress which shall not attract their attention. for otherwise they will not come down to a meal.

In order to see these birds. French observers must leave home. There are no vultures nearer than the high plateaus of central Auvergne, the Alps. and the Pyrenees. where there may be found (very rarely however) the *Gyps occidentalis.* which is—on a smaller scale—the duplicate of *Gyps fulvus.* or tawny vulture of Africa.

If chance does not bring to us one of the latter master soarers. we must entice him: a dead carcass planted in some isolated spot is the best means to attract him. By crossing the Mediterranean to Algeria one is certain. with a proper bait. to see the bird, particularly in the autumn; for it is rather an uncertain enterprise to endeavor to find him in northern Africa in other months than September, October, and November. There are undoubtedly a few at all seasons, but it is only during those three months that they are in considerable numbers; either in consequence of their annual migrations from north to south, or from other causes. In any case, even where there are many they are not uniformly abundant. Sometimes we may chance upon a flock of a hundred, and then remain for years without seeing them except afar off.

It is unfortunately an unknown bird to those interested in the problem of flight, for not one person in a hundred has seen it in the air. In Algeria, even in Cairo, (where there are some sailing over the city every day during three months of the year,) most of the European residents are unaware of their existence. But when the student takes the pains to go where the bird is to be found; when he sees this great animal, large as a sheep, painfully rising from the ground with strokes upon the air whose hissing is heard 300 yards away in the silence of the desert; when he sees them afterwards describing their endless sweeps. he appreciates this most interesting sight; every human being is chained to the spot; even the Arab is stirred with emotion; for in this bird we have found motion under a new aspect; it resembles as to majesty and impressiveness the action of a locomotive at full speed.

When we watch a martin flashing through space we think of high speed mechanism; when it is a snipe or a partridge which flies off, we are reminded of the action of a released spring; a gull suggests perpetual motion or the endless sweep of a pendulum; but the view of the great vulture in sailing flight inspires at once the desire for imitation; it is a dirigible parachute which man may hope to re-produce.

THE VULTURE TYPE.

Common name. French.	Common name. British or American.	Scientific name.		Weight of bird.	Surface within contour.	Spread of wings. (b)	Mean width of wing. (a)	Proportion. b/a	One gram of bird's weight sustained by—	One square meter sustains—	Relative surface required to sustain 80 kilos or 176·4 pounds avoirdupois.
			[illegible]	Grams.	Sq. meters.	Meters.	Meters.		Sq. meters.	Grams.	Sq. meters.
Aura		Cathartes aura									
Urubu		Cathartes fœtens									
Percnoptère	Egyptian vulture	Neophron percnopterus	[illegible]	1705	0·413215	1·615 5·30 ft.	0·25	6·46:1	0·000242	4132	19·36
Papa		Sarco ramphos papa									
Gypoierax		Gypohierax									
Gyps occidentalis		Gyps occidentalis									
Vautour fauve	Tawny vulture	Gyps fulvus	[illegible]	7501	1.044576	2·51 8·21 ft.	0·445	5·64:1	0·000139	7180	11·14
Do.				*7240							
Arrian		Vulture monachus									
Oricou	Nubian vulture	Otogyps auricular	[illegible]	8152	1·112950	2·66 8·75 ft.	0·46	5.76:1	0·000136	7323	10.88
Condor		Sarcoramphos gryphus									

* Mean weight of specimens.

THEORY OF THE AEROPLANE.

Vertical and horizontal equilibrium.—To change the equipoise of his aëroplane in the vertical direction, the sailing bird makes use of his tail, which under the action of the imping air serves in all respects as a rudder; but he has a much more energetic means of displaying his center of gravity, which consists in altering his center of figure; that is to say by changing the form of his sustaining surface, and by displacing it in relation to his body.

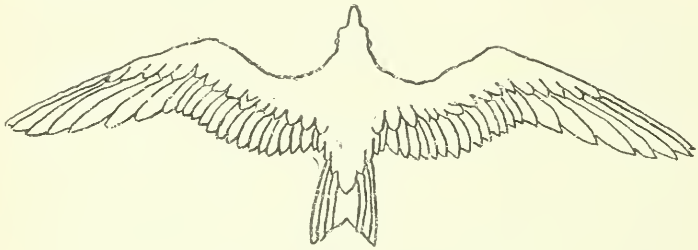

FIG. 12.—Wings in normal attitude.

When the bird has disposed his organs of sailing flight in proper equipoise, when his aëroplane is set for efficient progress, as for example in Fig. 12, should any necessity whatever require him to ascend suddenly, he will not employ his tail for that purpose, especially if it be a feeble one, because it will not produce sufficient action, but he stretches his wings forward, Fig. 13. His center of gravity and his center of figure thus recede decidedly to the rear, and upward gliding and ascension must follow.

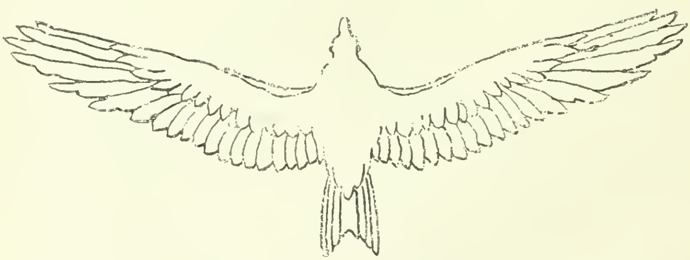

FIG. 13.—Wings projected.

If, on the other hand, the bird assumes the following attitude, Fig. 14, the center of gravity being carried forward produces downward gliding.

These displacements, produced at will by the variable position of the wings and the guidance obtained by the action of the air on the tail, constitute the bird's directing power in a vertical direction.

As to guidance in the horizontal direction, it is very simply brought about. It is also almost always procured through the derangement in the equipoise of the aëroplane, except with birds having very ample tails and thus possessing an organ capable of this service; these use it constantly, as witness the naucler and the kite.

When a bird is sweeping in a circle, the wing pointing towards the center of that circle is always less extended than that which sweeps the circumference, so that when a sailing bird is seen slightly to fold one of his wings, it may be known that he is about to turn towards that side.

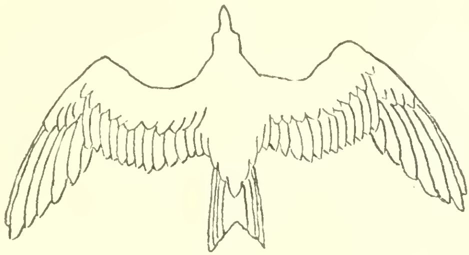

FIG. 11.—Wings retracted

The whole body aids in this movement; the whole bird bears itself to that side, the tail, even when rudimentary and hence feeble in action, concurs in the execution of this maneuver. It is an instinctive action among the feathered tribe, just as with man, when he uses his arms to equilibrate himself on his legs.

The two wings never balance each other perfectly; one side is always heavier than the other, as the surfaces are not equally divided. Differences in weights and surfaces cause a tilt towards the side most heavily loaded or exposing least surface, as the case may be; hence aëroplanes, whether machines or birds, always tend to sweep to one side or the other. To obtain rectilinear progression some corrective force must intervene. In the animated aëroplane this force consists in life; in artificial flying apparatus man will needs produce this force. It might be possible to produce rectilinear progression automatically, in large aëroplanes, by means of electrical apparatus, in which contacts would be made through the use of mercury which would seek its level.

When we observe attentively a sailing bird gliding in a strong and irregular current of wind, we are struck with the rapidity with which the center of gravity is shifted in order to satisfy the needs for support and for maintaining the course. A puff of wind immediately results in a flexing of wings, their tips swing to the rear, the center of gravity advances in consequence, and thus neutralizes the increased pressure produced by the acceleration in the current of air.

This adroit evolution, performed just at the right time, which at first sight seems to be an instinctive action of the creature, is probably after all simply automatic. We may be assured that this change in form of surface, this alteration in the equipoise is not produced by the conscious vital action of the nerves, but is simply a phenomenon of muscular elasticity. The bird receives the shock of the wind unconsciously, his attention is otherwise engaged, his wings are stretched at their usual tension. When they encounter a pressure greater than usual, the tips yield, swing to the rear, and automatically perform the necessary manœuver.

In mechanical aëroplanes it will be indispensable, and very easy, to imitate nature in this act; two springs of calculated strength, maintaining the wings in the position of ordinary equipoise, might very well answer the purpose.

It follows from the facts just stated that it is probable that birds often sail unconsciously. This is the conclusion resulting from attentive observation. Whosoever has closely watched sailing birds will infer that during three-fourths of the time they expend neither force nor will power, that direct action on the part of the creature only occurs when he makes a decision, such as to change his gait or his direction.

This line of thought leads us to fancy that the soaring birds sleep on the wing. Assuredly no bird actually goes dead to sleep during flight, yet those sufficiently gifted to spend six or eight hours in the air for no particular purpose must reach a state nearly approaching slumber, and which must be very restful. This may resemble the slumber of the horse while standing, in which he still retains sufficient control over his muscles to preserve the equipoise on his four legs.

How far will automatic mechanism permit man to progress with his aëroplanes? It is easy to foresee, at first glance, that he need take action only when first starting, upon reaching decisions, and in final alighting; the rest of the time his faculties may be otherwise occupied, and it is quite certain that mere support will be attained without compelling him to intervene at each instant.

MAN-FLIGHT THEORETICALLY CONSIDERED.

Is the reader then to infer that the author has dared to dream of surpassing nature in aërial evolutions? It is certain that before talking of improving upon nature, it would be more becoming to make an attempt to imitate her; not as a lord of creation, but as an humble adept. Yet, as the author has seriously contemplated producing a larger bird than any existing in nature, and as there may be some value in his thinkings, notwithstanding the deficiency of experiments, he will enter upon the question of possible man-flight.

We are led to consider this question by nature herself; she occasionally lifts a corner of the veil through certain evolutions of her favorite children. In point of fact, when we continually observe the sailing birds, when we expend on this study much time, much action, and much thought, we are rewarded once in a while—rarely, it is true—by the sight of some manœuver which sets us to dreaming of its imitation.

We say to ourselves upon observing it: But why does not the bird, instead of fatiguing himself by wheeling, rowing or struggling as he generally does, always employ his present evolution, so economical of force?

The answer is simple. If there be a creature with super-abundant life, it is the bird. With him, movement is not the result of reflection; it proceeds from the great excess of power he possesses, and as he knows no will but his own, he can not resist the desire and follows it to the detriment of his force.

The simple comparison of the different modes of flight is already a step towards success; we have been able to select one mode as most available to man, and as best within the reach of his means of imitation. Let us take another step forward and consider which evolutions, among the numerous maneuvers performed by our chosen type, the great tawny vulture, it is most easy to re-produce, and what is most profitable for our purposes. Then, even while canvassing this selection, if we meet any happy thought, let us analyze it coolly but without shyness; for, by carrying it to an extreme we may perhaps find something new.

Assuredly, when man shall have succeeded in utilizing the wind in flight, he will bring to bear his ingenuity upon that art and it will enable him not only to imitate nature but to surpass her performances. Thus it will not be impracticable for him to produce a sailing apparatus more steady and slow than the condor or the oricou vulture; or perhaps a motor machine possessing greater speed than the teal; this he will do by exaggerating the features of these different modes of flight. But he will excel especially in the profound study of the science of flight. He will not, like the bird, be constantly distracted by his necessities or by fear; every movement will be foreseen and provided for; every danger and contingency will be vanquished in advance, and he will need but to mind his evolutions — a duty which he will fulfill resolutely, with his characteristic science.

As methods of getting about—not to mention the railway, the steamboat, or the balloon—man has invented, out and out, two new modes of locomotion, complete in all their parts and with no analogue in nature: I mean the skate and the velocipede; why then should he not bring to perfection a mode already known, which nothing warrants considering as having reached its utmost limit?

When the first dread has been conquered, when the horror of empty space has been mastered through habitude, intelligent man, after having re-produced all the gaits of the birds, will want to improve upon them. He will inquire whether there are not possibilities beyond them. Then with varied sustaining surfaces he will attempt to rise, advancing into the wind, or he will rise with a stern wind, both evolutions being performed without sweeping in circles; and beyond all this, he will attempt gliding backward.

With the wind dead against him, man will needs study whether it is most advantageous to rise direct, even advancing to windward, as in the case of the eagles already herein described, or to sweep around in circles, thus drifting back and afterwards regaining the lost ground at the expense of height. This last procedure is the one generally employed by the birds, but as we know that they can do better upon occasion, it might be well to experiment. Birds are like all inferior creatures: they do not like to tax their brain with sustained attention, and the circling sweep brings no tax upon their heads, while it enables them to search for food. Inasmuch as man will only desire to get forward over the ground, and possesses greater faculties for combinations than birds, the care in balancing required by direct ascension will be mere sport to him.

With the wind abeam nothing is more simple. All there is to do, so to speak, is to allow ourselves to sail. The sailing birds while doing this wear a happy look; the observer feels that they are laboring neither with body nor with brain, especially if the wind be brisk enough to sustain them well. If the breeze is feeble then they have to take to circling from time to time; but when it is sufficiently strong sailing on a quarter wind is certainly the most convenient, and it is the first mode which will be successfully employed by man. It will be the system causing the least difficulty, and which man will utilize much more than the bird, the former being always anxious to get to his journey's end.

A brisk wind ought to permit of direct ascent, even if blowing from the direction sought; by facing the wind and rising while drifting back, or even by receiving it in the rear of the æroplane, that is to say by gliding with the wind during a lull, and turning an angle and descending slightly during a puff of wind. These two different manœuvers are performed by the sailing birds, but they employ them so rarely that they may be said not to be in their line. With a good wind, when we desire to proceed in its direction, both the ascent and the horizontal progress will be achieved in the latter manner.

Summing up, even admitting that man shall invent no new manœuvers, he will nevertheless have a choice among many, and their combination will constitute what ought to be termed the human type of flight. We may condense our studies into a smaller compass and say: When the aëroplane enters into motion, its center of pressure varies in the direction of that motion and is displaced by an amount which varies with the speed.

With machines "heavier than the air" aërial navigation may be compassed with two separate classes of apparatus: (1) By machines with propellers. (2) By aëroplanes without propellers.

The first class is quite outside of my present design. Mechanical science will eventually furnish quite a number of different solutions; such as flapping wings, propelling screws, rocket propulsion, etc.

The second class—that is to say, the aëroplane without a propeller—it is the object of the present essay to promote. What has been stated herein permits me to affirm that in the flight of the sailing birds (the vultures, the eagles, and other birds which fly without beating the air), ascension is produced by the skillful use of the force of the wind, and that the guidance is the result of skillful maneuvers; so that by a moderate wind a man can, with an aëroplane, unprovided with any motor whatever, rise up into the air and direct himself at will, even against the wind itself. Man therefore can, with a rigid surface and a properly designed apparatus, repeat the maneuvers of ascension and guidance performed by the soaring birds, and will need to expend no muscular force whatever, save for guidance.

As to the exact shape to be given to the aëroplane, it need not be discussed in this chapter, because there are many shapes and devices which may be employed; but all forms of apparatus, however dissimilar, must be based upon this idea, which I repeat: Ascension is the result of the skillful use of the power of the wind, and no other force is required.

It will doubtless be very difficult for many persons to admit that a bird can, with a moderate wind, remain a whole day in the air with no expenditure of force. They will endeavor to suppose some indiscernible pressures or some imperceptible beats. In point of fact the human mind does not readily admit the above affirmation; it is astonished and seeks for all the evasions it can find. All those who have not seen the performance say, when ascension without expenditure of force is mentioned to them, "Oh, well, there were some motions which escaped your observation." It even occurs sometimes that a chance or superficial observer who has had the good fortune to see this maneuver well performed by a bird, when he turns it over in his mind afterwards feels a doubt invading his understanding; the performance seems so astonishing, so paradoxical, that he asks himself whether his eyes did not deceive him.

For observation of this maneuver, in order to carry absolute conviction, must bear upon the performance of the largest vultures, and upon them alone, and this is the reason: it is because all the other birds which ascend into the air by this process do not perform the necessary decomposition of forces required in all its naked simplicity.

If we observe small birds, we see creatures weighing only a few ounces, the martin, the bee-eater, etc., perform this maneuver in high winds and high up in the air. But even when carefully observed with a telescope a doubt remains, in consequence of the enormous power of the martin, who can project himself forward more than a yard with a single beat of wing.

The kites, buzzards, bustards, etc., when they rise, wheeling round, perform such complicated maneuvers as to permit a doubt.

To reach a vigorous, undeniable demonstration, we must discard even the great eagle, whose maneuver is not easily followed by the eye, and we must absolutely confine our observations to the vultures. Mere theorizing would never open up the conception of sailing flight; moreover, we may consider it a dead letter when human life is to be risked in experiment. The moral result of observation is infinitely more convincing, but, alas, it is beyond the reach of most persons. It is not in Paris that the seeker will become convinced, it is not even in Europe, where soaring birds are so rare that months may pass without one being seen.

In short, one must go abroad to enter this new path of investigation; the path through which I have reached absolute conviction, and which must be followed by all who desire to know what can be done. If they will thus observe the soaring birds in their own habitat, they will doubtless witness all the performances which I have described, and probably still others which have escaped me.

But to be convinced a man must see; for to see, even only once, is better than a whole volume of explanations. Therefore, O reader, if you are interested in this subject, go and see for yourself and be edified. Go to the regions where dwell the birds which perform these demonstrations; and when you have beheld them for a few instants, being already initiated as to what to observe, comprehension will at once come into your understanding.

* * * * * * *

Imperfect machines.—It is somewhat unfortunate that I have not sufficient space left for a little treatise upon "paper arrows." This school boy's toy, simple as it may seem, is quite instructive when its principles are studied. The arrow may be constructed in various forms, from the acute triangle, which is the type of speed, to the broadside rectangle, the aëroplane type, proportioned like a stormy petrel, in which the plane is narrowest in the direction of its motion.

Moreover, we note that nature has not constructed all sailing birds upon the same model. If we compare the aspect of the great tawny vulture with that of the stormy petrel, who sails wonderfully in a high wind; or with the aspect of the tern, or the gannet, or the frigate-bird, when the latter assume their arrow-like forms, we shall perceive that there is a great diversity of models; we might even say there is an antagonism in models, for we have noted that all of them are perfect in their flight as considered in relation to their life needs. But, notwithstanding these diversities, the gliding flight of each creature, whether supported on elongated or on square wings, is always based upon the same general principle; it results from the possibility of shifting the center of gravity by a change in the position of the sustaining surfaces, and this confers the faculty of maintaining equipoise in the air.

Aëroplanes, provided with the necessary sustaining surfaces, and equipped with this faculty, will be sufficient to reproduce the sailing evolutions of the birds. We may now conclude, therefore, that a particular, special shape is really not indispensable for aërial locomotion; all sorts of forms, even the most curious, may be utilized; only, they will produce the required decompositions of forces, under the action of the wind, in the ratio of their individual perfections.

Man may succeed in gliding on the wind with circular, triangular, or rectangular forms, with aërial rafts in the shape of an arrow, with irregular forms even, provided always that he can shift the center of gravity as required: provided also, that the sustaining surface be sufficient in extent, and that the speed of the wind, or the speed of the aeroplane, shall be about 22 miles per hour.

The problem, thus broadly stated, leads to some curious consequences. Eventually, when success is achieved, we shall perhaps be quite surprised to see some second-hand apparatus circulating in the air: some aeroplanes full of holes and rents, patched up, damaged and mended, holding together by the grace of Providence, and yet gliding along after a fashion. These will not be the best to resist the vicissitudes of the wind, but they will get along just the same. Now, what is the proof of this? I have conferred liberty upon kites and upon Egyptian vultures whose flying surfaces were in deplorable condition; some with wings almost plucked to bare poles, some with a wing and a half only (this lack of counterpoise in their sustaining surfaces troubles them greatly). I remember a particular pelican who glided upon an incredible pair of wings. He had lost six or seven primary feathers at least, and the rest of his plumage was far from complete. Yet, when the wind blew fresh, he launched out from sloping ground and sometimes succeeded in getting under way. Once fairly up in air, he became most surprising. Gliding upon his ragged wings he would skim within a yard of the observer, his neck bent back, his head resting upon his shoulders with an air of supreme impertinence. He would go out for a tour over the sea, would come back to inspect the market, and complete his perigrinations by settling down on the waves. A most curious thing it was to see this creature, which was quite tame, pass close at hand, very swiftly, near the spectators. He produced a strange sensation by gliding by with ease and no exertion. It was a foretaste of the pleasures of aerial speed—a sort of class-room gliding, in which the bird-professor was teaching the beholders the art of sailing flight.

After all these digressions, the main question which comes up is the following: What is the least surface required to sustain a man and apparatus, weighing 176 pounds?

The exact answer must be ascertained by experiment: but we may even now say that it probably will astonish by its mediocrity. My own idea is that 82 square feet will suffice, as a minimum, to sustain 176 pounds in sailing flight.

* * * * * * *

Speculations as to results—After having discussed the benefits to be derived from the conquest of the air, let us now consider the perturbations which it may cause. Let us see whether there is not some blot on the other side of the shield; for so important an achievement as this new mode of locomotion can not take place without producing disturbances.

Let us admit that the problem is solved, and let us speculate upon the effects upon society. Let us begin with property. Property will be riven with an enormous gap. With the patent insufficiency of inclosure, with intrusion into the privacy of home, hedges, walls, will no longer be of service: the inclosure under the roof will be incomplete and will need emendation. All this will constitute a curtailment of the privileges of possession, for a little consideration evidences the diminished efficiency of barriers. We shall no longer be at home as heretofore; there is no need to dwell on this, it is easily grasped.

But what of the collectors of customs and the police in the presence of this new mode of locomotion? They often fail to control existing ways of communication which nevertheless are upon well-defined lines, where all must pass and are easily inspected. What will these officers do when they must watch the air, that immense pathway some 4 or 5 miles high? During the day it may be possible to fancy some partly satisfactory surveillance: with a large force, good telescopes, fast cruisers of the air, we might perhaps exercise some control, but at night, what is to be done? How can we bar the empire of air? How can we so much as watch it when opaque fog annihilates the effects of electric reflectors? Smugglers will certainly have such facilities for plying their industry, that the only thing to do will be to suppress the custom-house entirely.

But then what will become of the revenues and the balance of the budget? These perturbations to property, to the customs, to the police, are mere bagatelles when compared to the perturbations which will result in political matters. After all there may be found in time means more or less sufficient to supervise the transportation of goods; men will become accustomed to the new limitation of privacy; but as to political matters we shall find ourselves in the presence of such facilities for confusion that the like has not been seen since the tower of Babel.

What will become of the army, this new invention being successful? All will have to be done over again; the fortifications, the maneuvers, the defenses of the frontiers, strategy, all is brought to naught. It will even cause, in a very short time, the suppression of nationalities: races will be rapidly commingled or destroyed, for there will no longer be efficient barriers, not even those movable barriers which we term armies. No more frontiers! No more insular seclusion! No more fortresses! Whither are we drifting?

It must be confessed that we are face to face with the great unknown. What will be the result? Will society perish? Assuredly no!

As to the procedure that society will adopt to conform to this new mode of existence I have not the least idea, but it may be affirmed that society will emerge victorious from the struggle; that after the tempest caused by injured interests a period of restored equilibrium will follow; and that in the end at the cost of a time of distress, humanity will enter into possession of the empire of the air.

Thus we may recover our equanimity and calmly consider the possibility of success. We may proceed toward that pharos, that beacon, which is the immeasurable law of nature and which we call progress; for human progress is synonymous with welfare.

Finally, I counsel the greatest possible prudence to all who undertake to solve the problem of sailing flight. Let them carefully canvass all the causes of accidents which it is possible to foresee; but once they have made this canvass, once they have completed their researches, I recommend them to act with energy and will, and I know of no better word to say to them than the one with which I began this monograph: "Osez"—daring wins.

PROGRESS OF ANTHROPOLOGY IN 1892.

By Prof. Otis T. Mason.

Anthropology has busied itself with the multiplication of societies, journals, congresses and other means of co-operative work. The benefit of this is seen in many ways; it prevents duplication; it puts material where it should be looked for; but, chief of all, it enables men to undertake enterprises that are entirely beyond the capacity and the resources of individuals. The increasing favor of the science is observed in the fact that most of the leading governments have at great expense organized explorations and studies. "The year 1892," said Prof. Macalister before Section H of the British Association, "has not been futile in discoveries bearing on those great questions that are of popular interest." Indeed, there has been a growth of wholesome doubt on questions concerning which men's minds were thought to be settled. This will be seen most apparent in the archaeological area, especially in America. The examination of ancient corner stones and foundations, the clearing away of encumbering materials, are preparatory to the strengthening of the structure at every point.

The American Association for the Advancement of Science was held in Rochester, N. Y. As usual, the science of anthropology received a larger amount of attention, even outside section H. This fact is noticeable especially in the large number of papers devoted to domesticated animals and plants.

The address of Vice-President Holmes had for its topic "the evolution of the æsthetic." The following papers were read:

Proposed classification and international nomenclature of anthropologic sciences. D. G. Brinton.

Tusayan legends of the Snake and Flute people. Matilda C. Stevenson.

Primitive number systems. L. L. Conant.

The Peabody Museum Honduras expedition. F. W. Putnam.

Exploration of the main structure of Copan, Honduras, M. H. Saville.

Vandalism among the antiquities of Yucatan and Central America, *id.*

Aboriginal quarries of flakable stone and their bearings upon the question of palaeolithic man. W. H. Holmes.

Sacred pipestone quarries of Minnesota and ancient copper mines of Lake Superior, *id.*

On the so-called palaeolithic implements of the upper Mississippi. *id.*

Brief remarks upon the alphabet of Landa. H. T. Cresson.

Comparative chronology, W. J. McGee.

The early religions of the Iroquois, W. M. Beauchamp.

Early Indian forts in New York, *id.*

Prehistoric earthworks in Henry County, Ind., T. B. Redding.

Prehistoric objects from the Whitewater Valley, Amos W. Butler.

Indian camping sites near Brookville, Ind., *id.*

Earthworks near Anderson, Ind., *id.*

Pebbles chipped by modern Indians as an aid to the study of the Trenton gravel implements, H. C. Mercer.

Ancient earthworks in Ontario, C. A. Hirschfelder.

Prehistoric trade in Ontario, *id.*

Fort Ancient, Ohio, S. S. Scoville.

Copper implements and ornaments from the Hopewell group, Ross County, Ohio; W. K. Moorehead.

The ruins of southern Utah, *id.*

Demonstration of a recently discovered cerebral porta.

Pueblo myths and ceremonial dances, F. H. Cushing.

Ancient hearth in stratified gravels on Whitewater River, Indiana, A. W. Butler.

Skull of a pig having an arrowhead imbedded in the bone, E. W. Claypole.

Ruins of Tiahuanaco, A. E. Douglas.

Involuntary movements, Joseph Jastrow.

Pottery from a mound in Peoria, Ill., J. Kost.

A definition of anthropology, O. T. Mason.

The Department of Anthropology at the World's Columbian Exposition, F. W. Putnam.

Model of serpent mound, Ohio, *id.*

The address before Section I by its vice-president, Lester F. Ward, should not be overlooked in this connection. The subject is, "The psychologic basis of social economics." The active co-operation of Section H in anthropology at the World's Fair was secured, and the association was adjourned to Madison, Wis., so as to be near the city of Chicago. Plans were laid to have the Association and the Congress of Anthropology continuous.

At the British Association for the Advancement of Science, held in Edinburgh, August, 1892, the following committees reported work done along the lines of American anthropology:

Report of the committee appointed for the purpose of editing a new edition of "Anthropological Notes and Queries."

Report of the committee for investigating the ruins of Mashonaland and the habits and customs of the inhabitants.

Report of the committee appointed to report on the pre-historic and ancient remains of Glamorganshire.

Eighth report of the committee appointed to investigate the physical characters, languages, and industrial and social condition of the Northwestern Tribes of the Dominion of Canada.

Remarks on linguistic ethnology, introductory to the report on the Kootenay Indians of Southeastern British Columbia.

Report on the Kootenay Indians of Southeastern British Columbia.

Report of the committee appointed to investigate the habits, customs, physical characteristics, and religions of the natives of India.

Report of the committee for the purpose of carrying on the work of the anthropometric laboratory.

The address before Section H—Anthropology—was delivered by President Alexander Macalister, M. D., F. R. S., professor of anatomy in the University of Cambridge.

The following papers were read:

(1) On the organization of local anthropological research, by E. W. Brabrook.

(2) Discovery of the common occurrence of palæolithic weapons in Scotland, by Rev. Frederick Smith.

(3) Notes on cyclopean architecture in the South Pacific Islands, by R. A. Sterndale.

(4) On a fronto-limbic formation of the human cerebrum, by Dr. L. Manouvrier, professor at the School of Anthropology, Paris.

(5) The Indo-Europeans' conception of a future life and its bearing upon their religions, by Prof. G. Hartwell Jones, M. A.

(6) Exhibition of photographs, weapons, etc., of the Toba Indians of the Gran Chaco, by J. Graham Kerr.

(7) Exhibition of pre-paleolithic flints, by J. Montgomerie Bell.

(8) The present inhabitants of Mashonaland and their origin, by J. Theodore Bent.

(9) On the value of art in ethnology, by Prof. A. C. Haddon.

(10) Similarity of certain ancient necropoleis in the Pyrenees and in North Britain, by Dr. Phené, F. S. A.

(11) A contribution to the ethnology of Jersey, by Andrew Dunlop, M. D., F. G. S.

(12) On the past and present condition of the natives of the Friendly Islands, or Tonga, by R. B. Leefe.

(13) Damma Island and its natives, by P. W. Bassett-Smith, surgeon R. N., F. R. M. S.

(A discussion on anthropometric identification was opened by Dr. L. Manouvrier, of Paris.)

(14) Some developmental and evolutional aspects of criminal anthropology, by T. S. Clouston, M. D., F. R. S. E.

(15) On a coiffure from the South Seas, by Sir W. Turner.

(16) On the articular processes of the vertebræ in the gorilla compared with those in man, and on costo-vertebral variation in the gorilla, by Prof. Struthers, M. D., LL. D.

(17) On the probable derivation of some characteristic sounds in certain languages from cries or noises made by animals, by J. Mansel Weale.

(18) On the prehensile power of infants, by Dr. Louis Robinson.

(19) The integumentary grooves on the palm of the hand and sole of the foot of man and the anthropoid apes, by David Hepburn, M. D., C. M., F. R. S. E., senior demonstrator of anatomy, University of Edinburgh.

(20) On the contemporaneity of man and the moa, by H. O. Forbes.

(21) A discussion on human osteometry was opened by Dr. J. G. Garson.

(22) Exhibition of composite photographs of United States soldiers, by Dr. J. G. Garson.

(23) Observations as to physical deviations from the normal as seen among 50,000 children, by Francis Warner, M. D.

(24) On the brain of the Australian, by Prof. A. Macalister.

(25) On skulls from Mobanga, Upper Congo, by Prof. A. Macalister.

(26) On some facial characters of the ancient Egyptians, by Prof. A. Macalister.

(27) On some very ancient skeletons from Medum, Egypt, by J. G. Garson, M. D.

(28) On a skull from Port Talbot, Glamorganshire, by C. Phillips, B. A.

(29) On trepanning the human skull in prehistoric times, by Robert Munro, M. A., M. D.

(30) On the use of narcotics by the Nicobar Islanders, and certain deformations connected therewith, by E. H. Man.

(31) Exhibition of the philograph—a simple apparatus for the preparation of lecture diagrams, by G. W. Bloxam, M. A.

(32) Exhibition of photographs representing the prehensile power of infants, by L. Robinson, M. D.

The strong point for anthropology in the British Association is its eminent committees, which have guided exploration in many directions. In the French Association for the Advancement of Science, held at Pau under the presidency of Dr. Magitot, September 15–21, the following papers on the program are of interest to anthropologists in general:

Affinities between the Basque language and certain idioms of the two continents Chareney, Vinson, Manouvrier, Azema, Guillibeau, Guido Cora, and Dodgson; Les Tziganes, Guido Cora; archæology of the Pyrenees, Cartailhac; depopulation of France, Chervin; prehistoric finds in the valley of the Vézère, Girod et Masserrat; anthropology and the archæology of the Pyrenees, a discussion, proposed by M. Piette; Le Tonkin, Barbier. The question of the Basques, their anthropological characters, their history, their language, their traditions, and folklore consumed the bulk of the time.

The twenty-third annual session of the German Anthropological Society was held in Ulm, August 1–3. The following important matters were discussed:

Ein Bild aus Schwabens Vorzeit, E. von Tröltsch.

Wissenschaftlicher Jahresbericht, J. Ranke.

Die Schädel von Cannstadt und Neanderthal, v. Hölder.

Die anthropologische Stellung der Juden, F. von Luschan.

Die Menschenrassen Europas und die Frage nach der Herkunft der Arier, J. Kollmann.

Anthropologisches aus Malacca, R. Virchow.

The German Anthropological Society devotes all its time to this one subject. In their national congress of naturalists and physicians, topics relating to man are also discussed by German Anthropologists.

At the eleventh session of the congrès internationaux d'archéologie préhistorique et d'anthropologie, convened at Moscow, the following papers were read:

What is the most ancient race of central Russia? Anatole Bogdanov.

The races of men in Europe and the Aryan question. Dr. Kollmann.

The anthropometric types of great Russians in the central governments of Russia. Zograf.

New classification of human crania. Prof. Sergi.

On ancient skulls in Russia artificially deformed. Dr. Anoutchine.

Review of the anthropometry of peoples of Transcaucasia. Ernest Chantre.

Race in anthropology. Paul Topinard.

Proposal for a reformed nomenclature of the peoples of Asia. Ernest Chantre.

Anthropometric methods practiced in Russia. Zograf.

Three commissioners were appointed during the congress, upon craneometry, on anthropometry, and on the nomenclature of the peoples of Asia.

The first named under the chairmanship of Virchow, reported at the meeting, as follows:

I. *Norma or orientation of the skulls.* Each one is free to take the one which he prefers. The *norma horizontalis* or *auriculo orbitaire* is recommended for drawings and for photographs.

II. *Great diameters.*—The maximum length and the maximum transvere width according to the French method are adopted to the exclusion of other analogous diameters. Whenever these last are employed they must be announced.

III. *Frontal diameters.*—To the minimum frontal width, adopted only in Germany, is added the maximum width, which ought to be measured on the Stephanic point, of Broca.

IV. *Total height of the skull.*—This measure should be preserved, but it ought to be taken or it will fall into disuse.

The committee prefer for this purpose the compass of Virchow. If this instrument is not adopted the legs of Broca's sliding compass must be lengthened. The utility of this modification is perceived in mensurations on the living. It is only with a compass with long branches that the total height of the skull can be taken through the auricular points.

V. *The curves.*—The curves must be taken with a steel metric ribbon. The horizontal should pass around the supraciliary arches and the most salient points. The transverse by the auditory openings and the bregma.

VI. *The face.*—The width ought to be taken no longer on the jugomaxillary sutures, but upon the two points that give the maximum width. The height of the nasion ought to be taken at the upper alveolar point. The total height of the nasion on the mentonal points.

VII. *The orbits.*—The diameters of the orbits ought to be measured on the internal borders. For the width the dacrion should be abandoned.

VIII. The ophrio-naso-alveolar angle ought to be taken with the facial goniometer of Ranke or with that of Broca. In this, as in all measures, the instruments and the methods should be stated.

In his paper before the tenth congress of archæology and anthropology, Ernest Chantre made a report on the measurements of the peoples of the Caucasus, of which the following is the abstract:

(1) Armenians, brown, brachycephalous, mesoprosopic, leptorrhine, and above the medium in stature.

(2) Aderbeïjanis, brown, dolichocephalous, dolichoprosopic, leptorrhine, and above the medium stature.

(3) Kurds, generally brown, with elongated faces, eyes never bridged, dolichocephalous, leptorrhine, and above the medium stature.

(4) Aïssori, brown, ultra-brachycephalous. There is also to be remarked among them mesoprosopism, leptorrhinism, and a stature below the mean.

(5) Tadjiks, very brown, mesoprosopic, leptorrhine, dolichocephalic, tall.

(6) Hadjemi Persians, very brown also, leptorrhine, dolichocephalic, dolichoprosopic, and of medium stature.

(7) Jews, medium color, ultra-brachycephalic. They are distinguished by their mesoprosopism, their leptorrhinism, and medium stature.

(8) The Afghans are brown, brachycephalic, mesoprosopic, leptorrhine, and tall in stature.

(9) The Kalmucks are brown, mesorrhine. The eyes are bridged, the face wide. They are brachycephalic and of stature above the mean.

(10) The Lesghians are chestnut in color, ultra-brachycephalic, mesoprosopic, leptorrhine, and very tall.

This is by far the most important assemblage of anthropologists in Europe. Through their increasingly closer co-operation it is hoped to unify methods of research that reports from one country may be taken up and utilized in another. This in some lines has been hitherto impracticable.

At the Australian Association for the Advancement of Science, held January 7 to 14, the president of the section of anthropology was the Rev. Lorimer Fison. The following is a list of subjects and authors:

The story of Tie and Rie, Hervey Is., Dr. Gill.
The omens of pregnancy, Mangaia, Dr. Gill.
New Britain and its people, B. Danks.
Sydney natives fifty years ago, W. B. Clarke.
Group marriage and relationship, L. Fison.
Nair polyandry and Dieri Pirauru, L. Fison.
Samoa and Loyalty islands, S. Ella.
Cave paintings of Australia, J. Matthews.
New Hebrides, D. Macdonald.
Notes on the Taunese, W. Gray.

At the eighth annual meeting of the Indiana Academy of Science, held in Indianapolis, December 28 and 29, the following papers of anthropologic interest were read:

Evidences of man's early existence in Indiana, from the oldest river gravels along the White Water River, by A. W. Butler.
The Crawford mound, by H. M. Stoops.
Notes on archaeology in Mexico, by J. T. Scovell.
Ancient earthworks near Anderson, Ind., by F. A. Walker.
Archaeology near Tippecanoe County, by O. J. Craig.
Some Indian camping sites near Brookville, by A. W. Butler.
Remarkable pre-historic relic, by E. Pleas.
The mounds of Brookville Township, Franklin County, Ind. by H. M. Stoops.
Remarks on archaeological map making, by A. W. Butler.

The preparation for the World's Columbian Exposition occupied the time of most of the American anthropologists in 1892. A classification of the material was first made upon a purely anthropological basis, and in its completed form made full provision in Department M for this subject under the topics: Ethnology, Archaeology, Progress of Labor and Invention.

The exhibit was bound by the law creating the Exposition to be double—the Government portion and the Exposition portion or department.

In order to avoid all conflicts it was arranged that the first-named display should set forth the resources and methods of the Government in the prosecution of anthropological work. The completion of the great linguistic map furnished the key-note, and all the national exhibits were set up around the ideas there set forth.

The area covered by the Department M was of a much wider scope. Somatic and functional anthropology were to have the widest range, and tribes of living peoples were to encamp on the grounds to give emphasis to the exhibits. A separate building was provided for, in which the phases of the subject should be separately treated and the different countries might make their displays. The following is the scheme of the display:

GROUP 159.

VIEWS, PLANS, OR MODELS, OF PRE-HISTORIC ARCHITECTURAL MONUMENTS AND HABITATIONS.

Class 939.—Caves, natural, artificial; dwellings, natural, artificial.

Class 940.—Lacustrine dwellings, dolmens, tumuli, menhirs, cromlechs, alignments, cupstones, graves, cists, crematories.

Class 941.—Cliff and other dwellings, models of dwellings, shelters, skin lodges, yourts, huts (of bark, grass, etc.), wooden houses.

Class 942.—Appurtenances. Sweat houses (models), totem posts, gable ornaments, locks.

GROUP 160.—Furniture and clothing of aboriginal, uncivilized, and but partly civilized races.

Class 943.—Household utensils and furniture.

Class 944.—Articles serving in use of narcotics.

Class 945.—Articles used in transportation.

Class 946.—Clothing and adornment.

GROUP 161.—Implements of war and the chase.

GROUP 162.—Tools and implements of industrial operations.

Class 947.—Gathering and storing food other than game. Water vessels.

Class 948.—Articles used in cooking and eating.

Class 949.—Apparatus for making clothing and ornaments and of weaving.

GROUP 163.—Athletic exercises. Games.

GROUP 164.—Objects of spiritual significance and veneration.

GROUP 165.—Historic archæology.

GROUP 166.—Models of ancient vessels.

GROUP 167.—Re-productions of ancient maps.

GROUP 168.—Ancient buildings, cities, and monuments of the period anterior to the Discovery.

GROUP 169.—Habitations, etc., built since the Discovery.

GROUP 170.—Originals, copies, or models of notable inventions.

GROUP 171.—Amelioration of life and labor.

GROUP 172.—Woman's work.

GROUP 173.—State, national, and foreign government exhibits.

GROUP 174.—The North American Indians.

GROUP 175.—Portraits, busts, and statues of great inventors and benefactors.

GROUP 176.—Isolated and collective exhibits.

By act approved May 2, 1892, the Congress of the United States authorized a representation in the Exposition of Madrid to commemorate the quadrocentennial of the discovery of America. The various Departments and the National Museum were authorized to participate. In addition to this Government display, the Hemenway Expedition, the Peabody Museum, the University of Pennsylvania, the Academy of Natural Sciences of Philadelphia took part in the exhibits from the United States. The South American republics were well represented, as well as Mexico and Central America. The Exposition, lasting six months, was held in the new museum and library building in Madrid. It afforded the rarest opportunity of bringing together a great variety of art products from the two Americas.

A great deal of the material mounted in Washington for the World's Fair in Chicago was exhibited in Madrid, adding to the interest of the exhibit. The catalogue was prepared by Mr. Walter Hough, of the U. S. National Museum, and an account given by the same author in the *American Anthropologist* for July, 1893, 271–277.

Dr. Brinton assumed control of the current notes on anthropology in *Science* (New York), enabling the reader to profit at small expense by a vast amount of research, especially into European literature inaccessible to most. The method pursued is to devote short paragraphs to the comprehensive statement of the author's aim and a short analysis of the work.

An extensive catalogue of anthropological literature is to be found in each volume of Archiv für Anthropologie, classified as follows:

I. Pre-history and Archaeology: I. Germany; II. Austria; III. Switzerland; IV. Great Britain; V. Denmark; VI. Sweden; VII. Norway; VIII. France; IX. Belgium; X. Italy; XI. America.

II. Anatomy: I. 1888; II. 1889; III. 1890.

III. Völkerkunde (1890): I. Sources; II. Ethnology (1. Methods, history of the science; 2. General anthropology; 3. Influence of climate and environment; 4. General sociology; 5. Special sociology).

III. Ethnography: I. General ethnography; II. Special ethnography (A. Europe, with 15 subdivisions; B. Asia, with 13 divisions, each with several subdivisions; C. Australia, with 4 divisions; D. Africa, with 9 divisions; E. America, with 4 divisions).

IV. Zoology: Account of zoological literature in connection with anthropology for the year 1890. (A. Mammals and human remains from the diluvium and prehistoric times; B. Mammals from the diluvium, with no near association with man; C. Mammals from the Tertiary and Mesozoic times; D. Recent mammals, both systematic study and distribution.)

There are many things to be said in favor of the classified bibliography, but the tendency nowadays is to a single alphabet. The title collection of the Archiv is excellently done, and frequently a brief review accompanies of great value. The only drawback to the handy use of such a bibliography is the impracticability of carrying so long an analysis in the memory. The list is especially full by reason of its including only works that are two years behind the date of the Archiv.

I. BIOLOGICAL ANTHROPOLOGY.

Dr. Friedrich Ratzel's Anthropogeographie at the close of 1891 reached the end of its second volume. In the first volume the physiographical and the climatological differences were discussed as conditioning the varied forms of settlement and civilization and the endless varieties of mankind.

The second volume is devoted to bio-geography, including a graphic picture of human distribution, a sketch of the peopling of the earth as a whole (*the œkumene* of the Greeks) and the effect of position in this *œkumene.* In the second part of this volume some important matters are taken up, namely, the significance of the density and the distribution of populations, the want of progress in some peoples, their extinction when brought into contact with higher culture, and their self-annihilation. The earth as modified by human action is an old theme, but with the new light of modern science the books of Guyot and Ritter and Marsh may be re-written. The author of this series has qualified himself for this task by a series of lectures, the repetition of which has made him quite familiar with all phases of the subject.

Anthropometry.—Dr. R. Collignon, of Cherbourg, France, issued a Projet d'Entente Internationale pour arrêter un Programme Commun de Recherches Anthropologiques. The object of this projet is to bring about uniformity everywhere in the matter of bodily measurements. In reading up the action of the several national associations and international congresses the reader will see that the old struggle for agreement concerning common measures and method goes on. The conviction is continually strengthened that no good results can precede such agreement.

M. Etienne Rollet published in Revue Scientifique in August (vol. 50, p. 170–175) a table of coefficients for deducing stature from the measurement of the long bones.

	Femur.	Tibia.	Fibula.	Humerus.	Radius	Ulna
Minimum........	3.66	4.53	4.58	5.06	6.86	6.41
Maximum	3.71	4.61	4.66	5.22	7.16	6.66

Multiply the length of the long bone named by the coefficient in the table to obtain the stature. The worth of the publication is greatly enhanced by a multitude of references to authorities.

In his work entitled L'Homme dans la Nature (Paris, 1891, Ballière), Paul Topinard makes the following résumé of his studies:

First Sub-order—	Man.
Second Sub-order, The Monkeys.	First family, Anthropoids. Second family, Pithecidæ. Third family, Cebidæ. Fourth family, Arctopithecidæ.
Third Sub-order—	The Lemurs.

[*Nature*, Lond., Mar. 17, 1892.

In comparing woman's brain with man's, Prof. Crichton Browne confirms the inferiority of the former, amounting to thirty grammes, correction made of the coefficient of stature. He has proved that the frontal lobes are not so well irrigated by the blood, and that, on the contrary, the circulation of blood is more active in the posterior and superior portions. The posterior parts of the encephalon, cervelet, and occipital lobes are more developed in women, and that their left brain weighs less than their right brain. The convolutions are less complicated than in men. The caliber of the internal and the vertebral carotid present marked differences in the two sexes. Whence it results that the distribution of blood in the brains of the two sexes differ greatly. The internal caroted with its principal branches (cerebral, anterior, and intermediate), which are distributed among the suborbital convolutions of the insula, of the Rolandic region, and of the first sphenoidal convolutions, are larger, absolutely and relatively, in men than in women. On the contrary, the vertebral carotid, which is distributed among the occipital and temporo-sphenoidal lobes, are larger in women than in men, and the basilar trunk, which is only a continuation of the vertebral, is also larger, its mean diameter being 28^{mm} in woman and 26^{mm} in man.

II. PSYCHOLOGY.

Prof. Ward, in his vice-presidential address before Section I of the American Association, says that the doctrines of physiocracy *laissez fairé* and Spencerian individualism and the biologic economy generally are not sustained, and that the facts which society presents are for the most part the reverse of those which were promised by them. The explanation is that the old political economy is true only of irrational animals and is altogether inapplicable to rational man. Darwin modestly confesses that he derived his original conceptions of natural selection from the reading of Malthus on Population. But he did not, perhaps, perceive that in applying the law of Malthus to the animal world he was introducing it into the only field in which it holds true. Yet such is the case, and for the reason that the advent with man of the thinking, knowing, foreseeing, calculating, designing, inventing, and constructing faculty, which is wanting in lower creatures, repealed the biologic law or law of nature and enacted in its stead the phychologic law, the law of mind.

In the *American Journal of Psychology* (1892, IV, 491–502) communications are made to the editor of courses in experimental psychology as follows: In London the present examiners in mental science are Dr. James Sully and Prof. Knight. In University College (Gower street) Prof. Croom Robertson conducts the instruction. King's College, Bedford College, and the City of London College affiliated with the University provide teaching in psychology. But there is no laboratory in any of them for experimental psychology and research, indeed the only one in England is at the University of Cambridge.

In Copenhagen there is at the university a psychological laboratory under the direction of Dr. Lehman. The instruction in philosophy is under the direction of Prof. Harold Höffding.

In 1891, a chair of experimental psychology was created in the faculty of sciences of the University of Geneva, but without a laboratory. Wladimir v. Tschisch presents a brief report on the clinic for nervous and mental diseases in Dorpat.

Yale University has provided a course of study in experimental philosophy with reference to the degree of Doctor of Philosophy.

Three courses of psychological instruction were pursued in Harvard.

A department of psychology was opened in Cornell University in connection with the Susan Linn Sage School of Philosophy.

In the German universities the following lectures were reported:

Leipzig.—Wundt, special investigations and exercises in the psychological laboratory; Kulpe, introductory course; Glöckner, pedagogical psychology; Flechsig, psychiatrical clinic, forensic psychiatry.

Berlin.—Dilthey, lectures on psychology and pedagogy; Lazarus, lectures on psychology; Ebbinghaus, lectures and experimental psychology; Jolly, pathology and therapeutics of mental diseases.

Bonn.—Elements of psychology; Pelman, mental disturbance that borders on insanity; Kochs, hypnotism, sleep, and the narcotic condition.

Göttingen.—G. E. Müller, lectures and experimental psychological investigations; Meyers, psychiatric clinic.

Heidelberg.—Kraepelin, physiological psychology and psychiatrical clinic.

Dr. William O. Krohn spent nine months working in the celebrated university centers of Europe, Heidelberg, Strasburg, Zurich, Freiberg, Munich, Prag, Berlin, Halle, Göttingen, and Bonn. In each of these the laboratories were carefully inspected and in some of them the doctor carried on experimental work. (See *Am. J. Psychol.*, IV, 585–594.)

The Institute Psycho-Physiologique de Paris was founded in 1891 for the theoretical and practical study of the psychological and therapeutical applications of hypnotism.

The Société d'Hypnologie of Paris held monthly meetings.

Prof. E. W. Scripture proposes in the psychological notes of the American Journal of Psychology (IV, 584) a list of terms with definitions for psychological use, according to the meanings attached to them:

(1) Feelings are the indivisible elements into which mental phenomena are composed. Every fact of consciousness that has not been proved to be a combination of other facts is to be called a feeling.

(2) Sensations are those feelings which are regarded as coming from without; they are passively experienced feelings.

(3) Impulses are those feelings that are regarded as originated in the mind itself; they are actively experienced feelings.

(4) Ideas are compounds of feelings of any kind.

(5) Percepts are those ideas that are composed mainly of sensations.

(6) Volitions are those ideas that are composed mainly of impulses.

The American branch of the Society for Physical Research was held in Columbia College, New York, February 10. Prof. James gave a communication on the census of hallucinations, and B. F. Underwood one on experiments in automatic writing. M. Binet contends that associated with the same physical individual there may be two or more personalities, both of which are conscious. They may be co-existent or successive. Anaesthesia is the barrier which separates co-existent personalities; amnesia the barrier which separates successive personalities. "En un mot, il peut y avoir chez un meme individu, pluralité de mémoires, pluralité de consciences, pluralité de personalités; et chacune de ces consciences, de ces personalités ne connait que ce qui se passe sur son territoiré. (*Nature*, Lond., July 7.)

In *La Revue Scientifique* (XLIX, 797) M. Lacassagne, director of the faculty of medicine in Lyon, publishes a questionnaire on physiological psychology. The object is to stimulate statistical researches on the relations between the sensorial apparatus, the quality of memory, and the mode of functioning of the centers of language and of ideation. Mm. H. Beaunis and A. Binet follow up this subject in the succeeding volume (L, 340–343) with a questionnaire addressed to painters, sculptors, and designers relative to a visual memory of color and form, the chief points of the inquiry being the distinctness of visual recollections, the qualities of visual memory, distinction between form memory and color memory, fidelity of this characteristic, the role of visual memory in the art of design, peculiarities. Dr. Riccardi's '*Anthropologia e Pedagogia* is a study in the science of education founded on a basis of experimental psychology and anthropology. He has collected during the last seven or eight years, with the help of teachers, some hundred thousand observations on two thousand children of Modena and Bologna, and in this first part of the work he presents the data concerning this psychological and sociological condition. He divides the pupils into good, middling, and bad, and investigates the characters of these classes with reference to family life, number in a family, healthiness of the family stock, social position, etc., in each case first taking the sexes together and then considering boys and girls separately. Italian children, to a large extent, live under bad conditions and are decidedly below the anthropometric standards of other nations. There is a marked contrast between the children of the poor and of the well-to-do classes, to the advantage of the latter. [*Rev. in J. Anthrop. Inst.*, XXII, 281.]

The second International Congress of Experimental Psychology convened in London on Tuesday, August 2.

The third Congress of Criminal Anthropology was held in Brussels from the 20th of August to the 3d of September.

A laboratory was established in the University of Toronto.

Prof. Angell occupied the chair of psychology at the Stanford University.

Dr. Edward Pace, a pupil of Wundt, organized a laboratory in the Catholic University in Washington.

Dr. Edmund Delabarre organized the study of experimental psychology in Brown University.

The following is the program of the International Congress of Experimental Psychology held in London, August 1:

Introspection and experiment in psychology, Alex. Bain.
Suggestion and will, M. Baldwin.
Psychological questioning, Prof. Beaunis.
Hypnotic suggestion and education, Prof. Bernheim.
Psychology of insects, M. Binet.

Appreciation of time by somnambulists, M. Delboeuf.
Laura Bridgman, Dr. Donaldson.
Psycho-therapeutics, Dr. Van Eeden.
Theory of color perception, Prof. Ebbinghaus.
Muscular sense of the blind, Dr. Goldscheider.
Psychology of the skin, Stanley Hall.
The visual center in the cortex of the calcarine tissue, Prof. Henschen.
Inhibition of presentations, Prof. Heymans.
The degree of localization of movements and correlative sensations, Prof. Horsley.
Loss of volitional power, Prof. Janet.
A law of perception, Prof. Lange.
The female poisoner of Aïn Fezza, Prof. Lugeois.
Relation of respiration to attention, Prof. Lehmann.
Direct and associative factors in judgments of æsthetic proportion, Dr. L. Witmer.
Sensibility of women, normal, insane, criminal, Prof. Lombroso.
Parallel law of Fechner, Dr. Mendelssohn.
Limits of animal intelligence, Prof. L. Morgan.
Experimental investigation of memory, G. E. Mütter.
Psychophysical basis of the feelings, Prof. Münsterberg.
Experimental induction of hallucination, F. W. H. Myers.
Characteristics and conditions of the simplest forms of belief, W. R. Newbold.
The origin of numbers, Prof. Preyer.
General ideas, Prof. Ribot.
The future of psychology, Prof. Ricket.
Anatomical and physiological relation of the frontal lobes, Prof. Schäfer.
Experiments in thought transference, Mrs. Sidgwick.
Binocular after-images, E. B. Titchener.
Relation of reaction time to the breadth of perception, Dr. Tschisch.
Physiological basis of rythmic speech, Dr. Verriest.
Functional attributes of the cerebral cortex, Dr. Walle

[*Nature*, London, July 11, August 11.

The following subjects are treated in the *American Journal of Psychology:*

Knee jerk (The) in sleep in a case of dementia, Noyes.
Memory in school children, growth of, Bolton.
Zöllneis figures and other related illusions, Jastrow (studies).
Involuntary movements, Jastrow (studies).
Smell, absence of the sense of, Jastrow (studies).
Classification time, Jastrow (studies).
Finding time, Jastrow (studies).
Anthropometric and psychologic tests on students, Jastrow (studies).
Natural realism, psychological foundation of, Fraser.
Nervous system, psychological literature, Donaldson.
Association, Cattell
Reaction, Cattell.
Hypnotism and suggestion, Jastrow.
Suggestion, hypnotism and —, Jastrow.
Sight, psychological literature, Sanford, Scripture.
Physiological psychology, Sanford.
Laura Bridgman, Donaldson.
Visual area of the cortex in man, Donaldson.

Voluntary movements, rapidity of, Dresslar.
Attention, phenomena of, Angell.
Contrast, effects of, Kirschmann.
Musical expressiveness, Gilman.
Regular variations, pitch, intensity, etc., Scripture.
Unconscious suggestion, Forel.
Disturbance of attention, Swift.
Pseudo-chromesthesia, Kohn.
Psychiatry, Noyes.
Taste and smell, Bailey.
Touch, pain, internal sensation, Bailey.
Linguistic psychology, Chamberlain.
Voluntary motor ability, Bryan.
Training of animals, Rossignol.
Judgment of angles, lines, etc., Jastrow.
Unconscious cerebration, Child.
Action and volition, Baldwin.

III. ETHNOLOGY.

Prof. Alexander Macalister, in his vice-presidential address before Section H of the British Association, regrets that there is not in our literature a more definite nomenclature for the divisions of mankind, and that such words as race, people, nationality, tribe, type, stock, and family are often used indiscriminately as though they were synonyms. There are several collateral series of facts, the terminologies of which should be discriminated: (1) Ethnic conditions whereby individuals of mankind are grouped into categories of different comprehension, as clans or families, as tribes or groups of allied clans, and as nations, the inhabitants of restricted areas under one political organization—Ethnology. (2) Individuals regarded as descendants of a limited number of original parents, each person having his place on the genealogical tree of humanity. As the successive branches were subjected to diverse environments, they have differentiated in characteristics. To each of these subdivisions is applied the name of Race. [Haeckel terms this study anthropogony.] (3) The third category is that of language, sometimes conterminous, but it is as absurd to speak of an Aryan skull as of a brachycephalic language.—*Nature*, London, 1892, August 18, p. 379.

The British Association appointed a committee to organize an ethnographical survey of the United Kingdom. The committee, in pursuance of the object for which they had been delegated by the Society of Antiquaries of London, the Folk-lore Society and the Anthropological Institute, and appointed by the British Association, propose to record for certain typical villages and the neighboring districts, (1) Physical types of the inhabitants; (2) current traditions and beliefs; (3) peculiarities of dialect; (4) monuments and other remains of ancient culture; (5) historical evidence as to continuity of race.

Dr. Georg Geoland has published through Justus Perthes, Gotha, an *Atlas der Völkerkunde*. There are in it fifteen folio maps, to wit: I. Distribution of skin and hair: II. Density of population: III. Distribution of religions: IV. Distribution of diseases; V. Clothing, food, dwelling, and occupation: VI. Locations of peoples in 1500 and 1880: VII. Europe in 1880: VIII. Asia in 1880; IX. Southeast Asia: X. Oceanica; XI. Africa: XII. Aboriginal America: XIII. America in 1880: XIV. Linguistic map: XV. Europe about 100-150 after Christ. The charts are preceded by descriptive text and an alphabetic catalogue of all tribes mentioned, with reference to the latitude and longitude of their habitat.

The origin of the Manchu race, to which the reigning dynasty in China belongs (see *Nature*, London, 1892, XLV, 523, quoting from North China Herald, Shanghai), is thus set forth:

> The Tungus tribes, to which the Manchu belong, are scattered about in Siberia and Manchuria in rather small communities. They appear in history in the Chow dynasty. The Mongols as a race are probably an offshoot from Tungus stock. The consanguinity that exists between Manchu and Mongol is greater than that which is found to prevail between Mongol and Turk, and therefore it may be concluded that the Tungus, either in Siberia or in Manchuria or on the Amur, threw off a branch which became Mongol. Genghis Khan and his tribe started on their conquest of the Asiatic continent from the neighborhood of the gold mines in Nuchinsk, and the Mongols are not fishermen by preference nor hunters of the sable, martin, and beaver. They are rather keepers of sheep and riders of horses and camels. They might easily develop their language in the vicinity of the Altai mountains and the Baikal.
>
> As to the Manchus, they have forgotten their early occupation since coming to China, and they attend now only to the duties of the public service or to military training. The language like the Mongol is rich with the spoils of antiquity. All the various forms of culture, whether belonging to Shamanism, Confucianism, or Buddhism, with which they have become successively familiar, have contributed a share. To these must be added the vocabulary of the huntsman, the fisherman, and the shepherd, and all the terms necessary to feudal relationship as well as those of the trades and occupations of the old civilization.

Ethnology of Mahgreb.—Dr. Brinton proposed to adopt the Arab name, Mahgreb, for that portion of Africa west of the Nile Valley and north of the southern boundary of the Sahara. From time immemorial it has been the home of the Berber, or Hamitic, or Protosemitic peoples. (For the prehistory of this region consult A. Chatelin, in *Revue Scientific*, April 9, 1892.) Palæolithic man is said to have been here, succeeded by neolithic communities and megalithic structures, erected by ancestors of the Berbers. The same Berber stock has possessed Mahgreb from the very earliest times to the present day.

Celts.—An instructive discussion on the origin and migration of the Celts was begun by Dr. Brinton in *Science* (March 11) and continued through subsequent numbers. This discussion is not only valuable for what the authors of the notes say, but for the excellent works quoted.

Prof. Sergi published in the *Bolletino della R. Accademia Medica di Roma*, Ann. XVIII, fasc. II, a paper on the varieties of mankind in Melanesia, which is reprinted in *Archiv für Anthropologie*, XXI, 339-384. The essay is remarkable, among other excellences, for the extensive list of connotive terms for measurements of the head. Many of these words are old but quite a number are new:

Index of length.—Dolichocephal, mesocephal, brachycephal, hyper-dolichocephal, hyper-brachycephal.

Index of height.—Hypsicephal, orthocephal, chamæcephal.

The face.—Leptoprosop, mesoprosop, chamæprosop.

The nose.—Leptorrhine, mesorrhin, platyrrhine.

The eye cavity.—Hypsiconch, mesoconch, chamæconch.

Cranial capacity.—Microcephal, elattocephal, oligocephal, metriocephal, megalocephal.

The jaws.—Prognathic, orthognathic, mesognathic. For alveolar prognathism, prophatnic; for the upper face, chamælognathic; for zygomatic width, euryzygic.

The shape of the skull.—Steno-cephalic, eu-cephalic, stenoteric, lopho-cephalic, spheno-cephalic, tetragonic, poikilo-cephalic, chomato-cephalic, pro-ophryo-cephalic, rhomboido-cephalic, ovoid, ellipsoid (dolicho-ovoid, brachy-ellipsoid, etc.)

The forehead.—Brachymetopic, brachyelitometopic, leiometopic, hypsistenometopic, eurymetopic, stenometopic, euryeletometopic, clitoplatymetopic, clitobrachystenometopic, eumetopic.

Parietal bones.—Eurybregmatic, euryhomalobregmatic, hypsistegobregmatic, euryoncobregmatic, oxyoncobregmatic.

Occipital bone.—Opisthocranion, cremnopisthocranial.

In the text the Greek roots are given and the etymologies worked out.

IV. GLOSSOLOGY.

The Seventh Annual Report of the Bureau of Ethnology to the Smithsonian Institution by J. W. Powell, director, bears the imprint of 1891, but was really made public in 1892. This is in one sense a jubilee volume, the crowning glory of American linguistics, commenced systematically by Gallatin and ended by Powell.

The names of American Indian tribes have been in very great confusion, each tribe having many names. This confusion, as for example with the Mohawks, arose by having the spelling in three languages, by having their own real name confounded with terms of reproach gathered from neighboring tribes, by imperfect and conflicting systems of transliteration. But in combining the North American tribes into one system rules were necessary, therefore Maj. Powell laid down the following:

I. The law of priority relating to the nomenclature of the systematic philology of the North American tribes shall not extend to authors whose works are of date anterior to the year 1836.

II. The name originally given by the founder of a linguistic group to designate it as a family or stock of languages shall be permanently retained to the exclusion of all others.

III. No family name shall be recognized if composed of more than one word.

IV. A family name once established shall not be canceled in any subsequent division of the group, but shall be retained, in a restricted sense, for one of its constituent portions.

V. Family names shall be distinguished as such by the terminations "an" and "ian."

VI. No name shall be accepted for a linguistic family unless used to designate a tribe or group of tribes as a linguistic stock.

VII. No family name shall be accepted unless there is given the habitat of tribe or tribes to which it is applied.

VIII. The original orthography of a name shall be rigidly preserved except as provided for in Rule III, and unless a typographical error is evident.

As fixed in Powell's last revision the families stand thus: Algonquian (Eastern North America); Athapascan (Northwest North America); Attacapan (Louisiana); Beothukan (Nova Scotia); Caddoan (Three groups, northern, Arikara, middle, Pawnee; southern, Caddo); Chimukuan (Puget Sound); Chimarikan (Trinity River, California); Chimmesyan (British Columbia); Chinookan (Columbia River); Chitimachan (Louisiana); Chumashan (Santa Barbara, Cal.); Coahuiltecan (Texas); Copehan (northern California); Costañoan (Golden Gate to Monterey, Cal.); Eskimauan (Arctic coast); Esselenian (Monterey Bay, California); Iroquoian (Great Lakes); Kaloopaian (Washington State); Karankawan (Texas); Keresan (New Mexico); Kiowan (upper Arkansas); Kitunahan (Columbia River); Koluschan (southeast Alaska); Kulanapan (Mendocino, Cal.); Kusan (Oregon); Lutuamian (Oregon); Mariposan (California); Moquelumnan (Calaveras County, Cal.); Muskhogean (Southern States); Natchesan (Mississippi); Palaihnihan (Pit River, California); Piman (Gila River, Arizona); Pujunan (Sacramento River, California); Quoratean (Salmon River, California); Salinan (Monterey County, Cal.); Salishan (Washington and British Columbia); Sastean (Northern California); Shahaptian (Fraser River); Shoshonean (Interior Basin); Siouan (Missouri River); Skittagetan (Queen Charlotte Islands); Takilman (Rogue River); Tañoan (Rio Grande River); Timuquanan (Florida); Tonikan (Red River, Arkansas); Tonkawan (Texas); Uchean (Georgia); Waiilatpuan (Wallawalla River); Wakashan (Vancouver Island); Washoan (Carson Valley, California); Weitspekan (Klamath River); Wishoskan (Eel River, Oregon); Yakonan (Umpqua River, California); Yanan (Pitt River, California); Yukian (Round Valley, California); Yuman (Colorado River, California); Zuñian (New Mexico).

Finns.—Dr. Theodor Koppen (*Archiv f. Anthrop.*, XX) defends the unity of the Finnic and the Aryan linguistic stock, alleging the ancestral home to have been on the middle Volga. The separation into eastern and western branches took place on the river Don, at which time also arose the Aryan and the Ugro-Finnic division.

The publication of Middendorf's sixth volume on the Peruvian languages completes a most valuable series. The languages considered are the Kechua, the Aymara, and the Chimu (Muchik or Yunca), with an appendix on the Chibcha. The work was issued by Brockhaus, Leipzig. (Brinton, *Science*, XX, 6.)

In Philadelphia has been established the de Laincel fund for the study of the graphic system of the ancient Mayas, by collecting vocabularies of the language and its dialects and photographs of the ruins and inscriptions and manuscripts. Dr. H. T. Cresson has charge of the explorations.

V. TECHNOLOGY.

A remarkable contribution to the natural history of æsthetics, which the author of this summary has elsewhere called æsthetology, is the address of William H. Holmes, as vice-president, before Section H of the American Association. The science of the beautiful was examined in order to study the phenomena of the beautiful as the botanist studies the real flowers of the field.

"The science of the beautiful must deal with actual phenomena; with facts as hard, with principles as fixed, and laws as inflexible, as do the sciences of biology and physics."

The author takes up the subject from the phenomenal side and ignores the purely metaphysical element altogether, which is alleged to have woven about it a dense and very subtle web of transcendental fancy!

The author's appreciation of the amount of time and energy given to this field of human activity is charming. "We totally fail to realize how much time and thought are given to æsthetic considerations, and what a large place they really fill in the thoughts and activities of the world. This would come home to us if by some sudden change in the constitution of things all that is æsthetic should be rudely torn from us and banished from the world. - - - To make this clear, let us suppose that some dire disease should destroy our perception of the beautiful, a world of useless things would encumber our existence. The fine arts would fall into disuse. Painting, sculpture, architecture, poetry, music, romance, the drama, and landscape gardening would disappear utterly. No picture would grace the wall of gallery or dwelling. Temples and halls would be without statuary and books without illustrations. Architecture would degenerate into the merest house building, without projections, moldings, carving, painting, frescoes, hangings, or carpeting. Churches would be but the plainest barns without archways or columns, or steeples, or towers, or stained glass; the organ and the choir and the singing of hymns as though they had never been. All artists, sculptors, architects, poets, authors, composers, and dramatists, and all the multitude that depend upon them, decorators, engravers, carvers, musicians, actors, book-makers, manufacturers of all that pertains to the polite arts, and all merhants who deal in æsthetic things would turn to other callings. The ships and railways that transport the products of æsthetic industry, silks and rugs, and laces, and ornamental goods, and furniture, and tiles, and paints, and dyes, and porcelains, and brasses, would cease to plow the sea and girdle the land. The range of human livelihood would be reduced to a dangerous degree, and existence—a burden without art, would be overwhelmed with poverty and distress. Now, there was a time when this picture was a true one, and men had no great results in æsthetic art to show. From then to our day, Mr. Holmes declares to be a question of evolution.

By passing up through the scale of culture stages from savagery to enlightenment, we see that each succeeding period has a larger share of art and a correspondingly larger share of the æsthetic, each stage being prophetic of the succeeding stage. The last stage, that upon which the nations of the world are now entering—the enlightened—is also necessarily prophetic of a still more advanced stage; and by adding to the number of æsthetic groups those yet to be conceived and prolonging the expanding lines of each group indefinitely, we are led to comprehend the true relations of the present to the marvellous future, and to form some notion of the magnificent sum total of the æsthetic that future generations will be privileged to enjoy.

VI. ARCHÆOLOGY.

In the *Proceedings of the Royal Geographical Society* (Lond., 1892, XIV, 273–309) and in other journals will be found an account of the marvellous ruins of Mashona-land, in the water-shed of South Africa, between 18° and 20° south, by Theodore Bent, the explorer. There are many ruins on the Limpopo and elsewhere in this area, but the author confines himself to those on the Great Zimbabwe, situated 20° 16′ South, and 31° 10′ East. They cover a vast area and consist of a large circular building with a network of smaller buildings extending in the valley below, and a labyrinthine fortress on the hill, about 400 feet above, naturally protected by huge granite bowlders, and by a precipice running round a considerable portion of it. The lower building is constructed of small blocks of granite broken with the hammer into uniform size and laid up without mortar. The encircling wall is 30 feet high in parts and 16 to 17 feet thick. There is a long narrow passage between walls conducting to what Mr. Bent calls "the sacred inclosure" in which are standing two towers, one of them 32 feet high, a wonderful structure of perfect symmetry, and with courses of unvarying regularity.

The principal part of Mr. Bent's work and his most interesting discoveries took place on the hill fortress, the labyrinthine nature of which is explained in the plans. The approach is protected at every turn with traverses and ambuscades, and then commences at the bottom of the precipice a flight of steps leading up. In fact, the redundancy of fortification all over this mountain, the useless repetition of walls over a precipice itself inaccessible, the care with which every hole in the bowlders through which an arrow could pass is closed, prove that the occupants were in constant dread of attack. Pottery and iron objects occurred in abundance, but the most interesting find was connected with the manufacture of gold, crucibles, broken quartz, and furnaces.

These ruins are in no way connected with the African race. They formed a garrison for gold workers in antiquity, who came, doubtless from the Arabian peninsula, in the pre-Mohammedan period.

One of the results of the Congress of Archæological Societies, in union with the London Society of Antiquaries, is the issue of an index of archæological papers, published in 1891. There is a list of 45 societies and journals in all, and 33 pages of titles, succeeded by an alphabetic list of places, subjects, authors, and societies with their publications. The secretary of this congress of societies is W. H. St. John Hope, Burleigh House, London.

M. A. C. Chatelier contributes to *La Revue Scientifique* (XLIX, 457–461) a résumé of prehistoric studies in North Africa. To the work of codification is added a bibliography of 70 titles upon the same subject.

M. Zabarowski calls attention to the doubtful antiquity of the Canstadt skull. It was discovered in 1700, but, according to Dr. Hervé it was really seen first in the vitrine of the museum of Stuttgard a hundred years after the digging from which it is supposed to have come. Dr. Brinton also reverts to the same question in *Science*. Indeed, the year 1892 marks an epoch of decline in the belief that man has had an exceedingly high antiquity in Europe or America. The result of such questionings will be a review of the grounds of belief, with a strengthening of the foundations of knowledge.

The article of Louis Theureau, in *La Revue Scientifique* (L, 364–369) on alimentation in India, calls especial attention to the fact that it has been from time immemorial a country whose food was essentially vegetal, under the influence of an idea on which is founded a philosophic and religious system, belief in metempsychosis or migration of the soul. About fifty titles bearing on the subject are quoted, adding great value to the article.

An epoch-making investigation for archæolgoists was that of William H. Holmes upon ancient quarries in the United States. The result of the first investigation into the quarry site on Piney Branch near Washington, is given in the *American Anthropologist*, (III, 1–26). Dr. Brinton calls attention sharply to this work in a short paragraph on 'quarry subjects,' in *Science* (November 4, 1892). Since then a controversy, characterized by no little acrimony, sprang up between what might be termed the old school and the new school on this subject. Two distinct questions are involved in the controversy, namely, whether the objects are palæolithic implements or the rejected pieces of the aboriginal quarryman; and, secondly, whether they are geologically situated to denote very great antiquity.

The trustees of the British Museum printed an album containing autotype facsimiles of the Tel-el-Amarna tablets. A review of this work will be found in *Nature*, vol. xlvi, pages 49–52. During the summer of 1887 a woman belonging to the household of one of the "antica" dealers, who live at or near Tel-el-Amarna in Upper Egypt, set out to follow her usual avocation of digging in the sand and loose earth at the foot of the hills for small antiquities. The exact details of her search will never be known, but it is certain that in a small chamber at no great depth below the surface she found a number of clay tablets, the like of which had never before been dug up in Egypt. There were over three hundred of them, of which number the British Museum secured 82, the Gizéh Museum 60, the Berlin Museum 160. The Tel-el-Amarna tablets are unique as an archaeological "find," and they are also unique as a means of weaving together the threads of the histories of two or three of the greatest nations of antiquity at a critical period. They were all written between the years 1500 and 1450 B. C. Those in the British Museum consist of a series of dispatches written from Kings of Babylonia, Alashiyah, Mitana, Phœnicia, Syria, and Palestine to Amenophis III, and to his son, Amenophis IV. Many of them are also of a personal or private nature.

Alfred P. Maudslay, who spent seven winters in Central America studying and photographing the ancient ruins, announced the forthcoming of a work on this subject, the gist of which is given in *Nature* of April 29. A map on page 618 lays down graphically the limits of Maya inscriptions.

The orientation of buildings is considered by Dr. Brinton in *Science* (XX, 6), and the orientation of the sides as in Egypt brought into contrast with that of the corners as in Mesopotamia and Zuñi. At Zimbabwe a series of ornaments on the walls of the great temple are so disposed that one group will receive directly the sun's rays at his rising and another at his setting at the period of the winter solstice, when these points in that latitude were respectively 25° south of east and west, while a third series of ornaments faced the full midday sun.

Prof. W. O. Atwater, in the *Forum* for June, discusses the scientific study of food as one of the most important problems in anthropology. At present the poorer classes the world over are scantily nourished and the majority of mankind live on a low nutritive plane. The coming man will not buy as expensive foods because some of the least expensive are most nutritive and palatable. He will value foods for their nutritive qualities. Much less food of the proper quality will be required to keep a man in his best estate. There will be a revolution in cooking, which is both wasteful and primitive.

Payne's History of the New World called America is a philosophical treatment of a historical subject. It is a history of America written by a trained anthropologist. In the author's own words, he has "undertaken the unusual course of explaining the facts under investigation by a theory of human advancement not only not generally recognized but not hitherto formally enunciated. Some may find it paradoxical, to assign to advancement no loftier origin than the organized provision of the food supply on an artificial as distinguished from a natural basis. The organization of food provision on the artificial basis has been combined with that of defense, and communities in which these combined organizations have been fully elaborated have extended their boundaries at the expense of others whose social arrangements were less advanced." The author sets himself "to restore, if possible, the true features of the advanced communities of the New World, to analyse their social structure and economy, to measure by some definite standard the degree of progress they had attained, and to trace their history, so far as it can be recovered, distinguishing what can fairly be accepted as fact, from what can be shown with reasonable certainty to be fabulous."

VII. SOCIOLOGY.

The *Quarterly Journal of Economics*, published for Harvard University, in Boston, is valuable to the student not only for the papers and original investigations which it reports, but for its bibliography of economics. The titles are classified under (1) general works, theory, and its history; (2) production, exchange, and transportation; (3) social questions, labor, and capital (4) land; (5) population, emigration, and colonies; (6) international trade and customs tariffs; (7) finance and taxation; (8) banking, currency, credit, and prices; (9) legislation; (10) economic history and description; (11) statistics; (12) not classified.

Native fairs in Alaska were reported to the Numismatic and Antiquarian Society of Philadelphia by Lieut. Gorgas, U. S. Navy. Beginning at the south a fair is held in June at Port Clarence, just south of the narrowest part of the straits. It is numerously attended by Chukchis of Siberia, the natives of St. Lawrence Island, south of the straits, and by others from Cape Prince of Wales on the American mainland. The second fair is held at Hotham inlet, on the north shore of Kotzebue Sound. It lasts through July and August, and is attended by about 1,500 people, some Siberians, but mostly natives, especially from Point Hope, these being the principal traders of the coast.

A third fair is at Point Lay, and a fourth at Camden Bay, not far from the mouth of Mackenzie River.

The trading boats make a regular round of these fairs, carrying articles in demand from one to another; so that some from the far interior of Asia will in a few years be transported along the shores of the Arctic Sea and southerly indefinitely into the center of the continent. (Brinton, *Science*, XIX., 287.)

Galton's work on finger prints is thus briefly reviewed in the *Journal of the Anthropological Institute:*

The author considers the subject under the following divisions: (1) Introductory. (2) The previous employment of finger prints among various nations, which has been almost wholly confined to making daubs, without paying any regard to the delicate lineations with which this book alone is concerned. (3) Various methods of making good prints from the fingers are described at length, especially those used at Mr. Galton's anthropometric laboratory at South Kensington. (4) The character and purpose of the ridges whose lineations appear in the finger print. (5) The various patterns formed by the lineations. (6) The question of persistence; whether the patterns are so durable as to afford a sure basis for identification. (7) An attempt to appraise the evidential value of finger prints by the law of probability. (8) The frequency with which various kinds of patterns appear on the different digits of the same person, severally and in connection. (9) Methods of Indexing. (10) Practical results of the inquiry. (11) Heredity. (12) Use in indicating race and temperament. (13) The nine fundamentally different patterns are considered as different genera or species.

Gustave le Bon having affirmed that higher races can not impose their civilization upon lower races, undertakes, in an address before the Congrès international, institué par le Gouvernement français pour l'étude des questions coloniales (*Rev. Scient.*, Paris, 1889, août 24 and 1892, Oct. 1) to show that to change the civilization of a people it is necessary to change their souls (âmes). Centuries and not conquests can accomplish a task like that. The empire of the world has always belonged to the convinced, whose great force consists in their slavery to an idea, and in their complete incapacity to reflect and to reason. Without these, perhaps, no civilization would have been born and humanity would not have arisen above barbarism.

Lombroso and Ferrero discuss, in a work entitled "La Donna delinquente," the subject of the criminality of women. To their view the crimes of men and those of women are two quite different maladies, having certain symptoms in common but many more in which they differ widely. Women commit fewer crimes than men, all statistics are agreed on that. M. Guillat estimates the criminality of men to be six times greater than that of women and, according to Quetelet and Tarde, the tendency to crime is five or six times more developed in men.

Leaving out of view difference in legislation as to the sexes, M. Proal attributes the freedom of women to their greater religious spirit, their indoor life, the smaller number of employments which provoke to crime, like forgery and defalcation. Women go about less, and drink less, than men.

From the evolutionist's standpoint, according to Ferrero, the female has been less exposed to the struggle for existence. The sexual struggle does not exist for her at all and in higher civilization her degeneration produces crime in men. Ferrero sums up the causes of woman's smaller susceptibility to crime as follows:

(1) Women are physically weaker and more timid.
(2) Feebler sexuality, strong maternity and pity.
(3) The intelligence of woman is less.

Migrations.—Dr. Sophus Müller, of Copenhagen, published in Mém. Soc. Roy. des Antiq. du Nord a study upon cutting implements in the Stone Age, drawing the conclusion that parts of France and the Iberian peninsula were inhabited first. The argument is based upon the ruder forms of the southern tools. M. Bertrand's work "Nos Origines," holds to the opinion, however, that about 1200 B. C. the Ligurians came southward, finding central France and Spain occupied by Iberians who were driven westward by Celts.

Pre-historic commerce.—In the Verhandlungen der Berliner anthropologische Gesellschaft the subject of ancient commerce is discussed by G. Schweinfurth and Merensky, the former dealing with the influence of western Asia and India upon Egypt, the latter with India as affecting even the industries of Central Africa.

The archæologists are also able to bring some noteworthy contributions to this enquiry. In America certain types of basketry and pottery are known to have been peculiar to certain linguistic stocks. But examples of these are found elsewhere in ever-decreasing numbers as they depart from this source.

VIII.—RELIGION AND FOLK-LORE.

On the 16th of April there was publicly opened in the Museum of Archæology of the University of Pennsylvania a loan collection of objects used in worship. It was divided into sections, that devoted to the religions of Egypt being in charge of Mrs. Cornelius Stevenson, that of India was arranged by Suamee Bhaskara Nand Saraswatee; that of China by Chinese scholars, and so on, each section being assigned to some one specially fitted to the task.*

The American Folk-lore Society was organized in December, 1892, for the ensuing year, as follows:

President, Horatio Hale.

Vice Presidents, Aleée Fortier and D. P. Penhallow.

Council, Franz Boas, H. Carrington Bolton, D. G. Brinton, A. F. Chamberlain, J. Owen Dorsey, Alice C. Fletcher, George Bird Grinnell, Otis T. Mason, Frederick W. Putnam.

Secretaries, W. W. Newell, J. Walter Fewkes.

Treasurer, John H. Hinton.

Curator, Stewart Culin.

The organ of this society is the *Journal of American Folk-lore*, issued quarterly. In addition to the original papers and proceedings of the society and its branches contained in this journal, there is a résumé of folk-lore throughout the world, and an extended bibliography, which is especially good in periodical literature.

The fourth annual meeting of the American Folk-lore Society was held at the Thorndike Hotel, Boston, Mass., on December 28, and at the Peabody Museum of American Ethnology and Archæology, Cambridge, Mass., on December 29, Prof. Edward S. Morse presiding. The following papers were read:

Two Biloxi tales, J. Owen Dorsey.

Relation of the tales of Uncle Remus to the animal stories of other countries, Adolph Gerber.

Survival of fire sacrifice among the Indians of Maine, Miss A. L. Alger.

Folklore of the Azorian Colonies, H. R. Lang.

A modern oracle and its prototypes, H. Carrington Bolton.

Tales of the Abenakis, A. R. Tisdale.

Chippewa tale of the end of Hiawatha, H. H. Kidder.

Pawnee mythology, G. B. Grinnell.

*See printed catalogue, and *Science*, N. Y., xix., 225.

Blackfoot mythology, J. Maclean.
The Algic Manabozho, J. C. Hamilton.
Medicine men and certain Indian myths, Henry Mott.
Doctrine of souls among the Chinook, Dr. Franz Boas.
Christ in folklore, A. F. Chamberlain.
Animal and plant weather proverbs, Fanny D. Bergen.
Customs and traditions of the Ainos of Japan, D. P. Penhallow.

The permanent results of the Folk-lore Congress held in London in 1891 are given to the public in a volume of 472 pages, entitled "Papers and Transactions." The material is arranged under the four sections called Folk-Tale; Mythology; Custom and Institution; General Theory and Classification. The president of the congress, Mr. Andrew Lang, and the vice presidents of the sections delivered addresses, and papers of great merit were read. The most important discussion was that concerning the independent origin of folk incidents. Under the title "Bibleöthèque de Carabas," David Nutt has issued seven volumes which are of especial delight to folk-lorists, to wit: Cupid and Psyche, by William Adlington: Euterpe, the Second Book of Herodotus, Englished by B. R., 1584; The Fables of Bidpai, or the Morall Philosophie of Doni, Englished out of Italian by Thomas North, 1570, now edited by Joseph Jacobs; The Fables of Esopas printed by W. Caxton in 1484, edited by J. Jacobs; The Attis of Caius Valerius Catullus, translated, etc., by Grant Allen; Plutarch's Romane Questions, translated in 1603 by Philemon Holland.

Plutarch's Romane Questions, translated in 1603, by Philemon Holland, M. A., of Trinity College, Cambridge, has again been edited by Mr. Jevons, of the University of Durham, with additional dissertations on Italian cults, myths, taboos, man-worship, Aryan marriage, sympathetic magic, and the eating of beans. Plutarch's Romane Questions is said to be "the earliest formal treatise on the subject of folk-lore." Plutarch was the first "to make a collection and selection of dates, and to give them a place of their own in literature." Plutarch's answers, however, are not in the modern vein, for they are framed on the assumption "that the customs that they are intended to explain were consciously and deliberately instituted by men who possessed at least as much culture and wisdom as Plutarch himself."

The current literature on the scientific study of religions is to be followed up in the Annales du Musée Guimet, and especially in the Revue de l'Histoire des Religions, published on the Guimet foundation under the direction of M. Jean de Réville, with the co-operation of Barth, Leclercq, Decharme, Hild, Lafaye, Maspero, Renan, and Tiele.

The volume of La Revue for the year 1892 contains the following original papers:

Le dieu romain Janus. J. S. Speyer.
Les hymnes du Rig Véda, sont-ils des prières. Paul Regnaud.
Bulletin de la Religion Juive.
Le dénombrement des sectes mohametanes. I. Goldziher.
Bulletin archéologique de la Religion Romaine, Aug. Adollent.

Contes Boudhiques: 1. La Légende de Çakhupala 2. La Légende de Maddhakundalc. Vallée-Poussin et Godefroy de Blonay.

Esquisse des huit sectes boudhistes de Japon, Gyau-neu (1289 B. C) trans. Alfred Millwud.

Ernest Renan, Albert Réville. Bulletin archéologique de la Religion Grecque. Pierre Paris.

Garci Ferrans de Terena et le juif Baena. Scènes de la vie religieuse en Espagne à la fin du XIV siècle. Lucien Dollfus.

Fragments d'évangile et d'apocalypses découverts en Égypte. Ad. Lods.

In each number is a review of books, a chronicle of what is doing along the line of the scientific study of religions, abstracts from periodical articles and from the transactions of learned societies, and a classified bibliography. For some reason the date of publication is omitted in every case, which detracts much from the value of the book lists; but in the abstracts from periodicals an indispensable list of journals and their contents will be found.

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THE ADVENT OF MAN IN AMERICA.*

By Armand de Quatrefages.

One of the chief problems of anthropologists in regard to America is that of the origin of its inhabitants. Were the original American people related to those of the Old World? Or were they indigenous to America and without ethnologic relation to other populations? Both these views, as you are aware, have had their partisans. I have already made known my opinion upon this subject, which is that America was originally peopled by emigrants from the Old World. I propose to give a brief résumé of the grounds of this conviction.

I recall two rules which I have constantly followed in the solution of questions sometimes so ardently contested, which are raised in the history of Man. The first rule is, to put aside absolutely every consideration borrowed from dogma or philosophy, and to invoke only science, that is, experience and observation. The second rule is, not to isolate man from other organized beings, but to recognize that he is subject (in all that is not exclusively human) to all the general laws which govern equally animals and plants. Hence no doctrine or opinion is to be regarded as true which, considering man as an animal, makes him an exception among organized beings.

Let us apply these principles to the question before us, but more broadly; for it is but a special case of a still more general problem. Man is everywhere now. Did he appear everywhere in the beginning? If not absolutely cosmopolitan in its origin, did the human race originate at an indefinite number of points; or, originating at a single and limited spot, has it gradually taken possession of the whole earth by migration? At first thought, we might suppose the answer to these questions would be different according as we admit the existence of one or many human species, but this is an error; for we shall see that on this point, at least, the Polygenists may shake hands with the Monogenists without being involved in any contradiction with the facts. Let us take first the Monogenistic view.

Physiology, which leads us to recognize the unity of the human species, teaches us nothing relative to its geographic origin; it is

*Presidential address delivered before the Eighth International Congress of Americanists at its session in Paris, 1890. (Proceedings of the 8th session: Congr. Internat., pp. 13-55.

otherwise with the science that is occupied concerning the distribution of animals and plants over the surface of the globe. The geography of organized beings has its general laws, which it is necessary we should know and interrogate if we would solve the problem of the peopling of the world.

The first result of this study is to show that true cosmopolitism—as attributed to man—does not any where exist either in the animal or in the vegetable kingdom. In support of this proposition it is proposed to cite some testimony. On the subject of the vegetable kingdom, Candolle says: "No phanerogamous plant extends over the whole surface of the earth. There are not more than eighteen of these plants which extend over an area equal to half the earth, and no tree or shrub is among those plants which has the greatest extension."

In my lectures upon this subject, I have cited the best scientific authorities respecting the principal groups of marine animals, either of salt or fresh water. I have reviewed the fauna of the air, beginning with the insects, and I have dwelt to some extent on fishes and reptiles. Omitting all the rest of the birds I notice the Peregrine falcon, the area of whose habitat is the most extended, occupying, as it does, all the temperate and warm regions of the Old and New World, but does not reach the Arctic regions or Polynesia.

Anatomically and physiologically, Man is a mammal, nothing more, nothing less. This class interests us more than the preceding, and furnishes us with knowledge more precise. Permit me, then, to enter upon certain details, taking for my guide the great work of Andrew Murray, which became classic upon its appearance. By reason of their strength, their great power of locomotion, and by the expanse and continuity of the seas which they inhabit, the Cetaceans would seem to have both the greatest capacity and opportunity to become cosmopolitan, but such is not the case. Each species is restricted to an area more or less extended, from which a few individuals occasionally make excursions, but always soon return to their proper limit. Two exceptions have been claimed to this general rule. A Rorqual, with large flippers [the "humpback"], and a boreal Balænoptera ["finback" whale], natives of temperate and frigid seas, have been found, the first at the Cape of Good Hope, the second at Java. But even these are said by Van Beneden and Gervais, the two greatest authorities on Cetology, to be at least doubtful; but accepting them as true, it yet remains that neither species has ever been found in the seas that border America or Polynesia. Among other animals than the Cetaceans, there is nothing to be found approaching even a narrow cosmopolitism. Here again I am to spare you details. It is familiar to all, that Edentates and Pachyderms have their respective countries clearly defined, and if the horse and the hog are to-day in America, it is because they were imported by Europeans.

The number of Ruminants which inhabit the north of the two continents is very small. The Reindeer and the Caribou are generally regarded as only varieties of one species. Brandt, with some reservations, says the same of the Bison and the Auroch, the Argali (Asiatic wild sheep), and the Bighorn (Rocky Mountain sheep). But none of these species are found in the warm regions of these two hemispheres, nor in all Oceanica. The Carnivora offer similar facts to the preceding: but when we come to the Cheiroptera and the Quadrumana we do not find a single species common to both continents, or to the rest of the world.

Thus among all organized beings, whether plant or animal, there is not a cosmopolitan after the manner of man. Now it is evident that the area of the actual habitat of any animal or vegetable species includes the center where that species first appeared. By virtue of the law of expansion the center should likewise be less in extent than the occupied area. No plant and no animal, therefore, originated in all the regions of the globe. To suppose that in the beginning man appeared everywhere that we now see him would be to make an exception of him which would be unique. The hypothesis therefore can not be accepted, and every monogenist will reject the supposition of the initial cosmopolitism of the human species as a false conclusion.

The Polygenists must accept the same conclusion unless they refuse to apply to man the laws of geography, botany, and zoölogy that govern all other beings. In fact, to whatever extent they have multiplied the species of man, whether they assume that there are two with Virey, fifteen with Bery Saint-Vincent, or an undetermined but considerable number with Gliddon, they have always united them into a single genus; and they could not do otherwise. Now a human genus can be no more cosmopolitan than a human species. Speaking of plants, Candolle says, "The same causes have borne on genera and on species," and this is as true of animals as of plants.

Restricting ourselves to mammals, among the cetaceans, Murray thinks that the genera of the rorqual and the dolphin are represented in all the seas. Van Beneden and Gervais dispute this. We will however admit it; it does not all weaken our conclusion, for, excepting the cetaceans, there can be no question of generic cosmopolitism. Of the ruminants, the genera of the deer, the ox, etc.; of the carnivora, the cat, bear, etc., have representatives in both worlds, but none in Australia or Polynesia. As we examine the higher groups, we see the number of these genera of large area diminishing, until finally not a single genus of monkey is known to be common to the Old and the New Continent: and the Simian type itself is wanting in the greater part of both worlds and Oceania.

Thus, whether we regard animals by species or by genera, the area of their habitat becomes more restricted as the animals are higher in the zoölogic scale. It is the same with the vegetable kingdom. Candolle says: "The mean area of species is as much smaller as the class to which they belong has a more complete, more developed, or in other words a more perfect organization."

The greater restriction of the area in proportion to the increasing perfection of the organism is then a general fact, a law applicable to all organized beings, and which is easily explained by physiology. Now this law is in direct opposition to the hypothesis that there can exist a human genus comprising several distinct species which have appeared in every quarter of the earth, wherever we now find men. Invoking the authority of Murray, and the universality of habitat which he attributes to the genera of the rorqual and the dolphin, polygenists might be tempted to say, "non-cosmopolitism already presents two exceptions. Why may there not be a third? Two genera of cetaceans are represented naturally in all the seas. Why may not the human genus have appeared at the start in every land?" This reasoning is faulty at its foundation. The rorqual and the dolphin belong to the lowest order of mammalia. Man, if we regard the body alone, is of the highest order. Unless we constitute them a single exception, they must obey the laws of the superior group; consequently they can not escape the law of increasing restriction of area.

It follows therefore that a human genus, as the polygenists understand it, must have occupied, in its origin, an area no more extended than that which has comprehended some genera of monkeys. But among the monkeys themselves all naturalists recognize a hierarchy; all place at their head the order of anthropoid apes. It is then from the secondary groups of this family that polygenists should ask for indications of the possible extent of area primarily accorded to the human genus; and it is well known how inconsiderable is the area occupied by the genera Gibbon, Orang, Gorilla, and Chimpanzee.

Whatever our point of view, we have either to assume that man alone escaped the laws which have regulated the geographical distribution of all other organized beings, or else admit that the primitive tribes were domiciled upon a very restricted space. Judging from present conditions, making the largest concessions, neglecting the incontestable superiority of the human type over the Simian type, all that the polygenistic hypothesis permits is to regard that area as having been nearly equivalent to that occupied by the different species of Gibbons which range on the continent from Assam to Malacca; in the islands from the Philippines to Java. Monogenism of course tends to restrict this area still more, and to make it equal at most to that of the Chimpanzee, which extends nearly from the Congo river to the White Nile. I would be the first to recognize that we may perhaps have to enlarge these limits at some later time. I consider the existence of man during the Tertiary geologic epoch to be demonstrated; and only the geographical distribution of the monkeys, his contemporary, can furnish more precise information upon the primary extension of the center of man's appearance. Palaeontology has taught us that the area formerly occupied by the Simian type was evidently more considerable than it is now. It may have been the same with the anthropoid apes, but down to the present time, no fossil is connected with that family. The extinct dryopithecus, long regarded as belonging to them, has been shown by the examination of the best preserved remains to be nothing more than an inferior ape. The general laws of the geographic distribution of beings, and especially that of increasing restriction of area, with superiority of organization, permit us to affirm that man primarily occupied only a very limited part of the globe, and that if he is now everywhere, it is because he has covered the earth by means of his emigrant tribes.

I am aware that this idea of the peopling of the globe by migrations has disquieted many persons. It puts directly before us an immense unknown; it raises a world of questions, a large number of which may appear inaccessible to our investigations. It has been often said, "Why create all these difficulties? It is more natural to confine ourselves to the popular movement attested by history, and accept autochthonism, especially among the lower savages. How could the Hottentots and the Fuegiens reach their present country starting from some undetermined point posited in the north of Asia? Such voyages are impossible; these peoples were born at the Cape of Good Hope and at Cape Horn." These suggestions may be answered by an anecdote borrowed from Livingston, the bearing of which will be easily comprehended. This illustrious traveler relates how in his youth he with his brothers made long excursions devoted to observations in natural history. "In one of these exploring tours," he says, "before the study of geology had become as common as it did later, we went into a limestone quarry. It is impossible to express with what joy and astonishment I set myself to picking out the shells which we found in the Carboniferous rocks. A quarryman looked at me with that air of compassion which a kindly man takes on at the sight of a person of feeble mind. I asked him how these shells came in these rocks, he answered, 'When God made the rocks, he made the shells and put them there.'" Livingston adds, "What pains geologists might have spared themselves by adopting the Ottoman philosophy of that workman." It may be asked, in turn, where would geology have been if men of science had adopted that philosophy? I ask the anthropologists to imitate the geologists: I invite them to inquire how and by what way the most distant peoples have radiated from the center of the first appearance of man to the extremities of the globe. I am not afraid to predict brilliant discoveries to those who will set themselves seriously to the study of the numerous and well-established migrations. In this the past permits a glimpse into the future.

Some years since, when objectors used the language I have just recalled, they did not fail to add Polynesia to the list of regions which man, then destitute of all our perfected arts, could not have reached.

It is now known how completely such assertions have been contradicted. Adding his personal researches to those of his predecessors, Hale first drew a map of Polynesian migrations. Twenty years later, aided by the documents subsequently collected, I was able to complete the work of the learned American. Now, as has been said by the lamented Gaussin (so competent to speak in all that relates to Oceanica), the peopling of Polynesia by migrations starting from the Indian Archipelago is as clearly demonstrated as the invasion of Europe by barbarians in the Middle Ages.

Like Polynesia, America was peopled by colonies of emigrants from the Old World. Their point of departure is to be discovered and their tracks to be followed. The labor will indeed be more difficult and longer upon the Continent than in Oceanica, principally because the migrations were more numerous and go back to a higher antiquity. The first Indonesian pioneers who, departing from the Island of Bouro, landed in the Samoan and Tongan archipelagos, probably made the passage near the end of the fifth century, or about the time of the conversion of Clovis. The peopling of New Zealand by emigrants from the Manaias goes back at most to the early years of the fifteenth century. Thus the peopling of Polynesia was all accomplished during our Middle Ages, while the first migrations to America date from geological times. Two savants, to whom we owe precious discoveries, Ameghino and Whitney, have traced the existence of man in America back to the Tertiary age. It is true that this opinion has been contested by men of equal repute; but I believe that the view of these men is confirmed by a comparison of the fossil faunas of the pampas of Brazil and the California gravels.

Judging by what little we know, man reached Lombardy and the Cantal before he had penetrated to America. It is necessary here, without doubt, to make the most formal reserves in favor of future discoveries; but if the fact is confirmed, it would seem to admit of easy explanation. Everything leads me to believe that the separation between America and Asia as now existing took place before the Quarternary epoch. If it was otherwise the species of mammalia common to the north of both continents would have been more numerous. The men and land animals on the shore of the Bering Sea and Strait have been stopped there. But when the great geologic winter substituted the polar temperature for a climate similar to that of California, the ancient Tertiary tribes were forced to migrate in every direction. A certain number of them may have embarked upon the ice extending between the two shores, and thus have arrived in America with the reindeer, as did their western congeners in France with the same animal. From that time the era of immigration was opened for America; it has never been closed since. Each year the winter rebuilds the bridge which unites East Cape with the Cape Prince of Wales; each year a road—relatively easy for the hardy pedestrians, stretches from one continent to the other; and we know that the population of the two opposing shores take advantage of it to maintain communications with each other.

Whenever one of those great social agitations of Asia made its waves felt in distant countries; whenever revolutions, political or social, overwhelmed them, is it not evident that the fugitives or the vanquished would often have taken this route, of the existence of which they were aware? To reject the idea of such migrations over the frozen seas, it would be necessary to suppose that since the commencement of the Quarternary period all neighboring regions have enjoyed a perpetual peace; but we all know that such a peace is not of this world. This sea may not have been the only route followed by American immigrations.

The chain formed by the Aleutian islands, and Alaska further to the south, opened a second route to tribes which possessed a little skill in navigation. The Aleuts occupy in Prof. Dall's ethnological chart the whole extremity of the peninsula.

By these ways, what we might call the normal peopling of America may have taken place. But bathed on either side by a great ocean, this continent could not fail to profit by the hazards of navigation, and we can recognize more and more how it may have been done. It may now be said, that with Europe and Africa on one side and Asia and Oceania on the other, these have sent to America a number of involuntary colonies more considerable perhaps than might be supposed.

Immigrations in America as well as in Europe have been intermittent, and separated sometimes by centuries. America has been peopled as if by a great human river, which, rising in Asia, has traversed the continent from north to south, receiving along its course a few small tributaries. This river resembles the torrent streams of which there exist examples in France. Usually, and sometimes for years at a time, their bed is nearly dry. Then a great storm comes and a liquid avalanche descends from the mountains where their sources rise, covers and ravages the plain, turning over the ancient alluviums, disturbing and mingling the old and new material, carrying farther each time the débris eroded on its passage. Such has been the career of our ethnological river. Its floods moreover have often been diverted to the right or left, and it has opened new channels. It has also had its eddies; but its general direction has not changed, and we can trace it down to the present.

One of the most agreeable tasks for the students of American prehistoric anthropology will be to retrace this river up to its source; to determine the succession of its floods; to distinguish the origin and nature of the elements which it has swept down; to follow those elements from stage to stage, and to thus recover the route each one of them has taken to the point of its arrival; in other words, to write the history of these migrations of the different people of America. The accomplishment of this task, as has already been said, is indeed much more difficult in America than in Polynesia. Those who undertake it will encounter nothing corresponding to the historic songs and the geneologies composing the oral archives and traditions so religiously preserved in all the islands of the Pacific. But modern science has resources, the power of which we are better and better coming to understand, combining the data furnished by the study of geologic strata and their fossils, of comparative craniology, of linguistics, and of ethnography. We may hope to enter upon this group of problems and to foresee their solution. Serious efforts have already been made in this direction, which have not been unfruitful. One can even now indicate upon the chart a considerable number of itineraries, even though as yet only partial and local. They are scarcely more than fragmentary traces, similar to those which the predecessors of Hale found in Oceania. Possibly it will be a long time thus; nevertheless, the Americanists should not lose courage; each new discovery, of however small importance it may at first appear, is some progress toward the general end. Year after year these fragmentary traces, now so isolated and scattered, will be consolidated and coördinated with each other, and then will come a day when a map of American migrations can be constructed showing the movement of early man from Asia to Greenland, and to Cape Horn, similar to the map already made of Polynesian migration from the Indian Archipleago to Easter Island, and from New Zealand to the Sandwich Islands.

PRIMITIVE INDUSTRY.*

By Thomas Wilson, LL. D.

The modern signification of primitive industry is the art work of primitive man, and as such is a test of his civilization. As we know of the earliest man only by his industries, it is justifiable under this head to consider man in the highest antiquity. The origin of man and his first known appearance upon earth have always been interesting subjects and have attracted the attention of all men throughout all time. It is mysterious, unknown; it awakens curiosity; it excites that portion of man's nature which desires to trace things to their origin, and to find a rational and satisfactory explanation of the cause and manner of man's appearing. It has been studied from various points; by biology, by palæontology, linguistics, history, psycho-physics, and by archæology. There are various branches of science by which the history of man can be studied, but they are all modern. The ancients knew nothing relating to the antiquity of man.

Until the times of Copernicus and Gallileo it was believed that the earth was the center of the solar system, and that the sun, moon, and stars revolved around it. Until the time of Michael Angelo, and Bernard Pallissy, fossil shells found in the earth were believed to be the fragments of stars fallen from the heavens. One hundred and fifty or two hundred years ago the science of geology commenced to be studied, and the formation of the earth, with its proper place in the solar system, began to be understood. At the beginning of the nineteenth century it was an accepted theory that man's appearance upon earth dated only about six thousand years ago. This theory was accepted for want of any better; those who rejected it did so *a priori*, and not because they had another or juster theory to propose. In the early part of this century, the Government of Denmark organized a commission, composed of a geologist, a zoölogist, and an archaeologist, charged with the duty of investigating that country on the lines of their respective sciences, in the course of which they came upon the art works of primitive man. They pursued their investigations for nigh thirty years before the first publication was made, which resulted,

*A Saturday lecture delivered in the lecture hall of the U. S. National Museum, under the auspices of the Anthropological Society of Washington.

after many disputes and much consultation, in the establishment of the Pre-historic Ages of Stone, Bronze, and Iron. This commission found various monuments and implements, evidently of human origin and manufacture, which being unlike anything belonging to the historic man of that country, were decided to be the evidence of an earlier and pre-historic man. The most important of these were the Dolmen, which was his tomb, and the stone hatchets. These discoveries were published in 1836 by Thomson, archæologist, and founder of the Prehistoric Museum at Copenhagen, of which he continued curator for fifty years. They were recognized throughout western Europe, and they accounted for similar monuments and implements which theretofore had been unexplained, or if so, were attributed to supernatural means; the hatchets especially being believed to have descended from heaven in a bolt of lightning or clap of thunder, and they were called by those names respectively, "Lightning Stone" or "Thunder Stone," and were guarded as amulets for the protection of property against fire. This was the first step in the discovery of primitive industry.

In 1859 Darwin published to the world his theory on the Origin and Evolution of Species, and thus he sought to establish and explain the antiquity of man. Contemporaneous with this was the discovery of Palæolithic implements by M. Boucher de Perthes in northern France. The place of their original and first discovery was St. Acheul on the river Somme, but afterward they were found in other places,—Chelles, on the river Marne, near Paris, being one of the principal. The latter station gave its name to the implements, and they have since been called Chellean. So far as can now be asserted with confidence, these implements are the earliest made or used by man. They may have served as axes, hatchets, or knives, spear-heads or what-not. They appear to have been a tool for every use, just as a sailor would use his jackknife if he had no other tool or weapon. They have been called in England "drift implements" because they were found in the river drifts or deposits. Their positions when thus found indicated for them an antiquity equal almost to the river valleys themselves, and as belonging to that geologic period called by the French geologists "Quarternary," by the English "Pleistocene," and by American "Post-pliocene."

There was a geologic period when the waters of the earth were engaged in carving out the river valleys, eroding and cutting them out between the bluffs on either side. In that time the rivers filled the valleys from the hills, pouring down their waters with a rush and carrying the greatest quantity of water to the sea. As time progressed the waters subsided more or less and the current became slower and less powerful. At the close of the Pliocene and at the beginning of the Quarternary period, the sand and gravel which had before been carried out to sea, began to be deposited here in this bend and on that point until the deposit came to the surface of the water and formed what is now the highest terrace. Thus the river was narrowed and the terrace became a new river bank. This process was repeated again and again until the river finally receded to its present bed, leaving sometimes three terraces, each one higher, deeper, and more distant from the river than the other. These terraces may not exist on the rapid mountain streams of the Atlantic slope, but they are plainly to be seen upon the longer rivers of the western slope of the Alleghenies. They are plainly manifest in the Mississippi river and its tributaries. One who has had the opportunity for inspection of these gravelly terraces, can see at once how the material was brought down by the water and here deposited. It is dependent upon amount and velocity of the water and the size of the pebble whether the deposit is of the finer debris or made up of pebbles only. Its layers or strata are plainly marked, and the volume and rapidity of the current can easily be surmised if not actually calculated. In France and England bones of animals belonging to that period, animals extinct in modern times, the mammoth, even its ancestor elephas antiquus, the rhinoceros merkii, the hippopotamus, the cave bear, the saber-toothed tiger, had been caught in the whirls of water, carried down and deposited with the pebbles. In these gravels, and associated with these animals, have been found these chipped stone implements called chellean. If these implements had been found as isolated specimens, only a few in number, they would not be nearly so convincing as when found as they have been in almost every river valley of Western Europe by the thousands if not the tens of thousands. They are there usually of flint, probably because flint was the material easiest procured and best suited to the purpose. In localities where flint was not indigenous, quartzite has been used, and there are in the U. S. National Museum specimens of this material from England, France, and Asia. They were made altogether by chipping, that is, by being struck with the hammer; it may have been another pebble; and so flakes knocked off, first from one side and then from the other, until the implement was reduced to an irregular but sharp edge and point. They are made sometimes of a bowlder, whether of flint or of quartzite, and the crust of the original pebble is shown and part left for the grip. They are of a size to be held in the hand and used as tools or weapons. There is no evidence that they were ever hafted, but on the contrary, their form is such as to render them most difficult for satisfactory handling. An envelope of hide, grass, leaves, moss, or something similar probably served to protect the hand. They have two or three peculiarities, which it is proper to notice, other than being chipped and having a grip. They are always of appropriate size for use; they are thicker in proportion to their width than any other stone-cutting implement; they are usually almond-shaped, and their cutting-edge is at the point. The conclusion that the implements were of human manufacture, and are evidence of the antiquity of man, was not admitted until after much discussion and investigation. The first of them was found in 1836. M. Boucher de Perthes soon after published his belief that they were evidence of what he called "Antedeluvian Man." It was disputed, first, that they were not of human manufacture. M. Mantel, an English geologist of some celebrity, once read an extended paper before one of the scientific societies of London to prove they were not. The fact of their discovery was disputed, the location had to be identified and established; and it was not until 1859 (thirteen years or more), that the conclusion as aforesaid was accepted, and then only after the investigation of a joint committee of fifteen prominent scientists, half from England, half from France, which met on the ground and were fortunate enough to find some specimens *in situ*. Since then the belief in the genuineness of their evidence as high antiquity of man has been accepted by all men. It was soon after the discoveries of M. Boucher de Perthes and those of M. Lartet of the caves of southern France, that Sir John Lubbock, noting the difference between this industry and that of the dolmens and polished stone hatchets of Denmark and other countries, and that they all belonged to the Stone age, took upon himself the division of that age into periods, of which he named the former Palæolithic, that is, the early period, and the other the Neolithic, or the later period of the Stone age. Thus it will be perceived that the existence of a Palæolithic period, the evidence of the occupation of that country by man in a period of time earlier than the Neolithic, was as much opposed, and required as long a time to secure a favorable settlement as has the discoveries of Dr. Abbot of similar implements in the Trenton gravels. From France and England the new evidence concerning the antiquity of man spread to other countries, and it was found that similar implements existed in nearly every country in the world. They have been found in Spain and Portugal. Mr. H. C. Mercer, a gentleman from Philadelphia, while at Madrid during the last exposition in 1892, visited one of the gravel beds of the neighborhood, San Isadore, where these implements were said to have been found, and he discovered one in place which he declares impossible to have been other than an original deposit. He secured all evidence by photographs, plaster casts, etc. So also of Italy. They have been found in various localities and are to be seen in the museums of different cities. Prof. H. W. Haynes, of Boston, found the same kind of implement on the left bank of the Nile, not in the alluvial deposit, but in an eroded gully or waterway in the original gravelly deposits. Christian missionaries to the Holy Land have found and reported similar implements, and they are deposited in the museum at Paris. Two great stations in Hindostan were also disclosed,—one near Madras, in southeastern Hindostan, and the other in Nerbudda, on the northwest coast. In many of these cases such implements were deposited deep in the gravel together with the bones of extinct animals, accompanied only by their necessary débris of chips, hammers, flakes, etc.; and except certain implements, the hammer, scraper, and leaf-shaped blade, which, from their nature, belonged to both periods, nothing was found which had any relation with the Neolithic or polished stone period. So it has come to pass that throughout the world, whatever differences there may have been between the scientists as to the antiquity of man, or the locality of his original appearance, manner of his civilization, use of implements (and these differences have been almost infinite), nearly all of them have agreed upon the existence of this Palaeolithic period, and that it was anterior to the Neolithic period. It is not therefore for me to continue in this country a discussion of matters which belong to other countries, and which have been fully investigated for years by the scientists of those countries and been accepted as settled. If the evidence as to Palaeolithic man in America be developed, arguments made and investigations required, it will be nothing more than what was required in France and England at the time of the original discovery; but I am not without the belief that it will be finally acknowledged to be true in our country, as it had been in other countries. A series of pertinent questions may have already suggested themselves: What is the Palaeolithic age? What are its characteristics? By what test is it to be known? Before the name Palaeolithic was given to it, indeed many times since, it was called the age of chipped stone. It must not however be considered that every stone implement belongs to the Stone age because it was chipped. Our own North American Indian, during all the time he has been known, even into the present century, has made —indeed pre-historic man has always made—his stone arrow and spear-heads by chipping. The term Palaeolithic age, synonymous with chipped stone age (to be translated as the early Stone age), is to be regarded as descriptive of a certain state of human culture,—a stage of human civilization belonging to the antiquity of man, and as its name indicates, one of the earliest, if not entirely the earliest, civilization known. Some pre-historic anthropologists believe there have been earlier civilizations, but this conclusion is disputed, and has not been generally accepted by scientific investigators. In this early state of culture primitive man employed stone as the material for all his cutting implements. He was unacquainted with the processes of pecking or grinding, and so, to reduce these stones to a sharp edge or point, he had recourse to chipping. This he accomplished by percussion with a hammer or punch, or a pusher of some kind, or possibly all three. With these he could knock off the large chips and flakes, and could push and press off the smaller ones. In this way he reduced his implement to a cutting edge or point. The first epoch or period of man's civilization was characterized by these implements. This epoch was called by M. de Mortillet the chellean epoch, but by M. Reinach and others, the alluvial period, because the implements were found in the alluvial deposits of the river valleys; while others called the age of the mammoth.

As time progressed man made certain improvements or inventions and attained a higher culture. These epochs have been differently divided and differently named; by some they have been called the cavern period; by others the reindeer period; and M. de Mortillet made finer distinctions to which he gave the names of localities in which the implements occur: Solutre, Moustier, and Madaleine. These were caverns or rock shelters, and they all represent the cavern period, with the mammoth and the reindeer the most abundant, as the representative animals. The flint implements of these epochs were changed in some degree,—the points become smaller, scrapers appeared; bone, horn, ivory was used; harpoons and fish-spears are found along the river banks, and there have been already discovered about 400 specimens of engraved animal bones, some of which are only ornaments while others are decorated implements, daggers, poignards, etc. It is coming to be somewhat fashionable in the United States to deny the authenticity of these works of Palæolithic art; to denounce them as frauds, declaring them to be too fine to have been the work of a savage. It is not my purpose on this occasion to enter into any defense thereof. When ever these charges shall take proper form and appear over responsible signatures in the scientific publications of this country, and be transmitted to France and England, their people. who are most interested and best acquainted with these objects, will be abudnantly able to make response thereto. Until that time, they will as I do—ignore all insinuations.

It has been announced that new discoveries made by some of our local archæologists, whose names were mentioned, had about demolished the Paleolithic age in Europe as well as in America. I dissent from this opinion, but it is not to be discussed here. When the proposition shall have been published, so that we may know exactly what is charged and what is to be combatted, then it can be turned over to the European pre-historic experts for them to defend their proposition, and no one will doubt their ability to do so. The seeker after knowledge may properly ask, how it can be known that these different stages of culture succeeded one another in the order named, and why they should be classed with the Palæolithic age. I can only upon this occasion state the facts which appeared satisfactory to the various investigators, without attempting to argue or prove them. In the alluvial period, the chellean epoch, these implements have been found in various parts of Europe by the ten thousand, and always without the slightest trace of the association with implements of polished stone. A single locality, it is agreed, would be little or no value, but when it comes to be repeated by the score of times in localities widely separated, belonging even to different countries, with never an exception, it has been admitted as satisfactory evidence that there was a Palæolithic age independent from the Neolithic. That it was earlier than the Neolithic seems to be established. The position in which the implements have been found, indicating their great age; the conditions under which they have been found, deep in the undisturbed gravels of the river valleys, and associated with the bones of extinct animals, which, in the opinion of the investigating geologists, proves that they belonged to a prior geologic period, the Quaternary, or Post-pliocene.

The progressive steps of culture and invention mentioned as belonging to the cavern period seem to have been satisfactorily established by investigation made in the caverns themselves, where in numerous instances the gradual filling up of the cavern has preserved the earlier occupation at the bottom, while the subsequent occupations have taken their respective places, each one above the other in their orders of time. For example, at Kent's Cavern, near Torquay, England, the caverns investigated with all possible care during a period of twelve or thirteen years, in which as many thousand dollars were expended, under the direction of a committee appointed by the British Association, where the strata of these early occupations were covered by layers of stalagmite spread over what was then the entire surface, separating and sealing it hermetically from subsequent occupation. Under it, in various parts ot the cavern, were found these same chipped flint implements, which have been denominated chellean, and beyond the chips and flakes possibly the hammers incident necessary for their fabrication. No other trace of human industry was found. In the Grotte de Placard, in southwestern France, the same super-position was found, which gave satisfactory evidence of this succession of human occupation and of the accompanying changes and improvements of human culture. The strata containing Neolithic and Palaeolithic objects are distinctly marked and are separated by a stratum entirely sterile so far as concerns archaeology, made up chiefly of broken stones from the roof of the cavern, several inches in thickness. The cavern of Laugerie Haute gives the same evidence and is even more positive, for the sterile stratum is about 4 feet 3 inches in thickness. In the Grotte de la Vache the stalagmitic stratum between the Palaeolithic and Neolithic industries is about 18 inches thick. The latest indications we have, occurred in the summer of 1892, when M. Boule was called from Paris to visit the pre-historic station cavern of Schweizersbild, near Schaffhausen, in the immediate neighborhood of the cavern of Thayingen, Thüringen, which gave the celebrated drawing, engraved on bone, of the reindeer browsing. M. Boule has just published a report of his investigations in the *Nouvelles Archives des Missions*, tome III, and he shows (pl. 3), the drawing which he has made of the debris left on the side of the cavern showing the superposed and consequently successive occupations and corresponding improvements in human invention and human culture.

The differences between the Palaeolithic and the Neolthic ages in Europe, the only place where it has been studied, are marked by differences in climate, geography, fauna, domesticity of animals, sociology and other things beside industry. Prof. Boyd Dawkins, "Early Man in Britain," page 265, says:

"The great changes in the fauna and geography of Great Britain, at the close of the Pleistocene age, rendered it very improbable that the cave men were in any way represented by the Neolithic tribes who are the first to appear in pre-historic Europe. The former possessed no domestic animals, just as the latter are not known to have been acquainted with any of the extinct species, with the exception of the Irish Elk. The former lived as hunters, unaided by the dog, in Britain, while it was part of the continent; the latter appear as farmers and herdsmen after it became an island. Their states of culture were wholly different. We might expect on *à priori* grounds that there would be an overlap, and that the former would have been absorbed into the mass of the new-comers. There is however no evidence of this. - - -

From the facts at present before us, we may conclude that they belong to two races of men, living in Europe in successive times, and separated from each other by an interval sufficiently great to allow of the above-mentioned changes taking place in the physical conditions of Britain." - - -

Sir John Evans, in "Ancient Stone Implements of Great Britain," page 618, says:

"There appears in Britain to have been a complete gap between the river drift and surface-stone periods (that is to say, the Palæolithic and Neolithic periods); so far as any intermediate forms of implements are concerned; and here at least, the race of men who fabricated the latest of the Palæolithic implements may have, and in all probability had, disappeared at an epoch remote from that when the country was again occupied by those who not only chipped out but polished their flint tools, and who were moreover associated with a mammalian fauna far nearer resembling that of the present day than that of the Quatenary times."

M. Gabriel de Mortillet, in "Le Prehistorique," page 479, discussing the difference between the Palæolithic and Neolithic periods, says the former belonged to the Quaternary geologic period while the latter belongs to the present or actual periods. "Between these two epochs there are differences everywhere; there exists a veritable revolution." And he puts these differences, one against the other, in the form of a table.

In the later epoch of the Palæolithic period the climate was cold and dry with extreme temperatures; while in the Neolithic period the climate was temperate and uniform.

In the Palæolithic period were living many great fossil animals like the cave bear, the giant beaver, and, most plentiful of all, the mammoth; in the Neolithic period all these were extinct. Out of 48 well-ascertained species living in the Palæolithic period in France, and England, only 31 were continued in the Neolithic period.

Of the animals living in the center of Europe on the plains, and associated with man in the Palæolithic period, no less than 18 were cold-loving. In the Neolithic period, 13 of them, such as the reindeer, antelope, musk ox, blue fox and white bear, emigrated to cold countries by latitude; while five, the chamois, marmot, wild goat, and others have emigrated to cold countries by altitude, going up the mountains.

In the Palæolithic period there were no domestic animals. In the Neolithic period they were abundant.

In the Palæolithic period. the population was nomadic; they were hunters and fishers. but not agriculturists. In the Neolithic period the population was sedentary, and agriculture was well developed.

In the Palæolithic period there was practically no pottery in France and England; in Belgium there have been two localities where pottery has been found.

In the Palæolithic period there were no monuments of burials, and apparently no respect for the dead. In the Neolithic period there were many and great monuments, dolmens, and menhirs of great size, with elaborate burials.

There is in the Palæolithic period nothing to show that man had any idea of religion or a future state; in the Neolithic period these sentiments and ideas were well developed.

In the Palæolithic period man has an artistic sentiment; in the Neolithic period he apparently had none.

So it appears that the revolution and contrast between the two periods is at once physical and industrial, natural and social. The changes in climate suggest changes of equal importance in orography and geography which must have been accompanied by profound geologic modification. All these changes in man's civilization, his surroundings and environments, took place between the Palæolithic and the Neolithic periods, and this in addition to the marked change in his industry from chipped to polished stone. Thus it will be seen that the latter difference is but slight, and only one out of a dozen, which equalled if it did not exceed it in importance and effect.

Sir John Evans, in "Ancient Stone Implements of Great Britain," page 618, says:

"The antiquity then that must be assigned to the implements in the highest beds of river drift may be represented (1) by the period requisite for the excavation of the valleys to their present depth; plus (2) the period necessary for the dying out and immigration of a large part of the quarternary or post-glacial fauna, and the coming on of the prehistoric; plus (3) the polished stone period; plus (4) the bronze, iron, and historic periods, which three latter in this country occupy a space of probably not less than three thousand years. A single equation involving so many unknown quantities is, as already observed, not susceptible of solution."

I resume the discussion of the existence of the Palæolithic age in the United States. There have been found in the Trenton gravels, numbers of rudely chipped implements of argillite which have been called Palæolithic. They were originally discovered by Dr. Abbott, who resided at Trenton and who has been interested in pre-historic archæology, and was employed by the Peabody Museum, and who for many years has been devoted to the pursuit of evidence of early man in the Delaware Valley. He is now curator of the museum of archæology in the University of Pennsylvania. Dr. Abbott, like M. Boucher de Perthes, was subjected to much investigation and had to stand under the light of fierce criticism from the opponents of his theory. Dr. Abbott's character or ability as an archæologist, a naturalist, or an observer, is not at issue at the present moment. No person can now deny the fact that he believes that he has found a number of these implements deeply inbedded in the original gravel deposit of the Delaware River at Trenton; the implements found at Trenton and otherwheres in the United States have the same general appearance of those heretofore shown from other parts of the world. In addition to my own testimony on this subject, I may add the testimony of M. Boule, a noted French geologist and student of pre-historic man, on the same subject, which has appeared in the last number of *Anthropologie*, vol. IV, pp. 36, 37:

"During my voyage in the United States in 1891, on the occasion of the International Congress of Geology, at Washington, I was able to see some of the chipped stones of Trenton, in the collection of prehistoric archæology at the Smithsonian Institution, and in the Peabody Museum. I could there study at leisure the collections of Dr. Abbott. That which struck me most forcibly was the similitude, I may say almost identity, of the form of the American instruments with the European palæolithic implement. At Trenton, as at Amiens, Paris, in the collections of Dr. Abbott as of those of M. d'Acy, there is, along with a certain number of chips and unformed pieces, also a number of finished pieces showing careful work, and which could not be 'rejects' of fabrication. The most careful and most competent archæologist of our country will be unable to distinguish otherwise than by the nature of the material the difference between the instruments of Trenton (as well as of other parts of the United States) from the pre-historic implements of Europe. There is, in this fact, an argument in favor of the antiquity of these specimens which will impress pre-historic archæologists of experience."

The fact that other gentlemen entitled to equal credit for accuracy as observers have sought at Trenton for these implements in 1893 and failed to find any, is no evidence that Dr. Abbott may not have found them there from 1876 to 1890. The gravel at Trenton speads over and fills up a saucer-like depression about three miles in diameter, and from 35 to 42 feet in depth in the center. That these gentlemen should have sought with all care and closeness these gravels in the great sewer which has been lately laid through the city of Trenton near the river, and have found none of these implements, is no evidence that Dr. Abbott may not have found them among the acres of gravels 10 to 30 or more feet in thickness that have been dug out a mile away from the aforesaid sewer by the Pennsylvania Railroad, during a period of ten or fifteen years past, to obtain gravel for its road ballast.

Illustrative of my proposition, I may cite the depot of Chelles, near Paris, where thousands of these implements have been found. It is an immense gravel bank, much the same as at Trenton, 20 or 30 feet thick, extending over an area of a hundred or more acres in the valley of the river Marne. It is located on a railroad, and was used as was the gravel at Trenton, having been dug out and transported as railroad ballast. I visited this station on the excursion of the International Pre-historic Congress at Paris in 1889, and there listened to an acrimonious discussion as to the precise locality in which the respective kinds of implement had been found, as for example what kinds were found at the top, and what kind at the bottom of the deposit; and it was there made apparent notwithstanding that the ten thousand implements obtained from that depot, the principal disputants, the leaders of opposing schools, those who had devoted their utmost time, care, and attention, to the investigation of these implements and the theory of antiquity and civilization to be based thereon, none of them had ever found these implements in place. M. Boule, himself a noted geologist, a close observer, and an ardent investigator, interested in this branch of study, makes the same declaration in the last number of the *Anthropologist*. As it is made since his visit to the United States, and bearing upon this discussion, I may be permitted to quote his opinion as to the want of value in the objection made that other persons than Dr. Abbott have not found these implements when they sought them in the Trenton gravels. M. Boule says, in the last number of *l'Anthropologie*, vol. IV, p. 38, in reporting his visit to the United States during the last international geologic congress:

"I did not myself find any of these chipped stone implements during my excursion to the gravel pit at Trenton, but there is a similar locality in the neighborhood of Paris, very rich in implements—in Chelles for example—where I have been many times and my searches have always been infructuous ; but the deposit of gravel presents entirely the same topographic and stratigraphic disposition of the paleolithic alluvium of the north of France and south of England."

This proposition will be better understood when the conditions are once explained. The depot at Chelles is in the neighborhood of 100 acres area, 44,000 square feet to an acre, 100 times that to 100 acres in surface measure. If the gravel bank be 20 feet deep, it would be twenty times that number, 88,000,000 cubic feet of gravel. I have said 1,000 implements; there may have been 10,000 such implements found at Chelles, which, scattered among 88,000,000 cubic feet of gravel, will give an average of one implement to every 8,800 cubic feet of gravel. Sometimes they are bunched so that one may find a dozen in a single pocket, or a hundred in a single day, but this only decreases the chances of finding them within any specified time. This explains M. Boule's statement that a man may stay there and watch the diggers for a week without finding a single implement—this too in a gravel bank which furnished 10,000 implements. I do not give these figures as exact. They are only to serve as illustrations. I do not know that there were just 100 acres, and I only speak from remembrance of its appearance when I estimate its depth at 20 feet, and I only estimate 10,000 implements as having been found there.

M. de Mortillet has made a similar estimate with regard to St. Acheul, that other great depot of Palæolithic implements which furnished a greater number probably than any other in the world; and he has shown that the dissemination of the implements through these gravels rendered it very unlikely that any person could find an implement in any given length of time. On the other hand, Dr. Capitan discovered a deposit in the southwestern part of France during the summer of 1892, of which he said he found an implement or a bit of worked flint every five minutes. It was quite different from this in the workshop of Bois de Rocher in Brittany, discovered by MM. Micault and Fornier. That was a workshop, and the implements were found all together and in a few days' or hours' excavation. Consider the comparative scarcity of these implements in the Chelles and St. Acheul gravel banks, and the comparative scarcity of these implements in the gravel deposits at Trenton will not appear strange, nor will the fact that gentlemen spend weeks or even months in the search through these gravels in what proved a vain attempt to find Palæolithic implements be evidence against their existence. Imagine the gravel bank adjoining the Pennsylvania Railroad depot at Trenton, extending by estimate, eastward half a mile, a quarter of a mile in width, the gravel 20 or even 30 feet in thickness, dug down and thrown into cars upon temporary tracks, which are moved each day or each week close into the bank—imagine, I say, this great mass of gravel, amounting to millions of cubic feet, with the number of Palæolithic implements said to have been found by Dr. Abbott, I care not whether we take the smallest number, 40, or the largest number 400 or 500, scatter them through this pile of gravel, and then consider what would be the chance of a person finding one of these, I care not what his ability as an observer, how ubiquitous he was, nor with what attention and zeal he followed the shovel of the diggers and inspected the fine gravel they threw out. I only repeat the sole conclusion intended by this line of argument—that it is no proof these implements do not exist in these gravels that other gentlemen have sought for and failed to find them; while Dr. Abbott, who has lived in the neighborhood all his life, has been engaged in the search for twenty years or more, has invoked the aid and enlisted the co-operation of his neighbors, the diggers, and the public in general, and during all that time has found only the number suggested, I care not whether it be 40 or 400. No attempt has been made by anyone to impeach the veracity of Dr. Abbott in this matter. We must accept his statement as to the finding of the implements. The conclusions to be drawn from his facts are fair subjests for argument, and I would not pretend because we must follow Dr. Abbott's facts, that therefore we must necessarily adopt his conclusions.

It may be said that in this matter Dr. Abbott has been deceived; that the implements to which he has attributed this antiquity have been fabricated, imposed upon him as genuine, when they might have been made by the workmen with intent to deceive. This has occurred in other places. M. Boucher de Perthes himself was sadly deceived in several cases. One proof of the antiquity of stone implements, or of some of them, is by the *patina* or weathering shown on the exposed surface. Any of the argillite implements from the Trenton gravels may be broken and thus show the difference of color between the inside and the outside. On the outside it is a dull gray, on the inside it is shining black; the black color is the natural appearance of the stone; this is shown when first chipped; the gray appearance is from the weathering, and it has been made by long exposure.

Evidences of the primitive industry of man have been found in many other places of the United States besides Trenton. At Loveland and Madison, Ohio, by Dr. Metz; at Newcomerstown, Ohio, by Mr. Mills; at Fedora, Ind., by Dr. Cresson, and at Little Falls, Minn., by Miss Babbitt. All of these localities have been attacked in late publications by disbelievers in the existence of Palaeolithic Man. In order that I may be fair in argument, and accept fully the facts according as they are found by the observer, it must be conceded that the evidence of palaeolithic occupation at Little Falls has been successfully assailed by the investigations of last summer made there by Mr. Holmes. I have upon another occasion complimented Mr. Holmes upon the system, thoroughness of his investigations there; and his conclusions, so far as they are based upon those investigations, must stand until some subsequent investigator, going over the same ground, shall change the facts. I may have disagreed with Mr. Holmes in his conclusions, but I concede his facts must stand.

Some years ago I made an appeal in the form of a circular from the Smithsonian Institution, asking for information, which was scattered throughout the United States, concerning these objects, and I accompanied this with cuts and engravings of similar objects, some from Europe and others from America. I received responses from nearly every State in the United States, and many States responded with great numbers. I do not propose to follow the result of this investigation in all its details, but to say that there was reported to the Smithsonian Institution a large number of implements similar in every regard to those found in the gravels at Trenton and other places, and to those from Western Europe. Many of those reported were not Palaeolithic—did not resemble Palaeolithic implements—many of them were but chips and rude flakes—some objects were manifestly Neolithic; but omit all these, still there was a considerable number of implements, representing practically every State in the United States, which would correspond in every particular (save in some cases material), with those from Europe. These identical implements, had they been found in western Europe, and presented before any committee of the best archaeologists, they would be pronounced Palaeolithic. In this connection, I refer again to the quotation made a little time ago from M. Boule, wherein he states the same thing. But these implements were not found in Europe, and their value as evidence of Pre-historic Man in the United States has been disputed.

You will ask what is my conclusion with regard to this matter. I conclude that this similarity of such vast numbers of these implements from two continents and representing widely separated peoples is, as M. Boule has said, "an argument in favor of their antiquity which will greatly impress pre-historic archæologists of experience." It is to be taken as serious evidence in favor of Palæolithic Man in America, as it has proved him to have existed in Europe. But it is only a single step in the ladder of pre-historic science; and is to be treated more as a working hypothesis calculated to direct attention and stimulate investigation. My conclusion is not announced dogmatically, nor will it be defended at all hazards. It is expressed under all reserve, and subject to future discoveries. It will have served a good purpose if it shall promote the search of the river valleys for these implements, cause them to be gathered and saved as of value to science, to note well their associations with other subjects to be noted, and to discover their material and if possible the original deposit and the place of their fabrication. By these means we may hope to arrive at the truth concerning these implements and their relation to Pre-historic Man.

PREHISTORIC NEW MEXICAN POTTERY.

By Henry Hales.

In January, 1889, I received some ancient pottery from a friend in New Mexico. I was surprised to find that so little was known of this description of ware among collectors. Not a single piece was seen in the New York museums. I met Prof. Frederick Starr in the Museum of Natural History, who took much interest in the ware. Referring to several works on American antiquities I found fragments of this ware illustrated in John R. Bartlett's personal narrative as boundary commissioner of the United States expedition, 1850 to 1853, which he found in the Gila Valley. Although so many fragments had been found, the whole ware had been seldom seen. All the information I could get about it was very meager. That it was found in the valleys and deep in the ground, accompanied by skeletons, also that much was broken in getting it out, was all I could hear about it. With these few hints I determined to go to New Mexico to observe for myself, and gain information regarding this peculiar pottery. My route was by the Rio Grande Railroad in New Mexico to Socorro, west to Magdalena. I then struck across the San Augustine plains to the Tule Rosa Canyon, which runs west to Arizona, near the heads of the Frisco and Gila rivers.

I found the ruins commenced in Tule Rosa Canyon, which I followed for 20 miles, and in two of its branches. All that can be seen to indicate ruins is a few loose stones on the surface of the ground, scarcely recognizable, which probably accounts for their lying so long undisturbed. In most cases I found them where the canyons opened into little valleys, with a brook, or, more often, what had once been one, but now a dry gully. Between the alluvial levels of these water courses and the foot of the mesas or mountains, there is generally a slightly rising ground, which appears to be out of danger of floods: these were the selected spots for building. The earth is not as rich as the lower levels, and is composed of clay, a little sand, and some broken stone, with a growth in patches, of piñon pine, junipers, a species of small walnut, and, in moist places, very large, tall pines, varieties of cacti and yuccas.

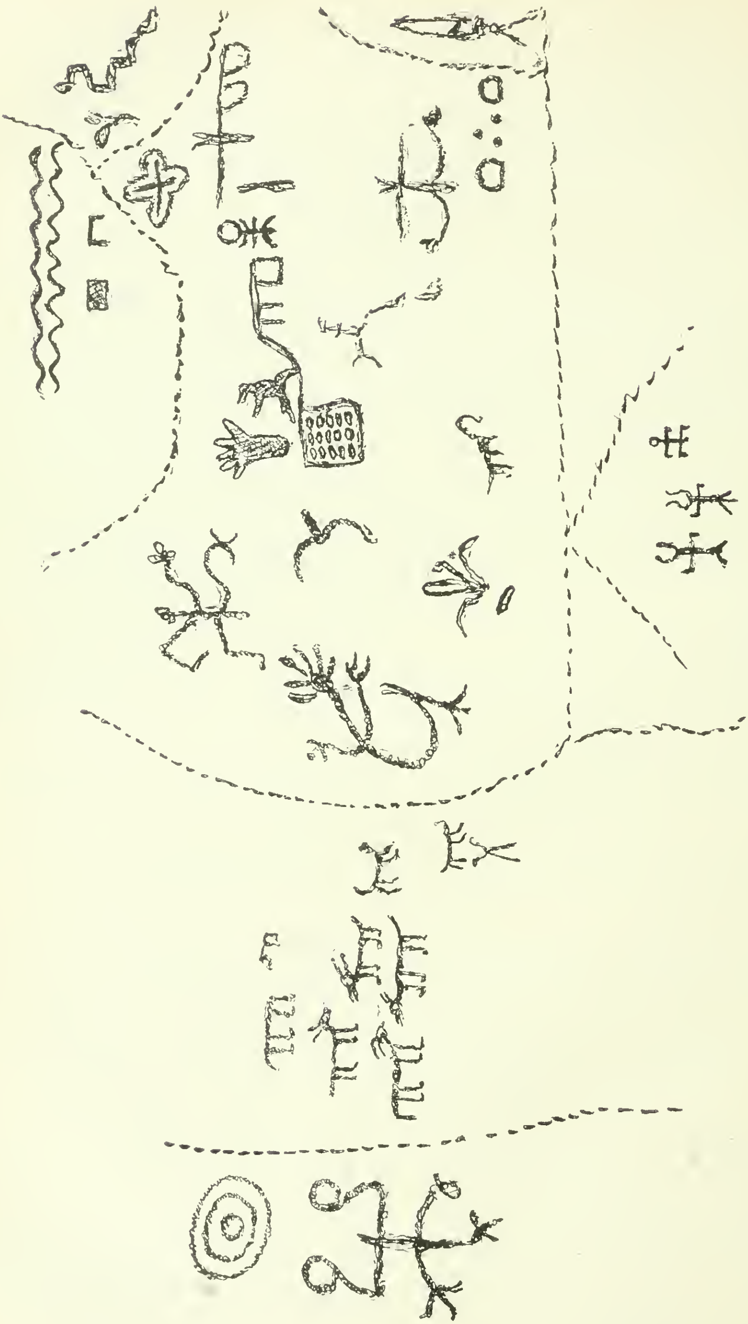


Fig. 1.

I found in some rooms very large old dead junipers that were larger than the surrounding trees. Some of the ruins are miles from any water. They are scattered at short intervals of a hundred yards to a mile or more apart in different directions, as the ground lies favorable, and at times on higher rising ground. From these conditions it would appear that the population was at one time numerous in these valleys. I found some extensive ruins on a branch canyon a long distance from water. The ruins I excavated in were 60 feet, plainly traceable, with appearances of extending 100 feet further, by 117 feet. The 60 feet was divided into the widths of three rooms—the first 16 by 20 feet, the second 18 by 24, and the third 15 by 18, which, with the width of walls 2 to 3 feet, made the distance. There were three other rooms which I could trace, adjoining the ends of these rooms, as seen in Fig. 1, and a small middle room 8 by 8 feet, but the walls were not accurately laid bare. The outside walls were laid up with roughly hewed stone, worked into squares of about 14 inches and about 3 inches thick. Some of the partition walls were laid with uncut bowlder stones. The walls were laid in clay cement smoothly plastered inside; most of the loose stone on the surface was uncut stone. The depth of walls was from 5 to 8 feet, with clay floors at the bottom of rooms. In rooms 16 by 20, on the outside wall, were two openings, one apparently each for door and window; they were blocked up with rough stone laid without cement. This would make it appear that the floors of the rooms were once about the level of the earth outside. Below these floors, and close to or under the foundations, were skeletons of adults, but so far decomposed that only the large bones and skulls were generally traceable; very few of these can be exhumed whole. Nearly all the teeth are very sound. I found in one room two skeletons in a doubled position, partially under the foundations, as shown in Fig. 2. There was a hearth made of four long pieces of square-dressed stone forming the frame, filled up with cement in the middle. Under this hearth I found the skeletons of two children. There were pots about the heads of the adults. Under the chin of one I found eleven shell rings and a turquoise bead. All the skeletons are not accompanied with pots; some have nothing with them, while others have several pieces; some contain bead necklaces, charred corn, beans, pumpkin seeds, fragments of woven fabrics, cord, braids, and human hair, etc. Also bone implements in a perfect state of preservation are found near the human bones that are so decomposed. The pottery consists of several kinds; there are the coil pots, as are found in mounds and cliff dwellings, but many are of a much finer ware. A red, smooth, glossy ware without ornamentation, all of a bottle or vase form. But the chief interest centers in the white or rather light gray and black ware, finely decorated and glazed. The designs are unique, both in form (which is various), and the style of figure in decorating, much of which is like steps in endless variety of changes, curves, and lines in a maze-like intricacy in some, with geometrical figures in others, but the equally well-distributed balance of color and forms mark this peculiar pottery. It is of a rather soft majolica-like material in body, while the glazed surface is hard and brittle, but it is thin and light in weight. Another ware, red and black, is found in the form of bowls and pitchers; this differs somewhat from the white and black, and is very fragile.

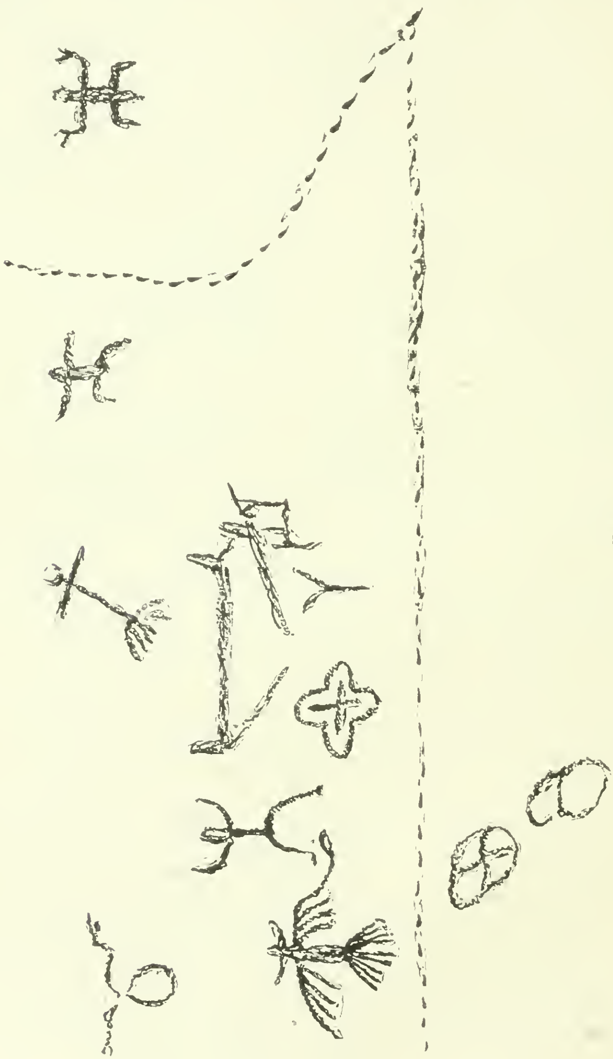


Fig. 2.

I have seen no whole specimens, but some are easily repaired and become harder after being exposed to the air for a time. A great many bowls are found of a drab or yellowish-brown color, smooth inside, with plumbago worked into the clay and made very smooth. These are often found blackened outside from fire, and inside a black charcoal-like dust. These were probably cooking utensils. Stone implements are quite

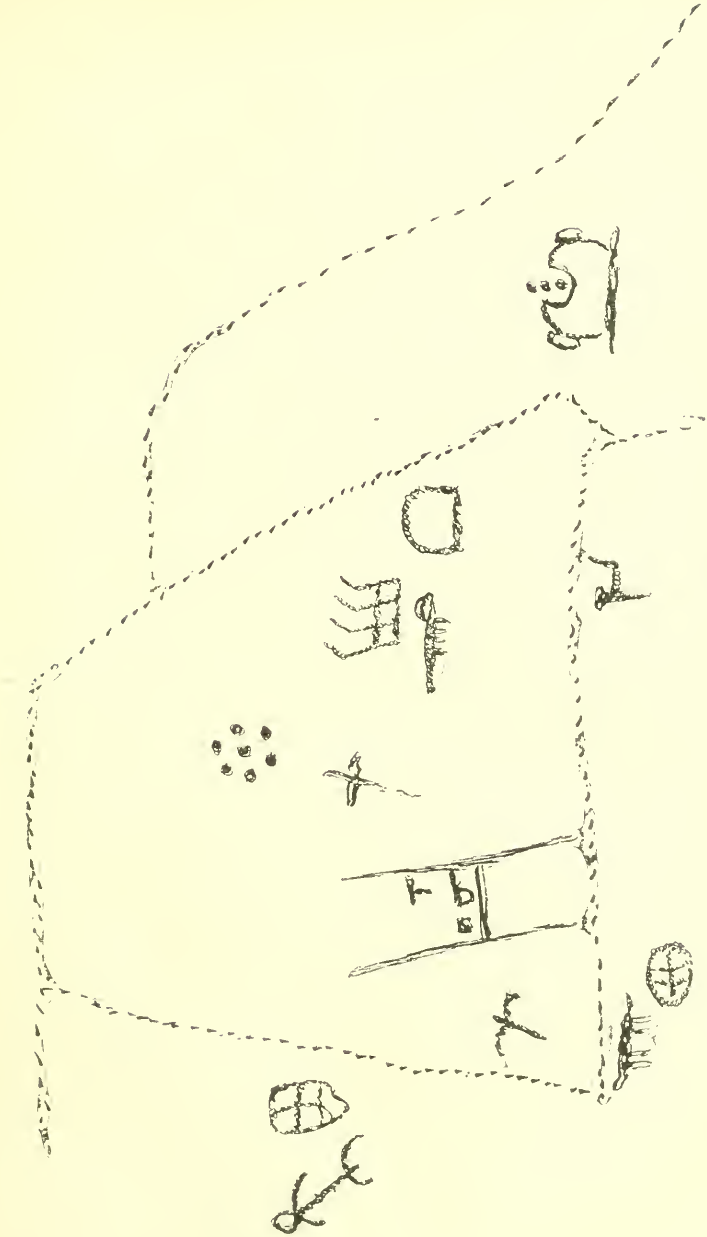


Fig.

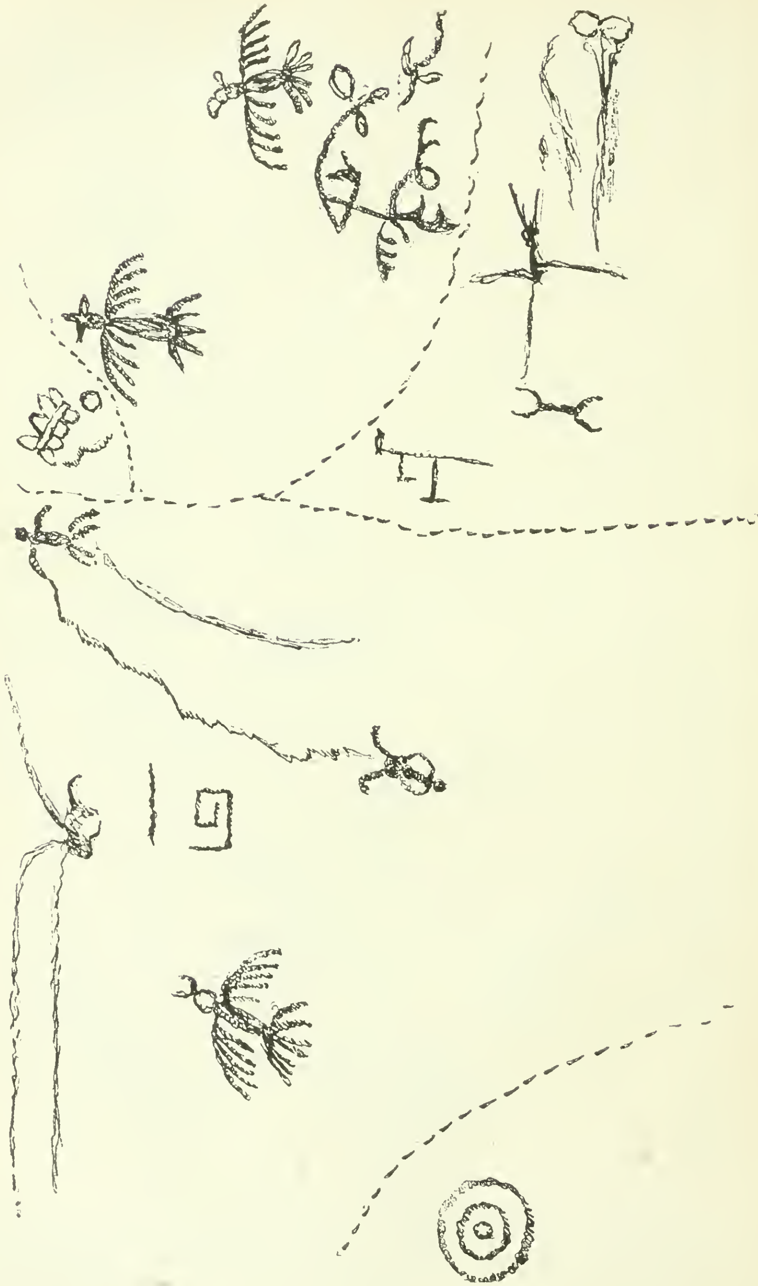


Fig. 4.

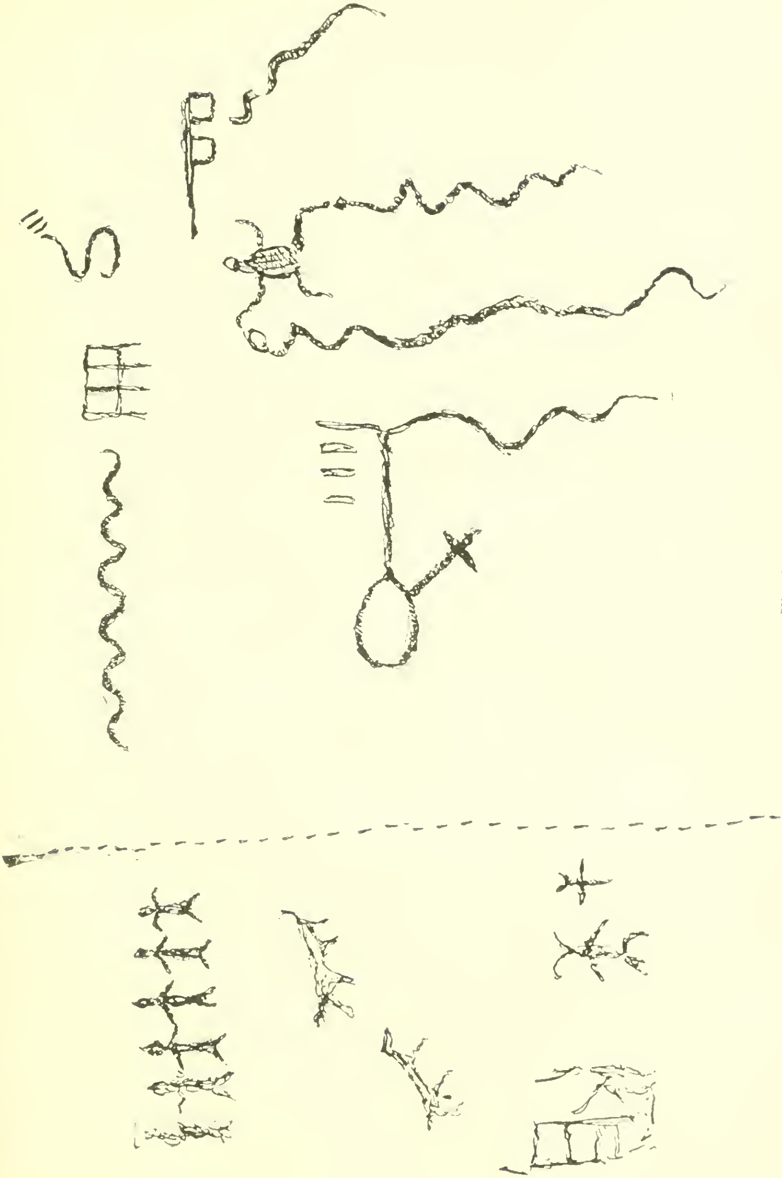


Fig. 5.

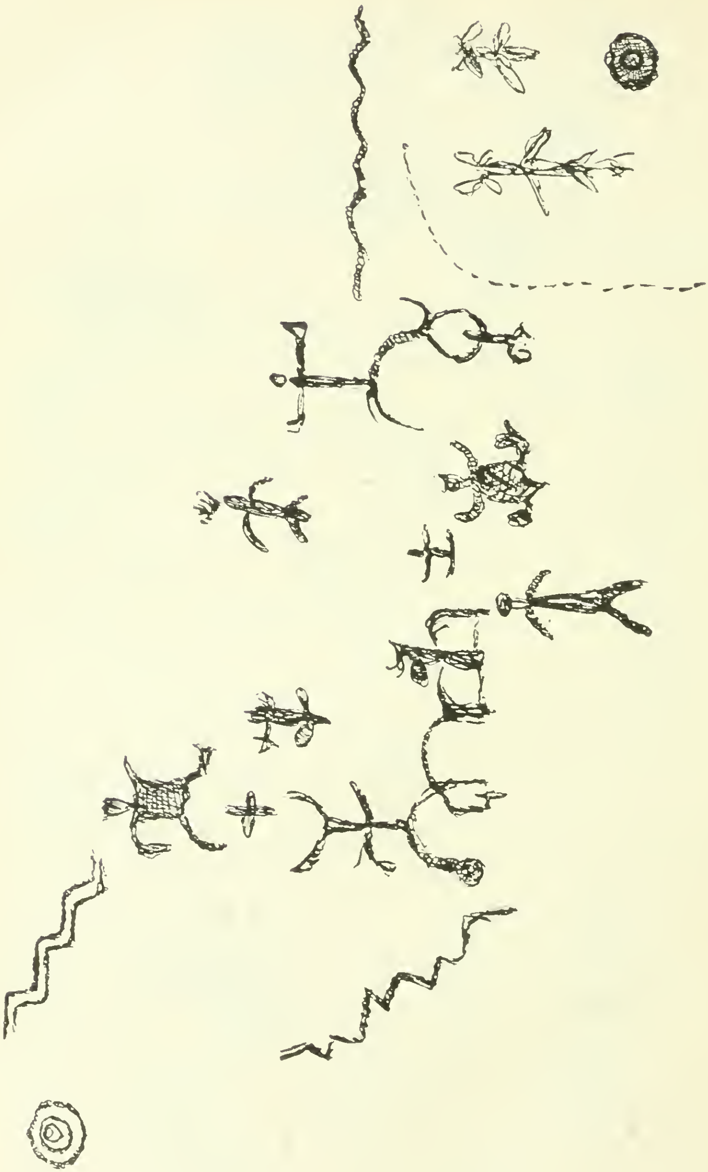


Fig. 6

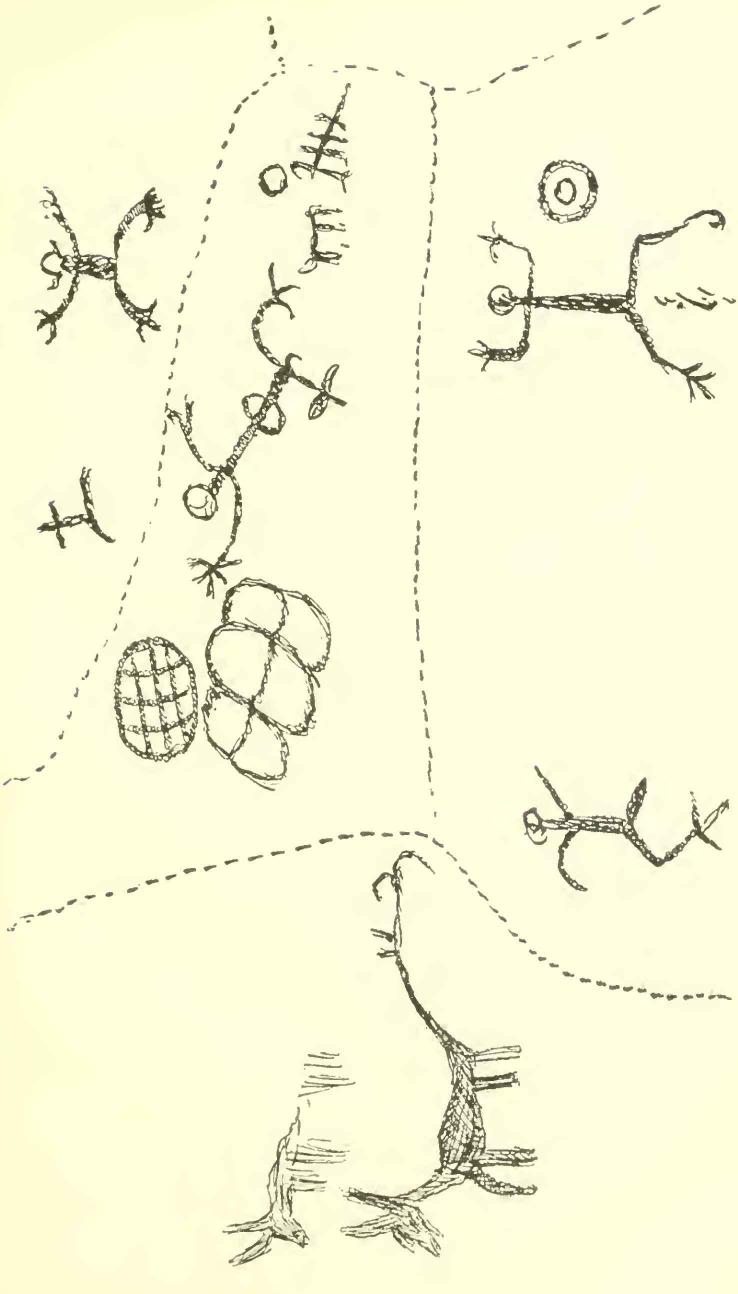


Fig. 7.

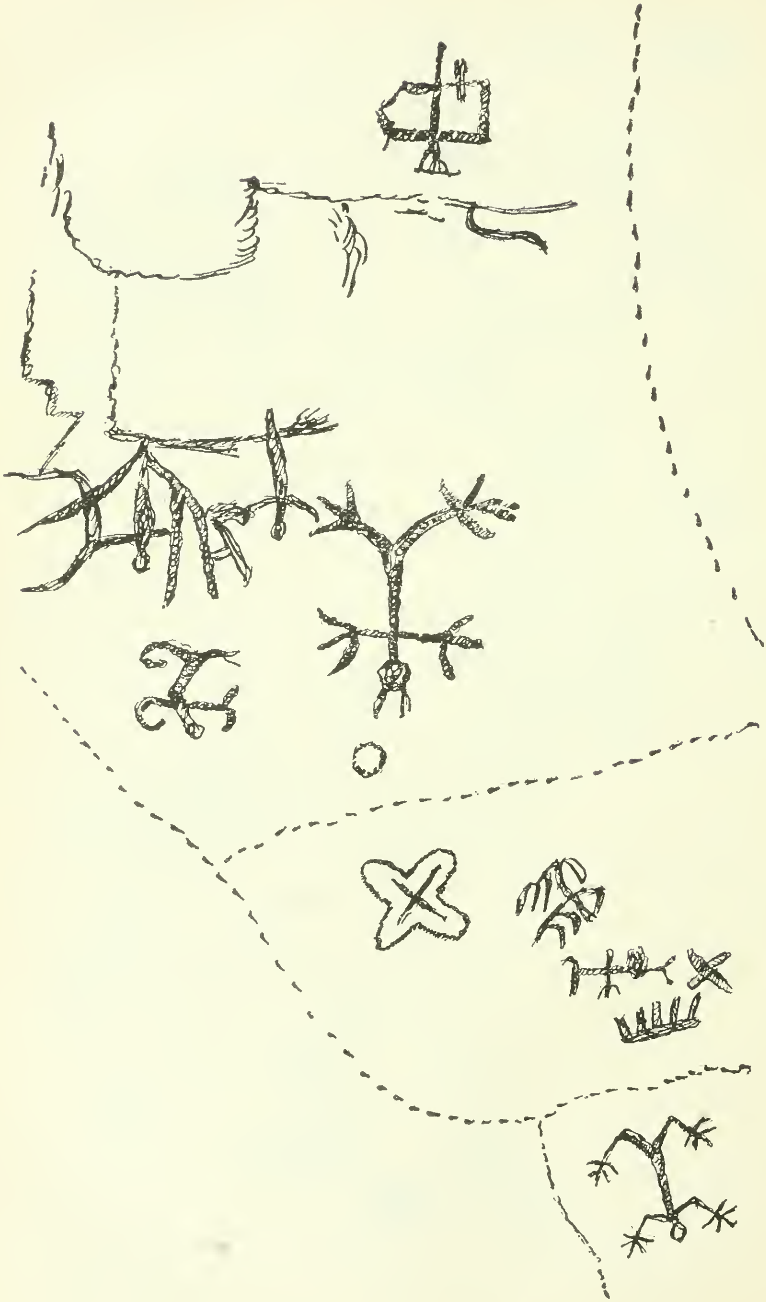


Fig. 8.

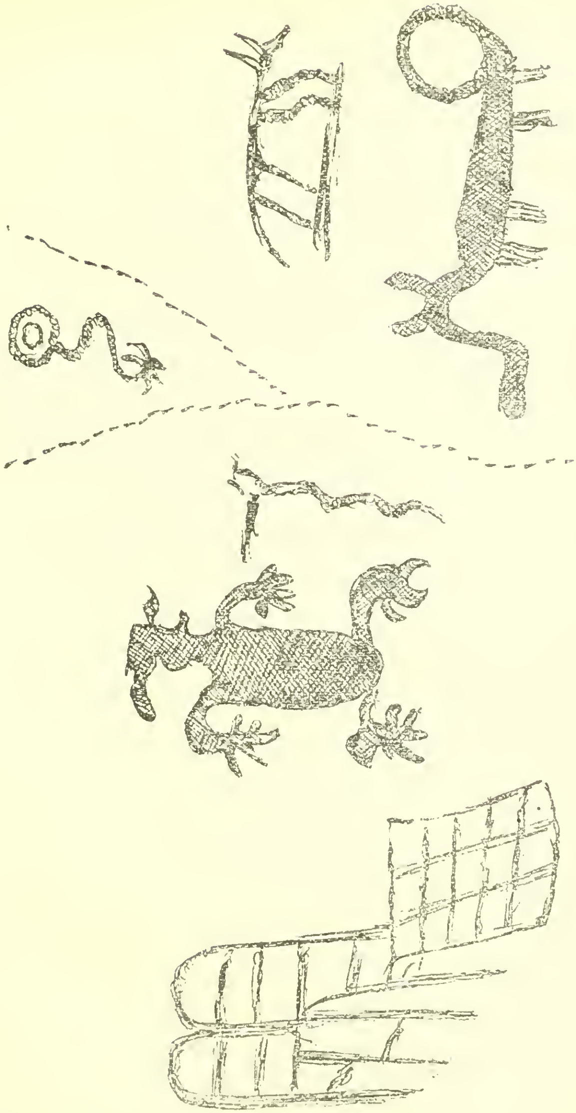


Fig. 9.

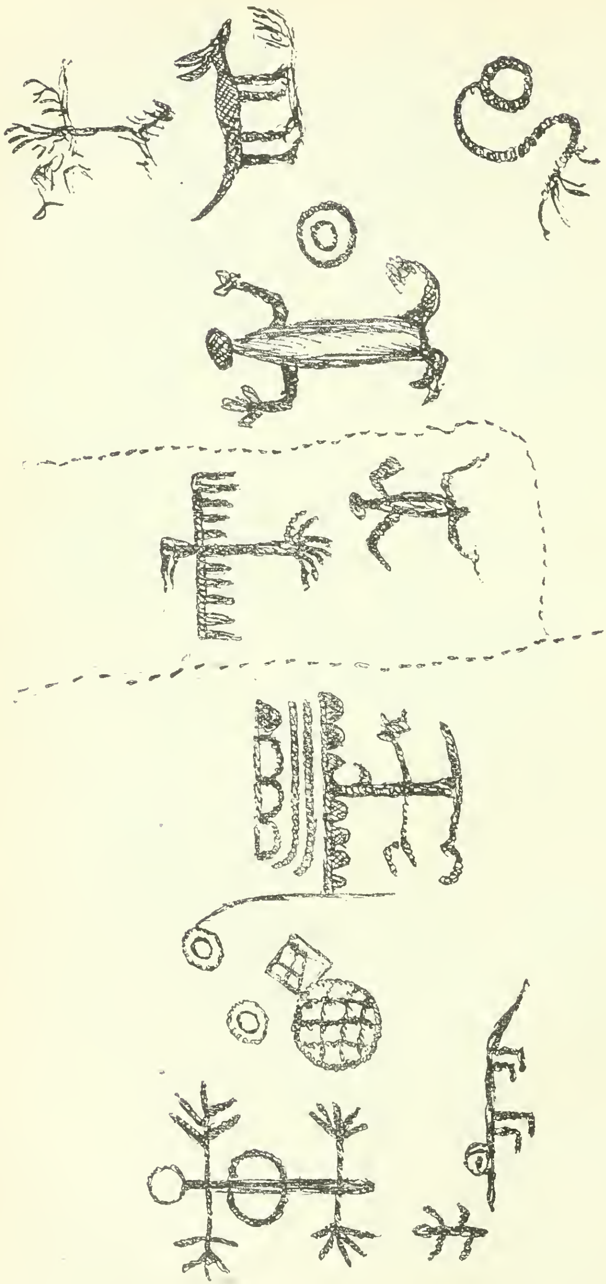


Fig. 10.

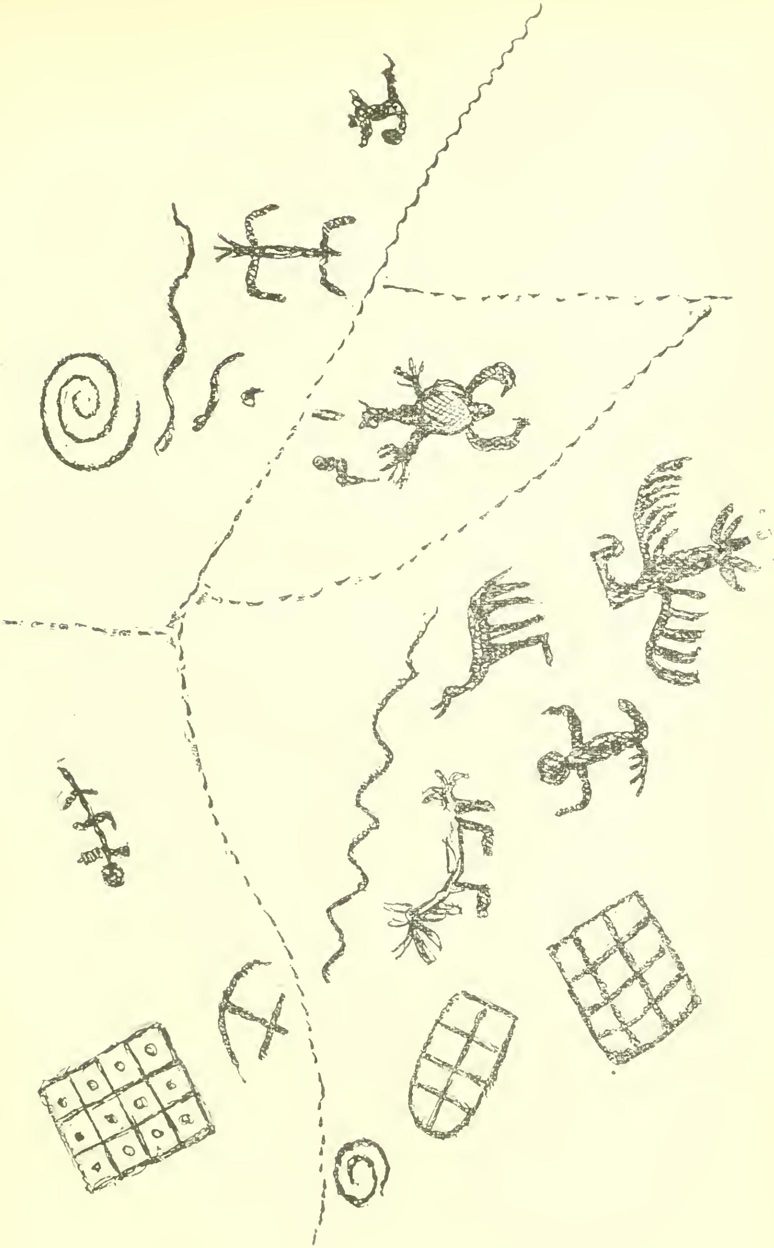


Fig. 11.

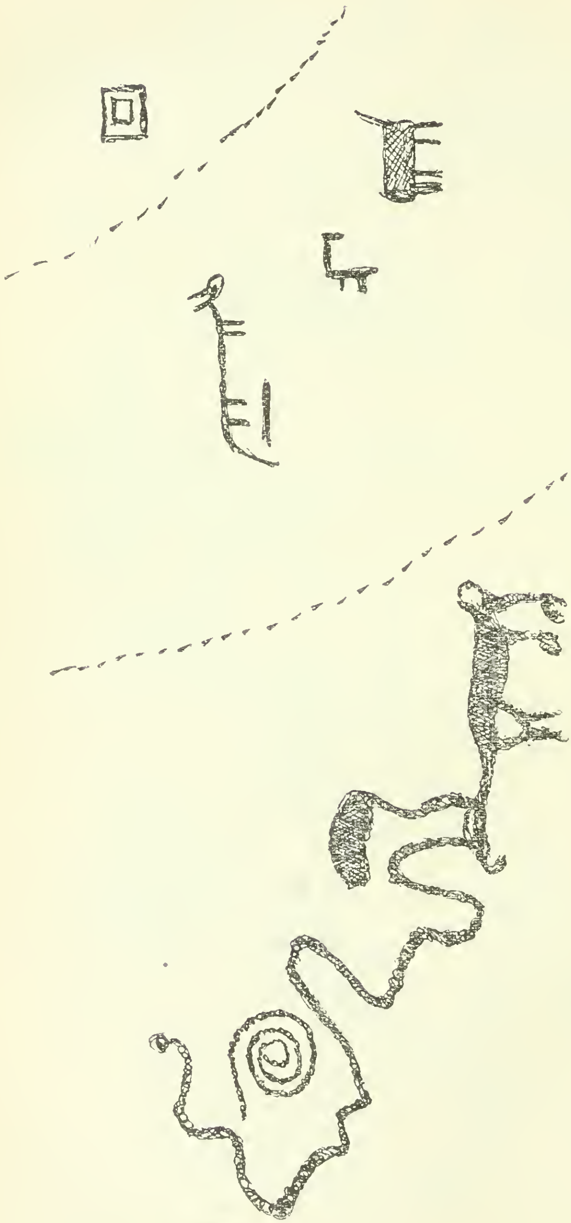


Fig. 12.

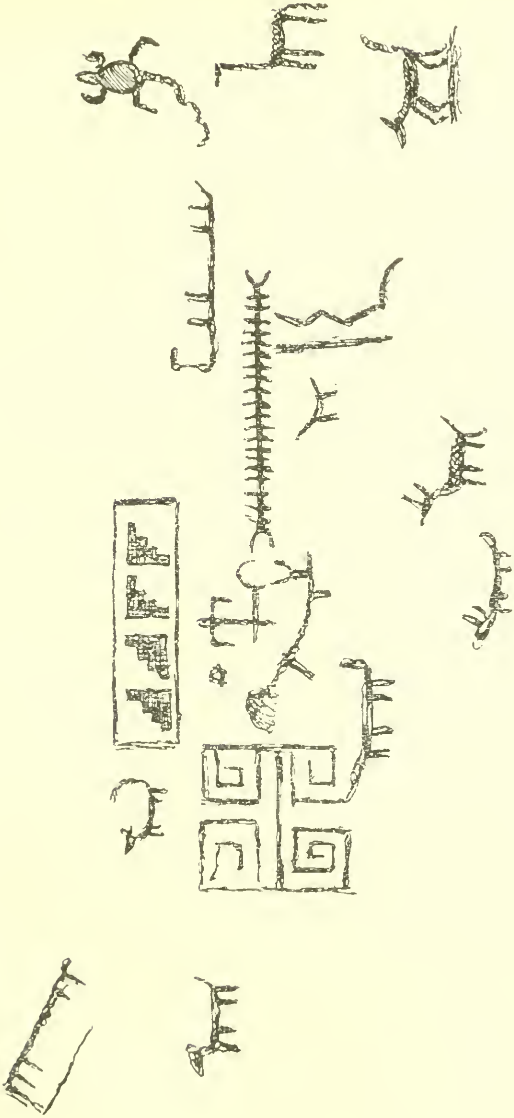


Fig. 13.

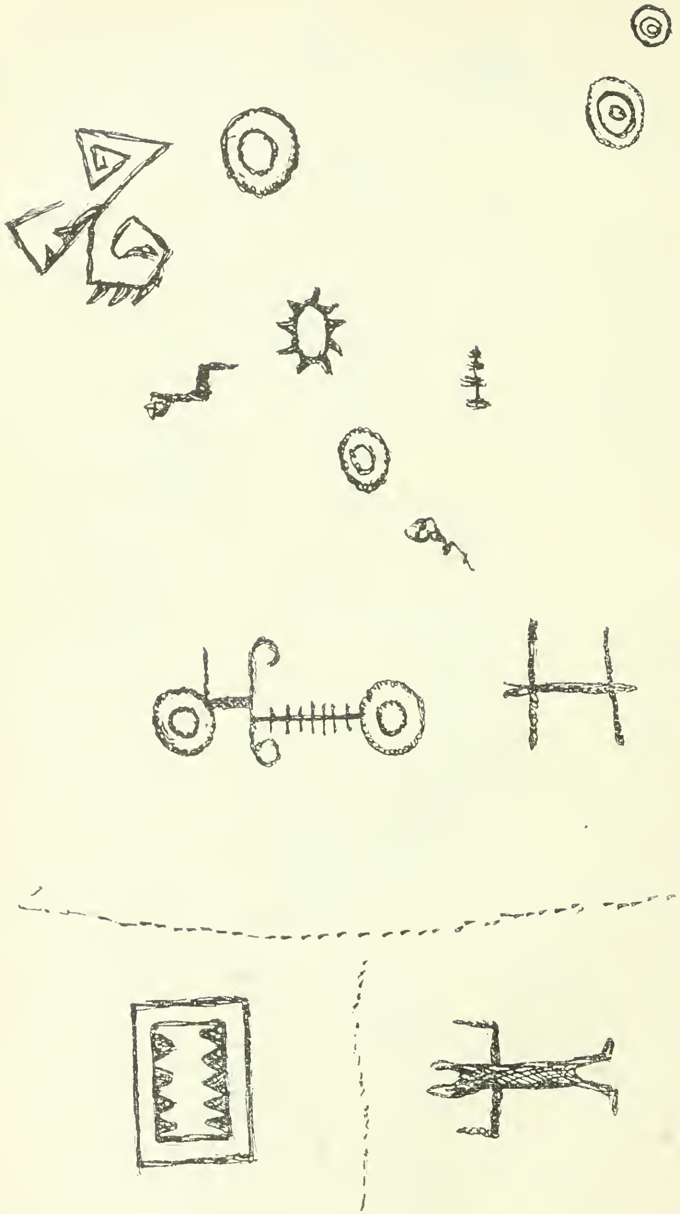


Fig. 14

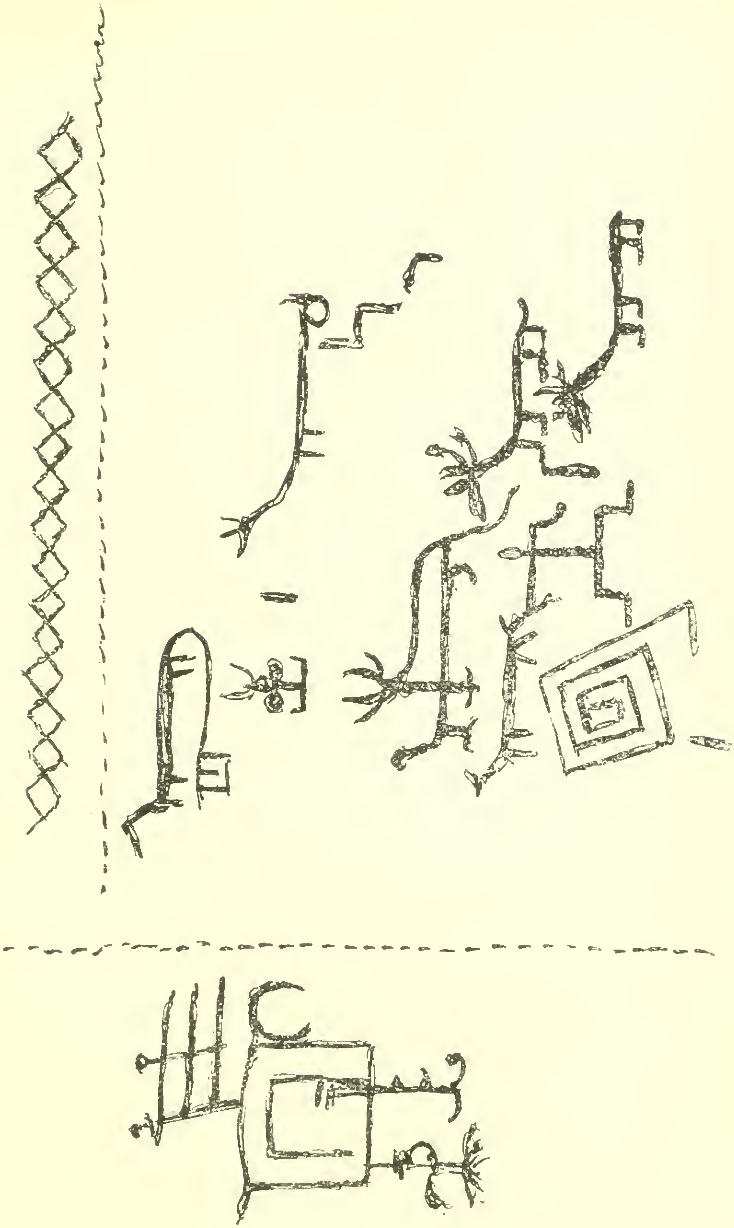


Fig. 15.

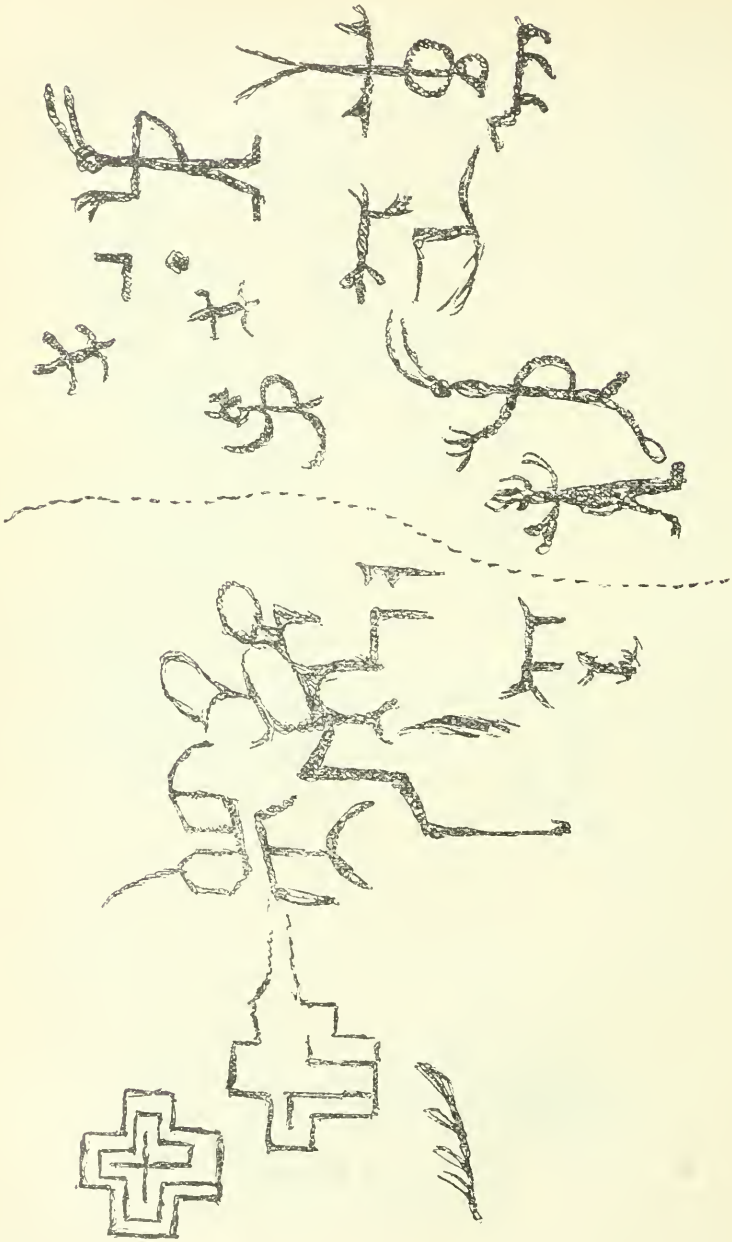


Fig. 16.

plentiful,—metatas, mortars, axes, hammers, and most of the usual articles of the kind, except spear-heads, or war implements, which I did not see. The little obsidian arrow points, very fine and small, are found all through the soil; I saw some crude smoking pipes, and two lengths

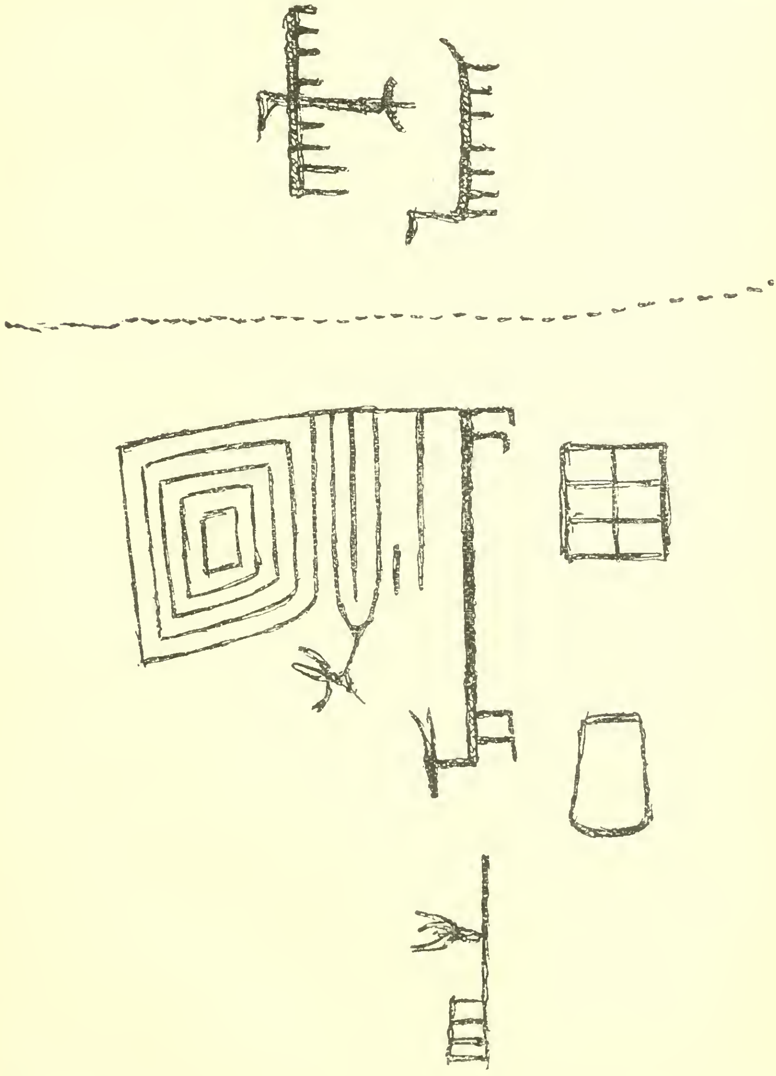


Fig. 17.

of what appeared to be water pipes; also a bell of bronze similar to Fig. 44, Sixth Annual Report of Bureau of Ethnology, but having the shoulder more like the triplet bells, Fig. 43. It is 3 inches long and 2 broad. I also saw some finely carved small figures of animals not over an inch long, of a greenish stone, resembling jade. Some pebbles I found were polished on one or more sides. A few were of black, fine-grained stone, which the Mexicans called moist stones, for after holding them in the warm hand and using a little friction they feel moist. In some of the rooms charred parts of beams of pine were found; all the raw wood had undoubtedly rotted away. Many were the speculations of the inhabitants I heard expressed about the race of people, and why they left, none of which gave any solution to the difficulty. It is evident that only a very few bodies were buried under the houses, or they would be much more numerous after a long continued occupation. It seems a great mystery how these buildings became filled up and have fragments of pottery through the earth from top to bottom. The earth in most localities is quite hard, and can be removed only by a pick-ax. If an archæologist had an entire building cleared out and laid bare inside and out, a better solution might be arrived at.

RELICS OF AN INDIAN HUNTING GROUND,
IN YORK COUNTY, PA.

BY ATREUS WANNER.

York County is assumed to have been only occasionally visited by Indians and reputed to be comparatively barren of relics. In a recently published history of the county, it is said that—

"It [York County] was, as it appears from the Indian complaints, preceding its settlement, a hunting ground, or on the way to hunting grounds, nearly all woods, and claimed by the Indians to have been expressly reserved for them by William Penn. The original settlers here found immense tracts of land entirely denuded of timber by the annual fires kindled by the Indians for the purpose of improving their hunting grounds."

Such a statement concerning the Indian occupation, without calling into question its accuracy, is too general and vague. It conveys to us no conception of the extent, character, location, and number of Indian settlements in the territory.

Desirous of learning more about these aboriginal settlements, the author selected a limited area of the county and then proceeded to carefully search the ground. The object was to ascertain just what evidences of Indian occupation could yet be found strewn over the fields, many of which have been cultivated for more than a hundred years. That the search was well rewarded is proven by the number and variety of specimens collected.

The region selected extends along the Codorus Creek, having a breadth of two miles, and a length of six, with the city of York, (York County, Pa.,) in its center. The area thus located reaches from the forks of the creek, above the city, to two high hills, between which the Codorus flows. The surface of the land is undulatory and well watered by numerous runs. It is now a thickly settled and highly cultivated part of the county. All the relics described in this paper have been found along the Codorus and its tributary runs, since 1882.

All the implements herein described were collected from the surface in various fields.

Leaf-shaped implements.—Whether the specimens illustrated were used as lanceheads or not is, of course, mere speculation. In addition to "the absence of a notched or stemmed base or both," by which Dr. Abbott separates lance from spear-heads, these specimens are of comparatively great thickness. The accompanying illustrations will more clearly serve to point out the difference between lance-heads and spear-heads, whether such differences are enough to warrant the inference that they were used for different purposes or not.

LANCE-HEADS.—(Half-size.)

1 2 3 4

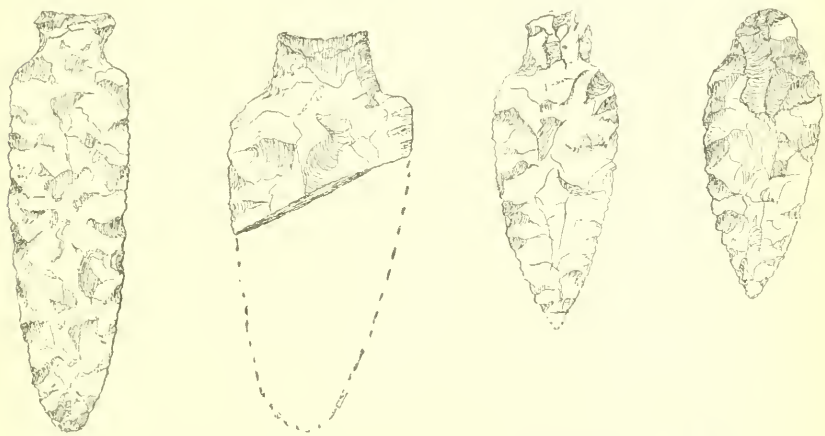
(1) Gray compact sandstone: Length, $4\frac{1}{2}$ inches; width, $1\frac{5}{8}$ ins.; thickness, $\frac{5}{8}$ ins.
(2) Felsitic rock, purple: Length, $4\frac{1}{2}$ ins.; width, greatest, $2\frac{1}{8}$ ins.; thickness, $\frac{1}{2}$ in.
(3) Quartz, gray: Length, $3\frac{1}{2}$ inches; width, $1\frac{5}{8}$ inches; thickness, $\frac{1}{2}$ inch.
(4) Quartz, gray: Thickness, $\frac{1}{2}$ inch.

Spear-heads.—All of the specimens illustrated under this head have bases so fashioned as to provide for the attachment of a shaft, whilst the preceding bear no evidences of having been so wrought. Figs. 9, 10, 11, 12, and 13 may have been used for "fishing spears." At any rate, owing to their shape, they would have answered that purpose better than any of the other stone implements we have found. The shallow Codorus, with its generally clear water, along the banks of which all of these slender spear-heads were found, must have been a good stream in which to spear fish. Only one point, Fig. 12, is represented, though we have a number of them, to which we referred when outlining the supposed shape of the basal pieces—6, 9, 10, 11, and 13. Figs. 9 and 11 bear some resemblance to perforators, but their appearance and better finish seem to indicate a different use. Fig. 10 has on each side, just below the notch, a row of prominent teeth, a peculiar variation of the usual form. Other specimens not described are like the ones illustrated. All those described were found in different fields.

Arrow-heads.—Several hundred arrowheads have been picked up within this area. They seem to be generally distributed over the fields adjacent to the Codorus and along all the various runs emptying into the same. There are five or six localities where more fragments and more whole specimens have been found than elsewhere; but in several of these the washing away of the soil and the consequent exposure of the stones account for the greater "find."

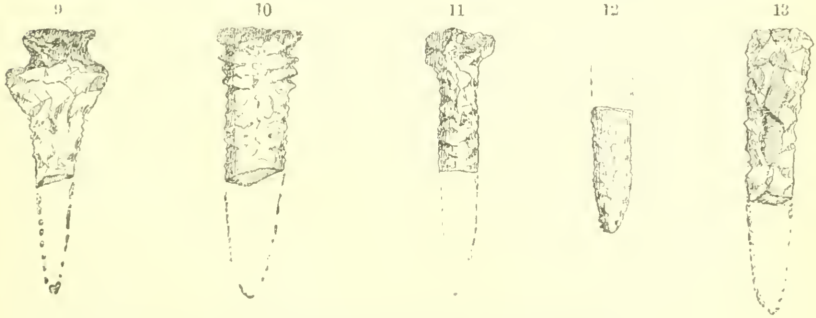
SPEAR-HEADS.—(Half-size.)

5 6 7 8



(5) Slate, purple: Length, $4\frac{3}{8}$ inches; width, $1\frac{1}{2}$ inches; thickness, $\frac{3}{8}$ inch.
(6) Slate, gray: Width, 2 inches; thickness, $\frac{3}{8}$ inch.
(7) Felsitic rock: Length, $3\frac{1}{2}$ inches; width, $1\frac{5}{8}$ inches; thickness, $\frac{3}{8}$ inch.
(8) Felsitic rock: Length, 3 inches; width, $1\frac{1}{2}$ inches; thickness, $\frac{3}{8}$ inch.

9 10 11 12 13



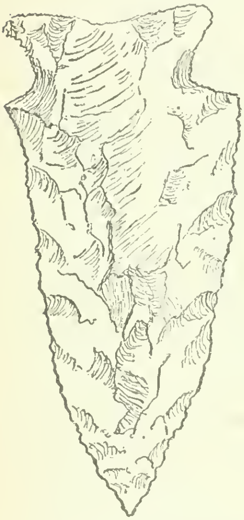
(9) Felsitic rock: Width, greatest, $1\frac{1}{8}$ inches; thickness, $\frac{1}{4}$ inch.
(10) Slate, black: Width, $\frac{5}{8}$ inch; thickness, $\frac{3}{8}$ inch.
(11) Felsitic rock: Width, gr., $\frac{5}{8}$ inch; thickness, $\frac{5}{16}$ inch.
(12) Quartz: Width, $\frac{7}{16}$ inch; thickness, $\frac{5}{16}$ inch.
(13) Felsitic rock: Width, gr., $\frac{3}{4}$ inch; thickness, $\frac{3}{8}$ inch.

The specimens selected for illustration are samples of the best wrought arrow-heads, showing variety in shape. They are far superior in workmanship to the average arrow-head. Before one such fine specimen can be picked up a dozen or more primitive ones will be found.

The minerals of which they are made are limestone, slate, quartzite, ordinary white quartz, a felsitic rock, jasper, agate, and chert. The first four of these substances occur within the region; the others do not. Were the arrow-heads of the other materials made here, or were they brought here already made from elsewhere? We shall refer to that question under the head of "Stoneworkers' Chips."

ARROW-HEADS.—(Full size.)

14



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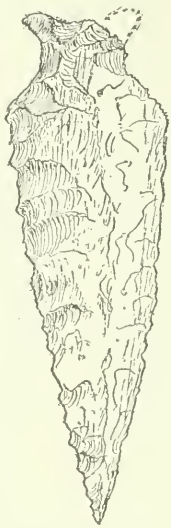
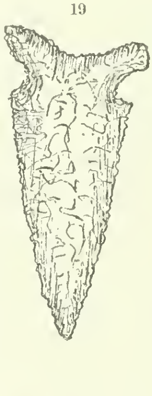


16

(14) Felsitic rock, gray: Length, $2\frac{5}{8}$ inches; width, $1\frac{3}{8}$ inches; thickness, $\frac{3}{8}$ inch.
(15) Felsitic rock, gray: Length, $2\frac{5}{8}$ inches; width, greatest, $\frac{7}{8}$ in.; thickness, $\frac{3}{8}$ in.
(16) Felsitic rock, gray: Length, $2\frac{1}{2}$ inches; width, $1\frac{2}{8}$ inches; thickness, $\frac{3}{8}$ inch.
(17) ....

18

19



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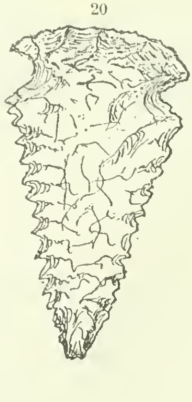

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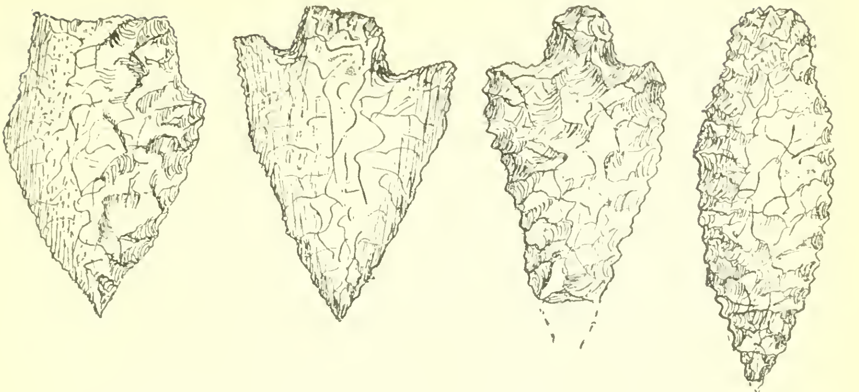
(18) Felsitic rock: Length, $1\frac{3}{8}$ inches; thickness, $\frac{15}{16}$ inch; width, $\frac{1}{4}$ inch.
(19) Quartz, milky: Length, $1\frac{5}{8}$ inches; width, greatest, $\frac{5}{8}$ inch; thickness, $\frac{1}{4}$ inch.
(20) Chert: Length, $1\frac{3}{4}$ inches; width, $\frac{7}{8}$ inch; thickness, $\frac{1}{4}$ inch.
(21) Quartz, milky: Length, ——; width, 1 inch; thickness, $\frac{3}{8}$ inch.

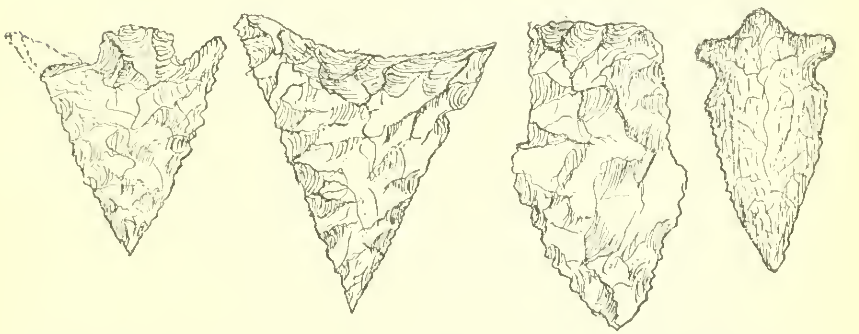
ARROW-HEADS.—(Full size.)

22 23 24 25



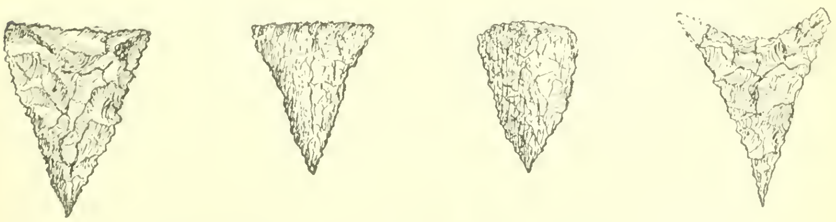
(22) Quartz, milky: Length, $1\frac{3}{8}$ inches; width, 1 inch; thickness, $\frac{1}{4}$ inch.
(23) Quartz, milky: Length, $1\frac{1}{2}$ inches; width, $1\frac{1}{4}$ inches; thickness, $\frac{1}{4}$ inch
(24) Felsitic rock, gray: Length, ——; width, $\frac{7}{8}$ inch; thickness, $\frac{1}{4}$ inch.
(25) Jasper: Length, $1\frac{7}{8}$ inches; width, $\frac{3}{4}$ inch; thickness, $\frac{1}{4}$ inch.

26 27 28 29



(26) Felsitic rock, gray: Length, $1\frac{1}{8}$ inches; width, $\frac{7}{8}$ inch; thickness, $\frac{1}{8}$ inch.
(27) Jasper: Length, $1\frac{1}{2}$ inches; width, $1\frac{1}{4}$ inches; thickness, $\frac{1}{4}$ inch.
(28) Felsitic rock, gray: Length, $1\frac{1}{2}$ inches; width, $\frac{7}{8}$ inch; thickness, $\frac{1}{4}$ inch.
(29) Quartz, red: Length, $1\frac{1}{4}$ inches; width, $\frac{5}{8}$ inch; thickness, $\frac{1}{4}$ inch.

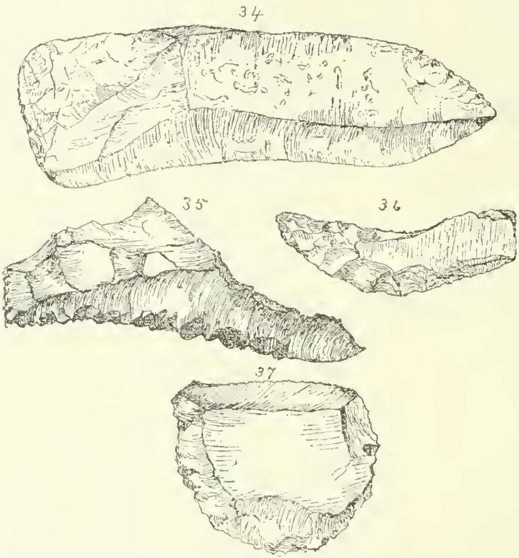
30 31 32 33



(30) Felsitic rock: Length, $\frac{7}{8}$ inch; width, [illegible] inch; thickness, $\frac{1}{4}$ inch.
(31) Quartz, milky: Length, $\frac{3}{4}$ inch; width, $\frac{5}{8}$ inch; thickness, $\frac{3}{16}$ inch.
(32) Quartz, milky: Length, $\frac{3}{4}$ inch; width, $\frac{1}{2}$ inch; thickness, $\frac{1}{4}$ inch.
(33) Quartz: Length, $\frac{7}{8}$ inch; width, $\frac{6}{8}$ inch; thickness, $\frac{1}{4}$ inch.

Knives.—Fig. 35 is a curved and somewhat angular piece of yellow and red jasper. Along its entire concave margin is a serrated cutting edge. It looks as though it had been originally a part of some larger implement and had been rudely chipped after detachment to its present shape.

KNIVES.—(Half size.)

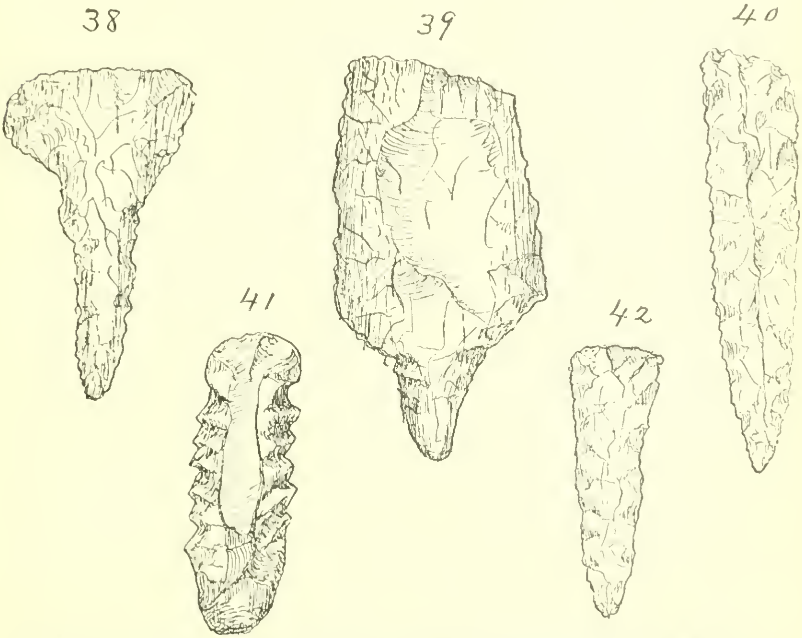


(34) Felsitic rock, gray: Length. 5 inches; width, $1\frac{1}{2}$ inches; thickness. $\frac{3}{8}$ inch.
(35) Jasper, yellow: Length, $3\frac{5}{8}$ inches; width, – inch; thickness, $\frac{1}{2}$ inch.
(36) Felsitic rock, gray: Length, $2\frac{1}{2}$ inches; width, $\frac{6}{8}$ inch; thickness, $\frac{3}{8}$ inch.
(37) Felsitic rock, gray: Thickness, $\frac{1}{2}$ inch.

The base of Fig. 37 is broad, concave, and not chipped. The rest of its margin is chipped to a cutting edge. Fig. 36 is a flake of felsitic rock with a somewhat blunt, serrated edge. Fig. 34 has been chipped to a remarkably good edge, with the exception of the basal end and a small flat area at the convex margin. By placing the forefinger on this flat surface and the thumb on the side a firm grip can be had which will enable one to make excellent use of the entire concave edge. This edge is decidedly the better of the two. We have nothing else from here like this specimen, but a knife of chert, from Ohio, in our collection resembles it.

Perforators.—One of these perforators, Fig. 41, of felsitic rock bears unmistakable evidences of having been used to drill holes. The point is worn smooth and more or less even, whilst above it, on both sides, the serrated edges are sharp and angular. Figs. 38 and 39 have broad bases and can be easily and firmly held between the thumb and finger. The points are cylindrical and stout. Figs. 40 and 42 might have answered very well for several purposes. Their shape is an excellent one for drilling holes, yet both are so well wrought as to suggest that perhaps they may have been spear or arrow-heads. The five illustrated are the only whole implements of the kind that we have found in this region. We occasionally pick up points that are cylindrical, but can not of course decide whether they belong to drilling stones or not.

PERFORATORS.—(Full size.)



(38) Quartz: Length, $1\frac{5}{8}$ inches; width, greatest, 1 inch; thickness, $\frac{1}{4}$ inch.
(39) Felsitic rock, blue: Length, 2 inches; width, 1 inch; thickness, $\frac{1}{4}$ inch.
(40) Felsitic rock, gray: Length, 2 inches; width, $\frac{3}{8}$ inch; thickness, $\frac{1}{4}$ inch.
(41) Felsitic rock, blue: Length $1\frac{1}{2}$ inches; width, $\frac{1}{2}$ inch; thickness, $\frac{3}{8}$ inch.
(42) Felsitic rock, gray: Length, $1\frac{2}{3}$ inch; width, $\frac{7}{16}$ inch; thickness, $\frac{1}{4}$ inch.

Celts.—Figs. 43 and 44 are so much alike in general outline as to justify the opinion that both were designated for the same purpose. Neither is pecked or sharpened, but both are chipped. Fig. 43 is made of quartzite and is rudely fashioned. The other, of slate, is much more symmetrical. The margins of both are very blunt. Either if sharpened would serve every purpose for which Figs. 45 and 46 might be used, and hence without speculating as to what they were intended for, we have called them chipped celts. Fig. 45*, of slate, is chipped and sharpened along the lower margin. Fig. 46*, made of trap, is smooth over its entire surface, and posesses a moderately sharp edge. There is no evidence of chipping or pecking, but the entire surface plainly shows that it was worn to its present shape by rubbing. Nearly all the celts from the Susque

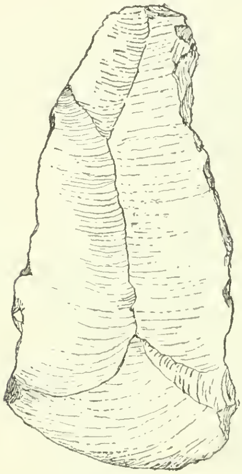
* In collection of Mr. George Miller.

hanna are chipped and pecked, or if smooth are simply water-worn stones that have been sharpened. The fact that it is made of a very hard and tough rock makes it all the more difficult to understand why this celt should have been laboriously rubbed to its present shape, and

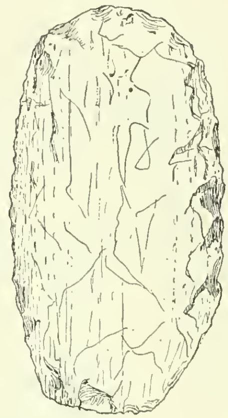
CELTS. (Half size.)

CHIPPED.

43



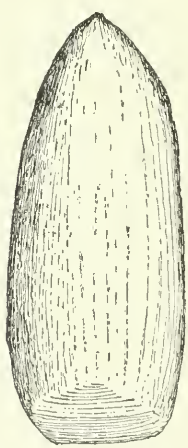
44



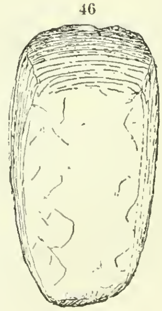
(43) Felsitic rock, gray; Length, 5 ins.; width, greatest, $2\frac{3}{8}$ ins.; thickness, 1 in.
(44) Slate, brown: Length, $4\frac{5}{8}$ inches; width, $2\frac{3}{8}$ inches; thickness, $\frac{3}{4}$ inch.

PECKED, OR GROUND.

45



46



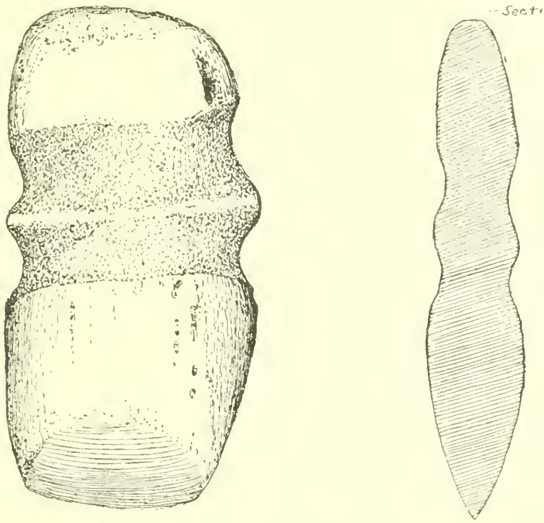
(45) Slate: Length, $4\frac{1}{2}$ inches; width, $1\frac{7}{8}$ inches; thickness, $\frac{3}{4}$ inch.
(46) Trap: Length, 3 inches; width $1\frac{1}{2}$ inches; thickness, $\frac{1}{2}$ inch.

also suggests that this specimen may have been used to dig in the ground and that the striæ on its surface may have resulted from some such use.

Axes.—The axes as a rule are small, with a groove extending around the stone. Most of those that come from the Susquehanna, near the mouth of the Codorus, where numbers are found, have one ungrooved side. Fully three out of every four are thus fashioned. Moreover, the groove often extends obliquely across the stone, yet I have not seen a single ax from this region with an oblique groove, and only one (in the collection of Casper Louks) that was not grooved entirely around the stone. Now, why should the predominating type of a region distant only about ten miles, and within easy access, be represented here

AXES.—(Half-size.)

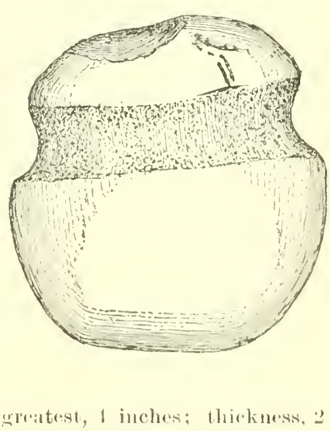
47 47 a



(47) Trap: Length, 5¼ inches; width, 2¾ inches; thickness, 1 inch; weight, 15 oz.

48

49



(48) Trap: Length, 7¼ inches; width, greatest, 4 inches; thickness, 2 inches.
(49) Quartzite: Length, 3¼ inches; width, 3½ ins.; thickness, 1 inch; weight, 8 oz.

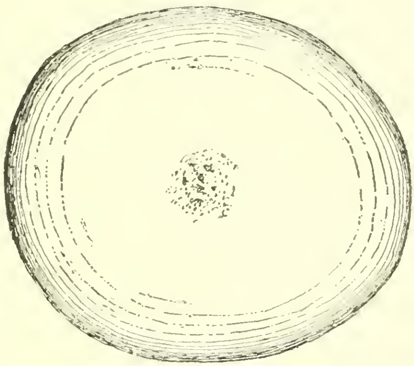
by but a single specimen? The most plausible inference is that the two types were intended for different purposes; probably the axes found here were carried about for general use, whilst the heavier ones form the Susquehanna, often differently grooved, as stated, were designed for some special use as boat-building. One of the axes, weighing only one pound (Fig. 47), has two parallel grooves extending entirely around. Of course, it is hard to assign the reason for two grooves in such a light stone, when other axes weighing much more, as Fig. 48, are provided with only one. This ax is slightly battered at the back, and has also a small piece out of one end of the moderately sharp edge. There is one noticeable difference between the edge of this specimen and that of Fig. 48. Fig. 47 bears transverse striæ on its smooth sides near the edge, which evidently were made in sharpening it, whilst Fig. 48 is marked with rather coarse longitudinal striæ. The latter looks very much as though it had been used as an agricultural implement and had been scratched through such use. It has a blunt edge, and, being of tough and hard material and of a pointed shape, would have made a good digging tool. Fig. 49 is made of quartzite. It is well wrought, and with the exception of a slightly broken back, is without a flaw. We were not able to collect many axes, and we do not know of more than 14 from the region in question. Of these, the illustrated ones are the best specimens. The number found seems comparatively large when the circumstances are considered. Axes, being conspicuous objects, are amongst the first specimens picked up. And in a region cultivated for more than a hundred years, such as this, it is quite probable that many of them were found and carried away. Moreover, it is the custom of our farmers to collect the stones from the fields and throw them into low and waste places. Several of the axes were picked up in the public road, where they had been thrown into mud holes along with other stones from the fields. Along the Susquehanna it is not an unusual thing for the fishermen to use these axes, on account of their convenient grooves, as sinkers for their fish nets! Of course, whenever the strings with which they are tied break, which often happens, the axes will be left amongst the water-worn stones at the bottom of the river.

Hammers.—Fig. 50 is a water-worn and smooth sandstone. It has been slightly roughened on each side, near the center, by pecking. The marginal area is less smooth than the rest of the surface, having been evidently roughened, but not battered by use. The evidences of its use by the Indians, whilst unmistakable, are very slight, and show that this particular stone was selected because it naturally possessed the desired shape. No doubt other worn pebbles were used as picked up by the Indians; at any rate, we occasionally find a spherical stone with a battered margin that looks exactly like a much-used hammer, only there are no pits pecked into it.

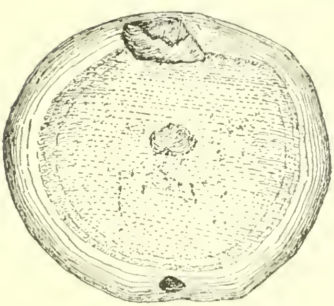
Fig. 51 is a close-grained sandstone, and was both hammer and polishing stone. The margin is quite rough and indented from its use as a hammer—a use also indicated by the presence of a shallow pit near the center of one side. Almost the entire surface of one side, the one shown in the illustration, is very smooth. It bears unmistakable evidence of having been so worn after the pit had been pecked. The stone is

HAMMERS.—(Half-size.)

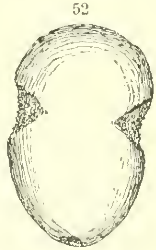
50



51



52



(50) Quartzite: Length, $1\frac{1}{4}$ ins.; width, $3\frac{7}{8}$ ins.; thickness, $1\frac{3}{4}$ ins.; weight, $1\frac{1}{2}$ lbs.
(51) Quartzite: Length, $3\frac{1}{2}$ ins.; thickness, 2 ins.; width, $3\frac{1}{4}$ ins.; weight, $1\frac{1}{8}$ lbs.
(52) Sandstone: Length, $2\frac{1}{4}$ ins.; width, $1\frac{1}{2}$ ins.; thickness, $1\frac{1}{4}$ ins.; weight, $\frac{1}{4}$ lb.

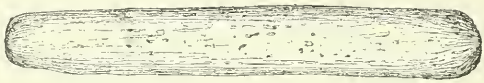
hemi-spherical, and when held in the hand is found to be well adapted for polishing purposes—a use also likely to have been suggested by the grit of the stone. It is a type common along the Susquehanna. Very few hammers have been collected in this region.

Fig. 52 is a water-worn oval pebble somewhat battered at one end. Very rough notches have been pecked into the opposite sides to provide for the attachment of a handle, incident to some subsequent use of the stone. It was found half a mile from the Codorus, near a spring, in a field plentifully strewn with "chips." Was it a "pogga-moggon" stone?

Pestles.—Even fragments of pestles are scarce. I know of only one, Fig. 53, that has been found whole. It may not be out of place to state here the origin of such specimens, as given by one not accustomed to collect relics. A very smooth and cylindrical section, about two inches long, of a pestle was shown to a farmer, near whose house it had been found. He immediately pronounced it a thunderbolt!

PESTLE. (One-sixth size.)

53



(53) Quartzite: Length, 15 inches; thickness, 2 inches.

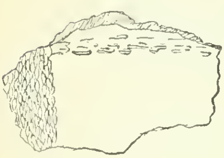
Pottery.—We found a few fragments of pottery in four widely separated localities. In two of these localities pieces of soapstone, parts of dishes, were also picked up. The pieces of pottery, made out of clay and broken pebbles, materials easily obtained here, are similar to pieces from the Susquehanna. The impressions are evidently of two kinds, those made by a stylus of some sort in the hands of the ancient potter and those which resulted from the structural irregularities of some receptacle within which the plastic clay was first shaped.

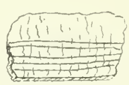
POTTERY, SOAPSTONE DISH. (Half-size.)

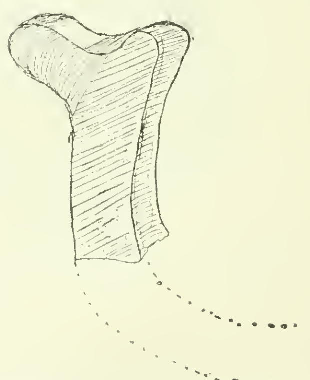
57

55

54







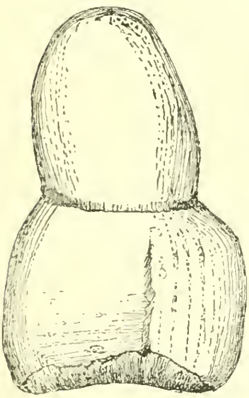
(54) Pottery Fragments: Thickness, $\frac{3}{16}$ inch.
(55) Pottery Fragment: Thickness, $\frac{1}{4}$ inch.
(56) Pottery Fragment: Thickness, $\frac{1}{8}$ inch.
(57) Soapstone Dish: Thickness, $\frac{3}{4}$ inch.

Soapstone dishes.—Fragments of soapstone dishes were collected in four or five separate localities. The "ear" piece illustrated is one of eight pieces found near together and evidently all parts of the same vessel. The largest of these fragments is six inches long and eight inches wide. The dish originally must have been a foot in length and nearly as broad, with a depth of five or six inches. Soapstone is not found *in situ* in this locality, but it occurs plentifully in the adjacent county of Harford, in the State of Maryland.

Implements of unknown uses.—Fig. 59 has been broken so that its original form is a matter of conjecture. However, it is so strikingly like another strange specimen (Fig. 58) from the Susquehanna, which is entire, that we have no hesitancy in concluding that both were designed for the same purpose. Both are made out of chlorite, and are not in the least battered. They could not have been used as weapons or as agricultural implements, since the stone is very brittle and is moreover so soft as to be easily scratched with the finger-nail. These are the only specimens of this shape, or of chlorite, that I have ever seen from this or adjacent localities. We think they were probably used as ceremonial implements.

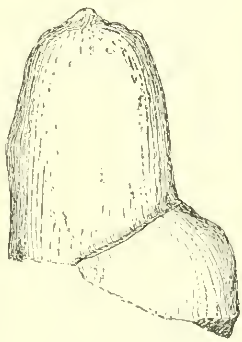
IMPLEMENTS OF UNKNOWN USES.—(Half-size.)

58



58 a

59



59 a

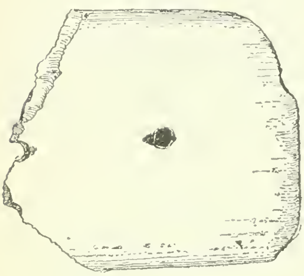
(58) Chlorite: Length, 4 inches; width, greatest, 2½ ins; thickness, greatest, 1 in.
(59) Chlorite: Length, 3½ inches; thickness, 1 inch.

Figs. 60, 61, 62, and 63, are pieces of slate. The holes in all of them were, apparently, made with stone drills, since they are irregularly grooved and taper towards the center of the stone from both sides. These pieces are so fragmentary as to prevent any attempt at restoration. No. 60 is worn quite smooth, with rounded edges, and has a slight polished depression extending a short distance from the inner margin of each hole along the surface of the stone. This polished surface was doubtless produced by a cord passing through both holes, from which the slate was suspended. The holes in Fig. 61 are polished, the result of friction.

Figs. 64 and 65 are both well wrought implements. From the care with which they have been finished they were evidently designed for some special use. They are the only specimens of the kind that we know of from this locality. They might have been used as teeth in war-clubs, but whether they were so used or not is of course mere speculation. Among our implements of unknown uses, perhaps the most interesting and valuable one is Fig. 66. So far as I have been able to ascertain, it is the only one that has thus far been found. It is a triangular prism of slate, with sides three fourths of an inch wide,

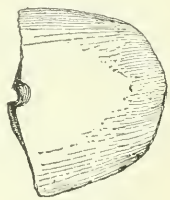
IMPLEMENTS OF UNKNOWN USES.—(Half-size.)

60



61

63

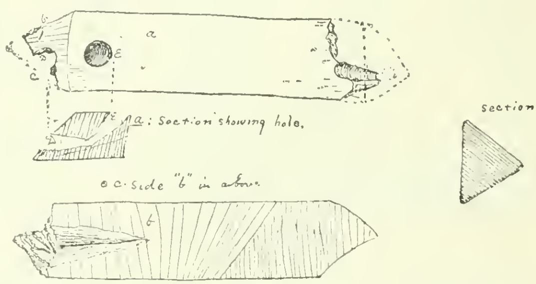


(60) Slate, black: Width, 2¾ inches; thickness, ¼ inch.
(61) Slate, brown: Thickness, ⅛ inch.
(62) Slate: Thickness, 1/16 inch.
(63) Slate: (Split).

64

65

66



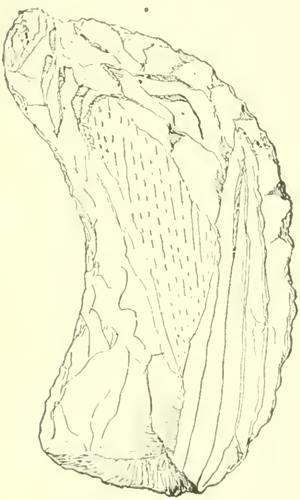
(64) Felsitic rock: Length, 2⅞ inches; width, greatest, 1⅓ inches; thickness, ⅝ inch.
(65) Felsitic rock: Length, 3¼ inches; width, greatest, 1⅓ inches; thickness, ½ inch.
(66) Slate, brown: Width of each side, ⅞ inches.

originally about 5 inches long, and having at each end two holes that meet. The one hole is bored with a slant of about 45 degrees (see section *a*) from near the end of one side till its junction with a hole bored from the end of the prism. The other end of the specimen, though much broken, was evidently fashioned in the same way. The holes are funnel-shaped and are such as would be produced by a stone drill. Two sides of the prism bear symmetrical scratches, evidently once of some significance, now in part defaced by wear and in part by the ancient use of the stone for whetting purposes.

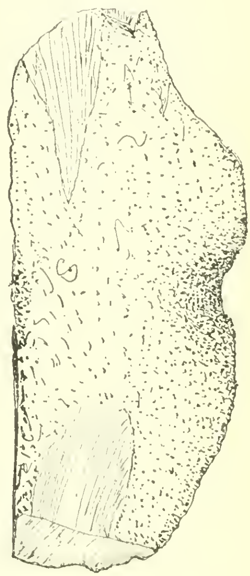
Fig. 67 is a gneissoid rock, very rudely flaked and somewhat pecked. It is hard to conjecture to what use it may have been put, though there is no question about its having been worked into its present shape. Fig. 68 is made of green slate. It is either an unfinished implement, or if completed, a very rudely fashioned one. The fact that it is made of slate, as well as its shape, incline us to call it a banner stone.

IMPLEMENTS OF UNKNOWN USE—(Half size.)

67



68



(67) Slate: Length, 5 inches; thickness, 1 inch.
(68) Slate: Length, 6 inches; thickness, $1\frac{3}{8}$ inches.

Stone Worker's Chips.—Flakes of felsite rock, of jasper, and of agate, are found well distributed along the Codorus and its tributary runs. The fact that the rocks of which these flakes are pieces are not naturally found here is very significant. The presence of these "chips" proves that implements were here wrought out of the rough stone into desirable shapes. But these minerals, felsite rock, jasper, and agate, are not found *in situ* in this region. The felsite rock occurs fully thirty miles distant, in the South Mountain. Where the agate and jasper were brought from has not been determined. Occasionally flakes of white quartz cover a small area in a field containing in another part a spot rich in flakes of felsitic rock. The presence of such spots seems to indicate that each ancient stone-worker confined his labors to chipping a particular mineral.

Conclusion.—Just what conclusions as to the Indian occupation of this part of York county can safely be drawn from the number and variety of specimens found is not so easily determined. One thing is certain, that as the result of persistent search, almost a complete "series" of relics has been collected. Though the author found nearly all the objects here illustrated and described, yet any one else, had he as thoroughly and persistently searched the same region, would have been equally successful. This is proved by the fact that several others (Casper Loucks, George Miller, and John J. Frick) interested in the subject have found specimens in the same territory. The discoveries here made lead us to infer that other places, in the eastern portion of the United States, now thickly settled, would be just as productive of specimens. We do not believe that this region is more favorable to the production of relics than other localities similar in natural features. Attention is called to a few difficulties that beset the careful searcher. Fields that now yield few relics may have them deeper down. The building of dams has materially changed our streams. Places that once were high and dry on the bank are now covered by every freshet. As a consequence, the sediment has accumulated, and the relics have been buried beyond the reach of the plow. Occasionally a field is washed bare of all the loose soil. In that event, you can not reasonably conclude that the number and variety of specimens found there indicate a more dense settlement than elsewhere. Taking these and other circumstances into consideration, in connection with the relics found, the author believes that this region was oftener frequented, and longer occupied, by larger bands of Indians than the historian leads us to infer. This place may have been the site of a well established settlement; a settlement in which much the same primitive occupations were engaged in as characterized well-known and more extensive settlements along the Susquehanna. If this region is an average sample of supposed "barren" lands, may we not conclude that America was more thickly settled, or longer inhabited (perhaps both), by the Indian than is generally supposed?

ABORIGINAL BURIAL MOUNDS, EDEN TOWNSHIP, SENECA COUNTY, OHIO.

By Russell J. Thompson.

These mounds, three in number, are, or were, located about 4 miles east of Tiffin (NE. ¼ of section 4, township 1 north, range 15 east, Eden Township, Seneca County, Ohio, and on the west side of the Morrison State road where it crosses Rock Creek). (Map, Fig. 1.)

Concerning their origin, the Mohawk * Indians then inhabiting that section could give the settlers no information. The Indians had no theory or tradition accounting for the presence of the landmarks. Large forest trees covered these monuments then, and among them were cherry trees 20 inches in diameter. Before the cultivation of the soil after the removal of the forest had altered the proportions of the mounds, they were well rounded, and the largest was perhaps 6 feet or more in height and about 40 feet in diameter. Two, the largest and the smallest, were near each other on the south side of the stream. (See map.) The former was slightly less than an eighth of a mile from the bank, and the smaller one about half that distance. The third was on the other side, about as far as the latter from the creek and a third of a mile from the largest. It was but a short way beyond the culmination of a gentle rise of about 25 feet above the creek valley. The banks on the south side were rather steep and 30 to 40 feet high. The large mound was located on the rounded border of an elongated depression tributary to the creek.

Fig. 1.

The mound on the north side of the creek was crossed by the fence dividing the road from a modern graveyard, and when a grading was made, not long prior to 1886, for the convenience of standing teams driven to the church, human remains were unearthed on the roadside of the fence. The other half probably still remains intact.

In 1886, the ground over the site of the smallest mound was on a

* The Senecas inhabited the region to the north and the Wyandots that to the south.

level with the general surface. It was tentatively excavated by one who had seen it before being plowed down and a small excavation, made in search of the charcoal and ashes, whose presence would confirm the hypothesis of position, discovered human remains. These however had been once exhumed and were reinterred in a confused heap. They were preserved by the writer, but no further search was made then. It was at the largest of the mounds, and the one least disturbed, that the systematic, though partial exploration was undertaken. This was in the summer of 1886, and then the mound was hardly, if any, more than 4 feet above the general surface, and 60 feet in diameter. (Fig. 2.) Hurried excavations brought to light the charcoal and ashes, clean shells, broken bones of animals, and broken pieces of primitive dark pottery. Another unorderly opening resulted in the discovery of the two smaller human skeletons, but they were not secured entire. The skull of one of these "individuals" probably remains in the mound.*

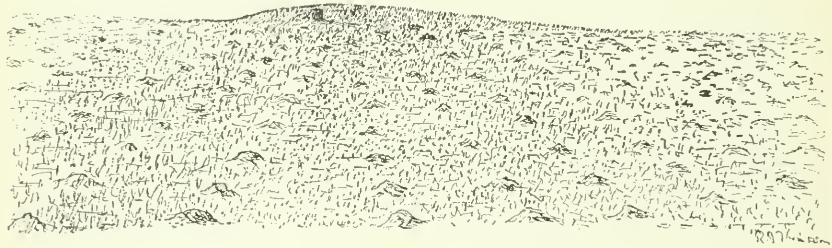


FIG. 2.—View of largest mound, looking southward.

Human remains were found in all of these. The largest mound contained three skeletons (Fig. 3) that were uncovered, and a complete excavation might be expected to reveal more.

Feasting accompanied the interment. Fires were built on the uncompleted funeral pile, with which meats were cooked. Good sized pottery vessels were brought to the grave, probably for use in the feasting, and at least some of them were either accidentally or intentionally demolished and the fragments scattered with the ashes and charcoal and broken bones of animals over the half-built mound. The fires were probably burning during the burial. The feasters enjoyed the meat of the deer, beaver, raccoon, squirrel, hare (?), turtle, birds, fish, and clam. The marrow was a much relished portion of the meat; every bone was broken so that the marrow was uncovered. The need of something with which to accomplish this breaking may account for the presence of moderate sized limestone bowlders a few feet apart

* This description well illustrates the detriment and sometimes irreparable damage to science arising from ill-informed opening of mounds. A mound once disturbed is valueless to science. Its evidence as to the life history of its constructors is destroyed. Circular 49, issued by the Smithsonian Institution, No. 730, explains this and contains directions for mound and cave explorations.

among the ashes and other stones of the size of a fist in great abundance.

A foot or two of earth was spread over all this charcoal and ash layer. The soil of the structure was even, light, and easily worked. The hard unmolested clay was 4 feet or so below the top, as the mound existed after the field had been under modern cultivation. A slight excavation was made in this hard soil to receive the remains.

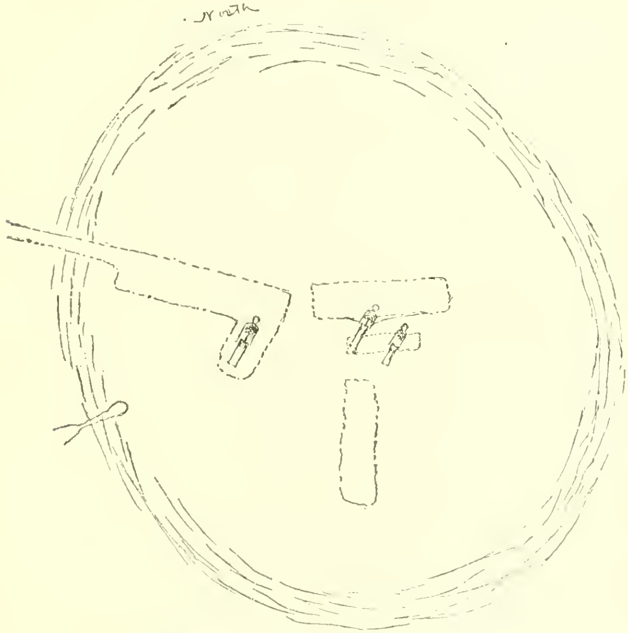


FIG. 3.—Excavations of largest mound. (See map.)
About 60 feet in diameter, 4½ in height.

The heads were laid to the northeast and the bodies flat and straight on their backs, at least so with the largest and what may be called the principal skeleton. This body lay furthest to the northwest and a few steps from the center. Nothing whatever of a durable nature inclosed the remains. A few pieces of charcoal were found under the bones of the principal corpse. These, an elegant pipe (Fig. 4), and a few canine jaw fragments and fragments of the jaws of some small carnivorous animal, were the only objects found with the skeleton. A bone turkey call, 3 inches long, and artificially grooved on both sides at the end, was found on the ground about one of the skeletons. The jaw fragments were laying beside the right shoulder; the pipe was on the same side next to the neck vertebræ. The condition of the bones of the principal individual gave no evidence of violence having been inflicted upon his body.

At the time of the excavations, the charcoal layer was met from a foot or two feet beneath the surface. The fragments of animal bones and pottery were scattered promiscuously, with the ashes and charcoal patches, through a vertical range of more than a foot. The limestone bowlders were about 6 inches in diameter, and lay with a rude uniformity a few feet apart throughout the layer. The underlying rock of the section is limestone (cornstone), the boundary between the outcrop of this and the water line is near, and the southwestward moving glaciers have made this element strong in the drift products of the country.

In the charcoal layer the most numerous bones were those of the deer. A good number of only partially broken lower jaws were found, and a few decayed horn fragments. Several specimens of fragmentary beaver skulls, retaining the teeth, and a few raccoon jaws, are in the collection, but some of the clam shells were not broken; these showed the effect of fire.

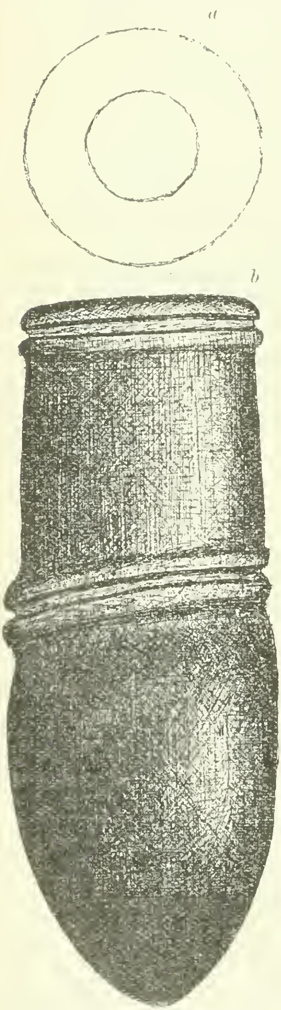


FIG. 4. Pipe (full size).
a, end view; *b*, side view.

Making exception of the two small pottery pipes unearthed from the charcoal layer, no entire specimens of pottery were discovered. The pottery was rude, blackish, and gritty. Minute feldspar crystals formed part of the material. The surfaces were roughened by perpendicular striations which could be imagined to have been impressed by bark. By projecting the curve of one of the rim fragments, the opening of the jar of which it was once a part, was found to have been 8 inches in diameter. A dark bluish or greenish slate, hard, tough, and fine, was the material of which the large pipe, buried with the largest skeleton, was made (Fig. 4). It is the same stone as that from which most of the fine mound ornaments were cut. Erosion revealed a kind of enamel, perhaps due to chemical action on the surface. The erosion occurred where the stone had been stained, presumably by the acids of the dead body. The finish was excellent. The form might suggest that the maker had intended the relic for a phallic emblem. The pipe is $3\frac{3}{4}$ inches in length. Little difference in size between the individual interred in the "levelled mound" and the principal male buried in the large mound, was shown by the remains. The bones of the two skeletons to the southeast of the latter were perceptibly lighter.

These bones were found in the center and a little more than 4 feet below the top. Not satisfied with this method of proceeding, the writer himself soon after spent three days in the field. A narrow trench was made to approach the mound until the charcoal was reached, and then with a width of 3 feet was extended to the center (Fig. 3). After the earth had been carefully removed from over the body, an entire afternoon was occupied in picking out the bones. Their condition was such that even then a number were broken. The occiput fell apart when the skull was lifted. The lines of fracture indicated a recent breaking.

INDIAN REMAINS ON THE UPPER YELLOWSTONE.*

By Col. Wm. S. Brackett.

If you look on almost any large map of Montana and Wyoming you will note the source of the Yellowstone River near a mountain marked on the map as "Youth's Peak," and lying about 25 miles southeast of the Yellowstone National Park. The river flows from an immense snow-field on this mountain, in a northwesterly direction, and empties into Yellowstone Lake, which lies wholly within the park; then it flows out of the lake at the lower or northern end and leaping downward a sheer depth of 360 feet, over the Great Falls of the river, it rushes still northward for a hundred miles—one of the most beautiful streams of the Rocky Mountains. The real name of the mountain where the Yellowstone rises is Yount's Peak, so called after a trapper who lived for a long time along the banks of the river in the early days of Montana's settlement. Perhaps the fine new maps of this region now being made by the United States Geological Survey will not rob Yount's Peak of its true name.

About 25 miles north of the park is a widening of the valley of the Yellowstone, where there are a number of fine ranches, and on one of them, opposite Emigrant Peak, where I am writing, there are interesting remains left by the Indians who lived and hunted in this now fertile valley as late as the year 1876.

Just above our ranch house is a mesa, or tableland, from whose flat top can be seen the green fields under irrigation along the river, and the lofty mountains hemming in the valley on every side. Only ten years ago there were no cultivated fields in this valley, and the elks and buffaloes found here their favorite feeding ground. The plain on this mesa is almost rectangular in shape, and at the corner, overlooking the whole region, are stone structures that we have named the "Indian Forts." We do not know whether the Indians, who undoubtedly built them, used them as forts for defending their village or camp up on the mesa, or whether they were used as watchtowers for their sentinels. Sometimes we think the Indian hunters used them to creep into and to spy out the large game feeding among the hills and in the valley below.

*From *The American Field*, Feb. 11, 1893, vol. xxxix (No. vi.), pp. 127, 128.

These forts are semicircular in form, and are built of selected square stones, piled up in a parapet or breastwork about four feet in height. They are open on the inner side of the plateau, and have space for two or three men to lie concealed and protected within. The forts must have been built many years ago because the stones are now pretty well covered with moss and lichens, and these do not grow as rapidly in this dry climate as in the Eastern States. No one is permitted to disturb these monuments of a race now almost departed, and I hope that some careful student of American archæology may hereafter explore this region and explain the ancient use of these so-called "Indian Forts."

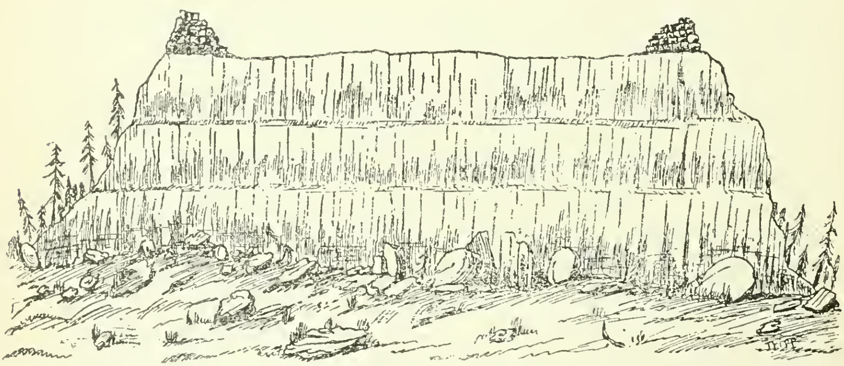


Fig. 1.—The Indian forts, Park County, Montana.

Just below one of the forts and at the bottom of the cliff I found, last summer, a buffalo skull and horns, over-grown and almost concealed by a wild rosebush. Perhaps the buffalo was shot by an Indian lying in the fort above. This made me think the forts might have been used for watching large game. But when you are up on the mesa you can easily see how well adapted the place is to prevent surprise and for military defense. The sides are perpendicular precipices of volcanic rock. At only one place can you go up on horseback; there are only two or three places where you can climb up on foot. On the level top a thousand men could be placed in camp. The forts may have been used, like watchtowers on the corners of a feudal castle, by the wild chivalry once inhabiting these mountains.

About half a mile from this mesa is a little sheltered valley, back in the foothills, where the Indians used to pass the winter one of the pioneers of this region tells me. The place is sheltered from the winds and the snow seldom drifts there. In a level spot in this valley are three circles of smooth flat stones laid on the ground, each circle being about 15 feet in diameter. Washed by the rains of many seasons, these stones are now partly imbedded in the ground. We do not know exactly the purpose of these water-worn rocks laid so regularly in circles, but one of our neighbors, an "old timer" in Montana, tells us the

Indians used them in winter to lay around the bottom or lower edge of their tepees to keep out the cold. Most Indian tepees are conical in shape and circular at the bottom, with a hole at the top where the poles meet for the escape of smoke from the fire built in the center of the structure. In the old days, when the buffalo and other large game were plenty, the Indians made their tepees of smoke-tanned hides. Now the buffaloes are entirely gone, and other large game is so scarce on the Indian reservations that the tepees are covered with cloth, generally thin white calico. The Indians have but few skins left and their calico tepees are very cold in winter.

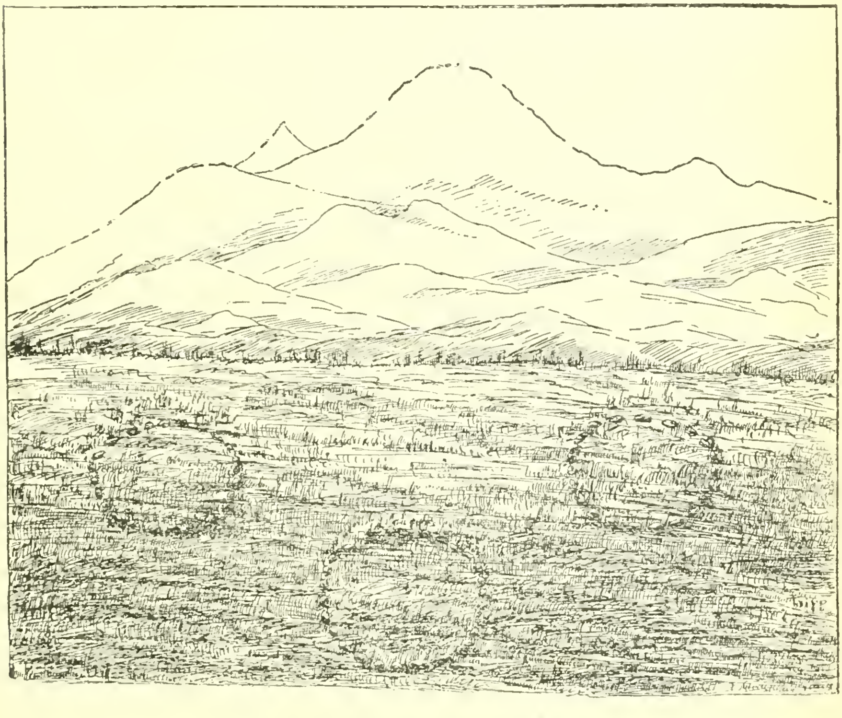


Fig. 2.—Tepee rings, Park County, Montana.

The most interesting of the Indian remains on our ranch is at Buffalo Bluff, where there is a remarkable game drive. Under the cliff, which is about 40 feet high, the ground is white with the splintered bones of large game animals that have been driven over the precipice—buffaloes, elks, and deer. Above is a level plain stretching back for several miles into the foothills. The cliff is only about a hundred yards wide at the steep part where the game was driven over. How did they manage to make wild animals run to this narrow cliff and leap over? You can see at once how this was accomplished when you climb to the plain above. There can be seen two long lines, composed of piles of stones, stretching out on the plains, each line about half a mile long and diverging from the edge of the cliff like the two arms of an open fan. The piles of stones are about 10 feet apart and each stone heap is 2 or 3 feet in height. When the Indians last used this game drive, which was about fifteen years ago, they set up wooden stakes about 4 feet long in each stone pile. From stake to stake were stretched lines of stout buckskin cord, like wires on a barbed

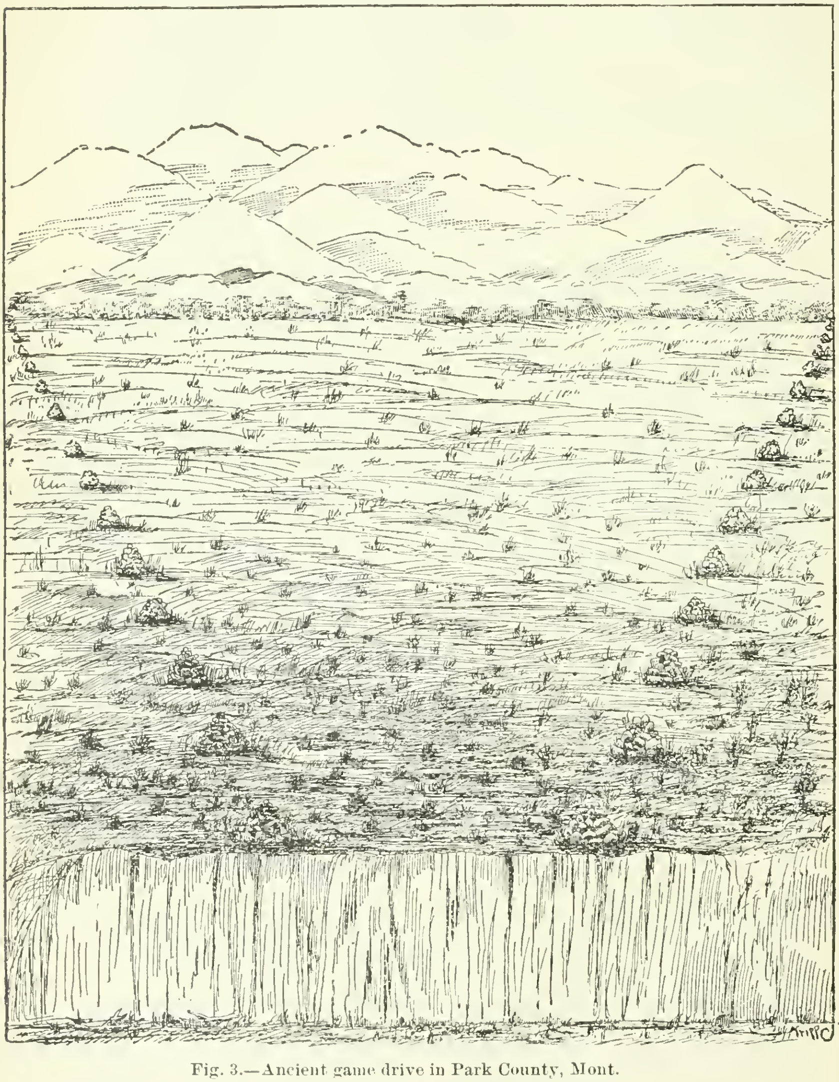


Fig. 3.—Ancient game drive in Park County, Mont.

wire fence, and from these cords were hung at short intervals feathers, strips of bright cloth, and scraps of white buckskin, fluttering in the wind. Of course this fence could be easily broken through, but the frightened animals always turned back from the fluttering rags, feathers, and other objects hanging from the long lines of cords.

A heard of buffalos or deer was carefully surrounded by the Indian hunters, and then gradually driven toward the opening of the drive, which was over half a mile wide. Once within these lines, the hunters drove the herd toward the bluff, waving their blankets as they rode forward. The terror-stricken animals rushed toward the precipice, keeping away and turning back in fright from the lines of "fence," which gradually converged toward the cliff. At last, in a wild stampede, the frantic animals were driven over the edge of the precipice, where those who were not killed outright were dispatched by another party of hunters below. Only spears and arrows were used below the cliff, because the noise of firearms would frighten back the animals approaching the edge of the bluff. Among the mass of crumbling white bones beneath this Buffalo Bluff (as it is called here), where so many wild animals have been slaughtered, you can to-day occasionally find spear and arrow heads, beautifully formed of shining black obsidian, or volcanic glass, the material being found in large quantities on the great plateau of the Yellowstone National Park.

PRIMITIVE NUMBER SYSTEMS.

By Levi L. Conant, Ph. D

Among the speculative questions which arise in connection with the study of mathematics from a historical standpoint the origin of number is one that has provoked much discussion and has led to extensive research among the primitive and savage languages of the human race. A few simple considerations will however show that such research must necessarily leave the question entirely unsettled, and will indicate that it is, from the very nature of things, a question to which no definite, or at least no final, answer can be given.

Among the barbarous tribes whose languages have been studied, even in a most cursory and imperfect way, none have ever been discovered which did not possess one or more words indicating familiarity with the number concept. Some tribes have been found in which knowledge of number was so slight that the statement has been made that their language contained no numerals. The Chiquitas, of South America, for example, have no word even which we can accept as a distinct substitute for "one." That numeral they express by a word meaning nearly the same as "alone." Here the number sense appears at its lowest ebb, but still it does exist; and going lower yet, one would be rash, indeed, if he were to assert that the higher animals can not distinguish between 1 and 2. Not a few tribes have been found who could not count beyond two; more yet with 3, 4, or 5 as their number limit, while 10 marks the boundary of the numeral systems of a very great number of the primitive races of the world. The assertion would seem then to be a safe one that the number sense is never wholly lacking. It is evident also that numerals must be among the earliest words to be formed in any language. They express ideas which are wholly concrete, which precede human intelligence, and which are in many ways manifested by the higher orders of the brute creation. The origin of number therefore must be conceded to lie beyond the proper limit of inquiry, and the primitive conception of number to be fundamental with human thought.

Historical investigation must begin not with number itself, but with modes of expression of number. Here, in precisely the same manner as in the expression of all forms of thought, desire, and emotion, the sign language preceded words. We are all familiar with the manner in which a child when learning to count makes use of his fingers. Children have for ages done the same; and the children of the human race, the savages of pre-historic times, unquestionably counted on their ten digits just as the African, the Eskimo, and the South Sea Islander do to-day. So universal has the finger method of counting always been that many investigators, prominent among whom is Grimm, have laid it down as an axiom that all numeral words arise from names of the fingers of the hands. Savage races employ, as might be expected, a great variety of methods of recording their counting—as splints, pebbles, shells, kernels of grain, knots, etc. Then come simple scratches, notches cut in a stick, Robinson Crusoe fashion, and other similar devices. But back of all these, and forming a common origin to which all may be referred, is the universal finger method of counting, the method with which all begin, the method which is too convenient to be entirely relinquished, even by civilized races.

This universal recourse to the fingers often resulted, as might be expected, in the development of a more or less extended pantomime number system in which the fingers were used in much the same way as in the deaf and dumb alphabet, though the signs actually employed were very different from those employed by mutes. A system of this kind was much in vogue among the ancients, by means of which any number up to 10,000 could be expressed: units and tens by inflections of the fingers of the left hand, hundreds by similar inflections of the fingers of the right hand, and thousands by a repetition on the left hand of the signs used to denote units and tens. The Chinese still employ a finger method of expressing numbers less than 100,000, and among nearly all Eastern peoples a digital arithmetic of one sort or another is to be found. Of so common use is this sign language that traders are said to communicate to each other the price at which they are willing to buy or sell, and at the same time to conceal their offers from bystanders, by putting their hands under each other's cloaks and touching each other's fingers.

Recent anthropological research has developed many interesting facts respecting the limits to which the number systems of the various uncivilized races of the world extend. As a matter of course, all races can indicate numbers as high as 10, the fingers serving as a means of showing what they have no words to express. In nearly all cases we find this limit extended to 20, the second 10 being told off on the toes, or on the fingers of a second man. But savages have in very many instances no words for numbers higher than 2, 3, or 4. The Botocudos have no definite number beyond 1. For 2 they say "urahú", many. * The Puris and the Watchandis stop at 2. The former express 3 by "prica", many, and the latter express the same number by the combination 2, 1. † The Andamans have only two numeral words, though

* Tylor: *Primitive Culture:* vol. I. p. 243. † *Ibid.*

they count as high as 10 by means of their fingers.* Ten they express by their word for "all." The Bushmen have the same number limit, expressing any number greater than 2 by the equivalent word for "many." The Veddas of Ceylon count "ekkamaï," 1, "dekkamaï," 2, and then continue by repeating again and again the word "otameekaï," meaning "and one more." †

Numerous as are the instances in which two stands as the number limit for savage tribes, three is thus used still more frequently. The New Hollanders have no names for numbers greater than three.‡ The low forest tribes of Brazil commonly express any number greater than three by their equivalent for "*many*."§ The Australians of Herbert River do the same.|| The Fuegans are supposed to have counted formerly to ten, but at the present time their entire number system is comprised in the three words: "kaoneli," 1; "compaïpi," 2; "maten," 3.** The Campas, of Peru, count: "patrio," 1; "pittem," 2; "mahuimi," 3. Beyond this they can express no number except by some such expressions as 1 and 3, 1 and 1 and 3, etc., showing the total indicated by holding up the proper number of fingers. As a definite number anything beyond ten is to them inconceivable, and they refer to it as "to haine," "many."†† The Australian tribe of the Wiraduroi have no numerals which enable them to count beyond 3. With them four is "many," and five "very many." Almost exactly the same statement may be made of the Dippil, the Kamilaroi, the Adelaide, the Turrubul, the West Australia, the Encounter Bay, the New South Wales, and the Tasmania tribes.‡‡ Some of these indicate four by expressions such as "two-two," or "two pair," and five by "two three," or "two-two-one." The Encounter Bay tribe uses an analogous reduplication for six even, saying "kuko-kuko-kuko," that is, "two-two-two." The Yaucos, of the Amazon, express the number three by the astounding word "poettarrarorincoaroac," at which La Condemaine duly remarks: §§ "Happily for those who have dealings with them, their arithmetic goes no further."

The general limitation of the number sense existing among the low races of the world now begins to become apparent. Specific words exist for one, two, three, etc., and beyond that anything is "many." The entire number system of a tribe may be "one," "many," or it may be "one, two, many." More numerous yet are the cases where the counting goes one step farther, and gives "one, two, three, many," as the scale through which the savages' number sense can conduct him. In the same way

* Müller: *Grundriss der Sprachwissenschaft.* B. IV, p. 47.

† Dechamp's *L'Anthropologie*, 1891, p. 318.

‡ Tylor *Primitive Culture*, vol. I, p. 243.

§ *Op. Cit.*, p. 242.

|| Lumholtz, C., *Bulletin de la Socité d'Anthropologie de Paris.*

** *Op. Cit.*, 1887, p. 310.

†† Weiner, *Perou et Bolivie*, p. 560.

‡‡ Müller, *Grundriss der Spr.*, B. IV., Abteilung 1, *multa loca.*

§§ *Voyage de la Riviere des Amazons*, p. 64.

we might expect to find the cases where counting stops with four more numerals than those where three is the limit, just as three is a much more common limit than two. Such is not the case, however. Investigation shows that if counting extends beyond three, it is almost sure to reach five, the commonest limit among races whose number sense is very weak. A few instances have been found where tribal numerals extend to four without going beyond that point. The Tupis, of South America, have only the four numerals "oyepe," 1; "mokoi," 2; "mosapira," 3, and "erundi," 4.* The Australians, of Lake Macquery, have no numeral beyond "woran," 4, except the indefinite expression "kanwol-kanwol," which signifies "great-great."† The Tasmanians also have four as their proper number limit, but they have a compound expression, "pagan-a-mara," 4 + 1, which they use for five.‡ A few other instances of the same limit are given by various authors, but they must be received with great caution. If a savage can count to four he is practically certain to extend his system one step further, and to make his scale contain the number of steps which corresponds to the number of fingers on one of his hands.

This brings us again to the consideration of the relation existing between the hand and its fingers, and primitive counting. Three common number limits are found among savage races, 5, 10, and 100. A savage counts on his fingers until he reaches 5, and then he often stops, saying merely "many," for any greater number. With a slightly higher degree of intelligence, or with an environment calling for more extended use of the number sense, others go on from this point, counting now on the fingers of the other hand. As a number limit 10 is used almost as commonly as 5; and it is no infrequent thing to find the toes as well as the fingers made to do duty as counters, thus bringing the total up to 20. The last named number is rarely the limit, however. If a savage can count to 20 he is usually able to go on to 100, and often to 1,000.

The manner of counting just indicated has given rise to a peculiarity in the names of certain numerals which must not pass unnoticed. Counting as he does, the savage, on reaching 5 says, not unnaturally, "one hand." At 10 he says, following the same anology, "both hands." At 20, having completed the tale of counters which Nature has placed at his disposal, he says, "one man." Though by no means universal, these names for 5, 10, and 20 are so common in all parts of the world that specific examples of them need not be given. We also find 6, 7, etc., often expressed by "hand one," "hand two," or "one on the other hand," "two on the other hand," etc.; and 11, 12, etc., by "one on the foot," "two on the foot," etc. So frequently are these equiva-

* Müller, *Grundriss der Spr.*, B. II., 1 Abt., p. 389.

† *Op. Cit.*, p. 14.

‡ *Op. Cit.*, p. 89.

lents found that they are recognized by competent authority as underlying one of the most common of the methods of numeral formation.*

In a consideration of the subject of primitive number one thought must be kept prominently in mind. Savage races may have numerals by means of which they count to 5, 10, 20, 100, or even 1,000, beyond which they rarely venture. But it by no means follows that, after passing beyond the very smallest, they have any exact notions of the numbers they are using. As long as they can check off on the fingers, or by means of pebbles, sticks or shells, they undoubtedly have a fairly distinct idea of the totals they name. But want of familiarity with the use of numbers, and lack of any convenient means of comparison, must result in extreme indefiniteness of mental conception, and must, when recourse can not be had to counters of some kind, inevitably give rise to great vagueness in the use of numbers. This has been noted and commented on by many observers, Humboldt among them, who remarks † that he never met an Indian who on being asked his age would not answer indifferently 16 or 20.

The statement has been made above that tne number systems of savage races rarely, if ever, extend beyond 1,000. A single observation respecting the development of the systems of civilized races may not here be out of place as showing of how universal application is this statement. The number systems of the civilized world to-day are of unlimited extent. But we need go back but a few centuries to find a time when our own systems were as diminutive as are those to which reference has just been made. Although in response to the demands of commerce and science our English system has been brought to its present elastic condition, the evidence of language shows conclusively that our Teutonic ancestors stopped at the same limit that has been named as the maximum for savage tribes. The higher numeral words of our language, *million*, *billion*, *trillion*, etc., are all borrowed words; while the word *thousand* is pure Saxon, like the words *one*, *two*, *three*, *ten*, *hundred*, etc. The German, the Scandinavian and other languages have borrowed their higher numeral words, and the same statement is probably true of the French. Other languages, like the Chinese, Sanskrit, Aztec, etc., contain only native numeral words; but however high these systems may be found to extend, there can be no doubt that they were at one time limited to a single thousand, and perhaps less. In this connection it is instructive to observe the number limits of the half civilized nations of the present day. The tribes of Arabia, the Persians, the Abyssinians, and most of the North African peoples have number systems terminating with *eleph* or *alph* (1,000). The Laplanders and the Erse have no words higher than *zhioette* and *ciad*, respectively, each of these words signifying 100. In ancient times the Latins were content with *mille* (1,000), and the Greeks with *μύριας* (10,000), as their number limits, and the Malays of to-day with *ribou*, also meaning

* Chase, *Proc. Am. Phil. Soc.*, 1865, p. 23.

† Pers. Narrative, vol. v, p. 165.

1,000. In general it may be said that in the childhood of any race the number concept is weak and the number system is correspondingly limited. As civilization develops the number sense and the number system are extended accordingly. To this law some remarkable exceptions have been noted, but they are exceptions and do not in the least invalidate the rule.

Respecting the bases used in the number systems of the various languages of the world, no full and comparative account has ever appeared in English. From the earliest times in which arithmetic began to assume the dignity of a science and its history to receive serious attention, it has been handed down as a tradition, the truth of which was never questioned until recently, that all races throughout the world used in their computation the decimal system. Aristotle indeed mentioned one obscure Thracian tribe which was said to reckon with a different base, but he seems to have regarded this solitary instance as the exception which proved the rule, for he taught that the universality of the decimal system proved it to have had its origin in nature. This tradition was for centuries accepted as true without question, and the naturalness of the decimal system was argued from the fact that the number of counters, the fingers, with which nature had equipped man, was ten. But the last three or four centuries have brought to the knowledge of civilization a multitude of tribes hitherto unknown, and among them a very great number have been found to use systems having some base other than 10. It was also pointed out by Peacock* and others that 10 was not the only natura lnumber base—that, as 5 was the number of fingers on one hand, and as 20 was the number of fingers and toes combined, either 5 or 20 constituted a base in all respects as natural as 10. Hence the use by any race of either of these numbers for that purpose could constitute no ground for surprise. Peacock indeed mentions many examples of Indian, negro, and Mongolian tribes, among whom such bases are actually used, and his list can now be enormously increased, so great has been the energy and activity displayed in anthropological research during the last half century. That 10 has been, and is, the practically universal base of the world's number systems is indisputably true, but to this general law the list of exceptions has been found to be so great that a brief consideration of the subject seems desirable.

Of all the numbers capable of use as a system base, 12 presents the greatest number of practical advantages. We have, through the familiarity which custom has produced, become so accustomed to the use of 10 in that capacity that the assertion just made seems unwarrantable. But a moment's reflection will show that the ten fingers of the human species have entailed upon us a number base decidedly inferior to 12. In the simple business affairs of life we deal most extensively with the three simple, familiar fractions, $\frac{1}{2}$, $\frac{1}{3}$, and $\frac{1}{4}$, and the auxiliary fractions $\frac{2}{3}$ and $\frac{3}{4}$. Such being the case it needs no argu-

*_Encyclopædia Metropolitana_, article "Arithmetic."

ment to prove that the most convenient base is that which will admit of division without a remainder by the numbers 2, 3, and 4. Ten can be divided by but one of these numbers without remainder; hence the confusion of fractions is at once introduced. Twelve, on the other hand, is an exact multiple of each of the three numbers. It offers, then, to the mass of mankind an enormous advantage over 10 or any other small number as a base for computation. With the growth of business in its many forms, the civilized world has long since come to recognize this fact, and in many ways to make practical use of it. The word "dozen," and its equivalent in other languages, has been coined as a noun to express the number 12, and in a very great number of the commercial transactions of the world the dozen and its square, the gross, are the common units of measure. So palpable are the advantages of 12 from this point of view that some writers have gone so far as to advocate the entire abolition of the decimal system and the substitution of a duodecimal system in its place. Charles XII, of Sweden, may be mentioned as an especially zealous advocate of this change, which he is said to have had in actual contemplation for his own dominions at the time of his death. The adoption of the duodecimal notation would involve the introduction of two new symbols, for 10 and 11, respectively. Twelve would then be represented by 10, thirteen by 11, fourteen by 12, twenty-four by 20, one hundred and forty-four by 100, etc. No such change can ever meet with general favor, so firmly has the decimal scale become intrenched; but it is more than probable that the world of trade and commerce will continue to use the dozen, its fractions and its multiples in many of its transactions in the future, as it has for centuries in the past. It was thus used by the Romans, and it has been and is used among all Teutonic nations at the present day. It is more than probable that the English divisions of weights, measures, and money were influenced by the ease with which mental computation is effected when fractional parts of 12 are involved. The duodecimal is not a natural scale in the same sense as are the decimal, the quinary and the vigesimal; but it is a system which is brought into use at a later day and at a higher stage of development, solely through its convenience when applied to the everyday transactions of business life. Humboldt, in discussing the number systems of the various peoples he had visited in his travels, remarked that no people had ever used exclusively that best of bases, 12. A possible exception to this has since Humboldt's time been noted by Robert Flegel, in the Aphos of Benuë, who count by simple words to 12, and then proceed with 12 and 1, 12 and 2, 12 and 3, etc.*

Remarkable as it may at first glance seem, the number 2 has in a few scattered instances been made to do duty as the base of number system. Thirion says† it was thus employed by Egyptian surveyors;

* Schubert, H., in Neumayer's *Anleitung zu Wissenschaftlichen Beobachtung auf Reisen*, p. 290.

† Histoire de l'arithmetique, p. 5.

in our own language we find an occasional hint of the same thing in the words "pair," "brace," "couple," etc.; obscure traces of a binary number system appear on some of the early Chinese monuments, but we have no real evidence that such a system was ever definitely and exclusively used by the Chinese. Certain savage tribes, however, count exclusively or in part by twos. The Baccaraaibi, a South American tribe of the Xingu region, count only to 6. But they call 4, 2 and 2, 5, 2 and 2 and 1, and 6, 2 and 2 and 2. The least developed of the Australian tribes are in many cases found to reckon in the same way.* The structure of the Arikara numerals would indicate that this people counted at first exclusively by pairs, the odd numbers being interpolated afterwards.† The lowest of the native tribes of the East Indian Archipelago count upon a binary scale, if indeed they can properly be said to use any.‡ Examples of this kind might be multiplied to a very considerable extent. But it should not be overlooked that these are hardly to be considered as fair examples of the use of *any* system. The tribes mentioned have no form of notation other than repeating scratches or piling pebbles; and their numeration is of the rudest kind. All that can be said is that, as far as any system is used among them, that system is the binary. Making the same qualification, we may note that the Cuchans of Colorado count with a mixed ternary and quaternary scale, expressing 6 by the phrase "2 3's," 9 by "3 3's," and 8 by "2 4's;"§ and that the Lulos of South America, the Triton Bay and the Endé Polynesians count with a quaternary scale, expressing numbers as far as 4 by simple words, and then counting 4-1, 4-2, etc. The last-named tribe gives a further indication of the use of the quaternary scale by using for 8 a word signifying "2 4's."‖ Occasionally we come, in the midst of some other well-defined system, upon sporadic traces of reckoning upon 4, 6, 8, or 9 as a base. The Wallachians, for example, say "deu-maw," 2-9 for 18. The Bretagnes call 18 "trioneheb," 3-6. But otherwise these languages contain no trace of the senary or octonary scales. Pott states** that the Bolans of West Africa appear to use 6 as their number base; but aside from this solitary instance we know of no tribe which employs 6, 7, 8, or 9 for that purpose. The most remarkable example of tribal eccentricity in this particular is that of the Maoris, of New Zealand, whose number base is 11. To that number they count by means of simple words; 12, 13, 14, etc., are with them 11-1, 11-2, 11-3, etc.; the multiples of 11, as 22 and 33, are formed directly on the word for 11; and the square and cube of 11, or 121 and 1331, are expressed by simple words having no connection with the names of smaller numbers.†† Occasionally a rude number system occurs which shows no

* Letourneaux, *Bull. Soc. Anthropol.*, Paris, 1886, p. 91.

† Trumbull, J. H., *Proc. Am. Antq. Soc.* 1875, p. 76.

‡ Marre, A. De *l'arithmetique dans l'archipel Indien*, p. 7.

§ Trumbull, J. H., *Trans. Am. Phil. Ass'n*, 1874, p. 46.

‖ Marre, A., *op. cit.*, p. 7.

** *Die Sprachvershiedenheit*, p. 30.

†† Neumayer, *op. cit.* B. II, p. 229.

trace of a base, the numbers, as far as they extend, being independent of each other. Such cases are, however, necessarily rare.

The only remaining example that need be mentioned of the use of an unusual number as the base of a system, is the Babylonians. As is well known, the base of their number system was 60, the largest number ever used for such a purpose. To the modern world, the fact that the Babylonians used 60 as their unit of reckoning is most important, for that fact has entailed upon us a sexagesimal system of astronomical computation.

With the exception of a small number of isolated cases, such as those mentioned above, it may be laid down as a universal law that every language containing a number system extending beyond 5 reveals the use of one of the three numbers, 5, 10, or 20, as the base of that system. Each of three numbers requires extended mention.

One of the most interesting points to be met with in connection with the study of numeral words, is the resemblance found to exist in many languages between the words for "hand" and "five." Counting as they do, by means of their fingers, savage races naturally use for five some expression like "one hand," or "a hand finished," or simply "hand." Then, proceeding with their count, they begin to build on this as a base, using for 6, 7, 8, etc., the expressions, "hand one," "hand two," "hand three," etc. In such a system, 10 is, of course, "two hands." Counting above 10, we find two common methods practised. The fingers being finished by the count up to 10, recourse may be had either to the toes or to the fingers of a second man. In the former case, 11 would be "one on the foot." Twenty, completing the tale of both fingers and toes, is called "one man." Beyond this point there is less uniformity in the method of counting than before, but examples are numerous of tribes which use exactly this method up to 100, calling 40, 60, 80, and 100, "two men," "three men," "four men," and "five men," respectively. But, as will be noted later, the use of the pure quinary scale above 20 is rarely if ever found. With tribes having a limited number sense, however, tribes whose systems do not extend beyond 10 or 15 or 20, this scale is almost as common as the decimal. The naturalness of this scale is very evident, and, as compared with the decimal, the wonder is, not that the quinary scale is so very common, but rather that it is not more common than the decimal. The reason for this will appear when we come to consider the latter.

Examples of the use of the quinary scale are common in all parts of the world.* It is the scale of many of the native North Siberian tribes, of the Aleuts, the Kamtschatkans, and at least three of the tribes of the New Hebrides. In Africa we note the Wolofs and Bulloms, which were visited by Mungo Park, the Kanuris, the Temnes, the

* The numerals of the tribes here mentioned, but for which no specific reference is made, are found in Müller, *op. cit.*, or in Pott, *Die Quinare und vigesimale Zahlmethode bei Völkern aller Welttheile.*

Efiks, and two tribes visited by Stanley: the Ki-yaus and the Ki-Nyassas.* These and a number of others use practically a pure quinary scale. The Dinkas, the Fulbes, the Pigmies,† and others use a mixed quinary and decimal scale, while the Nupes and one or two other tribes employ a quinary-vigesimal system. Among the Australasians and Polynesian islands abundant traces are found of quinary number systems, but they are in almost all cases nothing more than traces. Throughout that part of the world the quinary system has been superseded by the decimal. This has been widely spread through the islands of the Pacific and Indian Oceans by the Malays, who in turn obtained it from the Hindus. But the home par excellence of the quinary, or rather of the quinary-vigesimal scale, is America. It is practically universal among the Eskimo tribes of the Arctic regions. It prevailed among a considerable portion of the North American Indian tribes, and was almost universal with the native races of Central and South America. So numerous are the examples which might be given, that mention will be made rather of the exceptions, that is, of those using the decimal base. It is interesting to note also that a considerable number of languages show that the quinary system was once in use among peoples which, with the development of civilization, discarded that system for the decimal. The Greeks of Homer's time used a system in which traces of the quinary base are observable. The common Roman notation shows clearly that the ancient Romans made at least a limited use of the same base, as did also the Persians.

The exclusive use of 5 as a number base is never found in any system of any considerable extent. Whenever the quinary system is extended beyond the narrowest limits it invariably runs into either the decimal or the vigesimal. Touching this point Hankel says †† that no race, even though it began its number system on the quinary base, ever expressed 10 by 5-2 or 2-5, but always by a simple word; and hence that the system passes immediately into the decimal. This statement is only partially correct. The quinary in many instances runs into the vigesimal, no trace whatever of a decimal base appearing. Furthermore, even though 10 is never expressed by 5-2 or 2-5, it is often expressed by "two hands" or "both hands." Mungo Park observed this among the Yolofs and Foulkas of Africa; Humboldt and others among the Omaguas, the Zamuscas, the Tamanacs, the Toupinambos, and many more of the South American tribes; and Russian explorers found the same method common among the native Siberian races. Hence the statement as the German historian makes it needs important qualification.

Vigesimal-number systems are less common than quinary, but as the two are so persistently interwoven together it is difficult to separate them from each other. The use of a base as large as 20 must necessarily be cumbersome, and it can constitute no ground for sur-

* Stanley, *Through the Dark Continent,* II, p. 486.

† *Op. cit.* II, p. 492.

†† *Geschichte der Mathematik,* p. 20.

prise that either 5 or 10 should in general be preferred for that purpose. It is a matter of some surprise, however, that the quinary should in so many cases merge into the vigesimal rather than the decimal system.

The vigesimal system is never found entirely pure. Examination always shows some trace either of the quinary or the decimal system subordinate to it. Among the native races of America it is almost as common as the quinary and is more common than the decimal, but it is there always found mixed with either the one or the other. The same commingling is observed among Asiatic and African tribes. The elaborate Aztec system is the most perfect known example of the vigesimal-number system, but it contained both the quinary and the decimal scales subordinate to the vigesimal. The Muyscas of Bogota possessed an exceedingly elaborate and extended vigesimal system, but the decimal is used to supplement it. The same is true of the Basques of northern Spain.

For some unexplained reason vigesimal-number systems are rare in the Old World. The only European example I am able to cite is the Basque system. The Ainus of northern Siberia reckon by twenties, and a number of the tribes of the Caucasus do the same. In Africa this mode of counting is almost unknown, only two or three examples of it being on record. It is only in America that vigesimal-number systems have flourished and held their own. But it is a noteworthy fact that in ancient times 20 was the number base used in many parts of Europe, as is attested by abundant traces in the modern European languages. The Phœnicians, and presumably the Carthagenians, also used this method of reckoning, and through contact with them the Celtic nations of western Europe gradually became familiarized with it. From using it in commercial intercourse with these traders from the Mediterranean they may have adopted it as their own scale. Certain it is that the vigesimal-number system was a strongly marked characteristic of all the Celtic races, as their languages unequivocally prove. The Bretons still say "unnek ha tringent," 11 and 3 20's, for 71. The French say "quatre vignt" for 80, and from that point to 100 count upon a pure vigesimal scale, as far as the names of their numbers are concerned. The Welsh, the Erse, the Gælic, the Manx, and other Celtic races show in their languages similar traces of a former use of the vigesimal base. Singularly enough, like traces are to some slight extent found among Teutonic languages also, but they are so infrequent as to indicate but little and to prove nothing. A hundred consisting of 120, and known as "the great hundred" or "long hundred," was formerly in use in England, and was legal for eggs, spars, and certain other articles. That this was a common use would appear from the popular old distich quoted by Peacock:*

> Five score of men, money, and pins,
> Six score of all other things.

* *Encyclopædia Metropolitana,* vol. I, p. 381.

The very word "score," and one or two happily preserved expressions, as "three score and ten," show that an unconscious flavor of the vigesimal scale was to be found in the England of a few centuries ago. The Danish and other Teutonic languages contain words and expressions which indicate that the same was true of other north European countries. But here the reckoning by 20's seems always to have been restricted to material objects rather than applied to pure number; so that the Teutonic number systems can not be said ever to have been vigesimal systems. Ancient Palmyra possessed a number system of great extent which was almost purely vigesimal. But scanty traces of it remain however.

We have last of all to consider the decimal scale. However great the number of examples that may be given of races that have used or now use the quinary or the vigesimal scale, the fact remains that by far the greatest number of uncivilized people perform their reckoning by tens; and that, with five or six exceptions, all civilized peoples have done the same. The decimal scale is universal in Europe; in Africa it is almost universal; in Polynesia the same is true; in Asia all civilized peoples and the great majority of the uncivilized tribes count with this base; in North America it is used by the greater number of the Indian tribes; and in South America it is sometimes found, though the prevailing base is quinary or quinary-vigesimal. The simple and undoubtedly the correct explanation of the origin of this system is the laying aside of the counter, or the scoring of one mark on the completion of each tale of 10 on the fingers. This develops into a perfect decimal system, and needs only the device of characters for the representation of number to become a written number system like the Roman; or with value of place like the Arabic system of the present day. That it is preferable to either the quinary or the vigesimal scale is a fair inference to be deduced from the numerous instances in which it has superseded the one or the other. As a number base 5 is too small and 20 is too large. Probably no single-number scale would serve the needs of mankind better than the decimal with the single exception of the duodecimal. But the advantages of 12 as a base never become apparent until the arithmetic of a people has reached a degree of development such that a change in the scale used would be attended with difficulties so great as to render such a thing altogether impracticable. Civilization is apparently wedded to the decimal system; and though it may continue to barter by 12's and to perform its astronomical computations by 60's, it will always continue to use the arithmetic of 10's in preference to any other. It seems probable also that the decimal scale, already in use among all civilized nations and among the native races of so large a portion of the world, will tend more and more to displace the quinary and the vigesimal scales, and to become at last in reality what it was in the minds of the ancients, the universal number scale of the world.

THE ANTHROPOLOGY OF THE BRAIN.*

By D. Kerfoot Shute, M. D.

By "anthropology of the brain" we understand several distinct, but closely related, sciences, viz. the anatomy of the brain, its physiology, psychology, ethnology, etc.

In the short space at my disposal it will be impossible to do anything more than briefly mention the more important facts bearing upon this interesting subject.

We will refer briefly, in the first place, to the more salient features of the anatomy of the brain.

This complex viscus may be looked upon as a hollow bag, with several constrictions at different places, whose wall is trilaminar, the innermost layer of which is named ependyma, the middle one the nerve tissue, and the external layer the pia mater.

The inner (ependyma) and outer (pia mater) layers are quite thin, and may be said to maintain a uniform thickness; but the middle (nerve-tissue) layer possesses very varying degrees of thickness at different points in the wall of this hollow bag—the brain.

The nerve-tissue layer may be very thick at some places, and it may be entirely absent at other points. In this latter case the integrity of the cavity of the brain is maintained by the ependyma and pia mater coming in contact, this bilaminar portion of the brain wall taking the general name of velum.

In order to give you a necessary outline idea of the anatomy of the brain, it will be best to refer rapidly, but succinctly, to the more salient features of the embryology of the brain. At an early stage of prenatal growth the brain consists of three primary vesicles; soon these three, by additional growth and constrictions, become *five* vesicles, all in a series, one in front of the other (a hollow bag with four constrictions and trilaminar wall).

The technical name for brain being encephalon, these vesicles are designated from before backward, pros-encephalon, thalam-encephalon, mes-encephalon, ep-encephalon, and met-encephalon.

The prosencephalon, at first the smallest and most anteriorly (preaxially) situated of all the segments, is destined to grow out of all proportion to the other segments. It grows in all directions, upward,

* A Saturday lecture delivered in the lecture-hall of the U. S. National Museum, under the auspices of the Anthroplogical Society of Washington.

forward, backward, and downward, and more or less completely hides from view the remaining and, morphologically considered, more fundamental segments. This great increase in size of the prosencephalon is due to the tremendous growth of its middle layer—the nerve-tissue layer.

The cavity of the prosencephalon is called the prosencephalic cavity (lateral ventricles of adult human anatomy); that of the thalamencephalon, the thalamencephalic cavity (third ventricle of human anatomy), the point of communication between the two cavities being known as the foramen of Monroe.

The cavity of the mesencephalon is called mesencephalic cavity (sylvian passage, or *iter a tertio ad quartum ventriculum* of human anatomy).

The cavity of the epencephalon is known as epencephalic cavity (pre-axial half of the fourth ventricle); that of the metencephalon is the metencephalic cavity (post-axial half of the fourth ventricle).

The "fourth ventricle" thus becomes a cavity common to two segments of the brain.

Along the "floor" of the prosencephalic cavity (lateral ventricle), in the form of a double horse-shoe, is a portion of the brain wall in which the middle (nerve-tissue) layer is wanting. At this place the ependyma and pia mater come in contact, forming the prosencephalic velum (erroneously called, in human anatomy, "transverse fissure"), by which the integrity of the encephalic cavity, at this place, is preserved.

Likewise there is a thalamencephalic velum (velum interpositum of human anatomy) in the "roof" of the thalamencephalon: a metencephalic velum in the "roof" (dorsal surface) of the metencephalon. In the latter velum is found the foramen of Magendie, the opening by which the cerebro-spinal fluid in the encephalic cavities communicates with that in spaces on the outside of the pia mater known as sub-arachnoidian spaces.

The nerve tissue of the prosencephalon (cerebrum) consists of "inner" or "white matter" and "outer" or "gray matter," the individual elements of which are bound together by a tissue called neuroglia. The gray matter, otherwise called cortex, is about 3 millimeters in thickness. The cerebrum has numerous fissures and convolutions on its surface—these for the purpose of increasing the area of gray matter without unduly augmenting the bulk of the brain. The superficial area of the cortex is about 200,000 square millimeters.

Two of the most important fissures of the brain are the fissure of Rolando and the fissure of Sylvius. In relation with the former fissure are found the great "motor areas" of the brain, and in relation with the "forking" of the latter is "Broca's center" (the center for speech).

In the adult the cerebrum is the largest portion of the brain. The next largest portion of this viscus is the cerebellum, which constitutes the great bulk of the epencephalon.

In the brief space at my disposal I can but mention two of the great physiological processes connected with the brain. Probably two of the most conspicuous activities of a human being are those of intellection and locomotion; and, in consonance with this fact, we find the two portions of the brain presiding over these functions, the most conspicuous and bulky segments of it. The cerebrum is the physical basis of intellectual processes and the cerebellum of locomotion, in that the latter is the great coördinating center of the brain.

Whether we extend our studies along the lines of phylogeny or ontogeny, we will observe that the rule is very general, almost universal, that those animals having the largest cerebra possess the greatest degree of intelligence, and those with the largest cerebella are capable of the most varied and complicated motions.

From an ethnological standpoint the size and weight of the human brain are facts of great interest and importance.

The size and weight of the brain are capable of being estimated by two methods, viz, the direct and indirect. The direct method is to weigh the brain when it is accessible. The indirect is to ascertain the cubical capacity of the cranium, and then deduce the weight of the brain that once occupied it. This latter method is particularly applicable in the study of the brains of ancient peoples, the skulls of which have been preserved to this time. The average weight of the human male adult brain is 1,390 grams. That of the female is 1,250 grams.

The average cranial capacity of any race can only be determined by careful examination of a large number of skulls classified according to sex; for sex exercises a most potent influence over cranial capacity, often exceeding the difference of race.

The following are some of the principal modifying conditions which influence cranial capacity and thence brain weight, viz, age, weight of body, stature, sex, race, vigor of intellect, and education.

The earlier anatomists believed that the human brain attained its maximum development at 7 years of age. We now know that this is incorrect; yet from extensive researches it has been found that the male brain does actually reach five-sixths of its ultimate weight by the end of the seventh year, and in the female ten-elevenths its ultimate weight at the end of the same period.

The average weight of the brain undergoes a progressive increase up to a point between the twentieth and fortieth years. The greatest average weight for the male brain is reached at from 30 to 40 years. Women reach the full average brain weight from the twentieth to the thirtieth year.

There is a slight diminution in weight from 40 to 50 years of age, and a still greater diminution from 50 to 60 years. The rate of decrease is much greater after 60 years. In the eightieth year the brain weight has decreased by from 80 to 90 grams. "In the aged, brain weight and intelligence decrease *pari passu*" (Thurnam).

It has been truly said that there are many exceptions to this general law, especially among people of culture and learning, "who often preserve to extreme old age all the fullness and vigor of their faculties.

"The brain of such men, as the late Prof. Gratiolet observes, remains in a state of perpetual youth, and loses little or none of the weight which belonged to it in the prime of life" (Thurnam). The ratio of brain weight to body weight varies. In lean persons the ratio is often as 1:22 to 27; in stout persons as 1:50 to 100. The human brain is smaller in comparison with the body the nearer man approaches to his full growth.

As to stature, the weight of the brain, in both sexes, is relatively smaller in short persons than in tall ones. The difference between the two is about 5 per cent, *i. e.*, the brain of a man of short stature being represented by 95, that of a tall man would be 100.

The average weight of the adult male brain is about 10 per cent greater than that of the female. Nor is this difference due to the difference in stature of the sexes. The difference, as was shown by M. Parchappe, is greater than can be accounted for in this way. While the stature of woman is only 8 per cent less than that of man, her brain weight is 10 per cent less.

In relation to this question of the difference of cranial capacity due to sex, it is very interesting to note the remarkable fact, pointed out by Vogt, that the difference increases in favor of the male as the development of the race proceeds, so that the male European excels much more the female than the negro the negress.

In the words of Vogt, "The lower the state of culture, the more similar are the occupations of the two sexes. Among the Australians, the Bushmen, and other low races, possessing no fixed habitations, the wife partakes of all her husband's toils, and has, in addition, the care of the progeny. The sphere of occupation is the same for both sexes; whilst among civilized nations there is a division both in physical and mental labor. If it be true that every organ is strengthened by exercise, increasing in size and weight, it must equally apply to the brain, which must become more developed by proper mental exercise."

Le Bon has pointed out that the difference existing between the cranial capacities of the male and female modern Parisians is almost double that which obtains between the cranial capacities of the male and female inhabitants of ancient Egypt. These facts show the intimate, and mutually reacting, relations of civilization and brain weight; advancing civilization leading to increased development of the brain, and the enlarged brain making the people capable of higher and broader culture.

The average brain weight in different races of men has mostly been studied by the indirect method, *i. e.*, by the investigation of cranial capacities. Skulls having a cranial capacity of 1350–1450 cubic centimeters are classed as mesocephalic; those under 1350 cubic centimeters are microcephalic; those above 1450 cubic centimeters are megacephalic.

Below are given average brain weights in male adults of different people:

	Grams.
Scotch (Peacock)	1,117
English (Peacock)	1,388
English (Boyd)	1,351
Germans (Wagner)	1,371
French (Parchappe)	1,358
Negroes (Peacock)	1,255

Thurnam says that the average brain weight of the male negro is the same as that of the female European. - - -

What kind of brain weights are found among men of great mental powers and acquirements? The following table will show:

Brain weights of distinguished men (Thurnam).

Name.	Occupation.	Age.	Weights.
			Grams.
Cuvier	Naturalist	63	1,830
Abercrombie	Physician	64	1,785
Schiller	Poet	46	1,785
Daniel Webster	Statesman	70	1,516
Agassiz	Naturalist	66	1,512
De Morgan	Mathematician	73	1,496
Grote	Historian	76	1,410
Whewell	Philosopher	71	1,390
Hermann	Philologist	51	1,358
Hughes Bennett	Physician	63	1,332
Tiedemann	Anatomist	80	1,254
Hausman	Mineralogist	77	1,226

The above-named men have been among the foremost representatives of human intelligence.

The list is very interesting, not only from the fact that it includes some very high brain weights (which we would naturally expect from their high intellectual attainments), but also from the fact that it includes brain weights of four distinguished men which fall distinctly below the average (1390 grams), even when allowance is made for atrophy consequent upon age.

These facts, and many others that can be mentioned, naturally raise the question, "Is there any invariable connection between intelligence and mere weight or size of brain?" Before answering this question we desire to cite a few additional facts.

Very high brain weights are not only found among men of great intellectual attainments and culture, as noted in the above table, but also among very ordinary sane individuals and among epileptics and insane persons. Dr. Bucknill records a brain weight of 1830 grams for a male epileptic. This was the brain weight, it will be observed, of the celebrated Cuvier.

Dr. Spae records the heaviest female brain weight on record that I can find. The patient was not epileptic but "labored under a monomania of pride," dying at the age of 39. The brain had, for a female, the astounding weight of 1743 grams.

The heaviest human brain on record, as far as I have been able to ascertain, belonged to a man who was perfectly sane and healthy, but of very ordinary mental attainments. The man from whom it was taken was 38 years of age, a bricklayer, and died from blood poisoning, after a surgical operation, in a London Hospital in 1849. Dr. James Norris says of this brain:

"The weight of the brain, taken immediately on removal, exceeded 1945 grains. This weighing was most carefully made, and was witnessed by several students. The brain was well proportioned; the convolutions were not flattened, though the surface was fairly moist; it only lost about 32 grams weight after the usual dissection and draining for two hours. The man's height was 5 feet 9 inches, and he was of a robust frame. It was difficult to obtain any satisfactory history of him—his wife and his landlady gave different accounts. It seemed, however, that he was a native of Sussex, England; that he had left his native village and changed his name on account of some poaching troubles; that he was not very sober; had a good memory and was fond of politics. He could neither read nor write." - - -

How are these facts to be reconciled with one another? In this way. It is now universally held that it is the gray nerve tissue in the front portion of the cerebral hemi-spheres (prosencephalon) that has to do, more particularly, with the intellectual activities, the white nerve tissue consisting, essentially, of nerve threads that conduct impulses to and from the gray matter. The nerve elements, as stated above, are bound together and held in place by a form of connective tissue called neuroglia. The neuroglia has no connection, whatever, with the generation or conduction of nerve impulses—it is merely a supporting tissue. If this tissue, in consequence of disease, increases much in quantity it may add very much to the weight of the brain—as occurs in epileptics—without increasing the gray matter which is concerned with mental processes. In reality the increase of neuroglia decreases the gray matter and thus deteriorates the mind.

Or, if there is no increase in the neuroglia, there may be, with a large brain, an unduly small amount of gray cortex on account of a comparatively small number of fissures and convolutions. Or, again, the cortex of gray matter may not reach the average thickness. Or, the texture of the brain may be poor—its microscopic elements feeble and poorly related and correlated.

Thus it may be understood that a comparatively small brain—one below the average brain weight—may be capable of vastly finer and better work than a much larger one.

So, in answer to the question, "Is there any invariable connection between intelligence and mere weight or size of brain?" we answer, decidedly, "no."

Should the question be asked whether a larger number of mega-cephalic brains is likely to be found among races of high intelligence and culture, we have the answer of Le Bon emphatically in the affirmative, and it is in this direction, as Le Bon has taught us, that we must look for evidences of social superiority. As illustrating this proposition, the following table of percentage of Le Bon will prove very interesting and instructive:

Percentage of cranial capacity in different human races (Le Bon).

Cranial capacity.	Modern Parisians.	Parisians of the twelfth century.	Ancient Egyptians.	Negroes.	Australians.
Cubic centimeters.					
1200-1300. .	0.0	0.0	0.0	7.4	45.0
1300-1400.	10.4	7.5	12.1	35.2	25.0
1400-1500.	14.3	37.3	42.5	33.4	20.0
1500-1600	46.7	29.8	36.4	14.7	10.0
1600-1700.	16.9	20.9	9.0	9.3	0.0
1700-1800.	6.5	4.5	0.0	0.0	0.0
1800-1900.	5.2	0.0	0.0	0.0	0.0

In connection with this table it is interesting to remember that Le Bon says: "The cranial capacity of the gorilla often reaches 600 cubic centimeters, so that it follows that there are a large number of men more allied by volume of brain to the anthropoid apes than they are to some other men."

Among other things, this table reveals the interesting fact that in the course of seven hundred years of advancing civilization the average Parisian cranial capacity has distinctly increased in volume.

It may be well to state, in conclusion, that Broca estimated that a brain in the male, weighing 1,049 grams, is the lowest limit compatible with ordinary human intelligence; in the female 907 grams. Human beings with brain weight's lower than Broca's figures are idiots, etc.

THE BIRTH OF INVENTION.*

By Otis T. Mason.

In this apotheosis of invention and inventors, to me has been assigned the pleasing task of leading you back for a few moments to the cradle of humanity. Those are happy hours to most of us when we recall the days of childhood. To trace the lives of celebrated men and women to the springs of their moral and intellectual power brings never-fading delight. To study the rise and progress of a nation or any social unit is worthy of exalted minds. But the most profitable inquiry of all is the search for the origin of epoch-making ideas in order to comprehend the history of civilization, to conjure up those race memories in which each people transmits to itself and to posterity its former experiences.

Every invention of any importance is the nursery of future inventions, the cradle of a sleeping Hercules. But my task is to speak of primitive man and his efforts.

It will aid us in prosecuting our journey backward to orient ourselves with reference to the present. For two days we have listened to the eloquent papers of my predecessors, written to glorify the nineteenth century. Through this faculty of invention the whole earth is man's. There is not a lone island fit for his abode whereon some Alexander Selkirk has not made a home. Every mineral, plant, and animal is so far known that a place has been found for it in his *Systema Naturæ*. Every creature is subject to man; the winds, the seas, the sunshine, the lightning do his bidding. Projecting his vision beyond his tiny planet, this inventing animal has catalogued and traced the motion of every star.

But his crowning glory (which always fills me with admiration) is his ever increasing comprehensiveness. After centuries of cultivating acquaintance with the discrete phenomena around him, he has now striven to coordinate them, to make them organic, to read system into them. He has learned by degrees to comprehend all things as parts of a single mechanism. Sir Isaac Newton and Kepler conceived all objects and all worlds to be held by universal gravitation. And thus, in our century, von Baer and Humboldt taught that the world, in all its forces

* An address on the occasion of the centennial celebration of the organization of the U. S. Patent Office; delivered in Washington. *Proceedings and Addresses*, 1891, pp. 403–412.

and materials, is an integrated cosmos. Anyone who is the least familiar with the progress of philosophy will recall that since the dawn of written history the thoughts of men were tending to this unification. Shortly after this first effort at comprehensive unity Mayer, Rumford, and Joule invented the methods of demonstrating the oneness of physical forces, the conservation of energy. Wollaston, Kirchoff, and Bunsen devised the delicate apparatus to prove the chemical identity of all worlds. Lamarck, Geoffroy St. Hilaire, and Darwin taught the consanguinity of all living beings. Helmholtz and Meyer coordinated nervous excitation with mental activity. Comte and Spencer grasped the unity of all sensible phenomena. Newton, Leibnitz, and Hamilton projected their minds beyond phenomena and invented mathematics of four or more dimensions, conceiving of worlds and systems that under the present order of nature can have no objective reality. Over all this, into many great souls, have come the notions of infinite space and time and causation. The idea of limitation to thought or achievement no longer enters the imagination. The depth of the sea, the distances of the stars, the concealment of the earth's treasures, the minuteness of the springs of life and sense, the multiplicity and complicity of phenomena are only so many incitements to greater achievements. The daring souls of this decade are determined at any risk to answer the inquiry of Pontius Pilate, What is truth? With sympathetic enthusiasm we wave them on, bidding them God-speed.

But, I ask you now to forget all this and go with me to that early day when the first being, worthy to be called man, stood upon this earth. How economical has been his endowment. There is no hair on his body to keep him warm, his jaws are the feeblest in the world, his arm is not equal to that of a gorilla, he can not fly like the eagle, he can not see into the night like the owl, even the hare is fleeter than he. He has no clothing, no shelter. He had no tools or industries or experience, no society or language or arts of pleasure, he had yet no theory of life and poorer conceptions of the life beyond.

The road from that condition to our own lies next to the infinite. The one endowment that this creature possessed having in it the promise and potency of all future achievements, was the creative spark called invention. The superabundant brain, over and above all the amount required for mere animal existence, held in trust the possibilities of the future, and stamped upon man the divine likeness. This naked ignoramus is the father of the clothed philosopher, looking out into infinite space and time and causation. It may give you pleasure to know something about the connections between these two and the witnesses to these connections.

There are five guides whose services we have to engage on our interesting journey. The first is history, who does not know the way very far back—not over three thousand years—with much certainty. The second is philology, the study of which in our own century has enabled us to find the cradle-land of many peoples. The third is folk-lore, the survival of belief and custom among the uneducated. The fourth is archaeology, history written in things. The fifth is ethnology, which informs us that in describing this arc of civilization some races have only marked time, while others have moved with radii of varying lengths. The result of this is that we now have on the earth types of every sort of culture it has ever known. At the present moment, within hailing distance of yonder most beautiful dome in the world dwell all these witnesses—the relics of the stone age, the Indian village of Nacochtank or Anacostia, the folk-lore of both continents, and the literatures of the world. While you are listening to the encomiums of our decade, palaeolithic man sends in the testimony of his handicraft, the Smithsonian Institution treasures the inventions of the most primitive races, and the Bureau of Ethnology unravels the mysteries of savage tongues.

As the fragment of a speech or song, a waking or a sleeping vision, the dream of a vanished hand, a draught of water from a familiar spring, the almost perished fragrance of a pressed flower, call back the singer, the loved and lost, the loved and won, the home of childhood, or the parting hour, so in the same manner there linger in this crowning decade of the crowning century bits of ancient ingenuity which recall to a whole people the fragrance and beauty of its past.

From the testimony of these five witnesses we learn that there never was a time when man was not an inventor—never a time when he had not some sort of patent on his invention. They affirm that every art of living and all the arts of pleasure were born in the stone age; that graphic art, sculpture, architecture, painting, music, and the drama had their childish prototypes in that early day; that language is one of the very earliest of inventions, the vehicle of savage oratory, philosophy, and science. They affirm that society has been a series of inventions from the first; that legislation, justice, government, property, exchange, commerce, have not sprung out of the ground, but within our definition are inventions. And even the creeds and cults of mankind, whatever view you may take of the divine element underneath them, have been thought out and wrought out with infinite pains from time to time by earnest souls. But they had their origin in the cradle land and in the infancy of our race. What we enjoy is only the full-blown flower, the perfected fruit of which they possessed the germ. Let me enforce this idea, as we glorify the material prosperity of the nineteenth century, that many centuries ago men sat down and with great pains and sorrow invented the language, the art, the industries, the social order which made our machines feasible and desirable.

There is no conflict between the testimony of these witnesses and the doctrine commonly taught that men do not invent customs and languages, but fall into them. Reflect a moment upon your own daily life and you will recognize two sets of activity, those which you originate and those in which you follow suit. Animals can learn to follow suit, and to a very limited extent can originate. But it is the spark of originality which underlies every thought or device in this world. As one man invents a machine and others by thousands fall into the use of it, as the musician composes a song and millions sing it, so was it in the cradle-land of humanity the inventor, touched with fire from the divine altar, set new examples to be followed. If we were to interrogate our five witnesses, particularly with reference to the ancestry, the family tree of the notable inventions of the nineteenth century, their answer would be somewhat as follows:*

The ancestor of the steam plow is the digging stick of savagery, a branch of a tree sharpened at the end by fire; the progenitors of the steam harvester and thresher were the stone sickle, the roasting tray, or, later on, the tribulum.

The cotton gin and power loom are among the wonders of our age. Yet in that day of which we are speaking human fingers wrought the textile from first to last. They gathered the bark or wool, colored them to suit the primitive taste, spun and wove them with simple apparatus, and left upon the fabric patterns that are the despair of all modern machine-makers—patterns that are a pleasure to the eye by their infinite variety, replaced in modern fabrics by a dreary monotony that awakens pain instead of pleasure.

The first sewing machine was a needle or bodkin of bone, with dainty sinew thread from the leg of the antelope, and for thimble a little leather cap over the ends of the fingers. Coarse, indeed, the apparatus, but the hand was deft, the eye was true, the sense of beauty was there, and so that needlewoman of long ago wrought in fur from the mammals, feathers from the birds, grasses from the fields, shells from the sea, wings from the beetle, and skins of snakes with tasteful geometric figures. You do err who think those ancient needle-women had no taste. It would be hard to invent a pattern now that was unfamiliar to them.

The first engine was run by man power, then man subdued the horse, the ass, the camel, and invented engines for those to propel. He next domesticated the winds, the waters, the steam, the lightning, but the first common carriers and machine power were men and women. The first burden train was women's backs; the first passenger car was a papoose frame.

The poetry of to-day is the fact of yesterday; the dream of yesterday is the fact of to-day. When the savage woman a century or two ago, upon this very spot, strapped her dusky offspring to a rude frame, hung it upon the nearest sapling for the winds to rock, or lifted the unfor-

* We ought to remember, however, that an invention is not always a thing; but that it may be any series of actions conducing toward some new end. We should keep in mind, also, that all our activities involve materials and their qualities; human, animal, and physical forces; tools and machines; processes, and products; and that invention may take place in any or all of these.

tunate suckling from the ground to which it had been hurled by the bending of an unsafe bough, that was a fact, a stage in the history of invention. In our now-a-days couches of down, swung from gilded hinges, we have got far ahead of the papoose cradle, the memory of which we perpetuate in nursery rhymes sung to children, who wonder why babies should be hung in the tops of trees and think, doubtless, that the falling cradle was a just retribution on the silly parents.

What is more beautiful than an ocean steamer, with skin of steel drawn over ribs of steel and closed above against the intrusion of the waves? Have you never seen the picture of the Eskimo, still in the stone age, who, over a framework of driftwood or whale's rib, stretches a covering of sealskin and learned therein to defy the waves hundreds of years ago?

Only now and then the angry sky was lighted for the primitive man by electricity, and even then it filled him with terror. But it was he that invented the apparatus for conjuring from dried wood, by a rude sort of dynamo, the Promethean spark. It was our Aryan ancestors that paid their devotions to the rising sun by kindling fresh fire every morning as the orb of day flashed his first beam across the earth.

Who has not read, with almost breaking heart, the story of Palissy, the Huguenot potter? But what have our witnesses to say of that long line of humble creatures that conjured out of prophetic clay, without wheel or furnace, forms and decorations of imperishable beauty which are now being copied in glorified material in the best factories of the world? In ceramic as well as in textile art the first inventors were women. They quarried the clay, manipulated it, constructed and decorated the ware, burned it in a rude furnace, and wore it out in a hundred uses.

He had no printing press, but he could tie knots in a marvellous fashion and write letters on bark or on bits of raw hide and leave memorials of himself in the book of stone. He made words and sentences, invented language, developed artistic forms of speech handed down to us in the eloquent harangues of his sages. He breathed his thoughts in poetry, a kind of childish rhythm.

In the time of which we now are speaking the telegraph was a series of signal fires and a wonderful code of signs, which a distinguished scholar of our city has just unravelled.

Primitive man developed the art of war, means of offense and defense; weapons of percussion, for cutting and thrusting; projectiles, armor, fortification, strategy.

Nowhere has man pressed his hand so effectively upon nature as in the domestication of animals. It is almost incredible that ravening wolves and merciless felines should become faithful dogs and purring cats; that the wild sheep and goat should descend from their inaccessible fastnesses, and yield their fleece and flesh and milk; that horses, asses, camels, elephants, should be induced to lend their backs and limbs to lighten the loads of the first common carrier. This process of impressing his own qualities on wild creatures began very early in history, and has continued uniterruptedly from first to last.*

His affairs of state were managed through his patent system. The great inventors were made the rulers of the people, and his highest title to nobility was a most puissant and ingenious one.

He had courts of justice, heard witnesses, executed his laws. It is true that the methods were summary, when a chancery suit was settled by execution on the same day as the death of the devisor. But out of his struggles came our methods, and the greatest drawback to securing justice now is the survival of his antiquated customs into our new practices.

He invented philosophies and sciences, explained the universe and himself to himself. This seems puerile now, but it was the beginning of all our own speculations, necessary to us at present, but which will to-morrow become folk-lore. Over and over again, those who preceded me on this platform have pointed to James Watt as the true deliverer of mankind. Far be it from me to take one leaf from his laurel crown; but the inventor of the alphabet, of the decimal system of notation, of representative government, of the golden rule in morality were greater than he.

For the dream in stone and carving and decoration called a cathedral,

> "Where, through long-drawn aisle and fretted vault,
> The pealing anthem swells the notes of praise,"

that early day has only to offer wild shouts in unison under the starlit dome, touched by the first childish aspirations after the divine, or hopes of immortality.

While you look with admiration upon these panoramas of progress you can not have failed to observe on the canvas that the art, the process and rewards of inventing itself, have undergone the very same development and improvement as the things invented. There is in this a marvellous similarity to the life processes of animals and plants. The homogeneous yolk of the egg during incubation becomes wonderfully complex and heterogeneous; but all of these diverse parts come together into a higher unity, in which each organ ministers to the good of all.

*In a Saturday lecture delivered in the National Museum, March 18, 1882, the author sought to combine the result of Morgan's culture stages, being seven, with the work of Klemm, Tylor, Lane Fox, and Spencer, who had treated separate arts from an evolutionary or, I should say, an inventional motive. This any one may repeat for himself by ruling a broad sheet of paper into eight columns. At the top of the several columns write the words of Morgan, or, better, the first seven Roman numerals. In the lines down the left-hand margin write any words you choose to examine, say *music* or *weapons*. The seven stages of music or of weapons would appear by reading across the sheet from left to right. Care should be taken not to confound the species of the same thought, for example, bruising, piercing, or slashing weapons; or string music, with reed music or horn music. A table made thus for all activities would be an index of all culture in all time.

The earliest invention was a single homogeneous act, an original suggestion, a happy thought. The patent on this was an immediate and individual benefit. A sharper knife of flint, a better scraper, a longer spear, a stouter thread wrought better and the reward was more execution. Now, the man who made the best weapons killed the most game, from that game he got better food, that food made him stronger, that strength made him chief, that chieftaincy gave him more wives, more children, more cohorts to support his throne. The best woman to cook or sew or carry loads got the best husband; that was her patent. From these simple methods of inventing and rewarding invention we come on to the Olympic games, the monopolies, the patent system. And now, in the inventor's laboratory of Graham, Bell, or Edison the climax is reached, where one machine is the co operative result of any number of trained minds, and the reward is meted out to each by the manufacturer; or, in this Patent Congress itself, we may have a still more highly organized unit, wherein the inventors of America become a body social, and together shake hands under the sea with the Emperor of Germany, who sends his congratulations to-day on the occasion of our meeting.

The law of progress in the development of the thing invented, of the process of mind and hand in the act of inventing, of the reward paid to the inventor, of the changes in society itself through the invention, is from the homogeneous to the heterogeneous, as Herbert Spencer has well indicated. This applies to the uses of materials, the conquest of natural forces, the development of the qualities of things, the perfection of the instruments and modes of applying them, and the wants which are gratified and to be gratified by the finished products.

The great classes of industry that you are trying ever to serve are one and all your perpetual debtors. Producers, like farmers, fishermen, lumbermen, miners, breeders, or hunters, have passed through the foregoing school of experience.

No less indebted to you for lifting their burdens are the common carriers of the world, since you have trained the winds, the waters, the animal kingdom, to undertake for mankind journeys that would have utterly discouraged them. It is easy to show, in fact, that the common carrying organizations are as much an evolution or elaboration as the tools they use.

But what shall I say of the manufacturer—his methods, his rewards, his guilds, his interest in politics? *Pari passu* with those efficient tools, that complicated machinery in his hands and about him, he himself has been invented. He is no longer like the primitive artisan who struck the first flakes from brittle stone. He is in touch with many others, who together with him constitute the higher unit of an organized factory or association of factories. It was once said that it takes nine tailors to make a man, but, surely, it takes nine hundred men and women to make a suit of clothes, or a house, or a locomotive. The co-ordination and organization of these industrial cohorts, I affirm and repeat, is invention of the highest order. There are no letters patent on them. They enjoy natural patents, that is, by selection and the survival of the fittest; those who do the best and work together the best, get the reward.

The commerce of the world is an excellent example of invention affecting men as well as their tools. Merchants and bankers, exchangers of goods and exchangers of the prices of goods, have been also invented. It would hardly be affirmed that this world-encircling current of activity called trade had come about by merely following suit or following the fashion. Were that so, Wall street bankers and New York merchants would now be standing naked on the shores of Manhattan Island bartering peltries for clams. Patent Congresses would never have been thought of, and this essay would not have been written. At first every man was his own exploiter, carrier, manufacturer, merchant, banker, and customer. But now all men are servants of all men. By a system of credits only one one-thousandth part of the world's business is done for cash or barter. The human species, regardless of race or language or education, has become a universal combine for mutual helpfulness. And this combine has more parts playing into parts and wheels working into wheels than may be seen in a vast cotton factory. All this is the result of excogitation, of invention. The trader is the son of the trapper, the storekeeper is the son of the trader. In the direct line come the retailer, the wholesaler, the firm, the importer, the trust. The gatherer of cowries is the father of the wampum maker, and the son of the latter is maker of metallic slugs bearing the stamp of a domestic beast; his son issued the first coins, and the family tree brings you straight down to the Rothschilds, who have handled at least once all the money of the world.

Now, what have I to say about the consumer, who, after all, is said by doctrinaires to pay all the bills? The consumer also has been invented, from my point of view. The first consumer wore out little clothing, dwelt in an inexpensive habitation, and his bill of fare was limited. His service, equipage, variety of enjoyments, were circumscribed. Can you think of any one so bereft? In our cities, if we found wandering about a person so poorly endowed, our hearts would be filled with commiseration. Now, from that man to any successful modern, or more correctly to our whole modern time combined, is the road along which consumers have been invented. The kinds of wants have been refined and increased in number. Each want has become more exacting and discriminating. Intellectual, social, æsthetic, moral, and political wants have been created. And these, not in single persons only, but there have been composite wants, world-embracing wants and ambitions thought out, whose gratification come to human beings in families, clubs, guilds, corporations, cities, congresses, nationalities, and internationalities. And these, consuming what they have produced, find that the earth is inexhaustible.

We are assembled to glorify the first century of American patents. A few months ago the disciples of Daguerre met in our city and set up in the National Museum a monument to the inventor of photography. I do not know that there is another memorial in America to an inventor. There is no better way to insure for posterity the recollection of this day than by stimulating among the great industries the desire to continue this good work of memorializing their founders. Perhaps you may not build your monument of stone or bronze; you may set up a library, you may solicit a corner in the National Museum or Congressional Library, or you may secure a better Patent building.

In our public places we set up statues of the destroyers of mankind and erect monuments in our national cemeteries to the anonymous dead. When we go to hang garlands upon the eulogium-bearing tombs we do not forget to scatter flowers upon the mausoleum of the unknown.

We can not gather from the four corners of the world the bones of all the great inventors and honor them with a costly burial. Even their names have perished from the records of mankind, but their works endure. What better can we do than to gather these and guard them in our great museums, mute witnesses of antiquated arts. I can imagine these anonymous inventors looking upon us to-day and glad of this tardy recognition of their vicarious sufferings.

With loving recollection of your labors I pluck a flower from my heart and strew its petals over your neglected graves:

> In freta dum fluvii current, dum montibus umbrae
> lustrabunt convexa, polus dum sidera pascet,
> semper honos nomenque tuum laudesque manebunt,
> quae me cumque vocant terrae. *Æneid, I, 607.*

AMERICAN INVENTIONS AND DISCOVERIES IN MEDICINE, SURGERY, AND PRACTICAL SANITATION.*

By John S. Billings, M. D.

In connection with this celebration of a century's work of the American patent system, I have been requested by the advisory committee to prepare a brief paper upon inventions and discoveries in medicine, surgery, and practical sanitation, with special reference to the progress that has been made in this country in these branches of science and art.

It would be impossible to present on this occasion such a summary as would be of any special interest or use, of the progress which has been made in medicine and sanitation during the century, either by the world at large or by American physicians and sanitarians in particular; and I shall therefore confine my remarks mainly to the progress which has been made in these branches in connection with mechanical inventions and new chemical combinations devised by American inventors—which will require much less time.

The application of the patent system to medicine in this country has had its advantages for certain people, has given employment to a considerable amount of capital in production (and to a much larger amount in advertising), has contributed materially to the revenues of the Government, and has made a great deal of work for the medical profession.

So far as I know, but one complete system of medicine has been patented in this country, and that was the steam, Cayenne pepper, and lobelia system—commonly known as Thomsonianism—to which a patent was granted in 1836. The right to practice this system, with a book describing the methods, was sold by the patentee for $20, and perhaps some of you may have some reminiscences of it connected with your boyish days. I am certain I shall never forget the effects of "composition powder," or of "number six," which was essentially a concentrated tincture of Cayenne pepper, and one dose of which was enough to make a boy willing to go to school for a month.

From a report made by the Commissioner of Patents in 1849, it appears that 86 patents for medicines had been granted up to that date; but

*An address on the occasion of the centennial celebration of the organization of the U. S. Patent Office delivered in Washington. *Proceedings and Addresses*, 1891. pp. 413–422.

the specifications of most of those issued before 1836 had been lost by fire. The greater number of patents for medicines were issued between 1850 and 1860. The total number of patents granted for medicines during the last decade (1880–1890) is 540.*

This, however, applies only to "patent medicines," properly so called, the claims for which are, for the most part, presented by simple-minded men who know very little of the ways of the world. A patent requires a full and unreserved disclosure of the recipe, and the mode of compounding the same, for the public benefit when the term of the patent shall have expired; and the Commissioner of Patents may, if he chooses, require the applicant to furnish specimens of the composition and of its ingredients, sufficient in quantity for the purpose of experiment. The law, however, does not require the applicant to furnish patients to be experimented on, and this may be the reason why the Commissioner has never demanded samples of the ingredients. By far the greater number of the owners of panaceas and nostrums are too shrewd to thus publish their secrets, for they can attain their purpose much better under the law for registering trade-marks and labels, designs for bottles and packages, and copyrights of printed matter, which are less costly, and do not reveal the arcanum.

These proprietary medicines constitute the great bulk of what the public call "patent medicines."

The trade in patent and secret remedies has been, and still is, an important one. We are a bitters-and-pill-taking people; in the fried pork and saleratus biscuit regions the demand for such medicines is unfailing, but everywhere they are found. I suppose the chief consumption of them is by women and children—with a fair allowance of clergymen, if we may judge from the printed testimonials. I sampled a good many of them myself when I was a boy. Of course, these remarks do not apply to bitters. One of the latest patents is for a device to wash pills rapidly down the throat.

According to the census of 1880 there were in the United States 592 establishments devoted to the manufacture of drugs and chemicals, the capital invested being $28,598,458, and the annual value of the product $38,173,658, while there were 563 establishments devoted to the manufacture of patent medicines and compounds, the capital invested being $10,620,880, and the value of the product, $14,682,494.†

A patent automatic doctor, on the principle of "put a quarter in the slot and take out the pill which suits your case," has been proposed, but this patent is said to be of Dutch and not of American origin. The idea of this may have come from Japan, for an old medicine case from that country which I possess has four compartments filled with pills, and the label says that those in the first compartment are good for all

* For these figures, and other data used in this paper I am indebted to my friend Mr. H. H. Bates, Examiner in Chief, in the Patent Office.

† See the *Lancet*, October 5, 1889, p. 683.

diseases of the head, those in the second for all diseases of the body, those in the third for all diseases of the limbs, and those in the fourth are a sure vermifuge.

From the commercial and industrial point of view the great importance of patent and proprietary medicines is connected with advertising. The problem is to induce people to pay 25 cents for the liver-encouraging, silent-perambulating, family pills, which cost 3 cents. Some day I hope that the modern professional expert in advertising will favor us with his views as to the nature and character of those people who were induced to buy Jones's liver pills or Slow's specific by means of a huge display of these names on the sides and roofs of barns and outbuildings, which display forms such a prominent feature in many of our American landscapes, as seen by the traveller on the railway. I suppose there must be such people, for I have a high estimate of the business shrewdness of the men who pay for these abominations. I should also like to know how much a farmer gets for allowing his buildings to be thus defaced. He must be hard up; indeed such a display indicates that the place is probably mortgaged and that the poor man is heavily in debt.

Even the soap advertisers are not as guilty as the nostrum-makers in this particular style of nuisance, although they far exceed them in viciousness when it comes to applying art to ignoble purposes. The connection between progress in medicine and soap advertisements may not be clear to you; but it exists nevertheless, for many of these soaps make work for the doctors by producing skin troubles.

Upon the whole, I should think that the number of people who would take some trouble to avoid purchasing an article which is thus advertised must be rapidly increasing, so that such displays will soon be no longer profitable. The great importance of advertising does not relate to the placard or chromo business, but to its relations to periodical literature—to the daily and weekly press and the monthly magazines and journals.

To the establishment and support of some of our newspapers and journals, medical as well as others, these proprietary and secret medicines, cosmetics, food preparations, etc., have no doubt contributed largely.

I am sorry to say that I have been unable to obtain definite information as to the direct benefits which inventions of this kind have conferred on the public in the way of the cure of disease or preventing death. Among the questions which were *not* put in the schedules of the last census were the following, namely: Did you ever take any patent or proprietary medicine? If so, what and how much, and what was the result? Some very remarkable statistics would no doubt have been obtained had this inquiry been made. I can only say that I know of but four secret remedies which have been really valuable additions to the resources of practical medicine, and the composition of all these is now known. These four are all powerful and dangerous, and should only be used on the advice of a skilled physician. Most of such remedies have little value as curative agents, and some of them are prepared and purchased almost exclusively for immoral or criminal purposes.

In France the sale of secret and patent medicines is not allowed unless they have been examined and approved by the National Academy of Medicine, and the same general rule holds good in Italy and Spain.

The Japanese have followed the French method, and their experience is interesting. The Central Sanitary Bureau established a public laboratory for the analysis of chemicals as a medicine. The proprietors of each of such medicines were bound to present samples, and the names and proportions of the ingredients, directions for its use, and explanations of its supposed efficacy. According to a report in the *British Medical Journal*, during the first year there were 11,904 applicants for license to prepare and sell 148,091 patent and secret medicines. Permission for the preparation and sale of 58,638 different kinds were granted, 8,592 were prohibited, 9,918 were ordered to be discountenanced, and 70,943 remained to be reported on. The great majority of those which were authorized were of no efficacy, but few being remedial agents; but their sale was not prohibited, as they were not found to be dangerous to the health of the people.* I do not vouch for these figures, which throw our records entirely in the shade.

In 1849 a special committee of the U. S. House of Representatives reported to the House a bill to prevent the patenting of medicines, accompanied by a report. This bill provided that after the passage of the act letters patent shall not be granted for any article whatever as a medicine, provided that this shall not apply to machines, instruments, or apparatus. When the matter came before the House for consideration the bill was laid on the table.†

You are all aware that the great majority of the medical profession consider it to be improper and discreditable for a physician to patent a remedy. The medical code of ethics declares that it is derogatory to professional character "for a physican to hold a patent for any surgical instrument or medicine; or to dispense a secret nostrum whether it be the composition or exclusive property of himself or others. For if such nostrum be of real efficacy, any concealment regarding it is inconsistent with beneficence and professional liberality; and if mystery alone give it value and importance, such craft implies either disgraceful ignorance or fraudulent avarice. It is also reprehensible for physicians to give certificates attesting the efficacy of patent or secret medicines, or in any way to promote the use of them." Like all legislation, this is a formal declaration of the customs of the profession, which customs are of great antiquity. The principle upon which it is founded is thus expressed by Lord Bacon: "I hold every man a debtor to his profes-

* *British Medical Journal*, July 3, 1880, vol. II, p. 24.

† *Congressional Globe*, March 3, 1849, p. 697.

sion: from the which, as men of course do seek to receive countenance and profit, so ought they of duty to endeavor themselves by way of amends to be a help and ornament thereunto."

The rule, however, is not always adhered to by physicians, the most notable exception having been, perhaps, the use of Koch's lymph before its composition was revealed. As regards the patenting of surgical instruments and apparatus, the opinion of the great majority of physicians is in accordance with the rule just stated, but there are some who question its propriety, although they obey it—and there are few who would not use a patented instrument in a case to which they thought it was applicable.

The total number of surgical instruments and appliances patented during the past decade has been about 1,200, the patents having been in almost all cases taken out by manufacturers. With these may be classed dentists' tools and apparatus, of which about 500 have been patented during the last ten years, and in this field of invention the United States leads the world. The same may be said with regard to artificial limbs, of which our great war gave rise to many varieties.

As you know, the law prescribes that a patent may be given for a "new and useful art, machine, manufacture or composition of matter." I used to think that the word "useful" in this law had its ordinary meaning, and therefore wondered exceedingly as to why the Patent Office examiners allowed patents to certain things which came under my notice. One day, however, I received an article from the Patent Office, with the request for a report as to whether it was useful in the sense in which that word was used by the office, namely, "not pernicious or prejudicial to public interest—capable of being used"—and then for the first time I understood one of the first principles of the patent law of the United States, that is, that it does not take into consideration the degree of utility in the device, or, in other words, that "useful" means "harmless."

If a patent is granted to a medicine, it must be as a composition of matter as a special article of manufacture. The practice of the Patent Office in these matters is not generally understood. It does not now consider that medical prescriptions are inventions within the meaning of the law, or that a mere aggregation of well-known remedies to obtain a cumulative effect is a patentable composition of matter. A certain number of claims for Government protection in the form of patents or trade-marks are made for medical compounds or for apparatus under false pretenses: that is to say, the claim is for a new remedy for rheumatism or dyspepsia or displacement, with a warning against their use under certain conditions, the real design being that they are to be used under precisely these conditions in order to procure abortion, etc. These are sometimes difficult cases for the Patent Office to treat properly, for the law does not allow a large discretion for refusal on mere suspicion, and where there is ostensible and possible utility (in the

Patent Office sense) it can hardly reject the claim on the ground that the invention might be used for immoral purposes.

I said in the beginning that I can not on this occasion give any sufficient account of the progress of invention and discovery in medicine and sanitation during the century just gone. The great step forward which has been made has been the establishment of a true scientific foundation for the art upon the discoveries made in physics, chemistry, and biology. One hundred years ago the practice of medicine and measures to preserve health, so far as these were really efficacious, were in the main empirical—that is, certain effects were known to usually follow the giving of certain drugs or the application of certain measures, but why or how these effects were produced was unknown. They sailed then by dead-reckoning, in several senses of this phrase.

Since then not only have great advances been made by a continuance of these empirical measures in treatment, but we have learned much as to the mechanism and functions of different parts of the body and as to the nature of the causes of some of the most prevalent and fatal forms of disease, and, as a consequence, can apply means of prevention or treatment in a much more direct and definite way than was formerly the case. For example, a hundred years ago nothing was known of the difference between typhus and typhoid fevers. We have now discovered that the first is a disease propagated largely by aerial contagion and induced or aggravated by overcrowding, the preventive means being isolation, light, and fresh air; while the second is due to a minute vegetable organism, a bacillus, and is propagated mainly by contaminated water, milk, food, and clothing; and that the treatment of the two diseases should be very different.

The most important improvements in practical medicine made in the United States have been chiefly in surgery, in its various branches. We have led the way in the ligation of some of the larger arteries, in the removal of abdominal tumors, in the treatment of diseases and injuries peculiar to women, in the treatment of spinal affections and of deformities of various kinds. Above all, we were the first to show the uses of anæsthetics—the most important advance in medicine made during the century. In our late war we taught Europe how to build, organize, and manage military hospitals; and we formed the best museum in existence illustrating modern military medicine and surgery. Our contributions to medical literature have been many and valuable; and our Government possesses the largest and best working medical library in the world. We have more doctors and more medical schools, in proportion to the population, than any other country, and, while this is not good evidence of progress, I am glad to be able to say that the standard of acquirements in medical education has been and is now rising, and our leading medical schools are now being equipped with buildings, with apparatus, with laboratories, and, most important of all, with brains, which enable them to give means of practical instruction equal to any to be found elsewhere.

As regards preventive public medicine and sanitation, we have not made so many valuable contributions to the world's stock of knowledge, chiefly because, until quite recently, we have not had the stimulus to persistent effort which comes from density of population and its complicated relation to sewage disposal and water supplies; nor have we had the information relative to localized causes of disease and death, which is the essential foundation of public hygiene, and which can only be obtained by a proper system of vital statistics. We can, however, show enough and to spare of inventions in the way of sanitary appliances, fixtures, and systems for house drainage, sewerage, etc.; for the ingenuity of inventors has kept pace with the increasing demands for protection from the effects of the decomposition of waste matters as increase of knowledge has made these known to us. The total number of patents granted for sanitary appliances during the last decade (1880–1890) is about 1,175. If good fixtures necessarily involve good plumbing work we could easily make our houses safe so far as drainage is concerned: but a leaky joint or a tilted trap makes the best appliance worthless. The impulse to improvements in this direction has come mainly from England, where most of the principles of good work of this kind has been developed: but we have devised some details better adapted to our climate and modes of construction, and while many of the patent traps and sewer-gas excluders are only useful in the patent-law sense, and some not even in that, it is nevertheless true that the safety, accessibility, and good appearance of plumber's work has been largely increased during the last few years by patented inventions. Much the same may be said with regard to heating appliances, including ventilating stoves and fireplaces, radiators, etc., but I am unable to express any enthusiam with regard to what are commonly called patent ventilators.

No doubt the greatest progress in medical science during the next few years will be in the direction of prevention, and to this end mechanical and chemical invention and discovery must go hand in hand with increase in biological and medical knowledge. Neither can afford to neglect or despise the other, and both are working for the common good. If the American patent system has not given rise to any specially valuable inventions in practical medicine, in law, or in theology, it must be due to the nature of the subjects, and not to any fault of the system.

ENDOWMENT FOR SCIENTIFIC RESEARCH AND PUBLICATION.*

By Addison Brown.

Twenty years ago Prof. Tyndall delivered in New York and in other cities of this country a series of lectures upon light. The last of the series was an impressive plea for a more thorough prosecution of original research in pure science; and incidentally, for the need of endowments to maintain it. I was fortunate in having the opportunity to listen to that remarkable course of lectures, and to that plea for science. Its impression has never left me. The impression was the deeper, because Tyndall set upon it the seal of self-denial. Some $30,000, nearly the entire net proceeds of his lectures in the United States—money for which he undoubtedly had abundant use in his own affairs, or at least in the prosecution of researches in his own country, and which by all precedent and the example of other lecturers he would have taken with him—this he has given to the science of this country, endowing therewith, in 1885 three scholarships for the prosecution of original research in physics, one under the direction of Columbia College, one under Harvard, and a third at the University of Pennsylvania.

The truths uttered and the example set by this self-denying master have already many times borne fruit. The late President Barnard, of Columbia College, who was a warm supporter of Prof. Tyndall when here, bequeathed to Columbia upon his decease a few years since the sum of $10,000 for the endowment of another fellowship for the encouragement of scientific research, upon substantially the same terms as those of the Tyndall scholarships. In other parts of the country there have been some other endowments for similar purposes. In the last year Columbia has also received $100,000, the munificent bequest of Mr. Da Costa, for the establishment of the department of biology. Although this bequest is not primarily for the prosecution of original research, it is not restricted by hampering conditions, and will to some extent, it is hoped, admit of a direct and continuous support of the highest and most advanced studies.

*Address at the first joint meeting of the Scientific Alliance of New York, November 15, 1892. (Pamphlet Report, pp. 18–41.)

The appeal made by Tyndall has been often renewed by scientific men; by the heads of universities; by the presidents of scientific associations, here and abroad; and by none, perhaps, more eloquently than by Dr. Edwin Ray Lancaster, in his address before the biological section of the British Association at Southport, in 1883.

What shall we say to the call and the examples of such men? Was the gift of Tyndall based only upon an idle fancy? Or was it the result of a clear perception of a profound truth, viz, America's need of that money as a stimulus and support to more scientific research; the call on him being felt to be the more imperious, because the need of it was so plain to him, while obscure to others; and making his act, therefore, a noble instance of self-renunciation in an unappreciated cause?

"To keep society as regards science in healthy play," he says, "three classes of workers are necessary:

"1. The investigator of natural truth, whose vocation it is to pursue that truth and extend the field of discovery for truth's own sake, without reference to practical ends.

"2. The teacher, to diffuse this knowledge. - -

"3. The applier of these principles and truths to make them available to the needs, the comforts or the luxuries of life. - - -

"These three classes ought to co-exist and inter-act. The popular notions of science - - - often relate, not to science strictly so called, but to the application of science."

The great discoveries of scientific truth, he continues, are "not made by practical men, and they never will be made by them; because their minds are beset by ideas which, though of the highest value in one point of view, are not those which stimulate the original discoverer."

In a chance conversation, a few weeks since, I received a confirmation of these words, so direct and unexpected, that it may bear citation. I was talking with an electrical expert who had made several very interesting and important inventions. I asked him of how much importance he conceived that the scientific men of the closet, the original investigators, so-called, had been in working out the great inventions of electricity during the last fifty years—the telegraph cables, telephones, the electric lighting, and the electric motors; and whether these achievements were not in reality due, mainly, to the practical men, the inventors, who knew what they were after, rather than to the men of science, who rarely applied their work to practical use?

"Not at all," he said, "the scientific men are of the utmost importance; everything that has been done has proceeded upon the basis of what they have previously discovered, and upon the principles and laws which they have laid down. Nowadays we never work at random. Look at that electric light! Of the energy expended in producing it, only 7 per cent appears as light; the rest, 93 per cent, is wasted, mainly in heat. We are all now trying to prevent this enormous waste. I want to reverse that proportion; but if I can reduce the waste to only

33 per cent, a patent of my invention will be worth millions of dollars for its economy in production. In seeking this we do not work at random. I go to my laboratory; study the applications of the principles, facts, and laws which the great scientists like Faraday, Thompson, and Maxwell have worked out, and endeavor to find such devices as shall secure my aim."

This is but an expression, in another form, of what Tyndall said twenty years ago: "Behind all our practical applications, there is a region of intellectual action to which practical men have rarely contributed, but from which they draw all their supplies. Cut them off from that region, and they become eventually helpless."

What is true in one department of natural science is, I apprehend, equally true in all. The practical men do not work at random, but upon the basis of what scientific research and publication have previously put within their grasp.

It is evident therefore that not only the advancement of knowledge itself, but all possibility of any continuous advance in those great improvements which are to mitigate the sorrows, and promote the health, the conveniences and the comforts of men, is vitally dependent upon the progress of scientific research. In recent years how marvellous have these improvements been! Besides those that are most common and familiar to all, what miracles, almost, have been achieved through the photograph, the spectroscope, the microscope; by the discovery of the sources of fermentation and of putrefaction; by the discovery of anæsthetics and the application of antiseptic methods in surgery, and in the treatment of other lesions! These latter discoveries alone have ameliorated beyond expression the sufferings of man; they save more lives than war and pestilence destroy, surpassing even in that regard the safety lamp of Sir Humphrey Davy—an invention which, at the time it was made, was said to have exceeded every previous discovery as a means of saving human life, except, possibly, inoculation for smallpox.

This vital relation between the advancement of knowledge and the welfare of man furnishes an all-sufficient reason for the continuous and never-ending prosecution of original research. Of necessity the original work of discovery must always lead; that must always precede the practical applications. The necessity for such research must, therefore, continue, so long as science and human society endure. As there is no limit to the advance of knowledge, so there can be no limit to the benefactions it is capable of conferring upon mankind. The more rapid the advance, the more speedy the enjoyment of its fruits. In this relation alone, the need of ample provision for scientific progress is one that addresses itself equally to the nation, to the state, to philanthropists, and to all who would advance the welfare of man, on the broadest and most enduring lines.

How shall such research be maintained and extended? The investigator of pure science does not work for profit. His discoveries are not marketable. The law allows no patent upon a principle of nature or the discovery of a new truth. Newton could not patent the law of gravitation, nor Volta the galvanism of the voltaic pile; nor Ehrenberg and Schwann, the discovery of the widespread influence of bacteria; nor Faraday, nor Henry, electro-magnetism; nor Joule, his correlation of forces; nor Jackson, his anaesthetics; nor Lister, his antiseptic treatment; nor Koch nor Pasteur, their discoveries of the bacilli, the destruction of which may lead to the cure or amelioration of terrible diseases. To the practical men and to the inventors, on the other hand, who apply to the specific wants of men the truths and principles which the scientists have made known to them, the law, in the form of a patent, gives a monopoly of from fourteen to twenty-one years. They thus obtain, as a rule, a reasonable, and, in some cases, even an excessive, pecuniary reward. In this country alone nearly 500,000 patents have been issued: they are increasing at the rate of about 25,000 per year. In the extreme multiplication of patents affecting a large part of everything we use, the whole world, it might almost be said, is paying tribute to the inventors and practical men: while to the original discoverers who have made so much of all this possible, there is no promise of pecuniary reward.

This is not said by way of complaint. In the nature of things, it is scarcely avoidable. The aims, the motives, the methods, and the genius of the two classes of minds, are and ever must be widely distinct. Original discoverers can not be turned aside from their special work to become mechanics and inventors without infinite loss. Prof. Henry had one form of the electric telegraph in actual use some years before Morse conceived it.* But how great would have been the loss to science, without any corresponding gain, had Prof. Henry in 1830 turned away from pure science to do the subsequent work of Morse in adapting the telegraph to common and valuable use!

Research in pure science can never be made a self-supporting pursuit. It can never therefore be carried forward broadly, and continuously, and effectively, except through men sustained by some form of stipend or endowment. Occasionally, it is true, men of independent fortune, like Harvey, and Darwin, and Lyell, and Agassiz, have devoted themselves to original research upon their own means, and have accomplished most important results. But these instances are rare. Many other persons, too, with aptitudes and tastes for research, though not following a scientific career, have carried on private researches in the intervals of leisure, stolen from the exacting demands of professional or business life; and these have, in the aggregate, added no small amount to the common stock of knowledge.

It is no disparagement however of these subordinate workers to say that nearly all the great discoveries, and nearly all the great advances

* Smithsonian Report, 1878, pp. 159, 262

along the lines of knowledge, have been achieved by men who in the main have devoted their lives to the work, and have been supported through institutions or endowments which made this devotion possible. Government appointments, professorial chairs, or salaried positions in scientific institutions of some kind, have been and must continue to be our chief dependence. And it is manifest that these can only be maintained by Government aid, or by the bounty of private individuals. The former is mainly the European system; the latter, in the main, is ours. There, universities are founded by the government; here, chiefly by the people.

In Germany there are twenty-one universities maintained by the Government. In each of these, as Dr. Lancaster states, there are five independent establishments in the department of biology alone, viz, in physiology, anatomy, pathology, zoölogy, and botany. At the head of each of these establishments there is a professor, with two paid assistants, making altogether about 300 for biological research in Germany; and he estimates about one-quarter of that number in the same department in England. In all the sciences, therefore, there would probably be found in Germany from 800 to 1,000 persons of high scientific attainments, supported by the Government in the universities, who are regularly and systematically engaged in the discovery of new scientific truth. For it is there made both the object and the duty of the professors of natural science to carry on original investigations by work in the laboratory. Their positions are obtained through previous distinction in such investigations, and it is for this work that their small but fixed stipend is paid by the Government.

In the College de France, also maintained by the Government, there is the same requirement, though with a larger salary to the professors, and with the added duty imposed on them to deliver to the students about forty lectures yearly upon the subjects of the professors' researches; while in Germany the professors also receive from each student who attends their lectures, a moderate fee, which serves to increase their meager stipend, as well as to stimulate their activity and usefulness. Under this system, Germany has become the greatest school of science, and the resort of the whole world.

In this country the opposite system prevails. The colleges and universities are mainly private foundations, dependent on private gifts and endowments. The colleges are unwisely mutiplied. All are more or less cramped for money. This limits the number of professors and assistants appointed for instruction, and crowds them with routine work. The result is that in all but a few colleges, and in these until comparatively recently, the duties of instruction have left to the professors but little time or opportunity for the prosecution of original investigations; and these with but poor equipment and inadequate means.

In not one of all our colleges and universities, so far as I have been able to ascertain, is there a single professorship endowed or founded, even in part, for the avowed object of original scientific research. Instruction, not discovery, is the only avowed object. It is to the great credit of American professors and teachers that, with so much routine work on their hands, and so little leisure for research, they should have accomplished by purely voluntary studies so much as is shown in their contributions to our scientific publications.

To what is said above, perhaps a virtual exception should be made as respects our astronomical observatories, in which, the labors of instruction being less, original work has been perhaps expected, and has been accomplished with most signal success. To some extent this may possibly apply to our medical schools also. And in other departments, generally, wherever time and opportunity have been afforded, much original work has been done by our professors; some of it of the first class. This is attested, not to mention living instances, by the work of Prof. Henry at Princeton, Dr. Torrey at Columbia, Dr. Silliman at Yale, Dr. Gray at Harvard, and many others that might be named. In a number of the States, also, and at Washington, there have been maintained by the State or Nation a number of scientific men, in connection with certain State or national interests, who have accomplished most important results; of these, Dr. James Hall, of this State, is a conspicuous instance. At Harvard and at other colleges some noble opportunities for special study have been also provided in their scientific schools and museums; notably in the zoölogical museum, the Jefferson Physical Laboratory, and the Peabody Museum of Archæology at Cambridge, and also in the department of hygiene at the University of Pennsylvania. But in most of these the great complaint is the lack of neceessary endowments to make possible the active advanced work in original discovery for which those institutions are designed. In the Peabody Museum there was in 1891 a gift of $10,000 by Mrs. Hemenway to establish a post-graduate fellowship; and also a gift of like amount by Mr. Wolcott, for the general support of the museum's work. New York also has within a few years past seen spring up almost as by magic, through the efforts of a single leading spirit, seconded by other public spirited men and women, and by municipal aid, a museum of natural history that bids fair to stand in the front rank of scientific opportunities; but the endowments of fellowships and professors necessary to make its opportunities available in active research are as yet wanting.

England holds a position midway between the United States and Germany. Her scientific men lament her deficiencies. They are striving to increase their means for scientific work, and are doing so yearly.

If experience teaches anything, it is that no broad and general development of scientific work of the first class is possible, except either through independent establishments for special work, or else by the university system, in which professors in science and their assistants are first selected on account of their previous distinction in original research, and are then appointed to continue that work, and in the teaching of students, to transmit to them the zeal of discovery and the true methods of advance.

It matters little whether the support of the university or of special institutions for research comes from the Government or from private endowment, provided the provision is adequate and constant. The difficulty with us has been, and still is, that funds are insufficient, the means and equipment inadequate, and the time allowed to the professors for research insufficient. There has been too much of the schoolmaster, and too little of the real professor. Too great absorption of the professor's time in the work of instruction is injurious to both teacher and pupil. The most stimulating of teachers is he who by daily experiment is in vital touch with Nature,—he who brings from the fires of the laboratory the warmth, the illumination, and the inspiration of his own researches.

This is now well recognized: and so far as their means will permit, the leading colleges are by degrees relieving their professors of the work of elementary instruction, so that they may the better prosecute original researches, and at the same time become best qualified for the highest work of instruction. This system will doubtless demand watchfulness and discrimination. To prevent abuses, regulation and responsibility may have to be imposed. But it involves the appointment of additional instructors. It requires added means. And this is indispensable as a part of the transition of our leading colleges to the university system. It is indispensable, also, if we are to have in this country any considerable systematic prosecution of original research. We must use existing instrumentalities and existing institutions. And all experience shows that outside of the few Government positions, and in the absence of special institutions for research, the professorial chairs are best adapted to such investigations. No greater service could be done to science than to make such endowments as should insure systematic and continuous research by the professors as a part of the new university system.

Endowments for the same object, and operating in the same line, might also take a different form, viz., the endowment of several professorial fellowships, each, say, of $1,000 annual income: to be controlled and awarded by some independent scientific body (such as this Alliance might afford) for distinction in active scientific investigations, either within the country or within the State. I know of no more quickening impulse to original scientific research than such as would be given to it by those means.

How backward we have been in this country, through the lack of proper endowments, in making use of the best existing opportunities for research, may be illustrated by a single instance. Some twenty years ago a school was established at Naples for the prosecution of marine biological research. It is most thoroughly equipped, and, being a general resort, is the most advantageous for study in the world. It is maintained by a charge of $500 per year upon each table occupied, each occupant being entitled to all the advantages of the institution. Of these tables, the German States for several years have taken thirteen; Italy, eight; Austria, Russia, Spain, and England, each three; Switzerland, Belgium and Holland, each one; the United States, until 1891, none, except one table supported by Williams College for two years, and one by the University of Pennsylvania for one year. Prior to that time about fifteen other American students in all had obtained places at the tables taken and paid for by other nations. In 1890, this arrangement was prohibited by the administration of the institution; and the right to a table in 1891, was secured to Americans, only through the private benefaction of Maj. Alex. Henry Davis, of Syracuse. For the year 1892, the use of a table has been secured through a subscription started by the American Association for the Advancement of Science, toward which the Association itself granted out of its scanty funds $100 and was the means, I believe, of procuring the rest.*

We have not however been wholly without some such means of study in this country through the marine biological laboratories established some years ago at Newport and at Wood's Holl, by Prof. Alex. Agassiz. The former has been now enlarged so as to accommodate eight advanced students, besides the professor and his assistant.† The Johns Hopkins University also has supplied some opportunities of this kind by its summer school, formerly at Beaufort; later, at Jamaica; but at present, as I understand, it is without any permanent location.

Our neighbor, the Brooklyn Institute, has organized similar investigations, on a minor scale, during the summer months at different places on Long Island. But what is needed for the most effective work, is suitable endowments for professors and advanced students, in connection with an adequate biological laboratory, such as the Newport one enlarged might afford, equal in means and equipment to that at Naples, or at least to that recently completed, largely through private enterprise, at Plymouth, England.‡

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* See *Proc. American Association A. S.* 1891, vol. XL, p. 449–451.

† *Report Harvard Col.*, 1891, p. 182.

‡ In his address before the American Association for the Advancement of Science, in 1891, President Prescott, referring to this general subject, said:

"To nurture investigation in science is the largest opportunity before the American people. Research, systematic and wisely directed, requires good organization and strong support, the support of many powers. It must have the support of able and persistent men. It needs the conference of workers, and the dissemination of knowledge in societies like this. It wants the interest and the confidence of the public. It asks and will always obtain the constant, helpful use of the press. It requires distinct provision in colleges, and in the institutions of higher education. It ought to be sustained expressly by the Government, both in the several States and under the United States, and sustained on broad and permanent foundations. Still, it needs private benefactions. Research is the growth of years. Let it be the demand of all, and let this call find utterance everywhere."—*Proceedings Am. Assoc.*, 1891, vol. XL, p. 440.

II.

Immediately connected with our colleges and universities is another field, in which additional endowments are greatly needed, viz: for fellowships in science for post-graduate studies.

Upon the post-graduate workers, the future of science, and the recruits for future teachers and professors, must necessarily depend. In that view the importance of post-graduate endowments in science can scarcely be magnified. The great majority of the young men from whom all the new recruits must be drawn have little or no pecuniary means. After graduating, often through many difficulties, they must face the question of their future calling. They must consider what promise of a reasonable and comfortable support a life devoted to science affords. If this risk should not deter them, still there are many with talents of a high order who would be absolutely unable to proceed further in the advanced scientific studies necessary to qualify them to enter upon remunerative scientific work, or to obtain situations as professors or assistants, except by the aid of substantial endowments for their support, during the three or four years more of necessary assiduous study.

In the stress of modern life, and in the allurements towards more certain pecuniary results, nothing but such endowments can avert the withdrawal from scientific pursuits of many young men of high promise, whose genius and tastes and ambition strongly incline them to science, and who would be secured to it if this temporary support were afforded.

The endowments of our colleges and universities in aid of post-graduate work in science are much less, I suppose, than is commonly imagined. I find no such support for post-graduate work in science, either at Cornell University, at the University of the City of New York, at Brown University, at Amherst, or even at the Johns Hopkins University. No statement of the endowments of the new Clark University at Worcester has as yet been published. Princeton, though having a hundred under-graduate scholarships, has but one post-graduate fellowship for science; Yale but two,—the Silliman and the Sloane Fellowships.

Columbia College has two fellowships expressly restricted to science, viz.: The Tyndall Fellowship of \$648 annually, and the Barnard Fellowship, before referred to, of about \$500 annually. Besides these, however, twenty-four general university fellowships have been established, of \$500 each, for post-graduate study, of which eighteen are in present operation. About one-third of these are assigned to science; making now eight for science at Columbia, with probably two more in 1893 or 1894. In architecture, moreover, there are three additional noble post-graduate fellowships at Columbia, the Schermerhorn of \$1,300 annually, and the two McKim Fellowships of \$1,000 each, to support study in foreign travel. In the Medical Department, also, there are five valuable prizes for proficiency.

The University of Pennsylvania has the Tyndall Fellowship, before referred to; and, in the Department of Hygiene, an admirable laboratory fitted up by Mr. Henry C. Lea, with a fellowship of $10,000 endowed by Mr. Thomas A. Scott, at present applied to original research in bacteriology.

At Harvard, besides the three Bullard Fellowships of $5,000 each, established in 1891, to promote original research in the medical school, there are two post-graduate fellowships restricted to science exclusively; namely, the Tyndall Fellowship of about $500 annually, and the income of the recently established Joseph Lovering Fund, the principal of which is now about $8,000. There are also eleven other general fellowships, viz: The Parker, the Kirkland, and the Morgan Fellowships, available for promising graduate students in any branch, of which about five have been usually assigned to science. These fellowships give an income of from $450 to $700 a year. Harvard has also forty-six scholarships available for graduate students, varying in income from $150 to $300 each, of which about seventeen are assigned to science. During the last year, according to the report of Prof. Pierce, the dean, there were 193 applications for those post-graduate fellowships and scholarships, seventy-one of which were in science. Only one-third of the applicants could receive the aid. The Dean adds:

"The number of appointments is still *very insufficient* to meet the demands of promising students who wish to enter the graduate school and are unable to do so without assistance."* The tables published by him indicate that a considerable number of those not aided withdrew from science; and that many others who were entered for the first year in the graduate school, would, if not aided, afterwards leave. It is gratifying to observe the further fact, so encouraging also for the young graduates who wish, if possible, to enter upon a scientific career, that all who had enjoyed these fellowships for the full term of three years, and did not continue their studies further abroad, at once received honorable positions.

From the above synopsis it appears that in all these colleges (and I know of no other similar fellowships elsewhere) there are only about twenty-six adequately endowed post-graduate fellowships in science. As these should be continued for at least three years, there is provision altogether for only about nine per year—not one-fourth the number required to supply the annual loss in our 150 colleges, to say nothing of the increasing demand through the growth and improvements in the colleges themselves. As it is from such specially trained students that the great professors of the future must be drawn, the need of much greater endowments for new recruits is apparent.

In England the aids afforded by fellowships in their universities are familiar to all. Sir Isaac Newton, who is to modern science, what Shakespeare is in literature, was sustained from his student days suc-

* *Report Harvard Col.*, 1891, p. 92.

cessively in a scholarship, a fellowship, and as professor at Trinity College at Cambridge. Besides those aids, The Royal Commissioners of the Exhibition of 1851 instituted last year (1891) "Exhibition science scholarships" for advanced students, to which $25,000 yearly is to be applied in sums of $750 each. Last year sixteen appointments were made, to be held for two and probably for three years by students who show capacity, and "who advance science by experimental work."*

On this subject a most interesting discussion took place last year in the French Academy of Science. On April 27, 1891, the Secretary read the following extract from the will of the late M. Cahours, a deceased member of the Academy:

"I have frequently had the opportunity of observing, in the course of my scientific career, that many young men distinguished and endowed with real talent for science found themselves obliged to abandon it, because before beginning they had no efficacious help which provided them with the first necessities of life, and allowed them to devote themselves exclusively to scientific studies.

"With the object of encouraging such young workers, who for want of sufficient resources find themselves powerless to finish works in course of execution, - - - I bequeath to the Academy of Sciences - - - 100,000 francs, - - - the interest to be distributed yearly by way of encouragement to any young men who have made themselves known by some interesting works, and more particularly by chemical researches; - - - as far as possible to young men without fortune, not having salaried offices, and who, from want of a sufficient situation, would find themselves without the possibility of following up their researches. These pecuniary encouragements ought to be given for several years to the same young men, if the Commissioner thinks their productions have sufficient value; - - - to cease when they shall have other sufficiently remunerative positions."

M. Janssen, then addressing the Academy, said:

"This affords an example to all who hereafter may desire to encourage the sciences by their liberality. M. Cahours, who knew the urgent necessities of science, had, like most of us, become convinced of the need of introducing a new form of scientific recompenses.

"Our prizes will always continue to meet a great and noble necessity. Their value, the difficulty of obtaining them, and the *eclat* they take from the illustriousness of the body that grants them, will always make them the highest and most valuable of recompenses. But the value also of the works it is necessary to produce in order to lay claim to them, forbids them to beginners. It is a field only accessible to matured talents. But there are many young men endowed with precious aptitudes, inclined to pure science, but turned very often from this envied career by the difficulties of existence, and taking with regret a direction towards more immediate results. And yet many among

*Per Sir William Thompson, *Proceedings, Royal Society*, 1891, vol. L, p. 225.

them possess talents, which, if well cultivated, might do honor and good to science. - - - These difficulties are increased every day by the marked advance of the exigencies of life.

"We must find a prompt remedy for this state of things, if we do not wish to see an end of the recruitment of science. This truth is beginning to be generally felt. The Government has already created institutions, scholarships, and encouragements, which partly meet the necessity. Some generous donors are also working in this manner. I will mention specially the noble foundation of Mlle. Dosne, in accordance with whose instructions a hall is at this moment being built, where young men, having shown distinguished aptitudes for high administration, for the bar, or for history, will receive for three years all the means of carrying on high and peaceful studies. Let us say then plainly (and in speaking thus we only feebly echo the words of the most illustrious members of the Academy), that it is by following the way so nobly opened by Cahours, that the interests and prospects of science will be most efficaciously served."*

Huxley is said to have once stated that "any country would find it to its interest to spend $100,000 in first *finding* a Faraday, and then putting him in a position where he could do the greatest amount of work." It is the post-graduate endowments that must first find and retain to science the Faradays of the future.

A notable instance of the need and value of such aid is found in the recently appointed head of a great university, who by such endowments alone, here and abroad, it is said, was enabled to prosecute his studies for ten years successively, reaching thereby the front rank in his chosen department of philosophy.

III.

Another department in great need of pecuniary support is that of the learned and scientific societies. In these England is pre-eminent. Our own societies have endeavored to follow, so far as they could, their English models. The English societies have rendered to science invaluable service in three main lines:

1. In providing ample means for the publication of scientific papers, showing the progress and the results of their scientific work. In this every society has taken part.

2. In the direct maintenance of original research, in which the Royal Institution has been most conspicuous.

3. In the award of prizes for scientific distinction; but still more important, in the distribution of pecuniary aid, for the prosecution of special scientific researches.

(1) Of these, I regard publication as, perhaps, the most important; not only because it puts the world in possession of what has been done by investigators; but because the very fact that there are means of

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* *Nature*, May 7, 1891; vol. XLIV, p. 17.

publication, is one of the greatest incitements to complete and thorough original scientific work.

Of the English societies the Royal Society is the oldest, having been chartered in 1662. It has published 181 volumes of *Transactions* and about 50 volumes of *Proceedings*. For these purposes, in 1881 the expenditure was between $11,000 and $12,000. It has property to the value of about two-thirds of a million of dollars, more than half of which is in trust funds, held for scientific uses. The income on the trust funds in 1891 was about $17,500.* In 1828 Dr. Wollaston in giving it $10,000 in 3 per cent. consols "to promote scientific researches," charged upon the society "not to hoard the income parsimoniously, but to expend it liberally for the objects named."

The Royal Institution of Great Britain was founded in 1779, largely through our countryman James Thompson, of Rumford, Vt., afterwards Count Rumford. In 1888 it had property and invested funds for general purposes to the amount of $350,000, and about $40,000 of invested funds for the maintenance of its three professors. In 1887 it expended about $2,000 in publications, and it has issued about 40 volumes.†

The Linnaean Society, now furnished by the Government with permanent accommodations in Burlington House, free of rent, was founded by Sir James E. Smith in 1788, and is devoted to botany and zoölogy. Its property amounts to about $32,000, but it has no endowed funds for scientific investigation. For some years past its receipts, mainly from contributions, have been about $10,000 a year, of which one-half, about $5,000, is spent on its publications, which now number nearly 50 volumes of *Transactions* in quarto, and as many more of its *Journal*. In 1888 $7,000 were expended in publication.‡

Next in order of time is the British Association for the Advancement of Science, founded in 1831. It is sustained chiefly by yearly contributions. Its invested funds amount to about $62,000. Its income and contributions are about $10,000 annually, out of which it appropriates from $6,000 to $7,000 per annum for the encouragement of scientific investigations, and about $1,800 annually for its yearly volume of proceedings. Its publications now number twenty-five volumes.§

The Ray Society was founded in 1844. It was named after the Rev. John Ray, who lived from 1628 until 1705. Haller, himself one of the greatest scientists of his time, writing in 1771, in the full light of Linnaeus' fame, calls Ray "the greatest botanist within the memory of man."‖ The society has published about fifty volumes of scientific works of the highest importance. I have not seen any statistics concerning its means or acquisitions; nor have I found any financial report of the scientific societies of Edinburgh or Dublin.

* *Proceedings*, 1891, vol. L, p. 235.

† *Report*, 1888, p. 13.

‡ *Proceedings* [May 4, 1888], 1890, pp. 15, 45.

§ *Report*, 1891, pp. lxxxvii to c. 76.

‖ *Bibliotheca Botanica*.

(2) Of these societies, only the Royal Institution directly supports professors for scientific research. It has two laboratories, one chemical and one physical. These were re-built in 1872, "in order that original discovery might be more effectively carried on." The society was founded for the declared purpose of "promoting scientific and literary research." It has three professors,—one in chemistry, one in physics, and one in physiology. Davy, Faraday, Tyndall, and others who have spent their lives there, have made its annals immortal.

(3) In stimulating research by the appropriation of moneys for specific objects, the Royal Society and the British Association are the chief agencies. Besides some of its own funds, the Royal Society distributes annually £4,000, or $20,000, granted by the Government "for the advancement of science." This has been done by applying it to numerous purposes; in 1891, for fifty-seven different scientific objects, in sums ranging from $25 to $3,000 each; not confined to natural science alone, but including ethnology and magnetic surveys. Most of the grants were in sums of about $350 or less.*

The British Association has disbursed annually for the last forty years from $6,000 to $7,000 per annum, upon the same system of dividing it up for numerous specific purposes; usually from thirty to forty objects yearly, the grants being in sums ranging from $25 to $1,000. The grants are called for and expended for the specific purpose named, and under the direction of some prominent scientific man. Scientists like Sir William Thompson, and others of like renown, have had the administration of many of these grants. These have included for the last six years (save in 1890), the appropriation of $500 per year for a table in the Naples Marine Laboratory.†

We have no single society in this country, save the Smithsonian, that can rival in importance those that I have named in England. And the Smithsonian is not a society, but an institution, established by one man, and he an Englishman. This Institution, based upon the bequest of James Smithson, was founded by act of Congress of August 10, 1846. I doubt whether in any country or in any age the bequest of half a million of dollars has ever been followed by such beneficent results, or has ever so profoundly effected the life of science in any country, as the Smithsonian Institution has done in America during the last forty-four years of its existence. This has been owing (1) to the wisdom and the profound scientific insight of Prof. Henry, its first secretary and director; and (2) to the corps of able assistants and successors whom his spirit and policy have inspired. Its publications number 26 quarto volumes of *Contributions to Knowledge*, 40 volumes of *Miscellaneous Collections*, and 44 volumes of *Annual Reports*. Its *Contributions to Knowledge* rival, if they do not excel, in rarity and importance, the publications of any other society during the same period. Its expendi-

* *Proceedings*, 1891, vol. L, p. 242.

† *Report*, 1890, p. 90.

ture in publications is about $12,500 a year. Under Prof. Henry a good deal was done in research. Under Prof. Langley, the present director, astro-physical research is carried on. Besides the direct scientific work of the Institution, however, its influence has been very great, especially in its relations with the other departments at Washington, and as a medium for the prosecution of other scientific enterprises, under authority of Congress. Many of the appropriations of Congress for scientific expeditions for researches in ethnology, palæontology, chemistry, and physics have been due to the presence and co-operation of the Smithsonian Institution. For ethnologic researches alone, during the last twelve years, under the administration of the Smithsonian, Congress has appropriated $400,000; to palæontologic researches within the last three years, $160,000; to chemical and physical research, $68,000; and to astro-physical research, $10,000. Besides these, there have been for many years appropriations for maintaining the important investigations of the Coast and Geodetic Survey, and of the Weather Bureau in Meteorology; and for the great scientific work of the Naval Observatory, and of the various scientific divisions of the Agricultural Department and of the Geological Survey. Our Government has been by no means inactive in science.

The principal American scientific associations, omitting those of comparatively recent origin, are the American Philosophical Society of Philadelphia, originally founded in 1744; the American Academy of Arts and Sciences at Boston; the Boston Society of Natural History; the Academy of Natural Sciences, and the Franklin Institute, at Philadelphia; the latter founded in 1824 (see *Journal*, vol. I, pp. 71, 129); the New York Academy of Sciences (a continuation of the Lyceum of Natural History); the National Academy of Science at Washington, founded in 1863; and the American Association for the Advancement of Science. Of these, the Philosophical Society has published 29 volumes of its *Transactions*; the American Academy, 26 volumes of *Transactions* and 9 quarto volumes of *Memoirs*; the Boston Society of Natural History, 25 volumes, at a cost of about $600 per year; the Academy of Natural Science of Philadelphia, 48 volumes of *Proceedings* and 12 quarto volumes of its *Journal*, at an average cost of about $1,000 per year; the Franklin Institute, 133 volumes of its *Journal*; the New York Academy and its predecessor, about 30 volumes of *Transactions* and *Annals*; the National Academy, 3 quarto volumes of *Memoirs* and some volumes of *Proceedings*; and the American Association for the Advancement of Science, about 40 volumes of *Proceedings*.

The latter society had in 1891 a "Research Fund" of $5,254. (*Proceedings* 1891, p. 441.) None of the other societies, so far as I can find, has any fund specially devoted to research, or makes any specific appropriations therefor. The National Academy and the Academy of Philadelphia have each some funds for their support, and the latter also the Jessup Fund for students in science, on which the income is about $550 yearly. The Philosophical Society from time to time awards the prize established by John Hyacinth de Magellan in 1786, an oval gold plate, "for the most useful discovery or invention in navigation or science." One of the earliest awards of this prize was for painting lightning rods with black lead.

The American Academy of Arts and Sciences awards a gold and silver medal from a bequest of $5,000, made to it by Count Rumford, who in 1796 made a similar bequest to the Royal Society. In 1888 this prize was most worthily awarded to Prof. Michelson for his researches in light.*

The Boston Society of Natural History has a general fund, of which the income is about $6,000. It has also a small Walker prize fund and a grand prize fund, from which in 1884 it awarded a grand prize of $1,000 to James Hall, of Albany, "for his distinguished services to science." It also administers the expenditure of about $2,700 a year for instruction in laboratory work, drawn from the Boston University, and $1,500 from the Lowell fund for the instruction of teachers.†

From this comparison of the voluntary associations, it appears that the property, endowed funds, and equipment of the English societies named are nearly tenfold greater than the American, and their publications double; while for direct original research, our societies maintain no laboratories and no professors, as is done by the Royal Institution. The English societies distribute yearly from $25,000 to $30,000 for from sixty to seventy-five different scientific purposes, while ours make no such appropriations, simply because they have no funds. To supply this deficiency there is need of large endowments.

The publications of our societies are valuable; the papers have often been of a high character, rivaling those published abroad. But the funds available for publication are insufficient; it is always a question of means. There are a press and surplus of valuable scientific matter, which either is not printed at all, or only gets printed by special subscriptions for the purpose. This ought not to be. After valuable original matter has been produced with great pains and without hope of pecuniary reward, nothing is more discouraging to future research than that even publication can only be had as a charity. This I know, from repeated personal applications, is the condition of things in New York at this moment. It is not creditable that in a State and country like ours there should be practically nowhere adequate provision for even the publication of the researches of those who work for nothing but their love of science and its progress. There is very great need of a considerable publication fund, in the hands of some scientific body, through which every valuable contribution to science, not otherwise provided for, might be ensured a speedy publication, after it has been

*Pres. Lovering's Address, *Proceedings*, vol. XXIV, p. 380.

† *Proceedings*, vol. XXIV, p. 14.

found worthy, as in the practice of the Linnaean Society, first by a critical expert in the particular department, and then by the council of publication.*

The stimulus moreover to scientific research that would be imparted by the distribution of comparatively small sums, such as are given by the Royal Society and by the British Association, would also be very great: nor is there any reason why the founding of professorships for the express purpose of prosecuting original research in our scientific societies, after the model of the Royal Institution, should not in time be followed by results equally brilliant, and equally beneficial to mankind.

I have endeavored to point out three main directions in which there is urgent need in this country of pecuniary endowments.

(1) In relief of professors during the transition of the colleges from the school-master system to the university system, whereby all professors in science shall become actively enlisted in the prosecution of original discovery as a part of their duties.

(2) In providing for the future recruits in science, by more endowments for post-graduate study.

(3) By endowments of our scientific associations, both directly to promote original research, and especially also to supply larger means of publication.

It is gratifying to perceive what beginnings have been recently made in response to the needs of science. Only a short time since, in 1885, Mrs. Elizabeth Thompson, of Stamford, Conn., gave $25,000 to a board of trustees of which Dr. Bowditch, of Boston, is president, for the "advancement of scientific research in its broadest sense." The income is annually distributed in sums of from two hundred to five hundred dollars.

Mr. Hodgkins, of Setauket, Long Island, has recently bequeathed to the Smithsonian Institution $200,000, the income of one-half of which is to be devoted to research into the properties of atmospheric air.

Columbia College has, during the past year, received from Mr. De Costa's estate, before referred to, $100,000 for biology; Harvard, the Joseph Lovering fund, above stated; $10,000 from Henry Draper for the photography of stellar spectra; the endowments in archaeology, above named; and some smaller gifts for various scientific purposes. The University of Chicago and some other institutions have also received important gifts, not to mention those yet to be realized to other colleges from the estate of Mr. Fairweather.

By a recent bequest of Charles Lenning, the Academy of Sciences of Philadelphia will, in time, receive $20,000; while half a million of dollars will go to the University of Pensylvania in aid of instruction in theoretical and practical mechanics, and $200,000 to maintain scholarships. At this University, also, a superb structure for the "Wistar

* President Carruthers, *Proceedings, Lin. Soc.*, May, 1890, p. 39.

Institute of Anatomy" is now building by Gen. Isaac J. Wistar, at a cost of about $200,000, including endowments designed for original research.*

Our reliance in this country must be mainly upon private endowments and an intelligent appreciation of the needs of science. The national Government has done, and is doing, much in certain directions. But aside from the dispositions of legislators, it is restricted by the provisions of the Federal Constitution, and by debated questions of constitutional right. State aid is not thus hampered; but State aid is difficult to obtain, to any adequate degree, on account of the previous habits, prejudices, and political training of the people. No doubt this ought not so to be. The State of New York ought, abstractly considered, to maintain one university of the first class equal in every department to any in the world. But the multiplication of institutions already existing, local jealousies, and aversion to State taxation, make this now probably impracticable.

The remedy is with the people, and through their own voluntary methods. It is the people who have made our Government, its institutions, its methods, and the great aggregate, whatsoever it is, such as we see it to-day. Wealth is rapidly accumulating; much of it in the hands of those who, springing from the people, bear the love of the community in their hearts: and when they and the people at large shall come to see that the cause of scientific advance and the discovery of all new truth are in the deepest sense their cause, responses will, I believe, come to every urgent need; until the work of the people, by its own methods, shall, even in science, be able to confront, without shame, the best work of the monarchies of the Old World.

*Since the above was written an additional million of dollars has been given by Mr. John D. Rockefeller to the University of Chicago, making $3,600,000 given by him alone to that institution within less than three years, a munificence hitherto unexampled in private endowments, some portions of which, it is hoped, will be available for the maintenance of original scientific research.

THE INVENTORS OF THE TELEGRAPH AND TELEPHONE.*

By Prof. Thomas Gray, F. R. S. E.

The word telegraph was introduced about one hundred years ago as a name for a means of conveying intelligence to a distance by means of signs. The signs were produced in a variety of ways, as, for example, by the shapes or positions of bodies placed on high poles, or by letters or words of sufficient magnitude similarly exposed. The meaning of the word telegraph, interpreted by its original use, would thus be to write or make signs at one place in such a way that they could be read or interpreted at a distant place. It appears, therefore, that so long as we confine our attention to early methods of telegraphing, the signs or signals were made at the sending station and read from the receiving station. Modern usage gives a slightly different meaning to the word, namely, a means of producing visible, audible or written signs at a distance. That is to say, the signs are to be produced at the receiving station. This was first accomplished on an extensive scale and at great distances by means of electricity. Methods of transmitting sounds, or even speech, to moderate distances by means of tubes and by means of what we now call string or mechanical telephones have however been known for several centuries.

Methods of conveying intelligence to a distance have been known and used from very early times. Fires seem to have been the earliest means employed for giving signals, and we find such signs referred to in the writings of the Prophet Jeremiah, of Eschylus, of Polybius, and others. Schottus, in his *Technica Curiosa*, proposes the application of the telescope to view posts erected on an eminence at a distant station, and on which signs were to be placed. The Marquis of Worcester, in his *Century of Inventions*, enumerates a day and a night telegraph; and Kessler, in his *Concealed Arts*, proposes to cut out letters in boards and make them visible at a distance by placing them over the end of a cask in which a light is burning; the letters or other characters being exposed in proper succession any message can be transmitted.

One of the earliest telegraphs of which we have now a direct representative was the flag signals introduced about the middle of the sev-

* An address on the occasion of the Centennial celebration of the organization of the United States Patent Office, delivered in Washington. (*Proceedings and Addresses*, 1891, pp. 175–198.)

enteenth century by the Duke of York (afterward James II, of England), who was at that time admiral of the English fleet. This was the beginning of the flag telegraph still used for communicating between ships at sea, originally introduced for the purpose of directing the maneuvers of the fleet. In 1684, Dr. Robert Hook communicated to the Royal Society of London a proposal for a telegraph. In this method the signs were to consist of bodies of different shapes placed on high poles in an exposed position. Some years afterwards a similar method was proposed to the Academy of Sciences by M. Amontons, a French natural philosopher. In 1767 Mr. R. L. Edgeworth proposed to telegraph by means of the arms of a wind-mill, the positions of the arms of the mill to be used to indicate the signals. In 1784, the same author proposes to make the signals indicate numbers, and to interpret by means of vocabularies of numbered words. In 1794, the semaphore telegraph of M. Chappe was adopted by the French Government. This telegraph consisted of a high post and two bars of timber, the middle one pivoted to one end of the other, and the free end of this second bar pivoted to the top of the post, so that the whole of the motions could take place in a vertical plane. The positions, relative to the vertical or horizontal, of the two arms indicated the signal. These and other modifications of the semaphore have been at various times used, and are still used on railways for train signals.

The chief interest of these early telegraphs—a great many forms of which might be enumerated—is in illustrating the fact that some means of conveying intelligence to a distance quickly and without a messenger has, from the earliest times, been recognized as of great importance. It is well also to keep before us the things that have been done in earlier times when we attempt to judge of the advances which have been made by modern invention.

The telegraph of to-day is almost entirely electrical, and in its present form it is of comparatively recent growth. It may be well, however, in this branch also to glance briefly at the early history of the subject. To begin with what we may call the fable period, we find in the year 1617 an allusion in one of Strada's *Prolusiones Academicæ* to the belief that there existed a sympathy between needles which had been touched by a species of loadstone, which caused them always to set parallel to each other if they were free to take up such positions. Two such needles, it was said, could be used to convey intelligence to any distance, because if they were pivoted on cards marked with letters or words and the card properly placed, so that corresponding letters occupied similar positions, when one needle was made to point to any letter or mark the other needle would immediately point to the corresponding mark on its card. The same belief is referred to by Galileo in one of his dialogues in 1632, and again by the Abbe Barthelemy in a work entitled "Voyage du Jeune Anarcharsis," published in 1788. So far this may be said to be mere fable, but it gives an idea what were then looked upon as possibilities in magnetism, and we can hardly help comparing with these ideas some almost equally extraordinary ones which are occasionally expressed at the present day with respect to electricity.

The discovery of Stephen Gray, in 1729, that the electrical influence could be conveyed to a distance by means of an insulated wire, is probably the first of direct influence in connection with telegraphy. As a result of this discovery, and the investigations which followed it, we find a considerable number of proposals to use electrical forces for the transmission of intelligence. The first of these of which there is any record was made by Charles Morrison, of Renfrew Scotland, in a letter to *The Scots' Magazine*, Edinburgh, for February 17, 1753, and signed "C. M."* As many insulated wires as there were characters to be signaled were to be erected between the two stations. At the receiving station the ends of the different wires were to be connected to a series of balls, underneath which the characters, printed on light pieces of paper, were to be placed. If any one of the wires became electrified by the distant end being put in contact with the source of electricity, the character under the ball on the end of it would be attracted and thus indicate the signal. An interesting modification was suggested in the same letter, namely, to replace the balls by a series of bells of different pitch, arranged in such a way that when the wires became electrified they would discharge into the bells and cause them to sound: - - - "the electric spark, breaking on bells of different size, will inform his correspondent by the sound what wires have been touched: and thus, by some practice they may come to understand the language of the chimes in whole words without being put to the trouble of noting down every letter." A similar telegraph was invented in 1767 by Joseph Bozolus, a Jesuit and a lecturer on natural philosophy in Rome. (See a Latin poem, entitled "Mariani Parthenii Electrocorum," in VI Libros, Roma, 1767, p. 34.) In 1774, a telegraph on the same principle was established by Le Sage. In this system each wire terminated in a pith-ball electroscope, and the signals were read in accordance with the indications of these electroscopes, of which twenty-four were used. This telegraph was improved upon by Lomond in 1787, one wire only being used, and a code of signals forming the means of interpretation. A similar proposal was made by Betancourt in the same year and again by Cavallo in 1795. The latter proposed to use combinations of sparks as a code of signals. In 1794, Reizen proposed to cut letters out of tin foil, leaving a series of short interruptions of the tin foil at short distances apart, so that a discharge of electricity around the tin foil would illuminate the letter by a series of sparks. This method of producing illuminated patterns is still a common classroom experiment in physical lectures. The next to propose the use of

*The question of the identity of "C. M." is discussed in "A History of Electric Telegraphy," by J. J. FAHIE (London, 1884), pp. 68-77.

static electricity for telegraphic purposes seems to have been Ronalds, of Hammersmith, in 1816. In this telegraph the letters were printed on a disk which was mounted on the seconds arbor of a clock. One of the clocks was placed at the sending and the receiving stations, and arranged to bring corresponding letters simultaneously opposite a small window in the dial of the clock. When the proper letter was exposed a signal was sent by means of a pith-ball telegraph. This telegraph was more complicated than several which have been mentioned above, and required two clocks going synchronously.

In the year 1767 an important observation was made by Sulzer. He found that when two plates of different metals were placed one above and the other below the tongue, a peculiar sensation and taste were felt when the metals touched each other outside the tongue. Sulzer failed to find the explanation of this phenomenon, and no further advance was made until the well-known frog experiments of Galvani gave fresh impetus to the subject. The discoveries of Volta and the invention of the voltaic pile shortly followed. In the same year (1800) an attempt to close the circuit of a voltaic battery by means of a drop of water led Nicholson and Carlisle to the discovery that water is decomposed by the galvanic current.

This gave rise to the galvanic or electrolysis telegraphs of Sömmering, Coxe, and Sharpe, and is the basis of all the chemical printing and copying telegraphs which have in more recent times been produced. Sömmering's telegraph was invented in 1809, and was similar in principle to that of Morrison, except that the decomposition of water and consequent accumulation of gas in a series of tubes gave the necessary indications. To call attention, it was proposed in connection with the telegraph to liberate an alarm by means of an accumulation of gas.

Prof. Coxe, of Pennsylvania, described a similar telegraph in 1810, and proposed to use either the decomposition of water or of metallic salts. Mr. J. R. Sharpe proposed a voltaic telegraph in 1813, and exhibited it before the Lords of the Admiralty, "who spoke approvingly of it, but added, that as war was over and money scarce, they could not carry it into effect. (See *Repertory of Arts*, Second Series, vol. XXIX, p. 23.)

Perhaps the most important electrical discovery in its influence on telegraphy was made by Romagnési, of Trente, in 1805, but received little attention and no development until it was re-discovered by Oersted in 1819. This was the discovery that a wire conveying an electric current is capable of deflecting a magnetic needle. In the following year Schweigger discovered that the deflecting force was increased when he wound the wire several times round the needle. These two discoveries formed the foundation for the construction of the galvanoscopes and galvanometers since so much used in connection with electrical appliances and measurements. One of the most extensive applications has been to telegraphy, Galvanoscopic, or, as they have been more commonly called, needle telegraphs resulted very shortly from these discoveries. In this field of invention we find, prominent among the early workers, the distinguished names of Ampère, Gauss, and Weber. Ampère proposed a multiple wire telegraph with galvanoscope indicators in 1820. A modification of Ampère's telegraph was carried out by Ritchie, and afterwards exhibited in Edinburgh by Alexander. In this telegraph thirty wires were used, twenty-six for the letters of the alphabet, three for signs of punctuation, and one for the end of a word. The galvanoscope needles each carried a small screen which in its normal position covered the letter, but which, on the passage of a current through the wire, was drawn aside, exposing the letter to view. The transmitting keys were arranged like the keys of a piano-forte. With the exception of the use of galvanic instead of static electricity this telegraph was not much in advance of the proposal of Morrison. A single circuit telegraph was invented in the year 1828 by Tribaoillet, who also used a galvanoscope as the indicator.

In 1832, a five-needle telegraph was invented by Schilling, who also used a single needle and single circuit telegraph, using reverse currents and combinations of signals for an alphabet. Models of this telegraph were made and exhibited before the Emperor Alexander and others, but Schilling unfortunately died before any practical result was attained. In 1833, Schilling's telegraph was developed to some extent by Gauss and Weber, who used it for experimental purposes. The chief modification introduced by these experimenters was the substitution of induced currents, produced by the motion of a coil of wire surrounding a bar magnet, for the galvanic currents used by Schilling. The following translation of a part of a report of the magnetic observations of these physicists given in Poggandorf's *Annalen*, XXXII, p. 568, is quoted from Sabine's *Electric Telegraph*: "There is, in connection with these arrangements, a great and until now in its way novel project, for which we are indebted to Prof. Weber. This gentleman erected during the past year a double-wire line over the houses of the town (Göttingen) from the Physical Cabinet to the Observatory, and lately a continuation from the latter building to the Magnetic Observatory: thus an immense galvanic chain (line) is formed, in which the galvanic current, the two multipliers at the ends being included, has to travel a distance of nearly 9,000 (Prussian) feet. The line wire is mostly of copper, of that known in commerce as 'No. 3,' of which one meter weighs eight grams: the wire of the multipliers in the Magnetic Observatory of copper, 'No. 14,' silvered, and of which one gram measures 2.6 meters. This arrangement promises to offer opportunities for a number of interesting experiments. We regard, not without admiration, how a single pair of plates, brought into contact at the further end, instantaneously communicates a movement to the magnetic bar, which is deflected at once for over a thousand divisions of the scale." And further on in the same report: "The ease and certainty with which the manipulator has the direction of the current, and therefore the movement of the magnetic needle, in his command, by means of the communicator, had a year ago suggested experiments of an application to telegraphic signaling, which, with whole words and even short sentences, completely succeeded. There is no doubt that it would be possible to arrange an uninterrupted telegraph communication in the same way between two places at a considerable number of miles distance from each other."

The method of producing the currents in Gauss and Weber's experments was an application of the important discoveries of Faraday and Henry in the induction of currents by currents and by magnets, which have since borne so very important fruit in the field of dynamo-electric machinery.

On the recommendation of Gauss this telegraph was taken up by Steinheil, who, following their example, also used induced currents. The important contributions of Steinheil were the discovery of the earth circuit, made while attempting to use the rails of a railway as telegraphic conductors; the invention of a telegraphic alphabet and a recording telegraph. Of these the discovery of the earth circuit, made in 1837, has proved of great value. An interesting description of Steinheil's telegraph, together with illustrations of the magneto-electric and recording apparatus used on the line erected in 1837, between Munich and Bogenhausen, will be found in Sturgeon's *Annals of Electricity* (vol. III). This account, written by Steinheil himself, shows that he had at that time an excellent appreciation both of the mechanical and electrical properties which a good practical electric telegraph should have, and also that he was well versed in the knowledge then existing of electrical science. The relative merits of scopic, acoustic, and recording telegraphs are discussed, and the advantages, which experience has since brought into prominence, of the acoustic telegraph is pointed out. A very good discussion of the most economical method of arranging signals for a telegraphic alphabet will also be found in this paper.

Schilling's telegraph, which we have just seen, was the model on which Gauss and Weber's, and, therefore, also Steinheil's telegraphs were based, was, as we shall see presently, also the basis of Cooke's, and of Cooke and Wheatstone's needle telegraphs.

Previous to the date which we have now reached (1837), another epoch-making discovery had been made, which has had great influence on telegraphy. This was the discovery of the magnetizing influence of the current. The discovery of Oersted was followed by Ampere in a long series of researches, in which, among other things, he established the mutual attractions and repulsions of wires carrying currents, the fact that the voltaic element itself acts on a magnet like any other part of the circuit, and that a spiral of wire forming part of a circuit, would magnetize steel needles. In the same year M. Arago found that a wire conveying an electric current attracted iron filings, and in 1824, the law of the variation of magnetic force with varying distance from the wire was investigated by Barlow. In 1825, Sturgeon found that a bar of soft iron was rendered temporarily magnetic if surrounded by a helix of wire through which an electric current was passing. In the year 1827, Ohm propounded his celebrated law of the conduction of currents. In 1831, Faraday in England, and Henry in America, discovered the induction of currents by currents and by magnets. We see from these leading facts that in the twelve years succeeding Oersted's discovery the knowledge of electricity and of magnetism in the directions important for telegraphic application was very great, and we shall see that it quickly bore fruit.

Schilling's telegraph was exhibited at a meeting of German naturalists held at Bonn in 1835, and was there seen by Prof. Muncke, of Heidelberg, who, after his return to Heidelberg, made models of the telegraph and exhibited them in his class room. These models were seen by Cooke in the early part of 1836, and gave him the idea of introducing the electric telegraph in England. Cooke immediately set to work to construct a telegraph on a similar plan, and worked out a three-needle system of signals, which has been to some extent confounded with the five-needle telegraph afterwards patented and introduced by him in conjunction with Wheatstone. While arranging for experiments on the London and Manchester Railway, Cooke was introduced to Wheatstone, and afterward consulted him as to difficulties he had met with in his experiments. A partnership soon followed, which led Wheatstone to devote considerable attention to the subject. The result has been the production of a considerable variety of telegraphic apparatus of great value and ingenuity.

Steinheil was anticipated in the idea of making the electric telegraph self-recording by Morse, of New York, who, according to a considerable amount of evidence brought forward by Morse himself, thought out some arrangement as early as 1832. Exactly what Morse's first ideas were seems somewhat doubtful, and he did nothing till 1835, when he made a rough model of an electro-magnetic recording telegraph. This telegraph consisted essentially of a pendulum, which carried a marking pencil on its lower end, and which could be deflected by an electro-magnet. The deflections of the pendulum were recorded on a band of paper, which was moved forward by clockwork under the pendulum, and simple combinations of deflections were to represent numbers. The interpretation of the message was to be made by means of a telegraphic dictionary, in which the words, phrases or sentences were to be numbered. There was no hint at this time of the alphabet with which we are now so familiar as the "Morse Code" or the "Morse Alphabet." This alphabet now almost universally used and which has probably done more than anything else toward perpetnating the name of Morse, being that which perpetuates the name "Morse System," was not invented by Morse but by Vail, who was associated with him in the development of the telegraph.* The dictionary of numbered words proposed by Morse was proposed by Edgeworth in 1794, in connection with his semaphore telegraph. The model made in 1835 shows little mechanical ingenuity. The method of transmitting the signals, which was by means of type moved through a contact-making device, was somewhat crude and much less convenient than the simple make-and-break circuit devices of several previous workers; and the electro-magnet used to deflect the pendulum showed almost complete ignorance of the principles then known of electro-magnetism. The chief points of interest in connection with the early history of the Morse telegraph lie in the proposal to use electro-magnetism as the motive force to move the recording pendulum and the idea of making the telegraph self-recording. Morse made positive claims to have been the first to do both of these, and it seems proper that his claim should be examined.

After the discovery of Sturgeon in electro-magnetism became known among scientific men the subject was taken up by Prof. Henry, who was then teaching physics in Albany Academy. An account of part of Henry's experiments was published in Silliman's *American Journal of Science*, for January, April, and July, 1831.

The following, among other things, were subjects of investigation in these experiments: The laws which govern the magnetizing effect of a helix under varying conditions as to number of turns in the helix, nature or arrangement of the battery, and length of the external circuit; the carrying power of magnets having different kinds of winding and different lengths of wire in the coils; the construction of an electro-magnetic engine. The transmission of power to a distance by means of his electro-magnetic engine. Among the applications were the closing of a distant electric circuit by means of the armature of an electric magnet, the coils of which were included in another circuit passing through an operating or transmitting station, and the transmission of signals to a distance by causing the armature of an electro-magnet to strike a bell each time a current was sent through the coils of the magnet from the transmitting station. The latter of these applications was illustrated by means of a model apparatus included in a long circuit of wire taken several times round one of the rooms in Albany Academy. The following claims made in this connection by Professor Henry are well founded, and deserve quotation:

"(1) Previous to my investigations the means of developing magnetism in soft iron were imperfectly understood, and the electric magnet which then existed was inapplicable to the transmission of power to a distance.

"(2) I was the first to prove, by actual experiment, that in order to

* See *Smithsonian Report* for 1878, p. 341–344.

develop magnetic power at a distance a galvanic battery of "intensity" must be employed to project the current through the long conductor, and that a magnet surrounded by many turns of one long wire must be used to receive this current.

"(3) I was the first to actually magnetize a piece of soft iron at a distance, and to call attention to the fact of the applicability of my experiments to the telegraph.

"(4) I was the first to actually sound a bell at a distance by means of the electro-magnet.

"(5) The principles I had developed were applied by Dr. Gale to render Morse's machine effective at a distance."

It is to Henry, undoubtedly, that is due the credit not only of first pointing out the application of electro-magnetism to telegraphy, but also of supplying the requisite knowledge of how to make magnets suitable for the transmission of signals through long distances, which rendered the practical application possible at that time. Besides this, we see that Henry actually constructed an experimental line and made the first electro-magnetic sounder, which consisted of a receiving magnet with a polarized armature, one end of which was attracted by a magnet and the other end to sound a bell. Again, in the method of closing one circuit by means of a magnet in another circuit, we have the electro-magnetic relay, afterwards re-invented by Morse and others, and now very widely used on long telegraph circuits both for closing "local circuits" and for "translation."

The credit of inventing the electro-magnetic telegraph was claimed by, and has usually been, popularly at least, given to Morse. There has been some dispute as to who first suggested the electro-magnetic telegraph, the idea of it having arisen out of a conversation among the passengers on board the ship *Sully* during a passage from France to New York in 1832. Dr. Jackson, of Boston, claimed to have been the originator of the idea, and it seems not unlikely that information which he is said to have given with reference to the early experimental telegraphs then being worked on and exhibited in various parts of Europe did originate the idea. It is not clear however that the use of the electro magnet was suggested by Jackson, and there is sufficient evidence to show that Morse had had opportunities of seeing a copy of Sturgeon's magnet in Prof. Dana's laboratory in New York. The magnet made by Morse was itself almost an exact copy of this, and it was only after failure with it that he appealed to Dr. Gale for assistance. Dr. Gale gave the necessary information and supplied the materials for making the change, afterwards informing Morse that he had learned how to arrange such an apparatus from the writings of Prof. Henry. Probably the idea of using an electro-magnet was original with Morse. He did not know of Henry's work or indeed anything about the subject beyond the few experiments in which he had seen Sturgeon's magnet used, and would naturally turn to that means of obtaining motive force. It is not necessary, however, when giving Morse due credit for his originality, to ignore the fact that, although unknown to him, the scientific part of the invention had already been worked out by Henry, and besides that, through Dr. Gale, Morse actually made use of Henry's discoveries before he succeeded in making his scheme practicable. Morse afterwards objected to Henry's claims, which were brought before the public by enforced testimony in the law courts, and not by any individual motion on Henry's part. The public have lauded Morse and have paid him liberally for the little he actually did, while it was with great difficulty that Congress could be persuaded to make a petty allowance to Henry's family, although he had been for many years a public servant, and besides had probably added more than any other man to the scientific reputation of the United States. Many people think that scientific men ought not to patent their discoveries. Which is the better known name, Henry or Morse? Would not Henry have gained both in popularity and in scientific reputation if he had patented and made the public pay liberally for his discoveries?

From the brief sketch just given it will be seen that in looking over the history of the early endeavors to produce a telegraph many ideas have been brought forward as to codes of signals, alphabets, telegraphic dictionaries, methods of calling attention by alarm apparatus, methods of arranging and operating the circuits, and so on, that only required an efficient motive force to render them practical and reliable systems. In reviewing the subject, therefore, we are forced to the conclusion that the telegraph was not the invention of any man, but the result of a gradual growth toward which many minds, some of them the ablest the scientific world has known, have contributed.

We have now reached a stage in the history of this subject when inventors may be said to have had the fundamental principles of the subject, as it now stands, before them and we have simply to look for developments. These developments have been great and of a very varied character. It is impossible in this address to do more than sketch a few of their leading features.

As already stated, the telegraph of Schilling, through a model exhibited by Prof. Muncke, of Heidelberg, gave the ideas of an electric telegraph to Cooke in the year 1836. It appears, also, that Wheatstone was aware of these early experiments, and had himself paid some attention to the subject. His experiments on the velocity of electricity, made in 1834, are sufficient to show that he was at that time aware that signals could be produced at the end of long circuits of wire by electrical means. The joint work of Cooke and Wheatstone led, within a few years, to considerable improvements in the needle telegraphs. The various forms of needle telegraph used by them, resulting in the final adoption of the single-needle system, for a long time extensively used in England, were passed over in a few years. Various modifications of the needle telegraph were, somewhat later, patented by the brothers H. and E. Highton, including an interesting form in which the current was passed through a strip of gold leaf placed in front of the pole of a magnet. Each time the current passed the gold leaf was deflected, and thus served in place of an index needle.

A patent was granted to Wheatstone and Cooke in 1840 for improvements in giving signals and sounding alarms at distant places by means of electric currents. In this patent the first form of the letter-showing, dial, or A, B, C telegraph, as it has been variously called, is described. Improvements were subsequently made in this apparatus by Wheatstone, and several modifications have been made by other inventors, of which the best known are Brequet's, Froment's, Siemens', Chester's, Kramer's, Siemens and Halske's, and Hamblet's. The first apparatus devised by Wheatstone was actuated by voltaic electricity, but in the later forms magneto-electricity was applied. One or other of these methods has been used in the other forms of apparatus for the same purpose. Wheatstone also worked on a type-printing telegraph, which was a modification of his A, B, C instrument, but it never came into practical use. Probably the greatest achievement of Wheatstone, judged at least by its practical results, was his automatic recording telegraph, which is so largely used for press and other long dispatches in England, and which has attained to marvellous speeds for a mechanical recorder.

Morse's telegraph first came before the Patent Office in the form of a caveat filed by him on the 3d of October, 1837. The following inventions were specified: First, a system of signs by which numbers, and consequently words and sentences, are signified; second, a set of type, adapted to regulate and communicate the signs, with rules in which to set up the type; third, an apparatus called the port rule, for regulating the movement of the type rules, which rules, by means of the type, regulate the times and the intervals of the passage of electricity; fourth, a register, which records the signs permanently; fifth, a dictionary, or vocabulary of words, numbered and adapted to this system of telegraph; sixth, modes of laying conductors to preserve them from injury.

This caveat gives a good idea of the invention by Morse of the recording telegraph previous to his partnership with Vail. The partnership was agreed upon in September, 1837, and according to it Mr. Vail undertook to construct at his own expense and exhibit before a committee of Congress one of the telegraphs "of the plan and invention of Morse:" that he should give his time and personal services to the work, and assume the expense of exhibiting the apparatus and of procuring patents in the United States. In consideration, Vail was to receive one-fourth of all the rights in the invention in the United States. Provision was also made for securing to Vail an interest in any foreign patents which he might furnish the means to obtain.* A large amount

* See F. L. Pope in the *Century Magazine*, vol. XXXV, p. 924 *et seq.*

of documentary evidence bearing on the development of the telegraph exists in the possession of Mr. Vail's family, and in the National Museum at Washington. From this evidence there seems no doubt but that Morse assumed and has been accorded very much more than his share of the credit of the invention of the telegraph as it is now known. The patents taken out in Morse's name included many important improvements which were entirely due to Vail, and for which Morse promised to give him credit, a promise which was never publicly redeemed. The alphabet now used was, as I have already said, worked out by Vail, who, it appears, first began its formation by an attempt to classify the letters of the alphabet according to frequency of occurrence, with the view of giving these letters to simplest signs. After working on this for some time, it occurred to him that valuable information might be obtained in a printing office, and a visit to an adjacent newspaper office showed him the whole problem solved in the printers' type tray. The alphabet which he afterwards formed is still used in this country and also, with some simplifications, as a European and international code. The modification of the recording apparatus from the vertical pendulum and recording pencil to the compact instrument with a horizontal lever and metallic stylus, marking by indentation, used on the first telegraphic line between Washington and Baltimore, was also due to Vail. Many other things might be mentioned to show that in the early stages of this invention, which has marked so wide a step in our modern civilization, the name of Vail deserves a prominent place. It is very unfortunate that his own modesty, together with his confidence in Morse's promises to do him justice, prevented the matter from being publicly ventilated during the lifetime of the inventors.

After several unsuccessful attempts to induce Congress to assume the expense of building a line of sufficient length to practically test the proposals of Morse, an appropriation of $30,000 was made in March, 1843, for the purpose of building a line from Washington to Baltimore. This line was completed and successfully opened on the 24th of May, 1844. The system practically introduced with the opening of this line, modified in some of its mechanical details, has continued to be the principal one used, and is the basis of most of the recording telegraphs in all countries. One important modification should however be mentioned, that is the wide use of the click of the armature for reading the message in preference to the recorder. This is a return to the electro-magnetic acoustic telegraph of Henry. It gives one of the simplest possible receiving instruments, and as was long ago pointed out by Steinheil, possesses the great advantage that it leaves the eyes of the operator disengaged. Of other forms of telegraphic apparatus, the most important are the type-printing telegraph. Among the early inventors of these we find Vail, who invented a type-printing telegraph as early as 1837, and Wheatstone; but the first instrument practically used was invented in 1846 by Royal E. House, of Vermont. This instrument was used for some time in the United States, and was brought to a considerable degree of perfection. It worked on the step-by-step principle and was patented in 1846. Another type-printing telegraph of great ingenuity was invented by D. E. Hughes, of Kentucky. This apparatus embodies many of the features of the apparatus used at present in this country, which is a modification of Hughes's instrument due to Mr. Phelps. The Hughes instrument is still largely used in France and to some extent in other European countries. The Hughes patents in this country were purchased in 1856 by the American Telegraph Company, and the apparatus has undergone successive modification at the hands of Mr. Phelps, tending towards simplification, accuracy of working, and increased speed. One of the latest modifications is known as the Phelps's Electro-Motor Telegraph, in which the mechanism is driven by means of an electromotor which, running at a high speed, allows the clock-work train to be short and light. The principle here used is the synchronous movement of a transmitting shaft on the transmitter and type-wheel of the receiver. Sychronism is obtained by a governor, and continuous rapid motion is kept up. The letter printed is regulated by the position of the transmitting shaft when the circuit is closed, this position being under the control of the operator. Phelps is also the inventor of stock telegraphs and private line printing telegraphs, and, besides his similar instruments have been invented by Laws, Calahan, Gray, and others. These instruments work on the step-by step principle and all of them are beautiful specimens of mechanism and scientific ingenuity.

Another system of recording telegraph messages requires notice—that is the chemical method. We have seen that very early in telegraphic history the decomposition of liquids and of solutions of salts were made the basis of telegraphs. It was soon found that a ribbon of paper or cloth saturated with certain chemicals could be very readily marked by the passage through them of the electric current. One of Morse's first plans appears to have been a chemical telegraph, but that, I believe, was never worked out. The first patent for such a telegraph was given in England to Edward Davy in 1838, but the system never came into practical use. It was complicated in construction and required four line wires. One interesting feature was the use of an electro-magnetic escapement for moving the paper, an idea which had occurred to Cooke and to Wheatstone some years earlier. The first successful chemical telegraph was due to Bain, of Edinburgh, and was patented in 1846. In this system it was proposed to transmit the message by an automatic transmitter, using a punched slip of paper to regulate the contacts. Some difficulties with the mechanical operation of preparing the necessary stencil slips prevented this being very successfully used, but the chemical record was used for some years both in England and America. With the apparatus now available for transmission, very high speeds can be attained by this method of recording the signals.

The chemical method of recording has been mostly used for copying or autographic telegraphs, and of these a considerable number have been devised. The automatic method of transmission has been brought to a high state of perfection. Among others who have worked at the subject are Wheatstone, Siemens and Halske, Garnier, Humaston, Little, Edison, Park, Thomson.

The next important step in telegraphy was the employment of one line-wire to convey more than one message at the same time. A solution of the problem of sending two messages, one in each direction, was attempted by Gintt of Vienna, in 1853, and in the following year by Frischen and by Siemens and Halske. These methods were not very successful, but they were mechanically sufficient for the purpose. They however left out an important item in the account, namely, the electrostatic capacity of the line. The proper solution of the difficulty was given by J. B. Stearns, of Boston, in 1871, who solved the problem completely, so far at least as land lines were concerned. The same principle is sufficient for all purposes, but some important modifications in detail are necessary for submarine cables. These modifications were successfully made by Muirhead, of London, and at the present time duplex working is an ordinary accomplishment. The chief workers in this field were Frischen, Siemens and Halske, Stark, Edlund, Gintt, Nystrom Preece, Fur Nedden, Farmer, Maron, Winter, Stearns, and Muirhead.

Next the problem of sending two messages in each direction was worked out. This involves the additional problem of the simultaneous sending of two messages in the same direction. The solution of this problem was attempted by Dr. Wm. Gintt, of Vienna, in 1853, and during the following ten years it was worked at by Borscha, Kramer, Maron, Schaak, Shreder, Wartman, and others. The first to obtain success was Edison, in 1874; and his method, with some modifications, is still used. Systems of quadruplex were also invented by Gerrit Smith, in 1875 and 1876, of the Western Union Company, and a modification of Edison's method was made by Prescott and Smith. Smith's 1876 method is known as the Western Union Company's Standard Quadruplex.

A system of multiple transmission was devised by M. G. Farmer, of Salem, in 1852, in which, by a commutation arrangement, the line-wire was put successively in contact with a number of local circuits. A similar system was exhibited by Meyer at the Vienna Exposition in 1873, and an improved form was introduced a few years ago by Delany, which is in use in several countries. These systems are of use if the line-wire is capable of doing more work than any one of the stations is capable of supplying, and may be likened to one of the main wires from the central to a district telephone exchange, with this exception, that all the correspondence goes on simultaneously, and there need be no difficulty as to precedence. Distinctive from these is the harmonic telegraphs of Elisha Gray, Edison, and Bell. In this system, which has been most completely worked out by Gray, any number of messages may be sent simultaneously, without reference to speed of transmission. In principle, the method consists in causing each of a number of vibrating reeds at one end to pruduce pulsations of the current flowing through the line, which have the same period as the vibrations of the reed. A corresponding set of reeds at the receiving end of the line are arranged so as to be acted on electro-magnetically by the current. Each of these receiving reeds will respond only to the pulsations of its own natural period, and hence only to the vibrations of the corresponding reed at the sending end. The continuity of these vibrations may be broken up by means of a sending key, and thus a message transmitted in the ordinary "Morse" alphabet.

The autographic or writing telegraphic apparatus, which has been developed of recent years, is of great interest, both from the fact that the handwriting of the sender is reproduced in facsimile, and from the great ingenuity of the apparatus employed. The writing telegraph of Cowper and the telautograph of Elisha Gray are good examples of this mode of transmitting messages.

In Cowper's system two rectangular components of the motion of the pen are made to vary the resistance, and consequently the current, in two line wires. These currents act on two electro-magnets at the receiving station, and the armatures of the electro-magnets are arranged to produce two rectangular components of the motion of the receiving pen. Bands of paper are kept moving at approximately the same rate under each of these pens, and hence the characters traced by the motions of the transmitting pen are reproduced with considerable accuracy by the receiving pen in consequence of the varying positions of the armatures of the receiving magnets, caused by the variations of the current. In Gray's apparatus two rectangular components of the motion of the transmitting pen send pulsatory currents into the line wire. These pulsatory currents cause corresponding movements of the armatures of two receiving magnets, which are made to move the receiving pen in corresponding directions, and through proportionate distances. Separate electro-magnetic arrangements lift the pen off the paper between the words and at the end of the lines, and allow the receiving pen to be moved backwards or forwards without marking the paper. Still another electro-magnetic arrangement is used to move the paper forward between the lines. The whole apparatus is exceedingly ingenious, but much too extensive and complicated to admit of clear description here.

Although the mere extension of telegraphs from land to submarine lines can hardly be called an invention, yet very many new problems presented themselves for solution in this extension. Many of these problems were of a more purely scientific character than those presented in the developments which had been in progress, and consequently tested the knowledge then existing of the laws of electricity much more severely. It was very soon discovered, for example, that the rate at which signals could be transmitted, and the battery power or other electro-motive force necessary to effect the transmission, did not, as in land lines, depend almost entirely on the size and length of the conductors used. The electrostatic capacity of the line immediately began to play an important part, and signals were found not to be transmitted so instantaneously as they were on existing land lines. Again, there was no opportunity of using relays, so as to effectively shorten the longer lines, and the investigations of Thomson led him to point out that the rate of signalling would be inversely as the square of the length.

Such difficulties as these, combined with the very evident difficulties involved in manufacturing and submerging a cable in deep water, were, to say the least, discouraging. Experiments on short lengths in the English channel and elsewhere proving successful, faith in the possibility of longer cables grew, and very soon, through the enterprise of a few American and English business and scientific men, an attempt was made to lay a cable across the Atlantic. The history of that undertaking and its various failures are almost common knowledge, but perseverance conquered all the difficulties, and to-day no one thinks of the probability of failure when a long cable is proposed.

The laying of long cables brought out the fact that, as had been anticipated, existing telegraphic apparatus was not of great enough sensibility to render moderately rapid signaling possible. This difficulty was almost immediately met by the mirror galvanoscopic receiver of Thomson, followed some years later by his siphon recorder, which is undoubtedly by far the most sensitive recording telegraph known. Improved methods of working cables soon followed, among which, in the early days, probably the most notable is the introduction of condensers between the ends of the cable and the earth by Varley. The successful duplexing of cables by Muirhead has already been referred to, but it is somewhat curious to note that although the electricians interested in cable working were familiar, as early as 1856, and perhaps earlier, with the difficulty which had prevented success on land lines, no one seems to have thought of applying the remedy. As early as 1858, a patent was taken out by Thomson, in which he proposed to overcome the difficulity of duplexing a cable by a mechanical arrangement for varying the compensating currents at the same rate that the signaling current varies. He has since said that he did not propose the use of condensers, because a means of producing a sufficiently good model cable was not then known. Such a model cable was not available for nearly twenty years after the above date, and was finally produced by making practically a copy of the actual cable, using tinfoil strips for the conductor insulated from an earth plate by means of thin paraffined paper, so as to give electrostatic capacity.

The invention of the telephone constitutes one of the greatest advances that have been made in telegraphic communication. This is an acoustic telegraph, which has the very important merit that the audible signals are spoken words, and hence the instruments can be used by anyone who can hear and speak and who understands the language in which the message is transmitted.

It is well known that sound is transmitted through the air from the source to the hearer by waves of condensation and rarefaction, which affect the drum of the ear. Wheatstone, as early as 1831, showed that these waves could be transmitted from one place to another, at a moderate distance, through wooden rods and afterward conveyed to the ear by the vibrations given to the air by the end of the rod. Similarly, vibrations given to one diaphragm can be conveyed to another, at a considerable distance, by connecting the two diaphragms together by a stretched cord or wire. This appears to have been known for several centuries in the central districts of India, and a similar apparatus was described by Hook in 1667. A similar apparatus is now used and known as the mechanical telephone.

To cause the vibrations of one diaphragm to produce corresponding vibrations in another diaphragm at a distance, through the agency of an electric current, was the problem of the electric telephone. The first to propose this seems to have been Charles Bourseul, who, in 1854, suggested the use of two plates—one at the transmitting station, which, by the varying pressure of the air due to the sound waves, would open and close an electric circuit; while the other was to be acted on at the receiving station by an electro-magnet, through which the coils of the electric current passed. The varying strength of the electro-magnet, due to the rapid succession of currents, was thus to be taken advantage of to give the proper succession of impulses to the receiving diaphragm. In 1861 Philip Reis, of Friedrichsdorf, proposed, in a lecture delivered before the Physical Society of Frankfort, to use an instrument, which he called a telephone, for the reproduction at a distance of music and human speech. The apparatus consisted of a stretched membrane forming part of one side of a box, into which, by means of a mouthpiece, the sounds could be directed. This membrane was made to open and close an electric circuit at each vibration. At the receiving end an electro-magnet, consisting of a thin rod of iron surrounded by a coil, was placed. The successive interruptions and closings of this electric current was, in accordance with a discovery made by Dr. Page, of Salem, Mass., in 1837, to produce sounds of the same pitch as those of the sound directed into the box of the transmitter. This method failed for speech, for the simple reason that speech has more characteristics than pitch; and it was only partially successful for musical sounds, from its inability to produce, with any approach to accuracy, the necessary variations of loudness and quality.

To produce not only the frequency of vibration, but also the loudness and quality of the sounds evidently required a transmitter and a receiver which did not depend for its action on simple interruption of the current, but which varied it in an undulating manner, similar to the variations of pressure to which the diaphragm receiving the sound vibrations was subjected due to the sound waves. Such an apparatus of a very perfect type was produced by Graham Bell in 1876, who, in the descriptions of his apparatus given in his patent specifications and elsewhere, shows that he thoroughly understood what had to be done. We all know from actual experience that the instrument which he produced did it. Since the publication of Bell's invention a great many modifications have been produced. Most of them have, however, been held to embody the same essential principle as that of Bell, the variation being simply one of mechanical arrangement. One field of investigation has, however, been fruitful of improvement. In the original patent of Bell, and also in a caveat filed almost simultaneously by Elisha Gray, it is pointed out that the variations of the current may be produced by causing the vibrations of the diaphragm to vary the resistance of the circuit. This idea has proved of great value in increasing the loudness of the sounds given out by the Bell telephone when used as a receiver. A great many forms of these "microphone" transmitters have been invented. Among those who have made important contributions we may mention Berliner, Blake, Edison, Gower, Gray, Hughes and Hunnings.

Another form of telephone has been proposed by Prof. Dolbear. In this telephone system one diaphragm of the receiver is made to form one plate of an electric condenser, and the varying electric force on this plate, due to the fluctuations of the charge, causes it to vibrate in response to the varying electro-motive force produced by the transmitter. This condenser telephone can evidently be used either as a transmitter or as a receiver, and, as Dolbear has pointed out, may be rendered sensitive by keeping one plate of the condenser at a high potential.

Another interesting discovery in this subject should be mentioned, namely, the transmission of speech from one place to another by means of beams of light or radiant heat. This was based originally on the discovery by May and Smith of the variation of the electric resistance of selenium when exposed to light or radiant heat. Many other substances have since been found to have the same property in a greater or less degree. The experiments of Bell and Sumner Tainter have shown that if a beam of light be reflected from a thin mirror, and, by means of lenses or otherwise, made to pass as a parallel beam from the transmitter to the receiving station, and there received on a bar or series of bars, or a coil of a substance having the properties of selenium, the resistance of the selenium will be affected by vibration of the mirror. If, then, the mirror be used as a transmitting diaphragm, like that of a telephone transmitter, words spoken to the mirror will be repeated by a telephone in the circuit of which the selenium, is placed and through which an electric current is kept flowing.

In this address an attempt has been made to sketch very briefly the development of the application of electricity to the transmission of intelligence. Many important applications (as, for example, fire-alarms and railway signal systems, etc.) have not been referred to, and a host of important contributors have, as a matter of necessity, been entirely ignored. To go into detail and do justice to everyone who has contributed to the present state of the electric telegraph was an impossibility and has not been attempted.

EXPLORATIONS IN MONGOLIA AND TIBET.

By W. Woodville Rockhill.

On the 1st of December, 1891, I left Peking for a journey in Mongolia and Tibet, proposing, if possible, to traverse the latter country from north to south and reach British India—Sikkim or Nepal.

I was well provided with scientific apparatus, and very scantily with money, and so I started out with the anticipation of having to endure many discomforts, and possibly see my chance of ultimate success lost for want of a few hundred dollars and my collections poor for lack of funds and means of transportation. This is the one insurmountable difficulty a traveller can have to contend with; nearly every obstacle can be overcome or turned, but how to travel on an empty money bag (and an empty stomach, as it turned out in my case), in a strange land, is a more difficult problem for most men than the quadrature of the circle.

I will pass over the first few stages of my journey, which led me through Chang-chia k'ou to the great emporium of eastern Mongolia, Kuei-hua Ch'eng, where I arrived on the 18th of December.

This town was known in the T'ang period (A. D. 618–907), and how long before that I can not now say.

Col. Yule * thinks it was Tenduc, the capital of Prester John; but in this I can not quite agree, as I believe the latter town is to be identified with the present Tou Ch'eng (in Mongol Togto), at the mouth of the Hei-ho, which flows by Kuei-hua and empties into the Yellow River (Huang-ho) at the former place.

Father Gerbillon visited Kuei-hua Ch'eng in 1688, in the suite of the great Emperor K'ang-hsi. He describes the place as follows: "C'est une petite Ville qu'on dit avoir été autrefois fort marchande, et d'un grand abord, pendant que les Tartares d'Oüest étoient les maîtres de la Chine: à présent c'est fort peu de chose: les murailles bâties de briques sont assez entières par dehors; mais il n'y a plus de remparts au dedans: il n'y a même rien de remarquable dans la Ville, que les Pagodes et les *Lamas*." †

* See his Book of Ser Marco Polo, 2d edit., I, 277.

† Du Halde, "Description de l'Empire de la Chine," IV, 103. The Mongol name of this town is Koko hutun, or "Blue town." Chinese histories of the seventh century mention it under the name of Tung-shou Chiang.

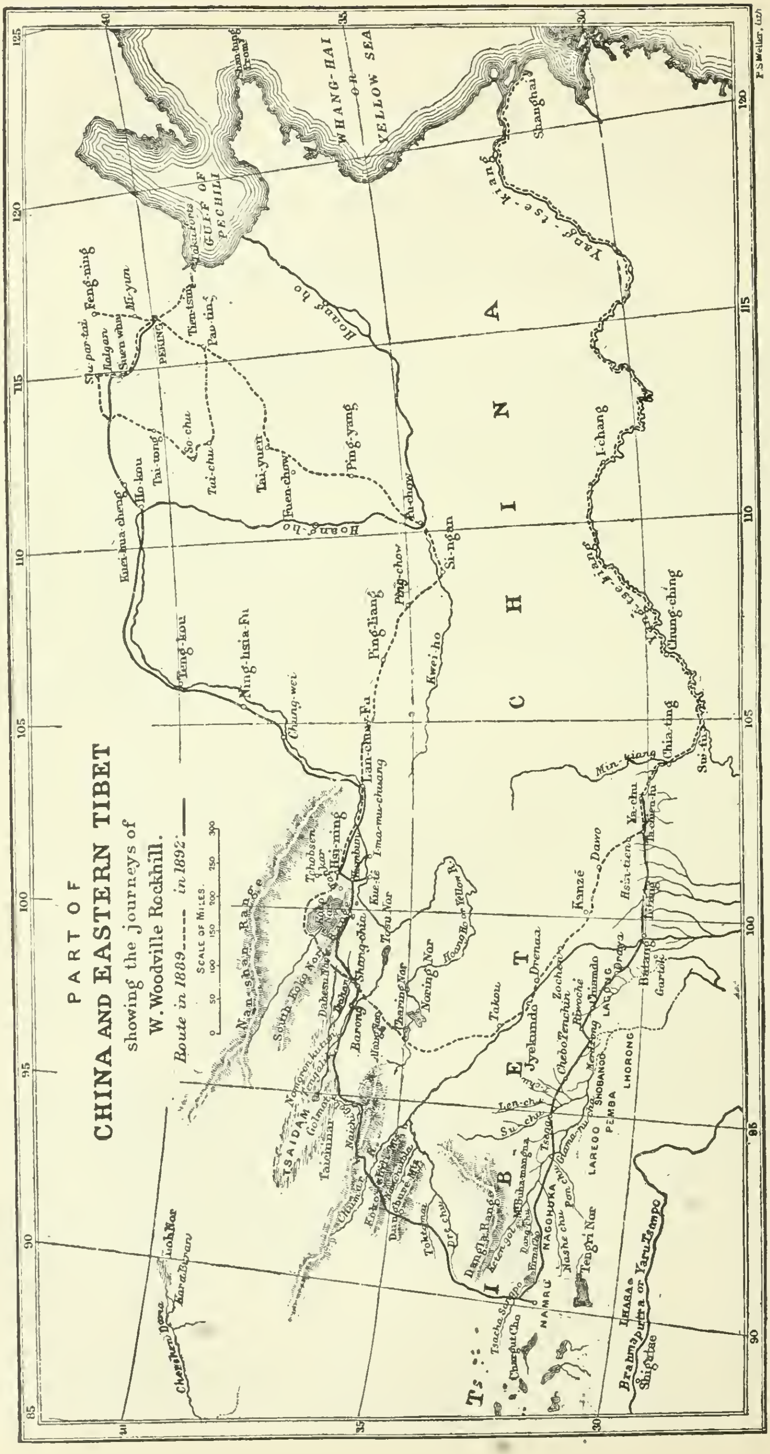
PART OF
CHINA AND EASTERN TIBET
showing the journeys of
W. Woodville Rockhill.
Route in 1889 ---- in 1892 ——
SCALE OF MILES
0 50 100 150 200 250 300
WHANG-HAI or YELLOW SEA
GULF OF PECHILI
CHINA
TIBET
PEKING
Shanghai
Yang-tse-kiang
Hoang-ho
Chung-ching
Lan-chow-fu
Hsi-ning
Nan-shan Range
TSAIDAM
LHASA
Tengri Nor
Brahmaputra or Yaru Tsampo
Shigatse
Batang
Ta-chien-lu
Kanzé
Jyekundo
Dang-la Range
Ch'eng-tu
F. S. Weller, lith.

FIG. 1.

In 1844, Father Huc, when on his way to Lh'asa, stopped for a while at Kuei-hua Ch'eng. He says of it: "With the exception of the lamaseries, which rise above the other buildings, one only sees an agglomeration of houses and shops huddled together without order, the one against the other. The ramparts of the old city still exist in their entirety, but the overflow of the population has been forced to cross them. Little by little numerous houses have been built outside the walls, vast quarters have been formed; and now the *extra muros* has acquired more importance than the city itself."*

Fifty years hardly count in the life of an inland city in Asia, and Kuei-hua to-day is what it was in the days of Huc—an irregular mass of tumble-down houses built around a small central walled town. Dirty, muddy, unpaved streets, innumerable small shops, crowded streets along which loaded camels and mules and clumsy carts are moving, and where an occasional Mongol, very often much the worse for liquor, is seen accompanied by his women folk in green satin dresses and much jewelry of silver and numerous strings of coral beads ornamenting their hair, neck, and ears.

The chief industry of the place is, and has been for at least a century, the preparation of sheep and goat skins. Tallow is also an important article of trade, and sheep and camels in vast numbers are annually sold here to supply the Peking market. The population, exculsively Chinese, of this place is probably between 75,000 and 100,000.

On the 25th of December, having completed arrangements for continuing my journey to Ning-hsia Fu in Kan-su in commodious carts like those which had brought me thus far on my way, I left Kuei-hua and in two days reached the Yellow River at Ho-k'ou,† where it makes a sharp bend southward.

Crossing the river—here about 400 yards wide—on the ice, we first travelled over a country with sand dunes intersecting it here and there, and finally entered the vast alluvial plains which stretch westward to Alashan and are bounded to the north—on the left bank of the river, by a range of mountains of an average altitude of some 1,800 feet. This chain is called on European maps the Inshan (a corruption, I believe, of Ch'ing shan, a name given to the eastern part of it) and is locally known by a variety of names—as are all ranges in eastern Asia—Ta ch'ing shan, Wula shan, Lang shan, etc.‡

For thirteen days we travelled through the sandy waste, now and then passing a small village of Chinese colonists settled in these Mongol lands, where they cultivate the soil after a great expenditure of labor on vast irrigation ditches, which are necessary to water the parched soil and which the sands, driven before the nearly incessant

* Huc, "Souvenirs d'un voyage damns la Tartarie et le Thibet," (12mo. edit.) I, 164.

† Huc's Tchagan Kouren, See *op. cit.*, I, 215.

‡ Timkowski, "Voy. a Peking," II, 265, 267, says this range is called Khadjar Khosho (Khajar hosho), or Onghin oola.

westerly winds, are continually filling up. We saw but few Mongols; they live remote from the route, or when they have remained in their former haunts, now settled by Chinese, have adopted Chinese modes of dress and of living, and too frequently their vices.

Some antelope, a few hares, and vast flocks of sand grouse (*Syrrhaptes Pallasii*) were occasionally seen; but what a sportsman's paradise these plains must have been in the days of K'ang-hsi, when Father Gerbillon came here with him to hawk and shoot, and the great Emperor never failed to return to camp with scores and scores of hares and other game killed by his arrows!

Father Huc has so fully and graphically described the Ordos country that I will not venture to try and improve on what he has said, especially as one forms a more agreeable opinion of the country from his narrative than one would from what I might say of it. It has, I fear, changed for the worse since his time.

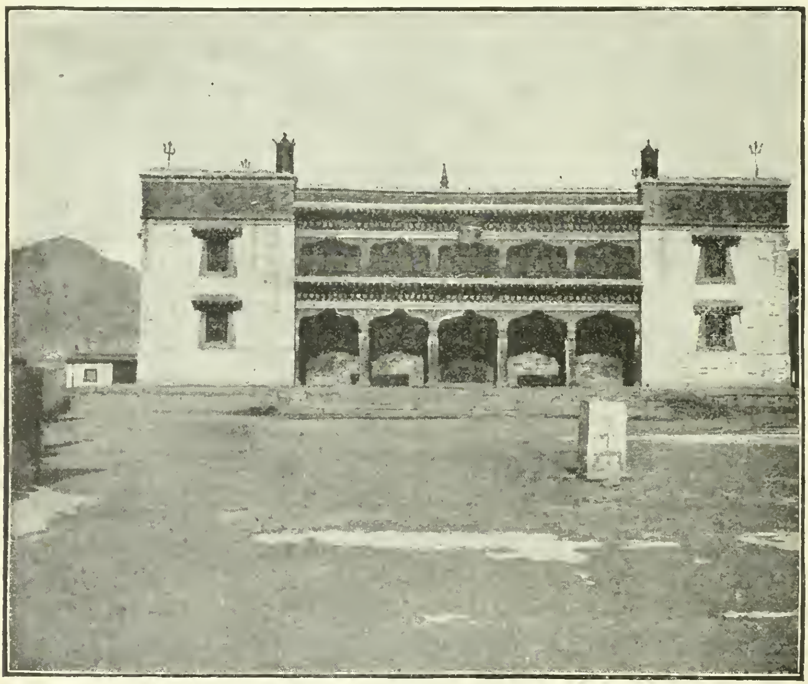

FIG. 2.—Baron gomba or Hsi Kung miao Lamaist Temple in the Ordos country.

The only place of any importance we saw was the palace of one of the Orat Mongol princes, the Hsi Kung or "Duke of the West," and near it a small but very handsomely built lamasery, the temple itself of pure Tibetan style. It is called by the Mongols, Baron gomba, and by the Chinese, Hsi Kung miao.

On the 9th of January, I reached the large Chinese Christian community (some three hundred families residing in four villages) of San-tao ho-tzŭ, created and managed by the Belgian Catholic foreign missions. Here I remained two days and was most hospitably entertained by the bishop and fathers of the mission. This locality is in the domains of the Mongol prince of Alashan, colloquially designated by the Chinese as Hsi Wang or Western Prince. His people, so Ts'aidam Mongols have told me, inhabited in old times the country west of Hsi-ning Fu in western Kan-su, and are of the same stock as the Ts'aidam Mongols. This agrees with what Timskowski tells us, who says this tribe of the Eleuts came to the country they now inhabit in 1686.*

Following the course of the Yellow River in a southerly direction, I passed successively through Shih-tsui (Hotun jeli in Mongol), the first town on our route in the Province ot Kan-su, Ning-hsia Fu, Chung-wei Hsien, and finally reached Lan-chou Fu, the capital of the Province of Kan-su, on the 31st of January, where I joined the route I had followed in 1888–'89 when on my way to Tibet for the first time.

Ning-hsia Fu was the most important town we traversed before reaching Lan-chou, but it has greatly fallen from its ancient importance, having suffered terribly during the late Mohammedan rebellion.†

Father Gerbillon, while journeying with the Emperor K'ang-hsi in 1697, visited this city. He says it was then one of the largest and most famous along the whole length of the Great Wall. It was densely populated, the houses built so closely together that there was no room even for court-yards. He also noted that "building timber is here very cheap, because they go to get it in that chain of mountains which is to the northwest, some 60 or 70 lys from the city,‡ where it is so abundant that from the neighboring localities, more than 400 or 500 lys away, they come to buy it at Ning-hia."§ At the present time not a forest tree is to be seen, only a few poplars recently planted along the irrigation ditches.

The father says further on (p. 372): "They presented also to his majesty several foot rugs, resembling enough our Turkey carpets, but coarser; they are made here, and the emperor had the curiosity to have the work done in his presence, as also paper which is made at Ning-hsia, with hemp beaten and mixed with lime water."

Now the town is, for half of its area, a desert of brick-bats, but rugs and paper making are still the chief—or rather the only—industries of the place.

I arrived at Lan-chou the day after Chinese New Year and on the fifth of the first moon. I witnessed the *ying-ch'un* festivities, in

Op. cit., II, 279. See also Du Halde, *op. cit.*, IV, 375, where we learn that the first Eleut prince of Alashan had only the rank of Beileh and was named Baturu Ts'onam. A Beileh is a prince of the third order, a Wang the second, and a ch'in Wang of the first.

†This city is called Irgé hotun by the Mongols, and is the Irghai of Mohammedan writers and the Egrigaia of Marco Polo.

‡This range is called Hsi shan by the Chinese, but on our maps it is usually designated by the name of Alashan Mountains.

§ Du Halde, *op. cit.*, IV, 370.

which the local magistrates go outside the east gate of the city to "welcome spring" (*ying-ch'un*). A huge cow made of wicker-work and coated over with mud was dragged along by scores of men, and following it was the image of the god T'ai-sui. A man disguised as a woman led the procession on foot and following him was another, in like disguise, riding a donkey. This one impersonated, I was told, the princess who introduced into China the practice of compressing women's feet. The cow was painted of a reddish brown color, a portent that con-

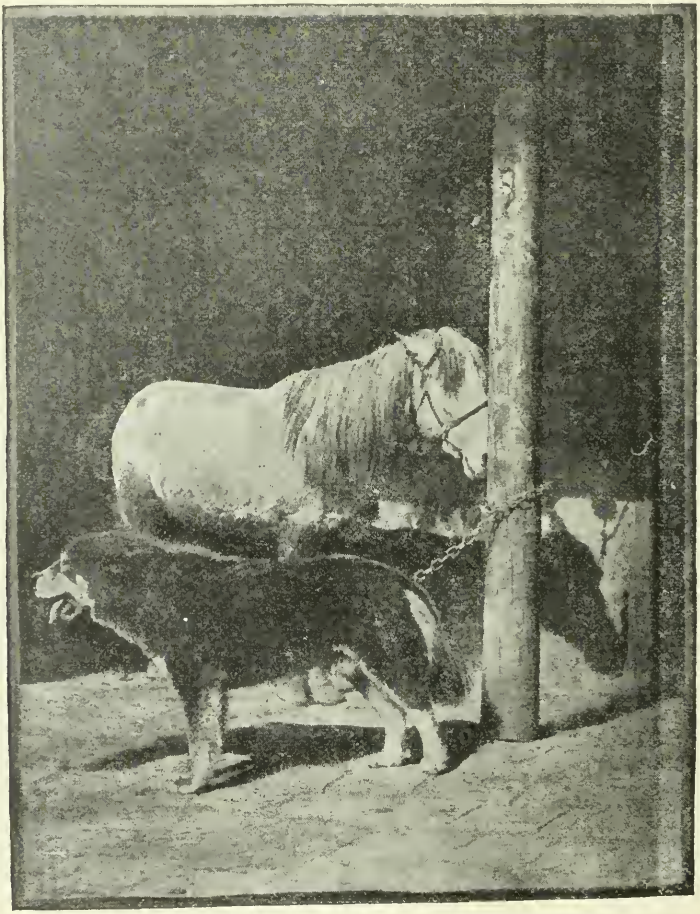

FIG. 3.—Kokonor Tibetan pony (Konsa stock). Tibetan mastiff (Panaka stock).

flagrations would take place in the year now beginning, for the colors used on this occasion are symbolical,—yellow means plentiful crops; white, floods; black, sickness; and blue, war. In like manner, if the image of T'ai-sui is bare-headed it is symbolical of heat; with his cap on, of cold; if he wears shoes it portends much rain and if he is barefooted, dry weather.*

* See G. Carter Stent, "Chinese and English Vocabulary," p. 714.

Theatricals, a banquet at the magistrate's office, and merry-making followed. On the morrow the cow was broken to pieces and farmers began to till their fields. This feast is observed over most of China.

Having engaged mules to carry me and my luggage to the lamasery of Kumbum, or rather the contiguous village of Lusar, some 20 miles south of Hsi-ning, I left Lan-chou on the 5th of February and following up the Yellow River and the Hsi-ho, a route I had taken previously in 1889,* I reached my destination on the 11th, and took up my quarters in an inn in the lower part of the village and at once began preparations for the journey into Tibet.

I secured the services of the men who had accompanied me on my first journey, bought six stout ponies and a supply of provisions—parched barley-meal (*tsamba*), rice, flour, vermicelli, tea, etc.—enough to last, if used with economy, for about five months. While my head man, Yeh Chi-ch'eng, was buying pack-mules, fitting the saddles to their backs, and purchasing all the thousand and one little things required on a long journey in a country devoid of every necessary of life save a few varieties of very coarse food, I went for a tour through the portion of country along the Yellow River due south of Lusar, a region of great ethnological interest, inhabited by tribes of Tibetan, Mongol, and Turkish descent; those of the latter called Salars or Salaris, being particularly interesting, as they have retained their original type and language though residing on Chinese soil for the last four hundred years and surrounded by Chinese and Tibetan peoples.† They number some 40,000 souls and are the most fanatical Mohammedans in western China. The Salar priests (*ahons*) began the late Mohammedan rebellion in or near the little town of Bayanrong. Fortunately for the Imperial Government, dissensions arose among the Mohammedans and they were soon fighting among themselves. It was this way: One said smoking was permissible (he was a Ho-chou teacher), another said it was forbidden, and so they came to blows. At the town of Tankar, 30 miles west of Hsi-ning, these two factions fought so savagely that the authorities made use of this quarrel to rid the place of them. All the male Mohammedans were invited to the mosque to talk over the matter in the presence of the colonel commanding the town. When all had assembled in the court-yard, there came men who called them out one by one, and as they issued out of the gate they were beheaded, and in this way 3,500 were made away with. Their wives and daughters

* See "The land of the lamas," p. 41–58.

† The principal branch of this people forms now one of the Turkoman tribes under Russian rule residing around Old Sarakhs. It numbers about 5,000 families. "The three nations of the Salars are named Yalawach, Githara, and Karawan. They have an evil reputation even among Turkomans, and are said to be generally hated." See Lieut. A. C. Yate, Travels with the Afghan boundary commission, p. 301–302. See also on the Chinese Salar. Rob. B. Shaw, *Journ. Roy. As. Soc.*, new ser. x, p. 305–316 and Deniker, *Bull. Soc. d'Anth. de Paris*, 3e Serie, x, 206–210.

were sold or otherwise disposed of when good-looking, and Tankar, with a remaining population of a few thousands or so, enjoyed quiet once more.

At Hsi-ning, for several years after the rebellion had been suppressed, no Mohammedan was allowed to enter the city (none of them could live in it) without having a stamp impressed on his cheek by the guard at the gate; and even now, after twenty years of peace, none of them may have a knife, even the usual small one which is carried by all travelling Chinese in a little case with their chop-sticks.*

On the 29th of February, I was back in Lusar, but though I used all diligence and expended a vast amount of energy, it was the 14th of March when we made our final start for the Kokonor country, the first stage of our journey to Tibet.

Fig. 4.—Chinese composing Mr. Rockhill's party.

My party, as finally organized, comprised four Chinese, three of them frontiersmen from near Lusar, and one, a cook, engaged at Kuei-hua Ch'eng, and a native of Tung-chou, near Peking. We had two small blue cotton tents, and our saddle blankets formed the bulk of our bedding, for the very heavy sheep-skin garments we wore were enough covering for the coldest weather.

* In the narrative of the journey of Benedict Goës (1603–1607) it is said that the Mohammedans at Su Chou (northwest Kan-su) were shut up every night within the walls of their own city, which was distinct from that inhabited by the Chinese. See H. Yule's Cathay and the Way Thither, p. 582.

In order to keep the pack-mules in good condition for as long a time as possible, I had the greater part of their loads carried by donkies from Lusar to the Muri-Wahon country, east of the Ts'aidam. Thence to Shang, yaks relieved them, and in the Ts'aidam, camels did their work to a great extent, so that when we started into the wilds north of Tibet my mules were still in fairly good condition—though very little fed—and stood well the terrible fatigues of the journey, but they finally gave out from foot-soreness and none reached the journey's end.

I began a survey of the road at Kalgan, north of Peking, and carried it on about 2,400 miles, to Bat'ang, in eastern Tibet, where my route joined that surveyed in 1877 by Capt. William Gill.* The method I followed in my work was to run the traverse by prismatic compass and aneroid, taking the distance between consecutive points by my watch and controlling frequently the distances thus obtained by pacing them off.

Every day the altitude of one point at least was determined by the temperature of boiling water, and all adjacent points, where aneroid readings were taken, were corrected by this and the one taken the day before. Sextant observations were made whenever possible for position, and thus the inevitable errors on my survey could not accumulate, but were divided over the whole length of the line.

Besides the work of surveying I had to take photographs, note the general characteristics of the country, keep an eye on the packs to see that they were not awry, and attend to innumerable details connected with the everyday life of the party. The animals gave me less trouble than the men (this is usually the case in this world, and how true is the saying, "Plus je vois les hommes, plus j'aime les bêtes")!

In 1889, I had, when going to the Ts'aidam, taken from Lusar the route leading along the north side of lake Kokonor. This time I decided to follow a new trail leading through an unexplored country (that of the Panaka living south of the Kokonor), and thence directly by the mountains to Shang, in the southeast corner of the Ts'aidam. I was most anxious to re-visit this place so as to be able to go once more to the Tosu nor (lake) and determine by actual observations its position and altitude.

The nature of the country to the south of the Kokonor lake is more mountainous than that to the north, but the climatic conditions are the same—violent westerly winds, great dryness; usually a clear sky, and though the nights are invariably cold, the temperature rises very high during the day. These peculiar conditions result from the high altitude of this region, which is over 11,000 feet above the sea level.

The route we took was as follows: Leaving the province of Kan-su at Sharakuto, on the southern main feeder of the headwaters of the Hsi-ho (which flows by Hsi-ning Fu), we traversed in a general west-

* See his River of Golden Sand: "The narrative of a journey through China and eastern Tibet to Burmah," 2 vols. 8vo., 1880.

southwest direction the country of the Panaka or Panakasum, as the Tibetan tribes inhabiting these regions are called. These tribes, which were in past centuries located principally south of the Yellow River all the way from the Chinese frontier to its sources at Karmat'ang, have within the last hundred years pushed northward and dispossessed the Mongol owners of these rich pasture lands, driving them either into the foothills around the swampy Ts'aidam or nearer to the Chinese borders. The Tibetan tribes which first came to the Kokonor were eight in number and all bore the word *Na* in their names, hence the

FIG. 5.—Panaka Tibetan camp in mountains near Shang.

mixed Chinese-Tibetan name of Panaka by which they are now known and which they use in speaking of themselves.*

The Panaka may number in all a hundred to a hundred and twenty-five thousand souls. I have described elsewhere the dress and mode of living of these tribes,† so will not dwell on these questions here, and

* Panaka (*i. e., Pa*, Chinese "eight," *Na*, patronymic, and *k'a* or *chia* (Chinese) "family" or "clan"). They also call themselves Panakasum; the last word, meaning in Thibetan "three," is added on account of three great divisions of these clans at the present time. The Arik (about 10,000 families), the Konsa (2,000 families), the Bumtok (2,000 families), are the largest of these tribes living north and west of the lake; the principal tribes of the Panaka south of the lake are the Chamri, the Tubchia, and the Wutushiu.

† See "Land of the Lamas," p. 73, *et passim*.

the illustrations will enable the reader to form a better idea of their camps and general appearance than could a long description.

Crossing a high and very difficult pass in the southwest corner of the Panakasum's country, we entered the basin of the Tsahan ossu, an important river of the Ts'aidam, the existence of which was not heretofore suspected; and on the 4th of April I reached the Mongol village of Shang (or Shang-chia), on the upper Bayan gol (or Yogoré gol), the main river of the Ts'aidam, which has its source in two lakes called Tosu-nor and Alang nor.

FIG. 6.—Foot of Wahon Jamkar Pass leading into the basin of the Tsahan ossu.

Sending the bulk of my baggage to the camp of a former acquaintance, the chief or Dzassak of Baron Ts'aidam, I went with two men and a Mongol guide to explore the Tosu-nor, reaching that large sheet of water (about 13,500 feet above sea level) on the 12th of April.

Dowé, the Mongol guide, the same who had led me in 1889, by the sources of the Yellow River to Jyäkundo, told me one evening while we gossiping over the camp fire, that he had heard at Sa-chou of wild men (*gérésun kun*). Two had been captured by some Mohammedan Sifan (or Huang fan), but one soon died and the other made his escape. These savages live between Sa-chou and the

Lob nor,* make their dwellings of reeds and feed on wild grapes, which they dry. From this description I have no doubt these people are the half-wild inhabitants of Turki origin seen by Prjevalsky and other travellers in the marshes and canebrakes of Lob-nor.

On the 14th of April, I started back for Shang. Crossing the Yogoré my pony broke through the ice and was drowned, I nearly sharing the same fate. The next day my saddle was recovered, also my notes and papers in my saddle-bags. On the 18th I joined my other men with the pack animals in the valley of Oim, where the Dzassak of Baron was

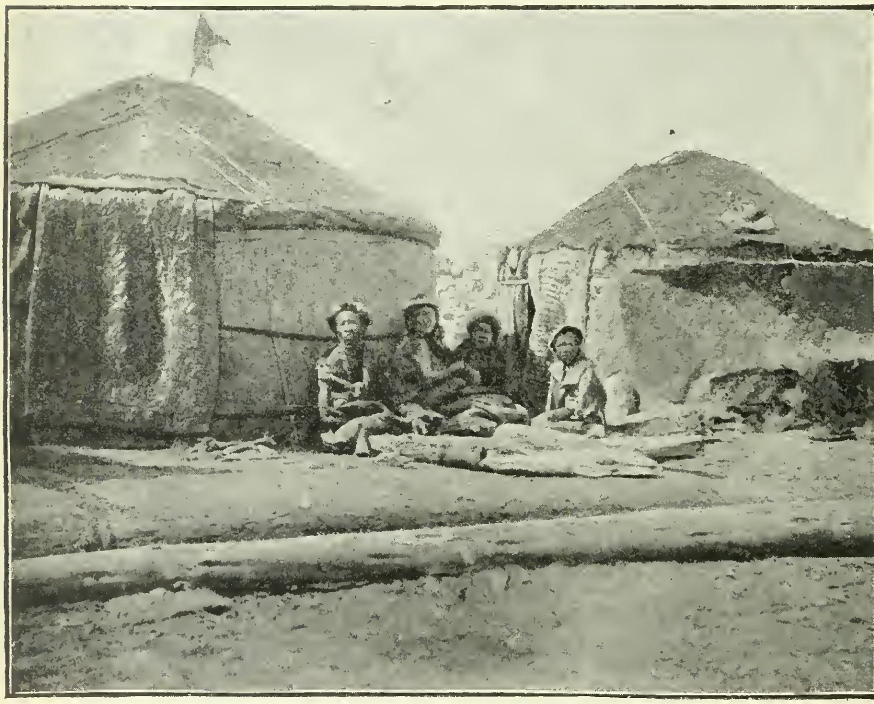

FIG. 7.—Scene in Mongol village of Shang (S. E. Ts'aidam).

*See "Land of the Lamas," p. 159. Douglas Forsyth, *Journal Roy. Geo. Soc.*, XLVII, p. 6, says: "There are numbers of encampments and settlements on the banks of the marshy lakes and their connecting channels; perhaps there are as many as a thousand houses or camps. These are inhabited by families who emigrated there about one hundred and sixty years ago. They are looked upon with contempt by true believers as only half Musselmans. The aborigines are described as very wild people—black men with long, matted hair, who shun the society of mankind and wear clothes made of the bark of a tree. The stuff is called "luff," and is the fiber of a plant called "toka chigha," which grows plentifully all over the sandy wastes bordering on the marshes of Lop." Wild men are said to live on the lower Tsangpo, in Tibet. The Mongol Lama Sherab jyats'o says that in Pemakoichhen (north of Mira Pedam) the Lh'opa "kill the mother of the bride in performing their marriage ceremony when they do not find any wild men, and eat her flesh." See Report on the Explorations in Sikkim, Bhutan, and Tibet, from 1856 to 1886, p. 7; also pp. 50 and 52.

camped. Here I was detained for eleven days trying to make arrangements with the chief to supply me with pack animals and a guide to go to Shigatsé, in Ulterior Tibet. After a vast and reckless expenditure of my limited store of patience, I failed to get more than four camels and a guide as far as Tengélik, a Mongol encampment in the marshes of the Ts'aidam, not a hundred miles away.

On the second day out from Oim we left the village of Baron (or Baron kuré) and travelling through sand and mud and brush for four days came to the pools of Tengélik. Life in camp in this horrible Ts'aidam is miserable indeed, and though I was used to the dirt and misery of such an existence, I had daily to use all my persuasive powers to keep myself in the belief that I would be able to stand it for six months more. The Mongols of the Ts'aidam have a saying that a Mongol eats 3 pounds of wool with his food yearly, a Tibetan 3 pounds of gravel, and a Chinese 3 quarts of dirt. Living in a Sinico-Mongolo-Tibetan style, I swallowed with my miserable food the dirt, the wool, and the grit, portioned by a harsh destiny to these peoples, and I verily believe that I found enough wool in my tea, my tsamba, my meat, and my bread while in Mongolia and Tibet to stuff a pillow. The dirt and the sand could be easily swallowed, but the wool—nothing could be done with it, no amount of mastication could dispose of it.

FIG. 8.—Prayer-wheel turned by wind. Erected over Mongol and Tibetan dwellings.

Leaving Tengélik on the 7th of May with four pack ponies, three oxen and a camel, the latter loaded with leather jars filled with water, we reached the Naichi gol in five days, travelling all the time through sand or swamp.

On the Naichi gol I stopped for a few days to engage a famous guide of whom I had heard tell in Shang, and also to replenish my store of provisions as far as possible in this poverty stricken country. We got a supply of fairly good tsamba, but the butter we here bought, made of sheep's milk, was the strongest smelling and the vilest I ever tasted in my life, but such as it was I had to eat it and be thankful till I reached the inhabited parts of Tibet in July.

Leaving this place we turned south and following up the Naichi River, entered the mountains which all along the south side of the Ts'aidam mark the northern edge of the great tableland dividing this country from Tibet, and is some 200 to 400 miles wide. Usually this region is called Northern Tibet, and though physically it belongs to that country, from a political point it is a no-man's land, a desert waste over which at rare intervals wander some robber bands that prey on passing caravans.

It would take me too long to describe this part of my journey, in which we crossed four chains of mountains of an average altitude of about 16,000 feet. Between each of these, in broad valleys running from west to east, flow shallow rivers over beds of soft sand or gravel in which we were forever getting bogged, we, our horses, and mules.

Though we were in May and lovely June we had snow-storms and hailstorms daily, the nights were bitterly cold, and in the middle of the day the thermometer rose to the nineties.

With no fuel but the droppings of wild yaks, with hardly any grass for our animals, to which we had daily to feed balls of our parched barley meal, it was no wonder we made slow progress, or that before we had neared the inhabited regions of Tibet our supplies gave out and we had to subsist for five days on tea alone. On the 7th of July we saw for the first time black tents and I learned, on sending two of my men to one of them, that we were among the Namru in Namru dé, a dependency of Lh'asa at the northwest corner of the great Tengri nor (or, as the natives call it, Dolma Nam-ts'o). My plan had been to go around this lake to the west, and had our provisions held out a fortnight longer I have no doubt we would have succeeded, so sparce is the population of this region, and reached our goal, Shigatsé, the capital of Ulterior Tibet. To accomplish my plan it was necessary to make detours around every camp we sighted, for I knew of the stringent orders issued by the Lh'asa government against admitting foreigners onto their soil, and I entertained no hopes of seeing them modified in my favor. Unfortunately our supplies did not hold out and so, when we made these first Namru tents and asked for food we got only a few handfuls of tsamba and a little cheese. The news rapidly spread that a small, but very suspicious looking party, had arrived from the northern desert. The next day, after making some 12 miles more in a southerly direction and reaching a broad valley dotted all over with tents, we were stopped by the local headman and ordered to remain camped where we were until the officers of the Lh'asa government, who resided about a day's ride away, could come and cross-question us.

This was on the 8th of July. By the 13th it had been decided that I was to go under escort of a detachment of soldiers, not the way I had planned, but by a circuitous route (of considerable geographical interest however), to the high-road leading to Lh'asa from Hsi-ning, joining it a little to the north of the first Tibetan station, Nagch'u or Nagch'u-k'a, where there was a high official, a warden of the borders, who would settle about my further movements.

For ten days my escort took me in a general easterly direction over the foothills of the great Dang la chain, which we frequently saw to the north, its peaks covered with eternal snows reaching far down their flanks (the snow line in this country being at about 17,500 feet above sea level). We crossed a number of streams, all flowing in a southeasterly direction and probably forming the head waters of the Jyama-nu ch'u, the upper Salween, it is believed. The rain fell daily in torrents, the spongy, tussocky ground was soaked, and dry fuel nowhere to be found, so that finally we had to burn our pack saddles to

FIG. 9.—Tibetan boys from Jyadé.

boil our kettle. In an utterly exhausted condition, we reached, on the 22d of July, the highroad to Lh'asa in the Dang ch'u valley, a day and a half's ride north of Nagch'uk'a.

Here the Namru men left me, but I was soon espied by some of the guards stationed along this road for the very purpose of arresting foreigners, and requested to remain where I was till the officer in command at Nag ch'u could come and see me.

Before this I had been obliged to give up all idea of carrying out my original plan of getting to India, and I had now solely in view reaching China by some heretofore unexplored route which would keep me in the inhabited parts of Thibet, so that my ethnological researches could be successfully carried on.

While waiting here on the Dang ch'u for the arrival of the Nag ch'u officials, I was visited by some natives from the left bank of the river, and I learned from them that they and the tribes to the east of them were not subject to Lh'asa, and that by traversing their country (called Jyadé or "Chinese Province") I could reach the important town of Ch'amdo, on the highroad to China, whence I would be able to continue my journey commodiously to Tachien-lu in Ssŭ-ch'uan.

I at once made up my mind to follow this route, only waiting to see the Nag ch'u officials to satisfy my curiosity, and possibly pick up some interesting details concerning them, their country, and its customs.

On the 27th of July, I crossed the Dang ch'u and was kindly received by the chief of the Péré band, who, on the following day, introduced me to one of the big chiefs or Débas of the country, Nor jyal-tsan by name, who was about to start for his home, a fortnight's ride to the east and on the road to Ch'amdo.

It was arranged, after a short consultation and the presentation to him of some presents (50 ounces of silver, some knives, red lacquer rice bowls, etc.), that he would take me with him, and see to all my wants on the way. On reaching his home he would further supply me with a guide as far as Mér djong, the first locality on Ch'amdo territory, beyond which neither he nor his people ever went; and he gave, among other reasons, for this that, while the Ch'amdo people professed lamaism, he and the people of Jyadé followed the Bönbo religion, the modern and corrupt form of the old pre-Buddhistic shamanism, which has, at one time or the other, prevailed over all Asia.

Since leaving the Ts'aidam in May, I had continually travelled over country with an average altitude of about 15,800 feet above sea level, frequently crossing ridges and plains considerably higher. On leaving the Dang ch'u we very gradually descended till we reached near the Rama-ch'u, the timber line on the 12th of August, something over 13,000 feet above sea level. At this altitude cultivation also began, barley and turnips being the only crops. These are eked out by the use of seeds of several kinds of plants found growing in profusion on the hillsides. Above this altitude the people subsist entirely on what their flocks and herds of yaks can supply them, the necessary tsamba and tea, being procured by them at Lh'asa or from traders, who annually visit these regions. The principal article of trade of the Namru and other adjacent tribes is salt, procured by evaporation from some of the large lakes to the west of the Dang la and brought thence on the backs of sheep, each one carrying about 25 pounds. All the salt I have seen in these parts is of a brick-red color and very impure.

On the 20th of August, we reached Mer djong gomba on Ch'amdo territory, having traversed the whole of Jyadé without any mishaps, and having met everywhere with the greatest courtesy and kindness from the chiefs and people. The country round Mer djong is, where-ever possible, well cultivated, barley and wheat are the principal crops, and near each of the houses is a little garden-patch, where we saw with delight cabbages, onions, peas and turnips, but we noticed no domestic fowls; these are found only in the Chinesified portions of the country.

From Mer djong, we went to Riwoché (a dependency of Lh'asa) on the Tsé ch'u, passing through some beautiful alpine country (along the Ké ch'u), the mountain sides covered with fine forest growth and the valley bottom a mass of flowers of every hue. Frequently we saw large bunches of silver pheasants (*Crassoptilon tibetanum*, in Tibetan *Saga*), moving rapidly about in the thickets of rhododendrons and laurel-like plants, calling their young with a cry peculiarly like that of the guinea fowl. Very few varieties of birds were noticed however here, or, in fact, anywhere along the route, singing birds being especially rare.

FIG. 10.—Half-breed yaks with loads.

Riwoché is a place of some importance commercially, but from a picturesque point of view it is especially noteworthy for its peculiar temple, with walls of white and red, and gold spires rising from its green-tiled roofs. Around the temple are the dwellings of some three hundred lamas, near which are the houses of perhaps a hundred families of laymen. The village is at the base of steep, forest-clad mountains, and before it flows the swift river. This place is one of the few in Tibet which can boast of a wall around it; it was built by the Chinese, in all probability, about 1717.

Two stages down the Zé ch'u valley brought us to Nyulda, a Chinese post station on the highroad to Lh'asa, where the soldiers supplied us with the first eggs and vegetables we had had for many a long month.

We were now about two and a half days journey from the town of Ch'amdo, which I was not however destined to see, for when I had advanced towards it another day's ride, I was stopped by the lamas of

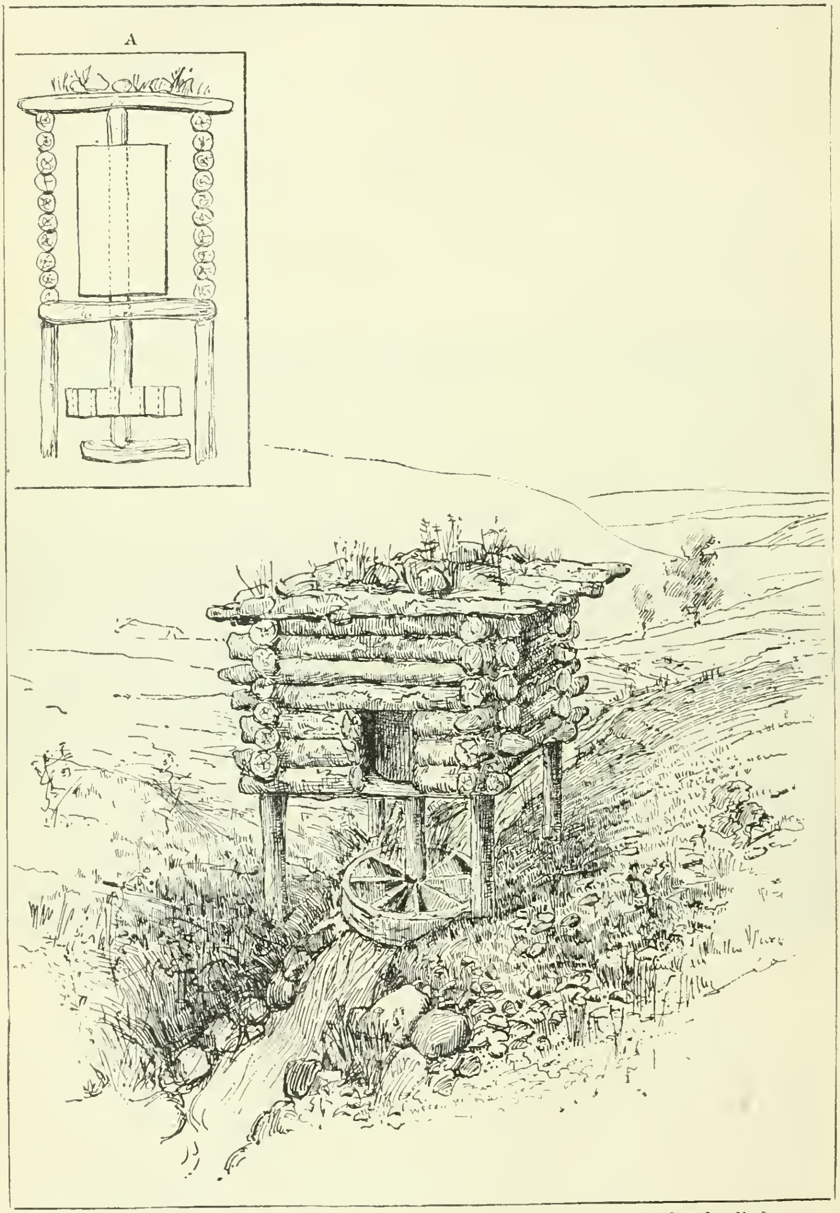

FIG. 11.—Tibetan prayer mill, turned by water. A, Section of water-wheel and cylinder.

that place, and requested to take a cross road leading around the town at some distance and joining again the highway to China near a place called Pung-dé.

I refused to follow this road and finally obtained permission to take another trail over the mountains to the south, which brought us out, after four days of travel through the most beautiful scenery I know of anywhere in Tibet, at the post station of Pung-dé, the Pao-tun of the Chinese.

The worst part of my long journey was now over, for from this point I travelled in comparative comfort, with an escort of Chinese soldiers, relays of pack and saddle horses, and houses every night to put up in; though I still frequently preferred my tent, where I could enjoy some privacy and escape the attack of the fleas which swarm in all Tibetan dwellings, to say nothing of rats and other vermin.

The first town of any importance we came to after leaving Riwoché was Draya, or Chamdun Draya as it is also called, the capital of an ecclesiastical, semi-independent state, on an affluent of the Om ch'u, which flows by Ch'amdo.

The town is prettily situated on a gentle slope, the lamasery, as usual, occupying the higher part of it, with a little plain in front, beyond which flows the Ombo ch'u, here met by two other streams of considerable size. The crops were ripening and fields of barley and wheat covered every little patch of ground susceptible of cultivation. On high frames, with which every country house is provided, grass twisted in cables was drying for the winter's forage, and in some places, where the high precipitous mountains did not overshadow the fields too much and the crops were early, barley, wheat, and turnips, were already hanging on these frames, which are used everywhere in Tibet for this purpose.

Though I was very roughly received at Draya—in fact, in lieu of fire-crackers I had a volley of stones let off at me as I entered the town—I remained here for two days and gathered a good deal of interesting information bearing on both the country and the people, which it is not possible to convey here, and for which I must refer the reader to my complete report now in preparation.

On the 6th of September, I left Draya, and after an interesting journey of five days, up hill and down dale, reached the important town of Gartok, or Chiangka as it is called by the Chinese, the chief town of the province of Merkam belonging to Lh'asa. It is curious in this connection to note that vassal states, governed by officials sent by Lh'asa, are found scattered all over Tibet; the Nyarong or "arable lowlands of the Nya River," the Tsarong, Riwoché, and innumerable localities in southern and southeastern Tibet belong to this class.

These districts have frequently given in their allegiance to Lh'asa (or "tied their head," *go-ta-wa*, as they say) on account of similarity of religious beliefs. Sometimes, however, Lh'asa has got possession of them through intrigues or open aggression.

Gartok is an important center for the musk trade, which of late years has taken considerable extension. It has a native population of about seven hundred, besides some two hundred or three hundred lamas.

From a hundred to a hundred and thirty Chinese also reside here, all, or nearly all, of them having native wives.

Wheat, oats (wild?), and barley are grown here extensively, and the gardens supplied us with cabbages, turnips, and several other kinds of vegetables, one, called in Chinese *o-sung*, I found especially palatable. Cats, pigs, and fowls were seen in every house, and I was presented by the Chinese officer in command of the little garrison here with grapes, peaches, and apricots (wild varieties, I believe), brought here from the Rongmi, or "*terres chaudes*," as the French missionaries call them, some two days' distance down the River of Golden Sands (Chin-sha ho or Chin chiang ho).

For the first time in Tibet I saw house sparrows (*cheuba*, in Tibetan) at Gartok.

Leaving Gartok on the 12th of September, we reached Bat'ang on the 15th, and here the geographical portion of my work was at an end. The people between Gartok and Bat'ang are Chinesified to a considerable extent, and have also a few customs introduced among them from intercourse with the tribes living south of them, Lissus, Mosso, and others. Among other things borrowed from these tribes is a peculiar jew's-harp, carried by every woman of this region, and consisting of three different toned harps of bamboo; two or three women often play together, and to this accompaniment they dance a slow, shuffling step in which grace and beauty are conspicuously absent.

I remained at Bat'ang four days, and then proceeded to Lit'ang, which I reached on the 24th, and finally arrived at Ta-chien-lu, on the Chinese frontier, on the 2d of October. From this locality to Shanghai, where I arrived on the 1st of November, I followed the route taken by me in 1889, and for a description of which I must again refer the reader to the published account of my first journey.

Before closing this brief account of my journey I must mention that in July, when on the Dang ch'u (and even earlier, when in Namru), I heard that some foreigners had passed through the country some six months previous, coming, it was supposed, from the west. In August I again heard vaguely of these travellers, and on the 18th of that month, while camped near the Zé ch'u at Lah'a in Nar peihu, I was shown by a native a note he had received from a foreigner commanding an expedition which had passed through here several months before. It was signed Capt. Henry Bower, of the Seventeenth Bengal Cavalry, and he had come, I learned later, from Ladak by way of the deserts to the northwest of Tibet

Since then I have had the pleasure of meeting Capt. Bower in London, and we have been able to compare notes. From this comparison it results that after the 10th of August (I had then reached the I ch'u Valley), our routes were very nearly parallel till we arrived near Ch'amdo, after which point they were identical.

Finally, I would like to call attention to the rich fields of research China and its dependencies afford the explorer, be he geographer, botanist, geologist, or ethnologist. Though volumes enough to fill a goodly library have been written about the Chinese Empire, a great deal remains to be done. Our geographical knowledge of China is still based on the surveys of the Jesuits, executed in the seventeenth century, to which a few itineraries have since been added. Pumpelly, Richthofen and a few others have only studied the geology of a part of this vast region; its botany is less well known perhaps than that of any other part of

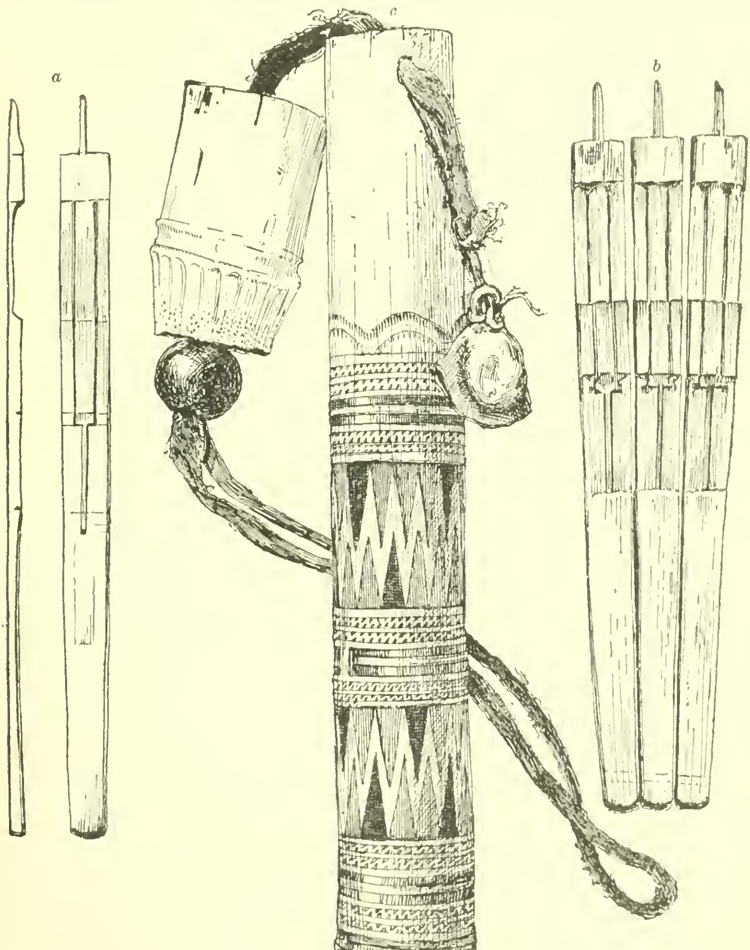

FIG. 12.—*a*, *b*. Tibetan jews's-harp (K'a pi); Bamboo. *c*, Bamboo case of same.

the globe. Its ethnology, though it has been more or less studied by hundreds of writers, has never, as far as I know, been systematically treated, and the scientific study of the languages of China is only just begun.

Of the scientific results of my journey I will here say nothing; they will be submitted in the report which I am at present preparing, together with a route map on a scale of 16 miles to an inch, reduced from my original survey. The illustrations accompanying this paper are from photographs taken by me on the journey, and of which I secured some two hundred fairly good ones.

PROGRESS OF ASTRONOMY FOR 1891 AND 1892.

By William C. Winlock.

A review of the progress of astronomy for the years 1879 and 1880 was contributed by Prof. E. S. Holden to the Smithsonian Report for 1880, and reviews for each succeeding year were continued by him in the annual reports of the Institution up to 1884; the reviews for 1885 and 1886, and for 1887-'88 and 1889-'90 were prepared by the present writer, the publication since 1886 being biennial instead of annual. The arrangement of the review for 1891-'92 is essentially the same as in previous years and, in its compilation as hitherto, notes in recent journals have been freely drawn upon without specific citation.

It should be borne in mind that the review is intended for those having a general interest in astronomy rather than for the professional astronomer who has access to a large working library. To the latter the bibliography appended may be found convenient as a reference, and will supplement the text in giving a general idea of recent publications on any special subject. Many very important papers are of such a nature that they do not lend themselves readily to condensation for the purposes of such a summary as the present.

Within the last few years many new aids have been provided to facilitate reference to the constantly-increasing volume of the literature of the subject. The most comprehensive of these is to be found in the *Bulletin astronomique*, published under the auspices of the Paris Observatory and the able editorship of M. Tisserand. In addition to extensive critical reviews of important memoirs, there is a brief summary of the contributions to other astronomical periodicals, and the whole is made easy of reference by an admirable index (wherein most journals are defective) at the close of the year, which, in fact, to a large extent, supplies a bibliography of astronomy for the year. The *Journal of the British Astronomical Association* contains a summary of current periodical literature, the value of which to the members is abundantly vouched for. The *Publications of the Astronomical Society of the Pacific* contains a great number of admirable reviews or notes, and this department is receiving increased attention in *Astronomy and Astrophysics*. *The Observatory* has perhaps the most complete notes, without an attempt at a systematic summary of current literature, to be found in English, while the excellent reviews in *Nature* and the more popular notes of the *Athenæum* need no special comment here. The *Astronomische Nachrichten* and the *Astronomical Journal* contain occasional notices of important works.

The "Notes on some points connected with the progress of astronomy during the past year" in the *Monthly Notices* of the Royal Astronomical Society have been increased in scope and fullness, and as the reviews in different branches of astronomy are furnished by specialists, these notes form a most valuable commentary on the year's work. The *Vierteljahrsschrift der astronomischen Gesellschaft* is, of course, the critical astronomical review, and is the recognized authority for summaries of cometary and planetary discoveries.

STELLAR SYSTEMS.

The Milky Way.—The independent researches of Prof. Pickering at the Harvard observatory and of Dr. Gill at the Cape of Good Hope have led to the conclusion that the stars of the Milky Way form a veritable sidereal system, separate and individual. This conclusion is entirely opposed to the views Sir William Herschel reached from his earliest observations (1785) which are still generally received by those who have not given much attention to this special question. Miss Clerke points out in the *Observatory* for September, 1891 (p. 302), that "the study of nebular distribution might alone, and long ago, have driven out of the field every form of 'projection theory' of the Milky Way. For it showed the great majority of gaseous nebulæ to be embraced within its circuit, and this alone amounted to a demonstration that a physical reality, and not simply a geometrical appearance, was in question."

A brief statement of the arguments of Prof. Pickering and of Dr. Gill is contained in a lecture by the latter delivered at the Royal Institution of Great Britain, May 29, 1891. Dr. Gill said:

I pass now to another recent result that is of great cosmical interest. The Cape photographic star-charting of the Southern Hemisphere has been already referred to. In comparing the existing eye estimates of magnitude by Dr. Gould with the photographic determinations of these magnitudes, both Prof. Kapteyn and myself have been greatly struck with a very considerable systematic discordance between the two. In the rich parts of the sky, that is, in the Milky Way, the stars are systematically photographically brighter by comparison with the eye observations than they are in the poorer part of the sky, and that not by any doubtful amount, but by half or three-fourths of a magnitude. One of two things was certain, either that the eye observations were wrong, or that the stars of the Milky Way are bluer or whiter than other stars. But Prof. Pickering, of Cambridge, America, has lately made a complete photographic review of the heavens and by placing a prism in front of the telescope he has made pictures of the whole sky. . . . He has discussed the various types of the spectra of the brighter stars, as thus revealed, according to their distribution in the sky. He finds thus that the stars of the *Sirius* type occur chiefly in the Milky Way, whilst stars of other types are fairly divided over the sky.

Now stars of the *Sirius* type are very white stars, very rich relative to other stars in the rays which act most strongly on a photographic plate. Here then is the explanation of the results of our photographic star-charting, and of the discordance between the photographic and visual magnitudes in the Milky Way.

The results of the Cape charting further show that it is not alone to the brighter stars that this discordance extends, but it extends also, though in a rather less degree, to the fainter stars of the Milky Way. Therefore we may come to the very remarkable conclusion that the Milky Way is a thing apart; and that it has been developed perhaps in a different manner, or more probably at a different and probably later epoch from the rest of the sidereal universe.*

NEBULÆ.

In a paper by Prof. Keeler, communicated to the Royal Society by Dr. Huggins on March 19, 1891, the question of the position of the chief nebular line seems to be definitely settled. Prof. Keeler has not only made a series of sixteen complete measures, obtained on eleven nights, of the chief line in the spectrum of the Orion nebula, thus defining its apparent position when corrected for the earth's motion, as λ 5006.22 ± 0.014, but has supplemented these by ten measures of the green hydrogen line on seven nights. The latter show the nebula to be moving relatively to the solar system with a motion of + 10.7 ± 1.0 miles per second, and oblige us to fix the true position of the chief line at λ 5005.93. The chief line is therefore 0.43 tenth meter more refrangible than the lower edge of the magnesium fluting, and as it has no resemblance to a fluting in appearance, and as flutings and lines of magnesium, which could not fail to appear at the same time with the fluting at λ 5006.36 are entirely absent from nebular spectra, the incorrectness of the view that the nebular line is the remnant of the magnesium fluting appears to be demonstrated.

Mr. Burnham has made a set of measures of the nebula in the Pleiades close to the star Merope. He remarks that it is one of the most singular and interesting objects in the heavens. With respect to its nearness to a bright naked-eye star (the distance between the centers is less than 40″) it is unique. There may be other examples, but certainly no other has ever been discovered, and this close association of a faint nebula and one of the prominent stars of the Pleiades is an interesting fact, whether such association is accidental or otherwise. The accurate measures made by Mr. Burnham and Mr. Barnard will enable this point to be ascertained when others shall have been made sometime hence, and it will be possible to determine by comparison whether the new nebula is drifting in space with Merope and the other stars of this famous group. We have, of course, many examples of large stars involved in widely diffused and extended nebulous masses,

*Publications of the Astronomical Society of the Pacific, 19.

but no instance has hitherto been known of a star bright enough to be visible to the naked eye having a small definite nebula within even several times the distance of this from Merope.

ASTRONOMICAL CONSTANTS.

The Constant of Aberration.—Prof. Comstock, of the Washburn Observatory, has been making careful trial of a modification of the method of determining the constant of aberration first suggested by M. Loewy. The essential feature of M. Loewy's method is the introduction of reflecting surfaces in front of the objective of a telescope, by means of which images of different portions of the heavens are simultaneously produced in the focal plane of the objective. By means of the micrometer the apparent distance between the images of two stars thus produced may be measured, and the angular distance between the stars determined from a simple relation involving the measured quantity and the angle included between the reflecting surfaces. It is obvious that great difficulties would attend the determination of this angle, and M. Loewy avoids these difficulties by measuring the distances of two pairs of stars and taking the difference of these distances, thus eliminating the angle between the mirrors. Prof. Comstock has found it advantageous to place before the objective three reflecting surfaces instead of two, making approximately equal angles among themselves, and to employ successively each pair of surfaces in measuring the distance between two given stars. If the normals to these surfaces all lie in the plane passing through the two stars and the earth, the mean of the three dihedral angles formed by the surfaces will be exactly 120°; and by taking the mean of the results furnished by the three pairs of surfaces the distance between a pair of stars may be determined independently of the angles between the mirrors. Prof. Comstock's provisional result for the constant of aberration is $20''.494 \pm 0''.017$.

MM. Loewy and Puiseux's work on the Constant of Aberration is summarized as follows in a communication to the *Comptes Rendus* for March 16, 1891.

1. Struve's value $20''.445$ is very near the truth. It would, in our opinion, be premature to alter it.

2. M. Fizeau's result, that reflection does not affect the behavior of rays with regard to aberration, is confirmed.

3. The new method for determining aberration can be regarded as satisfactory and definitive.

STAR CATALOGUES AND CHARTS.

The Star Catalogue of the Astronomische Gesellschaft.—The zone undertaken by the Harvard College Observatory +50° to +55° declination has been published as the fifth part of the great catalogue. The observations were made with the new meridian circle in the years 1870–'78 and 1883–'84, chiefly by Prof. W. A. Rogers, under whose direction the reductions have also been made. The right ascensions were observed chronographically over eleven vertical wires, and the declinations also chronographically over an inclined wire, the circle being read by two microscopes. The probable error of an observation in 1870–'78 is $\pm 0.^s054$ in right ascension and $\pm 0''.55$ in declination, and is rather greater for stars fainter than the eighth magnitude than for brighter stars.

The fifth volume of the Annals of the Leyden observatory contains the second half of the zone observations between $+30^\circ$ and $+35^\circ$—embracing ten thousand observations.

The Paris Catalogue.—The second part of this work, containing the places of stars from 6^h to 12^h of right ascension, has recently been issued, the first part having been published in 1887. There are really three catalogues, the first comprising observations from 1837 to 1853 reduced to 1845.0; the second, those made from 1854 to 1867 reduced to 1860.0, and the third from 1868 to 1881 reduced to 1875.0. The stars are arranged in the order of right ascension at 1875.0. A valuable memoir on the proper motions of the stars contained in the catalogue has been prepared by Bossert.

Second Munich Catalogue.—A second catalogue, containing 13,200 stars for the epoch 1880.0 has been published under the direction of Prof. Seeliger supplementary to the larger catalogue recently issued. The stars are from the seventh to tenth magnitude within 25° of the equator, and were observed with the meridian circle during the years 1884 to 1888. The positions depend upon Auwers's Fundamental Catalogue.

Pulkowa Catalogue.—The Pulkowa catalogue of 5,634 stars for 1875 is deduced from observations made with the meridian circle during the years 1874–'80, and prepared for publication by Herr Romberg. The stars are of various classes, including many of the Struve double stars. A comparison is made with the places of several other catalogues.

Oeltzen's Catalogue.—A new editon of Oeltzen's catalogue of Argelander's southern zones, —15° to —31°, has been published by Prof. Weiss. The total number of stars is 18,276, the positions being given for 1850.0 with the amount of the precession necessary to bring them to 1875.0. The places of stars north of —23° have been compared with Schönfeld's Southern Durchmusterung, and south of that limit with other catalogues, thereby eliminating a considerable number of errors from the original places.

Bœddicker's map of the Milky Way.—Dr. Bœddicker, of the Earl of Rosse's observatory at Birr Castle, has been at work since 1884 upon an elaborate map of the Milky Way from the North Pole to 10° south declination, and has at length finished this very laborious task. His plan has been to exhibit the ramifications of the Milky Way as it appears to the naked eye, a necessary first step to the knowledge of the structure of the sidereal universe. No optical help has been used.

STELLAR PARALLAX.

Prof. Pritchard has continued in Part IV of the publications of the Oxford University Observatory his work upon the photographic determination of stellar parallaxes. He has concluded "from actual and prolonged experience that an accuracy, amply sufficient in the present condition of astronomy, is secured by observations of each star made on twenty-five nights advantageously selected throughout the parallactic year, four exposures being usually made on each night."

The general result of the investigations of the parallax of thirty northern stars of the second magnitude is that the average parallax of a star of the second magnitude is 0.″056; and comparing with this the result of Drs. Gill and Elkin for the average parallax of fourteen first magnitude stars, viz, 0.″089 we see that there is distinct evidence that the brighter stars are nearer—though it should be borne in mind that the heliometer was used by Drs. Gill and Elkin, and the photographic method by Prof. Pritchard.

Following is a tabular statement of the Oxford results. Two results *a* and *b* are obtained, from two comparison stars; the probable error of each result is about ±0.″025:

Star.	Parallax. *a*	Parallax. *b*
	″	″
α Andromedæ	+0.0565	+0.0600
β Andromedæ	+ .0610	+ .0860
α Arietis	+ .0880	+ .0715
α Persei	+ .0996	+ .0738
β Persei	+ .0642	+ .0529
β Tauri	+ .0730	+ .0529
β Aurigæ	+ .0591	+ .0652
γ Geminorum	− .0135	− .0333
α Ursæ Majoris	+ .0486	+ .0436
β Ursæ Majoris	+ .1177	+ .0434
γ Ursæ Majoris	+ .0768	+ .1206
ε Ursæ Majoris	+ .0832	+ .0792
η Ursæ Majoris	− .0309	− .0628
β Leonis	+ .0490	+ .0087
β Ursæ Minoris	− .0200	+ .0644
α Coronæ	− .0255	− .0493
γ Draconis	+ .0625	+ .0371
γ Cygni	+ .1107	+ .0931
ε Cygni	+ .0927	+ .1629
α Pegasi	+ .0913	+ .0719
ε Pegasi	+ .0693	+ .0919

Yale heliometer determinations of stellar parallax.—Dr. Elkin publishes the following preliminary results of his investigations of the parallaxes of the first magnitude stars in the northern hemisphere, proposing to continue his observations until he has secured one hundred sets of measures of each of the ten stars—that number being required in his opinion to furnish parallaxes with probable errors not much above 0''.01:

Star.	Parallax.	Probable error.	No. of comparison stars.	No. of sets.
	"	"		
α Tauri	+0.101	±0.022	6	65
α Aurigæ	+0.095	0.021	5	51
α Orionis	+0.022	0.022	6	48
α Canis Minoris	+0.341	0.020	6	48
β Geminorum	+0.057	0.021	6	48
α Leonis	+0.089	0.026	10	43
α Bootis	+0.016	0.018	10	89
α Lyræ	+0.092	0.019	6	67
α Aquilæ	+0.214	0.023	10	46
α Cygni	—0.012	0.020	7	49

Determination of stellar parallax with a transit instrument.—Prof. Kapteyn has published a paper of much interest, upon the determination of relative stellar parallax by observations of the differences of right ascension between the selected star and neighboring comparison stars made with the transit instrument and chronograph. The comparison stars are selected of about the same declination as the star whose parallax is to be determined and symmetrically situated at slightly greater and less declinations. The differences of right ascension and of magnitude should be small. Special precautions are taken to eliminate all ordinary instrumental errors, particularly the error of clock rate, which has an important effect.

The following are the results published by Prof. Kapteyn. The probable error given in each case is not far from ±0.''03:

Star.	Parallax.	Star.	Parallax.
	"		"
Bonn VII 81	+0.074	*Bonn VII* 104	+0.428
θ Ursæ Majoris	+ .052	105	+ .168
Bonn VII 85	+ .064	110	+ .030
20 Leo Minoris	+ .062	111	+ .016
Bonn VII 89	+ .176	112	+ .139
Bonn VII 94	+ .101	114	+ .038
Bonn VII 95	+ .038	119	+0.056
Lal. 20670	—0.011		

Parallax of δ Herculis.—Prof. Leavenworth has found a parallax of +0.''050 ±0.''014 from his own observations of this star; and from a series of observations published by Dembowski in his "Double Star Observations," +0.''030 ±0.''015.

Parallax of P Ursæ Majoris.—Dr. Franz finds from heliometer observations of this star at Königsberg from 1883 to 1890 a parallax of

+0.″10 with a probable error of 0.″01. As the annual proper motion is 3″, this parallax implies that the star is moving through space at a rate of 88 miles a second. Dr. Franz's result is considerably smaller than that obtained by Prof. Geelmuyden from transit observations, π=0.″27 from differences of right ascension, and 0.″24 from differences of declination.

DOUBLE AND MULTIPLE STARS.

Gore's catalogue of binary stars.—Mr. Gore has compiled a useful catalogue of binary stars, for which orbits have been computed, giving, besides the elements, date of computation, etc., the magnitudes, colors, spectra, hypothetical parallax, observed parallax, relative brightness, and the constants A and B for use in Mr. Rambaut's method of computing the parallax from the orbital motion of the star in the line of sight. The more recent measures are given in a series of notes. The catalogue was originally communicated to the Royal Irish Academy, in June, 1890, and has been reprinted from the Proceedings.

Prof. Asaph Hall has made a further discussion of the relative motion of the two components of 61 Cygni and the question whether there is anything in the nature of a physical connection between the two. His conclusion is in favor of such connection, but although accurate observations of the mutual distances and angles of position date from 1825, and Prof. Hall includes in the discussion those made by himself up to 1891, it is not possible to reach any result with regard to the period of revolution, except that it is long.

Two lists of double stars discovered by Mr. Burnham, most of them with the 36-inch refractor, have appeared during 1892, bringing Mr. Burnham's double star discoveries up to 1264. Most of his measures are of the more difficult or interesting doubles, a measurement of 0″.1 being apparently quite a simple matter. Mr. Burnham has also published a number of investigations of double star orbits, and collected lists of measures.

Among lists of recently published measures of double stars should be mentioned the series of observations of 950 stars by Prof. Hall made from 1880 to 1891, with the 26-inch equatorial, of the United States Naval Observatory. With reference to the reduction and discussion of double star measures, Prof. Hall says: "The formulæ and corrections for personal equation of observation seem to me of doubtful utility, and a better way is to compare the measurements of the same star by different observers."

Discovery of double stars by means of their spectra.—In the review of astronomy for 1889–'90 attention was called to Prof. E. C. Pickering's discovery of the duplicity of ζ Ursæ Majoris and β Aurigæ through pecularities in their spectra which indicated differences in the motions of supposed components.

Prof. Pickering has more recently called attention to another interesting class of "invisible double stars," detected in a somewhat similar way by peculiarities in their spectra.

Of many double stars the brighter component is red or yellow, while the fainter component is green or blue. The spectroscope shows that this is due to the fact that the spectrum of the brighter component is of the second type, like our sun, while the spectrum of the fainter component is of the first type, traversed by strongly marked hydrogen lines. If the stars are near together the spectrum of the combined light resembles that of the sun, except that the hydrogen lines are all strong. Stars like β Cygni give such a spectrum, but the components are so far apart that the separation of their spectra is clearly shown. Several stars hitherto supposed to be single have been found whose spectrum is of the class described above, and the question arises whether they may not really be double with components so close that they can not be separated by ordinary means. In the detailed examination of the spectra of the brighter stars made by Miss Maury upon the Harvard photographs, stars occupying all intermediate grades from the first to the second type have been found, and it is difficult to determine whether there are really two spectra or merely changes in the spectrum of a single star due to physical causes. Upon the hypothesis of duplicity the hydrogen lines would probably show a periodic displacement, and in fact an examination of four photographs of the spectrum of Procyon does show a displacement of the lines which, if the phenomenon is due to the relative movement of a faint component, would seem to indicate that it is receding at the rate of 20 kilometers per second as compared with the bright component. The evidence of duplicity is not considered conclusive by Prof. Pickering, but, from an examination of ten other stars having a similar composite spectrum, five are well-known doubles, two have distant companions, leaving three, τ Persei, ζ Aurigæ, and δ Sagittarii, which it would seem from the above considerations may possibly be double.

VARIABLE STARS.

Algol.—Mr. Chandler has published the results of an interesting investigation of the variable star Algol, the periodicity of which appears to have been first discovered by Goodricke, at York, in 1782; and the explanation suggested by him of the periodic diminution of the brightness, that it is produced by the interposition of an opaque satellite, is now generally accepted, confirmed as it has recently been, by the investigations of Prof. Vogel. Mr. Chandler, after an elaborate investigation of the inequalities in the period, and also of the irregularity in the observed proper motion of Algol, has found that they may be satisfactorily accounted for by supposing that both Algol itself and the satellite which revolves round it in about 2 days 20.8 hours have a common revolution round a third, large, distant and opaque body, in a period of about 130 years. The size of this orbit around the common center of gravity is about equal to that of Uranus around the sun. The plane of the orbit is inclined about 20° to our line of vision.

Several interesting cases of variability have been detected in the examination of the photographs of stellar spectra at the Harvard Observatory all showing the bright hydrogen lines; the change in brightness exceeding two magnitudes. The director of the Harvard Observatory has called for the coöperation of astronomers provided with telescopes of moderate power and not otherwise engaged, for the observation of a list of seventeen circumpolar variables of long period. The methods to be followed are set forth in a circular issued by the observatory, and accessible to all who are interested.

Nova Aurigæ—One of the most remarkable outbursts of "new stars" or "*novæ*" that has ever been recorded, occurred during the year 1892—a phenomenon of double interest in that it afforded an opportunity of study under improved astronomical apparatus.

On February 1, 1892, an anonymous postal card was received at the Royal Observatory, Edinburgh, announcing the presence of a new star in the constellation Auriga. It subsequently turned out that the discoverer was Dr. Thomas S. Anderson, an amateur astronomer living in Edinburgh, that the discovery had been made by the help of a star-atlas and a small pocket telescope, and that the star had been seen by him for some days previous to February 1; it was of about the fifth magnitude. In the first observations at Edinburgh it was found to be of a yellow tint and about the sixth magnitude, its position for 1892 being right ascension $5^h\ 25^m\ 3^s$; declination $+30°\ 21'$. Very fortunately systematic photographs of this region had been made for some time by Prof. Pickering at the Harvard Observatory, and the *Nova* was in fact found to have been photographed on thirteen plates taken between December 10, 1891, and January 20, 1892; while it does not appear upon a plate taken at Heidelberg on December 8, which shows stars down to the ninth magnitude. The outburst, at least above the ninth magnitude seems, therefore, to be pretty well fixed between December 8 and 10, 1892.

The Nova remained of the fourth or fifth magnitude till the end of February, then diminished somewhat rapidly, and by the end of March it was of the twelfth to fourteenth magnitude.

In August it was again easily visible. At the Lick observatory it was found to be of 10.5 magnitude on August 17, and 9.8 on August 19, and further fluctuations in brightness have occurred.

The spectrum was of the greatest interest. The chief characteristic was a brilliant array of bright, broad lines, attended by dark companions on the more refrangible sides. Numerous finer details were then added, dark lines crossing the broad, bright bands, and bright lines marking the dark companions.

Three lines have attracted more especial attention on account of their intimate connection with the suspected physical constitution of the star. "These are (1) the bright green line near b_4, and the less refrangible edge of the hydrocarbon band; (2) the line near the chief nebular line λ5006, and (3) the line near the pair of chromospheric lines λ4923 and λ4921. When the wave length of these lines, as quoted by the observers, are corrected for motion in the line of sight, and arranged in a table, the mean values come out very close to the wave lengths of three notable pairs of solar chromospheric lines; while magnesium and the hydrocarbons, as possible origins of line (1), are excluded by the absence from the lists of their inseparable companion lines and flutings.

Line (2) is claimed by four observers for the chief nebular line, but the weight of evidence seems to be against its nebular origin, and the outburst would seem to be a vast chromospheric disturbance, a view confirmed by Dr. Huggins' observation of the complete series of bright hydrogen lines in the ultra-violet—the same that Hale and Deslandres found in the solar chromosphere—but each with its dark companion.

An interesting article advocating the meteoric theory in explanation of the outburst is given by Prof. Lockyer in Volume 31 of the Nineteenth Century. The chromospheric theory of the near approach of two stars is given by Dr. Huggins in the June number, 1892, of the Fortnightly Review; Seeliger's modification of the meteoric theory is translated in Astronomy and Astrophysics for December, and a single-star chromospheric theory is offered by Sidgreaves in the October number of the Observatory.

STELLAR SPECTRA.

Draper catalogue of stellar spectra.—Volume 27 of the Harvard Annals contains a catalogue of the photographic spectra of more than ten thousand stars north of 25° south declination. The photographs were taken with an 8-inch Voigtländer lens, in front of which was placed a prism 8 inches square, with a refracting angle of 13°. The edge of this prism was so fixed that the star's light was dispersed in declination, the length of the spectrum being about a centimeter, and the star being allowed to trail slightly gave the spectrum a width of about a millimeter. Each plate covered 10° square and the spectra of all stars to the sixth magnitude were photographed. The spectra are divided, for convenience, into a large number of classes—A B C D indicating varieties of the first type; E to L, varieties of the second type; M, the third type; N, the fourth type; and O P Q spectra that do not resemble any of the preceding types. One of the most important features of the work is the method by which photographic magnitudes have been assigned. "The quantity measured in each case is the intensity of the spectrum in the vicinity of the G line. Accordingly, when stars having different spectra are compared, the results will not be the same as if the entire light of the stars were measured. In the latter case, the results will differ with the color of the star, according to the method of measurement employed. This is a serious defect in the measures of the brightness of the stars in catalogues hitherto published. Since the present measures relate to rays of a single wave-length, the same result should be obtained whether the method of comparison was by the photographic plate, the eye, or the thermopile."

The Draper catalogue gives the approximate positions of the stars for the year 1900, with their reference numbers in the Bonn Durchmusterung and the Harvard Photometry; their class of spectrum by letters; their photographic magnitudes and the differences of these from the magnitudes of the Durchmusterung, the Argentine General Catalogue and the Harvard Photometry. A well arranged table gives the details of the measures of magnitude on the various plates on which each star appears. The whole sky to 25° south declination was photographed twice with plates overlapping.

Volume 26, part 1, of the Harvard Annals gives additional details respecting the photographs, their measurement and reduction not conveniently included in the catalogue volume itself—a complete history of the Draper Memorial. A point brought out in the various matters discussed in this volume is the predominance of the first type spectra in the Milky Way elsewhere referred to, and the systematic undervaluing of the brightness of the Galactic stars by about one-fifth of a magnitude, by the "Durchmusterung" and "Uranometria Argentina" as compared with the Harvard photometric and photographic magnitudes.

A third volume is to follow devoted to the work of the 8-inch Draper telescope during the years 1889 and 1892 and to the discussion of stars of peculiar spectra.

A fifth type of stellar spectra.—Prof. E. C. Pickering has proposed to class in a "fifth type" stars whose spectra resemble those of the stars discovered by Wolf and Rayet. In general, his photographic survey has confirmed Secchi's fourfold division of stellar spectra, but many stars in Orion and the neighborhood differ considerably from the ordinary first-type stars, the additional lines, instead of being faint as in Vega, being nearly as intense as the hydrogen lines, while two classes of objects, the planetary nebulæ and the stars, the spectra of which consist chiefly of bright lines, are left unprovided for. Prof. Pickering points out the close similarity of the grouping of the lines in these three classes and also the striking character of their distribution. While stars of the second and third types are about equally divided between the Milky Way and the regions remote from it, two-thirds of the first-type star lie in or near the Milky Way and of the Orion stars four-fifths are found in the Milky Way.

A similar distribution of the planetary nebulæ has long been recognized, and Prof. Pickering shows that, of thirty-three stars known as the "Wolf-Rayet," or as he suggests the "fifth type," every one lies within 10° of the Galactic equator, two-thirds within 2° of it.

α Virginis.—Dr. Vogel's more recent observations of α Virginis at Potsdam accord with his earlier observations of the same star, showing that it is a close binary. The method of observation is quite interesting: The spectrum of the star and of terrestrial hydrogen are photographed together, and the displacement of the star lines on the photograph in the neighborhood of H γ is afterwards measured under a microscope. Stars with spectra of the second and third types give results of considerable accuracy, as the lines in such stars are numerous and sharp.

In the case of α Virginis the difficulties of observation were greater, the hydrogen lines being broad and diffuse, without any definite maximum of intensity, and there were no distinct lines in the vicinity of H γ to which the measurements could be referred. Dr. Vogel's measurements of twenty-four photographs showed that the star lines were displaced alternately toward the upper and the lower end of the spectrum in a complete period of about four days, the maximum displacement toward the violet indicating a motion of the star toward the sun of 65.9 English miles, and that toward the red a receding motion of 47.5 miles per second. These observations are completely explained by supposing that *Spica* is a binary star having a period of one component about the other or the common center of gravity of about 4 days, (the orbital velocity of the larger component being 56.7 miles per second) and that the system is approaching the sun at the rate of 9.2 miles per second. On the assumption of a circular orbit, equal mass of the components, and the data given by observation, the mass of the system is 2.6 times that of the sun, and the distance between the components 6,260,000 miles.

In commenting upon Dr. Vogel's work Prof. Keeler says, "A wonderful picture of stellar motion is presented to our mind, and one to which the whole visible universe as revealed to us by our greatest telescopes offers no parallel. The spectacle of two great suns like our own, revolving around each other in only four days, at a distance no greater than that which separates the sixth satellite of Saturn from its primary, is one which the inadequacy of our optical powers will probably ever forbid us from actually beholding, but the indirect evidence that such extraordinary circumstances of motion exist is so complete that we must admit their reality."

β Aurigæ.—The Potsdam observations furnish a complete confirmation of Prof. Pickering's discovery of the duplicity of this star. The lines in the spectrum of the star appear double on every second day, and the component, in the line of sight, of the motion of the system can amount to nearly 150 miles a second, while the whole system has a motion relatively to the solar system of −4.03 miles; that is, a motion of this amount per second towards the solar system.

ζ Ursæ Majoris.—The duplicity of ζ Ursæ Majoris is not so satisfactorily confirmed. The maximum relative velocity of its two components seems to amount to about 100 miles per second.

α Bootis.—Mr. Keeler's recent measures upon the D line of the spectrum of *Arcturus* show that the velocity in the line of sight is not 80 kilometers per second, the value hitherto accepted, but 6.4 kilometers, which accords with the result obtained by Dr. Vogel. The mean of the measures at Potsdam from October 5, 1888, to May 23, 1890, is −7.1 ± 0.3 kilometers. The Lick observations from April 20, 1890, to August, 15, 1890, give −6.9 kilometers.

ASTRONOMICAL PHOTOGRAPHY.

The photographic chart of the sky.—The third* meeting of the permanent committee, appointed by the Astrophotographic Congress at Paris in 1887, was held at the Paris observatory from March 31 to April 4, 1892. Admiral Mouchez presided, the members of the committee pres, ent being Baillaud, Bakhuyzen, Beuf, Christie, Denza, Donner, Gill Henry (Paul), Henry (Prosper), Janssen, Kapteyn, Loewy, Mouchez, Pujazon, Rayet, Ricco, Tacchini and Trépied. The following astronomers were also present by invitation, Messrs. Abney, Andoyer, Belopolsky, Bouquet de la Grye, Cornu, Knobel, Gautier, Maturana, Plummer, Scheiner, Tisserand, and Wolf (C).

Drs. Bakhuyzen and Gill were elected vice-presidents and Prof. Kapteyn and Trépied secretaries.

From reports of progress made at different observatories the following notes indicate the advancement of the work:

Some delay had been experienced in securing the plates containing the reference lines or "*réseaux*," but provision was finally made to furnish them at an early day, as well as the photographic plates which it was necessary should be of a specially good quality of plate glass.

Algiers.—Instruments ready and only awaiting the plates and "*réseau.*"

Bordeaux.—Photographic installation has been ready for about a year; a number of experimental photographs have been taken and the work can commence as soon as a supply of plates is secured with the necessary "*réseau.*"

Cape of Good Hope.—Instrument practically ready.

Catania.—The instrument has been completed.

Helsingfors.—The instrument has been ready for several months and a considerable number of photographs have been taken.

La Plata.—Instrument ready.

Melbourne.—Instrument ready and a number of experimental plates have been secured.

Oxford.—Instrument ready and a number of plates submitted to the committee.

* The first meeting of the committee, for organization, etc., was held at the time of the Congress in April 1887, the second meeting or the first regular meeting for discussions, in September, 1889 (not 1890, as stated by a misprint in the review of Astronomy for 1889–'90).

Paris.—Instrument ready.

Potsdam.—Instrument ready and a number of plates submitted to the inspection of the committee.

Rio de Janeiro.—The photographic equatorial has been received and will be mounted at the new site of the observatory.

Rome (*Vatican*).—The instrument has been completed.

San Fernando.—The instruments are mounted and work can be begun as soon as the *réseau* is received.

Santiago.—The instrument is finished, but, owing to political disturbance in Chile it is impossible to fix a day for beginning the work.

Sydney.—Ready except for the "*réseau*."

Tacubaya.—Instrument ready and a number of experimental plates submitted.

Toulouse.—The instrument was one of the first to be mounted; the "*réseau*" and photographic plates are only needed to begin the work.

Following is a summary of the resolutions adopted at this meeting:

(1) No change is made in the conditions of distance and magnitude of the stars that have formed the different parts of the catalogue of guide stars.

If, however, the guide star of the catalogue is not bright enough, a brighter one may be selected up to a distance of 10′ from the center of the plate.

(2) The "*réseau*" is to be photographed upon each plate by parallel rays of light. (To replace resolution 15 adopted at the meeting of 1889.)

(3) The orientation of the plates in zones above 65° declination will be arranged for the equinox of 1900; for other stars the parallel will be referred to the apparent equinox.

(4) The work decided upon by the congress of 1887 comprises two series of negatives made with different exposures. The committee, while urging special activity in securing plates of shorter exposure (negatives intended for the catalogue), would suggest that the best nights be also taken advantage of for plates of longer exposure (for the chart).

(5) Negatives from which the catalogue is to be formed will have two exposures for the same plate, one showing faintly the images of stars of the eleventh magnitude, the other with an exposure twice as long, the distance of the two images being 0.2 to 0.3 of a millimeter. (To replace resolution 23 of the meeting of 1889.)

(6) MM. Abney and Cornu are added to the committee on reproduction of the negatives.

(7) With reference to the production of the chart, purely photographic methods will be used, to the exclusion of all manual intervention.

(8) For the chart proper (long exposures) a series of negatives with single exposure will be taken, having an *even* degree of declination in the center of the plate. Further study will show whether it is desir-

able that in the second series (those with an *odd* degree at the center) there should be two or three exposures.

(9) To make it possible to pass uniformly and with certainty from Argelander's ninth magnitude to the eleventh magnitude desired for the negatives of the photographic catalogue there will be distributed among the observatories fine wire-gauze screens, absolutely identical, which, when placed over the object glass of the telescope will diminish the magnitude of a star by two units (adopting the coefficient 2.512 for the ratio between two consecutive magnitudes). Each observatory will from time to time make type negatives of certain specified regions.

(10) The committee suggests forty minutes as the length of exposure of the plates for the chart (the series of *even* declinations) under the ordinary atmospheric conditions of Paris, and with the Lumière plates used.

The committee on metallic screens will furnish the Messrs. Henry with a screen with which they will determine the time *t* for obtaining the eleventh magnitude stars of Argelander's scale. Then for each observatory provided with an identical screen, the ratio 40 : t will be the factor by which to multiply the time of exposure necessary to secure satisfactory images of eleventh magnitude stars, in order to obtain the proper exposure for the chart plates.

(11) The questions of the number of reference stars for each negative for the catalogue, the choice of the stars, and the necessary steps to secure meridian observations are referred to a special committee, consisting of Messrs. Auwers, Bakhuyzen, Christie, Ellery, Gill, Kapteyn, and Loewy, with full powers.

(12) As soon as convenient each observer will prepare, or will have prepared by any observatory or bureau he may select—

(*a*) Measures of the position of each star on the catalogue referred by rectilinear coördinates to the nearest lines of the "*réseau*."

(*b*) Measures necessary for the determination of the stars' magnitudes.

The different observatories will publish the separate results of these measures and the Permanent Committee will undertake their reduction as soon as a sufficient number of meridian observations of the reference stars is at hand.

(13) The work upon the chart will commence at each observatory as soon as the metallic screen reducing the stars by two magnitudes is received, involving probably a delay of two months. Each observer may, however, begin before receiving the screen if he is confident that he can get all stars of the eleventh magnitude upon the catalogue plates.

(14) Without adopting a formal resolution, the committee would recommend as a separate and personal investigation, that a special series of negatives with long exposures be made of the region near the ecliptic.

The following distribution of the zones among the different observatories was definitively adopted in place of that previously published:

Observatories.	Latitude.		Zone.		No. of plates.
Greenwich	+51	29	+90 to	+65	1149
Rome	41	54	+64	55	1040
Catania	+37	30	+54	47	1008
Helsingfors	60	9	46	40	1008
Potsdam	52	23	+39	+32	1232
Oxford	+51	46	+31	25	1180
Paris	+48	50	+24	+18	1260
Bordeaux	+44	50	+17	11	1260
Toulouse	+43	37	+10	+5	1080
Algiers	36	48	+4	−2	1260
San Fernando	+36	28	−3	−9	1260
Tacubaya	19	21	−10	−16	1260
Santiago	−33	27	−17	−23	1260
La Plata	−34	35	−24	−31	1360
Rio de Janeiro	−22	54	−32	−40	1376
Cape of Good Hope	−33	56	−41	−51	1512
Sydney	−33	52	−52	−61	1400
Melbourne	−37	50	−65	−90	1149

(16) Every year before the end of January a report upon the progress of the work will be made to the bureau of the Permanent Committee.

(17) The thanks of the conference were voted for the courtesy of the Academy of Sciences in printing the Bulletin, and the hope was expressed that the different governments would provide the necessary means for the observations themselves and for the publication of the chart.

The sixth fasciculus of the Bulletin contains papers by Prof. Kapteyn and M. Sautier on the parallactic micrometer, and upon photographic magnitudes by Profs. Wolf and Dunér. The latter subject has also been discussed by Dr. Scheiner in the *Astronomische Nachrichten*, by Prof. Pritchard in the *Comptes Rendus*, and by Mr. Christie in the *Monthly Notices*.

Photographs of the Pleiades.—Rutherford's photographs of the Pleiades taken in 1872 and 1874 have been selected for measurement by Mr. Jacoby as offering an opportunity for comparing the accuracy of the photographs with that of heliometer and micrometer measures. Each plate contains two impressions of the cluster, both of which were measured. Mr. Jacoby's method consisted of measuring the position angle and distance from the star 24 *p*, and he finds the probable error of the mean of the twenty exposures to be about ± 0.″03 in each element.

A comparison between these photographic places and the places resulting from the Yale and Königsberg heliometers shows that the photographs are fully entitled to be taken into consideration in making a study of the proper motions or in forming a definitive catalogue of the group.

Dr. Max Wolf, of Heidelberg, with a portrait lens of 2½ inches aperture, has not only discovered new nebulæ on his long exposure photographs, but new minor planets; several meteors which crossed the field left perfectly distinct records.

COMETS.

In a series of papers in the *Bulletin Astronomique* M. Schulhof has developed in an interesting way the relations existing between the elements of a comet's orbit before and after it suffers perturbation by a planet. That the periodic comets of our system have been captured through the perturbing action of planets appears established; and Mercury has four comets assigned to it, Venus seven, the Earth ten, Mars four, Jupiter twenty-three, Saturn nine, Uranus eight, Neptune five, and a further group of comets appears to give a feeble indication of an ultra-Neptunian planet at a distance from the sun of about seventy times that of the earth.

The search for new comets has been systematized by the cometary section of the British Astronomical Association under the direction of Mr. W. F. Denning. The aims of this section are to secure observations of comets, to discover new comets and nebulæ, to record telescopic meteors, etc. It is intended to sweep the sky regularly for new comets, a definite region being assigned to each observer according to convenience and choice.

The following notes, relating chiefly to the comets of 1891 and 1892, will complete the list of comets published in these "Reports of Progress," from 1883 to 1892. It is hardly necessary to remark that the most complete and authoritative annual summary of cometary phenomena is that published by Dr. Kreutz in the *Vierteljahrsschrift der Astronomischen Gesellschaft.*

The arrangement adopted below is the order of perihelion passage, except in the case of well-known periodic comets, such as Encke's, Winnecke's, etc., which are arranged alphabetically by their recognized names. The table of elements appended is to be regarded as only approximate, but is sufficient to furnish an idea of the general form and position of the orbit.

Comet Encke:
=Comet 1891, III.

The return of Encke's well-known periodic comet; first found by Barnard, from the ephemeris, on August 1, 1891. It was then exceedingly faint, but in September it had increased to a nebulous mass of about the sixth to seventh magnitude. The comet was unfavorably situated for observation after the end of September, the last observation reported being October 11. It is noteworthy that its path at this return was almost the same as in the return of 1858, and a comparison of the brightness on these two occasions would seem to indicate that it has not undergone any material change in physical condition during the interval.

Comet Tempel.—Tempel's first periodic comet, and of rather unusual interest, was unfortunately missed at its return in 1892, being unfavorably situated for observation.

Comet Tempel$_3$-Swift:
=Comet 1891 V.

This periodic comet returns to the sun once in every five and a half years, but under conditions alternately favorable and unfavorable for observation. It was originally discovered by Tempel in 1869, was picked up again in 1880 by Swift, and again upon this return by Barnard. At its intermediate returns in 1875 and 1885 it was so situated with reference to the earth and sun as to have been entirely invisible. A very carefully prepared ephemeris by Bossert, taking account of the perturbations from 1880, enabled Barnard to find the comet on September 27, 1891, and it was independently found by Denning at Bristol on September 30. It was described as a faint, shapeless nebulosity, with slight condensation about the center, but even at its brightest, towards the end of November, it was a difficult object for precise observation, a fact all the more to be regretted as its position would render it of especial value for the determination of the distance of the sun.

Comet Winnecke:—Comet 1892 IV. Winnecke's well-known periodic comet was picked up at this return, through the help of von Haerdtl's ephemeris, by Spitaler at Vienna on March 18, 1892: it was then an exceedingly faint and small nebulous mass with stellar nucleus of the sixteenth magnitude. It increased in brightness towards perihelion (on June 30), and after perihelion was observed in the southern hemisphere till the end of September.

Comet 1886 IV,* which was discovered by Brooks on May 22, 1886, was expected to make its first return to perihelion in the latter part of 1892, but was not found. The orbit is somewhat uncertain.

Comet 1889 V.—To quote from the first of a series of masterly papers on the orbit published by Mr. Chandler in the Astronomical Journal: "The vicissitudes in the history of this comet give it an interest exceeded, perhaps, by no other in astronomical annals; and the settlement of the problems connected therewith promises to illuminate our knowledge of cometary mechanics in various important particulars.

While the manner in which the comet became separated into several parts, by its encounter with Jupiter in 1886, may possibly require for its precise exposition the observations which will be obtained at the next appearance in 1896, we may hope for an approximate answer in the careful discussion of those made in 1889 alone. . .

To begin with, it is necessary to notice some of the physical phenomena presented by the companions. The notation used will be the letters assigned by Barnard, *B C D* and *E* in order of the distances from the main comet *A*. As is known, *B* and *C* were detected by him on August 1, with the 12-inch, *D* and *E* on August 4, with the 36-inch. It is desirable to remark here that the reason for their not having been discovered in the previous month, on July 8, 9 and 10, can not have been superposition by perspective, at least in the case of *C* and the more distant companions; for the orbit of *C* . . . shows that

* See Smithsonian Report 1887, p. 123.

such superposition occurred two months previous to discovery of *A* by Brooks, and gives for July 8 a distance of 190″ at 62°.5 position angle. That the companions were not seen in July, may be naturally ascribed to interference of moonlight up to about July 20, and after that either to the fact that the attention of observers was not sufficiently directed to the phenomenon, or to the fact that the objects had not yet become bright enough to be easily discernible. We have the evidence of Spitaler that on July 30 and 31 nothing abnormal was noticed with the 27-inch; the slight elongation on those dates, seen by him in *A* having no relation to the matter in hand. Two nights after, at the time of discovery, Barnard estimated the brightness of *C* at about one-fifth that of *A*. It then gradually increased in brilliancy, also becoming less diffused and developing a strong condensation and nucleus, until at the end of August it was actually brighter than *A* although only one-third its size. In early September it was about equal in brightness to *A* but from the middle of that month faded, and became larger and more diffuse until it disappeared, late in November. The faint nucleus of *B*, in the beginning appears to have been a little brighter than that of *C*, and its coma smaller and less diffused. About the middle of August it had grown to be larger and fainter than at first, later more rapidly so, being excessively difficult to see or measure in the first few days of September, and invisible immediately thereafter. *D* and *E* were measured only on the night of discovery, and were seen only at rare intervals until the last time on August 29.

Such, briefly described, are the main features as to brightness and visibility of these objects. I beg courteously to dissent from the view which has been confidently expressed, that the diffusion and disappearance of *B*, while it was theoretically increasing in brightness, indicate 'that it actually dissipated itself into space and absolutely ceased to exist, if indeed it were not absorbed into the main comet.' Such a conclusion is inherently improbable, unwarranted by any knowledge we possess as to the process of cometary light development, and contradicted by inferences drawn from other cases, of which only the most analagous need be cited, namely, that of the two nuclei of Biela's comet, the capricious action of which affords a strict counterpart to the present instance. It will be recollected that fitful alternations of visibility occurred in 1846, and especially in 1852, when they repeated themselves almost from day to day. The two companions were not habitually seen at the same time, but sometimes one, sometimes the other; so that observers could not tell which they were looking at, without comparison with the ephemeris. Thus, in the space of one week, for example, 1852, September 15 to 22, both nuclei were visible, then only the southern, then only the northern, then both together; again only the southern, and, finally, only the northern, on successive nights, respectively.

It may be added that there appears to be little reason for interpreting these remarkable variations of brilliancy as standing in any relation of effect with cause which produced the disruption, either in Biela's comet or in 1889 V; but much more for supposing that similar behavior may be common, in greater or less degree, escaping attention ordinarily from the difficulty of photometric comparisons in the case of isolated comets, but easily attracting the eye, by contrast, when two objects nearly alike are in the same field."

Mr. Chandler's discussion of the orbits of these companions establishes the important proposition that the force which led to the separation of the components A and C, whatever its nature, operated in the plane of the comet's orbit, and produced no change in that plane or in the form of the conic section, but only in its size, and in the direction of its major axis. With reference to the identity with Lexell's comet, Mr. Chandler sees no sufficient reason in the differences of the period of revolution (28.18 years, according to Mr. Poor, instead of 27 years) to reject the supposition: it is necessary to carry the computation of the perturbations a little farther back.

Comet 1890 II.—The last observation in 1891 was on May 29 by Spitaler at Vienna; but it was again favorably situated in January and February, 1892, and was observed at Nice up to February 4, 1892.

Comet 1891 I: Comet *a* 1891. Discovered by Barnard at the Lick Observatory on March 29, 1891, and independently by Denning at Bristol on March 30. It was quite bright, tenth to eleventh magnitude, about 1′ in diameter and with a tail 10′ to 30′ long. At the time of discovery its position was $\alpha = 15^{h}$, $\delta = +45°$; it moved rapidly south, increasing in brilliancy, and was followed after perihelion till July, the last observation having apparently been obtained at Cordoba on July 9, 1891.

Comet 1891 II: Comet *b* 1891. =Comet 1881 III. Wolf's comet. First detected upon this its second appearance by Spitaler, of Vienna, on May 1, 1891, and by Barnard on May 3, its position agreeing closely with the ephemeris. It was at first small and faint, but in August it had a bright nucleus of the eleventh magnitude, with coma of 3′ to 4′ diameter; it decreased in brightness again after the middle of October, but was observed till March 31, 1892. Early in September the comet passed over the group of the Pleiades, and the circumstance was taken advantage of by a number of astronomers to determine whether the light from these stars underwent any refraction in passing through the material of which the comet was composed. The results obtained were for the most part negative, with the possible exception of an observation by Burnham on September 2, when the difference of declination between 21 and 22 Asterope seemed to show some change as the comet passed over them.

The orbit of this comet may bring it at times close to Jupiter, and indeed the perturbations by that planet in 1875 were so great that an altogether new orbit resulted. The period of revolution is about six and three-fourths years.

Comet 1891 III: *See* comet Encke.
=Comet *c* 1891.
=Encke's comet.

Comet 1891 IV: A telescopic comet of the twelfth magnitude, discovered by Barnard at the Lick Observatory on October 2, 1891. At the time of discovery it was in the constellation Argo; it moved farther south and was not seen at all in the northern hemisphere except at the Lick Observatory, where it was followed up to October 9; in the southern hemisphere it does not seem to have been followed beyond October 11.
=Comet *e* 1891.

Comet 1891 V: *See* comet Temple$_3$-Swift.
=Comet Tempel$_3$-Swift.
=Comet 1869 III.
=Comet 1880 IV.
=Comet *d* 1891.

Comet 1892 I: Discovered by Swift on March 6, 1892, at 17^h Rochester mean time, or 5 o'clock on the morning of March 7, in 30° south declination; the brightest comet seen in the northern hemisphere since the great September comet of 1882. At the time of its greatest brilliancy, which was at perihelion, April 6, it was as bright as a star of the third or fourth magnitude, with a bright, round head and nucleus of 10″ to 15″ diameter. The tail, on the other hand, was exceedingly faint, and was variously estimated at from 1° to 20° in length. Barnard reported it on April 3 as double. The photographs of the tail were of unusual interest, especially those taken in March at Sydney and in April at Mount Hamilton. On the morning of April 5 a photograph, made by Barnard at Mount Hamilton with a 6-inch lens, showed three main branches to the tail, each being separated into several others, so that in all at least a dozen could be counted. At a distance of two degrees from the head, along the northern side of the middle tail, there was a sudden bend southward. On the 7th "the southern component, which was the brightest on the 5th, had become diffused and fainter, while the middle tail was very bright and broad; its southern side, which was the best defined, was wavy in numerous places, the tail appearing as if disturbing currents were flowing at right angles to it. At 42′ from the head the tail made an abrupt bend towards the south, as if its current was deflected by some obstacle. In the densest portion of the tail, at the point of deflection, is a couple of dark holes similar to these seen in some of the nebulæ."
=Comet *a* 1892.

The comet was visible to the naked eye till the beginning of June, and was still under observation with the telescope at the close of the year.

The spectrum as observed by Konkoly on April 1 and 2 consisted of a continuous spectrum and five bright lines, while Campbell, at the

Lick Observatory, whose observations extend from April 5 to June 13, saw, in addition to the continuous spectrum, the three usual cometary bands, the less refrangible sides of these bands being sharply defined and the middle one, in fact, terminated by a very bright line.

The orbit of the comet is undoubtedly elliptic, belonging to the interesting group of comets with a period of about two thousand years. During this appearance, as it was for a considerable time in the neighborhood of Jupiter, its path may be considerably changed.

Comet 1892 II: =Comet *c* 1892.

Discovered by Denning, at Bristol, on March 18, 1892, in 23^h right ascension, and 59° north declination; it was then at its maximum brightness, small, round, with central condensation of from eleventh to twelfth magnitude, and no tail. It remained small and inconspicuous, but was under observation for several months. The orbit is parabolic, without specially interesting peculiarity.

Comet 1892 III: =Comet *f* 1892. =Holmes's comet.

Discovered by Mr. E. Holmes, at London, on November 6, 1892, near the great Andromeda nebula, and also independently on November 9, by Davidson, at Mackay, Queensland—a round nebulous mass 5′ in diameter with a central condensation, but no tail; the suspicion that it was a return of Biela's comet was shown to be unfounded as soon as sufficient observations were available for a determination of its orbit, though the orbit proved to be elliptic and of short period. A short faint tail was seen soon after discovery, and upon a photograph taken by Barnard, on November 10, it can be followed for half a degree, while about a degree from the head and beyond the tail there is a diffused nebulous object, apparently belonging to the comet, and this connection seems substantiated by Campbell's spectroscopic observations.

The comet was visible to the naked eye to the end of November and in telescopes of medium power during the first part of December, and then diminished very rapidly in brightness, not following at all the computed scale of brilliancy, but showing a remarkable and inexplicable outburst about the 16th of January, 1893. The spectrum was also peculiar in that it seemed to be purely continuous.

According to the elements computed by Schulhof the comet passed perihelion on June 13, 1892, and its period is 6.9 years; the orbit seems to lie entirely within that of Jupiter, the nearest possible approach of the two being 0.4, (the mean distance of the earth from the sun being 1,) but since 1861 the two bodies do not seem to have been very close at any time. The small eccentricity, not far from that of Tempel's first periodic comet, brings it quite near to the upper limits of the eccentricity of the asteroid orbits. But with such a short period, as it can not have experienced great perturbations since 1861, the reason for its never having been seen at a previous return, is a mystery which seems to be connected in some way with the very great and abnormal variation in brightness actually detected while under observations, the cause of which still lies beyond us in the unknown characteristics of cometary material.

Comet 1892 IV:
=Comet *b* 1892.
=Winnecke's comet.

Found by Spitaler, at Vienna, on March 18, 1892.

See Comet Winnecke.

Comet 1892 V:
=Comet *e* 1892.

Especial interest attaches to this comet, as it is the first discovered by photography, if we except the single case of the "Tewfik comet," shown near the sun on a plate exposed during the total eclipse of May 17, 1882. The present comet was detected as a suspicious looking object upon a plate exposed near α Aquila on October 12, 1892, by Barnard. On the following evening the cometary character of the object was confirmed by the 12-inch refractor. It was faint, 1′ in diameter, and from twelfth to thirteenth magnitude, somewhat condensed toward the center. It changed but little in appearance and was last seen in December.

Dr. Krueger's elements give a period of revolution of only 6.3 years and show a remarkable resemblance to those of Wolf's comet—so great, in fact, as to suggest a common origin for the two, as in the case of Biela's comet and Brooks's comet, 1889 V.

Comet 1892 VI:
=Comet *d* 1892.

Discovered on August 28, 1892, in the constellation Gemini, by Brooks, a quite bright, round nebula, with distinct nucleus and short faint tail; it was visible to the naked eye in November, and the tail could be followed, upon a photographic plate, November 26th, for 5°; after the middle of December the comet was observable only in the southern hemisphere.

The spectroscope showed a continuous spectrum with the three usual cometary bands.

Comet 1893 I:
=Comet *g* 1892.

This comet was also discovered by Brooks, at Geneva, N. Y., in the constellation Bootes, on the morning of November 19, 1892; it was then quite bright for a telescopic comet, but showed no tail, while its increase in brightness and northerly motion made it an easy object for observation during the rest of the year.

In chronicling the comets of the year 1892 mention should be made of a suspicious object detected by Prof. M. Wolf upon photographic plates exposed on March 19 and 20, 1892. It could not be found upon a photograph of March 22 nor in a later search with the great Vienna refractor.

The announcement of a comet discovered by Freeman on November 26, 1892, proved to be erroneous.

A comet announced by Swift on December 23, 1889, has been identitied by Dreyer with a nebula discovered by Herschel, and is, therefore, to be stricken from the list of lost comets.

Approximate elements of the comets of 1891 and 1892.

Designation.	Perihelion T (Greenwich mean time).	☊	ω	i	q	e
		° '	° '	° '		
1891 I	1891, Apr. 27.56	193 56	178 48	120 31	0.397	...
II	1891, Sept. 3.16	206 22	172 48	25 15	1.592	0.557
III	1891, Oct. 17.98	334 41	183 57	12 55	0.340	0.847
IV	1891, Nov. 12.94	217 39	268 33	77 43	0.977	....
V	1891, Nov. 17.34	296 31	106 43	5 23	1.087	0.653
1892 I	1892, Apr. 6.69	240 54	24 31	38 42	1.027	0.999
II	1892, May 11.22	253 26	129 19	89 42	1.971	.....
III	1892, June 13.27	331 42	14 11	20 47	2.140	0.410
IV	1892, June 30.89	104 5	172 6	14 32	0.886	0.726
V	1892, Dec. 11.05	206 39	170 14	31 12	1.429	0.581
VI	1892, Dec. 28.09	264 28	252 41	24 18	0.976	...
1893 I	1893, Jan. 6.52	185 39	85 14	143 52	1.195	...

Designation.	Discoverer.	Date of discovery.	Synonym.	Remarks.
		1891		
1891 I	Barnard	Mar. 29	1891 *a*	
II	Spitaler	May 1	1891 *b*	Wolf's comet. Period 6.8 years.
III	Barnard	Aug. 1	1891 *c*	Encke's comet. Period 3⅓ years.
IV	Barnard	Oct. 2	1891 *e*	
V	Barnard	Sept. 27	1891 *d*	Tempel$_3$-Swift. Period 5.5 years.
		1892		
1892 I	Swift	Mar. 7	1892 *a*	
II	Denning	Mar. 18	1892 *c*	
III	Holmes	Nov. 6	1892 *f*	Period 6.9 years.
IV	Spitaler	Mar. 18	1892 *b*	Winnecke's comet. Period 5¾ years.
V	Barnard	Oct. 12	1892 *e*	Period 6.3 years.
VI	Brooks	Aug. 28	1892 *d*	
1893.				
I	Brooks	Nov. 19	1892 *g*	

METEORS.

A fine shower of meteors, radiating from the neighborhood of γ Andromedæ, was seen in the United States and Canada on the night of the 23d of November, 1892. There seems to be no doubt that it was a part of the great stream connected with Biela's comet, which was encountered on the 28th of November, 1872 and 1885. On these two occasions the earth probably passed through the main swarm, while in 1892 it passed some days earlier through an associated branch of it. From a comparison of the positions of the comet and of the dates of the meteoric showers in 1798, 1838, and 1872 Prof. Newton was long ago led to conclude "that a long, extended group of meteor particles must accompany the comet in its periodical revolution, preceding it to a distance of 300,000,000 miles in front, and following it to a length of 200,000,000 miles in the rear of its actual position, or occupying, if there is no reason to suppose the elongated meteor current discontinuous, fully 500,000,000 miles in its observed length along the comet's path."

SOLAR SYSTEM.

Motion of the solar system.—Prof. Porter has discussed the proper motions of 1,340 stars contained in publication 12 of the Cincinnati observatory. Adopting Dr. Schönfeld's method of dividing the stars into four groups, according to the magnitude of their proper motions, he has confirmed Dr. Stumpe's result that the proper motion of a star is an index of its distance from us. The mean position of the "sun's way" from his figures is 281°.2 right ascension and +40°.7 declination.

Dr. Vogel has also published the results of an inquiry on this subject based on the measured velocities of stars in the line of sight. The motion of fifty-one stars has been determined at Potsdam, and the probable error in the measurement is below 1.16 geographical miles, but the resulting value of the apex of motion, though the observations have been discussed in various ways, is not in very satisfactory accord with other investigations. If the stellar motions be treated either with equal weights, or weights approximately proportional to those assigned by Dr. Vogel in his catalogue of proper motions, the coordinates of the apex are 206°.1 ± 12°.0 in right ascension, and + 45°.9 ± 9°.2 in declination, with a velocity of 11.60 ± 1.85 geographical miles.

SUN.

Diameter of the sun.—A large number of heliometer measures of the diameters of the sun and Venus made by the German transit of Venus parties in 1874 and 1882, incidental to the more important determination of the solar parallax, have been discussed by Dr. Auwers, who finds for the mean result of the sun's diameter (thirty-one observers) 1,919″.3, which differs considerably from that adopted in the various ephemerides; the Berlin Jahrbuch, for instance, uses 1,922″.4, the Connaissance des Temps and British Nautical Almanac1, 923″.6, and the American Ephemeris 1,924″.0. Dr. Auwers remarks that if the value he finds is affected by irradiation it can only be too large, while the adopted diameters are larger still. He announces that a change will be made in the value used by the Berlin Jahrbuch in the volume for 1895.

Temperature of the sun.—The numerous attempts that have been made to determine the temperature of the sun have led to the most discordant results, the figures varying from 1,500° to 5,000,000°. The method employed, however, has always been the same (that of Pouillet), and the experimental determinations have been sufficiently concordant in themselves, the divergencies arising from the different laws adopted to connect the radiation of incandescent bodies with their temperature. Newton's law, which holds only for an interval of a few degrees, gives for the temperature of the sun millions of degrees. Dulong's, which is only exact over a range of 150° at most, gives 1,500°. Rosetti's law, established by experiments made between 0° and 300°, gives 10,000°. A more recent series of experiments has been made by M. H. Le Chatelier, and is published in the *Comptes Rendus* for March 28, 1892, in which the temperatures employed cover a range of 1,100° (700° to 1,800°). The "effective" temperature that he finds for the sun is 7,600°, which he thinks may be subject to an uncertainty, on account of errors which may effect the law of radiation, not greater than 1,000°, the "effective" temperature being that temperature which a body of emissive power equal to unity must have in order to send us radiations of the same intensity as the sun. The actual temperature of the photosphere is higher, for a part of its radiations are absorbed by the less highly heated solar atmosphere, and perhaps also (although this seems hardly probable) because the emissive power of the sun may be less than unity.

Solar activity in 1892.—The development of the solar activity during 1892 was no less marked with regard to prominences than with regard to sun spots. On April 6 Trouvelot reported an arched prominence extending some 90,000 miles along the limb of the sun and attaining a height of over 57,500 miles. Two days later an enormous protuberance rose to a height of 71,970 miles, extending in a little over half an hour to 105,550, and a week later another extending over 255,000 miles along the circumference.

The great sun spot group of 1892.—It appears that the original formation of the group took place on the farther side of the sun, and it first came under observation on November 15, 1891, when it was seen as a spot of considerable size close to the east limb. On November 16 the group consisted of three spots, and by November 18 it had assumed the appearance so typical of the more important disturbances, of a long procession of spots of various sizes, the spot in the van and that in the rear being the largest. During the December appearance (December 12–24) it was throughout one well-defined circular spot.

One spot, roughly circular in shape, alone appeared on January 7. It is not quite clear whether it represented the principal group of the November appearance or the little group which formed in advance of it and which became prominent during December. It seemed to occupy a position nearly midway between the two, though the two are practically to be regarded as one disturbance.

Before its appearance at the east limb on March 4 a great change had taken place. The group, which on February 13 had covered more than 3,000 millionths of the sun's visible hemisphere, did not cover one-fifteenth of that area on March 5, though it revived somewhat before it was last seen at the west limb on March 17, but did not survive to make a sixth appearance at the east limb on March 31 or April 1.

According to Mr. Maunder the great spot, the largest on record at Greenwich, was 92,000 miles long and 62,000 miles broad, while the entire group of which it formed the principal part was 162,000 miles long and 75,000 broad. The area of the spot on February 13, 1892, was 2,940 million square miles, and the whole group 3,530 million square miles. This is about eighteen times the area of the earth, and seventy globes as large as ours could have lain side by side in the immense hollow. Mr. Maunder thinks that the effect upon the weather of a spot even of such enormous size must be very slight, if appreciable. The magnetic needle, however, undergoes violent disturbance upon their appearance.

In an article in Knowledge for April and May, 1892, Mr. Maunder brings forward some important evidence in regard to the connection between sun spots and magnetic storms. The article concludes as follows:

In a period of nearly nineteen years, therefore, we have three magnetic storms which stand out preëminently above all others during that interval. In that same period we have three great sun spot displays—counting the two groups of April, 1882, together—which stand out with equal distinctness far above all other similar displays. And we find that the three magnetic storms were simultaneous with the greatest development of the spots. Is there any escape from the conclusion that the two have a real and binding connection? It may be direct, it may be indirect and secondary only, but it must be real and effective.

Consider that the period in question is practically some six thousand eight hundred days. A magnetic storm does not last many hours; a sun spot soon declines from its greatest development, or soon passes away from the center of the apparent disk. Suppose we take an outside limit, and give a period of two days to a giant spot to exercise its influence or a magnetic storm to expend its violence; what are the probabilities against 3 out of 3,400 of such periods of the one phenomenon agreeing with 3 out of 3,400 of the other, if they are not related? If 3,400 numbers were placed in one box and 3,400 more in a second, and one from each box were drawn at a time, what is the chance that the three highest numbers would be drawn from the one box simultaneously with the three highest from the other, each to each, if the matter had not been prearranged? Indeed, we might legitimately call the coincidence of April, 1882, a double one, and ask the odds against the four highest numbers from each box being so drawn.

Between sun spots and storms of the second magnitude it is more difficult to make a satisfactory comparison, because it is not so easy to frame a satisfactory definition as to what constitutes a secondary disturbance. Nevertheless, the following brief table of large sun spots seen since the beginning of 1881, which were coincident with considerable disturbances, may prove of interest. The spotted area is given in millions of square miles:

Date.	Spotted area. Entire sun.	Spotted area. Largest group.	Date.	Spotted area. Entire sun.	Spotted area. Largest group.
1881—January 31	1,295	686	1883—November 1	2,100	784
September 12	2,089	917	November 19	3,682	1,600
1882—October 2	2,480	1,234	1884—March 2	1,510	609
October 5	2,065	1,198	April 24	2,348	1,510
1883—April 3	1,545	607	April 30	1,746	897
April 19	2,170	670	1885—January 23	1,687	592
June 30	3,650	2,210	February 5	1,345	571
July 11	1,887	1,009	February 13	1,569	480
July 29	1,425	1,264	May 26*	1,923	647
September 17	2,017	1,263	June 24*	2,348	1,681
October 16	4,730	1,733	July 18	1,835	504
October 20	1,650	1,369	1891—November 22*	1,966	1,371

Some of the above, those marked with an asterisk, may fairly be taken as confirming, though with less definiteness, the conclusion drawn from the correspondences between the greatest spots and the greatest storms. But with the others it is not so. Spots as important have been seen upon the sun, and the magnets have scarcely fluttered, and storms as distinct have occurred when there have been only few spots, and those but small, upon the visible disk of the sun. The table is important, therefore, not as adding to the weight of the evidence in favor of the connection between sun spots and magnetic disturbances, but as emphasizing a point which must not be forgotten. Though the diurnal and annual changes of terrestrial magnetism conclusively prove the solar influence upon it, though the conclusion between the general sun-spot cycle and the general magnetic cyle is clearly established, though even in minor irregularities the two curves closely correspond, and though unusually large sun spots are answered by unusually violent magnetic storms, we can not, as yet, proceed further and express the magnitude or character of the magnetic disturbances in terms of the spotted area of the sun or of its principal groups at the time of observation. The conclusion to my own mind seems to be that though sun spots are the particular solar phenomenon most easily observed, we must not infer, therefore, that their number and extent afford the truest indication of the changes in the solar activity which produce the perturbations we remark in our magnetic needles.

Solar prominences.—Especial attention has been given to the photography of solar prominences by Prof. G. E. Hale of the Kenwood observatory, Chicago, and by M. Deslandres of the Paris observatory. Prof. Hale suggested two plans for the purpose; the first was to allow the image of the sun to drift across the radial slit of a powerful spectroscope, the driving clock of the telescope being slowed to produce the drift. If then there were a prominence on the sun's limb the length of any bright line at the focus of the spectroscope would define the height of the prominence, and as the sun drifted across the slit this line would continually change in length. If now the line in use were made to pass through a slit just within the focus of the observing telescope of the spectroscope called the "second slit," so as to be in focus on a plate beyond the slit, all that is required to photograph the prominence is to move the plate slowly at right angle to the second slit. Fresh portions of the plate are thus exposed to corresponding portions of the prominence, and the prominence image is built up from a succession of bright line images of the slit. In the second method proposed, the clock of the equatorial is so adjusted that the image of the sun is kept in a fixed position. The plate on the end of the collimator which carries the slit, is then slowly moved across the sun's limb at the point where the prominence is present, and a second slit moving at the same speed before a stationary plate excludes the light from the spectrum on either side of the line in use, and reduces fogging to a minimum.

In April, 1891, Mr. Hale secured the first photograph of the spectrum of a prominence obtained without an eclipse. This showed two very strong, bright lines nearly at the centers of the dark solar bands H and K. The same lines were photographed on subseqent occasions, but it was not until June 23, that any new lines were discovered. On this occasion four lines were obtained in the ultra-violet—a number since increased to six. Of these six, 5 lines belong, unmistakably, to the series of hydrogen lines discovered by Dr. Huggins, in the ultra-violet of the spectra of Sirian stars. The sixth line forms a close double with one of these hydrogen lines (α) but its origin has not yet been accounted for.

Mr. Hale's conclusion that H and K are not due to hydrogen, is abundantly confirmed by Prof. Young and also by M. Deslandres, since the measures have shown beyond a doubt that the "companion line to H," and not H itself, is the one really due to hydrogen. Mr. Hale and M. Deslandres ascribe these two giant bands of the solar spectrum to calcium.

Mr. Hale has also met with considerable success in photographing the forms of solar prominences, some of the photographs showing a satisfactory amount of detail. In one instance a prominence photographed at Kenwood was being sketched by Herr Fényi at Kalocsa at the same moment of time, and drawing and photograph are in close accord. A suggestion by M. Deslandres that it might be possible to photograph the entire chromosphere at a single exposure has been carried into effect by Mr. Hale, by means of a "spectroheliograph," in which the slit of the spectroscope is made to travel across the image of the sun, and a precisely similar motion, but in an opposite direction, is given to a second slit nearly in the focus of the view telescope, and so arranged that the K line of the spectrum of the fourth order falls upon it. Since the K line is always bright in the spectrum of the chromosphere and prominences. it is easy, by shutting off the image of the sun by means of a diaphragm, to build up a complete picture of the entire chromosphere and prominences, and so to produce what may be described as an "artificial total solar eclipse." The discovery which Mr. Hale has made that the H and K lines are always reversed in the faculæ has enabled him to extend the application of this principle. If the diaphragm covering the image of the sun be discarded a photograph will be obtained, not merely of the chromosphere and prominences, but of the disc of the sun itself, showing the spots and the faculae, the latter being depicted, not merely when near the limb of the sun, but wherever they occur, even in the very center of the disc. In this manner it has been discovered that faculae, invisible to the eye frequently float above the spots, and one series of photographs in particular, show how, on July 15, a luminous outburst formed, spread, completely hid a large group of spots, and passed away, all in a few minutes of time.

The *double* reversal of the H and K lines from faculae, a phenomenon shown upon photographs taken at Kenwood, Paris, and Stonyhurst, is a discovery of special interest as bearing upon the interpretation of the enigmatical spectrum of *Nova Aurigæ*, and Prof. Hale has supplemented this discovery by obtaining a similar result with an integrating spectroscope, the sun being treated as a star would be, its light as a whole, and not only from special regions of the disc, being subjected to examination.

M. Deslandres has been making further experiments upon photographing the corona without an eclipse. The principle upon which he proceeds is to obtain photographs of the sun from light of limited refrangibility, not by using colored media or stained plates, but by means of two prisms, the second of which is arranged so as to recompose the light dispersed by the first. But only certain rays from the first prism are allowed to fall on the second: the resulting image of the sun is, therefore, confined to those rays which can be selected at pleasure. M. Deslandres' purpose is, therefore, to find out for what rays the corona has the greatest brightness as compared with that of the sun, and to photograph the sun and its surroundings by their aid alone. (*See* Month. Not., 52:292; 53:277.)

ECLIPSES.

Eclipse of the moon, 1888, January 28.—In number 23 of the Publications of the Astronomical Society of the Pacific is an unusually satisfactory drawing by Prof. Weinek, showing the delicate shades of color exhibited by the eclipsed moon.

Eclipse of the sun, 1889, January 1.—Prof. Pritchett's report of the Washington University party, which was stationed at Norman, Cal., is illustrated by an excellent artotype, a composite reproduced by hand from four negatives. The evidence given by these photographs upon the structure of the corona is thus summarized "The marked structural features of the corona are (*a*) the so-called filaments, and (*b*) the streamers extending approximately in the direction of the ecliptic. The filaments extend over a region of 20 degrees or more on each side of the poles. They are straight lines of light arranged somewhat like the spines of a fan, and are not radial. The dark spaces between them are not entirely free of coronal matter, but can be traced in some cases to within a short distance of the sun's limb. The broad and strongly marked equatorial belt stretches directly across this mass of filaments, apparently cutting off the filaments at the somewhat irregular line of separation. The impression conveyed to the eye is that the equatorial stream of denser coronal matter extends across and through the filaments, simply obscuring them by its greater brightness. There is nothing in the photographs to prove that the filaments do not exist all round the sun.

Eclipses of 1891 and 1892.—In the year 1891 there were two eclipses of the sun, an annular eclipse on June 6, and a partial eclipse on November 30–December 1; and two eclipses of the moon, May 23 and November 15, both total.

In 1892 there were also four eclipses, two of the sun and two of the moon: a total eclipse of the sun April 26, and a partial eclipse of the sun October 20; a partial eclipse of the moon May 11 and a total eclipse November 4.

Eclipse of the moon, 1891, May 23.—A total eclipse, visible throughout the western part of the Pacific Ocean, Australia, Asia, Africa, and Europe. No observations of special importance.

Eclipse of the sun, 1891, June 6.—Visible as an annular eclipse only in the northern part of Siberia and the Arctic Ocean. A few observations of contacts were secured in the western part of the United States.

Eclipse of the moon, 1891, November 15.—The total eclipse of the moon on November 15, 1891, was visible generally throughout North and South America, Europe, Asia, and Africa. The whole of the eclipse was visible in the eastern and central parts of the United States while in the western part the moon rose eclipsed. Dr. Döllen selected from photographic plates made at Potsdam some 138 stars to be occulted at established observatories, but the weather seems to have been generally unfavorable. A few contact observations were secured.

Eclipse of the sun, 1891, November 30.—A partial eclipse, visible only in the Antarctic ocean.

Eclipse of the sun, 1892, April 26.—Total eclipse, visible only in the Southern Pacific: no observations of importance reported.

Eclipse of the moon, 1892, May 11.—The phenomenon of the partial eclipse of the moon on May 11, 1892, was studied at Greenwich and elsewhere, and the occultation of a considerable number of small stars was observed.

Eclipse of the sun, 1892, October 20.—Partial eclipse, visible in North America; a few observations of contacts reported.

Eclipse of the moon, 1892, November 4.—Total eclipse visible generally in Europe and America. No observations of special importance.

SOLAR PARALLAX AND THE TRANSITS OF VENUS.

The United States transit of Venus observations.—In a report dated September 21, 1891, the Superintendent of the United States Naval Observatory states that no provision has yet been made for publishing in detail the work of the American parties upon the transit of Venus in 1882—a fact greatly to be regretted. The publication of the work upon the 1874 transit is only partly completed and considerable work still remains to be done upon the reductions for 1882, though results for the solar parallax and certain elements of the orbit of Venus, which are practically final, have been published. Some occultations of stars by the moon, telegrapic determinations of differences of longitude, tidal observations, and pendulum experiments still remain to be reduced, for which, however, no funds seem to be available.

Dr. Auwers' result* for the German heliometer measures of the transit of Venus in 1874 is a solar parallax of $8''.877 \pm 0''.043$, there being in all 308 measures at four different stations; in 1882 four stations were occupied and 446 measures were obtained, the resulting parallax being $8''.879 \pm 0''.037$.

Dr. Battermann, of the Berlin Observatory, has deduced a value of the solar parallax from 250 occultations of stars between April, 1884, and October, 1885, having by careful observation been able to utilize the occultations of a considerable number of faint stars near new moon. The resulting solar parallax is $8''.794 \pm 0''.016$.

The determination of the solar parallax by means of meridian observations of Mars at opposition was attempted in 1862, and again in in 1877, but the results obtained were generally considered by astronomers as too large, there being indications of a systematic error in the observations of Mars, or of the comparison stars, or of both. A slight modification of the previous methods of observation was suggested by Prof. Eastman, and a circular was issued by the U. S. Naval Observatory requesting the coöperation of other observatories in the observation of Mars during the opposition in the summer of 1892.

PLANETS.

MERCURY: *Diameter of Mercury.*—A new determination of the diameter of Mercury has been made by Mr. Ambronn from heliometer observations at Göttingen, the mean result being $6''.580$, comparing favorably with the generally adopted value.

Transit of Mercury, May 9, 1891.—A transit of Mercury over the sun's disk took place on May 9, 1891, the first since November 7, 1881. The observation of these transits no longer possesses special importance, as the determination of the solar parallax, for which they are theoretically valuable, can now be made more accurately by other means. Observations of the contacts between the disks of the sun and planet are useful in determinations of the planet's orbit and the physical phenomena are sometimes of interest. The transit on May 9, 1891, was only

* Astron. Nachr. 3066.

partially visible in the United States. On the Pacific coast the sun was two or three hours high at the time of the first and second contacts; it had set in most places on the Atlantic coast before the first contact, and in Washington was only ten minutes high. Reports from twenty-five observers in the United States have been forwarded to the Naval Observatory for reduction. The whole transit was visible in China, Japan, eastern Siberia, and the Malaysian Islands, while in England egress took place soon after sunrise. No phenomena of special importance seem to have been noted. In Europe several observers saw the "black drop" or ligament. At the Lick Observatory a careful series of observations was made, both visual and photographic, and the planet was looked for, but without success, before it entered upon the sun's disk.

For more than an hour after ingress the planet was also carefully examined, with the 36-inch Lick telescope, by Profs. Holden and Keeler. It "was perfectly round, and in the best moments sharply terminated . . . . Not the slightest trace of a satellite was seen: and both observers were confident that no such body could then be on the sun's face and escape detection unless it were exceedingly minute."

VENUS.—The conclusion reached by Schiaparelli that Venus rotates very slowly upon its axis, in fact in about the same time that it rotates about the sun, has been challenged by several observers. MM. Niesten and Stuyvaert, of the Brussels Observatory, have given the matter careful study, and M. Trouvelot has published a series of observations and sketches from 1876 to 1891, from which he concludes that the rotation does not differ greatly from twenty-four hours.

An exhaustive discussion of recent publications concerning the physical appearance of Venus is printed by Dr. Wislicenus in the *Vierteljahrsschrift*, v. 27, pp. 271–302. It is quite evident that further accurate observations are necessary.

The value of the diameter of Venus, deduced by Dr. Auwers from the heliometer measures by the German Transit of Venus parties, in 1874 and 1882, is 16″.80.

THE EARTH: *Variation of terrestrial latitude.*—One of the most important subjects that has been under discussion during the past two years—important to astronomy and geodesy alike—is the variation of terrestrial latitudes, the strong suspicion of which has been confirmed by recent very accurate observations, and when once admitted is abundantly fortified by the discussion of older observations.

There seems to be now distinct evidence of a rotation of the geographical round the astronomical pole in 427 days. The problem has of course attracted the attention of the ablest astronomers and mathematicians, but the credit for the ablest discussion and the most satisfactory solution is undoubtedly due to Mr. S. C. Chandler. The following summary of his work is taken from a review in the *Monthly Notices* (v. 53, No. 4).

Mr. Chandler's observations with the almucantar, in 1884 and 1885, first suggested to him, not only the possibility of a variation in latitude, but the law of the variation. Twelve months' observations were of course not sufficient to establish a periodicity of fourteen months, though they might suggest it; confirmation was, however, furnished by Dr. Küstner, who, in his determination of the aberration from a series of observations coincident in time with those of the almucantar, came upon similar anomalies. Further evidence bearing on the question was forthcoming in the parallel determinations at Berlin, Prague, Potsdam, and Pulkowa, which showed changes in apparent latitude, not only strikingly sympathetic among themselves, but of the same range and periodicity as those noticed in 1885; and Mr. Chandler "was led to make further investigations on the subject, which seem to establish the nature of the law of these changes, and proceeded (1891, November) to present them in due order."

The consequent series of papers in the *Astronomical Journal* can hardly fail to take its place as one of the astronomical classics. The following summary is made purposely very brief because the series is not yet complete, and no doubt much still remains to be said on such an important subject. But it will be seen that during the year 1892 (including perhaps the end of 1891) a most important advance has been made in fundamental astronomy.

The first paper (regarding that already mentioned as preliminary) deals with the observations with the Pulkowa vertical circle (1865–1875), "which have been provocative of so much inquiry, so far without any solution of the anomalies which they show in regard to the question of latitude variation," and the Washington prime vertical observations (1862–1867), "the most accurate determinations of declination ever made at the Naval Observatory," which yet "resulted in anomalous values of the aberration constant in the different years and a negative parallax in all." Mr. Chandler finds that a 427-day period in the latitude "furnishes the true key to the troublesome discordances in the Pulkowa latitudes," and "traces to their origin the anomalies in the Washington observations." Further, the comparison of the two series leads to the same conclusion as that already shown from the simultaneous series at Berlin and Cambridge (United States) in 1885 as to the direction of the polar motion. In the next paper it is mentioned that observations at Melbourne (1863–1881) and Leyden (1863–1867) are in complete accordance with those made at the same time at Pulkowa and Washington; and that the 427-day period accounts for the contradictory results obtained by Dr. Van Hennekeler at Leyden. The motion of the earth's pole for the period 1860–1870 is thus fairly established. The author proceeds to consider earlier observations. In the papers numbered 3 and 4, the systematic errors of Bradley's observations, which were reduced afresh for this purpose, are discussed, particularly the collimation error. It is then concluded that the observations indicate a rotation of the pole in little more than a year and with a larger radius than that of 1860–1880, the range being about 1″. In the same paper Mr. Chandler states that Brinkley's observations at Dublin (1808–1813 and 1818–1822) are found to indicate a rotation in about a year, with range more than 1″, "wherein lies the solution of the hitherto unsolved enigma of Brinkley's singular results which led to the spirited and almost acrimonious dispute between Brinkley and Pond with regard to stellar parallaxes." The details were promised in a later paper, but have not yet been given, owing doubtless to the necessity of attending to a vitally important point which will presently appear.

In papers 5 and 6 are presented the results of an enormous mass of reductions extending from 1837 to 1891, made at no fewer than seventeen observatories. The whole is broken up into forty-five series, or short groups, for the purposes of this particular discussion; and the result of this minute inquiry, confirmed (or perhaps suggested) by the observations of Bradley and Brinkley above mentioned, seemed clear, viz, that the "instantaneous rate of angular motion of the pole has been diminishing during the last half century at a sensibly uniform rate, by its one-hundred-thousandth part."

Mr. Chandler was led to modify this statement in a remarkable manner and within a few weeks.

Astronomers had hesitated to accept the 427-day period, even in face of the very strong evidence of the 1860–1880 observations, owing to the difficulty in accounting for it theoretically. It had been pointed out by Euler that, treating the earth as a rigid body, the period of rotation of the pole must be 306 days. Prof. Newcomb, however, happily pointed out that a qualified rigidity (either actual viscosity or the composite character due to the ocean) afforded an explanation of this longer period; and after this suggestion Mr. Chandler's 427-day period was well and even warmly received. But the further elaboration of this hypothesis by a changing period was a new difficulty.

Prof. Newcomb, who had reconciled the first article of the hypothesis with theory, was not slow to declare that the second was irreconcilable. Mr. Chandler's reply, in paper 6, is a model of controversial courtesy and skill. He says: "It should first be said that in beginning these investigations I deliberately put aside all teachings of theory, because it seemed to me high time that the facts should be examined by a purely inductive process: that the nugatory results of all attempts to detect the existence of the Eulerian period probably arose from a defect of the theory itself; and that the entangled condition of the whole subject required that it should be examined afresh by processes unfettered by any preconceived notions whatever. . . . The appeal to observation, treated irrespective of theory in the present series of papers, shows that a rotation of the pole really exists, but (*a*) at a daily rate of but 0°.85 (for 1875), and (*b*) that this velocity is subject to a slow retardation, which in its turn is not uniform . . . The result (*a*) was at first pronounced impossible, and it is even now so regarded in some quarters. Prof. Newcomb, however, soon after found the defect in the theory, and is now as cordially in favor of the result given by observation as he was originally against it. . . . Now, may it not reasonably be asked, if the direct deduction from observation has led to the correction of the theory in the first particular, is it beyond hope that it may do so in regard to the second?"

Such a truly scientific attitude inspires confidence that the search is being rightly conducted; but the most sanguine could hardly be prepared for the reconciliation of observation and theory in the very next paper of the series, published six weeks later.

By this time Mr. Chandler had rearranged his material, and found, not *one variable* rotation of the pole, but *two constant* rotations (with a qualification), one in 427 days and the other in about a year. The qualification is that the *amplitude* of the latter is apparently variable, not the period. The superposition of these two rotations is almost exactly equivalent, for the observations available, to the law (or summary of observation, as it might fairly be called) previously announced. To make clear the novelty of this discovery it may be remarked that, although fluctuations in zenith distances of annual period have long been recognized, they have generally been ascribed to temperature effects, in which case the maxima and minima for all stations in the Northern Hemisphere should occur at the same epoch—say, midsummer and midwinter. But this is not the case with the annual term now revealed. The epoch changes with the longitude, showing that the pole moves just as in the case of the 427-day term.

It is somewhat remarkable that two formulæ differing so much in form should be found to represent the observations almost equally well. Apparently this is to be attributed chiefly to the variability in amplitude of the annual term, and as yet this variability has not been accounted for. But to have advanced the work to this stage in such a short time is a great achievement, and much may confidently be expected from Mr. Chandler's future work. He points out, in a paper dated January 2, 1893, that the discovery of these periodic inequalities in the latitude makes it necessary to go over much old work afresh, and is himself leading the way with a discussion of the aberration constant.

In the same paper he shows that the recent results obtained at Berlin, Prague, Strasburg, Pulkowa, Rockville, and Honolulu give a mean correction to his final formula of only five days in the epoch; "and the accordance of the separate values is high testimony to the skill of the observers, to whom astronomers owe a deep debt of gratitude for their laborious and conscientious work."

Standard time.—In reviewing the recent progress made in the introduction of uniform standards of time M. Pasquier states that in Canada Parliament has declared as legal the normal hours from Greenwich adopted since 1883 by railways and later by a great number of towns. In England a commission has reported favorably upon the system of hourly meridians, and the Government has strongly recommended it to the colonies. In France, Paris mean time is used for all the country, including Algeria. In Belgium a commission has recommended the hourly meridian system, with Greenwich as the starting point. In Holland the Government has authorized the adoption of Greenwich time for interior railway service. In Prussia mean European time (mitteleuropäische Zeit or M. E. Z.) which is one hour greater than Greenwich time, replaced Berlin time from the 1st of June, 1891, for the railway service of the interior. Bavaria, Würtemberg, and Baden have also decided on M. E. Z., which will also be used in Alsace-Lorraine. Austria and Hungary adopted M. E. Z. from October 1, 1891, for railway, post, and telegraph service, and there is a strong feeling for its adoption in civil life. In Italy, at the instance of the Academy of Sciences of Bologna, which favors the meridian of Jerusalem, there was a plan for assembling a new congress at Rome, but this however, seems to have been abandoned. There is here as well as in Switzerland a strong sentiment in favor of Greenwich as the standard.

At the Cape of Good Hope the extension of railways brought about the adoption of a single-standard time throughout the colony in February, 1892. The meridian one and one-half hours east of Greenwich is in use for all purposes in Cape Colony and the Orange Free State, and all time signals are given at Greenwich noon.

Sky glows.—The after-glows that attracted so much attention in 1883 and 1884 showed some signs of return, though in lesser degree, in the early part of 1891. The tint and general appearance are reported to have greatly resembled the auroral displays.

The systematic study of these "luminous night clouds" has been taken up by Prof. Foerster and Herr Jesse, of the Berlin observatory.

The Moon.—A valuable contribution has been made to the study of the moon in "An essay on the distribution of the moon's heat and its variation with the phase," by Mr. F. W. Very, of the Allegheny Observatory—a paper which gained the prize proposed in July, 1890, by the Utrecht Society of Arts and Sciences. Mr. Very's investigation was made with one of Langley's "bolometers," and the principal results may best be described in the author's own words:

First, that visible rays form a much larger proportion of the total radiation at the full than at the partial phases, the maximum for light being much more pronounced than that for the heat. Next, as has been foreseen from the eccentricity of the heat areas, their greater extension toward the western limb, and the greater steepness of the sunset than of the sunrise gradient, the diminution of the heat from the full to the third quarter is slower than its increase from the first quarter to the full. Finally, there is a fair agreement between these results and those of Lord Rosse, which extends even to some minor details, such as the attainment of the highest heat at little before the full.

In a discussion of the moon's atmosphere Mr. Ranyard expresses the opinion that the moon can not have an atmosphere one two-thousandth part as dense as that of the earth at sea level. It must, however, be remembered that, were our atmosphere transferred to the moon, its density would only be one-sixth what it is on the earth.

Professor Weinek, of Prague, who has been making a special study of the Lick photographs of the moon, has detected several new rills and craters on the negatives.

MARS.—Mars was in opposition to the sun on August 3, 1892, and though the planet was also at this time very favorably situated as regards its proximity to the earth, its great southern declination was a serious impediment to observation in the northern hemisphere. At the Harvard observatory station, Arequipa, Peru, the planet was, however, almost in the zenith, and full advantage was taken of this by Prof. W. H. Pickering and his assistants. Many of Schiaparelli's canals were identified: some were seen double, and marked changes were detected in progress in various parts of the planet, especially in the neighborhood of the *Lacus solis* or Terby sea. Prof. Pickering and other observers detected a number of bright white spots besides the polar snow cap.

In an article in the March number of *L'Astronomie*, 1891, Flammarion describes various changes in the topography of Mars, the most striking of which are also in connection with the Terby sea. Drawings are given of its appearance in 1877, 1879, 1881, and 1890; briefly, it seems to have undergone cleavage, and while some former "affluent canals" have disappeared, other new ones have developed. The strait called Herschel II has been transformed into a straight double canal.

JUPITER: *Discovery of a fifth satellite.*—The most interesting astronomical event of the year 1892 was the discovery by Barnard, with the 36-inch Lick equatorial, on September 9, of a fifth satellite of the planet Jupiter.

Following is Prof. Barnard's own account of his discovery, in describing his search for new objects in the Astronomical Journal:

Nothing of special importance was encountered until the night of September 9, when, in carefully examining the immediate region of the planet Jupiter, I detected an exceedingly small star close to the planet and near the third satellite. I at once measured the distance and position angle with reference to satellite III. I then tried to get measures referred to Jupiter, but found that one of the wires had got broken, and the other loosened. Before anything further could be done the object disappeared in the glare about Jupiter. Though I was positive the object was a new satellite, I had only the one set of measures, which was hardly proof enough for announcement.

I replaced the wires the next morning. The next night with the great telescope, being Prof. Schaeberle's, he very kindly gave the instrument up to me, and I had the pleasure of verifying the discovery, and secured a good set of measures at elongation.

Just what the magnitude of the satellite is it is at present quite impossible to tell. Taking into consideration its position, however, in the glare of Jupiter, it would perhaps not be fainter than the thirteenth magnitude.

The satellite has been seen and its position observed at the University of Virginia, at Princeton, at Ealing, and at Evanston; the 18½ inch refractor at Evanston being apparently the smallest instrument with which it has thus far been seen, and it was then reported as being a much more difficult object than Ariel or Umbriel, the satellites of Uranus, though Mr. Reed with the 23-inch Princeton glass found it an easier object than Ariel.

The new satellite's orbit seems to lie sensibly in the plane of Jupiter's equator; the distance of the satellite from the center of the planet is probably over 110,000 miles and its period of rotation about $11^h\,57^m\,37^s$.

Diameter of Jupiter.—An admirable series of measures of the diameter of Jupiter, by Dr. Schur, with the Göttingen heliometer, is published in No. 3073 of the *Astronomische Nachrichten.* The effect of personal equation was eliminated by the use of a reversion prism eyepiece. Dr. Schur finds the disk a sensibly true ellipse with diameters 37″.4 and 35″.0, a flattening of 1–15½.

Mr. Burnham communicated to the November meeting of the Royal Astronomical Society, in 1891, a paper on the spots and markings of Jupiter as observed with the 12-inch equatorial of the Lick Observatory. Noting the decided changes of color in the different markings on the planet's surface, he expresses the opinion that the red color is an indication of age, or, in other words, when a spot or marking other than the white spots first appears it is dark or black, but after some time turns red. During the year 1891 the planet was extremely interesting, owing to the remarkable amount and variety of detail displayed on its surface. The two hemispheres were, as usual, strongly in contrast in their individual markings. In the southern hemisphere, besides the great red spot, new spots appeared, and a great number of round white spots were visible. These white spots are quite characteristic of the southern hemisphere, though individual white spots have at rare intervals been seen in the northern hemisphere. In the latter a system of small dark spots appeared, with very short periods of rotation. Mr. Burnham reports that the great red spot had regained much of its former distinctness, both in color and form.

SATURN.—On September 22, 1891, the earth passed through the plane of Saturn's rings. From the 22d of the month to October 30 the earth was above the plane of the rings, while the sun was below that plane and, consequently, shining on the southern side of the rings. After October 30 the sun was again shining on the north side. The phenomenon of the disappearance of the rings was described by several observers.

URANUS.—A search for new satellites made by several observers at the Lick Observatory from 1889 to 1891 has resulted negatively. The observers were satisfied "that no new satellite half as bright as Ariel at elongation exists within the orbit of Umbriel. It is not likely that any such object exists within the orbit of Titania."

NEPTUNE.—Mr. Asaph Hall, jr., finds that observations of the satellite with the 26-inch Washington refractor from October, 1891, to March, 1892, confirm the reality of the slow motion, nearly proportional to the time, of the orbit plane of the satellite with respect to the orbit of Neptune, to which Mr. Marth called attention and which M. Tisserand shows may result from a slight flattening of the planet.

MINOR PLANETS.

Asteroids of 1891.—Of the asteroids announced in the last report, No. 286 has been named Iclea, 296 Phaëtusa, 297 Caecilia, 298 Baptistina, 299 Thora, 300 Geraldina, 301 Bavaria. An asteroid, discovered on November 14, 1890, by Charlois, at Nice, and subsequently found to be a new one, has been named Clarissa. To No. 323, photographed by Dr. Wolf November 28 (the first asteroid discovered by photography), he has given the name Brucia, in honor of Miss Catherine W. Bruce, who has contributed so generously for the advancement of astronomy.

In 1891 twenty-two new asteroids were added to the group revolving between Mars and Jupiter, and photography now having become a powerful aid in the detection of these small bodies, the number still likely to be found seems almost limitless. The last on the list for 1891, No. 323, was discovered by Dr. Wolf upon his photographic plates. Another asteroid was, in fact, found upon the same plate, but it proved to be identical with 275 (Sapientia).

Palisa, on August 14, 1891, discovered what was for a time supposed to be a new planet, but it was found to be identical with 149, Medusa, discovered in 1875; 275, which had not been seen since the opposition of its discovery (1888), has been found again by the aid of photography; 318 is interesting from the almost exact commensurability of its period with that of Jupiter.

No. 302, Clarissa (discovered November 14, 1890), was not included in the last list published, and is, therefore, introduced here to make the lists complete.

List of minor planets of 1891.

Number.	Name.	Discoverer.	Date of discovery.
			1890.
302	Clarissa	Charlois at Nice	Nov. 14.
			1891.
303	Josephina	Millosevich, at Rome	Feb. 12
304	Olga	Palisa, at Vienna	Feb. 14
305	Gordonia	Charlois, at Nice	Feb. 16
306	Unitas	Millosevich, at Rome	Mar. 1
307	Nike	Charlois, at Nice	Mar. 5
308	Polyxo	Borelly, at Marseilles	Mar. 31
309	Fraternitas	Palisa, at Vienna	Apr. 6

List of minor planets of 1891—Continued.

Number.	Name.	Discoverer.	Date of discovery.
			1890.
310	Margarita	Charlois, at Nice	May 16
311	Claudia	do	June 11
312	Pierretta	do	Aug. 28
313	Chaldaea	Palisa, at Vienna	Aug. 30
314		Charlois, at Nice	Sept. 1
315	Constantia	Palisa, at Vienna	Sept. 4
316		Charlois, at Nice	Sept. 8
317	Roxane	do	Sept. 11
318		do	Sept. 24
319		do	Oct. 8
320	Katharina	Palisa, at Vienna	Oct. 11
321		do	Oct. 15
322	Phaeo	Borelly, at Marseilles	Nov. 27
323	Brucia	Wolf, at Heidelberg	Nov. 28

Asteroids of 1892.—The further and very successful application of photography to the discovery of asteroids by Dr. Wolf, at Heidelberg, and by M. Charlois, at Nice, resulted in such rapid additions to the list that the notation of these bodies was thrown into the utmost confusion. Hitherto the simple numbering in the order of discovery had been a rule easily applied by the discoverer, but where several asteroids were found upon a single photographic plate it was not always possible to determine until later observations and computations whether they were really new asteroids or not, and when the planetary character of the object was recognized it was frequently found imprinted upon some earlier photograph.

It was accordingly suggested, in No. 266 of the *Astronomical Journal*, that as a temporary omission of the number is attended with less inconvenience than is caused by the employment of an erroneous one, the numbers for the asteroids after number 322 should be omitted until the difficult task of fixing a definite enumeration should be delegated by common consent to some one authority to which all could defer.

Common consent seemed to point to the Berlin Rechen-Institut as the only place actually in possession of the needful resources for solving the questions of identity continually arising, and it was agreed that to avoid further confusion Prof. Krueger, director of the Kiel observatory, the European "Central-Stelle," and editor of the *Astronomische Nachrichten*, should assign to each asteroid a provisional notation (1892 A, 1892 B, 1892 C, etc.) in the order of its announcement to the "Telegraphische Central-Stelle;" and that the definitive numeration should be subsequently undertaken by Prof. Tietjen, director of the Rechen-Institut, in Berlin. In this definitive assignment of numbers those asteroids will be omitted, for which sufficient material is not available for a determination of the orbits.

The first asteroid to which the new notation was assigned was that discovered by Wolf, at Heidelberg, on August 22, 1892; it was provisionally known as 1892 A, and subsequently received its more permanent designation, number 333, from Prof. Tietjen, and its name, Badenia, from the discover.

In 1892 thirty new asteroids were announced; one of these, 1892 B, has already proved to be identical with Erigone, 163; and it is possible that further study will identify some of the others.

Some slight discrepancies are still found in the different lists of asteroids for the year, but as the new system of notation becomes established they will probably disappear.

In the following list all the discoveries were by photography except 324, 326, 327, and 331. In the case of the photographic discoveries the date given is that of the earliest photograph on which the planet appears. It was in many cases not noticed on the plate till considerably later, which accounts for the departures from chronological order in the "date of discovery":

List of minor planets of 1892.

Letter.	Number.	Name.	Discoverer.	Date of discovery.
......	324	......	Palisa, at Vienna	Feb. 25
......	325	Heidelberga	Wolf, at Heidelberg	Mar. 4
......	326	Tamara	Palisa, at Vienna	Mar. 19
......	327	Columbia	Charlois, at Nice	Mar. 22
......	328	Gudrun	Wolf, at Heidelberg	Mar. 18
......	329	Svea	......do	Mar. 21
......	330	Ilmatar	......do	Mar. 19
......	331	......	Charlois, at Nice	Apr. 1
......	332	Siri	Wolf, at Heidelberg	Mar. 19
1892 A	333	Badenia	......do	Aug. 22
1892 B	163	Erigone	......do	Sept. 1
1892 C	335	Roberta	Staus, at Heidelberg	Sept. 1
1892 D	336	......	Charlois, at Nice	Sept. 19
1892 E	337	......	......do	Sept. 22
1892 F	338	......	......do	Sept. 25
1892 G	339	Dorothea	Wolf, at Heidelberg	Sept. 25
1892 H	340	......	......do	Sept. 25
1892 J	341	......	......do	Sept. 25
1892 K	342	......	......do	Oct. 17
1892 L	334	......	......do	Aug. 23
1892 M	344	......	Charlois, at Nice	Nov. 15
1892 N	343	......	Wolf, at Heidelberg	Nov. 15
1892 O	......	......	Charlois, at Nice	Nov. 23
1892 P	......	......	......do	Nov. 25
1892 Q	......	......	......do	Nov. 28
1892 R	......	......	......do	Nov. 28
1892 S	......	......	......do	Dec. 8
1892 T	......	......	......do	Dec. 9
1892 U	......	......	......do	Dec. 14
1892 V	......	......	Wolf, at Heidelberg	Dec. 16

OBSERVATORIES.

The chief sources of information concerning the recent work of astronomical observatories are the *Vierteljahrsschrift der Astronomischen Gesellschaft*, for continental and a few American observatories, and the *Monthly Notices* for English observatories; in addition to these are the reports of the observatories themselves—though few publish independent annual reports—and notes in current journals, chiefly the *Observatory* and the *Sidereal Messenger.*

In the following résumé the length of the notes is by no means always in proportion to the importance of the institution. The character of the work of the older observatories is generally too well known to require more than the briefest mention, while for the new observatories an effort is made to put on record as much information with regard to the equipment, etc., as can be found. In most instances it has not seemed necessary to distinguish between the two years covered by the review.

Among papers of interest on observatories is a series of notes on visits to some American observatories made by Mr. H. F. Newall, of the Cambridge Observatory, England, and published in the *Observatory.*

ABASTUMAN.—A new mountain observatory has been established at Abastuman ($2^h\ 51^m\ 25^s$ E, $+\ 41°\ 42'.4$). It is 4,500 feet above sea level and is equipped with a 9-inch telescope by Repsold.

ADELAIDE: *Todd.*—Reobservation of Weisse stars; observations of Jupiter; weather service.

ALABAMA UNIVERSITY.—An astronomical observatory attached to the University of Alabama, near Tuscaloosa, was completed in the summer of 1844. The building was originally 54 feet in length by 22 in breadth in the center. In 1858 another apartment, 40 feet in length by 20 in width, was added to the east wing. The instruments consist of a 4-inch transit circle of 5-feet focus by Simms, the circle being 3 feet in diameter, divided to five minutes, and read by four microscopes to single seconds; a clock by Molyneaux; an equatorial, also by Simms, of 8 inches aperture and 12 feet focus, provided with a filar micrometer and double image micrometer, the hour circle being divided to one second of time and the declination circle to five seconds of arc, read by opposite verniers. As an accessory to the equatorial there is an excellent clock by Dent. There are also two portable achromatic telescopes—one by Dolland of 7 feet focal length and 4 inches aperture, the other by Simms of 5 feet focal length and 3 inches aperture—and reflecting circle by Troughton, of 10 inches aperture, read by three verniers to twenty seconds.

The observatory was built and the instruments purchased and mounted under the supervision of Prof. F. A. P. Barnard. A woodcut of the building is given in number 15 of the publications of the Astronomical Society of the Pacific, taken from the report of the United States Commissioner of Education for 1889.

ALLEGHENY: *Keeler.*—At a meeting of the board of trustees of the Western University of Pennsylvania on May 11, 1891, J. E. Keeler, of the Lick Observatory, was elected professor of astrophysics in the university and director of the Allegheny Observatory, Mr. F. W. Very being associated with him as adjunct professor of astronomy.

Through the generosity of Mrs. William Thaw the observatory has been provided with a very powerful spectroscope by Brashear; a new driving clock was presented by Mr. William Thaw, jr.

ARMAGH: *Dreyer.*—Micrometric measures of nebulæ; physical observations of Jupiter.

ATHENS: *Eginitis.*—The National Astronomical and Meteorological Observatory at Athens has been reorganized under the directorship of Prof. Eginitis.

BAMBERG: *Hartwig.*—The large heliometer of 184 millimeters aperture has been brought into regular use. Observations of variable stars and of a few occultations have been made, besides observations for the determination of change of latitude.

BASEL: *Riggenbach.*—Instruction of students.

BERLIN: *Foerster.*—Transit circle observations, measures of double stars, etc.

BERMERSIDE (Halifax): *Crossley.*—Measurement of double stars; observation of the phenomena of Jupiter's and Saturn's satellites. Meteorological observations.

BIDSTON: *See* Liverpool.

BIRR CASTLE: *Earl of Rosse.*—Observations for lunar heat. Meteorology.

BONN: *Küstner.*—Prof. Deichmüller was succeeded as director on October 1, 1891, by Dr. Küstner. Observation of the zone $+40^\circ$ to $+50^\circ$ was completed.

BOSTON UNIVERSITY.—A small observatory has been erected for purposes of instruction. Lat. $+42^\circ\ 21'\ 32''.5$; long. $4^h\ 44^m\ 15^s$ west of Greenwich. The chief instrument is an equatorial of 7 inches aperture and 8 feet 1 inch focus, objective by Clacey and mounting by Saegmuller.

BRESLAU: *Galle.*—Time service and meteorological observations. The one hundredth anniversary of the observatory was celebrated in 1891.

BRUSSELS: *Folie.*—Cloudy weather, an insufficient personnel, and the disturbance incident to the removal of the observatory to Uccle greatly interfered with the work of 1890. M. Niesten has continued his observations on the physical aspect of Mercury, Venus, Mars, and Jupiter, with the 38-centimeter (15 inches) equatorial. Since the death of M. Fievez the spectroscope has been in charge of M. Spée.

BUDAPEST: *Konkoly.*—The new observatory of the Royal Meteorological Reichsanstalt consists of a transit room $6\frac{1}{2}$ meters (21 feet) by

4 meters (13 feet) and a room for the refractor with a dome 4½ meters (15 feet) in diameter. The instrumental equipment is very meager, consisting chiefly of a 4½-inch telescope, a transit, clock, chronometer, chronograph, electrical and other subsidiary apparatus. The director reports but little astronomical work accomplished.

CAMBRIDGE (England): *Ball.*—Prof. Adams has been succeeded as director by Sir Robert Ball. Considerable progress has been made upon the zone work. The 25-inch Newall refractor has been used for physical observations of planets and photography of stellar spectra. A spectroscope has been provided from "the Bruce fund."

CAPE OF GOOD HOPE: *Gill.*—Transit circle observations of the sun, Mercury, Venus, and of stars for a new ten-year catalogue, stars occulted by the moon, stars employed for latitude determinations, etc.; with the heliometer, measures for stellar parallax and measures of Jupiter's satellites have been made, and with the zenith telescope, after its renovation, observations for an investigation of the constant of aberration. The photographic work has consisted of miscellaneous photographs of stars and planets, in addition to regular astrophotographic charting.

The catalogue of the Southern Photographic *Durchmusterung* has been made ready for the press.

CARLETON: *See* Goodsell.

CHAMBERLIN: *Howe*—The building has been completed at a cost of $25,000.

CHICAGO—*See* Kenwood, Yerkes.

COLUMBIA (Missouri): *Updegraff*—The observatory of the University of Missouri (lat. $+38° 56' 50''$; long. $1^h 1^m 6^s.4$ west of Washington) was first built in 1853, and then consisted of a small wooden structure in which were mounted a 4-inch Fitz equatorial, a $2\frac{1}{16}$-inch transit by Brunner, a sidereal clock, and other smaller instruments. It was used for the purpose of instructing students in astronomy, and few changes or additions were made till 1880, when a 7½-inch equatorial by Merz & Mahler was bought. The building was then removed to another part of the college grounds and enlarged by the erection of a brick tower, with a dome, for the newly acquired telescope. Soon after a sidereal clock, a chronograph, and spectroscope, all by Fauth & Co., were purchased. A 2-inch altazimuth, by Blunt, of New York, had been bought some years before. The director at that time was Prof. Joseph Ficklin, who died in September, 1886.

Prof. Milton Updegraff was appointed director in July, 1890, and while much of his time is taken up in teaching classes in astronomy and mathematics, he has done excellent work in the observation of planets and comets, besides a redetermination of latitude and longitude, the latter by telegraphic connection with the observatory of Washington University, St. Louis. The observatory building has been enlarged by the addition of an office room and a library.

COPENHAGEN: *Nielsen.*—M. Victor Nielsen at his private observatory has a 7½ inch refractor by Reinfelder and Hertel, the objective of Jena glass. The work has been chiefly upon the moon.

CROWBOROUGH HILL: *Roberts.*—Mr. Isaac Roberts' new observatory is one of the highest points in the south of England, 780 feet above sea level. The hemispherical dome has two slits to effect thorough ventilation. Photographs of stars, planets, nebulæ, and clusters. A photographic search has been made for a trans-Neptunian planet.

DAKOTA AGRICULTURAL COLLEGE (Brookings, Dakota.)—Founded in 1891; equipment, 5-inch equatorial, 2-inch transit, clock and chronograph.

DENVER.—*See* Chamberlin.

DRESDEN: *Dr. B. von Engelhardt.*—Observations of comets, nebulæ and asteroids, and micrometric measures of Bradley stars.

DUDLEY: *Boss.*—Miss Catherine Wolfe Bruce, of New York city, who is already known for her munificent gifts in aid of astronomy, has given $25,000 to the Dudley Observatory for the increase of its permanent endowment. From various sources the additional sum of $31,700 has been secured to defray the cost of rebuilding the observatory on a new site and of furnishing it with a new equatorial of 12 inches aperture, together with other improvements in its equipment. The cost of the telescope is provided for by Robert C. and Charles L. Pruyn; it is to be of the most approved modern construction. The cost of re-establishing the Olcott meridian circle (8 inches aperture), constructed by Pistor and Martins, of Berlin, in 1858, together with a collimating meridian mark and other improvements, is also provided for. The old site is very unfavorable to astronomical observation, owing to its proximity to the four tracks of the New York Central Railroad, which with a very heavy traffic, group around the base of Observatory Hill at a distance of about 150 yards from the instruments. The new site is about 2 miles southwest of the present location upon a plot of about 6 acres.

DUNSINK: *Rambaut.*—On February 20, 1892, Sir Robert Ball was appointed to succeed Prof. Adams at Cambridge, and the vacancy in the directorship of the Dunsink Observatory was filled October 22 by the appointment of Dr. A. A. Rambaut. Dr. Rambaut's assistant is Mr. A. E. Lyster. The 15-inch reflector has been used for stellar photography, principally for determinations of stellar parallax.

DÜSSELDORF: *Luther.*—Observations of asteroids and computation of ephemerides. The passage of railroad trains at a distance of 320 meters from the observatory has not seriously interfered with the observations.

EALING: *Common.*—An excellent 5-foot mirror and a new grating spectroscope have been made for the great telescope. Photographs of nebulæ and of the moon have been taken.

EDINBURGH: *Copeland.*—The reduction of the meridian observations of nebulæ has been undertaken and some further observations have been made at Dunecht, but little observing has been done pending the completion of the new buildings. The main building of the new observatory is to be 180 feet from east to west, terminating in two towers surmounted by domes, or rather "drums." The eastern tower, rising to a height of 75 feet, will contain the 15-inch Grubb refractor, while the 24-inch reflector from the old site at Calton Hill will be mounted in the western dome. A single range of rooms, opening on a corridor on the south, extends from tower to tower. The roof is designed as an asphalted platform, affording free communication between the towers. Beginning at the west there are a spectroscope room, general laboratory, electrical room, cleaning room, mechanic's workshop, chronograph and class room. Light and dark photographic rooms, as well as a computing room for the equatorial and photographic work, are in the eastern tower. A central extension of the building toward the south, 80 feet by 26 feet, will contain the chief computing room, hallway, etc., director's room, and fire-proof library—34 feet by 23 feet, with a light iron gallery affording access to the upper shelves. An upper story to the southern part of this portion of the building, 66 feet in length, is designed for optical work. In the basement will be placed the heating apparatus, a dynamo, and accumulators for supplying electricity for lighting the observatory and illuminating the instruments. In the observatory there will thus be but one chimney.

The transit circle will be in a separate building, with light walls and roof of corrugated iron, 80 feet west of the western tower, accessible by a covered way. The remaining buildings are the astronomer's house, two assistants' houses, and a gate lodge.

GENEVA: *Gautier.*—Col. Emile Gautier died on February 24, 1891, and was succeeded in the directorship by his son, R. Gautier. The principal work of the observatory is the testing of watches and chronometers, and meteorological observations. A number of observations of comets have also been made.

GLASGOW.—Meridian circle observations.

GÖTTINGEN: *Schur.*—Observations of asteroids, comets, and Præsepe, and measures for stellar parallax; regular meridian observations, and physical observations of the moon, Jupiter, Saturn and Uranus.

GOODSELL: *Payne.*—The observatory of Carleton College received a new name, in honor of Mr. C. F. Goodsell, the founder of Carleton College, on June 11, 1891.

The new Williams equatorial, costing $15,000, was installed in 1891. Its clear aperture is 16.2 inches and focal length 22 feet, the lenses having been figured by Brashear upon Hastings' curves and the mounting provided by Warner and Swasey.

GOTHA: *Harzer.*—Reduction of previous observations. The director's time has been given almost entirely to lectures and to his theoretical investigations.

GREENWICH: *Christie*.—The regular work of the observatory has been continued much the same as in previous years, and in addition astrophotographic observations have grown to be a part of the routine, more especially the catalogue of guide stars for the photographic chart. A 9-inch Grubb photographic telescope has been presented to the observatory by Sir Henry Thompson and has been mounted on the Lassell equatorial as a photoheliograph. The erection of the new 36-foot dome, which is to cover the 28-inch refractor, was begun in December, 1892. A discussion of Greenwich observations from 1851 to the present time by Mr. Thackeray has furnished a very satisfactory confirmation of Mr. Chandler's doubly periodic variation of the latitude (in about 365 and 427 days, respectively). A successful longitude campaign has been carried on with Montreal, and the difference of longitude between Greenwich and Paris has been redetermined by English and French observers. Some additions to the equipment have been made and the details of an electric-light installation have been settled.

HAMBURG: *Rümker*.—Observations of comets, asteroids, and comparison stars; chronometer work for the German navy; meteorological observations, and time service.

HARVARD COLLEGE: *Pickering*.—With the meridian circle the observation of stars in the southern Durchmusterung zone (—9° 50′ to —14° 10′) has advanced toward completion. The 15-inch equatorial has been employed on observations partly photometric and partly micrometric, while the principal work done with the west equatorial has been the study by Argelander's method of the changes in the light of the variable stars of long period.

Photographic observations, provided for by the Henry Draper memorial, have been carried on continuously, generally throughout every clear night, and with the aid of three telescopes.

An attempt to secure a suitable location for the Boyden Fund observing station on Wilson's Peak, in southern California, proved unsuccessful, but an expedition sent out to Peru, under the direction of Prof. W. H. Pickering, left Cambridge in December, 1890, and established a station about 3 miles northwest of Arequipa, where a 13-inch equatorial was mounted and observations were commenced. The station is over 8,000 feet above sea level, and has a nearly cloudless sky during a large part of the year; the thermometer rarely falls below 40° F., and rarely goes above 70°. The brilliancy of the stars is most striking; stars of the 6.5 magnitude are picked out easily with the naked eye, the eleven Pleiades can be counted, and the Gegenschein can be readily seen any evening after 9 o'clock.

The Harvard Observatory time service, which had been in operation with but little interruption since 1856, was discontinued after March 31, 1892, having become financially unprofitable, by reason of the fact that time signals from the United States Naval Observatory were offered to the public in Boston through the Western Union Telegraph Company at a lower rate than they could be furnished by the Harvard Observatory.

HATHORN (Saratoga Springs, N. Y.): *del Corral.*—Physical observations of Jupiter with a 6-inch telescope.

HAVERFORD COLLEGE: *Leavenworth.*—Work on stellar parallax; sun-spot observations.

HEIDELBERG: *Wolf.*—Stellar photography. photometric observations. Prof. Wolf has been very successful in the discovery of new asteroids by photography.

HERÉNY: *von Gothard.*—Spectroscopic researches; photography of nebulæ; observations of variable stars: meteorological observations; time service, and computations of asteroids.

HONGKONG: *Doberck.*—Time service; meteorological and magnetic observations.

IOWA UNIVERSITY: *Weld.*—A student's astronomical observatory has recently been established at the State University of Iowa, Iowa City, under the direction of Prof. L. G. Weld. The main building is 12 feet square, capped by a cylindrical turret in which is mounted a Grubb equatorial of 5 inches aperture, and 77½ inches focal length; a Würdemann transit of 1⅞ inches aperture and 24 inches focus is mounted in a wing 10 by 12 feet. Subsidiary apparatus consists of a 4-inch portable Fitz equatorial, clock, chronometer, and chronograph.

JACKSON (Mich.)—Small private observatory of Mr. U. W. Lawton.

JENA: *Knopf.*—The observatory was founded in 1812 by the Grand Duke of Saxe-Weimar. Observations of comets, occultations, phenomena of Jupiter's satellites, variable star observations; time service; meteorology. A new equatorial of 20 centimeters (7.9 inches) aperture and 3 meters (9.8 feet) has been installed.

KALOCSA: *Fényi.*—Solar and meteorological observations.

KENWOOD: *Hale.*—The Kenwood Physical Observatory, the private observatory of Prof. George E. Hale, had its inception in a spectroscopic laboratory erected in Chicago in the summer of 1888. The addition of a tower and wing during the winter of 1890–'91 brought the building to its present form, and it now includes a reception room, library, equatorial room, "slit room," "grating room," photographic dark room, general laboratory, and workshop. The grating room contains a 4-inch concave grating of 10 feet radius of curvature, mounted in the manner employed by Prof. Rowland. A shorter girder allows the use of a grating of only 5 feet radius in cases when the light source is too faint to admit of the highest dispersion. Sunlight is furnished by a heliostat on a pier some distance to the north of the building. Electrical power is supplied through a gas engine and storage battery and also from the main city wires.

The mounting of the equatorial was finished in March, 1891, by Warner and Swasey, and the excellent 12.2-inch object glass, figured from Dr. Hastings's calculation, by Brashear, was in place and ready for use in April, 1891. The spectroscope is large, the objectives being alike and of 3¼ inches clear aperture and 42½ inches focus, corrected for work in the visual region. The grating is a 4-inch flat, in addition to which there is a 30° white flint prism. A second photographic objective of exactly the same aperture and focal length as the visual glass will be provided and a double tube will replace the single tube, the object glass being so supported that either one may be used on either tube.

The observatory was formally dedicated on June 15, 1891; it has been incorporated under the laws of the State of Illinois and its control is vested in a board of trustees. The plan of work includes a study of solar phenomena, with especial attention to spectroscopic investigations of the spots, chromosphere, and prominences. An interesting and well-illustrated description of the observatory, together with an address delivered at the dedication by Prof. Young, is published in the Sidereal Messenger for August, 1891.

KIEL: *Krueger.*—"Centralstelle für astronomische Telegramme." Spectroscopic and photometric observations, observations of comets and asteroids and computations of orbits and ephemerides.

KÖNIGSBERG: *C. W. F. Peters.*—Meridian observations, heliometer measures of double stars, and for parallax; observations of comets and of the moon. Meteorology.

KREMSMÜNSTER: *Wagner.*—Observations of comets and sun spots. Time service, meteorological and magnetic observations.

LADD: *Upton.*—The Ladd observatory was formally presented to Brown University, Providence, R. I., by Governor Ladd on October 21, 1891. The building and equipment have cost nearly $30,000. The main part of the building is 43 by 27 feet, and the transit room 25 by 15 feet. The chief instrument is an equatorial of 12.2 inches aperture, objective by Brashear, and mounting by Saegmuller. It is one of three recently made by Mr. Brashear from the formulæ of Prof. C. S. Hastings. The mounting embodies several convenient devices. The spectroscope, which is of special excellence, is by Brashear.

The clock room is a chamber in the equatorial pier, and contains a Howard sidereal and a Molyneaux mean time clock. The other instruments are a 3-inch portable transit by Saegmuller; a smaller transit for students' use, a Warner and Swasey chronograph, several chronometers and sextants, a barograph, thermograph, and recording hygrometer by Richard Frères, a recording rain and snow gauge by Ferguson, and ordinary meteorological instruments. The observatory is designed primarily for the instruction of students, but also for research, and the equipment has been planned for a possible extension of the latter as the resources of the observatory may allow. The director is Prof. Winslow Upton.

LEIPZIG: *H. Bruns.*—Parallax measures of stars with large proper motions, observations of planets and asteroids. Zone work +5° to +10°, and +10° to +15°; triangulation of trapezium of Orion. Time service and meteorological observations.

LICK: *Holden.*—A new building has been erected to cover the Willard photographic lens (aperture 5.9 inches, focal length 31 inches), and its mounting by Brashear, presented by Hon. C. F. Crocker. The dome is 10 feet in diameter, and attached to it is a photographic dark room about 10 by 11 feet.

A graduate school of astronomy has been established at the Lick Observatory as a part of the graduate system of the University of California, and a special fund established by Mrs. Phebe Hearst is in part available for the expenses of advanced students elected fellows by the regents.

Barnard's discovery of a fifth satellite of Jupiter with the 36-inch refractor has been referred to elsewhere.

LIÈGE: *Folie.*—This observatory is attached to the Royal Observatory at Brussels, and its observations are published in the Brussels volumes. Much excellent theoretial work has been done by M. de Ball while awaiting repairs to the meridian circle.

LIVERPOOL: *Plummer.*—Time service, chronometers, meteorological observations. The 8-inch equatorial has been used in the systematic observation of comets.

LUND: *Folke Engström.*—Work on zone +35° to +40°.

MCCORMICK: *Stone.*—Prof. Stone has published a continuation of the Bonn Durchmusterung, upon which he has been engaged for a number of years.

MADRAS: *Smith.*—Mr. C. Michie Smith, since the death of Mr. Pogson, has been chiefly engaged in pushing forward the publication of observations of earlier years. Observations other than those required for the efficient maintenance of the time service have been entirely subordinated to the work of publication. Two volumes of the valuable *Madras Meridian Circle Observations* have been issued.

MELBOURNE: *Ellery.*—Meridian circle work has been continued. The photographic telescope was mounted in January, 1891, and considerable progress has been made towards the photographic catalogue. Meteorological and magnetic observations, time service, and chronometer rating have been kept up, but the observatory has been seriously crippled by the reduction of its appropriations, necessitating the retirement of two assistants.

MILAN: *Schiaparelli.*—Measurements of double stars; preparations for a catalogue of 1,100 stars, zone +2° to +6° observed from 1860 to 1872. Longitude work, time service, and magnetic observations.

MISSISSIPPI UNIVERSITY: *Fulton.*—Under date of July 6, 1891, it was reported (Sid. Mess., No. 97) that a "twin equatorial" (a 15-inch visual telescope and a 9-inch photographic telescope side by side on the same mounting) was under construction by Grubb.

MOUNT HOLYOKE (South Hadley, Mass.)—Sun-spot observations.

MOUNT ROSA.—A small observatory is in course of construction on Mount Rosa, 15,000 feet above sea level, consisting of a wooden hut 10 by 30 feet.

MUNICH: *Seeliger.*—A new Repsold 6-inch meridian circle was mounted in July, 1891. Observations of comets and of Saturn with the 10½-inch refractor; investigations of personal equation dependent on the magnitudes of stars; longitude work, meteorological observations.

NATAL.—Observations of the Moon's position and of Mars.

O'GYALLA: *Konkoly.*—Observations of sun spots, drawings of Jupiter: a few spectroscopic observations and some photographic experiments. Much time has been spent in the reorganization of the Government Meteorological Bureau.

OXFORD (University).—The series of observations for determining by photography the parallax of about 30 stars chiefly of the second magnitude has been completed and the results published. Much time has been spent in the preparation of the new instruments to be used on the international chart of the heavens, and a considerable number of plates comprised in the zone assigned to Oxford have been completed. Experimental work has also been done for the committee in charge of the international chart.

A convenient observatory has been erected contiguous to the main building for the exclusive use of university students. This observatory is furnished with two small transit circles, three telescopes, one of which is a reflector of 15 inches aperture, and subsidiary apparatus.

PARIS: *Tisserand.*—In the report for 1891 the director stated that the Gambey circle had been applied to the investigation of the latitude and the question of its variation; observations to determine the constant of aberration were completed, and besides the usual planetary and cometary observations, a considerable number of measurements of double stars and micrometric measures of nebulæ were made.

Photographic work upon the great chart and upon the moon has been continued, and the newly organized department of spectroscopy has obtained interesting results under M. Deslandres.

The report for 1892 contains a tribute to Admiral Mouchez, the late director, an account of gratifying progress in the photographic and spectroscopic work, and with the equatorial coudé. A "Bureau des Mesures des Clichés du Catalogue" has been organized, with Mlle. Klumpke at its head.

POTSDAM: *Vogel.*—The spectroscopic determination of the motion of stars in the line of sight to which Dr. Vogel has given especial attention has been continued, and many of the results have been published. Dr. Scheiner has worked upon stellar spectra and the spectra of solar prominences. Prof. Müller and Dr. Kempf have completed their observations for a photometric Durchmusterung, and Prof. Müller his long series of photometric observations of the planets. Dr. Lohse and Prof.

Spoerer have been engaged in photographic and visual observations of the sun.

PRAGUE: *Safarik.*—Variable stars.

PRAGUE (University): *Weinek.*—Drawings of the moon. Determination of latitude, observations of Jupiter's satellites, time service, magnetic and meteorological observations.

PROVIDENCE (R. I.).—*See* Ladd.

RADCLIFFE (Oxford): *Stone.*—Work on the general catalogue of 6,350 stars for 1890; meridian observations of the sun and moon. Observations of comets, double stars, and occultations. Meteorology.

ROME.—The first fascicule of the publications of the new Vatican observatory contains the interesting Papal Brief founding the observatory, an historical introduction, and two papers on astronomical photography, to which the observatory is to be for the present devoted.

ROUSDON (Lyme Regis): *Peek.*—Variable stars; time service.

SAN DIEGO (Cal.)—Mrs. Proctor, widow of the late R. A. Proctor, proposes to erect an observatory at San Diego as a memorial to her husband; an 18-inch object glass has been ordered.

SAN FERNANDO: *Viniegra.*—Capt. J. Viniegra has been appointed director, to succeed Capt. Pujazon.

SMITH (Beloit, Wis.): *Bacon.*—Sun-spot observations, etc.

STONYHURST: *Sidgreaves.*—Photography of the solar spectrum and of stellar spectra; drawings of sun spots and measures of the chromosphere and prominences. A new 15-inch refractor has been purchased with the fund raised to the memory of the late Father Perry.

STRASBURG: *Becker.*—The meridian circle has been used in observing the zone $-2°$ to $-6°$, and also the sun, moon, and planets. Some defects in the construction of the altazimuth were remedied and the instrument was used in a careful series of observations for the determination of the variation of latitude, beginning in May, 1891, and ending in March, 1892.

SYDNEY.—Transit-circle work, observations of double stars and of comets; photographic work for the international chart, photographs of comets and of Mars. Weather-chart service.

TEMPLE (Rugby): Double stars; nebulæ photography.

TOULOUSE: *Bigourdan.*—From an account of the history of the observatory by M. Bigourdan it appears that it was originally established in 1729 on one of the towers of the rampart of the town. Garipuy made some observations there, but afterwards erected an observatory on his own house and superseded it by a larger and more commodious one in 1770. Darquier assisted him for a time, but afterwards erected an observatory of his own. Vidal had commenced his astronomical work at the observatory of Garipuy, which, however, became the property of the states of Languedoc after the death of the founder in 1782. Vidal retired in 1807, and, after several attempts to improve the observatory, it was decided in 1840 to erect a new one at the extremity of the town. The buildings were commenced the following year, but it was not until the end of 1846 that they received their instrumental equipment. Garipuy's observatory was then abandoned.

UNDERWOOD (Appleton, Wis.): *L. W. Underwood.*—The Underwood observatory in connection with Lawrence University at Appleton, Wis., was equipped at the opening of the college year of 1892-'93. The outfit consists of a 10-inch Clark equatorial, 4-inch meridian circle, mean-time and sidereal clocks, chronometer, and chronograph. A local time service has been established.

UNITED STATES NAVAL OBSERVATORY: *McNair.*—At the time of the last report of the Superintendent, September 29, 1892, the new buildings were not ready for occupancy. The usual routine observations have been somewhat interrupted by preparations made for the removal of the instruments to the new site, advantage being taken of the interruption of observations to advance the reductions of previous years.

Prof. Asaph Hall was retired by law from active service as a staff officer of the Navy on October 15, 1891.

UPPER TULSE HILL: *Huggins.*—Visual and photographic observations of Nova Aurigæ (1892).

UPSALA: *Dunér.*—Variable stars; stellar photography. A new photographic refractor of 33 centimeters (13 inches) objective has been under construction, and has necessitated some alterations in the building. Time service.

VASSAR: *Miss Whitney.*—Sun-spot observations, observations of comets, etc.

WESTMEATH: *Wilson.*—The 2-foot Grubb reflector has been remounted and used for stellar photography. Some photographs of Jupiter have been taken with a photographic photometer, to determine the relative albedo of the planet and his moons.

WINDSOR: *Tebbutt.*—Observations of comets, double stars, occultations, and the phenomena of Jupiter's satellites.

WOLSINGHAM: *Espin.*—Spectroscopic zone work; double stars. A number of new variable stars have been discovered. Meteorological observations.

YALE.—Dr. Elkin's heliometer work constitutes the chief astronomical activity. In 1891 the series of observations to determine the parallaxes of the first magnitude stars of the northern hemisphere was completed. Observations have also been made of comparison stars for Victoria, and the computations on the Iris series in 1888 have been carried forward chiefly by Miss Palmer. Observations of comets and asteroids were made by Mr. Chase with the 8-inch Reed equatorial.

From July, 1891, to January, 1892, the heliometer was devoted to a series of measures on the satellites of Jupiter for the determination of their orbits, and the mass of the planet. After the completion of this work Dr. Chase completed a triangulation of the principal stars in Coma Berenices. A series of measures of Algol has been made to test the theory of a sensible orbital motion of the bright component, and the theoretical parallax, suggested by Mr. Chandler.

YERKES (University of Chicago): *Hale.*—Through the munificence of Mr. Charles J. Yerkes, of Chicago, the University of Chicago is to have an astronomical observatory of the first class. No definite limit has been assigned to the expenditure contemplated, but it is intimated that the equipment shall be equal to any in existence. The principal instrument will be a 40-inch refractor, the disks for which were made some years since for the University of Southern California.

The remainder of the equipment is still undetermined, but it will probably include a 16-inch refractor, 12-inch "twin" equatorial with visual and photographic objectives, 6-inch meridian circle, and 20-inch siderostat.

ZÜRICH: *R. Wolf.*—Sun-spot observations; observations for determining the variation of latitude; time service.

ASTRONOMICAL INSTRUMENTS.

Brashear-Hastings objectives.—Three large object glasses recently made by Brashear are of more than ordinary interest, as they have been ground by Prof. Hastings' formula. They are the 16-inch of the Goodsell Observatory, the 12.2 of the Ladd Observatory, and the 12-inch of the Kenwood Physical Observatory. The crown glass was obtained from Mantois, of Paris, and the flint from the optical works at Jena, Germany.

A new instrument has been devised by A. Beck, called a "Nadir-Instrument," for the determination of time and latitude by observation of the transits of stars over a circle whose pole is the zenith. The instrument is adjusted for a circle of 60° zenith distance.

To amateurs a series of articles on the "Adjustment of a small Equatorial," in the Journal of the British Astronomical Association (February, 1892), by Mr. Maunder, will undoubtedly prove of interest and value.

MISCELLANEOUS.

Prizes.—The Lalande prize of the Paris Academy for 1891 was awarded to M. G. Bigourdan for the work he has undertaken and partly carried out, of micrometrically measuring all the known nebulæ, about six thousand in number, observable at Paris; this will be a first step to obtaining some knowledge of their proper motions, and ultimately, perhaps, of their distances from the sun. No memoir was presented to the Academy on the special subject proposed for the Damoiseau prize, "To perfect the theory of the inequalities of long periods caused by the planets in the motion of the moon." It was, therefore, proposed again for 1892, and its value fixed at 4,000 francs. Prizes were, however, adjudged, for their planetary and cometary investigations, to MM. Gaillot, Callandreau, and Schulhof. The Janssen prize was awarded to M. Rayet, for his spectroscopic work. This prize is awarded annually for the first seven years after its foundation (1887), and becomes biennial in 1891.

The Lalande prize of the French Academy of Sciences was awarded on December 19, 1892, to Mr. E. E. Barnard for his astronomical discoveries, especially the discovery of the fifth satellite of Jupiter, and to Prof. Max Wolf for his work in astronomical photography, especially in the discovery of asteroids. The Damoiseau prize to MM. Radau and Leveau; the Valz prize to M. Puiseaux for his work on the theory of astronomical instruments and the constant of aberration; the Janssen prize to M. Tacchini for his work on the solar spectrum.

The Donohoe Comet Medals of the Astronomical Society of the Pacific.—The following amended rules for the bestowal of the medal took effect on February 26, 1891.

I. A medal of bronze is established as a perpetual foundation to be given for the discovery of comets, as follows:

The medal is to bear on the obverse side the effigy of a bright comet among stars, with the legend "ASTRONOMICAL SOCIETY OF THE PACIFIC" around the border, and on the reverse the inscription, "THIS MEDAL, FOUNDED IN 1890 BY JOSEPH A. DONOHOE, IS PRESENTED TO ——— (the name of the discoverer) TO COMMEMORATE THE DISCOVERY OF A COMET ON ——— (the date)."

It is to be understood that this medal is intended solely as a recognition of merit, and not as a reward.

II. The medal will be given to the actual discoverer of any unexpected comet.

III. The discoverer is to make his discovery known in the usual way, and, in order to simplify the work of the committee, which, in certain cases may be called upon to consider the merits of several independent discoveries of the same object, he should also address a letter to the Director of the Lick Observatory, which should state the exact time of the discovery, the position of the comet, the direction of its motion (when this can be determined), and the physical appearance of the object.

No application for the bestowal of the medal is required. The letters received from discoverers of comets will be preserved in the records of the Lick Observatory. Cable telegrams to the Lick Observatory are to be addressed to "Astronomer, San Francisco."

IV. All communications will be referred to a committee consisting of the Director of the Lick Observatory, *ex officio*, and of two other persons, members of the Astronomical Society of the Pacific, who are to be annually appointed by the Board of Directors. The decisions of this committee are to be final upon all points relating to the award of the medal. The committee will print an annual statement of its operations in the publications of the society.

Under ordinary circumstances the comet medal will be awarded within two months after the date of the discovery. In cases of doubt a longer period may elapse. The medal will not be awarded (unless under the most exceptional circumstances) for the discovery of a comet until enough observations are secured (by the discoverer or by others) to permit the calculation and verification of its orbit.

V. This medal is to be a perpetual foundation from and after January 1, 1890.

The fourth award of the Donohoe medal was made to Dr. R. Spitaler, assistant in the Imperial Observatory of Vienna, for his discovery of a comet "in the morning hours" of November 16, 1890. This was the first comet discovered by Dr. Spitaler.

The fifth award was made to Prof. T. Zona, adjunct astronomer in the Royal Observatory of Palermo, for his discovery of a comet at 9^h 31^m, Greenwich mean time, November 15, 1890. Also, his first discovery of a comet.

The sixth award was made to Mr. E. E. Barnard, astronomer of the Lick Observatory, for his discovery of a comet at 16 hours, Greenwich mean time, on March 29, 1891. This was the fifteenth comet discovered by Mr. Barnard.

The seventh award was also made to Mr. Barnard for a comet discovered at 0^h 55^m, Greenwich mean time, on October 3, 1891.

The eigth award was made to Dr. Lewis Swift for his discovery of an unexpected comet on March 6, 1892.

The ninth award was made to Mr. W. F. Denning, of Bristol, England, for his comet of March 18, 1892.

The tenth award to Mr. W. R. Brooks, of the Smith Observatory, Geneva, New York, for a comet on August 28, 1892.

The eleventh award was made to Mr. E. E. Barnard for his discovery by photography of an unexpected comet on October 12, 1892, at Mount Hamilton.

The twelfth award was made to Mr. Edwin Holmes, of London, England, for his comet of November 6, 1892.

The thirteenth award to Mr. W. R. Brooks for his comet on November 19, 1892.

The Acton prize.—Once in seven years the Acton prize of £100 is awarded to the person whose scientific writings have been most serviceable to the cause of natural religion. The last prize was adjudged to Prof. G. Stokes, of Cambridge University. The recipient in 1892 was Miss Agnes Clerke, author of the "History of Astronomy in the Nineteenth Century;" of the "System of the Stars;" and of "Studies in Homer."

The Bruce fund.—The fund of $6,000 placed by Miss Bruce in Prof. Pickering's hands to be used in aid of astronomical work, has been applied as follows: To Prof. Newcomb, for a discussion of the contact observations of Venus during the transits of 1874 and 1882; Dr. Plassmann, printing observations of meteors and of variable stars; Astronomische Gesellschaft, construction of tables for the computation of the absolute perturbations of the asteroids by Gyldéns method; International Geodetic Commission, dispatch of a party to the Sandwich Islands for a study of the variations of latitude; Mr. H. H. Turner, computation of tables for reductions of star places; Prof. E. S. Holden, reduction of meridian observations of Struve stars; Prof. H. A. Rowland, identification of metals in the solar spectrum; Dr. L. Struve, reduction of the occultations observed during the eclipse of January 28, 1888.

It may not be out of place here to note that a legacy of 100,000 francs ($20,000) has been left by an old lady of Pau to the Institute of France, as a reward for the person of any nationality who shall, within the next ten years, succeed in communicating with the inhabitants of some other celestial body. Apropos of this legacy, Flammarion has written an interesting article in L'Astronomie as to the possibility of our ever being able to accomplish communication with our neighbors.

The Danish Academy of Sciences and Letters has awarded a gold medal to Baron E. von Haerdtl, of Innspruck, for his memoir on a case of the problem of three bodies proposed by the Acadamy in 1889.

Astronomy and astro-physics.—With its one hundred and first number, the *Sidereal Messenger*, which has been edited by Prof. W. W. Payne, at Northfield, Minn., since 1882, takes a new name, and is enlarged, so that a considerable portion of each number is devoted, under the able editorship of Prof. G. E. Hale, of Chicago University, to what is now known as astro-physics. Prof. Payne continues as senior editor in "General Astronomy," assisted by Prof. H. C. Wilson. The bibliographer will note that, though the journal has a new name, the volume and current number are continued from the *Sidereal Messenger*; thus the initial number of Astronomy and Astro-physics is "Number 101," forming part of "Volume XI."

It is stated that there are to be erected in Berlin three hundred "Urania pillars." These pillars will be about 18 feet high, made of cast iron, and will each contain a clock, meteorological instruments, weather charts, astronomical and geographical announcements, and also, as in the streets of Paris, a plan of the neighboring streets in enlarged form to enable strangers to find their way. The instruments are to be regulated from the observatory.

A star atlas by Herr Jacob Messer, of St. Petersburg, the page being about $4\frac{1}{2}$ inches by $8\frac{3}{4}$ inches, will be found extremely convenient for amateur observers who do not care to burden themselves with the larger works. It contains all the stars visible to the naked eye (first to sixth magnitudes, inclusive), from the north pole down to 35° south declination, together with a selection of the most interesting double stars, variables, nebulæ, clusters, etc.

Much interesting light has been thrown of late on Babylonian astronomy by Fathers Epping and Strassmaier. A series of lunar and planetary observations has recently been found in the cuneiform tablets of the British Museum, and among others an observation of a lunar eclipse, one of the nine used by Ptolemy in his Almagest. Another work of the same authors shows that the Babylonians were able to predict the rising and setting of the moon, and the hour and magnitude of an eclipse.

Mr. A. M. W. Downing, superintendent of the computations at the Royal Observatory, Greenwich, was appointed to succeed Dr. Hind, who retired from the position of superintendent of the British Nautical Almanac office on January 1, 1892.

The Astronomische Gesellschaft held its fourteenth biennial meeting at Munich August 5–7, under the presidency of M. Gyldén. The society numbers 318 members.

A new astronomical society.—An association was formed in Berlin, in 1891, called the "Union of Friends of Astronomy and Cosmical Physics," for the purpose of securing co-operation in the study of these sciences in the countries of central Europe. The strength of the new society is perhaps best indicated by the names of its officers, Prof. Lehmann-Filhés being president, and Herrn Förster, M. W. Meyer, Plassmann, Jesse, Weinstein, and Reimann the presidents of its six sections.

The question of the ownership of an aerolite has been referred for settlement to the courts, and the decision reached is of some interest. On May 2, 1890, an aerolite weighing 66 pounds fell on the land of John Goddard, in Winnebago County, Iowa. It was dug up by Peter Hoagland, carried to his house, and sold for $105. Goddard claimed it as it had fallen on his land, while Hoagland claimed it as he discovered it and as it fell from heaven. In the suit that resulted the court held that the stone became part of the soil on which it fell, and that Hoagland had no right to remove it. The defense claimed that whatever was movable and found on the surface of the earth unclaimed by any owner was supposed to be abandoned by the proprietor.

NECROLOGY OF ASTRONOMERS, 1891-92.

ADAMS (JOHN COUCH). Born near Launceston, Cornwall, June 5, 1819; died at Cambridge, England, January 21, 1892.

AIRY (GEORGE BIDDELL). Born at Alnwick, July 27, 1801; died at Greenwich, January 2, 1892.

BRÜNNOW (FRANZ FRIEDRICH ERNST). Born at Berlin, November 18, 1821; died at Heidelberg, August 20, 1891.

CLARK (GEORGE BASSETT). Born at ——, February 14, 1827; died at Cambridgeport, December 30, 1891.

DE GASPARIS (ANNIBALE). Born at Bugnaca, November 9, 1819; died at Naples, March 21, 1891.

GAUTIER (EMILE). Born at Geneva, April 18, 1822; died at Geneva, February 25, 1891.

GRANT (ROBERT). Born at Grantown-on-Sprey, June 17, 1814; died at Grantown, October 24, 1892.

HARTNUP (JOHN). Born in London, 1811; died at Liverpool, April 21, 1892.

VON HAYNALD (LUDWIG). Born at Szécseny, 1816; died at Kalocsa, July 4, 1891.

KLEIBER (JOSEPH). Born at St. Petersburg, December 15, 1863; died at Nice, February 12, 1892.

MOUCHEZ (AMÉDÉE ERNEST BARTHÉLEMY). Born at Madrid, August 24, 1821; died at Wissons, June 29, 1892.

POGSON (NORMAN ROBERT). Born at Nottingham, March 23, 1829; died at Madras, June 23, 1891.

RUTHERFURD (LEWIS MORRIS). Born at Morrisania, November 25, 1816; died May, 30, 1892.

SCHÖNFELD (EDUARD). Born at Hildburghausen, December 22, 1828; died at Bonn, May 1, 1891.

SEYDLER (AUGUST). Born at Senftenberg, June 1, 1849; died at Prag, June 22, 1891.

ASTRONOMICAL BIBLIOGRAPHY FOR 1891 AND 1892.

The following bibliography or index catalogue is arranged upon the plan adopted in the review of astronomy for 1886, thus making this series of indexes complete from that year to 1892, except for the year 1890, an index for which was published in the *Sidereal Messenger* for 1891 (vol. 10, pp. 84, 356).

The principal books, memoirs, and journal articles published in 1891 and 1892 that have come under the compiler's notice are here included, and there are also a few titles that belong to earlier years but were not found in time to insert in previous lists. References to series of observations, preliminary orbits of comets and asteroids, reviews, etc., are omitted, and to condense it into reasonable limits the bibliography has not been made exhaustive even to the extent of printing all titles that were originally collected.

The subject headings are in alphabetical order, with a subarrangement by authors. The references to periodicals are by volume and page separated by a colon; thus: Obsry. 15:173–89 indicates volume 15, pages 173 to 189, of *The Observatory*.

The following is a list of the principal periodicals examined:

American Journal of Science, vols. 141–144.
Astronomical Journal, Nos. 233–283.
Astronomische Nachrichten, Nos. 3010–3139.
L'Astronomie, vols. 10 and 11.
Astronomy and Astrophysics, vol. 11.
Bulletin Astronomique, vols. 8 and 9.
Comptes Rendus, vols. 112, 113, 114, 115.
Journal of the British Astronomical Association.
Monthly Notices of the Royal Astronomical Society, vols. 51, No. 3, to vol, 52, No. 9.
The Observatory, vols. 14, 15.
Publications of the Astronomical Society of the Pacific, Nos. 13 to 26.
Sidereal Messenger, vol. 10.
Vierteljahrsschrift der Astronomischen Gesellschaft, 26.–27. Jahrg.

ABBREVIATIONS.

Abstr. = Abstract.
Am. = American.
Bd. = Band.
d. = di, der, del, etc.
ed. = edition.
Hft. = Heft.
hrsg. = herausgegeben.
il. = illustrated.
j., jour. = journal.
k. k. = kaiserlich-königlich.
Lfg. = Lieferung.
n. d. = no date.
n. p. = no place of publication.
n. F. = neue Folge.
n. s. = new series.
Not. = Notices.
Obsry. = Observatory.
p. = page.
pl. = plates.
portr. = portraits.
pt. = part.
r. = reale.
Rev. = Review.
s. = series.
sc. = science, scientific.
vol. = volume.

Abastumân Observatory.

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