ANNUAL REPORT

OF THE

BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING

THE OPERATIONS, EXPENDITURES, AND CONDITION
OF THE INSTITUTION

TO

JULY, 1897.

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1898.

LETTER

FROM THE

SECRETARY OF THE SMITHSONIAN INSTITUTION,

ACCOMPANYING

The Annual Report of the Board of Regents of the Institution for the year ending June 30, 1897.

SMITHSONIAN INSTITUTION,
Washington, D. C., April 14, 1898.

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, 1897.

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

S. P. LANGLEY,
Secretary of Smithsonian Institution.

Hon. GARRET A. HOBART,
President of the Senate.

ANNUAL REPORT OF THE SMITHSONIAN INSTITUTION FOR THE YEAR ENDING JUNE 30, 1897.

SUBJECTS.

1. Proceedings of the Board of Regents for the session of January, 1897.

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 ending June 30, 1897.

3. Annual report of the Secretary, giving an account of the operations and condition of the Institution for the year ending June 30, 1897, 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. These memoirs relate chiefly to the calendar year 1897.

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THE SMITHSONIAN INSTITUTION.

MEMBERS EX OFFICIO OF THE "ESTABLISHMENT."

William McKinley, President of the United States.
Garret A. Hobart, Vice-President of the United States.
Melville W. Fuller, Chief Justice of the United States.
John Sherman, Secretary of State.
Lyman J. Gage, Secretary of the Treasury.
Russell A. Alger, Secretary of War.
Joseph McKenna, Attorney-General.
James A. Gary, Postmaster-General.
John D. Long, Secretary of the Navy.
Cornelius N. Bliss, Secretary of the Interior.
James Wilson, Secretary of Agriculture.

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.

Richard Rathbun, *Assistant Secretary,*

In charge of Office and Exchanges.

Charles D. Walcott, *Acting Assistant Secretary,*

In charge of National Museum.

REGENTS OF THE SMITHSONIAN INSTITUTION.

By the organizing act approved August 10, 1846 (Revised Statutes, Title LXXIII, section 5580), and amended March 12, 1894, "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, 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 ENDING JUNE 30, 1897.

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:

GARRET A. HOBART (March 4, 1897).

	Term expires.
United States Senators:	
JUSTIN S. MORRILL (appointed Feb. 21, 1883, Mar. 23, 1885, Dec. 15, 1891, and Mar. 15, 1897)	Mar. 3, 1903
SHELBY M. CULLOM (appointed Mar. 23, 1885, Mar. 28, 1889, and Dec. 18, 1895)	Mar. 3, 1901
GEORGE GRAY (appointed Dec. 20, 1892, and Mar. 20, 1893)	Mar. 3, 1899
Members of the House of Representatives:	
JOSEPH WHEELER (appointed Jan. 10, 1888, Jan. 6, 1890, Jan. 15, 1892, Jan. 4, 1894, and Dec. 20, 1895)	Dec. 22, 1897
ROBERT R. HITT (appointed Aug. 11, 1893, Jan. 4, 1894, and Dec. 20, 1895)	Dec. 22, 1897
ROBERT ADAMS, JR. (appointed Dec. 20, 1895)	Dec. 22, 1897
Citizens of a State:	
JAMES B. ANGELL, of Michigan (appointed Jan. 19, 1887, and Jan. 9, 1893)	Jan. 19, 1899
ANDREW D. WHITE, of New York (appointed Feb. 15, 1888, and Mar. 19, 1894)	Mar. 19, 1900
WILLIAM PRESTON JOHNSTON, of Louisiana (appointed Jan. 26, 1892)	Jan. 26, 1898
Citizens of Washington:	
JOHN B. HENDERSON (appointed Jan. 26, 1892)	Jan. 26, 1898
GARDINER G. HUBBARD (appointed Feb. 27, 1895)	Feb. 27, 1901
WILLIAM L. WILSON (appointed Jan. 14, 1896)	Jan. 14, 1902

Executive Committee of the Board of Regents.

J. B. HENDERSON, *Chairman*. WILLIAM L. WILSON. GARDINER G. HUBBARD.

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

Annual Meeting of the Board of Regents.

January 27, 1897.

In accordance with a resolution of the Board of Regents adopted January 8, 1890, by which its stated annual meeting occurs on the fourth Wednesday of January, the Board met to-day at 10 o'clock a. m.

Present: The Chancellor (Mr. Chief Justice Fuller) in the chair; the Hon. A. E. Stevenson, Vice President of the United States; the Hon. J. S. Morrill, the Hon. S. M. Cullom, the Hon. George Gray, the Hon. Joseph Wheeler, the Hon. R. R. Hitt, the Hon. Robert Adams, jr., the Hon. William L. Wilson, Dr. J. B. Angell, Dr. Andrew D. White, the Hon. John B. Henderson, the Hon. Gardiner G. Hubbard, and the Secretary, Mr. S. P. Langley.

At the Chancellor's suggestion the Secretary read the minutes of the last meeting in abstract. There being no objection the minutes were approved.

The Secretary presented his annual report for the fiscal year ending June 30, 1896, remarking that as the Regents had already been supplied with copies by mail he would say nothing further upon it now. On motion the report was accepted.

Senator Henderson, as chairman of the Executive Committee, presented the report of the Executive Committee for the fiscal year ending June 30, 1896. On motion the report was adopted.

Senator Henderson presented the following customary resolution relative to income and expenditure, which was adopted:

Resolved, That the income of the Institution for the fiscal year ending June 30, 1898, be appropriated for the service of the Institution, to be expended by the Secretary with the advice of the Executive Committee, with full discretion on the part of the Secretary as to items.

Senator Henderson, as Chairman of the Permanent Committee (composed of the Executive Committee and the Secretary), stated that the matters which affect the Avery estate, and which were before the Board at the last meeting, had had the committee's consideration. There was no special report, but he hoped to have a statement to make by the next meeting.

The Chancellor then announced that if there was no objection the matters in the Avery estate would be postponed for a report at the next annual meeting of the Board.

The Secretary said:

The Regents know of the irreparable loss which the Institution has sustained in the death of Dr. Goode, a man who can not be replaced; a man who was devoted to its service; who, it almost might be said, laid down his life for it, and who possessed a combination of administrative ability and general scientific knowledge with every element of moral trustworthiness for which I do not know where to look again. I have thought that the Regents might like to make, by exception, an acknowledgment of this by some resolution, and I will request Dr. White to present those prepared.

Dr. White then read the following resolutions:

Whereas the Assistant Secretary of the Smithsonian Institution, Dr. G. Brown Goode, died on September 6, 1896,

Resolved, That the Board of Regents wish to here record their sense of the devotion to duty which in the late Dr. Goode came before any considerations of personal advancement, or even before the care of his own health, and of their recognition that his high administrative ability and wide knowledge were devoted unselfishly to the service of the Institution, with results whose value they can not too highly acknowledge; and they desire to express their feeling of the loss that the Institution, the National Museum, and the cause of science has sustained in his untimely death.

Resolved, That a copy of these resolutions be suitably engrossed and transmitted to the family of Dr. Goode.

Dr. White said:

In being asked to offer these resolutions I wish to say that I accept the duty as one especially grateful to my own feelings. I became acquainted with Dr. Goode about sixteen years ago when he went abroad to Berlin, representing the Smithsonian Institution at the Fisheries Exposition. I think every one who met him then conceived a very high opinion not only of his executive abilities, which he had much occasion to show, but of his personal qualities. I took occasion to bring him into connection with the leading German men of science, and he at once seemed to win not merely their respect but also their most kindly feelings. You are aware that on that occasion the United States exhibition was by far superior to any of their own, and it was then that we received the great prize from the Emperor Wilhelm, which now stands in the Museum under our care. At various times since I have had occasion to renew my acquaintance with Dr. Goode, and I have never ceased to hold the good opinion which his admirable qualities aroused in me.

Mr. Hubbard said:

Mr. Brown Goode was a very warm personal friend of mine. I personally regret his loss, and I regret it still more on account of the loss to the Museum. I do not suppose that there was any man living who knew better what was necessary for the Museum than he. I do not believe any man ever lived, or ever will live, who will give so much of his time and thought to the work of this Museum as Mr. Brown Goode did, and his death is a blow which the Museum will feel as long as it lasts.

Senator Morrill said:

I frequently had occasion to converse with Dr. Goode in relation to the future growth of the Museum. It is, perhaps, well known to most of you that I have been making an effort for some years now to secure an additional building for the Museum, and in talking over the matter Dr. Goode was very earnest in his purpose to fill it with something that would be worthy of our country, and he was also strongly of the opinion that there was no country that could afford as many valuable and attractive collections for Museum purposes as the United States, and, with the Smithsonian, was capable of doing so in a more economical manner than perhaps any other country in the world.

Mr. Wilson said:

My personal relations with Dr. Goode were so pleasant and cordial that though I can add nothing to what has been already said by Dr. White, Mr. Hubbard, and Senator Morrill, I feel at least like seconding these resolutions. I was thrown a great deal with Dr. Goode in a most informal and unofficial way and I learned to have the highest possible respect for him, not only as a scientific man, but as an individual. The simplicity, modesty, and general kindliness of his character were always his striking features, and I remember no greater shock in recent months than the announcement of the death of this still young man, which I learned from an American paper as soon as I returned from abroad last summer.

On motion, the resolutions were adopted by a rising vote.

The Secretary said that before leaving this subject he might say that the year had been a painful one for the Institution in the loss of other people not known to the Regents, but only less essential to it than Dr. Goode. Mr. R. E. Earll and others who were identified with it had been thus taken away, and it had been difficult to find persons of efficiency to attend to their work. The Secretary added that he had lost not only Dr. Goode, but also Mr. Winlock, who was assistant in charge of the Institution, who came next to Dr. Goode in authority, and who was trusted and trustworthy in every way. It had been a sad year here, but he would say nothing further about its losses except that the deaths included a number of employees who were also valuable in their positions.

The Chancellor stated that the appointment of an Assistant Secretary was made by the Secretary, with the consent of the Regents; in other words, the initiative, under the law, came from the Secretary. He would like to hear from the Secretary about it.

The Secretary then addressed the Board as follows:

The Board is aware that a vacancy exists in the Assistant Secretaryship, caused by the death of Dr. Goode, a like successor to whom can hardly be found.

Under correction of the Chancellor, I will recall that while the law authorizes the Secretary to, with the consent of the Board of Regents, employ assistants, he is not required to employ anyone.

In the early days of the Institution there was one Assistant Secretary in charge of the library, to which there was added later by Professor Henry one in charge of the Museum. The latter of these was Professor Baird, who was in turn appointed Secretary by the Regents, and who, during a term of nearly ten years, appointed no assistants until January 12, 1887, when he received the consent of the Regents to the appointment of one in charge of the exchanges, library, and publications, and another in charge of the Museum. There have been, therefore, periods in the history of the Institution when there was but one Assistant Secretary, a long period immediately preceding the present incumbency when there was none, and subsequently a brief period when there were two. The work of the Institution has enormously increased, even since the death of Secretary Baird.

In regard to subordinate positions, the power of appointment has been exercised by the successive Secretaries for a period of over 40 years, and with an absence of any suggestion of favoritism, partiality, or harshness, which is rare in office. In all these appointments and in every official relationship the recognition of the plenary authority of the Regents, as exercised through the Secretary, has been the foundation of good government. It is because the authority of the Board and its method of action is so absolutely recognized that it has been so very rarely needed to display it to make it effective, and thus during ten years of the Museum's administration I do not recall a single occasion in which the Secretary has not found himself able to act in accordance with the wish of the Assistant Secretary in all appointments and removals.

Nothing can be more desirable in administration than a condition which can permit this freedom without fear of laxity. It was justified in this case, as it is hoped it will always be justified, by the fact that the person in this important position possessed the entire confidence of the Secretary.

It is more than ever before desirable that the officer in charge of the Museum shall be a man of wide experience in administrative affairs, as well as a man of scientific position, and that the whole body of scientific men throughout the country shall be eligible, but I have found myself unable as yet, after the most anxious pains, to present any name to the Regents which appears entirely satisfactory.

After laying before the Regents the names of different gentlemen proposed for the position, with the recommendatory letters, the Secretary said that most of these, with others he had considered among the more prominent ones of the country, were without the civil service and came under the exclusions of its rules. He would add that the names outside the civil service among whom the Institution would naturally seek were those of persons already enjoying positions of high trust, and who, in several of these instances, could only accept the place at a heavy pecuniary sacrifice.

The Secretary then presented the name of Mr. Charles D. Walcott, honorary curator of the Museum, and now director of the United States Geological Survey, and after explaining the grounds for his confidence in Mr. Walcott and the reasons which made it impracticable for the latter to then accept the position, while it was desirable that some immediate, if provisionary, arrangement should be made, asked the consent of the Regents to the appointment of Mr. Walcott as acting assistant secretary of the Smithsonian Institution, with the understanding that his duties to the Institution were to be confined exclusively to the charge of the Museum. Senator Morrill then introduced the following resolution, which was adopted:

Resolved, That the appointment by the Secretary of Prof. Charles D. Walcott as acting assistant secretary of the Smithsonian Institution, with duties confined to the charge of the National Museum, be approved.

The Secretary then said:

I have received the following letter from the President of the United States:

"EXECUTIVE MANSION,
"*Washington, June 18, 1896.*

"DEAR SIR: I inclose you a classification of the employees of the Smithsonian Institution, which is a copy of those signed by the other Executive Departments and bureaus.

"I wish you would insert the date and sign it and notify the Civil Service Commission that you have done so. I think it should be filed in your office.

"Yours, truly,
"GROVER CLEVELAND.

"S. P. LANGLEY, *Secretary, etc.*"

It has been taken as a matter of course that this order does not apply to the parent institution, and it is only as regards its application to Smithsonian bureaus supported wholly or in part by Government appropriations that I bring the letter to the attention of the Regents. In the absence of their instructions the expression of the President's wish has been taken as a command, and the classification referred to signed.

The Secretary then communicated to the Regents a letter from one of their number, the Hon. W. L. Wilson, Postmaster-General, on the subject of the relations of the Smithsonian bureaus with the United States civil service. Mr. Wilson also personally communicated to the Board the fact that he had had a conference on this subject with the President, who recognized that his letter could not apply to the Smithsonian Institution proper, which was not supported by Government appropriations.

The Regents then discussed with some fullness the subject of these relationships, and without adopting any resolution, indicated a line of action which seemed to them suitable.

The Secretary stated that he had been requested to appear before the Joint Committee on the Library in the early part of last month, when he had been asked what the National Museum had which would serve to decorate the new Library building.

Senator Gray, after discussion, offered the following resolution, which was adopted:

Resolved, That in the opinion of the Board of Regents of the Smithsonian Institution it will not be expedient or wise to interfere with the integrity of the National Museum by lending, for the decoration of the Library building, any of the articles or property now in its care.

The Secretary then exhibited to the Board the "Half Century" volume, explaining to what degree its preparation had arrived. He also read a letter from Mrs. Coppée, conveying her acknowledgment of the resolutions adopted on the death of her husband, Dr. Coppée.

The Secretary then exhibited the Hodgkins medals, in silver and bronze, with a statement of the number sent to contestants.

Mr. Hubbard presented resolutions to the effect that the Chancellor should appoint a committee, of which the Secretary of the Smithsonian Institution should be ex officio, a member, to inquire into the condition of various bureaus, with special reference to what could best be done to increase their usefulness. Senator Henderson expressed the wish that the Regents would revise the work of the Executive Committee, and stated that the Institution needed help in Congress.

Senator Morrill remarked that, in view of the lateness of the hour, the resolutions which had been proposed should be considered at another meeting, and at the suggestion of the Chancellor it was arranged that an adjourned meeting should be held on February 1, at 10 o'clock a. m.

FEBRUARY 1, 1897.

The Board met this morning.

Present: The Chancellor (Mr. Chief Justice Fuller) in the chair; the Vice-President, the Hon. A. E. Stevenson; the Hon. J. S. Morrill, the Hon. S. M. Cullom, the Hon. George Gray, the Hon. Joseph Wheeler, the Hon. R. R. Hitt, the Hon. W. L. Wilson, Dr. Andrew D. White, the Hon. J. B. Henderson, the Hon. Gardiner G. Hubbard, and the Secretary, Mr. S. P. Langley.

At the Chancellor's suggestion an abstract of the minutes of the meeting of the 27th of January was read and approved.

The Secretary then read the resolutions introduced by Mr. Hubbard, who made an explanation of their purpose. A general discussion ensued, and the resolutions were finally adopted in the following form:

Resolved, That a committee of five be appointed by the Chancellor, the Secretary of the Smithsonian Institution being ex officio a member, upon the National Museum, the Bureau of American Ethnology, and the National Zoological Park, with especial reference to the ascertainment of what can be done to promote their usefulness and value, and to report to the Board of Regents at its next meeting.

The Chancellor appointed as members of the committee Mr. Hubbard, Senator Cullom, Mr. Henderson, Mr. Wilson, and the Secretary.

The Secretary then said that a matter of importance had just been brought to his attention in connection with a difficulty which had occurred to the Chancellor with regard to the possible future appointment of an acting secretary, and which in the Chancellor's opinion made it, though not indispensable, yet desirable, that an assistant secretary should be appointed.

The Secretary said that Mr. Richard Rathbun was a person whose relationships to the Institution had been intimate for nearly twenty years, and who had the Secretary's personal confidence. He now held the position of aid-in-charge of the Institution, and possessed both the ability and experience which would warrant his selection as assistant secretary. The Secretary accordingly asked the consent of the Regents to his appointment.

The Vice-President then offered the following resolution, which was adopted:

Resolved, That the appointment by the Secretary of Mr. Richard Rathbun as assistant secretary of the Smithsonian Institution, with duties connected with the bureaus of the Institution other than the National Museum, be approved.

The Secretary presented a suggestion made by Mr. Walcott in his capacity as Director of the Geological Survey, and after discussion by the Regents Senator Gray offered the following resolution, which was adopted:

Resolved, That the Board of Regents of the Smithsonian Institution look with favor upon the proposition to establish a museum of practical and industrial geology in the neighborhood of the National Museum.

There being no further business to come before the Board, on motion it adjourned.

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

FOR THE YEAR ENDING JUNE 30, 1897

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 Zoological Park, and the Astro-Physical Observatory for the year ending June 30, 1897, and balances of former years:

SMITHSONIAN INSTITUTION.

Condition of the fund July 1, 1897.

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; a gift from Thomas G. Hodgkins, of New York, of $200,000, and $8,000, being a portion of the residuary legacy of Thomas G. Hodgkins, and $1,000, the accumulated interest on the Hamilton bequest, making in all, as the permanent fund, $912,000.

The Institution also holds the additional sum of $42,000, received upon the death of Thomas G. Hodgkins, in registered West Shore Railroad 4 per cent bonds, which were, by order of this committee, under date of May 18, 1894, placed in the hands of the Secretary of the Institution, to be held by him subject to the conditions of said order.

Statement of the receipts and expenditures from July 1, 1896, to June 30, 1897.

RECEIPTS.

Cash on hand July 1, 1896		$57,065.78	
Interest on fund July 1, 1896	$27,360.00		
Interest on fund January 1, 1897	27,360.00		
		54,720.00	
Interest to January 1, 1897, on West Shore bonds		1,680.00	
			$113,465.78
Cash from sales of publications		460.95	
Cash from repayments, freight, etc		5,667.76	
			6,128.71
Total receipts			119,594.49

EXPENDITURES.

Building:			
Repairs, care, and improvements	$2,201.82		
Furniture and fixtures	225.24		
		$2,427.06	
General expenses:			
Postage and telegraph	226.85		
Stationery	972.35		
General printing	357.23		
Incidentals (fuel, gas, etc.)	4,549.64		
Library (books, periodicals)	2,478.31		
Salaries[1]	22,001.03		
Gallery of art	18.00		
Meetings	159.50		
		30,762.91	
Publications and researches:			
Smithsonian Contributions	3,205.19		
Miscellaneous collections	6,127.95		
Reports	689.26		
Special publications	739.99		
Researches	3,984.06		
Apparatus	151.43		
Hodgkins fund	6,349.77		
Explorations	300.00		
		21,547.65	
Literary and scientific exchanges		3,324.37	
			58,061.99
Balance unexpended June 30, 1897			61,532.50

The cash received from the sale of publications, from repayments for freights, etc., is to be credited to the items of expenditure as follows:

Smithsonian Contributions	$247.60
Miscellaneous collections	166.20
Reports	11.11
Special publications	36.04
	460.95

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[1] In addition to the above, $22,001.03, paid for salaries under general expenses, $6,860.64 were paid for services, viz, $2,036 charged to building account, $965.87 to Hodgkins fund account, $1,361.05 to library account, $2,439.97 to researches account, and $57.75 to reports account.

Hodgkins fund	$75.00
Zoological Park	499.45
Exchanges	3,334.33
Incidentals	958.98
Explorations	700.00
Salaries	100.00
	6,128.71

The net expenditures of the Institution for the year ending June 30, 1897, were therefore $51,933.28, or $6,128.71 less than the gross expenditures, $58,061.99, as above stated.

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:

Detailed statement of disbursements from appropriations committed by Congress to the care of the Smithsonian Institution for the fiscal year ending June 30, 1897, and from balances of former years.

INTERNATIONAL EXCHANGES.

Receipts.

Appropriated by Congress for the fiscal year ending June 30, 1897, "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" (sundry civil act, June 11, 1896)	$19,000.00

Disbursements from July 1, 1896, to June 30, 1897.

Salaries or compensation:	
1 curator, 2½ months 5 days, at $225	$600.00
1 curator, 6 months, at $225	1,350.00
1 chief clerk, 7 months, at $150	1,050.00
1 chief clerk, 5 months, at $175	875.00
1 clerk, 12 months, at $130	1,560.00
1 clerk, 12 months, at $100	1,200.00
1 clerk, 12 months, at $85	1,020.00
1 clerk, 12 months, at $75	900.00
1 clerk, 12 months, at $70	840.00
1 stenographer, 12 months, $60	720.00
1 clerk, 10 months, at $45	450.00
1 copyist, 12 months, at $35	420.00
1 packer, 10½ months, at $55	577.50
1 clerk, 2 months, at $100	200.00
1 messenger, 7 months, at $25	175.00
1 messenger, 4½ months 10 days, at $25	121.43
1 carpenter, 24¾ days, at $3	74.25
1 laborer, 313 days, at $1.50	469.50
1 laborer, 313 days, at $1.50	469.50

Salaries or compensation—Continued.			
1 cleaner, 92 days, at $1		$92.00	
1 agent, 12 months, at $91.66⅔		1,100.00	
1 agent, 12 months, at $50		600.00	
Total of salaries or compensation		14,864.18	
General expenses:			
Freight	$2,291.56		
Boxes	710.20		
Postage	200.00		
Stationery and supplies	397.15		
Traveling expenses	357.28		
		3,956.19	
Total disbursements			18,820.37
Balance July 1, 1897, to meet liabilities			179.63

INTERNATIONAL EXCHANGES, 1896.

Balance July 1, 1896, as per last report		$180.92

Disbursements.

Freight	$167.12	
Supplies	13.77	
		180.89
Balance July 1, 1897		.03

INTERNATIONAL EXCHANGES, 1895.

Balance July 1. 1896, as per last report	$0.21

Balance carried, under the provisions of the Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1897.

NORTH AMERICAN ETHNOLOGY, 1897.

Appropriation by Congress for the fiscal year ending June 30, 1897, "for continuing ethnological researches among the American Indians, under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employees, $45,000, of which sum not exceeding $1,000 may be used for rent of building" (sundry civil act, June 11, 1896)	$45,000.00

The actual conduct of these investigations has been continued by the Secretary in the hands of Maj. J. W. Powell, Director of the Bureau of American Ethnology.

Disbursements July 1, 1896, to June 30, 1897.

Salaries or compensation:		
1 director, 12 months, at $375		$4,500.00
1 ethnologist in charge,	4 months, at $300	1,200.00
	8 months, at $325	2,600.00
1 special ethnologist, 12 months, at $200		2,400.00
1 ethnologist,	4 months, at $150	600.00
	8 months, at $166.67	1,333.36
1 ethnologist,	4 months, at $150	600.00
	8 months, at $166.67	1,333.36
1 ethnologist, 12 months, at $150		1,800.00
1 ethnologist,	½ month, at $125	62.50
	11½ months, at 150	1,725.00

Salaries or compensation—Continued.

1 ethnologist, 4 months, at $116.66		$466.64	
8 months, at $125		1,000.00	
1 ethnologist, 12 months, at $125		1,500.00	
1 ethnologist, 12 months, at $110		1,320.00	
1 ethno-photographer, 4½ months, at $116.66		524.97	
1 custodian, 12 months, at $100		1,200.00	
1 illustrator, 6 months, at $100		600.00	
1 clerk, 12 months, at $100		1,200.00	
1 clerk, 3 months, at $100		300.00	
1 clerk, 4 months, at $83.33		333.32	
8 months, at $100		800.00	
1 clerk, 12 months, at $75		900.00	
1 clerk, 12 months, at $75		900.00	
1 messenger, 12 months, at $60		720.00	
1 copyist, 12 months, at $40		480.00	
1 messenger, 12 months, at $50		600.00	
1 skilled laborer, 12 months, at $60		720.00	
1 skilled laborer, 12 months, at $45		540.00	
Total salaries		32,259.15	
General expenses:			
Drawings and illustrations	$1,429.70		
Freight	216.39		
Postage, telegraph, etc	120.00		
Publications	1,474.06		
Office furniture	21.00		
Rental	999.96		
Special services	1,231.66		
Specimens	378.22		
Stationery	330.60		
Supplies	1,750.43		
Traveling and field expenses	3,859.34		
Reports	517.40		
Miscellaneous	194.05		
		12,522.81	
Total disbursements			44,781.96
Balance July 1, 1897			218.04

NORTH AMERICAN ETHNOLOGY, 1896.

Balance July 1, 1896, as per last report	$1,444.13

Disbursements.

Freight	$218.80	
Postage, telegraph, etc	42.50	
Services	500.80	
Specimens	50.00	
Supplies	41.22	
Traveling and field expenses	534.29	
Total disbursements		1,387.61
Balance July 1, 1897		56.52

NORTH AMERICAN ETHNOLOGY, 1895.

Balance July 1, 1896, as per last report	$100.08

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1897.

NATIONAL MUSEUM.

PRESERVATION OF COLLECTIONS, JULY 1, 1896, TO JUNE 30, 1897.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1897, "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 employees" (sundry civil act, June 11, 1896)	$153,225.00

Expenditures.

Salaries or compensation	$134,357.74		
Special services	4,654.33		
Total services		$139,012.07	
Miscellaneous:			
Supplies	2,833.84		
Stationery	1,018.58		
Specimens	3,179.57		
Books	1,311.12		
Travel	438.99		
Freight	1,228.90		
		10,011.00	
Total expenditure			149,023,07
Balance July 1, 1897, to meet liabilities			4,201.93

Analysis of expenditures for salaries or compensation.

DIRECTION.

1 Assistant Secretary of the Smithsonian Institution, in charge of the National Museum, 2 months, at $333.34	$666.68

SCIENTIFIC STAFF.

1 executive curator { 9 months, at $225 3 months, at $250 }	$2,775.00
1 curator, 11 months 5 days, at $167	1,864.83
3 curators, 12 months, at $200	7,200.00
1 curator { 3 months 19 days, at $175 8 months 12 days, at $200 }	2,309.68
1 curator, 12 months, at $175	2,100.00
1 acting curator, 11 months 5 days, at $150	1,675.00
1 assistant curator, 12 months, at $150	1,800.00
1 assistant curator, 12 months, at $130	1,560.00
1 assistant curator, 2 months 23½ days, at $125	344.76
1 assistant curator, 12 months, at $125	1,500.00
1 assistant curator { 6 months, at $100 6 months, at $116.66 }	1,299.96

1 assistant curator, 12 months, at $84	$1,008.00
1 assistant curator, 12 months, at $150	1,800.00
1 assistant curator, 12 months, at $135	1,620.00
1 assistant curator, 12 months, at $115	1,380.00
1 second assistant curator, 12 months, at $80	960.00
1 aid, 12 months, at $50	600.00
1 aid, 12 months, at $80	960.00
1 aid, 32 days, at $40	41.46
1 aid, 10 months, at $100	1,000.00
1 aid, 12 months, at $100	1,200.00
1 aid, 4 months 42 days, at $50	268.39
1 aid, 2 months 29 days, at $35	103.27
1 aid 6 months, at $84 6 months, at $100	1,104.00
1 collector, 5 months, at $100	500.00
	36,974.35

PREPARATORS.

1 photographer, 12 months, at $158.33	1,899.96
1 osteologist, 12 months, at $90	1,080.00
1 preparator, 12 months, at $50	600.00
1 preparator, 30 days, at $3.20	96.00
1 preparator, 1 month 5 days, at $50	58.06
1 preparator, 12 months, at $80	960.00
1 preparator, 9 months 24 days, at $60	589.29
1 preparator, 10 months 21 days, at $80	854.19
1 preparator, 12 months, at $45	540.00
1 preparator, 12 months, at $110	1,320.00
1 preparator, 12 months, at $80	960.00
1 preparator, 12 months, at $45	540.00
1 preparator, 12 months, at $50	600.00
1 taxidermist, 12 months, at $60	720.00
1 taxidermist, 12 months, at $90	1,080.00
1 taxidermist, 9 months 24 days, at $100	982.14
1 taxidermist, 8 months 21 days, at $100	870.85
1 taxidermist 3 months 34 days, at $50 2 months 25 days, at $40 1 month 28½ days, at $60	436.94
	14,187,43

CLERICAL STAFF.

1 chief clerk, 6 months, at $200 6 months, at $208.33	2,450.00
1 editor, 12 months, at $167	2,004.00
1 chief of division, 12 months, at $200	2,400.00
1 registrar, 12 months, at $167	2,004.00
1 disbursing clerk, 12 months, at $116.67	1,400.04
1 assistant librarian, 12 months, at $117	1,404.00
1 property clerk, 6 months, at $100 6 months, at $115	1,290.00
1 stenographer, 12 months, at $45	540.00
1 stenographer, 3 months, 6 days, at $50	159.68
1 stenographer, 4 months, 19 days, at $100	467.86
1 stenographer, 7 months, 11 days, at $120 4 months, 17 days, at $150	1,578.21

1 typewriter, 7 months, at $50	$350.00
1 typewriter, 12 months, at $50	600.00
1 typewriter, 12 months, at $75	900.00
1 clerk, 12 months, at $83.34	1,000.08
1 clerk, 10 months, at $55	550.00
1 clerk, 3 months, at $50 6 months, at $60	510.00
1 clerk, 12 months, at $90	1,080.00
1 clerk, 12 months, at $55	660.00
1 clerk, 12 months, at $55	660.00
1 clerk, 12 months, at $60	720.00
1 clerk, 12 months, at $50	600.00
1 clerk, 12 months, at $55	660.00
1 clerk, 12 months, at $115	1,380.00
1 clerk, 12 months, at $70	840.00
1 clerk, 12 months, at $100	1,200.00
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, 12 months, at $50	600.00
1 clerk, 12 months, at $75	900.00
1 clerk, 2 months, 31 days, at $50	151.51
1 clerk, 2 months, at $60	120.00
1 clerk, 10 days, at $60	19.35
1 clerk, 12 months, at $50	600.00
1 clerk, 12 months, at $50	600.00
1 clerk, 12 months, at $60	720.00
1 clerk, 12 months, at $50	600 00
1 clerk, 12 months, at $115	1,380.00
1 clerk, 6 months, at $100	600.00
1 clerk, 12 months, at $50	600.00
1 clerk, 12 months, at $90	1,080.00
1 clerk, 12 months, at $45	540.00
1 copyist, 12 months, at $35	420.00
1 copyist, 12 months, at $45	540.00
1 copyist, 12 months, at $25	300.00
1 copyist, 10 months, 3 days, at $40	403.87
1 copyist, 7 months, 23 days, at $25	194.17
1 copyist, 12 months, at $45	540.00
1 copyist, 12 months, at $45	540.00
1 copyist, 9 months, 15 days, at $45	427.50
1 copyist, 10 months, 24 days, at $40	430.97
1 copyist, 12 months, at $30	360.00
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, 12 months, at $35	420.00
1 copyist, 12 months, at $35	420.00
1 copyist, 12 months, at $45	540.00
1 copyist, 1 day, at $40	1.43
1 copyist, 12 months, at $30	360.00
1 copyist, 6 months, 8 days, at $40	250.32
1 copyist, 12 months, at $30	350.00
	45,666.99

BUILDINGS AND LABOR.

1 superintendent, 2 months, 29 days, at $137.50	$407.92
1 assistant superintendent, 6 months, at $110 $115	1,350.00
1 foreman, 12 months, at $50	600.00
1 chief of watch, 12 months, at $65	780.00
1 chief of watch, 12 months, at $65	780.00
1 chief of watch, 12 months, at $65	780.00
1 watchman, 12 months, at $50	600.00
1 watchman, 10 months, 53 days, at $40	469.46
1 watchman, 11 months, 29 days, at $50	598.34
1 watchman, 4 months, at $45 8 months, at $50	580.00
1 watchman, 3 months, at $40 9 months, at $50	570.00
1 watchman, 12 months, at $65	780.00
1 watchman, 2 months, 25 days, at $45	127.50
1 watchman, 12 months, at $45	540.00
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00
1 watchman, 1 month, 56 days, at $45	129.00
1 watchman, 1 month, at $45	45.00
1 watchman, 12 months, at $45	540.00
1 watchman, 12 months, at $45	540.00
1 watchman, 12 months, at $45	540.00
1 watchman, 11 months, 9 days, at $50	516.07
1 watchman, 12 months, at $50	600.00
1 watchman, 3 months, at $50 2 months, at $60	270.00
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00
1 watchman, 6 months, 25 days, at $45	306.29
1 acting watchman, 11 months, 28 days, at $45	535.65
1 acting watchman, 5 months, 48 days, at $35	229.91
1 skilled laborer, 2 months, at $55	110.00
1 skilled laborer, 12 months, at $50	600.00
1 workman, 12 months, at $50	600.00
1 workman, 315 days, at $1.50	472.50
1 workman, 364 days, at $1.50	546.00
1 workman, 321 days, at $1.50	481.50
1 workman, 359 days, at $1.50	538.50
1 workman, 364 days, at $1.50	546.00
1 workman, 327 days, at $1.50	490.50
1 workman, 8 months, at $45 1 month, at $51 2 months, at $48 1 month, at $46.50	553.50
1 workman, 341 days, at $1.50	511.50
1 workman, 335 days, at $1.50	502.50
1 laborer, 313 days, at $1.50	469.50
1 laborer, 7 days, at $1.50	10.50
1 laborer, 313 days, at $1.50	469.50
1 laborer, 7 days, at $1.50	10.50
1 laborer, 102½ days, at $1.50	159.75

1 laborer, 10 months, at $45 1 month, at $46.50 1 month, at $48	$544.50
1 laborer, 5 months, at $40 184 days, at $1.50	476.00
1 laborer, 5 months, 27 days, at $40	234.84
1 laborer, 239 days, at $1.50	358.50
1 laborer, 271 days, at $1.50	406.50
1 laborer, 313 days, at $1.50	469.50
1 laborer, 9 months, at $40 1 month, at $44.50 1 month, at $47.50 1 month at $46	498.00
1 laborer, 12 months, at $40	480.00
1 laborer, 238½ days, at $1.50	357.75
1 laborer, 11 months, at $40 1 month, at $41.50	481.50
1 laborer, 235 days, at $1.50	352.50
1 laborer, one-half day, at $1.50	.75
1 laborer, 12 months, at $40	480.00
1 laborer, 131 days, at $1.50	196.50
1 laborer, 2½ days, at $1.50	3.75
1 laborer, 14 days, at $40	18.49
1 laborer, 285 days, at $1.50	427.50
1 laborer, 12 months, at $40	480.00
1 laborer, 288 days, at $1.50	432.00
1 laborer, 8 months, at $40 1 month, $40.75	360.75
1 laborer, 247 days, at $1.50	370.50
1 laborer, one-half day, at $1.50	.75
1 laborer, 313 days, at $1.50	469.50
1 laborer, 28¾ days, at $1.50	43.13
1 laborer, 12 months, at $40	480.00
1 messenger, 12 months, at $20	240.00
1 messenger, 12 months, at $20	240.00
1 messenger, 4 months 16 days, at $20	91.43
1 messenger, 9 months, at $30	270.00
1 messenger, 6 months 32 days, at $20	140.85
1 messenger, 6 months, at $20 6 months, at $25	270.00
1 messenger, 4 months, 42 days, at $25	209.41
1 messenger, 4 months, at $45	180.00
1 messenger, 4 months, at $25 6 months, 61 days, at $40	420.00
1 messenger, 12 months, at $30	360.00
1 attendant, 12 months, at $40	480.00
1 cleaner, 12 months, at $30	360.00
1 cleaner, 3 months, at $30	90.00
1 cleaner, 12 months, at $30	360.00
1 cleaner, 12 months, at $30	360.00
1 cleaner, 12 months, at $30	360.00
1 cleaner, 12 months, at $30	360.00
1 cleaner, 12 months, at $30	360 00
	36,862.29
Total salaries	134,357.74

Preservation of Collections, 1896.

Balance, as per last annual report, July 1, 1896		$2,846.53
Expenditures.		
Special service	$392.01	
Supplies	508.96	
Stationery	54.94	
Specimens	1,115.13	
Books	617.28	
Travel	3.45	
Freight	153.44	
Total expenditure		2,845.21
Balance July 1, 1897		1.32

Total Expenditure of the Appropriation for Preservation of Collections, 1896.

Appropriation			$143,225.00
Expenditures.			
Services or compensation	$125,950.49		
Extra services	3,308.26		
Total services		$129,258.75	
Miscellaneous:			
Supplies	$3,013.66		
Stationery	820.90		
Specimens	4,925.38		
Books	2,862.67		
Travel	602.80		
Freight	1,739.52		
Total		13,964.93	
			143,223.68
Balance July 1, 1897			1.32

Preservation of Collections, 1895.

Balance, as per last report, July 1, 1896		$42.31
Expenditures.		
Books	$31.26	
Freight	4.93	
Total expenditure		36.19
Balance July 1, 1897		6.12

Balance carried, under the provisions of the Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1897.

Total Expenditure of the Appropriation for Preservation of Collections, 1895.

Appropriation		$143,000.00
Expenditures.		
Salaries	$126,142.26	
Special services	4,064.33	
Total services		130,206.59

Supplies	$3,183.65	
Stationery	1,076.00	
Specimens	3,366.47	
Travel	676.25	
Freight	1,920.84	
Books	2,564.08	
Total expenditure		$142,993.88
Balance		6.12

FURNITURE AND FIXTURES, JULY 1, 1896, TO JUNE 30, 1897.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1897, "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" (sundry civil act, June 11, 1896)	$15,000.00

Expenditures

Salaries or compensation	$8,062.43		
Special services	247.65		
Total salaries		$8,310.08	
Miscellaneous:			
Cases	150.50		
Drawers	286.93		
Frames	36.80		
Glass	613.74		
Hardware	954.99		
Tools	83.54		
Cloth	86.44		
Glass jars	625.52		
Lumber	837.59		
Paints and oils	369.73		
Office furniture	606.62		
Rubber	34.64		
Plumbing	51.00		
Iron brackets	146.81		
Brick	4.00		
		4,888.85	
Total expenditures			13,198.93
Balance July 1, 1897, to meet liabilities			1,801.07

Analysis of expenditures for salaries.

1 clerk, 2 months, 24 days, at $50	$138.71
1 copyist, 12 months, at $40	480.00
1 cabinetmaker, 313 days, at $3	939.00
1 carpenter, 243 days, at $3	729.00
1 carpenter, 176½ days, at $3	529.50
1 carpenter, 313 days, at $3	939.00
1 carpenter, 62 days, at $3	186.00
1 carpenter, 97 days, at $3	291.00
1 carpenter, 190½ days, at $3	571.50
1 carpenter, 46 days, at $3	138.00

1 carpenter, 10 days, at $3	$30.00
1 carpenter, 111 days, at $3	333.00
1 carpenter, 7 days, at $3	21.00
1 carpenter, 30 days, at $3	90.00
1 carpenter, 5 days, at $3	15.00
1 carpenter, 9 days, at $3	27.00
1 carpenter, 7 days, at $3	21.00
1 painter, 2 months, 21 days, at $65	176.05
1 skilled laborer, 301 days, at $2	602.00
1 skilled laborer, 7 months, 21 days, at $60 1 skilled laborer, 1 month, at $64	526.52
1 skilled laborer, 12 months, at $50	600.00
1 skilled laborer, 73 days, at $2	146.00
1 skilled laborer, 13 days, at $2	26.00
1 workman, 320 days, at $1.50	480.00
1 workman, 90½ hours, at 30 cents	27.15
Total salaries	8,062.43

FURNITURE AND FIXTURES, 1896.

Balance July 1, 1896, as per last report		$1,315.09

Expenditures.

Special services	$80.00	
Drawers	331.60	
Glass	17.01	
Hardware	532.23	
Tools	58.74	
Cloth	4.00	
Glass jars	41.80	
Lumber	223.98	
Paints	11.75	
Office furniture	8.50	
Rubber	5.28	
Total expenditures		1,314.89
Balance July 1, 1897		.20

TOTAL EXPENDITURE OF THE APPROPRIATION FOR FURNITURE AND FIXTURES, 1896.

Receipts.

Appropriation		$12,500.00

Expenditures.

Salaries	$5,866.70	
Special services	474.75	
	6,341.45	
Cases	300.00	
Drawings	22.88	
Drawers	1,387.35	
Frames	5.00	
Glass	240.58	
Hardware	1,185.28	
Tools	77.70	
Cloth	115.73	
Glass jars	456.41	

Lumber	$1,171.52	
Paints	358.05	
Office furniture	201.86	
Rubber	75.54	
Plumbing	463.00	
Apparatus	6.45	
Iron brackets	91.00	
Total expenditure		$12,499.80
Balance July 1, 1897		.20

FURNITURE AND FIXTURES, 1895.

Balance July 1, 1896, as per last annual report	$0.53

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1897.

HEATING, LIGHTING, ETC., JULY 1, 1896, TO JUNE 30, 1897.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1897, "for expense of heating, lighting, electrical, telegraphic, and telephonic service for the National Museum (sundry civil act, June 11, 1896)	$13,000.00

Expenditures.

Salaries or compensation	$6,269.05	
Special services	21.75	
Total services	6,290.80	

General expenses.

Coal and wood	3,676.82	
Gas	966.90	
Telephones	499.54	
Electric supplies	426.61	
Rental of call boxes	110.00	
Heating supplies	281.87	
Telegrams	5.35	
Total expenditure		12,257.89
Balance, July 1, 1897, to meet liabilities		742.11

HEATING AND LIGHTING, 1897.

Analysis of expenditure for salaries.

1 engineer, 12 months, at $115	$1,380.00
1 telephone operator, 12 months, at $45	540.00
1 fireman, 12 months, at $50	600.00
1 fireman, 12 months, at $50	600.00
1 fireman, 12 months, at $50	600.00
1 acting fireman, 10 months 52 days, at $45	526.60
1 skilled laborer, 12 months, at $75	900.00
1 skilled laborer, 12 months, at $60	720.00
1 skilled laborer, 1 month at $55 29 days at $51.45	106.45
1 laborer, 2 months, at $40	80.00
1 laborer, 144 days, at $1.50	216.00
Total expenditure for salaries	6,269.05

HEATING, LIGHTING, ETC., 1896.

Balance as per last report, July 1, 1896		$947.33

Expenditures.

Special service	$60.00	
Gas	84.38	
Telephones	139.25	
Rental of call boxes	10.00	
Heating supplies	338.38	
Electric supplies	311.43	
Telegrams	3.47	
Total expenditure		946.91
Balance July 1, 1897		.42

TOTAL EXPENDITURE OF THE APPROPRIATION FOR HEATING AND LIGHTING, ETC., 1896.

Appropriation		$13,000.00

Expenditures.

Salaries or compensation	$4,984.95	
Special services	99.50	
Total services	5,084.45	
Coal and wood	3,202.32	
Gas	1,625.01	
Telephones	551.75	
Electric supplies	1,793.87	
Rental of call boxes	120.00	
Heating supplies	506.10	
Telegrams	12.08	
Heating repairs	104.00	
Total expenditure		12,999.58
Balance July 1, 1897		.42

HEATING, LIGHTING, ETC., 1895.

Balance July 1, 1896, as per last annual report	$1.15

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1897.

POSTAGE, JULY 1, 1896, TO JUNE 30, 1897.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1897, "for postage stamps and foreign postal cards for the National Museum" (sundry civil act, June 11, 1896)	$500.00

Expenditure.

City post-office, for stamps and cards	$500.00

PRINTING, JULY 1, 1896, TO JUNE 30, 1897.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1897, "for the Smithsonian Institution for printing labels and blanks and for the 'Bulletins' and annual volumes of the 'Proceedings' of the National Museum, the editions of which shall not be less than three thousand copies, and binding scientific books and pamphlets presented to and acquired by the National Museum Library" (sundry civil act, June 11, 1896)	$12,000.00

Expenditures.

Government Printing Office, for Bulletins, National Museum, Nos. 47 and 49, and Special Bulletins Nos. 2 and 3	$7,718.39	
Proceedings National Museum, vols. 18 and 19	3,640.14	
Labels	239.50	
Letter-heads, pads, and envelopes	49.00	
Blanks	86.49	
Binding	222.15	
Congressional Records	36.00	
Total expenditure		11,991.67
Balance July 1, 1897		8.33

RENT OF WORKSHOPS, JULY 1, 1896, TO JUNE 30, 1897.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1897, "for rent of workshops for the National Museum" (sundry civil act, June 11, 1896)	$2,000.00

Expenditure.

Rent of shops, 12 months, $166.66	1,999.92
Balance July 1, 1897	0.08

RENT OF WORKSHOPS, 1896.

Balance, as per last report, July 1, 1896	$75.00

Expenditure:

Rent of shops (June)	$75.00

NATIONAL MUSEUM: RENT OF WORKSHOPS, 1895.

Balance July 1, 1896, as per last annual report	$12.54

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1897.

NATIONAL MUSEUM: BUILDING REPAIRS, JULY 1, 1896, TO JUNE 30, 1897.

Receipts:

Appropriation by Congress for the fiscal year ending June 30, 1897, "for repairs to buildings, shops, and sheds, National Museum, including all necessary labor and material" (sundry civil act, June 11, 1896)	$4,000.00

Expenditures:

Salaries	$2,792.37		
Special services	489.00		
Total services		$3,281.37	
Miscellaneous:			
Lumber	78.89		
Frames and woodwork	486.30		
Glass	30.29		
Hardware	5.20		
Brick	2.70		
		603.38	
Total expenditure			$3,884.75
Balance July 1, 1897			115.25

Analysis of expenditure for salaries.

1 carpenter, 228½ days, at $3	$685.50
1 carpenter, 27 days, at $3	81.00
1 carpenter, 27 days, at $3	81.00
1 carpenter, 27 days, at $3	81.00
1 painter, 6 months, at $65	390.00
1 skilled laborer, 211½ days, at $2	423.00
1 skilled laborer, 239 days, at $2	478.00
1 skilled laborer, 158 days, at $2	316.00
1 skilled laborer, 2 months, 81 days, at $55	256.87
Total expenditures for salaries	2,792.37

National Museum: Building Repairs, 1896.

Balance, as per last report, July 1, 1896		$929.51

Expenditures.

Granito and mosaic flooring	$799.62	
Paints and brushes	100.33	
Lumber	3.24	
Advertising	24.94	
Total expenditure		928.13
Balance July 1, 1897		1.38

Total Expenditure of Appropriation for Building Repairs, 1896.

Appropriation by Congress		$4,000.00

Expenditures.

Services	$1,965.15	
Granito and mosaic flooring	1,399.62	
Paints	489.31	
Glass	28.00	
Advertising	63.13	
Lumber	12.24	
Hardware	6.42	
Brick, cement, etc	23.50	
Brushes	11.25	
Total expenditure		3,998.62
Balance July 1, 1897		1.38

National Museum: Building Repairs, 1895.

Balance July 1, 1897, as per last annual report	$4.78

Balance carried, under the provisions of Revised Statutes, section 3090, by the Treasury Department, to the credit of the surplus fund, June 30, 1897.

National Museum: Galleries, July 1, 1896, to June 30, 1897.

Receipts.

Appropriation by Congress for the fiscal year ending June 30, 1897, "for the erection of galleries in two or more halls of the National Museum building, said galleries to be constructed of iron beams, supported by iron pillars, and protected by iron railings, and provided with suitable staircases, the work to be done under the direction of the Architect of the Capitol, and in accordance with the approval of the Secretary of the Smithsonian Institution" (sundry civil act, June 11, 1896)	$8,000.00

Expenditures.

Salaries	$246.25		
Special services	272.78	$519.03	
Steel beams and iron columns	3,200.00		
Drawings and blue prints	141,95		
Brick, sand, cement, gravel	54.05		
Advertising proposals	60.62	3,456.62	
Total expenditure			3,975.65
Balance July 1, 1897			4,024.35

Analysis of expenditure for salaries.

1 skilled laborer, 3 months, at $55	$165.00	
1 laborer, 1 month	42.25	
1 laborer, 26 days, at $1.50	39.00	246.25

Astrophysical Observatory—Smithsonian Institution, 1897

Receipts.

Appropriation by Congress "for maintenance of Astrophysical Observatory, under the direction of the Smithsonian Institution, including salaries of assistants, apparatus, and miscellaneous expenses" (sundry civil act, June 11, 1896)	$10,000.00

Disbursements from July 1, 1896 to June 30, 1897.

Salaries or compensation:	
1 aid, 12 months, at $133.34	$1,600.08
1 junior assistant, 12 months, at $100	1,200.00
1 junior assistant, 3 months, at $75	300.00
1 clerk, 1 month, at $100	100.00
1 clerk, one month, 8 days, at $40	50.32
1 stenographer, 6 months, 18 days, at $60	394.84

Salaries or compensation—Continued.			
1 instrument maker, 3 months, at $65		$195.00	
1 instrument maker, 9 months, at $70		630.00	
1 carpenter, 45 days, at $3		135.00	
1 carpenter, 6 days, at $3		18.00	
1 carpenter, 6 days, at $3		18.00	
1 bricklayer, 5 days, at $4		20.00	
1 painter, 8 days, at $2		16.00	
1 painter, 7 days, at $2		14.00	
1 fireman, 3½ months 20 days, at $45		186.77	
1 fireman, 17 days, at $1.50		25.50	
1 skilled laborer, 2¾ days, at $2.50		6.88	
1 laborer, 3 days, at $1.50		4.50	
1 laborer, 4 days, at $1.50		6.00	
1 laborer, 8 days, at $1.50		12.00	
Total salaries or compensation		4,932.89	
General expenses:			
Apparatus	$1,627.09		
Books	152.66		
Freight	23.46		
Fuel	101.42		
Illustrations	192.50		
Lumber	26.12		
Stationery	16.46		
Supplies	500.96		
		2,640.67	
Total disbursements			$7,573.56
Balance July 1, 1897, to meet liabilities			2,426.44

Astrophysical Observatory, 1896.

Balance July 1, 1896, as per last report		$698.25

Disbursements, July 1, 1896, to June 30, 1897.

General expenses:		
Apparatus	$289.07	
Books	74.75	
Heating apparatus	195.00	
Freight	2.55	
Supplies	80.38	
		641.75
Balance July 1, 1897, to meet liabilities		56.50

Astrophysical Observatory, 1895.

Balance July 1, 1896, as per last report	$4.42
Amount carried, under the provisions of the Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1897	3.48
Balance July 1, 1897	.94

National Zoological Park, 1897.

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; erecting and repairing buildings and inclosures; care, subsistence, transportation of animals, including salaries or compensation of all necessary employés, and general incidental expenses not otherwise provided for, sixty-seven 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 of the sum hereby appropriated five thousand dollars shall be used for continuing the entrance into the Zoological Park from Woodley Lane and opening driveway into Zoological Park from said entrance along the bank of Rock Creek, and five thousand dollars shall be used toward the construction of a road from the Holt Mansion entrance (on Adams Mill road) into the park to connect with the roads now in existence, including a bridge across Rock Creek" (sundry civil act, June 11, 1896) $67,000.00

Disbursements, July 1, 1896, to June 30, 1897.

Salaries or compensation:

1 superintendent, 4 months, at $208.33	$833.32
8 months, at $225	1,800.00
1 property clerk, 12 months, at $125	1,500.00
1 clerk, 7 months, at $60	420.00
5 months, at $75	375.00
1 stenographer, 12 months, at $62.50	750.00
1 copyist, 12 months, at $50	600.00
1 copyist, 7 days, at $50	11.29
1 typewriter, 20 days, at $1	20.00
1 head keeper, 12 months, at $100	1,200.00
1 keeper, 12 months, at $60	720.00
1 keeper, 12 months, at $60	720.00
1 keeper, 12 months, at $60	720.00
1 keeper, 12 months, at $60	720.00
1 keeper, 12 months, at $60	720.00
1 keeper, 6 months, at $75	450.00
1 foreman, 8 months, at $75	600.00
1 assistant foreman, 12 months, at $60	720.00
1 landscape gardener, 84 days, at $3	252.00
1 blacksmith, 12 months, at $75	900.00
1 assistant blacksmith, 12 months, at $60	720.00
1 carpenter, 12 months, at $75	900.00
1 watchman, 12 months, at $60	720.00
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00
1 workman, 6 months, at $60	360.00
laborer, 6 months, at $50	300.00
1 workman, 5 months 18 days, at $50	279.03
1 workman, 12 months, at $50	600.00
1 workman, 12 months, at $50	600.00
1 laborer, 5 months, at $35	175.00
7 months, at $45	315.00
1 laborer, 2 months 10 days, at $45	105.00

Salaries or compensation—Continued.

1 laborer, 12 months, at $50	$600.00	
1 laborer, 11 months 19 days, at $50	580.65	
1 laborer, 12 months, at $50	600.00	
1 laborer, 12 months, at $50	600.00	
1 laborer, 12 months, at $20	240.00	
Total salaries or compensation		$23,526.29

Miscellaneous:

Buildings	110.41	
Building materials	762.82	
Fencing and cage materials	1,421.91	
Food	4,829.14	
Freight and transportation	877.80	
Fuel	557.78	
Lumber	1,757.02	
Machinery, tools, etc	1,060.89	
Miscellaneous	1,019.88	
Paints, oils, glass, etc	217.11	
Postage, telephones, and telegraph	189.98	
Road material, grading, and bridges	11,559.49	
Surveying, plans, etc	200.00	
Stationery, books, printing, etc	348.75	
Trees, plants, etc	1,178.21	
Water supply, sewerage, etc	229.41	
Total miscellaneous		26,320.60

Wages of mechanics and laborers and hire of teams in constructing buildings and inclosures, laying water pipes, building roads, gutters, and walks, planting trees, and otherwise improving the grounds:

1 laborer,	365 days, at $2	$730.00
1 laborer,	259½ days, at $1.50	389.25
1 laborer,	336¾ days, at $1.50	505.12
1 laborer,	26 days, at $1.25	32.50
	263 days, at $1.50	394.50
1 laborer,	14 days, at $1.75	24.50
	191 days, at $1.50	286.51
1 laborer,	20½ days, at $1	20.50
	164¼ days, at $1.25	205.32
	71¾ days, at $1.50	107.62
1 laborer,	13¾ days, at $1.75	24.06
	254¼ days, at $1.50	381.38
1 laborer,	14 days, at $1.50	21.00
	141 days, at $1.25	176.25
1 laborer,	60 days, at $1.50	90.00
1 laborer,	195¾ days, at $1.50	293.63
1 laborer,	27¼ days, at $1.50	40.87
1 laborer,	271¾ days, at $1.50	407.63
1 laborer,	255¾ days, at $1.50	383.62
1 laborer,	14 days, at $1.75	24.50
	137½ days, at $1.50	206.25
1 laborer,	14 days, at $1.75	24.50
	258 days, at $1.50	387.00
1 laborer,	341¾ days, at $1.50	512.63
1 laborer,	154¾ days, at $1.50	232.12
1 laborer,	338¾ days, at $1.50	508.11

Wages of mechanics and laborers, etc.—Continued.

1	laborer, 167¼ days, at $1.50	$250.89
1	laborer, 72 days, at $1.50	108.00
1	laborer, 176½ days, at $1.50	264.76
1	laborer, 56¼ days, at $1.50	84.38
1	laborer, 59¾ days, at $1.25	74.68
1	laborer, 9¾ days, at $1.25	12.19
1	laborer, 10¼ days, at $1.25	12.81
1	laborer, 59¾ days, at $1.25	74.68
1	laborer, 54 days, at $1.25	67.50
	laborer, 74 days, at $1.50	111.00
	engineer, 86 days, at $1.50	129.00
1	laborer, 10½ days, at $1.25	13.12
1	laborer, 158 days, at $1.25	197.50
1	laborer, 18¼ days, at $1.25	22.81
1	laborer, 68¼ days, at $1.25	85.31
1	laborer, 196 days, at $1.25	244.99
1 laborer,	44 days, at $1.25	55.00
	73 days, at $1.50	109.50
1	laborer, 17¾ days, at $1.25	22.19
1	laborer, 17¾ days, at $1.25	22.19
1	laborer, 19½ days, at $1.25	24.38
1	laborer, 263½ days, at $1.25	329.37
1	laborer, 298½ days, at $1.25	373.13
1	laborer, 206 days, at $1.25	257.49
1	laborer, 101 days, at $1.25	126.25
1	laborer, 48 days, at $1.25	60.01
1	laborer, 145 days, at $1.25	181.25
1	laborer, 70½ days, at $1.25	88.13
1	laborer, 70¾ days, at $1.25	88.43
1	laborer, 10 days, at $1.25	12.50
1	laborer, 65 days, at $1.25	81.25
1	laborer, 57 days, at $1.25	71.26
1	laborer, 2 days, at $1.25	2.50
1	laborer, 30 days, at $1.25	37.50
1	laborer, 6½ days, at $2	13.00
1 laborer,	260 days, at $2	520.00
	14¼ days, at $2.25	32.06
1	laborer, 29 days, at 50 cents	14.50
1	laborer, 35¼ days, at $1.50	52.88
	engineer, 90 days, at $1.75	157.50
1	laborer, 98½ days, at $1.50	147.75
	stonemason, 8½ days, at $4	34.00
1	laborer, 87½ days, at $1	87.50
	weeder, 66¼ days, at 75 cents	49.69
	water boy, 94¼ days, at 50 cents	47.12
1	laborer, 60 days, at 75 cents	45.00
	weeder, 100 days, at 75 cents	75.00
	weeder, 124¼ days, at 50 cents	62.13
1 workman,	181 days, at $1.75	316.75
	184 days, at $1.50	276.00
1	draftsman, 175¾ days, at $2	351.50
1	painter, 41 days, at $3	123.00
1 carpenter,	11 days, at $2.80	30.80
	69 days, at $3	207.00
1 carpenter,	9 days, at $2.50	22.50
	53¾ days, at $2.80	150.50

Wages of mechanics and laborers, etc.—Continued.

1 carpenter, 32 days, at $2.80	$89.60	
1 carpenter, 49 days, at $2.80	137.20	
1 carpenter, 5 days, at $2.80	14.00	
1 carpenter, 57 days, at $2.80	159.60	
14½ days, at $2.50	36.25	
1 carpenter, 68¼ days, at $2.80	191.10	
10 days, at $2.50	25.00	
1 carpenter, 68 days, at $2.80	190.40	
11 days, at $2.50	27.50	
1 carpenter, 38¾ days, at $2.50	96.87	
1 carpenter, 47 days, at $2.50	117.50	
1 carpenter, 34 days, at $2.50	85.00	
1 carpenter, 32 days, at $2.50	80.00	
1 carpenter, 5½ days, at $2.50	13.75	
1 engineer, 73 days, at $1.75	127.75	
1 water boy, 22¼ days, at 75 cents	16.69	
1 water boy, 44 days, at 50 cents	22.00	
1 water boy, 105½ days, at 50 cents	52.75	
1 stonebreaker, 40 cubic yards, at 60 cents	24.00	
33 1/12 cubic yards, at 50 cents	16.55	
1 stonebreaker, 33½ cubic yards, at 60 cents	20.10	
20 cubic yards, at 50 cents	10.00	
1 stonebreaker, 20¼ cubic yards, at 60 cents	12.15	
25 7/12 cubic yards, at 50 cents	12.79	
1 stonebreaker, 3½ cubic yards, at 60 cents	2.10	
1 stonebreaker, 36⅚ cubic yards, at 60 cents	22.10	
26 cubic yards, at 50 cents	13.00	
1 stonebreaker, 11½ cubic yards, at 50 cents	3.45	
30 cubic yards, at 30 cents	9.00	
1 stonebreaker, 37 cubic yards, at 30 cents	11.10	
1 stonebreaker, 42 cubic yards, at 30 cents	12.60	
1 weeder, 143¼ days, at 50 cents	71.62	
1 wagon and team, 5½ days, at $3.50	19.25	
1 wagon and team, 39¾ days, at $3.50	139.12	
horse and cart, 12½ days, at $1.75	21.88	
1 wagon and team, 9¾ days, at $3.50	34.13	
horse and cart, 241 days, at $1.75	421.75	
1 wagon and team, 1 day, at $3.50	3.50	
1 wagon and team, 1 day, at $3.50	3.50	
1 wagon and team, 2½ days, at $3.50	8.75	
horse and cart, 28 days, at $1.75	49.00	
1 horse and cart, 2½ days, at $1.75	4.37	
1 horse and cart, 8½ days, at $1.75	14.87	
1 horse and cart, 3½ days, at $1.75	6.12	
1 horse and cart, 2 days, at $1.75	3.50	
1 horse and cart, 3¼ days, at $1.75	5.69	
1 horse and cart, 39 days, at $1.75	68.25	
1 horse and cart, 9¼ days, at $1.75	16.19	
1 horse and cart, 61¼ days, at $1.75	107.19	
1 horse, 28 days, at 50 cents	14.00	
Total wages, mechanics, etc		$15,586.08
Total disbursements		65,432.97
Balance July 1, 1897, to meet liabilities		1,567.03

NATIONAL ZOOLOGICAL PARK, 1896.

Balance July 1, 1896, as per last report $4,305.26

Disbursements.

Buildings	$1.70	
Building materials	38.63	
Fencing, cage materials	72.43	
Food	425.10	
Freight and transportation	14.95	
Fuel	6.25	
Lumber	39.43	
Machinery, tools, etc	212.21	
Miscellaneous	111.89	
Paints, oils, glass, etc	11.11	
Postage, telephones, etc	63.19	
Road material, grading, etc	2,637.75	
Surveying, plans, etc	370.00	
Stationery, books, etc	65.33	
Trees, plants, etc	35.97	
Water supply, sewerage, etc	176.09	
Total disbursements		$4,282.03
Balance July 1, 1897		23.23

ENTRANCE AND DRIVEWAY, NATIONAL ZOOLOGICAL PARK, 1895 AND 1896.

Balance July 1, 1896, as per last report $95.49

Balance carried, under the provisions of the Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1897.

NATIONAL ZOOLOGICAL PARK, 1895.

Balance July 1, 1896, as per last report $2.48

Balance carried, under the provisions of the Revised Statutes, section 3090, by the Treasury Department to the credit of the surplus fund, June 30, 1897.

RECAPITULATION.

The total amount of funds administered by the Institution during the year ending June 30, 1897, appears, from the foregoing statements and the account books, to have been as follows:

Smithsonian Institution.

From balance of last year, July 1, 1896		$57,065.78	
(Including cash from executors of Dr. J. H. Kidder)	$5,000.00		
(Including cash from gift of Alex. Graham Bell)	5,000.00		
	10.000.00		
From interest on Smithsonian fund for the year		54,720.00	
From sales of publications		460.95	
From repayments of freight, etc		5,667.76	
Interest on West Shore bonds		1,680.00	
			$119,594.49

Appropriations committed by Congress to the care of the Institution.

International Exchanges—Smithsonian Institution:		
From balance of 1894–95	$0.21	
From balance of 1895–96	180.92	
From appropriation for 1896–97	19,000.00	
		$19,181.13
North American Ethnology:		
From balance of 1894–95	100.08	
From balance of 1895–96	1,444.13	
From appropriation for 1896–97	45,000.00	
		46,544.21
Preservation of collections—Museum:		
From balance of 1894–95	42.31	
From balance of 1895–96	2,846.53	
From appropriation for 1896–97	153,225.00	
		156,113.84
Printing—Museum:		
From balance of 1895–96	52.71	
From appropriation for 1896–97	12,000.00	
		12,052.71
Furniture and fixtures—Museum:		
From balance of 1894–95	.53	
From balance of 1895–96	1,315.09	
From appropriation for 1896–97	15,000.00	
		16,315.62
Heating and lighting, etc.—Museum:		
From balance of 1894–95	1.15	
From balance of 1895–96	947.33	
From appropriation for 1896–97	13,000.00	
		13,948.48
Rent of workshops, etc.—Museum:		
From balance of 1894–95	12.54	
From balance of 1895–96	75.00	
From appropriation for 1896–97	2,000.00	
		2,087.54
Postage—Museum:		
From appropriation for 1896–97		500.00
Building repairs—Museum:		
From balance of 1894–95	4.78	
From balance of 1895–96	929.51	
From appropriation for 1896–97	4,000.00	
		4,934.29
Galleries—Museum:		
From appropriation for 1896–97		8,000.00
National Zoological Park:		
From balance of 1894–95	2.48	
From balance of 1895–96	4,305.26	
From appropriation for 1896–97	67,000.00	
		71,307.74
Entrance and driveway, Zoological Park, D. C.:		
From balance of 1895–96		95.49
Fire protection—Smithsonian Institution and National Museum:		
From balance of 1895–96		1.71
Astrophysical Observatory, Smithsonian Institution:		
From balance of 1894–95	4.42	
From balance of 1895–96	698.25	
From appropriation for 1896–97	10,000.00	
		10,702.67

SUMMARY.

Smithsonian Institution	$119,594.49	
Exchanges	19,181.13	
Ethnology	46,544.21	
Preservation of collections	156,113.84	
Printing	12,052.71	
Furniture and fixtures	16,315.62	
Heating and lighting	13,948.48	
Rent of workshop	2,087.54	
Postage	500.00	
Building repairs, National Museum	4,934.29	
Galleries	8,000.00	
Fire protection, Smithsonian Institution and National Museum	1.71	
National Zoological Park	71,307.74	
Entrance and driveway, Zoological Park	95.49	
Astrophysical Observatory	10,702.67	
		$481,379.92

The committee has examined the vouchers for payment from the Smithsonian income during the year ending June 30, 1897, 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 has been accepted and his bond approved by the Secretary of the Treasury.

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

Statement of regular income from the Smithsonian fund available for use in the year ending June 30, 1898.

Balance on hand June 30, 1897		$61.532.50
(Including cash from executors of J. H. Kidder)	$5,000.00	
(Including cash from Dr. Alex. Graham Bell)	5,000.00	
	10,000.00	
Interest due and receivable July 1, 1897	27,360.00	
Interest due and receivable January 1, 1898	27,360.00	
Interest, West Shore Railroad bonds, due July 1, 1897	840.00	
Interest, West Shore Railroad bonds, due January 1, 1898	840.00	
		56,400.00
Total available for year ending June 30, 1898		117,932.50

Respectfully submitted.

J. B. HENDERSON,
WM. L. WILSON,
Executive Committee.

WASHINGTON, D. C., *January 10, 1898.*

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

(In continuation from previous reports.)

[Fifty-fourth Congress, second session, and Fifty-fifth Congress, first session.]

INTERNATIONAL EXCHANGES.

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, nineteen thousand dollars. (Sundry civil appropriation act, approved June 4, 1897, statutes of Fifty-fifth Congress, first session, p. 22.)

To pay amounts found due by the accounting officers of the Treasury on account of the appropriation "International exchanges, Smithsonian Institution," for the fiscal year eighteen hundred and ninety-six, one dollar and seventy-nine cents. (Deficiency appropriation act, approved July 19, 1897, statutes of Fifty-fifth Congress, first session, p. 115.)

NATIONAL MUSEUM.

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

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

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 employees, one hundred and sixty thousand dollars, of which sum three thousand five hundred dollars may be used for necessary drawings and illustrations for publications of the National Museum.

For repairs to buildings, shops, and sheds, National Museum, including all necessary labor and material, four thousand dollars.

For rent of workshops for the National Museum, two thousand dollars.

For postage stamps and foreign postal cards for the National Museum, five hundred dollars.

For the continuation of the construction of galleries in the National Museum building, said galleries to be constructed under the direction of the superintendent of the Congressional Library in accordance with the approval of the Secretary of the Smithsonian Institution, eight thousand dollars.

For removal of the sheds from their present location south of and adjacent to the Smithsonian building, and rebuilding them, including all necessary labor and material, two thousand five hundred dollars. (Sundry civil appropriation act, approved June 4, 1897, statutes of the Fifty-fifth Congress, first session, p. 22.)

For expenses of heating the United States National Museum, one thousand and ninety-seven dollars and sixty-five cents. (Deficiency appropriation act, approved July 19, 1897, statutes of Fifty-fifth Congress, first session, p. 115.)

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, the editions of which shall not be less than three thousand copies, and binding scientific books and pamphlets presented to and acquired by the National Museum Library, twelve thousand dollars. (Sundry civil appropriation act, approved June 4, 1897, statutes of Fifty-fifth Congress, first session, p. 60.)

BUREAU OF AMERICAN ETHNOLOGY.

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-five thousand dollars, of which sum not exceeding one thousand dollars may be used for rent of building. (Sundry civil appropriation act, approved June 4, 1897, statutes of Fifty-fifth Congress, first session, p. 22.)

ASTROPHYSICAL OBSERVATORY.

Astrophysical Observatory.—For maintenance of Astrophysical Observatory, under the direction of the Smithsonian Institution, including salaries of assistants, apparatus, and miscellaneous expenses, ten thousand dollars. (Sundry civil appropriation act, approved June 4, 1897, statutes of Fifty-fifth Congress, first session, p. 22.)

NATIONAL ZOOLOGICAL PARK.

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; care, subsistence, purchase, and transportation of animals, including salaries or compensation of all necessary employees and general incidental expenses not otherwise provided for, fifty-five 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 of the sum hereby appropriated five thousand dollars shall be used for continuing the entrance into the Zoological Park from Woodley lane and opening driveway into Zoological Park from said entrance along the bank of Rock Creek. (Sundry civil appropriation act, approved June 4, 1897, statutes of Fifty-fifth Congress, first session, p. 22.)

OMAHA EXPOSITION.

Omaha Exposition.—For construction of building or buildings and for Government exhibit, including each and every purpose connected therewith, at the Trans-Mississippi and International Exposition at the city of Omaha, in the State of Nebraska, as provided by and within the limitations and restrictions of the act approved June tenth, eighteen hundred and ninety-six, entitled "An act to authorize and encourage the holding of a Trans-Mississippi and International Exposition at the city of Omaha, in the State of Nebraska, in the year eighteen hundred and ninety-eight," including the return of said Government exhibit, two hundred thousand dollars, to be immediately available. (Sundry civil appropriation act, approved June 4, 1897, statutes of Fifty-fifth Congress, first session, p. 26.)

TENNESSEE CENTENNIAL EXPOSITION.

AN ACT to aid and encourage the holding of the Tennessee Centennial Exposition at Nashville, Tennessee, in the year eighteen hundred and ninety-seven, and making an appropriation therefor.

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That there shall be exhibited at the Tennessee Centennial Exposition, to be held at Nashville, Tennessee, in the year eighteen hundred and ninety-seven, by the Government of the United States, from its Executive Departments, the Smithsonian Institution and National Museum, and the United States Fish Commission, such articles and materials as illustrate the function and administrative faculty of the Government in time of peace and its resources as a war power, tending to demonstrate the nature of our institutions and their adaptation to the wants of the people; and to secure complete and harmonious arrangement of said Government exhibit a board of management shall be created, to be charged with the selection, purchase, preparation, arrangement, safe-keeping, and exhibition of such articles and materials as the heads of said departments and institutions of the Government may, respectively, decide shall be embraced in said Government exhibit. The President may also designate additional articles for exhibition. Such board shall be composed of one member to be detailed by the head of each executive department, one by the head of the Smithsonian Institution and National Museum, and one by the head of the United States Fish Commission; and the President shall name one of said persons so detailed as chairman; and the members of said board shall have no compensation in addition to their regular salary, and their actual necessary expenses only shall be paid out of the sum hereinafter appropriated.

SEC. 2. That the Secretary of the Treasury shall cause a suitable building or buildings to be erected on the site selected for the Tennessee Centennial Exposition for the Government exhibit, and he is hereby authorized and directed to contract therefor, in the same manner and under the same regulations as for other public buildings of the United States; but the contract for said building or buildings shall not exceed the sum of thirty thousand dollars; and there is hereby appropriated for said building or buildings, out of any money in the Treasury not otherwise appropriated, the sum of thirty thousand dollars. The Secretary of the Treasury is authorized and required to dispose of such building or buildings, or the material composing the same, at the close of the exposition, giving preference to the city of Nashville or to the said Tennessee Centennial Exposition Company to purchase the same at an appraised value, to be ascertained in such manner as he may determine, and whatever sum may be realized on sale of said building shall be covered into the Treasury of the United States.

SEC. 3. That for the purpose of paying the expenses of the selection, purchase, preparation, transportation, installation, care, and return of said Government exhibit, and for the employment of proper persons as officers and assistants by the board of management created by this act and for their expenses, and for the maintenance of the building hereinbefore provided for, and for other contingent expenses incidental to the Government exhibit, to be approved by the chairman of the board of management and by the Secretary of the Treasury upon itemized accounts and vouchers, there is hereby appropriated, out of any money in the Treasury not otherwise appropriated, the sum of one hundred thousand dollars, or so much thereof as may be necessary, to be disbursed by the board of management hereinbefore created, of which not exceeding the sum of ten thousand dollars shall be expended for clerical service.

SEC. 4. That all articles which shall be imported from foreign countries for the sole purpose of exhibition at said exposition, upon which there shall be a tariff or customs duty, shall be admitted free of payment of duty, customs fees or charges, under such regulations as the Secretary of the Treasury shall prescribe; but it shall be lawful at any time during the exhibition to sell, for delivery at the close of the exposition, any goods or property imported for and actually on exhibition in the exposition buildings or on its grounds, subject to such regulations for the security of the revenue and for the collection of import duties as the Secretary of the Treasury shall prescribe: *Provided*, That all such articles when sold or withdrawn for consumption in the United States shall be subject to the duty, if any, imposed upon such articles by the revenue laws in force at the date of importation, and all penalties prescribed by law shall be applied and enforced against such articles and against the persons who may be guilty of any illegal sale or withdrawal.

SEC. 5. That medals with appropriate devices, emblems, and inscriptions commemorative of said Tennessee Centennial Exposition and of the awards to be made to exhibitors thereat, be prepared at some mint of the United States for the board of directors thereof, subject to the provisions of the fifty-second section of the coinage act of eighteen hundred and ninety-three, upon the payment by the Tennessee Centennial Exposition Company of a sum not less than the cost thereof; and all the provisions whether penal or otherwise of said coinage act against the counterfeiting or imitating of coins of the United States shall apply to the medals struck and issued under this act.

SEC. 6. That the United States shall in no manner and under no circumstances be liable for any bond, debt, contract, expenditure, expense, or liability of any kind whatever of the said Tennessee Centennial Exposition Company, its officers, agents, servants, or employees, or incident to or growing out of said exposition, nor for any amount whatever in excess of the one hundred and thirty thousand dollars herein authorized; and the heads of the Executive Departments, the Smithsonian Institution and National Museum, and United States Fish Commission, and the board of management herein authorized, their officers, agents, servants, or employees, shall in no manner and under no circumstances expend or create any liability of any kind for any sum in excess of the appropriations herein made, or create any deficiency.

SEC. 7. That the appropriation herein made shall take effect when the Secretary of the Treasury shall be satisfied that the solvent appropriations made by the State of Tennessee, its counties and cities, and by individuals or companies to said centennial exposition, together with solvent subscriptions to the stock of the Centennial Company made by the State, its counties and cities, and by private corporations, and by individuals shall amount to at least the sum of one-half million dollars.

Approved, December 22, 1896.

REPORT

OF

S. P. LANGLEY,

SECRETARY OF THE SMITHSONIAN INSTITUTION,

FOR THE YEAR ENDING JUNE 30, 1897.

To the Board of Regents of the Smithsonian Institution.

GENTLEMEN: I have the honor to present herewith the Secretary's report, showing the operations of the Institution during the year ending June 30, 1897, including the work placed under its direction by Congress in the United States National Museum, the Bureau of American Ethnology, the International Exchanges, the National Zoological Park, and the Astrophysical Observatory.

Following the custom of several years, I have in the body of this report given a general account of the affairs of the institution and its bureaus, while the appendix presents more detailed statements by the persons in direct charge of the different branches of the work. Independently of this, the operations of the National Museum are fully treated in a separate volume of the Smithsonian Report prepared by Acting Assistant Secretary C. D. Walcott, and the report of the work of the Bureau of American Ethnology constitutes a volume prepared under the supervision of Maj. J. W. Powell, the Director of that Bureau.

THE SMITHSONIAN INSTITUTION.

THE ESTABLISHMENT.

The Smithsonian Establishment, as organized at the end of the fiscal year, consisted of the following ex officio members:

WILLIAM MCKINLEY, *President of the United States.*
GARRET A. HOBART, *Vice-President of the United States.*
MELVILLE W. FULLER, *Chief Justice of the United States.*
JOHN SHERMAN, *Secretary of State.*
LYMAN J. GAGE, *Secretary of the Treasury.*
RUSSELL A. ALGER, *Secretary of War.*
JOSEPH MCKENNA, *Attorney-General.*
JAMES A. GARY, *Postmaster-General.*
JOHN D. LONG, *Secretary of the Navy.*
CORNELIUS N. BLISS, *Secretary of the Interior.*
JAMES WILSON, *Secretary of Agriculture.*

THE BOARD OF REGENTS.

In accordance with a resolution of the Board of Regents, adopted January 8, 1890, by which its annual meeting occurs on the fourth Wednesday of each year, the Board met on January 27, 1897, at 10 o'clock a. m., and not being able to complete the business before it on that day, an adjourned meeting was held on February 1. The journal of its proceedings will be found, as hitherto, in the annual report of the Board to Congress, though reference is made in this report to several matters upon which action was taken at these meetings.

The Secretary formally announced to the Board the death of Dr. G. Brown Goode, assistant secretary of the Institution, in charge of the National Museum, and the following resolutions were adopted by a rising vote:

Whereas the assistant secretary of the Smithsonian Institution, Dr. G. Brown Goode, died on September 6, 1896:

Resolved, That the Board of Regents wish to here record their sense of the devotion to duty which in the late Dr. Goode came before any consideration of personal advancement, or even before the care of his own health, and of their recognition that his high administrative ability and wide knowledge were devoted unselfishly to the service of the Institution, with results whose value they can not too highly acknowledge; and they desire to express their feeling of the loss that the Institution, the National Museum, and the cause of science has sustained in his untimely death.

Resolved, That a copy of these resolutions be suitably engrossed and transmitted to the family of Dr. Goode.

In this connection, the Secretary brought up the matter of a successor to Dr. Goode, and stated that he had decided to ask the permission of the Board to appoint as acting assistant secretary in charge of the National Museum, Prof. Charles D. Walcott, Director of the United States Geological Survey, and for more than twelve years honorary curator in the Museum. Professor Walcott having consented to assume the duties of the office until a permanent selection could be made, the Secretary's action was approved by the Board by the adoption of the following resolution:

Resolved, That the appointment by the Secretary of Prof. Charles D. Walcott as acting assistant secretary of the Smithsonian Institution, with duties confined to the charge of the National Museum, be approved.

The Secretary also announced the death of Mr. W. C. Winlock, assistant in charge of office and curator of exchanges, and stated that he had already appointed as his successor in that office Mr. Richard Rathbun. Mr. Rathbun had been for twenty years connected with the Institution, and, for reasons which the Secretary had submitted to the Regents, he now nominated Mr. Rathbun as Assistant Secretary in charge of office and exchanges. The Board approved the Secretary's action by the adoption of the following resolution:

Resolved, That the appointment by the Secretary of Mr. Richard Rathbun as Assistant Secretary of the Smithsonian Institution, with duties connected with the bureaus of the Institution other than the National Museum, be approved.

ADMINISTRATION.

The present writer has been occupied during his tenure of office as Secretary much more with administrative than with purely scientific duties, which latter have been relegated to moments of comparative leisure. In the pursuit of duties in the past years, as executive officer of the Regents, he has endeavored to improve the business methods in use in the Institution for its correspondence, its relations to its bureaus, and in some measure for the details of its finance. It is perhaps well to speak briefly here of the system which is followed in the expenditures of the Institution, which are in the main like those of other Government bureaus with some slight modifications as regards the Institution's own practice.

The Secretary is by law the custodian of the funds of the Institution, consisting at present of moneys deposited in the United States Treasury and of certain bonds, held by the Regents through him under the instructions of the permanent committee on the administration of bequests and their investment, consisting of the executive committee and the Secretary.

The Revised Statutes of the United States, section 5593, provide:

> Whenever money is required for the payment of the debts or performance of the contracts of the Institution, incurred or entered into in conformity with the provisions of this title, or for making the purchases and executing the objects authorized by this title, the Board of Regents, or the executive committee thereof, may certify to the chancellor and secretary of the Board that such sum of money is required, whereupon they shall examine the same, and, if they shall approve thereof, shall certify the same to the proper officer of the Treasury for payment.

In practice, the Chancellor and the Secretary of the Institution make a written requisition upon the Secretary of the Treasury twice a year for the semi-annual interest on the fund, and this sum is held in the Treasury subject only to the order of the Secretary. Money is not, as a rule, kept on hand or drawn except for the payment of a specific account, while at the same time it has been the practice of the present Secretary, as of his predecessors, neither to receive nor pay personally any moneys of the Institution, but to perform such transactions through a bonded disbursing officer of the Government, who is also the accountant of the Institution. The Secretary, as the disbursing officer of the Institution, never makes payments in cash, but only through checks prepared by the accountant. The actual course of an ordinary account is as follows:

Every purchase is preceded by a requisition which is approved by the Secretary, except in certain excepted cases of expenditure of a very minute amount. Upon this approved requisition an order is issued by the Assistant Secretary, and when the bill is rendered it is certified by the proper official and a voucher is prepared which receives the certificate of two persons, one to the effect that the article has been received or the services rendered, the other to the effect that the charge is reasonable and just. The voucher is then examined by the Secretary, and, if approved, payment is made by a check on the United States Treasury signed by him.

The actual conduct of these transactions is through the bonded disbursing officer above referred to. The Secretary makes it a rule as far as possible to examine personally all the vouchers; but while it is not always possible for him to thus examine every one or to be personally cognizant of every item of detail, he has always the foregoing assurance of the propriety of his signature before he affixes it to the check, which finally concludes the monetary transaction. These are the safeguards which the Secretary employs in regard to the actions of subordinate officials, in whom, nevertheless, he has the fullest personal confidence; and the Secretary's own accounts are in turn examined by the executive committee—a most important function, which completes the chain of responsibility. In thus briefly describing the business forms of the monetary transactions of the institution, it will be understood that the integrity of the officials on whom the Secretary relies has never in any instance been called in question, and he desires to repeat this, and to acknowledge in particular the acceptable service of the present accountant, in thus speaking of what may be called the mechanism of this part of the administrative order.

What has preceded refers particularly to the administration of the private funds of the Institution. In regard to the bureaus supported by Government appropriations which are placed under the charge of the Regents, the methods of keeping the accounts are assimilable to those of other Government departments, the moneys being placed by the United States Treasurer, on requisition by the Secretary, at the disposal of a bonded disbursing officer, who prepares the vouchers, which are then certified by the heads of the different bureaus to the Secretary for his approval.

The methods of conducting the finances and of regulating expenditures and payments here described have been so effective that in the fifty years of the life of the Institution no loss has ever occurred.

With the steady growth of the several bureaus under the direction of the Institution there come increased demands for their general administration not only upon the Secretary but also upon his immediate assistants. The clerical force of the Secretary's office has been chiefly supported from the income of the Institution, though a great deal of the work pertains directly to the business of the bureaus, which should be at the cost of Government appropriations. I have several times called attention to this matter, and the Regents have authorized me to request from Congress a specific appropriation for the Secretary's office, to be expended for necessary assistance in the administration of

Government trusts, but it has thus far seemed inopportune to include a request for such appropriation in my estimates to Congress.

FINANCES.

The unexpended balance at the beginning of the fiscal year, July 1, 1896, as stated in my last annual report, was $57,065.78. Interest on the permanent fund in the Treasury and elsewhere, amounting to $56,400, was received during the year, which, together with a sum of $6,128.71 received from the sale of the publications and from miscellaneous sources, made the total receipts $62,528.71.

The disbursements for the year amounted to $58,061.99, the details of which are given in the report of the executive committee. The balance remaining to the credit of the Secretary on June 30, 1897, for the expenses of the Institution was $61,532.50, which includes the sum of $10,000 referred to in previous reports, being $5,000 received from the estate of Dr. J. H. Kidder, and a like sum from Dr. Alexander Graham Bell, the latter a gift made personally to the Secretary to promote certain physical researches. This latter sum was, with the donor's consent, deposited by the Secretary to the credit of the current funds of the Institution.

This balance also includes the interest accumulated on the Hodgkins donation, which is held against certain contingent obligations, besides relatively considerable sums held to meet obligations which may be expected to mature as the result of various scientific investigations or publications in progress.

The permanent funds of the Institution are as follows:

Bequest of Smithson, 1846		$515, 169. 00
Residuary legacy of Smithson, 1867		26, 210. 63
Deposits from savings of income, 1867		108, 620. 37
Bequest of James Hamilton, 1875	$1, 000. 00	
Accumulated interest on Hamilton fund, 1895	1, 000. 00	
		2, 000. 00
Bequest of Simeon Habel, 1880		500. 00
Deposits from proceeds of sale of bonds, 1881		51, 500. 00
Gift of Thomas G. Hodgkins, 1891		200, 000. 00
Portion of residuary legacy, T. G. Hodgkins, 1894		8, 000. 00
Total permanent fund		912, 000. 00

The Regents also hold certain approved railroad bonds, forming a part of the fund established by Mr. Hodgkins for investigations of the properties of atmospheric air.

By act of Congress approved by the President March 12, 1894, an amendment was made to section 5591 of the Revised Statutes, the fundamental act organizing the Institution, as follows:

> The Secretary of the Treasury is authorized and directed to receive into the Treasury, on the same terms as the original bequest of James Smithson, such sums as the Regents may, from time to time, see fit to deposit, not exceeding, with the original bequest, the sum of $1,000,000: *Provided*, That this shall not operate as a limitation on the power of

the Smithsonian Institution to receive money or other property by gift, bequest, or devise, and to hold and dispose of the same in promotion of the purposes thereof.

Under this section, 5591 of the Revised Statutes, modified as above noted, the above fund of $912,000 is deposited in the Treasury of the United States, bearing interest at 6 per cent per annum, the interest alone being used in carrying out the aims of the Institution.

During the fiscal year 1896–97 Congress charged the Institution with the disbursement of the following appropriations:

For International Exchanges	$19, 000
For North American Ethnology	45, 000
For United States National Museum:	
Preservation of collections	153, 225
Furniture and fixtures	15, 000
Heating and lighting	13, 000
Postage	500
Repairs to buildings	4, 000
Rent of workshops	2, 000
Galleries	8, 000
For National Zoological Park	67, 000
For Astrophysical Observatory	10, 000

The executive committee has examined all the vouchers for disbursements made during the fiscal year, and a detailed statement of the receipts and expenditures will be found reported to Congress, in accordance with the provisions of the sundry civil acts of October 2, 1888, and August 5, 1892, in a letter addressed to the Speaker of the House of Representatives.

The vouchers for all the expenditures from the Smithsonian fund proper have been likewise examined and their correctness certified to by the executive committee, whose statement will be published, together with the accounts of the funds appropriated by Congress, in that committee's report.

The estimates for the fiscal year ending June 30, 1898, for carrying on the Government interests under the charge of the Smithsonian Institution, and forwarded as usual to the Secretary of the Treasury, were as follows:

International Exchanges	$23, 000
American Ethnology	50, 000
National Museum:	
Preservation of collections	180, 000
Furniture and fixtures	30, 000
Heating and lighting	15, 000
Postage	500
Galleries	8, 000
Repairs to buildings	8, 000
Removal of sheds	2, 500
Rent of workshops	2, 000
National Zoological Park	75, 000
Astrophysical Observatory	10, 000

AVERY FUND.

In regard to the bequest of Mr. Robert Stanton Avery, referred to in previous reports, a definite settlement has not been reached with the heirs at law, so that it is not possible to state the exact amount that this fund will reach.

BUILDINGS.

No important changes were made in the Smithsonian Building during the year. Two Museum storage sheds adjacent to the building have been removed, with a great improvement in the appearance of the south front, while at the same time a source of danger from fire is averted. It is still necessary to retain some workshops south of the western portion of the building, no rooms being elsewhere available, but it is hoped that these also will soon be removed.

I may call attention to the need of additional room for the proper storage of such publications of the Institution and its bureaus as must be retained in reserve. These are comparatively few in number for each particular work, but the accumulations of fifty years occupy in the aggregate so much space as to demand more storage room than is now available and create a positive danger in the excessive weight that is now placed upon the floors of upper stories, while the work of distribution of publications is now carried on in very inconvenient and inaccessible quarters. I have under consideration the feasibility of some changes in the interior arrangement of the main north and south towers of the building which would render suitable for storage purposes much space which can not now be utilized.

I may also mention the very decided improvement that would result from the remodeling of the steep and long iron stairways leading to the great hall of the building, which is now used for archæological collections.

The improvements in progress in the Museum by the erection of galleries in several of the halls are alluded to elsewhere.

RESEARCH.

Although the time of the Secretary must be almost wholly given to administrative affairs, yet, as in years past, in carrying out the wish of the Regents[1] and in continuation of investigations begun prior to my connection with the Institution, I have devoted such time as I could spare to researches upon the solar spectrum and to experiments in connection with certain physical data of aerodynamics.

Both of these investigations have reached a stage at which it is possible to give to the world somewhat full statements of results. In my

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[1] *Resolved*, That the Secretary continue his researches in physical science and present such facts and principles as may be developed for publication in the Smithsonian Contributions. (Adopted at meeting of the Board of Regents January 26, 1847.)

remarks on the operations of the Astrophysical Observatory I discuss more fully the researches upon the solar spectrum.

In my report for the previous year I brought to the attention of the Board the fact that my experiments in aerodynamics had finally resulted in a successful trial on May 6, 1896, of a mechanism, built chiefly of steel and driven by a steam engine, which made two flights, each of over half a mile, and I appended a brief statement of my own and of Mr. Alexander Graham Bell, originally communicated in French to the Academy of Sciences of the Institute of France, describing the actual flight. Since that time a third and a much longer flight was made on November 28, 1896, with another machine, built of steel like the first and driven like that by propellers actuated by a steam engine of between 1 and 2 horsepower, making a horizontal flight of over three-quarters of a mile and descending in safety.

I have thus brought to the test of actual successful experiment the demonstration of the practicability of mechanical flight, which has been so long debated and till lately so discredited. To satisfy a nearly universal interest, I am now engaged in the preparation of a full description of these experiments since 1891, when my first memoir on aerodynamics was published. This memoir, with those on "Experiments in Aerodynamics" and "Internal Work of the Wind," will form volume 27 of the Smithsonian Contributions to Knowledge, which will thus contain a complete record of all experiments carried on thus far under my direction upon this subject.

HODGKINS FUND.

The Hodgkins medals of award were received at the Institution on the 13th of July, 1896, and were transmitted on the same day to those competitors for the Hodgkins fund prizes who were recommended by the committee to receive medals. A replica of the medal was sent to each of the members of the Hodgkins advisory committee and to certain specialists who, without compensation, had rendered valuable aid in connection with the competition. A replica was also sent to the firm of Evarts, Choate & Beaman, the legal counsel of Mr. Hodgkins, and to Dr. Chambers, his medical adviser and long-time friend, as a memento of valued services rendered in connection with the Hodgkins bequest to the Institution.

In July, 1896, Mr. E. C. C. Baly, of University College, London, a Hodgkins competitor, whose memoir received honorable mention, was awarded a grant of $750 to enable him to prosecute further his investigations on the decomposition of the atmosphere by means of the passage of the electric spark. A report of the research, so far as it has progressed, has been received from Mr. Baly.

Under an additional grant to Dr. S. Weir Mitchell and Dr. John S. Billings investigations have been conducted in the Laboratory of Hygiene of the University of Pennsylvania, upon the effect which a prolonged exposure to vitiated air has upon the power of individuals to resist infectious diseases. Dr. D. H. Bergey, who conducted the experiments, reports that he subjected certain animals to an impure atmosphere and found that while it apparently lowered their vitality, he was unable to attenuate the fluids used for inoculating the diseases so that they would kill such a weakened animal while not affecting a vigorous one. Still, animals inoculated for tuberculosis died much earlier when exposed to impure air. As these results may doubtless be applied to all warm-blooded animals, including man, it would appear that we have here an important confirmation of the clinical observation that tuberculosis thrives most in vitiated air.

January 15, 1897, a grant of $500 was made to Mr. A. Lawrence Rotch, director of the Blue Hill Meteorological Observatory at Readville, Mass., to be used in securing automatic kite records of meteorological conditions at an altitude of 10,000 feet or more. An additional grant of $400 was later made to Mr. Rotch for continuing his experiments in connection with the explorations of the upper air.

With a view to being prepared to apply most advantageously the accruing interest from that portion of the fund devoted to investigations connected with the atmosphere the Secretary has conferred, during the year, with specialists in this country and Europe, upon the subject of researches suitable to be aided from the Hodgkins fund.

The six Hodgkins memoirs which have been published by the Institution were issued in February and March, 1897, and a copy of each was sent to all persons who had submitted papers in connection with the competition.

NAPLES TABLE.

As stated in my last report, the Institution has renewed the lease of the Smithsonian table at the Zoological Station of Naples for a second term of three years, this action being in accordance with the urgent solicitation of the faculties of several colleges and universities and of many of the leading biologists of the country.

At my earnest request Dr. Billings has continued as chairman of the advisory committee, which has rendered most efficient aid in examining testimonials and in recommending action with regard to applications for the occupancy of the table. The following applications have been favorably acted upon:

Dr. F. H. Herrick, professor of biology at Adelbert College, Cleveland, occupied the table in November, 1896, and Dr. S. E. Meek, formerly of the Arkansas Industrial University but more recently connected with the United States Fish Commission, received the appointment for two months in the spring of 1897. The application of Dr. H. S. Jennings, of the University of Michigan and later of Harvard, was approved for three months during the spring and summer of 1897. Through the continued courtesy of Dr. Dohrn, in permitting two persons nominated by the Institution to occupy tables at the same time, the residence of

Dr. Jennings began before the termination of Dr. Meek's appointment. Applications for the coming year are now under consideration.

EXPLORATIONS.

Ethnological and natural-history explorations have been continued under the direction or with the assistance of the Institution in various parts of the world by the Bureau of Ethnology and the National Museum. This work is more fully described elsewhere, but I may mention here that a large number of objects of interest from various parts of the world have been added to the Museum collections, and much valuable information has been acquired regarding the history and the language of the American Indians. Among the explorations of the year were those by Dr. William L. Abbott in Siam, Prof. O. F. Cook in Africa, Dr. E. A. Mearns in Minnesota and elsewhere, Mr. Frank H. Cushing in Maine, Mr. J. W. Fewkes in Arizona, Mr. E. T. Perkins in Idaho, Mr. W J McGee in Iowa, Mr. J. B. Hatcher in Patagonia and Tierra del Fuego, and Dr. Willis E. Everette in Oregon, British Columbia and Mexico.

PUBLICATIONS.

The publications of the Institution and its bureaus during the year comprised two works in quarto form, four in royal octavo, and fourteen in octavo, aggregating 9,630 pages, covering to a greater or less degree nearly all branches of human knowledge.

The Smithsonian Institution proper issues three series of works: The Contributions to Knowledge, the Miscellaneous Collections, and the Annual Report. By the bureaus of the Institution there are issued the Annual Report and the Bulletin of the Bureau of American Ethnology and the Proceedings and Bulletin of the National Museum, and the Secretary transmits to Congress the Annual Report of the American Historical Association. The Smithsonian Contributions and Miscellaneous Collections are printed at the expense of the Institution and the other publications from Congressional appropriations.

Contributions to Knowledge.—Two memoirs of this series were issued during the year, both having been submitted in competition for the Hodgkins fund prizes.

The memoir by Lord Rayleigh and Professor Ramsay describes the discovery of argon, for which achievement the authors were awarded the first Hodgkins prize of $10,000. It gives an account of the reasons which led the investigators to suspect the existence of a new element in the atmosphere and a detailed description of the apparatus and methods by which the presence of this hitherto unknown gas was definitely established. The importance of the discovery was recognized independently by the Institute of France, which awarded a prize of 50,000 francs, and by the National Academy of Sciences, which granted to the discoverers the Barnard medal.

The memoir by Prof. E. Duclaux, of Paris, entitled Atmospheric Actinometry and the Actinic Constitution of the Atmosphere, describes the methods and results of numerous experiments on the chemical rays of the sun by the exposure of oxalic acid to their action. Professor Duclaux found that the chemical action of the rays when the sky was overcast was much less than on a fine day and that with light cumulus clouds the combustion might be more active than with a clear blue sky or slight cirrus, so that it appeared evident that the chemical activity and hygienic power of the sun's rays are not related to the apparent fineness of the day.

Miscellaneous Collections.—Nine papers of the "Miscellaneous" series were issued and others are in progress. The completed works were Smithsonian Physical Tables, by Prof. Thomas Gray; Equipment and Work of an Aerophysical Observatory, by Alexander McAdie; Air in Relation to Human Life and Health, by Prof. F. A. R. Russell; Air of Towns, by Dr. J. B. Cohen; Air and Life, by Dr. Henri de Varigny; Mountain Observatories, by Prof. E. S. Holden; Methods of Determining Organic Matter in Air, by Dr. D. H. Bergey; Recalculation of Atomic Weights, by Prof. F. W. Clarke, and Virginia Cartography, by P. Lee Phillips.

The Catalogue of Scientific and Technical Periodicals, by Dr. H. Carrington Bolton, mentioned in my last report, is in type and will soon be published. It comprises the titles of more than 8,500 scientific and technical periodicals in all languages, adding 3,500 titles to the first edition published in 1885.

There is also completed, ready for the printer, a voluminous supplement to Dr. Bolton's Select Bibliography of Chemistry.

As a special work, there has been printed the International Exchange List of the Smithsonian Institution, being a list of the foreign correspondents, aggregating 9,414 learned societies, museums, universities, etc., with which American publications are exchanged.

Annual reports.—The Smithsonian Annual Report is in two volumes, one of which is devoted to the work of the National Museum. In the general appendix of Part I are included memoirs on all branches of knowledge, selected chiefly from publications of learned societies of the world that are not readily accessible to the public, the basis of selection being that the papers are written by a competent person, give an account of some important or at least interesting scientific discovery, are untechnical in language and suitable to nonprofessional readers.

The History of the First Half Century of the Smithsonian Institution, outlined with some detail in my last report, is now printed and will soon be issued. The Institution was founded August 10, 1846, by act of Congress approved by President Polk, and it seemed an appropriate memorial of the completion of its first fifty years to publish a volume which should give an account of its origin and history, its achievements, and its present condition.

The editorial supervision of the volume was undertaken by the late Dr. G. Brown Goode, and to his thorough acquaintance with the history of the Institution, and his skill and critical knowledge, the comprehensive plan of the work is entirely due. At the time of his death, in September, 1896, the manuscript was sufficiently advanced to permit of its completion on his general plan.

The volume is royal octavo of 866 pages, with a preface by William McKinley, President of the United States, ex-officio the head of the establishment. It is illustrated by full-page portraits of James Smithson, the Chancellors, several of the Regents, the three Secretaries, and of Assistant Secretary Goode, besides illustrations of the Smithsonian building and of the infra-red spectrum investigations by the present Secretary. The main divisions of the work are fifteen chapters, descriptive of the history of the Institution, and a like number of chapters giving appreciations of its work in the several branches of knowledge, mainly by persons not connected with the Institution, followed by an appendix of 8 pages narrating the principal events in its history.

Since it is impossible in a single volume to exhaust the subject, it became necessary to mention but briefly many topics which it was hoped might be elaborately treated. The book is printed from type in an edition of 2,000, with 250 additional copies on handmade paper. It is not classed in either of the regular series of Smithsonian publications, and will receive a special rather than a general distribution. This course is found necessary by reason of the cost of the work.

The Annual Report of the Museum for 1894, which includes several special papers by Museum officers or collaborators, has been issued, and the Museum has published a volume of Proceedings, and separate papers of other volumes, besides two octavo and two quarto bulletins, the contents of all of which are given elsewhere.

The Bureau of Ethnology has published three reports, the fourteenth, fifteenth, and sixteenth, bringing the work down to the close of the fiscal year 1894–95.

The Annual Report of the American Historical Association for 1895 has been published, and the report for 1896 has been sent to the printer. These reports are transmitted by the secretary of the association to the Secretary of the Institution, who submits the whole or portions of the reports to Congress, in accordance with the act of incorporation of the association. Prior to the report for 1894 the Institution had no share in the distribution of these volumes, but, beginning with the report for 1894, a limited number is available for purposes of exchange by the Institution with historical and other learned societies of the world. The reports contain papers relating to American history or to the study of history in America. A most important contribution in the report for 1895 is a bibliography of the historical societies of the United States and British America, covering 561 printed pages, which is a very useful reference work for writers and students of American history.

LIBRARY.

The library continues to grow steadily, the accessions in volumes, parts of volumes, pamphlets, and charts reaching 35,912 during the past year. Special mention should be made of the gift of Mr. S. Patcanof, of St. Petersburg, of over 300 volumes, consisting mostly of oriental works and including some Arabic manuscripts and many rare Armenian publications.

As stated in my last report, the Secretary of State had named, in accordance with my suggestion, Dr. John S. Billings, United States Army, retired, director of the New York Public Library, and Prof. Simon Newcomb, United States Navy, Superintendent of the Nautical Almanac, as the delegates of the United States to a conference to be held at the instance of the British Government at London in July, 1896, to consider the preparation of an international catalogue of scientific literature. This conference met July 14 to 17, 1896, twenty-two countries being represented. The conference drew up a plan which the respective delegates submitted to the countries they represented. The report of Professor Newcomb and Doctor Billings, submitted to the Secretary of State October 15, 1896, recommended that the United States Government should take part in this work, and that the Smithsonian Institution be made the agent of the Government in this important scientific enterprise.

In accordance with this suggestion the Secretary of State invited my opinion as to the propriety and feasibility of the United States taking part in this work through the Smithsonian Institution, and requested an estimate of the probable expense attendant thereto. To this I replied that I fully concurred in the view of the delegates as to the great importance of a successful execution of the conclusions of the conference and as to the propriety of this Government taking its share of the proposed work by providing for the cataloguing of the scientific publications of the United States. This opinion is strengthened by the fact that the recommendations made are due to results emanating from an international conference, at which the United States was officially represented, and by the further considerations that the benefits to be derived from this undertaking are not only great and far-reaching for the scientific progress of America, but also of universal value, and that all the great and many of the smaller nations will take part in the work. I recognized also the propriety of the suggestion that the Government should employ the Smithsonian Institution as an agent in this matter, particularly since the Institution first suggested this subject in 1855, and since it has been from its earliest organization interested in scientific bibliography.

I was, however, reluctant to commit the Institution to the appearance of soliciting Congress in this matter in any case, or to the undertaking of the enterprise, however worthy, unless provision could be made for the necessary expenses of the work. After considering the subject, it seemed to me that the work, if assigned to the Smithsonian Institution, would require a person of special qualifications to immediately assist the Secretary, together with a number of trained clerical assistants, and that the salaries for these persons and the expenses incident to the work would require an appropriation of not less than $10,000 per annum.

In accordance with this recommendation, Secretary Olney transmitted this correspondence to Congress. Although the catalogue will not begin until 1900, much preliminary work will be necessary. I have accordingly brought the matter to the attention of Secretary Sherman, and the Department of State has agreed to submit an item for this purpose in its regular estimates for the year 1898–99.

Although the new building for the Library of Congress was completed in February, 1897, its occupancy had not begun at the close of the fiscal year. The east stack was provisionally assigned for the Smithsonian collection of transactions. In the past only this portion of the Smithsonian Library has been kept together, the remainder of the collection being distributed throughout the Library of Congress. I trust that in the new building, with its ample space and largely increased force, it will be found possible, in accordance with the resolution of the Regents in 1889,[1] to assemble the entire collection in one place.

CORRESPONDENCE.

In 1855 (February 24) the Board of Regents adopted the following resolution:

> *Resolved*, That all correspondence of this Institution with any person or society shall be conducted by the Secretary, and no assistant or employee shall write or receive any official letter or communication pertaining to the affairs of the Institution except under the authority and by the direction of the Secretary; and all such correspondence shall be duly registered and recorded in such manner as the Secretary shall direct.

The Secretary has given much care to organizing a system of correspondence which would insure that letters should receive prompt replies, and that a record of receipt, subject, and date of reply shall be kept in such a form as to be readily accessible. The system in question was fully described in my report for 1890, and its adoption has proved of the greatest convenience during the seven years it has been in operation.

The correspondence has greatly increased during the past few years,

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[1] *Resolved*, That since the Smithsonian deposit now numbers over 250,000 titles, and is still increasing at the cost of the Institution, it is, in the opinion of the Regents, desirable that in the new building for the Library of Congress sufficient provision shall be made for its accommodation and increase in a distinct hall or halls, worthy of the collections, and such as, while recalling to the visitor the name of Smithson, shall provide such facilities for those consulting the volumes as will aid in his large purpose of the diffusion of knowledge among men.

both in regard to such letters as directly concern the Institution proper and also the mass of correspondence which, though pertaining more particularly to the Museum and other bureaus, must nevertheless receive the attention of the Secretary's office.

A very unusual number of letters has been received from all parts of the country seeking information both on scientific matters and on political, economic, historical, and other questions in almost every branch of knowledge. Many correspondents have the impression that the Institution is a bureau of general information, and in dealing with this class the policy has been to courteously answer all queries legitimately within the range of the Institution, in other cases referring inquirers to possible sources of information.

Of the more important correspondence of the Secretary's office, 3,834 entries were made in the registry book of letters received, while many times that number were received and referred to the different bureaus of the Institution.

MISCELLANEOUS.

Nashville Exposition.—The Institution participated in the Tennessee Centennial Exposition at Nashville during the summer of 1897, under act of Congress making appropriation for a General Government exhibit. Accounts of the exhibit will be given after the close of the exposition.

Glover Collection.—A very valuable and interesting collection of Chinese coins, bequeathed to the Smithsonian Institution by the late Mr. George B. Glover, of the Chinese Imperial maritime customs service, has been received from the estate. It is believed that this collection is the most perfect of its kind in existence. It includes 2,025 specimens of Chinese, Annamese, Siamese, Japanese, and Korean coins; amulets and bamboo tally sticks used as money; Chinese paper money; foreign coins in circulation in China; and molds for casting coins. The series dates back to about the year 770 B. C., the earliest authentic period of coinage. The peculiarly shaped bronze pieces, representing knives, cloth, and other objects used as money are fully represented, and the series is continuous in the coinage of each dynasty for more than 2,600 years.

Catalogue of Egyptian papyri and royal antiquities.—In April, 1896, the Egyptian minister of foreign affairs addressed a communication to the representatives of all foreign Governments at Cairo, pointing out the desirability of a complete catalogue of Egyptian papyri and royal antiquities, and inviting the cooperation of the various Governments in securing the information needed for such a work, the editing and publication to be undertaken by the administration of antiquities of Egypt.

The consul-general of the United States informed the Egyptian Government that the hearty cooperation of the United States could be relied upon, and, in his dispatch to the Department of State, suggested that the Smithsonian Institution might be willing to collect the desired information.

The communications on the subject being referred to the Institution by the Department of State, I informed the Department that the Institution would undertake the task of collecting this information, though I stated at the same time that the ultimate success would depend on the willingness which museums and private individuals exhibited in cooperating.

I have entered into communication with the custodians of all collections known to me, and have thus far received some useful information.

At the request of Dr. D. C. Gilman, president of the American Oriental Society, a communication on the subject was presented to the society at its meeting in April, 1897, and the society appointed a committee of five to cooperate with the Institution in securing the desired information.

Congress of Orientalists.—On April 27, 1897, Dr. Paul Haupt, of Johns Hopkins University, was appointed to represent the Smithsonian Institution at the Congress of Orientalists at Paris in September of this year.

NATIONAL MUSEUM.

The most notable occurrence of the year affecting the National Museum was the death of Dr. G. Brown Goode, the assistant secretary in its charge, which occurred on September 6, 1896. I alluded briefly to this sad event in my last report, which had not gone to press at the time of Dr. Goode's death.

When 22 years of age Dr. Goode became associated with the Smithsonian Institution, and soon afterwards was designated by Professor Baird to prepare the Government natural history exhibit for the Centennial Exhibition of 1876. This work involved severe labor, and almost unaided he unpacked and assorted the large accumulations of material which had been stored away in the basements of the Smithsonian building. His work was entirely satisfactory, but the strain to which he was subjected in accomplishing it told severely on his strength, and it became necessary for him to go to Bermuda to regain his health. In 1881 the National Museum building was completed and Dr. Goode was appointed assistant director. In 1887 he was appointed assistant secretary of the Institution in charge of the National Museum, which position he held at the time of his death.

There are many sides from which to view the character and work of this remarkable man. His talents and the careful scientific training which he had received, gave him a very prominent place as a zoologist, but as a museum administrator he was perhaps skilled above all others. Personally I had absolute confidence in him, and the relations which obtained between us were those of the most intimate trust, while those which in turn existed between him and his subordinates were of the most fortunate kind.

I have in a special memoir spoken at some length of Dr. Goode's life and work. On February 13, 1897, a meeting to commemorate his life and services was held in the lecture hall of the Museum. A full account of that meeting will be found in the Museum volume of the Smithsonian Report.

After extended consideration of the needs of the Museum and the special qualifications of several persons who had been suggested as capable of filling the position rendered vacant by Dr. Goode's death, I signified to Mr. Charles D. Walcott my desire that, with the approval of the Regents, he should take charge of the Museum, and I was glad to learn that, notwithstanding his arduous duties as Director of the United States Geological Survey, he felt willing to put provisionally at the service of the Institution his known scientific and executive qualifications, together with those which a long previous connection with the Museum rendered doubly acceptable.

His appointment by me as acting assistant secretary in charge of the National Museum was ratified by the Board of Regents at its meeting on January 27, 1897.

Although comparatively little effort was made during the year to obtain specimens for the Museum, the accessions were 168 more than those of the previous year, representing a total increase of nearly 40,000 specimens over the receipts during 1896. The number of specimens of all kinds now recorded is 3,720,237.

In my estimates for 1897 I asked Congress to appropriate $180,000 for the preservation of the collections in the Museum. It is regrettable that only $153,225 were allowed for this purpose. As the collections in the Museum increase, it is imperative that a reasonable increase should be made in the funds devoted to their preservation and elaboration, and I most earnestly hope that the full amount which I have estimated to be necessary for the operations of the next fiscal year will be appropriated by Congress.

It is gratifying to record that, in accordance with my recommendation, the sum of $8,000 was allowed by Congress for the erection of galleries in the Museum building. They will materially add to the area available for exhibition purposes. By a fortunate circumstance the appropriation for the galleries became available at a time when the cost of ironwork of all kinds was unusually low, and at the time of writing this report four galleries are in process of construction. The desire of Congress to relieve the congested conditions which have so long existed in the Museum has been further manifested by an additional allowance of $8,000 in the Museum appropriations for 1898.

When the galleries are ready for the reception of fittings and furniture, there will arise the necessity for the expenditure of a considerable sum of money for this purpose. Congress has allowed $15,000 to be thus expended during the coming fiscal year.

It seems proper to remark that while the galleries which can be erected with this amount ($16,000) will be of very great benefit, the need of another building is not in any degree lessened, since the additional space obtained will be necessary to relieve the overcrowded condition of the floors, which has given the exhibition halls almost the appearance of storerooms.

The proper expansion of the various series now exhibited, and the addition of others from the abundant materials already in the possession of the Museum, can not have effect within the present walls. For these purposes, and for the increase which succeeding years may confidently be expected to bring, a new building is necessary. The national collections, as a whole, greatly surpass in value and extent those of any other museum in the United States, but they are at the present time by no means so well provided for as the collections assembled in some of the larger cities of the country.

For reasons explained at length in a previous report, it was deemed very desirable to remove the sheds adjoining the Smithsonian building on the south. The sum of $2,500 was allowed, and the objectionable sheds will be removed at once.

In my estimates for 1897 I explained the need of a larger allotment for printing the Proceedings and Bulletins of the Museum. The amount asked for was $18,000, but only $12,000 was allowed. I repeated the expression of my desire that $18,000 be allowed for 1898, but again only $12,000 was allotted. I feel, therefore, it is my duty to state that the sum provided has proved entirely inadequate.

The complete manuscript of an important and comprehensive work by the late Professor Cope on the reptiles of North America, based on the Museum collections, is necessarily withheld from the printer for want of funds for its publication, and at least four others, equally valuable and extensive, are in an advanced stage of preparation. Delay in the publication of these works will prove a hindrance to the progress of American natural history.

It has long been my wish to distribute the publications of the Museum not only to Government depositories but also to all public libraries, colleges, scientific schools, and scientific and technical societies in this country, as well as to the principal centers of learning throughout the world. After this has been done, there should still remain a surplus for distribution among investigators and teachers who really need the books in connection with their professional work. By such an extended distribution the Museum would greatly widen the scope of its usefulness, and would also be able to obtain in exchange a large number of books and pamphlets for its own working library. It often happens, too, that arrangements can be made to exchange the publications of the Museum for desirable specimens.

In the estimates for 1898–99 I have inserted an item for $17,000 for printing and binding. It is my wish to use $15,000 for printing and $2,000 for binding serials into volumes. Many books in constant use in the Museum library are in urgent need of permanent binding in order to prevent their destruction. Several series already partially bound should be completed, and thus rendered easier of access for purposes of reference. The amount above stated is the lowest sum which will produce the desired result, and I therefore very much hope that Congress will not diminish it.

In my last report I alluded to the death of Mr. William C. Winlock, which occurred on September 20, 1896. In addition to his duties as curator of exchanges, he held the position of honorary curator of physical apparatus in the National Museum.

Maj. Charles Bendire, United States Army (retired), honorary curator of the department of birds' eggs in the National Museum, died at Jacksonville, Fla., on February 4, 1897.

Major Bendire was a native of Germany. He served in the United States Army and was brevetted major for bravery in action against the Indians at Canyon Creek, Montana, in 1877. In 1886 he was retired for disabilities incurred in the line of service. During his residence in the West he became interested in ornithology through correspondence with Professor Baird. He made extensive collections of birds' eggs, which he afterwards presented to the National Museum. After leaving the Army he devoted a large amount of time to scientific investigation, and had recently published in the series of Special Bulletins of the National Museum an important work entitled Life Histories of North American Birds, consisting of two large quarto volumes, handsomely illustrated.

In obedience to the provisions of the act of Congress directing that the Institution and its dependencies should participate in the Tennessee Centennial Exposition, which opened on May 1, 1897, special exhibits have been prepared. Dr. Frederick W. True was appointed representative of the Smithsonian Institution and the National Museum on the Government board of management, and Mr. W. V. Cox was designated special agent in charge of the exhibit, and later was appointed secretary of the board.

BUREAU OF AMERICAN ETHNOLOGY.

The researches relating to the American Indians, conducted under the Smithsonian Institution by authority of law, have been continued by Maj. J. W. Powell, the director of the Bureau, assisted by Mr. W J McGee as ethnologist in charge, Mr. F. W. Hodge acting as chief clerk, and several other scientific collaborators. The field operations have been extended into a large number of States and Territories, and incidentally into those districts of neighboring countries occupied by native tribes closely affiliated with the aborigines of the territory now comprised in the United States. The examinations and surveys in field and office have been conducted with the view of obtaining for publication definite information concerning the arts and industries, the institutions and languages, and the beliefs of the natives.

During the year special attention has been given to the classification of the tribes in such manner as to indicate their origin and development, and to this end the rich archives of the Bureau, comprising the accumulations of eighteen years of research, have been subjected to careful study, and important conclusions have been reached.

The work of exploration within the United States was limited to Arizona and New Mexico, where extensive surveys and excavations were made by Dr. J. Walter Fewkes with most gratifying results. He was able to obtain the richest collection of prehistoric pottery and associated industrial products thus far made. This noteworthy collection has been installed in the National Museum, and special reports relating to it are in preparation. Other collections made in the Southwest include a highly interesting series of ceremonial masks used by the shamans in the pueblos of Zuñi and Sia; these also are installed in the Museum, and will be discussed in forthcoming reports. A collection of prehistoric ware from the mounds of the Mississippi Valley was acquired, and a smaller one of typical character was obtained from Ohio, while a small collection of special value in comparative study was obtained through the collaboration of an agent in Patagonia.

The investigation of the rich collection of prehistoric material obtained in Florida has been continued, and has yielded interesting results. Comparative studies have also been made of the stone implements used by the Seri Indians of Tiburon Island, and those of various other tribes.

The work relating to the social organization prevailing among the native tribes has been carried forward successfully, certain points hitherto obscure having been cleared up during the year.

The researches in linguistics have gone forward steadily. Progress has been made in the preparation of the comparative dictionary of the Algonquian dialects and in the collection of new material to be incorporated therein, and a systematic study has been made of the Iroquoian languages.

The study of decoration has been continued with renewed activity with the accession of the rich collections from Arizona, New Mexico, and Florida, while substantial progress has been made also in the researches concerning the aboriginal systems of mythology and their bearing on the industrial and social development of the tribesmen.

The publication of reports, which had fallen somewhat in arrears, has been brought up, and three reports, forming four large volumes, have been published and distributed during the year. The demand for these has been unprecedented.

An exhibit arranged for display in the Government building at the Tennessee Centennial Exposition included a representation of a Kiowa camp-circle, and a report on the Kiowa Indians explaining the social features illustrated by the exhibit is now in preparation for the press.

Further details concerning the operations of the bureau may be found in Director Powell's report, forming Appendix II.

INTERNATIONAL EXCHANGES.

The International Exchange Service was inaugurated in 1849, when Volume I of the Smithsonian Contributions to Knowledge was distributed to 173 foreign institutions, and within a few years it became the medium for the exchange of publications between the principal scientific institutions of the world. The functions of this Service being "the diffusion of knowledge" are in direct accord with one of the fundamental objects for which the Smithsonian Institution was endowed, and the fact that exchanges are now made with 28,000 correspondents in every part of the civilized world demonstrates to some degree the far-reaching influence of the Institution. The weight of matter handled by the Service during the past fiscal year was 247,444 pounds, comprising 81,162 packages of publications. From 1886 to 1895 there were received from foreign Governments and institutions 344,078 books or pamphlets, and 601,637 books or pamphlets were shipped abroad.

The entire cost of international exchanges prior to 1881 was borne by the Smithsonian Institution, but for the last fifteen years appropriations have been made by Congress. The first grant, in 1881, was $3,000, and for the past year it was $19,000. The exchange of Government documents is a most important part of the service, and I may repeat the statement made in several previous reports that for the continuation of such exchanges the Smithsonian Institution has annually advanced varying amounts in excess of the Congressional appropriations, so that the aggregate amount advanced from the private funds of the Institution since the Government has appropriated money for international exchanges is about $46,000.

The service now provides for the distribution of the United States Government publications to foreign libraries, and also for the distribution of books, pamphlets, and other printed matter sent as exchanges or donations from literary and scientific societies or individuals to correspondents abroad, without expense to senders beyond delivery at the Smithsonian Institution. Publications are received at the several foreign agencies of the Institution and forwarded to Washington for record and are distributed under the Smithsonian frank, the entire service being conducted under a careful system of registration which well nigh precludes possibility of loss. Purchased books, apparatus and instruments, are not received for transmission through the service, which is exclusively limited to donations or exchanges.

Through the long-extended courtesies from several ocean transportation companies, permitting the free transmission of exchange packages, it has been feasible to carry on the work in a very much more comprehensive manner than would otherwise have been possible with the resources available.

I may mention that exchanges with Mexico and Japan, which were temporarily interrupted at the time of my last report, have been renewed. Exchanges with Turkey, Greece, and Cuba, have necessarily been entirely suspended, although there is near prospect of renewed communications with the former country.

The international exchange of official documents has aggregated 10,694 parcels received from foreign countries for various Departments of the United States Government, and 30,008 packages have been received from United States Government Departments and shipped abroad.

This branch of the service is naturally of most importance to the Government, the foreign receipts having resulted in building up a very complete collection of publications of the various governments of the world.

The total amount available for use of the service during the year was $22,334.33, which included the Congressional appropriation of $19,000 and $3,334.33 received from various Executive Departments for transmission of their outgoing and incoming exchanges.

The slight increase in the annual appropriation granted by Congress during the past year for improving the facilities for the transportation of exchanges has permitted an improvement in the promptness of dispatch by the payment of ocean freight on a portion of the exchanges, which have been forwarded by some of the faster steamers instead of necessarily delaying shipments until opportunity permitted free transmission by later vessels. This slight improvement has benefited the service to such an extent that it is hoped Congress will realize the benefits derived from expenditures of this character, and in future will appropriate sufficient means to enable all exchanges to be forwarded to their destination without interruption. A great deal of matter forwarded through the International Exchanges is most valuable at the time of publication, and there should be no impediment to prompt delivery.

The number of correspondents at present on the records of the exchange service is 28,008, of which 21,427 are foreign—an increase of 2,527 over last year—and 6,581 are domestic, being an increase over 1896 of 567.

Appended to my report for the year ended June 30, 1895, was a map of the world showing the relative extent of the exchange service. Since that time the far-reaching influence of the service has added so many correspondents, and so many more remote countries have been reached, that the preparation of a new map to more nearly represent the number of correspondents in each country will soon be necessary.

THE NATIONAL ZOOLOGICAL PARK.

I have always been of the opinion that the collection of living animals is a legitimate method of furthering the objects for which the Smithsonian Institution was established. It was inevitable that sooner or later an intelligent interest in such collections should be awakened in this country, and some small collections have, in fact, for many years existed; but I believe that the Regents of the Institution were the first to consider the national importance of the preservation of the great American fauna, and with that object in view to recognize the need of establishing a preserve for their care and maintenance like the National Zoological Park, action in this matter being all the more imperative in view of the imminence of the extermination of many species of American animals, particularly the bison of our Great Plains.

It gives me pleasure to state that this interest in living animals has during the last few years greatly increased in this country, as is shown by the many new collections that have been established and the fact that still others are projected. A number of private individuals have undertaken the preservation of animals, paying particular attention to those likely to become extinct or that are best adapted to be kept on a range or in a forest. I can not but feel that the example of the National Zoological Park has been of some value in stimulating such enterprises, and that, though as yet comparatively incomplete, it has tended to develop an interest in our native animals and in their proper protection and preservation.

As instances of what has been done in this way may be mentioned the large forest park of the late Mr. Austin Corbin, the game preserves that have been established in various parts of the country, and the extensive zoological collections planned for the city of Pittsburg, Pa., and for the New York Zoological Society near New York City. Details with regard to some of these parks are given in the appendix.

Attention is called here to these enterprises because it seems desirable that an institution of this kind, placed at the seat of Government and under its control and direction, should be representative in character and not fall below the average of such collections, either at home or abroad.

It should be noted in this connection that most cities of the size of Washington are well provided with public parks possessing improvements in the way of roads, walks, bridges, buildings for public comfort, lawns, and plantations far in excess of what is possible under the present circumstances in the National Zoological Park. A moderate estimate, made by experienced landscape gardeners, places the cost of such desirable improvements at $3,000 per acre at least. Yet all that is here allowed for these purposes is the small sum that can be reserved after properly caring for the animals and erecting such structures as are required for their needs. While in most establishments of this kind the municipal authorities defray from a separate fund all the expenses of preparing the grounds and buildings and properly policing and maintaining the same, here all must be paid from a general sum that is not more than sufficient to merely maintain the animals in a proper condition.

The buildings and inclosures now existing are plainly inadequate.

One of the most important needs is the establishment of suitable houses for the preservation and care of birds. There should be a large building that could be heated in winter, and in addition a spacious flying cage similar to that used in the Golden Gate Park, in San Francisco, in which large numbers of our native birds could be harbored and fed. This would enable the park to extend its scope to the species of native birds now threatened with extinction. The passenger pigeon, once found in astonishing numbers throughout the Northern United States, has nearly disappeared because of the slaughter of the species by man,[1] and the Carolina parrakeet, a bird of most beautiful plumage, is already becoming scarce. At present it is impossible to take advantage of numerous opportunities of obtaining specimens of birds, as they can not be properly cared for within the park. Other buildings urgently

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[1] The following extract from Audubon's Birds of America may be of interest:

"The multitudes of wild pigeons in our woods are astonishing. Indeed, after having viewed them so often and under so many circumstances, I even now feel inclined to pause and assure myself that what I am going to relate is fact. Yet I have seen it all, and that, too, in the company of persons who, like myself, were struck with amazement.

"In the autumn of 1813 I left my house at Henderson, on the banks of the Ohio, on my way to Louisville. In passing over the barrens a few miles beyond Hardensburg, I observed the pigeons flying from northeast to southwest in greater numbers than I thought I had ever seen them before, and feeling an inclination to count the flocks that might pass within the reach of my eye in one hour, I dismounted, seated myself on an eminence, and began to mark with my pencil, making a dot for every flock that passed. In a short time, finding the task which I had undertaken impracticable, as the birds poured in in countless multitudes, I rose, and, counting the dots then put down, found that 163 had been made in twenty-one minutes. I traveled on, and still met more the farther I proceeded. The air was literally filled with pigeons; the light of noonday was obscured as by an eclipse; the dung fell in spots, not unlike melting flakes of snow, and the continued buzz of wings had a tendency to lull my senses to repose.

"Whilst waiting for dinner at Young's Inn, at the confluence of Salt River with the Ohio, I saw, at my leisure, immense legions still going by, with a front reaching far beyond the Ohio on the west and the beech-wood forests directly on the east of me. Not a single bird alighted, for not a nut or acorn was that year to be seen in the neighborhood. They consequently flew so high that different trials to reach them with a capital rifle proved ineffectual, nor did the reports disturb them in the least. I can not describe to you the extreme beauty of their aerial evolutions when a hawk chanced to press upon the rear of a flock. At once, like a torrent, and with a noise like thunder, they rushed into a compact mass, pressing upon each other toward the center. In these almost solid masses they darted forward in undulating and angular lines, descended and swept close over the earth with inconceivable velocity, mounted perpendicularly, so as to resemble a vast column, and, when high, were seen wheeling and twisting within their continued lines, which then resembled the coils of a gigantic serpent.

"Before sunset I reached Louisville, distant from Hardensburg 55 miles. The pigeons were still passing in undiminished numbers, and continued to do so for three days in succession. The people were all in arms. The banks of the Ohio were crowded with men and boys incessantly shooting at the pilgrims, which there flew lower as they passed the river. Multitudes were thus destroyed. For a week or more the population fed on no other flesh than that of pigeons and talked of nothing but pigeons."

needed are a new elephant house, in place of the hastily constructed temporary shelter now used, and a reptile house.

Many most interesting and at the same time inexpensive features, such as a vivarium for small animals, a small house and a runway for pheasants, and ponds for aquatic birds and mammals, are necessarily deferred for want of the funds required for their installation.

The appropriation for the fiscal year ending June 30, 1897, was in the following terms:

> 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, care, subsistence, transportation of animals, including salaries or compensation of all necessary employees, and general incidental expenses not otherwise provided for, sixty-seven 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 of the sum hereby appropriated, five thousand dollars shall be used for continuing the entrance into the Zoological Park from Woodley Lane, and opening driveway into Zoological Park, from said entrance along the bank of Rock Creek, and five thousand dollars shall be used toward the construction of a road from the Holt Mansion entrance (on Adams Mill road) into the park to connect with roads now in existence, including a bridge across Rock Creek.

It will be noted that a considerable portion of the sum appropriated is for the improvement of certain specified roads within the park and for the construction of a new bridge in connection therewith. Accordingly, a great part of the time that could be devoted to the improvement of the grounds has been spent in the development of these works. A narrow but sufficient roadway has been constructed along the line formerly occupied by the cart path known as the Adams Mill road and continued onward to the sharp bend in the stream near the animal house of the park, where an inexpensive but picturesque rustic bridge has been placed. This plan was deemed preferable to the building of a wide driveway with a high bridge that would be very costly, far beyond the scope of any appropriations to be expected from Congress, and likely at the same time to be an intrusion in the peaceful quiet of the valley, which it would span for some hundreds of feet.

A view of the bridge as finally erected is shown in the appendix. The foundation at either end rests upon the rock, and the main structure is formed of an arch of heavy oak logs solidly bolted together. The strength of the structure is far greater than is required for the amount of travel that may be expected over it, but not more than is needed to withstand the force of freshets that occur from time to time in the turbulent stream. A satisfactory result was reached at comparatively small expense, and though the superstructure will undoubtedly be in danger during times of great floods, it is believed that the foundation will always remain uninjured, so that even if the bridge is carried away, it can be renewed at slight cost.

The road which passes over the bridge should be continued onward up the valley of the creek as far as the northern boundary of the park, which is separated from the Rock Creek National Park by the Klingle road. This would give the public an easy access to the animal paddocks situated on the lower levels of the park, establish a thoroughfare between the Zoological Park and the Rock Creek Park, and afford to the residents of the country lying immediately to the west of the lower end of Rock Creek Park a convenient outlet toward the city.

But little has been done during the year to improve the means of access to the park. This is owing in great measure to the fact that the plans for the extension of the streets of the city are not yet fully matured, and no appropriations have yet been made for carrying them into execution. In consequence of this, there is no properly improved roadway connecting the recently completed road in the park with the city system.

The colony of beavers which has been installed along one of the streams of the park and temporarily secluded, has done very well, and the animals, while thus secured from annoyance or interference have worked as in a state of nature. It is hoped that they may soon be so far domesticated as to admit of their carrying on their very interesting pursuits under the eyes of the public, which is not yet possible.

The accessions to the collection during the past year have been few. The supply from the Yellowstone National Park, which was confidently counted upon, has in some respects been disappointing. Owing to the inroads of poachers and to inadequate winter pasturage, the bison in that park have greatly decreased in numbers and are widely scattered in almost inaccessible regions, so that it has been practically impossible to secure any of them in the corral built for their protection and preservation. Stringent penalties against poaching within that park have been enacted by Congress, and it is hoped that by careful attention a remnant of the herd of bison may yet be preserved. It is thought best not to disturb them at present by any attempts looking toward herding or capture.

While the bison have been decreasing in the somewhat unfavorable locality of the Yellowstone National Park, in other parts of the country some increase has occurred. Thus it is found that a portion of the great northern herd has been isolated near Ronan, Montana, and in the so-called panhandle of Texas a remnant of the southern herd continues to be maintained. It may be remarked that the vast regions of Texas contain many animals that would be of advantage to our national collection. Many of the Mexican species of deer range into this country, and many distinctively Mexican animals, such as the peccary and the jaguar, occur, while on the plains are found wild horses, supposed by some to be indigenous, and therefore anterior to the Spanish occupation, and there are even a few camels running wild in some of the more inaccessible portions of the country, relics of a herd imported for purposes of transportation before the days of the Pacific railroads.

ASTROPHYSICAL OBSERVATORY.

The operations of the Astrophysical Observatory have consisted chiefly in experiments in the bolographic analysis of the infra-red solar spectrum and the preparation of a report thereon, which was completed in May of the present year, and contains, in addition to introductory, historical, descriptive, and theoretical matter and accounts of subsidiary investigations, tables of positions of 222 absorption lines in the infra-red solar spectrum in terms of angular deviations and refractive indices for a rock-salt prism, and of the approximate wave-lengths corresponding.

These results are based on observations taken between October, 1896, and January, 1897, which are far superior to any before obtained, by reason of the great improvements in instrumental equipment spoken of in my last report. In this research it has thus been necessary to bestow what may seem inordinate time on the production of preliminary results chiefly useful in indicating requisite refinements of apparatus and of method. This once done, the results to be obtained in a few months exceed in value, for the main purpose of the investigation, all that have been obtained in as many years. An examination of the earlier bolographs has, however, recently been undertaken, and a great number of measurements made on the relative amounts of energy in the various parts of the spectrum, which, it is hoped, will lead to interesting results when compared with weather records covering the same interval of time.

A more detailed account of the work of the Observatory will be found in the appendix. It is regretted that owing to unforeseen delays, not in any way due to the Observatory, the report above mentioned is not yet in type, so that the results obtained, which it is believed will be a contribution of interest and value to physical science, are not yet generally accessible. I trust that the obstacle preventing publication may be removed immediately after the assembling of Congress.

Respectfully submitted.

S. P. LANGLEY,

Secretary of the Smithsonian Institution.

APPENDIX TO SECRETARY'S REPORT.

APPENDIX I.

THE NATIONAL MUSEUM.

SIR: The following statement constitutes a résumé of the most important operations of the National Museum during the fiscal year ended June 30, 1897:

Accessions.—The records show the receipt of 1,467 separate accessions. These figures indicate an excess of 168 lots over the receipts of the previous year. The number of specimens embraced in these accessions is nearly 112,000, representing an increase of more than 50 per cent over last year. The following accessions are among the most interesting:

Of the zoological material, mention should first be made of two exceedingly valuable and interesting collections from Trong, Lower Siam, presented by Dr. W. L. Abbott. This material is of unusual interest, the insects and shells representing many species new to the Museum collection. The specimens were received in a very satisfactory condition. Mr. A. Boucard, Spring Vale, Isle of Wight, England, transmitted a specimen of the rare mound fowl, *Leipoa ocellata*, from Australia. Dr. L. T. Chamberlain, New York City, presented shells from Central America and the West Indies, and specimens of tourmaline from Paris, Me. From Cornell University, Ithaca, N. Y., invertebrates collected by the Cornell Expedition to Greenland in 1896 were transmitted by Prof. J. H. Comstock. Mr. D. W. Coquillett, United States Department of Agriculture, sent 860 specimens of beetles of the family Tachinidæ, including type specimens. From Hon. W. B. Brownlow, Member of Congress, have been received, on deposit, birds' skins and other natural-history specimens from British Honduras. Mr. J. G. Foetterle, Petropolis, Brazil, presented 172 specimens of Brazilian Lepidoptera, representing 115 species. Mr. S. Nozawa, Sapporo, Japan, sent a collection of reptiles, batrachians, and fishes obtained at Yesso Island. A very interesting collection of beetles collected in the Kongo region was received from Dr. D. W. Snyder, Nashville, Tenn. Dr. W. L. Ralph, Utica, N. Y., presented birds' skins from the western section of the United States. Zoological material representing several groups was collected by Prof. O. F. Cook during a trip to Liberia. A collection of land and fresh-water shells was transmitted by the Perak Museum, Straits Settlements.

Among the most important additions to the herbarium are included a series of 1,000 plants, representing the private collection of Dr. W. H. Forwood, U. S. A., Soldiers' Home, Washington, D. C.; 432 herbarium specimens, also from Dr. Forwood; 500 specimens from the Biltmore Herbarium, Biltmore, N. C.; a collection of plants obtained by Prof. O. F. Cook, of the National Museum, while in Africa, and a collection of Spanish plants presented by Mrs. Cook.

A number of microscopic sections from the collection of the Fortieth Parallel Survey were transmitted from the United States Geological Survey by Mr. Arnold Hague.

Mr. R. D. Lacoe, Pittston, Pa., forwarded 208 specimens of tertiary plants from Florissant, Colo., for addition to the Lacoe collection. Dr. C. E. Beecher, Yale Museum, New Haven, Conn., presented a fine series of fossils, models of *Triarthrus becki*, a species of crustacean, and a model of a trilobite with appendage.

Mrs. J. Crosby Brown, New York City, contributed a collection of ethnological objects from the western coast of Africa; Dr. W. L. Abbott transmitted some very valuable ethnological specimens from Lower Siam, and Dr. D. W. Snyder sent material of a similar character from the Kongo region.

A collection of engraved diplomas, inscriptions, etc., was presented to the Museum by Messrs. Tiffany & Co., New York City. From Mr. Yang Yü, Chinese minister in Washington, a large blue porcelain vase, a bronze urn, and a string of beads were received.

Prof. D. P. Todd forwarded from Amherst College two frames of photographs illustrating the work of the Amherst Eclipse Expedition to Japan in 1885. Miss M. A. Henry, Washington, D. C., transmitted specimens of electrical apparatus, and a number of diplomas and medals presented to her father, Prof. Joseph Henry.

The scientific and administrative staff.—On September 6, 1896, by the untimely and unexpected death of Dr. G. Brown Goode, the Museum was deprived of its immediate head. It was not feasible to fill at once and permanently the vacancy thus most unfortunately occurring, and on January 27, 1897, I had the honor to receive the temporary appointment of acting assistant secretary of the Smithsonian Institution, in charge of the Museum.

Mr. W. C. Winlock, who, in addition to his special work as curator of the Smithsonian Bureau of International Exchanges and aid in charge of the office of the Institution, held the honorary position of curator of physical apparatus in the National Museum, died on September 20, 1896. Maj. Charles Bendire, United States Army, honorary curator of the department of birds' eggs, died on February 4, 1897. Mr. M. L. Linell, aid in the department of insects, died on May 3, 1897.

Dr. L. T. Chamberlain, of New York City, was, on January 11, 1897, appointed honorary custodian of the collection of gems and precious stones. Mr. J. N. Rose, Mr. C. L. Pollard, and Prof. O. F. Cook, have been appointed assistant curators in the department of botany. Mr. J. L. Willige was designated acting chief clerk in February, 1897, relieving Mr. W. V. Cox, who was appointed special agent in charge of the exhibit of the Smithsonian Institution at the Tennessee Centennial Exposition. Mr. Henry Horan, who, since 1880, had filled the position of superintendent of buildings, died on September 29, 1896. Mr. Horan first became connected with the Smithsonian Institution in 1857. Mr. J. E. Watkins, curator of the technological collections, was placed in charge of the new division of buildings and superintendence, retaining, in addition, the curatorship of the technological collections.

Distribution of specimens.—More than 26,000 specimens have been distributed during the year, including gifts to institutions, exchanges, and specimens sent for study. About one-half of the entire number consisted of herbarium specimens, and of the remainder more than 3,700 were marine invertebrates, and about 2,400 geological specimens.

Visitors.—The number of visitors to the Museum building during the year was 229,606, and to the Smithsonian building 115,709, making a total of 345,315. It should be remarked that in the total for this year is included the increase in the number of visitors occasioned in March by the inaugural ceremonies of President McKinley.

Specimens received for determination.—The number of lots of material received for determination continues to increase. During the fiscal year just closed there were 716 accessions of this character, 174 more than during the preceding year. The labor involved in the identification of this material consumes a large share of the time of several of the curators, especially in the departments of birds, insects, mollusks, and geology, for which the Museum receives very little in return. It is believed, however, that an important field of usefulness is found in this direction.

Tennessee Centennial Exposition.—The sum of $30,000 was appropriated by Congress for the erection of a building for the Government exhibit at the Tennessee Centennial Exposition, and the sum of $100,000 for the exhibit proper and for other incidental expenses. Of the latter amount an allotment of $14,500 was made to the Smithsonian Institution and the National Museum. This amount was afterwards increased by $1,700. Dr. Frederick W. True was designated representative of the Institution and the Museum on the Government board of management. Mr. W. V. Cox was made special agent in charge of the exhibit, and was afterwards also selected as secretary of the Government board. Exhibits were prepared by 18 departments and sections of the Museum, and were shipped to Nashville in the latter part of April, 1897, the Exposition opening on May 1. A detailed statement regarding the participation by the Institution and its dependencies in this Exposition will be presented in the report for 1898.

Foreign exchanges.—Exchanges of specimens have been made with a number of foreign museums. Among them may be named the Albany Museum, Grahamstown, South Africa; Australian Museum, Sidney, New South Wales; Berlin Zoological Museum, Berlin, Germany; Botanical Gardens, Calcutta, India; Branicki Museum, Varsovie, Russia; British Museum, London, England; Canterbury Museum, Christchurch, New Zealand; Manchester Museum, Manchester, England; Museum of Natural History, Paris, France; Museum of Natural History, Lyons, France; Museum of Natural History, Genoa, Italy; Museum of Natural History, Geneva, Switzerland; Provincial Museum, Victoria, British Columbia; Royal Zoological Museum, Turin, Italy; St. John's College, Shanghai, China; Zoologiske Museum, Copenhagen, Denmark; Zurich Botanical Gardens, Zurich, Switzerland. Exchanges of interest and value have also been made with individuals, among whom may be named Mr. A. Batalin, St. Petersburg, Russia; Mr. Stefan Chernel von Chernelháza, Köszeg, Hungary; Mr. C. Copineau, Doullens, Somme, France; Dr. Wheelton Hind, Stoke-upon-Trent, England; Prof. Wilhelm Leche, Stockholm, Sweden; Mr. J. de Morgan, Gizeh Museum, Egypt; Mr. Victor Ritter von Tschusi zu Schmidhoffen, Hallein, Germany.

Publications.—The Report of the Museum for 1894 was published during the year, and on June 30 the Report for 1895 was practically all in type.

Volume 18 of the Proceedings was issued in bound form. About twenty papers belonging to Volume 19 were published separately, and advance sheets of three papers, to be included in Volume 20, appeared during the year.

In the series of Bulletins two numbers have been issued—No. 47, the first part of an elaborate work entitled, "The Fishes of North and Middle America," by David Starr Jordan and Barton W. Evermann; and No. 49, "A Bibliography of the Published Writings of Philip Lutley Sclater," by the late Dr. G. Brown Goode.

Two important monographs have been issued as special bulletins. The first of these (Special Bulletin No. 2) consists of a work on the deep sea and pelagic fishes of the world, by Drs. G. Brown Goode and Tarleton H. Bean. This volume contains 553 pages and is accompanied by an atlas of 123 plates. The second (Special Bulletin No. 3) constitutes the second volume of Major Bendire's "Life Histories of North American Birds," and contains 518 pages and seven colored plates.

Explorations.—Dr. William L. Abbott has extended his travels into Lower Siam, and as the result of his explorations the Museum has already received two very large and exceedingly valuable collections, including natural history specimens and ethnological objects. Many of the mammals were obtained at high altitudes, and all of the material is of a peculiarly interesting character.

Additional collections of ethnological objects from Arizona and New Mexico have been received from Mr. J. Walter Fewkes. A specimen of Canyon Diablo meteorite was also obtained and forwarded by him.

Dr. David Starr Jordan, president of the Leland Stanford Junior University, transmitted, in behalf of the Fur Seal Commission, a collection of natural history specimens obtained in Japan and Bering Sea.

Prof. O. F. Cook obtained during his travels in Africa valuable collections of flowers, ferns, etc., which have been added to the herbarium.

Collections of mammals, plants, invertebrates, and other natural history specimens, were made by Dr. E. A. Mearns, United States Army, in connection with his explorations in New York, Minnesota, Virginia, and Maryland, and these have been transmitted by him to the National Museum.

Collections of natural history specimens obtained by the Fish Commission steamers *Albatross* and *Fish Hawk*, and by field parties sent out by the Fish Commission, have been received during the year. Important collections have been transferred to the Museum by the Department of Agriculture. The United States Geological Survey has sent in large collections from all parts of the country, including material obtained by the Fortieth Parallel Survey.

Respectfully submitted.

CHAS. D. WALCOTT,
Acting Assistant Secretary in charge of the
United States National Museum.

MR. S. P. LANGLEY,
Secretary of the Smithsonian Institution.
AUGUST 1, 1897.

Appendix II.

REPORT OF THE DIRECTOR OF THE BUREAU OF AMERICAN ETHNOLOGY FOR THE YEAR ENDING JUNE 30, 1897.

SIR: Ethnologic researches have been conducted during the fiscal year in accordance with the act of Congress making provision "for continuing researches relating to the American Indians, under the direction of the Smithsonian Institution," approved June 11, 1896.

The researches have been carried forward in accordance with a plan of operations submitted on June 13, 1896. The field operations of the regular officers of the Bureau have extended into Arizona, Indian Territory, Iowa, Maine, New Brunswick, New Mexico, New York, Oklahoma, and Ontario, and operations have been carried on by special agents in California, Colorado, Idaho, Oregon, Utah, and Washington, as well as in British Columbia and different provinces in Argentina, Chile, and Mexico. The office researches have dealt with material from most of the States and from many other portions of the American continents.

The chief duty imposed on the Bureau at the outset was the classification of the Indian tribes; and, since the sciences of man were inchoate when the Bureau was instituted, this duty involved the organization of those branches of the science dealing with ethnic relations. Accordingly, a classification of ethnic science has grown up in connection with the classification of the tribes, and has been perfected from year to year; and during recent years, and particularly during the fiscal year just closed, the operations have been shaped by this classification of the subject-matter of the science. The primary lines of special research relate to (1) arts or esthetology, (2) industries or technology (including archeology), (3) institutions or sociology, (4) languages or philology, and (5) myths or sophiology, as well as the requisite classificatory work involving researches in somatology and especially in psychology. The special researches are initiated in the field and completed in the office, giving rise to (I) field research (including exploration), and (II) office research, which together constitute the original scientific work of the Bureau; while the demands of the public service and the needs of the collaborators give rise to (III) work in descriptive ethnology, (IV) bibliographic work, (V) work in collecting, (VI) publication, and (VII) concomitant administrative and miscellaneous work.

FIELD RESEARCH AND EXPLORATION.

At the beginning of the fiscal year the Director was engaged in a reconnoissance of shell mounds and other antiquities on the coast of Maine; here he was joined by Mr. Frank Hamilton Cushing, and a number of shell mounds were surveyed and excavated with success. Later in the season the Passamaquoddy Indians still living in the vicinity were studied with some care, and their industries, especially in house building, were investigated; subsequently some of the older men of the tribe were employed to collect material for and to erect an aboriginal wigwam, which was afterwards transferred to the Zoological Park at Washington.

During July and August Dr. J. Walter Fewkes was occupied in making surveys and excavations of ruins, chiefly in Arizona, with the primary object of collecting prehistoric material for the enrichment of the National Museum, but with the secondary purpose of investigating those activities of the aborigines recorded in the products of their handiwork still extant. His operations were notably successful.

Early in July Mrs. Matilda Coxe Stevenson proceeded to Zuñi pueblo for the purpose of investigating certain ceremonies not adequately studied hitherto, to the end that they might be incorporated in her monograph on the Zuñi Indians. She remained throughout half of the fiscal year, and was able to complete her researches in a satisfactory manner. Incidentally, she obtained at Zuñi and Sia a number of sacred masks used in the peculiar religious ceremonies of the people of the pueblos, which have been transferred to the National Museum.

Toward the end of July Dr. Albert S. Gatschet repaired to eastern Maine and adjacent portions of New Brunswick in search of linguistic material among the tribesmen still living on St. Croix River. His mission was successful. A large body of vocables, paradigms, and texts representing the Passamaquoddy dialects of the Algonquian linguistic stock was secured, and he was able also to trace definitely, for the first time, the derivation of many of the peculiar place names of eastern Maine.

From the middle of August until the middle of December Mr. J. N. B. Hewitt was occupied in collecting material representing the languages and mythology of the Iroquoian Indians located in central New York and southern Ontario. His work was eminently productive, yielding a large amount of material of exceptional use for comparative studies in the philology and sophiology of the Indians.

Toward the end of September Mr. James Mooney repaired to Indian Territory and Oklahoma, where he spent several months in collecting information and material relating chiefly to the Kiowa Indians. The primary purpose of the trip was research concerning the peculiar heraldic system of the tribe; another purpose was the continuation of study of the use of peyote or "mescal" (a toxic plant corresponding measurably with hashish) in the ceremonies of the Kiowa, Apache, and other Indians; later in the season advantage was taken of his presence on the ground to make a collection representing the Kiowa camp-circle for exhibition at the Tennessee Centennial Exposition at Nashville.

In April Mr. W J McGee visited the Muskwaki Indian settlement near Tama, Iowa, with the object of beginning a special study of the social organization of this little-known tribe. Although preliminary only, his operations were successful. Incidentally he collected a quantity of aboriginal material for the National Museum.

Early in 1896 Mr. J. B. Hatcher, of Princeton University, was commissioned as a special agent of the Bureau to obtain photographs and other data pertaining to the aborigines of Patagonia and Tierra del Fuego. He reached the field and commenced operations in the course of a few months, and reports of progress were received early in the fiscal year. His field work was completed in June. The photography was moderately successful only, but the pictures were supplemented by a small though interesting collection of objects representing the handiwork of these southernmost representatives of the American aborigines. The success of the work, due in part to Mr. Hatcher's energy and intrepidity, was promoted through the courtesy of various officials of Argentina and Chile, special credit being due to Dr. Estanislao Zeballos, formerly minister plenipotentiary from Argentina to the United States.

On December 17, 1894, Dr. Willis E. Everette was given an honorary commission to collect linguistic and other material among the aborigines of Oregon, Washington, British Columbia, and western Mexico, and from time to time he has submitted valuable linguistic material produced by his researches in these provinces. Especially noteworthy contributions during the year relate to the Tĕnĕ or Athapascan Indians of Oregon.

Early in September Mr. E. T. Perkins, jr., of the United States Geological Survey, reported the discovery of certain remarkable Indian carvings in Snake River Valley, Idaho; and Mr. Perkins was temporarily detailed, through the courtesy of Hon. C. D. Walcott, Director of the Survey, to make studies and photographs representing these carvings. The work was completed about the close of October.

Early in 1897 Mr. H. S. Gane, of the Geological Survey, while on a temporary furlough, made a trip through the San Juan country in southwestern Colorado and northwestern New Mexico, under a commission from the Bureau, for the purpose of reconnoitering and photographing prehistoric works. His notes and pictures were duly transmitted and have been found of special value.

The information and material obtained by means of these field operations have been utilized in large part in the preparation of reports; other portions have been added to the archives for use in prospective investigations, while most of the objective material has already been arranged in the National Museum in such manner as to be accessible for study. The scientific results of the work are set forth in other paragraphs.

Office Research.

WORK IN ESTHETOLOGY.

During the greater part of the year Mr. Frank Hamilton Cushing was employed in arranging and cataloguing the remarkable collection of relics exhumed from salt marshes in western Florida during the previous fiscal year and in preparing his report for the press. The objects collected embrace a wide variety of domestic implements and utensils, weapons for use in war and the chase, fabrics for apparel and fishing, appurtenances to water craft, etc. In addition, there were many objects such as are used in primitive ceremony, comprising elaborately painted and carved masks and effigies, while many of the industrial devices are painted and carved in a manner remarkable for wealth of imagery and delicacy of execution.

An important part of Mr. Cushing's work was comparative study of the designs, in form and color, found in connection with the ceremonial and other objects; and substantial progress was made in the interpretation of the designs. Most of these are zoic. The bear, the wolf, the wild-cat, the woodpecker, and different waterfowls and aquatic animals are represented in carvings and paintings with a fidelity to detail which renders them not only readily identifiable but really artistic. Some of the effigies approach the natural size, and are attached to other articles in such manner as to indicate that they were worn as masks or crests, probably in dramatic ceremonies analogous to those of the Indians of the pueblos and other primitive peoples. These elaborate carvings are associated with wooden masks, shaped to fit the face, bearing painted and carved designs of corresponding character, but more or less conventionized in form and color. The realistic or partially conventionized forms displayed on the masks are imitated not only on other ceremonial objects but also on the industrial devices, and the degree of conventionism increases as the representations are reduced in size or distorted to fit forms determined by various conditions, so that an unbroken series of stages in the development of convention may be traced all the way from the essentially realistic representation of the animal head to the design carved on the arrowshaft or tomahawk handle, which, at first sight, would seem to be decorative merely.

The sequence displayed in these esthetic designs is, indeed, paralleled in other collections; but the remarkably rich assemblage of aboriginal handiwork from the Floridian salt marshes, in which such perishable materials as wood, bone, plant fiber, feather work, paint, and even leathern thongs are preserved, is especially noteworthy for the completeness of the sequence and the large number of links represented. Accordingly the series of objects would seem to establish the view already advocated by different collaborators of the Bureau that higher esthetic decoration originates in symbolism, which may gradually be transformed through conventionizing, either in the interests of economy or to meet other industrial conditions.

During the previous year Dr. J. Walter Fewkes made a collection of fictile ware and other aboriginal material among the ruins of Arizona and New Mexico, which was regarded as rich beyond precedent. During the year just closed he made explorations yielding a still larger body of material, which has been subjected to preliminary study, and has already been arranged in the Museum. As during the preceding year, fictile ware was the predominant material. This ware is characterized by symbolic and decorative designs, represented sometimes by modeling or by inscribed figures, but more commonly by colors; and for the first time material has been obtained in sufficient quantity to afford presumptively complete series of designs for certain groups of aborigines at certain periods antedating Caucasian invasion, so that various stages in the development of esthetic designs may be traced nearly as definitely as in the Florida collection. In general the course of development traced in this way is parallel to that made out on the Florida coast. The course of development is from the essentially (though often crudely) symbolic to the conventional, and through various stages of conventionizing to forms and colors which, at first sight, would be regarded as decorative merely.

Accordingly the collection is important as a source of new light on the development of artistic concepts, while, at the same time, that course of developmental succession which it so clearly attests has been used successfully in tracing certain movements of the aboriginal population. It has long been known that, while most of the traditions of the pueblo peoples recount migrations in a southerly or southeasterly direction, there are subordinate indications of a northerly or northeasterly drift from snowless lowlands or from saline and shell-yielding shores, and at least one of the collaborators (Mr. McGee) has found indications of a culture migration from the once populous valleys of Sonora, with adjacent refuges in the form of entrenched mountains, northward into the region of cliff houses, whence the mesa-protected pueblos seem to have sprung. Now Dr. Fewkes is able to trace a similar northward drift of the esthetic designs characterizing the aboriginal pottery of the pueblos. This application of the researches in the development of esthetics among the American Indians is essentially new and is highly suggestive. Some of the results of the work are already incorporated in reports prepared for publication; others are held for comparison and elaboration as the research progresses.

While in Zuñi, and afterward at Sia, Mrs. Matilda Coxe Stevenson gave special attention to the masks and other regalia used in ceremonies, and, as already noted, obtained a number of especially sacred masks. She found the ceremonial regalia to be essentially symbolic; the masks themselves represent zoic deities, and their appurtenances are designed to express real or ideal attributes of the animals deified, while the associated regalia and insignia, including apparel and the paint applied to faces, bodies, and extremities, are symbolic of similar or related concepts. All of the symbols are conventionized in greater or less degree, yet the accompaniments of voice and gesture, and even the terms of the ritual, are designed to emphasize the symbolism, i. e., to concentrate attention on the idea symbolized and divert attention from the conventionism.

Primarily the ceremonies and regalia are dramaturgic, and the use of the more important regalia is limited to the ceremonial representation; yet to some extent the mystical or sacred characteristics are supposed to cling to the actors in the mystical drama, and in some measure affect their everyday life; sometimes the actors are thereby strengthened in their positions as shamans, and they, as well as others, may continue to wear the less important regalia, or carry about their persons miniature symbols of the specially deific objects. In this way the devotional sentiment and the symbolism in which it is crystallized are expressed in everyday life and commonplace manners; and the devotion and symbolism find some expression in ordinary handiwork and still clearer expression in the more unusual handiwork involved in making and decorating the many articles connected with ceremonial rites. The observations are highly significant, in that they indicate the characteristics and the dominant influence of devotional sentiment among primitive peoples; they are especially useful, too, in that they aid in interpreting the symbolism depicted on prehistoric relics and corroborate the interpretations already rendered.

In 1877 Mr. E. W. Nelson, an acute observer and trained naturalist, was commissioned to make collections for the United States National Museum in Alaska and adjacent territory in North America and Asia. In connection with other duties, he was authorized to make ethnologic studies and collections among the Eskimo and other Indians at the cost of the Bureau soon after its institution. He spent some years among the tribes, obtaining vocabularies and other linguistic material and making large collections of esthetic and industrial handiwork. He also prepared a preliminary draft of a report on the ethnology of the region covered by his operations. On his return to Washington the collections were transferred to the National Museum, but failure of health prevented him from completing the preparation of the report, so that the collections have hitherto remained without adequate explanation. During the present fiscal year he returned to Washington from a prolonged absence, chiefly in Mexico, and at once undertook the completion of the long-delayed report.

Through the courtesy of Museum officials the collection was brought together for renewed study and the preparation of necessary illustrations. 'Mr. Nelson's original manuscripts were placed in his hands and, before leaving the city in April, he had practically completed a general report with illustrations of typical objects representing the handicraft of the hyperborean tribes with whom he came in contact during his sojourn about the Arctic border. The report is particularly valuable in its full description and illustration of the decorative designs characterizing Eskimo art. The Eskimo are distinctive in many respects, but in none more strongly than in their artistic development; they are clever draftsmen and fairly deft carvers of wood, bone, and ivory; many of their implements, weapons, and utensils are graved with artistic devices or sculptured in artistic forms, and the graving and carving apparently represent a highly conventionized symbolism. Mr. Nelson's motive is accurate description and faithful illustration of objects rather than analysis and synthetic arrangement of designs; yet his memoir is a rich repository of material from which the course of development represented by Eskimo art may be traced.

WORK IN TECHNOLOGY.

While in contact with the Passamaquoddy Indians on the coast of Maine, the Director and Mr. Cushing had opportunity for studying certain primitive industries yet retained by this partially accultured people. Conspicuous among these were the industries connected with the building and furnishing of domiciles. The long persistence of domiciliary industries among these Indians may be explained, at least in part, by the fact that the birch-bark wigwams are remarkably serviceable and economic, so that they were only slowly displaced by the little more commodious and much more expensive houses of civilization. At the same time, there are strong indications of ceremonial observances in connection with the erection of habitations, which doubtless serve to prolong the retention of the aboriginal type.

There is a single model for the dwellings of this branch of the Algonquian Indians. The structure is rectangular in plan, about 12 by 15 feet, with a narrow doorway in one end; the end walls stand vertical, while the sides, after rising vertically for 5 or 6 feet, are continued upward to form a curved roof, interrupted by an orifice over the center of the earthen floor for the exit of smoke. The framework is of light arbor-vitæ poles, neatly cut and shaped by stone implements and fire, the uprights set in the ground and lashed to the horizontal pieces by means of withes or splints; the walls and roof are made from large sections of birch-bark, carefully overlapped shinglewise and skilfully sewn together with slender splints of ash. The door is a dressed deer skin, attached to a light crossbar, while the smoke hole is provided with a shifting wind guard which may be so adjusted as to draw out the smoke and exclude most of the rain or snow in case of storm. The wigwam constructed in this way is practically wind proof and nearly rain proof, strong enough to resist the force of storms and the weight of winter's snow, and is capacious and commodious in almost the highest possible degree in proportion to the material employed in construction. It lasts five years or more without repairs, and with occasional repairs as needed may last a generation. As a means of studying the house and house building, two aged Indians were employed to set up a wigwam near the field of work in Maine, and with a view of extending the study and at the same time perpetuating this form of aboriginal handicraft, they were afterward engaged to erect and furnish the structure in Washington. It was at first designed to place it in the National Museum, but in view of the limitations of space it was afterward decided to locate the building in the National Zoological Park.

While supervising the work of the Indians on the wigwam, the Director and Mr. Cushing observed them using a curved knife, held in the hand with the blade projecting toward the body (the handle being flattened to fit the face of the thumb by which the attitude of the curved blade is controlled), and drawn toward the body in use; and the resemblance of the implement to that found among the primitive peoples of Japan and the similarity in use were at once noted. At the same time Mr. Cushing, who was fresh from the tidal marshes of Florida in which curved knives of shell are entombed, was enabled to interpret more clearly the Floridian shell knives and tooth knives, and infer the manner of their use, which must have been prevailingly centripetal or inward, rather than centrifugal or outward from the body like the tools of civilization. This simple discovery throws strong light on the development of primitive industries and removes difficulties hitherto encountered in the interpretation of primitive implements and workmanship. Then, on examining the shell mounds and house mounds on the Maine coast, Mr. Cushing was enabled to explain the occurrence of certain split teeth of the beaver found in such associations as to suggest habitual use; for he found, on attaching them to handles similar to those of the curved knives, that they constituted surprisingly effective implements for shaving and carving wood, for opening the skins and severing the tissues of animals, and indeed for performing all of the multifarious functions of the knife. At once it became evident that the beaver-tooth knife was much more efficient, and among hunters more economic in making and carrying, than the knife of chipped stone; and on investigating the history of the curved steel knives made by smiths for the Indians in accordance with their own designs, it became evident that the beaver-tooth knife was the prototype of that in use by the tribesmen today. At the same time, the connection between the shell knife of the Florida coast and the beaver-tooth knife of the Maine coast seemed so close as to indicate similarity in origin, the animal substance used in each case being that possessing at once the advantages of accessibility and of economy in manufacture and use.

Connected in bearing with the foregoing researches are those conducted during the year by Mr. W J McGee. During previous years he visited the Seri Indians of the Gulf of California, and collected various specimens of their handicraft. The collection comprises a series of stone implements, of which a number were observed in use, representing a stage in the development of stone art which has hitherto been obscure. Initially, these implements are natural pebbles picked up from among the quantities of similar pebbles shingling the beach; yet they are used for breaking the shells of crustaceans; for crushing bones of fish, fowl, and animals; for pounding apart the tough tissues of larger animals, or perchance for crushing and grinding mesquite beans, cactus seeds, and other vegetal substances. Originally selected almost at random, the stone is commonly used but once and then thrown away; but, if the habitation happens to be located near, the fitter stones are used over and over again, perhaps proving so serviceable that when the always temporary residence is changed they are carried away as a part of the domestic property of the matron. Eventually the stone becomes battered and worn by use, so that its shape is changed; then, if rendered less useful by the change, it is thrown away, while, if made more serviceable, it is retained to become a highly esteemed piece of property, always carried by the matron in her wanderings and buried with her body at death.

The series of implements collected, and the much larger series seen in Seriland, but not collected, show no trace of predetermined design in form or finish. The implements are fairly uniform in size, apparently because the users are fairly uniform in strength and the uses fairly uniform in force required, and they are fairly uniform in shape because of similarity in applications; but as a whole, the series is characterized by absence of design, by fortuitous adaptation rather than that complex invention represented by even the simplest chipping or flaking. The culture stage represented by the series has already been designated *protolithic*. It is to be noted that the Seri Indians have no other stone industry, save a little known and apparently accultural custom of chipping stone for the sole purpose of making arrowpoints, and that their knives, scrapers, awls, needles, and ordinary arrow points are made from shell, bone, wood, and other substances of organic origin. Now, on assembling the industrial devices of the Florida marshes, the Maine shell mounds, the Seri Indians, and the more primitive survivors of the Algonquian tribes located in the Maine woods, and comparing these with the corresponding devices of the American tribes generally, it is found that the industries involving the use of stone for implements or weapons fall into a highly significant order, which, despite some overlapping of phases, seems to represent the normal course of industrial evolution.

The first stage is that in which stone is used in natural or fortuitous condition, without predetermined design or invention, as among the Seri Indians; this is the *protolithic stage*. It is noteworthy that, in the typical case, and presumptively in others, the prevailing industrial devices of this stage are of organic material and approach in form and function the biotic armament of lower animals. They are the readiest substitutes for, and the direct analogues of, teeth and claws. The second stage is that represented by wrought stone, shaped largely or wholly in accordance with predetermined design, whether by battering (undoubtedly the original method) or by flaking and chipping; it may be called the *technolithic stage*. This stage is represented by most of the American tribes. It is clearly to be noted that this arrangement of stages in the development of primitive industry is based wholly on research among the American Indians and among the relics of their prehistoric ancestors. It is not designed to supplant or discredit classifications based on the industrial devices of other countries. It is constructive and not destructive, and is formulated merely as a contribution to scientific knowledge concerning the aborigines of the Western hemisphere.

Another line of research in technology, conducted chiefly during the year, though the results were incorporated in a paper accompanying the preceding report, relates to primitive surgery and medicine. The work, which was based on a collection of Peruvian crania, was conducted by Mr. McGee. Its details are significant, in that the interpretations are based on the primitive sophiology known to have prevailed among the aborigines up to the time of Caucasian invasion, rather than on the more realistic philosophy by which civilized practitioners are guided The stages of development of curative surgical treatment, as traced in the course of the researches, need not be repeated; suffice it to say that the investigation appears to illumine the previously obscure origin of surgery, and at the same time to throw much light on the origin and development of medical treatment in general.

In earlier paragraphs, summarizing the results of researches concerning the origin and development of the arts, incidental allusion is made to the intimate relation between the esthetic and the industrial. The relation is double—indeed, manifold—and reciprocal: In the first place, the industrial device is usually a medium for esthetic devices, graved or carved or painted upon it, usually as symbolic invocations to mystical powers whereby the efficiency of the implement or utensil may be augmented; while, in the second place, the execution of the esthetic devices constitutes an important and, in some lands, apparently a preponderant part, of the occupation of primitive people. Accordingly, the researches in esthetology, carried forward during the year by various collaborators, including Messrs. Cushing, Fewkes, and Nelson, and Mrs. Stevenson, have thrown light on the motives and other causes underlying industrial development.

WORK IN SOCIOLOGY.

In continuing the examination and digestion of material collected during the eighteen years of the existence of the Bureau, the Director has given special attention to the principles underlying the social organization of the American aborigines.

A portion of the results are summarized in a chapter on regimentation incorporated in a preceding report. The researches are still in progress.

Mr. W J McGee has continued the comparative study of social organization with special reference to the Seri and Papago Indians. In the former tribe the social organization appears to rest wholly on kinship traced through the female line; and one of the consequences of this organization and of the peculiar isolation of the people is found in a singular marriage custom which has been noted in previous reports. The Papago Indians, on the other hand, have an organization based primarily on kinship traced in the male line, but displaying also certain indications of transition into some such artificial system as that which, on further development, matures in civilization, i. e., sometimes the gentes are united in such manner that a single kinship group combines two totems; the kinship terminology is incomplete in such way as to suggest curtailment through disuse; through seasonal migrations and other causes there is a constant breaking up of family groups, followed by intermingling in new combinations in the form of colonies of patriarchal or even feudalistic character; there is clear recognition of patriarchal property right in the waters in which the material values of their arid territory inhere; while the governmental control, though nominally vested in patriarchal shamans, is really regulated by an officer selected through popular approval, who may be designated the people's attorney.

It is noteworthy that the Spanish invaders of the Western Hemisphere assimilated the aboriginal much more completely than the Anglo-Saxon invaders of more northerly regions, so that in many instances the social institutions prevailing in Mexico today have sprung from aboriginal germs. This is especially true of the patriarchal organization characteristic of the Mexican provinces remote from the greater cities and railways, which differs in no essential particular from the organization still found among the Papago Indians and recorded in their time-honored traditions. Now, the comparative studies of the Seri and Papago social organizations, with the analogue of the latter among the modern Mexicans, gives opportunity for clearing up certain misapprehensions concerning primitive society. In barbaric culture, in which descent is reckoned in the male line, the governmental control is vested in an elder man (whose seniority may be either real or assumed), so that the organization is patriarchal; and it has been inferred, without adequate observation and with undue influence growing out of the convenience of antithetic terms, that in savage culture, in which descent is reckoned in the female line, the social organization is matriarchal.

The case of the Seri Indians is perhaps the most striking among many examples, indicating that, even when descent is traced exclusively through the female line to the extent that the father has no control over his wife's property or his own children, the tribal control is, nevertheless, vested in male rulers, who may be either shamans of exceptional shrewdness or warriors of exceptional valor and cunning. Accordingly the term "matriarchal" can only be regarded as erroneous and misleading when applied to this culture stage. This becomes especially clear in the light of the observations among the Papago Indians and the mixed-blood Mexicans, in which the rule is patriarchal, but in which there is an associated matriarchy, for the wife of the patriarch occupies a position among the women and children of the group corresponding to that of her spouse, primarily among the men, but secondarily among all, so that patriarchy and matriarchy are in reality complementary aspects of that culture stage in which descent is traced in the male line. Confusion is avoided by designating the more primitive organization as *maternal* and the more advanced as *paternal*, and by restricting the terms patriarchal and matriarchal to their legitimate functions, as indicated by the usage of southwestern peoples. The details of the researches on this subject are too extended for summary statement; but the principles developed through the study are important as a means of interpreting observation and thus guiding special research and contributing to scientific knowledge of the aborigines. The work is still in progress.

WORK IN PHILOLOGY.

Linguistic studies were pushed forward energetically during the earlier years of the existence of the Bureau, partly as a means of classifying the Indians in such manner as to guide grouping on reservations. A considerable portion of the material collected was, after the immediate practical use, placed on file for comparison and study with a view to the discovery of the principles of linguistic development. During the fiscal year the Director has reviewed these records in conjunction with those pertaining to sociology and sophiology, and has made progress in developing the principles of philology and applying them to the ethnic problems presented by the American aborigines. In primitive society language grows in two ways: On the one hand there is a steady enrichment and differentiation due to the coining of expressions for new ideas; on the other hand there is a spasmodic enrichment and modification, both in terms and in grammatic structure, produced by the shock of contact (whether peaceful or bloody) with other peoples—the changes consequent on conquest being especially important, as has been shown by different philologists. At the same time both the lexic and the structural forms—i. e., both words and sentences—are simplified through the natural tendency toward economy in expression. These and other processes connected with the growth of language have been indicated in some detail in earlier reports.

Now, on examining the aboriginal languages of America, it is found that many of them are interrelated in such manner as to indicate specific courses of development, and in all such cases the dominant process has been the union or blending of more or less diverse elements, both lexic and structural. This blending can be explained only as a record of intertribal contact, and the cases are so numerous—indeed, they are characteristic of all of the aboriginal tongues—as to indicate that practically all of the native languages have been built up and shaped chiefly by the combination and blending of antecedently distinct and presumptively discrete tongues. This conclusion as to the development of oral speech in America is corroborated by the simpler history of the development of the so-called gesture speech, which was widely used by the Indians as a partial substitute for, and convenient supplement to, oral speech as an intertribal language. When the course of development ascertained by these comparisons is so extended as to apply to the entire assemblage of native American peoples, it at once becomes evident that the sixty linguistic stocks and five hundred dialects extant at the time of the discovery (themselves the product of long-continued combination and blending of distinct tongues, as the researches have shown) are indubitable records of still more numerous and still more widely distinct languages of an earlier time, and the more carefully the record is scanned the more numerous and the more distinct do the original components appear.

It is accordingly a necessary inference that a vast number of distinct, albeit simple if not inchoate, tongues originally existed in North America, and that the subsequent history has been chiefly one of linguistic integration. It is a corollary of this proposition, which is but the generalization of all known facts relating to the aboriginal languages of America, that the Western Hemisphere must have been peopled by the ancestors of the modern Indian tribes before the birth of language among them. Both the main proposition and the corollary run counter to earlier opinions entertained in this and other countries; yet they are not only sustained by the unprecedentedly rich collection of linguistic facts preserved in the Bureau archives or published in the reports, but by the cumulative evidence obtained through the researches concerning the arts, industries, institutions, and beliefs of the American aborigines. A more detailed report on this subject is in an advanced stage of preparation.

Dr. Albert S. Gatschet has continued the collection of linguistic material pertaining to the Algonquian Indians, and has made progress in the preparation of the comparative dictionary of Algonquian terms. The new material collected during the year was obtained chiefly among the Passamaquoddy Indians living in the woods of Maine and adjacent parts of New Brunswick. Advantage was taken of an opportunity to obtain a Nez Percé vocabulary, representing the Shahaptian stock, from Lewis D. Williams, an educated member of the tribe, who spent some months in Washington during the earlier part of the fiscal year. This record is deemed of special value, not only in that it is more complete than those representing the same stock already on file, but in that it affords means of checking and clearing up doubtful points in the earlier records.

In addition to collecting a rich body of material relating to the languages and beliefs of several Iroquoian tribes, Mr. J. N. B. Hewitt made considerable progress in the systematic arrangement of material collected during preceding years. One of the more important lines of his work was a study of the pronoun with special reference to its function in primitive language and its relation to other parts of speech. His researches indicate with greater clearness than others hitherto conducted that the pronoun occupies a much more prominent position in primitive speech than in the highly developed languages of cultured peoples. The preparation of a special paper on the subject was commenced by Mr. Hewitt toward the end of the year. Another line of work by Mr. Hewitt, originating in the collection of mythologic texts, was a comparative study of the creation myths of different Iroquoian and Algonquian tribes. The preliminary results of this study are especially significant in their bearing on conclusions derived from the study of language. On comparing half a dozen versions of the Indian cosmogony, he was able to detect unmistakable indications of interchange of such sort as to prove that originally independent myths have undergone considerable coalescence and blending, so that the myth, like the speech in which it is crystallized, is a composite of many elements. Coupled with the features indicating coalescence there are, indeed, certain features indicating differentiation, chiefly in the direction required to adjust the mythic personages to the local fauna; but the indications of differentiation are far subordinate to the evidence of coalescence or integration. A number of typical myths representing the aborigines of the northeastern United States have been brought together with a view to publication so soon as the general discussion is completed.

WORK IN SOPHIOLOGY.

The scope and extent of the researches in sophiology during the fiscal year are in some measure set forth in the foregoing paragraphs, for the various demotic activities are interdependent, and neither arts, industries, institutions, nor languages can be developed without the concomitant development of opinions, whether mythic or rational. Important additions to the material representing the symbolism and ceremonies of the Indians have been made through the labors of Mr. Cushing in Florida, Dr. Fewkes and Mrs. Stevenson in Arizona and New Mexico, Dr. Gatschet in Maine, and Mr. Hewitt in New York and Ontario, as already noted. Mr. James Mooney continued his researches relating to the Kiowa Indians, giving special attention to their heraldic and calendric systems, and to the use of the peyote or mescal in their ceremonies. It is well known that dreams and visions, commonly induced by fasting, play an important rôle in connection with the beliefs and religious usages of primitive peoples; it is known also that among some peoples drugs are used to intensify the abnormal condition attended by visions; but there is probably no better example of this custom than that afforded by the Kiowa and some neighboring tribes in their use of the peyote. The mental effects of the drug are something like those produced by hashish; its influence is so strong and so certain that the Indians using it have come to rely on it for the production of the ecstatic state regarded as essential to the proper performance of their ceremonial rites, while, in turn, the rites have been so adjusted to the effects produced by the drug that they are, in Mr. Mooney's opinion, completely dependent on it for their existence. Although the researches concerning the subject are not complete, preliminary announcements have been made concerning the results of scientific examination of the peyote and concerning its influence on the religious practices of the tribe.

In connection with his work on this subject, Mr. Mooney completed during the year a memoir on the Kiowa calendar system, which has been incorporated in the seventeenth annual report. This memoir is deemed noteworthy as a remarkably exhaustive rendering of what may be called the autobiographic history of an important tribe.

In his comparative studies of the Seri, Papago, and other tribes, Mr. McGee was led to consider the course of development of myth or of the explanation of phenomena in terms of the supernatural. It is significant that, so far as can be ascertained, supernaturalism is a more potent factor in determining conduct among the warlike Seri than among the peaceful Papago, and the examination of other tribes indicates that the relation is general, i. e., that the tendency toward supernatural explanation, with its concomitant effect on conduct, is gradually rectified by intertribal contact in a manner akin to that in which myths and languages are blended. The studies are still in progress.

DESCRIPTIVE ETHNOLOGY.

The preparation of material for the Cyclopedia of Indian Tribes was continued during the year under the immediate supervision of Mr. F. W. Hodge. As other duties permitted, Mr. Hodge continued extracting and placing on cards material relating to the Pueblo Indians and other southwestern tribes. The greater part of the work on the cyclopedia performed during the year was that of Dr. Thomas, who continued and nearly completed the revision, extension, and final arrangement of the voluminous body of material relating to the Algonquian Indians, the largest and most diversified of the aboriginal stocks of the territory of the United States. In his detailed report Dr. Thomas acknowledges gratefully the facilities afforded by several libraries of the national capital, especially the Congressional Library, whose rich store of rare literature has been most courteously made accessible by Librarian Ainsworth R. Spofford. Some additions to the cyclopedia were made also by other collaborators, particularly Mr. Mooney.

BIBLIOGRAPHY.

The bibliographic work of the Bureau was interrupted in 1895 by the death of James C. Pilling, who had prepared a series of reports on the literature relating to the languages of several aboriginal stocks (which were issued as bulletins during preceding years), and who had partially completed a similar report concerning the aboriginal languages of Mexico. During the last fiscal year an arrangement was made whereby this portion, at least, of the bibliographic work may be completed. The task was generously undertaken by Mr. George Parker Winship, librarian of the John Carter Brown Library, in Providence, already a contributor of valuable material to the Bureau. Mr. Winship began operations toward the end of the year. The material pertaining to Mexico, brought together by Mr. Pilling, was transferred to his custody, and by the end of the year he was able to report substantial progress in the work.

COLLECTING.

The chief work of the year in this department was that of Dr. J. Walter Fewkes conducted under the more immediate direction of the Secretary. Already in the field at the beginning of the fiscal year, Dr. Fewkes proceeded to the extensive ruin of Chevlon, on Little Colorado River, early in July. Later he excavated another ruin of imposing dimensions near Chavez Pass. His work was successful beyond precedent, yielding by far the finest and most extensive collection of aboriginal fictile ware and associated artifacts ever made in the United States. As noted in earlier paragraphs, the material is especially rich in symbolic painting and other expressions of the remarkable religious beliefs of the pueblo peoples during prehistoric times.

A noteworthy collection of ceremonial masks was made at Zuñi and Sia by Mrs. Matilda Coxe Stevenson, and has been duly installed in the National Museum. In the course of his field operations, Mr. Mooney obtained additional material illustrating the handiwork and ideas of the Kiowa Indians; and toward the close of the fiscal year, while temporarily detailed to make and arrange collections for the Tennessee Centennial Exposition at Nashville, he brought together and, with the aid of the Indians, constructed an exhibit showing in miniature the characteristics of the Kiowa camp-circle, the significance of which is not generally understood. Toward the end of the year Mr. Hatcher reported the transmission of a small collection representing the primitive industries of the aborigines of southern Patagonia. In April Mr. McGee obtained a small but interesting collection of aboriginal matting and wooden ware from the Muskwaki Indians, near Tama, Iowa. The greater part of the collection has been transferred to the Museum. Among the articles is a carved wooden dish corresponding in form, dimensions, and ornamentation with an earthenware type frequently found in the mounds; the specimen is of peculiar interest in that its form was determined by the curved beaver-tooth knife with which it was fashioned and in that its esoteric and essentially prescriptorial symbolism was ascertained, so that it explains one of the most persistent forms of aboriginal ware. Several other collaborators made minor collections, and a few others were acquired from correspondents. One of these is a series of iron tomahawk pipes, made for the Indian trade by the French pioneers and long used by the tribesmen in lieu of the aboriginal weapons of stone, shell, wood, and copper; another was a particularly fine collection, obtained from the mounds of Missouri and the adjoining part of Illinois by Col. F. F. Hilder; still another was a series of stone implements from the mounds of northern Ohio, which are regarded as especially desirable for purposes of comparative study in the National Museum.

PUBLICATION.

Mr. Hodge has remained in charge of the details of publication, and it is gratifying to be able to report activity, almost beyond precedent in the history of the Bureau, in this branch of the work. At the beginning of the year the Fourteenth Annual Report was partly in type, the Fifteenth was in the printer's hands, and proofs of illustrations had been received. The Sixteenth Report was in nearly the same condition. The editorial work was pushed forward successfully. About the end of the calendar year the Fourteenth Report was issued, in two volumes, and the distribution was at once commenced. The demand for the document was unprecedented, so that the edition was practically exhausted within three months. It may be observed that this report was more extensively noticed and reviewed, both in scientific journals and the ephemeral press, than any preceding publication by the Bureau, and that the tone of the reviews has been favorable or still more highly commendatory, without exception so far as known. Meantime the Fifteenth and Sixteenth Reports received constant attention, and both were completed and published about the end of the fiscal year. The demand for these documents also is pressing, and they, too, are being favorably received by the reviewers.

The manuscript of the Seventeenth Annual Report was transmitted for publication on June 18, 1897. The accompanying papers comprise "The Seri Indians," by W J McGee; "The Calendar History of the Kiowa Indians," by James Mooney; "Navajo Houses," by Cosmos Mindeleff; together with a fully illustrated account of the "Archeological Expedition in Arizona in 1895," by Dr. J. Walter Fewkes.

The material for the Eighteenth Report also was brought together, and the editorial work was well advanced before the end of the year. It is accompanied by two memoirs, each of considerable magnitude, so that it will be necessary to issue it in two volumes; the first of these is "The Eskimo of Bering Strait," by E. W. Nelson, and the other is the memoir on "Indian Land Cessions," by C. C. Royce and Cyrus Thomas, which has been described in earlier reports; the former is fully illustrated by photographs and drawings, representing the people and the extensive collections made by Mr. Nelson; the latter is accompanied by numerous maps.

MISCELLANEOUS.

Library.—The additions to the working library of the Bureau were unprecedented in number and value, particularly in respect to standard works of reference; meantime the normal growth due to accessions through exchange has continued. At the close of the fiscal year the contents of the library comprised 7,138 volumes, in addition to several thousand pamphlets and periodicals.

Illustrations.—During the earlier part of the year the preparation of illustrations for reports was continued under the direction of Mr. De Lancey W. Gill, the photographic work being executed by Mr. William Dinwiddie. Toward the end of the calendar year Mr. Dinwiddie retired from the Bureau, and on January 1 Mr. Wells M. Sawyer, formerly of the Geological Survey, was placed in charge of the illustrative work, including photography. This arrangement has been found satisfactory, and the illustrative work is now carried forward acceptably in all of its phases. Mr. Henry Walther has aided Mr. Sawyer efficiently in cataloguing and classifying negatives and prints, as well as in photographic printing.

Exhibits.—As noted incidentally in earlier paragraphs, an exhibit was prepared for the Tennessee Centennial Exposition in Nashville. It comprises half of a Kiowa camp-circle, represented in miniature, occupying a semicircular area with a radius of about 20 feet in a central portion of the Government Building. The installation of the material was completed in time for the formal opening, and before the end of the fiscal year it became evident that the display will be generally regarded as attractive and successful.

Respectfully submitted.

J. W. POWELL,
Director.

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

APPENDIX III.

REPORT ON THE OPERATIONS OF THE BUREAU OF INTERNATIONAL EXCHANGES FOR THE YEAR ENDING JUNE 30, 1897.

SIR: I have the honor to submit the following report upon the operations of the Bureau of International Exchanges for the fiscal year ending June 30, 1897:

The number of packages received from all sources for distribution during the year was 81,162, or 7,716 less than during the preceding year, although the territory reached by exchanges emanating from the Smithsonian Institution was increased by the addition of 2,527 names of institutions, libraries, and individuals in other countries than the United States, while the domestic list was increased by 567. The aggregate weight of packages handled was 247,444 pounds.

By reference to previous reports it will be noticed that three or four boxes of United States Congressional publications have been forwarded to each of the foreign national libraries every year, while but two boxes were forwarded during the fiscal year ending June 30, 1897. One more box to each of the fifty recipients would have made the total number of packages transmitted by the Institution equal to that of the previous year. It is also worthy of note that while the number of packages was less than during the preceding year, the number of boxes shipped abroad was nearly one-fourth greater than during the previous year. The difference is accounted for by the fact that the number of exchange packages reported as having been received during the fiscal year were those that had been delivered to the Institution between July 1, 1896, and June 30, 1897, while the cases shipped during the same period contained many tons of exchanges that had been delivered principally from United States Government Departments and Bureaus during the last week of the previous fiscal year, and which could not be forwarded during the same year in which they were received.

The comparison of exchanges during past years shows that transmissions are exceedingly variable, especially those from abroad, and while during one year thousands of parcels may be received from a single society for distribution in the United States, no more may come from the same society for two or three years, and when there may be several such instances in a single year it is easy to understand that the annual statistical tables must vary to a marked degree.

TABULAR STATEMENT OF THE WORK OF THE BUREAU.

The work of the Bureau is succinctly given in the following table:

Transactions of the Bureau of International Exchanges during the fiscal year 1896–97.

Date.	Number of packages received.	Weight of packages received.	Correspondents, June 30, 1897.				Packages sent to domestic addresses.	Cases shipped abroad.
			Foreign societies.	Domestic societies.	Foreign individuals.	Domestic individuals.		
1896.								
July	4,319	11,250						
August	4,434	12,073						
September	6,782	29,235						
October	9,422	24,603						
November	4,320	15,043						
December	3,799	12,119						

Transactions of the Bureau of International Exchanges, etc.—Continued.

Date.	Number of packages received.	Weight of packages received.	Correspondents, June 30, 1897.				Packages sent to domestic addresses.	Cases shipped abroad.
			Foreign societies.	Domestic societies.	Foreign individuals.	Domestic individuals.		
1897.								
January	8, 969	33, 980						
February	6, 434	22, 423						
March	7, 169	20, 973						
April	14, 276	24, 261						
May	5, 054	23, 866						
June	6, 184	17, 618						
Total	81, 162	247, 444	9, 414	2, 445	12, 013	4, 136	23, 619	1, 300
Increase over 1895–96	*a* 7, 716	*a* 11, 287	1, 392	330	1, 135	237	*a* 10, 472	257

a Decrease.

For the purpose of comparison the following table represents the number of packages of exchanges transmitted, and the increase in the number of recipients each year from 1891 to 1897:

	1890–91.	1891–92.	1892–93.	1893–94.	1894–95.	1895–96.	1896–97.
Number of packages received	90, 666	97, 027	101, 063	97, 969	107, 118	88, 878	81, 162
Weight of packages received	237, 612	226, 517	200, 928	235, 028	326, 955	258, 731	247, 444
Ledger accounts:							
Foreign societies	5, 981	6, 204	6, 896	6, 991	8, 751	8, 022	9, 414
Foreign individuals	7, 072	7, 910	8, 554	8, 619	9, 609	10, 878	12, 013
Domestic societies	1, 588	2, 044	2, 414	1, 620	2, 014	2, 115	2, 445
Domestic individuals	4, 207	4, 524	5, 010	2, 993	3, 034	3, 899	4, 136
Packages to domestic addresses	29, 047	26, 000	29, 454	32, 931	29, 111	34, 091	23, 619
Cases shipped abroad	962	1, 015	878	905	1, 364	1, 043	1, 300

EXPENSES.

The expense of the exchange system is provided for in part by direct appropriation by Congress to the Smithsonian Institution, and in part by the various Executive Departments of the United States Government, which in most instances pay 5 cents per pound for the transportation of their exchanges, both outgoing and incoming. This charge was exacted by the Regents before the United States Government made any appropriation whatever for exchanges, and since appropriations have been made the exchange system has been taxed to such an extent that it has never been possible to relinquish the practice of making this charge, which, during the past year, has realized an income amounting to $3,334.33.

The Congressional appropriation made to the Institution in support of the exchange system for the fiscal year ending June 30, 1897, read as follows:

> "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, nineteen thousand dollars."

The following statement represents the receipts and expenditures on account of the system of international exchanges for the year preceding July 1, 1897.

RECEIPTS.

	Congressional appropriation.	Other sources.	Total.
Direct appropriation by Congress	$19,000.00		$19,000.00
Repayments from United States Government Departments		$2,724.87	2,724.87
Repayments from State institutions		126.55	126.55
Repayments from other sources		482.91	482.91
Total	19,000.00	3,334.33	22,334.33

EXPENSES.

	From Congressional appropriation.	From other sources.	Total.
Salaries and compensation	$14,864.18		$14,864.18
Freight	2,291.56	$2,633.90	4,925.46
Postage and telegraph	200.00	1.00	201.00
Stationery and supplies	397.15	296.19	693.34
Packing boxes	710.20	393.28	1,103.48
Traveling expenses	357.28		357.28
Balance to meet outstanding liabilities June 30, 1897	179.63	9.96	189.59
Total	19,000.00	3,334.33	22,334.33

On account of the inadequacy of available resources with which to bear the expense necessary to the forwarding of all exchanges by the most expeditious routes, it has at all times been necessary to obtain free ocean freight when practicable, and while some of the steamship companies have forwarded exchanges as promptly as other freight on which the full rates were paid, several of the lines have only been willing to take exchange cases when there was ample room.

To provide more adequate means with which to defray the expense of forwarding exchanges promptly and by the fastest steamers, $2,000 was added to the appropriation for 1896-97. Of this amount $1,283.53 was directly expended in the improvement of freight facilities and the balance for the necessary increase in the cost of packing boxes and postage.

CORRESPONDENTS.

The publication of a revised foreign exchange list authorized by the Secretary in March, 1895, has been accomplished, and the book is now being sent to societies and libraries in the United States, to which it will be of assistance in locating the names of all institutions outside of the United States which have received packages through the exchange system. This list, corrected to July 1, 1897, contains the names of 9,414 institutions, libraries, and societies, but owing to the frequent change in the addresses of individuals, the names of persons, with few exceptions, have not been included in this list, although the same minute ledger account of the interchange of publications is kept with individuals as with institutions.

There are now a total of 28,008 names on the records of the exchange bureau, an excess of 3,094 over the preceding year; 21,427 of this number being foreign and 6,581 domestic.

INTERNATIONAL EXCHANGE OF OFFICIAL DOCUMENTS.

The following table shows that during the past year 10,694 parcels have been received through the exchange service and delivered to the various Departments of the United States Government, while 30,008 packages have been received from Government Departments and bureaus and sent abroad. The new Library of Congress being now completed, the accumulations of valuable books that for years have not been made accessible for want of sufficient room, will soon be catalogued and placed upon the shelves.

The extent to which exchanges have been effected by the bureaus of the United States Government during the year is shown in the following table:

Statement of Government exchanges during the year 1896–97.

Name of bureau.	Packages.	
	Received for.	Sent by.
American Historical Association	4	117
Bureau of American Ethnology	227	90
Bureau of American Republics	2	5
Bureau of Education	91	....
Bureau of Equipment, Navy Department	1	....
Bureau of Medicine and Surgery	9	....
Bureau of the Mint	3	....
Bureau of Navigation	4	....
Bureau of Ordnance, Navy Department	1	....
Bureau of Statistics, Department of State	1	....
Bureau of Statistics, Treasury Department	23	....
Bureau of Steam Engineering, Navy Department	1	....
Census Office	8	2
Civil Service Commission	45	42
Coast and Geodetic Survey	83	519
Commissioner of Weights and Measures	1	....
Comptroller of the Currency	1	....
Department of Agriculture	213	4
Department of the Interior	17	1,229
Department of Labor	17	300
Department of State	16	2
Entomological Commission	8	....
Fish Commission	68	428
General Land Office	5	....
Geological Survey	473	1,926
Hydrographic Office	88	....
Interstate Commerce Commission	9	22
Library of Congress	6,260	10,800
Life-Saving Service	1	....
Light-House Board	2	1
Marine-Hospital Service	9	....
National Academy of Sciences	79	89
National Board of Health	1	....
National Museum	234	1,385
National Zoological Park	1	....
Nautical Almanac Office	16	133
Naval Observatory	138	1,550
Navy Department	4	....
Office of the Chief of Engineers, U. S. A.	39	100
Office of Indian Affairs	4	....
Ordnance Office, War Department	2	....
Patent Office	49	3,955
President of the United States	3	....
Signal Office	30	....
Smithsonian Institution	2,168	5,976
Superintendent of Documents	1	4
Surgeon-General's Office, U. S. Army	154	362
Treasury Department	6	....
War Department Library	11	....
War Records Office	....	107
Weather Bureau	63	860
Total	10,694	30,008

RELATIVE INTERCHANGE OF PUBLICATIONS BETWEEN THE UNITED STATES AND FOREIGN COUNTRIES.

Since July 1, 1896, a new feature has been adopted in the system of records of the Exchange Bureau, showing the number of packages exchanged between the United States and each of the other countries, thus supplying information that has not heretofore been obtainable without a tedious tabulation of each year's transactions.

Statement of packages transmitted through the Smithsonian Exchange Service during the fiscal year ending June 30, 1897.

Country.	Packages.		Country.	Packages.	
	For.	From.		For.	From.
Algeria	68	49	Java	113	113
Angola	1	....	Liberia	29	....
Argentina	1,214	378	Madagascar	5	....
Austria-Hungary	2,887	1,636	Madeira	1	....
Azores	15	....	Malta	40	....
Bahamas	14	....	Mauritius	43	....
Barbados	5	....	Mexico	1,108	389
Belgium	1,412	393	Netherlands	1,194	544
Bermudas	7	....	New Guinea	....	1
Bolivia	10	34	New South Wales	712	196
Borneo	3	....	New Zealand	433	4
Brazil	820	778	Nicaragua	19	....
British America	1,576	856	Norway	755	46
British Burmah	6	....	Paraguay	12	253
British colonies *a*	62	....	Persia	3	....
British Guiana	24	2	Peru	307	92
Cape Colony	172	4	Philippine Islands	49	....
Chile	598	17	Portugal	526	227
China	114	108	Queensland	466	....
Colombia	265	....	Roumania	48	63
Corea	2	....	Russia	2,246	1,862
Costa Rica	165	613	Santo Domingo	2	....
Cuba	128	....	San Salvador	43	....
Denmark	652	215	Servia	16	....
Dutch Guiana	8	....	Siam	13	....
Ecuador	30	....	South African Republic	26	1
Egypt	69	....	South Australia	358	3
France	5,877	1,915	Spain	646	....
Friendly Islands	3	....	St. Helena	7	....
Germany	10,506	4,988	Straits Settlements	24	....
Great Britain and Ireland	10,092	8,145	Syria	5	....
Greece	105	30	Sweden	1,161	612
Greenland	3	....	Switzerland	1,570	827
Guadeloupe	5	....	Tasmania	299	....
Guatemala	57	....	Trinidad	39	....
Guinea	1	....	Tunis	2	....
Haiti	232	....	Turkey	293	....
Hawaiian Islands	58	....	Turks Island	1	....
Honduras	8	....	United States	23,619	52,185
Iceland	48	....	Uruguay	317	184
India	817	182	Venezuela	272	....
Italy	2,763	1,393	Victoria	652	119
Jamaica	49	....	West Australia	238	....
Japan	805	12	Zanzibar	1	....

a Other than those specifically mentioned.

EFFICIENCY OF THE SERVICE.

During the past year the exchange of parliamentary documents with Mexico has been resumed, and twenty cases that had accumulated were forwarded on December 29, 1896. Hereafter each case as completed will be transmitted promptly.

The exchange of public documents with Japan has also been renewed during the past year, but, although several attempts have been made through the Japanese minister to induce his Government to assume the distribution of miscellaneous publications, the effort has not yet met with success.

The only countries with which exchange relations are at present entirely suspended are those disturbed by strife, viz, Turkey, Greece, and Cuba. The exchange of public documents with Turkey, however, has been reestablished, but, on account of the difficulties in delivering consignments at Constantinople, the steamship companies for the moment have declined to accept freight for that port.

The efficiency of the force of the Exchange Bureau, as well as its agents, Messrs. William Wesley & Son, London, and Dr. Felix Flügel, at Leipsic, is well established. On account, however, of the increasing duties of the Leipsic agency, covering as it does the territory of Germany, Austria-Hungary, the Balkan countries, and Switzerland, I would respectfully recommend that the territory so covered be subdivided and that other agents be appointed, in order to avoid too much pressure at Leipsic, which condition is liable to exist where so much is expected of a single agency.

The following list represents the names of companies and other mediums of transportation that continue to contribute free freight on exchanges or lend their aid in the transportation and delivery of such consignments.

LIST OF SHIPPING AGENTS AND CONSULS TO WHOM THE EXCHANGE SERVICE IS INDEBTED FOR SPECIAL COURTESIES.

American Board of Commissioners for Foreign Missions, Boston, Mass.
Atlantic Transport Line, Baltimore, Md.
Atlas Steamship Line (Pim, Forward & Co.), New York.
Board of Foreign Missions of the Presbyterian Church, New York.
Börs, C., consul-general for Sweden and Norway, New York.
Boulton, Bliss & Dallet, New York.
Calderon, Climaco, consul-general for Colombia, New York.
Cameron, R. W., & Co., New York.
Compagnie Générale Transatlantique, New York.
Cunard Royal Mail Steamship Company (Vernon H. Brown & Co., agents), New York.
Grace, W. R., & Co., New York.
Hamburg-American Line (R. J. Cortis, manager), New York.
Hensel, Bruckman & Lorbacher, New York.
Murguiondo, Prudencio de, consul-general for Uruguay, Baltimore, Md.
Navigazione Generale Italiana (Phelps Bros. & Co.), New York.
Netherlands American Line (W. H. Vanden Toorn, agent), New York.
North German Lloyd Line. (Agents: Oelrichs & Co., New York; A. Schumacher & Co., Baltimore.)
Obarrio, Melchor, consul-general for Bolivia, New York.
Panama Railroad Steamship Company, New York.
Perry, Edward & Co., New York.
Pomares, Mariano, consul-general for San Salvador, New York.
Red Star Line (International Navigation Company, agents), New York.
Röhl, C., consul-general for Argentina, New York.
Royal Danish consul, New York.
Royal Portuguese consul-general, New York.
Stewart, Alexander, consul-general for Paraguay, Washington, D. C.
Toriello, Enrique, consul-general for Guatemala, New York.
Turkish legation, Washington, D. C.
White Cross Line (Funch, Edye & Co.), New York.

The following is a list of the Smithsonian correspondents abroad acting as distributing agents, or receiving publications for transmission to the United States:

Algeria: Bureau Français des Échanges Internationaux, Paris, France.
Argentina: Museo Nacional, Buenos Ayres.

Austria-Hungary: Dr. Felix Flügel, No. 9 Schenkendorf Strasse, Leipzig, Germany.
Brazil: Bibliotheca Nacional, Rio de Janeiro.
Belgium: Commission des Échanges Internationaux, Rue du Musée, No. 5, Brussels.
Bolivia: University, Chuquisaca.
British America: Packages sent by mail.
British Colonies: Crown Agents for the Colonies, London, England.
British Guiana: (*See* British colonies.)
Cape Colony: Colonial Secretary, Cape Town.
Chile: Universidad de Chile, Santiago.
China: Zi-ka-wei Observatory, Shanghai.
Colombia (U. S. of): National Library, Bogotà.
Costa Rica: Oficina de Deposito, Reparto y Canje Internacional, San José.
Denmark: Kongelige Danske Videnskabernes Selskab, Copenhagen.
Dutch Guiana: Surinaamsche Koloniale Bibliotheek, Paramaribo.
East India: Director-General of Stores, India Office, London.
Ecuador: Observatorio del Colegio Nacional, Quito.
Egypt: Société Khédiviale de Géographie, Cairo.
France: Bureau Français des Échanges Internationaux, Paris.
Germany: Dr. Felix Flügel, No. 9 Schenkendorf Strasse, Leipzig.
Great Britain and Ireland: William Wesley & Son, 28 Essex street, Strand, London.
Guadeloupe. (*See* France.)
Guatemala: Instituto Nacional de Guatemala, Guatemala.
Haiti: Secrétaire d'État des Relations Extérieures, Port-au-Prince.
Honduras: Biblioteca Nacional, Tegucigalpa.
Iceland: (*See* Denmark.)
Italy: Biblioteca Nazionale Vittorio Emanuele, Rome.
Japan: Minister of Foreign Affairs, Tokyo.
Java. (*See* Netherlands.)
Liberia: Liberia College, Monrovia.
Madeira: (*See* Portugal.)
Malta. (*See* British colonies.)
Mauritius: (*See* British colonies.)
Mexico: Packages sent by mail.
Mozambique: (*See* Portugal.)
Natal: Agent-General for Natal, London.
Netherlands: Bureau Scientifique Central Néerlandais, Den Helder.
Newfoundland: Transmissions direct by mail.
New South Wales: Government Board for International Exchanges, Free Public Library, Sydney.
New Zealand: Colonial Museum, Wellington.
Norway: Kongelige Norske Frederiks Universitet, Christiania.
Paraguay: Government, Asunçion.
Peru: Biblioteca Nacional, Lima.
Philippine Islands: (*See* Spain.)
Polynesia: Department of Foreign Affairs, Honolulu.
Portugal: Bibliotheca Nacional, Lisbon.
Queensland: Registrar-General of Queensland, Brisbane.
Roumania. (*See* Germany.)
Russia: Commission Russe des Échanges Internationaux, Bibliothèque Impériale Publique, St. Petersburg.
St. Helena. (*See* British colonies.)
San Salvador: Museo Nacional, San Salvador.
Servia. (*See* Germany.)
South Australia: Astronomical Observatory, Adelaide.
Spain: R. Academia de Ciencias, Madrid.
Sweden: Kongliga Svenska Vetenskaps Akademien, Stockholm.

Switzerland: Central Library, Berne.
Tasmania: Royal Society of Tasmania, Hobarton.
Turkey: American Board of Commissioners for Foreign Missions, Boston, Mass.
Uruguay: Oficina de Depósito, Reparto y Canje Internacional, Montevideo.
Venezuela: Museo Nacional, Caracas.
Victoria: Public Library, Museum, and National Gallery, Melbourne.
Western Australia: Agent-General, London.

Transmission of exchanges to foreign countries.

Country.	Date of transmission.
Argentina	July 22, November 9, December 14, 1896; March 19, May 25, 1897.
Austria-Hungary	July 1, 7, 10, 16, August 5, October 10, 26, November 14, December 5, 1896; January 6, 15, 28, February 6, 18, March 8, 31, April 27, May 8, 18, June 7, 17, 26, 1897.
Belgium	July 18, October 19, November 23, December 22, 1896; January 19, February 6, March 12, April 24, June 2, 22, 1897.
Bolivia	March 19, 1897.
Brazil	July 22, November 9, December 14, 1896; March 19, May 25, 1897.
British colonies	August 3, November 21, 1896; March 1, 27, June 11, 1897.
Cape Colony	October 20, December 29, 1896; March 22, 1897.
China	September 2, December 3, 1896; April 2, June 28, 1897.
Chile	July 22, November 9, December 14, 1896: March 19, May 25, 1897.
Colombia	November 9, 1896; March 19, 1897.
Costa Rica	December 3, 1896; March 20, 1897.
Denmark	August 13, October 21, December 22, 1896; February 9, March 12, April 28, June 3, 1897.
Dutch Guiana	March 19, 1897.
East India	September 2, December 3, 1896; April 30, June 11, 1897.
Ecuador	(Shipments temporarily suspended.)
Egypt	October 20, 1896; March 22, 1897.
France and colonies	July 1, 7, 8, 10, 16, August 4, September 30, October 26, November 12, December 8, 1896; January 5, 21, February 3, 23, March 9, 17, April 13, May 7, 20, 29, June 15, 23, 30, 1897.
Germany	July 1, 7, 10, 16, August 10, September 11, 19, October 2, 26, November 14, December 5, 15, 1896; January 6, 15, 28, February 6, 18, March 8, 31, April 27, May 8, 18, 24, June 7, 17, 26, 1897.
Great Britain, etc	July 1, 8, 10, 16, 30, August 3, September 2, 11, 26, October 8, 27, November 11, December 11, 1896; January 9, 15, 25, February 1, 6, March 1, 27, April 23, 30, May 7, 15, 21, 28, June 8, 11, 19, 25, 1897.
Guatemala	December 3, 1896; March 20, 1897.
Haiti	March 20, 1897.
Honduras	December 3, 1896; March 20, 1897.
Italy	July 2, 9, 11, 16, September 5, October 3, 27, November 25, 1896; January 16, February 6, 24, March 11, April 15, May 10, June 1, 17, 26, 1897.
Japan	November 2, 1896; April 12, 1897.
Liberia	March 22, 1897.
Mexico	(By registered mail.)
Natal	March 27, 1897.
New South Wales	September 3, December 1, 1896; May 5, 1897.
Netherlands and colonies	July 17, October 13, November 28, December 14, 31, 1896; February 6, 24, March 15, April 28, June 3, 28, 1897.
New Zealand	September 2, December 1, 1896; May 5, 1897.
Nicaragua	September 10, December 3, 1896; March 20, 1897.
Norway	July 20, October 22, December 31, 1896; March 16, May 1, June 3, 1897.
Peru	July 22, November 9, 1896; March 19, 1897.
Polynesia	September 14, December 1, 1896; May 5, 1897.

Transmission of exchanges to foreign countries—Continued.

Country.	Date of transmission.
Portugal	July 20, October 23, December 30, 1896; February 10, March 17, May 1, June 24, 1897.
Queensland	September 2, 14, December 1, 25, 1896; March 1, 27, May 5, June 11, 1897.
Roumania	(Included in Germany.)
Russia	July 3, 9, 11, August 13, October 15, 27, November 24, December 17, 1896; January 20, February 6, 15, March 13, April 19, May 11, June 2, 22, 1897.
San Salvador	December 3, 1896; March 20, 1897.
Servia	(Included in Germany.)
South Australia	September 14, December 1, 1896; May 5, 1897.
Spain	September 3, October 22, December 21, 1896; February 8, March 16, May 1, June 14, 1897.
Sweden	July 3, 9, 11, September 9, October 15, 27, December 17, 1896; January 20, February 15, March 13, April 3, May 11, June 2, 22, 1897.
Switzerland	July 3, 9, 11, September 28, October 27, November 27, 1896; January 18, February 6, March 10, 13, April 24, June 4, 28, 1897.
Tasmania	September 14, December 1, 1896; May 5, 1897.
Turkey	December 12, 1896; June 14, 1897.
Uruguay	November 9, 1896; March 19, May 25, 1897.
Venezuela	July 22, November 9, 1896; March 19, 1897.
Victoria	September 14, December 1, 1896; May 5, 1897.
Western Australia	September 14, December 1, 1896; May 5, 1897.

The distribution of exchanges to foreign countries was made in 1,190 cases, representing 299 transmissions, as follows:

Argentina	22
Austria-Hungary	66
Belgium	38
Bolivia	1
Brazil	13
British colonies	8
Cape Colony	7
China	4
Chile	10
Colombia	2
Costa Rica	3
Denmark	16
Dutch Guiana	1
East India	14
Egypt	2
France and colonies	130
Germany	206
Great Britain	293
Guatemala	2
Haiti	1
Honduras	2
Italy	64
Japan	24
Liberia	1
Natal	1
New South Wales	10
Netherlands	34
New Zealand	7
Nicaragua	3
Norway	13
Peru	5
Polynesia	3
Portugal	12
Queensland	12
Roumania (included in Germany).	
Russia	50
San Salvador	2
Servia (included in Germany).	
South Australia	5
Spain	15
Sweden	30
Switzerland	34
Tasmania	3
Turkey	2
Uruguay	3
Venezuela	3
Victoria	11
Western Australia	2

Shipments of United States Congressional publications were made on November 7, 1896, and April 10, 1897, to the Governments of the following-named countries:

Argentina.
Austria.
Baden.
Bavaria.
Belgium.
Buenos Ayres, Province of.
Brazil.
Canada (Ottawa).
Canada (Toronto).
Chile.
Colombia.
Denmark.
France.
Germany.
England.
Haiti.
Hungary.
India.
Italy.
Japan.
Mexico.
Netherlands.
New South Wales.
New Zealand.
Norway.
Peru.
Portugal.
Prussia.
Queensland.
Russia.
Saxony.
South Australia.
Spain.
Sweden.
Switzerland.
Tasmania.
Uruguay.
Venezuela.
Victoria.
Western Australia.
Würtemberg.

Shipments to Greece and Turkey are temporarily suspended.

Recapitulation.

	Cases.
Total Government shipments	110
Total miscellaneous shipments	1,190
Total shipments	1,300
Total shipments last year	1,043
Increase over last year	257

Respectfully submitted.

RICHARD RATHBUN,
Assistant Secretary in Charge of Office and Exchanges.

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

Appendix IV.

REPORT OF THE SUPERINTENDENT OF THE NATIONAL ZOOLOGICAL PARK.

Sir: I have the honor to submit the following report of the operations of the National Zoological Park for the fiscal year ending June 30, 1897:

The close of the last year found the system of roadways in the park, authorized by acts of Congress, still in an unfinished condition. This state of affairs has been markedly improved by the completion of the Adams Mill road, by making the road from Woodley practicable for carriages, and by completing the macadamization of the main driveway through the park.

The Adams Mill road, laid out with a width of about 20 feet, has been fully macadamized, the base used being broken quartz rock and the surface layer pulverized blue limestone. It makes a very pleasant winding driveway, affording an excellent view of the valley of Rock Creek, and while the grades upon it are steeper than is desirable for easy driving, they are not more so than most of the roads that approach the valley in this region.

In order to connect the roadway from Woodley with the main system of the park, it was necessary to construct a bridge at a point considerably below the one at the Quarry road entrance. By boring it was found that solid rock could be reached at a short distance below the surface, and it was accordingly decided that it would not be necessary to build a masonry foundation. By excavating the sand and gravel sufficiently to denude this rock, and by blasting enough to obtain a good bearing surface, a suitable bed was formed upon which to lay concrete, forming a massive block of sufficient size to afford a solid support for the flat stones upon which could rest the beams of an arch forming a single span of 70 feet, made as low and flat as practicable.

The main driveway through the park, which was laid out so as to make the grades as light as possible, proved upon practical trial to be too tortuous for the safety of the numerous carriages that frequent it in ever increasing numbers. Some of the more abrupt curves were accordingly modified, and its macadamization, which was commenced last year, was then completed. Broken quartzite was used for the base of this road, the surface being of fine gravel mixed with a small quantity of clay. As far as can be judged by the experience of one winter, the result is satisfactory. It is hoped that the coming year may see the entire road system of the park in such condition that it will be possible to reach all the principal exhibits over good roads during any season or weather.

The walks of the park have also been improved to some extent during the year. About the principal animal house there has been constructed a granolithic footway of ample width leading along the outer or summer cages. This walk will be properly shaded, and is a much needed improvement. From the animal house to the main driveway there has been laid a pathway of pulverized bluestone, which is found to be much better than the board walk formerly in use. As fast as the available funds permit, foot walks of this kind should be constructed in all parts of the park. Although the first cost is somewhat more, their much greater durability makes them really more economical than the unsightly board walks that now disfigure the roadsides.

A well graded and macadamized carriage road from the street-car line of the Capital Traction Company, upon Cincinnati street, to the entrance of the park near the

Bridge in National Zoological Park.

Holt house, communicating there with the Adams Mill road, would be a very desirable improvement, as it would allow a much better approach than is now obtained by the steep grade upon the Quarry road. Another carriage road should be constructed from Connecticut avenue extended to the park, a distance of only about 180 feet, but almost impassable during wet winter weather because of deep mud. The attention of the Commissioners of the District has been called to this matter, and I am informed that Congress has already appropriated funds that can next year be applied to remedying these defects.

The only permanent house that it has been possible to build during the year is one for the zebus, or East Indian cattle. This has been placed on a hillside opposite the buffalo house, a location not wholly desirable, yet one that brings the animals fully to the notice of the public. It is, properly speaking, a shelter barn, made of concrete mixed with large pebbles, the roof of tile supported upon wooden beams. An illustration showing this house is appended hereto. About it there have been inclosed paddocks of sufficient size for the accommodation of the animals.

Some effort has been made during the year to procure a satisfactory exhibit of the varieties of the domesticated dog, both for the purpose of showing the amount of variation that occurs in a single species, as well as to display typical specimens of each breed for the information of those unacquainted with their characteristic appearances. A similar exhibit is made at the Jardin d'Acclimatation, near Paris, forming one of the principal features of the collection there. By corresponding with breeders and dog fanciers a number of typical specimens were secured, and a temporary wooden structure to be used as a kennel was erected not far from the principal animal house. The exhibit has excited considerable public interest, but the dogs being very noisy, so much so as to greatly disturb the residents on the eastern side of the park, the principal kennel was finally removed to a more distant and secluded situation on the western side.

An unfortunate accident has greatly retarded the growth of this collection. Lieutenant Peary, U. S. N., temporarily withdrew from the park, for purposes of exhibition, two of the Eskimo dogs deposited by him. After these were returned, one of them almost immediately developed a case of distemper which, in spite of all that could be done in the way of isolation and disinfection, ran entirely through the kennels, affecting not only the dogs but also the wolves and foxes. Many animals, particularly the younger ones, died from it. The Eskimo dogs suffered with especial severity, the fine group possessed by the park last year being now reduced to a single specimen.

An extension of the paddocks for the wapiti or American elk was made imperatively necessary by the increase in growth of the young males received from the Yellowstone National Park. Accordingly, there was selected for this purpose a large tract, comprising some 9 acres, situated between Rock Creek and the fence on the eastern side of the park, and still covered with primitive forest. This was inclosed by means of the Page wire fence, and within it, at a short distance from the fence, was built a small shelter for holding feed. Access to this is by an elevated walk, a precaution rendered necessary by the ferocity of the males during the fall and winter season, which makes it dangerous to enter the inclosure at that time. The animals have made themselves entirely at home within this precinct, and appear very much as they do in their native haunts.

It has been necessary to replace the fence for the inclosure situated on one of the little tributaries of Rock Creek within the park where the beavers have built quite extensively and also brought forth young. The fence, already used for some years to confine these animals, was of strong wire mesh, but proved insufficient to withstand their powerful teeth. Severing it in several places, they went outside their inclosure and felled small trees, which they endeavored to drag into their dams. On several occasions, notwithstanding the repeated repair of the fence, they escaped at night, returning to the inclosure during the day, and finally one of them established himself permanently outside and has not as yet been recovered. It being necessary to construct a new fence of sufficient strength to withstand the attacks of their teeth, one was built of vertical steel rods, $\frac{5}{16}$ inch in diameter, curved inward at the top, so as to prevent any possibility of climbing over. This is believed to be perfectly effective in preventing their escape.

During the year the scope of the operations of these animals has been greatly increased, and they have now constructed three large dams, one of which is at least 4 feet high. Each of these has been built wholly by the beavers themselves, either from trees felled by them within the inclosure or from branches furnished them for food. They cut this material into suitable lengths, which they drag to the water, float to the dam, and there combine with mud and twigs to form a compact structure. In connection with each dam they have built houses, together with several smaller burrows in the bank. The entrance to the houses is always under water and can only be reached by diving.

The animals have become quite accustomed to the presence of man, and it is believed that under proper restrictions the public may be allowed to see them at work. The paddock in which they are inclosed should be somewhat enlarged, as it is now so small that the different families interfere with each other, and the weaker ones can not escape from their pursuers.

On September 29, 1896, the city of Washington was visited by one of the severest storms that ever occurred in this region. The velocity of the wind reached for a short time 80 miles per hour, and its force was terrific, unroofing and blowing down houses and uprooting trees, so that the whole vicinity was a scene of wreck and devastation. Within the park hundreds of trees were laid prostrate, and a portion of the roof of the Holt house, where the office is situated, was blown off. The roads were so blocked with fallen trees as to be impassable, and the fences of the elk and buffalo inclosures were, by the same cause, crushed in, so that for a short time there was considerable danger that the animals might escape. Fortunately, this did not occur; none of the animals were seriously injured, and no very serious damage was done to any of the buildings. The cost of clearing away the wreckage from this storm was over $500, and the repairing of the roof of the Holt house cost $100, sums that could ill be spared from the limited appropriation at the disposal of the park.

The accessions to the collection during the year have not been very numerous. As no purchase of animals was allowed, all additions must be by gift, by births, by collections from the Yellowstone National Park, or by exchange.

A list of all the animals donated is appended hereto. Especially valuable and welcome additions from this source were four harbor seals (*Phoca vitulina*), presented by Mr. J. H. Starin, already well-known for his generosity; a mule deer (*Cariacus macrotis*), presented by Mr. Frank Maumey, of Telluride, Colorado, and a brush-tailed rock-kangaroo (*Petrogale penicillata*), presented by Mr. M. J. Flood, of Sydney, Australia. Other valuable animals presented by Mr. Flood unfortunately died en route.

The most important births have been those of two sea lions (*Zalophus californianus*), a buffalo (*Bison americanus*), a zebu (*Bos indicus*), and two beaver (*Castor fiber*).

From the Yellowstone Park there has been received during the year one consignment, comprising, among other animals, six young prong-horn antelopes (*Antilocapra americana*). These beautiful creatures were captured when very young, and fed by hand until sufficiently well-grown to endure transportation. They are quite accustomed to the sight of men, horses, and even dogs. They have been placed in a small paddock at the western side of the park, and have thriven well since their arrival, a fact of considerable interest when it is remembered that much difficulty has been experienced by others in rearing them in captivity.

As surplus stock of any particular species accumulates in the park, effort is made to exchange it for other animals, and thus produce a greater variety of exhibits. By this means several most important accessions have been obtained. Among these were an African ostrich (*Struthio camelus*), an Indian antelope (*Antilope cervicapra*), and two of the exceedingly rare West Indian seals (*Monachus tropicalis*). Unfortu-

Zebu House, National Zoological Park.

nately there has been great difficulty in inducing these latter animals to eat, for they do not seem to relish any of the fish or other sea products procurable in the markets of Washington. These seals are among the most important acquisitions ever made by the National Zoological Park. The species has been known for more than two centuries past to exist in the Caribbean Sea, but it has been exceedingly difficult to get specimens, even the extensive zoological garden at London never having possessed one. Those in the possession of the park were obtained on a small island in the Gulf of Campeche.

I am informed by Dr. J. Eugene Jarnigan, United States consul at Utilla, Honduras, that the species is occasionally seen among the reefs about 70 miles north of Cape Gracios-á-Dios.

In concluding my report I wish to call attention to the fact that since the establishment of the National Zoological Park there have been many other enterprises of a similar character projected and established in various parts of the country, and that these are, as a rule, supported by far greater resources than are allowed for the national collection. Among these I will mention the following:

The Blue Mountain Forest Park, established by the late Mr. Austin Corbin, is a large tract of forest and abandoned farm land, situated in the western part of New Hampshire, comprising an inclosed area of 26,000 acres. Within this inclosure are kept about 4,000 wild animals, including 74 bison, 200 moose, 1,500 elk, 1,700 deer of different species, and 150 wild boars. These animals are rapidly multiplying, and, with the exception of the bison, which are sheltered and fed during the winter, live in perfect freedom.

In the Adirondack region of New York a game preserve of 9,000 acres has been stocked with elk, Virginia deer, mule deer, rabbits, pheasants, etc., and Mr. W. C. Whitney has established a preserve of 1,000 acres in the Berkshire hills, near Lenox, Mass., where he maintains not only the species of animals above mentioned, but also bison and antelope. Other preserves are Ne-ha-sa-ne Park, in the Adirondacks, 8,000 acres; Tranquillity Park, near Allamuchy, N. J., 4,000 acres; the Alling preserve, near Tacoma, Wash., 5,000 acres; North Lodge, near St. Paul, Minn., 400 acres, and Furlough Lodge, in the Catskills, New York, 600 acres. These are all fenced inclosures, well stocked with animals.

At Pittsburg, Pa., certain public-spirited citizens have undertaken, in one of the public parks, the construction of a number of buildings intended for the exhibition of animals. These are already nearing completion and will cost more than $200,000, exclusive of the animals they are to contain. A further collection of buildings and inclosures intended for American animals only is also projected for that city.

In 1895 certain gentlemen interested in natural history organized the New York Zoological Society, whose objects are stated to be as follows:

> "*First.*—The establishment of a free zoological park containing collections of North American and exotic animals, for the benefit and enjoyment of the general public, the zoologist, the sportsman, and every lover of nature.
>
> "*Second.*—The systematic encouragement of interest in animal life or zoology among all classes of the people and the promotion of zoological science in general.
>
> "*Third.*—Cooperation with other organizations in the preservation of the native animals of North America and encouragement of the growing sentiment against their wanton destruction."

To carry out this plan these gentlemen obtained from the city of New York a grant of some 261 acres in the southern portion of Bronx Park, near that city, for the purpose of establishing there a zoological garden, which is to be free to the public for at least five days in each week. Plans are now being prepared for the development of a collection on the most generous and attractive scale. The bulletin issued by the society states that it is expected that there will be at once spent $125,000 for preparing the ground and $250,000 for buildings and inclosures.

Animals in the National Zoological Park June 30, 1897.

Name.	Number.
MAMMALS.	
American bison (*Bison americanus*)	7
Zebu (*Bos indicus*)	4
Common goat (*Capra hircus*)	9
Cashmere goat (*Capra hircus*)	4
Indian antelope (*Antilope cervicapra*)	1
Prong-horn antelope (*Antilocapra americana*)	6
American elk (*Cervus canadensis*)	13
Virginia deer (*Cariacus virginianus*)	17
Mule deer (*Cariacus macrotis*)	2
Solid-hoofed hog (*Sus scrofa*, var. *solidungulata*)	1
Peccary (*Dicotyles tajaçu*)	5
Llama (*Auchenia glama*)	8
Guanaco (*Auchenia huanacos*)	1
Indian elephant (*Elephas indicus*)	2
Lion (*Felis leo*)	4
Tiger (*Felis tigris*)	1
Leopard (*Felis pardus*)	2
Puma (*Felis concolor*)	5
Spotted lynx (*Lynx rufus maculatus*)	5
Spotted hyena (*Hyæna crocuta*)	3
Beagle hound	1
Russian wolf hound	2
Stag hound	1
Mastiff	1
St. Bernard dog	2
Pointer	2
Chesapeake Bay dog	2
Bedlington terrier	1
Smooth-coated fox terrier	3
Wire-haired fox terrier	1
Brown French poodle	1
Eskimo dog	1
Gray wolf (*Canis lupus griseo-albus*)	6
Black wolf (*Canis lupus griseo-albus*)	3
Coyote (*Canis latrans*)	7
Red fox (*Vulpes fulvus*)	3
Swift fox (*Vulpes velox*)	3
Tayra (*Galictis barbara*)	1
Mink (*Putorius vison*)	5
American badger (*Taxidea americana*)	4
Kinkajou (*Cercoleptes caudivolvulus*)	1
Gray coati-mundi (*Nasua narica*)	2
Cacomistle (*Bassaris astuta*)	1
Raccoon (*Procyon lotor*)	27
Black bear (*Ursus americanus*)	6
Cinnamon bear (*Ursus americanus*)	3
Grizzly bear (*Ursus horribilis*)	2
California sea lion (*Zalophus californianus*)	5
West Indian seal (*Monachus tropicalis*)	2
Harbor seal (*Phoca vitulina*)	1
Macaque monkey (*Macacus cynomolgus*)	3
MAMMALS—continued.	
Bonnet monkey (*Macacus sinicus*)	1
Rhesus monkey (*Macacus rhesus*)	3
Arabian baboon (*Cynocephalus hamadryas*)	2
Olive baboon (*Cynocephalus anubis*)	2
Owl monkey (*Nyctipithecus trivirgatus*)	2
European hedgehog (*Erinaceus europæus*)	1
Albino rat (*Mus rattus*)	10
American beaver (*Castor fiber*)	6
Woodchuck (*Arctomys monax*)	5
Prairie dog (*Cynomys ludovicianus*)	10
Red-bellied squirrel (*Sciurus aureogaster*)	1
Fox squirrel (*Sciurus niger*)	1
Gray squirrel (*Sciurus carolinensis*)	20
Crested porcupine (*Hystrix cristata*)	3
Canada porcupine (*Erethizon dorsatus*)	2
Crested agouti (*Dasyprocta cristata*)	3
Hairy-rumped agouti (*Dasyprocta prymnolopha*)	2
Mexican agouti (*Dasyprocta mexicana*)	2
Azara's agouti (*Dasyprocta azaræ*)	2
Guinea pig (*Cavia porcellus*)	16
Rocky Mountain varying hare (*Lepus americanus bairdii*)	1
English rabbit (*Lepus cuniculus*)	16
Angora rabbit (*Lepus cuniculus*)	1
Six-banded armadillo (*Dasypus sexcinctus*)	1
Peba armadillo (*Tatusia novemcincta*)	6
Gray kangaroo (*Macropus* sp.)	3
Brush-tailed rock kangaroo (*Petrogale penicillata*)	1
Common opossum (*Didelphys virginiana*)	1
BIRDS.	
Clarke's nutcracker (*Nucifraga columbiana*)	7
Raven (*Corvus corax*)	1
Black-headed jay (*Cyanocitta stelleri annectens*)	1
European magpie (*Pica pica*)	1
Sulphur-crested cockatoo (*Cacatua galerita*)	2
Leadbeater's cockatoo (*Cacatua leadbeateri*)	1
Bare-eyed cockatoo (*Cacatua gymnopis*)	1
Yellow and blue macaw (*Ara ararauna*)	1
Red and yellow and blue macaw (*Ara macao*)	3
Green parrakeet (*Conurus* sp.)	1
Orange-winged amazon (*Amazona amazonica*)	1
Yellow-naped amazon (*Amazona auropalliata*)	1
Gray parrot (*Psittacus erithacus*)	4
Great horned owl (*Bubo virginianus*)	6

Prong-horn Antelope, National Zoological Park.

Animals in the National Zoological Park June 30, 1897—Continued.

Name.	Number.
BIRDS.	
Barred owl (*Syrnium nebulosum*)	1
Barn owl (*Strix pratineola*)	1
Bald eagle (*Haliæetus leucocephalus*)	12
Broad-winged hawk (*Buteo latissimus*)	1
Red-shouldered hawk (*Buteo lineatus*)	1
Red-tailed hawk (*Buteo borealis*)	4
Turkey vulture (*Cathartes aura*)	1
Ring dove (*Columba palumbus*)	6
Chachalaca (*Ortalis vetula maccallii*)	6
Razor-billed curassow (*Mitua tuberosa*)	1
Lesser razor-billed curassow (*Mitua tomentosa*)	1
Pea fowl (*Pavo cristatus*)	22
White turkey (*Meleagris gallopavo*)	2
Guinea fowl (*Numida meleagris*)	1
King rail (*Rallus elegans*)	1
Sandhill crane (*Grus mexicana*)	1
Whooping crane (*Grus americana*)	1
Great blue heron (*Ardea herodias*)	2
Cariama (*Cariama cristata*)	1
Whistling swan (*Olor columbianus*)	1
Mute swan (*Cygnus gibbus*)	5
Black swan (*Chenopis atrata*)	1
Brant (*Branta bernicla*)	6
Canada goose (*Branta canadensis*)	6
Chinese goose (*Anser cygnoides*)	7
Toulouse goose (*Anser* sp.)	2
Mandarin duck (*Dendronessa galericulata*)	2
Black duck (*Anas obscura*)	2
Mallard duck (*Anas boschas*)	7
Pekin duck (*Anas* sp.)	13

Name.	Number.
BIRDS—continued.	
American herring gull (*Larus argentatus smithsonianus*)	1
African ostrich (*Struthio camelus*)	1
REPTILES.	
Alligator (*Alligator mississippiensis*)	15
Snapping turtle (*Chelydra serpentina*)	2
Painted turtle (*Chrysemys picta*)	6
Musk turtle (*Aromochelys odorata*)	2
Mud turtle (*Cinosternum pennsylvanicum*)	5
Terrapin (*Pseudemys* sp.)	1
Gopher turtle (*Xerobates polyphemus*)	1
Gila monster (*Heloderma suspectum*)	4
Glass snake (*Ophiosaurus ventralis*)	1
Diamond rattlesnake (*Crotalus adamanteus*)	4
Copperhead (*Ancistrodon contortrix*)	1
Water moccasin (*Ancistrodon piscivorus*)	5
Python (*Python* sp.)	2
Boa (*Boa constrictor*)	3
Anaconda (*Eunectes murinus*)	1
Scarlet snake (*Cemophora coccinea*)	1
Red-bellied snake (*Farancia abacura*)	1
Bull snake (*Pituophis sayi*)	4
Pine snake (*Pituophis melanoleucus*)	1
Milk snake (*Ophibolus doliatus*)	1
King snake (*Ophibolus getulus*)	3
Black snake (*Bascanium constrictor*)	3
Mountain black snake (*Coluber obsoletus*)	8
Garter snake (*Eutænia sirtalis*)	1
Water snake (*Natrix sipedon*)	7
Hog-nosed snake (*Heterodon platyrhinus*)	2
Gopher snake (*Spilotes corais couperi*)	4

	Indigenous.	Foreign.	Domesticated.	Total.
Mammals	193	47	87	327
Birds	64	26	61	151
Reptiles	83	6	....	89
Total	340	79	148	567

List of accessions.

ANIMALS PRESENTED.

Name.	Donor.	Number of specimens.
Marmoset	Mrs. K. D. Brown, Washington, D. C	1
Beagle hound	H. L. Kreuder, Nanuet, N. Y	1
Collie	B. B. Smith, Washington, D. C	1
Do	B. Alton Smith, North Attleboro, Mass	1
Bedlington terrier	John Hopkinson, Newark, N. J	1
Fox terrier	John E. Thayer, Lancaster, Mass	1
French poodle	H. H. Hunnewell, Wellesley, Mass	1
Mastiff	J. Russell, Bedford City, Va	1
Russian wolf hound	Col. B. G. Daniels, Washington, D. C	1
Gray wolf	W. M. Dunaway, Hugo, Colo	1
Black wolf	F. W. Okie, Casper, Wyo	1
Red fox	Dr. C. E. Robinson, Washington, D. C	1
Gray fox	J. I. Whiting, Washington, D. C	1
Do	A. M. Nicholson, Orlando, Fla	1
Do	Thos. Walsh, Washington, D. C	1
Mink	Edward Topscott, Palace Valley, W. Va	3
Do	E. C. Yount, Herndon, Va	4
Raccoon	Thos. Walsh, Washington, D. C	2
Black bear	.....do	1
Cinnamon bear	Geo. H. Tice, Monero, N. Mex	1
Harbor seal	Hon. John H. Starin, New York	4
Common goat	E. S. Schmid, Washington, D. C	3
Do	Mrs. Sparo, Washington, D. C	1
Do	Thos. Walsh, Washington, D. C	1
Virginia deer	Arthur S. Nester, Munising, Mich	1
Do	Thos. Walsh, Washington, D. C	1
Mule deer	Frank Maumey, Telluride, Colo	1
Albino rat	C. D. Walcott, Washington, D. C	4
Do	R. B. Leathers, Washington, D. C	25
Woodchuck	E. S. Schmid, Washington, D. C	1
Albino woodchuck	E. G. Pendleton, Augusta Springs, Va	1
Gray squirrel	R. E. Bruff	1
Do	Robt. Imbrie, Washington, D. C	1
English rabbit	Dr. Hale, Washington, D. C	1
Do	L. L. Kahlet, Washington, D. C	5
Do	G. W. Henderson, Washington, D. C	2
Angora rabbit	Dr. Hale, Washington, D. C	1
Rocky Mountain varying hare.	H. Z. Fish, Bellows Falls, Vt	1
Six-banded armadillo	Miss M. Townsend, Washington, D. C	1
Brush-tailed rock kangaroo	M. J. Flood, Sydney, Australia	1
Opossum	Miss L. J. White, Washington, D. C	1
Do	Master Walters, Washington, D. C	2
Do	W. S. Anderson & Co., Washington, D. C	1
Golden eagle	A. M. Brooking, Funk, Nebr	2
Bald eagle	J. W. Gladstone, Cape Charles, Va	1
Do	Thos. Walsh, Washington, D. C	1
Broad-winged hawk	Dr. W. F. Hutchinson, Winchester, Va	1
Red-tailed hawk	.....do	2
Do	No data	1
Do	F. Kraus, Washington, D. C	1
Do	Albert Wise, Washington, D. C	1
Do	Thos. Walsh, Washington, D. C	1

List of accessions—Continued.

ANIMALS PRESENTED—Continued.

Name.	Donor.	Number of specimens.
Great horned owl	B. G. True, Clinton, Me	1
Do	R. F. Foster, Washington, D. C	1
Do	Dr. W. F. Hutchinson, Winchester, Va	1
Do	O. W. White, Washington, D. C	1
Do	No data	1
Barn owl	F. Hardman, San Antonio, Tex	2
European magpie	Miss Dillie Love, Washington, D. C	1
Ring dove	E. A. Estes, Washington, D. C	2
Do	Harold Pole, Washington, D. C	4
Peafowl	Miss Rachel Weems, Upper Falls, Md	2
Whooping crane	Hugh Elliott, Collyer, Kans	1
King rail	Dr. S. S. Stearns, Washington, D. C	1
Brant	J. D. Cordon, Washington, N. C	6
Common loon	P. J. Duffy, Washington, D. C	1
Alligator	Capt. E. A. Roderick, Washington, D. C	2
Do	Ralph Berecher, Washington, D. C	1
Do	D. J. Howell, Alexandria, Va	1
Do	M. A. Tappan, Washington, D. C	1
Do	Dr. Henry Stewart, U. S. A., Canandaigua, N. Y	1
Do	Claude Edwards, Washington, D. C	1
Do	John Wilkinson, Washington, D. C	1
Glass snake	Dr. J. J. Kinyoun, U. S. Marine Hospital, Washington, D. C	1
Terrapin	Gilbert Thompson, U. S. Geological Survey	1
Diamond rattlesnake	John Y. Detwiler, New Smyrna, Fla	1
Do	A. M. Nicholson, Orlando, Fla	5
Banded rattlesnake	P. T. Horning, Washington, D. C	1
Prairie rattlesnake	L. W. Purinton, Banner, Kans	2
Water moccasin	A. M. Nicholson, Orlando, Fla	5
Scarlet snake	John Y. Detwiler, New Smyrna, Fla	1
Red-bellied snake	D. W. Prentiss, jr., Washington, D. C	1
King snake	Wm. Ogle, Washington, D. C	1
Bull snake	L. W. Purinton, Banner, Kans	3
Pine snake	D. A. Christie, Washington, D. C	1
Gopher snake	A. M. Nicholson, Orlando, Fla	4
Mountain black snake	F. G. Shaw, Silver Springs, Md	2
Do	Dr. E. A. Mearns, U. S. A., Fort Meyer, Va	1
Hog-nosed snake	Chas. H. Miller, Washington, D. C	1
Do	R. G. Paine, Washington, D. C	1
Do	E. J. Brown, Washington, D. C	1
Do	A. M. Nicholson, Orlando, Fla	1
Garter snake	C. G. Rorebeck, Falls Church, Va	1
Water snake	Louis Chisley, Washington, D. C	2
Do	Thos. Hamlet, Washington, D. C	1

List of accessions—Continued.

ANIMALS LENT.

Name.	Lender.	Number of specimens.
Bonnet monkey	Geo. Rapps, Washington, D. C	1
Collie	M. E. Phelps, Owego, N. Y	1
Russian wolf hound	C. E. Clifton, Washington, D. C	1
Polar bear	Lieut. R. E. Peary, U. S. N	2
California sea lion	Hon. Jas. A. Bradley, Asbury Park, N. J	1
European hedgehog	E. S. Schmid, Washington, D. C	1
Cashmere goat	J. A. Bailey, New York	2
Red and yellow and blue macaw.	Geo. Rapps, Washington, D. C	1
Orange-winged amazon	Miss E. T. Reiss, Washington, D. C	1
Sulphur-crested cockatoo	Geo. Rapps, Washington, D. C	1
Peafowl	E. S. Schmid, Washington, D. C	3
White turkey	do	2
Mandarin duck	do	2
Mallard duck	do	8
Canada goose	do	2
European swan	do	3
Gila monster	A. O. Beaumel, Washington, D. C	1
Python	E. S. Schmid, Washington, D. C	1
Do	R. G. Payne, Washington, D. C	1
King snake	do	2

ANIMALS RECEIVED IN EXCHANGE.

Name.	From—	Number of specimens.
Olive baboon	W. A. Brady, New York	2
Arabian baboon	do	2
Leopard	J. A. Bailey, New York	1
California sea lion	Wm. Bartels, New York	4
West Indian seal	W. A. Brady, New York	2
Black buck	Zoological Garden, Philadelphia, Pa	1
American elk	do	1
Azara's agouti	E. S. Schmid, Washington, D. C	2
African ostrich	do	1

Animals born in the National Zoological Park.

Spotted lynx (*Lynx rufus maculatus*)	2
Gray wolf (*Canis lupus griseo-albus*)	2
Coyote (*Canis latrans*)	7
California sea lion (*Zalophus californianus*)	2
Buffalo (*Bison americanus*)	1
Zebu (*Bos indicus*)	1
Common goat (*Capra hircus*)	1
American elk (*Cervus canadensis*)	1
Virginia deer (*Cariacus virginianus*)	6
Peccary (*Dicotyles tajaçu*)	2
Llama (*Auchenia glama*)	3
American beaver (*Castor fiber*)	2

Peafowl (*Pavo cristatus*)	8
Pekin duck (*Anas* sp.)	6
European swan (*Cygnus gibbus*)	1

Animals collected in the Yellowstone National Park.

Black bear (*Ursus americanus*)	2
Prong-horn antelope (*Antilocapra americana*)	6
American elk (*Cervus canadensis*)	1
Mountain wood rat (*Neotoma cinerea*)	1
American beaver (*Castor fiber*)	4

SUMMARY OF ACCESSIONS.

Animals presented		169
Animals lent		37
Animals received in exchange		16
Animals born in the Zoological Park		45
Animals received from the Yellowstone National Park		14
Total		281
Number of specimens on hand June 30, 1896		553
Accessions during the year ending June 30, 1897		281
Total		834
Deduct—		
Deaths	177	
Used for food for animals	37	
Animals escaped or liberated	7	
Animals exchanged	21	
Animals returned to owners	25	
		267
Animals on hand June 30, 1897		567

Respectfully submitted.

FRANK BAKER,
Superintendent.

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

APPENDIX V.

REPORT ON THE WORK OF THE ASTROPHYSICAL OBSERVATORY FOR THE YEAR ENDING JUNE 30, 1897.

SIR: The work of the observatory during the past year has consisted largely in preparing for publication an account of the research on the positions of absorption bands in the infra-red solar spectrum.

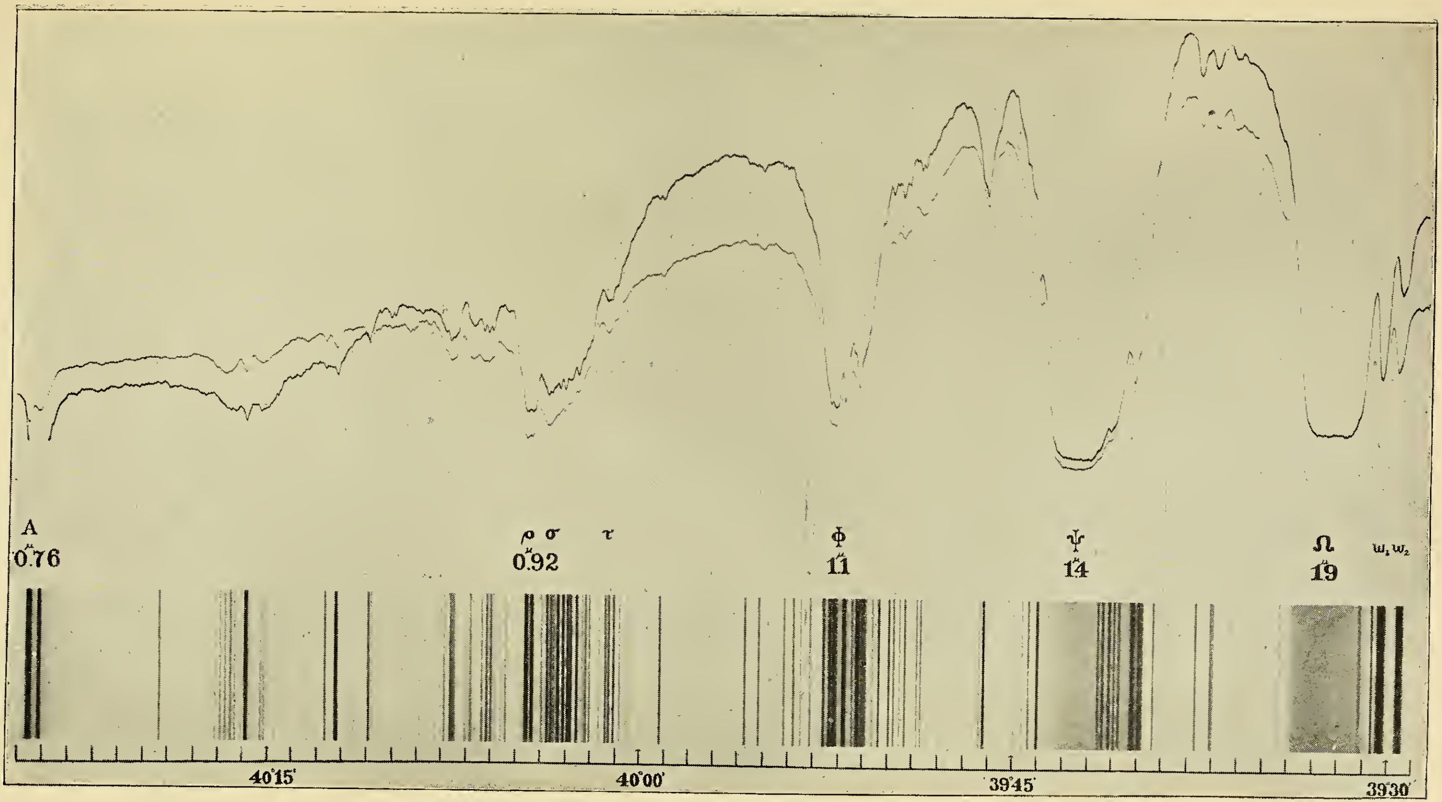
In this report, which was completed in May, 1897, but which, owing to unavoidable delay, has not yet appeared in type, the positions of about 225 absorption lines and bands are determined in deviation and refractive index for a 60° rock-salt prism at the temperature of 20° centigrade. These lines are distributed in the salt spectrum between deviations of 40° 25′ and 38° 45′, corresponding to wave lengths 0.76 μ and 5.20 μ, respectively. The average error probable in the absolute angular deviations of these lines is about 4 seconds of arc, but in their relative deviations measured from the A line in the visible spectrum the probable error averages only about 0.5 seconds of arc, a degree of accuracy even exceeding the anticipations held forth in last year's report. The bolographs from which these results were obtained were taken on exceptionally favorable occasions between October 26, 1896, and January 9, 1897. These bolographs, 13 in number, were selected from among many more, of all degrees of excellence, taken during the same interval.

The forthcoming report contains, in addition to the results above mentioned, an account of the investigations leading up to this research on the infra-red absorption lines; a description of the successive improvements in the instrumental conditions which have rendered possible the present results; a description of the present apparatus and its adjustment; and a discussion of present sources of error and the possibility of future progress. The results of several subsidiary investigations are given, among others being the change of deviation of rock salt with change of temperature, the heat conductivity of rock salt, the effect of diffraction in decreasing the intensity of energy in the spectrum with narrowing slit widths. The report is illustrated with reproductions of photographs and drawings of apparatus, platted curves explanatory of various matters, bolographic curves, and line spectra similar to that here given. With the illustrations, the report is expected to cover about 175 quarto pages.

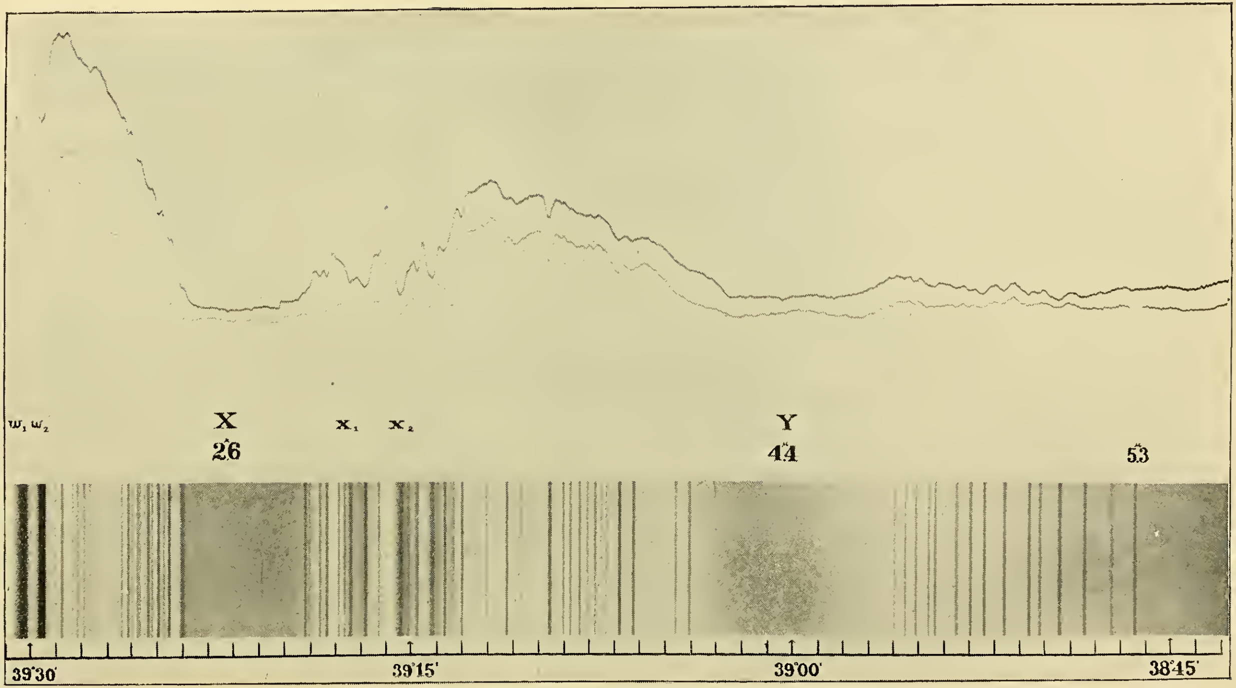
DETAILS OF THE WORK OF THE YEAR.

There have been taken during the year 83 bolographs (none were taken after January 20, 1897), of which about one-third are notably superior to any of previous years in freedom from "drift" and accidental inflections, in genuine detail, and in accuracy of position. On 6 bolographs covering the region 0.76 μ to 2 μ, and 7 continuing from 1.8 μ to 5.5 μ, all the lines determined to be "real," after the manner described in the report of last year, have been measured in position upon a comparator, both to determine the deviation in the spectrum and the relative intensity of energy at the various points. Each curve was twice independently measured.

These measurements were reduced and tables prepared of deviations, indices of refraction, approximate wave lengths, and giving the relative importance of the lines discovered (about 225 in number), and the relative intensities of energy at the corresponding points in the solar spectrum. A composite linear photograph has been prepared from three curves, for purposes of illustration after the manner heretofore described.



Infra-red Solar Spectrum of a 60° Rock-salt Prism, *a*.



Infra-red Solar Spectrum of a 60° Rock-salt Prism, *b*.

A determination has been made of the change in deviation of the A line with change of temperature of the salt prism. The value obtained was —12.0″ ± 0.4″ for the change in deviation, corresponding to 1° rise in the temperature of the prism.

Interesting results were obtained, experimentally, showing that for slit widths less than 0.15 millimeter the increase in the loss of energy by diffraction is very rapid. An investigation was begun, and bids fair to end successfully, with the design of constructing a galvanometer far more sensitive than the one in present use, which may enable a considerable gain to be made in the number of lines to be discovered by the bolographic method of spectrum analysis.

ACQUISITIONS AND ALTERATIONS OF APPARATUS.

1. A concave mirror 26 centimeters in diameter and of 230 centimeters principal focal length was obtained for use in the system of collimation by two mirrors, as proposed in last year's report, and actually installed with advantage in August, 1896. The metal work required in the mirror mountings was done at the Observatory shop.

2. Three very accurate optical flats, 12 centimeters in diameter, were procured for testing the faces of the salt prism during its occasional polishings, and for use as plane mirrors when required.

3. A train of totally reflecting prisms, so adjusted as to invert an image, was obtained for use with the 6-inch telescope as an erecting eyepiece.

4. A number of large blocks of salt were received through the courtesy of the Russian Government, and from one of the blocks, which promised good optical quality, there was cut a large prism. On polishing, however, the quality of the interior was found to be so far below the exterior appearance that the prism has not been made use of. The optical work on the pieces of apparatus above mentioned was done by Mr. J. A. Brashear.

5. A Zeiss anastigmatic wide-angle photographic lens was procured for use in photographing interior rooms.

6. A Nachet compound microscope with mechanical stage and various accessories was obtained and is frequently used for measurements of bolometer threads and other small objects.

7. There have been made at the Observatory shop during the year, in addition to such repairs as were required, the following pieces of apparatus:

a. Bearing for circle shaft to insure greater accuracy of motion.

b. A mounting for oblong plane mirror and prism to form an auxiliary fixed-arm spectroscope. This piece of apparatus was used in the determination of the change of deviation of rock salt with change of temperature.

c. A special rheostat for balancing the bolometer, provided with a slide wire operated mechanically from without, and all inclosed by a water jacket.

8. It was shown that the circle of the spectrometer moved less accurately than was desired, and that the error arose in the action of the worm-and-wheel mechanism, which connects it with the clock. Messrs. Warner and Swasey undertook the task of replacing the worm and wheel-segment in use, by others of the desired degree of accuracy. In this they were successful, and the degree of accuracy attained may be inferred when it is said that it is required that the position of the circle shall not be in error by more that 0.5 second of arc, which corresponds to a linear distance of 0.00003 inch on the circumference where the worm and wheel-segment is applied.

9. A comparator for the measurement of bolographs in ordinates and abscissæ was procured of Warner & Swasey. The specifications were prepared at the Observatory with a provisional design which was elaborated and modified by the makers.

10. The large Zeiss photographic lens has been fitted with a Prosch rapid shutter.

11. A new motor and a set of small taps and dies have been added to the shop equipment.

12. On the recommendation of Dr. Kayser, of the University of Bonn, who inspected the Observatory during a visit to this country, sulphuric acid has been used to dry the

spectrometer chamber in the place of lime, as heretofore. A lead tank about 6 feet long and 15 inches wide is supplied with about a gallon at a time of crude sulphuric acid. This acid will abstract more than an equal volume of water from the air before requiring changing, and its use has been attended with very satisfactory results in preserving the faces of the rock-salt optical apparatus.

GENERAL ALTERATIONS.

The Observatory has received necessary repairs, and has been repainted. Its color is now a light drab, and the roof is white in place of the previous brown. It is found that the difference of the temperature maxima of thermometers within and without the Observatory has become nearly 4° C. greater since the repainting. This greatly ameliorates the condition of the observers.

It is proposed to introduce a system of cooling by ammonia gas, to be regulated automatically, so that the apparatus can be maintained through the months of April, May, June, July, August, September, and October, as in the winter, at a standard, constant temperature. As much of the clear observing weather comes in these months, much is hoped from this change.

PERSONNEL.

Mr. L. E. Emerson closed his period of service at the Observatory July 1, 1897.

SUMMARY.

I may sum up the result of the year's work by saying that the positions of between 200 and 300 lines in the infra-red solar spectrum have been accurately established; that an account of the researches of the observatory has been prepared for publication; and that the instrumental equipment has reached a state of excellence never before equaled.

Respectfully submitted.

C. G. Abbot,
Aid Acting in Charge, Astrophysical Observatory.

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

APPENDIX VI.

REPORT OF THE LIBRARIAN FOR THE YEAR ENDING JUNE 30, 1897.

SIR: I have the honor to present herewith a report upon the operations of the library of the Smithsonian Institution during the fiscal year ended June 30, 1897.

The entry numbers of accessions to the Smithsonian deposit at the Library of Congress extend from 339,340 to 364,972.

The following table gives an analysis, in volumes, parts of volumes, pamphlets, and charts, during the year:

Publications received between July 1, 1896, and June 30, 1897.

	Quarto or larger.	Octavo or smaller.	Total.
Volumes	607	1,306	1,913
Parts of volumes	17,975	8,420	26,395
Pamphlets	731	4,126	4,857
Charts			371
Total			33,536

In addition to this there have been added to the Secretary's library, office library, and the library of the Astrophysical Observatory, 332 volumes and pamphlets, and 2,044 parts of volumes, making a total of 2,376, and a grand total of accessions for the year of 35,912 volumes, parts of volumes, pamphlets, and charts.

These accessions show a gain of 992 in volumes, parts of volumes, and charts over the previous year, and in the number of entries, 793.

The following universities have sent complete sets of their academic publications, including inaugural dissertations:

Basel.
Berlin.
Bern.
Bonn.
Breslau.
Cornell.
Dorpat.
Erlanger.
Freiberg.
Giessen.
Griefswald.
Halle-am-Saale.
Heidelberg.
Helsingfors.
Jena.
Johns Hopkins.
Kiel.
Konigsberg.
Leipzig.
Louvain.
Lund.
Marburg.
Montevideo.
Montreal.
Pennsylvania.
Strassburg.
Tubingen.
Utrecht.
Wurzburg.
Zurich.

While it is not possible to specify the large number of academies, learned societies, editors of periodicals, and private persons who have sent their publications to the Institution, one gift is deserving of especial notice, that of Mr. S. Patcanof, of St. Petersburg. Mr. Patcanof presented over 300 volumes, including manuscripts, printed books, and pamphlets, relating to oriental and Russian philology, archæology, geography, and folklore, and more especially works relating to Armenian literature. The collection includes quite a number of rare works.

Several months were spent in revising the list of foreign establishments to which Smithsonian publications were being sent. It was found that many of them were not sending an adequate return, and a systematic correspondence has been begun with a view to remedying this state of affairs.

A check list of scientific periodicals in the Smithsonian library is being prepared for the new edition of Dr. Bolton's Bibliography of Scientific Periodicals, to be issued by the Institution. In view of the very large number of publications of this character belonging to the Institution, the preparation of this list entails much labor and is not yet completed.

In accordance with the plan of the Secretary for increasing the library exchanges, 673 letters were written for new exchanges and for completing series already in the library, with the result that 104 new exchanges were added to the list and 101 defective series were either completed or added to as far as the publishers could supply the missing parts.

I have little doubt but that much more could be done in this direction if more assistance could be had. The preparation and revision of lists and the examination of the library records for the ascertainment of missing parts requires much and careful labor, and must necessarily proceed slowly. The time occupied in writing the letters is but trifling, compared with that required for the preliminary work.

During the year I attended two meetings of the American Library Association—one at Cleveland, in August, 1896, at which I presented a paper entitled "Fifty years of library promotion at the Smithsonian Institution," and at the meeting of the same association held at Philadelphia in June, 1897, a paper on the proposed "International Catalogue of Scientific Literature."

Respectfully submitted.

CYRUS ADLER, *Librarian.*

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

APPENDIX VII.

REPORT OF THE EDITOR FOR THE YEAR ENDING JUNE 30, 1897.

SIR: I have the honor to submit the following report on the publications of the Smithsonian Institution for the year ending June 30, 1897:

I. CONTRIBUTIONS TO KNOWLEDGE.

Two memoirs of the Contributions have been issued this year, each of them having been submitted in the Hodgkins fund prize competition.

No. 1033. Argon; a New Constituent of the Atmosphere; by Lord Rayleigh and Prof. William Ramsay. (Part of Vol. XXIX of Smithsonian Contributions to Knowledge.) Quarto pamphlet of 43 pages, illustrated with 5 text figures.

This memoir was submitted in competition for one of the Hodgkins Fund prizes offered by the Smithsonian Institution, and the first prize of $10,000, for a treatise embodying some new and important discovery in regard to the nature or properties of atmospheric air, was awarded to Lord Rayleigh and Professor Ramsay for their discovery of "Argon," a new element of the atmosphere. The authors give a detailed description of the apparatus and methods of their investigation.

No. 1034. Atmospheric Actinometry and the Actinic Constitution of the Atmosphere; by E. Duclaux, professor of physics in the Agronomical Institute at Paris. (Part of Vol. XXIX of Smithsonian Contributions to Knowledge.) Quarto pamphlet of 48 pages.

Professor Duclaux summarizes his work as follows:

"1. The oxidation of oxalic acid in a weak solution takes place mainly, and almost exclusively, under the influence of the chemical rays of solar light; it can therefore be used as an actinometric measure.

"2. It depends on the concentration of the liquid, which for the best results should not exceed about 3 grams per liter.

"3. With an equal volume of solution combustion decreases as depth increases. There is an absorption of chemical rays, although the liquid is and remains very transparent.

"4. For equal depths of liquid, combustion is proportional to the surface, and consequently also to the volume.

"5. It depends on the age of the solution—that is to say, of the time which has elapsed since preparation. As it grows older an oxalic solution becomes more sensitive and attains a certain maximum which is quite stable and quite regular. It is well to wait till this state of sensitiveness has been produced.

"6. The daily combustion, such as is measured with sterilized liquids, varies from one day to another much more than any other meteorological phenomenon, and, while subject to the influence of what we call 'fine weather' and 'overcast weather,' it manifests very clearly other influences which are less visible.

"7. It shows also the influence of the seasons, and manifestly exhibits a maximum in spring.

"8. It is but feebly subject to the influence of altitude.

"9. On the other hand, it betrays so strongly the presence of divers oxidizable essences or substances in the air that we must consider local and daily variations as due to the presence in the atmosphere of actinic clouds, which are discoverable only by the reduction and absorption which they produce in the chemical radiations of sunlight.

"10. The atmosphere of extreme northern regions is less absorbent than that of our temperate zones, and, consequently, at the same hours of the day actinic radiation is more powerful, at the level of the soil, in the north than at the center of Europe.

"11. Northern countries add to this cause of superiority, which they owe to the constitution of their atmosphere, another, which is due to their geographical position, namely, that the actinic effect of the sun increases more rapidly than the duration of its presence above the horizon. The very long days of the north during the period of vegetation are, therefore, in their actinic effect more active than an equal number of days in our temperate regions, and we can thus explain the particularly intense rate of the progress which vegetation makes in the vicinity of the polar circle.

"12. This increase of sensitiveness which oxalic acid experiences in the sun does not cease when the light begins to fade, and may continue several days. Hence follows a conclusion which may also be applied to our temperate regions. This is that the actinic effect of a number of fine days in succession increases more rapidly than its duration, and also that the effect of a fine morning is not lost by a dark and cloudy evening.

"13. We must therefore give up the hope of finding, in the duration of a day or of solar action, a measure of its effects, and meteorological instruments which accept such a proportionality are to be rejected.

"14. The importance of these actinic phenomena in the general economy of the world is great enough to make it necessary that we should approach the investigation by appropriate means."

II. SMITHSONIAN MISCELLANEOUS COLLECTIONS.

The series of Miscellaneous Collections now includes 36 completed volumes, embracing 173 distinct papers, besides parts of 4 additional volumes. The following were published during the past fiscal year:

No. 1035. Mountain Observatories in America and Europe, by Edward S. Holden, director of the Lick Observatory. (Part of Vol. XXXVII of Smithsonian Miscellaneous Collections.) Octavo pamphlet of VI, 77 pages, with 24 full page illustrations.

In this paper Professor Holden describes the conditions of good vision at mountain stations all over the globe, and makes a short study of the high-level observatories in this country and Europe. The main scientific and practical conclusions to be drawn from the facts presented are stated by Professor Holden as follows:

"Briefly they show the necessity for a careful examination of the sites proposed for an astronomical (or meteorological) observatory before a final choice is made. They prove that while some mountain stations present great advantages for astronomical and astrophysical observatories, this is by no means the case for all. And they point out that the more frequent use of balloons, etc., in meteorology is likely to result in a rapid advance in our knowledge of the physics of the atmosphere, and to do away, in a great degree, with the need for permanent meteorological stations at high levels.

"It appears that different researches require different conditions. All would be best done at a station where both steadiness and transparency were absolute. But some can be very well performed under less perfect conditions. If one is searching for the site for a new observatory, both conditions should be insisted upon; if one is planning work at a station already established, the work should be chosen so that it can be well done under existing conditions.

"None of these (and other) obvious conclusions are new. The mass of evidence will, however, bring new conviction even to those most familiar with it; and it may serve as a check on the wasteful expenditure of public and private endowments. The subsidies to science, great as they are, thanks to the generosity of governments and of individuals, must be carefully husbanded if we are to exploit its entire domain, which is enlarging day by day."

No. 1037. Methods for the Determination of Organic Matter in Air, by David Hendricks Bergey, B. S., M. D., of the University of Pennsylvania. (Part of Vol. XXXIX of Smithsonian Miscellaneous Collections.) Octavo pamphlet of I, 28 pages, with 3 text figures.

The difficulties that were encountered in estimating the quantity of organic matter in expired air, while conducting the research on the Composition of Expired Air and its Effects upon Animal Life, are said by Dr. Bergey to have demonstrated the fact that some of the methods in use were unsatisfactory. He reviews various forms of apparatus used by foreign and American investigators, and concludes as follows:

"1. The quantity of organic matter bears an intimate relation to the amount of dust floating in the air. It is probable that the gaseous organic matter forms but an exceedingly small proportion of the total organic matter.

"2. The attachment of a dust filter of asbestus to the absorption apparatus produces results that are constantly lower than those obtained without the dust filter.

"The most reliable method for the estimation of organic matter in air is that known as Remsen's method, and is called Method III*a* in this research.[1] The pumice stone seems to be the best form of absorbent material, because it can be thoroghly cleansed by heat without changing its condition or usefulness.

"4. Those methods which determine the organic matter from its reducing action on permanganate do not seem to afford as satisfactory results as those in which the organic matter is estimated as ammonia."

No. 1038. Smithsonian Physical Tables, prepared by Thomas Gray. (Part of Vol. XXXV of Smithsonian Miscellaneous Collections.) Octavo volume of xxxiv, 301 pages.

"In connection with the system of meteorological observations established by the Smithsonian Institution about 1850, a series of meteorological tables was compiled by Dr. Arnold Guyot, at the request of Secretary Henry, and was published in 1852. A second edition was issued in 1857, and a third edition, with further amendments, in 1859. Though primarily designed for meteorological observers reporting to the Smithsonian Institution, the tables were so widely used by physicists that, after twenty-five years of valuable service, the work was again revised and a fourth edition was published in 1884. In a few years the demand for the tables exhausted the edition, and it appeared to Secretary Langley desirable to recast the work entirely, rather than to undertake its revision again. It was decided to publish a new work in three parts—Meteorological Tables, Geographical Tables, and Physical Tables—each representative of the latest knowledge in its field, and independent of the others, but the three forming a homogeneous series. Although thus historically related to Dr. Guyot's tables, the present work is so entirely changed with respect to material, arrangement, and presentation that it is not a fifth edition of the older tables, but essentially a new publication."

The first volume of the new series, the Meteorological Tables, appeared in 1893, and a second edition was published in 1896. The second volume, the Geographical Tables, prepared by Prof. R. S. Woodward, was published in 1894. The volume of Physical Tables, forming the third of the series, was published during this year. It was prepared by Prof. Thomas Gray, of the Rose Polytechnic Institute, Terre Haute, Ind.

In the preface to the tables Professor Gray says:

"In the space assigned to this book it was impossible to include, even approximately, all the physical data available. The object has been to make the tables easy of reference and to contain the data most frequently required. In the subjects included it has been necessary in many cases to make brief selections from a large number of more or less discordant results obtained by different experimenters. I have endeavored, as far as possible, to compile the tables from papers which are vouched for by well-known authorities, or which, from the method of experiment and the apparent care taken in the investigation, seem likely to give reliable results.

"Such matter as is commonly found in books of mathematical tables has not been included, as it seemed better to utilize the space for physical data. Some tables of a mathematical character which are useful to the physicist, and which are less easily found, have been given. Many of these have been calculated for this book, and where they have not been so calculated their source is given.

"The authorities from which the physical data have been derived are quoted on the same page with the table, and this is the case also with regard to explanations of the meaning or use of the tabular numbers. In many cases the actual numbers given in the tables are not to be found in the memoirs quoted. In such cases the tabular numbers have been obtained by interpolation or calculation from the published results. The reason for this is the desirability of uniform change of argument in the tables, in order to save space and to facilitate comparison of results. Where it seemed desirable the tables contain values both in metric and in British units, but as a rule the centimeter, gram, and second have been used as fundamental units. In the comparison of British and metric units, and quantities expressed in them, the meter has been taken as equal to 39.37 inches, which is the legal ratio in the United States. It is hardly possible that a series of tables, such as those here given, involv-

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[1] METHOD III.—Absorbent material, freshly ignited, finely granular pumice stone. Absorption apparatus, a small glass tube, 20 cm. in length, consisting of a narrow portion 4 cm. long and 3 mm. in its internal diameter, and an expanded portion 16 cm. long and 12 mm. in its internal diameter, similar in size and form to the absorption tube used by Remsen and by Abbott in their experiments.

ing so much transcribing, interpolation, and calculation, can be free from errors, but it is hoped that these are not so numerous as to seriously detract from the use of the book."

No. 1039. Virginia Cartography: A Bibliographical Description, by P. Lee Phillips. (Part of Vol. XXXVII, Smithsonian Miscellaneous Collections.) Octavo pamphlet of 85 pages.

Mr. Phillips's work is a bibliographical description of all known maps of Virginia since the year 1585. Special mention is made of the John With, or John White, map of 1585, the Capt. John Smith map of 1608, and the map made by Augustine Herman in 1670, now exceedingly rare. These three maps were copied by nearly all map makers up to 1751, when the Fry and Jefferson map appeared. The list comprises in all about 300 maps.

No. 1071. Air and Life, by Henry De Varigny, of the Paris Museum of Natural History. (Part of Vol. XXXIX of Smithsonian Miscellaneous Collections.) Octavo pamphlet of 69 pages.

Dr. Varigny's essay was awarded the Hodgkins prize of $1,000 for the best popular essay on atmospheric air. In order to give this work, as also those of Mr. Russell and Dr. Cohen, as wide a circulation as possible they have been printed both in the Smithsonian Miscellaneous Collections and in the Annual Report, separate editions from the report being also issued in pamphlet form. Dr. Varigny in a popular manner discusses air from the physical and from the chemical point of view, and after explaining the biological rôle of the chemical constituents of the air concludes with a chapter on the biological rôle of air physically considered.

No. 1072. The Atmosphere in Relation to Human Life and Health, by Francis Albert Rollo Russell, vice-president of the Royal Meteorological Society. (Part of Vol. XXXIX of Smithsonian Miscellaneous Collections.) Octavo pamphlet of 148 pages.

This essay by Mr. Russell which was awarded a silver medal in the Hodgkins prize competition, is a discussion of the principal functions of the various elements and substances of which the atmosphere is composed, with special reference to their influence upon human life and welfare. It also discusses the influence of climate upon national health and shows in what manner the spreading of infectious or epidemic diseases in the animal world and in mankind depends, in a very great degree, upon aerial influences. In conclusion the author indicates lines of research in the study of atmospheric air that may be beneficial to mankind.

No. 1073. The Air of Towns, by Dr. J. B. Cohen, of Yorkshire College, Leeds, England. (Part of Vol. XXXIX of Smithsonian Miscellaneous Collections.) Octavo pamphlet of 41 pages, with 2 text figures and 21 full-page illustrations.

Dr. Cohen's paper was also entered in the Hodgkins prize competition, and was granted honorable mention by the committee of award and recommended for publication by the Institution. The author treats the subject in the form of four lectures on "Close rooms," "Smoke," "Town fog," and "The germs of the air." In the discussion of the several topics Dr. Cohen indicates the many impurities in the air of cities, especially where there is much smoke from factory chimneys, and suggests various remedies for exterminating the impurities or for alleviating the danger therefrom. The paper is accompanied by a number of illustrations of atmospheric microbes and of apparatus used in his investigation of the atmosphere of cities.

No. 1075. The Constants of Nature. Part V. A Recalculation of the Atomic Weights, by Frank Wigglesworth Clarke, chief chemist of the United States Geological Survey. New edition, revised and enlarged. (Part of Vol. XXXVIII, Smithsonian Miscellaneous Collections.) Octavo pamphlet of VI, 370 pages.

"This work is one of a series devoted to the discussion and more precise determination of various 'constants of nature,' and forms the fifth contribution to that subject published by this Institution.

"The first number of the series, embracing tables of 'Specific gravities' and of 'Melting and boiling points of bodies,' prepared by the same author, Prof. F. W. Clarke, was published in 1873. The fourth part of the series, comprising a complete digest of the various 'Atomic weight' determinations of the chemical elements published since 1814, commencing with the well-known 'Table of equivalents,' by Wollaston (given in the Philosophical Transactions for that year), compiled by Mr. George F. Becker, was published by the Institution in 1880. The present work comprises a very full discussion and recalculation of the 'Atomic weights' from all the existing data and the assignment of the most probable value to each of the elements.

"The first edition of this work was published in 1882, and this new edition, revised and enlarged by Professor Clarke, contains new information accumulated during the past fifteen years."

No. 1077. Equipment and Work of an Aero-physical Observatory, by Alexander McAdie. (Part of Vol. XXXIX, Smithsonian Miscellaneous Collections.) Octavo pamphlet of 30 pages. Mr. McAdie's essay was submitted in the Hodgkins fund prize competition and was awarded honorable mention with a bronze medal.

SMITHSONIAN ANNUAL REPORTS.

No. 992. B. 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, 1894. Report of the National Museum. Washington: Government Printing Office. 1896. 8°. xxvi, 1030 pp., with 25 full-page illustrations, chart, map, and 374 text figures.

Part I of this volume comprises the report of the assistant secretary of the Smithsonian Institution in charge of the National Museum, with Appendices, and Part II consists of papers describing and illustrating collections in the United States National Museum, as follows: Primitive Travel and Transportation, by Otis Tufton Mason; Mancala, the National Game of Africa, by Stewart Culin; The Golden Patera of Rennes, by Thomas Wilson; The Wooden Statue of Baron Ii Kamon-no-Kami Naosuké, translated from the Japanese by A. Satoh; A Study of Primitive Methods of Drilling, by J. D. McGuire; The Swastika, by Thomas Wilson.

No. 1078. Annual Report of the Board of Regents of the Smithsonian Institution, showing the operations, expenditures, and condition of the Institution to July, 1895. Washington: Government Printing Office. 1896. 8°. xliii, 837 pp., with 80 plates and 2 text figures.

This volume contains the Journal of Proceedings of the Board of Regents at the annual meeting, held January 23, 1895; the Report of the Executive Committee of the Board for the year; acts and resolutions of Congress relative to the Institution, and the Report of the Secretary of the Institution; concluding with the general appendix, containing the following papers:

Atmospheric Electricity, by Prof. Arthur Schuster; The General Bearings of Magnetic Observations, by Ettrick W. Creak; Recent Progress in Optics, by Prof. Le Conte Stevens; Air and Life, by Dr. Henry de Varigny; The Atmosphere in Relation to Human Life and Health, by Francis Albert Rollo Russell; The Air of Towns, by Prof. J. B. Cohen; The Composition of Expired Air and its Effects upon Animal Life, by Drs. J. S. Billings, S. Weir Mitchell, and D. H. Bergey; Physiological Light, by Raphael Dubois; Oceanography, Bionomics, and Aquiculture, by William A. Herdman; Botanical Work of the British Association, by W. T. Thiselton-Dyer; Zoology since Darwin, by Prof. Ludwig v. Graff; The Evolution of Modern Scientific Laboratories, by Dr. William H. Welch; The Yellow Races, by Dr. E. T. Hamy; Compulsory Migrations in the Pacific Ocean, by Otto Sittig; The Old Settlements and Architectural Structures in Northern Central America, by Dr. Carl Sapper; The Cliff Villages of the Red Rock Country, and the Tusayan Ruins of Sikyatki and Awatobi, Arizona, by J. Walter Fewkes; Race and Civilization, by Prof. W. M. Flinders Petrie; Polychromy in Greek Statuary, by Maxime Collignon; Relation of Primitive Peoples to Environment, Illustrated by American Examples, by J. W. Powell; Influence of Environment upon Human Industries or Arts, by Otis Tufton Mason; The Japanese Nation—A Typical Product of Environment, by Gardiner G. Hubbard; The Tusayan Ritual: A Study of the Influence of Environment on Aboriginal Cults, by J. Walter

Fewkes; The Relation of Institutions to Environment, by W J McGee; The Centennial of the Institute of France, by Jules Simon; Science in Early England, by Charles L. Barnes; The Place of Research in Education, by H. E. Armstrong; Huxley and His Work, by Theodore Gill; Pasteur, by George M. Sternberg; Helmholtz, by T. C. Mendenhall.

III. PAPERS FROM ANNUAL REPORT.

No. 1040. Proceedings of Regents. Report of Executive Committee. Acts and resolutions of Congress. (From the Smithsonian Report for 1895.) Octavo pamphlet of 43 pages.

No. 1041. Atmospheric Electricity, by Arthur Schuster, F. R. S. (From the Smithsonian Report for 1895.) Octavo pamphlet of 15 pages, with 1 plate.

No. 1042. The General Bearings of Magnetic Observations, by Ettrick W. Creak. (From the Smithsonian Report for 1895.) Octavo pamphlet of 8 pages.

No. 1043. Recent Progress in Optics, by Prof. W. LeConte Stevens. (From the Smithsonian Report for 1895.) Octavo pamphlet of 17 pages.

No. 1044. Air and Life, by Henry De Varigny. (From the Smithsonian Report for 1895.) Octavo pamphlet of 60 pages.

No. 1045. The Atmosphere in Relation to Human Life and Health, by Francis Albert Rollo Russell. (From the Smithsonian Report for 1895.) Octavo pamphlet of 145 pages.

No. 1046. The Air of Towns, by J. B. Cohen. (From the Smithsonian Report for 1895.) Octavo pamphlet of 38 pages, with 21 plates of illustrations.

No. 1047. The Composition of Expired Air and its Effects upon Animal Life, by Drs. J. S. Billings, S. Weir Mitchell, and D. H. Bergey. (From the Smithsonian Report for 1895.) Octavo pamphlet of 23 pages.

No. 1048. Physiological Light, by Prof. Raphael Dubois. (From the Smithsonian Report for 1895.) Octavo pamphlet of 18 pages, with 4 plates of illustrations.

No. 1049. Oceanography, Bionomics, and Aquiculture, by William A. Herdman. (From the Smithsonian Report for 1895.) Octavo pamphlet of 21 pages.

No. 1050. Botanical Work of the British Association, by W. T. Thiselton-Dyer, F. R. S. (From the Smithsonian Report for 1895.) Octavo pamphlet of 20 pages.

No. 1051. Zoology since Darwin, by Ludwig v. Graff. (From the Smithsonian Report for 1895.) Octavo pamphlet of 14 pages, with full-page portrait of Darwin.

No. 1052. The Evolution of Modern Scientific Laboratories, by William H. Welch, M. D. (From the Smithsonian Report for 1895.) Octavo pamphlet of 11 pages.

No. 1053. The Yellow Races, by Dr. E. T. Hamy. (From the Smithsonian Report for 1895.) Octavo pamphlet of 12 pages.

No. 1054. Compulsory Migrations in the Pacific Ocean, by Otto Sittig. (From the Smithsonian Report for 1895.) Octavo pamphlet of 16 pages, illustrated with colored map.

No. 1055. The Old Indian Settlements and Architectural Structures in Northern Central America, by Dr. Carl Sapper. (From the Smithsonian Report for 1895.) Octavo pamphlet of 18 pages, illustrated with 6 full-page plates.

No. 1056. Preliminary Account of an Expedition to the Cliff Villages of the Red Rock Country, and the Tusayan Ruins of Sikyatki and Awatobi, Arizona, in 1895, by J. Walter Fewkes. (From the Smithsonian Report for 1895.) Octavo pamphlet of 31 pages, illustrated with 32 full-page plates.

No. 1057. Race and Civilization, by Prof. W. M. Flinders Petrie, D. C. L., LL. D. (From the Smithsonian Report for 1895.) Octavo pamphlet of 11 pages.

No. 1058. Polychromy in Greek Statuary, by Maxime Collignon. (From the Smithsonian Report for 1895.) Octavo pamphlet of 22 pages.

No. 1059. Relation of Primitive Peoples to Environment, illustrated by American examples, by J. W. Powell. (From the Smithsonian Report for 1895.) Octavo pamphlet of 12 pages.

No. 1060. Influence of Environment upon Human Industries or Arts, by Otis Trifton Mason. (From the Smithsonian Report for 1895.) Octavo pamphlet of 26 pages, with 1 full-page illustration.

No. 1061. The Japanese Nation: A Typical Product of Environment, by Gardiner G. Hubbard. (From the Smithsonian Report for 1895.) Octavo pamphlet of 14 pages.

No. 1062. The Tusayan Ritual: A Study of the Influence of Environment on Aboriginal Cults, by J. Walter Fewkes. (From the Smithsonian Report for 1895.) Octavo pamphlet of 17 pages, with 4 full-page plates of illustrations.

No. 1063. The Relations of Institutions to Environment, by W J McGee. (From the Smithsonian Report for 1895.) Octavo pamphlet of 10 pages.

No. 1064. (Includes 1059 to 1063 in one cover.)

No. 1065. The Centennial of the Institute of France, by Jules Simon. (From the Smithsonian Report for 1895.) Octavo pamphlet of 14 pages.

No. 1066. Science in Early England, by Charles L. Barnes, M. A., F. C. S. (From the Smithsonian Report for 1895.) Octavo pamphlet of 12 pages.

No. 1067. The Place of Research in Education, by H. E. Armstrong. (From the Smithsonian Report for 1895.) Octavo pamphlet of 15 pages.

No. 1068. Huxley and His Work, by Theodore Gill. (From the Smithsonian Report for 1895.) Octavo pamphlet of 20 pages, with full-page portrait of Huxley.

No. 1069. Pasteur, by George M. Sternberg, M. D., LL. D. (From the Smithsonian Report for 1895.) Octavo pamphlet of 5 pages.

No. 1070. Helmholtz, by T. C. Mendenhall. (From the Smithsonian Report for 1895.) Octavo pamphlet of 6 pages.

No. 1074. Report of S. P. Langley, Secretary of the Smithsonian Institution, for the year ending June 30, 1896. An octavo pamphlet of 77 pages, with 6 full-page illustrations.

IV. SPECIAL PUBLICATIONS NOT INCLUDED IN REGULAR SERIES.

No. 1081. International Exchange List of the Smithsonian Institution, corrected to July, 1897. City of Washington. Published by the Smithsonian Institution, 1897. 8°, ix, 331 pp.

No. 1082. Memoir of George Brown Goode, 1851–1896. By S. P. Langley. Read before the National Academy April 21, 1897. Washington, 1897. 8°, 30 pp.

No. 1086. The Smithsonian Institution, 1846–1896. The History of its First Half Century. Edited by George Brown Goode. City of Washington, 1897. Royal 8°, x, 856 pp., with 26 full-page illustrations. The contents of this volume are as follows:

Preface, by the President of the United States.
Introduction, by the Secretary of the Institution.
History of the Smithsonian Institution:
I. James Smithson, by Samuel Pierpont Langley.
II. The Founding of the Institution, 1835–1846, by George Brown Goode.
III. The Establishment and the Board of Regents, by George Brown Goode.
IV. The Three Secretaries, by George Brown Goode.
V. The Benefactors, by Samuel Pierpont Langley.
VI. The Smithsonian Building and Grounds, by George Brown Goode.
VII. The Smithsonian Library, by Cyrus Adler.
VIII. The United States National Museum, by Frederick William True.
IX. Bureau of American Ethnology, by W J McGee.
X. The International Exchange System, by William Crawford Winlock.
XI. The Astrophysical Observatory, by Samuel Pierpont Langley.
XII. The National Zoological Park, by Frank Baker.
XIII. Exploration Work of the Smithsonian Institution, by Frederick William True.
XIV. The Smithsonian Publications, by Cyrus Adler.
XV. Biographical Sketch of George Brown Goode, by David Starr Jordan.

Appreciations of the work of the Smithsonian Institution:

I. Physics, by Thomas Corwin Mendenhall, president of the Worcester Polytechnic Institute.

II. Mathematics, by Robert Simpson Woodward, professor of mechanics, Columbia University, New York City.

III. Astronomy, by Edward S. Holden, director of the Lick Observatory, Mount Hamilton, Cal.

IV. Chemistry, by Marcus Benjamin.

V. Geology and Mineralogy, by William North Rice, professor of geology, Wesleyan University, Middletown, Conn.

VI. Meteorology, by Marcus Benjamin.

VII. Paleontology, by Edward Drinker Cope, professor of zoology and comparative anatomy, University of Pennsylvania, Philadelphia, and editor of the American Naturalist.

VIII. Botany, by William Gilson Farlow, professor of cryptogamic botany, Harvard University, Cambridge, Mass.

IX. Zoology, by Theodore Gill, professor of zoology, Columbian University, Washington.

X. Anthropology, by Jesse Walter Fewkes, editor of the Journal of American Ethnology and Archæology.

XI. Geography, by Gardiner Greene Hubbard, president of the National Geographic Society, Washington.

XII. Bibliography, by Henry Carrington Bolton, lecturer on the history of chemistry, and professor of bibliography, Columbian University.

XIII. The Cooperation of the Smithsonian Institution with other Institutions of Learning, by Daniel Coit Gilman, president of Johns Hopkins University, Baltimore, Md.

XIV. The Influence of the Smithsonian Institution upon the Development of Libraries, the Organization of the Work of Societies, and the Publication of Scientific Literature in the United States, by John Shaw Billings, director of the New York Public Library.

XV. Relation between the Smithsonian Institution and the Library of Congress, by Ainsworth Rand Spofford, Librarian of Congress.

Appendix.

Principal Events in the History of the Institution, compiled by William Jones Rhees.

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V. NATIONAL MUSEUM PUBLICATIONS.

The publications by the National Museum are issued directly by the Museum and will be described in detail in the Museum volume of the Smithsonian report. It seems proper, however, to here mention the works issued during the year:

Report on the National Museum for 1894, included under Smithsonian annual reports.

Proceedings of the United States National Museum, Vol. XVIII. Published under the direction of the Smithsonian Institution. Washington: Government Printing Office, 1896. 8°, xiv, 819 pp., with text figures and 35 full-page plates. This volume contains 68 papers on natural history subjects, describing many families, genera, and species new to science.

Proceedings of the United States National Museum, Vol. XIX. About twenty papers belonging to this volume were issued during the year.

Proceedings of the United States National Museum, Vol. XX. Advance sheets of three papers for this volume appeared during the year.

Bulletin of the United States National Museum, No. 47. The Fishes of North and Middle America. A descriptive catalogue of the species of fish-like vertebrates found in the waters of North America north of the Isthmus of Panama. By David

Starr Jordan, Ph. D., and Barton Warren Evermann, Ph. D. Part I. Washington: Government Printing Office, 1896. 8°, lx, 1240 pp.

Bulletin of the United States National Museum, No. 49. Bibliography of the published writings of Philip Lutley Sclater, F. R. S., secretary of the Zoological Society of London. Prepared under the direction of G. Brown Goode. Washington: Government Printing Office, 1896. 8°, XIX, 135 pp., with portrait of Dr. Sclater.

Two special bulletins of the museum were also published during the year, consisting of Oceanic Ichthyology, by Drs. Goode and Bean, and Life Histories of North American Birds, by Major Bendire. Both of these works were also issued in the series of Smithsonian Contributions to Knowledge, and were mentioned in last year's report.

VI. PUBLICATIONS OF THE BUREAU OF AMERICAN ETHNOLOGY.

The Bureau of American Ethnology, under the direction of the Smithsonian Institution, issued the following publications during the year:

Nos. 1079 and 1080. Fourteenth Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution, 1892-93, by J. W. Powell, director. In two parts. Part 1 [-part 2]. Washington: Government Printing Office, 1896 [1897]. Roy. 8°, two parts, LXI, 1-637; 639-1136 p., 122 pl., 104 fig. Report of the Director, pp. XXV-LXI. The Menomini Indians, by Walter James Hoffman, M. D., pp. 3-328, pls. -XXXVI, figs. 1-55. The Coronado Expedition, 1540-1542, by George Parker Winship, pp. 329-613, pls. XXXVIII-LXXXIV. Index to Part 1, pp. 615-637. The Ghost-dance Religion and the Sioux Outbreak of 1890, by James Mooney, pp. 641-1110, pls. LXXXV-CXXII, figs. 56-104. Index to Part 2, pp. 1111-1136.

No. 1083. Fifteenth Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution, 1893-94, by J. W. Powell, director. Washington: Government Printing Office, 1897. Roy. 8°, CXXI, 366 p., 125 pl., 48 (†1 fig.). Report of the Director, pp. XV-CXXI. Stone Implements of the Potomac-Chesapeake Tidewater Province, by William Henry Holmes, pp. 3-152, pl. I-CIII and frontispiece, fig. 1-29a. The Siouan Indians: a Preliminary Sketch, by W J McGee, pp. 153-204. Siouan Sociology: a Posthumous Paper, by James Owen Dorsey, pp. 205-244, figs. 30-38. Tusayan Katcinas, by Jesse Walter Fewkes, pp. 245-313, pl. CIV-CXI, figs. 39-48. The Repair of Casa Grande Ruin, Arizona, in 1891, by Cosmos Mindeleff, pp. 315-349, pl. CXII-CXXV. Index, pp. 351-366.

No. 1085. Sixteenth Annual Report of the Bureau of American Ethnology to the Secretary of the Smithsonian Institution, 1894-95, by J. W. Powell, director. Washington: Government Printing Office, 1897. Roy. 8°, CXIX, 326, p., 81 pl., 83 fig. Report of the Director, pp. XIII-CXIX. Primitive Trephining in Peru, by Manuel Antonio Muñiz and W J McGee, pp. 3-72, pl. I-XL. Cliff Ruins of Canyon de Chelly, Arizona, by Cosmos Mindeleff, pp. 73-198, pls. XLI-LXIII, figs. 1-83. Day Symbols of the Maya Year, by Cyrus Thomas, pp. 199-265, pls. LXIV-LXIX. Tusayan Snake Ceremonies, by Jesse Walter Fewkes, pp. 267-312, pl. LXX-LXXXI. Index, pp. 313-326.

VII. ANNUAL REPORTS OF THE AMERICAN HISTORICAL ASSOCIATION.

Annual Report of the American Historical Association for the year 1895. Washington: Government Printing Office, 1896. 8°, x, 1247 pp.

This volume contains the report of proceedings of the eleventh annual meeting of the Association, held in Washington City, December 26 to 27, 1895, by Herbert B. Adams, secretary; report of the treasurer; list of committees; necrology; inaugural address, by President George F. Hoar, on Popular Discontent with Representative Government; The Surroundings and Site of Raleigh's Colony, by Talcott Williams; Governor Edward Winslow, His Part and Place in Plymouth Colony, by Rev. William C. Winslow; Arent Van Curler and His Journal of 1634-35, by Gen. James Grant Wilson; Political Activity of Massachusetts Towns during the Revolution, by Harry A. Cushing; The Land System of Provincial Pennsylvania, by

William R. Shepherd; The Electoral College for the Senate of Maryland and the Nineteen Van Buren Electors, by Dr. B. C. Steiner; Libraries and Literature of North Carolina, by Dr. S. B. Weeks; Suffrage in the State of North Carolina (1776–1861), by Prof. J. S. Bassett; Locating the Capital, by Gaillard Hunt; "Free Burghs" in the United States, by James H. Blodgett; The Employment of Indians in the War of 1812, by Ernest Cruikshank; Commodore John Barry, by Martin I. J. Griffin; Agreement of 1817: Reduction of Naval Forces upon the American Lakes, by J. M. Callahan; The "Underground Railroad" for Liberation of Fugitive Slaves, by Prof. W. H. Siebert; Some Bold Diplomacy in the United States in 1861, by Gen. Marcus J. Wright; The Battle of Gettysburg, by Harold P. Goodnow; Historical Testimony, by Dr. James Schouler; A Plea for the Study of History in Northern Europe, by Prof. A. C. Coolidge; The French Revolution as Seen by the Americans of the Eighteenth Century, by Prof. C. D. Hazen; Napoleon's Concordat with Pope Pius VII, 1801, by Prof. Charles L. Wells; The German Imperial Court, by O. G. Villard; Dismemberment of the Turkish Empire, by Prof. E. K. Alden; Colonies of North America, and the Genesis of the Commonwealths of the United States, by Dr. J. M. Toner; Classification of Colonial Governments, by Prof. H. L. Osgood; Slavery in the Province of South Carolina (1670 to 1770), by Edward McCrady; Bibliography of Historical Societies of the United States and British America, by A. P. C. Griffin; Index.

Respectfully submitted.

A. HOWARD CLARK, *Editor*.

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

GENERAL APPENDIX

TO THE

SMITHSONIAN REPORT FOR 1897.

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; reports of investigations made by collaborators of the Institution; and memoirs of a general character or on special topics that are 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, zoology, 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 1897.

ASPECTS OF AMERICAN ASTRONOMY.[1]

By Simon Newcomb.

The University of Chicago yesterday accepted one of the most munificent gifts ever made for the promotion of any single science, and with appropriate ceremonies dedicated it to the increase of our knowledge of the heavenly bodies.

The president of your university has done me the honor of inviting me to supplement what was said on that occasion by some remarks of a more general nature suggested by the celebration. One is naturally disposed to say first what is uppermost in his mind. At the present moment this will naturally be the general impression made by what has been seen and heard. The ceremonies were attended, not only by a remarkable delegation of citizens, but by a number of visiting astronomers which seems large when we consider that the profession itself is not at all numerous in any country. As one of these, your guests, I am sure that I give expression only to their unanimous sentiment in saying that we have been extremely gratified in many ways by all that we have seen and heard. The mere fact of so munificent a gift to science can not but excite universal admiration. We knew well enough that it was nothing more than might have been expected from the public spirit of this great West; but the first view of a towering snow peak is none the less impressive because you have learned in your geography how many feet high it is, and great acts are none the less admirable because they correspond to what you have heard and read, and might therefore be led to expect.

The next gratifying feature is the great public interest excited by the occasion. That the opening of a purely scientific institution should have led so large an assemblage of citizens to devote an entire day, including a long journey by rail, to the celebration of yesterday is something most suggestive from its unfamiliarity. A great many scientific establishments have been inaugurated during the last half century, but if on any such occasion so large a body of citizens has gone so

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[1] Address delivered at the University of Chicago, October 22, 1897, in connection with the dedication of the Yerkes Observatory. Printed in the Astrophysical Journal, November, 1897.

great a distance to take part in the inauguration the fact has at the moment escaped my mind.

That the interest thus shown is not confined to the hundreds of attendants, but must be shared by your great public, is shown by the unfailing barometer of journalism. Here we have a field in which the nonsurvival of the unfit is the rule in its most ruthless form. The journals that we see and read are merely the fortunate few of a countless number, dead and forgotten, that did not know what the public wanted to read about. The eagerness shown by the representatives of your press in recording everything your guests would say was accomplished by an enterprise in making known everything that occurred, and, in case of an emergency requiring a heroic measure, what did not occur, showing that smart journalists of the East must have learned their trade, or at least breathed their inspiration, in these regions. I think it was some twenty years since I told a European friend that the eighth wonder of the world was a Chicago daily newspaper. Since that time the course of journalistic enterprise has been in the reverse direction to that of the course of empire, eastward instead of westward.

It has been sometimes said—wrongfully, I think—that scientific men form a mutual admiration society. One feature of the occasion made me feel that we, your guests, ought then and there to have organized such a society and forthwith proceeded to business. This feature consisted in the conferences on almost every branch of astronomy by which the celebration of yesterday was preceded. The fact that beyond the acceptance of a graceful compliment I contributed nothing to these conferences relieves me from the charge of bias or self-assertion in saying that they gave me a new and most inspiring view of the energy now being expended in research by the younger generation of astronomers. All the experience of the past leads us to believe that this energy will reap the reward which nature always bestows upon those who seek her acquaintance from unselfish motives. In one way it might appear that little was to be learned from a meeting like that of the present week. Each astronomer may know by publications pertaining to the science what all the others are doing. But knowledge obtained in this way has a sort of abstractness about it a little like our knowledge of the progress of civilization in Japan, or of the great extent of the Australian continent. It was, therefore, a most happy thought on the part of your authorities to bring together the largest possible number of visiting astronomers from Europe, as well as America, in order that each might see, through the attrition of personal contact, what progress the others were making in their researches. To the visitors at least I am sure that the result of this meeting has been extremely gratifying. They earnestly hope, one and all, that the callers of the conference will not themselves be more disappointed in its results; that however little they may have actually to learn of methods and results, they will feel stimulated to well-directed efforts and find themselves inspired by thoughts which, however familiar, will now be more easily worked out.

We may pass from the aspects of the case as seen by the strictly professional class to those general aspects fitted to excite the attention of the great public. From the point of view of the latter it may well appear that the most striking feature of the celebration is the great amount of effort which is shown to be devoted to the cultivation of a field quite outside the ordinary range of human interests.

A little more than two centuries ago Huyghens prefaced an account of his discoveries on the planet Saturn with the remark that many, even among the learned, might think he had been devoting to things too distant to interest mankind an amount of study which would better have been devoted to subjects of more immediate concern. It must be admitted that this fear has not deterred succeeding astronomers from pursuing their studies. The enthusiastic students whom we see around us are only a detachment from an army of investigators who, in many parts of the world, are seeking to explore the mysteries of creation. Why so great an expenditure of energy? Certainly not to gain wealth, for astronomy is perhaps the one field of scientific work which, in our expressive modern phrase, "has no money in it." It is true that the great practical use of astronomical science to the country and the world in affording us the means of determining positions on land and at sea is frequently pointed out. It is said that an Astronomer Royal of England once calculated that every meridian observation of the moon made at Greenwich was worth a pound sterling on account of the help it would afford to the navigation of the ocean. An accurate map of the United States can not be constructed without astronomical observations at numerous points scattered over the whole country, aided by data which great observatories have been accumulating for more than a century, and must continue to accumulate in the future.

But neither the measurement of the Earth, the making of maps, nor the aid of the navigator is the main object which the astronomers of to-day have in view. If they do not quite share the sentiment of that eminent mathematician, who is said to have thanked God that his science was one which could not be prostituted to any useful purpose, they still know well that to keep utilitarian objects in view would only prove a handicap on their efforts. Consequently they never ask in what way their science is going to benefit mankind.

As the great captain of industry is moved by the love of wealth, and the politician by the love of power, so the astronomer is moved by the love of knowledge for its own sake, and not for the sake of its application. Yet he is proud to know that his science has been worth more to mankind than it has cost. He does not value its results merely as a means of crossing the ocean or mapping the country, for he feels that man does not live by bread alone. If it is not more than bread to know the place we occupy in the universe, it is certainly something which we should place not far behind the means of subsistence. That we now look upon a comet as something very interesting, of which the sight affords us a pleasure unmixed with fear of war, pestilence, or other calamity, and of which we therefore wish the return, is a gain we can not measure by money. In all ages astronomy has been an index to the civilization of the people who cultivated it. It has been crude or exact, enlightened or mingled with superstition, according to the current mode of thought. When once men understand the relation of the planet on which they dwell to the universe at large, superstition is doomed to speedy extinction. This alone is an object worth more than money.

Astronomy may fairly claim to be that science which transcends all others in its demands upon the practical application of our reasoning powers. Look at the stars that stud the heavens on a clear evening. What more hopeless problem to one confined to earth than that of determining their varying distances, their motions, and their physical constitution? Everything on earth we can handle and investigate. But how investigate that which is ever beyond our reach, on which we can never make an experiment? On certain occasions we see the moon pass in front of the sun and hide it from our eyes. To an observer a few miles away the sun was not entirely hidden, for the shadow of the moon in a total eclipse is rarely 100 miles wide. On another continent no eclipse at all may have been visible. Who shall take a map of the world and mark upon it the line on which the moon's shadow will travel during some eclipse a hundred years hence? Who shall map out the orbits of the heavenly bodies as they are going to appear in a hundred thousand years? How shall we ever know of what chemical elements the sun and the stars are made? All this has been done, but not by the intellect of any one man. The road to the stars has been opened only by the efforts of many generations of mathematicians and observers, each of whom began where his predecessor had left off.

We have reached a stage where we know much of the heavenly bodies. We have mapped out our solar system with great precision. But how with that great universe of millions of stars in which our solar system is only a speck of star dust, a speck which a traveler through the wilds of space might pass a hundred times without notice? We have learned much about this universe, though our knowledge of it is still dim. We see it as a traveler on a mountain top sees a distant city in a cloud of mist, by a few specks of glimmering light from steeples or roofs. We want to know more about it, its origin and its destiny; its limits in time and space, if it has any; what function it serves in the universal economy. The journey is long, yet we want, in knowledge at least, to reach the stars. Hence we build observatories and train observers and investigators. Slow indeed is progress in the solution of the greatest of problems, when measured by what we want to know. Some questions may require centuries, others thousands of years for their answer. And yet never was progress more rapid than during our time. In some directions our astronomers of to-day are out of sight of those of fifty years ago; we are even gaining heights which twenty years ago looked hopeless. Never before had the astronomer so much work—good, hard, yet hopeful work—before him as to-day. He who is leaving the stage feels that he has only begun, and must leave his successors with more to do than his predecessors left him.

To us an interesting feature of this progress is the part taken in it by our own country. The science of our day, it is true, is of no country. Yet we very appropriately speak of American science from the fact that our traditional reputation has not been that of a people deeply interested in the higher branches of intellectual work. Men yet living can remember when in the eyes of the universal church of learning all cisatlantic countries, our own included, were partes infidelium.

Yet American astronomy is not entirely of our generation. In the middle of the last century Professor Winthrop, of Harvard, was an industrious observer of eclipses and kindred phenomena, whose work was recorded in the transactions of learned societies. But the greatest astronomical activity during our colonial period was that called out by the transit of Venus in 1769, which was visible in this country. A committee of the American Philosophical Society, at Philadelphia, organized an excellent systems of observations, which we now know to have been fully as successful, perhaps more so, than the majority of those made on other continents, owing mainly to the advantages of air and climate. Among the observers was the celebrated Rittenhouse, to whom is due the distinction of having been the first American astronomer whose work has an important place in the history of the science. In addition to the observations which he has left us, he was the first inventor or proposer of the collimating telescope, an instrument which has become almost a necessity wherever accurate observations are made. The fact that the subsequent invention by Bessel was quite independent does not detract from the merits of either.

Shortly after the transit of Venus, which I have mentioned, the war of the Revolution commenced. The generation which carried on that war and the following one, which framed our Constitution and laid the bases of our political institutions, were naturally too much occupied with these great problems to pay much attention to pure science. While the great mathematical astronomers of Europe were laying the foundation of celestial mechanics their writings were a sealed book to everyone on this side of the Atlantic, and so remained until Bowditch appeared, early in the present century. His translation of the Mécanique Céleste made an epoch in American science by bringing the great work of Laplace down to the reach of the best American students of his time.

American astronomers must always honor the names of Rittenhouse and Bowditch. And yet in one respect their work was disappointing of results. Neither of them was the founder of a school. Rittenhouse left no successor to carry on his work. The help which Bowditch afforded his generation was invaluable to isolated students who, here and there, dived alone and unaided into the mysteries of the celestial motions. His work was not mainly in the field of observational astronomy, and therefore did not materially influence that branch of science. In 1832 Professor Airy, afterwards astronomer royal of England, made a report to the British Association on the condition of practical astronomy in various countries. In this report he remarked that he was unable to say anything about American astronomy because, so far as he knew, no public observatory existed in the United States.

William C. Bond, afterwards famous as the first director of Harvard Observatory, was at that time making observations with a small telescope, first near Boston and afterwards at Cambridge. But with so meager an outfit his establishment could scarcely lay claim to being an astronomical observatory, and it was not surprising if Airy did not know anything of his modest efforts.

If at this time Professor Airy had extended his investigations into yet another field, with a view of determining the prospects for a great city at the site of Fort Dearborn, on the southern shore of Lake Michigan, he would have seen as little prospect of civic growth in that region as of a great development of astronomy in the United States at large. A plat of the proposed town of Chicago had been prepared two years before, when the place contained perhaps half a dozen families. In the same month in which Professor Airy made his report, August, 1832, the people of the place, then numbering 28 voters, decided to become incorporated, and selected five trustees to carry on their government.

In 1837 a city charter was obtained from the legislature of Illinois. The growth of this infant city, then small even for an infant, into the great commercial metropolis of the West has been the just pride of its people and the wonder of the world. I mention it now because of a remarkable coincidence. With this civic growth has quietly gone on another, little noted by the great world, and yet in its way equally wonderful and equally gratifying to the pride of those who measure greatness by intellectual progress. If it be true that—

> In Nature nothing is great but man; in man nothing is great but mind—

then may knowledge of the universe be regarded as the true measure of progress. I therefore invite attention to the fact that American astronomy began with your city and has slowly but surely kept pace with it until to-day our country stands second only to Germany in the number of researches being prosecuted and second to none in the number of men who have gained the highest recognition by their labors.

In 1836 Prof. Albert Hopkins, of Williams College, and Prof. Elias

Loomis, of Western Reserve College, Ohio, both commenced little observatories. Professor Loomis went to Europe for all his instruments, but Hopkins was able even then to get some of his in this country. Shortly afterwards a little wooden structure was erected by Captain Gilliss on Capitol Hill, at Washington, and supplied with a transit instrument for observing moon culminations, in conjunction with Captain Wilkes, who was then setting out on his exploring expedition to the Southern Hemisphere. The date of these observatories was practically the same as that on which a charter for the city of Chicago was obtained from the legislature. With their establishment the population of your city had increased to 703.

The next decade, 1840 to 1850, was that in which our practical astronomy seriously commenced. The little observatory of Captain Gilliss was replaced by the Naval Observatory, erected at Washington during the years 1843–44 and fitted out with what were then the most approved instruments. About the same time the appearance of the great comet of 1843 led the citizens of Boston to erect the observatory of Harvard College. Thus it is little more than a half century since the two principal observatories in the United States were established. But we must not for a moment suppose that the mere erection of an observatory can mark an epoch in scientific history. What must make the decade of which I speak ever memorable in American astronomy was not merely the erection of buildings, but the character of the work done by astronomers away from them as well as in them.

The Naval Observatory very soon became famous by two remarkable steps which raised our country to an important position among those applying modern science to practical uses. One of these consisted of the researches of Sears Cook Walker on the motion of the newly discovered planet Neptune. He was the first astronomer to determine fairly good elements of the orbit of that planet, and, what is yet more remarkable, he was able to trace back the movement of the planet in the heavens for half a century and to show that it had been observed as a fixed star by Lalande in 1795, without the observer having any suspicion of the true character of the object.

The other work to which I refer was the application to astronomy and to the determination of longitudes of the chronographic method of registering transits of stars or other phenomena requiring an exact record of the instant of their occurrence. It is to be regretted that the history of this application has not been fully written. In some points there seems to be as much obscurity as with the discovery of ether as an anæsthetic, which took place about the same time. Happily no such contest has been fought over the astronomical as over the surgical discovery, the fact being that all who were engaged in the application of the new method were more anxious to perfect it than they were to get credit for themselves. We know that Saxton, of the Coast Survey; Mitchell and Locke, of Cincinnati; Bond, at Cambridge, as well as Walker and other astronomers at the Naval Observatory, all worked at the apparatus; that Maury seconded their efforts with untiring zeal; that it was used to determine the longitude of Baltimore as early as 1844 by Captain Wilkes, and that it was put into practical use in recording observations at the Naval Observatory as early as 1846.

At the Cambridge Observatory the two Bonds, father and son, speedily began to show the stuff of which the astronomer is made. A well-devised system of observations was put in operation. The discovery of the dark ring of Saturn and of a new satellite to that planet gave additional fame to the establishment.

Nor was activity confined to the observational side of the science. The same decade of which I speak was marked by the beginning of Professor Pierce's mathematical work, especially his determination of the perturbations of Uranus and Neptune. At this time commenced the work of Dr. B. A. Gould, who soon became the leading figure in American astronomy. Immediately on graduating at Harvard in 1845, he determined to devote all the energies of his life to the prosecution of his favorite science. He studied in Europe for three years, took the doctor's degree at Göttingen, came home, founded the Astronomical Journal, and took an active part in that branch of the work of the Coast Survey which included the determination of longitudes by astronomical methods.

An episode which may not belong to the history of astronomy must be acknowledged to have had a powerful influence in exciting public interest in that science. Prof. O. M. Mitchell, the founder and first director of the Cincinnati Observatory, made the masses of our intelligent people acquainted with the leading facts of astronomy by courses of lectures which, in lucidity and eloquence, have never been excelled. The immediate object of the lectures was to raise funds for establishing his observatory and fitting it out with a fine telescope. The popular interest thus excited in the science had an important effect in leading the public to support astronomical research. If public support, based on public interest, is what has made the present fabric of American astronomy possible, then should we honor the name of a man whose enthusiasm leavened the masses of his countrymen with interest in our science.

The civil war naturally exerted a depressing influence upon our scientific activity. The cultivator of knowledge is no less patriotic than his fellow citizens, and vies with them in devotion to the public welfare. The active interest which such cultivators took, first in the prosecution of the war and then in the restoration of the union, naturally distracted their attention from their favorite pursuits. But no sooner was political stability reached than a wave of intellectual activity set in, which has gone on increasing up to the present time. If it be true that never before in our history has so much attention been given to education as now; that never before did so many men devote themselves to the diffusion of knowledge, it is no less true that never was astronomical work so energetically pursued among us as now.

One deplorable result of the civil war was that Gould's Astronomical Journal had to be suspended. Shortly after the restoration of peace, instead of reestablishing the journal, its founder conceived the project of exploring the southern heavens. The northern hemisphere being the seat of civilization, that portion of the sky which could not be seen from our latitudes was comparatively neglected. What had been done in the southern hemisphere was mostly the occasional work of individuals and of one or two permanent observatories. The latter were so few in number and so meager in their outfit that a splendid field was open to the inquirer. Gould found the patron which he desired in the government of the Argentine Republic, on whose territory he erected what must rank in the future as one of the memorable astronomical establishments of the world. His work affords a most striking example of the principle that the astronomer is more important than his instruments. Not only were the means at the command of the Argentine Observatory slender in the extreme when compared with those of the favored institutions of the North, but, from the very nature of the case, the Argentine Republic could not supply trained astronomers. The difficulties thus growing out of the administration can not be overestimated. And yet the sixteen great volumes in which the work of the institution has been published will rank in the future among the classics of astronomy.

Another wonderful focus of activity, in which one hardly knows whether he ought most to admire the exhaustless energy or the admirable ingenuity which he finds displayed, is the Harvard Observatory. Its work has been aided by gifts which have no parallel in the liberality that prompted them. Yet without energy and skill such gifts would have been useless. The activity of the establishment includes both hemispheres. Time would fail to tell how it has not only mapped out important regions of the heavens from the north to the south pole, but analyzed the rays of light which come from hundreds of thousands of stars by recording their spectra in permanence on photographic plates.

The work of the establishment is so organized that a new star can not appear in any part of the heavens nor a known star undergo any noteworthy change without immediate detection by the photographic eye of one or more little telescopes, all-seeing and never sleeping policemen that scan the heavens unceasingly while the astronomer may sleep, and report in the morning every case of irregularity in the proceedings of the heavenly bodies.

Yet another example, showing what great results may be obtained with limited means, is afforded by the Lick Observatory, on Mount Hamilton, Cal. During the ten years of its activity its astronomers have made it known the world over by works and discoveries too varied and numerous to be even mentioned at the present time.

The astronomical work of which I have thus far spoken has been almost entirely that done at observatories. I fear that I may in this way have strengthened an erroneous impression that the seat of important astronomical work is necessarily connected with an observatory. It must be admitted that an institution which has a local habitation and a magnificent building commands public attention so strongly that valuable work done elsewhere may be overlooked. A very important part of astronomical work is done away from telescopes and meridian circles and requires nothing but a good library for its prosecution. One who is devoted to this side of the subject may often feel that the public does not appreciate his work at its true relative value from the very fact that he has no great buildings or fine instruments to show. I may therefore be allowed to claim as an important factor in the American astronomy of the last half century an institution of which few have heard and which has been overlooked because there was nothing about it to excite attention.

In 1849 the American Nautical Almanac office was established by a Congressional appropriation. The title of this publication is somewhat misleading in suggesting a simple enlargement of the family almanac which the sailor is to hang up in his cabin for daily use. The fact is that what started more than a century ago as a nautical almanac has since grown into an astronomical ephemeris for the publication of everything pertaining to times, seasons, eclipses, and the motions of the heavenly bodies. It is the work in which astronomical observations made in all the great observatories of the world are ultimately utilized for scientific and public purposes. Each of the leading nations of western Europe issues such a publication. When the preparation and publication of the American ephemeris was decided upon the office was first established in Cambridge, the seat of Harvard University, because there could most readily be secured the technical knowledge of mathematics and theoretical astronomy necessary for the work.

A field of activity was thus opened, of which a number of able young men who have since earned distinction in various walks of life availed themselves. The head of the office, Commander Davis, adopted a policy well fitted to promote their development. He translated the classic work of Gauss, Theoria Motus Corporum Cælestium, and made the office a sort of informal school, not, indeed, of the modern type, but rather more like the classic grove of Hellas, where philosophers conducted their discussions and profited by mutual attrition. When, after a few years of experience, methods were well established and a routine adopted, the office was removed to Washington, where it has since remained. The work of preparing the ephemeris has, with experience, been reduced to a matter of routine which may be continued indefinitely, with occasional changes in methods and data and improvements to meet the increasing wants of investigators.

The mere preparation of the ephemeris includes but a small part of the work of mathematical calculation and investigation required in astronomy. One of the great wants of the science to-day is the rereduction of the observations made during the first half of the present century, and even during the last half of the preceding one. The labor which could profitably be devoted to this work would be more than that required in any one astronomical observatory. It is unfortunate for this work that a great building is not required for its prosecution because its needfulness is thus very generally overlooked by that portion of the public interested in the progress of science. An organization especially devoted to it is one of the scientific needs of our time.

In such an epoch-making age as the present it is dangerous to cite any one step as making a new epoch. Yet it may be that when the historian of the future reviews the science of our day he will find the most remarkable feature of the astronomy of the last twenty years of our century to be the discovery that this steadfast earth of which the poets have told us is not after all quite steadfast; that the north and south poles move about a very little, describing curves so complicated that they have not yet been fully marked out. The periodic variations of latitude thus brought about were first suspected about 1880, and announced with some modest assurance by Küstner, of Berlin, a few years later. The progress of the views of astronomical opinion from incredulity to confidence was extremely slow until, about 1890, Chandler, of the United States, by an exhaustive discussion of innumerable results of observations, showed that the latitude of every point on the earth was subject to a double oscillation, one having a period of a year, the other of four hundred and twenty-seven days.

Notwithstanding the remarkable parallel between the growth of American astronomy and that of your city, one can not but fear that if a foreign observer had been asked only half a dozen years ago at what point in the United States a great school of theoretical and practical astronomy, aided by an establishment for the exploration of the heavens, was likely to be established by the munificence of private citizens, he would have been wiser than most foreigners had he guessed Chicago. Had this place been suggested to him, I fear he would have replied that were it possible to utilize celestial knowledge in acquiring earthly wealth, here would be the most promising seat for such a school. But he would need to have been a little wiser than his generation to reflect that wealth is at the base of all progress in knowledge and the liberal arts; that it is only when men are relieved from the necessity of devoting all their energies to the immediate wants of life that they can lead intellectual lives, and that we should therefore look to the most enterprising commercial center as the likeliest seat for a great scientific institution.

Now we have the school, and we have the observatory, which we hope will in the near future do work that will cast luster on the name of its founder as well as on the astronomers who may be associated with it. You will, I am sure, pardon me if I make some suggestions on the subject of the future needs of the establishment. We want this newly founded institution to be a great success, to do work which shall show that the intellectual productiveness of your community will not be allowed to lag behind its material growth. The public is very apt to feel that when some munificent patron of science has mounted a great telescope under a suitable dome, and supplied all the apparatus which the astronomer wants to use, success is assured. But such is not the case. The most important requisite, one more difficult to command than telescopes or observatories, may still be wanting. A great telescope is of no use without a man at the end of it, and what the telescope may do depends more upon this appendage than upon the instrument itself. The place which telescopes and observatories have taken in astronomical history are by no means proportional to their dimensions. Many a great instrument has been a mere toy in the hands of its owner. Many a small one has become famous.

Twenty years ago there was here in your own city a modest little instrument which, judged by its size, could not hold up its head with the great ones even of that day. It was the private property of a young man holding no scientific position and scarcely known to the public. And yet that little telescope is to-day among the famous ones of the world, having made memorable advances in the astronomy of double stars, and shown its owner to be a worthy successor of the Herschels and Struves in that line of work.

A hundred observers might have used the appliances of the Lick Observatory for a whole generation without finding the fifth satellite of Jupiter; without successfully photographing the cloud forms of the Milky Way; without discovering the extraordinary patches of nebulous light, nearly or quite invisible to the human eye, which fill some regions of the heavens.

When I was in Zurich last year I paid a visit to the little but not unknown observatory of its famous polytechnic school. The professor of astronomy was especially interested in the observations of the sun with the aid of the spectroscope, and among the ingenious devices which he described, not the least interesting was the method of photographing the sun by special rays of the spectrum, which had been worked out at the Kenwood Observatory in Chicago. The Kenwood Observatory is not, I believe, in the eye of the public one of the noteworthy institutions of your city which every visitor is taken to see, and yet this invention has given it an important place in the science of our day.

Should you ask me what are the most hopeful features in the great establishment which you are now dedicating, I would say that they are not alone to be found in the size of your unequaled telescope, nor in the cost of the outfit, but in the fact that your authorities have shown their appreciation of the requirements of success by adding to the material outfit of the establishment the three men whose works I have described.

Gentlemen of the Trustees, allow me to commend to your fostering care the men at the end of the telescope. The constitution of the astronomer shows curious and interesting features. If he is destined to advance the science by works of real genius, he must, like the poet, be born, not made. The born astronomer, when placed in command of a telescope, goes about using it as naturally and effectively as the babe avails itself of its mother's breast. He sees intuitively what less gifted men have to learn by long study and tedious experiment. He is moved to celestial knowledge by a passion which dominates his nature. He can no more avoid doing astronomical work, whether in the line of observations or research, than a poet can chain his Pegasus to earth. I do not mean by this that education and training will be no use to him. They will certainly accelerate his early progress. If he is to become great on the mathematical side, not only must his genius have a bend in that direction, but he must have the means of pursuing his studies. And yet I have seen so many failures of men who had the best instruction, and so many successes of men who scarcely learned anything of their teachers, that I sometimes ask whether the great American celestial mechanician of the twentieth century will be a graduate of a university or of the back woods.

Is the man thus moved to the exploration of nature by an unconquerable passion more to be envied or pitied? In no other pursuit does success come with such certainty to him who deserves it. No life is so enjoyable as that whose energies are devoted to following out the inborn impulses of one's nature. The investigator of truth is little subject to the disappointments which await the ambitious man in other fields of activity. It is pleasant to be one of a brotherhood extending over the world, in which no rivalry exists except that which comes out of trying to do better work than anyone else, while mutual admiration stifles jealousy. And yet, with all these advantages, the experience of the astronomer may have its dark side. As he sees his field widening faster than he can advance he is impressed with the littleness of all that can be done in one short life. He feels the same want of successors to pursue his work that the founder of a dynasty may feel for heirs to occupy his throne. He has no desire to figure in history as a Napoleon of science whose conquests must terminate with his life. Even during his active career his work may be of such a kind as to require the cooperation of others and the active support of the public. If he is disappointed in commanding these requirements, if he finds neither cooperation nor support, if some great scheme to which he may have devoted much of his life thus proves to be only a castle in the air, he may feel that nature has dealt hardly with him in not endowing him with passions like to those of other men.

In treating a theme of perennial interest one naturally tries to fancy what the future may have in store. If the traveler, contemplating the ruins of some ancient city which in the long ago teemed with the life and activities of generations of men, sees every stone instinct with emotion and the dust alive with memories of the past, may he not be similarly impressed when he feels that he is looking around upon a seat of future empire—a region where generations yet unborn may take a leading part in molding the history of the world? What may we not expect of that energy which in sixty years has transformed a straggling village into one of the world's great centers of commerce? May it not exercise a powerful influence on the destiny not only of the country but of the world? If so, shall the power thus to be exercised prove an agent of beneficence, diffusing light and life among nations, or shall it be the opposite?

The time must come ere long when wealth shall outgrow the field in which it can be profitably employed. In what direction shall its possessors then look? Shall they train a posterity which will so use its power as to make the world better that it has lived in it? Will the future heir to great wealth prefer the intellectual life to the life of pleasure?

We can have no more hopeful answer to these questions than the establishment of this great university in the very focus of the commercial activity of the West. Its connection with the institution we have been dedicating suggests some thoughts on science as a factor in that scheme of education best adapted to make the power of a wealthy community a benefit to the race at large. When we see what a factor science has been in our present civilization, how it has transformed the world and increased the means of human enjoyment by enabling men to apply the powers of nature to their own uses, it is not wonderful that it should claim the place in education hitherto held by classical studies. In the contest which has thus arisen I take no part but that of a peacemaker, holding that it is as important to us to keep in touch with the traditions of our race, and to cherish the thoughts which have come down to us through the centuries, as it is to enjoy and utilize what the present has to offer us. Speaking from this point of view, I would point out the error of making the utilitarian applications of knowledge the main object in its pursuit. It is an historic fact that abstract science—science pursued without any utilitarian end—has been at the base of our progress in the utilization of knowledge. If in the last century such men as Galvani and Volta had been moved by any other motive than love of penetrating the secrets of nature they would never have pursued the seemingly useless experiments they did, and the foundation of electrical science would not have been laid. Our present applications of electricity did not become possible until Ohm's mathematical laws of the electric current, which when first made known seemed little more than mathematical curiosities, had become the common property of inventors. Professional pride on the part of our own Henry led him, after making the discoveries which rendered the telegraph possible, to go no further in their application, and to live and die without receiving a dollar of the millions which the country has won through his agency.

In the spirit of scientific progress thus shown we have patriotism in its highest form—a sentiment which does not seek to benefit the country at the expense of the world, but to benefit the world by means of one's country. Science has its competition, as keen as that which is the life of commerce. But its rivalries are over the question who shall contribute the most and the best to the sum total of knowledge; who shall give the most, not who shall take the most. Its animating spirit is love of truth. Its pride is to do the greatest good to the greatest number. It embraces not only the whole human race but all nature in its scope. The public spirit of which this city is the focus has made the desert blossom as the rose, and benefited humanity by the diffusion of the material products of the earth. Should you ask me how it is in the future to use its influence for the benefit of humanity at large, I would say, look at the work now going on in these precincts, and study its spirit. Here are the agencies which will make "the voice of law the harmony of the world." Here is the love of country blended with love of the race. Here the love of knowledge is as unconfined as your commercial enterprise. Let not your youth come hither merely to learn the forms of vertebrates and the properties of oxides, but rather to imbibe that catholic spirit which, animating their growing energies, shall make the power they are to wield an agent of beneficence to all mankind.

THE BEGINNINGS OF AMERICAN ASTRONOMY.[1]

By Edward S. Holden.

It is impossible, even in the briefest sketch, not to emphasize the debt of American science and learning to the intelligent interest and patronage of our early Presidents—Washington, John Adams, Jefferson, Madison, Monroe, John Quincy Adams. The powerful impetus given by them and through them has shaped the liberal policy of our governments, National and State, toward education and toward science. Sir Lyon Playfair, in his address to the British Association for the Advancement of Science (1885) has recognized this influence in the truest and most graceful way. He said: "In the United Kingdom we are just beginning to understand the wisdom of Washington's Farewell Address to his countrymen (1796) when he said: 'Promote then, as an object of primary importance, institutions for the general diffusion of knowledge. In proportion as the structure of a government gives force to public opinion, it is essential that public opinion should be enlightened.'"

Until the Revolution (1776) American science was but English science transplanted, and it looked to the Royal Society of London as its censor and patron. Winthrop, Franklin, and Rittenhouse were, more or less, English astronomers. Franklin was the sturdiest American of the three. As early as 1743 he suggested the formation of the American Philosophical Society of Philadelphia. John Adams founded the American Academy of Arts and Sciences in Boston in 1780. These two societies, together with Harvard College (founded in 1636), Yale College (1701), the University of Virginia (founded by Jefferson in 1825), and the United States Military Academy at West Point (1801), were the chief foci from which the light of learning spread. Other colleges were formed or forming all over the Eastern and Middle States during the early years of the century.

The leading school of pure science was the Military Academy at West Point, and it continued to hold this place until the civil war of 1861. From its corps of professors and students it gave two chiefs to the United States Coast Survey; and the Army, particularly the Corps of Engineers, provided many observers to that scientific establishment,

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[1] Printed in Science, June 18, 1897.

besides furnishing a large number of professors and teachers of science to the colleges of the country. The observatory of the academy was founded by Bartlett in 1841, and much work was done there, only a small part of which is published. The Coast Survey was a school of practice for army officers, and their experience was utilized in numerous boundary surveys during the period 1830–1850. Col. J. D. Graham, for example, was astronomer of the survey of the boundary between Texas and the United States in 1839–40; commissioner of the Northeast boundary survey, 1840–1843; astronomer of the Northwest boundary survey, 1843–1847; of the boundary between the United States and Canada, 1848–1850; of the survey of the boundary between Pennsylvania and Virginia, 1849–50; of the boundary survey between Mexico and the United States, 1850–51. The names of Bonneville, Talcott, Cram, Emory, and other army officers are familiar in this connection, and their work was generally of a high order. It was in such service that Talcott invented or reinvented the zenith telescope, now universally employed for all delicate determinations of latitude. The mechanical tact of Americans has served astronomy well. The sextant was invented by Thomas Godfray, of Philadelphia, in 1730, a year before Hadley brought forward his proposal for such an instrument.[1] The chronograph of the Bonds, the zenith telescope of Talcott, and the break-circuit chronometer of Winlock are universally used to-day. The diffraction gratings of Rutherfurd were the best to be had in the world till they were replaced by those of Rowland. The use of a telescope as a collimator was first proposed by Rittenhouse. The pioneer opticians of the United States were Holcomb (1826), Fitz (1846 or earlier), Clark (1845), Spencer (1851). Only the Clarks have a world-wide reputation. Würdemann, instrument maker to the United States Coast Survey (1834), had a decided influence on observers and instrument makers throughout the United States, as he introduced extreme German methods and models among us, where extreme English methods had previously prevailed. The system of rectangular land surveys, which proved to be so convenient for the public lands east of the Rocky Mountains, was devised and executed by Mansfield, a graduate of the Military Academy.

The list of army officers who became distinguished in civil life as professors in the colleges of the country is a very long one. Courtenay (class of 1821 at West Point) was professor of mathematics at the University of Pennsylvania, 1834–1836; at the University of Virginia, 1842–43, and was the author of admirable text-books. Norton (class of 1831) became professor at New Haven, and wrote a very useful text-book of astronomy in 1839; and the list could be much extended. The excellent training in mathematics at West Point (chiefly in French

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[1] In 1700 Sir Isaac Newton sent drawings and descriptions of a reflecting sextant to Hadley for his advice. At Hadley's death these were found among his papers. Hadley's device (1731) was undoubtedly derived from Newton's MSS. The Royal Society of London granted £200 to Godfray for his invention, which his brother, Captain Godfray, had previously put into practical use in the West Indies.

methods) early made itself felt throughout the whole country. The mathematical text-books of Peirce, of Harvard, and of Chauvenet, of the Naval Academy, brought the latest learning of Europe to American students. Mitchell (class of 1829 at West Point) was the only graduate who became a professional astronomer (1842–1861). His direct service to practical observing astronomy is small, but his lectures (1842–1848), the conduct of the Cincinnati observatory (1845–1859), and his publication of the Sidereal Messenger (1846–1848), together with his popular books, excited an intense and widespread public interest in the science, and indirectly led to the foundation of many observatories. He was early concerned in the matter of using the electric current for longitude determinations, and his apparatus was only displaced because of the superior excellence of the chronograph devised by the Bonds. His work was done under immense disadvantages, in a new community (Ohio), but the endowment of astronomical research in America owes a large debt to his energy and efforts.

The Navy and the United States Naval Academy (founded by Bancroft in 1845, at the suggestion of Chauvenet) were very active in astronomical work. Chauvenet (Yale College, 1840) published a text-book of trigonometry in 1850, which had an important share in directing attention to rigid, elegant, and general methods of research. His astronomy (1863) is a handbook for all students. Walker, Gilliss, Coffin, Hubbard, Ferguson, Keith, Yarnall, Winlock, Maury, Wilkes, were all connected with the Navy, more or less intimately. Walker's career was especially brilliant; he graduated at Harvard College in 1825, and established the observatory of the Philadelphia High School in 1840. He was the leading spirit in the United States Naval Observatory at Washington (1845–1847) and introduced modern methods into its practice at the beginning. From the observatory he went to the Coast Survey to take charge of its longitude operations, and he continued to direct and expand this department until his death, in 1853. To him, more than to any single person, is due the idea of the telegraphic method ("the American method") of determining differences of longitude. His assistant in this work was Gould, who succeeded to the charge of it in 1853. His researches extended to the field of mathematical astronomy also, and his theory of the planet Neptune (then newly discovered) marks an important step forward. His investigations and those of Peirce were conducted in concert and attracted general and deserved attention.

The exploring expedition of Wilkes required corresponding observations to be made in America, and during the period 1838–1842 William Bond, at Dorchester, and Lieutenant Gilliss, at Washington, maintained such a series with infinite assiduity and with success. The results of Gilliss' astronomical expedition to the southern hemisphere (Chile, 1849–1852) were most creditable to him and to the Navy, though his immediate object—the determination of the solar parallax—was not attained.

The Coast Survey began its work in 1817 under Hassler, a professor from West Point, who impressed upon the establishment a thoroughly scientific direction. Bache, his successor (a grandson of Benjamin Franklin), was a graduate of West Point in the class of 1825, and took charge of the Survey in 1843. He is the true father of the institution, and gave it the practical efficiency and high standard which characterized its work. He called around him the flower of the Army and Navy, and was ably seconded by the permanent corps of civilian assistants—Walker, Saxton, Gould, Dean, Blunt, Pourtales, Boutelle, Hilgard, Schott, Goodfellow, Cutts, Davidson, and others.

Silliman's (and Dana's) American Journal of Science had been founded at New Haven in 1818, and served as a medium of communication among scientific men. A great step forward was made in the establishment of the Astronomical Journal by Dr. Gould on his return from Europe at the close of 1849.[1] Silliman's Journal was chiefly concerned in the nonmathematical sciences, though it has always contained valuable papers on mathematics, astronomy, and physics, especially from the observers of Yale College—Olmsted, Herrick, Bradley, Norton, Newton, Lyman, and others. In Mason, who died in 1840 at the age of 21, the country lost a practical astronomer of the highest promise.[2] Gould's Journal was an organ devoted to a special science. It not only gave a convenient means of prompt publication, but it immediately quickened research and helped to enforce standards already established and to form new ones. The Astronomical Notices of Brünnow (1858–1862) might have been an exceedingly useful journal with an editor who was willing to give more attention to details, but, in spite of Brünnow's charming personality and great ability, it had comparatively little influence on the progress of the science.

The translation of the Mécanique Céleste of Laplace by Nathaniel Bowditch, the supercargo of a Boston ship (1815–1817), marks the beginning of an independent mathematical school in America. The first volume of the translation appeared in 1829. At that time there were not more than two or three persons in the country who could read it critically. The works of the great mathematicians and astronomers of France and Germany—Laplace, Lagrange, Legendre, Olbers, Gauss, W. Struve, Bessel—were almost entirely unknown.

Bowditch's translation of the Mécanique Céleste, and, still more, his extended commentary, brought this monumental work to the attention of students and within their grasp. His Practical Navigator[3] contained

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[1] The Astronomische Nachrichten had been founded in Altona, by Schumacher, in 1821.

[2] See International Review, Vol. X, page 585.

[3] First edition, 1802. Sumner's method in navigation (1843)—a very original and valuable contribution from a Boston sea captain—and Maury's Wind and Current Charts, begun in 1844, are two other notable contributions from a young country to an art as old as commerce.

the latest and best methods for determining the position of a ship at sea, expressed in simple rules. American navigators had no superiors in the first half of this century. Nantucket whalers covered the Pacific, Salem ships swarmed in the Indies, and the clipper ships made passages round the Horn to San Francisco, which are a wonder to-day. Part of their success is due to the bold enterprise of their captains (who were said to carry deck loads of studding-sail booms to replace those carried away!), but an important part depended on their skill as observers with the sextant. One of the sister ships to the one of which Bowditch was supercargo was visited at Genoa by a European astronomer of note (Baron de Zach), who found that the latest methods of working lunar distances to determine the longitude were known to all on board, sailors as well as officers. His bewilderment reached its climax when the navigator called the negro cook from the galley and bade him expound the methods of determining the longitude to the distinguished visitor.

On Bowditch's own ship there was "a crew of twelve men, every one of whom could take and work a lunar observation as well, for all practical purposes, as Sir Isaac Newton himself." Such crews were only to be found on American ships in the palmy days of democracy. All were cousins or neighbors and each had a "venture" in the voyage. But these anecdotes may serve as illustrations of the intellectual awakening which came about as soon as our young country was relieved from the pressure of the two wars of 1776 and 1812. An early visitor, Baron Hyde de Neuville (1805) felt "an unknown something in the air," "a new wind blowing." This new spirit, born of freedom, entered first into practical life, as was but natural; science next felt its impulse, and, last of all, literature was born. Emerson hailed it (in 1837) "as the sign of an indestructible instinct." "Perhaps the time has already come," he says, "when the sluggard intellect of this country will look from under its iron lids and fill the postponed expectation of the world with something better than the exertions of mechanical skill. Our day of dependence, our long apprenticeship to the learning of other lands, draws to a close. The millions that around us are rushing into life can not always be fed with the sere remains of foreign harvests."

Benjamin Peirce, a graduate of Harvard in the class of 1829, had been concerned with the translation of the Mécanique Céleste, and was early familiar with the best mathematical thought of Europe. He became professor in Harvard College in 1833, and, after the death of Bowditch in 1838, he was easily the first mathematical astronomer in the country. His instruction was precisely fitted to develop superior intelligences, and this was his prime usefulness. Just such a man was needed at that time. Besides his theoretical researches on the orbits of the planets (specially Uranus and Neptune) and of the moon, his study of the theory of perturbations, and his works on pure mathematics and mechanics, he concerned himself with questions of practical astronomy, although the observations upon which he depended were the work of others. He was the consulting astronomer of the American Ephemeris and Nautical Almanac from its foundation in 1849, and its plans were shaped by him to an important degree. His relative, Lieutenant Davis, United States Navy (the translator of Gauss's Theoria Motus Corporum Cœlestium (1857)), was placed in charge of the Ephemeris, and the members of its staff—Runkle, Ferrel, Wright, Newcomb, Winlock, and others—most effectively spread its exact methods by example and precept. Professor Peirce undertook the calculations relating to the sun, Mars, and Uranus in the early volumes of the Ephemeris. As a compliment to her sex, Miss Maria Mitchell was charged with those of Venus; Mercury was computed by Winlock, Jupiter by Kendall, Saturn by Downes, Neptune by Sears Walker.

The Smithsonian Institution was founded in 1846, and Joseph Henry was called from Princeton College to direct it. There never was a wiser choice. His term of service (1846–1878) was so long that his ideals became firmly fixed within the establishment and were impressed upon his contemporaries and upon a host of younger men. The interests of astronomy were served by the encouragement of original research through subsidies and otherwise, by the purchase of instruments for scientific expeditions, by the free exchange of scientific books between America and Europe, and by the publication of the results of recondite investigations. It is by these and like services that the Institution is known and valued among the wide community of scientific men throughout the world.

But this enumeration of specific benefits does not convey an adequate idea of the immense influence exercised by the Institution upon the scientific ideals of the country. It was of the first importance that the beginnings of independent investigation among Americans should be directed toward right ends and by high and unselfish aims. In the formation of a scientific and, as it were, a moral standard a few names will ever be remembered among us, and no one will stand higher than that of Henry. His wise, broad, and generous policy and his high personal ideals were of immense service to his colleagues and to the country.

The establishment of a National Observatory in Washington was proposed by John Quincy Adams in 1825, but it was not until 1844 that the United States Naval Observatory was built by Lieutenant Gilliss, of the Navy, from plans which he had prepared. By what seems to have been an injustice Gilliss was not appointed to be its first director.[1] This place fell to Lieut. M. F. Maury. Gilliss had been on detached service for some years, and a rigid construction of rules required that he should be sent to sea, and not remain to launch the institution which he had built and equipped.

The first corps of observers at Washington (1845) contained men of

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[1] He was, however, director during the years 1861-1865.

first-class ability—Walker, Hubbard, Coffin. Gilliss's work as astronomer to Wilkes Exploring Expedition (1838–1842) at his little observatory on Capitol Hill had shown him to be one of the best of observers, as well as one of the most assiduous. His study and experience in planning and building the Naval Observatory had broadened his mind. To the men just named, with Peirce, Gould, and Chauvenet, and to their coadjutors and pupils, we owe the introduction of the methods of Gauss, Bessel, and Struve into the United States, and it is for this reason that American astronomy is the child of German and not of English science.

The most natural evolution might seem to have been for Americans to follow the English practice of Maskelyne and Pond. But the break caused by the War of Independence, by the War of 1812, and by the years necessary for our youthful governments to consolidate (1776–1836) allowed our young men of science to make a perfectly unbiased choice of masters. The elder Bond (William Cranch Bond, born 1789, director of Harvard College Observatory, 1840–1859) was one of the older school and received his impetus from British sources during a visit to England in 1815.

In estimating the place of the elder Bond among scientific men it is necessary to take into account the circumstances which surrounded him. He was born in the first year of the French Revolution (1789); he was absolutely self-taught; practically no astronomical work was done in America before 1838. When Admiral Wilkes was seeking for coadjutors to prosecute observations in the United States during the absence of his exploring expedition he was indeed fortunate in finding two such men as Bond and Gilliss. Their assiduity was beyond praise and it led each of them to important duties. Bond became the founder and director of the Observatory of Harvard College, while Gilliss is the father of the United States Naval Observatory at Washington, as well as that of Santiago de Chile, the oldest observatory in South America. Cambridge, though the seat of the most ancient university in America, was but a village in 1839. The college could afford no salary to Bond, but only the distinction of a title, "Astronomical Observer to the University," and the occupancy of the Dana house, in which his first observatory was established. His work there, as elsewhere, was well and faithfully done, and it led the college authorities to employ him as the astronomer of the splendid observatory which was opened for work in 1847. At that time the two largest telescopes in the world were those of the Imperial Observatory of Russia (Poulkova) and its companion at Cambridge. Each of these instruments has a long and honorable history. Their work has been very different. Who shall say that one has surpassed the other? We owe to Bond and his son the discovery of an eighth satellite to Saturn, of the dusky ring to that planet, the introduction of stellar photography, the invention of the chronograph by which the electric current is employed in the registry of observations, the conduct of several chronometric expeditions between Liverpool and Boston to determine the trans-Atlantic longitude, and a host of minor discoveries and observations.

Gilliss visited France for study in 1835, before he took up his duties at Washington. The text-books of Bond and Gilliss were the Astronomies of Vince (1797–1808) and of Pearson (1824–1829). The younger Bond (George Phillips Bond, born 1825, Harvard College 1844, director of the Harvard College Observatory 1859–1865) and his contemporaries, on the other hand, were firmly grounded in the German methods, then, as now, the most philosophical and thorough.

It was not until 1850, or later, that it was indispensable for an American astronomer to read the German language and to make use of the memoirs of Bessel, Encke, and Struve and the text-books of Sawitsch and Brünnow.[1] This general acquaintance with the German language and methods came nearly a generation later in England. The traditions of Piazzi and Oriani came to America with the Jesuit Fathers of Georgetown College (1844), of whom Secchi and Sestini are the best known.

The dates of the foundation of a few observatories of the United States may be set down here. Those utilized for the observation of the transit of Venus in 1769 were temporary stations merely. The first college observatory was that of Chapel Hill, N. C. (1831); Williams College followed (1836); Hudson Observatory (Ohio) (1838); the Philadelphia High School (1840); the Dana House Observatory of Harvard College (1840); West Point (1841); the United States Naval Observatory (1844); the Georgetown College Observatory (1844); the Cincinnati Observatory (1845); the new observatory of Harvard College (1846); the private observatory of Dr. Lewis M. Rutherfurd in New York City (1848); the observatory at Ann Arbor (1854); the Dudley Observatory at Albany (1856); and that of Hamilton College (1856).

These dates and the summary history just given will serve to indicate the situation of astronomy in the United States during the first half of the present century. A little attention to the dates will enable the reader to place an individual or an institution on its proper background. It must constantly be kept in mind that the whole country was very young and that public interest in astronomical matters was neither educated nor very general. The data here set down will have a distinct value as a contribution to the history of astronomy in America. The developments of later years have been so amazing that we forget that the first working observatories were founded so late as 1845.

American science is scarcely more than half a century old. The day will soon come—it is now here—when we shall look back with wonder and gratitude to ask who were the men who laid the wide and deep foundations which already maintain so noble an edifice.

MOUNT HAMILTON, CAL., *April, 1897.*

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[1] Dr. Bowditch learned to read German in 1818, at the age of 45.

THE EVOLUTION OF SATELLITES.[1]

By G. H. Darwin.

I.

The Atlantic Monthly for October, 1897, contains an interesting paper by Mr. See on "Recent discoveries respecting the origin of the universe." In the present article I propose to explain, in greater detail than the necessary limitations of space permitted him, the theory which forms the point of departure for his speculations. Although the natural sequence is thus inverted, it may be hoped that the postponement of explanation to application will be condoned. In any case, this article owes its origin to the former one, and it might not otherwise have been justifiable to expound a theory which was laid before the scientific world some fifteen years ago in the pages of the Philosophical Transactions of the Royal Society.[2]

After the explanation of this theory I have added some comments on Mr. See's views.

II.

If familiarity does not always breed contempt, yet at least it generally breeds indifference. This is the case with most of us in regard to the rise and fall of the tide by the seashore, and so the problem as to whether the tide will serve conveniently to allow the children to dig in the sand or search for seaweed looms larger than that presented by the gigantic forces which now produce only these somewhat insignificant pulsations of the sea. Yet the tides should call forth in us a deeper interest—I might almost say an emotion—for, as I shall show,

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[1] Reprinted from the Atlantic Monthly of April, 1898, by permission of the publishers. This article forms a portion of Mr. Darwin's forthcoming work On Tides.

[2] It was very natural that Mr. See should find in certain tidal investigations which I undertook for Lord Kelvin the source of my papers, but as a fact the subject was brought before me in a somewhat different manner. Some unpublished experiments on the viscosity of pitch induced me to extend Lord Kelvin's beautiful investigation of the strain of an elastic sphere to the tidal distortion of a viscous planet. This naturally led to the consideration of the tides of an ocean lying on such a planet, which forms the subject of certain paragraphs now incorporated in Thomson and Tait's Natural Philosophy.

they are the feeble residue of influences which have probably exercised a predominant control over the history of the earth and the moon since an indeterminate but remote epoch in the past, and will continue that control into the distant future.

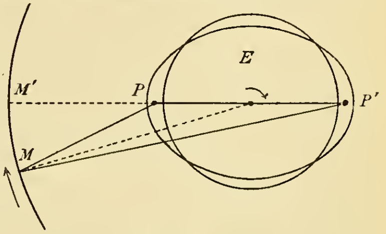
Newton was the first to prove that the tides are caused by the attractions of the moon and the sun. It would need much space to explain fully the manner in which those attractions operate, yet it is possible to give in a few words a rough sketch of the mode in which the tide-generating forces arise. It will suffice for this purpose to confine our attention to the more important of the two bodies, the moon, since the action of the sun will then follow by parity of reasoning. According to the law of universal gravitation, the moon attracts matter which stands near to her more strongly than that which is more remote. It follows that the attraction on the ocean, at the side of the earth which is nearest to the moon must be greater than that exercised on the solid earth itself. Hence there is a tendency for the sea to depart from its natural spherical shape, and to bulge outward toward the moon. So far the matter is simple, but it is perplexing to many that the moon should apparently repel the water lying on the farther side of the earth. This action, however, is not due to any ideal repulsion from the moon, but results from the fact that on the farther side the moon must attract the solid earth more strongly than she does the water. On the nearer side the moon pulls the water away from the earth, and on the farther side she pulls the earth away from the water, thus producing an apparent repulsion of the water to an extent equal to the attraction on the other side. In this way there arises a tendency for the ocean to bulge equally toward and away from the moon, and to assume an egg-like shape, with the length of the egg pointed toward the moon.

If the whole planet were fluid, instead of being partly fluid and partly solid, the same tendency would still exist, but the tide-generating force would have the whole mass of the planet as its field of operation, instead of merely the superficial ocean. The fact that the earth, the moon, and the planets are all nearly spherical proves that in early times they were molten and plastic, and that they assumed their present round shape under the influence of gravitation. When the material of which any planet is formed was semiliquid through heat, its satellites, or at any rate the sun, must have produced tidal oscillations in the molten rock, just as the sun and the moon now raise tides in our oceans.

Molten rock and molten iron are rather sticky or viscous substances, and any movement which agitates them must be subject to much friction. Even water, which is a very good lubricant, is not entirely free from friction, and so our present oceanic tides must be influenced by fluid friction, although to a far less extent than the molten solid just referred to. Now, all moving systems which are subject to friction gradually come to rest. A train will run a long way when the steam is turned off, but it stops at last, and a fly wheel will continue to spin for only a limited time. This general law renders it certain that the friction of the tide, whether it consists in the swaying of molten lava or of an ocean, must be stopping the rotation of the planet, or at any rate stopping the motion of the system in some way.

It is the friction upon its bearings which brings a fly wheel to rest; but as the earth has no bearings, it is not easy to see how the friction of the tidal wave, whether corporeal or oceanic, can tend to stop its rate of rotation. The result must clearly be brought about, in some way, by the interaction between the moon and the earth. Action and reaction must be equal and opposite, and if we are correct in supposing that the friction of the tides is stopping the earth's rotation, there must be a reaction upon the moon tending to hurry her onward. To give a homely illustration of the effects of reaction, I may recall to mind how a man riding a high bicycle, on applying the brake too suddenly, was shot over the handles. The desired action was to stop the front wheel, but this could not be done without a reaction on the rider, which sometimes led to unpleasant consequences.

The general conclusion as to the action and reaction due to tidal friction is of so vague a character that it is desirable to consider in detail how they operate. The circle in the figure is supposed to represent the undisturbed shape of the planet, which rotates in the direction of the curved arrow. A portion of the orbit of the satellite is indicated by part of a larger circle, and the direction of its motion is shown by an arrow. I will first suppose that the water lying on the planet, or the molten rock of which it is formed, is a perfect lubricant, devoid of friction; and that at the moment represented in the figure the satellite is at M′. The fluid will then be distorted by the tidal force until it assumes the egg-like shape marked by the ellipse, projecting on both sides beyond the circle. When there is no friction, the long axis of the egg is always directed straight toward the satellite M′, and the fluid maintains a continuous rhythmical movement, so that as the planet rotates and the satellite revolves, it always preserves the same shape and attitude toward the satellite.



But when, as in reality, the fluid is subject to friction, it gets belated in its rhythmical rise and fall, and the protuberance is carried onward by the rotation of the planet beyond its proper place. In order to make the same figure serve for this condition of affairs, I set the satellite backward to M; for this amounts to just the same thing, and is less confusing than redrawing the protuberance in its more advanced position. The planet then constantly maintains this shape and attitude with regard to the satellite, and the interaction between the two will be the same as though the planet were solid, but continually altering its shape.

We have now to examine what effects must follow from the attraction of the satellite on an egg-shaped planet, when the two bodies constantly maintain the same attitude relatively to each other. It will make the matter somewhat easier of comprehension if we replace the tidal protuberances by two particles of equal masses, one at P, and the other at P′. If the masses of these particles be properly chosen, so as to represent the amount of matter in the protuberances, the proposed change will make no material difference in the result.

The gravitational attraction of the satellite is greater on bodies which are near than on those which are far, and accordingly it attracts the particle P more strongly than the particle P′. It is obvious from the figure that the pull on P must tend to stop the planet's rotation, while the pull on P′ must tend to accelerate it. If a man pushes equally on the two pedals of a bicycle, the crank has no tendency to turn; and besides, there are dead points in the revolution of the crank where pushing and pulling have no effect. So also in the astronomical problem, if the two attractions were exactly equal, or if the protuberances were at a dead point, there would be no resultant effect on the rotation of the planet. But it is obvious that here the retarding pull is stronger than the accelerating pull, and that the set of the protuberances is such that we have passed the dead point. It follows from this that the primary effect of fluid friction is to throw the tidal protuberance forward, and the secondary effect is to retard the planet's rotation.

Action and reaction are equal and opposite, and if the satellite pulls at the protuberances, they pull in return at the satellite. The figure shows that the attraction of the protuberance P tends in some measure to hurry the satellite onward in its orbit, while that of P′ tends to retard it. But the attraction of P is stronger than that of P′, and therefore the resultant of the two is a force tending to carry the satellite forward more rapidly in the direction of the arrow. When the satellite is thus influenced, it must move in a spiral curve, ever increasing its distance from the planet. Besides this, the satellite has a longer path to travel in its circuit, and takes longer to get round the planet, than was the case before tidal friction began to operate.[1]

Now, let us apply these ideas to the case of the earth and the moon. A man standing on the planet, as it rotates, is carried past places where the fluid is deeper and shallower alternately; at the deep places he says that it is high tide, and at the shallow places that it is low tide. In the figure it is high tide when the observer is carried past P. Now,

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[1] It is somewhat paradoxical that the effect of attempting to hurry the satellite is to make it actually move slower. It would be useless to attempt an explanation of this in such an article as the present one, but the converse case, where a retarding force acts on the body, may be more intelligible. When a meteorite rushes through the atmosphere it moves faster and faster, because it gains more velocity by the direct action of the earth's gravity on it than it loses by the friction of the air. And yet it is the friction of the air which allows gravity to have play; so that we have the paradox of friction accelerating the motion.

it was pointed out that when there is no fluid friction we must put the moon at M′, but when there is friction she must be at M. Accordingly, if there is no friction it is high tide when the moon is over the observer's head, but when there is fluid friction the moon has passed his zenith before he reaches high tide. Hence he would remark that fluid friction retards the time of high water.[1]

A day is the name for the time in which the earth rotates once, and a month for the time in which the moon revolves once. Then, since tidal friction retards the earth's rotation and the moon's revolution, we may state that both the day and the month are being lengthened, and that these results follow from the retardation in the time of high tide. It must also be noted that the spiral in which the moon moves is an increasing one, so that her distance from the earth increases. These are absolutely certain and inevitable results of the mechanical interaction of the two bodies.

At the present time the rates of increase of the day and month are excessively small, so that it has not been found possible to determine them with any approach to accuracy. It may be well to notice in passing that if the rate of change of either element were determinable that of the other would be deducible by calculation.

The extreme slowness of the changes within historical times is established by the records in early Greek and Assyrian history of eclipses of the sun which occurred on certain days and at certain places. Notwithstanding the changes in the calendar, it is possible to identify the day according to our modern reckoning, and the identification of the place presents no difficulty. Astronomy affords the means of calculating the exact time and place of the occurrence of an eclipse even three thousand years ago, on the supposition that the earth spun at the same rate then as now, and that the complex laws governing the moon's motion are unchanged. The particular eclipse referred to in history is known, but any considerable change in the earth's rotation and in the moon's motion would have shifted the position of visibility on the earth from the situation to which modern computation would assign it. Most astronomical observations would be worthless if the exact time of the occurrence were uncertain, but in the case of eclipses the place of observation affords just that element of precision which is otherwise wanting. As, then, the situations of the ancient eclipses agree fairly well with modern computations, we are sure that there has been no great change within the last three thousand years either in the earth's rotation or in the moon's motion. There is, however, a small outstanding discrepancy which indicates that there has been some change. But the exact amount involves elements of uncertainty, because our knowl-

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[1] This must not be considered as a fair statement of the case when the oceans are as shallow as in actuality. The reader must accept the assurance that the friction of the tides of shallow seas also causes retardation of the planet's rotation, although in a somewhat different manner from that explained above.

edge of the laws of the moon's motion is not yet quite accurate enough for the absolutely perfect calculation of eclipses which occurred many centuries ago. In this way it is known that within historical times the retardation of the earth's rotation and the recession of the moon have been, at any rate, very slight.

It does not follow from this that the changes have always been equally slow, and indeed it may be shown by mathematical arguments that the efficiency of tidal friction increases with enormous rapidity as we bring the tide raising satellite nearer to the planet. The law of tidal friction is that it varies according to the inverse sixth power of the distance; so that with the moon at half her present distance, the rate of retardation of the earth's rotation would be sixty-four times as great as it now is. Thus, although the action may now be almost insensibly slow, yet it must have proceeded with much greater rapidity when the moon was nearer to us.

There are many problems in which it would be very difficult to follow the changes in the system according to the times of their occurrence, but where it is possible to banish time, and to trace the changes themselves in due order, without reference to time. In the sphere of common life, we know the succession of stations which a train must pass between New York and Boston, although we may have no time-table. This is the case with our astronomical problem; for although we have no time-table, yet the sequence of the changes in the system may be traced accurately.

Let us then banish time, and look forward to the ultimate outcome of the tidal interaction of the moon and the earth. The day and the month are now lengthening at relative rates which are calculable, although the absolute rates in time are unknown. It will suffice for a general comprehension of the problem to know that the present rate of increase of the day is much more rapid than that of the month, and that this will hold good in the future. Thus, the number of rotations of the earth in the interval comprised in one revolution of the moon diminishes; or, in other words, the number of days in the month diminishes, although the length of each day increases so rapidly that the month itself is longer than at present. For example, when the day shall be equal in length to two of our actual days, the month may be as long as thirty-seven of our days, and then the earth will spin round only about eighteen times in the month.

This gradual change in the day and the month proceeds continuously until the duration of a rotation of the earth is prolonged to fifty-five of our present days. At the same time, the month, or the time of a revolution of the moon around the earth, will also occupy fifty-five of our days. Since the month here means the period of the return of the moon to the same place among the stars, and since the day is to be estimated in the same way, the moon must then always face the same part of the earth's surface, and the two bodies must move as though they were united by a bar. The outcome of the lunar tidal friction will therefore be that the moon and the earth will go round as though locked together in a period of fifty-five of our present days, with day and month identical in length.

Now, looking backward in time, we find the day and the month shortening, but the day changing more rapidly than the month. The earth was therefore able to complete more revolutions in the month, although that month was itself shorter than it is now. We get back, in fact, to a time when there were twenty-nine rotations of the earth in the time of the moon's revolution, instead of twenty-seven and one-third, as at present. This epoch is a sort of crisis in the history of the moon and the earth, for it may be proved that there never could have been more than twenty-nine days in the month. Earlier than this epoch, the days were fewer than twenty-nine; and later, fewer also. Although measured in years this epoch in the earth's history must be very remote, yet when we contemplate the whole series of changes it must be considered as a comparatively recent event. In a sense, indeed, we may be said to have passed recently through the middle stage of our history.

Now, pursuing the series of changes farther back than the epoch when there was the maximum number of days in the month, we find the earth still rotating faster and faster and the moon drawing nearer and nearer to the earth and revolving in shorter and shorter periods. But a change has supervened, so that the rate at which the month is shortening is more rapid than the rate of change in the day. Consequently, the moon now gains, as it were, on the earth, which can not get round so frequently in the month as it did before. In other words, the number of days in the month declines from the maximum of twenty-nine, and is finally reduced to one. When there is only one day in the month the earth and the moon go round at the same rate, so that the moon always looks at the same side of the earth, and as far as concerns the motion they might be fastened together by iron bands.

This is the same conclusion at which we arrived with respect to the remote future. But the two cases differ widely; for whereas in the future the period of the common rotation will be fifty-five of our present days, in the past we find the two bodies going round each other in between three and five of our present hours. A satellite revolving round the earth in so short a period must almost touch the earth's surface. The system is therefore traced until the moon nearly touches the earth, and the two go round each other like a single solid body in about three to five hours.

The series of changes has been traced forward and backward from the present time, but it will make the whole process more intelligible, and the opportunity will be afforded for certain further considerations, if I sketch the history again in the form of a continuous narrative.

Let us imagine a planet attended by a satellite which revolves in a circular orbit so as nearly to touch its surface and continuously to face the same side of the planet. If now, for some cause, the satellite's month comes to differ very slightly from the planet's day, the satellite will no longer continuously face the same side of the planet, but will pass over every part of the planet's equator in turn. This is the condition necessary for the generation of tidal oscillations in the planet, and as the molten lava, of which we suppose the planet to be formed, is a sticky or viscous fluid, the tides must be subject to friction. Tidal friction will then begin to do its work, but the result will be very diferent according as the satellite revolves a little faster or a little slower than the planet. If it revolves a little faster, so that the month is shorter than the day, we have a condition not contemplated in the figure above. It is easy to see, however, that as the satellite is always leaving the planet behind it, the apex of the tidal protuberance must be directed to a point behind the satellite in its orbit. In this case the rotation of the planet must be accelerated by the tidal friction, and the satellite must be drawn inward toward the planet, into which it must ultimately fall. In the application of this theory to the earth and the moon, it is obvious that the very existence of the moon negatives the hypothesis that the initial month was even infinitesimally shorter than the day. We must then suppose that the moon revolved a little more slowly than the earth rotated. In this case the tidal friction would retard the earth's rotation, and force the moon to recede from the earth, and so perform her orbit more slowly. Accordingly, the primitive day and the primitive month lengthen, but the month increases much more rapidly than the day, so that the number of days in the month becomes greater. This proceeds until that number reaches a maximum, which in the case of our planet is about twenty-nine.

After the epoch of maximum number of days in the month, the rate of change in the length of the day becomes less rapid than that in the length of the month; and although both periods increase, the number of days in the month begins to diminish. The series of changes then proceeds until the two periods come again to an identity, when we have the earth and the moon, as they were at the beginning, revolving in the same period, with the moon always facing the same side of the planet. But in her final condition the moon will be a long way off from the earth, instead of being quite close to it.

Although the initial and final states resemble each other, yet they differ in one respect, which is of much importance; for in the initial condition the motion is unstable, while finally it is stable. The meaning of this is that if the moon were even infinitesimally disturbed from the initial mode of motion, she would necessarily either fall into the planet or recede therefrom, and it would be impossible for her to continue to move in that neighborhood. She is unstable in the same sense in which an egg balanced on its point is unstable, the smallest mote of dust will upset it, and practically it can not stay in that position. But the final condition resembles the case of an egg lying on its side, which only rocks a little when we disturb it. So if the moon were slightly disturbed from her final condition, she would continue to describe very nearly the same path round the earth, and would not assume some entirely new form of orbit.

It is by methods of rigorous argument that the moon is traced back to the initial unstable condition when she revolved close to the earth. But the argument here breaks down, and calculation is incompetent to tell us what occurred before, and how she attained that unstable mode of motion. We can only speculate as to the preceding history, but there is some basis for our speculation, for I say that if a planet, such as the earth, made each rotation in a period of three hours, it would very nearly fly to pieces. The attraction of gravity would be barely strong enough to hold it together, just as the cohesive strength of iron is insufficient to hold a fly wheel together if it is spun too fast. There is, of course, an important distinction between the case of the ruptured fly wheel and the supposed break-up of the earth, for when the fly wheel breaks the pieces are hurled apart as soon as the force of cohesion fails, whereas when a planet breaks up, through too rapid rotation, gravity must continue to hold the pieces together after they have ceased to form parts of a single body.

Hence, we have grounds for conjecturing that the moon is composed of fragments of the primitive planet which we now call the earth, which detached themselves when the planet spun very swiftly, and afterwards became consolidated. It surpasses the powers of mathematical calculation to trace the details of the process of this rupture and subsequent consolidation, but we can hardly doubt that the system would pass through a period of turbulence before order was reestablished in the formation of a satellite.

I have said that rapid rotation was probably the cause of the birth of the moon, but this statement needs qualification. There are certain considerations which prevent us from ascertaining the common period of revolution of the moon and the earth with accuracy. It may lie between three and five hours. I think that such a speed might not, perhaps, be quite sufficient to cause the planet to break up. Is it possible, then, to suggest any other cause which might have cooperated with the tendency to instability of the rotating planet? I think that there is such a cause, and though we are here dealing with guesswork, I will hazard the suggestion.

The primitive planet, before the birth of the moon, was rotating rapidly with reference to the sun, and it must, therefore, have been agitated by tidal oscillations due to the sun's attraction. Now, the magnitude of these solar tides is much influenced by the speed of rotation of the planet, and mathematical reasoning appears to show that when the day was about three or four hours in length the oscillations must have been very great, although the sun stood no nearer to the earth then than it does now. May we not conjecture that the oscillation of the molten planet became so violent that, in cooperation with the rapid rotation, it shook the planet to pieces, detaching huge fragments, which ultimately were consolidated into the moon? There is nothing to tell us whether this theory affords the true explanation of the birth of the moon, and I say that it is only a wild speculation, incapable of verification.

But the truth or falsity of this speculation does not militate against the acceptance of the general theory of tidal friction, which, standing on the firm basis of mechanical necessity, throws much light on the history of the earth and the moon, and correlates the lengths of our present day and month.

I have said above that the sequence of events has been stated without reference to the scale of time. It is of the utmost importance, however, to gain some idea of the time requisite for all the changes in the system. If millions of millions of years were necessary, the applicability of the theory to the moon and the earth would have to be rejected, because it is known from other lines of argument that there is not an unlimited bank of time on which to draw. The uncertainty as to the duration of the solar system is wide, yet we are sure that it has not existed for an almost infinite past.

Now, although the actual time scale is indeterminate, it is possible to find the minimum time adequate for the transformation of the moon's orbit from its supposed initial condition to its present shape. It may be proved, in fact, that if tidal friction had always operated under the conditions most favorable for producing rapid change, the sequence of events from the beginning until to-day would have occupied a period of between fifty and sixty millions of years. The actual period, of course, must have been much greater. Various lines of argument as to the age of the solar system have led to results which differ widely among themselves, yet I can not think that the applicability of the theory of tidal friction is negatived by the magnitude of the period demanded. It may be that science will have to reject the theory in its full extent, but it seems improbable that the ultimate verdict will be adverse to the preponderating influence of the tide on the evolution of our planet.

III.

If this history be true of the earth and the moon, it should throw light on many peculiarities of the solar system. In the first place, a corresponding series of changes must have taken place in the moon herself. Once on a time she must have been molten, and the great extinct volcanoes revealed by the telescope are evidences of her primitive heat. The molten mass must have been semifluid, and the earth must have raised in it enormous tides of molten lava. Doubtless the moon once rotated rapidly on her axis, and the frictional resistance to her tides must have impeded her rotation. She rotated then more and more slowly until the tide solidified, and thenceforward and to the present day she has shown the same face to the earth. Helmholtz was, I believe, among the first in modern times to suggest this as the explanation of the fact that the moon always shows us the same face.[1] Our theory, then, receives a striking confirmation from the moon; for, having ceased to rotate relatively to us, she has actually advanced to that condition which may be foreseen as the fate of the earth.

Thus far I have referred in only one passage to the influence of solar tides, but these are of considerable importance, being large enough to cause the conspicuous phenomena of spring and neap tides. Now, while the moon is retarding the earth's rotation, the sun is doing so also. But these solar tides react only on the earth's motion around the sun, leaving the moon's motion around the earth unaffected. It might perhaps be expected that parallel changes in the earth's orbit would have proceeded step by step, and that the earth might be traced to an origin close to the sun. But the smallness of the earth's mass compared with that of the sun here prohibits the application of the theory of tidal friction, and it is improbable that our year is now longer, from this cause at any rate, by more than a few seconds than it was at the very birth of the solar system.

Although the solar tides can have had no perceptible influence upon the earth's movement in its orbit, they will have affected the rotation of the earth to a considerable extent. Let us imagine ourselves transported to the indefinite future, when the moon and the earth shall be revolving together in fifty-five of our days. The lunar tide in the earth will then be unchanging, just as the earth tide in the moon is now fixed; but the earth will be rotating with reference to the sun, and, if there are unfrozen oceans, its rotation will still be subject to retardation in consequence of the solar tidal friction. The day will then become longer than the month, which for a very long time will continue to occupy about fifty-five of our present days. It is known that there are neither oceans nor atmosphere on the moon; but if there were, she would have been subject to solar tidal friction, and would have undergone a parallel series of changes.

Up to recent times it might have been asserted plausibly that the absence of any such mode of motion in the solar system afforded a reason for rejecting the actual efficiency of tidal friction in celestial evolution. But in 1877 Prof. Asaph Hall discovered in the system of the planet Mars a case of the kind of motion which we have reason to foresee as the future fate of the earth and the moon; for he found two satellites, one of which has a month shorter than the planet's day.

In his paper on the discovery of these satellites Professor Hall gives

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[1] Kant, in the middle of the last century, drew attention to the importance of tidal friction in celestial dynamics; but as he did not clothe his argument in mathematical form, he was unable to deduce most of the results which are explained in this paper.

an interesting account of what had been conjectured, partly in jest and partly in earnest, as to the existence of satellites attending that planet. He quotes Kepler as writing, after the discovery of the satellites of Jupiter, "I am so far from disbelieving the existence of the four circumjovial planets" (that is, satellites) "that I long for a telescope to anticipate you, if possible, in discovering two around Mars, six or eight around Saturn, as the proportion seems to require, and perhaps one each around Mercury and Venus." This was, of course, serious, although based on fantastic considerations. At a later date Swift poured contempt on men of science in his account of the inhabitants of Laputa, whom he describes as dexterous enough on a piece of paper and in the management of the rule, the pencil, and the dividers, but as a clumsy, awkward, and unhandy people, and perplexed in their conceptions upon all subjects except mathematics and music. He writes, however, of the Laputans, "They have likewise discovered two lesser stars or satellites which revolve about Mars, whereof the innermost is distant from the center of the primary exactly three of his diameters, and the outermost five." In one of his satires Voltaire also represents an imaginary traveler from Sirius as making a similar discovery.

These curious prognostications were at length verified by Prof. Asaph Hall in the discovery of two satellites, which he named Phobos and Deimos—Fear and Panic, the dogs of war. The period of Deimos is about thirty hours, and that of Phobos about eight hours, while the Martian day is of nearly the same length as our own. The month of the inner minute satellite is thus less than a third of the planet's day; it rises to the Martians in the west, and passes through all its phases in a few hours. Sometimes it must even rise twice in a single Martian night. As we here find an illustration of the condition foreseen for our own planet and satellite, it seems legitimate to suppose that solar tidal friction has slowed down the planet's rotation. The ultimate fate of Phobos must almost certainly be absorption by the planet.

Several of the satellites of Jupiter and Saturn present faint inequalities of coloring, and telescopic examination has led astronomers to believe that they always present the same face to their planets. The theory of tidal friction would certainly lead us to expect that these enormous planets would have worked out the same result for these relatively small satellites that the earth has effected in the moon.

The efficiency of solar tidal friction must be far greater in its action on the planets Mercury and Venus than on the earth. The determination of the periods of rotation of these planets thus becomes a matter of much interest. But the markings on their disks are so obscure that their rates of rotation have remained under discussion for many years. Until recently the prevailing opinion was that in each case the day was of nearly the same length as our own; but a few years ago Schiaparelli, of Milan, an observer endowed with extraordinary acuteness of vision, announced as the result of his observation that both Mercury and Venus rotate only once in their respective years, and that each of them always presents the same face to the sun. These conclusions have recently been confirmed by Mr. Percival Lowell from observations made in Arizona, and are exactly conformable to our theoretical expectation. While it is not easy to see how these astronomers can have been mistaken, yet it is proper to note that others possessing apparently equal advantages have failed to detect the markings on the planets. Accepting, however, this conclusion, we have the planets Mercury and Venus, the satellites of the earth, and Jupiter and Saturn presenting evidence favorable to the theory of tidal friction, while the case of the Martian system is yet more striking as an instance of an advanced stage in evolution.

It would need another article to discuss the various aspects of this theory in relation to the histories of the planets and of their satellites. I may say, however, that it serves in great measure to explain the fact that the earth is tilted over with reference to its orbit round the sun, and that it throws light on the fact that the plane of the moon's orbit is not coincident with that of the earth. The same cause may also be proved to tend toward making the orbit of a satellite eccentric, and it is this effect of tidal friction to which Mr. See has appealed. I shall not here repeat his arguments, but in Section IV I will make some comments on his theories.

With respect to the efficacy of tidal friction as a factor in the evolution of the earth, it is not too much to say that if we postulate a planet consisting partly or wholly of molten lava, and rapidly rotating about an axis at right angles to its orbit around the sun, and if that planet have a single satellite, revolving nearly as rapidly as the planet rotates, then a system will necessarily be evolved in time closely resembling our own.

A theory reposing on true causation, which brings into quantitative correlation the lengths of the present day and month, the obliquity of the ecliptic, and the eccentricity and inclination of the moon's orbit, must, I think, have strong claims to acceptance.

IV.

There are in the heavens many pairs of closely neighboring stars which revolve about each other under the influence of their mutual gravitation. The fact that both members of a pair are visible seems to indicate that they do not differ widely in mass, and it is also a striking peculiarity of these binary systems that the orbit is commonly very eccentric. The distinction is great between our solar system, with its large central mass and infinitesimal planets moving in nearly circular orbits, and these binary systems, and hence there is abundant reason for supposing that the course of evolution has been very different in the two cases.

Mr. See explains the high degree of eccentricity in these binary orbits by the influence of tidal friction. The tide undoubtedly operates under conditions which give it a wide scope, when two large masses are revolving about one another; and tidal friction is the only known cause capable of converting a nearly circular orbit into a very eccentric one. But this does not afford quite sufficient reason for the acceptance of the theory, for the assumption is involved that orbits now very eccentric were formerly nearly circular. Mr. See accordingly also puts forward a theory of the method by which double stars originated, and to this I shall return later.

At first it may not be easy to see how the truth of this theory of the origin of the eccentricity is to be tested; it may be worth while, therefore, to point out the direction which to me, at least, seems the most promising in the search for confirmation or refutation.

It is thought by some spectroscopists that the ages of the stars are already determinable by the nature of their spectra, and although the theories which have been advanced do not meet with universal acceptance, yet they foreshadow views which may some day be universally accepted. It has been plausibly contended that stars which are young in their evolution must consist of incandescent gas, and must therefore have spectra furrowed by bright lines; later in their histories they are supposed to become more condensed and to give continuous spectra. Now, if from theories of this kind we could ascertain the stage of evolution of a binary system, we should be able to form a judgment of the truth of the tidal theory; for the younger systems should present smaller eccentricity or orbit than the older ones, and the periodic times in the young systems should be shorter on the whole than those in the old ones. Delicate spectroscopic measurements make it theoretically possible to determine the relative masses of a binary pair, but hitherto the measurements have been carried to a successful issue in only a very few cases. It is to be expected, however, that the number of known masses will be largely multiplied in the future. A small star must cool more rapidly than a large one, and should present the appearance of greater age. We may hope, then, in time, not only to attain to crucial tests of spectroscopic theories of age, but also to be furnished with the materials for judging of the truth of the tidal theory of evolution of stellar systems.

The second and yet more speculative branch of Mr. See's theory is that which concerns the mode of origin of binary systems. Man must ultimately be brought face to face with the incomprehensibility of the origin of matter and motion, but this consideration will never prevent him from peering into the past to the utmost of his powers. It is certain that the stars are continually undergoing change, and it seems impossible to accept their existence as an ultimate fact not susceptible of explanation. Thus we feel bound to trace their histories back to a past so remote that their preceding course of evolution becomes inscrutable.

The fact that two stars are now found to be revolving about each other leads to the conviction that their relationship is not a casual one, but that they have been connected from an early epoch, which for convenience we may call the origin of the system. It appears almost beyond question that this starting point must have been at a time when the two stars were united in a single rotating mass. As the basis of his explanation of the manner in which a single mass may split into two, Mr. See takes certain theoretical investigations as to the shapes which a mass of gravitating and rotating fluid is capable of maintaining. I will not recapitulate his theories, but I wish to emphasize the uncertainties with which we are here brought face to face.

Many years ago Sir John Herschel drew a number of twin nebulæ as they appear through a powerful telescope. The drawings probably possess the highest degree of accuracy attainable by this method of delineation, and the shapes present evidence confirmatory of Mr. See's theory of the fission of nebulæ. But since Herschel's time it has been discovered that many details, to which our eyes must remain forever blind, are revealed by celestial photography. The photographic film is, in fact, sensitive to those photographic rays which we may call invisible light, and many nebulæ are now found to be hardly recognizable, when photographs of them are compared with drawings. A conspicuous example of this is furnished by the great nebula in Andromeda; for whereas the drawing exhibits a cloud with a few dark streaks in it, the photograph shows a flattened disk surrounding a central condensation; moreover, the disk is seen to be divided into rings, so that the whole system might have been drawn by Laplace to illustrate his celebrated nebular hypothesis of the origin of the solar system.

Photographs, however, do not always aid interpretation, for there are some which serve only to increase the chaos visible with the telescope. We may suspect, in fact, that the complete system of a nebula often contains masses of cool and photographically invisible gas, and in such cases it would seem that the true nature of the whole will be forever concealed from us.

Another group of strange celestial objects is that of the spiral nebulæ, whose forms irresistibly suggest violent whirlpools of incandescent gas. Although in all probability the motion of the gas is very rapid, yet no change of form has been detected. We are here reminded of a rapid stream rushing past a post, where the form of the surface remains constant whilst the water itself is in rapid movement, and it seems reasonable to suppose that in these nebulæ it is only the lines of flow of the gas which are visible. Again, there are other cases in which the telescopic view may be almost deceptive in its physical suggestions. Thus, the Dumb-Bell Nebula (27 Messier Vulpeculæ), as viewed telescopically, might be taken as a good illustration of a nebula almost ready to split into two stars. If this were so, the rotation would be about an axis at right angles to the length of the nebula.

But a photograph of this object shows that the system really consists of a luminous globe surrounded by a thick and less luminous ring, and that the opacity of the sides of the ring takes a bite, as it were, out of each side of the disk, and so gives it the apparent form of a dumb bell. In this case the rotation must be about an axis at right angles to the ring, and therefore along the length of the dumb-bell.[1]

From what I have said it must be obvious that the subject is surrounded by difficulties and uncertainties. Mr. See is therefore to be congratulated on having laid before the world an hypothesis which appears to explain the facts as far as we know them. The subject is necessarily a speculative one, and we must look forward to future spectroscopic and photographic researches for the confirmation or refutation of his theories.

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[1] It is proper to state that Mr. See does not refer to this nebula as confirmatory of his theory.

ELECTRICAL ADVANCE IN THE PAST TEN YEARS.[1]

By Elihu Thomson.

The variety of service to which electricity has already contributed can not fail to impress every one. We communicate by telegraph over the land and under the seas. Our electric signals may bring into almost instantaneous action the machinery of a modern fire department, or simply note the flight of time in a clock system.

The stock ticker records the changing values; the police telegraph anticipates the criminal in his flight. The same agent, which in the telephone carries the inexpressibly feeble overtones of the voice to great distances with the speed of light, conveys energy equal to thousands of horsepower and distributes it for lighting our streets, our factories, our shops, and our homes. The electric search light may rival the sun in the brilliancy of its beams, or a tiny incandescent lamp may not equal one-tenth of a candle light. Electric motors ventilate our buildings, drive our machinery, and run our elevators. We travel swiftly on electric cars, propelled by current from wires which also furnish the means for lighting and heating the cars.

In mills and factories the power is carried to the different buildings oftentimes by electricity, and electric railways distribute the raw materials and deliver the finished products for shipment. In mines, coal is cut and transported to the pit's mouth by electric power, and the same power works the ventilating fans. Metals are welded and forged by electric heat, and some are smelted from their ores by electricity. The electrolytic bath either refines crude metals or coats and protects them, as in nickel plating.

Power is now transmitted over great distances by wire, and the energy of waterfalls is made available for innumerable uses far from its source. New and valuable products arise from the high heats of the electric furnace. That paragon of nature, the diamond, can now be fashioned in an electric crucible from plain black soot.

Nearly all the larger electrical work has been the result of the past twenty years of progress. Before directing our attention to the great work of advance in recent years, we may recall some of the more

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[1] Reprinted from The Forum, January, 1898, by permission of The Forum Publishing Company.

notable events in applied electricity which occurred in the late seventies and immediately thereafter. It was then that the commercial beginnings of arc lighting took place. The incandescent lamp or burner and the electric main for supply of current soon followed. The telephone itself, considered as a practically working speech transmitter, belongs to the period referred to. Its birth was first made known at the Centennial Exhibition in Philadelphia in 1876. The almost ideal power of electric motors was applied in a limited way. The fruits of the pioneer work of that period have ripened in recent years. The experimental work in electric railways, begun in the early eighties, resulted in the enormous electric traction development of to-day, when almost all our street railways are operated by electricity.

In this connection, it is very interesting to note that, at a convention of street-railway men held so recently as 1887, a discussion of electric traction as applied to horse railways was vigorously criticised as a waste of time which, it was urged, might have been better applied to practical subjects, instead of to such a fanciful or theoretical one. In fact, the contention was that the care and feeding of horses should take precedence of so unimportant a subject as electricity, considered as the motive power of a car system. Yet, in less than five years from that time, the horse question had everywhere become an exploded one. A convention of the same association in the present year assumed in its papers and discussions the universal application of electricity to street car propulsion. Had the advent of the electric railway marked the only great advance within the ten years just past, that period might still be well characterized as one of great technical progress in electricity. Had the decadence of horse traction occupied a much longer period than it did, the advance could justly be deemed rapid.

Many of the largest street-railway systems were transformed in a few months' or in a year's time. The advance still goes on by extensions of existing lines, by the establishment of additional interurban and suburban traffic facilities, by the increase of equipment, and by the steady improvement in the quality of that equipment.

To appreciate the real progress of the past ten years demands a wider view. We must consider many other branches of electrical work besides electric traction.

What, then, was the condition of the art ten years ago? By comparison with the present status, we may, generally speaking, get some idea of the growth during the past ten years. In thus looking backward, we find that there were telephone-exchange systems, but practically no long-distance extensions. We also find that in the larger cities and towns arc-lighting circuits for street and store service were in use, employing only the constant-current or series system; while today arc lights of various kinds are worked on several plans, or with different kinds of current supply. There were, in addition, a moderate number of electric stations, supplying incandescent lamps, together with a few electric motors. Here and there, isolated lighting plants in mills and other large buildings were in operation; but the alternating current, so large a factor in electrical enterprises nowadays, had scarcely become known or applied practically. There were perhaps not more than twenty trolley cars in actual service in 1887, and these were of doubtful success. There were no regularly constituted electric railways worthy of the name. The telephone and electric-lighting wires were largely overhead, and frequently the construction was of the most imperfect and temporary character. Among some notable exceptions stood prominent the Edison three-wire underground system, which had the elements of permanence. The extensive underground mains and wires in use in cities to-day testify to the great progress which has taken place in the means of distributing electric energy. They represent a very large investment of capital, but they also confer that reliability and permanence which was before lacking.

Within the past eight or ten years much has been done in the perfection of thoroughly practical forms of meters and other instruments for the measurement of electric forces and quantities. While such work resembles in its delicacy that demanded by watch mechanism, on the other hand the large station dynamos are examples of the heaviest machine construction. Some of them demand steel castings more than 30,000 pounds in weight. Indeed, in the same electric factory we may find watchmaking tools turning out the fine pieces of electric meters, which may not weigh more than a few grains, and electric cranes handling masses of metal of many tons—parts of the larger dynamos under construction. A few years ago a dynamo was large if it demanded 100 or 200 horsepower to drive it, while now such machines are diminutive when compared with those of 2,000 horsepower commonly constructed. Dynamos are in use at Niagara of 5,000 horsepower capacity. A single one of these would supply more than 50,000 incandescent lights such as are ordinarily used, or would give motion to 500 trolley cars.

The period since 1887 has been marked by great extension in electric lighting by both arc and incandescent lamps. Prior to that year only the largest cities, broadly speaking, possessed any electric-lighting service. Now, however, even the smaller towns have their electric stations, their arc lamps for street lighting, and the smaller incandescents for general use. The same wires or mains frequently supply both kinds of lights. The incandescent lamps in use in the United States are numbered by millions, and there are several hundred thousand arc lamps beside. There are in operation nearly 3,000 electric-light-supply stations, and these, together with isolated electric plants, represent a capital of about $500,000,000.

One of the chief factors in this great extension has been the application of alternating electric currents, or currents of wave-like nature, reversing their direction many times in each second. The direct or continuous current had previously occupied the field alone. But the alternating current possessed the advantage of readily permitting the sending out over a long distance of a high-pressure current with but little loss and by means of comparatively small and inexpensive lines. This current, relatively dangerous, could then be exchanged for a safe low-pressure current on the house mains for working the lights.

The device which makes the exchange is called a transformer. It is in reality a modified induction coil—a simple structure of copper wire, sheet iron, and insulating materials, with no moving parts to need attention or to get out of order. The properties and use of the transformer in an alternating-current system were comparatively unknown before 1887, but since that time it has played a part in electric development the importance of which can not easily be overestimated. It has been, furthermore, brought to a high degree of perfection by the persistent and painstaking effort of numerous workers.

In transforming a current of high pressure to one of lower pressure, or the reverse, only a very slight loss of power or energy is suffered. On a large scale, this loss is barely 3 per cent of the energy of the transformed current. The larger sizes of transformers now in use have capacities equivalent to considerably over 1,000 horsepower. Some of these structures are employed at Niagara and others at Buffalo.

As in the case of the apparatus just mentioned, the effort spent in the perfection of the huge dynamo-electric generators used in lighting and power stations has resulted in machines so perfect as to leave but little chance of further increase of effectiveness. They waste only a small percentage in converting mechanical power into electrical energy, and run for years with but little attention or need of repairs.

Along with all this improvement has gone a like betterment in the thousand and one details and minor devices which go to make up an electric system. Both incandescent lamps and arc lamps are not only much improved, but, also, their cost is greatly reduced by the use of special machinery and processes of manufacture. Wires, insulating materials, switches, etc., are all far in advance of what they were a few years ago. Safety is secured by many ingenious devices, and the methods of operating have been made far more effective.

It can not, with truth, be said that electrical arts or industries are still in their infancy, if we are to judge by the perfection of electric manufactures. It has been many years since electrical work could in any sense be regarded as empirical, except by the uninformed. Few of the older arts have possessed or do possess the means for such exact measurement or research; few, indeed, are based upon simpler laws of action. Had it been otherwise, the rapid progress which has characterized the past twenty years would have been impossible.

A striking feature of electrical energy is that it may be readily applied to widely varied work.

A few instances of this may be given: The large electric-lighting stations in our cities not only supply from the same mains, at the same time, electric current which lights both arc and incandescent lamps indiscriminately, but the system carries also a large load in electric motors employed for such service as running elevators, driving ventilating fans, supplying power for pumping, and driving machinery in shops of all kinds. The same mains supply current for charging storage batteries, for heating metals for welding or working, for warming rooms by electric heat, or for cooking by electric heaters. The physician or surgeon draws upon the same system for current for the treatment of disease, for galvano cautery, for electrolysis, and for the generation of Röntgen rays.

Another example is found in a modern war ship, which may embody an electric plant for working its incandescent lights. The same machinery supplies the search light, which is essentially an arc light of great power. There are also electric cranes and hoists, turret-turning and gun-training apparatus, motors for ventilating fans or for forced draft in the boiler furnaces, all depending on the same supply.

Perhaps, however, no better example of the varied application of electric energy exists than at Niagara. Certainly no grander exemplification of the way in which electric forces may be called into play, to replace other and unlike agencies, can be cited. Here at Niagara we may forcibly realize the importance of cheap and unfailing power developed from water in its fall. We find the power of huge water wheels delivered to the massive dynamos for giving out electric energy. This energy is variously employed. The electric lighting of the city of Niagara and surroundings and the electric railways naturally depend upon the water power. Besides these, which may be termed the ordinary applications of electricity, there are clustered at Niagara a number of unique industrial establishments, the importance of which will undoubtedly increase rapidly. In the carborundum factory we find huge furnaces heated by the passage of electric current, and attaining temperatures far beyond those of the ordinary combustion of fuel. These electric furnaces produce carborundum, a new abrasive nearly as hard as the diamond, which is a combination of carbon and silicon, unknown before the electric furnace gave it birth. Sand and coke are the raw substances for its production, and these are acted upon by the excessively high heat necessary to form the new product, already in extensive use for grinding hard materials.

The metal aluminum, which not many years ago cost $2 an ounce, is now produced on a large scale at Niagara, and sold at a price which makes it, bulk for bulk, cheaper than brass. Here, again, electricity is the agent; but in this case its power of electrolyzing or breaking up strong chemical unions is employed. Great vats containing fused compounds, such as fluorides of certain metals in which the aluminum ore is dissolved, are arranged so that a powerful electric current sent through the fused mass separates out the metallic aluminum. The metal is then collected and cast into ingots for shipment, or is rolled into sheets or rods, or drawn into tubes or wire.

Works for the production of metallic sodium and other metals similarly depend upon the decompositions effected by the electric current.

Solutions of ordinary salt or brine are electrolyzed on a large scale in extensive works established for the purpose. The chlorine of the salt is used with lime to make bleaching powder, so important an agent in paper making and textile industries. The sodium of the decomposed salt goes to form caustic soda, which is the base of soap, and is employed in many manufactures.

The very high temperature which exists in an electric arc, or between the carbons of an arc lamp, has in recent years found application in the manufacture of another important compound, which was formerly but slightly known as a chemical difficult to prepare. Carbide of calcium is the compound referred to, and large works for its production exist at Niagara. Here again, as in the carborundum works, raw materials of the simplest and cheapest kind are acted upon in what may be termed an electric-arc furnace. Coke, or carbon, and lime are mixed and charged into a furnace in which an enormous electric arc is kept going. The carbons in an ordinary arc lamp are usually less than one-half inch in diameter, or they have a section of less than one-fifth of a square inch, while in the carbide of calcium furnaces the section of the carbon may be upward of half a square foot. The light of the enormous arc produced is, however, smothered, so to speak, in powdered lime and coke—the raw materials mentioned above. The importance of carbide of calcium rests in the fact that, by contact with water, it produces acetylene gas. The illuminating power of this gas, when burned, is its remarkable property.

It will be seen that the metallurgical and chemical developments at Niagara are the direct outgrowth of electrical utilization of water power. With many water powers, however, the outlet for the application of the electrical energy exists many miles away from the place at which the water power is found. Even at Niagara there is an example of the beginning of long-distance transmission, by a high-pressure line extending to Buffalo and delivering electric energy to an electric station there.

In this case "step-up" transformers, as they are called, are employed at the Niagara power plant to step up or raise the electrical pressure or potential from that given by the dynamos to that required for the transmission to Buffalo. This transformation is from about 2,500 up to 10,000 volts. At the Buffalo end the reverse process is carried on by "step-down" transformers, and the energy is delivered to the trolley lines at about 500 volts. At Buffalo the "step down" in pressure is accompanied by a conversion of the alternating current into a continuous current in one direction, or a direct current. It would require too much space to explain the meaning of these technical designations of the kinds of current; and they are referred to here solely to illustrate the extreme flexibility of electrical work as lately developed. The whole Niagara plant has grown into existence within the past five years, and as a consequence of the technical advances within the period of the past ten years. There are, however, in active operation, besides the Niagara power plant, several other water-power transmissions, some of them far exceeding in distance that between Niagara and Buffalo, and some in which the amount of power conveyed, as well as the pressure of the current used upon the line, is much greater than is yet to be found at Niagara.

Electric transmissions are in particular favor in regions where the cost of steam power, owing to dear fuel, is a stimulus to the utilization of water powers which already exist or which are capable of development. It is not surprising, therefore, that the far West should furnish some of the most notable examples.

No limit can as yet be definitely set as to the distance which can be covered in an electrical transmission. The higher the voltage or electrical pressure which may be found practicable, the greater the distance which may exist between the transmitting and receiving machinery. So, also, the higher the cost of fuel in a locality, the greater the distance over which it is feasible to make the transmission. It may be said that at present the range of distances is between 30 and 100 miles.

It is interesting to compare the conditions in long-distance telephony with those of a power transmission. With the former an exceedingly feeble current is sent out; and though only a small percentage reach the receiving telephone, still it may be sufficient to produce the sounds of the voice with such distinctness as to enable them to be recognized. To secure this result the long-distance telephone lines are made of heavy copper wires, and the longer the distance to be covered the thicker must be the line wire.

The cost of the copper in the line becomes very heavy for great distances, over 1,000,000 pounds of copper being required for a single circuit from Boston to Chicago. In a power transmission, on the other hand, the currents are of great pressure and sometimes represent thousands of horsepower, and it is essential that in the transmission not more than a certain percentage of the energy be lost. Thus in some cases a 20 per cent loss would be too much to allow and in others a 25 to 30 per cent loss might not be inordinate.

In this case, again, heavy copper wires are used for the lines, insulated as well as possible; and the cost of the copper for obtaining conducting power sufficient to prevent undue loss, other things being equal, sets the limit of distance. In the telephonic transmission the percentage of loss is not important, provided the characteristics which represent speech in the receiver are not lost, while in the power transmission the percentage of loss is vital, as the object of the plant is simply to transmit energy under economical conditions.

In the large work of to-day the general practice is to build the dynamo directly upon the shaft of the engine which drives it or upon the water-wheel shaft, as the case may be. This avoids loss in belts or other forms of gearing.

Indeed, these large machines for producing electricity from power have in late years reached a perfection far beyond that of the steam engine itself. The steam engine, in fact, has been forced to a higher development in response to the demands of the electrical engineer.

No service demanded of electricity has taxed the resources of electrical and mechanical engineering so much as that of railway work. The electric motors must work under the most varied conditions, stand the hardest service, and run in the presence of water, slush, mud, and dirt. They must run at all speeds, and be, so to speak, mechanically and electrically invulnerable.

In the same way the engines and dynamos, together with other parts of the system, must be of the most robust character. Inventive and engineering talent was required to provide for the new and urgent conditions. In the early days of electric-railway work the prospect was not always bright or promising; and one of the chief setbacks was the enormous wear and tear of certain parts of the machinery—chiefly those known as commutators. This difficulty was solved by the invention and application of carbon blocks in place of metal "brushes" used with the offending commutators. The "carbon brush" thenceforth became almost as essential to the railway-motor machinery as the carbon stick is to an arc lamp and did more than anything else to change the prospect of failure into inevitable success. These technical matters make a long story which would be out of place here. They are merely alluded to for the purpose of emphasizing the fact that pioneer work in these advance movements has not been without its trials and that a glimpse behind the scenes might have disclosed at times a none too rosy aspect.

In spite of the difficulties to be overcome, the electric railway has, in a very few years, put an end to horse traction on city railways, the cruelties of which—not always to be avoided, perhaps—remain now only as a fading memory. Electric traction has given greater speed, better cars, which are lighted and heated electrically, and a resulting cleanliness and comfort not otherwise attainable.

But facts so evident call for no comment. Meanwhile it has been shown that single cars may be propelled at high train speeds with comparative safety. Even 60 miles an hour has been exceeded. It has also been proved, by the construction of several huge electric locomotives for the Baltimore and Ohio Railroad, expressly for tunnel service, that such electric machinery can haul the heaviest train loads and can more than equal in power locomotives worked by steam.

Electric traction is now generally regarded as the ideal method for elevated railways and as practically indispensable to underground or tunnel traffic in cities. A new underground road is now being constructed in London, which, when completed, will be a splendid example of the latest methods of the distribution and application of electricity to train service.

Indeed, electricity seems destined at no distant day to play an important part in revolutionizing passenger traffic between large centers of population. The facility with which electric service may be superposed on ordinary steam roads will greatly further this development. The work with the third-rail system, undertaken by one of our prominent railway organizations, has abundantly demonstrated the practicability of such superposition. The future will witness the growing substitution of either single motor cars or two or three coupled cars for long, heavy trains drawn by locomotives, and a more frequent service will result. There is an eventual possibility of higher average speeds, since stops will not consume much time, and the time required to recover the speed after a stop will be much less than at present. There will be no annoyance due to escaping steam, smoke, or cinders; no sparks to cause forest or brush fires; no stopping to change engines nor for taking up water or coal. The locomotive will be supplanted by electric motors driving the axles of the cars as in street-railway service. Cheap fuel can be used to generate the power in the electric stations and the best conditions for economy of fuel maintained. Where water power is available within 30 or 40 miles it may be transmitted to the railway line and used instead of power obtained from coal.

The present outlook, then, is most encouraging, so far as electric-railway extension is concerned; and, just as in electric lighting the foundations of present practice were laid fifteen or twenty years ago, so it may be said that the foundations of the railway practice of twenty years hence can be found in the work of to-day. In fact, great enterprises are now being planned and undertaken which will mean much to the future of the electric railroad.

Besides the work which is thus going on, and in which the electrical forces may be publicly witnessed in full operation, there are now other forms of industry in which the part played by electricity is not distinctly evident. Thus enormous amounts of crude copper are annually refined by electrolysis, with the result that a nearly pure metal is obtained, where formerly impurities lessened the value of the copper. Not only is this the case, but in some instances amounts of the precious metals, gold and silver, have been separated in the refining sufficient to pay the cost of the process. This work is all comparatively recent in its development.

The heating power of the electric current is now also utilized in a variety of ways. Electric welding machinery has been put into service either for accomplishing results which were not possible to be obtained before its development, or to improve the work and lessen the cost.

Here again the part played by the electric current sometimes leaves little indication in the finished product. As an instance, it may be mentioned that the solid rubber tires of carriages are held in place by wires welded into bands by electric welding machines built for the purpose. Similarly, carriage hardware, axles, wheel tires, parts of bicycles, parts of machines, tools, and innumerable other articles are made. Metal bands for pails, tubs, and barrels are now largely made by electric welding. Even steel tubes for bicycles and vehicle frames are formed by the same means; and new industries are based upon it. A curious and instructive instance of the adaptability of electric methods to new uses is seen in the annealing of armor for war vessels. A serious difficulty arose in the application of armor plate having a hardened face and known as harveyized armor. It was found almost impossible to drill or cut holes in the face—an operation frequently rendered necessary in the construction of an armored ship. Various methods of annealing or softening the spots where the plate was to be drilled were tried, with indifferent results. The construction of some of our battle ships was delayed on account of this difficulty. It was overcome by a special electric method, with appropriate machinery somewhat resembling that used for electric welding, capable of heating to redness the desired spots in the face of the heaviest armor plate, and of automatically reducing the heat of the spots so as to anneal them. The heating and control of the cooling is perfectly brought about, in spite of the enormous mass of cold metal surrounding the portion under treatment. Together with electric welding work, this armor annealing is a striking instance of the extreme localization of heating in metal, possible only by the delivery of electrical energy and its conversion into heat at the desired point. In electric welding, the electric heat is sharply localized at the weld itself, softening and uniting the pieces; the operation being under the same perfect control as in the armor annealing referred to. Before the advent of the electric process, iron and platinum only were known as the weldable metals. Afterwards, all metals became capable of welding under electric treatment.

Electric heating is now also applied in many other ways. There are to be found electric cooking utensils, electric sad irons, electric soldering tools, and similar devices; while many street cars are provided with electric heat in winter.

The chief bar to the employment of electricity for general heating lies in the fact that in using coal to develop power by steam engines—which power in turn is sent out as electrical energy—85 to 90 per cent of the heating value is lost in the boiler and engine. This loss is so great as to make it undesirable, from the standpoint of cost, to reconvert the electric energy back into low heat, which can be more economically obtained from the direct use of burning fuel.

Besides the technical and industrial development which has gone on so rapidly in the electrical field, the science of electricity, considered simply as a department of physics, has advanced very rapidly. It is but a few years since the late Dr. Hertz gave to the world his experimental demonstrations of the fact that light of all kinds and from all sources is really an electrical phenomenon, differing from ordinary alternate-current waves only in the rate or frequency of vibrations. We produce electric waves of about one hundred vibrations per second for alternating-current work; and in the waves of red light the rapidity is as high as four hundred millions of millions of vibrations per second. Hertz and others used waves of some millions per second, and showed how they could transmit signals to distances without wires; these invisible waves being recognized by suitable receivers. The recently announced Marconi wireless telegraph is much the same thing, with certain improvements in detail. It may be of limited use, but will not replace telegraph lines and submarine cables.

Our store of scientific facts has been greatly increased and our electrical theories have been made more precise in late years; while the enormous industrial expansion has furnished the means for researches otherwise difficult to carry on.

Hardly had the work of Hertz and others who followed in his footsteps been assimilated, before the truly remarkable, not to say astounding, discovery by Professor Röntgen of what he called the X-rays produced a profound impression not only in the scientific world, but upon the general public as well. The interest of the scientist had a different basis from the popular one of disclosure of objects hidden in opaque structures; for he saw in the discovery a new weapon of attack upon the secrets of nature. This weapon has already proved to be so serviceable as to show that his anticipations were not unfounded. The X-rays, which became at once indispensable to surgery, are the result of electrical actions in certain vacuum bulbs; and the discovery is properly an electrical one.

The rapid extension of electrical application must naturally be of importance in social and economic questions. Changes in our methods give rise to extension of possibilities in the lives of our people. The effect of electric railways alone must be an important study for the economist and social scientist. Fresh questions of law and equity arise out of the conflict of the new and the old.

The increasing importance of electrical work has had a powerful effect upon the development of many other arts. It has stimulated workers in other than electrical fields to the attainment of higher standards, to the improvement of materials and construction, to the bringing out of new products and processes in response to the demands of electric engineering. As a consequence, we have better and more economical engines, improved methods in the casting, forging, and working of iron, brass, copper, and other metals. We have new alloys with special properties, special grades and kinds of steel, improved methods of working such substances as glass, porcelain, rubber, asbestos, mica, etc. In street railways we have far better rails and rolling stock.

No existing industry employs a greater range of materials, from the rarest to the most common, than does electric work. None requires or employs such a variety, in character, kind, and quality, of materials, or of treatment of them, to supply daily needs. Nature has been ransacked to discover whatever may possess qualities desirable in electrical construction; and the resources of art and ingenuity have been called to supply whatever might be lacking.

This material progress, coupled with the civilizing and educative influences naturally accompanying it, as well as the many other advances in the application of science to the needs of mankind, will ever remain the crowning glory of the latter half of the nineteenth century.

THE X-RAYS.

By W. C. RÖNTGEN.

I.—UPON A NEW KIND OF RAYS.[1]

1. If the discharge of a great Ruhmkorff induction coil be passed through a Hittorf vacuum tube, or a Lenard's, Crooke's, or similar apparatus containing a sufficiently high vacuum, then, the tube being covered with a close layer of thin black pasteboard and the room darkened, a paper screen covered on one side with barium-platinum cyanide and brought near the apparatus will be seen to glow brightly and fluoresce at each discharge whichever side of the screen is toward the vacum tube. The fluorescence is visible even when the screen is removed to a distance of 2 meters from the apparatus.

The observer may easily satisfy himself that the cause of the fluorescence is to be found at the vacuum tube and at no other part of the electrical circuit.

2. It is thus apparent that there is here an agency which is able to pass through the black pasteboard impenetrable to visible or ultra violet rays from the sun or the electric arc, and having passed through is capable of exciting a lively fluorescence, and it is natural to inquire whether other substances can be thus penetrated.

It is found that all substances transmit this agency, but in very different degree. I will mention some examples. Paper is very transmissible.[2]

I observed fluorescence very distinctly behind a bound book of about 1,000 pages. The ink presented no appreciable obstacle. Similarly fluorescence was seen behind a double whist pack. A single card held between the fluorescent screen and the apparatus produced no visible effect. A single sheet of tin foil, too, produces hardly any obstacle, and it is only when several sheets are superposed that their shadow appears

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[1] Translation of paper by Professor Röntgen in the Sitzungsber. der Würzburger Physik-Medic. Gesellsch. Jahrg. 1895, as reprinted in Annalen der Physik und Chemie (Neue Folge) 64, 1, 1898.

[2] By the transmissibility of a substance I designate the ratio between the brightness of a fluorescent screen held behind the body and that which the screen would have under the same conditions in the absence of the interposed substance.

distinctly on the screen. Thick wooden blocks are transmissible. Slabs of pine 2 or 3 centimeters thick absorb only very little. A plate of aluminum about 15 millimeters thick diminished the effect very considerably, but did not cause the fluorescence to entirely disappear. Blocks of hard rubber several centimeters thick still transmitted the rays.[2]

Glass plates of equal thickness behave very differently according to whether they contain lead (flint glass) or not. The first class are much less transmissible than the second.

If the hand is held between the vacuum tube and the screen, the dark shadow of the bones is seen upon the much lighter shadow outline of the hand. Water, carbon bisulphide, and various other liquids investigated proved very transmissible. I could not find that hydrogen was more transmissible than air. The fluoresence was visible behind plates of copper, silver, lead, gold, and platinum, when the thickness of the plate was not too great. Platinum 0.2 millimeter thick is still transmissible, and silver and copper plates may be still thicker. Lead 1.5 millimeters thick is practically impenetrable, and advantage was frequently taken of this characteristic. A wooden stick of 20 millimeters square cross section, having one side covered with white lead, behaved differently when interposed between the vacuum tube and the screen according as the X-rays traversed the block parallel to the painted side or were compelled to pass through it. In the first case there was no effect appreciable, while in the second a dark shadow was thrown on the screen. Salts of the metals, whether solid or in solution, are to be ranged in almost the same order as the metals themselves for transmissibility.

3. These observations and others lead to the conclusion that the transmissibility of equal thicknesses of different substances depends on their density. At least no other characteristic exerts so marked an influence as this.

The following experiment shows, however, that the density is not the sole factor. I compared the transmissibility of nearly equally thick plates of glass, aluminum, calcspar, and quartz. The density of these substances is substantially the same, and yet it was quite evident that the calcspar was considerably less transmissible than the others, which are about alike in this respect.

4. All bodies became less transmissible with increasing thickness. For the purpose of finding a relation between transmissibility and thickness I have made photographic exposures, in which the photographic plate was partly covered with a layer of tin foil consisting of a progressively increasing number of sheets. I shall make a photometric measurement when I am in possession of a suitable photometer.

5. Sheets were rolled from platinum, lead, zinc, and aluminum of such

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[2] For brevity's sake I employ the word "rays," and in distinction from others make use of the expression "X-rays."

thickness that all appeared to be equally transmissible. The following table gives the measured thickness in millimeters, the relative thickness compared with platinum, and the specific gravity:

	Thickness.	Relative thickness.	Specific gravity.
Platinum	0.018	1	21.5
Lead	0.05	3	11.3
Zinc	0.10	6	7.1
Aluminum	3.5	200	2.6

From these values it may be seen that the transmissibility of plates of different metals so chosen that the product of the thickness and density is constant would not be equal. The transmissibility increases much faster than this product falls off.

6. The fluoresence of barium-platinum-cyanide is not the only action by which X-rays may be recognized. It should be remarked that they cause other substances to fluoresce, as for example the photophorescent calcium compounds, uranium glass, common glass, calcspar, rock salt, etc.

It is of particular importance from many points of view that photographic dry plates are sensitive to X-rays. It thus becomes possible to fix many phenomena so that deceptions are more easily avoided; and I have where practicable checked all important observations made with a fluorescent screen by photographic exposures.

It appears questionable whether the chemical action upon the silver salts of the photographic plate is produced directly by the X-rays. It is possible that this action depends upon the fluorescent light which, as is mentioned above, may be excited in the glass plate, or perhaps in the gelatine film. "Films" may indeed be made use of as well as glass plates.

I have not as yet obtained experimental evidence that the X-rays are capable of giving heat. This characteristic might, however, be assumed as present, since in the excitation of fluorescent phenomena the capacity of the energy of the X-rays for transformation is proved, and since it is certain that of the X-rays falling upon a body not all are given up.

The retina of the eye is not sensitive to these rays. Nothing is to be noticed by bringing the eye near the vacuum tube, although according to the preceding observations the media of the eye must be sufficiently transmissible to the rays in question.

7. After I had discovered the transmissibility of various bodies of relatively great thickness I hastened to investigate whether or not the X-rays were refracted in passing through a prism. Experiments with water and carbon bisulphide in mica prisms of 30 degrees refracting angle showed no deviation either when observations were made with the fluorescent screen or with the photographic plate. For comparison, the deviation of light rays was observed under the same conditions. The

refracted portion lay from 10 to 20 centimeters distant from that not refracted. With prisms of hard rubber and aluminum of about 30 degrees refracting angle I obtained exposures on a photographic plate which perhaps indicated a slight refraction. This is, however, very doubtful and the deviation is, if present, so small that the index of refraction for X-rays in these substances can not exceed 1.05. I could not observe with the fluorescent screen any deviation in these cases. Experiments with prisms of the denser metals have so far yielded no certain results on account of the slight transmissibility and the consequent decrease of the intensity of the transmitted ray.

In consideration of these results on the one hand, and on the other of the importance of the question whether or not the X-rays in passing from one medium to another undergo refraction, it is very gratifying that this question may be investigated by other means than by the help of prisms. Finely pulverized bodies in suitable layers allow but little light to pass, in consequence of refraction and reflection. If now the X-rays are transmitted equally well through powder as through the coherent substance, equal masses being presupposed, it is proved that neither refraction nor regular reflection is present in any marked degree. This experiment was performed using finely pulverized rock-salt, finely divided silver, obtained by electrolysis, and the zinc dust so frequently utilized in chemical processes. In no case was any difference in transmissibility between the powder and the coherent substance detected either by the use of the fluorescent screen or the photographic plate.

It follows of course from the results thus obtained that the X-rays can not be concentrated by the use of lenses; and, indeed, a great hard rubber lens and a glass lens actually proved without effect. The shadow of a round rod is darker in the middle than at the edges, while that of a tube which is filled with some substance more transmissible than the material of which the tube is composed is darker at the edges than at the center.

8. The question as to the reflection of X-rays is so far settled by the experiments already described that no marked regular reflection was to be found with any of the substances examined. Other experiments which I will here pass over lead to the same results.

Nevertheless an observation should be mentioned which indicated at first glance an opposite result. A photographic plate shielded from the action of light rays by a black paper was exposed to X-rays so that the glass side was toward the discharge tube. The sensitive film was partially covered with bright plates of platinum, lead, zinc, and aluminum, arranged in a star-shaped figure. Upon development it was observed that the darkening of the film under the platinum, the lead, and especially the zinc, was distinctly greater than in the other parts. No such effect was produced by the aluminum. Thus it seemed as if the three metals mentioned reflected. However, there were other causes to be conceived which might have produced the increased darkening, and in order to be sure I performed a second experiment, interposing a thin sheet of aluminum foil (very transmissible to X-rays, but not to those of the ultraviolet) between the metals and the sensitive film. Since in this case again practically the same result was obtained, the fact of reflection of X-rays by the metals above mentioned is established.

Taking this result together with the observation that powder is as transmissible as coherent substance, and further, that bodies with rough surfaces behave in the transmission of X-rays and also in the experiments just described exactly like polished bodies, the conclusion is reached that there is, as before remarked, no regular reflection, but that the bodies behave toward X-rays in the same manner as a turbid medium with reference to light.

As I have not been able to discover any refraction in the passage from one medium to another, it appears as if the X-rays travel with equal velocity in all bodies, and hence in a medium which is everywhere present and in which the particles of the bodies are embedded. These latter act as a hindrance to the propagation of the X-rays, which is in general greater the greater the density of the body in question.

9. In accordance with this supposition it might be possible that the arrangement of the molecules of the body would exert an influence on its transmissibility, and that, for example, a piece of calcspar would be unequally transmissible for equal thicknesses when the rays passed along or at right angles to the axis. Experiments with calcspar and quartz gave, however, a negative result.

10. It will be recalled that Lenard, in his beautiful experiments on the transmission of the Hittorf cathode rays through thin aluminum foil, obtained the result that these rays are disturbances in the ether, and that they diffuse themselves in all bodies. We may make a similar statement with regard to our rays.

In his last research Lenard has determined the relative absorption of different substances for the cathode rays, and in determining the same for air at atmospheric pressure has given the values 4.10, 3.40, 3.10 as referred to 1 centimeter thickness according to the density of the gas in the discharge tube. Judging from the length of spark observed, I have, in my researches, generally employed tubes of about equal exhaustion and only seldom those of much greater or less density. Using the photometer of L. Weber, the best at my command, I compared the intensity of fluorescence on the screen in two positions distant 100 and 200 millimeters, respectively, from the discharge tube. From the results of these experiments, agreeing well with each other, it appeared that the intensity varies inversely as the square of the distance. Hence the air absorbs a much smaller portion of the X-rays passing through it than of cathode rays. This result is in accord with the observation above mentioned, that it is possible to distinguish fluorescence at 2 meters distance from the discharge tube.

Most other substances are, like the air, more transmissible for X-rays than for the cathode rays.

11. Another very noteworthy difference between the behavior of the cathode rays and the X-rays was exhibited in that I was unable to produce any deviation of the latter by the action of the most powerful magnetic fields. The property of being subject to deviation by magnets is, on the other hand, very characteristic of the cathode rays. Hertz and Lenard have observed various kinds of cathode rays which "are to be distinguished by their differences in their capacities for exciting phosphorescence in their absorptibility and in their deviation by the magnet," but a considerable magnetic deviation was to be observed with all of them, and I do not believe that this characteristic would be given up except for the most urgent reasons.

12. According to the results of experiments particularly directed to discover the source of the X-rays, it is certain that the part of the wall of the discharge tube which most strongly fluoresces is the principal starting point. The X-rays therefore radiate from the place where, according to various observers, the cathode rays meet the glass wall. If one diverts the cathode rays within the tube by a magnet, the source of the X-ray is also seen to change its position so that these radiations still proceed from the end points of the cathode rays. The X-rays being undeviated by magnets can not, however, be simply cathode rays passing unchanged through the glass wall. The greater density of the gas outside the discharge tube can not, according to Lenard, be made answerable for the great difference of the deviation.

I come therefore to the results that the X-rays are not identical with the cathode rays, but that they are excited by the cathode rays in the glass wall of the vacuum tube.

13. This generating action takes place not only in glass, but as I observed it in apparatus with aluminum walls 2 millimeters thick, exists also for this metal. Other substances will be investigated.

14. The warrant for giving the title "rays" to the agent which proceeds from the wall of the discharge tube arose in part from the quite regular formation of shadows appearing when more or less transmissible substances are interposed between the generating apparatus and a phoshorescent screen or photographic plate. I have many times observed and sometimes photographed such shadow forms, in whose production there lies a particular charm. I have, for example, photographs of the shadow of the profile of a door which separates the two rooms, in one of which was the discharge apparatus, in the other the photographic plate; of the shadow of the hand bones; of the shadow of a wooden spool wound with wire; of a set of weights in a box; of a compass in which the magnetic needle is quite inclosed in metal; of a piece of metal which is shown to lack homogeneity by the use of X-rays, etc.

The propagation of the X-rays in right lines is shown by pin-hole photography, which I have been able to do with the discharge apparatus covered with black paper. The picture is weak, but unmistakably correct.

15. I have much sought to obtain interference phenomena with X-rays, but unfortunately—perhaps on account of their slight intensity—without result.

16. Experiments have been begun to see if electrostatic forces can in any way influence X-rays, but these are not yet finished.

17. If the question is asked what the X-rays—which certainly are not cathode rays—really are, one might at first, on account of their lively fluorescent and chemical action, compare them to ultra-violet light. But here one falls upon serious difficulties. Thus, if the X-rays were ultra-violet light, then this light must possess the following characteristics:

(*a*) That in passing from air into water, carbon bisulphide, aluminum, rock salt, glass, zinc, etc., it experiences no notable refraction.

(*b*) That it is not regularly reflected by these substances.

(*c*) That it can not be polarized by the usual materials.

(*d*) That its absorption by substances is influenced by nothing so much as by their density.

In other words, one must assume that these ultra-violet radiations comport themselves quite differently from all previously known infra-red, visible, and ultra-violet rays.

I have not been able to admit this, and have sought some other explanations.

A kind of relation seems to subsist between the new radiation and light radiation, or at least the shadow formation, the fluorescence, and the chemical action, which are common phenomena of these two kinds of radiation, point in this direction. It has been long known that longitudinal as well as transverse vibrations are possible in the ether, and according to various physicists must exist. To be sure, their existence has not, up to the present time, been proved, and hence their characteristics have not thus far been experimentally investigated.

Should not the new radiations be ascribed to longitudinal vibrations in the ether? I may say that in the course of the investigation this hypothesis has impressed itself more and more favorably with me, and I venture to propose it, although well aware that it requires much further examination.

WÜRZBURG, PHYSIK. INSTITUT D. UNIV., *December, 1895.*

II.—UPON A NEW KIND OF RAYS (ABSTRACT).[1]

As my work must be interrupted for several weeks, I take the opportunity of presenting in the following some new results:

18. At the time of my first publication I was aware that the X-rays

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[1] Translation of a portion of the paper by Professor Röntgen in the Sitzungsber. der Würzburger Physik-Medic. Gesellschaft, Jahrg. 1895, as reprinted in Annalen der Physik und Chemie, 64, p. 12, 1898.

have the property of discharging electrified bodies, and I intimated that it was the X-rays and not the cathode rays passing unchanged through the aluminum window of his apparatus which produced the effect described by Lenard on electrified bodies at a distance. I have, however, delayed publication of my experiments until I could present conclusive results.

These can be obtained only when the observations are carried on in a room which is not only completely insulated from the electrostatic forces emanating from the vacuum tube, the conducting wires, the induction apparatus, etc., but is also closed to the air which comes in the neighborhood of the discharge apparatus.

For this purpose I had a box constructed by soldering together zinc sheets, and this box was large enough to contain me and the necessary apparatus, and was air-tight with the exception of an opening which could be closed by a zinc door. The side opposite to the door was mostly lined with lead, and immediately adjacent to the discharge tube an opening 4 centimeters wide was cut in the lead and zinc wall, and its place filled up air-tight with aluminum foil. Through this window passed the X-rays to be investigated. I have with this apparatus verified the following results:

(*a*) Positively or negatively electrified bodies placed in air are discharged when immersed in X-rays, and the action is the more rapid the more intense the radiations. The intensity of the rays is determined by their action upon a fluorescent screen or a photographic plate.

It is in general immaterial whether the electrified substance is a conductor or nonconductor. Thus far I have discovered no difference in the behavior of different bodies relative to the rapidity of their discharge, or between positive or negative charges. These points are, however, open to further investigation.

(*b*) When an electrified conductor is surrounded by a solid insulator, as for example, paraffine, the radiation produces the same effect as would the flashing of the insulating shell by a flame placed in contact with the ground.

(*c*) If this insulator be in its turn closely surrounded by a grounded conductor and both itself and this outer conductor be transmissible to X-rays, the action of the X-radiations upon the inner conductor is unnoticeable with the apparatus at my command.

(*d*) The observations recorded under (*a*), (*b*), and (*c*) indicate that the air through which X-rays pass possesses the property of discharging any electrified bodies with which it comes in contact.

(*e*) If this be indeed the case, and if the air retains for some considerable time this property imparted to it by the X-rays, it must be possible to discharge electrified bodies not themselves under the influence of X-rays by bringing to them air which has been subject to these radiations.

One may satisfy himself in various ways that this is the case. The following, though perhaps not the simplest method, may be mentioned:

I employed a brass tube 3 centimeters wide and 45 centimeters long. At 1 centimeter's distance from one end a portion of the tube was cut away and replaced by a thin sheet of aluminum. At the other end there was introduced a brass ball, which was supported by a metal support, and this end was closed air-tight. Between the brass ball and the closed end of the tube a side tube was soldered in, which was connected with an air-pump. By this means a current of air was made to flow by the brass ball, after having passed the aluminum window. The distance from the ball to the window was 20 centimeters.

I mounted this tube in the zinc box in such a manner that the X-rays entered the tube at right angles to its axis, and the insulated ball lay outside the reach of these rays, in the shadow. The tube and zinc box were placed in contact and the ball was connected with a Hankel electroscope.

It was shown that a charge on the ball, whether positive or negative, was not influenced by X-rays so long as the air remained quiet in the tube, but that a marked diminution of the charge was produced by sucking a strong current of air through. If the ball was kept at constant potential by connecting it with accumulators, and a continuous current of air was kept flowing in the tube, an electrical current was set up just as if the ball was connected with the walls of the tube by a conductor of high resistance. * * *

20. In section 13 of my first article it was stated that the X-rays may be generated not only in glass but in aluminum. In conducting experiments in this direction no solid bodies were found which were not capable of producing X-rays when under the influence of cathode rays. I know no reason to suppose that liquids and gases also do not act similarly.

Different substances, however, possess this property in different degrees. For example, if cathode rays are caused to fall upon a plate of which one-half is composed of platinum foil 0.3 millimeter thick and the other half of aluminum 1 millimeter thick, one may observe in the photographic image taken with the pinhole camera that the platinum foil sends out many more X-rays from the side bombarded by the cathode rays than does the aluminum on the same side. But from the back side of the plate there go out almost no X-rays from the platinum, while the aluminum sends out a relatively large number. These latter rays are generated at the front layers of the aluminum and pass through the plate.

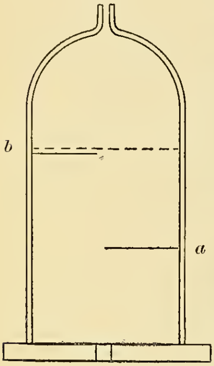
It should be remarked that these observations have a practical significance. For the generation of X-rays of the greatest possible intensity my experience recommends the employment of platinum. I have used for some weeks with advantage a discharge apparatus having a concave mirror of aluminum as cathode, and as annode a platinum plate placed in the center of curvature, and at an angle of 45 degrees with the axis.

21. The X-rays proceed from the annode with this apparatus. As I have concluded from experiments with apparatus with various forms, it is immaterial with regard to the intensity of the X-rays whether they proceed from the annode or not. * * *

WÜRZBURG, PHYSIK. INSTITUT D. UNIVERSITÄT, *March 9, 1896.*

III.—FURTHER OBSERVATIONS ON THE PROPERTIES OF X-RAYS (ABSTRACT).[1]

1. If between a discharge tube sending out intense X-rays[2] and a fluorescent screen one interposes an untransmissible plate so placed that it shades the whole screen, one can still observe a luminosity of the barium-platinum cyanite. This luminescence is still visible when the screen is close up to the plate, and one is at first inclined to regard the plate as transmissible. When, however, one covers the fluorescent plate with a thick glass slide the fluorescence becomes much weaker, and it completely disappears when instead of using the glass plate one covers the screen with a lead cylinder 1 millimeter thick, which passes entirely over the head of the observer and is closed by the untransmissible plate.



The phenomena described might be due to the diffraction of rays of great wave length or to the sending out of X-rays by the objects surrounding the discharge tube, such, for example, as the air through which the rays pass. The latter explanation is the true one, as may be easily shown by the use of the following apparatus. The figure shows a thick-walled glass bell jar 20 centimeters high and 10 centimeters in diameter, which is closed by a thick zinc plate inserted with cement.

At *a* and *b* are lead shelves of the form of segments of a circle, each of an area somewhat greater than half the cross section of the jar. At the end of the glass jar is a zinc plate with a central opening covered with a collodium film, and the two plates above mentioned prevent X-rays which have passed through this opening from reaching the part of the jar which lies above the lead plate *b*. Upon the upper side of

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[1] Translation of a portion of the paper by Professor Röntgen in the Sitzungsber. der k. preuss. Akad. der Wissensch. zu Berlin, Jahrgang 1897, as reprinted in Annalen der Physik und Chemie, 64, p. 18, 1898.

[2] All the discharge tubes mentioned in the following communication are constructed according to the principle given under section 20 of my second communication. A great part have been obtained from the firm of Greiner & Friedrichs, in Stützerbach i. Th., to whom I wish to express my thanks for the great quantity of material which they have supplied to me without cost.

this plate is a fluorescent screen which fills almost the whole cross section of the jar. This screen can receive neither the direct rays nor those a single time diffusely reflected from solid substances, as, for example, the glass walls. Before each experiment the jar is filled with dust-free air. When X-rays are allowed to enter in such a way that they are all received by the lead plate *a* there is no fluorescence to be observed at *b*. When the jar is inclined so that the rays can pass through the space between *a* and *b* the fluorescent screen is illuminated over that portion not covered by the plate *b*. When the jar is connected with an air pump the fluorescence becomes weaker the further the exhaustion proceeds, and when air is admitted the intensity again increases.

I found that no noticeable fluorescence was excited by simple contact with air through which X-rays had shortly before passed, so that it follows that the air while it is receiving X-rays also sends them out in all directions.

If our eyes were as sensitive for X-rays as they are for light rays, an actively operating discharge tube would appear to us like a light burning in a room uniformly filled with tobacco smoke. It might perhaps be that the colors of the rays coming direct would be different from those sent out by the air particles.

The question whether the rays sent out by bodies are the same as those they receive, or, in other words, whether this phenomena is due to diffused reflection or to an action similar to fluorescence, I have not thus far been able to decide. It may be readily shown that the rays coming from air particles are photographically active, and this characteristic makes itself manifest in a most unwelcome manner. In order to guard against this effect it is necessary in long exposures to protect the plate by appropriate lead screens.

2. In order to compare the intensity of the radiation from the discharge tubes, and for various purposes, I have made use of a contrivance which is constructed similarly to the Bouguer photometer, and which, for the sake of simplicity, I will call a photometer. A rectangular piece of sheet lead 35 centimeters high, 150 centimeters long, and 0.15 millimeter thick is supported vertically on a piece of board in the center of a long table. On each side of this sheet and movable upon the table stands a discharge tube. At one end of the lead strip is a fluorescent screen[1] so arranged that each half is vertically radiated upon. In the measurements the arrangements were adjusted until the two parts were equally bright.

Some remarks made upon the use of this instrument may not be out

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[1] In this and other experiments the Edison fluorescent screen has proved very useful. This consists of a stereoscopic case which can be secured light-tight to the head of the observer, and whose pasteboard bottom is covered with barium-platinum cyanide. Edison employs Scheelite in place of barium-platinum cyanide; but for many reasons I prefer the latter.

of place. First of all it should be noted that the determinations are rendered much more difficult because of the unsteadiness of the sources of radiation. The tubes are sensitive to each irregularity of the interruption of the primary current; and such frequently occur with the Dupret and especially with the Foucault form of interrupter. Many repetitions of each measurement are therefore necessary. Second, I may indicate upon what factors the brightness of a given fluorescent screen depends which is acted upon by a shower of X-rays so rapid that the observer's eye can not detect the intermittent character of the illumination. This brightness depends (1) upon the intensity of the radiation proceeding from the platinum plate of the discharge tube; (2) very probably upon the nature of the rays falling on the screen, for, as will be shown, different kinds of radiation are not equally active in exciting fluorescence; (3) upon the distance of the screen from the source of the rays; (4) upon the absorption which the rays experience in their journey to the fluorescent screen; (5) upon the number of discharges per second; (6) upon the duration of each separate discharge; (7) upon the duration and the strength of the after light of the barium-platinum cyanide, and (8) upon the radiation to the screen from the surrounding bodies. In order to avoid errors, one must bear in mind that he has to do with a matter which is as if he were conducting experiments to compare by the aid of fluorescence two intermittent light sources of different colors and was obliged to carry on his experiments within an absorbing shell and in a turbid or fluorescing medium.

3. According to section 12 of my first communication, the source of the X-rays is the place where the cathode rays meet the discharge tube and the X-rays radiate outward in all directions. It is now of interest to investigate how the intensity of the rays varies with the direction.

For this investigation the spherical form of discharge apparatus with a well-polished platinum plate inclined at an angle of 45 degrees to the direction of the cathode rays is best adapted. Without other aid, there appears evidence in the uniformly bright fluorescence which is visible in the hemispherical glass wall above the platinum plate that there are no great differences in the intensity of the illumination, so that Lambert's law can not hold. However, this fluorescence may be largely excited by the cathode rays.

For the purpose of more exact investigation experiments were made with the photometer upon the intensity of the rays emanating from several tubes in various directions, and I have besides exposed photographic films which were bent in a half circle to a radius of 25 centimeters about the platinum plate as a center. In both experiments the varying thickness of the glass at different parts of the tube walls entered as a serious disturbing factor, because the X-rays were thus unequally absorbed in the various directions. It was, however, possible to make the thickness of the glass approximately uniform by the interposition of thin plates.

The result of these experiments is that the radiation upon an imaginary hemisphere constructed upon the platinum plate as a center is nearly uniform almost to the borders of this hemisphere. I could detect a slight diminution of intensity at an emanation angle of 80 degrees, but this diminution is relatively small, so that the principal part of the change in intensity occurs between 89 and 90 degrees. I was not able to detect any difference in kind between rays emitted at different angles.

In consequence of the distribution of intensity of X-rays, just described, it follows that images of the platinum plate formed in the pinhole camera, whether upon the photographic plate or the fluorescent screen, will be more intense the greater the angle which is made by the platinum plate with the screen or photographic film, providing this angle does not exceed 80 degrees. By appropriate arrangements which enabled me to make a comparison between images formed simultaneously by radiations from the same tube on screens at various angles this result was confirmed. A similar case of intensity distribution is found in optics in connection with fluorescence. If a few drops of a solution of fluorescein be allowed to fall into a square trough of water, and the trough be illuminated by white or violet light, it will be noticed that the brightest fluorescent light goes out from the edges of the slowly sinking column of fluorescein, or, in other words, from the places where the angle of emanation of the fluorescent light is the greatest. As Stokes has remarked, referring to a similar experiment, this appearance depends upon the fact that the light exciting the fluorescence is considerably more absorbed by the fluorescein solution than is the fluorescent light. It is worth mentioning that the cathode rays which generate the X-rays are much more strongly absorbed by platinum than the X-rays, and it may therefore be surmised that there exists a similarity between these two processes, the conversion of light into fluorescent light and the conversion of cathode rays into X-rays. There is as yet, however, no firm ground on which to rest such a conclusion.

With reference to practical applications, the observation of the distribution of intensity of the rays proceeding from the platinum plate has some value in connection with the formation of shadow pictures by means of X-rays. In accordance with the observations above recorded it is to be recommended that the discharge tube be so arranged that the rays employed for formation of pictures be those making a large angle, though not much exceeding 80 degrees, with the platinum plate. In this way the sharpest possible delineation will be obtained, and if the platinum plate is flat and the construction of the tube such that the rays proceeding obliquely pass through not much greater thickness of glass than those going out at right angles to the platinum plate, then no material loss in intensity will be experienced in this arrangement.

4. In my first communication I designated as the transmissibility of

a body the ratio of a brightness which a fluorescent screen held behind the body at right angles to the rays bears to its brightness in the absence of the interposed body, but under conditions otherwise identical. Referring the transmissibility to unit thickness we obtain what may be called the specific transmissibility. This will be the *d*th root of the transmissibility where *d* is the thickness of the transmitting layer measured along the direction of the rays. In order to determine the transmissibility I have since my first communication made use principally of the photometer described above. The two parts of the fluorescent screen having been brought to equal brightness, the plate of the substance to be investigated, as for instance, aluminum, tin, glass, etc., was interposed before one of the tubes, and the distance of one or other of the discharge tubes was altered so that the screen became again uniformly illuminated. The ratio between the squares of the distances of the platinum plates from the screen before and after the interposition of the body under investigation gives the value of the transmissibility sought. By interposing a second plate its transmissibility may be found for rays which have already passed through one plate of the same kind.

In this procedure it is assumed that the brightness of a fluorescent screen is inversely proportional to the square of its distance from the source of the rays, and this can only be the case on condition, first, that the air absorbs or emits no X-rays, and, second, that the brightness of the fluorescent light is proportional to the intensity of the radiation falling upon it. The first condition is certainly not fulfilled, and it is questionable whether the second is or not. I have therefore first satisfied myself, as already set forth in section 10 of my first communication, that the deviations from strict proportionality are so slight as to be negligible for the purposes of experiment in the case at hand. Again, with reference to the fact that X-rays are secondarily radiated from bodies under their influence, it may be remarked, first, that no difference was to be detected with the photometer between the transmissibility of a single plate of aluminum 0.925 millimeter thick, and of 31 superposed plates each of 0.0299 millimeter thickness, giving a total thickness of 0.927 millimeter; and, second, that the brightness of the fluorescent screen was not appreciably different whether the plate was placed close up to the screen or at a considerable distance from it.

The results of these experiments on transmissibility are for aluminum as follows:

Transmissibility for rays falling vertically.	Tube—			
	2	3	4	2
The first, 1 millimeter thick, aluminum plate	0.40	0.45		0.68
The second, 1 millimeter thick, aluminum plate	.55	.68		.73
The first, 2 millimeters thick, aluminum plate		.30	0.39	.50
The second, 2 millimeters thick, aluminum plate		.39	.54	.63

From these and similar experiments with glass and tin we may draw the following result: if a body be imagined to be made up of successive layers with their faces perpendicular to the direction of the X-rays, each of these layers will be more transmissible than the one next preceding. In other words, the specific transmissibility of a body is greater the thicker the body. This result is completely in accord with observations which may be made by photography of a tin scale as described in section 4 of my first communication, and also with the fact that occasionally in photographic shadow pictures, the shadows of thin layers, as for example the paper used in wrapping the plate, came out relatively strongly.

5. If two plates of different substances are equally transmissible this equality will not in general be retained for another pair of plates of the same substances with thicknesses altered in the same ratio. This fact may be shown very easily by the use of thin sheets, as, for example, of platinum and aluminum. I used for this purpose platinum foil 0.0026 millimeter thick and aluminum foil 0.0299 millimeter thick. I found in one instance that one sheet of platinum was equally transmissible with six sheets of aluminum; but the transmissibility of two sheets of platinum was less than that of twelve sheets of aluminum and about equal to that of sixteen sheets of the latter metal. Using another discharge tube, I found 1 platinum equal 8 aluminum but 8 platinum equal 90 aluminum. From these experiments it follows that the ratio of thicknesses of platinum and aluminum of equal transmissibility is less the thicker the sheets under examination.

6. The ratio of the thicknesses of two equally transmissible plates of different material is dependent on the thickness and the material of the body, as, for instance, the glass wall of the discharge tube, through which the rays have to pass before they reach the plates investigated.

* * *

7. The experiments described in sections 4, 5, and 6 relate to the alterations which the X-rays proceeding from a discharge tube experience in their transmission through different substances. It will now be shown that one and the same body may for the same thickness be unequally transmissible for rays emitted from different discharge tubes.

In the following table are given the values of the transmissibility of an aluminum plate 2 millimeters thick for the rays given out by different tubes:

	Tube.					
	1	2	3	4	2	5
Transmissibility for vertically incident rays of a 2-millimeter thick aluminum plate	0.0044	0.22	0.30	0.39	0.50	0.59

The discharge tubes were not materially different in their construction or in the thickness of their glass wall, but varied in the density of the gas within them, and hence in the potential required to produce discharge. Tube 1 required the least and tube 5 the greatest potential, or, as we may say for short, the tube 1 is the "softest" and tube 5 the "hardest." The same Ruhmkorff in direct connection with the tubes, the same circuit breaker, and the same current strength in the primary circuit were used in all cases.

Various other substances which I have investigated behaved similarly to aluminum. All are more transmissible to rays from harder tubes. This fact seemed to me particularly worthy of attention.

The relative transmissibility of plates of different substances proved also to be dependent on the hardness of the discharge tube employed. The ratio of the thickness of platinum and aluminum plates of equal transmissibility becomes less the harder the tubes from which the rays proceed, or, referring to the results just given, the less the rays are absorbed.

The different behavior of rays excited in tubes of different hardness is also made apparent in the well-known shadow picturing of hands, etc. With a soft tube a dark shadow is obtained in which the bones are little prominent; when a harder tube is used the bones are very distinct and visible in all their details, whereas the softer portions are less marked, and with very hard tubes even the bones themselves become only weak shadows. From these considerations it appears that the choice of the tube must be governed by the character of the objects which it is desired to portray.

8. It remains to remark that the quality of rays proceeding from one and the same tube depends on various conditions. Of these the most important are the following: (1) The action of the interrupter,[1] or, in other words, the course of the primary current. In this connection should be mentioned the phenomena frequently observed that particular ones of the rapidly succeeding discharges excite X-rays which are not only more intense, but which also differ from the others in their absorption. (2) The character of the sparks which appear in the secondary circuit of the apparatus. (3) The employment of a Tesla transformer. (4) The degree of evacuation of the discharge tube (as already stated). (5) The varying, but as yet not satisfactorily known, procedure within the discharge tube. Separate ones among these conditions require further comment. * * *

The hardness of a tube had been considered to be brought about solely by the continuation of the evacuation by means of the pump; but this characteristic is affected in other ways. Thus a sealed tube of medium hardness becomes gradually harder by itself—unfortunately to the shortening of the period of its usefulness when used in a suitable manner for the production of X-rays, that is to say when discharges which do not cause the platinum to glow or at least to glow only weakly are passed through. A gradual self-evacuation is thus effected.

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[1] A good Duprez interrupter works more regularly than a Foucault interrupter; the latter, however, conserves the primary current better.

With a tube thus become very hard I took a very fine photograph of a double-barreled gun with inserted cartridges, which showed all the details of the cartridges, the inner faults of the Damascus barrels, etc., very sharply and distinctly. The distance from the platinum plate of the discharge tube to the photographic plate was 15 centimeters and the exposure twelve minutes—comparatively long in consequence of the small photographic action of the very slightly absorbable rays (see below). The Duprez interrupter had to be replaced by the Foucault form. It would be of interest to construct tubes which would make it possible to use still higher potentials than before.

Self-evacuation has been above assigned as the cause of the growing hardness of sealed tubes, but this is not the only cause. There are changes in the electrodes which produce this effect. I do not know the nature of these changes. * * *

The observations recorded in these paragraphs and others not given have led me to the view that the composition of the rays proceeding from a platinum anode of a discharge tube depends upon the frequency and form of the discharge current. The degree of tenuity, the hardness, is important only because the form of the discharge is thereby influenced. If it were possible to produce the proper form of discharge for the generation of X-rays in any other way, the X-rays might be obtained with relatively high pressures.

9. The results appearing in the five preceding paragraphs have been those most evidently to be derived from the accompanying experiments. Summing up these separate results, and being guided in part by the analogy which holds between the behavior of the visible radiations and X-rays, one arrives at the following conclusions:

(*a*) The radiations emitted by a discharge tube consist of a mixture of rays of different absorbability and intensity.

(*b*) The composition of this mixture is in a marked degree dependent on the frequency and form of the discharge current.

(*c*) The rays receiving preference in absorption vary with different bodies.

(*d*) Since the X-rays are generated by the cathode rays and have in common with them various characteristics—as the exciting of fluorescence, photographic and electrical actions, an absorbability depending in a marked degree on the density of the medium traversed, etc.—the conjecture is prompted that both phenomena are processes of the same nature. Without committing myself unconditionally to this view, I may remark that the results of the last paragraphs are calculated to raise a difficulty in the way of this hypothesis. This difficulty consists in the great difference between the absorption of the cathode rays investigated by Lenard and the X-rays, and second that the transmissibility of bodies for the cathode rays is related to their density by other laws than those which govern their transmissibility for X-rays.

With regard to the first point, considerations present themselves under two heads: (1) As we have seen in section 7, there are X-rays of different absorbability, and the investigations of Hertz and Lenard show that the cathode rays are similarly to be discriminated. While the "softest" tubes investigated generated rays much less subject to absorption than any cathode rays investigated by Lenard, yet there is no reason to doubt the possibility of X-rays of greater absorbability, and cathode rays of less. It therefore appears probable that in future investigations rays will be found bridging over the gap between X-rays and cathode rays, so far as their absorption is concerned. (2) We found in section 4 that the specific transmissibility of a body becomes less the thinner the plate passed through. Consequently, had we made use in our experiments of plates as thin as those employed by Lenard it would have been found that the X-rays were more nearly like those of Lenard in their absorbability.

10. Besides the fluorescent phenomena, there may be excited by X-rays photographic, electric, and other actions, and it is of interest to know how far these various manifestations vary in similar ratio when the source of the rays is altered. I must restrict myself to a comparison of the first two phenomena. * * *

A hard and a soft tube were so adjusted as to give equally bright fluorescence as compared by means of the photometer described in section 2. Upon substituting a photographic plate in the place of the fluorescent screen it was found, on development, that the portion subject to the rays from the hard tube was blackened to a less degree than the other. The rays, though producing equal fluorescence, were thus for photographic purposes unequally active. * * *

The great sensitiveness of a photographic plate even for rays from tubes of medium hardness is illustrated by an experiment in which 96 films were superposed, placed at a distance of 25 centimeters from the discharge tube, and exposed five minutes with due precautions to protect the films from the radiations of the air. A photographic action was apparent on the last film, although the first was scarcely overexposed. * * *

If the intensity of the radiations is augmented by increasing the strength of the primary current, the photographic action increases in the same measure as the intensity of the fluorescence. In this case, as in the case where the intensity of the radiation was increased by an alteration of the distance of the fluorescent screen, the brightness of the fluorescence is at least approximately proportional to the intensity of the radiation. This rule should not, however, be too generally applied.

11. In conclusion, mention should be made of the following particulars:

With a discharge tube of proper construction, and not too soft, the X-rays are chiefly generated in a spot of not more than 1 or 2 millimeters diameter where the cathode rays meet the platinum plate. This, however, is not the sole source. The whole plate and a part of the tube walls emit X-rays, though in less intensity. Cathode rays proceed in all directions, but their intensity is considerable only near the axis of the concave cathode mirror, and, consequently, the X-rays are strongly emitted only near the point where this axis meets the platinum plate. When the tube is very hard and the platinum thin, many rays proceed also from the rear surface of the platinum plate, but, as may be shown by the pinhole camera, chiefly from the spot lying on the axis of the mirror. * * *

I can confirm the observation of G. Brandes that the X-rays are able to produce a sensation of light upon the retina of the eye. In my record book appears a notice entered in the early part of November, 1895, to the effect that when in a darkened chamber near a wooden door I perceived a weak appearance of light when a Hittorf tube upon the other side of the door was put in operation. Since this appearance was only once observed, I regarded it as a subjective, and the reason that it was not then repeatedly observed lay in the fact that other tubes were substituted for the Hittorf tube which were less completely evacuated and not provided with platinum anodes. The Hittorf tube furnishes rays of slight absorbability on account of its high vacuum, and, at the same time, of great intensity on account of the employment of a platinum anode for the reception of the cathode rays. * * *

With the tubes now in use I can easily repeat the Brandes experiment. * * *

Since the beginning of my investigation of X-rays I have repeatedly endeavored to produce diffraction phenomena with them. I obtained at various times, when using narrow slits, appearances similar to diffraction effects, but when modifications were made in the conditions for the purpose of thoroughly proving the accuracy of this explanation of the phenomena it was found in each case that the appearances were produced in other ways than by diffraction. I know of no experiment which gives satisfactory evidence of the existence of diffraction with the X-rays.

WÜRZBURG, PHYSIK. INSTITUT D. UNIV., *March 10, 1897.*

CATHODE RAYS.[1]

By Prof. J. J. THOMSON, F. R. S.

The first observer to leave any record of what are now known as the cathode rays seems to have been Plücker, who in 1859 observed the now well-known green phosphorescence on the glass in the neighborhood of the negative electrode. Plücker was the first physicist to make experiments on the discharge through a tube in a state anything approaching what we should now call a high vacuum. He owed the opportunity to do this to his fellow-townsman Giessler, who first made such vacua attainable. Plücker, who had made a very minute study of the effect of a magnetic field on the ordinary discharge which stretches from one terminal to the other, distinguished the discharge which produced the green phosphorescence from the ordinary discharge by the difference in its behavior when in a magnetic field. Plücker ascribed these phosphorescent patches to currents of electricity which went from the cathode to the walls of the tube and then for some reason or other retraced their steps.

The subject was next taken up by Plücker's pupil, Hittorf, who greatly extended our knowledge of the subject, and to whom we owe the observation that a solid body placed between a pointed cathode and the walls of the tube cast a well-defined shadow. This observation was extended by Goldstein, who found that a well marked, though not very sharply defined, shadow was cast by a small body placed near a cathode of considerable area. This was a very important observation, for it showed that the rays casting the shadow came in a definite direction from the cathode. If the cathode were replaced by a luminous disk of the same size, this disk would not cast a shadow of a small object placed near it, for though the object might intercept the rays which came out normally from the disk, yet enough light would be given out sideways from other parts of the disk to prevent the shadow being at all well marked. Goldstein seems to have been the first to advance the theory, which has attained a good deal of prevalence in Germany, that these cathode rays are transversal vibrations in the ether.

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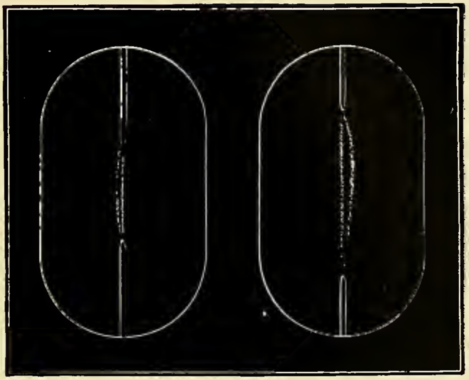
[1] Address before the Royal Institution of Great Britain, April 30, 1897. Printed in Proceedings of the Royal Institution, 1897.

The physicist, however, who did more than any one else to direct attention to these rays was Mr. Crookes, whose experiments, by their beauty and importance, attracted the attention of all physicists to this subject, and who not only greatly increased our knowledge of the properties of the rays, but by his application of them to radiant matter spectroscopy has rendered them most important agents in chemical research.

Recently a great renewal of interest in these rays has taken place, owing to the remarkable properties possessed by an offsprings of theirs, for the cathode rays are the parents of the Röntgen rays.

I shall confine myself this evening to endeavoring to give an account of some of the more recent investigations which have been made on the cathode rays. In the first place, when these rays fall on a substance they produce changes physical or chemical in the nature of the substance. In some cases this change is marked by a change in the color of the substance, as in the case of the chlorides of the alkaline metals. Goldstein found that these, when exposed to the cathode rays, changed color, the change, according to E. Wiedemann and Ebert, being due to the formation of a subchloride. Elster and Geitel have recently shown that these substances become photo-electric—i. e., acquire the power of discharging negative electricity under the action of light after exposure to the cathode rays. But though it is only in comparatively few cases that the change produced by the cathode rays shows itself in such a conspicuous way as by a change of color, there is a much more widely spread phenomenon, which shows the permanence of the effect produced by the impact of these rays. This is the phenomenon called by its discoverer, Prof. E. Wiedemann, thermoluminescence. Professor Wiedemann finds that if bodies are exposed to the cathode rays for some time, when the bombardment stops the substance resumes to all appearance its original condition. When, however, we heat the substance, we find that a change has taken place; for the substance now, when heated, becomes luminous at a comparatively low temperature, one far below that of incandescence. The substance retains this property for months after the exposure to the rays has ceased. The phenomenon of thermoluminescence is especially marked in bodies which are called by Van t'Hoff solid solutions. These are formed when two salts, one greatly in excess of the other, are simultanoously precipitated from a solution. Under these circumstances the connection between the salts seems of a more intimate character than that existing in a mechanical mixture. I have here a solid solution of $CaSo_4$ with trace of $MnSo_4$, and you will see that after exposure to the cathode rays it becomes luminous when heated. Another proof of the alteration produced by these rays is the fact, discovered by Crookes, that after glass has been exposed for a long time to the impact of these rays, the intensity of its phosphorescence is less than when the rays first began to fall upon it. This alteration lasts for a long time, certainly for months, and Mr. Crookes has shown that it is able to survive the heat-



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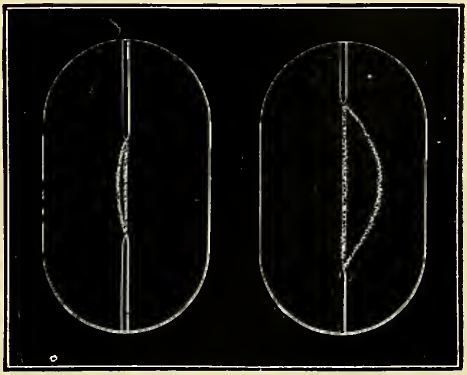

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ing up of the glass to allow of the remaking of the bulb. I will now leave the chemical effects produced by these rays and pass on to consider their behavior when in a magnetic field.

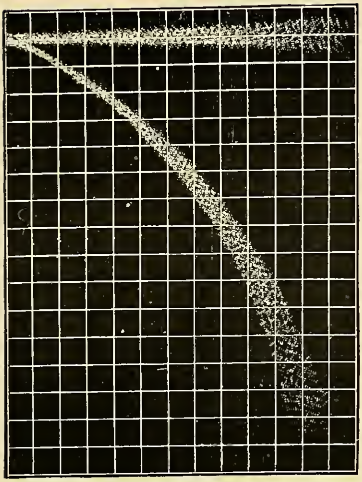
First, let us consider for a moment the effect of magnetic force on the ordinary discharge between terminals at a pressure much higher than that at which the cathode rays begin to come off. I have here photographs (see figs. 1 and 2, Pl. I) of the spark in a magnetic field. You see that when the discharge, which passes as a thin bright line between the terminals, is acted upon by the magnetic field it is pulled aside as a stretched string would be if acted upon by a force at right angles to its length. The curve is quite continuous, and though there may be gaps in the luminosity of the discharge, yet there are no breaks at such points in the curve, into which the discharge is bent by a magnet. Again, if the discharge, instead of taking place between points passes between flat discs, the effect of the magnetic force is to move the sparks as a whole, the sparks keeping straight until their terminations reach the edges of the discs. The fine thread-like discharge is not much spread out by the action of the magnetic field. The appearance of the discharge indicates that when the discharge passes through the gas it manufactures out of the gas something stretching from terminal to terminal, which, unlike a gas, is capable of sustaining a tension. The amount of deflection produced, other circumstances being the same, depends on the nature of the gas. As the photographs (figs. 3 and 4, Pl. I) show, the deflection is very small in the case of hydrogen and very considerable in the case of carbonic acid. As a general rule it seems smaller in elementary than in compound gases.

Let us contrast the behavior of this kind of discharge under the action of a magnetic field with that of the cathode rays. I have here some photographs (Pl. II) taken of a narrow beam formed by sending the cathode rays through a tube in which there was a plug with a slit in it, the plug being used as an anode and connected with the earth, these rays traversing a uniform magnetic field. The narrow beam spreads out under the action of the magnetic force into a broad fan-shaped luminosity in the gas. The luminosity in this fan is not uniformly distributed, but is condensed along certain lines. The phosphorescence produced when the rays reach the glass is also not uniformly distributed. It is much spread out, showing that the beam consists of rays which are not all deflected to the same extent by the magnet. The luminous patch on the glass is crossed by bands along which the luminosity is very much greater than in the adjacent parts. These bright and dark bands are called by Birkeland, who first observed them, "the magnetic spectrum." The brightest places on the glass are by no means always the terminations of the brightest streaks of luminosity in the gas; in fact, in some cases a very bright spot on the glass is not connected with the cathode by any appreciable luminosity, though there is plenty of luminosity in other parts of the gas.

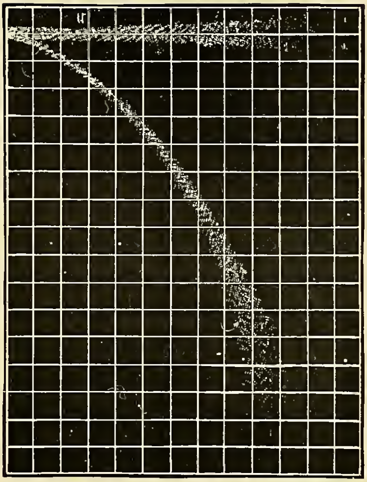
One very interesting point brought out by the photographs is that in a given magnetic field, with a given mean potential difference between the terminals, the path of the rays is independent of the nature of the gas. Photographs were taken of the discharge in hydrogen, air, carbonic acid, methyl iodide, i. e., in gases whose densities range from 1 to 70, and yet not only were the paths of the most deflected rays the same in all cases, but even the details, such as the distribution of the bright and dark spaces, were the same; in fact, the photographs could hardly be distinguished from one another. It is to be noted that the pressures were not the same; the pressures were adjusted until the mean potential difference was the same. When the pressure of the gas is lowered, the potential difference between the terminals increases, and the deflection of the rays produced by a magnet diminishes, or at any rate the deflection of the rays where the phosphorescence is a maximum diminishes. If an air break is inserted in the circuit an effect of the same kind is produced. In all the photographs of the cathode rays one sees indications of rays which stretch far into the bulb, but which are not deflected at all by a magnet. Though they stretch for some 2 or 3 miles, yet in none ot these photographs do they actually reach the glass. In some experiments, however, I placed inside the tube a screen, near to the slit through which the cathode rays came, and found that no appreciable phosphorescence was produced when the non deflected rays struck the screen, while there was vivid phosphorescence at the places where the deflected rays struck the screen. These non deflected rays do not seem to exhibit any of the characteristics of cathode rays, and it seems possible that they are merely jets of uncharged luminous gas shot out through the slit from the neighborhood of the cathode by a kind of explosion when the discharge passes.

The curves described by the cathode rays in a uniform magnetic field are, very approximately at any rate, circular for a large part of their course. This is the path which would be described if the cathode rays marked the path of negatively electrified particles projected with great velocities from the neighborhood of the negative electrode. Indeed, all the effects produced by a magnet on these rays, and some of these are complicated, as, for example, when the rays are curled up into spirals under the action of a magnetic force, are in exact agreement with the consequences of this view.

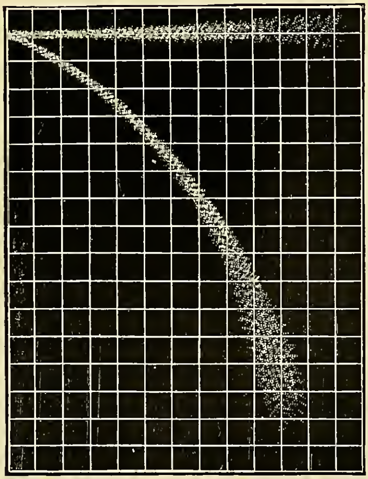
We can, moreover, show by direct experiment that a charge of negative electricity follows the course of the cathode rays. One way in which this has been done is by an experiment due to Perrin, the details of which are shown in the accompanying illustration. In this experiment the rays are allowed to pass inside a metallic cylinder through a small hole, and the cylinder, when these rays enter it, gets a negative charge, while if the rays are deflected by a magnet, so as to escape the hole, the cylinder remains without charge. It seems to me that to the experiment in this form it might be objected that, though the experiment shows that negatively electrified bodies are projected normally



5. HYDROGEN.
(Ammeter, 12; voltmeter, 1,600.)



6. AIR.



7. CARBONIC ACID GAS.
(Ammeter, 12; voltmeter, 1,600.)

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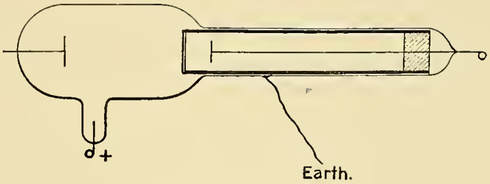
from the cathode and are deflected by a magnet, it does not show that when the cathode rays are deflected by a magnet the path of the electrified particles coincides with the path of the cathode rays. The supporters of the theory that these rays are waves in the ether might say, and indeed have said, that while they did not deny that electrified particles might be shot off from the cathode, these particles were, in their opinion, merely accidental accompaniments of the rays, and had no more to do with the rays than the bullet has with the flash of a rifle. The following modification of Perrin's experiment is not, however, open to this objection: Two coaxial cylinders with slits cut in them, the outer cylinder being connected with earth, the inner with the electrometer, are placed in the discharge tube, but in such a position that the cathode rays do not fall upon them unless deflected by a magnet. By means of a magnet, however, we can deflect the cathode rays until they fall on the slit in the cylinder. If under these circumstances the cylinder gets a negative charge when the cathode rays fall on the slit, and remains uncharged unless they do so, we may conclude, I think, the stream of negatively electrified particles is an invariable accompaniment of the cathode rays. I will now try the experiment. You notice that when there is no magnetic force, though the rays do not fall on the cylinder, there is a slight deflection of the electrometer, showing that it has acquired a small negative charge. This is, I think, due to the plug getting negatively charged under the torrent of negatively electrified particles from the cathode, and getting out cathode rays on its own account which have not come through the slit. I will now deflect the rays by a magnet, and you will see that at first there is little or no change in the deflection of the electrometer, but that when the rays reach the cylinder there is at once a great increase in the deflection, showing that the rays are pouring a charge of negative electricity into the cylinder. The deflection of the electrometer reaches a certain value and then stops and remains constant, though the rays continue to pour into the cylinder. This is due to the fact that the gas traversed by the cathode rays becomes a conductor of electricity, and thus, though the inner cylinder is perfectly insulated when the rays are not passing, yet as soon as the rays pass through the bulb the air between the inner cylinder and the outer one, which is connected with the earth, becomes a conductor, and the electricity escapes from the inner cylinder to the earth. For this reason the charge within the inner cylinder does not go on continually increasing. The cylinder settles into a state of equilibrium in which the rate at which it gains negative electricity from the rays is equal to the rate at which it loses it by conduction through the air. If we charge up the cylinder positively it rapidly loses its positive charge and acquires a negative one, while if we charge it up negatively it will leak if its initial negative potential is greater than its equilibrium value.

I have lately made some experiments which are interesting from the bearing they have on the charges carried by the cathode rays, as well as on the production of cathode rays outside the tube. The experiments are of the following kind: In the tube (see fig.) A and B are terminals. C is a long side tube into which a closed metallic cylinder fits lightly. This cylinder is made entirely of metal, except the end farthest from the terminals, which is stopped by an ebonite plug, perforated by a small hole so as to make the pressure inside the cylinder equal to that in the discharge tube. Inside the cylinder there is a metal disk supported by a metal rod which passes through the ebonite plug and is connected with an electrometer, the wires making this connection being surrounded by tubes connected with the earth so as to screen off electrostatic induction. If the end of the cylinder is made of thin aluminum about one-twentieth of a millimeter thick, and a discharge sent between the terminals, A being the cathode, then at pressures far higher than those at which the cathode rays come off, the disk inside the cylinder acquires a positive charge; and if it is charged up independently the charge leaks away, and it leaks more rapidly when the disk is charged negatively than when it is charged positively. There is, however, a leak in both cases, showing that conduction has taken place through the gas between the cylinder and the disk. As the pressure in the tube is diminished the positive charge on the disk diminishes until it becomes unappreciable. The leak from the disk, when it is charged still continues, and is now equally rapid, whether the original charge on the disk is positive or negative. When the pressure falls so low that cathode rays begin to fall on the end of the cylinder, then the disk acquires a negative charge, and the leak from the disk is more rapid when it is charged positively than when it is charged negatively. If the cathode rays are pulled off the end of the cylinder by a magnet, then the negative charge on the disk and the rate of leak from the disk when it is positively charged is very much diminished. A very interesting point is that these effects, due to the cathode rays, are observed behind comparatively thick walls. I have here a cylinder whose base is brass about 1 millimeter thick, and yet when this is exposed to the cathode rays the disk behind it gets a negative charge, and leaks if charged positively. The effect is small, compared with that in the cylinder with the thin aluminum base, but is quite appreciable. With the cylinder with the thick end I have never been able to observe any effect at the higher pressure when no cathode rays were coming off. The effect with the cylinder with the thin end was observed when the discharge was produced by a large number of small storage cells, as well as when it was produced by an induction coil.

It would seem from this experiment that the incidence of the cathode rays on a brass plate as much as 1 millimeter thick and connected with the earth can put a rarefied gas shielded by the plate into a condition in which it can conduct electricity, and that a body placed behind this screen gets a negative charge, so that the side of the brass away

PLATE III.



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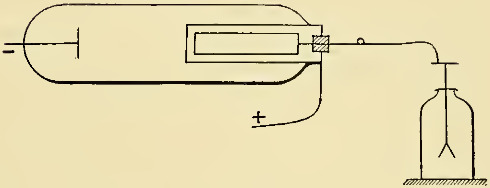

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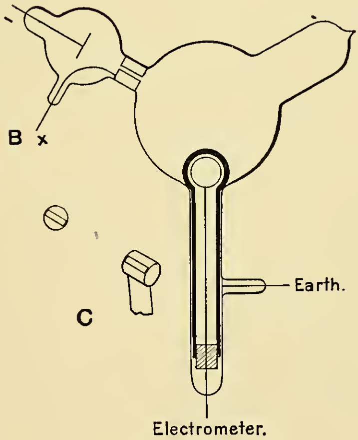

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from the cathode rays acts itself like a cathode, though kept permanently to earth. In the case of the thick brass the effect seems much more likely to be due to a sudden change in the potential of the outer cylinder at the places where the rays strike rather than to the penetration of any kinds of waves or rays. If the discharge in the tube was perfectly continuous the potential of the outer cylinder would be constant, and since it is connected to earth by a wire through which no considerable current flows, the potential must be approximately that of the earth. The discharge there can not be continuous; the negative charge must come in gusts against the ends of the cylinder, coming so suddenly that the electricity has no time to distribute itself over the cylinder so as to shield off the inside from the electrostatic action of the cathode rays; this force penetrates the cylinder and produces a discharge of electricity from the far side of the brass.

Another effect which I believe is due to the negative electrification carried by the rays is the following: In a very highly exhausted tube provided with a metal plug I have sometimes observed, after the coil has been turned off, bright patches on the glass. These are deflected by a magnet and seem to be caused by the plug getting such a large negative charge that the negative electricity continues to stream from it after the coil is stopped.

An objection sometimes urged against the view that these cathode rays consist of charged particles is that they are not deflected by an electrostatic force. If, for example, we make, as Hertz did, the rays pass between plates connected with a battery, so that an electrostatic force acts between these plates, the cathode ray is able to traverse this space without being deflected one way or the other. We must remember, however, that the cathode rays when they pass through a gas make it a conductor, so that the gas, acting like a conductor, screens off the electric force from the charged particle, and when the plates are immersed in the gas, and a definite potential difference established between the plates, the conductivity of the gas close to the cathode rays is probably enormously greater than the average conductivity of the gas between the plates, and the potential gradient on the cathode rays is therefore very small compared with the average potential gradient. We can, however, produce electrostatic results if we put the conductors which are to deflect the rays in the dark space next the cathode. I have here a tube in which, inside the dark space next the cathode, two conductors are inserted; the cathode rays start from the cathode, and have to pass between these conductors; if, now, I connect one of these conductors to earth there is a decided deflection of the cathode rays, while if I connect the other electrode to earth there is a deflection in the opposite direction. I ascribe this deflection to the gas in the dark space either not being a conductor at all, or if a conductor, a poor one compared to the gas in the main body of the tube.

Goldstein has shown that if a tube is furnished with two cathodes, when the rays from one cathode pass near the other they are repelled from it. This is just what would happen if the dark space round the electrode were an insulator, and so able to transmit electrostatic attractions or repulsions. To show that the gas in the dark space differs in its properties from the rest of the gas, I will try the following experiment. I have here two spherical bulbs connected together by a glass tube; one of these bulbs is small, the other large; they each contain a cathode, and the pressure of the gas is such that the dark space round the cathode in the small bulb completely fills the bulb, while that round the one in the larger bulb does not extend to the walls of the bulb. The two bulbs are wound with wire, which connects the outsides of two Leyden jars; the insides of these jars are connected with the terminals of a Wimshurst machine. When sparks pass between these terminals currents pass through the wire which induce currents in the bulbs, and cause a ring discharge to pass through them. Things are so arranged that the ring is faint in the larger bulb, bright in the smaller one. On making the wires in these bulbs cathodes, however, the discharge in the small bulb, which is filled by the dark space, is completely stopped, while that in the larger one becomes brighter. Thus the gas in the dark space is changed, and in the opposite way from that in the rest of the tube. It is remarkable that when the coil is stopped the ring discharge on both bulbs stops, and it is some time before it starts again.

The deflection excited on each other by two cathodic streams would seem to have a great deal to do with the beautiful phosphorescent figures which Goldstein obtained by using cathodes of different shapes. I have here two bulbs containing cathodes shaped like a cross. They are curved and of the same radius as the bulb, so that if the rays came off these cathodes normally the phosphorescent picture ought to be a cross of the same size as the cathode. You see that in one of the bulbs the image of the cross consists of two large sectors at right angles to each other, bounded by bright lines, and in the other, which is at a lower pressure, the geometrical image of the cross instead of being bright is dark, while the luminosity occupies the space between the arms of the cross.

So far I have only considered the behavior of the cathode rays inside the bulb, but Lenard has been able to get these rays outside the tube. To do this he let the rays fall on a window in the tube, made of thin aluminum about one one-hundredth of a millimeter thick, and he found that from this window there proceeded in all directions rays which were deflected by a magnet, and which produced phosphorescence when they fell upon certain substances, notably upon tissue paper soaked in a solution of pentadekaparalolylketon. The very thin aluminum is difficult to get, and Mr. McClelland has found that if it is not necessary to maintain the vacuum for a long time oiled silk answers admirably for a window. As the window is small, the phosphorescent patch produced by it is not bright, so that I will show instead the other property of the cathode rays—that of carrying with them a negative charge. I will place this cylinder in front of the hole, connect it with the electrometer, turn on the rays, and you will see the cylinder gets a negative charge. Indeed, this charge is large enough to produce the well-known negative figures when the rays fall on a piece of ebonite which is afterwards dusted with a mixture of red lead and sulphur.

From the experiments with the closed cylinder we have seen that when the negative rays come up to a surface even as thick as a millimeter the opposite side of that surface acts like a cathode and gives off the cathodic rays, and from this point of view we can understand the very interesting result of Lenard that the magnetic deflection of the rays outside the tube is independent of the density and chemical composition of the gas outside the tube, though it varies very much with the pressure of the gas inside the tube. The cathode rays could be started by an electric impulse, which would depend entirely on what was going on inside the tube. Since the impulse is the same, the momentum acquired by the particles outside would be the same, and, as the curvature of the path only depends on the momentum, the path of these particles outside the tube would only depend on the state of affairs inside the tube.

The investigation by Lenard on the absorption of these rays shows that there is more in his experiment than is covered by this consideration. Lenard measured the distance these rays would have to travel before the intensity of the rays fell to one-half their original value. The results are given in the following table:

Substance.	Coefficient of absorption.	Density.	Absorption density.
Hydrogen (3-millimeter pressure)	0. 00149	0. 000000368	4, 040
Hydrogen (760)	0. 476	0. 0000484	5, 640
Air (0.760-millimeter pressure)	3. 42	0. 00123	2, 780
SO_2	8. 51	0. 00271	3, 110
Collodion	3, 310	1. 1	3, 010
Glass	7, 810	2. 47	3, 160
Aluminum	7, 150	2. 70	2, 650
Silver	32, 200	10. 5	3, 070
Gold	53, 600	19. 3	2, 880

We see that though the densities and the coefficient of absorption vary enormously, yet the ratio of the two varies very little, and the results justify, I think, Lenard's conclusion that the distance through which these rays travel only depends on the density of the substance—that is, the mass of matter per unit volume—and not upon the nature of the matter.

These numbers raise a question which I have not yet touched upon, and that is the size of the carriers of the electric charge. Are they or are they not the dimensions of ordinary matter?

We see from Lenard's table that a cathode ray can travel through air at atmospheric pressure a distance of about half a centimeter before the brightness of the phosphorescence falls to about one-half of its original value. Now the mean free path of the molecule of air at this pressure is about 10^{-5} centimeters, and if a molecule of air were projected it would lose half its momentum in a space comparable with the mean free path. Even if we suppose that it is not the same molecule that is carried, the effect of the obliquity of the collisions would reduce the momentum to one-half in a short multiple of that path.

Thus, from Lenard's experiments on the absorption of the rays outside the tube, it follows, on the hypothesis that the cathode rays are charged particles, moving with high velocities, that the size of the carriers must be small compared with the dimensions of ordinary atoms or molecules. The assumption of a state of matter more finely subdivided than the atom of an element is a somewhat startling one; but an hypothesis that would involve somewhat similar consequences, viz, that the so-called elements are compounds of some primordial element, has been put forward from time to time by various chemists. Thus, Prout believed that the atoms of all the elements were built up of atoms of hydrogen, and Mr. Norman Lockyer has advanced weighty arguments, founded on spectroscopic consideration, in favor of the composite nature of the elements.

Let us trace the consequence of supposing that the atoms of the elements are aggregations of very small particles, all similar to each other. We shall call such particles corpuscles, so that the atoms of the ordinary elements are made up of corpuscles and holes, the holes being predominant. Let us suppose that at the cathode some of the molecules of the gas get split up into these corpuscles, and that these, charged with negative electricity and moving at a high velocity, form the cathode rays. The distance these rays would travel before losing a given fraction of their momentum would be proportional to the mean free path of the corpuscles. Now, the things these corpuscles strike against are other corpuscles, and not against the molecules as a whole; they are supposed to be able to thread their way between the interstices in the molecule. Thus the mean free path would be proportional to the number of these corpuscles; and, therefore, since each corpuscle has the same mass to the mass of unit volume—that is, to the density of the substance, whatever be its chemical nature or physical state, the mean free path, and therefore the coefficient of absorption, would depend only on the density. This is precisely Lenard's result.

We see, too, on this hypothesis, why the magnetic deflection is the same inside the tube, whatever be the nature of the gas, for the carriers of the charge are the corpuscles, and these are the same whatever gas be used. All the carriers may not be reduced to their lowest dimensions; some may be aggregates of two or more corpuscles; these would be differently deflected from the single corpuscle; thus we should get the magnetic spectrum.

I have endeavored by the following method to get a measurement of the ratio of the mass of these corpuscles to the charge carried by them. A double cylinder with slits in it, such as that used in a former experiment, was placed in front of a cathode which was curved so as to focus to some extent the cathode rays on the slit; behind the slit, in the inner cylinder, a thermal junction was placed which covered the opening so that all the rays which entered the slit struck against the junction, the junction got heated, and knowing the thermal capacity of the junction, we could get the mechanical equivalent of the heat communicated to it. The deflection of the electrometer gave the charge which entered the cylinder. Thus, if there are N particles entering the cylinder each with a charge e, and Q is the charge inside the cylinder,

$$N\,e = Q.$$

The kinetic energy of these

$$\tfrac{1}{2}\,N\,m\,v^2 = W$$

where W is the mechanical equivalent of the heat given to the thermal junction. By measuring the curvature of the rays for a magnetic field we get

$$\frac{m}{e}v = I.$$

Thus

$$\frac{m}{e} = \frac{1}{2}\frac{Q\,I^2}{W}.$$

In an experiment made at a very low pressure, when the rays were kept on for about one second, the charge was sufficient to raise a capacity of 1·5 microfarads to a potential of 16 volts. Thus

$$Q = 2{\cdot}4 \times 10^{-6}.$$

The temperature of the thermo junction, whose thermal capacity was 0·005 was raised 3·3° C. by the impact of the rays, thus

$$\begin{aligned} W &= 3{\cdot}3 \times 0{\cdot}005 \times 4{\cdot}2 \times 10^7 \\ &= 6{\cdot}3 \times 10^5. \end{aligned}$$

The value of I was 280, thus

$$\frac{m}{e} = 1{\cdot}6 \times 10^{-7}.$$

This is very small compared with the value 10^{-4} for the ratio of the mass of an atom of hydrogen to the charge carried by it. If the result stood by itself we might think that it was probable that e was greater than the atomic charge of atom rather than that m was less than the mass of a hydrogen atom. Taken, however, in conjunction with Lenard's results for the absorption of the cathode rays, these numbers seem to favor the hypothesis that the carriers of the charges are smaller than the atoms of hydrogen.

It is interesting to notice the value of e/m, which we have found from the cathode rays, is of the same order as the value of 10^{-7} deduced by Zeeman from his experiments on the effect of a magnetic field on the period of the sodium light.

STORY OF EXPERIMENTS IN MECHANICAL FLIGHT.[1]

By Samuel Pierpont Langley.

The editor of The Annual has asked me to give matter of a somewhat personal nature for a narrative account of my work in aerodromics.

The subject of flight interested me as long ago as I can remember anything, but it was a communication from Mr. Lancaster, read at the Buffalo meeting of the American Association for the Advancement of Science, in 1886, which aroused my then dormant attention to the subject. What he said contained some remarkable but apparently mainly veracious observations on the soaring bird, and some more or less paradoxical assertions, which caused his communication to be treated with less consideration than it might otherwise have deserved. Among these latter was a statement that a model, somewhat resembling a soaring bird, wholly inert, and without any internal power, could, nevertheless, under some circumstances, advance against the wind without falling; which seemed to me then, as it did to members of the association, an utter impossibility, but which I have since seen reason to believe is, within limited conditions, theoretically possible.

I was then engaged in the study of astrophysics at the Observatory in Allegheny, Pa. The subject of mechanical flight could not be said at that time to possess any literature, unless it were the publications of the French and English aeronautical societies, but in these, as in everything then accessible, fact had not yet always been discriminated from fancy. Outside of these, almost everything was even less trustworthy; but though, after I had experimentally demonstrated certain facts, anticipations of them were found by others on historical research, and though we can now distinguish in retrospective examination what would have been useful to the investigator if he had known it to be true, there was no test of the kind to apply at the time. I went to work, then, to find out for myself, and in my own way, what amount of mechanical power was requisite to sustain a given weight in the air and make it advance at a given speed, for this seemed to be an inquiry which must necessarily precede any attempt at mechanical flight, which was the very remote aim of my efforts.

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[1] From the Aeronautical Annual. 1897.

The work was commenced in the beginning of 1887 by the construction, at Allegheny, of a turntable of exceptional size, driven by a steam engine, and this was used during three years in making the "Experiments in Aerodynamics," which were published by the Smithsonian Institution under that title in 1891. Nearly all the conclusions reached were the result of direct experiment in an investigation which aimed to take nothing on trust. Few of them were then familiar, though they have since become so, and in this respect knowledge has advanced so rapidly, that statements which were treated as paradoxical on my first enunciation of them are now admitted truisms.

It has taken me, indeed, but a few years to pass through the period when the observer hears that his alleged observation was a mistake; the period when he is told that if it were true, it would be useless; and the period when he is told that it is undoubtedly true, but that it has always been known.

May I quote from the introduction to this book what was said in 1891? "I have now been engaged since the beginning of the year 1887 in experiments on an extended scale for determining the possibilities of, and the conditions for, transporting in the air a body whose specific gravity is greater than that of the air, and I desire to repeat my conviction that the obstacles in its way are not such as have been thought; that they lie more in such apparently secondary difficulties, as those of guiding the body so that it may move in the direction desired and ascend or descend with safety, than in what may appear to be primary difficulties, due to the air itself," and, I added, that in this field of research I thought that we were, at that time (only six years since), "in a relatively less advanced condition than the study of steam was before the time of Newcomen." It was also stated that the most important inference from those experiments as a whole was that mechanical flight was possible with engines we could then build, as one horsepower rightly applied could sustain over 200 pounds in the air at a horizontal velocity of somewhat over 60 feet a second.

As this statement has been misconstrued, let me point out that it refers to surfaces, used without guys or other adjuncts, which would create friction; that the horsepower in question is that actually expended in the thrust, and that it is predicated only on a rigorously horizontal flight. This implies a large deduction from the power in the actual machine, where the brake horsepower of the engine, after a requisite allowance for loss in transmission to the propellers and for their slip on the air, will probably be reduced to from one-half to one-quarter of its nominal amount; where there is great friction from the enforced use of guys and other adjuncts; but, above all, where there is no way to insure absolutely horizontal flight in free air. All these things allowed for, however, since it seemed to me possible to provide an engine which should give a horsepower for something like 10 pounds of weight, there was still enough to justify the statement that we possessed in the steam engine, as then constructed or in other heat engines, more than the indispensable power, though it was added that this was not asserting that a system of supporting surfaces could be securely guided through the air or safely brought to the ground, and that these and like considerations were of quite another order, and belonged to some inchoate art which I might provisionally call *aerdromics.*

These important conclusions were reached before the actual publication of the volume, and a little later others on the nature of the movements of air, which were published under the title of "The internal work of the wind" (Smithsonian Contributions to Knowledge, Volume XXVII, 1893, No. 884). The latter were founded on experiments independent of the former, and which led to certain theoretical conclusions unverified in practice. Among the most striking, and perhaps paradoxical of these, was that a suitably disposed free body might, under certain conditions, be sustained in an ordinary wind, and even advance against it without the expenditure of any energy from within.

The first stage of the investigation was now over, so far as that I had satisfied myself that mechanical flight was possible with the power we could hope to command, if only the art of directing that power could be acquired.

The second stage (that of the acquisition of this art) I now decided to take up. It may not be out of place to recall that at this time, only six years ago, a great many scientific men treated the whole subject with entire indifference, as unworthy of attention, or as outside of legitimate research, the proper field of the charlatan, and one on which it was scarcely prudent for a man with a reputation to lose to enter.

The record of my attempts to acquire the art of flight may commence with the year 1889, when I procured a stuffed frigate bird, a California condor, and an albatross, and attempted to move them upon the whirling table at Allegheny. The experiments were very imperfect and the records are unfortunately lost, but the important conclusion to which they led was that a stuffed bird could not be made to soar except at speeds which were unquestionably very much greater than what served to sustain the living one, and the earliest experiments and ali subsequent ones with actually flying models have shown that thus far we can not carry nearly the weights which Nature does to a given sustaining surface without a power much greater than she employs. At the time these experiments were begun, Penaud's ingenious but toy-like model was the only thing which could sustain itself in the air for even a few seconds, and calculations founded upon its performance sustained the conclusion that the amount of power required in actual free flight was far greater than that demanded by the theoretical enunciation. In order to learn under what conditions the aerodrome should be balanced for horizontal flight, I constructed over thirty modifications of the rubber-driven model, and spent many months in endeavoring from these to ascertain the laws of "balancing;" that is, of stability leading to horizontal flight. Most of these models had two propellers, and it was extremely difficult to build them light and strong enough. Some of them had superposed wings; some of them curved and some plane wings; in some the propellers were side by side; in others one propeller was at the front and the other at the rear, and so every variety of treatment was employed, but all were at first too heavy, and only those flew successfully which had from 3 to 4 feet of sustaining surface to a pound of weight, a proportion which is far greater than Nature employs in the soaring bird, where in some cases less than half a foot of sustaining surface is used to a pound. It had been shown in the "Experiments in aerodynamics" that the center of pressure on an inclined plane advancing was not at the center of figure, but much in front of it, and this knowledge was at first nearly all I possessed in balancing these early aerodromes. Even in the beginning, also, I met remarkable difficulty in throwing them into the air, and devised numerous forms of launching apparatus which were all failures, and it was necessary to keep the construction on so small a scale that they could be cast from the hand.

The earliest actual flights with these were extremely irregular and brief, lasting only from three to four seconds. They were made at Allegheny in March, 1891, but these and all subsequent ones were so erratic and so short that it was possible to learn very little from them. Penaud states that he once obtained a flight of thirteen seconds. I never got as much as this, but ordinarily little more than half as much, and came to the conclusion that in order to learn the art of mechanical flight it was necessary to have a model which would keep in the air for at any rate a longer period than these, and move more steadily. Rubber twisted in the way that Penaud used it will practically give about 300 foot-pounds to a pound of weight, and at least as much must be allowed for the weight of the frame on which the rubber is strained. Twenty pounds of rubber and frame, then, would give 3,000 foot-pounds, or 1 horsepower for less than six seconds. A steam engine having apparatus for condensing its steam, weighing in all 10 pounds, and carrying 10 pounds of fuel, would possess in this fuel, supposing that but one-tenth of its theoretical capacity is utilized, many thousand times the power of an equal weight of rubber, or at least 1 horsepower for some hours. Provided the steam could be condensed and the water reused, then the advantage of the steam over the spring motor was enormous, even in a model constructed only for the purpose of study. But the construction of a steam-driven aerodrome was too formidable a task to be undertaken lightly, and I examined the capacities of condensed air, carbonic-acid gas, of various applications of electricity, whether in the primary or storage battery, of hot-water engines, of inertia motors, of the gas engine, and of still other material. The gas engine promised best of all in theory, but it was not yet developed in a suitable form. The steam engine, as being an apparently familiar construction, promised best in practice, but in taking it up, I, to my cost, learned that in the special application to be made of it, little was really familiar and everything had to be learned by experiment. I had myself no previous knowledge of steam engineering, nor any assistants other than the very capable workmen employed. I well remember my difficulties over the first aerodrome (No. 0), when everything, not only the engine, but the boilers which were to supply it, the furnaces which were to heat it, the propellers which were to advance it, the hull which was to hold all these—were all things to be originated, in a construction which, as far as I knew, had never yet been undertaken by anyone.

It was necessary to make a beginning, however, and a compound engine was planned which, when completed, weighed about 4 pounds, and which could develop rather over a horsepower with 60 pounds of steam, which it was expected could be furnished by a series of tubular boilers arranged in "bee-hive" form and the whole was to be contained in a hull about 5 feet in length and 10 inches in diameter. This hull was, as in the construction of a ship, to carry all adjuncts. In front of it projected a steel rod, or bowsprit, about its own length, and one still longer behind. The engines rotated two propellers, each about 30 inches in diameter, which were on the end of long shafts disposed at an acute angle to each other and actuated by a single gear driven from the engine. A single pair of large wings contained about 50 square feet, and a smaller one in the rear about half as much, or in all some 75 feet, of sustaining surface, for a weight which it was expected would not exceed 25 pounds.

Although this aerodrome was in every way a disappointment, its failure taught a great many useful lessons. It had been built on the large scale described, with very little knowledge of how it was to be launched into the air, but the construction developed the fact that it was not likely to be launched at all, since there was a constant gain in weight over the estimate at each step, and when the boilers were completed it was found that they gave less than one-half the necessary steam, owing chiefly to the inability to keep up a proper fire. The wings yielded so as to be entirely deformed under a slight pressure of the air, and it was impossible to make them stronger without making them heavier, where the weight was already prohibitory. The engines could not transmit even what feeble power they furnished, without dangerous tremor in the long shafts, and there were other difficulties. When the whole approached completion, it was found to weigh nearer 50 pounds than 25, to develop only about one-half the estimated horsepower at the brake, to be radically weak in construction, owing to the yielding of the hull, and to be, in short, clearly a hopeless case.

The first steam-driven aerodrome had, then, proved a failure, and I reverted during the remainder of the year to simpler plans, among them one of an elementary gasoline engine.

I may mention that I was favored with an invitation from Mr. Maxim to see his great flying machine at Bexley, in Kent, where I was greatly impressed with the engineering skill shown in its construction, but I found the general design incompatible with the conclusions that I had reached by experiments with small models, particularly as to what seemed to me advisable in the carrying of the center of gravity as high as was possible with safety.

In 1892 another aerodrome (No. 1), which was to be used with carbonic acid gas, or with compressed air, was commenced. The weight of this aerodrome was a little over 4½ pounds, and the area of the supporting surfaces 6½ square feet. The engines developed but a small fraction of a horsepower, and they were able to give a dead lift of only about one-tenth of the weight of the aerodrome, giving relatively less power to weight than that obtained in the large aerodrome already condemned.

Toward the close of this year was taken up the more careful study of the position of the center of gravity with reference to the line of thrust from the propellers, and to the center of pressure. The center of gravity was carried as high as was consistent with safety, the propellers being placed so high, with reference to the supporting wings, that the intake of air was partly from above and partly from below these latter. The lifting power (i. e., the dead lift) of the aerodromes was determined in the shop by a very useful contrivance which I have called the "pendulum," which consists of a large pendulum which rests on knife edges, but is prolonged above the points of support, and counterbalanced so as to present a condition of indifferent equilibrium. Near the lower end of this pendulum the aerodrome is suspended, and when power is applied to it, the reaction of the propellers lifts the pendulum through a certain angle. If the line of thrust passes through the center of gravity, it will be seen that the sine of this angle will be the fraction of the weight lifted, and thus the dead-lift power of the engines becomes known. Another aerodrome was built, but both, however constructed, were shown by this pendulum test to have insufficient power, and the year closed with disappointment.

Aerodrome No. 3 was of stronger and better construction, and the propellers, which before this had been mounted on shafts inclined to each other in a V-like form, were replaced by parallel ones. Boilers of the Serpolet type (that is, composed of tubes of nearly capillary section) were experimented with at great cost of labor and no results; and they were replaced with coil boilers. For these I introduced, in April, 1893, a modification of the æolopile blast, which enormously increased the heat-giving power of the fuel (which was then still alcohol), and with this blast for the first time the boilers began to give steam enough for the engines. It had been very difficult to introduce force pumps which would work effectively on the small scale involved, and after many attempts to dispense with their use by other devices, the acquisition of a sufficiently strong pump was found to be necessary in spite of its weight, but was only secured after long experiment. It may be added that all the aerodromes from the very nature of their construction were wasteful of heat, the industrial efficiency little exceeding half of 1 per cent, or from one-tenth to one-twentieth that of a stationary engine constructed under favorable conditions. This last aerodome lifted nearly 30 per cent of its weight upon the pendulum, which implied that it could lift much more than its weight when running on a horizontal track, and its engines were capable of running its 50-centimeter propellers at something over 700 turns per minute. There was, however, so much that was unsatisfactory about it, that it was deemed best to proceed to another construction before an actual trial was made in the field, and a new aerodrome, designated as No. 4, was begun. This last was an attempt, guided by the weary experience of preceding failures, to construct one whose engines should run at a much higher pressure than heretofore, and be much more economical in weight. The experiments with the Serpolet boilers having been discontinued, the boiler was made with a continuous helix of copper tubing, which, as first employed, was about three millimeters internal diameter; and it may be here observed that a great deal of time was subsequently lost in attempts to construct a more advantageous form of boiler for the actual purposes than this simple one, which, with a larger coil tube, eventually proved to be the best; so that later constructions have gone back to this earlier type. A great deal of time was lost in these experiments from my own unfamiliarity with steam engineering, but it may also be said that there was little help either from books or from counsel, for everything was here sui generis, and had to be worked out from the beginning. In the construction which had been reached by the middle of the third year of experiment, and which has not been greatly differed from since, the boiler was composed of a coil of copper in the shape of a hollow helix, through the center of which the blast from the æolipile was driven, the steam and water passing into a vessel I called the "separator," whence the steam was led into the engines at a pressure of from 70 to 100 pounds (a pressure which has since been considerably exceeded).

From the very commencement of this long investigation the great difficulty was in keeping down the weight, for any of the aerodromes could probably have flown had they been built light enough, and in every case before the construction was completed the weight had so increased beyond the estimate, that the aerodrome was too heavy to fly, and nothing but the most persistent resolution kept me in continuing attempts to reduce it after further reduction seemed impossible. Toward the close of the year (1893) I had, however, finally obtained an aerodrome with mechanical power, as it seemed to me, to fly, and I procured, after much thought as to where this flight should take place, a small house boat, to be moored somewhere in the Potomac; but the vicinity of Washington was out of the question, and no desirable place was found nearer than 30 miles below the city. It was because it was known that the aerodrome might have to be set off in the face of a wind, which might blow in any direction, and because it evidently was at first desirable that it should light in the water rather than on the land, that the house boat was selected as the place for the launch. The aerodrome (No. 4) weighed between 9 and 10 pounds, and lifted 40 per cent of this on the pendulum with 60 pounds of steam pressure, a much more considerable amount than was theoretically necessary for horizontal flight. And now the construction of a launching apparatus, dismissed for some years, was resumed. Nearly every form seemed to have been experimented with unsuccessfully in the smaller aerodromes. Most of the difficulties were connected with the fact that it is necessary for an aerodrome, as it is for a soaring bird, to have a certain considerable initial velocity before it can advantageously use its own mechanism for flight, and the difficulties of imparting this initial velocity with safety are surprisingly great, and in the open air are beyond all anticipation.

Here, then, commences another long story of delay and disappointment in these efforts to obtain a successful launch. To convey to the reader an idea of its difficulties a few extracts from the diary of the period are given. (It will be remembered that each attempt involved a journey of 30 miles each way.)

November 18, 1893. Having gone down to the house boat, preparatory to the first launch, in which the aerodrome was to be cast from a springing piece beneath, it was found impossible to hold it in place on this before launching without its being prematurely torn from its support, although there was no wind except a moderate breeze; and the party returned after a day's fruitless effort.

Two days later a relative calm occurred in the afternoon of a second visit, when the aerodrome was mounted again, but, though the wind was almost imperceptible, it was sufficient to wrench it about so that at first nothing could be done, and when steam was gotten up the burning alcohol blew about so as to seriously injure the inflammable parts. Finally, the engines being under full steam, the launch was attempted, but, owing to the difficulties alluded to and to a failure in the construction of the launching piece, the aerodrome was thrown down upon the boat, fortunately with little damage.

Whatever form of launch was used, it became evident at this time that the aerodrome must at any rate be firmly held up to the very instant of release, and a device was arranged for clamping it to the launching apparatus.

On November 24 another attempt was made to launch, which was rendered impossible by a very moderate wind indeed.

On November 27 a new apparatus was arranged, to merely drop the aerodrome over the water, with the hope that it would get up sufficient speed before reaching the surface to soar, but it was found that a very gentle intermittent breeze (probably not more than 3 or 4 miles an hour) was sufficient to make it impossible even to prepare to drop the aerodrome toward the water with safety.

It is difficult to give an idea in few words of the nature of the trouble, but unless one stands with the machine in the open air he can form no conception of what the difficulties are, which are peculiar to practice in the open, and which do not present themselves to the constructor in the shop, nor probably to the mind of the reader.

December 1, another failure; December 7, another; December 11, another; December 20, another; December 21, another. These do not all involve a separate journey, but five separate trips were made of a round distance of 60 miles each before the close of the season. It may be remembered that these attempts were in a site far from the conveniences of the workshop and under circumstances which took up a good deal of time, for some hours were spent on mounting the aerodrome on each occasion, and the year closed without a single cast of it into the air. It was not known how it would have behaved there, for there had not been a launch even in nine trials, each one representing an amount of trouble and difficulty which this narrative gives no adequate idea of.

I pass over a long period of subsequent baffled effort, with the statement that numerous devices for launching were tried in vain and that nearly a year passed before one was effected.

Six trips and trials were made in the first six months of 1894 without securing a launch. On the 24th of October a new launching piece was tried for the first time, which embodied all the requisites whose necessity was taught by previous experience, and, saving occasional accidents, the launching was from this time forward accomplished with comparatively little difficulty.

The aerodromes were now for the first time put fairly in the air, and a new class of difficulties arose, due to a cause which was at first obscure—for two successive launches of the same aerodrome, under conditions as near alike as possible, would be followed by entirely different results. For example, in the first case it might be found rushing, not falling, forward and downward into the water under the impulse of its own engines; in the second case, with every condition from observation apparently the same, it might be found soaring upward until its wings made an angle of 60 degrees with the horizon, and, unable to sustain itself at such a slope, sliding backward into the water.

After much embarrassment the trouble was discovered to be due to the fact that the wings, though originally set at precisely the same angle in the two cases, were irregularly deflected by the upward pressure of the air, so that they no longer had the form which they appeared to possess but a moment before they were upborne by it, and so that a very minute difference, too small to be certainly noted, exaggerated by this pressure, might cause the wind of advance to strike either below or above the wing and to produce the salient difference alluded to. When this was noticed all aerodromes were inverted, and sand was dredged uniformly over the wings until its weight represented that of the machine. The flexure of the wings under those circumstances must be nearly that in free air, and it was found to distort them beyond all anticipation. Here commences another series of trials, in which the wings were strengthened in various ways, but in none of which, without incurring a prohibitive weight, was it possible to make them strong enough. Various methods of guying them were tried, and they were rebuilt on different designs—a slow and expensive process. Finally, it may be said, in anticipation (and largely through the skill of Mr. Reed, the foreman of the work), the wings were rendered strong enough without excessive weight, but a year or more passed in these and other experiments.

In the latter part of 1894 two steel aerodromes had already been built, which sustained from 40 to 50 per cent of their dead-lift weight on the pendulum, and each of which was apparently supplied with much more than sufficient power for horizontal flight (the engine and all the moving parts furnishing over one horsepower at the brake weighed in one of these but 26 ounces); but it may be remarked that the boilers and engines in lifting this per cent of the weight did so only at the best performance in the shop, and that nothing like this could be counted upon for regular performance in the open. Every experiment with the launch, when the aerodrome descended into the water, not gently, but impelled by the misdirected power of its own engines, resulted at this stage in severe strains and local injury, so that repairing, which was almost rebuilding, constantly went on; a hard but necessary condition attendant on the necessity of trial in the free air. It was gradually found that it was indispensable to make the frame stronger than had hitherto been done, though the absolute limit of strength consistent with weight seemed to have been already reached, and the year 1895 was chiefly devoted to the labor on the wings and what seemed at first the hopeless task of improving the construction so that it might be stronger without additional weight, when every gram of weight had already been scrupulously economized. With this went on attempts to carry the effective power of the burners, boilers, and engines further, and modification of the internal arrangement and a general disposition of the parts such that the wings could be placed further forward or backward at pleasure, to more readily meet the conditions necessary for bringing the center of gravity under the center of pressure. So little had even now been learned about the system of balancing in the open air, that at this late day recourse was again had to rubber models, of a different character, however, from those previously used; for in the latter the rubber was strained, not twisted. These experiments took up an inordinate time, though the flight obtained from the models thus made was somewhat longer and much steadier than that obtained with the Penaud form, and from them a good deal of valuable information was gained as to the number and position of the wings and as to the effectiveness of different forms and dispositions of them. By the middle of the year a launch took place with a brief flight, where the aerodrome shot down into the water after a little over 50 yards. It was immediately followed by one in which the same aerodrome rose at a considerable incline and fell backward with scarcely any advance after sustaining itself rather less than 10 seconds, and these and subsequent attempts showed that the problem of disposing of the wings so that they would not yield and of obtaining a proper "balance" was not yet solved.

Briefly it may be said that the year 1895 gave small results for the labor with which it was filled, and that at its close the outlook for further substantial improvement seemed to be almost hopeless, but it was at this time that final success was drawing near. Shortly after its close I became convinced that substantial rigidity had been secured for the wings; that the frame had been made stronger without prohibitive weight, and that a degree of accuracy in the balance had been obtained which had not been hoped for. Still there had been such a long succession of disasters and accidents in the launching that hope was low when success finally came.

I have not spoken here of the aid which I received from others, and particularly from Dr. Carl Barus and Mr. J. E. Watkins, who have been at different times associated with me in the work. Mr. R. L. Reed's mechanical skill has helped me everywhere, and the lightness and efficiency of the engines are in a large part due to Mr. L. C. Maltby.

THE AERODROMES IN FLIGHT.[1]

The successful flights of Dr. Langley's aerodrome were witnessed by Dr. Bell, and described by him as follows:[2]

"Through the courtesy of Dr. S. P. Langley, Secretary of the Smithsonian Institution, I have had, on various occasions, the privilege of witnessing his experiments with aerodromes, and especially the remarkable success attained by him in experiments made upon the Potomac River on Wednesday, May 6, 1896, which led me to urge him to make public some of these results.

"I had the pleasure of witnessing the successful flight of some of these aerodromes more than a year ago, but Dr. Langley's reluctance to make the results public at that time prevented me from asking him, as I have done since, to let me give an account of what I saw.

"On the date named two ascensions were made by the aerodrome, or so-called 'flying machine,' which I will not describe here further than to say that it appeared to me to be built almost entirely of metal, and driven by a steam engine which I have understood was carrying fuel and a water supply for a very brief period, and which was of extraordinary lightness.

"The absolute weight of the aerodrome, including that of the engine and all appurtenances, was, as I was told, about 25 pounds, and the

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[1] The following descriptions follow Dr. Langley's article in The Aeronautical Annual, 1897.

[2] Nature, London, May 28, 1896.

distance from tip to tip of the supporting surfaces was, as I observed, about 12 or 14 feet. The method of propulsion was by aerial screw propellers, and there was no gas or other aid for lifting it in the air except its own internal energy.

"On the occasion referred to, the aerodrome, at a given signal, started from a platform about 20 feet above the water, and rose at first directly in the face of the wind, moving at all times with remarkable steadiness, and subsequently swinging around in large curves of, perhaps, a hundred yards in diameter, and continually ascending until its steam was exhausted, when, at a lapse of about a minute and a half, and at a height which I judged to be between 80 and 100 feet in the air, the wheels ceased turning, and the machine, deprived of the aid of its propellers, to my surprise did not fall, but settled down so softly and gently that it touched the water without the least shock, and was in fact immediately ready for another trial.

"In the second trial, which followed directly, it repeated in nearly every respect the actions of the first, except that the direction of its course was different. It ascended again in the face of the wind, afterwards moving steadily and continually in large curves accompanied with a rising motion and a lateral advance. Its motion was, in fact, so steady, that I think a glass of water on its surface would have remained unspilled. When the steam gave out again, it repeated for a second time the experience of the first trial when the steam had ceased, and settled gently and easily down. What height it reached at this trial I can not say, as I was not so favorably placed as in the first; but I had occasion to notice that this time its course took it over a wooded promontory, and I was relieved of some apprehension in seeing that it was already so high as to pass the tree tops by 20 or 30 feet. It reached the water one minute and thirty-one seconds from the time it started, at a measured distance of over 900 feet from the point at which it rose.

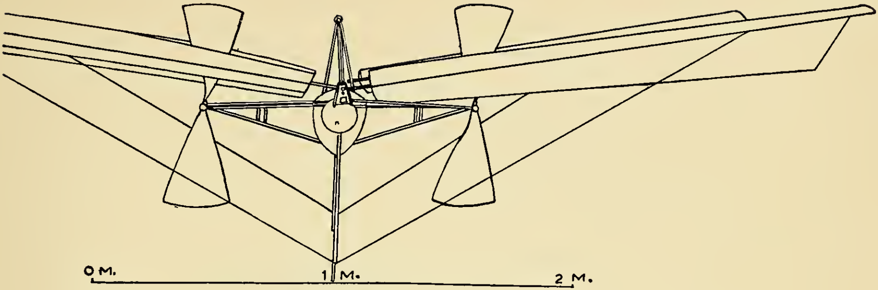
"This, however, was by no means the length of its flight. I estimated from the diameter of the curve described, from the number of turns of the propellers as given by the automatic counter, after due allowance for slip, and from other measures, that the actual length of flight on each occasion was slightly over 3,000 feet. It is at least safe to say that each exceeded half an English mile.

"From the time and distance it will be noticed that the velocity was between 20 and 25 miles an hour, in a course which was taking it constantly 'up hill.' I may add that on a previous occasion I have seen a far higher velocity attained by the same aerodrome when its course was horizontal.

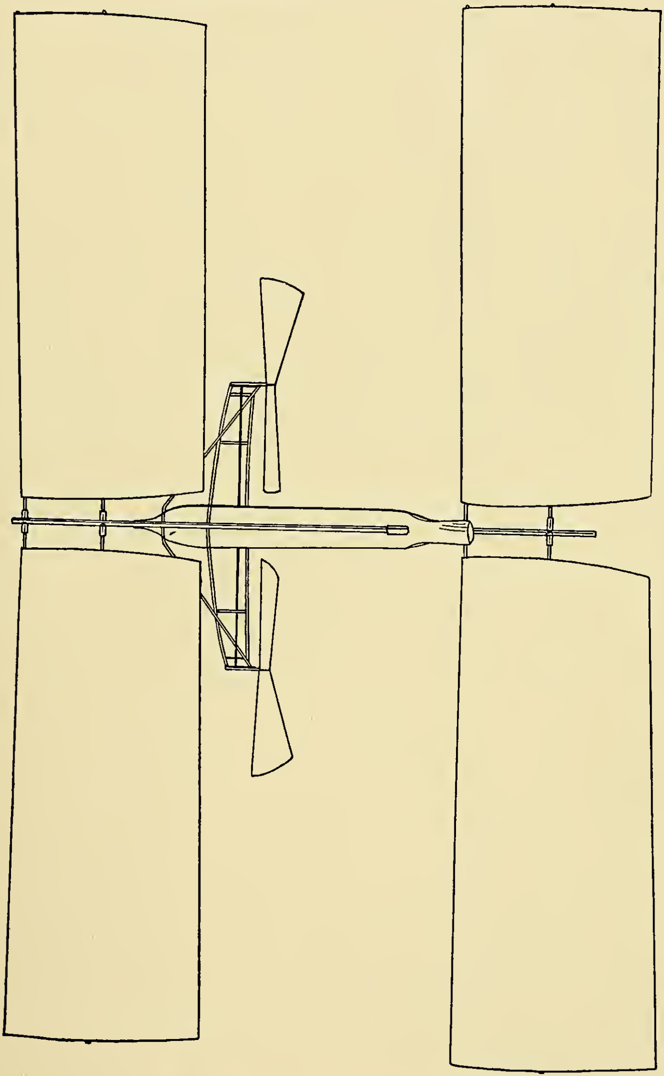
"I have no desire to enter into detail further than I have done, but I can not but add that it seems to me that no one who was present on this interesting occasion could have failed to recognize that the practicability of mechanical flight had been demonstrated.

"ALEXANDER GRAHAM BELL."

Not long after the May experiments Dr. Langley went abroad for needed rest and recreation, and in the autumn, after his return, further experiments were tried. On the 28th of November a flight was made which was more than three-quarters of a mile in length, the time occupied being precisely one minute and three-quarters. Mr. Frank G. Carpenter was a fortunate witness of this, the longest flight ever made, and with Dr. Langley's approval he wrote a detailed account of it for the Washington Star of December 12, 1896. His article is interesting from beginning to end.



AERODROME NO. 5.



AERODROME NO. 5.

SCALE DRAWINGS OF LANGLEY'S AERODROME NO. 5.

AERODROME NO. 5 (1896).

Dr. Langley has two successful aerodromes, No. 5 and No. 6; the former made the flights of May 6 and the latter that of November 28. The Plate gives scale drawings of No. 5. The weight of this, with fuel and water sufficient for the flights described, is about 30 pounds. The weight of the engine and boiler together is about 7 pounds. The power of the engine under full steam is rather more than 1 horsepower. There are two cylinders, each having a diameter of $1\frac{1}{4}$ inches. The piston stroke is 2 inches. The two screws are 39 inches from tip to tip, and are made to revolve in opposite directions; the pitch is $1\frac{1}{4}$; they are connected to the engines by bevel gears most carefully made; the shafts and gears are so arranged that the synchronous movement of the two screws is secured. The boiler is a coil of copper tubing; the diameter of the coil externally is 3 inches; the diameter of the tubing externally is three-eighths of an inch; the pressure of steam when the aerdrome is in flight varies from 110 to 150 pounds to the square inch. The flame is produced by the æolopile, which is a modification of the naphtha "blow torch" used by plumbers; the heat of this flame is about 2,000° F. Four pounds of water are carried at starting, and about 10 ounces of naphtha. In action the boiler evaporates about 1 pound of water per minute. Flights could be greatly lengthened by adding a condenser and using the water over and over again, but, as Dr. Langley says, the time for that will come later.

ON SOARING FLIGHT.

By E. C. Huffaker.

With an introduction by S. P. Langley.

INTRODUCTION.

It is generally known that birds sustain themselves in the air in two distinct ways:

First. By the direct exercise of mechanical power, as in a large class of birds that flap their wings. Although the exact motions and power of the wing have not yet been studied exhaustively, there is nothing in this method of support, considered as a mechanical contrivance, in apparent contradiction to known principles.

Second. Another and important class of birds, including the largest, can fly without flapping the wings, and are able to glide over the landscape (sometimes from horizon to horizon), on nearly motionless pinions, in a manner and with an effect which is not easily explained on known mechanical principles, and which is in striking contrast with the labored way of other birds. This manner, which has never yet been completely accounted for, and which is called "soaring flight," forms the special subject of the following article.

In this latter case the bird is in some way held up, as though by an invisible hand, upon the thin and yielding air, on which it seems to float almost like a ship, although its specific gravity is nearly a thousand times as great as that of the air, far greater, in proportion, than that of a ship of solid lead or gold would be to water.

There is no obvious explanation of this soaring flight, nor has any yet been offered which is not open to some objection. Passing by the childish idea of the support being derived from the lightness of the birds' hollow bones, or quills, we find ourselves restricted to a very few hypotheses indeed.

Perhaps the first of these is that the bird is everywhere upborne by invisible ascending currents. Without in any way denying that such currents exist or that the bird may frequently utilize them, it seems almost superfluous to enter upon a refutation of the idea that these are universally present, even if we allow that they can have ascensional force sufficiently to sustain such masses in the air. "What goes up must come down," and there must be areas where the currents are descending to supply the void. But perhaps a better answer lies in the citation of the simple and most familiar fact of observation; that the great soaring birds (which are chiefly of the vulture class) are found uniformly suspended, frequently in large numbers, above the carrion or other object of their interest, wherever it lies, and it is too grotesque a supposition that an ascending current capable of sustaining them should always emanate from such a source. The hypothesis of such ascending currents is not, then, irrational as a partial explanation, but wholly insufficient as a complete one.

The next which arises, and which to an observer untrained in mechanics seems extremely plausible, is that the wind holds the bird up as it bears up a kite. The sight, familiar from childhood, of kites sustained at great heights in the air without any power emanating from themselves is perhaps responsible largely for this delusion; for it is one to suppose that the bird, not upheld by any string, visible or invisible, actual or virtual, can sustain itself in a wind, at least if the wind be what it has until lately been treated as being, a nearly homogeneous moving mass of air, with occasional little eddies or disturbances which do not affect its fundamental quality of a current flowing altogether, like a river or a tide.

It is absolutely contradictory to mechanical principles, however, that in such a uniformly moving mass of air a kite or any other body without internal power or external support, or any bird on rigidly extended pinions, can sustain itself except momentarily, any more than in an absolute calm. The fact, however, that the soaring birds very rarely indeed perform their special evolutions except in a wind, and do have to resort to flapping their wings in a calm, is so obvious that many writers have tried to persuade themselves that in some way or other well-known laws can be evaded, and that the birds can continuously soar in such a wind by a power derived in some way from it. I think it superfluous to do here more than repeat that such action is mechanically impossible.

Next, it is indeed true that if there be two winds, or two strata of a wind, moving at different velocities, it is in this case mechanically possible that the evolution can be performed, and this Lord Rayleigh has pointed out. Though this is a true cause as far as it goes, it seems hardly necessary to say that it can account but for a very limited portion of the actual phenomena.

Another hypothesis, in accord with mechanical principles, and by which the work of supporting the bird can be derived from the wind in which it moves, has been put forth by the writer, after a study of the internal movements of the wind, which he has shown by much experiment, are incomparably more complex than had been supposed before attention had been brought to them; movements whose possible effect may be illustrated in this untechnical article, by saying that if we could see the wind, it would not appear a smooth-flowing tide like the Gulf

Stream, but rather seem broken into infinitely varied internal[1] movements like the rapids below Niagara, some of which are often opposed to the movement of the main current which bears them on, and by means of which internal movements it is quite possible in theory that work may be done sufficient to bear a vessel against the main current itself.

The attention of the reader who is interested in the matter may be again called to the fact that the present writer does not conceive this to be the sole sufficient cause, in the sense that the bird uses it to the rejection of aid from ascending currents and the like, where they present themselves; but while there is no doubt that the wind's internal horizontal movements are often alone sufficient to sustain a bird, it is difficult to believe that this cause can account for *all* the flights we see performed, either at great altitudes, where we must suppose the wind relatively uniform, or in wind of such a small velocity that it is hard to suppose that the bird can support its weight with as little work as would then be furnished by the still smaller variations.

It is indeed possible that as we further study that most marvelous structure, the bird's wing, we may find it capable of utilizing power latent in these internal movements of the wind, in a different way than we now fully understand, and in a degree greater than now seems possible. It remains, nevertheless, true that this hypothesis (of the internal work of the wind), the last which seems to offer itself, is, if trustworthy in theory and able to account for much of what we see, yet apparently insufficient in some instances of the kind we have just noted.

We seem, then, to have exhausted every suggestion, and yet the soaring bird still soars, and remains sustained in midair almost without an effort, as anyone may see in the regions it frequents. Under these circumstances we may feel justified in receiving at least with considerate examination, a new hypothesis which does not necessarily violate any mechanical principle, and which, though it may at first have a certain artificial appearance, seems not unsupported by some facts of observation.

Its author, Mr. Huffaker, is one of the most acute observers of this class of phenomena whom I have known, and I put trust in the good faith with which he reports his observations, and in the conscientious care with which he has made them. What he has to say about his actual observations is at any rate, then, worth the attention of those interested in the subject. I do not make myself responsible for the validity of his suggested hypothesis, but it seems to me novel, not in contradiction of any mechanical principle, and deserving of consideration, if only because it appears to apply to some cases where every other seems to have failed.

S. P. LANGLEY.

SMITHSONIAN INSTITUTION, *January, 1898.*

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[1] The "Internal Work of the Wind." Smithsonian Contributions to Knowledge, Vol. XXVII, 1893. No. 884.

HYPOTHESIS OF UNSTABLE EQUILIBRIUM IN THE LOWER ATMOSPHERE.

I wish to offer an explanation of some of the seemingly inexplicable phenomena referred to by Mr. Langley in his introduction to this paper under the name of soaring flight, by which term may be included all sustained flight which is accomplished without flapping. This is the characteristic flight of the eagles, hawks, vultures, albatrosses, and the frigate bird, which may daily be seen sweeping through the air on nearly motionless wings, making long journeys, ascending to great heights, and in some undiscovered way keeping aloft in apparent defiance of the laws of gravity, for they so seldom flap their wings, and move them so little, that the power visibly expended seems wholly inadequate to the purpose of keeping their heavy bodies, as it were, afloat in a fluid so subtle and yielding as the air. No hypothesis has as yet been suggested which offers a complete and satisfactory explanation of the problem. The theory of the internal work of the wind, which in the present state of our knowledge seems to offer the only solution of the problem of flight in high winds, apparently fails to account for soaring flight in very light winds, and the theory of ascending currents as hitherto advocated, while offering a doubtful solution of flight in light winds, fails completely to explain how the bird soars in high ones. It does not seem to me likely that any single explanation can be found which will adequately account for flight in both light and heavy winds; by which I mean that we may be forced to recognize that one method must be employed for light winds and another and totally different method for high winds. Our theories must be formed in accordance with facts, but I believe there are two important but not obvious facts which have hitherto escaped observation and which I shall endeavor to establish. The first is, as I have just said, that the bird employs a distinctively different method for soaring in light winds from that used in heavy ones; and the second, and the more important fact of the two, is that this method reposes on a widely acting cause, which has never yet been connected with the observations in question.

Incredible as it may seem to those who have been denied the opportunity for observing the soaring birds, the stronger the wind blows, within certain limits, the more readily can the bird penetrate it. Thus the soaring bird can maintain a steady advance in a direct line into a wind having a velocity of 40 miles an hour, without flapping and without loss of elevation, and this it may continue to do indefinitely. Such paradoxical cases however have been considered and explained by Mr. Langley in his treatise on the "Internal work of the wind."

I wish here to point out the fact that if the wind have a velocity of only 4 or 5 miles an hour it lies wholly beyond the power of the bird to penetrate the wind at all in the manner just described. When he attempts to do so—that is, to sail straight ahead—he invariably descends, and if he continues to do so without flapping he must quickly and inevitably come to the earth, and though he may, and sometimes does, soar continuously for hours in a wind whose velocity does not exceed 4 miles an hour, and may do so without flapping and without apparent effort, he can accomplish this only in one way, by frequently soaring in circles. Whenever he wishes either to maintain his elevation or to increase it he must resort to circular or spiral flight, by which maneuver he may rise to incredible heights; but whenever he wishes to traverse the country in direct flight he is constrained to do so by descending, the distance to which he may sail depending upon the height to which he may previously have risen. In high winds, therefore, the bird need never soar in spirals, and indeed it seldom does; whereas in light winds there is no other way in which it can remain long in the air. Our task will therefore be an easier one if we recognize at the outset that there is a radical difference in the character of flight in light and strong winds, and that nature does not demand of us a single method of universal application. In presenting a new hypothesis as a possible explanation of soaring flight in light winds, I accordingly wish to state distinctly that it is applicable only to spiral flight, and does not apply to direct flight in strong winds.

My hypothesis is based upon the fact, as I believe it to be, that when the surface of the earth is exposed to the heating influence of the sun the normal condition of the lower atmosphere is one of unstable equilibrium; by which I mean that the rate of decrease in temperature in the lower strata is greater than 1 degree Fahrenheit for each 183 feet of ascent, this being the rate of decrease when the atmosphere is in neutral equilibrium. This unstable equilibrium results from the viscosity of the air and the diffusion of gases, in consequence of which the warm air near the surface of the earth does not readily penetrate the colder strata above, and there is thus a tendency at all times toward an accumulation of heat in the lower strata.

In order to fix our ideas we may consider a specific, if imaginary, case, and suppose that on a clear, calm morning a large, level field, surrounded on all sides by woodlands, is exposed to the influence of the sun's rays; and further, that during the preceding night the condition of the atmosphere has become one of stable equilibrium. As the air in contact with the earth becomes heated it will tend to rise in slender streams or bubbles from the irregularities of the surface, and being lighter than the encompassing colder air, it will be carried upward, the velocity of its ascent and the height to which it will rise depending in part upon its temperature and in part upon the resistances to be overcome, which are due partly to the displacement of the overlying masses of the air and partly to friction. The frictional resistances, though readily overcome where large volumes are involved, are greatly increased when a mass of air is broken up into smaller masses, as seen in the case of large and small bubbles of air rising through water. The tendency of a mass of air to disintegration and diffusion when penetrating strata in a state of stable equilibrium is very marked, as seen in the spreading and disappearance of smoke from locomotives and furnaces, which, though possessing a very high temperature, rises more and more slowly as it becomes diffused through the surrounding air. The extent to which this diffusion is carried by nature we have no means of ascertaining, but if it extends to the ultimate molecules of the air the further rise of the heated air must be almost wholly checked. The air, then, rising from innumerable points of the heated surface of the field, is diffused through the overlying masses, whose temperature is thus slowly increased until a layer of constantly increasing depth is brought to a condition of unstable equilibrium, while the air above remains in the stable condition. The diffusion of a rising mass of warm air will, I think, be very much less while passing through strata whose equilibrium is unstable, for when the air is in the stable condition it resists any force tending to displace it, and, if displaced vertically in any manner, tends to return to its original position; whereas with an unstable equilibrium, if the air is displaced vertically, its tendency is to continue moving in whatever direction it may be going, whether up or down, and the displaced air, instead of retarding the moving mass, tends to move on with it, so that the mass tends all the while to increase in volume, and, meeting with little resistance, loses little of its mass by diffusion. We are thus led to the singular conclusion that the rise of a mass of heated air from the earth's surface is most rapid while passing through the warmer stratum next the earth, and that it is checked through diffusion on entering the colder air above. As the accumulation of heat increases, the equilibrium will become more and more unstable; bubbles and streams will rise with more violence, and will penetrate farther and farther into the cold air above before being checked by diffusion, until at length the equilibrium will be entirely destroyed, the cold air from the woodlands will press in on all sides, winds will begin blowing in all directions, and the entire mass of heated air will be drained away through some forced opening in the cold strata above. Cold air will take its place, and the whole process will be repeated. The above reasoning, which is based upon the diffusion and viscosity of gases, is universally applicable under the assumed conditions; and if we were to consider the case of a hill slope instead of a plain the general course of the argument would be the same. The condition of unstable equilibrium, therefore, is the normal condition, whereas if the tension thus brought about reach the requisite degree of intensity there will result the abnormally unstable condition which it is believed by many meteorologists gives rise to cyclones, thunderstorms, white squalls, and similar occurrences. Below are given a number of quotations bearing upon the question under discussion, but in general having reference to the abnormal condition just referred to.

THE ORIGIN OF CYCLONES, THUNDERSTORMS, AND SIMILAR DISTURBANCES, AND THEIR RELATION TO SOARING FLIGHT.

The following account of experiments to determine the permeability of the air by vapor of water is taken from Espy's Report on Meteorology, 1849:

"I next took a glass tube about 2 feet long, with its internal diameter one-third of an inch, bent at one end into the form of a shepherd's crook, and hermetically sealed at the short end. The open end was plugged with chloride of calcium, the outer end of the calcium being excluded from the atmosphere. A film of air touching the chloride of calcium would be made perfectly free from vapor at one end of the tube; at the other a film of air touching the water would be as near saturation as evaporation could make it; and at a temperature of 80 degrees the pressure of the vapor at one end of this tube was near half a pound to the square inch, and at the other nothing at all. With the tube set vertically, so that the vapor of water would reach the calcium chloride plug by ascending, the evaporation at the end of three months, with a daily temperature of about 70 degrees, amounted to but one-eighth of an inch." From this and similar experiments Mr. Espy inferred, "contrary to the general belief of scientific men, that vapor permeates air from a high to a low dew point with extreme slowness, if indeed, it permeates it at all; and in meteorology it will hereafter be known that vapor rises into the regions where clouds are formed only by being carried up by currents of air containing it." Incidentally, the experiment seems to prove that convection also took place very slowly; for the result would have been the same whether the vapor reached the calcium chloride by permeation or convection.

The following abstract is from an article on meterology in the New American Cyclopædia: "The explanation, or at least the approximate cause of the fall of rain and of fitful winds, is found in the unstable condition of the atmosphere produced by the introduction into the lower strata of the vapor of water. This, with the accompanying heat, tends to expand the air, and consequently to render it lighter; and when the amount of vapor becomes sufficiently great the order of density is reversed and a state of tottering equilibrium is produced, the lower stratum tends on the least disturbances to break through into the colder."

The following statement is taken from Modern Meteorology, by Dr. Frank Waldo: "Where the addition of heat takes place too rapidly and the gradient exceeds the theoretical value, then the condition of unstable equilibrium ensues for a short time; such being the condition which Reye and others have assigned to tornadoes and thunderstorms."

In Elementary Meteorology, he says: "The principal condition for the formation of a tornado is the local unstable condition of the air, due to the abnormal heating of a mass of air either at the earth's surface or at some locality above it. The mass of air, being warmer than the surrounding air at the same level, is in unstable equilibrium, and when some slight disturbance frees it from its abnormal position it is forced upward by the pressure of the air below and around it." "Heat thunderstorms are the result of the local heating of the lower air, which makes its condition unstable."

The same author, commenting on the great Paris storm of September 10, 1896, says: "We know that when air is compressed it becomes warmer, and when it is expanded it becomes cooler, even though no heat be added or subtracted from the air mass. And the change, called adiabatic change, proceeds according to a regular law. The air pressure, and consequently the air density, decreases with the increase of altitude above the earth's surface, and so when air moves upward it expands and becomes cooler, at the rate of about 1 degree Fahrenheit for each 183 feet of ascent; and likewise it becomes warmer 1 degree for each 183 feet of descent in cases where it moves downward.

"So, then, if the temperature of a mass of air decreases 1° Fahrenheit for each 183 feet of increase in altitude, then the air is said to be in indifferent equilibrium, and any air carried upward or downward in it will remain in its new position, because its adiabatic change of temperature has been just such as to allow the air so moved to accommodate itself to the temperature of the surrounding air in its new position.

"If a mass of air decreases at a rate of less than 1° Fahrenheit for each 183 feet of increase in altitude, then the air is in stable equilibrium, and if any air is forced upward in it, it would gradually become denser than the air at its level and would sink back again to its starting place after the force which had caused it to move upward had ceased to act.

"If the mass of air decreases in temperature at a rate greater than 1° Fahrenheit for 183 feet of increase in altitude, then it is in unstable equilibrium, and if any of the air is started upward or downward it will continue so to move, as it will become lighter than the surrounding air with the upward, and heavier with the downward motion. It is on this condition of unstable equilibrium that most squalls depend for their origin and in great part for their maintenance."

Dr. Buchan, in the "Encyclopædia Britannica," says: "Whirlwinds occur where for the time the air is unusually calm and moist, and where, consequently, temperature and humidity diminish with height at an abnormally rapid rate. Whirlwinds and tornadoes have their origin in vertical disturbance of atmospheric equilibrium."

The following extracts are from the writings of Dr. Ferrel, whose theories of cyclones and tornadoes have perhaps met with more general acceptance than those of any other writer. Even Dr. Hann, who advocates a different theory of cyclones, seems to accept his theory of the formation of tornadoes, whirlwinds, and similar smaller disturbances in the air:

"The principal condition of a tornado is the unstable condition of the atmosphere, from which, with any very light disturbance, arises a bursting up of the air of the lower strata of the atmosphere through those above."

"The vertical circulation is the initial stage in the formation of a tornado, and so the tornado can not originate without the condition of unstable equilibrium which gives rise to a vertical circulation."

"In very hot, dry climates, where there is a sandy soil, sand spouts and whirlwinds are of frequent occurrence. The dry air of such climates, especially over a sandy soil, is often in a state of unstable equilibrium from the accumulation of heat on the earth's surface."

"Small waterspouts observed on seas and lakes in clear, calm, and hot weather usually arise from a state of unstable equilibrium in the lower strata of the atmosphere."

Mirage is defined by the Century Dictionary as "An optical illusion, due to the excessive bending of the light rays in traversing adjacent layers of air of widely different densities. * * * The heated earth rarifies the air in the lower strata faster than it can escape, so producing the mirage."

Professor Tait speaks of the mirage of the desert as formed by the refraction in the hot layer of air near the sand.

Deschanel says: "Mirage is explained by the heating and consequent rarefaction of the air in contact with the hot soil. The density within a certain distance of the ground increases upward, and rays traversing this portion are bent upward in accordance with the general rule that the concavity must be turned toward the denser side."

Guyot says: "The mirage is most frequent in arid plains where the soil, exposed to the burning rays of the sun, becomes intensely heated, and in consequence the strata near the ground are less dense than those above."

There thus seems to be abundant authority for the supposition that the air not infrequently exists in the condition of unstable equilibrium. And if the superheating of the lower strata of the air and the consequent unstable equilibrium which ensues is at times sufficient to produce thunderstorms, tornadoes, and mirages, it seems not unreasonable to suppose that a similar condition of unstable equilibrium, less intensified, may be of more frequent occurrence when the results are not so manifest. We are thus confronted with a question of fact about which recorded experiments give us but little information. We have been told so often that heated air will rise that it seems somewhat incredible to suppose that there are circumstances in which it will not do so. Laboratory experiments can have but little value in this connection, as it is next to impossible to reproduce the conditions as they exist in nature, nor have we the instruments for measuring with precision the small differences in temperature which suffice to produce unstable equilibrium within the walls of a laboratory. The following simple experiment, however, appears to indicate that a layer of heated air rises with difficulty from a surface. Let a quantity of tobacco smoke be gently exhaled upon the top of a table; instead of rising, as it would do if free of the table, it will remain a surprisingly long time as a dense layer upon the surface, like a fog upon a river, and perhaps for the same reason. Still, the crucial test must be applied in the open air, and I shall presently show that when such a test is applied experiment seems to favor the theory here set forth. Instead of unstable equilibrium, unstable motion would perhaps be the more accurate term, but the former will be used as sufficiently exact for our purpose.

My hypothesis is that when the air is in this condition of unstable equilibrium the soaring bird, by the mere act of moving in circles, so disturbs the equilibrium as to produce within the circle of its flight a feeble ascending current of warm air, which grows in magnitude, slowly at first but with increasing rapidity, through the pressure of outlying masses of heavier air above, until it attains a sufficient force to bear the bird up with it. Thus a natural chimney, which gradually enlarges, is produced, through which the warm air over an extended area finds the means of escape, the column thus formed often rising to a great height. The heat of the sun is stored up in vast quantities in the lower strata of the air on every warm day, and by tapping these natural reservoirs of energy the bird obtains all the power necessary for flight in light winds. The process of penetration and permeation in slender streams must at all times be a slow one, and if the lower strata receive heat from the sun and the earth more rapidly than it can in this way be carried off, an accumulation of heat must result and a correspondingly increased tension throughout the masses involved. Sooner or later the lower air must burst through the overlying masses, the violence of the resulting disturbances depending upon the degree of instability existing at the time of the upheaval. Under such conditions the bird by moving in circles either with or without flapping easily destroys the equilibrium and produces an ascending current which it utilizes in ascending along a spiral course, by which means it keeps within the ascending column.

I carefully examined the records of temperature taken by aeronauts in balloon ascents, as well as those obtained by means of kites, and these records all go to show that the normal condition of the atmosphere taken as a whole up to a height of several thousand feet is one of stable equilibrium. But it is to be noted that these temperatures have usually been recorded at great altitudes and that a stable condition of the upper air is in nowise incompatible with a condition of unstable equilibrium in the lower strata, in which the flight of the birds usually begins.* On several occasions I have found the outer air

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* Since the above was written Mr. Eddy and his assistants have made some experiments in the lower atmosphere which showed that at the time the experiments were made the equilibrium was unstable up to an altitude of 1,500 feet and much more so up to a height of 400 feet. Thus the average rate of decrease for 1,500 feet was found to be 1° for 163 feet of ascent, while for 400 feet the average was 1° for 67 feet of ascent.

at the summit of the Washington Monument 7 degrees colder than that at its base, 500 feet below, whereas neutral equilibrium would have required but 2 degrees. The equilibrium was, therefore, unstable and continued so for more than half an hour.

EXPERIMENTS IN GENERATING ASCENDING CURRENTS.

If the condition of unstable equilibrium is a possible one, and if the normal condition of the air near the earth's surface on a warm day is unstable the fact should be susceptible of verification, and I accordingly made a large number of experiments in the open air, under the conditions known to be favorable to soaring flight. On a warm day in August, with light irregular winds and intervals of calms I tried the effect of producing artificial ascending currents by means of a fan; reasoning that if the air was in a condition of unstable equilibrium I should be able, with little difficulty, to generate a current which, once started, might extend to a considerable height. These experiments were made in the open park surrounding the Smithsonian Institution building. In order to detect the presence of ascending currents I provided strands of white China silk, 1 foot in length, pulled apart so that they were as light almost as spider's webs and yet visible by irradiation in the sunlight at a considerable distance. Holding one of these strands aloft in one hand, I fanned the air upward underneath it; then liberating the strand I continued to toss the air beneath it until it was well under way upward. When once started the silk would be borne up to a height of 20, 30, 50, or 100 feet, and often to much greater heights. In numerous instances the strands were carried upward entirely out of sight, and in the bright sunlight they could be followed to a height of 200 feet, as estimated by comparison with the height of the adjacent towers of the Smithsonian Institution building. I repeated these experiments during the extremely hot days of September, 1897, choosing those localities where the ground was level, and where there were no buildings or trees near-by which might give rise to ascending currents. I succeeded in sending the strands of silk upward almost as often as I made the attempt, and by changing the locality sought to eliminate, as much as possible, the probability that the strands were caught up by currents already in existence. Often a single stroke of the fan sufficed to produce the necessary current, the silk rising steadily and often rapidly. When strands ceased to rise beyond a height of some 25 or 30 feet, I could usually send them on again by tossing the air upward beneath them with the fan, and frequently when falling they were sent upward out of sight. On one occasion a strand had risen to a height of 75 feet, and immediately afterwards a bit of thistle down, which chanced to float horizontally to the spot where I stood, turned upward without help and followed the path of the silk, continuing upward until lost to view. The current produced thus appears to have had a certain degree of permanence. On cold cloudy days no amount of effort would cause the strands to rise. It seems, therefore, that very slight disturbances, such as the circling and flapping of the bird might produce, are sufficient to produce ascending currents under the conditions which observation shows to be favorable to flight.

THE CONDITIONS RENDERING SOARING FLIGHT POSSIBLE.

We are now in a position to turn to the soaring birds and inquire if in the manner of their soaring or otherwise they furnish any evidence as to the correctness of the hypothesis here set forth. And first we shall consider the conditions of the atmosphere which. as observation shows, render soaring flight possible.

Of recent years writers upon this subject have almost universally agreed that winds are necessary to flight. But notwithstanding this widespread belief among scientific observers I venture to say that while winds may attain a velocity sufficient to furnish through their internal movements all the energy necessary for sustained flight without flapping, the birds may yet soar perfectly when the winds are in themselves too feeble to support them, and that the condition of the atmosphere which gives rise to local winds—that is, an unstable condition of the lower strata—is also the condition which renders soaring flight possible in the absence of strong winds. A light wind on this view is to be regarded not as a cause at all but as an effect, and we may, on this view, at once dismiss the question as to the power which such a wind may furnish, and look instead to the condition of the atmosphere which gives rise to it for the explanation of the flight of the birds, whose ability to soar seems to be so entirely independent of the strength of the winds that they may occasionally be found soaring in what appears to be an absolute calm. I have many times seen the turkey buzzard soaring in sheltered localities when there was no indication of any wind whatever. Early one morning, while following the railroad up the narrow valley of the French Broad River, near the Warm Springs, N. C., I saw a turkey buzzard fly from the western slope of a high ridge and begin soaring in circles above a narrow sunlit meadow upon the banks of the river, alternately beating the air violently with its wings and sailing. This he continued to do for the space of perhaps two minutes, after which he entirely ceased flapping, and rose steadily along a spiral path to a height of 300 feet, before sailing away along a direct descending course. On this occasion there was not the slightest breath of wind to be detected. Not a blade of grass moved, not a tassel of corn, not a leaf upon the trees. I came to the spot where the bird had been soaring, hemmed in by high ridges and mountains, and could nowhere detect any sign of motion in the air. On another occasion I witnessed one of these birds soar, without flapping, 30 feet above the tops of some cedars in a valley and 75 feet from where I stood on a railroad embankment, while the smoke from some neighboring chimneys rose almost vertically and no motion whatever was to be detected in the trees immediately below. Again, I frightened one from its perch upon a tree in the edge of a woodland upon the summit of a hill, at a moment when no indication of a wind could be detected in the trees. It at once began soaring, with less than a dozen beats of the wings, circling round 30 feet overhead and slowly rising. I could easily multiply similar instances, in which the bird has been seen soaring in winds so light as to be scarcely discernible and under circumstances which render it wholly improbable that any strong current of air could have escaped detection.

On the other hand, I have, on numerous occasions, seen the birds attempt soaring and fail, both singly and in companies, when light winds were blowing steadily, especially on dark days and when the earth was cold; nor do I remember at any time to have seen them soar in light winds when the ground was frozen or covered with snow, nor to have seen them attempt it.

Observation seems to show that the condition which renders soaring flight possible in light winds is that the surface of the earth shall be warmer than the air above it; and this, it will be seen, is the very condition which gives rise to light, local, irregular winds. It is not surprising, then, that the birds should begin soaring with the rising of the winds, nor that observers should have sought for an explanation of their flight in the winds themselves. But it sometimes happens that they begin soaring before any winds can be detected, and as I have endeavored to show, it is this condition of a cold atmosphere above a warm surface resulting in an unstable equilibrium and not the prevalence of winds that makes soaring possible under these conditions. Moreover, as this condition will ensue whenever the earth is exposed to the heating influence of the sun's rays, and since, if it prevails at all it must prevail over vast regions of the earth's surface, it will be seen that it is neither an abnormal nor a local condition in those countries where the birds are known to habitually soar. This, however, is the condition requisite to flight in light winds only, for observation shows that while the birds may usually soar in light winds, they may invariably do so in heavy winds. High winds, therefore, also furnish conditions rendering such flight possible.

But, as already pointed out, there is a marked difference in their manner of soaring under these different circumstances, and it will accordingly be convenient to consider the subject under two divisions—spiral flight in light winds; direct flight in heavy winds—and to include under the latter a supplementary discussion of flight in winds of moderate velocity.

SPIRAL FLIGHT IN LIGHT WINDS.

If when the winds are light a score of vultures, including turkey buzzards and black vultures, be frightened from a carcass they will immediately take wing, flying near the earth and flapping vigorously.

They will not, however, disperse, as a covey of partridges would do, but will range back and forth, covering an area of perhaps from 5 to 10 acres of ground. Flapping flight will alternate with downward-gliding flight, and whenever in gliding a vulture approaches the earth it will be seen to rise again by turning upon a curve and flapping. Within a very few minutes the birds will be found ranging less widely, their flight will become more circular, stragglers will be drawn in, and the flock will unite into a more compact mass, covering perhaps an acre of ground or less, and will move in circles varying from 40 to 200 feet in diameter. The entire flock will slowly rise to a height of from 100 to 200 feet, the amount of flapping becoming noticeably less. After reaching a certain altitude the flapping will entirely cease, somewhat suddenly, and after that, it matters not how high they may rise, not a bird, as a rule, will flap again. After rising to a height of perhaps 600 to 1,000 feet they will simultaneously disperse in all directions, no two birds keeping company, and all gliding downward in direct lines toward the earth, the altitude gained enabling each bird to glide a distance of a mile or more without effort of any kind.

Following the flight of any one of the birds which have thus become dispersed it will be found to make its way toward any flock which it may see soaring. But if none is to be seen it will, on approaching the earth, select a spot from which to rise again as before, but it will rarely be seen to flap its wings again. If other birds are in the neighborhood they will join the one that is soaring and all will rise together. The spot selected may be a hillside, hilltop, valley, or plain, and the birds will ascend with equal ease in any case, the selection of a spot from which to rise being apparently a matter of indifference to them. Given the condition of a warm surface beneath them and they will quickly begin soaring when frightened from a carcass, usually drifting with the wind, which is here assumed to be light, and rising within a very few hundred feet of the spot; and this they will do day after day, wherever the carcass may be located. But soaring above a carcass forms but a small fraction of their daily flight, by far the greater part of their time being spent in searching for food; and since they must be at all times either rising in circles or descending at the rate of from 1 to 3 feet per second it will be seen that when they have mounted to a height of from 500 to 1,000 feet, beyond which height the smaller vultures in temperate climates do not habitually rise, it will take them but a few minutes to reach the earth again if they cease to move in circles. Their descending flight is usually in a direct line, or in two or three broken lines, and they will continue to descend until within perhaps 200 or 100 feet of the earth. Allowing that the rate of descent is but 2 feet per second, a bird will then have but little more than one minute of time in which to select a spot from which to rise again, for it is certain that if he continues to follow a straight course he will within less than two minutes land upon the earth. Very often he will approach within 50 feet of the earth before attempting to rise, and yet with not more than thirty seconds of time remaining he will select a spot and perhaps without a single beat of the wings once more begin rising. Furthermore, his object when near the earth is apparently to search for food, not to find a spot from which he may rise; and if at any time he detects a carcass, or fancies that one may be concealed beneath him, he will at that spot turn upon a curve and begin an investigation, circling round and round and meanwhile maintaining his elevation, so that the same maneuver which enables him to investigate the locality also enables him to remain in the air. When we consider that he is under the necessity of finding a new locality from which to rise on an average of perhaps once in every ten minutes during several successive hours, and that when he is near the earth a suitable spot must be found quickly or he must resort to flapping, and further, that he rarely finds it necessary to flap, it seems clear that he must in some way be able to rise almost when and where he will. How this is accomplished is easily explained upon the hypothesis of rising currents artificially produced when the air is in a state of unstable equilibrium, and as already stated it seems impossible to find a satisfactory explanation on any other hypothesis hitherto advanced.

TENDENCY TO DRIFT INTO ASCENDING CURRENTS.

Early in the morning the fine sands of the Modjave Desert rise in slender columns to a height of 1,000 feet, being fed doubtless by the stratum of heated air near the surface, and we have here almost positive proof of unstable equilibrium long continued, the warm air flowing underneath the cold air above in order to reach an ascending column, for if the air near the surface were not warmer than neutral equilibrium demanded it would not rise at all, and as it appears to rise only in widely separated localities, as indicated by the columns of sand, at other points the rate of decrease in temperature must be greater than that which gives rise to neutral equilibrium; and yet the air does not rise at those points. It is probable that similar currents of warm air are constantly being formed in the warmer sections of the country, and whenever an ascending current exists the surface winds in the vicinity will be found blowing toward it. This aids to a fuller comprehension of a difficulty which may seem to attach to the previous explanation—the difficulty, that is, of explaining how the bird finds the ascending currents. Let us suppose that on a warm day, when light, local, irregular winds are succeeded by intervals of calm, on an extended plain, a number of vultures or a pair of hawks be frightened into the air. They will soon be found soaring near the spot from which they took wing and often flapping vigorously. If there be a wind they will drift with it as they soar, and as all local winds in general blow toward a rising column of air the birds in soaring will drift toward the rising column, and if it be not too far distant will drift into it, and the difficulty of explaining how the bird finds an ascending current is in a measure overcome. Let us now apply this hypothesis to a special case, representing the typical flight of the large squirrel hawks in light local winds. On a clear, calm morning I once frightened two of these hawks from their perches on a dead tree in a level field of perhaps 20 acres in extent. They at once flew away, alternately flapping and sailing, and making no effort to keep together. At times they were on opposite sides of the field, each pursuing its own course, while ranging back and forth. After numerous preliminary attempts the two at length began soaring steadily at a distance apart of several hundred feet. While still circling, and without further flapping, the two gradually approached until the circles of their flight, which had a diameter of perhaps 75 feet, coincided. (One will watch in vain to see these hawks rise to any great height along separated spirals.) After this their flight was steadily upward until they had attained a height so great that they seemed but gray specks against the blue sky. At length they ceased circling and sailed away along separate courses. If we assume that over some spot a strong, steady current was ascending, fed from the stratum of warm air near the earth, and that from all quarters it was slowly being drawn into it, at an average velocity of 2 feet per second, then a period of eight minutes would have sufficed to draw the birds into the current from a distance of 1,000 feet. It might seem, therefore, that upon this hypothesis we have arrived at a full and satisfactory explanation of soaring flight in light winds. But such is very far from being the case, for while the supposed drifting of the birds into ascending currents doubtless accounts for many of the phenomena of flight, it leaves others wholly unexplained, for while we know but little in regard to ascending currents, either as to their number, dimensions, or duration, it is yet certain that they must be limited in all these respects, and over extended plains the element of chance must be an important factor in their location. Under such circumstances we can not suppose that ascending currents of large magnitude are to be looked for over any given spot where vultures may congregate above a carcass or that such currents are to be found at all hours of the day and for days in succession. Yet the vultures will soar day after day over such a spot. Of their ability to do so at will the following example may be given: Some vultures found the remains of a rabbit which had been rolled in the dirt by a harrow, and others seeing those upon the ground came in large numbers and soared directly over the spot for some minutes, but, not finding prey, did not come down, and rose instead and sailed away. An hour later there was a repetition of the same performance. They came a third time during the day and again on the following day. There was at no time any flapping, there were no windbreaks to produce ascending currents, and the birds in each instance soared immediately above the same spot. Mouillard thus describes the descent of the Egyptian vultures on the discovery of a carcass: "These myriads of great birds whirl like a water spout. The descent still continues in those enormous circlings, those mad wheelings which give one the vertigo merely to watch them. Sometimes those nearest, not quite sure of perfect safety, return upon an upward glide, and thus a broad, horizontal layer of vultures serves as a base for this interminable whirling column. This continues until the surrounding country has been thoroughly examined and the carcass is deemed accessible, when the hungriest dart down upon it." From this it appears that the birds in the act of descending and when restrained through fear have it in their power to stop and remain in the air indefinitely over any given locality. It is a common occurrence for numbers of vultures to be drawn together through the mere chance of obtaining a meal. Thus a single vulture, flying low and inspecting every nook below for prey, pauses to reconnoiter, and begins soaring round and round above some spot. In an incredibly short time the air will be filled with birds, all soaring and intent on finding the supposed carcass. Failing in this, the search is abandoned. Little by little the birds collect into a close cluster above the spot which chance alone selected. For a time they scarcely seem to rise at all; then they go higher, suddenly, rapidly, unexpectedly, as if caught upon a rising swell of air. For such occurrences the theory of ascending currents naturally produced fails to satisfactorily account.

BIRDS OFTEN BEGIN CIRCLING WHERE NO ASCENDING CURRENT EXISTS.

Upon the hypothesis of ascending currents artificially produced by the bird many such phenomena of flight, not otherwise easily understood, may be readily explained, as for instance the scarcely perceptible ascent in the early stages of spiral flight, with the subsequent rapid rise. If the birds availed themselves of ascending currents already established they should be found rising as soon as they entered the current; but if the current were produced by their own efforts a certain time would elapse before it attained its full force, and we should find as is actually the case, the birds suddenly and rapidly rising, after a more or less prolonged flight at a fixed elevation. The hypothesis explains also why the birds so often flap vigorously at the beginning of their flight, and, what is significant, why their flapping ceases not gradually but suddenly. It has been suggested that as the bird rises it encounters winds of greater velocity and consequently of greater buoyant power. But spiral flight occurs usually when the winds are either light or absent, and the simultaneous cessation of flapping and the steady rise which follows is commonly attained at low altitudes, nor is there any apparent difference in the character of their flight at the heights of 200 feet and 1,000 feet. Besides high winds enable the bird to soar without circling at all and seem rather to be unfavorable to any great ascent by spiral flight. Incidentally it may be stated that the soaring birds seldom if ever rise to great heights by flapping. But aside from these general considerations the birds occasionally furnish evidence of a much more convincing character. Thus on one of the calmest mornings I ever knew I saw two vultures make an attempt to soar in circles at a height of about 100 feet above the earth. In order to maintain and increase their altitude they continued to flap vigorously for about three minutes, after which, having risen to a height of 200 feet, they ceased flapping and continued rising. A third vulture now approached the spot, flapping as it came, and began soaring immediately beneath the other two, over the same spot above which their flight began, but at a lower elevation. But unlike the other two, it did not flap at all, but rose steadily from the first. The elevation in this case evidently had no influence on the character of the flight, and the fact that the last vulture soared steadily at the exact spot where three minutes before the two others were unable to soar at all offers strong presumptive evidence that the first two had effected some change in the condition of the atmosphere at that spot before the third one entered it. On another occasion, in a moderate wind, three vultures being frightened from a carcass, began soaring in circles near the earth above a meadow. During the early part of their flight they flapped vigorously, but after a time ceased flapping entirely, and rose steadily along a course slanting with the wind. When they had reached a considerable altitude a fourth passed beneath them in direct flight, and continued on until it reached the identical spot at which the three had begun their ascent by flapping. It at once began soaring and rising rapidly, but was not under the necessity of flapping as the others had been. As it ascended it followed directly in the course the others had taken.

On another occasion with light winds prevailing I witnessed the ascent of two flocks of vultures near the Dan River, Virginia. The first flock, which numbered fourteen birds, rose after some preliminary flapping from a wide alluvial plain near the banks of the river. The second flock, numbering fifteen birds, rose from the slope of a hill about 3,000 feet distant. After each flock had continued soaring for some minutes, all the birds above the slope of the hill began flapping and in a very short while began to disperse. And now a singular thing took place. Each bird on leaving the spot sailed directly toward the spot from which the first flock had ascended, and each of the fifteen birds as it reached the spot, at an elevation varying from 75 feet to 200 feet, was seen to rise as if buoyed up by an ascending current, and each at once turning upon a circle began to follow the course taken by the birds of the first flock. Only two out of the fifteen flapped their wings at all and all rose steadily, and soon, with those of the first flock, formed a column of vultures, slanting in the direction the wind was blowing and reaching to a height of perhaps 1,000 feet. Here we may suppose that the vultures upon the hillside began flapping because the supply of heated air which buoyed them up became exhausted, and that being no longer supported at that point, they sought the spot from whence the first flock was to be seen rising; while the fact that each bird was successively buoyed up on reaching the spot indicated the existence of a well-established ascending current which extended to a height of 1,000 feet or more.

Again on a clear day with light winds a flock of forty vultures was frightened from a carcass on a summit of a ridge. They flew directly down a valley in a direct line with much flapping, a distance of a half mile and with a common impulse stopped and began flying in circles in a somewhat compact mass. Not one of the entire flock succeeded in soaring without flapping although they continued to make the attempt for two minutes. Evidently there was no rising current at that point and they did not succeed in making one. But it is significant that they should have selected a spot in which to soar where no current existed, for it weakens the assumption which might be made that the birds begin soaring in circles only when they encounter rising currents, and that they have some delicate sense by means of which they are able to detect them and find their way into them. At the end of two minutes the flock divided into two. The one moved a few hundred feet to the north over an open field and after some preliminary flapping soon began soaring; the other moved south and attempted to soar above a wooded slope on a north hillside, but failing in the attempt a large portion abandoned the spot and sailed away to join the first flock where they at once began soaring with the others. Those which remained, not being able to soar, finally alighted on some trees and remained there. Here again the birds which flew south endeavored to soar over a spot where, as the event proved, no rising current existed, and this a part of them continued to do in the apparent expectation that one would rise if they continued to circle long enough, their instincts not teaching them that the cold earth underneath them did not favor the production of such a current. But the greater part, seeing that the other flock had began soaring, set off at once to join them, and the fact that when they had done so they also at once began soaring shows clearly that, whether the first flock found it or generated it, they were borne up by a rising current, and that it is the condition of the air and not the form of the wing only which enables the bird to soar. That the birds are borne up by ascending currents, either naturally or artificially produced, is indicated by the not unusual sight of a bird passing in a direct line beneath another soaring in circles, the former rising and falling precisely as it enters and leaves the current.

Again, I once saw a half dozen vultures try in vain to soar above a carcass on the summit of a wooded hill. Wearying of the attempt, three of them deliberately set sail to a spot over a valley a few hundred feet distant, and with a common impulse began flying in circles. After a short time they began rising, but only rose to a height sufficient for their purpose, when they one after another set sail for the hill again.

So deliberate seemed to be their purpose, and so well executed, that for the first time it occurred to me that these birds might be masters of the situation and able to command the winds almost at their will, and that nature had stored up for their use in the heated strata near the earth inexhaustible stores of energy which they might take and utilize as their needs might require; and that they had learned, what man had never suspected, that the heated air so situated could make its escape from the earth with difficulty, and that the simple maneuver of circling over one spot facilitated its rise and produced an ascending current at the precise locality and time where it was needed. Observation thus first suggested the hypothesis that the bird had it in his power, through the unstable equilibrium of the air, to generate ascending currents; subsequent observation seemed to confirm my supposition, and later numerous experiments seemed to indicate that such currents could be produced artificially. All my observations have gone to show that the diffusion of heated air through cold air is widespread and rapid. A column of smoke from a furnace chimney a few square feet in cross section in an incredibly short time becomes so diffused as to have a cross section of many hundred square feet. In a strong wind we may all have seen dense black volumes of smoke from a locomotive vanish almost in an instant. A balloon filled with heated air rises steadily and rapidly, because its mass is kept intact, but the same air in ascending through a stable atmosphere is disrupted, diffused, and lost, and can only rise through the slow process of permeation. Furthermore, the more stable the equilibrium of the upper air the more rapidly will diffusion take place and the more slowly will the heated air rise, so that an accumulation of heated air is continually taking place in the lower strata, and the tension is only relieved at intervals with a certain degree of violence by an upheaval of the masses. For the rest, there seems to be no serious objection to the supposition that if the equilibrium is unstable the birds are able to destroy it; for almost any disturbance, and especially any so violent as vigorous flapping, would perhaps be sufficient for the purpose.

DIRECT FLIGHT IN HIGH WINDS.

The hypothesis of ascending currents is applicable to spiral flight only, and can not account for direct flight without loss of elevation, for the reason that under the actual circumstances of flight currents of the necessary magnitude and strength can not by any rational supposition be accounted for. Thus, it is incredible that the albatross, while wandering back and forth for miles across a stormy sea at an elevation seldom exceeding 100 feet, can be supported by ascending currents like those which, there is good reason to believe, carry the land birds upward in light winds. Any ascending current which might exist would of necessity be swept on by the winds which enveloped it, as tornadoes are known to be, and if the birds were swept on with it it could only be kept afloat by moving in circles so as to maintain a high velocity relative to the current, whereas in reality the bird is able to maintain a steady course into the wind, and must consequently soon pass out of any ascending current it may encounter. We of necessity, therefore, are driven to look to some other hypothesis for an explanation of those marvelous flights of the albatross and other sea birds whose evolutions fairly bewilder the beholder; and, having worked out an hypothesis which seems in harmony with all the phenomena of spiral flight, we now come face to face with a new class of phenomena and a new kind of flight under conditions which the present hypothesis does not demand. A theory has been outlined by Mr. Langley, in his treatise on The Internal Work of the Wind, which I think supplies us with a key to the mystery, and my purpose is only to point out in what way that theory finds its practical application in the flight of such birds as the albatross, which, I am informed by Professor Ridgway, never rises, as the vultures do, to great heights in circles, and which rarely, if ever, resorts to that mode of flight. M. Mouillard states that it can not soar in winds having a velocity of less than 11 miles an hour, and while Professor Ridgway is confident he has seen the smaller species soar in much lighter winds, the fact is well established that as the winds increase in violence its power of sustained flight without flapping increases accordingly. We have here to look rather to the internal movements of the wind, to its innumerable slight changes in direction and velocity, rather than to ascending masses of air, for a solution of the problem. Some experiments which I have made go to show that the changes in direction take place with astonishing rapidity, amounting usually to many times each second and varying several degrees from the general trend of the wind, and there is reason for believing that they increase in rapidity and violence with the velocity of the wind. While the bird in light winds may utilize ascending currents and avoid the descending, it is evident that in direct flight it has no such choice, and while buoyed up by the ascending it must deal with a like number which are descending and which tend to depress it. It is here that the concavo-convex form of the wing finds its advantage. Given an equal number of ascending and descending currents, beating incessantly upon the wing, the concave side will prove the more efficient surface, and there will be an excess of pressure from below upward. We see the operation of the same principle in the action of the cups of an anemometer and in the wing of the flapping birds. The effect is in reality the same whether the bird beats its wings in a still air or holds them rigid while the wind beats against them. In the first case the necessary energy is supplied by the bird, in the second by the wind, and in each case the energy expended becomes effective through the peculiar form of the wing. The albatross habitually flies low, seldom rising more than 50 feet above the water, and it might be suggested that the waves act as wind breaks to produce ascending currents, which serve to sustain the bird. But in answer we have only to point to the flight of the vultures, which, unlike the sea birds, fly high and may maintain their course uninterruptedly into a high wind at great altitudes. Nor need we, in fact, look away from the sea birds for an evidence to show that wind breaks, although often utilized by the bird, offer no satisfactory solution of the problem. Quite recently I spent some time in watching the flight of two gulls on the Potomac River. A stiff breeze was blowing, but the waves which it produced were scarcely 12 inches high—entirely too small to be effective as wind breaks—while the broad expanse of water, the level nature of the surrounding country, and the direction from which the wind came precluded any assumption of a general rising current of the air. Considered in its entire mass, there is every reason to believe that the wind was horizontal. In this horizontal wind, by rising to a height of about 40 feet the gulls were able to soar with the utmost ease, sailing steadily into the wind or across it without loss of elevation and without flapping. This and similar occurrences go to show that a strong horizontal wind, though not, so far as we have any evidence, a homogeneous one, suffices for sustained soaring flight. Only the small and rapid changes in the direction of the actual wind, whose course, in general, is horizontal, are here supposed to become effective in sustaining the bird. However, in his work on The Internal Work of the Wind, Mr. Langley, while including under this title the energy arising from all changes in the condition of the wind, whether in velocity or direction, applies the theory more particularly to a consideration of the available power resulting from changes in the velocity. In addition, then, to the energy obtained from the winds in the manner just described, there must be considered that which arises from the latter source, and all these variations taken together constitute, in their effect upon resisting bodies, the internal work of the wind.

We have finally to consider flight in winds of moderate velocity. Looking again to the birds for the facts from which to reason, we find in such winds they still soar, but in a manner which is neither that adopted in feeble winds nor in strong ones, but intermediate in character. In moderate winds the bird avails himself of the power found in ascending currents and also in the internal movements of the wind. In light winds it rises only by soaring in circles; in heavy winds this method may be, and usually is, wholly discarded, the flight becoming sustained and direct; while in moderate winds it is still under the necessity of rising in circles, but less frequently than when the winds are light, and while sustained flight in direct courses is no longer possible as in the case of high winds, it yet obtains from the winds a certain amount of uplift which renders its rate of descent less rapid than it would be in a calm or in light winds. Observation abundantly shows that in direct flight the rate of descent is indirectly proportional to the velocity of the wind. Thus if a bird in a calm descends at the rate of 3 feet per second, the rate of descent will be reduced to 2 feet per second in a wind of a certain velocity, to 1 foot per second as the velocity becomes greater, while for a sufficiently high velocity there will be no descent at all, and as the velocity still further increases the bird may ascend where before it had been descending. In direct flight in a calm the black vulture will be found to maintain his elevation only by flapping its wings one-third of the time; in a light wind it will flap less frequently; in a moderate wind still less frequently; while in a high wind it will cease flapping entirely. The supporting power of the wind thus varies not only as its velocity relative to the bird, but also as its absolute velocity. In moderate winds, therefore, the bird, if it does not flap, can only increase its altitude by soaring in circles, but in direct flight its descent is less rapid than in light winds, and it can consequently sail further and need not so frequently resort to spiral flight. But as an ascending column of air can not maintain a fixed locality, but must be carried forward with the wind, the bird, if it utilizes such currents, must also drift with the wind. This it will almost invariably be found doing when the winds are light or moderate. Thus if it be advancing into a wind it will turn upon its course and drift with it whenever it attempts to rise by soaring in circles. The bird may, of course, in high winds circle without drifting, as there is then no necessity for utilizing ascending currents, and we find in this fact a complete refutation of the supposition that ascending currents depend for their maintenance upon wind-breaks, since wind-breaks can not move across the country as ascending currents do.

As some writers have seemingly regarded the effect of wind-breaks as an important factor in the problem of flight, I may state on the authority of Professor Ridgway that on the level, treeless prairies of the West, where no wind-breaks occur, the birds soar as perfectly as in regions where wind-breaks are of common occurrence. My own observation has been that the birds habitually take advantage of existing wind-breaks, such as hill slopes, ridges, buildings, and railroad embankments, as they may often be seen following the crest of a ridge upon the windward side for long distances, but that when every allowance has been made for their possible influence the problem of flight in its essential features remains unchanged when this factor has been taken out.

What I have said seems in general to be corroborated by the observations of M. Mouillard, who says of the great tawny vulture: "If the wind be feeble he can climb into the air by circling round and drifting back somewhat. If the wind be brisk enough to sustain him thoroughly he can rise perpendicularly, facing the wind, without wheeling round, and even advance into the wind while rising. This unlimited advance against the wind current without beat of wing, without apparent effort, and almost without act of guidance, is performed every day by myriads of winged creatures; and if the reader doubts the fact he can thoroughly satisfy himself by a trip to the trade-wind latitudes."

Unquestionably the problem presented by the soaring birds is a difficult one, as the air in which the bird moves is invisible, our knowledge of its movements is limited, and the bird himself must be studied from a distance. No solution amounting to a demonstration can at present, therefore, be looked for. Nevertheless, some sort of a solution is desirable if only to give direction to our investigations, and the proposed solution here given of flight in light winds, if it should prove to be the correct one, opens up possibilities of artificial flight which can scarcely be overestimated, for the sources of power which enable the bird to fly with so little expenditure of its energies are all perhaps equally available for man in artificial flight, since the maneuver of flying in circles is very simple and is one which could be easily reproduced.

THE REVIVAL OF ALCHEMY.[1]

By H. Carrington Bolton, Ph. D.

> "Superfluous rehearsalls I lay asyde,
> Intendyng only to give trew informatyon
> Both of the theoryke and practicall
> operatyon;
> That by my wrytyng who so will
> guyded be,
> Of hys intente perfyctly speed thall he."
>
> George Ripley (1471).

Fraud, folly, and failure have been deeply written into the annals of alchemy in all ages. It was early characterized as an "art without art, beginning with deceit, continued by labor, and ending in poverty," and in modern times its extravagant pretensions have been condemned by an exact and critical science; yet notwithstanding there are to-day indications of a resuscitation of the captivating theories and of renewed attempts at their practical application, of great interest to students of the intellectual vagaries of mankind.

Belief in the possibility of prolonging life by an artificial elixir and of transmuting base metals into silver and gold was generally entertained in the Middle Ages, not only by the ignorant masses, but even by serious-minded philosophers imbued with all the learning of the time; and the popular faith was sustained by the tricks of unprincipled imposters who found it profitable to prey upon the credulity and avarice of their fellow-men. Those who in modern times have written of alchemists find in the extravagant views of a Paracelsus, and in the careers of a Flamel, a Sendivogius, or of a John Dee more entertaining materials than in the abstract conceptions of sober philosophers, and consequently most readers are more familiar with the misdeeds of adventurers than with the honest beliefs of respectable men of science. Before condemning those who labored day and night to solve the problems of transmutation and the elixir of life, we should consider their intellectual environment. Superstitious beliefs of every kind prevailed; even the sciences were in bondage; astronomy was dominated by

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[1] Read before the New York Section of the American Chemical Society, October 1, 1897. Reprinted from Science, N. S., Vol. VI, No. 154, pages 853–863, December 10, 1897.

astrology; medicine was influenced by magic; natural history was subject to blind belief in authorities, and scientific chemistry was entirely overwhelmed by the chimeras of alchemy. Kepler and Tycho Brahe, at the court of Rudolph II did not think it beneath their dignity to cast horoscopes for gain and to predict the future by consulting the positions of celestial bodies, even while formulating the laws governing their motions. European crowned heads retained astrologers and alchemists as members of their courts. A century later Sir Isaac Newton dabbled with furnaces and chemicals in true hermetic style; and Leibnitz showed the courage of his convictions by acting as secretary of an alchemical society in Germany. The influence of superstition on the mental attitude of truly great men decreased with the advancement of learning, and when the foundations of scientific chemistry were laid by Priestley, Lavoisier, Scheele, and their contemporaries the doctrines of alchemy were abandoned. And yet not wholly abandoned, for there seems to have been a small number of persons in all countries who have clung to the hope of realizing transmutation, a hope sustained by the desire to reap the golden reward. This minority rejected the extravagant belief in a life-prolonging elixir, and in the divine origin of the profound secrets of the initiated, and sought to appropriate from the growing sciences such discoveries and theories as could be interpreted in favor of transmutation.

The printing press has never ceased to issue works devoted to the subject. Some authors have written of a "higher chemistry," and others have sought to reconcile the new doctrines of chemists with the ancient theories of alchemists. As recently as 1832 a German professor wrote a learned volume with the avowed intent of proving the verity of transmutation from historical sources (Schmieder's Geschichte der Alchemie, Halle, 1832). The number of reprints of the grotesque writings of reputed adepts which have appeared since chemistry has become an exact science is surprisingly large, and the fact that they find purchasers indicates a small but zealous class of hermetic students. So eminent a chemist as Sir Humphrey Davy did not hesitate to affirm that some of the doctrines of alchemy are not unphilosophical.

Recent discoveries in physics, chemistry, and psychology have given the disciples of Hermes renewed hopes, and the present position of chemical philosophy has given the fundamental doctrine of alchemy a substantial impetus. The favorite theory of a prima materia, or primary matter, the basis of all the elementary bodies, has received new support by the discoveries of allotropism of the elements, isomerism of organic compounds, the revelations of the spectroscope, the practical demonstrations by Norman Lockyer, the experiments on the specific heat of gaseous bodies at a high temperature by Mallard and Le Châtelier, the discoveries of Sir William Crookes (as set forth in his monograph on Meta-elements), the discovery by Carey Lea of several singular allotropic forms of silver, and, most weighty of all, the mass of related facts and phenomena which find their ultimate expression in the periodic law of the elements, so that many chemists of the present day are inclined to believe in the mutual convertibility of elements having similar chemical properties. Daniel Berthelot, in his notable work entitled "De l'allotropie des corps simples," boldly affirms his belief in the unity of matter. He says: "Without seeking to find in any one of the known elements the generator of the others, can we not invoke the facts that we have revealed in our study of carbon in favor of the hypothesis of a unique matter unequally condensed?" And elsewhere he writes: "The transmutation of an element is nothing more than the transformation of the motions which determine the existence of said element and which gives it special properties into the specific motions peculiar to the existence of another element."

Simultaneously with the development of the truly scientific aspect of alchemical theory, there has arisen an extraordinary revival of the metaphysical side of the question; this goes hand in hand with the interest in chiromancy, astrology, theosophy, and occult sciences which occupy so large a place in modern thought, literature, and polite society on both sides of the Atlantic. This tendency to cultivate the esoteric manifests itself in the study of the Kabala, the investigation of the mysteries of Buddhism, Confucianism, and other oriental philosophies, in researches into the phenomena of spiritualism, so called, and in the foundation of societies to study psychic force and the tenets of the followers of Madame Blavatsky; crystal gazing, reading in magic mirrors, slate writing, planchette, the quasi-scientific study of apparitions, of table turnings, of rappings by unseen powers, of telepathy, of subliminal self, are now regarded as legitimate pursuits in no wise necessarily associated with the black arts of mediæval times, provided only they are conducted in a spirit of inquiry and for the purpose of discovering the latent power underlying these phenomena. And this line of research receives stimulus from the results secured by students of experimental psychology, of hypnotism, from such discoveries as the phenomena of the X-rays, and from the transcendental physicists who theorize on the miraculous consequences of four dimensional matter. Crowded lecture halls reward exhibitions of trance mediums, speakers on theosophy, palmistry, and occultism; in lower walks of life fortune tellers and clairvoyants reap a modest harvest; books treating of occult themes enjoy great notoriety; writers of fiction find it profitable to introduce the mysterious into the children of their brains; even secular journals, especially those of France, give space to the all-absorbing discussions on hermetism; these are some of the evidences of great popular interest in the unknowable. Only persons with special intellectual equipment are able to measure, weigh, sift, and coordinate the novel phenomena gathered by researches in the field of hypnotism, psychology, and occultism; those of weaker mental powers fail to perceive the real significance of the discoveries and are led away into unprofitable and dangerous superstitions.

In the Middle Ages alchemy was nurtured by ignorant superstition; now it is fostered by the prevalent devotion to esoteric studies; formerly the popular belief was in part supported by the fraudulent claims of impostors; now a higher standard of intelligence rejects the transparent tricks of imitators of Cagliostro. There are, indeed, occasional attempts to swindle the credulous by appeals to avarice. We read in the daily press of an American confidence man who tried to cheat a London jeweler by pretending to "multiply" sovereigns; of a vulgar scheme of fraud among ignorant tradesmen on the east side of New York City, in which lead, iron scraps, crucibles, and furnaces formed the properties; and of the larger operations of an educated French chemist who found dupes in both South and North America; but in each of these cases the severe logic of law courts intervened and abruptly discomfited the swindlers. It is not by sleight-of-hand that the revival of alchemy is now being engineered, but by a company of educated charlatans.

The movement to resuscitate alchemical doctrines and practices has been particularly successful in France, where there are to-day four societies and a "university" claiming to possess occult knowledge of hermetic mysteries. These secret societies are named "Ordre de la Rose-Croix," "L'Ordre Martiniste," "La Société d' Homéopathie Hermétique" and "L' Association Alchimique de France."

The first two of these societies seem to work on lines similar to Free Masonry, and claim that their secret mysteries were bequeathed by the last sages of Atlantis and by the Lemures to their brethren in Asia and Egypt, dwellers in sanctuaries whence issued Krishna, Zoroaster, Hermes, Moses, Pythagoras, and Plato. The priestly magi who preserved this lore in the temples of Thebes, Heracleopolis, Aphrodite, Pthah, and Serapis were succeeded by secret alchemical societies of the first centuries of our era; then followed the hermetic lodges of the Arabs, and these gave rise to the Templars, the Rosicrucians, and the Martinists.

The third society cultivates especially occult therapeutics, a system of medicine invented in the sixties by Count Cæsar Mattei, of Bologna, which unites the principles of Hahnemann with those of the Iatro-chemists, disciples of Paracelsus. This new departure in medicine publishes four monthly organs and special treatises all its own.

The Alchemical Association of France is successor to the Société Hermétique which was founded by the late Albert Poisson († 1894), also known by the pen-name Philophotes. Its seat is in Paris. The objects of the association, as set forth in its constitution, are "the theoretical and experimental study of evolution and of the transmutation of bodies. Its members, with this end in view, study the processes of the ancient alchemists and compare them with the work of modern chemists." These objects are to be accomplished as follows: "The association proposes to assist in reviving the unitary doctrines of chemistry: First, by grouping the efforts of isolated workers by means of 'L'Hyperchimie;' second, by furnishing them the aid of advanced students; third, by supplying so far as possible books and apparatus to its members. Researches of the members, when approved by the masters, should be forwarded in duplicate to the secretary-general; one will be printed in 'L'Hyperchimie,' and the other will be preserved in the archives of the association for the benefit of members, who can secure it on demand." "Candidates for admission must pass an examination in, first, the theory and history of alchemy, and, second, the elements of physics and of chemistry (without mathematics). A diploma from a normal, polytechnic, or industrial school will be accepted in place of No. 2."

The affairs of the association are controlled by the secretary-general, F. Jollivet-Castelot (of Douai), assisted by seven councilors, who hold an annual meeting. There are at present (July, 1897) two honorary members—Camille Flammarion, the popular writer on astronomy, and August Strindberg, a Swede residing in Austria, author of several hermetic essays. There are two other classes of members, masters (maîtres), who are chosen from the ordinary members of the council after an examination of their writings; and ordinary members (membres adhérents), of which the number is unlimited. Modest dues entitle the members to the organ of the association, 'L'Hyperchimie,' a monthly review of alchemy and hermetism founded in 1896.

The councilors of the Alchemical Association have combined with the active members of the other societies named to establish a Université Libre des Hautes Études. At present this includes three faculties:

1. Faculté des Sciences Hermétiques, of which the Association Alchimique forms a section. The director of this faculty is Dr. G. Encausse, and the course of instruction embraces the study of the Tarot, alchemical philosophy and practice, occultism, mysticism, Hebrew, etc. The curriculum leading to the "baccalauréat-en-Kabbale" is under the supervision of the group of esoteric students, while candidates for the degrees of master and doctor are under the direction of the Martinist Order.

2. Faculté des Sciences Magnétiques, represented by the École de Magnétisme de Paris, and under the direction of M. Durville. It has branches at Lyons, Bordeaux, and other cities.

3. Faculté Spirite, comprising several sections of spiritism.

Each faculty preserves complete independence, being "united only by moral bonds destined to hasten expansion of the rational spiritualistic movement."

The nature of the instruction given at this university will appear in the review of the philosophy of its promoters.

The leading spirits in these secret societies and in this university are as follows: F. Jollivet-Castelot, secretary-general of the Alchemical

Association, special delegate to the supreme council of the Martinists, editor of L'Hyperchimie, and author of "Comment on devient Alchimiste," "L'Hylozoisme," and other alchemic treatises; Dr. M. H. E. Lalande, whose pen name is Marc Haven; F. Ch. Barlet, author of "Essai sur l'Evolution de l'Idée." Dr. G. Encausse, who generally conceals his identity under the signature Papus, is president of the esoteric group,[1] president of the supreme council of the Martinists, and the author of sixteen treatises on hermetism and magic, among which may be named "Traité élémentaire de magie pratique." Papus is also editor of "L'Initiation," a journal devoted to theosophy, magic, and occultism, and of "Le Voile d'Isis," a weekly review of spiritualism. Stanislas de Guaita is best known as the author of "Le Temple de Satan," "Clef de la Magie Noire," and "Le Probleme du Mal," works dealing with sorcery, the astral light in man, and other mysteries. Marius Decrespe's essay on "Les Microbes de l'Astral," Paul Sédir's "Les Incantations," and Albert de Rocha's "Exteriorisation de la Motricité" are works which indicate the mental attitude of those engaged in the revival of alchemy and hermetism. A bibliography of this class of works is here out of place. A single trade catalogue enumerates one hundred and twenty titles, chiefly of recent date.

One of the oldest workers in the Alchemical Association is the "master," Théodore Tiffereau. In 1854–55 he sent to the French Academy of Sciences six memoirs, in which he claimed to have discovered a method of converting silver into gold. Tiffereau had made his experiments in Mexico at great expense, supporting himself meanwhile by taking daguerreotypes. His process was repeated at the mint in Paris before the assayer, M. Levol, but with little success. The substance of his memoirs was published in 1855 in a volume entitled "Les Métaux sont des Corps Composés;" of this a new edition was published by Lermina in 1889. Tiffereau has never abandoned his claim, and as recently as October, 1896, he addressed another memoir to the academy, in which he attempts to prove that the metal aluminum is a compound. Briefly stated, his process is as follows: He placed in a stout glass tube a piece of aluminum foil with pure nitric acid and sealed the tube hermetically. He then exposed the tube and contents to the sun's rays during two months. At the end of this time he opened the tube; it gave out an odor which he thought was due to ether, and it yielded a few grams of crystals, which he thought tasted like acetic acid. Since both ether and acetic acid are compounds of carbon, Tiffereau concluded that this element was derived from the aluminum. Analytical chemists would criticize this experiment in several points. They would say Tiffereau did not demonstrate the absence of carbon in the

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[1] The Groupe Indépendant d'Études Ésotériques has 1,600 members, 104 branches and correspondents. It embraces members of the following societies: Ordre Martiniste, Ordre Kabbalistique de la Rose-Croix, Église Gnostique, Société Alchimique de France. Membership is free.

metal used, and that he depended upon smell and taste for proofs of the carbon compounds. The tongue and the nose are incontestably useful adjuncts to the reagents of a chemical laboratory, but additional tests for ether and acetic acid would have been more conclusive. In Tiffereaü's recent writings he attributes the transmutation of a base metal into the most precious one to the action of the "microbe of gold."

For a student of chemistry to read, digest, and write down in intelligible language, in a limited space, the principles of this new school of chemical philosophers is a difficult task, even for one somewhat familiar with the literature of the ancient alchemists; consequently the following analysis falls far short of the ideal. It is properly the work of a kabalist, a theosophist, and a magician, proud designations which the writer disclaims. The modern alchemists accept all the traditions of their ancient predecessors, but give them a new significance, and interweave the novel phenomena derived from researches in pure science. They claim that during the fourteenth, fifteenth and sixteenth centuries the official schools of instruction taught exclusively the physical part of the sciences, and that the metaphysical part (which is the real life and soul of the study) has been rejected under the opprobrious name of occult science. This living aspect of science has, however, been studied in the secret societies of the initiated, which have preserved the traditions of the kabala, the mysteries of hermetism, and the practice of transmutation. The study of science is as much a religious question as an intellectual one, and worship at an altar should sustain and enlighten the worker in a laboratory. "Chemistry, alchemy, and hermetic philosophy form three steps of the ladder which leads the initiated from the laboratory, through artistic realization, to the oratory: *Labora*, *Opera*, *Ora et Invenies*."

The modern alchemists also maintain that Darwin and his disciples appreciated but a small part of the great doctrine of evolution, which should be applied to the chemical elements as well as to living beings. The starting point in the evolution of elements is the ether (the universal astral fluid of the kabalists), the infinitely divisible particles of which form chemical atoms by agglomeration. This ether is condensed energy, and hence all matter is resolved into energy.

Energy, matter, and motion form a trinity analogous to the Divine Trinity, one in substance, three in appearance. Matter is one in kind, and the diversity of chemical bodies results from differences in grouping and in motions of the constituent particles. Intelligence is allied in a mysterious way with matter and energy, forming another trinity. Every atom centralizes intelligence, is in itself a living entity, and by a process of self-evolution yields the diverse natural bodies. "Ether is the father of hydrogen, from which are derived oxygen, nitrogen, carbon, etc., combinations due to etheric vortices." "Perhaps helium should precede hydrogen." This view of matter as a living entity is greatly insisted on, and the doctrine is called Hylozoism. An alchemist who expects to succeed must possess psychic power over the atoms, so that by the action of his will they shall group themselves to form the metal desired.

Such is the physical philosophy of modern alchemists. The kabalistic philosophy is by no means so clear, being closely linked to the Tarot, which signifies the "hieroglyphs and algebraic calculations of the primordial genesis."

Students of the mystical philosophy of the Hebrews discover profound occult significance in accidental similarities of widely differing objects and phenomena. The seven planets, seven days of the week, seven colors, seven orifices in the head, seven metals known to the ancients, seven archangels, and seven infernal demons present to the truly kabalistic mind marvelous and precious analogies. In the Table of Concordance of Major Arcana these correspondences are given: Heth–Justice–Elementary existence–Nizah–Cancer–June–Hydrogen–Fire.

Jollivet-Castelot has written of Kabalistic Alchemy, and a perusal of his essays leaves in the mind of the uninitiated confused memories of colors, numbers, signs of the zodiac, alchemical operations as distillation, fixation and the like, the names of the sons of Jacob, certain precious stones, geometrical figures, Hebrew characters, Azoth, quintessence, and the devil, all discussed in a language as obscure as the symbolism portrayed. To conceal esoteric mysteries abbreviations are often used, but one does not have to be very deeply initiated to recognize in P .·. Ph .·. pierre philosophale, and in G .·. O .·. grand oeuvre.

Astral light is an important factor in modern hermetism, and is related very closely to the "radiant matter" of chemists and the "ether" of physicists. "Astral light is the universal agent, the universal plastic mediator, the common receptacle of vibrations of motion and of the phantoms of form." It is also the Od of the Hebrews and of Baron von Reichenbach; it is the great Thelesma of Hermes Trismegistus, and the control of this force constitutes the great arcana of practical magic. It heats, illuminates, magnetizes, attracts, repels, vivifies, destroys, coagulates, separates, crushes, and gathers all things under the stimulus of powerful wills; it is a perpetual and transformable vibration. Its kabalistic figure, represented by the Serpent of Theogonies, is:

$$\begin{gathered}\text{Od} = + \\ \text{Ob} = - \\ \text{Aour} = \infty\end{gathered}$$

When the universal light magnetizes the universe it is called astral light; when it forms metals it is called Azoth, or the Mercury of the Sages; when it gives light to animals it is called animal magnetism. The astral undulations determine the position of the atoms or neutralize them. Herein lies the secret of transmutation, and it becomes the privilege of the hermetist to acquire the power of controlling this agent.

The adepts in this phase of hermetism still appeal to the Tabula Smaragdina of Hermes Trismegistus as the embodiment of alchemic lore. The "Father of Alchemy," who has been identified with Canaan, Noah's grandson, invented arithmetic, geometry, astronomy, and music, taught writing to the Egyptians, and gave laws and religious rites to the people. He was perfectly acquainted with the "philosophers' stone," and, desirous that posterity should inherit the wonderful secret, he had the whole art of creating gold engraved on an emerald tablet, which was placed in his sepulcher. Many years later this was removed by Sarah, Abraham's wife, and she concealed it in a cave near Hebron. There it remained until again discovered by Alexander the Great. The inscription reads in part as follows:

> "I speak not of fictitious things, but of that which is most true and certain. Whatsoever is below is like that which is above, and that which is above is like that which is below, to accomplish the miracles of one thing. Also, since all things were made from one by the help of one, so all things are made from one thing by conjunction. The Father thereof is the Sun and the Mother thereof is the Moon; the wind carries it in his belly, and the nurse thereof is the earth. * * * This thing has more fortitude than fortitude itself, because it will overcome every subtile thing and penetrate every solid thing. By it this world was formed. Hence proceed wonderful things which in this wise were established. For this reason I am called Hermes Trismegistus, because I possess three parts of the philosophy of the whole world. What I had to say about the work of the sun is completed."

Writers on modern alchemy discuss the marvels of palingenesis, of homunculi, and of gamahes; they write of the materialization of a metal through the medium (mediumnité) of a metal; they cite the "Rules of Philalethes," the works of George Ripley and of the Cosmopolite, and refer in the same essay to Berzelius, Berthelot, and Moissan. We are told that "le diable est le singe de Dieu," and that the "Cherubim of the Ark of the Covenant symbolizes the male and female of the Universe, the Alchemical Father and Mother," by the very authors who show acquaintance with the most recent advances in pure chemistry. In liquid fluorin they perceive a realization of the Alkahest, or universal solvent, long sought by mediæval alchemists.

Accustomed to juggle with numbers, the kabalist finds abundant opportunity in the atomic weights of the elements, and he makes the most of his opportunity. When the arithmetical sum of the atomic weights of elements entering into a given compound chances to equal the atomic weight of gold, this accidental correspondence is seized upon as a pretext for claiming hermetic relationship between the two substances.

August Strindberg has devoted much study to such correspondence, and points out the following:

$$K_2MnO_4 = 197 = Au$$
$$Cu_2Cl_3 = 196 = Au$$
$$Fe_3S = 197 = Au$$
$$C_2H_5I + \frac{1}{2} Br = 197 = Au$$

He uses both 196 and 197 as the atomic weight of gold to suit his purposes, and seems to be very weak in arithmetic, for the hypothetical body Fe_3S has a molecular weight of 200.

The ammonium-ferrous-sulphate crystallized with six molecules of water, which chances to have a molecular weight equal to that of gold, is used by Strindberg as basis of the following experiment, which serves to show his method of reasoning and of operating.

Ammoniacal sulphate of iron $= 392 = Au$ is perhaps the solution of the enigma. Sulphate of iron (green copperas) precipitates solutions of gold. To precipitate, according to monist-chemistry, is to enter as a factor into the reconstitution of the body of a compound. Soak a strip of paper in a solution of sulphate of iron, and expose to the fumes of ammonia, the paper will assume a bluish-green color, like that of the protoxide of gold. Dry the strip of paper over a lighted cigar and the paper will acquire a chestnut-brown color, like that of the deutoxide of gold. Little by little metallic flakes of a golden-yellow color appear, forming a nonsolid (non-fixé) gold, when the sulphate of iron produces an auto-fecondation by self-precipitation. However, the golden flakes amalgamate with mercury, which property is not shared by iron. After showing by appropriate tests that iron is still present, the hermetic chemist proceeds to explain the reaction by assuming the formation of the hypothetical $Fe_3S = 196 = Au$, or of the imaginary compound

$$Fe_2O_3 + 2H_2O = 196 = Au,$$

or of the ferrous-ammonium-sulphate $= 392 = Au_2$; and he adds: "The chloride of gold is reduced by the nicotine of the cigar." Since, however, no reagent containing chlorine in any form was used in the experiment, this element must have been created at the same time with the gold, which, however, is "incomplete" gold, soluble in unmixed acids.

A preacher should never be judged by a single sermon, and to do justice to these nineteenth century alchemists one more "recipe for gold" may be transcribed: "Put into a crucible layers of sheet iron and of powdered vitriol; place over it another crucible pierced with a hole for respiration. Heat in an intense fire. But a flux must be added to the crucible to prevent melting, viz, 1 kilo litharge, 1 kilo clean white sand, mix and add to the crucible at a red heat. Remove with an iron spoon the yellow oil and put it aside. The two compounds have not lost weight. This oil is dry water, a fire, a salamander. * * * You obtain a metal of a golden yellow, having a density of 24, not capable of being minted. This is changed into ordinary gold." With the exception of a few unimportant sentences, this is the entire recipe, but how the final transformation is to be effected is not given.

To acquire knowledge and power for successful hermetic labors, to become eligible for initiation in the occult societies, is no easy task. The aspirant must strive valiantly against the passions that assail him, casting out of his soul pride, anger, jealousy, hatred, avarice, hypocrisy, idleness. If the candidate for honors desires to become worthy of the name hermetic philosopher he must prove himself a Magian; he must learn to exercise his will on matter in all its forms, and to acquire this power he should practice crystal-gazing and reading in magic mirrors; to learn to perceive the invisible he must withdraw from the visible, imposing on himself psychic sleep, called by some hypnosis. As adjuncts to the attainment of the ideal mental state he should use perfumes, music, and light; and eventually the astral body, separated from the physical body, will supply the intellectual, moral, and material illumination indispensable to the great work.

It is rather discouraging to learn that, even after fulfilling all these hard conditions, no one can realize the perfection desired until he has passed through several of our planetary existences. The would-be alchemist must also follow the percepts of Albertus Magnus; he should be discreet, silent, and must not reveal the result of his labors; he must reside in an isolated place, and choose the time and the hours for his tasks; he must be patient, assiduous, and persevering, and he must be rich enough to bear the expenses of his pursuit. Besides ordinary chemical apparatus, he should provide several objects indispensable to his work, a magic wand, a sword to dissolve the astral coagulations, a magic mirror, a brazier for perfumes, a wooden altar covered with a white cloth, and an alchemist's robe of white linen to be worn with a girdle embroidered in gold and silver. In all his chemical operations he must project psychic force into the reagents.

Bright prospects for the future of chemical science are claimed by this school of philosophers. Inorganic chemistry is destined to follow the lines in which inorganic chemistry has prospered; the formation, derivation, or rather the evolution of metalloids (so called) and of metals will be realized through etheric cyclones, different degrees of condensation of hydrogen. Chemical bodies are of one kind only, and they are all organic and living.

There is a growing belief among advanced chemists in the theory that the elementary bodies as known to us are compounds of a unique primary matter (protyle), and that transformation of one kind into a similar one is not beyond the bounds of possibility, but we do not think that the modern hermetists are pursuing the right path to accomplish this end; nor do we believe that the world of science is any nearer the coveted goal of alchemical avarice.

DIAMONDS.[1]

By WILLIAM CROOKES, F. R. S.

It seems but the other day I saw London in a blaze of illumination to celebrate Her Majesty's happy accession to the throne. As in a few days the whole Empire will be celebrating the Diamond Jubilee of our Queen, who will then have reigned over her multitudinous subjects for sixty years, what more suitable topic can I bring before you than that of diamonds? One often hears the question asked: "Why Diamond Jubilee?" I suppose it is a symbol intended to give a faint notion of the pure brilliancy and durability of the Queen's reign; and in thus associating Her Majesty with the precious diamond, to convey an idea of those noble qualities, public and private, which have earned for her the love, fealty, and reverence of her subjects.

From the earliest times the diamond has occupied men's minds. It has been a perennial puzzle—one of the riddles of creation. The philosopher Steffans is accredited with the dictum that "Diamond is quartz which has arrived at self-consciousness;" and an eminent geologist has parodied this metaphysical definition, saying, "Quartz is diamond which has become insane."

Professor Maskelyne, in a lecture "On diamonds," thirty-seven years ago, in this very theater, said: "The diamond is a substance which transcends all others in certain properties to which it is indebted for its usefulness in the arts and its beauty as an ornament. Thus. on the one hand, it is the hardest substance found in nature or fashioned by art. Its reflecting power and refractive energy, on the other hand, exceed those of all other colorless bodies, while it yields to none in the perfection of its pellucidity;" but he was constrained to add, "The formation of the diamond is an unsolved problem."

Recently the subject has attracted many men of science. The development of electricity, with the introduction of the electric furnace, has facilitated research, and I think I am justified in saying that if the diamond problem is not actually solved, it is certainly no longer insoluble.

GRAPHITE.

Intermediate between soft carbon and diamond come the graphites. The name graphite is given to a variety of carbon, generally crystalline, which in an oxidizing mixture of chlorate of potassium and nitric

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[1] A lecture delivered at the Royal Institution, June 11, 1897, by William Crookes, F. R. S. Printed in Nature, No. 1449, vol. 56, August 5, 1897.

acid forms graphitic acid easy to recognize. Graphites are of varying densities, from 2 to 3, and generally of crystalline aspect. Graphite and diamond pass insensibly into one another. Hard graphite and soft diamond are near the same specific gravity. The difference appears to be one of pressure at the time of formation.

Some forms of graphite exhibit a remarkable property, by which it is possible to ascertain approximately the temperature at which graphites were formed, or to which they have subsequently been exposed. Graphites are divided into "sprouting" and "nonsprouting." When obtained by simple elevation of temperature in the arc or the electric furnace they do not sprout; but when they are formed by dissolving carbon in a metal at a high temperature, and then allowing the graphite to separate out on cooling, the sprouting variety is formed. One of the best varieties is that which can be separated from platinum in ebullition in a carbon crucible. The phenomenon of sprouting is easily shown. Place a few grains in a test tube and heat it to about 170° C., when it increases enormously in bulk and fills the tube with a light form of amorphous carbon.

The resistance of graphite to oxidizing agents is greater the higher the temperature to which it has previously been exposed. Graphites which are easily attacked by a mixture of fuming nitric acid and potassium chlorate are rendered more resistant by strong heat in the electric furnace.

I will now briefly survey the chief chemical and physical characteristics of the diamond, showing you, by the way, a few experiments that bear upon the subject.

COMBUSTION OF THE DIAMOND.

When heated in air or oxygen to a temperature varying from 760° to 875° C., according to its hardness, the diamond burns with production of carbonic acid. It leaves an extremely light ash, sometimes retaining the shape of the crystal, consisting of iron, lime, magnesia, silica, and titanium. In bort and carbonado the amount of ash sometimes rises to 4 per cent, but in clear crystallized diamonds it is seldom higher than 0.05 per cent. By far the largest constituent of the ash is iron.

The following table shows the temperatures of combustion in oxygen of different kinds of carbon:

	°C.
Condensed vapor of carbon	650
Carbon from sugar, heated in an electrical furnace	660
Artificial graphites, generally	660
Graphite from ordinary cast iron	670
Carbon from blue ground, of an ochery color	690
Carbon from blue ground, very hard and black	710
Diamond, soft Brazilian	760
Diamond, hard Kimberley	780
Bort from Brazil	790
Bort from Kimberley	790
Bort, very hard, impossible to cut	900

At the risk of repeating an experiment shown so well at this table by Professor Dewar, I will heat a diamond to a high temperature in the oxyhydrogen blowpipe and then suddenly throw it in a vessel of liquid oxygen. Notice the brilliant light of its combustion. I want you more especially to observe the white opaque deposit forming in the liquid oxygen. This deposit is solid carbonic acid produced by the combustion of the carbon. I will lead it through baryta water, and you will see a white precipitate of barium carbonate. With a little more care than is possible in a lecture I could perform this experiment quantitatively, leading the carbonic acid and oxygen, as they assume the gaseous state, through baryta water, weighing the carbonate so formed, and showing that 1 gram of diamond would yield 3.666 grams of carbonic acid—the theoretical proportion for pure carbon.

Some crystals of diamonds have their surfaces beautifully marked with equilateral triangles, interlaced, and of varying sizes. Under the microscope these markings appear as shallow depressions sharply cut out of the surrounding surface, and these depressions were supposed by Gustav Rose to indicate the probability that the diamonds at some previous time had been exposed to incipient combustion. Rose also noted that striations appeared on the surface of diamonds burnt before the blowpipe. This experiment I have repeated on a clear, smooth diamond, and have satisfied myself that during combustion in the field of a microscope, before the blowpipe, the surface becomes etched with markings very different in character from those naturally inscribed on crystals. The artificial striæ are cubical and closer massed, looking as if the diamond during combustion had been dissected into rectangular flakes, while the markings natural to crystals appear as if produced by the crystallizing force as they were being built up.

I exhibit on a diagram a form of graphite from the Kimberley blue ground (reproduced from M. Moissan's work), which in its flaky crystalline appearance strangely resembles the surface of a diamond whose internal structure has been partially dissected and bared by combustion. It looks as if this piece of graphite was ready to separate out of its solvent as diamond, but owing to some insufficient factor it retained its graphitic form.

PHYSICS OF THE DIAMOND.

The specific gravity of the diamond is from 3.514 to 3.518. For comparison, I give in tabular form the specific gravities of the different varieties of carbon:

Amorphous carbon	1.450 to 1.700
Graphite	2.110 to 3.000
Hard gas coke	2.356
Bort	3.470 to 3.490
Carbonado	3.500
Diamond	3.514 to 3.518

The following table gives the specific gravities of the minerals found on the sorting tables. I have also included the specific gravities of two useful liquids:

Hard graphite	2.5
Quartzite and granite	2.6
Beryl	2.7
Mica	2.8
Hornblende	3.0
Methylene iodide	3.3
Diamond	3.5
Thallium lead acetate	3.6
Garnet	3.7
Corundum	3.9
Zircon	4.4
Barytes	4.5
Chrome and titanic iron ore	4.7
Magnetite	5.0

This table shows that, if I throw the whole mixture of minerals into methylene iodide, the hornblende and all above that mineral will rise to the surface, while the diamond and all minerals below will sink to the bottom. If I now take these heavy minerals, and throw them into thallium lead acetate, they will all sink except the diamond, which floats, and can be skimmed off.

The diamond belongs to the isometric system of crystallography. It frequently occurs with curved faces and edges. Twin crystals (macles) are not uncommon. Having no double refraction, it should not act on polarized light; but, as is well known, if a transparent body which does not so act is submitted to strain of an irregular character, it becomes doubly refracting, and in the polariscope reveals the existence of the strain by brilliant colors arranged in a more or less defined pattern, according to the state of tension in which the crystal exists. Under polarized light I have examined many hundred diamond crystals, and, with few exceptions, all show the presence of internal tension. On rotating the polarizer, the black cross, which is most frequently seen, revolves round a particular point in the inside of the crystal, and, on examining this point with a high power, we see sometimes a slight flaw, more rarely a minute cavity. The cavity is filled with gas at an enormous pressure, and the strain is set up in the stone by the effort of the gas to escape.

It is not uncommon for a diamond to explode soon after it reaches the surface, and some have been known to burst in the pockets of the miners or when held in the warm hand. Large crystals are more liable to burst than smaller pieces. Valuable stones have been destroyed in this way, and it is whispered that cunning dealers are not averse to allowing responsible clients to handle or carry in their warm pockets large crystals fresh from the mine. By way of safeguard against explosion, some dealers embed large diamonds in raw potato to insure safe transit to England.

In the substance of many diamonds we find inclosed black uncrystallized particles of graphite. There also occur what may be considered intermediate forms between the well-crystallized diamond and graphite. These are "bort" and "carbonado." Bort is an imperfectly crystallized diamond, having no clear portions; therefore it is useless for gems. Bort is frequently found in spherical globules, and may be of all colors. It is so hard that it is used in rock drilling, and when crushed it is employed for cutting and polishing other stones. Carbonado is the Brazilian term for a still less perfectly crystallized form of carbon. It is equally hard, and occurs in porous masses, and in massive black pebbles, sometimes weighing two or more ounces.

Diamonds vary considerable in hardness, and even different parts of the same crystal are decidedly different in their resistance to cutting and grinding. The famous Koh-i-noor, when cut into its present form, showed a notable variation in hardness. In cutting one of the facets near a yellow flaw, the crystal became harder and harder the farther it was cut into, until, after working the mill for six hours at the usual speed of 2,400 revolutions a minute, little impression was made. The speed was accordingly increased to more than 3,000, when the work slowly proceeded. Other portions of the stone were found to be comparatively soft, and became harder as the outside was cut away.

Beautifully white diamonds have been found at Inverel, New South Wales, and from the rich yield of the mine and the white color of the stones great things were expected. A parcel of many hundred carats came to England, when it was found they were so hard as to be practically unworkable as gems, and I believe they were ultimately sold for rock-boring purposes.

I will illustrate the intense hardness of the diamond by an experiment: I place a diamond on the flattened apex of a conical block of steel, and on the diamond I bring down a second cone of steel. With the electric lantern I will project an image of the diamond and steel faces on the screen, and force them together by hydraulic power. Unless I happen to have selected a diamond with a flaw, I shall squeeze the stone right into the steel blocks without injuring it in the slightest degree.

But it is not the hardness of the diamond so much as its optical qualities that make it so highly prized. It is one of the most refracting substances in nature, and it also has the highest reflecting properties. In the cutting of diamonds advantage is taken of these qualities. When cut as a brilliant the facets on the lower side are inclined so that light falls on them at an angle of 24° 13′, at which angle all the incident light is totally reflected. A well-cut diamond should appear opaque by transmitted light, except at a small spot in the middle where the table and culet are opposite. All the light falling on the front of the stone is reflected from the facets, and the light passing into the diamond is reflected from the interior surfaces and refracted into colors when it passes out into the air, giving rise to the lightnings and coruscations for which the diamond is supreme above all other gems.

The following table gives the refractive indices of diamonds and other bodies:

Refractive indices for the D line.

Chromate of lead	2.50–2.97
Diamond	2.47–2.75
Phosphorus	2.22
Sulphur	2.12
Ruby	1.78
Thallium glass	1.75
Iceland spar	1.65
Topaz	1.61
Beryl	1.60
Emerald	1.59
Flint glass	1.58
Quartz	1.55
Canada balsam	1.53
Crown glass	1.53
Fluor-spar	1.44
Ice	1.31

According to Dr. Gladstone, the specific refractive energy, $\frac{\mu-1}{d}$, will be for the D line 0.404, and the refraction equivalent, $P\frac{\mu-1}{d}$, will be 4.82.

After exposure for some time to the sun many diamonds glow in a dark room. Some diamonds are fluorescent, appearing milky in sunlight. In a vacuum, exposed to a high-tension current of electricity, diamonds phosphoresce of different colors, most South African diamonds shining with a bluish light. Diamonds from other localities emit bright blue, apricot, pale blue, red, yellowish green, orange, and pale green light. The most phosphorescent diamonds are those which are fluorescent in the sun. One beautiful green diamond in my collection, when phosphorescing in a good vacuum, gives almost as much light as a candle, and you can easily read by its rays. The light is pale green, tending to white.

CONVERSION OF DIAMOND INTO GRAPHITE.

I will now draw your attention to a strange property of the diamond, which at first sight might seem to argue against the great permanence and unalterability of this stone. It has been ascertained that the cause of phosphorescence is in some way connected with the hammering of the gaseous molecules, violently driven from the negative pole, on to the surface of the body under examination; and so great is the energy of the bombardment that impinging on a piece of platinum, or even iridium, the metal will actually melt. When the diamond is thus bombarded in a radiant matter tube the result is startling. It not only phosphoresces, but assumes a brown color, and when the action is long continued becomes almost black.

I will project a diamond on the screen and bombard it with radiant matter before your eyes. Some diamonds visibly darken in a few minutes, while others, more leisurely in their ways, require an hour.

This blackening is only superficial, but no ordinary means of cleaning will remove the discoloration. Ordinary oxidizing reagents have little or no effect in restoring the color. The black stain on the diamond is due to a form of graphite which is very resistant to oxidation. It is not necessary to expose the diamond in a vacuum to electrical excitement in order to produce a change.

I have already signified that there are various degrees of refractoriness to chemical reagents among the different forms of graphite. Some dissolve in strong nitric acid; other forms of graphite require a mixture of highly concentrated nitric acid and potassium chlorate to attack them, and even with this intensely powerful agent some graphites resist longer than others. M. Moissan has shown that the power of resistance to nitric acid and potassium chlorate is in proportion to the temperature at which the graphite was formed, and with tolerable certainty we can estimate this temperature by the resistance of the specimen of graphite to this reagent.

The superficial dark coating on a diamond after exposure to molecular bombardment I have proved to be graphite,[1] and M. Moissan[2] has shown that this graphite, on account of its great resistance to oxidizing reagents, can not have been formed at a lower temperature than 3,600° C.

It is therefore manifest that the bombarding molecules carrying with them an electric charge, and striking the diamond with enormous velocity, raise the superficial layer to the temperature of the electric arc, and turn it into graphite, while the mass of diamond and its conductivity to heat are sufficient to keep down the general temperature to such a point that the tube appears scarcely more than warm to the touch.

A similar action occurs with silver, the superficial layers of which can be raised to a red heat without the whole mass becoming more than warm.[3]

This conversion of diamond into graphite is, I believe, a pure effect of heat. In 1880[4] Professor Dewar, in this theater, placed a crystal of diamond in a carbon tube, through which a current of hydrogen was maintained. The tube was heated from the outside by an electric arc, and in a few minutes the diamond was converted into graphite. I will now show you that a clear crystal of diamond, heated in the electric arc (temperature 3,600° C.), is converted into graphite, and this graphite is most refractory.

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[1] Chemical News, Vol. LXXIV, page 39, July 1896.

[2] Comptes rendus, CXXIV, page 653.

[3] Proc. Roy. Soc., Vol. L, page 99, June 1891.

[4] Proceedings of the Royal Institution, Friday evening meeting, January 16, 1880.

The diamond is remarkable in another respect. It is extremely transparent to the Röntgen rays, whereas highly refracting glass, used in imitation diamonds, is almost perfectly opaque to the rays. I exposed over a photographic plate to the X-rays for a few seconds the large Delhi diamond, of a fine pink color, weighing 31½ carats, a black diamond weighing 23 carats, together with an imitation in glass of the pink diamond lent me by Mr. Streeter; also a flat triangular crystal of diamond of pure water, and a piece of glass of the same shape and size. On development, the impression where the diamond obscured the rays was found to be strong, showing that most rays passed through, while the glass was practically opaque. By this means imitation diamonds and some other false gems can readily be detected and distinguished from the true gems. It would take a good observer to distinguish my pure triangular diamond from the adjacent glass imitation.

GENESIS OF THE DIAMOND.

Speculations as to the probable origin of the diamond have been greatly forwarded by patient research, and particularly by improved means of obtaining high temperatures. Thanks to the success of Professor Moissan, whose name will always be associated with the artificial production of diamonds, we are able to-day to manufacture diamonds in our laboratories—minutely microscopic, it is true—all the same veritable diamonds, with crystalline form and appearance, color, hardness, and action on light the same as the natural gem.

Until recent years carbon was considered absolutely nonvolatile and infusible; but the enormous temperatures at the disposal of experimentalists—by the introduction of electricity—show that instead of breaking rules, carbon obeys the same laws that govern other bodies. It volatilizes at the ordinary pressure at a temperature of about 3,600° C., and passes from the solid to the gaseous state without liquefying. It has been found that other bodies which volatilize without liquefying at the ordinary pressure will easily liquefy if pressure is added to temperature. Thus, arsenic liquefies under the action of heat if the pressure is increased; it naturally follows that if along with the requisite temperature sufficient pressure is applied, liquefaction of carbon will be likely to take place, when on cooling it will crystallize. But carbon at high temperatures is a most energetic chemical agent, and if it can get hold of oxygen from the atmosphere or any compound containing it, it will oxidize and fly off in the form of carbonic acid. Heat and pressure, therefore, are of no avail unless the carbon can be kept inert.

It has long been known that iron when melted dissolves carbon, and on cooling liberates it in the form of graphite. Moissan discovered that several other metals have similar properties, especially silver; but iron is the best solvent for carbon. The quantity of carbon entering into solution increases with the temperature, and on cooling in ordinary circumstances it is largely deposited as crystalline graphite.

Professor Dewar has made a calculation as to the critical pressure of carbon, that is the lowest pressure at which carbon can be got to assume the liquid state at its critical temperature, that is, the highest temperature at which liquefaction is possible. He starts from the vaporizing or boiling point of carbon, which, from the experiments of Violle and others on the electric arc, is about 3,600° C., or 3,874° Absolute. The critical point of a substance on the average is 1.5 times its absolute boiling point. Therefore the critical point of carbon is 5,811° Ab., or, say, 5,800° Ab. But the absolute critical temperature divided by the critical pressure is for elements never less than 2.5. Then—

$$\frac{5{,}800^\circ \text{ A.}}{\text{PCr}} = 2.5 \text{ or PCr} = \frac{5{,}800^\circ \text{ A.}}{2.5}, \text{ or } 2{,}320 \text{ atmospheres.}$$

The result is that the critical pressure of carbon is about 2,300 atmospheres or, say, 15 tons on the square inch. The highest critical pressure recorded is that of water, amounting to 195 atmospheres, and the lowest that of hydrogen, about 20 atmospheres. In other words, the critical pressure of water is ten times that of hydrogen, and the critical pressure of carbon is ten times that of water.

Now, 15 tons on the square inch is not a difficult pressure to obtain in a closed vessel. In their researches on the gases from fired gunpowder and cordite, Sir Frederick Abel and Sir Andrew Noble obtained in closed steel cylinders pressures as great as 95 tons to the square inch, and temperatures as high as 4,000° C. Here, then, if the observations are correct, we have sufficient temperature and enough pressure to liquefy carbon; and if the temperature could only be allowed to act for a sufficient time on the carbon there is little doubt that the artificial formation of diamonds would soon pass from the microscopic stage to a scale more likely to satisfy the requirements of science, industry, and personal decoration.

ARTIFICIAL MANUFACTURE OF THE DIAMOND.

I will now proceed to manufacture a diamond before your eyes. Don't think I yet have a talisman that will make me rich beyond the dreams of avarice. Hitherto the results have been very microscopic, and are chiefly of scientific interest in showing us nature's workshop, and how we may ultimately hope to vie with her in the manufacture of diamonds. Unfortunately, the operations of separating the diamond from the iron and other bodies with which it is associated are somewhat prolonged, nearly a fortnight being required to detach it from the iron, graphite, and other matters in which it is embedded. I can, however, show the different stages of the operations, and project on the screen diamonds made in this manner.

In Paris, recently, I saw the operation carried out by M. Moissan, the discoverer of this method of making carbon separate out in the transparent crystalline form, and I can show you the operations straight, as it were, from the inventor's laboratory. I am also indebted to the directors of the Notting Hill Electric Lighting Company and to the general manager, Mr. Schultz, for enabling me to perform several operations at their central station, where currents of 500 amperes and 100 volts were placed at my disposal.

The first necessity is to select pure iron—free from sulphur, silicon, phosphorus, etc.—and to pack it in a carbon crucible with pure charcoal from sugar. Half a pound of this iron is then put into the body of the electric furnace, and a powerful arc formed close above it between carbon poles, utilizing a current of 800 amperes at 40 volts pressure. The iron rapidly melts and saturates itself with carbon. After a few minutes' heating to a temperature above 4,000° C.—a temperature at which the lime of the furnace melts like wax and volatilizes in clouds—the current is stopped, and the dazzling fiery crucible is plunged beneath the surface of cold water, where it is held till it sinks below a red heat. As is well known, iron increases in volume at the moment of passing from the liquid to the solid state. The sudden cooling solidifies the outer layer of iron, and holds the inner molten mass in a tight grip. The expansion of the inner liquid on solidifying produces an enormous pressure, and under the stress of this pressure the dissolved carbon separates out in a transparent, dense, crystalline form—in fact, as diamond.

Now commences the tedious part of the process. The metallic ingot is attacked with hot nitro-hydrochloric acid until no more iron is dissolved. The bulky residue, consisting chiefly of graphite, together with translucent flakes of a chestnut-colored carbon, black opaque carbon of a density of from 3 to 3.5, and hard as diamonds—black diamonds or carbonado, in fact, and a small portion of transparent colorless diamonds showing crystalline structure. Besides these, there may be carbide of silicon and corundum, arising from impurities in the materials employed.

The residue is first heated for some hours with strong sulphuric acid at the boiling point, with the cautious addition of powdered niter. It is then well washed and allowed for two days to soak in strong hydrofluoric acid in the cold, then in boiling acid. After this treatment the soft graphite will disappear, and most, if not all, of the silicon compounds will be destroyed. Hot sulphuric acid is again applied to destroy the fluorides, and the residue, well washed, is repeatedly attacked with a mixture of the strongest nitric acid and powdered potassium chlorate, kept warm, but, to avoid explosions, not above 60° C. This ceremony must be repeated six or eight times, when all the hard graphite will gradually be dissolved, and little else left but graphitic oxide, diamond, and the harder carbonado and bort. The residue is fused for an hour in fluorhydrate of fluoride of potassium, then boiled out in water, and again heated in sulphuric acid. The well-washed grains which resist this energetic treatment are dried, carefully deposited on a slide, and examined under the microscope. Along with numerous pieces of black diamond are seen transparent, colorless pieces, some amorphous, others with a crystalline appearance, as I have attempted to reproduce in diagrams. Although many fragments of crystals occur, it is remarkable that I have never seen a complete crystal. All appear broken up, as if on being liberated from the intense pressure under which they were formed they burst asunder. I have direct evidence of this phenomenon. A very fine piece of artificial diamond, carefully mounted by me on a microscopic slide, exploded during the night and covered my slide with fragments. This bursting paroxysm is not unknown at the Kimberley mines.

On the screen I will project fragments of artificial diamond, some lent me by Professor Roberts-Austen, others of my own make, while on the wall you will see drawings of diamonds copied from M. Moissan's book on the electric furnace. Unfortunately these specimens are all microscopic. The largest artificial diamond, so far, is less than 1 millimeter across.

Laboratory diamonds burn in the air before the blowpipe to carbonic acid, and in luster, crystalline form, optical properties, density, and hardness they are identical with the natural stone.

Many circumstances point to the conclusion that the diamond of the chemist and the diamond of the mine are strangely akin as to origin. It is conclusively proved that the diamond has not been formed in situ in the blue ground. The diamond genesis must have taken place at great depths under enormous pressure. The explosion of large diamonds on coming to the surface shows extreme tension. More diamonds are found in fragments and splinters than in perfect crystals, and it is noteworthy that although many of these splinters and fragments are derived from the breaking up of a large crystal, yet in no instance have pieces been found which could be fitted together. Does not this fact point to the conclusion that the blue ground is not their true matrix? Nature does not make fragments of crystals. As the edges of the crystals are still sharp and unabraded the locus of formation can not have been very distant from the present sites. There were probably many sites of crystallization differing in place and time, or we should not see such distinctive characters in the gems from different mines, nor, indeed, in the diamonds from different parts of the same mine.

THE MECHANISM OF THE DIAMANTIFEROUS PIPES.

How the great diamond pipes originally came into existence is not difficult to understand in the light of the foregoing facts. They certainly were not burst through in the ordinary manner of volcanic eruption. The surrounding and inclosing walls show no signs of igneous action, and are not shattered nor broken even when touching the "blue ground." These pipes after they were pierced were filled from below, and the diamonds formed at some previous epoch too remote to imagine were erupted with a mud volcano, together with all kinds of débris eroded from the adjacent rocks. The direction of flow is seen in the upturned edges of some of the strata of shale in the walls, although I was unable at great depths to see any upturning in most parts of the walls of the De Beers mine.

Let me again refer you to the section through the Kimberley mine. There are many such pipes in the immediate neighborhood. It may be that each volcanic pipe is the vent for its own special laboratory—a laboratory buried at vastly greater depths than we have reached or are likely to reach—where the temperature is comparable with that of the electric furnace, where the pressure is fiercer than in our puny laboratories and the melting point higher, where no oxygen is present, and where masses of carbon-saturated iron have taken centuries, perhaps thousands of years, to cool to the solidifying point. Such being the conditions, the wonder is, not that diamonds are found as big as one's fist, but that they are not found as big as one's head. The chemist arduously manufactures infinitesimal diamonds, valueless as ornamental gems; but nature, with unlimited temperature, inconceivable pressure, and gigantic material, to say nothing of measureless time, produces without stint the dazzling, radiant, beautiful crystals I am enabled to show you to-night.

The ferric origin of the diamond is corroborated in many ways. The country round Kimberley is remarkable for its ferruginous character, and iron-saturated soil is popularly regarded as one of the indications of the near presence of diamonds. Certain artificial diamonds present the appearance of an elongated drop. From Kimberley I have with me diamonds which have exactly the appearance of drops of liquid separated in a pasty condition and crystallized on cooling. At Kimberley, and in other parts of the world, diamonds have been found with little appearance of crystallization, but with rounded forms similar to those which a liquid might assume if kept in the midst of another liquid with which it would not mix. Other drops of liquid carbon retained above their melting point for sufficient time would coalesce with adjacent drops, and on slow cooling would separate in the form of large, perfect crystals. Two drops, joining after incipient crystallization, would assume the not uncommon form of interpenetrating twin crystals. Illustrations of these forms from Kimberley are here to-night. Other modified circumstances would produce diamonds presenting a confused mass of borty crystals, rounded and amorphous masses, or a hard, black form of carbonado.

Again, diamond crystals are almost invariably perfect on all sides. They show no irregular side or face by which they were attached to a support, as do artificial crystals of chemical salts; another proof that the diamond must have crystallized from a dense liquid.

When raised the diamond is in a state of enormous strain, as I have already shown by means of polarized light. Some diamonds exhibit cavities which the same test proves to contain gas at considerable pressure.

The ash left after burning a diamond invariably contains iron as its chief constituent; and the most common colors of diamonds, when not perfectly pellucid, show various shades of brown and yellow, from the palest "off color" to almost black. These variations accord with the theory that the diamond has separated from molten iron, and also explains how it happens that stones from different mines, and even from different parts of the same mine, differ from each other. Along with carbon, molten iron dissolves other bodies which possess tinctorial powers. One batch of iron might contain an impurity coloring the stones blue, another lot would tend toward the formation of pink stones, another of green, and so on. Traces of cobalt, nickel, chromium, and manganese—all metals present in the blue ground—might produce all these colors.

An hypothesis, however, is of little value if it only elucidates one-half of a problem. Let us see how far we can follow out the ferric hypothesis to explain the volcanic pipes. In the first place we must remember these so-called volcanic vents are admittedly not filled with eruptive rocks, scoriaceous fragments, etc., constituting the ordinary contents of volcanic ducts. At Kimberley the pipes are filled with a geological plum pudding of heterogenous character—agreeing, however, in one particular. The appearance of shale and fragments of other rocks shows that the mélange has suffered no great heat in its present condition, and that it has been erupted from great depths by the agency of water vapor or some similar gas. How is this to be accounted for?

It must be borne in mind I start with the reasonable supposition that at a sufficient depth[1] there were masses of molten iron at great pressure and high temperature, holding carbon in solution, ready to crystallize out on cooling. In illustration, I may cite the masses of erupted iron in Greenland. Far back in time the cooling from above caused cracks in superjacent strata through which water[2] found its ways. Before reaching the iron the water would be converted into gas, and this gas would rapidly disintegrate and erode the channels through which it passed, grooving a passage more and more vertical in the endeavor to find the quickest vent to the surface. But steam in the presence of molten or even red-hot iron rapidly attacks it, oxidizes the metal and liberates large volumes of hydrogen gas, together with less quantities of hydrocarbons[3] of all kinds—liquids, gaseous, and solid. Erosion

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[1] The requisite pressure of 15 tons on the square inch would exist not many miles beneath the surface of the earth.

[2] There are abundant signs that a considerable portion of this part of Africa was once under water, and a fresh-water shell has been found in apparently undisturbed blue ground at Kimberley.

[3] The water sunk in wells close to the Kimberley mine is somtimes impregnated with paraffin, and Sir H. Roscoe extracted a solid hydrocarbon from the "blue ground."

commenced by steam would be continued by the other gases, and it would be no difficult task for pipes, large as any found in South Africa, to be scored out in this manner. Sir Andrew Noble has shown that when the screw stopper of his steel cylinders in which gunpowder explodes under pressure is not absolutely perfect, gas finds its way out with a rush so overpowering as to score a wide channel in the metal. Some of these stoppers and vents are on the table. To illustrate my argument Sir Andrew Noble has been kind enough to try a special experiment. Through a cylinder of granite is drilled a hole 0.2 inch diameter, the size of a small vent. This is made the stopper of an explosion chamber, in which a quantity of cordite is fired, the gases escaping through the granite vent. The pressure is about 1,500 atmospheres, and the whole time of escape is less than half a second. Notice the erosion produced by the escaping gases and by the heat of friction, which have scored out a channel over half an inch diameter and melted the granite along their course. If steel and granite are thus vulnerable at comparatively moderate gaseous pressure, is it not easy to imagine the destructive upburst of hydrogen and water-gas grooving for itself a channel in the diabase and quartzite, tearing fragments from resisting rocks, covering the country with débris, and finally, at the subsidence of the great rush, filling the self-made pipe with a water-borne magma in which rocks, minerals, iron oxide, shale, petroleum, and diamonds are churned together in a veritable witch's cauldron! As the heat abated the water vapor would gradually give place to hot water, which, forced through the magma, would change some of the mineral fragments into the now existing forms.

Each outbreak would form a dome-shaped hill, but the eroding agency of water and ice would plane these eminences until all traces of the original pipes were lost.

Actions, such as I have described, need not have taken place simultaneously. As there must have been many molten masses of iron with variable contents of carbon, different kinds of coloring matter, solidifying with varying degrees of rapidity, and coming in contact with water at intervals throughout long periods of geological time—so must there have been many outbursts and upheavals, giving rise to pipes containing diamonds. And these diamonds, by sparseness of distribution, crystalline character, difference of tint, purity of color, varying hardness, brittleness, and state of tension, would have impressed upon them, engraved by natural forces, the story of their origin—a story which future generations of scientific men may be able to interpret with greater precision than we can to-day.

Who knows but that at unknown depths in the earth's metallic core beneath the present pipes there are still masses of iron not yet disintegrated and oxidized by aqueous vapor—masses containing diamonds, unbroken, and in greater profusion than they exist in the present blue ground, inasmuch as they are inclosed in the matrix itself, undiluted by the numerous rock constituents which compose the bulk of the blue ground. If this be the case a careful magnetic survey of the country around Kimberley might prove of immense interest, scientific and practical. Observations, at carefully selected stations, of the three magnetic elements—the horizontal component of direction, the vertical component of direction, and the magnetic intensity—would soon show whether any large masses of iron exist within a certain distance of the surface. It has been calculated that a mass of iron 500 feet in diameter could be detected were it 10 miles below the surface. A magnetic survey might also reveal other valuable diamantiferous pipes which, owing to the absence of surface indications, would otherwise remain hidden.

METEORIC DIAMONDS.

There is another diamond theory which appeals to the fancy. It is said that the diamond is a direct gift from heaven, conveyed to earth in meteoric showers. The suggestion, I believe, was first broached by A. Meydenbauer,[1] who says: "The diamond can only be of cosmic origin, having fallen as a meteorite at later periods of the earth's formation. The available localities of the diamond contain the residues of not very compact meteoric masses which may, perhaps, have fallen in historic ages, and which have penetrated more or less deeply, according to the more or less resistant character of the surface where they fell. Their remains are crumbling away on exposure to the air and sun, and the rain has long ago washed away all prominent masses. The inclosed diamonds have remained scattered in the river beds, while the fine, light matrix has been swept away."

According to this hypothesis, the so-called volcanic pipes are simply holes bored in the solid earth by the impact of monstrous meteors—the larger masses boring the holes, while the smaller masses, disintegrating in their fall, distributed diamonds broadcast. Bizarre as such a theory may appear, I am bound to say there are many circumstances which show that the notion of the heavens raining diamonds is not impossible.

In 1846 a meteorite fell in Hungary (the "Ava meteorite") which was found to contain graphite in the cubic crystalline system. G. Rose thought this cubic graphite was produced by the transformation of a diamond. Long after this prediction was verified by Weinschenk, who found transparent crystals in the Ava meteorite. Mr. Fletcher has found in two meteoric irons—one from Youndegin, East Australia, and one from Crosbys Creek, United States—crystals absolutely similar to those in the Ava meteorite.

In 1886 a meteorite falling in Russia contained, besides other constituents, about 1 per cent of carbon in light-gray grains, having the hardness of diamond, and burning in oxygen to carbonic acid.

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[1] Chemical News, Vol. LXI, page 209, 1890.

Daubrée says the resemblance is manifest between the diamantiferous earth of South Africa and the Ava meteorite, of which the stony substance consists almost entirely of peridot. Peridot being the inseparable companion of meteoric iron, the presence of diamonds in the meteorites of Ava, of Youndegin, and of Crosbys Creek, bring them close to the terrestrial diamantiferous rocks.

Hudleston maintains that the bronzite of the Kimberley blue ground is in a condition much resembling the bronzite grains of meteorites; whilst Maskelyne says that the bronzite crystals of Dutoitspan resemble closely those of the bronzite of the meteor of Breitenbach, but are less rich in crystallographic planes.

But the most striking confirmation of the meteoric theory comes from Arizona. Here, on a broad open plain, over an area about 5 miles diameter, were scattered 1,000 or 2,000 masses of metallic iron, the fragments varying in weight from half a ton to a fraction of an ounce. There is little doubt these masses formed part of a meteoric shower, although no record exists as to when the fall took place. Curiously enough, near the center, where most of the meteorites have been found, is a crater with raised edges three quarters of a mile in diameter and about 600 feet deep, bearing exactly the appearance which would be produced had a mighty mass of iron or falling star struck the ground, scattering in all directions, and buried itself deep under the surface. Altogether 10 tons of this iron have already been collected, and specimens of the Canyon Diablo meteorite are in most collectors' cabinets.

An ardent mineralogist, the late Dr. Foote, in cutting a section of this meteorite, found the tools were injured by something vastly harder than metallic iron, and an emery wheel used in grinding the iron had been ruined. He examined the specimen chemically, and soon after announced to the scientific world that the Canyon Diablo meteorite contained black and transparent diamonds. This startling discovery was afterwards verified by Professors Friedel and Moissan, who found that the Canyon Diablo meteorite contained the three varieties of carbon—diamond (transparent and black), graphite, and amorphous carbon. Since this revelation, the search for diamonds in meteorites has occupied the attention of chemists all over the world.

I am enabled to show you photographs of true diamonds I have myself extracted from pieces of the Canyon Diablo meteorite, 5 pounds of which I have dissolved in acids for this purpose—an act of vandalism in the cause of science for which I hope mineralogists will forgive me. A very fine slab of the meteorite, weighing about 7 pounds, which has escaped the solvent, is on the table before you.

Here, then, we have absolute proof of the truth of the meteoric theory. Under atmospheric influences the iron would rapidly oxidize and rust away, coloring the adjacent soil with red oxide of iron. The meteoric diamonds would be unaffected, and would be left on the surface to be found by explorers when oxidation had removed the last proof of their celestial origin. That there are still lumps of iron left in Arizona is merely due to the extreme dryness of the climate and the comparatively short time that the iron has been on our planet. We are here witnesses to the course of an event which may have happened in geologic times anywhere on the earth's surface.

Although in Arizona diamonds have fallen from above, confounding all our usual notions, this descent of precious stones seems what is called a freak of nature rather than a normal occurrence. To the modern student of science there is no great difference between the composition of our earth and that of extraterrestrial masses. The mineral peridot is a constant extraterrestrial visitor, present in most meteorites. And yet no one doubts that peridot is also a true constituent of rocks formed on this earth. The spectroscope reveals that the elementary composition of the stars and the earth are pretty much the same; so does the examination of meteorites. Indeed, not only are the selfsame elements present in meteorites, but they are combined in the same way to form the same minerals as in the crust of the earth.

This identity between terrestrial and extraterrestrial rocks recalls the masses of nickeliferous iron of Ovifak. Accompanied with graphite, they form part of the colossal eruptions which have covered a portion of Greenland. They are so like meteorites that at first they were considered to be meteorites till their terrestrial origin was proved. They contain as much as 1.1 per cent of free carbon.

It is certain from observations I made at Kimberley, corroborated by the experience gained in the laboratory, that iron at a high temperature and under great pressure will act as the long-sought solvent for carbon, and will allow it to crystallize out in the form of diamond—conditions existent at great depths below the surface of the earth. But it is also certain, from the evidence afforded by the Arizona and other meteorites, that similar conditions have likewise existed among bodies in space, and that a meteorite, freighted with its rich contents, on more than one occasion has fallen as a star from the sky. In short, in a physical sense, heaven is but another name for earth, or earth for heaven.

THE DISCOVERY OF NEW ELEMENTS WITHIN THE LAST TWENTY-FIVE YEARS.[1]

By CLEMENS WINKLER.

In considering the relative frequency of the substances composing the crust of the earth, Clarke[2] admits that down to a depth of 16 kilometers below sea level the composition of that envelope is the same as that of the superficial strata already examined. The average specific gravity of these strata is 2.5, which is not much more than half that of the earth, taken as a whole. Including the ocean and the atmosphere, this outer crust is half oxygen and one-fourth silicon, the remaining fourth being made up of the other elements: Aluminum, 7 per cent; iron, 5.1; calcium, 3.5; magnesium, 2.5; sodium, 2.2, and the same amount of potassium. Some elements, whose numerous combinations have long attracted the attention of the human mind, are of but slight importance quantitatively considered; thus, there is found in the crust of the earth but 0.94 per cent of hydrogen, 0.21 of carbonic acid, 0.09 of phosphorus, and 0.02 of nitrogen. These elements, therefore, which form immense oceans, and are the very basis of life, furnish but a very small fraction of the above-mentioned external shell, 16 kilometers thick, and it seems probable, from soundings hitherto made, that at the greatest depths they are absent or exist in very small quantities. Considered with reference to the mass of the whole globe they may be almost wholly neglected. The amount of chlorine does not exceed 0.15 per cent; yet the amount of chloride of sodium dissolved in the ocean would suffice to cover the surface of the continents and bury the highest mountains.

It will be seen from this how little we can judge of the average mass of the globe, as indicated by its mean density, from an examination of its external surface. There can not be the least doubt but what the internal portions of the earth are formed from substances different from those composing its external strata, and involuntarily the mind compares our planet with those meteorites whose mass is of iron trav-

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[1] A paper read January 11, 1897, before the German Chemical Society at Berlin. Translated from the Revue Scientifique, fourth series, Vol. VIII, pages 258-262.

[2] F. W. Clarke, Bulletin of the Philosophical Society of Washington, Vol. II, pages 129-142.

ersed and enveloped by silicates, and which have an amount of phosphorus and carbon, as well as of hydrogen and nitrogen, as insignificant in proportion as is that of the earth, all of which makes it seem probable that they have had a gaseous envelope that has been lost during their passage through the terrestrial atmosphere.

But if the elements of low specific gravity or great volatility which, like hydrogen or nitrogen, exist in great quantity around us form but a very small part of the total constituents of our globe, it will be seen that the elements called rare must compose but an infinitesimal part of the mass of the earth, especially since, as far as we know at present, these elements are never found at great depths. I at least am not aware that the heavy metals—gold, silver, lead, etc.—are found in materials extracted from great depths or thrown out by volcanoes. At the time of the great eruption of Krakatoa, for example, I sought vainly for such elements in the cinders ejected by the volcano, which apparently came from great depths. The supposed discovery of a new element in the old lavas of Vesuvius has been found to be erroneous.

As we approach the surface of the globe elementary bodies seem to multiply, and two hypotheses occur for explaining this—displacements of cosmic matter or the formation of new elements upon the surface of the globe.

Displacements of cosmic matter are, as is well known, incessant. The fall of meteorites furnishes a particularly striking example of this, but it is probable that quantitatively cosmic dust has a greater importance. Still, neither the meteorites found at various points nor the dust gathered by Nordenskiöld[1] on the ice fields of the polar regions, and whose extra-terrestrial origin can not be doubted, contain rare elements. The hypothesis of increase by exterior agency seems, then, to lack foundation. The new formation of elementary bodies seems, however, still less probable. We can, at most, support it by the hypothesis, suggested but never proven, that bodies now supposed to be simple may be reduced. Without doubt spectrum analysis reveals to us transformations that are gradually going on in the matter of the fixed stars, but the question is whether known elements transform themselves into others equally well known. Besides, the conditions of temperature and aggregation of the fixed stars that have been observed can not be compared with those of the earth.

Evidently the increase of simple bodies in the exterior strata of the terrestrial globe is only apparent. It must be recognized that human science has made great progress and that this progress can not but have an influence upon the discovery of new elements. The first electrolytic decompositions of salts and earths attempted at the commencement of this century by Davy by means of a feeble Voltaic pile demonstrated the existence of metallic radicals that had not been in the least suspected up to that time; while Moissan, by means of the powerful currents used to-day, has been able to isolate from its combi-

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[1] Nordenskiöld. Pogg., Ann., 151.

nations fluorine, hitherto almost unknown. Spectrum analysis has brought to light the existence of an entire series of elements with characteristic spectra, and it has been possible to demonstrate the presence of one of these, helium, in the sun, before it was known that it also entered into the composition of our globe. The conclusions drawn by D. Mendelejeff from the law of periodicity also led to the discovery of several elements whose existence was theoretically indicated before the chemist had isolated them.

I will again recur to the results obtained by Mendelejeff, but I will now mention scandium discovered in 1879 by Nilson in euxonite, gadolinite, and yttrotitanite. This metal, of whose oxide only a few grams exist, and which, perhaps, no one except its discoverer has ever had in his hands, has a considerable scientific importance, because its atomic weight of 44, determined by Nilson, is precisely that indicated by Mendelejeff for ekabor, an element whose existence had been foretold by the law of periodicity.

As early as 1794 Gadolin had separated from the gadolinite of Ytterby an earth which he called ytter earth, and which later was recognized as formed of erbine, terbine, and of ytter properly so called. These earths were again found in a great number of rare minerals, but the oxides derived from these minerals appeared to be different in nature and behavior, seeming rather to be mixtures very difficult to break up into their constituent parts, their various elements giving no very characteristic reactions. It was necessary to have recourse to spectrum analysis and to the determination of their atomic weights, trying to isolate them by repeated fractionation, either by the action of the sulphates of potash or ammonium or by the partial decomposition of the nitrates under the action of heat. We will not enter here into the details of these operations which are special researches in the strict sense of the word, and whose results are not yet clearly established as to certain points. The major part of the work has, however, been accomplished in the last quarter century; and not only has it given us more precise ideas concerning scandium and yttrium, but it has also revealed the existence of numerous other rare elements whose decomposition does not appear impossible, among which we will cite: Erbium,[1] holmium,[2] thulium,[3] dysprosium,[4] terbium,[5] gadolinum,[6] samarium,[7] decipium,[8] ytterbium.[9] Lucium,[10] recently announced by P. Barrière, has been since contested.

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[1] Cleve, Comptes rendus, 91-381.

[2] Id., ibid., 89 et 91.

[3] Id., ibid.

[4] Lecoq de Boisbaudran, ibid., 102.

[5] Delafontaine, Ann. de chim. et de phys., 14.

[6] Lecoq de Boisbaudran, Comptes rendus, 102.

[7] Id., ibid., 89.

[8] Delafontaine, Comptes rendus, 87.

[9] Marignac, Comptes rendus, 87.

[10] W. Crookes, Chem. Zeitung, 1896.

Cerium, lanthanum, and didymium have recently been the objects of attentive researches having in view a practical end—the construction of mantles used in incandescent gaslights. It had been for some time suspected that didymium was not a simple body, but it was Carl Auer von Welsbach,[1] the inventor of this method of illumination, who has the honor of having succeeded, in 1885, in separating didymium into its two elements, praseodymium and neodymium. By the use of monazite, as was shown at the Chicago Exposition in 1893, a greater quantity of the salts of these remarkable metals was prepared and the practical application of them assured.

The existence of metacerium announced by M. Brauner[2] does not appear as yet absolutely established, neither does that of russium[3] which M. Chruschtschow found associated with thorium in certain zirconiums and in monazite, and whose atomic weight is said to be 220. The jargonium of Sorby,[4] the austrium of Linnemann,[5] as well as the norvegium of Dahll,[6] the actinium of Phipson,[7] the idumium of Websky,[8] the masrium of Richmond and Off,[9] and an unknown element which M. K. J. Bayer thought he had found in French bauxite have vanished into the void.

We will also mention, as curiosities, kosmium and neokosmium, that take their names, not from the kosmos but from Kosmann,[10] who, on the 16th of November, 1896, took out a patent for the preparation of their oxides. If it were not for the expense of the patent, it might have been thought a pleasantry, like that perpetrated a few years ago by the Chemiker Zeitung,[11] that told its readers over the signature of M. Fried. Much, the marvellous history of the discovery of damarium.

The world of chemical processes is like the stage of a theatre on which are exhibited the details of the action of the play, but in this world the characters are represented by elements, each of which plays its part, whether it be a silent or a speaking one. Among the latter may be classed two elements discovered during the last twenty-five years—gallium and germanium.

Gallium was discovered on the 27th of August, 1875, by Lecoq de Boisbaudran,[12] in the blende of Pierrefitte, by means of two quite well-marked lines that appeared in the violet portion of that spectrum of that blende, which, however, as was afterwards shown, contains but a

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[1] Carl Auer von Welsbach, Monatsch. für Chemie, 6.

[2] B. Brauner, Chem. News, 71.

[3] K. D. Chruschtschow, Chem. Zeitung, 1890.

[4] Sorby, Berichte der deutschen Chem. Gesellsch., 2.

[5] E. Linnemann, Monatsch. f. Chemie, 7.

[6] Tellef Dahll, Berichte d. deut. Chem. Gesellsch., 12 et 13.

[7] T. L. Phipson, ibid., 14 et 15.

[8] M. Websky, Sitzungsber. d. Akad. d. Wissensch. zu Berlin, 30.

[9] H. D. Richmond and Off, Chem. Zeitung, 1892.

[10] Kosmann, Zeitsch. f. Elektrochemie, 1896–97.

[11] Chem. Zeitung, 1890.

[12] Lecoq de Boisbaudran, Comptes rendus, 81.

very small proportion of the new metal. This proportion is not, in fact, more than a ten-thousandth per cent, while the richer blende of Bernbryer contains a thousandth. The preparation of any considerable quantities of gallium was naturally attended with great difficulties because there was no ore known from which it could be obtained, and yet the study of the new mineral is of the greatest interest because of the theoretical speculations of Mendelejeff, already mentioned. Scandium and germanium had not yet been discovered, so that nothing could either justify or confirm the conclusions derived from the law of periodicity. As early as 1869, in a communication presented to the Russian Society of Chemistry at St. Petersburg, "on the correlations existing between the properties of elements and their atomic weight," Mendelejeff[1] had, in fact, affirmed the existence of simple bodies not yet discovered, whose atomic weight, ought, for example, to be between 65 and 75. He had even gone further yet, by studying and describing in all their details the properties of three hypothetical elements,[2] ekabor, eka-aluminum, and ekasilicon. It will be seen, then, what interest attached to the question of ascertaining whether the properties of gallium agreed with the previsions of the Russian chemist.

At first it seemed that no agreement existed, at least the determination made on the small quantities of gallium that could be obtained gave for its specific gravity the unexpected value of 4.7. But as several of the properties of the new metal, such as precipitation from its solutions by carbonate of baryta, its tendency to form basic salts and its capacity for furnishing alums, denoted a very close relationship with aluminum, Mendelejeff did not hesitate, in the Mémoires de l'Academie des Sciences of France, to declare that the element in question seemed to correspond to that whose existence he had foretold in 1871 as analogous to aluminum, and which he had provisionally designated under the name of eka-aluminum. And, in fact, new determinations made with more considerable quantities of gallium, obtained by the electrolytic process, showed that its specific gràvity was 59, a value exactly corresponding to that calculated by Mendelejeff for the hypothetical eka-aluminum. The same agreement was later shown for its specific heat (0.08) as well as for its specific gravity, so that the exactitude of the previsions of Mendelejeff were confirmed. It was thus well established that it is possible to deduce from the properties of known elements those of elements yet unknown, but whose existence may be predicted.

Mendelejeff did not hope for such a rapid confirmation of his previsions, but his triumph was still more complete, for to gallium there was soon added scandium (ekabor), discovered by M. L. F. Nilson in 1879, and germanium (ekasilicon) by myself in 1886.[3]

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[1] D. Mendelejeff, Journ. d. russ. chem. Ges., 1869.

[2] D. Mendelejeff, Ann. chem. Supp., 8.

[3] Cf. Winkler, Berich. d. d. chem. Gesellsch, 19.

The discovery of germanium, predicted under the name of ekasilicon by Mendelejeff, recalls the discovery, by Galle, of the planet Neptune, whose existence had been shown by the calculations of Adams and Leverrier. This discovery was not due to the concurrence of favorable circumstances or to a happy accident; it resulted from researches undertaken because of theoretical previsions, and the agreement between the predicted and real properties was such that Mendelejeff considers the discovery of germanium the principal justification of the law of periodicity.[1]

On one point only has germanium completely deceived expectations; that is, with regard to its formations in nature. It might have been expected that it would rather have been found as an oxide in the rare minerals of the North, in company with titanium and zirconium, than as a sulphide in company with analogous combinations of arsenic and antimony in the vein rock of argentiferous minerals. This circumstance, together with the comparative rarity of its mineral, argyrodite, has contributed not a little to retard the elucidation of its true character; I am myself inclined to consider it as eka-antimony, while Mendelejeff, after my first incomplete communications to him, supposed it to be ekacadmium. At about the same time M. von Richter expressed his conviction that germanium could be nothing else than the long-expected ekasilicon; an opinion which he justified by the agreement of their atomic weights.

Although gallium and germanium keep peace with each other, showing that science is above all national quarrels and political agitations, the denomination of germanium which I have given to the new element has aroused some criticism,[2] and it has been said that I ought to renounce that name, which has too marked a territorial flavor. I need not say that this demand seems to me quite unjustifiable, for I have only followed the example given by the denominations gallium and scandium, concerning which the same criticism could be made.

The success of the bold speculations of Mendelejeff allows us to affirm that the elaboration of the periodic system is a great forward step for science. In the course of only fifteen years all the predictions of the Russian chemist have been confirmed, new elements have been placed in the gaps which he left in his table, and there is reason to hope that it will be the same for those which still remain in the natural system.

Still, the last two elements discovered—argon and helium—seem to have no relation with the periodic system. After Lord Rayleigh,[3] in 1892, had proved that nitrogen obtained from chemical combinations was about one-half per cent lighter than that obtained from the atmosphere, a determination that was again verified in 1894,[4] Lord Rayleigh

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[1] D. Mendelejeff, Principes de chimie; St. Petersburg, 1891 (p. 692).

[2] Moniteur Scientifique, June 1886 and March 1887.

[3] Lord Rayleigh, Chem. News, 69.

[4] Lord Rayleigh, Proc. Roy. Soc., 55.

and Professor Ramsay[1] separated from atmospheric nitrogen an elementary gas of great density which, by reason of its chemical indifference, they called argon. They proved that this gas formed about 0.8 or 0.9 per cent of the volume of nitrogen, from which it could be separated either by incandescent magnesium or by the continued action of the electric spark. It was established beyond doubt that Cavendish produced this gas a hundred years ago by the use of the electric spark.[2] Argon, either alone or accompanied by helium, has also been found in natural waters as well as in minerals. Its discovery in a meteorite of Augusta County, Virginia, United States of America, may perhaps lead us to ascribe to it an extraterrestrial origin.

The physical properties of argon are very distinct, and its characteristic spectrum enables us to at once distinguish it with certainty from any other substance, but from a chemical point of view this gas is most extraordinarily inactive, and we have not yet succeeded in making it form combinations as the other elements do. This peculiarity, and also the impossibility of finding a place in the periodic system for a simple body having the molecular weight of argon (39.88), have given rise to all sorts of hypotheses relative to the nature of this gas. Should it be considered as a monatomic element having an atomic weight of 37 and a place in the system between chlorine and potassium, or a diatomic one with an atomic weight of 20, which would place it after fluorine and before sodium? May it not be an allotropic form of nitrogen, N_3, having a molecular weight of 42, or a triatomic element whose atomic weight would not exceed 13? The question has not as yet been answered.

Another most interesting discovery was that of helium, made by Professor Ramsay.[3] In 1891 Hillebrand showed that uranium ore and ores of the same family when dissolved in acids or fused with alkaline carbonates, or even merely heated in a vacuum, may give off as much as 3 per cent of nitrogen. Professor Ramsay[4] obtained this gas from cleveite and by means of spectroscopic examination demonstrated the presence of argon; and in the course of his experiments—in March, 1895—he observed beside the spectrum of argon another bright, yellow line that did not belong to that spectrum, and which Crookes[5] recognized as identical with the line D^3 that Lockyer[6] had already observed in 1868 in the spectrum of the solar chromosphere, and which he had attributed to an element as yet unknown upon the earth—helium. The same line had also been distinguished in the spectra of other fixed stars, particularly in the spectrum of Orion, so that it may be admitted that helium exists in large quantities extraterrestrially.

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[1] Lord Rayleigh et W. Ramsay, Journ. prakt. Chem., 51.

[2] Cavendish, Crell. Ann., 1786.

[3] W. Ramsay, Comptes rendus, 120.

[4] W. Ramsay, Chem. News, 71.

[5] C. Crookes, Chem. News, 71.

[6] N. Lockyer, Nature, 53.

On our planet it appears, on the contrary, to be very rare, and may be ranked among the rarest of elements. Still, it had been nearly discovered several times; in 1882 Palmieri[1] actually observed the line of helium in the course of his researches upon the lava of Vesuvius, but he pursued the matter no further; it was the same with Hillebrand,[2] who, in 1891, obtained, in the spectrum of the gas obtained from uranite, lines which were probably those of helium.

Since then helium has been found in a great number of ores, generally associated with uranium, yttrium, and thorium; in mineral waters, and in very small quantities in atmospheric air. Helium is the lightest of all the gases except hydrogen; Stoney[3] deduces from this fact an explanation of the existence of these two elements in but very small quantities in a free state upon the face of the earth, while they are distributed in enormous masses throughout the universe. The comparatively small force of the earth's gravitation does not form a sufficient counterpoise to the velocity of their molecules, which therefore escape from the terrestrial atmosphere unless restrained by chemical combination. They then proceed to reunite around great centers of attraction, such as the fixed stars, in whose atmospheres these elements exist in large quantities.

The study of the spectrum of helium is of the greatest importance, because it gives us information concerning the nature of distant celestial bodies. It also, as is shown by the researches of Runge and Paschen,[4] suggests doubts as to the elementary character of the new body. However that may be, if we admit that helium is composed of two gases (Mr. Lockyer has already proposed the name of asterium for the second), one of these gases ought to have a boiling point nearly as low as the absolute zero, and in any case below 264° C., for the master in liquefaction of gases, Mons. K. Olszewsky,[5] has not, so far, succeeded in producing a change of state in helium; so that he proposes to use this gas for filling gas thermometers to be used for measuring very low temperatures.

Up to the present time helium has shown itself as refractory as argon to chemical combination, and there is such an uncertainty as to the position to which it ought to be assigned that I will not discuss the hypotheses that have been put forward with regard to it.

It is not impossible that the discovery of these two new elements, argon and helium, may give rise to a remodeling if not a transformation of the periodic system; a remodeling which will cause the disappearance of some uncertainties, or even contradictions, which now exist. Thus, for example, the atomic weight of tellurium, recently determined

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[1] Palmieri, Rend. Acc. di Napoli, 20.

[2] Hillebrand, Sill. Am. Journ., 38 et 40.

[3] J. Stoney, Chem. News, 71. (See also Martin Mugdon: Argon et hélium, deux éléments gazeux nouveaux; Stuttgart, 1896.)

[4] Sitzungsber. d. Akad. d. Wissensch. Berlin, 1895.

[5] K. Olszewski, Anzeiger der Akad. d. Wissenschaft in Krakau (Cracow), June 1896.

by B. Brauner[1] and Ludwig Staudenmaier,[2] does not fit at all into the periodic system; on the other hand the existence in this substance of a foreign element, such as austriacum, suggested by B. Brauner, does not appear to be established.[3] As to the much agitated question of whether and to what extent the atomic weight of cobalt differs from that of nickel, I believe that I have answered it in a satisfactory manner and destroyed the hypothesis[4] of Gerhard Krüss and F. W. Schmid[5] with regard to the existence in one of these substances of a third element which had received the name of gnomium.

The rapid review that we have just made of the discovery of new elements during the last twenty-five years shows that new researches in this line have been pursued with great activity and have led to results of considerable value. And yet the speculations to which these researches have given rise may be considered as quite uncertain as regards the question of the possibility of the ultimate decomposition of those bodies that now appear to be simple; and per contra as regards the progressive development of a primitive substance and the new formation of the numerous elements that are now recognized. I will only recall in this connection the hypothesis of Mr. Lockyer[6] as to the dissociation of the elements in the interior of the atmosphere of the sun. Hypotheses of this sort will remain such as long as no one has succeeded in decomposing a body hitherto regarded as undoubtedly simple, or in transforming any element whatever into another. And yet they should not be considered as entirely inadmissible; a day may come when some unexpected event may open to science new ways of investigation. Four hundred years ago Nicholas Copernicus left, as a young master of philosophy and of medicine, the old university of Ulica St. Anny, at Cracow, to go to Bologna and to Rome for the purpose of consecrating his talents as a mathematician to the study of astronomical sciences. There, attacking the enigma of the firmament, he finally attained the certainty that the earth was not, as had been hitherto believed, a central fixed world, but a sphere suspended freely in space, a planet similar to the other planets, turning around the sun and having a movement of rotation around its own axis under the action of gravitation. It was, indeed, a true revolution in the theories that had been hitherto held, this theory that fixed the sun in the firmament in spite of its daily ascent and disappearance; an idea that, at the present day, has become familiar to us. And further, we now know that neither is the sun itself fixed, but that it is drawn with all its cortège of planets along a course without end, across space without

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[1] B. Brauner, Sitzungsber. d. k. k. Akad. der Wissensch., Wien, 1889.

[2] Zeitschr. für anorgan. Chemie, 10.

[3] Cl. Winkler, Zeitschr. für anorgan. Chemie, 8.

[4] Cl. Winkler, Zeitsch. f. anorgan. Chemie.

[5] Gerhard Kruss and F. W. Schmidt, Berichte d. deutsch. chem. Gesellsch., 22, and Zeitsch. f. anorg. Chemie, 2.

[6] N. Lockyer, Berichte d. deutsch. chem. Gesellschaft, 6, 11 et 12.

limit. Whence comes it and whither goes it? Properly speaking, we know nothing about it, and doubtless we will never know either its origin or its end; but as the earth turns around this movable sun, it hence results that our planet does not describe a closed path, but a sort of spiral, and that it never returns to a spot that it has once quitted. Each second takes our planet to a new point in the universe, and from this incessant displacement it ought to follow that no phenomenon or event can ever reproduce exactly any anterior phenomenon or event. Clouds may resemble each other, as one sunrise resembles another, but there is never an absolute coincidence, and it would seem that these variations ought to be perpetuated throughout the course of time that is embraced by the history of humanity.

It would be useless to push further these considerations, they are merely speculations; but they lead to this thought, which, although unsupported, continually recurs to our mind—the possibility of a progressive transformation of matter in a given direction, in that they show that everything that is with us is drawn along in a dizzy course across an unknown immensity.

AN UNDISCOVERED GAS.[1]

By Prof. WILLIAM RAMSAY, Ph. D., LL. D., Sc. D., F. R. S.

A sectional address to members of the British association falls under one of three heads. It may be historical, or actual, or prophetic; it may refer to the past, the present, or the future. In many cases, indeed in all, this classification overlaps. Your former presidents have given sometimes an historical introduction, followed by an account of the actual state of some branch of our science, and, though rarely, concluding with prophetic remarks. To those who have an affection for the past the historical side appeals forcibly; to the practical man, and to the investigator engaged in research, the actual, perhaps, presents more charm; while to the general public, to whom novelty is often more of an attraction than truth, the prophetic aspect excites most interest. In this address I must endeavor to tickle all palates; and perhaps I may be excused if I take this opportunity of indulging in the dangerous luxury of prophecy, a luxury which the managers of scientific journals do not often permit their readers to taste.

The subject of my remarks to-day is a new gas. I shall describe to you later its curious properties; but it would be unfair not to put you at once in possession of the knowledge of its most remarkable property—it has not yet been discovered. As it is still unborn, it has not yet been named. The naming of a new element is no easy matter, for there are only twenty-six letters in our alphabet, and there are already over seventy elements. To select a name expressible by a symbol which has not already been claimed for one of the known elements is difficult, and the difficulty is enhanced when it is at the same time required to select a name which shall be descriptive of the properties (or want of properties) of the element.

It is now my task to bring before you the evidence for the existence of this undiscovered element.

It was noticed by Döbereiner, as long ago as 1817, that certain elements could be arranged in groups of three. The choice of the elements selected to form these triads was made on account of their analogous

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[1] Address to the chemical section of the British Association for the Advancement of Science by Prof. William Ramsay, Ph. D., LL.D., Sc. D., F. R. S., president of the section, Toronto, 1897. From Report of the British Association, 1897.

properties and on the sequence of their atomic weights, which had at that time only recently been discovered. Thus calcium, strontium, and barium formed such a group; their oxides, lime, strontia, and baryta are all easily slaked, combining with water to form soluble lime-water, strontia-water, and baryta-water. Their sulphates are all sparingly soluble, and resemblance had been noticed between their respective chlorides and between their nitrates. Regularity was also displayed by their atomic weights. The numbers then accepted were 20, 42.5, and 65; and the atomic weight of strontium, 42.5, is the arithmetical mean of those of the other two elements, for $(65 + 20) / 2 = 42.5$. The existence of other similar groups of three was pointed out by Döbereiner, and such groups became known as "Döbereiner's triads."

Another method of classifying the elements, also depending on their atomic weights, was suggested by Pettenkofer, and afterward elaborated by Kremers, Gladstone, and Cooke. It consisted in seeking for some expression which would represent the differences between the atomic weights of certain allied elements. Thus, the difference between the atomic weight of lithium, 7, and sodium, 23, is 16; and between that of sodium and of potassium, 39, is also 16. The regularity is not always so conspicuous. Dumas, in 1857, contrived a somewhat complicated expression which, to some extent, exhibited regularity in the atomic weights of fluorine, chlorine, bromine, and iodine, and also of nitrogen, phosphorus, arsenic, antimony, and bismuth.

The upshot of these efforts to discover regularity was that, in 1864, Mr. John Newlands, having arranged the elements in eight groups, found that when placed in the order of their atomic weights, "the eighth element, starting from a given one, is a kind of repetition of the first, like the eighth note of an octave in music." To this regularity he gave the name "The Law of Octaves."

The development of this idea, as all chemists know, was due to the late Prof. Lothar Meyer, of Tübingen, and to Professor Mendeléeff, of St. Petersburg. It is generally known as the "Periodic Law." One of the simplest methods of showing this arrangement is by means of a cylinder divided into eight segments by lines drawn parallel to its axis; a spiral line is then traced round the cylinder, which will, of course, be cut by these lines eight times at each revolution. Holding the cylinder vertically, the name and atomic weight of an element is written at each intersection of the spiral with a vertical line, following the numerical order of the atomic weights. It will be found, according to Lothar Meyer and Mendeléeff, that the elements grouped down each of the vertical lines form a natural class. They possess similar properties, form similar compounds, and exhibit a graded relationship between their densities, melting points, and many of their other properties. One of these vertical columns, however, differs from the others, inasmuch as on it there are three groups, each consisting of three elements with approximately equal atomic weights. The elements in question are iron, cobalt, and nickel; palladium, rhodium, and ruthenium; and platinum, iridium, and osmium. There is apparently room for a fourth group of three elements in this column, and it may be a fifth. And the discovery of such a group is not unlikely, for when this table was first drawn up Professor Mendeléeff drew attention to certain gaps, which have since been filled up by the discovery of gallium, germanium, and others.

The discovery of argon at once raised the curiosity of Lord Rayleigh and myself as to its position in this table. With a density of nearly 20, if a diatomic gas, like oxygen and nitrogen, it would follow fluorine in the periodic table; and our first idea was that argon was probably a mixture of three gases, all of which possessed nearly the same atomic weights, like iron, cobalt, and nickel. Indeed, their names were suggested, on this supposition, with patriotic bias, as Anglium, Scotium, and Hibernium! But when the ratio of its specific heats had, at least in our opinion, unmistakably shown that it was molecularly monatomic, and not diatomic, as at first conjectured, it was necessary to believe that its atomic weight was 40, and not 20, and that it followed chlorine in the atomic table, and not fluorine. But here arises a difficulty. The atomic weight of chlorine is 35.5, and that of potassium, the next element in order in the table, is 39.1; and that of argon, 40, follows, and does not precede, that of potassium, as it might be expected to do. It still remains possible that argon, instead of consisting wholly of monatomic molecules, may contain a small percentage of diatomic molecules, but the evidence in favor of this supposition is, in my opinion, far from strong. Another possibility is that argon, as at first conjectured, may consist of a mixture of more than one element; but, unless the atomic weight of one of the elements in the supposed mixture is very high, say 82, the case is not bettered, for one of the elements in the supposed trio would still have a higher atomic weight than potassium. And very careful experiments, carried out by Dr. Norman Collie and myself on the fractional diffusion of argon, have disproved the existence of any such element with high atomic weight in argon, and, indeed, have practically demonstrated that argon is a simple substance and not a mixture.

The discovery of helium has thrown a new light on this subject. Helium, it will be remembered, is evolved on heating certain minerals, notably those containing uranium; although it appears to be contained in others in which uranium is not present, except in traces. Among those minerals are clèveite, monazite, fergusonite, and a host of similar complex mixtures, all containing rare elements, such as niobium, tantalum, yttrium, cerium, etc. The spectrum of helium is characterized by a remarkably brilliant yellow line, which had been observed as long ago as 1868 by Professors Frankland and Lockyer in the spectrum of the sun's chromosphere, and named "helium" at that early date.

The density of helium proved to be very close to 2, and, like argon, the ratio of its specific heat showed that it, too, was a monatomic gas. Its atomic weight, therefore, is identical with its molecular weight, viz, 4, and its place in the periodic table is between hydrogen and lithium, the atomic weight of which is 7.

The difference between the atomic weights of helium and argon is thus 36, or 40—4. Now there are several cases of such a difference. For instance, in the group the first member of which is fluorine we have—

Fluorine	19	16.5
Chlorine	35.5	19.5
Manganese	55	

In the oxygen group—

Oxygen	16	16
Sulphur	32	20.3
Chromium	52.3	

In the nitrogen group—

Nitrogen	14	17
Phosphorus	31	20.4
Vanadium	51.4	

And in the carbon group—

Carbon	12	16.3
Silicon	28.3	19.8
Titanium	48.1	

These instances suffice to show that approximately the differences are 16 and 20 between consecutive members of the corresponding groups of elements. The total differences between the extreme members of the short series mentioned are—

Manganese—Fluorine	36
Chromium—Oxygen	36.3
Vanadium—Nitrogen	37.4
Titanium—Carbon	36.1

This is approximately the difference between the atomic weights of helium and argon, 36.

There should, therefore, be an undiscovered element between helium and argon with an atomic weight 16 units higher than that of helium and 20 units lower than that of argon, namely 20. And if this unknown element, like helium and argon, should prove to consist of monatomic molecules, then its density should be half its atomic weight, 10. And pushing the analogy still further, it is to be expected that this element should be as indifferent to union with other elements as the two allied elements.

My assistant, Mr. Morris Travers, has indefatigably aided me in a search for this unknown gas. There is a proverb about looking for a needle in a haystack; modern science, with the aid of suitable magnetic appliances, would, if the reward was sufficient, make short work of that proverbial needle. But here is a supposed unknown gas, endowed no doubt with negative properties, and the whole world to find it in. Still the attempt had to be made.

We first directed our attention to the sources of helium—minerals. Almost every mineral which we could obtain was heated in a vacuum, and the gas which was evolved examined. The results are interesting. Most minerals give off gas when heated, and the gas contains, as a rule, a considerable amount of hydrogen, mixed with carbonic acid, questionable traces of nitrogen, and carbonic oxide. Many of the minerals, in addition, gave helium, which proved to be widely distributed, though only in minute proportion. One mineral—malacone—gave appreciable quantities of argon, and it is noteworthy that argon was not found except in it (and, curiously, in much larger amount than helium) and in a specimen of meteoric iron. Other specimens of meteoric iron were examined, but were found to contain mainly hydrogen, with no trace of either argon or helium. It is probable that the sources of meteorites might be traced in this manner, and that each could be relegated to its particular swarm.

Among the minerals examined was one to which our attention had been directed by Professor Lockyer, named eliasite, from which he said that he had extracted a gas in which he had observed spectrum lines foreign to helium. He was kind enough to furnish us with a specimen of this mineral, which is exceedingly rare, but the sample which we tested contained nothing but undoubted helium.

During a trip to Iceland, in 1895, I collected some gas from the boiling springs there; it consisted, for the most part, of air, but contained somewhat more argon than is usually dissolved when air is shaken with water. In the spring of 1896 Mr. Travers and I made a trip to the Pyrenees to collect gas from the mineral springs of Cauterets, to which our attention had been directed by Dr. Bouchard, who pointed out that these gases are rich in helium. We examined a number of samples from the various springs, and confirmed Dr. Bouchard's results, but there was no sign of any unknown lines in the spectrum of these gases. Our quest was in vain.

We must now turn to another aspect of the subject. Shortly after the discovery of helium its spectrum was very carefully examined by Professors Runge and Paschen, the renowned spectroscopists. The spectrum was photographed, special attention being paid to the invisible portions, termed the "ultraviolet" and "infrared." The lines thus registered were found to have an harmonic relation to each other. They admitted of division into two sets, each complete in itself. Now, a similar process had been applied to the spectrum of lithium and to that of sodium, and the spectra of these elements gave only one series each. Hence, Professors Runge and Paschen concluded that the gas, to which the provisional name of helium had been given, was, in reality, a mixture of two gases, closely resembling each other in properties. As we know no other elements with atomic weights between those of hydrogen and lithium, there is no chemical evidence either for or against this supposition. Professor Runge supposed that he had obtained evidence of the separation of these imagined elements from each other by means of diffusion; but Mr. Travers and I pointed out that the same alteration of spectrum, which was apparently produced by diffusion, could also be caused by altering the pressure of the gas in the vacuum tube, and shortly after Professor Runge acknowledged his mistake.

These considerations, however, made it desirable to subject helium to systematic diffusion in the same way as argon had been tried. The experiments were carried out in the summer of 1896 by Dr. Collie and myself. The result was encouraging. It was found possible to separate helium into two portions of different rates of diffusion and consequently of different density by this means. The limits of separation, however, were not very great. On the one hand, we obtained gas of a density close on 2; and on the other, a sample of density 2.4 or thereabouts. The difficulty was increased by the curious behavior, which we have often had occasion to confirm, that helium possesses a rate of diffusion too rapid for its density. Thus, the density of the lightest portion of the diffused gas, calculated from its rate of diffusion, was 1.874; but this corresponds to a real density of about 2. After our paper, giving an account of these experiments, had been published, a German investigator, Herr A. Hagenbach, repeated our work and confirmed our results.

The two samples of gas of different density differ also in other properties. Different transparent substances differ in the rate at which they allow light to pass through them. Thus light travels through water at a much slower rate than through air, and at a slower rate through air than through hydrogen. Now, Lord Rayleigh found that helium offers less opposition to the passage of light than any other substance does, and the heavier of the two portions into which helium had been split offered more opposition than the lighter portion. And the retardation of the light, unlike what has usually been observed, was nearly proportional to the densities of the samples. The spectrum of these two samples did not differ in the minutest particular. Therefore it did not appear quite out of the question to hazard the speculation that the process of diffusion was instrumental not necessarily in separating two kinds of gas from each other, but actually in removing light molecules of the same kind from heavy molecules. This idea is not new. It had been advanced by Professor Schützenberger (whose recent death all chemists have to deplore) and later by Mr. Crookes that what we term the atomic weight of an element is a mean; that when we say that the atomic weight of oxygen is 16 we merely state that the average atomic weight is 16; and it is not inconceivable that a certain number of molecules have a weight somewhat higher than 32, while a certain number have a lower weight.

We therefore thought it necessary to test this question by direct experiment with some known gas, and we chose nitrogen as a good material with which to test the point. A much larger and more convenient apparatus for diffusing gases was built by Mr. Travers and myself, and a set of systematic diffusions of nitrogen was carried out. After 30 rounds, corresponding to 180 diffusions, the density of the nitrogen was unaltered, and that of the portion which should have diffused most slowly had there been any difference in rate was identical with that of the most quickly diffusing portion; i. e., with that of the portion which passed first through the porous plug. This attempt, therefore, was unsuccessful, but it was worth carrying out, for it is now certain that it is not possible to separate a gas of undoubted chemical unity into portions of different density by diffusion; and these experiments rendered it exceedingly improbable that the difference in density of the two fractions of helium was due to separation of light molecules of helium from heavy molecules.

The apparatus used for diffusion had a capacity of about 2 liters. It was filled with helium, and the operation of diffusion was carried through 30 times. There were 6 reservoirs, each full of gas, and each was separated into two by diffusion. To the heavier portion of one lot the lighter portion of the next was added, and in this manner all 6 reservoirs were successively passed through the diffusion apparatus. This process was carried out 30 times, each of the 6 reservoirs having had its gas diffused each time, thus involving 180 diffusions. After this process the density of the more quickly diffusing gas was reduced to 2.02, while that of the less quickly diffusing had increased to 2.27. The light portion, on rediffusion, hardly altered in density, while the heavier portion, when divided into three portions by diffusion, showed a considerable difference in density between the first third and the last third. A similar set of operations was carried out with a fresh quantity of helium, in order to accumulate enough gas to obtain a sufficient quantity for a second series of diffusions. The more quickly diffusing portions of both gases were mixed and rediffused. The density of the lightest portion of these gases was 1.98, and after other 15 diffusions the density of the lightest portion had not decreased. The end had been reached; it was not possible to obtain a lighter portion by diffusion. The density of the main body of this gas is therefore 1.98, and its refractivity, air being taken as unity, is 0.1245. The spectrum of this portion does not differ in any respect from the usual spectrum of helium.

As rediffusion does not alter the density or the refractivity of this gas, it is right to suppose that either one definite element has now been isolated, or that if there are more elements than one present they possess the same or very nearly the same density and refractivity. There may be a group of elements, say three, like iron, cobalt, and nickel; but there is no proof that this idea is correct, and the simplicity of the spectrum would be an argument against such a supposition. This substance, forming by far the larger part of the whole amount of gas, must, in the present state of our knowledge, be regarded as pure helium.

On the other hand, the heavier residue is easily altered in density by rediffusion, and this would imply that it consists of a small quantity of a heavy gas mixed with a large quantity of the light gas. Repeated rediffusion convinced us that there was only a very small amount of the heavy gas present in the mixture. The portion which contained the largest amount of heavy gas was found to have the density 2.275, and its refractive index was found to be 0.1333. On rediffusing this portion of gas until only a trace sufficient to fill a Plücker's tube was left, and then examining the spectrum, no unknown lines could be detected; but on interposing a jar and spark gap the well-known blue lines of argon became visible, and even without the jar the red lines of argon and the two green groups were distinctly visible. The amount of argon present, calculated from the density, was 1.64 per cent, and from the refractivity 1.14 per cent. The conclusion had therefore to be drawn that the heavy constituent of helium, as it comes off the minerals containing it, is nothing new, but, so far as can be made out, merely a small amount of argon.

If, then, there is a new gas in what is generally termed helium, it is mixed with argon, and it must be present in extremely minute traces. As neither helium nor argon has been induced to form compounds, there does not appear to be any method other than diffusion for isolating such a gas if it exists, and that method has failed in our hands to give any evidence of the existence of such a gas. It by no means follows that the gas does not exist; the only conclusion to be drawn is that we have not yet stumbled on the material which contains it. In fact, the haystack is too large and the needle too inconspicuous. Reference to the periodic table will show that between the elements aluminum and indium there appears gallium—a substance occurring only in the minutest amount on the earth's surface; and following silicon and preceding tin appears the element germanium—a body which has as yet been recognized only in one of the rarest of minerals, argyrodite. Now, the amount of helium in fergusonite, one of the minerals which yields it in reasonable quantity, is only 33 parts by weight in 100,000 of the mineral, and it is not improbable that some other mineral may contain the new gas in even more minute proportion. If, however, it is accompanied in its still undiscovered source by argon and helium, it will be a work of extreme difficulty to effect a separation from these gases.

In these remarks it has been assumed that the new gas will resemble argon and helium in being indifferent to the action of reagents and in not forming compounds. This supposition is worth examining. In considering it the analogy with other elements is all that we have to guide us.

We have already paid some attention to several triads of elements. We have seen that the differences in atomic weights between the elements fluorine and manganese, oxygen and chromium, nitrogen and vanadium, carbon and titanium are in each approximately the same as that between helium and argon, viz, 36. If elements farther back in the periodic table be examined, it is to be noticed that the differences grow less the smaller the atomic weights. Thus, between boron and scandium the difference is 33; between beryllium (glucinum) and calcium, 31; and between lithium and potassium, 32. At the same time we may remark that the elements grow more like each other the lower the atomic weights. Now, helium and argon are very like each other in physical properties. It may be fairly concluded, I think, that in so far they justify their position. Moreover, the pair of elements which show the smallest difference between their atomic weights is beryllium and calcium. There is a somewhat greater difference between lithium and potassium. And it is in accordance with this fragment of regularity that helium and argon show a greater difference. Then again, sodium, the middle element of the lithium triad, is very similar in properties both to lithium and potassium; and we might, therefore, expect that the unknown element of the helium series should closely resemble both helium and argon.

Leaving now the consideration of the new element, let us turn our attention to the more general question of the atomic weight of argon and its anomalous position in the periodic scheme of the elements. The apparent difficulty is this: The atomic weight of argon is 40; it has no power to form compounds, and thus possesses no valency; it must follow chlorine in the periodic table and precede potassium, but its atomic weight is greater than that of potassium, whereas it is generally contended that the elements should follow each other in the order of their atomic weights. If this contention is correct, argon should have an atomic weight smaller than 40.

Let us examine this connection. Taking the first row of elements, we have:

$$\text{Li} = 7,\ \text{Be} = 9.8,\ \text{B} = 11,\ \text{C} = 12,\ \text{N} = 14,\ \text{O} = 16,\ \text{F} = 19,\ ? = 20.$$

The differences are:

$$2.8,\ 1.2,\ 1.0,\ 2.0,\ 2.0,\ 3.0,\ 1.0.$$

It is obvious that they are irregular. The next row shows similar irregularities. Thus:

$$(? = 20),\ \text{Na} = 23,\ \text{Mg} = 24{\cdot}3,\ \text{Al} = 27,\ \text{Si} = 28,\ \text{P} = 31,\ \text{S} = 32,\ \text{Cl} = 35.5,\ \text{A} = 40.$$

And the differences:

$$3.0,\ 1.3,\ 2.7,\ 1.0,\ 3.0,\ 1.0,\ 3.5,\ 4.5.$$

The same irregularity might be illustrated by a consideration of each succeeding row. Between argon and the next in order, potassium, there is a difference of – 0.9; that is to say, argon has a higher atomic weight than potassium by 0.9 unit; whereas it might be expected to have a lower one, seeing that potassium follows argon in the table. Farther on in the table there is a similar discrepancy. The row is as follows:

Ag = 108, Cd = 112, In = 114, Sn = 119, Sb = 120.5, Te = 127.7, 1 = 127.

The differences are:

4.0, 2.0, 5.0, 1.5, 7.2, −0.7.

Here, again, there is a negative difference between tellurium and iodine. And this apparent discrepancy has led to many and careful redeterminations of the atomic weight of tellurium. Professor Brauner, indeed, has submitted tellurium to methodical fractionation, with no positive results. All the recent determinations of its atomic weight give practically the same number, 127.7.

Again, there have been almost innumerable attempts to reduce the differences between the atomic weights to regularity by contriving some formula which will express the numbers which represent the atomic weights with all their irregularities. Needless to say, such attempts have in no case been successful. Apparent success is always attained at the expense of accuracy, and the numbers reproduced are not those accepted as the true atomic weights. Such attempts, in my opinion, are futile. Still, the human mind does not rest contented in merely chronicling such an irregularity; it strives to understand why such an irregularity should exist. And, in connection with this, there are two matters which call for our consideration. These are: Does some circumstance modify these "combining proportions" which we term "atomic weights?" And is there any reason to suppose that we can modify them at our will? Are they true "constants of nature," unchangeable, and once for all determined? Or are they constant merely so long as other circumstances, a change in which would modify them ,remain unchanged?

In order to understand the real scope of such questions, it is necessary to consider the relation of the "atomic weights" to other magnitudes, and especially to the important quantity termed "energy."

It is known that energy manifests itself under different forms, and that one form of energy is quantitatively convertible into another form, without loss. It is also known that each form of energy is expressible as the product of two factors, one of which has been termed the "intensity factor" and the other the "capacity factor." Professor Ostwald, in the last edition of his Allgemeine Chemie, classifies some of these forms of energy as follows:

Kinetic energy is the product of mass into the square of velocity.

Linear energy is the product of length into force.

Surface energy is the product of surface into surface tension.

Volume energy is the product of volume into pressure.

Heat energy is the product of heat capacity (entropy) into temperature.

Electrical energy is the product of electric capacity into potential.

Chemical energy is the product of "atomic weight" into affinity.

In each statement of factors the "capacity factor" is placed first and the "intensity factor" second.

In considering the "capacity factors" it is noticeable that they may be divided into two classes. The two first kinds of energy, kinetic and linear, are independent of the nature of the material which is subject to the energy. A mass of lead offers as much resistance to a given force, or, in other words, possesses as great inertia as an equal mass of hydrogen. A mass of iridium, the densest solid, counterbalances an equal mass of lithium, the lightest known solid. On the other hand, surface energy deals with molecules, and not with masses. So does volume energy. The volume energy of 2 grams of hydrogen, contained in a vessel of 1 liter capacity, is equal to that of 32 grams of oxygen at the same temperature and contained in a vessel of equal size. Equal masses of tin and lead have not equal capacity for heat; but 119 grams of tin has the same capacity as 207 grams of lead; that is, equal atomic masses have the same heat capacity. The quantity of electricity conveyed through an electrolyte under equal difference of potential is proportional, not to the mass of the dissolved body, but to its equivalent; that is, to some simple fraction of its atomic weight. And the capacity factor of chemical energy is the atomic weight of the substance subjected to the energy. We see, therefore, that while mass or inertia are important adjuncts of kinetic and linear energies, all other kinds of energy are connected with atomic weights, either directly or indirectly.

Such considerations draw attention to the fact that quantity of matter (assuming that there exists such a carrier of properties as we term "matter") need not necessarily be measured by its inertia or by gravitational attraction. In fact, the word "mass" has two totally distinct significations. Because we adopt the convention to measure quantity of matter by its mass the word "mass" has come to denote "quantity of matter;" but it is open to anyone to measure a quantity of matter by any other of its energy factors. I may, if I choose, state that those quantities of matter which possess equal capacities for heat are equal, or that "equal numbers of atoms" represent equal quantities of matter. Indeed, we regard the value of material as due rather to what it can do than to its mass; and we buy food, in the main, on an atomic, or, perhaps, a molecular basis, according to its content of albumen; and most articles depend for their value on the amount of food required by the producer or the manufacturer.

The various forms of energy may therefore be classified as those which can be referred to an "atomic" factor and those which possess a "mass" factor. The former are in the majority, and the periodic law is the bridge between them—as yet an imperfect connection, for the atomic factors, arranged in the order of their masses, display only a partial regularity. It is undoubtedly one of the main problems of physics and chemistry to solve this mystery. What the solution will be is beyond my power of prophecy; whether it is to be found in the influence of some circumstance on the atomic weights hitherto regarded as among the most certain "constants of Nature" or whether it will turn out that mass and gravitational attraction are influenced by temperature or by electrical charge I can not tell; but that some means will ultimately be found of reconciling these apparent discrepancies I firmly believe. Such a reconciliation is necessary, whatever view be taken of the nature of the universe and of its mode of action; whatever units we may choose to regard as fundamental among those which lie at our disposal.

In this address I have endeavored to fulfill my promise to combine a little history, a little actuality, and a little prophecy. The history belongs to the Old World. I have endeavored to share passing events with the New, and I will ask you to join with me in the hope that much of the prophecy may meet with its fulfillment on this side of the ocean.

FLUORINE.[1]

By Prof. Henri Moissan,
Membre de l'Académie des Sciences, Paris.

There has long been known a curious mineral, fluor spar, which occurs in nature in great cubic crystals, sometimes colorless, sometimes tinted green or violet. This mineral is a binary compound of a metal, calcium, united with another element hitherto impossible to isolate, which has been named fluorine.

This fluoride of calcium has very often been compared with the chloride of sodium, the composition of which is perfectly well known to chemists. In fact, there are great and profound analogies between the fluorides and the chlorides; potassium chloride and potassium fluoride both crystallize in the cubic system. In their chief properties the chlorides resemble the fluorides. They usually give parallel reactions; treated with sulphuric acid, both yield hydrogen acids which are soluble in water and which fume strongly in the air.

In addition to calcium fluoride, other compounds containing fluorine are found in nature. We know, for example, a complex compound of calcium phosphate and calcium fluoride which is called apatite. This mineral, which occurs sometimes in very pretty crystals, has also been obtained synthetically in the laboratory; but, which is more important, Henri Sainte-Claire Deville has succeeded in preparing a chlorinated apatite, and this new compound forms crystals identical with those of the apatite containing fluorine. We may therefore say with propriety that in these compounds chlorine can replace fluorine, or act as its substitute. Here is a remarkable analogy, a bond which connects well-studied, well-known chlorine with the elementary substance not yet isolated, fluorine.

Need I cite other examples? They are not lacking. We know the mineral wagnerite, which contains fluorine naturally, and we can prepare the similar chlorinated compound.

These analogies between chlorine and fluorine go still further.

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[1] A lecture delivered by Prof. Henri Moissan before the Royal Institution of Great Britain, May 28, 1897. Translated from the French, as printed in Proceedings of the Royal Institution, 1897.

Let us treat common salt, the chloride of sodium, with sulphuric acid. You see that it gives at once an abundant disengagement of gaseous hydrochloric acid.

We will do the same with sodium fluoride. Let us add, in a leaden vessel, sulphuric acid to the alkaline fluoride. We shall see copious fumes produced. In each case, at a temperature of 20° C., we shall have disengaged a gaseous body which fumes strongly in the air, is colorless, has the characteristics of an energetic acid, combines in the dry state with ammonia, is very soluble in water, and dissolves in the latter with a great increase in temperature.

If we give to sodium fluoride, to the binary compound of fluorine and sodium, the formula NaF, that of the acid substance produced by the action of sulphuric acid can only be HF. The two reactions are identical.

The acid gaseous body formed in this reaction is, therefore, a compound of fluorine and hydrogen; a body analogous to hydrochloric acid, and to which the name hydrofluoric acid is given.

But in the natural sciences analogy is not sufficient; the scientific method can only accept that which is rigorously proved. It is therefore necessary to demonstrate that hydrofluoric acid is a hydrogen acid. And this will take us back to the beginning of the century. You know well how great was the influence of Lavoisier upon the upward flight of chemistry, and indeed upon all true science. You know how this great genius, by the continual use of the balance in the study of reactions, gave to the science which we follow a mathematical exactness. Struck by the important part which oxygen plays in combustion, he believed that that element was indispensable to the formation of acids. To Lavoisier every acid was an oxygen compound; hydrochloric acid, therefore, according to his theories, was regarded as containing oxygen; and, by analogy, hydrofluoric acid must contain it also.

To your great investigator, Humphry Davy, belongs the honor of having proved that hydrofluoric acid contains no oxygen. But allow me, before coming to the beautiful researches of Davy, to recall to you the history of the discovery of hydrofluoric acid. We need not consider the investigations of Margraff, which were published in 1768, but we must not forget that it was Scheele who definitely characterized hydrofluoric acid in 1771, without, however, obtaining it in a state of purity. In 1809 Gay Lussac and Thénard took up the study of the compound, and succeeded in producing an acid sufficiently pure and highly concentrated, although far from being anhydrous. The action of hydrofluoric acid upon silica and the silicates was then perfectly elucidated.

Let us now come down to about the year 1813, the time when Davy undertook the study of hydrofluoric acid. A little earlier Ampère, in two letters addressed to Humphry Davy, advanced the opinion that hydrofluoric acid might be regarded as formed by the combination of hydrogen with an element yet unknown—fluorine—or, in brief, that it was not an oxygenated acid.

Davy, who shared this view, sought at once to prove that hydrofluoric acid contained no oxygen. For this purpose he neutralized the pure acid with ammonia, and strongly heating the salt in an apparatus of platinum, collected in the colder parts of the latter only the sublimed fluohydrate of ammonia containing no trace of water.

Let us repeat the experiment, but with an oxygenated acid; let us take sulphuric acid and neutralize it with ammonia. We thus obtain ammonium sulphate. If now we heat this salt in the same platinum apparatus, it will fuse at about 140° C.; then, at about 180°, it will decompose into ammonia and the bisulphate, and the latter, at a still higher temperature, will be transformed into volatile ammonium bisulphite, nitrogen, and water.

Thus, upon strongly heating ammonium sulphate there has been a formation of water, and in this experiment of Davy, when performed with an oxygenated acid, the quantity of water collected is so great as to be unquestionable. The fluohydrate of ammonia, like the chlorhydrate, gives no water upon decomposition, which leads us therefore to say that hydrofluoric acid contains no oxygen, and that it is analogous to hydrochloric acid. Now we know by experimental demonstration that hydrochloric acid is composed of chlorine and hydrogen. It is therefore logical to think that hydrofluoric acid is formed by the combination of hydrogen with fluorine.

This important experiment, made by skillful hands, did not, however, compel a general belief in the existence of hydracids.

The views of Lavosier concerning the part played by oxygen in the formation of acids, ideas which had been opposed at first, were then so generally admitted that many persons refused to accept the existence of hydrogenated acids at all. It was only after the memorable researches of Guy Lussac upon cyanogen and hydrocyanic acid that it was proved beyond discussion that energetic acids could exist which contained no trace of oxygen.

Furthermore, when we compare the acid compounds formed by chlorine, for example, or sulphur with hydrogen we have two types of combination which are entirely different.

Let us take one volume of chlorine and one volume of hydrogen. By the action of light or of a spark from an induction coil they unite to form two volumes of hydrochloric-acid gas, a compound having all the properties of a very energetic acid.

If we combine two volumes of hydrogen with one volume of sulphur vapor we shall obtain two volumes of sulphuretted hydrogen gas, which has, it is true, an acid reaction, but incomparably weaker than that of hydrochloric acid.

It is very evident that by virtue of its powerful reactions, by the disengagement of heat which it produces upon contact with water or with bases, that hydrofluoric acid should be compared with hydrochloric acid and not with the sulphur compound. It resembles absolutely the acid formed from one volume of chlorine and one volume of hydrogen united without condensation.

Let me now recall to you a much more recent experiment of Gore. This chemist heated fluoride of silver in an atmosphere of hydrogen. Under these conditions he saw the volume of gas double itself; it was apparent, then, that hydrofluoric acid was formed by the union of one volume of hydrogen with one volume of the element not yet isolated, fluorine. Furthermore, it was evidently that same element which had left the silver fluoride to unite with hydrogen and to generate the hydrofluoric acid of which we have spoken.

Thus, without preparing fluorine, without being able to separate it from the substances with which it is united, chemistry has been able to study and to analyze a great number of its compounds. The body was not isolated, and yet its place was marked in our classifications. This well demonstrates the usefulness of a scientific theory, a theory which is regarded as true during a certain time, which correlates facts and leads the mind to new hypotheses, the first causes of experimentation; which, little by little, destroy the theory itself, in order to replace it by another more in harmony with the progress of science.

Thus certain properties of fluorine were foreseen even before its isolation became possible.

Let us now see what attempts were made not only with hydrofluoric acid, but also with the fluorides to isolate fluorine.

I have already spoken of Davy's experiments, in which, most notably, he proved that hydrofluoric acid contained no oxygen. In addition to these experiments Davy made many others which I will briefly recall.

We can in a general way divide the researches upon fluorine into two great classes:

1. Experiments made by the electrolytic method, either upon the acid or upon fluorides.

2. Experiments in the dry way. From the beginning of these researches it was foreseen that fluorine, when isolated, would decompose water; consequently all the attempts made by the wet way since the first work of Davy had no chance of success.

Humphrey Davy made many electrical experiments, and these were carried out in apparatus of platinum or of fused (cast) chloride of silver with the powerful voltaic pile of the Royal Society.

He found that hydrofluoric acid was decomposed, despite the fact that it contained water, and then that the electric current seemed to pass with much more difficulty. He tried also throwing the electric sparks into the acid liquid, and was able in some attempts to obtain by this method a small quantity of gas. But the acid, although cooled, was rapidly dissipated in vapor and the laboratory soon became uninhabitable. Davy even became quite ill from breathing the vapor of hydrofluoric acid, and he advised all chemists to take the greatest precautions to avoid its action upon the skin and the bronchial tubes. Gay Lussac and Thénard also suffered much from the same acid vapors.

The other experiments of Davy (I can not cite them all) were chiefly directed to the reaction of chlorine upon fluorides. They presented very great difficulties, for at that time the fluohydrates of the fluorides were unknown, nor was it known how to prepare the majority of the anhydrous fluorides.

These researches of Davy are, as should be expected, of the highest importance, and one remarkable property of fluorine was put in evidence. In those experiments which yielded a small quantity of this radicle of the fluorides the vessels of gold or platinum in which the reaction took place were profoundly attacked. In this case fluorides of gold or of platinum were formed.

Davy varied in many ways the conditions of his experiments. He repeated the reaction of chlorine upon a metallic fluoride in vessels of sulphur, of carbon, of gold, of platinum, etc.; and he never attained to a satisfactory result. He was thus led to think that fluorine undoubtedly possessed a chemical activity much greater than that of known substances.

In closing his memoir, Humphry Davy suggests that these experiments might succeed if they were performed in vessels of fluor spar. We shall see that this idea has been taken up by different investigators. To read the work of Davy will interest you, captivate you to the highest degree. I can best compare this fine memoir with those pictures of the masters to which time only adds new charms. One never tires of admiring them, and discovers in them without end new details and new beauties.

It was by operating in apparatus made of calcium fluoride that the brothers Knox sought to decompose silver fluoride with chlorine. The chief objection to their experiments is based on the fact that the fluoride of silver employed was not dry. In fact, it is extremely difficult to completely dehydrate the fluorides of silver and mercury. Furthermore, we shall see, in the researches of Fremy, that the action of chlorine upon fluorides tends rather to form addition products—fluochlorides—than to set the fluorine free.

In 1848, Louyet, also working with apparatus of fluor spar, studied an analogous reaction. He acted with chlorine upon the fluoride of mercury. The objections raised against the researches of the brothers Knox also apply to the labors of Louyet. Fremy has shown that fluoride of mercury prepared by Louyet's process contains a notable amount of water. Furthermore, the results obtained were quite variable. The gas collected was a mixture of air, chlorine, and hydrofluoric acid, whose properties varied during the course of preparation.

The brothers Knox complained much of the action of hydrofluoric acid upon the respiratory passages, and one of them states that after the close of their investigation he spent three years at Genoa and returned still suffering. As for Louyet, carried away by his researches, he took insufficient precautions to avoid the irritating action of the acid vapors, and paid with his life for his devotion to science.

These researches of Louyet led Fremy, about the year 1850, to take up again the question of the isolation of fluorine. Fremy first studied, systematically, the metallic fluorides. He proved the existence of numerous fluohydrates of fluorides, and ascertained their properties and composition. Next he caused many gaseous substances to react upon different fluorides, the action of chlorine and of oxygen being studied with care. Finally, all his attention was drawn to the electrolysis of metallic fluorides.

Most of these experiments were performed in vessels of platinum, at temperatures which were sometimes very high. When, after the general examination of the fluorides, Fremy studied the action of chlorine upon the fluorides of lead, antimony, mercury, and silver, he showed clearly that it was almost impossible then to obtain these compounds in a condition of absolute dryness. Hence we can understand why, in his electrolytic researches, this chemist devoted his attention mainly to calcium fluoride.

Having seen that many fluorides retained water most tenaciously, he fell back upon fluor spar, which often occurs in nature very pure and absolutely dry. This fluoride of calcium, liquefied at a high temperature, he sought to electrolyze in a platinum vessel.

Under these conditions the metal calcium is carried to the negative pole, while around the platinum rod which formed the negative electrode, and which was rapidly corroded, there was visible a boiling, indicating the escape of a new gas.

Undoubtedly, in these experiments, fluorine was set free; but consider that the electrolysis was effected at the temperature of a bright red heat. How difficult experimentation must become under such conditions. How is it possible to collect the gas or to ascertain its properties? This gaseous body displaces iodine from the iodides, but after a few experiments the alkaline metal, set at liberty, pierces the platinum walls of the apparatus, the latter becomes useless, and all must be begun anew.

Far from being discouraged by his failures Fremy, on the contrary, brought to his work an inconceivable perseverance. He varied his experiments, modified his apparatus; the difficulties only encouraged him to continue his labors.

Two important facts at once stood out by themselves. One entered immediately into the domain of science; the other seems to have attracted much less attention.

The first was the preparation of pure, anhydrous hydrofluoric acid. Until the researches of Fremy, the acid absolutely deprived of water was unknown. Having prepared and analyzed the fluohydrate of potassium fluoride, Fremy made use of it at once as a source of the pure, dry acid.

He thus obtained a compound which was gaseous at ordinary temperatures, and which condensed in a freezing mixture to a colorless liquid having a great affinity for water. Here, then, is a reaction of great importance—the preparation of hydrofluoric acid in a state of purity.

Allow me to remark incidentally that when Humphry Davy electrolyzed concentrated hydrofluoric acid the badly conducting liquid which he obtained at the end of his experiment was the acid very nearly anhydrous.

The second fact, which, as I have said, was almost unnoticed, and which has been of great interest to me, especially at the end of my researches, was that fluorine has the greatest tendency to unite with nearly all compounds to form addition products.

In brief, fluorine easily forms ternary and quaternary compounds. Let chlorine act upon a fluoride, instead of isolating fluorine we shall prepare a fluochloride. Employ oxygen, and we shall make an oxyfluoride. This property explains to us the failures of Louyet, of the brothers Knox, and of other experimenters. Even when dealing with dry fluorides in an atmosphere of chlorine, bromine, or iodine we shall obtain ternary compounds instead of free fluorine. This fact was clearly established by Fremy. His memoir covers so great a number of experiments that it seems to have discouraged chemists, to have stopped further attempts. Since 1856, the date of publication of Fremy's memoir, researches upon hydrofluoric acid and the isolation of fluorine have been few. The question seems to have been in a state of arrested development. Nevertheless, in 1869, Gore took up methodically the study of hydrofluoric acid. He started with the anhydrous acid prepared by Fremy's method. He determined its boiling point, the tension of its vapor at different temperatures; indeed, all of its principal properties. His memoir is one of remarkable exactitude. Among the numerous investigations of Gore we will consider for the moment only the following, to which I beg your attention:

In a special apparatus this chemist electrolyzed anhydrous hydrofluoric acid containing a little fluoride of platinum in such manner that the gases produced could be collected at each electrode. At the negative pole he saw hydrogen disengaged abundantly, while the rod which terminated the positive pole was rapidly corroded. This phenomenon was identical with that observed by Faraday during the electrolysis of calcium fluoride. Gore next verified the observation of Faraday, that hydrofluoric acid containing water allows the current to pass, but that the absolutely pure anhydrous acid is a nonconductor. In one of his experiments Gore tried to electrolyze a hydrofluoric acid, which, because of an impurity, was a good conductor; and, seeking to avoid the wasting of the electrode, replaced the latter by a stick of carbon.

This carbon was prepared with great care, by heating in a current of hydrogen a dense wood, which gave him a sonorous rod, a good conductor of electricity. The apparatus was put together; the experiment begun. All at once a violent explosion occurred, and fragments of the carbon were thrown to the remotest parts of the laboratory. Gore repeated the experiment several times. The result was always the same. To-day we are able to give an explanation of the phenomenon.

The carbon, which was thus prepared by the distillation of a very hard wood, was filled with hydrogen. You all know how easily gases condense in carbon; the beautiful experiments of Melsens have established this most clearly. When we electrolyze, with a negative pole of such carbon, a conducting hydrofluoric acid, fluorine is set free, which, as we shall see later, unites with hydrogen, producing a violent detonation. In this experiment of Gore a little fluorine was set free, and it was to the combination of that with the hydrogen occluded in the carbon that the explosion was due.

Now I come to the new experiments, to which I call your attention.

I began these researches with a preconceived opinion. If we suppose for a moment that chlorine had not yet been isolated, although we knew how to prepare the chlorides of phosphorus and other similar compounds, it is clear that we should increase our chances of success in attempting to isolate that element by working with the compounds which it could form with the metalloids.

It seemed to me that we could obtain chlorine rather by seeking to decompose the pentachloride of phosphorus or hydrochloric acid than by attempting the electrolysis of chloride of calcium or of an alkaline chloride.

Should not the same considerations hold good for fluorine?

Again, since fluorine, according to the earlier investigations, and especially those of Davy, is a body endowed with very energetic affinities, we should, in order to collect the element, work at the lowest possible temperatures.

Such are the general conceptions which led me to take up systematically a study of the compounds formed by fluorine with the metalloids.

My attention was first given to the fluoride of silicon, and I was struck at once by the great stability of that compound. With the exception of the alkaline metals, which, at a dull red heat, decompose it easily, few substances act upon silicon fluoride. It is easy to account for this property if we remember that its formation is attended by a very great evolution of heat. M. Berthelot showed long since that the stability of compounds is greatest when the most heat is generated during their formation.

I supposed then, rightly or wrongly, before having isolated fluorine, that if we ever succeeded in preparing the element, its combination with crystallized silicon should be attended by incandescence. And every time during my long researches that I hoped to have set fluorine free I did not fail to try that reaction, and we shall see later that it succeeded perfectly.

After these first experiments upon silicon fluoride, I took up the investigation of the compounds of fluorine with phosphorus.

Thorpe discovered the compound PF_5, the pentafluoride of phosphorus. I prepared the trifluoride PF_3, and I gave all my attention to the reactions which might lead to its decomposition. I made the experiment of which Humphry Davy had dreamed, of burning phosphorus trifluoride in oxygen, and I found that there was no formation of phosphoric acid with liberation of fluorine, as the English scholar had expected, but that the trifluoride and the oxygen united to form a new gas—phosphorus oxyfluoride.

Here is a new example of the ease with which fluorine yields products of addition.

I next tried, but without avail, the action of the induction spark upon phosphorus trifluoride. The pentafluoride of phosphorus discovered by Thorpe has, however, been decomposed by very strong sparks into the trifluoride and fluorine.

This experiment was made in a glass tube over mercury. You will see that at once fluoride of mercury and fluoride of silicon were formed. There was no hope, under these conditions, of preserving the fluorine, even when it was diluted by an excess of pentafluoride. I then thought of another reaction.

We have known, since the researches of Fremy, that the fluoride of platinum, produced during the electrolysis of alkaline fluorides, decomposes at a high temperature. Having found that the fluorides of phosphorus are easily absorbed by hot platinum sponge, with the final production of platinum phosphide, I thought that this method of preparing fluoride of platinum might lead to the isolation of fluorine. Heating gently at first, the absorption of phosphorus fluoride should give a mixture of phosphorus and platinum fluoride; and, the quantity of the latter being sufficient, a subsequent increase of temperature should disengage fluorine. These experiments and others analogous to them were tried under conditions most favorable to success. They yielded interesting results, which were not, however, sufficiently sharp to settle the question of the isolation of fluorine.

While still pursuing the above-mentioned studies, I prepared the trifluoride of arsenic, which had already been obtained by Dumas in great purity. I determined its physical constants, together with some new properties, and investigated with great care the action of the electric current upon it.

The fluoride of arsenic, liquid at ordinary temperatures (a binary compound, formed by a solid, arsenic, with a gas, fluorine) seemed to be admirably suited to electrolytic experiments.

I was obliged at four different times to interrupt these researches upon arsenious fluoride, the manipulation of which is more dangerous than that of anhydrous hydrofluoric acid, and whose toxic properties made it impossible for me to continue the experiments.

I succeeded, however, in effecting the electrolysis of this compound upon employing the current produced by a battery of 90 Bunsen cells.

Under these conditions the current passed continuously; pulverulent arsenic was deposited at the negative pole, and at the positive electrode gaseous bubbles were formed which rose in the liquid, but were almost instantly absorbed. The liberated fluorine was at once taken up by the trifluoride of arsenic, AsF_3, which was transformed into the pentafluoride, AsF_5. This investigation, carried on for a long time, gave me no fluorine, but it yielded me precious data concerning the electrolysis of the liquid compounds of fluorine, and led me to the decomposition of anhydrous hydrofluoric acid.

In order to effect the electrolysis of hydrofluoric acid I had made the small apparatus which is before you, and which consists of a platinum U tube, carrying on each limb an exit tube placed above the level of the fluid.

The two openings of the U tube were closed by corks previously saturated with paraffin, as was done in all of my experiments upon the electrolysis of arsenious fluoride.

A platinum wire passed through each stopper and was connected with a battery of fifty Bunsen elements.

I prepared at first pure anhydrous hydrofluoric acid, and found, as shown by Faraday and by Gore, that it was a nonconductor.

The experiment was varied in many ways. The result was always the same. With the current given by ninety Bunsen cells decomposition occurred only with the hydrous acid, and it stopped as soon as all the water had been separated into hydrogen and oxygen. It therefore seemed impossible to effect by this process the decomposition of hydrofluoric acid into its elements—hydrogen and fluorine.

At this point I remembered that in the previous study of arsenious fluoride I had sought to make that liquid a good conductor by adding to it a little fluoride of manganese or acid fluoride of potassium. This process was applied to the hydrofluoric acid, and then, after three years of investigation, I reached the first important experiment upon the isolation of fluorine.

Hydrofluoric acid containing the acid fluoride of potassium decomposes under the action of the current; and in the apparatus which is before your eyes one could obtain at the negative pole a regular disengagement of hydrogen. What was there at the positive pole? Nothing. A slight increase of pressure—that was all. Only, in dismounting the apparatus it was found that the cork of the positive pole had been burnt, carbonized, to the depth of a centimeter. The paraffined stopper of the negative pole was unaltered. Hence there had been disengaged at the positive pole a substance having an action upon cork quite different from that of hydrofluoric acid.

I should add that in order to lessen the vapor tension of the hydrofluoric acid the liquid was cooled by means of methyl chloride, which by rapid evaporation produces a cold of —50° C.

It was necessary to modify the apparatus, and especially the closing of the U tube. Stoppers of fluorspar smoothly ground did not give me good results. The gum lac or gutta-percha which surrounded them was rapidly attacked by the gas produced at the positive pole. It was necessary, therefore, to resort to a closure by means of platinum screws, and after much groping the experiment was thus arranged.

The platinum U tube was closed by screw stoppers. Each stopper was formed by a cylinder of fluorspar, carefully inserted in a hollow cylinder of platinum, whose outer surface carried the screw thread. Each stopper of fluorspar was penetrated by a square rod of platinum. The lower ends of these rods, which served as electrodes, dipped into the liquid. Finally, two branches of platinum, soldered to the two limbs of the U tube below the stoppers, but above the level of the liquid, allowed the gases generated by the action of the current to escape.

In order to obtain pure anhydrous hydrofluoric acid, one begins by preparing the fluohydrate of potassium fluoride, taking all the precautions indicated by Fremy. Having obtained this salt in a state of purity, it is dried over the water bath at a temperature of 100° C.; and afterwards the capsule containing it is placed in vacuo in presence of sulphuric acid and of caustic potash fused in a silver crucible. The acid and potash are replaced every morning during fifteen days, and the vacuum in the bell jar is always maintained to a pressure of about 1 centimetre of mercury.

During this desiccation it is necessary to pulverize the salt from time to time in an iron mortar, in order to expose fresh surfaces. When the fluohydrate no longer contains water, it falls into fine powder, and can then be used for the preparation of hydrofluoric acid. It is to be noted that well-made fluohydrate of potassium fluoride is much less deliquescent than the normal fluoride.

When the fluohydrate is thoroughly dry, it is quickly transferred to a platinum alembic, which has been dried at a red heat a little while before. It is heated gently for an hour or an hour and a half, in order that decomposition may begin slowly; and the first portions of the hydrofluoric acid formed, which may contain traces of water remaining in the salt, are rejected. The platinum receiver is then attached to the retort, which is heated more strongly, but still so as to effect the decomposition of the fluohydrate somewhat slowly. The receiver is surrounded by a mixture of ice and salt; and from this point all the hydrofluoric acid is condensed as a clear liquid, boiling at 19.5° C., very hygroscopic, and, as we know, fuming abundantly in presence of the moisture of the air.

During this operation the platinum U tube, dried with the utmost care, has been fixed by means of cork in a cylindrical glass vessel and surrounded by methyl chloride. Up to the moment of introducing the hydrofluoric acid the exit tubes have been connected with exsiccators containing fused potash. The hydrofluoric acid is brought into this little apparatus by inserting one of the lateral tubes into the receiver in which it is condensed.

When a determinate volume of liquid hydrofluoric acid has been collected in the platinum apparatus, and cooled by gently boiling methyl chloride to a temperature of — 23° C., the current from twenty-five large Bunsen cells, mounted in series, is passed through it. An ampere meter placed in the circuit enables us to take account of the intensity of the current.

In order to make the acid a conductor there is added to it before the experiment a little of the dried and fused fluohydrate of potassium fluoride, about 2 grams to 10 cubic centimeters of the liquid. Under these conditions the decomposition takes place continuously, and we obtain, at the negative pole, a gas which burns with a colorless flame, and which has all the characteristics of hydrogen. At the positive pole there is a colorless gas of a very disagreeable, penetrating odor, resembling that of hypochlorous acid, and irritating to the mucous membrane of the throat and the eyes. The new gas is endowed with very energetic properties—for instance, sulphur inflames upon contact with it.

Phosphorus takes fire in the gas and yields a mixture of oxyfluoride and fluoride. Iodine combines with it, giving a pale flame and losing its color. Powdered arsenic and antimony combine with fluorine incandescently.

Crystallized silicon, even when cold, kindles immediately upon contact with the gas, and burns with much brilliancy, sometimes giving off sparks. The product is silicon fluoride, which can be collected over mercury and clearly identified.

Pure boron ignites also, giving fluoride of boron. Amorphous carbon becomes incandescent upon contact with fluorine. In order to make these different experiments it suffices to put the solid substance in a small glass tube, which is brought close to the extremity of the platinum tube from which the fluorine emerges. We can also repeat the experiments by putting small fragments of the solid bodies to be studied upon the cover of a platinum crucible held near the opening of the exit tube.

The gas decomposes water in the cold, yielding hydrofluoric acid and ozone; it ignites carbon disulphide, and when collected in a platinum crucible containing carbon tetrachloride it produces a continuous liberation of chlorine.

Fused potassium chloride is attacked in the cold with disengagement of chlorine. In presence of mercury the gas is completely absorbed, forming light-yellow mercurous fluoride. Potassium and sodium become incandescent, yielding fluorides. In general, however, the metals are attacked less vigorously than the metalloids. This, we think, is due to a superficial formation of fluoride which hinders further attack. Powdered iron and manganese burn in the gas with a shower of sparks.

Organic bodies are violently attacked. A piece of cork placed near the mouth of the platinum exit tube carbonizes at once and inflames. Alcohol, ether, benzene, turpentine, and petroleum take fire upon contact with fluorine.

Working under good conditions one can obtain from each pole of the apparatus two to four liters of gas per hour.

When the experiment has lasted for several hours and the quantity of hydrofluoric acid remaining at the bottom of the U tube is not sufficient to separate the two gases they recombine in the apparatus with a violent detonation.

We are assured, by direct experiment, that a mixture of ozone saturated with hydrofluoric acid produces none of the reactions just described. The same is true of gaseous hydrofluoric acid. It may be added that the hydrofluoric acid employed, and also the fluohydrate, were absolutely free from chlorine. Finally, it can not be objected that the new gas might be a perfluoride of hydrogen; for, passed over iron heated to redness in a platinum tube it is completely absorbed, without liberation of hydrogen.

In the most recent investigations I have found that it is possible to make these experiments in an apparatus of copper, constructed like the platinum device which is before you.

By the electrolysis of hydrofluoric acid rendered conductive with the acid fluoride of potassium, we have obtained, at the negative pole, hydrogen, and at the positive pole the continuous evolution of a gas having new properties and endowed with very energetic affinities; that gas is fluorine. We have been able to determine its density, its color, and its spectrum, and to study its action upon both elements and compounds.

Now that we know the chief properties of fluorine, now that the element has been isolated, I am convinced that in spite of its energetic reactions, new means for its preparation will be discovered.

We may even suppose that purely chemical methods for the preparation of fluorine may be found, which shall give a better yield than the electrolytic process.

Will fluorine ever have practical applications?

It is very difficult to answer this question. I may, however, say in all sincerity that I gave this subject little thought when I undertook my researches, and I believe that all the chemists whose attempts preceded mine gave it no more consideration.

A scientific research is a search after truth, and it is only after discovery that the question of applicability can be usefully considered.

It is evident that, as we see the great industrial transformations which take place to-day before our eyes, we can not well dogmatize on the subject. After the preparation of Bessemer steel, the manufacture of manganese in the blast furnace, and the synthesis of alizarin, the chemist dares not deny the industrial vitality of any reaction of his laboratory.

When we think of the value which certain metals, such as sodium and potassium, had when Davy prepared them by electrolysis; when we recall that by the process of Gay-Lussac and Thénard they cost some thousands of francs a kilogram, and that to-day, by electrolytic methods, they can be made for not more than 5 francs, we can not say of any chemical reaction that it shall have no industrial uses.

Only—and here I shall close—it is curious to see how many continuous efforts, how many different points of view, are involved in the solution of one of these scientific questions; or rather, I should say, to advance one of them, for in reality no subject is ever closed. It remains always open to our successors; we can only add a link to an infinite chain.

The advancement of science is slow; it is effected only by virtue of hard work and perseverance. And when a result is attained, should we not in recognition connect it with the efforts of those who have preceded us, who have struggled and suffered in advance? Is it not truly a duty to recall the difficulties which they vanquished, the thoughts which guided them; and how men of different nations, ideas, positions, and characters, moved solely by the love of science, have bequeathed to us the unsolved problem? Should not the last comer recall the researches of his predecessors while adding in his turn his contribution of intelligence and of labor? Here is an intellectual collaboration consecrated entirely to the search for truth, and which continues from century to century.

This scientific patrimony which we ever seek to extend is a part of the fortune of humanity; we should preserve it with full recognition of those who gave it the warmth of their hearts and the best of their intelligence.

LIGHT AND ITS ARTIFICIAL PRODUCTION.[1]

By Dr. O. Lummer.

PART I.

HISTORICAL INTRODUCTION.

Of the five senses of man that of sight is without doubt the one which first makes known to us the intimate relationship between internal self and the external world.

If we close our eyes, the magnificent colors and the manifold forms of nature with their lights and shadows all vanish. Everything is wrapped in dreary impenetrable darkness; we have lost the safe guidance of sight, and left to the uncertain guidance of the sense of touch, we grope around in this darkness. There is no light for us except where there is sight. The Greek philosophers were wrong in their belief that light was a something emanating from the human eye, which returning from outside objects rendered these visible. The evidence points to a something emanating from visible objects which, penetrating to the retina, evokes in the brain the sensations of light and darkness, color and luster. Newton and his school believed that the something emitted by luminous bodies consisted of minute material particles (corpuscles).

This theory was, however, supplanted by the wave theory of Huygens, according to which that something which is emitted by luminous bodies is not a stream of particles, but a wave-like motion of the "ether," which must be considered as permeating all forms of matter from the lightest gas to the densest metal. Infinite space is an ocean of ether in which all natural phenomena take place. Unresisted by friction, the planets glide swiftly through it. But just as a stone dropped into water produces ripples in its smooth surface, just as a vibrating tuning fork communicates its vibrations to the air, so does a source of light set in motion the ether. Sound and light, pitch and color, exactly correspond to one another. A source of light produces light waves in the ether just as a tuning fork produces sound waves in the air. The sound waves penetrating to the auditory nerves through the drum of the ear produce in the brain the sensation of sound, and similarly the ether waves, reaching the retina through the lens, produce the sensations of

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[1] Illustrated lecture by Dr. O. Lummer, delivered February 5, 1897, before the Berlin Polytechnical Society. Translated from the German.

light and color. Color and musical pitch are therefore purely subjective phenomena.

If the air is not in vibration no sensation of sound can arise, and similarly if the ether is not in vibration then there is no light. A tuning fork which makes as few as one hundred vibrations per second, affects the auditory nerves. But at least four hundred billion ether vibrations per second are necessary to produce the sensation of sight. We can form no conception of this extremely rapid vibration nor of the immense velocity of its propagation.

The lightning flashes, and in one-seventh of a second its light travels more than 26,000 miles, a distance greater than the circumference of the earth; in eight minutes it would reach the sun.

Bodies which have the property of emitting light by their own energy are generally called self-luminous bodies. To-day we shall consider only the terrestrial sources of illumination whose light we let shine as soon as that source of all light and being, the sun, turns aside its bright and benignant rays. As soon as the great sun sinks like a red ball of fire below the horizon the suns of other planetary systems, the stars, begin to twinkle and send their pale light to the earth. Their rays, tired and weak from their endless journey, tell us the happenings of many years ago. If Sirius should be extinguished to-day its light would nevertheless long continue shining brightly, for it takes years for a ray of light to journey to our earth from that star. Starlight can therefore be of no practical importance, and dark are the nights that the moon does not wish us well, reflecting graciously a few of the rays of the sun. But even she, the ever-faithful and true companion of the earth, fails to completely satisfy our longing for more light. The striving of man to lengthen the days and shorten the nights can only be fulfilled by his own energies, by exchanging philosophical speculations for a study of reality, and by making use of experimental research once so despised. How successfully this has been accomplished this brilliantly illumined hall gives the plainest testimony.

It, however, took many centuries to enable man to surround himself with such a dazzling flood of artificial light. Still, in the dim antiquity, as the book of invention records, the Persians, the Medes, the Assyrians, and Egyptians illuminated their temples, their palaces, their plazas, and streets in luxurious prodigality. In Memphis, Thebes, Babylon, Susa, and Nineveh they are said to have hardly known the difference between day and night. Along the streets there stood rows of bronze or stone vases filled with as much as 100 pounds of oil, which burned from a wick 3 inches in diameter. If such a long-buried civilization of the distant East could develop such a dazzling brightness, how far back must be the day that man learned for the first time to kindle the "divine spark." As valuable as time may appear to modern civilization, the amount of it at the disposal of science in the interpretation of physical phenomena is practically unlimited. For this reason the exact time that a spark was first kindled by man is of no importance here. We might rather be interested in the manner in which it was accomplished. Did fire in the shape of a meteor fall from heaven, was its terrible force first manifested in the glowing lava of the volcano, or are we indebted for it to the hard labor of man in his struggle with nature for existence?

The most natural and most probable explanation is that man learned to produce a spark at will while engaged in making the first weapons of stone. Fire was certainly discovered independently in numerous parts of the earth.

The significance of this first acquaintance of man with fire for the development of higher civilization can not be overestimated. It is reflected in the mythology and songs of all lands. Greek mythology elevates the fire bringer to the dispenser of light in a spiritual sense, while the Romans worshiped Vesta as the goddess of the hearth and also of the sacrificial fires, and in honor of the birth of light the eternal fire was guarded by the vestal virgins.

It is a great leap from the hearth fire and the sacrificial fire to the incandescent electric light and incandescent gaslight. For a long time the fire on the hearth served at the same time as a source of light, a custom preserved to this day in many German spinning rooms, and among the Eskimo no other light is known. First came the flickering kindling-wood pan, then the resin and pitch fagots, then reeds covered with wax, pointing to the impending important separation of light and fire, which was nearly accomplished in the lamp of the ancients and the taper of the middle ages. The principal aim to-day of artificial illumination is this separation of light and heat, an end we are gradually attaining, even though the time may still be distant when light will be produced, at least for common use, without attendant heat effect.

The candle owed its origin to the development of chemical technology in the early part of this century, by which solid fats, burning excellently, are made from cheap raw materials, and by which new substances, such as paraffine, are separated from coal tar. Neither was the oil lamp of the ancients neglected. The introduction of the hollow wick by Count Argand in 1786 and of the chimney by the apothecary Quinquet, of Paris, in 1765 were specially important, even if the substitution of petroleum for rape seed and olive oil was necessary to increase its efficiency to what it is to-day.

Gas lamps mark the transition to illuminating gas. In these the very volatile products of the dry distillation of tar, e. g., ligroin, benzine, petroleum, ether, etc., are first vaporized, and the resulting vapor is burned. Hydrogen gas passed through petroleum gives an excellent illuminating gas.

The ordinary illuminating gas, which was utilized in England as early as 1792, was not introduced into Germany until more than thirty years later. It was formed by heating bituminous coal in retorts to a red heat in the absence of air. The escaping gases, after being thoroughly cleaned by washing, etc., are collected in large receivers and maintained under pressure, by means of which they are distributed to consumers through iron pipes. Illuminating gas, or "philosophical light," as one of the first producers called it in his ecstatic joy, is therefore coal gas. That which remains of the coal in the retort is coke.

Looked upon at first as a wonder, illuminating gas seemed destined to entirely supplant tallow and oil. But it was not thus. It rather formed an incentive to improve the existing sources of illumination so that they could compete with it.

In the same manner the introduction of the electric light has not been a deathblow to gas, but has given it brighter life in the shape of Auer's incandescent gas light, and, as the discovery of acetylene shows, there are still further successes possible.

Every system of illumination has its individual peculiarities and especial advantages, which justify its existence and its worth. For this reason it is not easy to estimate the relative values of the various kinds of illumination unless the comparison be made on the rather unsatisfactory basis of the price per candlepower; that is, by making photometry the supreme judge.

MEASUREMENT OF CANDLEPOWER.

It is a comparatively simple matter to determine by photometry the relative intensity of the light emitted by two sources. All it requires is a piece of paper with a grease spot on it and a graduated scale along which the sources to be compared can be moved. [Experiment.] If the source R (fig. 1) is alone acting, the grease spot F on the paper screen SS appears to the eye at A dark on a bright background, since less light is reflected from the greased paper than from the ungreased portion of the screen. On the other hand, if the source L alone is active, the grease spot appears bright on a dark background. If both sources are active at the same time the effect will be similar to the above cases according to whether R sends more or less light, respectively, to the screen than L.[1]

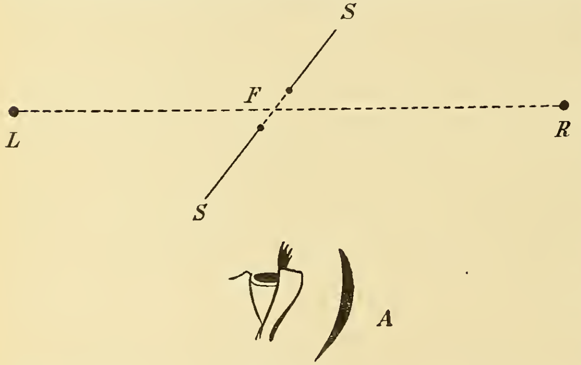

Fig. 1.

The luminous intensity produced at the screen depends, firstly, on the intensity of the source or its candlepower, and secondly, on the distance of the source from the screen, so that the intensity diminishes

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[1] This is on the assumption that the screen and the grease spot on it satisfy certain conditions. For these and for the bibliography see O. Lummer and E. Brodhun: Photometrische Untersuchungen, Zeitschrift für Instrumenten-Kunde, 1889.

to one-fourth, one-ninth, one-sixteenth, etc., when the distance is doubled, trebled, quadrupled, etc. By regulating the distance R F and L F the grease spot then can be made to disappear as well as by varying the intensity of the sources. In this case the ratio $\frac{RF^2}{LF^2}$ gives directly the ratio of the candlepower of L to that of R. (Compare note on page 276.)

In Germany the luminous intensities of all sources are expressed in terms of that of a Hefner-Alteneck lamp; e. g., gaslight is said to have an intensity of a certain number of the above units. Measurements of this kind are made in the Physikalisch-Technischen Reichsanstalt in Charlottenburg, and are officially certified to. Instead of the Bunsen photometer, above described, a more exact and more easily manipulated photometer is used. This was designed by E. Brodhun and myself and was constructed according to our specifications by Fr. Schmidt & Haensch, Berlin. It differs from Bunsen's principally in that the grease spot is replaced by a purely optical device by means of which the "ideal grease spot" is realized, one which transmits all the incident light and reflects none. [Experiment.]

By means of this device, and on account of the better definition of the ideal grease spot in comparison with Bunsen's, the accuracy of photometric measurements is much increased. I can not dwell on this point, but let it suffice to say that by means of the Lummer-Brodhun photometer the intensity of a source can with ease be determined to one fourth per cent of its value.

SOURCES OF ILLUMINATION ARRANGED ON A PHOTOMETRIC BASIS IN ORDER OF THEIR RELATIVE COST.

If we know in addition to the intensity of a light its price per hour of use, we have a measure of its relative economic value.

In the following table you will find interesting data concerning the price per unit intensity (1 unit = 1 Hefner lamp) per hour of the various kinds of illumination most used at present, on the basis of the assumptions made, as stated in the table.

Kind of light.	Price of materials or energy consumed.	Possible intensity in Hefner units.	Amount of material or energy consumed per hour of 1 Hefner unit.	Price per hour of 1 Hefner unit.
				Cent.
Incandescent gaslight	$1.25 per 1,000 cubic feet	30–60	0.06 cubic feet	0.008
Arc light (without globe)	.15 per kilowatt-hour	200–100,000	1 watt-hour	.015
Petroleum	.07 per quart	2–50	.003 quart	.021
Arc light (with globe)	.15 per kilowatt-hour	200–100,000	1.7 watt-hours	.026
Gaslight (argand burner)	1.25 per 1,000 cubic feet	about 20	.35 cubic feet	.045
Acetylene	12.50 per 1,000 cubic feet	2–50	.04 cubic feet	.050
Incandescent electric light	.15 per kilowatt-hour	10–500	4 watt-hours	.060
Gaslight (fish-tail burner)	1.25 per 1,000 cubic feet	2–20	.57 cubic feet	.070

NOTE.—The table given by the author has been converted into customary units by the translator. One Hefner unit equals approximately one candle power.

According to this table, the Welsbach light is cheapest; next comes the arc light; then follow in order the light of a petroleum lamp, arc light with ground-glass globe, ordinary gas light as furnished by an Argand burner, acetylene light, incandescent electric light, and finally gas light as obtained from the fish-tail burners, which are in most common use. A candle, as you perhaps all know, is even at the present time an article of luxury.

It must be admitted that this arrangement is quite arbitrary, as it is based on the average market price of materials, etc. For example, electric light is decidedly cheaper when supplied by a private installation and if the machines are always loaded to their full capacity. A plant of at least 100 horsepower can supply power at the price of 2½ to 3 cents per kilo-watt-hour; for smaller plants this becomes 5 or 6 cents. In the first case the arc light would be the cheapest, and the electric incandescent light would come third.

This order of value is entirely upset when we consider other conditions besides the question of cheapness as influencing the value. According to the purposes which a light is to serve these conditions are very different. For example, the very cheap arc light can not for one moment be considered for illuminating dwelling rooms, for it is not sufficiently divisible, and similarly the relatively more expensive incandescent electric light far surpasses the incandescent gas light on account of its enormous sanitary advantages.

Part II.

THE NATURE OF THE DIFFERENT KINDS OF LIGHT.

The consideration of these secondary actions of a gas flame introduces us to the second part of our subject, which deals with the nature of the different kinds of light and with the physical processes involved. In the third part I will try to elucidate the physical laws connecting the luminous intensity of a body with its temperature and with its nature, on which factors the intensity of a source of light depends. We will divide sources of illumination into two classes, namely, those in which the luminosity is produced by highly heating a body and those which produce the sensation of light at a relatively low temperature—cold flames, fluorescence, and others of that character.

In the first class we must distinguish between free-burning flames, such as a candle, a petroleum lamp, a gas flame, and an acetylene lamp, and electric lights—that is, according to the manner in which the luminous bodies are brought to incandesence.

Although my lecture will deal principally with the theory of luminosity produced by high temperatures, since this alone is the basis of all the sources of light in common use, I can not omit reference to the "cold flames" as long as they point out the direction in which we must look for the light of the future.

So-called "cold flames."—To these belong all those free-burning flames, such as that of carbon bisulphide, which emits a bluish, weakly light even at the relatively low temperature of 150° C. The classical representatives of this class are, however, the lightning bug and the will-o'-the-wisp. The phosphoresence at sea is also included in this class. Those who have not yet seen this natural phenomenon can form no idea of its magnificence, especially in southern seas. As soon as night falls and the stars appear in the heavens bright spots begin to glisten in the water, first only here and there at the bow of the ship; these increase in numbers; they come and go; they enlarge almost without limit, until finally spiral strands of gold seem to rise from the depths, often condensing to large nuggets.

This phosphorescence is also produced by living organisms, for billions of infusoria unite their weak light to produce the magnificent effect. Its origin is as unknown to us as that of the faint light emitted by wood in the process of rotting and other phenomena which have become generally known since the discovery of the X-rays. I refer to the luminosity of gases in Geissler tubes.

You are all familiar with the action of the Ruhmkorff coil used so much in medicine for the treatment of partial muscular paralysis, etc. If an electric current from such a coil—that is, one of high tension and high frequency—is sent through a highly exhausted Geissler tube, the residual gas shines with a magic blue-violet light. [Experiment.] By touching the tube we can assure ourselves that its temperature is not high. The peculiar striation of the light in the neighborhood of the electrode is an indication of the presence of a rarefied gas. In this second Geissler tube we have, in addition to the light emitted by the gas, the colored light of the glass, and especially at those parts of the tube made of uranium glass. We say that the glass fluoresces, without attempting to give an explanation of the curious phenomenon. Now, if a Geissler tube is still further exhausted the stratification of the glass becomes more indistinct and the luminosity of the gas finally ceases entirely.[1] Curious rays (cathode rays) radiate from the cathode—that is, from the negative terminal of the circuit—into the almost completely exhausted interior of the tube.

We can not see these rays, but they can be made visible by placing in their path fluorescent materials. In this tube asbestus is caused to fluoresce. [Experiment.] In this second tube the glass fluoresces at those parts on which the cathode rays impinge. You can plainly see the green surface of the glass wall which is turned toward you, and within this surface is a dark cross. That is the shadow produced by the screening of these mysterious cathode rays by a metal cross in the interior of the tube. If this latter is overturned the shadow disappears and the whole surface shines brilliantly. But the phenomenon appears

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[1] This tube is called by the lecturer a Hittorf tube, but is similar to that commonly known in America as a Crooke's tube. (Translator.)

still more wonderful if I move a magnet along the tube. You can see that the shadow of the cross moves with the motion of the magnet, showing that the cathode rays are deflected by a magnet.[1] Only those parts of the tube fluoresce to which I thus direct the cathode rays. If I introduce into a Hittorf tube in the neighborhood of the cathode a bar magnet, the cathode rays begin to rotate about the pole of the magnet and the fluorescence glides in a circle around the magnet along the sides of the tube. [Experiment.] In a similar manner it has been found by difficult experiments that cathode rays can be reflected, etc. But as long as they were confined it was difficult to get at them. Great progress was made when Lenard discovered a method of enticing the cathode rays out of the tube. Hertz had found that the cathode rays pass through aluminum with relative ease. Lenard constructed a Hittorf tube with an opening at one end sealed air tight by a piece of sheet aluminum one-hundredth of a millimeter in thickness. On exhausting this tube Lenard found that substances placed in front of the aluminum window could be made to fluoresce. In addition, these Lenard rays showed almost the same peculiarities which made the X-rays, discovered later, known to the whole civilized world. There was only one fault to be found with them—their intensity was diminished by passing through the aluminum and their action was therefore weak.

How superior in this respect to the Lenard rays are the X-rays discovered by W. C. Röntgen. No window is needed for these. They radiate freely from those portions of the tube on which the cathode rays fall, into the exterior space, and pitilessly destroy photographic plates which are placed as obstructions in their paths, however carefully they may be protected from the action of ordinary light. It is remarkable that they should have remained undiscovered so long and that they should have been discovered by happy accident. These Röntgen rays after an existence of hardly a year have already been so well known that I will confine myself to showing you the latest form of X-ray tube of the firm of Siemens & Halske, which is so constructed that the vacuum can be regulated at will. A screen coated with barium platino-cyanide is so brightly illuminated as to be visible in the farthest part of the hall. We now introduce between the X-ray tube and the screen a wooden block 10 centimeters in thickness (4 inches). On this is nailed a cross made of sheet lead, and you can probably all see the shadow of this cross on the screen, while the wood, although thirty times as thick, only gives the suggestion of a shadow. After the lecture, however, you can individually convince yourselves of the efficiency of these tubes. If the radiations are passed through a human body the beating of the heart and the motion of the diaphragm in breathing can be plainly observed.

The Röntgen rays owe their immense popularity to their ability of

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[1] Professor Goldstein has discovered cathode rays which are not affected by magnets.

penetrating flesh about a hundred times more easily than bones or metallic substances, aluminum excepted, and in this they have found their widest application. We can not help admiring this wonderful quality, whether the rays are an entirely new kind of longitudinal ether waves, or whether, as is more probable, they are merely transverse ether waves just like ordinary light waves, although of a very minute wave length, so that they may easily pass between the molecules of substances of moderate density. Let us rejoice that they have helped suffering mankind in their application to surgery, and let us hope that they will point out to science new lines of investigation. In all these experiments the electric current of the Ruhmkorff coil was conducted directly through the electrodes into the interior of the exhausted tube. If alternating electric currents of millions of alternations per second are used, so-called electrical oscillations such as are produced by the discharge of a Leyden jar or condenser, the Geissler tube shines brightly even if it is only held near the wire without any metallic connection whatever with it. The name of Nicola Tesla, an American, will be permanently associated with these phenomena, since he was the first to carry out experiments in this field on a large scale. The light effects produced by "electrical oscillations" are especially distinguished in that by means of them nearly the whole of the electrical energy supplied is transformed into light. H. Ebert conducted high frequency currents of a definite periodicity to a tube in the interior of which a specially selected fluorescent substance was placed. By the fluorescence produced by the cathode rays he obtained a luminosity of about one-thirtieth of a Hefner unit by an expenditure of one-millionth of a watt. The total energy consumed was about one-two-thousandth of that of a Hefner lamp. We have here realized very nearly an ideal artificial illumination, and with justice Ebert calls his lamp "the lamp of the future," although, on account of the technical difficulties involved and the small luminosity, it may be a long time before these lamps will be able to compete with those now used.

LUMINOSITY DUE TO HIGH TEMPERATURES.

The transition from luminosity at low to that at high temperatures is found in the ghost-like glowing observed in the dark during the slow oxidization of some substances—for example, phosphorus. This action is not really a combustion. Oxidation and combustion are essentially similar to each other; in both there is a combination of the substance with oxygen. But while the oxidation can take place at relatively low temperatures, ignition and combustion occur only at relatively high temperatures.

The development of heat from light which takes place in every fire, in every freely-burning flame, in a candle, a lamp, etc., is therefore nothing more than an oxidation or combustion; that is, the combination of a substance with oxygen at a high temperature.

THE PROCESS OF COMBUSTION.

Substances like rocks, which have no affinity for oxygen, do not oxidize, do not burn, and furnish no light and heat. But what a strong affinity do carbon, hydrogen, and the hydrocarbons manifest for all-consuming oxygen! When these elements are brought together there is a great development of heat attended most frequently by light effects.

If hydrogen is burned alone, water vapor is formed—that is, the combination of hydrogen and oxygen. [Experiment showing the oxy-hydrogen blowpipe.] If pure carbon is used, as when a diamond is burned, carbon dioxide is ordinarily formed, although poisonous carbon monoxide is formed with an insufficient supply of air. Both reactions take place simultaneously in all free-burning flames in which chemical compounds of carbon and hydrogen, so-called hydrocarbons, combine with oxygen. The most commonly used illuminating materials, as oils, tallow, fats, stearine, and wax consist principally of hydrocarbons and burn under favorable conditions. In order that oxygen may combine with hydrogen and carbon with the development of light, the combustible substance must both be in a gaseous state and be heated to a high temperature. In the case of the gas flame the works furnish the gaseous hydrocarbons, while the match supplies the necessary heat for ignition. The hot gas is then immediately attacked by the oxygen of the surrounding air with the formation of carbon dioxide and noncombustible water vapor. The heat produced is sufficient to heat the gas which follows so that this also can burn, and this action continues as long as gas escapes from the burner and as long as the surrounding air contains sufficient oxygen. The processes involved in every combustion are similar to those involved in the production of illuminating gas on a large scale. Every fire is the luminous effect of a gas undergoing the process of combustion. Every flame is a gas flame, and many other sources of light, petroleum lamps, candles, etc., are therefore miniature gas works.

Let us for a few moments consider the candle which, notwithstanding its modest appearance, is a small miracle, and whose importance is indicated by the fact that it forms the subject of six lectures by the celebrated English physicist, Faraday. The heat furnished by the match takes the place of the fire under the retort in the gas works. The stearine melts, rises in the wick, and is also vaporized by the heat of the match, in the same manner as coal is made into gas in the retorts by the heat of the fire beneath. The process of combustion from here on is that described above, the hot hydrocarbon gas produced at the end of the wick unites with the oxygen of the air, while the heat developed heats the gas which follows previous to its combustion. If all these processes are to be continuous and harmonious, as is the case with a candle which burns well and does not drip, we can not help admiring it as a small work of art. This involves a proper adjustment of the rate at which the material melts, the rate at which the molten material is vaporized, and the rate at which the vapor thus developed is consumed.

The proper burning of a luminous flame depends principally on the rate at which air is supplied. If the wick is too large or too small the candle does not burn regularly; it either smokes or gives an insufficient quantity of light. You all understand what is meant by saying that a lamp "smokes." Under those circumstances the wick is too large, the amount of gas developed is too great for the air supply, and clouds of smoke are formed. If the flame is reduced by screwing down the wick the smoking ceases, but even then the carbon is not entirely consumed. With an insufficient air supply all the carbon particles can not be satisfied with oxygen. This is fortunate for us, for the uncombined particles of carbon are heated to incandescence by the hot carbon dioxide.

A flame can not emit light unless it contains uncombined solid particles in an incandescent state. If I mix illuminating gas with air before combustion and ignite the mixture the flame no longer shines brightly, but gives forth a weak blue-violet light. [Experiment.] The explanation is that in such a mixture the oxygen can combine with all the carbon particles so that the flame contains no solid particles at all, for to these alone a flame owes its luminosity. Transparent gases heated ever so high hardly emit any light and hence the weak luminosity of a Bunsen flame.

One consequence of the combustion of all the carbon contained in illuminating gas is the increase of the temperature of the flame, since there are no foreign particles to be heated as in the ordinary flame. In the Bunsen flame all the carbon contributes to the heat developed, while in a light-giving flame less carbon is burned in the first place, and this uncombined carbon abstracts heat from the hot products of combustion in the second place. The Bunsen flame is therefore hotter than the ordinary gas flame. But on this account it emits less light.

In order to make it luminous it is necessary to introduce in it incombustible materials. You see how brilliantly a piece of sheet platinum shines on being placed in a Bunsen flame. [Experiment.] If it is introduced into the still hotter oxyhydrogen flame it is heated to whiteness and then melts. [Experiment.] By substituting for the platinum an infusible substance, such as lime, chalk, or magnesia, a dazzling light is produced which illumines the whole hall. [Experiment.] Brighter still than this so-called Drummond's lime light is the zirconium light, in which zirconium is burned in the oxyhydrogen flame. The intensity of ordinary gas flame can not be compared with that produced by solid substances in an incandescent state. It was therefore a great step in advance in the art of illumination when Mr. Auer von Welsbach succeeded in producing greater luminosity in a gas flame in a similar manner to that in the lights just mentioned, by placing a mantle of incombustible material in the very hot but nonluminous Bunsen flame, thus bringing the mantle to incandescence. [Experiment.] If the maximum luminosity is to be obtained, the incandescent body must satisfy many theoretical conditions; especially, it must not reduce the temperature of the products of combustion and must emit as much light as possible corresponding to its temperature. Auer's invention is not new in principle, and indeed had been anticipated in practice. For instance, it is said that the streets of Nantes were illuminated by incandescent gaslights in which a mantle was brought to incandescence in the hottest part of a Bunsen flame. The reason that these lamps were so soon relegated to forgetfulness was entirely due to the nature of the mantle, which was made of platinum wire instead of the material Auer used. This example illustrates once more how the very best principle can fail, and must fail if ignorantly and improperly applied. In order to understand the superiority of the Auer light over the ordinary gaslight and the related questions, why does one source of light emit more light than another, and how does the increase of temperature influence the intensity, we must first familiarize ourselves with two fundamental physical laws known as Stefan's law of radiation and Kirchoff's law of the absorption and emission of light. Before doing this however, I will briefly consider the other sources of light commonly employed.

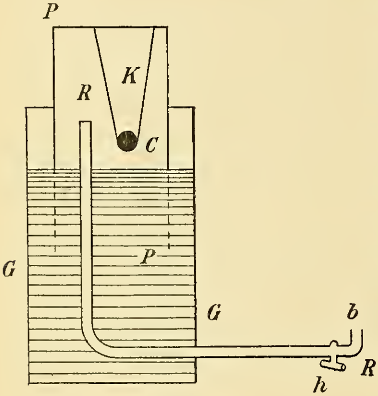

Fig. 2.

A new addition has recently been made to the free-burning flames, namely, the much-renowned and now almost notorious acetylene light. Director Schultz-Hencke was kind enough to place at my disposal for this evening a generating apparatus which you see before you. [Experiment.] Figure 2 shows its construction. It is built like an ordinary gasometer, the outer vessel G G is partly filled with water, through which the tube R R, provided with a cock *h*, extends above the level of the water. In the outer vessel is contained a second vessel P P, which carries suspended from the lid in the interior a basket K containing about 200 grams of calcium carbide. The cylinder P P floating on the water sinks by gravity as soon as the cock at H is opened, and the inclosed air escapes through the tube R. But as soon as P P has sunk so deep that the calcium carbide is brought into contact with the water, acetylene gas is produced, and a very explosive mixture of air and acetylene gas escapes from the burner *b*. Nevertheless, I apply a burning match to the burner. The gas is finally ignited and burns with increasing brightness, while the cylinder P P sinks with the basket K into the water; that is, with diminishing air content of the gas. Finally only pure acetylene gas escapes, and you are no doubt surprised at the fullness of the white light which fills the dark hall. But in this flame, as in the others, the luminosity is due to uncombined carbon particles, though judging from the color of the light, the temperature is far in excess of that of the ordinary gas flame.

The question may be asked, why does not the mixture of air and acetylene first issuing from the burner explode? The answer is, that the heat from the match can not penetrate through the narrow aperture of the burner and through the long tube R into the interior of the cylinder PP, otherwise a violent explosion would surely have taken place; for all combustible vapors mixed with air are extremely explosive and are similar in their action to gunpowder. If a spark comes in contact with them the ignition spreads rapidly from the given point through the whole mixture. The enormous increase in volume thereby produced exerts a sudden pressure correspondingly large on the inclosure which, therefore, bursts if it is not able to withstand the pressure to which it is thus subjected. [Experiment.]

The apparently empty space in this bottle, containing a small quantity of petroleum, similar to the reservoir of a nearly empty petroleum lamp, is filled with petroleum vapors mixed with air. I ignite the mixture by means of an electric spark and you hear the report produced by the blowing out of the cork stopper. In blowing out a lamp care must be taken not to turn down the wick too far, so that no opening into the interior of the petroleum reservoir is produced, through which the flame can enter when being extinguished. It is best to cut off the air supply by covering the wick with a cap which can be manipulated from without. At all events the flame should not be turned down too low before it is blown out.

That which makes acetylene gas so dangerous is its property of spontaneously exploding at a pressure of only two atmospheres. By exerting a greater pressure on the reservoir PP, as I now do (experiment), this alone will suffice to produce an explosion.

NATURE OF ELECTRIC LIGHTS.

In electric lights the light is produced by incandescent carbon just as it is in freely burning flames. In the electric incandescent light a carbon filament (for instance, of carbonized bamboo fiber with chemically precipitated carbon) serves to carry an electric current, and in consequence of the resistance which it offers to the current it becomes heated. In the electric arc light the current passes between two rods of retort carbon through an air space, under which conditions an electric arc is formed in this space and the ends of the carbon rods are heated to the temperature of volatilization of carbon. In this respect electric lights are similar to gas flames, for in both highly heated carbon is the source of the luminosity. But while in all flames, and also in the incandescent gaslight, the heating of the carbon or the mantle is produced by the heat developed in the combustion of the gas, in electric lights the heat of combustion of coal or of gas serves first for the production of electric energy, which can be transmitted over considerable distances and which, far from the furnace, heats up the filament of the incandescent lamp or the carbons of the arc lamp to a white heat. Naturally a considerable quantity of energy is lost in this roundabout process. For while in a gas flame the heat energy of the gas is directly transformed into light, with electric lights, for which, for example, the power is originally supplied by a gas motor, the gas first sets in motion the gas motor, this in turn drives the dynamo, and finally the electric current thus produced brings the carbon to incandescence. Only a few per cent of the heat of combustion of the fuel reappear as light. For this reason, however, this roundabout process furnishes most acceptable light. In it only the power absolutely necessary for heating the carbon filament is conducted into the room, while the noxious products of combustion of coal, of petroleum, of gas, etc., are taken care of in the electric power house. In the incandescent electric lights the only heat introduced into the room is that necessary for raising the temperature of the filament and which the filament radiates in the form of light and heat.

The gas flame, the petroleum light, the candle, in short, all gaslights in the broadest sense of the word necessitate very much greater heat effects. In all of them there is a continuous current of the products of combustion, together with the carbon particles producing incandescence. In order to maintain around the flame the high temperature required for the incandescence of the carbon particles a large space must be brought to a high temperature. All parts of the flame, the chimney of the lamp, and the surrounding air must become hot if the combustion temperature of the gas is to reach its highest value, and thus enable the carbon particles or the incandescent mantle to radiate most efficiently. The heat carried off by the products of combustion is partly utilized in the regenerative burner of Fr. Siemens, in which the gas supplied to the burner is first heated by these hot gases. We can thus see why all flames are attended by the evolution of large quantities of heat. But, unfortunately, the flame emits not only heat. The most important product of combustion is carbon dioxide, which is produced at the expense of the oxygen of the air, and with which, together with other noxious gases (carbon monoxide, sulphur dioxide, etc.), the air becomes charged.

This harmful action accompanies the ordinary gaslight as well as the incandescent gaslight, although in the latter it is much less for the same candle power. While the electric light is much more expensive than the incandescent gaslight on account of the very poor return for the heat of combustion of the fuel, its advantages over the free-burning flame is very great from a sanitary standpoint. By giving proper consideration to these secondary actions of the different sources of light it is surprising that, in the choice between the two kinds, the consideration of cheapness plays such an important rôle. In addition, the danger of an explosion is entirely excluded in an electric light, and, moreover, the process of turning it on is an exceedingly simple one.

Having considered the nature and performance of the sources of illumination in common use, we will proceed to the third part of our subject, in which we will familiarize ourselves with the physical reasons why the intensity and color of different lights is so different, and in which we will discuss the factors on which the luminosity of a source of light depends.

PART III.

THE PHYSICAL LAWS OF LUMINOSITY.

The radiation of light and heat.—The sensation of light is purely subjective, as has already been pointed out. A pressure on the eye is sufficient to produce it, but ordinarily it is produced by ether waves penetrating to the retina and there producing the sensation.

A source of light—the sun, for example—excites the optic nerve. The same ray of sunlight falling on our skin and evoking the sensation of warmth would produce in the eye the sensation of light and would decompose the sensitive silver salts of a photographic plate. We therefore speak of "heat rays," "light rays," and "actinic rays" corresponding to these three different effects, although the three kinds of rays correspond to motions of the ether, differing principally in their periods of oscillation. Every luminous solid emits ether waves of all possible wave lengths, of which only those of a length between four ten-thousandths and eight ten-thousandths millimeter can affect the optic nerve. The existence of the other waves can not be demonstrated by the eye, but can be determined by sensitive physical apparatus. [Experiment.] A spectrum is produced on the distant screen by the light emitted by zirconium heated in the oxyhydrogen blowpipe. The spectrum is similar to the rainbow, produced by the refraction of sun rays in rain drops. Every color of the spectrum corresponds to ether waves of a definite wave length, the wave length decreasing continuously from the red to the blue. This visible spectrum, however, embraces only a small portion of the waves emitted by the zirconium light. Both to the left of the red and to the right of the blue there are ether waves the existence of which can be demonstrated. The former, in the infrared, are called heat rays, for their existence can be shown by sensitive thermometers, such as the radiometer, the thermopile, the bolometer, etc., while the latter, the ultraviolet rays, are called chemical rays, on account of their photographic activity. They all agree, however, in that they carry with them a definite quantity of energy which is transformed into heat when intercepted by a thermometer. In this respect all the rays emitted by a luminous body are "heat rays," although the energy corresponding to the violet and the ultraviolet rays is very small compared with that of the red and infrared rays. The heat emitted by a candle at a distance of 1 meter would raise the temperature of 1 gram of water, hardly a thimbleful, by only 1° C. in one and one-fourth years, provided it could be stored for that length of time. This will give you some notion of the extreme sensitiveness of the eye for light rays. But nevertheless it takes a definite quantity of energy, a very small quantity, to produce the sensation of light. Hence a body radiating light waves of this intensity just begins to become visible. This incipient incandescent state begins at a relatively high temperature in the sources of light commonly used. It is not that these waves are not present at lower temperatures, but their energy is too small to affect the optic nerve. Just as soon as the critical temperature is passed, the heated body becomes luminous and its brightness increases rapidly with increasing temperature. The amount of energy producing the sensation of light is however a minute fraction of the total energy radiated by the body. This total radiation obeys definite laws in respect to its increase with the temperature, and so does that portion of it which affects the eye. Before considering these laws, let us first see how these two kinds of radiation can be separated.

Separation of heat radiation from light radiation.—I have here a strip of sheet platinum through which an electric current of any strength up to 100 amperes can be transmitted. [Experiment.] The current heats the strip, and as it increases the temperature of the metal increases until it finally begins to glow, and by a still further increase it is heated to intense whiteness and then melts. Before it begins to glow it nevertheless emits energy, for the heat radiated by it can readily be felt by placing the hand near it. In order to show this development of heat to all of you I make use of a so-called "differential thermometer" (fig. 3). It consists of two glass bulbs connected by a tube *acb*, which contains a quantity of liquid separating the air in the two bulbs. This liquid column is displaced when either bulb is heated. On the projection screen you see an enlarged image of one of the liquid surfaces. As soon as I bring up the platinum strip, through which the current is passing, to one of the bulbs previously coated with lampblack, the level of the liquid immediately begins to change, although the platinum is not emitting visible radiations. The ether waves emitted are simply absorbed by the lampblack and transformed into heat, which is imparted to the inclosed air. This expands, and the liquid is forced over into the other bulb.

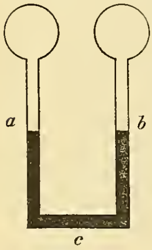

FIG. 3.

The higher the temperature of the platinum the more rapidly does the air of the differential thermometer expand when the platinum is brought near it. Now it glows with dull redness, and the level of the liquid is displaced beyond the limits of the screen. But as soon as a glass plate, or still better a vessel containing water, is interposed between the bulb of the thermometer and the platinum, the level of the liquid returns to its original position. This shows that glass and water absorb the whole of the heat emitted by the glowing platinum. Our method is not sensitive enough to detect the light radiations transmitted through the water when the platinum is heated to whiteness, but this can, however, be very easily measured by a sensitive bolometer. As soon as the vessel is removed, the level of the liquid changes rapidly.

The property of absorbing heat rays and of freely transmitting light rays is utilized in the construction of glass windows. Although long unknown to man, they separate, for his comfort, warmth and light, preventing the escape of the heat of the room from within and preventing the penetration of cold from without, while they transmit sunlight almost undiminished.

On the same property depends the action of clouds in retaining the heat of the earth. Impenetrable for the long heat waves, the clouds prevent the earth from radiating its warmth received from the sun during the day into infinite space and thus protect it from loss of heat. Without this moist blanket the earth would lose enormous quantities of heat by radiation during the long, starry, winter nights, and thus a strong cooling would take place. The fact that our eye is insensitive to waves of great wave length is due to the absorptive action of liquids. The infra-red rays are simply absorbed by the lens and aqueous humor of the eye before they reach the retina. According to Darwin, this indicates that our eye is modeled after that of the Amphibia. For these, the absorption of the infra-red rays by the eye is of small importance, for they have already been absorbed by the upper layers of the water, in whose depths their eyes must still be able to see.

From the experiment you have just seen we recognize that the transition from a nonluminous to a luminous body, or better, from one radiating heat to one radiating light and actinic rays, is in reality determined by a sufficient increase of its temperature. It matters not how its temperature be increased, whether by the electric current or by the direct application of heat, a material heated electrically or in a furnace begins to glow at the same temperature.

Moreover, the luminosity of a hot body is a secondary property—one might almost say an accidental property. If it were not for the eye the luminous state could only be distinguished from the nonluminous in that the energy of the different waves is not the same. This is not surprising when we consider that the heat residing in a body, and on which its temperature depends, consists solely in the energy of motion of its molecules; that is, of its smallest mechanically conceivable parts.

HEAT IS MOTION.

It was long supposed that a heated body emitted a subtle material in the same way as it was supposed that light was a material substance.

We now know, thanks to the researches of Rumford, Joule, Mayer, and Helmholtz, that heat and molecular motion are identical, and that the greater this molecular motion the greater is the temperature of the body. By hammering, drilling, etc., the materials operated on finally become so hot that they can not be touched without burning the fingers.

The work performed by our muscles in hammering a piece of lead is partly transformed into heat and thus applied to increasing its temperature. The motion of the hammer has been transformed into the motion of the molecules of the lead. This transformation of work into heat follows definite laws, so that a definite quantity of heat is equivalent to a definite amount of work done. The law of the "mechanical equivalent of heat" states the numerical relation of one to the other. In nature, where a certain quantity of heat appears to have been lost in the transformation—as, for example, in the production of electric light—it certainly reappears somewhere in the process as heat; that is, as an increase in temperature. The law of the conservation of energy expresses the recognition of this fact.

Every body in nature must then be considered as consisting of molecules in a state of rapid motion. Solids, liquids, and gases only differ in the nature of the motion. If a solid is heated to a sufficiently high temperature it is finally transformed into the liquid or gaseous state. Even carbon evaporates at 3,600° C. Corresponding to this increase in temperature, the motion of the molecules becomes more and more rapid and the energy of the ether waves emitted increases with it. All apparatus which can absorb heat—our skin, a thermopile, a bolometer, etc.—indicate how rapidly the radiation of a body increases with increasing temperature. By means of our differential thermometer we can show the increase of the radiation of the electrically heated platinum strip with its temperature. The law involving this relation for solids was first stated by Stefan, and was deduced by theory for a so-called "black" body by Boltzmann. If we call the total radiation of the body the sum total of the energy corresponding to all the ether waves emitted, and if we call the absolute temperature of a body its temperature on the Centigrade scale, increased by 273°, Stefan's law of radiation may be stated as follows: The total radiation is proportional to the fourth power of the absolute temperature.

An example will make this law plainer. Let a body at 27° C. be heated to a temperature of 327° C., i. e., from an absolute temperature of $273° + 27° = 300°$ to one of $273° + 327° = 600°$, thus doubling the absolute temperature of the body; but if the absolute temperature is doubled the total energy emitted is increased, according to Stefan's law, 2^4 or 16 times. If the absolute temperature is trebled the total radiation is increased 3^4 or 81 times, etc. You will thus recognize how rapidly the energy of radiation increases with increasing temperature. I must not omit to state that the energy maximum of the whole spectrum is displaced from the red end toward the violet. If the energy of certain ether waves is great enough they affect the eye as light and the body emitting them is "self-luminous." We can now distinguish between sources of heat and sources of light—between heat radiation and light radiation. Objectively both kinds of radiation are parts of the total radiation emitted by the body. In the same manner as the total radiation, the partial radiation must rapidly increase in intensity with increasing temperature. The light radiated by an incandescent body will therefore increase much more rapidly than the temperature.

To form a general notion of the relation between light radiation and the temperature we will make use of a law recently proposed by Wien, following in Boltzmann's footsteps. The law reads as follows: "In the normal emission spectrum of a black body the energy maximum is so displaced by the variations of the temperature that the product of the temperature and wave length remains constant." In connection with Stefan's law it states that the maximum energy in the normal spectrum is proportioned to the fifth power of the absolute temperature. Basing our conclusions on both laws, we may certainly assume that the light radiation increases still more rapidly with increasing temperature than the total energy, since the energy maximum in highly heated bodies is certainly in the visible part of the normal spectrum. Unfortunately there are practically no experimental data on the relation between the light radiation and the total radiation at different temperatures, and we will therefore assume that light radiation as well as the maximum energy in the normal spectrum is proportional to the fifth power of the absolute temperature; that is, that the brightness of a luminous black body—for instance, the carbon filament of an incandescent lamp—will increase 2^5 or 32 times if its absolute temperature be doubled, or 3^5 or 243 times by trebling its absolute temperature.

Before we deduce the consequences of these laws we must discuss whether they can be applied to the incandescent bodies used in practice. To do this it will be necessary to familiarize ourselves with a law which forms the basis of spectrum analysis and which has attained widespread importance. I refer to Kirchhoff's law of absorption and emission of light, which states that a heated body at any temperature emits only those particular rays which it absorbs at the very same temperature. If we apply this law to bodies rendered luminous by high temperatures it states that all those bodies are nonluminous, however high the temperature, which either freely transmit the light rays or which reflect them entirely instead of absorbing them. Highly heated gases absorb no light and therefore do not emit any, as you can see in the Bunsen flame. [Experiment.] Carbon absorbs a large proportion of incident light even in an incandescent state. If heated to the same temperature as a Bunsen flame it emits a correspondingly large amount of light. All solid bodies and all metals exhibit a similar behavior. These absorb to a greater or less degree waves of all wave lengths and therefore emit waves of all wave lengths. Consequently they emit at a sufficiently high temperature white light, since white light consists of a mixture of light of all wave lengths. The absorptive power of an incandescent body can therefore be determined by the light it emits, and conversely, the color of the light emitted can be determined from its absorption. The greenish color of the Welsbach light is most probably due to the selective absorption of green light by the material of the mantle at its high temperature. In order to produce white light it would be necessary to combine the substances now used in its construction with materials which would be more partial to the red light.

Bodies which completely absorb all wave lengths at all temperatures and therefore emit rays of all wave length are called "absolutely black" bodies. In nature these ideal conditions are never realized, and, moreover, the laws of Stefan and Wien apply only to them. Nevertheless these properties are almost realized if the interior of a hollow sphere is permitted to radiate through a small opening in its wall.[1]

Experiments on the radiation of an absolutely black body are in contemplation at the Reichs-anstalt, by which it is to be determined how nearly carbon, the metals, and their oxides correspond to an absolutely black body. It is, however, certain that the absolutely black body is the only one which emits the maximum amount of energy theoretically possible. It emits, therefore, other things being equal, more light than any other substance. The absolutely black body would be the first to evoke the sensation of light with gradually increasing temperature and would emit light when metals, e. g., platinum, at the same temperature would still be nonluminous. The luminosity of a body, especially its first visible manifestation, depends also to a great extent on the organ of sensation; therefore we must in addition study the structure of the eye. But we will first take up the study of the conditions under which heated bodies just begin to emit light.

THE FIRST SIGNS OF LUMINOSITY OF HEATED BODIES (GRAY HEAT AND RED HEAT).[2]

According to the modern theory of heat discussed above, we must assume that a solid body, certainly a black body, emits waves of all possible wave lengths, at all possible temperatures, and therefore even at the ordinary temperature. At every temperature ether waves, of a wave length corresponding to light waves, reach the retina of the eye. These waves are, however, not recognized as light waves until the energy accompanying them is great enough to affect the optic nerve. Under these circumstances a body is said to emit both heat and light.

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[1] W. Wien and O. Lummer: A method of investigating the law of radiation of an absolutely black body. Wied. Ann. d. Physik u. Chemie, 1895, Bd. 56, S. 451–456.

[2] The statements concerning gray heat and red heat mentioned in the lecture are partly the results of some personal observations as yet unpublished.

If we gradually heat this platinum strip [experiment] you will see that it first shines with a reddish light. This first stage of luminosity is called red heat. All solid substances which are incombustible at high temperatures behave in the same manner as platinum. The temperature at which solids begin to glow was determined by Draper some fifty years ago and was found by him to be about 525° C. Most solid bodies, therefore, would have to be heated to an absolute temperature of $273° + 525°$ or 800° before becoming self-luminous. In order that the other wave lengths may affect the eye the temperature must be increased very much above 525° C. The observations of Draper, that solids first emit a red glow, were not contested for a long time, since they agreed with ordinary daily experiences. I need only instance a red hot poker, a very hot stove, etc. It was all the more interesting, therefore, when W. F. Weber showed, some ten years ago, that the red heat is not at all the first stage of luminosity.

On repeating the experiments of Draper in 1886, Weber carefully excluded light from all external sources and observed that solid bodies emit, at much lower temperatures than that corresponding to redness, a foggy, grayish light. The first trace of the grayish light appears to the eye as an unsteady gleaming, flitting hither and thither. Its intensity increases very rapidly with increasing temperature, while its appearance changes from dim gray successively to ashen gray, a yellowish gray, and finally to red. To use Weber's words, "with the first appearance of the redness the last trace of grayness disappeared, as also did the unsteadiness, which has been prominent in all stages of gray heat."

As might be deduced from Kirchhoff's law, the redness as well as the preliminary stage of luminosity begins at a temperature depending upon the nature of the substance. In fact, investigations by R. Emden show that gold begins to glow at 403° while platinum does not begin until its temperature is 423° C.

Weber's observations are specially interesting to me because they seem to give us some explanation of the structure of the eye, and especially of the properties of the retinal elements (the rods and cones) and of their functions in color sensation. Stenger first pointed out that Weber's experiments did not give any evidence concerning the physical nature of the spectrum emitted by the body. I will show that these queer and ghost-like phenomena of the "gray" light can only be explained by attributing entirely different functions to the rods on the one hand and to the cones on the other, considering them as two different organs of sight, just as is done in modern physiology.

Based on the latest physiological researches of Hering, Hillebrand, Ebbinghaus, Preyer, Brodhun, Tonn, and others, and based on the theory of A. König[1] of the function of "visual purple" in sight, J. v. Kries[2] of Freiburg, suggested a theory of vision which easily explains

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[1] A. König: "Ueber den menschlichen Sehpurpur," etc. Sitzsber. d. Akad. zu Berlin, 1894, S. 577.

[2] J. von Kries: "Ueber die Funktion der Netzhautstäbchen." Z. S. f. Psychologie u. Physiol. d. Sinnesorgane, Bd. IX, S. 81–123.

many contradictions in the observations hitherto made, and which unfolds many new relations. According to this theory the cones produce the sensation of sight when the stimulus is very intense, and their excitation produces in the brain a color sensation, while the rods represent the organs of a color-blind eye and make no distinction of color whatever. If the intensity is very small the rods are at first only affected and evoke in the brain an impression of light without color, and thus increase the sensitiveness of the organ very greatly in the dark. But as soon as the stimulus is sufficiently increased, the cones are affected and the sensation of color is added to that of light.

Even König had assigned a special rôle to the rods in his color theory. Based on the absorptive properties of "visual purple" for waves of all wave lengths, König drew the conclusion that the absence of color to the normal eye in the spectrum, if its intensity is very small, and the sense of sight of persons who are totally color blind, is essentially a consequence of the decomposition of "visual purple."[1] Since only the rods are charged with "visual purple," and since the cones do not contain any, the rods are the only parts of the eye affected in colorless vision of the normal eye when the intensity is very small, and are the only elements that can be affected in total color blindness by any luminous intensity. But while König draws the conclusion from his experiments that the cones do not respond to blue light, since "visual yellow" or decomposed "visual purple" produces the sensation of blue, Kries was led to the hypothesis that the cones are excited by light of all colors without exception. Now there is on the retina a central portion called the retinal depression or fovea centralis, on which there are only cones and no rods, while on the rest of the retina both elements are found; so that as we pass from the center outward the number of rods increases in comparison with the number of cones, until near the periphery the former outnumber the latter. Moreover, the fovea centralis is that portion of the retina corresponding to most distinct vision, the part on which the image of an object which we wish to closely scrutinize is focused.

In direct or foveal vision, therefore, the rods are not concerned at all, while in indirect or peripheral vision both the rods and cones are excited. Therefore if the luminous intensity is small enough the rods only are affected, producing the sensation of grayness, of light without color. Long before physiologists had reached these conclusions comparative anatomists were led to the recognition that the rods of a retina make it possible for an eye to see in the dark.

Zoologists, Max Schultze for example, as early as 1866 were aware that animals such as the owl, which preys in the darkness, or the mole, condemned to spend its life beneath the ground, were provided

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[1] This kind of total color blindness for small intensities in normal eyes, discovered and thoroughly investigated by Hering and Hillebrand in 1889, had already been observed in 1873 by W. v. Bezold in connection with his investigations on the law of color mixtures and the theory of color vision.

with rods even at the point of most distinct vision, where we have only cones, and that there are even prowlers of the night which have only rods on the retina and no cones whatever. Like the latter class of animals, we see in the dark by the same retinal elements, and indeed we are color-blind if the intensity of the light is too weak to excite the cones.

The theory of Kries receives a new confirmation if applied to the explanation of Weber's experiments on the initial stages of luminosity of bodies, and at the same time it throws a new light on these little-understood phenomena.

Let us first deduce from the theory of Kries what phenomena should be observed if a normal eye intently views the surface of a piece of sheet platinum gradually heated by an electric current. Let us assume, moreover, that the eye is directed to the center of the sheet, so that not only the fovea centralis, but also the surrounding parts of the retina, may be affected by the rays emitted. If the eye has been sufficiently rested so that the rods have attained their maximum sensibility, the latter are excited as soon as the platinum reaches a certain definite temperature (400° C., according to Weber), producing in the brain the sensation of light without color, which increases in intensity rapidly with increasing temperature. So long as the cones are not sensibly affected, the foveal parts of the retina transmit no sensation to the brain. This is attended to by those parts of the retina which are not ordinarily involved in direct vision. The curious condition then arises that we see something which we are not looking at, and therefore the object thus seen escapes as soon as the eye is directed to it.

The sensation of gray is therefore produced by the rods, and can only be produced by indirect vision. This easily explains the "unsteady flitting about of the light" observed in all stages of the phenomenon by Weber. It ceases as soon as the temperature, and therefore the intensity, is increased to a sufficient amount to excite the cones. Then we begin to see the glowing by direct vision, exactly as we are accustomed to do, and the unsteady gray changes gradually into the reddish glow observed. This transition occurs, according to Draper, at about 525° C.

The observed color depends partly on the size of the portion of the retina producing the sensation. Each set of elements acts independently of the other in transmitting its intelligence to the brain, and therefore the effect produced is a composite one.

If we assume that the cones first respond to yellowish-red waves, the first color sensation would likely be a foggy red, changing gradually to a reddish-yellow, just as Weber describes.

On account of the greater number of the cones, the rods, only endowed with their greatest sensibility in the dark, lose their supremacy with the increase of luminosity, and thus the color becomes more and more fiery—600° C. corresponding to bright redness.

The phenomenon transpires quite differently if the observations are made so that only the rods or only the cones can be affected. In the first place, the intensity of the gray sensation would have to increase with increasing temperature without ever passing to redness. We might expect to observe this state if the radiations are confined to the outer portions of the retina, where the rods predominate.

If, however, a very small surface be observed so that only the yellow spot, where there are no rods, is affected, the gray stage would be entirely eliminated, and the surface would, when first visible, appear red.

A few observations I have personally made verify on the whole these deductions. It is very easy to show that a small platinum surface heated electrically gives out a grayish light by indirect vision before it is visible at all by direct vision. The following experiment will illustrate this more clearly:

I look at a surface of molten niter (temperature 550° C.) through a small hole in the cover of the bath. The conditions are such that very close to the hole the whole surface is simultaneously visible, while at greater distances the visible portion is limited by the size of the opening.

In accordance with theory the reddish glow of the surface, seen from the distance, assumes on approaching the eye a whitish appearance, until finally there remains hardly a trace of color in the sensation produced.

The following experiment indicates the far greater sensitiveness of the rods at small intensities. If a small opening in a hollow copper sphere, heated to 600° C., is viewed by direct vision, it appears bright red, corresponding almost to the radiation emitted by an absolutely black body at the same temperature. But by directing the eye slightly to one side the opening appears to emit the colorless, grayish light, and assumes the appearance of the full moon in the heavens on a starry night.

These experiments have been repeated, and their results confirmed by Prof. E. Pringsheim.

PHYSICAL BASIS OF CLASSIFICATION OF SOURCES OF LIGHT.

As soon as the initial stages have been passed, the intensity increases very rapidly with increasing temperature, at the same time taking on more and more of a color such as is produced when we add to red yellow, then green, then blue, and finally violet. At a sufficiently high temperature the light produces the sensation of white and the temperature is called white heat. You can follow the various stages of luminosity produced by gradually heating this strip of platinum up to its melting point. [Experiment.] Immediately before melting, the strip emits a fullness of white light which makes this large hall almost as bright as day. But nevertheless the temperature corresponding to bright redness is a little less than half of that corresponding to whiteness, for the melting point of platinum is about 2,000°, while the redness corresponds to 800° on the absolute scale. If we assume Wien's law as expressing the increase in intensity with the temperature, the energy emitted by platinum at the absolute temperature of 800° is to that at 2,000° as 800^5 is to $2,000^5$, i. e., roughly in the ratio of 1 to 100. The mantle of an incandescent gas light is probably at about the same temperature as that of melting platinum if we assume that it attains the highest temperature of the flame. If we assume, in addition, that thorium oxide radiates as much energy as platinum at the same temperature, an assumption not altogether justifiable, the amount of light emitted by a given area of the incandescent substance would have to be equal in both cases. In reality thorium oxide corresponds more nearly to a black body, in Kirchhoff's sense of that word, and therefore the former would radiate, ceteris paribus, more than the latter. The experiment I will now show will teach us how much more light ferric oxide radiates than polished platinum. I have written with ink a few words on the piece of platinum, and I now heat it to incipient whiteness by passing an electric current through it. I will project on the screen the side of the strip on which I have written and you can plainly see the words appear bright on a darker background. This shows that the iron oxide left on heating the ink radiates much more light than does the polished surface of the platinum at the same temperature.

By the assistance of our knowledge of the radiating power of luminous substances and of the temperature at which they become luminous I can compare theoretically the intensities of different sources of light. Unfortunately, considerable difficulties are met with in the measurement of high temperatures. According to the latest measurement of the radiation of the sun by Paschen its temperature is about 5,400° C., and according to Violle's measurements the temperature of the electric arc is about 3,600° C. Assuming that the material of the sun radiates light as well as carbon, the quantity of light emitted by a given area in the two sources is in the ratio of 3^5 to 2^5, or about as 8 is to 1. Now the sun subtends at the earth an angle of 32 minutes of arc, and a surface of a square centimeter in area would subtend about the same angle at a distance of 1 meter. On the assumptions made we obtain the result that the sun will illuminate a surface eight times as brightly as an incandescent carbon surface 1 square centimeter in area at a temperature of 3,600° (that of the electric arc) at a distance of 1 meter, or, cæteris paribus, just as brightly as a surface of 8 square centimeters at the same distance.

The results obtained by comparing sources of light in which the same substance, for instance carbon, is brought to incandescence are more reliable. To this class belong, besides all free burning flames, the incandescent electric light and the arc light. Since in all of these the incandescent substance approximates a black body, their relative temperatures can be determined by the color of the light emitted. The higher the temperature the whiter is the color of the light, since the short violet rays are added to the long waves only at higher temperatures. On the scale of whiteness, the arc light come first; it is similar in its composition and its appearance to sunlight. After this comes the acetylene light, with its dazzling whiteness. Then comes the reddish-colored lights, such as the gas flame, the incandescent electric light, the candle, and the petroleum lamp, and finally come the red lights, such as the hearth fire. The absolute temperature of the arc, as stated above, is about 3,600° C. That of the petroleum lamp is certainly below 2,000° and the melting point of platinum is, let us say, 1,800°. Then the absolute temperatures of the two are in the ratio of 3,600 to 1,800, or as 2 is to 1. For the same area the carbon of the arc light would emit about 16 times as much light as the petroleum lamp. And yet the price per candle power is almost as great for the arc light as for the petroleum lamp, as shown above. This indicates how large a proportion of the energy must be lost in the transformation of the heat equivalent of the fuel into energy of motion of the dynamo and the transformation of this energy into electrical energy and finally into light. It is therefore easily understood why the incandescent electric light occupies almost the last place in the table. In addition to the energy which is lost in the production of the electric current we must consider that the temperature of the filament of an incandescent lamp is much lower than that of the carbon in the arc. In the arc lamp the maximum temperature is practically determined by the volitilization temperature of the carbon. In the incandescent lamp the filament must be kept intact. This can only be done if it is heated to moderate whiteness in the absence of air—i. e., of oxygen. If a long life of the incandescent lamp is not to be considered, by increasing the temperature of the filament to intense whiteness the glow lamp emerges from its darkness and outshines all luminous flames. I now gradually increase the electric current passed through this incandescent lamp. [Experiment.] The filament becomes brighter and brighter, and you see what a flood of light is emitted by this single lamp. It, however, lasts only a short while, for the filament disintegrates. While the increase in temperature has quadrupled its brightness, the current passed through it only had to be doubled. I can not make it any plainer to you than by this experiment that an improvement in the incandescent light can only be attained if we succeed in increasing the temperature of the filament without at the same time disintegrating it or volitilizing it. But even now, with the low market price of incandescent lamps, less stress is to be laid on their life than on their luminosity, and it is more economical to use three lamps, each capable of burning three hundred hours and with very bright filaments, than to use one lamp outlasting all the others but with a reddish filament. The radiation of light is proportional to the fifth power of the absolute temperature, while the electric energy which is supplied to the lamp is proportional to the second power of the temperature.

The aim, on the whole, is then to raise the incandescent substances to the highest possible temperature even at the expense of reducing the size of the luminous area. In this sense the Argand burner with chimney corresponds to a great improvement over the fish-tail burner, just as does the petroleum lamp of to-day over the oil lamp of the ancients. In both cases an increase in temperature of the combining gases and therefore of the incandescent carbon particles is produced. And while the former is not important practically, the intensity increases as the fifth power of the temperature, so that the flame of an Argand burner is only about half as expensive as that of a fish-tail burner. Still cheaper is the gaslight obtained from a Siemen's regenerative burner, in which the gas before combustion is heated up by the heat otherwise carried off by the products of combustion, and by this means the temperature of the flame is still further increased.

Cheapest of all is, however, the Welsbach light, because the temperature of combustion is still higher than that of the regenerative burner. The mantle had to be constructed such that a large part of it would assume a high temperature (2,000° absolute) and had to be at the same time of such a composition as to radiate almost as freely as carbon. Both objects have been attained by Auer von Welsbach to whom, therefore, great credit is due.

In the acetylene light the incandescence is also due to highly heated carbon, as mentioned above, but, judging from the whiteness of the light produced, its temperature must far exceed that of the Argand burner. Although the price of acetylene light is dearer at present than gaslight, it is principally due to the excessive cost of the calcium carbide, from which the acetelyne is produced. If both acetylene gas and ordinary illuminating gas could be furnished at the same price the acetylene would be cheapest of all, the price being under these circumstances .005 cent per hour per candle power.

But we could reason similarly in regard to the cost of electric light and of petroleum light. The relative cost as given in the table depends on the assumptions made in the second and fourth columns.

The struggle for supremacy will never end, for much can still be attained if the directions pointed out by science are followed. There are, however, many difficulties to be overcome before we can expect to realize the ideal source of illumination theoretically possible.

EXPLORATIONS OF THE UPPER ATMOSPHERE.[1]

By HENRI DE GRAFFIGNY.

As meteorologists have become convinced of the many advantages that balloons present for the study of aerial phenomena and the exploration of the atmosphere, there has been formed an international scientific commission for aerostation, having for its president M. Bouquet de la Grye, and the study of the higher regions by means of unmounted recording balloons, commenced in France by M. Gustave Hermite and in Germany by M. Assmann, has been pursued with a success which leads us to hope for the most surprising results, when, by means of an uninterrupted series of experiments, we shall be able to deduce a complete theory from ascertained facts.

It appears to us that the time has come to summarize the recent work of physicists of different countries relating to this most interesting question, so as to bring out, from the indications furnished by the registering apparatus, the facts that result from these novel observations. These have an incontestable interest, for the benefits that science confers by the forecasting of the weather are of a general character and benefit the entire human race, which derives the elements of life from the aerial ocean. We will, then, recount the various experiments made up to the present time, both in France and abroad, taking them up in order of date, and afterwards discussing the results obtained by these new means of investigation.

I.—RECORDING BALLOONS.

Until recent years meteorological observations at great heights were made from mounted balloons by physicists who carried with them apparatus whose indications they themselves recorded from time to time. Among celebrated ascensions of this kind we note the following, calling attention to the fact that greater and greater altitudes have been successively attained.

	Meters.
Robertson and Lhoest in 1803	7,400
Gay-Lussac in 1804	7,016
Barral and Bixio in 1850	7,039
Welsh and Green in 1861	6,910
Glaisher and Coxwell in 1862	8,840
Sivel and Crocé Spinelli in 1874	7,300
Sivel, Crocé, and Tissandier in 1875	8,600
Jovis and Mallet in 1884	7,100
Berson at Berlin in 1892	9,000

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[1] Translated from the Revue Scientifique, fourth series, Vol. VII, pages 488–497.

The danger of these high altitudes is well known, especially since the dramatic ascent of the balloon Zenith; hence it seemed wholly desirable, in view of the progress that has been made in the construction of automatic registering apparatus, that unmounted aerostats carrying these improved instruments should be used. Thus there would be no risk to the lives of the observers, and the difficulties of ascents to great heights, requiring balloons of enormous capacity, would be much diminished.

We owe to M. Gustave Hermite the first experiments of this kind, these being first made by means of recording balloons formed of paper, then by an "Aerophile," made entirely of gold-beater's skin, having a capacity of about 113 cubic meters. The first ascent was from the aerostatic park of Vaugirard, March 21, 1892, when the height attained was 16,000 meters, but the tracing made was incomplete, the ink of the registering pen having frozen because of the excessive cold of the upper regions. Many other experiments were afterwards made during succeeding years, and in each of them some new improvement was effected.

In Germany, M. Assmann, chief of section in the Meteorological Institute at Berlin, under the direction of M. von Bezold, had a balloon of 250 cubic meters capacity built, which, released on two different occasions, reached heights of 16,600 and 18,000 meters, and fell the first time in Russia, the second time in Bosnia.

M. Besançon, the young and already celebrated aeronaut, having combined his efforts with those of M. Hermite, there resulted from the collaboration of these two investigators great improvements in all parts of the apparatus hitherto used. Considering that the essential condition for reaching higher and higher altitudes consisted in progressively increasing the ratio between the ascensional force and the volume of the balloon, the weight of every part of their new aerophile was reduced to a minimum. The tissue employed for the envelope was a marvel of lightness and strength. Its resistance to traction was 550 kilograms per linear meter, its weight 30 grams per square meter when unfinished, and 120 grams after it was made impermeable by four successive coats of varnish. The volume of the balloon being 380 cubic meters, its envelope, therefore, weighed 9 kilograms before varnishing, and 31 kilograms after the last coat of varnish had been applied, the total varnished surface being 250 square meters. Now, the weight of the same volume and surface made of cotton or silk, having a lifting power of 1,000 kilograms per meter, is about 110 kilograms. We perceive at once what progress was made in the construction of this balloon without in any way sacrificing its strength.

This balloon was provided at its upper extremity with a valve of discharge, having a diameter of 48 centimeters, acting automatically to produce descent, and effecting collapse in a few moments. It was covered by a net of white Anjou hemp, of exceptional strength in spite of its light weight of 5 kilograms. Its total resistance, which was 5,500 kilograms at the top, was 8,300 kilograms at the equator, and thence diminished to the 8 suspension cords, which had a total resistance of 4,500 kilograms. These cords were united below in a single point by means of a special apparatus called a cone. It is at this point of union that are fixed, first, a thimble in which slides the cable serving to manage the balloon; then two cords, one light, at the end of which the registering apparatus is suspended, the other strong, to which is attached a tray charged with ballast when the balloon has mounted to the desired height by the running out of the controlling cable. When it is wished to let the aerostat go, one end of the cable is pulled; then, when it has fallen to the ground, it is sufficient to cut with a knife the cordage that holds it. By this means there is avoided all shock to the instruments, which, in the first experiments, were considerably injured.

The complete apparatus weighs only 41 kilograms, the same as does the German balloon of 250 cubic meters, the Cirrus, mentioned above. A simple calculation shows that this balloon, filled with pure hydrogen having a weight of 1,150 grams per cubic meter, ought to rise to a height of more than 20 kilometers. Still it has not reached anything like that height, but this fact is explained when we learn that it has only been filled with illuminating gas, having a lifting power of only 700 grams per meter, and that it has always been loaded with from 10 to 25 kilograms of registering apparatus. Besides, we may doubt whether the formula of Laplace, by which barometers are graduated, can still be applied to those regions of our atmosphere in which the pressure is reduced to a few centimeters of mercury.

II.—THE INSTRUMENTS.

The experience had by MM. Hermite and Besançon at the time of their previous trials with their Aerophile of gold-beater's skin led them to modify considerably the arrangement of the instruments carried by the recording balloon, so as to make the diagrams more accurate.

The principal record desired was that of the temperature of the regions traversed, and for this purpose a registering apparatus was constructed by M. Richard and tested in the frigorific chamber of M. de la Baume-Pluvinel. This instrument, made almost entirely of aluminum, weighs only about 1,200 grams, and gives three different diagrams; that of variations of pressure (barometer), of temperature (thermometer), and of degree of humidity (hygrometer). The tracing is made upon paper ribbons covered with lampblack, fixed on a revolving cylinder driven by clockwork. This barothermograph, when set up, is suspended in a loosely woven wicker basket by rubber bands, thus guarding the instrument from injury during the shock of landing. This basket is itself hung in the same manner within a large wicker cylinder covered externally with silvered paper. In this way the barothermograph is completely protected from solar radiation as well as from shocks.

Besides obtaining the temperature of atmospheric spaces, it was considered desirable to bring down in a special receptacle a certain quantity of air taken in at the highest point of the ascension, 12,000 to 15,000 meters. As early as the beginning of 1896, M. Hermite tried to solve this difficult problem, and the Aerophile that was released on the 22d of March from the gas works of La Villette and that landed near Cambrai, after having reached a height of 14,000 meters, and having been subjected to a cold of —63° C., carried an automatic apparatus for taking in air, which, unfortunately (probably because of the low temperature), gave no result. The reservoir, in which a vacuum had been made previous to the ascent, was to have been opened at the highest point by means of the action of sulphuric acid on chlorate of potash. The reservoir once filled, a continuation of the same action was to hermetically seal it by melting and closing the tube for ingress of air, and thus the air of the higher regions might be brought down and analyzed.

In succeeding experiments, following the advice of M. Cailletet, M. Hermite changed the arrangement of his apparatus, and successively used a cock moved by clockwork adjusted before the ascent, a mixture of paraffin and glycerin, and finally closure by means of a ball valve of a quite novel character. After many disappointments and difficulties successively overcome by dint of ingenuity and perseverance, the physicist attained his end, his apparatus worked admirable, and we have now ascertained the nature of the air in the higher regions, as will be seen later on.

Another problem, likewise very important, is the determination of the actual altitude to which the recording balloon rises with the attached instruments, for it is evident that the basis of any serious study of the higher atmosphere must rest upon a knowledge of the law of the decrease of barometric pressure with increase of altitude, a law which, when scrutinized closely, is an evident corollary to the law of universal gravitation.

As a micrometer telescope had not proved successful, it became necessary to devise another method, and M. Hermite was led to contrive another apparatus, based upon the principle already employed in geodesy for measuring the height of clouds, and which consists in observing a moving point by means of two theodolites set on a previously determined base. But in practice this proceeding has a serious inconvenience, for the rapid displacement of the object prevents the observer who watches the balloon at the telescope of the theodolite from noting the azimuth and zenith distances and the time. An aid is necessary for each of these, and it will be easily seen that if the operators are not very well drilled they will embarrass each other and the observations will fail.

Hermite's dromograph is a sort of registering theodolite, with which one may obtain a continuous automatic registration of the position of the balloon as long as it remains visible by observing it behind the crossed threads of the instrument. Movements of the telescope in the vertical and in the horizontal planes are transmitted, by means of cogwheels and a chain invented by Galle, to an arm carrying a writing pen. The tracing made by this pen on a sheet of cross-section paper borne on a vertical cylinder gives the various angles of the balloon in zenith and azimuth, and constitutes a complete diagram. In the model used in the ascension of the 5th of August, 1896, the registering cylinder was 18 centimeters high and 7 in diameter, revolving in fifty-five minutes. Using cross-section paper with millimeter squares, we have, for every minute of time, 4 millimeters of horizontal displacement of the cylinder. For zenith distances from 0° to 90°, 1 millimeter equals one-half a degree, and for azimuths, 2°. To obviate confounding the tracings, the pens carry ink of different colors.

This apparatus can not pretend to the extreme precision of the theodolites employed in geodesy, but it has the compensating advantage that it furnishes a document that can be examined at leisure. With two dromographs established on a suitable base, the actual height of the aerostat may be determined with a certain amount of approximation, and this may then be compared with the registering diagrams of the balloon.

While waiting, M. Hermite has made interesting observations upon the velocity and direction of winds at high altitudes, using the barometric diagram to solve the triangle.

Now that we have become acquainted with the apparatus employed, we may consider the three last trials that have been made in France with the instruments just described.

III.—THE ASCENSION OF AUGUST 5, 1896.

At 11.40 a. m. the Aerophile, provided with its basket protector containing the barothermograph carrying within the balloon a registering thermometer and provided with a 12 pound apparatus for automatically taking in air, was ready to start. The signal being given and the mooring cord cut, the balloon darted up with great velocity, carrying its load of instruments. The dromograph properly set up registered the trajectory followed; but at 12.10 o'clock at a height of about 8,500 meters and at a distance of 22 kilometers from the gas works of La Villette, the place of departure, the Aerophile disappeared behind thick clouds while still continuing its ascent. That very evening a dispatch arrived at Paris announcing that the recording balloon had come to the ground in Germany, at Niedermiebach, about 30 kilometers beyond Cologne, where M. Hermite went the next day to get it, finding the apparatus in a perfect state of preservation.

An examination of the barogram showed that the ascent was made with an average velocity of 6 meters per second, and that the maximum altitude was reached at 12.33 p. m., about three-quarters of an hour after the start. The barometric pressure had then fallen to about 135 millimeters of mercury, which corresponds to a height of 13,843 meters, according to the formula of Laplace. Then, after a period of equilibrium lasting three hours, the descent commenced, slow at first, then more and more rapid, until it attained a velocity of 3 meters per second at landing, 4.30 p. m.

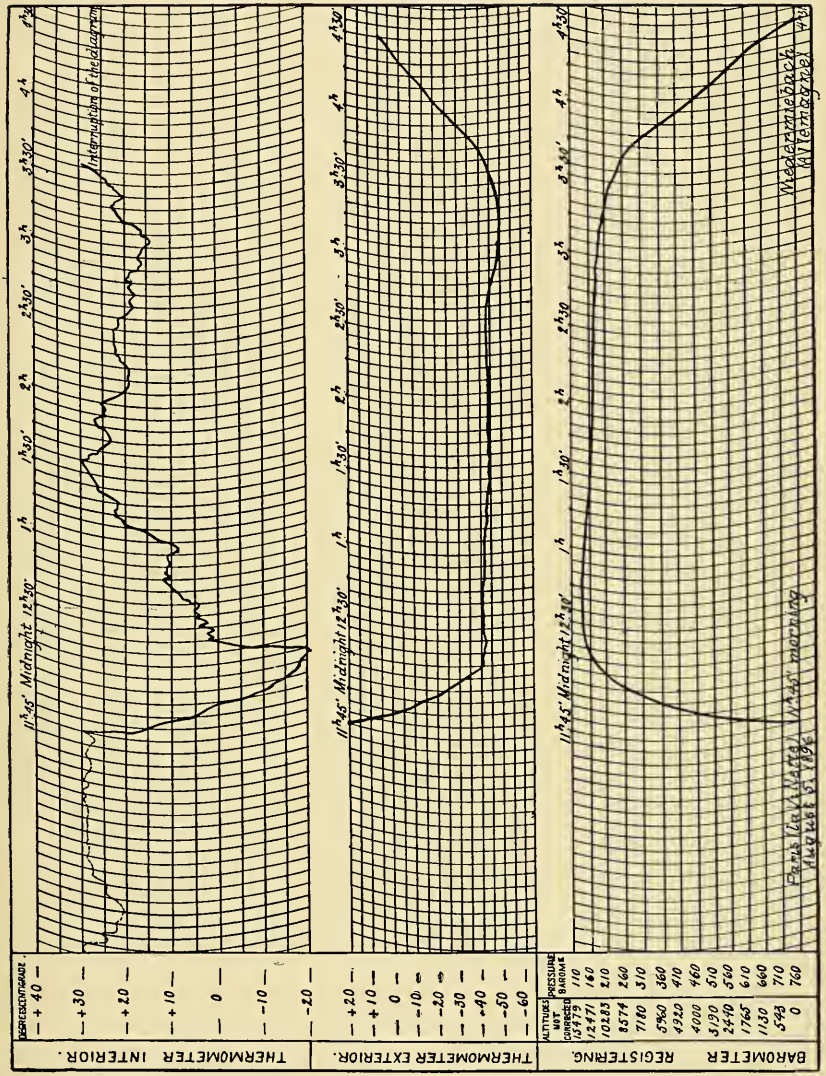
The thermometric diagram showed a regular decrease of temperature, which reached –50° C. at the moment when the descent commenced. But this record, which does not agree with those obtained in preceding experiments, appears somewhat uncertain, and M. Hermite thinks that it may have been falsified by various more or less hypothetical causes—for example, the insufficient ventilation of the apparatus.

A registering thermometer had been inclosed, as we have said, in the interior of the balloon. An examination of the tracing obtained showed that the temperature of the gas was about 30° C. during the inflation, while the temperature outside was only 18°. But as soon as the ascent commenced the carbureted hydrogen naturally expanded with great rapidity, and the temperature of the gas fell to —21°, a result to-day well understood. The balloon forms a gigantic actinometer, which shows the effects of solar radiation, and the intensity of that radiation increases with the altitude, and is in inverse proportion to the density of the air.

The apparatus for taking in air appeared to have worked according to the conditions that M. Cailletet had foreseen, and M. Hermite brought it back intact, its orifices hermetically closed, to Paris, where M. Muntz, director of the Laboratory of Agricultural Chemistry, was to analyze it. But as the vacations were about to commence he could not take up this analysis until some months later—in October, and it was then found that, during this long interval, the air in the receiver had resumed its normal pressure. There had therefore been a leakage, and the experiment would have to be repeated.

It has been found possible to reconstruct the trajectory of the aerostat from the time of its departure by using the diagrams furnished by the dromograph and the barometric curve. We thus have on the one hand the azimuth of the balloon at every moment, and on the other the angle α, whose value represents the height of the balloon. The sine of the angle α is furnished by the barometric curve, using the formula of Laplace; a quite simple trigonometric operation then suffices to resolve the right-angled triangle. In this way also it has been possible to determine the velocity of the currents which transported the Aerophile at different altitudes. This velocity was as follows:

38 kilometers per hour between 3,000 and 5,000 meters.
70 kilometers per hour between 5,000 and 6,200 "
80 kilometers per hour between 6,200 and 7,000 "
102 kilometers per hour between 7,000 and 7,700 "
132 kilometers per hour between 7,700 and 8,200 "
158 kilometers per hour between 8,200 and 9,700 "



Diagrams of Balloon Ascensions of August 5, 1896, at Paris, France.

This appears to indicate that the velocity of the wind increases with the altitude. The distance traveled by the balloon was 430 kilometers in four hours and forty-five minutes, which gives an average velocity of 90 kilometers an hour.

IV.—INTERNATIONAL ASCENTS OF THE 14TH NOVEMBER, 1896.

The International Conference of Meteorology, held in the month of September, 1896, under the presidency of M. Mascart, having manifested a desire to see the experiment tried of simultaneous ascents of several balloons provided with registering apparatus, an agreement was entered into by MM. Hermite and Besançon and various foreign observers—M. Hergesell, president of the international committee of scientific aerostation; M. Assmann, chief of division in the Meteorologic Institute of Berlin; General Rykatcheff, at St. Petersburg, and M. Erk, at Munich. It was decided that simultaneous ascensions should be made on the night of the 13th–14th November, from the gas works of La Villette, at Paris, from Strasburg, from Berlin, from Munich, and from St. Petersburg. Many mounted balloons were also freighted to start at about the same hour from different cities, and thus contribute to the general result.

The ascent from Paris took place, during magnificent weather, at 2.06 a. m. The air was calm, but saturated with moisture. The barometric pressure was 761 millimeters and the temperature 3° C. The ascensional force of the Aerophile was 248 kilograms, which gives the gas a lifting force of about 800 grams per cubic meter.[1] The total weight of the material taken up was 45.5 kilograms, the only instruments carried up being the barothermograph, which we have already described.

The balloon disappeared in the night with extraordinary rapidity and for three days we heard nothing further from it. It was thought that it might have been lost at sea or in some distant forest, when a letter came to Paris, informing MM. Hermite and Besançon that their apparatus had fallen at break of day at Graide, in Belgian Luxemburg, and, by a singular coincidence, less than a kilometer away from where we

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[1] The density of illuminating gas is very variable, and at the works of La Villette its lifting force per cubic meter oscillates between 620 and 820 grams. It would therefore be desirable to inflate the Aerophile with pure hydrogen, as the Germans do with the Cirrus. But pure hydrogen is not found in France, and MM. Hermite and Besançon, who had asked the minister of war for permission to purchase the quantity of gas necessary for inflation from the persons who furnish hydrogen to the army, received no reply. We then offered to assist the two persevering physicists in their attempt to avoid the difficulty and to purify—to decarburise, in brief—the nauseous compound furnished to aeronauts by the Parisian gas company. The apparatus is now completed, and if, as there is reason to hope, the experiment demonstrates the justice of our theory, the Aerophile will be inflated with almost pure hydrogen, purified by passing through a decarburator before being stored in the sphere of silk, and the ascensional force will be found to be increased by about one-fourth.

ourselves, in company with M. Besançon and four other aeronauts, nad landed with the balloon Touring Club just three months before.

The material of the Aerophile was unhappily in a very deplorable state. As the peasants who possessed themselves of it at the time of its fall did not know how to read they did not think of opening the envelope containing the instructions given by the aeronauts to the persons who might find the balloon, which, in order to detach from the trees upon which it had fallen, they tore in several places. The net was torn to pieces and completely useless. As to the sheets covered with lampblack wound about the registering cylinder, the peasants, attaching no sort of importance to them, had detached them and thrown them aside in a corner, where M. Hermite fortunately found them.

The diagram which thus escaped those whom we can scarcely call the rescuers of the balloon shows that the aerostat reached equilibrium at a pressure of 115 millimeters of mercury, which corresponds to a height of 15,000 meters according to the formula of Laplace, no correction being made for the temperature of the air. The vertical velocity at starting was about 8 meters per second, and the highest point was reached in forty minutes. The velocity of descent was 6 meters per second when near the ground. It was then twice as rapid as usual, a fact which may be attributed to an accumulation of moisture upon the envelope of the aerostat.

The thermometer registered a minimum temperature of 60° C. at about 6 a. m., the barometric pressure being then 160 millimeters of mercury.

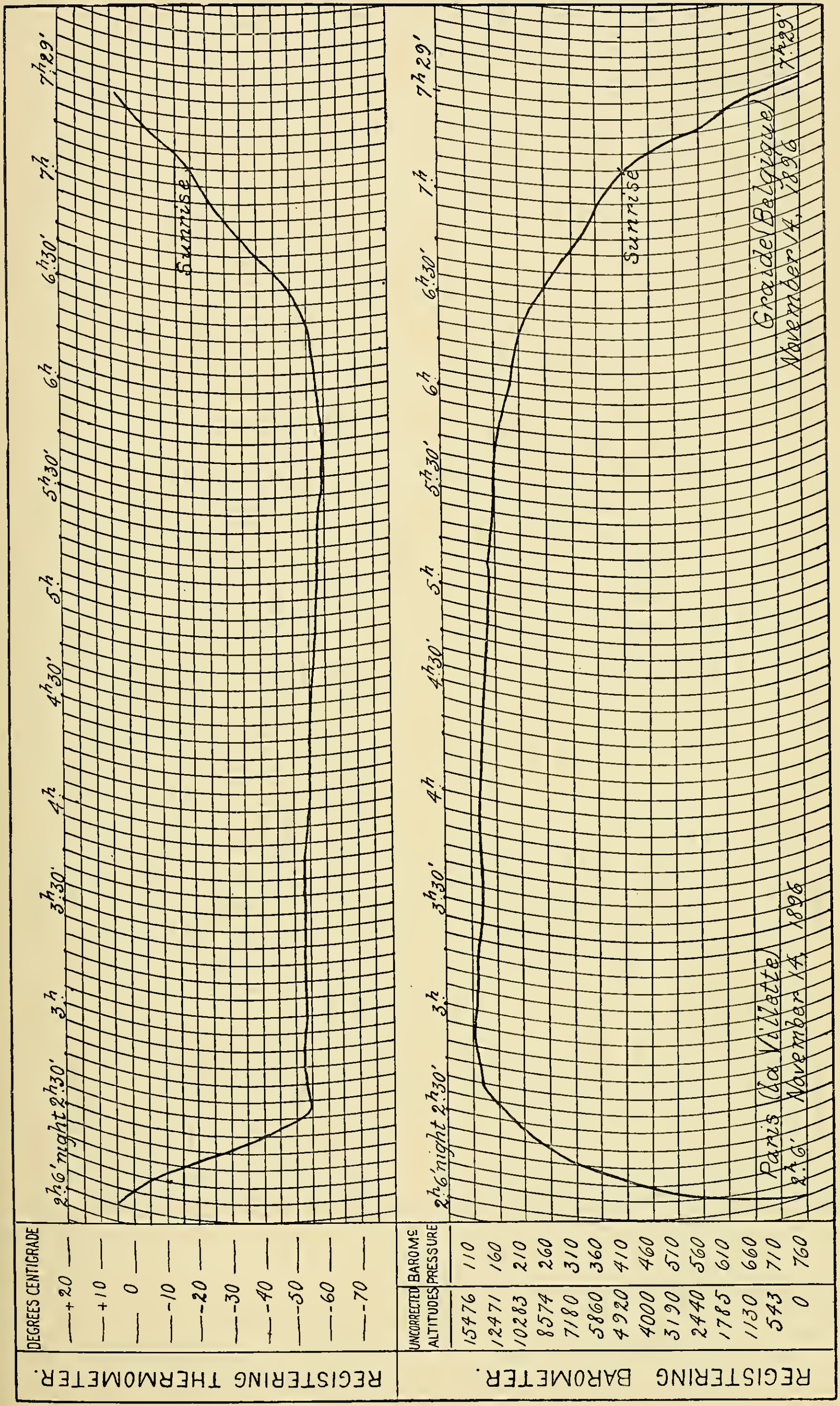
This observation agrees completely with those that have been made not only in a balloon but on the ground, for it has been shown that under almost all circumstances a marked lowering of temperature takes place a little before sunrise. Still, before relying absolutely upon this, it would be well to assure ourselves that the registering apparatus was sufficiently ventilated; for, in spite of the absence of every cause due to heating during this nocturnal ascension, it is possible that aeration plays a very important part in the precision of thermometric observations. In order to remove these uncertainties there should be, in subsequent experiments, comparative observations made with several similar registering thermometers under various conditions of aeration.

As compared with the preceding ascent, the motion of translation of the Aerophile was quite slow. The distance from Paris to Graide being about 220 kilometers as the crow flies, and being traversed in four hours and twenty-five minutes, the balloon traveled only 36 kilometers an hour.

V.—FOREIGN ASCENTS ON THE 14TH OF NOVEMBER.

As we have already said, three recording balloons and four mounted balloons were released at different points on the night of the 14th of November. The results of these experiments were as follows:

The recording balloon of Strasburg, having a capacity of 350 cubic



DIAGRAMS OF BALLOON ASCENSIONS OF NOVEMBER 14, 1896, AT PARIS, FRANCE.

meters, was filled with illuminating gas having an ascensional power of 800 grams per cubic meter, and was released at 2 a.m. An hour and a half later the apparatus fell in the Black Forest, at the foot of Mount Hornsgrinde, 30 kilometers northeast of Strasburg. The maximum height attained was 800 meters, and the barometric diagram shows a perfect regularity, while that of the thermometer, undoubtedly falsified by some derangement of the mechanism, shows a rise of temperature not corresponding to any natural phenomenon. M. Hergesell, struck with this irregularity, has since that time thought proper to have M. Richard modify the construction of the thermometric reservoir, in order to prevent the recurrence of a similar accident.

The recording balloon Cirrus, of Berlin, received only 150 cubic meters of pure hydrogen gas, its capacity being 250 meters. It carried a barothermograph and a Richard barograph, as well as a thermograph of German construction. It was released at 2.51 a. m., local time, and its descent occurred a short time after in the Grünenwald, a forest situated a few kilometers to the southeast of Berlin. The maximum altitude attained was only 5,800 meters, the balloon having exploded in the air, a phenomenon that was by no means surprising when we consider the vacuum that existed in the aerostat at the time of departure. M. Assmann adds, also, that the Cirrus was old and worn by reason of prolonged service.

The recording balloon released at St. Petersburg by M. Rykatcheff had a similar unfortunate fate, for it burst at an elevation of 1,500 meters. It was released at 4 a. m., local time, and the voyage lasted a quarter of an hour. Temperature at starting, –5°; at the culminating point of its ascent, –13°.

The mounted balloon Buzzard, having a capacity of 1,300 cubic meters, was released from the aerostatic park of Schöneberg, at Berlin, at 2.48 a. m. It had received 1,000 cubic meters of pure hydrogen, and was accompanied by Lieutenant Von Keller, an officer of the balloon service, and by M. Berson, a well-known meteorologist and aeronaut. The voyage was a very remarkable one; it lasted eleven hours and thirty-seven seconds, and ended on the seashore at Volkhagen, near Ribnitz, in Mecklenburg. The average velocity was 5 meters per second, the maximum height 5,600 meters, which was attained at 11.43 a. m. The lowest temperature observed at this height was –24.4°, by means of an aspiration apparatus invented by M. Assmann.

At St. Petersburg the balloon Vannesky, having a capacity of 1,000 cubic meters, started at 4.45 a. m., accompanied by MM. Kowenko and Semkowski. The lowest temperature observed was –22.8° C., at the height of 3,500 meters, and –23° at 4,560 meters for the mercurial thermometer. With the alcohol thermometer the lowest was –27.2° at 3,840 meters. The descent was made at 11.30 a. m. near Pskow. The first rays of the sun reached the aerostat at 7.48, and somewhat disturbed the course of the meteorological observations.

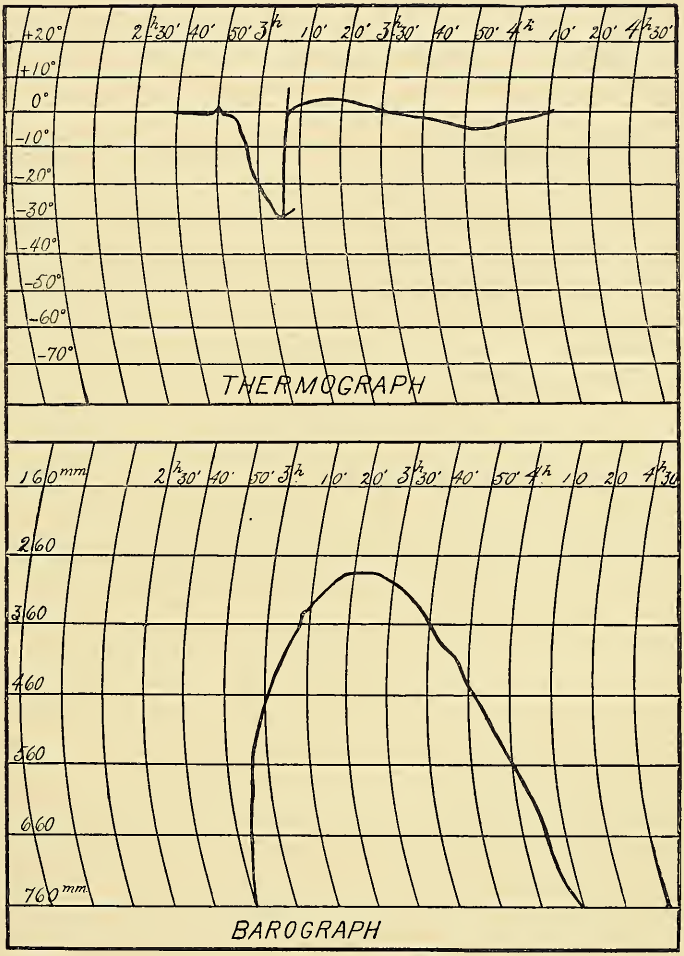
At Warsaw the balloon Strella, having a cubic capacity of 1,000 meters, filled with ordinary illuminating gas, and directed by Lieutenant Prince Obolensky, with Lieutenant Oülianin as observer, ascended at 3.15 a. m. and descended near Brzozow, in Austria, at 12.50. It was up, therefore, for nine hours and thirty-five minutes. Its maximum altitude was 3,500 meters, but the temperature could not be observed beyond 2,000 meters, where 20° C. was shown. Beyond this the observations were interrupted, the mercury being wholly within the bulb of the thermometer.

At Munich, the balloon Academie, having a cubic capacity of 1,300 meters, and filled with pure hydrogen, ascended at 6.45 a. m., central European time, having on board M. Erk, director of the Meteorological Observatory at Munich, and Captain Guttenberg. The lowest temperature observed was —6.5° C., with a barometrical pressure of 505 millimeters, taken at 12.02. The wet bulb thermometer gave —7.9°. This expedition was certainly one of the most interesting of the day. As the sun drew near to the meridian the balloon rose progressively above a sea of clouds, between which glimpses were obtained of the immense panorama of the Alps. The temperature, which had regularly descended up to that time, rose again when above the clouds, and attained a maximum of +2.7° at 1,600 meters, to redescend to —6.5° at the culminating point of the ascension, at 3,350 meters. A landing was effected without any notable incident after traversing about 200 kilometers, at 1.45, at Longvitz. In none of the ascents were any shooting stars perceived.

It would at this time be premature to attempt to draw any conclusions from these simultaneous observations, although it may be affirmed in a general way that up to an altitude of from 8,000 to 10,000 meters the local influences and the configurations of the ground cause great differences in the temperature of the air, observed at the same time and altitude. We must wait until successive observations made upon the higher atmosphere shall have changed in some degree the theories believed to be the best settled; but it should not be forgotten that observations made in meteorological stations on mountains have prepared us for conclusions of this kind. There, as is well known, the differences in the temperature of the air taken with the metallic thermometer in the shade and in the sun vary with the altitude. Much is yet to be studied before we can settle in a definite and certain manner these various questions.

VI.—ASCENTS SIMULTANEOUSLY MADE ON THE 18TH OF FEBRUARY, 1897.

In accordance with the advice given by the Scientific Commission at Paris, at its session of February 3, a new exploration of the distant strata of the atmosphere was decided upon, in concert with the International Committee of Aeronautics and Meteorology. The Aerophile, which returned in a pitiable state after its journey to Belgium, had been



DIAGRAMS OF BALLOON ASCENSIONS OF NOVEMBER 14, 1896, AT STRASBURG, GERMANY.

carefully repaired in the workshops of the central establishment for aerostation, its net being entirely renewed, and M. Besançon had added to it an equatorial band which increased its volume to 480 cubic meters. The ascension was fixed for February 18 at 10 a. m.

Besides the barothermograph of Richard, which was hung freely within the protecting basket, the Aerophile carried a thermograph suspended in the interior of the balloon and an apparatus for taking in air which had been contrived with the greatest care by M. Cailletet. This exploration, the seventh one made by the French in the higher regions of the atmosphere, had for its sponsor on this occasion Prince Roland Bonaparte, who had assumed the entire cost of the enterprise.

At 10.15 a. m., during cold and foggy weather, the cord attaching the aerostat to the ground was cut, and in less than a minute it had disappeared in a dense fog. There was no delay this time in getting news from it, for about the middle of the afternoon a dispatch arrived at Paris informing MM. Hermite and Besançon that their balloon had descended near Chaulnes, at Meharicourt, in the department of the Somme, 105 kilometers from Paris, at 12.30. But this time, also, the material had been considerably damaged, having dragged nearly 5 kilometers over ground wet with rain. The silk was torn in several places and the net frayed into shreds. Happily the diagrams were intact.

The scientific results of this experiment were as follows:

The maximum height attained by the Aerophile was 15,500 meters, as shown by the barogram. This height was reached at 11.30, about an hour after the ascent from the gas works of La Villette. The descent was effected very quickly, as was also the case at the nocturnal ascent of the 14th of November, and it is possible that this effect was produced by a deposit, in the region of the cirri, of fine particles of ice, which, accumulating on the dome of the aerostat, overcharged it and caused its rapid fall.

An examination of the curve traced by the style of the external thermograph shows that the lowest range of temperature −66° C., corresponds not to the highest point of the ascent, but to a little before the final descent; that is to say, to a pressure of 109 millimeters of mercury. The curve of the registering thermometer hung within the balloon is no less curious. It shows a rapid fall, as low as −22°, due to the simultaneous action of the expansion of the gas and the cooling of the exterior. Then the temperature ascended during the descent of the balloon, attaining +14° at the level of the ground. During the entire voyage, therefore, the temperature of the gas was considerably above that of the surrounding medium, and when the temperature of the outside was at the minimum there was a difference of 46° C. between the air and the interior of the Aerophile. This phenomenon was already known, as we especially remarked it in our experiment of August 12, 1896.

The most important result of this experiment was the entire success in obtaining, by means of the automatic apparatus devised by M. Hermite, in conjunction with M. Cailletet, member of the institute, the air of unknown regions where respiration can not be supported; an experiment that, as we have above stated, had hitherto failed. The arrangement of this apparatus was as follows:

The air reservoir, cylindrical in form, is of red copper, tinned within so as to close all fissures and blowholes that might exist in the metal. The tinning has no effect upon the gas imprisoned in the upper atmosphere, and has the advantage of assuring the complete impermeability of the reservoir. Glass would not have answered because of its fragility and the difficulty of attaching the necessary tubing.

This receiver, holding about 6 liters, communicates with a tight cock inclosed in a special box formed by a coil of red copper; an ingenious arrangement that obviates, by its elasticity, the rupture of the joints from violent shock.

The box that contains the cock, the essential and delicate organ of this apparatus, is lined inside with felt 2 centimeters thick, surrounding another receptacle of tin filled with acetate of sodium, and within this is the cock that opens and closes the pipe allowing ingress of air. The outer envelope of this cock turns very slowly upon itself, driven by powerful clockwork also set in the receptacle containing the acetate. A needle attached to this cock turns about a graduated dial, and allows one to set the time for opening the cock, which, having a hole 1 millimeter in diameter, remains open for four minutes only. A tube that extends for 2 meters below the apparatus serves to admit the external air to the receiver, in which, it is of course understood, a vacuum has been formed by means of an air pump. Before sending it off the entire apparatus was inclosed in a loose wicker case filled with straw, to protect it against the shock of descent. Its total weight was about 11.5 kilograms.

It was only with very great difficulty, after repeated attempts and patient studies, that MM. Cailletet and Hermite attained this result of an absolutely tight cock maintaining a perfect vacuum.

This cock was constructed by MM. Ducretet & Co. The conduit for ingress of air passing to the reservoir is a canal, hollowed obliquely out of the axis of the movable piece of the cock, so as to end at two points situated at different heights, corresponding to the intake of the air and to its discharge into the reservoir. Thanks to this arrangement, the leakage that generally occurs through the circular and almost invisible grooves that result from the wear of metallic pieces upon each other is avoided. In the arrangement adopted these grooves, so difficult to prevent, no longer correspond to the orifices of ingress and egress, and the apparatus may retain its vacuum indefinitely.

The fixed bronze piece in which the key of the cock moves has the form of a sewing thimble, as in the apparatus of M. Carré intended for producing cold.

Still, perfect tightness has only been obtained by a perfect adaptation of the movable sleeve and a quite close fitting of the cock which at first stopped the effect of the train of clockwork. It was necessary to add to the square piece terminating the sleeve of the cock a toothed sector upon which engages the pinion of a barrel containing a spring wound up until it effected the turning of the cock. The apparatus being found to work well in the labaratory could then be used for experimenting in inaccessible regions.

M. Muntz, member of the institute, who undertook the analysis of this air, obtained the following results:

	Volumes.
Oxygen	20.79
Nitrogen	78.27
Argon	0.94

The volume of the air obtained at 15,500 meters, at the temperature of zero C, and at a pressure of 760 millimeters was 1.18581 liters.

Composition of the air.—The amount of carbonic acid in 100 volumes of air is 0.033 volume.

In each 100 volumes of air deprived of carbonic acid the relation of the argon to the sum of the nitrogen and argon is 0.01185.

M. Th. Schloesing, jr., has determined the volume of the argon, by the very exact method which he has invented, and has likewise supervised the analysis that determined the amount of oxygen.

M. Cailletet, nevertheless, thinks that it will be necessary in the future to gather anew quantities of air and submit them to comparative analyses which will allow us to determine with certainty the composition of the air that fills these elevated regions, where scientific instruments had never before penetrated.

M. Muntz adds, in his communication to the Academy of Sciences, that he has shown that the receiving apparatus worked in an irreproachable manner and that the cock would maintain the vacuum indefinitely. There is no fear that other air will be introduced into the reservoir.

The quantity of air contained in the reservoir may then be used to calculate the barometric pressure of the altitude at which the air was taken. The results of the analysis show already, what was indeed to be expected, that at the altitude attained the composition of the air does not differ much from that of the lower regions.

But these figures must be accepted with some reserve. It will, in fact, be necessary to still further improve the method of obtaining the air so as to avoid all possible alteration in its composition. For oiling the cock it will be necessary to use a mineral oil incapable of absorbing a trace of oxygen and consequently emitting a trace of carbonic acid, at least under the conditions under which the experiment is made. It will likewise be necessary to use a reservoir whose walls can not absorb any trace of oxygen. For this purpose a glass receiver would be an ideal one, but a reservoir of gilded copper would seem to answer the purpose equally well. In the present case we may suppose that the small quantity of carbonic acid found in excess of that in normal air (0.033 in place of 0.029) is due to the oxidation of the oil employed, which may have produced the tenth of a milligram of excess which corresponds to the volume of air employed.

In the same way the smaller quantity of oxygen found than that of normal air (20.79 in place of 20.96), and which represents for the volume of air collected 3 milligrams, may come from the absorption of that gas by the oil of the cock or indeed by the metallic surface of tinned copper.

By eliminating the possible causes of error in new ascensions, we may hope to show with certainty whether or not real differences exist in the air taken at various altitudes; for the methods of gas analysis are to-day so perfect, thanks to the efforts of M. Th. Schloesing, jr., that they show extremely small differences; if, in fact such differences exist.

But as it is to be expected that in the regions of the atmosphere possible to explore by means of recording balloons the air is still subject to that intimate mixing that renders the air of lower regions practically uniform, we ought to expect to find the differences in its composition very slight, and only to be established with certainty by taking the most minute precautions.

The results of this fine experiment were the subject of a note presented to the Academy of Sciences and read at its session of the 8th of March by MM. Hermite and Besançon at the same time that there were presented communications on the subject by MM. Cailletet and Muntz, as we have said above. These persevering investigators were warmly felicitated by the whole assembly, and upon the motion of M. Berthelot a grant of 1,200 francs was made to MM. Hermite and Besançon, in order that they might pursue their interesting researches, which will reveal still other unknown facts in meteorology and the physics of the globe.

VII.—FOREIGN ASCENTS ON THE 18TH OF APRIL, 1897.

At the same time that the Aerophile was ascending from the works of La Villette several ascensions were going on in different cities, and we will pass them rapidly in review, so as to understand the results obtained by foreign observers.

The ascensions at Berlin were undertaken by the military aerostatic establishment at Schöneberg, in presence of the Emperor and the Empress of Germany, as well as the ambassadors of France and Russia. Like those of the 14th of November, they will be the subject of a circumstantial report, which M. Assmann has drawn up for the Emperor.

The recording balloon, Cirrus II, having exploded in the preceding experiment of November 14, 1896, because the net was not sufficiently strong, it was not thought prudent to completely inflate the aerostat. The envelope could not unfold with a speed corresponding to the dilatation, and the aerostat was torn. The barograph and thermograph, which were attached to a protecting basket, were saved and placed during the ascent of the 18th of February on a normal military balloon having a capacity of 560 cubic meters, used for captive ascents in the army. The weight of this aerostat, with its attached instruments, was exactly 225 kilograms.

This aerostat was picked up in the little village of Sereen, 150 kilometers to the east of Berlin, 70 kilometers to the east of the city of Frankfort on Oder, and in the neighborhood of the little village of Meseritz, department of "Ost Stenberg." The ascension lasted from 11.04 to 3.20. The military balloon rose to a height corresponding to a pressure of 248 millimeters, finding a temperature of —42°, which gives an altitude, when corrected, of 8,850 meters above the level of the sea. The imperial Geodesic Institute had established three stations provided with theodolites. The geodesists took some 30 observations, which gave for the maximum altitude 8,800 meters, a coincidence truly remarkable, and which may be considered as a verification of Laplace's law of barometric heights.

There were also two mounted balloons released. The first, which had a capacity of 1,300 cubic meters and was inflated with illuminating gas, was called the Condor. It carried M. Juring, a physicist attached to the Meteorological Institute, and was directed by M. von Kehler, an officer of the balloon service. The Condor, starting at 9.40, rose to a height of 3,258 meters where the temperature was —7.5°. It descended at 4.30 p. m., 250 kilometers east of Berlin, at Schneidemuhl, in the duchy of Posen.

M. Berson, a member of the International Commission, made an ascent with a military balloon having the same weight and volume as that of the recording balloon. He descended at Naeckel, 295 kilometers N. 4° E. of Berlin, after having reached an altitude of 4,632 meters, where he encountered a temperature of —13.8° and air almost saturated with humidity. The voyage lasted seven hours and forty-seven minutes.

The barometric and thermometric curves of the recording balloon could be read very well, although the diagrams were covered with a great number of spots produced at the time of the landing and in returning to Berlin.

During the month of March M. Assmann intends to make a private experiment, with illuminating gas and a discharge sac, as well as new instruments.

The thermometric tracings are found to be affected with a great number of little serrations that seem to have an almost periodic character, being reproduced every fifteen or sixteen minutes. M. Assmann attributes them to the fact that the protecting basket does not give sufficient protection, and supposes that they are caused by a rotation of the balloon.

At Strasburg.—The recording balloon Strasburg was released during a thick fog, and, like Aerophile III, was immediately lost to sight. It was not until after eight days of search that it was found attached to the branches of a tree in a forest near Rosenthal, a little village of Hesse, 400 kilometers to the north. The diagrams were uninjured, and according to what M. Hergesell writes, it appears that they will give information as to the results of insolation. The altitude attained was 12,000 meters, and the temperature observed was —67°. The diagrams will be published hereafter.

M. Hergesell took part in the ascent of a mounted balloon which ascended to the height of 2,500 meters. The descent took place near Frankfort. M. Hergesell found that the temperature rose until the height of 1,500 meters was reached. This effect was due to the action of the sun, which warmed the air until that stratum was reached where no vapor offered an obstacle to the action of its rays. After this the lowering of the temperature was very rapid.

At St. Petersburg.—The Government organized an ascent under the direction of M. Porker, an officer of the aerostatic service. The descent took place at Luban, in the government of Novgorod.

The conclusion of this examination of the work of meteorologists in the new line of researches in which the latter are engaged is thus reached by the summarizing of the results obtained up to the present time; and the high approbation given by the Academy of Sciences shows what our opinion ought to be regarding it. We should applaud these observers for resolutely leaving the rut of the beaten track and for seeking by new methods to complete the observations made in terrestrial stations scattered over the surface of the globe. There remains to be gathered an abundant harvest of new facts, and the explorers of the higher atmosphere may with reason hope that from their work, continued over a sufficient space of time, there will arise a new theory, generalizing all the phenomena that occur in the aerial ocean, and enabling us to correctly forecast the weather and thus profit all mankind.

THE EXPLORATION OF THE FREE AIR BY MEANS OF KITES AT BLUE HILL OBSERVATORY, MASSACHUSETTS.

By A. Lawrence Rotch, Director.

The first use of kites for scientific purposes, so far as is known, was in 1749 when Dr. Alexander Wilson and his pupil Thomas Melville, of Glasgow, raised thermometers attached to kites into the clouds.[1] Three years later Benjamin Franklin performed, in Philadelphia, his celebrated experiment of collecting the electricity of the thunderstorm by means of a kite.[2] During the first part of the present century kites were used in Europe to carry thermometers into the air, and about 1840 Espy, in this country, employed kites to verify calculations of the height of clouds by means of the dew-point.[3]

Modern kiteflying for scientific purposes may be said to date from 1883, when Douglas Archibald, in England, fastened to the kite wire anemometers which registered the total wind movements on dials, and so obtained differential measures of wind velocity at heights up to 1,200 feet.[4] In 1885 Alexander McAdie (now of the United States Weather Bureau) repeated Franklin's experiments on Blue Hill, using an electrometer,[5] and in 1891 and 1892 he measured, simultaneously, the electric potential at the base of Blue Hill, on the hill, and with kites as collectors, several hundred feet above the hilltop. About the same time L. Weber in Breslau, Germany, made a more extended use of kites to collect atmospheric electricity.[6]

To William A. Eddy, of Bayonne, N. J., is due the credit of again turning the attention of scientific men to kiteflying, and thus causing the widespread interest in it which now exists. About 1890 Mr. Eddy used an ordinary kite to lift thermometers, but soon afterwards devised a tailless kite which resembled the kite flown in Java. This kite has the ends of its horizontal cross stick bent backward, and the convex surface exposed to the wind obviates the necessity of a tail and

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[1] Trans. Roy. Soc. of Edin., Vol. X, Part II, pages 284–286.

[2] Sparks's Works of Franklin, Vol. V, page 295.

[3] Espy's Philosophy of Storms, page 75.

[4] Nature, Vol. XXXI.

[5] Proc. Am. Acad. Arts and Sci., Vol. XXI, pages 129–134.

[6] Electrotechnische Zeitschrift, November, 1886; August, 1889.

enables the kite to be easily flown. In 1891 Mr. Eddy attached a minimum thermometer to several of these tailless kites flown tandem, and proposed to obtain in this way data for forecasting the weather.[1] In the Proceedings of the Aeronautical Conference held in connection with the Chicago Exposition of 1893 Prof. M. W. Harrington, then Chief of the United States Weather Bureau, quoted Mr. Eddy's estimate of the cost of exploring the air to a height of 15,000 feet by means of kites flown in a series. Up to this time it does not appear that self-recording instruments—that is to say, those which make continuous graphic records—had been raised by kites. In the days of the early experimenters such instruments were too heavy to be lifted by more or less unmanageable kites. Within the past few years the self-recording instruments made in Paris by MM. Richard are both light and simple, so that it became possible to obtain simultaneous records at the kite and at a station on the ground from which to study the changes of temperature and humidity. This seems to have been done first at Blue Hill Observatory by my assistants, Messrs. Clayton and Fergusson. In August, 1894, Mr, Eddy brought his kites to Blue Hill and with them lifted a Richard thermograph, which had been partly reconstructed of aluminum by Mr. Fergusson so that it weighed but 2¼ pounds, to the height of 1,500 feet, where the earliest automatic record of temperature was obtained by a kite.[2] During the next summer Mr. Eddy assisted again in the experiments at Blue Hill and secured photographs from a camera carried between his kites. Now that the possibility of lifting self-recording meteorological instruments to considerable heights had been demonstrated, an investigation of the thermal and hygrometric conditions of the free air was undertaken by the staff of the Blue Hill Observatory, who had already made a detailed study of the movements of the air at great heights from the observations of clouds.[3]

The development of the kite and its accessory apparatus, and the acquisition of the knowledge how to use them, required much time and resulted in the damage or loss of many kites. Two costly meteorographs, as the combination of self-recording instruments is called, were dropped from a great height and no trace of them could be found. Usually, however, when by the breaking of the line kites and instruments were carried away, they fell gently to the ground and were recovered uninjured. It would be tedious to recount the vicissitudes of kiteflying at Blue Hill which have resulted in the present system of work, so only a brief historical statement will be given, to be followed by a description of the methods now in use. At first the Eddy, or Malay kite, as it is sometimes called, was employed. Several of these kites, which were covered first with paper and later with var-

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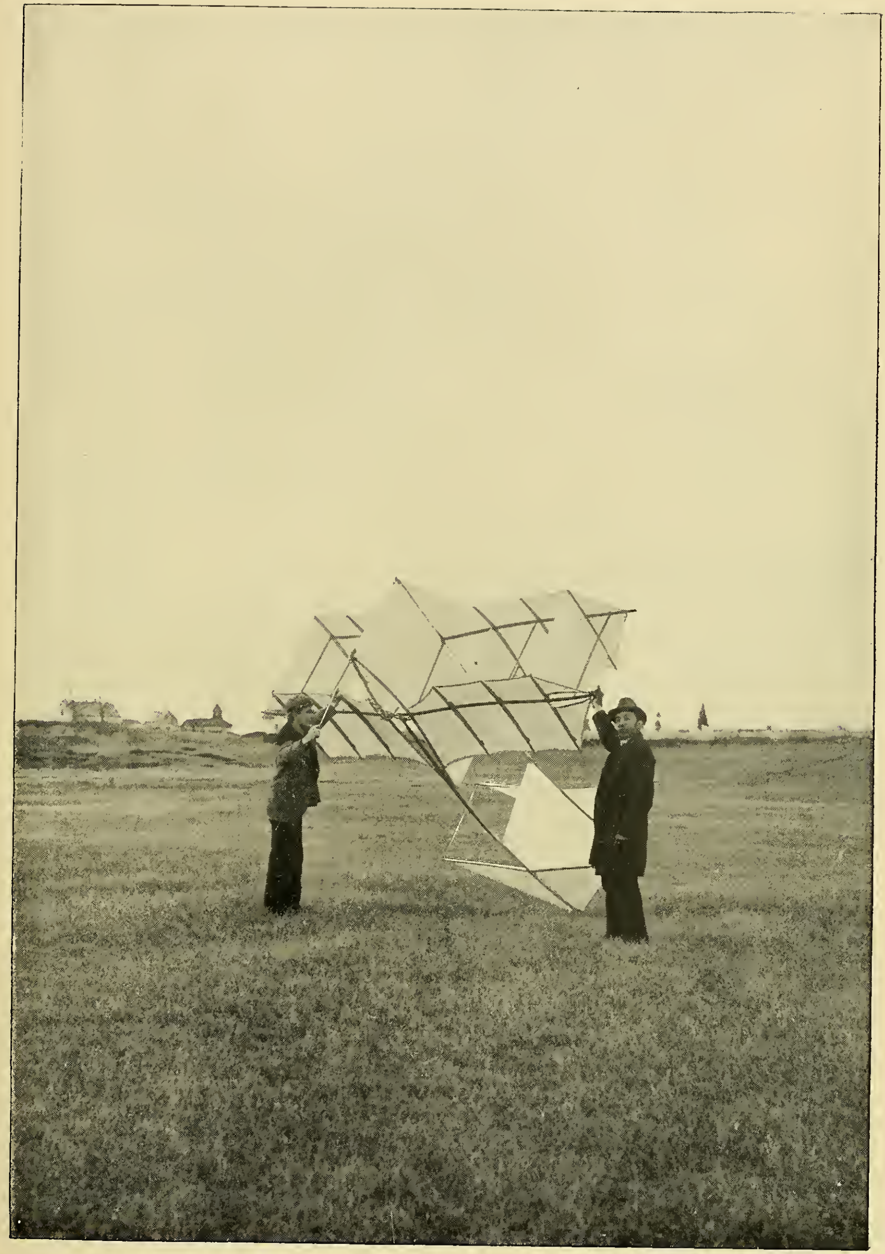
[1] Am. Met. Journal, Vol. VIII, pages 122–125.

[2] Scientific American, September 15, 1894.

[3] Annals Astr. Obs. Harv. Col., Vol. XXX, Part III.



LAMSON'S AERO-CURVE FOLDING KITE.

nished cloth, were coupled tandem to secure greater safety and more lifting power. The first meteorograph, being a combined recording thermometer and barometer, was constructed by Mr. Fergusson, who in November, 1895, united an anemometer with the thermometer. One of these meteorographs was hung to a ring at the point of attachment of the two kite lines to the main line, a method which still obtains. In August, 1895, in conjunction with the Eddy kites there was first used the cellular or box kite invented by Lawrence Hargrave, of Sydney, New South Wales, which was built from a description published shortly before.[1] At the present time some form of the Hargrave kite is generally employed at Blue Hill and elsewhere. Notwithstanding the efficiency of these kites, on account of the weight of the large cord necessary to control them and the surface which this cord presented to the wind, it was found impossible to lift the meteorograph to the height of 2,000 feet. During January, 1896, following Archibald's example and the methods of deep-sea sounding described by Captain Sigsbee, U. S. N., steel pianoforte wire was substituted for cord, and with this wire, which, although lighter and smaller, had greater strength than the cord, the height of a mile was reached in July and a mile and three-quarters in October, 1896.[2]

Up to this time a reel turned by two men sufficed to draw down the kites, but with the increasing pull and length of wire recourse to steam was necessary. In January, 1897, a grant of money was allotted from the Hodgkins fund of the Smithsonian Institution for the purpose of obtaining meteorological records at heights exceeding 10,000 feet. With this grant a steam windlass was built by Mr. Fergusson with ingenious devices for distributing and measuring the wire, but the cumulative pressure of the successive coils of wire finally crushed the drum. Our next apparatus was copied from the deep-sea sounding machine of Sir William Thomson (now Lord Kelvin), and with it records were brought down from the prescribed height in October, 1897. Two meteorographs were made by J. Richard, of Paris, that recorded three elements, viz, atmospheric pressure (from which the height of the instrument can be calculated), air temperature, and relative humidity. The first of these meteorographs was ordered in 1896 on the model of one carried by French aeronauts; but since for use with kites lightness is all essential, M. Richard constructed his triple recorder for the first time of aluminum and thereby reduced its weight to $2\frac{1}{5}$ pounds.[3]

The kites and apparatus now used at Blue Hill are as follows: The kites generally have two rectangular cells covered with nainsook cloth, except at their tops and bottoms, and one is secured above the other by four or more sticks. The wooden frames are as light as possible, but are made rigid by guys of steel wire which bind them in all direc-

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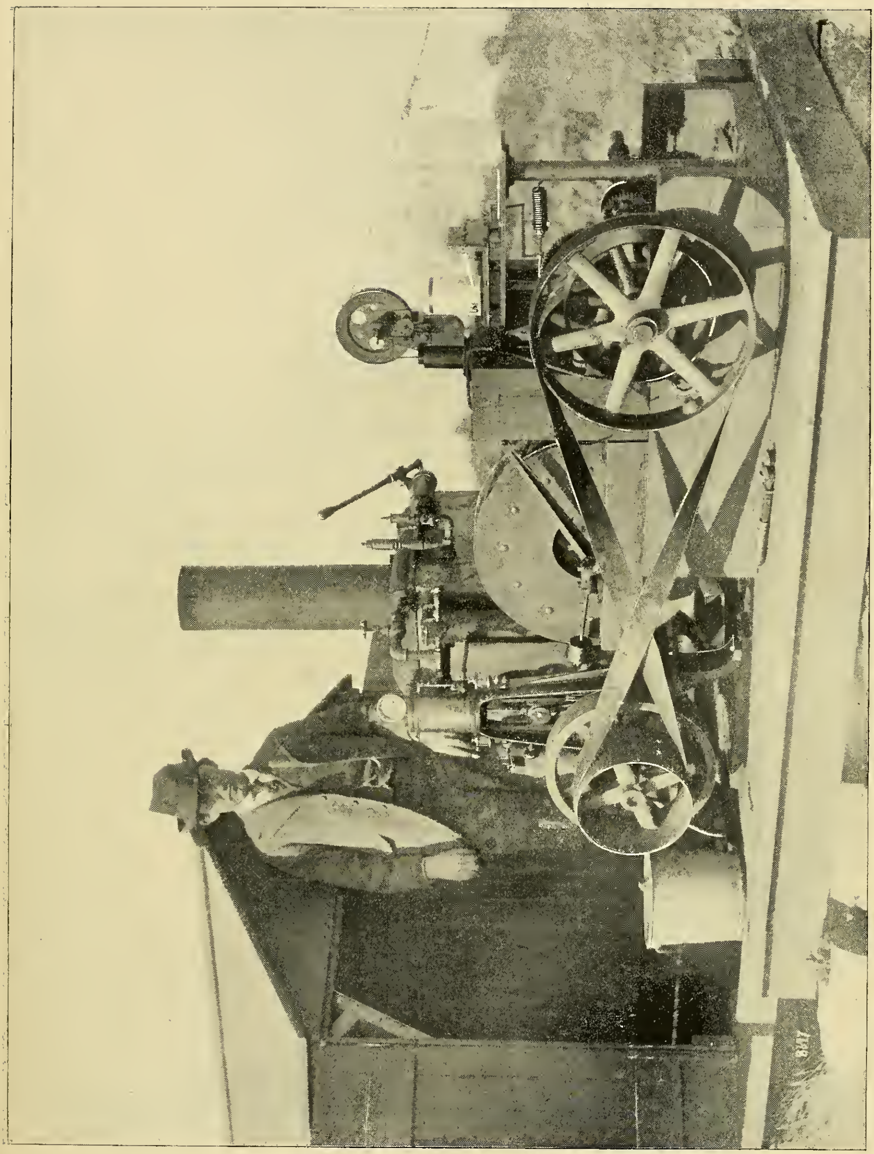
[1] Engineering, February 15, 1895.

[2] Monthly Weather Review of United States Weather Bureau, September, 1896.

[3] La Nature, February 8, 1896.

tions. Some of the kites stand nearly 9 feet high and have 64 square feet of lifting surface. A very efficient but complex form is Lamson's aero-curve folding kite which has the sustaining surfaces of its forward cell curved like a bird's wing, while the rear cell, with triangular superposed planes, acts as a tail or rudder. Figure 1 shows this kite in position to rise from the ground. In the bridle or hanger of the Blue Hill kites an elastic cord has been inserted which stretches when the wind pressure increases beyond a safe amount, causing the kite to fly more nearly horizontal while the gust lasts. These kites may be started in a wind which blows more than 12 miles an hour, and they continue to fly in gales of 50 miles per hour. On Blue Hill the average velocity of the wind for the year is 18 miles an hour, consequently there are few days when kites can not be flown there. The flying cords are fastened into a ring at the end of the wire, where the meteorograph is hung. The main line is steel "music wire," capable of withstanding a pull of 300 pounds, and weighing 15 pounds per mile. Several miles of this wire are spliced together with the greatest care, special pains being taken that no sharp bends occur which would cause it to break. To manipulate this wire the deep-sea sounding apparatus, driven by steam, has been modified by Mr. Fergusson to pull obliquely downwards. The wire from the kites passes over a swiveling pulley which follows the direction of the wire and registers on a dial the exact length of wire unreeled. Next the wire bears against a pulley carried by a strong spiral spring, by which the pull upon the wire at all times is graphically recorded on a paper-covered drum turned by clockwork. The wire then passes several times around the strain pulley and is finally coiled under slight tensions upon a large storage drum. When the kites are to be pulled down, the strain pulley is driven by a belt from a 2-horsepower Shipman automatic steam engine. When they are rising, the belt is disconnected and the pull of the kites is sufficient to unreel the wire. The usual rate of speed at which the wire is drawn in is from 3 to 6 miles an hour. Figure 2 shows the steam windlass and its constructor. In order to lift the increasing weight of wire it is customary to attach kites at various points on the wire, according to the pull upon it, so that nearly the same angular elevation may be maintained. This is done by screwing on the wire aluminum clamps to which the flying lines of the kites are tied or connected by toggles. The meteorograph, made in Paris, is contained in an aluminum cage of about a foot cube and weighs complete less than 3 pounds. The cage contains a barometer, a thermometer, and a hygrometer which all record their readings automatically on one cylinder turned by clockwork in twelve hours. It is only necessary to screen the thermometer from the sun's rays to obtain the true air temperature, since the wind insures the circulation of air. A kite meteorograph just constructed by Mr. Fergusson contains an anemomter, barometer, thermometer, and hygrometer, and yet weighs but 3 pounds. Figure 3 shows this instrument in the air supported by two of the largest Hargrave kites.



Steam Reeling Apparatus for Kites.

The method of making a kite flight for meteorological purposes at Blue Hill is as follows: Two large kites, fastened by their cords to the ring in the main wire, being in the air the meteorograph is hung to the ring by a cord with a spiral spring interposed to lessen shocks. The kites are then allowed to rise and to unreel the wire until the angle with the horizon becomes low, when, by means of the clamp just described, another kite is added, and so on. After a pause at the highest attainable altitude the reel is connected with the steam engine and the kites are drawn down. The pauses at the highest point, and when kites are attached or detached, are necessary to allow the recording instruments to acquire the conditions of the surrounding air, and because at these times the meteorograph is nearly stationary, measurements of its angular elevations are then made with a surveyor's transit. Since the length of the wire is known the height of the meteorograph can be calculated, it having been found that the sag of the wire or its deviation, either in a vertical or horizontal plane, from the assumed chord of the approximate arc which the wire follows, does not introduce an error of more than 3 per cent in the height so computed. When the meteorograph is hidden by clouds the altitude is computed from the barometer record by Laplace's formula. The time of making each angular measurement is noted in order that the corresponding point on the curve of the meteorograph may be determined.

The greatest heights ever reached by kites were above Blue Hill in the autumn of 1897. On September 19, the meteorograph was lifted 9,250 feet above the hill which is 630 feet above sea level. Seven kites, having a total lifting surface of 213 square feet, were employed to lift it and the wire which weighed about 60 pounds, was nearly 4 miles in length, and transmitted to the reel a maximum pull of 150 pounds. The flight occupied less than six and a half hours, and during nearly five hours the meteorograph was a mile or more above sea level. On October 15, the meteorograph was raised still higher by only four kites, of which the largest was Lamson's aero-curve kite, already described, the total lifting surface being 153 square feet. The height of 11,080 feet above the hill, or 11,710 feet above the sea, was reached with nearly the same length of wire and amount of pull as on September 19. The velocity of the wind at the ground varied from 13 to 22 miles per hour during the flight, which occupied but four and a half hours. In both these flights the temperature fell on the average about 1° F. for each 360 feet of ascent, but in the day the decrease was slower in the upper air, confirming Glaisher's observations in balloons. Although the sky was clear, strata of high humidity were encountered both in the ascent and descent, corresponding to the levels at which the cumulus and alto-cumulus clouds are known to form. At the highest altitude the air contained a very small percentage of moisture.

To understand the value of such data it may be necessary to state that the progress of the science of meteorology demands a knowledge of the conditions high above the earth's surface. Although some of this knowledge has been gained by meteorological stations on mountains in various parts of the world, yet these observations, which have the distinction of being the only long-continued ones possible at a fixed height, are affected to a greater or less extent by the mountain itself, and so do not give the true conditions of the free air. Balloons carrying aeronauts have long been used for this purpose, but the results obtained, from a meteorological standpoint, are incommensurate with the expense and risk incurred. Nevertheless, at the last international meteorological conference in Paris it was voted to recommend the use of balloons, both captive and free, which carried aeronauts, and free balloons provided only with automatic instruments to record at far greater heights than man can live. The successful experiments with kites at Blue Hill led to the recommendation that this method also should be included in the international scheme for exploring the atmosphere. An international committee, of which the writer is the American member, was appointed to carry out the recommendations. International cooperation in the work with balloons has been secured in Europe, but in America attention has been given chiefly to kites. To obtain the atmospheric conditions up to a height of at least 10,000 feet, kites present many advantages over free balloons whenever there is wind. Captive balloons, besides being more expensive than kites, can not attain nearly so great heights on account of the weight of the cable necessary to control the balloon. Kites then have these points of superiority:

1. Economy in installation and working.
2. Accurate determination of height by angular measurements.
3. Adequate exposure and ventilation of the thermometers which in a balloon are bathed in heated and stagnant air. Therefore the temperatures observed at a given height in a balloon are generally warmer during the ascent than during the descent, which is not the case with kites.
4. The possibility of making frequent ascents and descents permits observations to be obtained almost simultaneously in different air strata and nearly over a station at the ground where there may be instruments recording simultaneously.

On the few occasions when the wind is lacking near the ground there can be substituted the "kite balloon" already tried in Germany, which will always carry instruments to moderate heights, because it is not driven down, like the ordinary captive balloon, by the high winds which prevail in the upper air.

The advantages of kites are now becoming recognized, not only in this country, but abroad. Our Government Weather Bureau has expressed its intention to equip 20 stations with kite appliances to obtain synoptic data at the height of a mile in the free air for forecasting. In Europe the Blue Hill experiments have been repeated at Paris and at Strassburg.

About 200 records from kites have been obtained in the free air at

KITES AND METEOROGRAPH IN THE AIR.

heights of 100 to 11,000 feet above Blue Hill, in all kinds of weather. Most of them are discussed by Mr. Clayton in Appendix B to Part I of Vol. XLII of the Annals of the Astronomical Observatory of Harvard, and some general conclusions can only be stated here. As regards wind, the observations show that, as a rule, the wind steadily increases with elevation, confirming the measurements made upon clouds. The tendency of the kites as they rise is to come into a current from the west, and it is possible, with no great difference of height, to find currents almost diametrically opposed to each other. The decrease of temperature with increasing elevation varies under different conditions. On most days when there are no clouds the temperature falls at the adiabatic rate for unsaturated air, i. e., 1° F. for each 180 feet of ascent, to the height of a mile or more. On fair days with clouds the fall is at the above rate to the base of the clouds. In the cloud the rate of fall is slow, and it is still slower above the cloud. Kite records have been obtained during nearly twenty-four consecutive hours at a height of about half a mile above the ground. From these records it appears that the diurnal change in temperature in the free air nearly disappears at about 2,500 feet, which is much lower than has been hitherto supposed. During calm nights there is a marked inversion of temperature, so that the air near the ground is usually much colder than at the height of a few hundred feet. Indeed, on some occasions the air may be colder at the ground than at the height of several thousand feet. Kite flights, which were made daily for a week, confirm the theory that temperature changes in the upper air are cyclonic in character, being due to the passage of "warm and cold waves," that are more strongly felt in the upper air than near the ground. The changes with altitude which precede a warm wave are these: During the day a decrease of temperature at the adiabatic rate from the ground up to more than 1,000 feet, then a sudden rise of temperature, amounting to perhaps 15°, followed by a slow fall.

Clouds form when the dew point of the warm current, which overflows the cold current, is sufficiently high. Such conditions announce the arrival of a "warm wave" eight to twenty-four hours in advance of its appearance at the earth's surface. The conditions which indicate the coming of a "cold wave" are a rapid fall of temperature, which exceeds the adiabatic rate up to about 1,000 feet and above that is the adiabatic rate to 3,000 feet or higher. During the prevalence of the cold wave the temperature at the height of a mile, which is sometimes its upper limit, may be 25° or 30° F. lower than at the ground. After the cold wave has passed, and with the coming of a southeast storm, the temperature rises rapidly up to a height of 1,000 or 2,000 feet and then slowly falls. Cloud usually occurs where the temperature begins to fall, and sometimes this cloud extends downward to the earth as fog. The relative humidity generally increases to saturation in the clouds and above them rapidly decreases. In clear weather there may be no change of relative humidity with altitude, as during cold waves, or with no change of temperature vertically, both the absolute and relative humidity may decrease rapidly with increase of altitude, as is the case in areas of high barometric pressure. At the height of half a mile the diurnal changes of relative humidity are the inverse of those at the ground. In brief, then, during fair weather in the upper air the days are relatively cold and damp, while the nights are warm and dry, as compared with surface conditions. Electricity is usually noticed on the kite wire whenever the altitude of the kites exceed 1,700 feet. At higher altitudes during snowstorms and near thunderstorms the potential increases and is sufficient to cause strong sparking discharges. It therefore appears to be only necessary to tap the great atmospheric reservoir to obtain an inexhaustible supply of electricity, which perhaps may be applied to the service of man.

THE DEBT OF THE WORLD TO PURE SCIENCE.[1]

By John J. Stevenson.

The fundamental importance of abstruse research receives too little consideration in our time. The practical side of life is all absorbent; the results of research are utilized promptly, and full recognition is awarded to the one who utilizes while the investigator is ignored. The student himself is liable to be regarded as a relic of mediæval times, and his unconcern respecting ordinary matters is serviceable to the dramatist and newspaper witlet in their times of need.

Yet every thoughtful man, far away as his calling may be from scientific investigation, hesitates to accept such judgment as accurate. Not a few, engrossed in the strife of the market place, are convinced that, even from the selfish standpoint of mere enjoyment, less gain is found in amassing fortunes or in acquiring power over one's fellows than in the effort to solve nature's problems. Men scoff at philosophical dreamers, but the scoffing is not according to knowledge. The exigencies of subjective philosophy brought about the objective philosophy. Error has led to the right. Alchemy prepared the way for chemistry; astrology for astronomy; cosmogony for geology. The birth of inductive science was due to the necessities of deductive science, and the greatest development of the former has come from the trial of hypotheses belonging in the borderland between science and philosophy.

My effort this evening is to show that discoveries, which have proved all important in secondary results, did not burst forth full-grown; that in each case they were, so to say, the crown of a structure reared painfully and noiselessly by men indifferent to this world's affairs, caring little for fame and even less for wealth. Facts were gathered, principles were discovered, each falling into its own place until at last the brilliant crown shown out and the world thought it saw a miracle.

This done, I shall endeavor to draw a moral, which it is hoped will be found worthy of consideration.

The heavenly bodies were objects of adoration from the earliest antiquity; they were guides to caravans on the desert as well as to mariners far from land; they marked the beginning of seasons, or, as

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[1] Presidential address delivered at the annual meeting of the New York Academy of Sciences, February 28, 1898. Printed in Science, March 11, 1898.

in Egypt, the limits of vast periods embracing many hundreds of years. Maps were made thousands of years ago showing their positions; the path of the sun was determined rudely; the influence of the sun and moon upon the earth was recognized in some degree, and their influence upon man was inferred. Beyond these matters, man, with unaided vision and with knowledge of only elementary mathematics, could not go.

Mathematical investigations by Arabian students prepared the means by which, after Europe's revival of learning, one, without wealth, gave a new life to astronomy. Copernicus, early trained in mathematics, during the last thirty years of his life spent the hours, stolen from his work as a clerk and charity physician, in mathematical and astronomical studies, which led him to reject the complex Ptolemaic system and to accept in modified form that bearing the name of Pythagoras. Tycho Brahe followed. A mere star-gazer at first, he became an earnest student, improved the instruments employed, and finally secured recognition from his sovereign. For twenty-five years he sought facts, disregarding none, but seldom recognizing economic importance in any. His associate, Kepler, profiting by his training under Brahe, carried the work far beyond that of his predecessors—and this in spite of disease, domestic sorrows, and only too frequent experience of abject poverty. He divested the Copernican hypothesis of many crudities, and discovered the laws which have been utilized by astronomers in all phases of their work. He ascertained the causes of the tides, with the aid of the newly invented telescope made studies of eclipses and occultations, and just missed discovering the law of gravitation. He laid the foundation for practical application of astronomy to everyday life.

In the eighteenth century astronomy was recognized by governments as no longer of merely curious interest, and its students received abundant aid. The improvement of the telescope, the discovery of the law of gravitation, and the invention of logarithms had made possible the notable advances marking the close of the seventeenth century. The increasing requirements of accuracy led to exactness in the manufacture of instruments, to calculation and recalculation of tables, to long expeditions for testing methods as well as conclusions, until finally the suggestion of Copernicus, the physician, and of Kepler, the ill-fed invalid, became fact, and astronomical results were utilized to the advantage of mankind. The voyager on the ocean and the agriculturist on land alike reap benefit from the accumulated observations of three centuries, though they know nothing of the principles or of the laborers by whom the principles were discovered. The regulation of chronometers as well as the fixing of boundary lines between great nations is determined by methods due to slow accumulation of facts, slower development in analysis and calculation, and even slower improvement in instruments.

Galvani's observations that frogs' legs twitch when near a friction machine in operation led him to test the effect of atmospheric electricity upon them. The instant action brought about the discovery that it was due not to atmospheric influence, but to a current produced by contact of a copper hook with an iron rail. Volta pursued the investigation and constructed the pile which bears his name. With this, modified, Davy, in 1807, decomposed potash and soda, thereby isolating potassium and sodium. This experiment, repeated successfully by other chemists, was the precursor of many independent investigations, which directed to many lines of research, each increasing in interest as it was followed.

Volta's crown of cups expanded into the clumsy trough batteries which were displaced finally in 1836 by Daniel's constant battery, using two fluids, one of which was cupric sulphate. De la Rue observed that, as the sulphate was reduced, the copper was deposited on the surface of the outer vessel and copied accurately all markings on that surface. Within two or three years Jacobi and Spencer made the practical application of this observation by reproducing engravings and medals. Thus was born the science of electro-metallurgy. At first mere curiosities were made, then electroplating in a wider way, the electrotype, the utilization of copper to protect more easily destructible metals, the preparation of articles for ornament and utility by covering baser metals with copper or silver or gold, while now the development of electro-generators has led to wide applications in the reduction of metals and to the saving of materials which otherwise would go to waste.

Oersted, in 1819–20, puzzling over the possible relations of voltaic electricity to magnetism, noticed that a conductor carrying an electrical current becomes itself a magnet and deflects the needle. Sturgeon, working along the same lines, found that soft iron inclosed in a coil through which a current passes becomes magnetic, but loses the power when the current ceases. This opened the way for our own Henry's all-important discovery of the reciprocating electro magnets and the vibrating armature—the essential parts of the magnetic telegraph. Henry actually constructed a telegraph in 1832, winding the wires around his class room in Albany and using a bell to record the making and breaking signals. Here, as he fully recognized, was everything but a simple device for receiving signals.

Several years later Professor Morse, dreaming night and day of the telegraph, was experimenting with Moll's electro-magnet and finding only discouragement. His colleague, Professor Gale, advised him to discard the even then antiquated apparatus and to utilize the results given in Henry's discussion. At once the condition was changed, and soon the ingenious recording instrument bearing Morse's name was constructed. Henry's scientific discoveries were transmuted by the inventor's ingenuity into substantial glory for Morse and proved a source of inconceivable advantage to the whole civilized world. Steinhal's discovery that the earth can be utilized for the return current completed the series of fundamental discoveries, and since that time everything has been elaboration.

Oersted's discovery respecting the influence of an electric current, closely followed by that of Arago in the same direction, opened the way for Faraday's complete discovery of induction, which underlies the construction of the dynamo. This ascertained, the province of the inventor was well defined—to conjure some mechanical appliance whereby the principle might be utilized. But here as elsewhere the work of discovery and that of invention went on almost pari passu; the results of each increased those of the other. The distance from the Clark and Page machines of the middle thirties, with their cumbrous horseshoe magnets and disproportionate expenditure of power, to the Siemens machine of the fifties was long, but it was no leap. In like manner slow steps marked progress thence to the Gramme machine, in which one finds the outgrowth of many years of labor by many men, both investigators and inventors. In 1870, forty years after Faraday's announcement of the basal principle, the stage was reached whence progress could be rapid. Since that time the dynamo has been brought into such stage of efficiency that the electro-motor seems likely to displace not merely the steam engine, but also other agencies in direct application of force. The horse is passing away and the trolley road runs along the country highway; the longer railways are considering the wisdom of changing their power; cities are lighted brilliantly where formerly the gloom invited highwaymen to ply their trade, and even the kitchen is invaded by new methods of heating.

Long ago it was known that if the refining of pig iron be stopped just before the tendency to solidify became pronounced the wrought iron is more durable than that obtained in the completed process. Thus, imperfectly refined metal was made frequently, though unintentionally and ignorantly. A short railroad in southwestern Pennsylvania was laid in the middle sixties with iron rails of light weight. A rail's life in those days rarely exceeded five years, yet some of those light rails were in excellent condition almost fifteen years afterwards, though they had carried a heavy coke traffic for several years. But this process was uncertain, and the best puddlers could never tell when to stop the process in order to obtain the desired grade.

When a modification of this refining process was attempted on a grand scale almost contemporaneously by Martien in this country and Bessemer in England, the same uncertainty of product was encountered; sometimes the process was checked too soon, at others pushed too far. Here the inventor came to a halt. He could use only what was known and endeavor to improve methods of application. Under such conditions the Bessemer process was apparently a hopeless failure. Another, however, utilized the hitherto ignored work of the closet investigator. The influence of manganese in counteracting the effects of certain injurious substances and its relation to carbon when present in pig iron were understood as matters of scientific interest. Mushet recognized the bearing of these facts and used them in changing the process. His method proved successful, but, with thorough scientific forgetfulness of the main chance, he neglected to pay some petty fees at the Patent Office and so reaped neither profit nor popular glory for his work.

The Mushet process having proved the possibility of immediate and certain conversion, the genius of the inventor found full scope. The change in form and size of the converter, the removable base, the use of trunnions, and other details, largely due to the American, Holley, so increased the output and reduced the cost that Bessemer steel soon displaced iron and the world passed from the age of iron into the age of steel.

Architectural methods have been revolutionized. Buildings ten stories high are commonplace; those of twenty no longer excite comment, and one of thirty arouses no more than a passing pleasantry respecting possibilities at the top. Such buildings were almost impossible a score of years ago, and the weight made the cost prohibitive. The increased use of steel in construction seems likely to preserve our forests from disappearance.

In other directions the gain through this process has been more important. The costly, short-lived iron rail has disappeared and the durable steel rail has taken its place. Under the moderate conditions of twenty-five years ago, iron rails rarely lasted for more than five years; in addition, the metal was soft, the limit of load was reached quickly, and freight rates, though high, were none too profitable.

But all changed with the advent of steel rails as made by the American process. Application of abstruse laws, discovered by men unknown to popular fame, enabled inventors to improve methods and to cheapen manufacture until the first cost of steel rails was less than that of iron. The durability of the new rails and their resistance to load justified increased expenditure in other directions to secure permanently good condition of the roadbed. Just here our fellow-member, Mr. P. H. Dudley, made his contribution, whose importance can hardly be overestimated. With his ingenious recording apparatus it is easy to discover defects in the roadway and to ascertain their nature, thus making it possible to devise means for their correction and for preventing their recurrence. The information obtained by use of this apparatus has led him to change the shape and weight of rails, to modify the type of joints and the methods of ballasting, so that now a roadbed should remain in good condition and even improve during years of hard use.

But the advantages have not inured wholly to the railroad companies. It is true that the cost of maintenance has been reduced greatly; that locomotives have been made heavier and more powerful; that freight cars carry three to four times as much as they did twenty-five years ago, so that the whole cost of operation is very much less than formerly. But where the carrier has gained one dollar the consumer and shipper have gained hundreds of dollars. Grain and flour can be brought from Chicago to the seaboard as cheaply by rail as by water; the farmer in Dakota raises wheat for shipment to Europe. Coal mined in West Virginia can be sold on the docks of New York at a profit for less than half the freight of twenty-five years ago. Our internal commercial relations have been changed, and the revolution is still incomplete. The influence of the Holley-Mushet-Bessemer process upon civilization is hardly inferior to that of the electric telegraph.

Sixty years ago an obscure German chemist obtained an oily liquid from coal-tar oil, which gave a beautiful tint with calcium chloride; five years later another separated a similar liquid from a derivative of coal-tar oil. Still later, Hofmann, then a student in Liebig's laboratory, investigated these substances and proved their identity with an oil obtained long before by Zinin from indigo, and applied to them all Zinin's term, Anilin. The substance was curiously interesting, and Hofmann worked out its reactions, discovering that with many materials it gives brilliant colors. The practical application of these discoveries was not long delayed, for Perkins made it in 1856. The marvelous dyes, beginning with Magenta and Solferino, have become familiar to all. The anilin colors, especially the reds, greens, and blues, are among the most beautiful known. They have given rise to new industries and have expanded old ones. Their usefulness led to deeper studies of coal-tar products, to which is due the discovery of such substances as antipyrin, phenacetin, ichthyol, and saccharin, which have proved so important in medicine.

One is tempted to dwell for a little upon meteorology, that border land where physics, chemistry, and geology meet, and to speak of the signal-service system, the outgrowth of the studies of an obscure school teacher in Philadelphia, but the danger of trespassing too far upon your endurance makes proper only this passing reference.

While men of wealth and leisure wasted their energies in literary and philosophical discussions respecting the nature and origin of things, William Smith, earning a living as a land surveyor, plodded over England, anxious only to learn, in no haste to explain. His work was done honestly and slowly; when finished as far as possible with his means, it had been done so well that its publication checked theorizing and brought men back to study. His geological map of England was the basis upon which the British survey began to prepare the detailed sheets showing Britain's mineral resources.

In our country Vanuxem and Morton early studied the New Jersey Cretaceous and Eocene, containing vast beds of marl. Scientific interest was aroused, and eventually a geological survey of the State was ordered by the legislature. The appropriation was insignificant, and many of the legislators voted for it hoping that some economic discovery might be made to justify their course in squandering the people's money. Yet there were lingering doubts in their minds and some found more than lingering doubts in the minds of their constituents. But when the marls were proved to contain materials which the chemist Liebig had shown to be all-important for plants, the conditions were changed and criticism ceased. The dismal sands of eastern New Jersey, affording only a scanty living for pines and grasses, were converted by application of the marl into gardens of unsurpassed fertility. Vanuxem's study of the stratigraphy and Morton's study of the fossils had made clear the distribution of the marls, and the survey scattered the information broadcast.

Morton and Conrad, with others scarcely less devoted, labored in season and out of season to systematize the study of fossil animals. There were not wanting educated men who wondered why students of such undoubted ability wasted themselves in trifling employment instead of doing something worthy of themselves so as to acquire money and fame. Much nearer to our own time there were wise legislators who questioned the wisdom of "wasting money on pictures of clams and salamanders," though the same men appreciated the geologist who could tell them the depth of a coal bed below the surface. But the lead diggers of Illinois and Iowa long ago learned the use of paleontology, for the "lead fossil" was their guide in prospecting. The importance and practical application of this science, so largely the outgrowth of unappreciated toil in this country as well as in Europe, is told best in Professor Hall's reply to a patronizing politician's query: "And what are your old fossils good for?" "For this: Take me blindfolded in a balloon; drop me where you will; if I can find some fossils I'll tell you in ten minutes for what minerals you may look and for what minerals you need not look."

Many regard botany as a pleasing study, well fitted for women and dilletanti, but hardly deserving attention by strong men. Those who speak thus only exercise the prerogative of ignorance, which is to despise that which one is too old or too lazy to learn. The botanist's work is not complete when the carefully-gathered specimen has been placed in the herbarium with its proper label. That is but the beginning, for he seeks the relations of plants in all phases. In seeking these he discovers facts which often prove to be of cardinal importance. The rust which destroys wheat in the last stage of ripening, the disgusting fungus which blasts Indian corn, the poisonous ergot in rye, the blight of the pear and other fruits, fall as much within the botanist's study as do the flowers of the garden or the Sequoias of the Sierra. Not a few of the plant diseases which have threatened famine or disaster have been studied by botanists unknown to the world, whose explanations have led to palliation or cure.

The ichthyologist, studying the habits of fishes, discovered characteristics which promptly commended themselves to men of practical bent. The important industry of artificial fertilization and the transportation of fish eggs, which has enabled man to restock exhausted localities and to stock new ones, is but the outgrowth of closet studies which have shown how to utilize nature's superabundant supply.

The entomologist has always been an interesting phenomenon to a large part of our population. Insects of beauty are attractive, those of large size are curious, while many of the minuter forms are efficient in gaining attention. But that men should devote their lives to the study of the unattractive forms is to many a riddle. Yet entomology yields to no branch of science in the importance of its economic bearings. The study of the life habits of insects, their development, their food, their enemies, a study involving such minute detail as to shut men off from many of the pleasures of life and to convert them into typical students, has come to be so fraught with relations to the public weal that the State entomologist's mail has more anxious letters than that of any other officer.

Insects are no longer regarded as visitations from an angry deity, to be borne in silence and with penitential awe. The intimate study of individual groups has taught in many cases how to antagonize them. The scab threatened to destroy orange-culture in California; the Colorado beetle seemed likely to ruin one of our important food crops; minute aphides terrified raisers of fruit and cane in the Sandwich Islands. But the scab is no longer a frightful burden in California; the potato bug is now only an annoyance, and the introduction of lady birds swept aphides from the Sandwich Islands. The gypsy moth, believed for more than a hundred years to be a special judgment, is no longer thought of as more than a very expensive nuisance. The curculio, the locust, the weevil, the chinch bug, and others have been subjected to detailed investigation. In almost all cases methods have been devised whereby the ravages have been diminished. Even the borers, which endangered some of the most important timber species, are now understood, and the possibility of their extermination has been changed into probability.

Having begun with the "infinitely great," we may close this summary with a reference to the "infinitely small." The study of fermentation processes was attractive to chemists and naturalists, each claiming ownership of the agencies. Pasteur, with a patience almost incredible, revised the work of his predecessors and supplemented it with original investigations, proving that a very great part of the changes in organic substances exposed to the atmosphere are due primarily to the influence of low animals or plants, whose germs exist in the atmosphere.

One may doubt whether Pasteur had any conception of the possibilities hidden in his determination of the matters at issue. The canning of meats and vegetables is no longer attended with uncertainty, and scurvy is no longer the bane of explorers; pork, which has supplied material for the building of railroads, the digging of canals, the construction of ships, can be eaten without fear. Flavorless butter can be rendered delicious by the introduction of the proper bacteria; sterilized milk saves the lives of many children; some of the most destructive plagues are understood and the antidotes are prepared by the culture of antagonistic germs; antiseptic treatment has robbed surgery of half its terrors, and has rendered almost commonplace operations which, less than two decades ago, were regarded as justifiable only as a last resort. The practice of medicine has been advanced by outgrowths of Pasteur's work almost as much as it was by Liebig's chemical investigations more than half a century ago.

In this review the familiar has been chosen for illustration in preference to the wonderful, that your attention might not be diverted from the main issue, that the foundation of industrial advance was laid by workers in pure science, for the most part ignorant of utility and caring little about it. There is here no disparagement of the inventor; without his perception of the practical and his powers of combination the world would have reaped little benefit from the student's researches. But the investigator takes the first step and makes the inventor possible. Thereafter the inventor's work aids the investigator in making new discoveries, to be utilized in their turn.

Investigation, as such, rarely receives proper recognition. It is usually regarded as quite a secondary affair, in which scientific men find their recreation. If a geologist spends his summer vacation in an effort to solve some perplexing structural problem he finds, on his return, congratulations because of his glorious outing; the astronomer, the physicist, and the chemist are all objects of semienvious regard, because they are able to spend their leisure hours in congenial amusements; while the naturalist, enduring all kinds of privation, is not looked upon as a laborer, because of the physical enjoyment which most good people think his work must bring.

It is true that investigation, properly so-called, is made secondary, but this is because of necessity. Scientific men in Government service are hampered constantly by the demand for immediately useful results. Detailed investigation is interrupted because matters apparently more important must be considered. The conditions are even more unfavorable in most of our colleges and none too favorable in our greater universities. The "literary leisure" supposed to belong to college professors does not fall to the lot of teachers of science, and very little of it can be discovered by college instructors in any department. The intense competition among our institutions requires that professors be magnetic teachers, thorough scholars, active in social work, and given to frequent publication, that, being prominent, they may be living advertisements of the institution. How much time, opportunity, or energy remains for patient investigation some may be able to imagine.

The misconception respecting the relative importance of investigation is increased by the failure of even well-educated men to appreciate the changed conditions in science. The ordinary notion of scientific ability is expressed in the popular saying that a competent surgeon can saw a bone with a butcher knife and carve muscle with a handsaw. Once, indeed, the physicist needed little aside from a spirit lamp, test tubes, and some platinum wire or foil; low power microscopes, small reflecting telescopes, rude balances, and home-made apparatus certainly did wonderful service in their day; there was a time when the finder of a mineral or fossil felt justified in regarding it as new and in describing it as such; when a psychologist needed only his own great self as a basis for broad conclusions respecting all mankind. All of that belonged to the infancy of science, when little was known and any observation was liable to be a discovery; when a Humboldt, an Arago, or an Agassiz was possible. But all is changed; workers are multiplied in every land; study in every direction is specialized; men have ceased the mere gathering of facts and have turned to the determination of relations. Long years of preparation are needed to fit one to begin investigation; familiarity with several languages is demanded; great libraries are necessary for constant reference, and costly apparatus is essential even for preliminary examination. Where tens of dollars once supplied the equipment in any branch of science, hundreds, yes, thousands, of dollars are required now.

Failure to appreciate the changed conditions induces neglect to render proper assistance. As matters now stand, even the wealthiest of our educational institutions can not be expected to carry the whole burden, for endowments are insufficient to meet the too rapidly increasing demand for wider range of instruction. It is unjust to expect that men, weighted more and more by the duties of science teaching, involving, too often, much physical labor from which teachers of other subjects are happily free, should conduct investigations at their own expense and in hours devoted by others to relaxation. Even were the pecuniary cost comparatively small, to impose that would be unjust, for, with few exceptions, the results are given to the world without compensation. Scientific men are accustomed to regard patents much as regular physicians regard advertising.

America owes much to closet students as well as to educated inventors who have been trained in scientific modes of thought. The extraordinary development of our material resources—our manufacturing, mining, and transporting interests—shows that the strengthening of our educational institutions on the scientific side brings actual profit to the community. But most of this strengthening is due primarily to unremunerated toil of men dependent on the meager salary of college instructors or Government officials in subordinate positions. Their aptitude to fit others for usefulness, coming only from long training, was acquired in hours stolen from sleep or from time needed for recuperation. But the labors of such men have been so fruitful in results that we can no longer depend on the surplus energy of scientific men, unless we consent to remain stationary. If the rising generation is to make the most of our country's opportunities it must be educated by men who are not compelled to acquire aptness at the cost of vitality. The proper relation of teaching-labor to investigation-labor should be recognized, and investigation, rather than social, religious, or political activity, should be a part of the duty assigned to college instructors.

Our universities and scientific societies ought to have endowments specifically for aid in research. The fruits of investigations due to Smithson's bequest have multiplied his estate hundreds of times over to the world's advantage. He said well that his name would be remembered long after the names and memory of the Percy and Northumberland families had passed away. Hogkins' bequest to the Smithsonian Institution is still too recent to have borne much fruit, but men already wonder at the fruitfulness of a field supposed to be well explored. Nobel knew how to apply the results of science; utilizing the chemist's results, he applied nitroglycerin to industrial uses; similarly he developed the petroleum industry of Russia and, like that of our American petroleum manufacturers, his influence was felt in many other industries of his own land and of the Continent. At his death he bequeathed millions of dollars to the Swedish Academy of Sciences that the income might be expended in encouraging pure research. Smithson, Hodgkins, and Nobel have marked out a path which should be crowded with Americans.

The endowment of research is demanded now as never before. The development of technical education, the intellectual training of men to fit them for positions formerly held by mere tyros, has changed the material conditions in America. The surveyor has disappeared—none but a civil engineer is trusted to lay out even town lots; the founder at an iron furnace is no longer merely a graduate of the casting house—he must be a graduate in metallurgy; the manufacturer of paints can not intrust his factory to any but a chemist of recognized standing; no graduate from the pick is placed in charge of mines—a mining engineer alone can gain confidence; and so everywhere. With the will to utilize the results of science there has come an intensity of competition in which victory belongs only to the best equipped. The profit awaiting successful inventors is greater than ever, and the anxious readiness to apply scientific discoveries is shown by the daily records. The Röntgen rays were seized at once and efforts made to find profitable application; the properties of zirconia and other earths interested inventors as soon as they were announced; the possibility of telegraphing without wires incited inventors everywhere as soon as the principle was discovered.

Nature's secrets are still unknown and the field for investigation is as broad as ever. We are only on the threshold of discovery, and the coming century will disclose wonders far beyond any yet disclosed. The atmosphere, studied by hundreds of chemists and physicists for a full century, proved for Rayleigh and Ramsay an unexplored field within this decade. We know nothing yet. We have gathered a few large pebbles from the shore, but the mass of sands is yet to be explored.

And now the moral has been drawn. The pointing is simple. If America, which, more than other nations, has profited by science, is to retain her place, Americans must encourage, even urge, research; must strengthen her scientific societies and her universities, that under the new and more complicated conditions her scientific men and her inventors may place and keep her in the front rank of nations.

THE AGE OF THE EARTH AS AN ABODE FITTED FOR LIFE.[1]

By the Right Hon. Lord **Kelvin**, G. C. V. O.

1. The age of the earth as an abode fitted for life is certainly a subject which largely interests mankind in general. For geology it is of vital and fundamental importance—as important as the date of the battle of Hastings is for English history—yet it was very little thought of by geologists of thirty or forty years ago; how little is illustrated by a statement,[2] which I will now read, given originally from the presidential chair of the Geological Society by Professor Huxley in 1869, when for a second time, after a seven years' interval, he was president of the society.

> "I do not suppose that at the present day any geologist would be found * * * to deny that the rapidity of the rotation of the earth *may* be diminishing, that the sun *may* be waxing dim, or that the earth itself *may* be cooling. Most of us, I suspect, are Gallios, 'who care for none of these things,' being of opinion that, true or fictitious, they have made no practical difference to the earth, during the period of which a record is preserved in stratified deposits."

2. I believe the explanation of how it was possible for Professor Huxley to say that he and other geologists did not care for things on which the age of life on the earth essentially depends is because he did not know that there was valid foundation for any estimates worth considering as to absolute magnitudes. If science did not allow us to give any estimate whatever as to whether 10 million or 10 billion years is the age of this earth as an abode fitted for life, then I think Professor Huxley would have been perfectly right in saying that geologists should not trouble themselves about it, and biologists should go on in their own way, not inquiring into things utterly beyond the power of human understanding and scientific investigation. This would have left geology much in the same position as that in which English history would be if it were impossible to ascertain whether the battle of Hastings took place 800 years ago, or 800 thousand years ago, or 800 million

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[1] The 1897 annual address of the Victoria Institute, with additions written at different times from June to December, 1897. Printed in Victoria Institute Transactions.

[2] In the printed quotations the italics are mine in every case, not so the capitals in the quotation from Page's Text-Book.

years ago. If it were absolutly impossible to find out which of these periods is more probable than the other, then I agree we might be Gallios as to the date of the Norman Conquest. But a change took place just about the time to which I refer, and from then till now geologists have not considered the question of absolute dates in their science as outside the scope of their investigations.

3. I may be allowed to read a few extracts to indicate how geological thought was expressed in respect of this subject, in various largely used popular text-books, and in scientific writings which were new in 1868, or not too old to be forgotten. I have several short extracts to read and I hope you will not find them tedious.

The first is three lines from Darwin's Origin of Species, 1859 edition, page 287:

> "In all probability a far longer period than 300 million years has elapsed since the latter part of the secondary period."

Here is another still more important sentence, which I read to you from the same book:

> "He who can read Sir Charles Lyell's grand work on the Principles of Geology, which the future historian will recognize as having produced a revolution in natural science, yet does not admit how *incomprehensibly vast* have been the vast periods of time, *may at once close this volume.*"

I shall next read a short statement from Page's Advanced Students' Text-Book of Geology, published in 1859:

> "Again where the FORCE seems unequal to the result, the student should never lose sight of the element TIME: *an element to which we can set no bounds in the past*, any more than we know of its limit in the future.
>
> "It will be seen from this hasty indication that there are two great schools of geological causation—the one ascribing every result to the ordinary operations of nature, combined with the element of *unlimited time*, the other appealing to agents that operated during the earlier epochs of the world with greater intensity, and also for the most part over wider areas. *The former belief is certainly more in accordance with the spirit of right philosophy*, though it must be confessed that many problems in geology seem to find their solution only through the admission of the latter hypothesis."

4. I have several other statements which I think you may hear with some interest. Dr. Samuel Haughton, of Trinity College, Dublin, in his Manual of Geology, published in 1865, page 82, says:

> "The infinite time of the geologists is in the past; and *most of their speculations regarding this subject seem to imply the absolute infinity of time*, as if the human imagination was unable to grasp the period of time requisite for the formation of a few inches of sand or feet of mud, and its subsequent consolidation into rock." (This delicate satire is certainly not overstrained.)
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> "Professor Thomson has made an attempt to calculate the length of time during which the sun can have gone on burning at the present rate, and has come to the following conclusion: 'It seems, on the whole,

most probable that the sun has not illuminated the earth for 100,000,000 years, and almost certain that he has not done so for 500,000,000 years. As for the future, we may say with equal certainty, that the inhabitants of the earth can not continue to enjoy the light and heat essential to their life for many million years longer, unless new sources, now unknown to us, are prepared in the great storehouse of creation."

I said that in the sixties and I repeat it now; but with charming logic it is held to be inconsistent with a later statement that the sun has not been shining 60,000,000 years; and that both that and this are stultified by a still closer estimate which says that probably the sun has not been shining for 30,000,000 years! And so my efforts to find some limit or estimate for geological time have been referred to and put before the public, even in London daily and weekly papers, to show how exceedingly wild are the wanderings of physicists, and how mutually contradictory are their conclusions as to the length of time which has actually passed since the early geological epochs to the present date.

Dr. Haughton further goes on:

" *This result* (100 to 500 million years) of Professor Thomson's, *although very liberal in the allowance of time, has offended geologists, because, having been accustomed to deal with time as an infinite quantity at their disposal, they feel naturally embarrassment and alarm at any attempt of the science of physics to place a limit upon their speculations.* It is quite possible that even a hundred million of years may be greatly in excess of the actual time during which the sun's heat has remained constant."

5. Dr. Haughton admitted so much with a candid open mind; but he went on to express his own belief (in 1865) thus:

"Although I have spoken somewhat disrespectfully of the geological calculus in my lecture, yet I believe that the time during which organic life has existed on the earth is practically infinite, because it can be shown to be so great as to be inconceivable by beings of our limited intelligence."

Where is inconceivableness in 10,000,000,000? There is nothing inconceivable in the number of persons in this room, or in London. We get up to millions quickly. Is there anything inconceivable in 30,000,000 as the population of England, or in 38,000,000 as the population of Great Britain and Ireland, or in 352,704,863 as the population of the British Empire? Not at all. It is just as conceivable as half a million years or 500 millions.

6. The following statement is from Professor Jukes's Students' Manual of Geology:

" The time required for such a slow process to effect such enormous results must of course be taken to be inconceivably great. The word 'inconceivably' is not here used in a vague but in a literal sense, to indicate that the lapse of time required for the denudation that has produced the present surfaces of some of the older rocks is vast beyond any idea of time which the human mind is capable of conceiving.

" Mr. Darwin, in his admirably reasoned book on the origin of species, so full of information and suggestion on all geological subjects, estimates the time required for the denudation of the rocks of the Weald of Kent,

> or the erosion of space between the ranges of chalk hills, known as the North and South Downs, at *three hundred millions of years.* The grounds for forming this estimate are of course of the vaguest description. It may be possible, perhaps, that the estimate is a hundred times too great, and that the real time elapsed did not exceed three million years, but, on the other hand, it is just as likely that the time which actually elapsed since the first commencement of the erosion till it was nearly as complete as it now is, was really a hundred times greater than his estimate, or thirty thousand millions of years."

7. Thus Jukes allowed estimates of anything from 3,000,000 to 30,000 millions as the time which actually passed during the denudation of the Weald. On the other hand, Professor Phillips in his Rede lecture to the University of Cambridge (1860), decidedly prefers 1 inch per annum to Darwin's 1 inch per century as the rate of erosion, and says that most observers would consider even the 1 inch per annum too small for all but the most invincible coasts. He thus, on purely geological grounds, reduces Darwin's estimate of the time to less than one one-hundredth. And, reckoning the actual thicknesses of all the known geological strata of the earth, he finds 96,000,000 years as a possible estimate for the antiquity of the base of the stratified rocks, but he gives reasons for supposing that this may be an overestimate, and he finds that from stratigraphical evidence alone we may regard the antiquity of life on the earth as possibly between 38,000,000 and 96,000,000 of years. Quite lately a very careful estimate of the antiquity of strata containing remains of life on the earth has been given by Professor Sollas, of Oxford, calculated according to stratigraphical principles which had been pointed out by Mr. Alfred Wallace. Here it is:[1]

> "So far as I can at present see, the lapse of time since the beginning of the Cambrian system is probably less than 17,000,000 years, even when computed on an assumption of uniformity, which to me seems contradicted by the most salient facts of geology. Whatever additional time the calculations made on physical data can afford us may go to the account of pre-Cambrian deposits of which at present we know too little to serve for an independent estimate."

8. In one of the evening conversaziones of the British Association during its meeting at Dundee, in 1867, I had a conversation on geological time with the late Sir Andrew Ramsay, almost every word of which remains stamped on my mind to this day. We had been hearing a brilliant and suggestive lecture by Professor (now Sir Archibald) Geikie on the geological history of the actions by which the existing scenery of Scotland was produced. I asked Ramsay how long a time he allowed for that history. He answered that he could suggest no limit to it. I said, "You don't suppose things have been going on always as they are now? You don't suppose geological history has run through 1,000,000,000 years?" "Certainly I do." "10,000,000,000 years?" "Yes." "The sun is a finite body. You can tell how many tons it is. Do you think it has been shining on for a million million years?"

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[1] "The Age of the Earth," Nature, April 4, 1895.

"I am as incapable of estimating and understanding the reasons which you physicists have for limiting geological time as you are incapable of understanding the geological reasons for our unlimited estimates." I answered, "You can understand physicists' reasoning perfectly if you give your mind to it." I ventured also to say that physicists were not wholly incapable of appreciating geological difficulties; and so the matter ended, and we had a friendly agreement to temporarily differ.

9. In fact, from about the beginning of the century till that time (1867), geologists had been nurtured in a philosophy originating with the Huttonian system; much of it substantially very good philosophy, but some of it essentially unsound and misleading—witness this from Playfair, the eloquent and able expounder of Hutton:

> "How often these vicissitudes of decay and renovation have been repeated is not for us to determine; they constitute a series of which, as the author of this theory has remarked, we neither see the beginning nor the end; a circumstance that accords well with what is known concerning other parts of the economy of the world. In the continuation of the different species of animals and vegetables that inhabit the earth, we discern neither a beginning nor an end; in the planetary motions, where geometry has carried the eye so far both into the future and the past, we discover no mark either of the commencement or the termination of the present order."

10. Led by Hutton and Playfair, Lyell taught the doctrine of eternity and uniformity in geology; and to explain plutonic action and underground heat, invented a thermo electric "perpetual" motion on which, in the year 1862, in my paper on the "Secular cooling of the earth,"[1] published in the Transactions of the Royal Society of Edinburgh, I commented as follows:

> "To suppose, as Lyell, adopting the chemical hypothesis, has done,[2] that the substances, combining together, may be again separated electrolytically by thermo-electric currents, due to the heat generated by their combination, and thus the chemical action and its heat continued in an endless cycle, violates the principles of natural philosophy in exactly the same manner, and to the same degree, as to believe that a clock constructed with a self-winding movement may fulfill the expectations of its ingenious inventor by going forever."

It was only by sheer force of reason that geologists have been compelled to think otherwise, and to see that there was a definite beginning, and to look forward to a definite end, of this world as an abode fitted for life.

11. It is curious that English philosophers and writers should not have noticed how Newton treated the astronomical problem. Playfair, in what I have read to you, speaks of the planetary system as being absolutely eternal, and unchangeable; having had no beginning and showing no signs of progress toward an end. He assumes also that

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[1] Reprinted in Thomson and Tait, Treatise on Natural Philosophy, first and second editions, Appendix D (g).

[2] Principles of Geology, Chapter XXXI, edition 1853.

the sun is to go on shining forever, and that the earth is to go on revolving round it forever. He quite overlooked Laplace's nebular theory; and he overlooked Newton's counterblast to the planetary "perpetual motion." Newton, commenting on his own First Law of Motion, says, in his terse Latin, which I will endeavor to translate: "But the greater bodies of planets and comets, moving in spaces less resisting, keep their motions longer." That is a strong counterblast against any idea of eternity in the planetary system.

12. I shall now, without further preface, explain, and I hope briefly, so as not to wear out your patience, some of the arguments that I brought forward between 1862 and 1869, to show strict limitations to the possible age of the earth as an abode fitted for life.

Kant[1] pointed out in the middle of last century, what had not previously been discovered by mathematicians or physical astronomers, that the frictional resistance against tidal currents on the earth's surface must cause a diminution of the earth's rotational speed. This really great discovery in natural philosophy seems to have attracted very little attention—indeed to have passed quite unnoticed—among mathematicians, and astronomers, and naturalists, until about 1840, when the doctrine of energy began to be taken to heart. In 1866 Delaunay suggested that tidal retardation of the earth's rotation was probably the cause of an outstanding acceleration of the moon's mean motion reckoned according to the earth's rotation as a timekeeper found by Adams in 1853 by correcting a calculation of Laplace which had seemed to prove the earth's rotational speed to be uniform.[2] Adopting Delaunay's suggestion as true, Adams, in conjunction with Professor Tait and myself, estimated the diminution of the earth's rotational speed to be such that the earth as a timekeeper, in the course of a century, would get twenty-two seconds behind a thoroughly perfect watch or clock rated to agree with it at the beginning of the century. According to this rate of retardation the earth, 7,200 million years ago, would have been rotating twice as fast as now; and the centrifugal force in the equatorial regions would have been four times as great as its present amount, which is $\frac{1}{289}$ of gravity. At present the radius of the equatorial sea-level exceeds the polar semidiameter by $21\frac{1}{2}$ kilometers,

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[1] In an essay first published in the Königsberg Nachrichten, 1754, Nos. 23, 24; having been written with reference to the offer of a prize by the Berlin Academy of Sciences in 1754. Here is the title-page, in full, as it appears in Vol. VI of Kant's Collected Works, Leipzig, 1839: Untersuchung der Frage: Ob die Erde in ihrer Umdrehung um die Achse, wodurch sie die Abwechselung des Tages und der Nacht hervorbringt, einige Veränderung seit den ersten Zeiten ihres Ursprunges erlitten habe, welches die Ursache davon sei, und woraus man sich ihrer versichern könne? welche von der königlichen Akademie der Wissenschaften zu Berlin zum Preise aufgegeben worden, 1754.

[2] Treatise on Natural Philosophy (Thomson and Tait) § 830, ed. 1, 1867, and later editions; also Popular Lectures and Addresses, Vol. II (Kelvin), Geological Time being a reprint of an article communicated to the Glasgow Geological Society February 27, 1868.

which is, as nearly as most careful calculations in the theory of the earth's figure can tell us, just what the excess of equatorial radius of the surface of the sea all round would be if the whole material of the earth were at present liquid and in equilibrium under the influence of gravity and centrifugal force with the present rotational speed, and one-fourth of what it would be if the rotational speed were twice as great. Hence, if the rotational speed had been twice as great as its present amount when consolidation from approximately the figure of fluid equilibrium took place, and if the solid earth, remaining absolutely rigid, had been gradually slowed down in the course of millions of years to its present speed of rotation, the water would have settled into two circular oceans round the two poles; and the equator, dry all round, would be 64.5 kilometers above the level of the polar sea bottoms. This is on the supposition of absolute rigidity of the earth after primitive consolidation. There would, in reality, have been some degree of yielding to the gravitational tendency to level the great gentle slope up from each pole to equator. But if the earth, at the time of primitive consolidation, had been rotating twice as fast as at present, or even 20 per cent faster than at present, traces of its present figure must have been left in a great preponderance of land, and probably no sea at all, in the equatorial regions. Taking into account all uncertainties, whether in respect to Adams' estimate of the rate of frictional retardation of the earth's rotatory speed, or to the conditions as to rigidity of the earth once consolidated, we may safely conclude that the earth was certainly not solid 5,000 million years ago, and was probably not solid 1,000 million years ago.[1]

13. A second argument for limitation of the earth's age, which was really my own first argument, is founded on the consideration of underground heat. To explain a first rough and ready estimate of it I shall read one short statement. It is from a very short paper that I communicated to the Royal Society of Edinburgh on the 18th of December, 1865, entitled "The Doctrine of Uniformity in Geology briefly refuted."

"The 'Doctrine of Uniformity' in geology, as held by many of the most eminent of British geologists, assumes that the earth's surface and upper crust have been nearly as they are at present, in temperature and other physical qualities, during millions of millions of years. But *the heat which we know, by observation, to be now conducted out of the earth yearly* is so great that if this action had been going on with any approach to uniformity for 20,000 million years the amount of heat lost out of the earth would have been about as much as would heat by 100° C. a quantity of ordinary surface rock of one hundred times the earth's bulk. This would be more than enough to melt a mass of sur-

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[1] "The fact that the continents are arranged along meridians, rather than in an equatorial belt, affords some degree of proof that the consolidation of the earth took place at a time when the diurnal rotation differed but little from its present value. It is probable that the date of consolidation is considerably more recent than a thousand million years ago." Thomson and Tait. Treatise on Natural Philosophy, second edition, 1883, § 830.

face rock equal in bulk to the whole earth. No hypothesis as to chemical action, internal fluidity, effects of pressure at great depth, or possible character of substances in the interior of the earth, possessing the smallest vestige of probability, can justify the supposition that the earth's upper crust has remained nearly as it is, while from the whole or from any part of the earth so great a quantity of heat has been lost."

14. The sixteen words which I have emphasized in reading this statement to you (italics in the reprint) indicate the matter-of-fact foundation for the conclusion asserted. This conclusion suffices to sweep away the whole system of geological and biological speculation demanding an "inconceivably" great vista of past time, or even a few thousand million years, for the history of life on the earth and approximate uniformity of plutonic action throughout that time, which, as we have seen, was very generally prevalent thirty years ago among British geologists and biologists, and which, I must say, some of our chiefs of the present day have not yet abandoned. Witness the presidents of the geological and zoological sections of the British Association at its meetings of 1893 (Nottingham) and of 1896 (Liverpool).

Mr. Teall: Presidential Address to the Geological Section, 1893. "The good old British ship 'Uniformity,' built by Hutton and refitted by Lyell, has won so many glorious victories in the past, and appears still to be in such excellent fighting trim, that I see no reason why she should haul down her colors either to 'Catastrophe' or 'Evolution.' Instead, therefore, of acceding to the request to 'hurry up,' we make a demand for more time."

President Poulton: Presidential Address to the Zoological Section, 1896. "Our argument does not deal with the time required for the origin of life, or for the development of the lowest beings with which we are acquainted from the first formed beings, of which we know nothing. Both these processes may have required an immensity of time; but as we know nothing whatever about them, and have as yet no prospect of acquiring any information, we are compelled to confine ourselves as to much of the process of evolution as we can infer from the structure of living and fossil forms—that is, as regards animals, to the development of the simplest into the most complex Protozoa, the evolution of the Metazoa from the Protozoa, and the branching of the former into its numerous Phyla, with all their classes, orders, families, genera, and species. But we shall find that this is quite enough to necessitate *a very large increase in the time estimated by the geologist.*"

15. In my own short paper, from which I have read you a sentence, the rate at which heat is at the present time lost from the earth by conduction outward through the upper crust, as proved by observations of underground temperature in different parts of the world and by measurement of the thermal conductivity of surface rocks and strata, sufficed to utterly refute the doctrine of uniformity as taught by Hutton, Lyell, and their followers, which was the sole object of that paper.

16. In an earlier communication to the Royal Society of Edinburgh[1]

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[1] On the Secular Cooling of the Earth, Trans. Roy. Soc., Edinburgh, Vol. XXIII, April 28, 1862, reprinted in Thomson and Tait, Vol. III, pages 468–485, and Math. and Phys. Papers, Art. XCIV, pages 295–311.

I had considered the cooling of the earth due to this loss of heat, and by tracing backward the process of cooling had formed a definite estimate of the greatest and least number of million years which can possibly have passed since the surface of the earth was everywhere red-hot. I expressed my conclusion in the following statement:[1]

> "We are very ignorant as to the effects of high temperatures in altering the conductivities and specific heats and melting temperatures of rocks, and as to their latent heat of fusion. We must, therefore, allow very wide limits in such an estimate as I have attempted to make; but I think we may with much probability say that the consolidation can not have taken place less than 20 million years ago, or we should now have more underground heat than we actually have; nor more than 400 million years ago, or we should now have less underground heat than we actually have. That is to say, I conclude that Leibnitz's epoch of emergence of the *consistentior status* [the consolidation of the earth from red-hot or white-hot molten matter] was probably between those dates."

17. During the thirty-five years which have passed since I gave this wide-ranged estimate experimental investigation has supplied much of the knowledge then wanting regarding the thermal properties of rocks to form a closer estimate of the time which has passed since the consolidation of the earth, and we have now good reason for judging that it was more than 20 and less than 40 million years ago, and probably much nearer 20 than 40.

18. Twelve years ago, in a laboratory established by Mr. Clarence King in connection with the United States Geological Survey, a very important series of experimental researches on the physical properties of rocks at high temperatures was commenced by Dr. Carl Barus for the purpose of supplying trustworthy data for geological theory. Mr. Clarence King, in an article published in the American Journal of Science,[2] used data thus supplied to estimate the age of the earth more definitely than was possible for me to do in 1862 with the very meager information then available as to the specific heats, thermal conductivities, and temperatures of fusion of rocks. I had taken 7,000° F. (3,871° C.) as a high estimate of the temperature of melting rock. Even then I might have taken something between 1,000° C. and 2,000° C. as more probable, but I was most anxious not to underestimate the age of the earth, and so I founded my primary calculation on the 7,000° F. for the temperature of melting rock. We know now from the experiments of Carl Barus[3] that diabase—a typical basalt of very primitive character—melts between 1,100° and 1,170° C. and is thoroughly liquid at 1,200°. The correction from 3,871° to 1,200° C., or 1/3.22 of that value, for the temperature of solidification would, with no other change of assumptions, reduce my estimate of 100,000,000 to $1/(3.22)^2$ of its

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[1] On the Secular Cooling of the Earth, Math. and Phys. Papers, Vol. III, § 11 of Art. XCIV.

[2] On the Age of the Earth, Vol. XLV, January, 1893.

[3] Phil. Mag., 1893, first half-year, pages 186, 187, 301–305.

amount, or a little less than 10,000,000 years; but the effect of pressure on the temperature of solidification must also be taken into account, and Mr. Clarence King, after a careful scrutiny of all the data given him for this purpose by Dr. Barus, concludes that without further experimental data "we have no warrant for extending the earth's age beyond 24,000,000 years."

19. By an elaborate piece of mathematical bookkeeping I have worked out the problem of the conduction of heat outward from the earth, with specific heat increasing up to the melting point as found by Rücker and Roberts-Austen and by Barus, but with the conductivity assumed constant; and, by taking into account the augmentation of melting temperature with pressure in a somewhat more complete manner than that adopted by Mr. Clarence King, I am not led to differ much from his estimate of 24,000,000 years. But until we know something more than we know at present as to the probable diminution of thermal conductivity with increasing temperature, which would shorten the time since consolidation, it would be quite inadvisable to publish any closer estimate.

20. All these reckonings of the history of underground heat, the details of which I am sure you do not wish me to put before you at present, are founded on the very sure assumption that the material of our present solid earth all round its surface was at one time a white-hot liquid. The earth is at present losing heat from its surface all round from year to year and century to century. We may dismiss as utterly untenable any supposition such as that a few thousand or a few million years of the present régime in this respect was preceded by a few thousand or a few million years of heating from without. History, guided by science, is bound to find, if possible, an antecedent condition preceding every known state of affairs, whether of dead matter or of living creatures. Unless the earth was created solid and hot out of nothing, the régime of continued loss of heat must have been preceded by molten matter all round the surface.

21. I have given strong reasons[1] for believing that immediately before solidification at the surface the interior was solid close up to the surface, except comparatively small portions of lava or melted rock among the solid masses of denser solid rock which had sunk through the liquid, and possibly a somewhat large space around the center occupied by platinum, gold, silver, lead, copper, iron, and other dense metals still remaining liquid under very high pressure.

22. I wish now to speak to you of depths below the great surface of liquid lava bounding the earth before consolidation, and of mountain heights and ocean depths formed probably a few years after a first emergence of solid rock from the liquid surface (see 24 below), which must have been quickly followed by complete consolidation all round the globe. But I must first ask you to excuse my giving you all my

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[1] On the Secular Cooling of the Earth, Vol. III, Math. and Phys. Papers, §§ 19–33.

depths, heights, and distances in terms of the kilometer, being about six-tenths of that very inconvenient measure the English statute mile, which, with all the other monstrosities of our British metrical system, will, let us hope, not long survive the legislation of our present parliamentary session, destined to honor the sixty years' jubilee of Queen Victoria's reign by legalizing the French metrical system for the United Kingdom.

23. To prepare for considering consolidation at the surface let us go back to a time (probably not more than twenty years earlier as we shall presently see—24), when the solid nucleus was covered with liquid lava to a depth of several kilometers; to fix our ideas let us say 40 kilometers (or 4,000,000 centimeters). At this depth in lava, if of specific gravity 2.5, the hydrostatic pressure is 10 tons weight (10,000,000 gram) per square centimeter, or 10,000 atmospheres approximately. According to the laboratory experiments of Clarence King and Carl Barus[1] on diabase, and the thermodynamic theory[2] of my brother, the late Prof. James Thomson, the melting temperature of diabase is 1,170° C. at ordinary atmospheric pressure, and would be 1,420° under the pressure of 10,000 atmospheres, if the rise of temperature with pressure followed the law of simple proportion up to so high a pressure.

24. The temperature of our 40 kilometers deep lava ocean of melted diabase may therefore be taken as but little less than 1,420° from surface to bottom. Its surface would radiate heat out into space at some such rate as 2 (gram-water) thermal units centigrade per square centimeter per second.[3] Thus, in a year (31,500,000 seconds) 63,000,000 thermal units would be lost per square centimeter from the surface. This is, according to Carl Barus, very nearly equal to the latent heat of fusion abandoned by a million cubic centimeters of melted diabase in solidifying into the glassy condition (pitchstone) which is assumed when the freezing takes place in the course of a few minutes. But, as found by Sir James Hall in his Edinburgh experiments[4] of one hundred years ago, when more than a few minutes is taken for the freezing, the solid formed is not a glass but a heterogeneous crystalline solid of rough fracture; and if a few hours or days, or any longer time, is taken, the solid formed has the well-known rough crystalline structure of basaltic rocks found in all parts of the world. Now, Carl Barus finds that basaltic diabase is 14 per cent denser than melted diabase and 10 per cent

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[1] Phil. Mag., 1893, first half year, page 306.

[2] Trans. Roy. Soc., Edinburgh, January 2, 1849; Cambridge and Dublin Mathematical Journal, November, 1850. Reprinted in Math. and Phys. Papers (Kelvin), Vol. I, page 156.

[3] This is a very rough estimate which I have formed from consideration of J. T. Bottomley's accurate determinations in absolute measure of thermal radiation at temperatures up to 920° C. from platinum wire and from polished and blackened surfaces of various kinds in receivers of air pumps exhausted down to one ten-millionth of the atmospheric pressure. Phil. Trans. Roy. Soc., 1887 and 1893.

[4] Trans. Roy. Soc., Edinburgh.

denser than the glass produced by quick freezing of the liquid. He gives no data, nor do Rücker and Roberts-Austen, who have also experimented on the thermodynamic properties of melted basalt, give any data as to the latent heat evolved in the consolidation of liquid lava into rock of basaltic quality. Guessing it as three times the latent heat of fusion of the diabase pitchstone, I estimate a million cubic centimeters of liquid frozen per square centimeter per centimeter per three years. This would diminish the depth of the liquid at the rate of a million centimeters per three years, or 40 kilometers in twelve years.

25. Let us now consider in what manner this diminution of depth of the lava ocean must have proceeded by the freezing of portions of it; all having been at temperatures very little below the assumed 1,420° melting temperature of the bottom when the depth was 40 kilometers. The loss of heat from the white-hot surface (temperatures from 1,420° to perhaps 1,380° in different parts), at our assumed rate of 2 (gram-water centigrade) thermal units per square centimeter per second produces very rapid cooling of the liquid within a few centimeters of the surface (thermal capacity 0.36 per gram, according to Barus), and in consequence great downward rushes of this cooled liquid and upward of hot liquid, spreading out horizontally in all directions when it reaches the surface. When the sinking liquid gets within perhaps 20 or 10 or 5 kilometers of the bottom, its temperature[1] becomes the freezing point as raised by the increased pressure; or, perhaps more correctly stated, a temperature at which some of its ingredients crystallize out of it. Hence, beginning a few kilometers above the bottom, we have a snow shower of solidified lava or of crystalline flakes, or prisms, or granules of feldspar, mica, hornblende, quartz, and other ingredients; each little crystal gaining mass and falling somewhat faster than the descending liquid around it till it reaches the bottom. This process goes on until, by the heaping of granules and crystals on the bottom, our lava ocean becomes silted up to the surface.

PROBABLE ORIGIN OF GRANITE.

26. Upon the suppositions we have hitherto made we have, at the stage now reached, all round the earth at the same time a red-hot or white-hot surface of solid granules or crystals, with interstices filled by the mother liquor still liquid, but ready to freeze with the slightest cooling. The thermal conductivity of this heterogeneous mass, even before the freezing of the liquid part, is probably nearly the same as that of ordinary solid granite or basalt at a red heat, which is almost certainly[2] somewhat less than the thermal conductivity of igneous rocks at ordinary temperatures. If you wish to see for yourselves how

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[1] The temperature of the sinking liquid rock rises in virtue of the increasing pressure; but much less than does the freezing point of the liquid or of some of its ingredients. (See Kelvin Math. and Phys. Papers, Vol. III, pages 69, 70.)

[2] Proc. R. S., May 30, 1895.

quickly it would cool when wholly solidified, take a large macadamizing stone and heat it red-hot in an ordinary coal fire. Take it out with a pair of tongs and leave it on the hearth or on a stone slab at a distance from the fire, and you will see that in a minute or two, or perhaps in less than a minute, it cools to below red heat.

27. Half an hour[1] after solidification reached up to the surface in any part of the earth the mother liquor among the granules must have frozen to a depth of several centimeters below the surface, and must have cemented together the granules and crystals, and so formed a crust of primeval granite comparatively cool at its upper surface and red-hot to white-hot, but still all solid a little distance down, becoming thicker and thicker very rapidly at first, and after a few weeks certainly cold enough at its outer surface to be touched by the hand.

PROBABLE ORIGIN OF BASALTIC ROCK.

28. We have hitherto left without much consideration the mother liquor among the crystalline granules at all depths below the bottom of our shoaling lava ocean. It was probably this interstitial mother liquor that was destined to form the basaltic rock of future geological time. Whatever be the shapes and sizes of the solid granules when first falling to the bottom, they must have lain in loose heaps with a somewhat large proportion of space occupied by liquid among them; but at considerable distances down in the heap the weight of the superincumbent granules must tend to crush corners and edges into fine powder. If the snow shower had taken place in air, we may feel pretty sure, even with the slight knowledge which we have of the hardnesses of the crystals of feldspar, mica, and hornblende, and of the solid granules of quartz, that at a depth of 10 kilometers enough of matter from the corners and edges of the granules of different kinds would have been crushed into powder of various degrees of fineness to leave an exceedingly small proportionate volume of air in the interstices between the solid fragments. But in reality the effective weight of each solid particle, buoyed as it was by hydrostatic pressure of a liquid less dense than itself by not more than 20 or 15 or 10 per cent, can not have been more than from about one-fifth to one-tenth of its weight in air, and therefore the same degree of crushing effect as would have been experienced at 10 kilometers with air in the interstices, must have been experienced only at depths of from 50 to 100 kilometers below the bottom of the lava ocean.

29. A result of this tremendous crushing together of the solid granules must have been to press out the liquid from among them, as water from a sponge, and cause it to pass upward through the less and less closely packed heaps of solid particles and out into the lava ocean

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[1] Witness the rapid cooling of lava running red-hot or white-hot from a volcano, and after a few days or weeks presenting a black, hard crust, strong enough and cool enough to be walked over with impunity.

above the heap. But, on account of the great resistance against the liquid permeating upward 30 or 40 kilometers through interstices among the solid granules, this process must have gone on somewhat slowly, and during all the time of the shoaling of the lava ocean there may have been a considerable proportion of the whole volume occupied by the mother liquor among the solid granules, down to even as low as 50 or 100 kilometers below the top of the heap or bottom of the ocean at each instant. When consolidation reached the surface, the oozing upward of the mother liquor must have been still going on to some degree. Thus probably for a few years after the first consolidation at the surface, not probably for as long as one hundred years, the settlement of the solid structure by mere mechanical crushing of the corners and edges of solid granules may have continued to cause the oozing upward of mother liquor to the surface through cracks in the first-formed granite crust and through fresh cracks in basaltic crust subsequently formed above it.

LEIBNITZ'S CONSISTENTIOR STATUS.

30. When this oozing everywhere through fine cracks in the surface ceases we have reached Leibnitz's consistentior status; beginning with the surface cool and permanently solid and the temperature increasing to 1,150° C. at 25 or 50 or 100 meters below the surface.

PROBABLE ORIGIN OF CONTINENTS AND OCEAN DEPTHS OF THE EARTH.

31. If the shoaling of the lava ocean up to the surface had taken place everywhere at the same time the whole surface of the consistent solid would be the dead level of the liquid lava all round, just before its depth became zero. On this supposition there seems no possibility that our present day continents could have risen to their present heights and that the surface of the solid in its other parts could have sunk down to their present ocean depths during the twenty or twenty-five million years which may have passed since the consistentior status began, or during any time, however long. Rejecting the extremely improbable hypothesis that the continents were built up of meteoric matter tossed from without upon the already solidified earth, we have no other possible alternative than that they are due to heterogeneousness in different parts of the liquid which constituted the earth before its solidification. The hydrostatic equilibrium of the rotating liquid involved only homogeneousness in respect to density over every level surface (that is to say, surface perpendicular to the resultant of gravity and centrifugal force); it required no homogeneousness in respect to chemical composition. Considering the almost certain truth that the earth was built up of meteorites falling together, we may follow in imagination the whole process of shrinking from gaseous nebula to liquid lava and metals, and solidification of liquid from central regions

outward, without finding any thorough mixing up of different ingredients, coming together from different directions of space—any mixing up so thorough as to produce, even approximately, chemical homogeneousness throughout every layer of equal density. Thus, we have no difficulty in understanding how even the gaseous nebula, which at one time constituted the matter of our present earth, had in itself a heterogeneousness from which followed, by dynamical necessity, Europe, Asia, Africa, America, Australia, Greenland and the Antarctic Continent, and the Pacific, Atlantic, Indian and Arctic ocean depths, as we know them at present.

32. We may reasonably believe that a very slight degree of chemical heterogeneousness could cause great differences in the heaviness of the snow shower of granules and crystals on different regions of the bottom of the lava ocean when still 50 or 100 kilometers deep. Thus we can quite see how it may have shoaled much more rapidly in some places than in others. It is also interesting to consider that the solid granules falling on the bottom may have been largely disturbed, blown as it were into ridges (like rippled sand in the bed of a flowing stream or like dry sand blown into sand hills by wind) by the eastward horizontal motion which liquid descending in the equatorial regions must acquire, relatively to the bottom, in virtue of the earth's rotation. It is indeed not improbable that this influence may have been largely effective in producing the general configuration of the great ridges of the Andes and Rocky Mountains and of the west coasts of Europe and Africa. It seems, however, certain that the main determining cause of the continents and ocean depths was chemical differences, perhaps very slight differences, of the material in different parts of the great lava ocean before consolidation.

33. To fix our ideas let us now suppose that over some great areas, such as those which have since become Asia, Europe, Africa, Australia, and America, the lava ocean had silted up to its surface, while in other parts there still were depths ranging down to 40 kilometers at the deepest. In a very short time, say, about twelve years according to our former estimate (24), the whole lava ocean becomes silted up to its surface.

34. We have not time enough at present to think out all the complicated actions, hydrostatic and thermodynamic, which must accompany and follow after the cooling of the lava ocean surrounding our ideal primitive continent. By a hurried view, however, of the affair we see that in virtue of, let us say, 15 per cent shrinkage by freezing, the level of the liquid must, at its greatest supposed depth, sink six kilometers relatively to the continents; and thus the liquid must recede from them and their bounding coast lines must become enlarged. And just as water runs out of a sand bank, drying when the sea recedes from it on a falling tide, so rivulets of the mother liquor must run out from the edges of the continents into the receding lava ocean. But, unlike

sand banks of incoherent sand permeated by water remaining liquid our uncovered banks of white-hot solid crystals, with interstices full of the mother liquor, will, within a few hours of being uncovered, become crusted into hard rock by cooling at the surface and freezing of the liquor at a temperature somewhat lower than the melting temperatures of any of the crystals previously formed. The thickness of the wholly solidified crust grows at first with extreme rapidity, so that in the course of three or four days it may come to be as much as a meter. At the end of a year it may be as much as 10 meters, with a surface almost or quite cool enough for some kinds of vegetation. In the course of the first few weeks the régime of conduction of heat outward becomes such that the thickness of the wholly solid crust, as long as it remains undisturbed, increases as the square root of the time; so that in one hundred years it becomes ten times, in twenty-five million years five thousand times, as thick as it was at the end of one year. Thus, from one year to twenty-five million years after the time of surface freezing, the thickness of the wholly solid crust might grow from 10 meters to 50 kilometers. These definite numbers are given merely as an illustration, but it is probable they are not enormously far from the truth in respect to what has happened under some of the least disturbed parts of the earth's surface.

We have now reached the condition described above in 30, with only this difference, that instead of the upper surface of the whole solidified crust being level we have, in virtue of the assumptions of 33, 34, inequalities of 6 kilometers from highest to lowest levels, or as much more than 6 kilometers as we please to assume it.

36. There must still be a small but important proportion of mother liquor in the interstices between the closely packed uncooled crystals below the wholly solidified crust. This liquor, differing in chemical constitution from the crystals, has its freezing point somewhat lower—perhaps very largely lower—than the lowest of their melting points. But when we consider the mode of formation (25) of the crystals from the mother liquor we must regard it as still always a solvent ready to dissolve and to redeposit portions of the crystalline matter when slight variations of temperature or pressure tend to cause such actions. Now as the specific gravity of the liquor is less, by something like 15 per cent, than the specific gravity of the solid crystals, it must tend to find its way upward, and will actually do so, however slowly, until stopped by the already solidified impermeable crust, or until itself becomes solid on account of loss of heat by conduction outward. If the upper crust were everywhere continuous and perfectly rigid the mother liquor must inevitably, if sufficient time be given, find its way to the highest places of the lower boundary of the crust, and there form gigantic pockets of liquid lava, tending to break the crust above it and burst up through it.

37. But in reality the upper crust can not have been infinitely strong;

and, judging alone from what we know of properties of matter, we should expect gigantic cracks to occur from time to time in the upper crust, tending to shrink as it cools and prevented from lateral shrinkage by the nonshrinking uncooled solid below it. When any such crack extends downward as far as a pocket of mother liquor underlying the wholly solidified crust, we should have an outburst of trap rock or of volcanic lava just such as have been discovered by geologists in great abundance in many parts of the world. We might even have comparatively small portions of high plateaus of the primitive solid earth raised still higher by outbursts of the mother liquor squeezed out from below them in virtue of the pressure of large surrounding portions of the superincumbent crust. In any such action, due to purely gravitational energy, the center of gravity of all the material concerned must sink, although portions of the matter may be raised to greater heights; but we must leave these large questions of geological dynamics, having been only brought to think of them at all just now by our consideration of the earth antecedent to life upon it.

38. The temperature to which the earth's surface cooled within a few years after the solidification reached it must have been, as it is now, such that the temperature at which heat radiated into space during the night exceeds that received from the sun during the day, by the small difference due to heat conducted outward from within.[1] One year after the freezing of the granitic interstitial mother liquor at the earth's surface in any locality the average temperature at the surface might be warmer by 60° or 80° C. than if the whole interior had the same average temperature as the surface. To fix our ideas, let us suppose at the end of one year the surface to be 80° warmer than it would be with no underground heat; then at the end of one hundred years it would be 8° warmer, and at the end of ten thousand years it

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[1] Suppose, for example, the cooling and thickening of the upper crust has proceeded so far that at the surface, and therefore approximately for a few decimeters below the surface, the rate of augmentation of temperature downward is one degree per centimeter. Taking as a rough average 0.005 c. g. s. as the thermal conductivity of the surface rock, we should have for the heat conducted outward 0.005 of a gram water thermal unit centigrade per square centimeter per second (Kelvin Math. and Phys. Papers, Vol. III, p. 226). Hence if (*ibid.*, p. 223) we take $\frac{1}{8000}$ as the radiational emissivity of rock and atmosphere of gases and watery vapor above it radiating heat into the surrounding vacuous space (ether), we find $8000 \times .005$ or 40° C. as the excess of the mean surface temperature above what it would be if no heat were conducted from within outward. The present augmentation of temperature downward may be taken as 1° C. per 27 meters as a rough average derived from observations in all parts of the earth where underground temperature has been observed. (See British Association Reports from 1868 to 1895. The very valuable work of this committee has been carried on for these twenty-seven years with great skill, perseverance, and success by Professor Everett, and he promises a continuation of his reports from time to time.) This with the same data for conductivity and radiational emissivity as in the preceding calculation makes 40°/2700 or 0.0148° C. per centimeter as the amount by which the average temperature of the earth's surface is at present kept up by underground heat.

would be 0.8 of a degree warmer, and at the end of twenty-five million years it would be 0.016 of a degree warmer than if there were no underground heat.

39. When the surface of the earth was still white-hot liquid all round, at a temperature fallen to about 1,200° C., there must have been hot gases and vapor of water above it in all parts, and possibly vapors of some of the more volatile of the present known terrestrial solids and liquids, such as zinc, mercury, sulphur, phosphorus. The very rapid cooling which followed instantly on the solidification at the surface must have caused a rapid downpour of all the vapors other than water, if any there were; and a little later, rain of water out of the air, as the temperature of the surface cooled from red heat to such moderate temperatures as 40° and 20° and 10° C. above the average due to sun heat and radiation into the ether around the earth. What that primitive atmosphere was, and how much rain of water fell on the earth in the course of the first century after consolidation, we can not tell for certain; but natural history and natural philosophy give us some foundation for endeavors to discover much toward answering the great questions, Whence came our present atmosphere of nitrogen, oxygen, and carbonic acid? Whence came our present oceans and lakes of salt and fresh water? How near an approximation to present conditions was realized in the first hundred centuries after consolidation of the surface?

40. We may consider it as quite certain that nitrogen gas, carbonic-acid gas, and steam escaped abundantly in bubbles from the mother liquor of granite before the primitive consolidation of the surface, and from the mother liquor squeezed up from below in subsequent eruptions of basaltic rock; because all, or nearly all, specimens of granite and basaltic rock which have been tested by chemists in respect to this question[1] have been found to contain, condensed in minute cavities within them, large quantities of nitrogen, carbonic acid, and water. It seems that in no specimen of granite or basalt tested has chemically free oxygen been discovered, while in many chemically free hydrogen has been found, and either native iron or magnetic oxide of iron in those which do not contain hydrogen. From this it might seem probable that there was no free oxygen in the primitive atmosphere, and that if there was free hydrogen it was due to the decomposition of steam by iron or magnetic oxide of iron. Going back to still earlier conditions we might judge that, probably, among the dissolved gases of the hot nebula which became the earth the oxygen all fell into combination with hydrogen and other metallic vapors in the cooling of the nebula, and that although it is known to be the most abundant material of all the chemical elements constituting the earth none of it was left out of combination with other elements to give free oxygen in our primitive atmosphere.

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[1] See, for example, Tilden, Proc. R. S., February 4, 1897, "On the Gases inclosed in Crystalline Rocks and Minerals."

41. It is, however, possible, although it might seem not probable, that there was free oxygen in the primitive atmosphere. With or without free oxygen, however, *but with sunlight*, we may regard the earth as fitted for vegetable life as now known in some species, wherever water moistened the newly solidified rocky crust cooled down below the temperature of 80° or 70° of our present Centigrade thermometric scale, a year or two after solidification of the primitive lava had come up to the surface. The thick, tough velvety coating of living vegetable matter covering the rocky slopes under hot water flowing direct out of the earth at Banff (Canada)[1] lives without help from any ingredients of the atmosphere above it, and takes from the water and from carbonic acid or carbonates, dissolved in it, the hydrogen and carbon needed for its own growth by the dynamical power of sunlight; thus leaving free oxygen in the water to pass ultimately into the air. Similar vegetation is found abundantly on the terraces of the Mammoth Hot Springs and on the beds of the hot water streams flowing from the geysers in the Yellowstone National Park of the United States. This vegetation, consisting of confervæ, all grows under flowing water at various temperatures, some said to be as high as 74° C. We can not doubt but that some such confervæ, if sown or planted in a rivulet or pool of warm water in the early years of the first century of the solid earth's history and if favored with sunlight would have lived, and grown, and multiplied, and would have made a beginning of oxygen in the air if there had been none of it before their contributions. Before the end of the century if sun heat, and sunlight, and rainfall were suitable the whole earth not under water must have been fitted for all kinds of land plants which do not require much or any oxygen in the air, and which can find or make place and soil for their roots on the rocks on which they grow, and the lakes or oceans formed by that time must have been quite fitted for the life of many or all of the species of water plants living on the earth at the present time. The moderate warming, both of land and water, by underground heat, toward the end of the century, would probably be favorable rather than adverse to vegetation, and there can be no doubt but that if abundance of seeds of all species of the present day had been scattered over the earth at that time an important proportion of them would have lived and multiplied by natural selection of the places where they could best thrive.

42. But if there was no free oxygen in the primitive atmosphere or primitive water several thousands, possibly hundreds of thousands, of years must pass before oxygen enough for supporting animal life, as we now know it, was produced. Even if the average activity of vegetable growth on land and in water over the whole earth was, in those early times, as great in respect to evolution of oxygen as that of a Hessian forest, as estimated by Liebig[2] fifty years ago, or of a culti-

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[1] Rocky Mountains Park of Canada, on the Canadian Pacific Railway.

[2] Liebig: "Chemistry in its application to Agriculture and Physiology," English, second edition, edited by Playfair, 1842.

vated English hayfield of the present day, a very improbable supposition, and if there were no decay (eremacausis, or gradual recombination with oxygen) of the plants or of portions such as leaves falling from plants, the rate of evolution of oxygen, reckoned as three times the weight of the wood or the dry hay produced, would be only about 6 tons per English acre per annum, or 1½ tons per square meter per thousand years. At this rate it would take only one thousand five hundred and thirty-three years, and therefore in reality a much longer time would almost certainly be required, to produce the 2.3 tons of oxygen which we have at present resting on every square meter of the earth's surface, land and sea.[1] But probably quite a moderate number of hundred thousand years may have sufficed. It is interesting, at all events, to remark that at any time the total amount of combustible material on the earth, in the form of living plants or their remains left dead, must have been just so much that to burn it all would take either the whole oxygen of the atmosphere or the excess of oxygen in the atmosphere at the time, above that, if any, which there was in the beginning. This we can safely say, because we almost certainly neglect nothing considerable in comparison with what we assert when we say that the free oxygen of the earth's atmosphere is augmented only by vegetation liberating it from carbonic acid and water, in virtue of the power of sunlight, and is diminished only by virtual burning[2] of the vegetable matter thus produced. But it seems improbable that the average of the whole earth—dry land and sea bottom—contains at present coal, or wood, or oil, or fuel of any kind originating in vegetation, to so great an amount as 0.767 of a ton per square meter of surface, which is the amount at the rate of 1 ton of fuel to 3 tons of oxygen that would be required to produce the 2.3 tons of oxygen per square meter of surface which our present atmosphere contains. Hence it seems probable that the earth's primitive atmosphere must have contained free oxygen.

43. Whatever may have been the true history of our atmosphere, it seems certain that if sunlight was ready, the earth was ready, both for vegetable and animal life, if not within a century, at all events within a few hundred centuries after the rocky consolidation of its surface. But was the sun ready? The well-founded dynamical theory of the sun's heat, carefully worked out and discussed by Helmholtz, Newcomb, and myself,[3] says NO if the consolidation of the earth took place as long ago as fifty million years; the solid earth must in that case have waited

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[1] In our present atmosphere, in average conditions of barometer and thermometer, we have, resting on each square meter of the earth's surface 10 tons total weight, of which 7.7 is nitrogen and 2.3 is oxygen.

[2] This "virtual burning" includes eremacausis of decay of vegetable matter, if there is any eremacausis of decay without the intervention of microbes or other animals. It also includes the combination of a portion of the food with inhaled oxygen in the regular animal economy of provision for heat and power.

[3] See Popular Lectures and Addresses, Vol. I, pages 376–429, particularly page 397.

twenty or thirty million years for the sun to be anything nearly as warm as he is at present. If the consolidation of the earth was finished twenty or twenty-five million years ago the sun was probably ready—though probably not then quite so warm as at present, yet warm enough to support some kind of vegetable and animal life on the earth.

44. My task has been rigorously confined to what, humanly speaking, we may call the fortuitous concourse of atoms in the preparation of the earth as an abode fitted for life, except in so far as I have referred to vegetation, as possibly having been concerned in the preparation of an atmosphere suitable for animal life as we now have it. Mathematics and dynamics fail us when we contemplate the earth, fitted for life but lifeless, and try to imagine the commencement of life upon it. This certainly did not take place by any action of chemistry, or electricity, or crystalline grouping of molecules under the influence of force, or by any possible kind of fortuitous concourse of atoms. We must pause, face to face with the mystery and miracle of the creation of living creatures.

RISING OF THE LAND AROUND HUDSON BAY.[1]

By Robert Bell,
Of the Geological Survey of Canada.

In the Provinces of Ontario and Quebec it has been found from actual levelings by Gilbert, Spencer, and Upham that the old shore lines are not perfectly horizontal, but that they slope upward in a northeasterly direction at rates varying in different regions from a few inches to a foot and even 2 feet per mile. If this upward slope were continued in the same direction to the northeastern extremity of Labrador, 1,300 miles from Lake Huron, the increase in the elevation might there amount to 1,000 or 2,000 feet. It is scarcely probable that the differential elevation is constant and regular for such a great distance. Still, it is a fact that well-preserved shore lines are to be seen at great heights in the northern parts of Labrador. In my Geological Survey Report for 1884 I have mentioned ancient beaches at Nachvak, 140 miles south of Hudson Strait, which have an estimated altitude of 1,500 feet above the sea.

The two sides of Hudson Bay present very different physical characters. The eastern is formed mostly of crystalline rocks, and, as a rule, is more or less elevated, with a broken surface sloping somewhat rapidly westward or toward the bay; while the western side is mostly very low and much of it is underlaid by nearly horizontal Silurian and Devonian strata. These low shores are accompanied by shallow water extending far to seaward. The head of James Bay, which forms the southern prolongation of Hudson Bay, is extremely shallow, but the various rivers which flow into it have cut channels through the soft shallows, and by means of these the land may be approached with seagoing vessels. The whole of Hudson Bay may be said to be shallow in proportion to its great area, as the soundings show that it does not average more than 70 fathoms in depth.

The shores of the bay everywhere afford abundant evidence that there has been a comparatively rapid rise in the land and that the elevation is still going on. I have mentioned numerous proofs of this in my various official reports on the geology of these regions from 1875

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[1] Read before the Geological Society of America, Philadelphia, December 27, 1895. Abstract as printed in American Journal of Science, fourth series, Vol. I, March, 1896.

to 1886, and I shall now recall a few of those and give fresh ones in addition, some of which came to my knowledge on a journey to the bay during the past summer. It is well known to those who have paid any attention to the subject that since the establishment of the posts of the Hudson Bay Company in the mouths of the rivers around the bay, two hundred years ago, there has been an ever-increasing difficulty in reaching these establishments from the sea.

On the eastern side the most striking evidence of the rising of the land is afforded by the numerous well-preserved and conspicuous terraces cut in the till and other deposits. Near the sea these may be seen at various heights, up to about 300 feet, but above this elevation the scarcity of soft material out of which terraces might be excavated renders this kind of evidence less apparent than it might otherwise be at higher levels.

On this side of the bay one of the best evidences that the elevation of the land is still going on is furnished by the long lines of driftwood which one sees in many places far above the reach of the highest tides.

The old beaches, on which this wood is plainly seen, occur at various levels up to about 30 feet above high tide, but the remains of rotten wood may be detected in some localities up to nearly 50 feet, above which it has disappeared from the ancient shores by long exposure to the weather. This driftwood consists principally of spruce, but a little white cedar and other kinds, which have been brought down by the rivers, are also mixed with it. The bark having been worn off by the action of the waves while the trunks were still fresh has tended to their preservation. Owing principally to the salt water and the cold climate, wood endures for an incredibly long time in exposed situations in this region wherever it has an opportunity of drying quickly after rain. Some of the wood which may still be seen upon the higher levels may be upward of six hundred years old.

It has been suggested that all this driftwood along hundreds of miles of coast may have been thrown up by some extraordinarily high tide. But there are many reasons why this is quite unlikely. It seems impossible that any modern tide could rise to such a great height and deposit so much wood at different levels all at once and in such even lines, following all the sinuosities of more than one of the raised beaches. The suppositious extraordinary tide would necessarily be of brief duration, and would be accompanied by a tremendous gale blowing upon the coast. This would have the effect of throwing the wood in confused heaps and only into situations favorable for catching it, such as angles of the shore. But instead of this we find it at different levels laid longitudinally all along, as if accumulated by slow degrees with moderate winds from every quarter. The fact that the wood is freshest along the lower lines and becomes progressively more decayed as we ascend, and that finally only traces remain on the higher levels, shows that it must have been stranded from time to time as the land was rising above the sea, and we are forced to adopt this obvious view of the case.

In support of the paroxysmal tide theory, it is related that once during a northern gale the tide was forced as high as the front gate in the palisaded inclosure at Rupert House, near the head of James Bay, and it is added that this would be equivalent to a height of about 30 feet. When at Rupert House last summer, I could hear no authentic account of such an extraordinary rise in the water, and, besides, the gate referred to did not appear to be more than 15 feet above the sea level. But even if such a great rise in the water had once occurred at this place, it would prove nothing in regard to the raised beaches on the long straight shore out on the open sea. Hudson Bay is about 1,000 miles long and its outline is funnel-shaped, with James Bay representing the contracted extremity. Rupert House is situated near the end of this narrow continuation, so that just here we should expect very high water with a spring tide and northern gales driving the sea in from the broad expanse outside and heaping it up at the extremity of the constantly narrowing termination.

The gravel terraces seen at various elevations around the coves and upon the thousands of small islands along the east coast of James Bay are remarkably sharp and well preserved and almost as fresh-looking as if they had been formed but yesterday. They are generally bare of trees or bushes and the yet smooth surface pebbles are only partially covered by lichens. Similar terraces may be seen farther north on this coast and in Hudson Strait, wherever material exists out of which they may be formed. On Marble Island the raised beaches are very plainly visible on account of the whiteness of their smooth quartzite shingle.

On the west side of Hudson Bay the land is generally too low to admit of the relatively higher sea levels of former times having been recorded in the shape of terraces near the present shore line, but if we go back into the woods we shall find unmistakable evidence of the existence of such higher levels at comparatively recent periods. These consist of long, low ridges of drifted materials, such as we see in a fresher state at the present high-tide mark. They are made up of driftwood and other vegetable débris in a completely decayed condition, covered by moss and having trees and shrubs growing upon them. In some places we may still trace the forms of the larger trunks which had been cast ashore by the waves at high tide. Between these ridges and the present shore there is a thick growth of the coniferous forest and the ground is carpeted with moss, over which the tide has never passed. Examples of these low ridges may be seen near the head of tide water at the mouth of Nelson River, at Attawapishkat River, and in places between the latter and Albany River.

To the west and southwest of James Bay the till, covering the nearly flat Silurian and Devonian rocks, is generally overspread by stratified clays. Marine shells are found in these up to an elevation of 400 to

500 feet, but on the eastern side of the bay no fossils have yet been detected at such high levels, owing perhaps to the scarcity there of marine deposits and to the fact that but little search has yet been made for them. In the sandy deposits among the hills about 20 miles south of Cape Wolstenholme I saw abundance of *Saxicava rugosa* and *Tellina grœnlandica*, with smaller numbers of a few other species, at heights varying from the sea level up to about 200 feet; and last summer I found brackish water varieties of a number of the commoner species of our northern marine shells up to 70 feet above the sea in the clay banks along the lower portion of the Noddawai River.

Around the head of James Bay and up its western side the encroachment of the outer lines of the forest upon the wide alluvial flats which extend all along these shores and are constantly broadening toward the sea is good evidence that a rising of the land is now going on. The existing condition in this part of the bay is well described by Mr. A. P. Low in speaking of Agoomski Island. On page 24, J. Geol. Survey Report for 1887, he says:

> "The island closely resembles the adjoining mainland in physical character, being very low and swampy. The shore line above high-water mark is made up of muddy flats covered in part with grasses and sedges, followed farther inland by thick growths of small willows, these in turn giving place to small black spruce and tamarack as slightly higher ground is reached. The line of these trees is often over 2 miles inland from high-water mark, itself a long distance from the sea at low water."

No living mollusks are to be found in James Bay, except perhaps in the northern part, owing probably to the muddy and brackish nature of the water, but abundance of the dead shells of a considerable number of kinds are washed out of the clays forming the present shores. Some of these belong to moderately deep-water species and are well preserved, retaining the epidermis. This, of course, shows a recent elevation of the sea bottom.

Richmond Gulf, on the eastern side, is separated from the main bay by a high bar of stratified rocks, which strike with its length and dip westward or toward the open sea. This bar is cut through by several gaps, all resembling one another, except in their heights above the sea, and all bearing evidence of their having been well-worn channels of communication at more or less remote times according to the greater or less elevation of their beds above the sea. Only one narrow passage now remains open or low enough to admit the water, but two others are as yet only slightly raised above the tides.

Some of the aboriginal geographical names around the head of James Bay are significant of considerable changes in the topography since these shores became inhabited by the natives who still occupy them. The large peninsula between Hannah and Rupert bays is called Ministik-oo-watum, which means wooded island with a cove or hole in it, ministik being the Cree for a wooded island and watum for a cove or hole. The heads of the channels, which now run in behind the present peninsula from the opposite sides, are separated by a strip of low ground, some 10 miles long, covered by bushes. Midway across this strip the elevation is estimated to be about 15 feet above high tide. The most prominent point on the coast between Moose Factory and Fort Albany is now called "Cockispenny" by the whites, but the Cree name is Ka-ka-ki-sippin-a-wayo Minis, or Island where the Crow-duck (Cormorant) lays eggs. Since this island became connected with the mainland, bushes have taken the place of the grasses and sedges which first grew upon the low ground between them, and the former are constantly acquiring a stronger growth. Many years ago the winter trail of the coast passed over the neck of this peninsula, but now it has become necessary to go outside of it, because the bushes have grown so large that they catch the snow which, in such situations, remains too soft for dog teams and snow shoers.

The salt marshes along the west coast of James Bay and also in the vicinity of York Factory, which used to attract vast numbers of wild geese and ducks, have been gradually drying up, much to the inconvenience of the Hudson Bay Company's people, who depended largely upon them for food.

The character of the lower portions of such rivers as the Moose, Albany, and Attawapishkat shows a recession of the sea. This is particularly observable in the lower 30 miles of the Moose, where very long and narrow or ribbon-like islands run parallel to one another for many miles. The process of their formation appears to have been a constant drawing out of their lower extremities as the sea receded from them, just as the lowest islands of the present day are growing.

On the east main coast, where the land is comparatively high, the grade of the rivers is rapid as they approach the bay, and in some of them, as the Nastapoka and the Langlands, there are perpendicular falls of about 100 feet almost directly into the sea. This condition indicates recent elevation.

One of the best evidences of the modern rising of the land is to be found in the beach dwellings of the Eskimo, which may be seen at all elevations up to about 70 feet. In summer these people generally camp on the shore, and their favorite locations are at the mouths of small streams into which the sea trout run at high tide. Here they construct weirs of stones, which impound the fish when the tide retires. On Outer Digges Island, I have found these fish traps and the rings of stones and other structures marking their old camping places up to a height estimated at 70 feet.

Among the historical evidences bearing upon this question since the advent of the white man may be mentioned the fact that in 1610 Henry Hudson, the navigator, wintered in a bay full of islands on the east coast south of latitude 53°. None of the bays in this region would now be possible for this purpose, showing that a considerable change in the level of the sea has taken place in less than three hundred years.

In 1674 Charles Bayley, then local governor for the Hudson Bay Company, sailed through in a sloop between Agoomski Island and the main west shore of James Bay. It would now be impossible to pass here in a seagoing vessel of any kind. In 1886 I found it difficult to get through in bark canoes, drawing only a few inches of water. The shoaling is not due to a silting up, since the almost dry bottom consists of a level surface of till with bowlders scattered thickly over it.

From 1675 to 1685 the Hudson Bay Company's establishment in the mouth of Moose River was upon Hayes Island, which, it is to be presumed, was selected for convenience of landing goods from their vessels and shipping out their returns. This island is now unapproachable except by canoes and small boats. For more than two hundred years the factory[1] has stood upon Moose Island, the next below Hayes Island. The annual ship from England anchors in the channel cut through the sands off the mouth of Moose River. On account of the risk of rough water it is necessary to discharge the cargo by schooners. Within the memory of living men these schooners could ascend to a wharf built opposite the large storehouse of the factory. But for many years the same schooners have been unable to ascend all the way, and the cargo requires to be transferred into scows, which complete the trip to the wharf; and the distance to which the schooners can ascend is constantly diminishing. In the beginning of the present century Princess Island, a narrow, bushy strip immediately in front of the factory, was separated by a channel with a good depth of water at the lowest tides. Last autumn I saw it quite dry on several occasions during ebb tide. It is well known to everyone who has lived at this post in the present generation that every now and then a new "lump" will appear in the bed of the river and become permanent, growing higher and higher, eventually escaping submergence at most tides and at length becoming covered with grass and then with bushes. Some islands which were covered only with bushes forty or fifty years ago now support a growth of young trees. The small one on the west side of Middleboro, below Moose Island, is an example of this, and the appearance of the trees upon it is within the memory of Mr. Broughton, the gentleman now in charge of Moose Factory. Middleton Island, between the mouths of Rupert and Noddawai rivers, lies close to the east shore of Rupert Bay. Up to a few years ago canoes and boats could pass at high tide through the long, narrow, grassy channel behind this island, but last autumn I found it impossible to do so with my canoes, and we were obliged, at great inconvenience, to go round outside.

Two hundred years ago the ships of the Hudson Bay Company appear to have had no difficulty in entering the mouths of various rivers on the Eastmain Coast, which can not now be used as harbors. In old times the principal post of the company on that coast was in the mouth of Eastmain River, which had no doubt been chosen because it afforded

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[1] Factory, a residence of a factor or agent.

a good harbor. It is only a few years since the mouth of Little Whale River, several hundred miles farther north, had to be abandoned as a harbor on account of the increasing shallowness of the water.

At York Factory there is a "ship hole" in the channel of Hayes River, directly in front of the storehouse. The seagoing vessels of light draft employed in the Hudson Bay Company's trade have been accustomed to anchor in this hole, and formerly they remained afloat at all stages of the tide, but of late years vessels drawing even less than those of former times have begun to "take the ground" at low water. In objection to the belief that the land is rising it may be said this may be due to a silting up of the hole, but on examining the material brought up on the flukes of the anchors I found it to consist of light-colored stiff bowlder clay or till.

In 1782, after the French Admiral Lepeyrouse had destroyed Fort Prince of Wales at the mouth of Churchill River, he landed with cannon on the southeast side of Nelson River, and, hauling them across the point between it and Hayes River, captured York Factory. Two ships belonging to the Hudson Bay Company which were then lying in Hayes River, laden with valuable cargoes, escaped under cover of the darkness of the following night and got safely to England. At the present time it is only possible for a seagoing vessel to get out from this river at the top of high water with favorable wind and careful piloting in daylight. To say nothing of the difficulty caused by the darkness, it is unlikely that all the other conditions now necessary to enable a vessel to leave the river conspired to aid the escape of these ships. It is much more reasonable to believe that the water was deeper then than it is now. The landing of Lepeyrouse with his guns on the shore of Nelson River abreast of York Factory was a feat the like of which could not be accomplished at the present day, owing to the extreme shallowness of the water.

The present Fort Churchill, or "New Fort," as it is still called, was built in 1782 on the west side of the river, about 4½ miles above Fort Prince of Wales, as soon as the French had retired after destroying the latter establishment. The residents now suffer much inconvenience on account of the continued shoaling of the water, and they have been obliged to lengthen out their "launch" or long landing trestle from time to time in order to be able to reach the outer end of it with their coast boats.

Off the western side of the lagoon, within the mouth of Churchill River, is Sloops Cove, a small elliptical pond connecting with the lagoon by a very narrow entrance, through which the water barely passes at high tide. On the arkose rocks beside this little cove many inscriptions have been cut and some ring bolts have been fastened for mooring vessels, all of which indicate that the cove was used for wintering ships in old times. Indeed, it is known that the *Furnace* and the *Discovery*, two small ships commanded by Captain Middleton, passed the winter of 1741–42 in this cove. I have examined the place on various occasions and have copied most of the sketches and inscriptions on the rocks, and it always appeared to me that the conditions which we observe indicate a rise in the land since the last ship wintered there. At the present time the tide does not rise high enough to allow of the passage into it of crafts larger than ordinary rowboats. No seagoing vessel could now enter it, which would indicate an elevation nearly equal to the draft of the ships formerly frequenting it. It would be a boon to the agents of the Hudson Bay Company at Churchill if they could now winter their small schooner in this cove instead of being obliged to send her every autumn to winter at York Factory. The captain who commands her happens to be the person now in charge of the company's post at Churchill, and both he and his crew are obliged to walk back 150 miles through the mud from York Factory after leaving their vessel there in the autumn, and to walk the same distance again to bring her back in the spring. Mr. J. B. Tyrrell visited Sloops Cove in the autumn of 1893, and in a paper published in the Geological Magazine for August, 1894, says he thinks the land is here in a state of equilibrium. Two inscriptions which he saw on the rocks, namely, "May 25 and May 27, 1753," were about 7 feet above the present high tide, and he thinks these were cut by men standing on the ice. This, however, does not prove much, for the men were quite as likely to have sat as stood while engraving these inscriptions. As the tide still enters the cove and keeps it full of water the average relative level of its ice to the rocks surrounding it may not have differed much from what it is now. When I visited Fort Prince of Wales in 1879 oak planks brought from England while the fort was still occupied, as well as timbers of native wood, all charred by Lepeyrouse's fire, were found stranded far out of reach of the present tides and still in perfect preservation. On the occasion referred to I met at the "New Fort" children of some of the people who were living at the "Old Fort" when it was captured by the French, and from them some information could be obtained as to the conditions at that time. We have, besides, the description and illustrations in the book by Samuel Hearne, who was then in charge of the place. Any light which these accounts may throw on the state of matters then as compared with the present time points in the direction of some elevation having taken place.

Among the photographs which I took around Fort Prince of Wales in 1879 is one which shows strips of dry land grasses alternating with little parallel ridges of gravel thrown up by the waves and now above the highest tide mark, but below the level of the spot which was pointed out to me as the landing place of Lepeyrouse. The ground on which the fort stands was an island during high tide at the time the place was occupied, and a bridge was thrown across the narrowest part of the little separating channel to connect the island with the mainland. This channel is now entirely dry.

If anything further were wanting to show that an elevation of the land is now going on in this region we have some direct personal evidence in the lifetime of the witness himself in support of the facts already cited. About twenty years ago a very aged Indian, who was said to have "seen more than a hundred winters," and who was quietly passing the last years of his extraordinarily long life at Norway House, told me in presence of the factor, Mr. Roderick Ross, and the other gentlemen of that establishment that he had, when a boy, witnessed the landing of Lepeyrouse and the destruction of Fort Prince of Wales. He gave graphic details of every circumstance, which agreed perfectly with Lepeyrouse's own account, and he answered all my questions on other points entirely satisfactorily and without a moment's hesitation. Among other things, he mentioned that the spot where the Frenchmen's boats landed was quite close to that portion of the western wall which they undermined and blew up with gunpowder. He said that when all was ready they laid a "rope" (train) of gunpowder across the beach and, setting fire to the end of it, ran off to a safe distance to witness the effect. It is now a considerable distance from this spot to the nearest point of water at high tide.

The proofs of the rising of the land around Hudson Bay in post-glacial times would be admitted by any geologist, and the question of the continuance of the movement at the present time is, I think, answered in the affirmative by the actual general shoaling of the water which is going on, and the encroachment of the land on all sides, some proofs of which have been given in the foregoing pages. All the facts which have been mentioned (and many more might be added) point in the same direction, while there appears to be no evidence of a contrary character. The officers of the Hudson Bay Company are an intelligent set of men, and their universal opinion, based upon life-times of observation, is that the land all around the bay is rising. The following is part of a letter recently received from Mr. Joseph Fortescue, lately a chief factor in the Hudson Bay Company, in answer to my request for his opinion on this subject:

> "Regarding the rising of the shores of Hudson Bay I have no doubt whatever. When I was at York Factory I heard several Indians say that the sea or tide had retired 2 miles from places they remembered when they were young, and my own observations during twenty years there would lead me to entertain the same opinion. When I revisited Moose Factory, after nearly forty years' absence, I found a great change in the appearance of the coast and river. Channels which were navigable at all times of the tide formerly could now only be used at high water."

CRATER LAKE, OREGON.[1]

By J. S. DILLER,
United States Geological Survey.

Of lakes in the United States there are many and in great variety, but of crater lakes there is but one of great importance. Crater lakes are lakes which occupy the craters of volcanoes or pits (calderas) of volcanic origin. They are most abundant in Italy and Central America, regions in which volcanoes are still active; and they occur also in France, Germany, India, Hawaii, and other parts of the world where volcanism has played an important rôle in its geologic history.

The one in the United States belongs to the great volcanic field of the northwest, but it occurs in so secluded a spot among high mountains that it is almost unknown to tourists and men of science who are especially interested in such natural wonders. Crater Lake of southern Oregon lies in the very heart of the Cascade Range, and, while it is especially attractive to the geologist on account of its remarkable geologic history, it is equally inviting to the tourist and others in search of health and pleasure by communion with the beautiful and sublime in nature.

According to W. G. Steel[2] the lake was first seen by white men in 1853. It had long previously been known to the Indians, whose legends have contributed a name, Llao Rock, to one of the prominences of its rim. They regarded the lake with awe as an abode of the Great Spirit. Prospectors were the earliest explorers of the lake.[3] The first travelers of note who visited the lake were Lord Maxwell and Mr. Bentley, who, in 1872, with Capt. O. C. Applegate, of Modoc war fame, and three others, made a boat trip along its borders and named several of the prominences on the rim after members of the party.[4] Mrs. F. F.

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[1] Published by permission of the Director of the United States Geological Survey. Reprinted from the National Geographic Magazine, February, 1897, Vol. VIII, pages 33–48.

[2] The Mountains of Oregon, by W. G. Steel, 1890, page 13.

[3] The Discovery and Early History of Crater Lake, by M. W. Gorman, Mazama, Vol. I, No. 2, Crater Lake Number, 1897, pages 159. This number contains much valuable information concerning Crater Lake in addition to that referred to.

[4] The names Watchman, Glacier, Llao, and Vidae, which appear on the map of the lake, have recently been adopted by the United States Board on Geographic Names.

Victor saw the lake in 1873, and briefly describes it in "Atlantis Arisen."[1] The same year Mr. S. A. Clarke gave an interesting account of the lake in the December number of the Overland Monthly.

The first Geological Survery party visited the lake in 1883, when Everett Hayden and the writer, after spending several days in examining the rim, tumbled logs over the cliffs to the water's edge, lashed them together with ropes to make a raft, and paddled over to the island. In 1886, under the direction of Capt. (now Maj.) C. E. Dutton, many soundings of the lake were made by W. G. Steel, and a topographic map of the vicinity was prepared by Mark B. Kerr and Eugene Ricksecker. Dutton was the first to discover the more novel and salient features in the geological history of the lake, of which he has given, for his entertaining pen, an all too brief account.[2]

Under the inspiration of the "Mazamas," a society of mountain climbers at Portland, Oregon,[3] a more extended study of the lake has just been made by Government parties from the Department of Agriculture, the Fish Commission, and the Geological Survey.

Crater Lake is deeply set in the summit of the Cascade Range, about 65 miles north of the California line. As yet it may be reached only by private conveyance over about 80 miles of mountain roads from Ashland, Medford, or Gold Hill, on the Southern Pacific Railroad, in the Rogue River Valley of southern Oregon (see fig. 1). This valley marks the line between the Klamath Mountains of the Coast Range on the west and the Cascade Range on the east. The journey from the railroad to Crater Lake affords a good opportunity to observe some of the most important features of this great pile of lavas. The Cascade Range in southern Oregon is a broad irregular platform, terminating rather abruptly in places upon its borders, especially to the westward, where the underlying Cretaceous and Tertiary sediments come to the surface. It is surmounted by volcanic cones and coulees, which are generally smooth, but sometimes rough and rugged. The cones vary greatly in size and are distributed without regularity. Each has been an active volcano. The fragments blown out by violent eruption have fallen about the volcanic orifice from which they issued and built up cinder cones. From their bases have spread streams of lava (coulees), raising the general level of the country between the cones. From some vents by many eruptions, both explosive and effusive, large cones, like Pitt, Shasta, and Hood have been built up. Were we to examine their internal structure, exposed in the walls of the canyons carved in their slopes, we should find them composed of overlapping layers of lava and volcanic conglomerate, a structure which is well illustrated in the rim of Crater Lake.

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[1] "Atlantis Arisen," by Mrs. Francis Fuller Victor, page 179.

[2] Science, Vol. VII, 1886, pages 179–182, and Eighth Annual Report of the United States Geological Survey, pages 156–159.

[3] The National Geographic Magazine, Vol. VIII, 1897, page 58.

Contour Interval 500 feet.

FIG. 1.—MAP SHOWING ROUTES TO CRATER LAKE FROM ASHLAND AND MEDFORD ON THE OREGON AND CALIFORNIA LINE OF THE SOUTHERN PACIFIC RAILROAD.

Reduced from United States Geological Survey Ashland Sheet, Oregon.

The journey from Ashland by the Dead Indian road crosses the range where the average altitude is less than 5,000 feet. The road passes within a few miles of Mount Pitt and skirts Pelican Bay of Klamath Lake, famous for its fishing. After following northward for some 20 miles along the eastern foot of the range, it ascends the eastern slope, along the castled canyon of Anna Creek to the rim of Crater Lake.

From Medford or Gold Hill, the trip is a trifle shorter by the Rogue River road. It affords some fine views of the canyons and rapids of that turbulent stream and of the high falls, where it receives its affluents. Striking features along both roads, within 20 miles of the lake, are the plains developed upon a great mass of detritus filling the valleys. Across these plains Anna Creek and Rogue River have carved deep, narrow canyons with finely sculptured walls, which the roads follow for some distance.

Approaching the lake from any side, the observer sees, as in the distant part of figure 2, a broad cluster of gentle peaks rising about a thousand feet above the general crest of the range on which they stand, but not until after he has left the main road, 3 miles from the lake, does he begin to feel the steepness of the ascent. The way winds over a large moraine littered with lava bowlders and well studded with firs. Arriving at the crest, the lake in all its majestic beauty, as it appears in figure 3, comes suddenly upon the scene, and is profoundly impressive. Descending the wooded slope a short distance within the rim to Victor Rock, an excellent general view of the lake is obtained. Upon the left is the western border of the lake (fig. 4), and upon the right its southern border (fig. 5). The eye beholds 20 miles of unbroken cliffs ranging from over 500 to nearly 2,000 feet in height, encircling a deep blue sheet of placid water, in which the mirrored walls vie with the originals in brilliancy and greatly enhance the depth of the prospect.

The first point to fix our fascinated gaze is Wizard Island, lying nearly 2 miles away, near the western margin of the lake. Its irregular western edge and the steep but symmetrical truncated cone in the eastern portion are very suggestive of volcanic origin. We can not, however, indulge our first impulse to go at once to the island, for the various features of the rim are of greater importance in unraveling the earlier stages of its geological history.

The outer and inner slopes of the rim are in strong contrast; while the one is gentle, ranging in general from 10° to 15°, the other is abrupt and full of cliffs. This difference is well expressed by the contour map in figure 6. The vertical interval of the contours is 200 feet. Upon the inner slope the contours are crowded close together to show a slope so steep that one needs to travel but a little way to descend 200 feet, while upon the outer slope the contours are so far apart that to descend 200 feet one needs to travel a considerable portion of a mile. The outer slope at all points is away from the lake, and as the rim rises at least 1,000 feet above the general summit of the range, it is evidently the basal portion of a great hollow cone in which the lake is contained.

In addition to the strong contrast between the outer and inner slopes of the rim the map shows the occurrence of a number of small cones upon the outer slope of the great cone. These adnate cones are of peculiar significance when we come to consider the volcanic rocks of which the region is composed. The rim is ribbed by ridges and spurs radiating from the lake, and the head of each spur is marked by a prominence on the crest of the rim. The variation in the altitude of the rim crest is 1,460 feet (from 6,759 to 8,228) with seven points rising above 8,000 feet. The crest generally is passable, so that a pedestrian may follow it continuously around the lake, with the exception of short intervals about the notches in the southern side. At many points the best going is on the inner side of the crest, where the open slope, generally well marked with deer trails over beds of pumice, affords an unobstructed view of the lake.

Reference has already been made to the glacial phenomena of the outer slope of the rim. There are scattered bowlders upon the surface, and also in piles of glacial moraine (fig. 7) which contain besides bowlders much gravel and sand. Such glacial drift is spread far and wide over the southern and western portion of the rim, extending down the watercourses in some cases for miles to broad plains through which the present streams have carved the deep and picturesque canyons already observed on the ascent. At many points the lavas are well rounded, smoothed, and striated by glacial action. This is true of the ridges as well as of the valleys, and the distribution of these marks is coextensive with that of the glacial detritus.

A feature that is particularly impressive to the geologist making a trip around the lake on the rim crest is the general occurrence of polished and striated rocks, in place on the very brow of the cliff overlooking the lake. The best displays are along the crest for 3 miles northwest of Victor Rock, but they occur also on the slopes of Llao Rock, Round Top, Kerr Notch, and Eagle crags, thus completing the circuit of the lake. On the adjacent slope toward the lake the same rocks present rough fractured surfaces, showing no striæ. The glaciation of the rim is a feature of its outer slope only, but as shown in figure 8, it reaches up to the very crest. The glaciers armed with stones in their lower parts, that striated the crown of the rim, must have come down from above, and it is evident that the topographic conditions of to-day afford no such source of supply. The formation of glaciers requires an elevation extending above the snow line to afford a gathering ground for the snow that it may accumulate, and under the influence of gravity descend to develop glaciers lower down on the mountain slopes. During the glacial period Crater Lake did not exist. Its site must then have been occupied by a mountain to furnish the conditions necessary for the extensive glaciation of the rim, and the magnitude of the glacial phenomena indicates that the peak was a large one, rivaling, apparently, the highest peaks of the range.

The Mazamas held a meeting in August, 1896, at Crater Lake in connection with the Crater Lake clubs of Medford, Ashland, and Klamath Falls, of the same State. Recognizing that the high mountain which once occupied the place of the lake was nameless, they christened it, with appropriate ceremonies, Mount Mazama. The rim of the lake is a remnant of Mount Mazama, but when the name is used in this paper reference is intended more especially to that part which has disappeared.

The inner slope of the rim, so well in view from Victor Rock, although precipitous, is not a continuous cliff. It is made up of many cliffs, whose horizontal extent is generally much greater than the vertical. The cliffs are in ledges, and sometimes the whole slope from crest to shore is one great cliff, not absolutely vertical, it is true, but yet at so high an angle as to make it far beyond the possibility of climbing. Dutton Cliff on the southern and Llao Rock on the northern borders of the lake are the greatest cliffs of the rim. Besides cliffs, the other elements of the inner slope are forests and talus, and these make it possible at a few points to approach the lake, not with great ease, but yet, care being taken, with little danger. Southwest of the lake the inner slope, clearly seen from Victor Rock, is pretty well wooded, and from near the end of the road, just east of Victor Rock, a steep trail descends to the water. Where fresh talus slopes prevail there are no trees, and the loose material maintains the steepest slope possible without sliding. Such slopes are well displayed along the western shore opposite the island and near the northeast corner of the lake under the palisades, illustrated in figure 10. At this point the rim is only 520 feet high, and a long slide, called from its shape the Wineglass, reaches from crest to shore.

The best views of the rim are obtained from a boat on the lake, which affords an opportunity to examine in detail the position and structure of the cliffs. They are composed wholly of volcanic conglomerate and streams of lava arranged in layers that dip into the rim and away from the lake on all sides. Both forms of volcanic material are well exposed on the trail descending the inner slope, and, although most of the cliffs are of lava, many are of conglomerate.

On arriving at the water's edge the observer is struck with the fact that there is no beach. The steep slopes above the surface of the lake continue beneath its waters to great depths. Here and there upon the shore, where a rill descends from a melting snow bank near the crest, a small delta deposit makes a little shallow, turning the deep-blue water to pale green.

As the boat skirts the western shore and passes toward Llao Rock, the layered structure of the rim is evident. This feature is best illustrated in figure 4. On the whole, the lava streams predominate. although there is much conglomerate. Of all the flows exposed upon the inner slope, that of Llao Rock is most prominent and interesting. In the middle it is over 1,200 feet thick, and fills an ancient valley down the outer slope of the rim. Upon either side it tapers to a thin edge against the upper slope of the valley. Figure 11, from a color sketch of the photograph from which figure 3 was prepared, shows the Llao Rock flow distinctly. To the lake it presents a sheer cliff—that is, it is abruptly cut off—and one wonders how much farther it may have extended in that direction. Beneath the rock the outline of the valley in cross section is evident, and it rests upon many layers of older lavas, forming the rim down to the water's edge. The direction of flow in this great lava stream forces us to believe that it was erupted from a large volcano which once stood upon the site of the lake. Every layer of lava in the rim is a coulee, dipping away from the lake. This is especially well shown in the canyon of Sun Creek, cut in its outer slope. The sections of these radiating flows exposed upon the inner slope of the rim all tell the same story as to their source. By projecting the lavas in their course toward a common center we can reconstruct in fancy the great volcano, Mount Mazama, which once occupied the place of the lake, and, like Shasta or Rainier, formed a great landmark of the region. Proceeding eastward from Llao Rock the rim loses somewhat in height, and at the head of Cleetwood Cove one sees the remarkable spectacle of a lava stream descending the inner slope of the rim. It is the only one that has behaved in this way, and its action throws much light upon the disappearance of Mount Mazama.

The Palisades are less than 600 feet in elevation above the lake, and are composed almost wholly of one great flow. The streams of lava extending northeast from this portion of the rim are broad and much younger in appearance than those forming the great cliffs south of the lake, where the flows are thinner and more numerous.

Round Top is a dome-shaped hill over the eastern end of the Palisades, and is made up chiefly of the lava stream that formed the Palisades, overlain by two sheets of pumice separated by a layer of rhyolite. The upper surface of the Palisade flow, where best exposed upon the lakeward slope of Round Top, bears glacial striæ, that extend beneath the layers of pumice and rhyolite of later eruption from Mount Mazama. It is evident from this relation that Mount Mazama was an active volcano during the glacial period. The occurrence of eruptions from a snow-capped volcano must necessarily produce great floods, and these conditions may account in some measure at least for the detritus-filled valleys of the streams rising on the rim of Crater Lake.

Returning from this glacial digression to the boat trip on the lake, it is observed upon the eastern side of the lake that Red Cloud Cliff is rendered beautiful by the pinnacles of reddish tuff near the summit, where it is capped by a great, dark flow of rhyolite, filling a valley in the older rim and extending far to the northeast. Here the springs begin to gush from the inner slope and cascade their foaming rills to the lake. They recur at Sentinel Rock, Dutton Cliff, and especially under Eagle Crag, as well as farther westward. Their sources in many cases can be seen in the banks of snow above, but in others they gush forth as real springs, whose water must find its way in from the snow upon the outer slope.

The boldest portion of the rim, excepting perhaps Llao Rock, is Dutton Cliff, which is made more impressive by the deep U-shape notches on either side and the Phantom Ship at its foot (fig. 12). The notches mark points where the canyons of Sun and Sand creeks pass through the rim to the cliff overlooking the lake. In figure 13, which shows the same view as figure 12, but in the opposite direction, the notch at the head of Sun Creek Canyon is well illustrated. These canyons, due to erosion on lines of drainage, belong to the period when the topographic conditions in that region were quite unlike those of today. They were carved out by streams of ice and water descending from a point over the lake, and their presence, ending as they do in the air hundreds of feet above the present water level, affords strong evidence in favor of the former reality of Mount Mazama.

The Phanton Ship (fig. 14) is a craggy little islet near the border of the lake under Dutton Cliff. Its rugged hull, with rocks towering like the masts of a ship, suggests the name, and, phantom-like, it disappears when viewed in certain lights from the western rim. Standing in line with an arête that descends from an angle of the cliff, it possibly marks a continuation of the sharp spur beneath the water, or perhaps, but much less likely, it is a block slid down from the cliff. Whatever its history, it attracts everyone by its beauty and winsomeness.

At times of volcanic eruption the lava rises within the volcano until it either overflows the crater at the top or, by the great pressure of the column, bursts open the sides of the volcano and escapes through the fissure to the surface. In the latter case, as the molten material cools, the fissure becomes filled with solid lava and forms a dike. The best example of this sort about Crater Lake appears along the inner slope directly north of Wizard Island, and is locally known as the Devil's Backbone. It is shown in figures 3 and 11 across the left end of Wizard Island. This dike rock, standing on edge, varies from 5 to 25 feet in thickness and cuts the rim from water to crest. Dikes are most numerous in the older portion of the rim under Llao Rock. They do not cut up through Llao Rock and are clearly older than the lava of which that rock is formed. Dikes occur at intervals all around the lake and radiate from it, suggesting that the central volcanic vent from which they issued must have been Mount Mazama.

There is another important feature concerning the kinds of volcanic rocks and their order of eruption and distribution about the rim of Crater Lake that is of much interest to the geologist. All the older lavas comprising the inner slope of the rim, especially toward the water's edge, are andesites. The newer ones, forming the top of the rim in Llao Rock, Round Top, and the Rugged Crest about the head of Cleetwood Cove as well as at Cloud Cap, are rhyolites. Other later flows, all of which escaped from the smaller adnate cones upon the outer slope of the rim, are basalts. The eruptions began with lavas containing a medium amount of silica (andesites), and after long-continued activity lavas both richer (rhyolites) and poorer (basalts) in silica follow, giving a completeness to the products of this great volcanic center that make it an interesting field of study. Furthermore, the remarkable opportunity afforded by the dissected volcano for the examination of its structure and succession of lavas is unsurpassed. It should be stated, before dismissing the kinds of lava, that there are some rhyolites in the Sun Creek Canyon south of the lake that appear to be older than those upon the north side, and that the final lava of the region on Wizard Island is andesite.

The glaciation and structure of the rim clearly establish the former existence of Mount Mazama, but there may well be doubt as to its exact form and size. Judging from the fact that Mount Shasta and the rim of Crater Lake have the same diameter at an altitude of 8,000 feet, and that their lavas are similar, it may with some reason be inferred that Mount Mazama and Mount Shasta were nearly of equal height. The slopes of Mount Shasta may be somewhat steeper than those of the rim of Crater Lake at an equal altitude, but the glaciation of the rim is such as to require a large peak for its source.

In figure 9 is given a section of Crater Lake and its rim, with the probable outline of Mount Mazama. Wonderful as the lake, encircled by cliffs, may be, it serves but to conceal in part the greatest wonder—that is, the enormous pit or caldera which is half filled by the lake. The caldera is 4,000 feet deep. An impressive illustration of it is seen in figure 15 which was prepared from a photograph of a model of Crater Lake now in the United States National Museum. The water surface is represented by glass, so that one may see through to the bottom and get the full impression of the depth of this tremendous hole in the ground. It extends from the top of the rim, which is the very summit of the Cascade Range, halfway down to the sea level, and nearly a square mile of its bottom is below the level of Upper Klamath Lake at the eastern foot of the range. The volume of the caldera is nearly a dozen cubic miles, and if we add the volume of the lost Mount Mazama, that amount would be increased by at least one-half. How was it possible to remove so large a mass and in the process develop so great a depression?

The caldera is completely inclosed, so that it can not be regarded as an effect of erosion. The volcanic origin of everything about the lake would suggest in a general way that this great revolution must have been wrought by volcanism, either blown out by a great volcanic explosion or swallowed up by an equally great engulfment. It is well known that pits have been produced by volcanic explosions, and some of them are occupied by lakes of the kind usually called crater lakes. Depressions produced in this way, however, are, with rare exceptions, surrounded by rims composed of the fragmental material blown out from the depression.

At first sight the rim about Crater Lake suggests that the caldera was produced by an explosion, and the occurrence of much pumice in that region lends support to this preliminary view; but on careful examination we find, as already stated, that the rim is not made up of fragments blown from the pit, but of layers of solid lava interbedded with those of volcanic conglomerate erupted from Mount Mazama before the caldera originated. The moraines deposited by glaciers descending from the mountain formed the surface around a large part of the rim, and as there is no fragmental deposits on these moraines, it is evident that there is nothing whatever to indicate any explosive action in connection with the formation of the caldera.

We may be aided in understanding the possible origin of the caldera by picturing the conditions that must have obtained during an effusive eruption of Mount Mazama. At such a time the column of molten material rose in the interior of the mountain until it overflowed at the summit or burst open the sides of the mountain and escaped through fissures. Fissures formed in this way usually occur high on the slopes of the mountain. If instead, however, an opening were effected on the mountain side at a much lower level—say some thousands of feet below the summit—and the molten material escaped, the mountain would be left hollow, and the summit, having so much of its support removed, might cave in and disappear in the molten reservoir.

Something of this sort is described by Professor Dana as occurring at Kilauea, in Hawaii. The lake in that case is not water, but molten lava, for Kilauea is yet an active volcano. In 1840 there was an eruption from the slopes of Kilauea, 27 miles distant from the lake and over 4,000 feet below its level. The column of lava represented by the lake of molten material in Kilauea sank away in connection with this eruption to a depth of 385 feet, and the floor of the region immediately surrounding the lake, left without support, tumbled into the depression. In the intervals between eruptions the molten column rises again toward the surface, only to be lowered by subsequent eruptions, and the subsidence is not always accompanied by an outflow of lava upon the surface. Sometimes, however, it gushes forth as a great fountain a hundred feet or more in height.

The elevated position of the great caldera occupied by Crater Lake makes its origin by subsidence seem the more probable. The level of the lowest bed of the lake reaches the surface within 15 miles down the western slope of the range. That Mount Mazama was ingulfed is plainly suggested by the behavior of its final lava stream. The greater portion of this last flow descended and spread over the outer slope of the rim, but from the thickest part of the flow where it fills an old valley at the head of Cleetwood Cove some of the same lava, as already noted, poured down the inner slope. The only plausible explanation of this phenomena seems to be that soon after the final eruption of Mount Mazama, and before the thickest part of the lava effused at that time had solidified, the mountain collapsed and sank away and the yet viscous portion of the stream followed down the inner slope of the caldera.

It has been suggested, but perhaps not in serious thought, that the cone on Wizard Island may represent the summit of the sunken Mount Mazama projecting above the water. To determine the truth of the matter we must cross over to the island. Wizard Island has two portions—an extremely rough lava field and a cinder cone. These parts may be distinguished in figure 16, a view of the island from the Watchman. A portion of the lava field is shown in the foreground of figure 18. The lava is dark and has a much more basaltic look than any seen in the main body of the rim. It has evidently been erupted from the base of the cinder cone in its present position. The cinder cone, too, is a perfect little volcano, with steep symmetrical slopes, 845 feet in height, and surmounted by a crater 80 feet deep. A portion of this crater is shown in figure 17. It is so new and fresh that it is scarcely forested, and shows no trace of weathering. Instead of being a part of the sunken Mount Mazama, it is an entirely new volcano built up since the subsidence by volcanic action upon the bottom of the caldera. Were it not for the lake the whole bottom of the caldera could be examined, and it is possible that other small volcanic cones might be found. This suggestion is borne out by the soundings of the lake, which appear to reveal two other cases, but they do not rise to within 400 feet of the surface of the water. It is evident that the volcanic eruptions upon the bottom of the caldera have partially filled it up. Originally it may have been much more than 4,000 feet deep.

Given the caldera with water-tight walls, there is no difficulty in forming Crater Lake, for in that region precipitation is greater than evaporation. Observations upon precipitation and evaporation have not been made at Crater Lake, but, judging from those made at nearest points, the annual precipitation should be between 60 and 70 inches, while the annual evaporation is between 50 and 60 inches. The average diameter of the lake is nearly 5 miles. Its area, including Wizard Island, is about 21.30 square miles. The drainage area inclosed by the rim of the lake, according to Mr. E. C. Barnard, is 27.48 square miles. During the winter great masses of snow drift within the rim, and thus considerably augment the normal precipitation of the lake. The lake does not fill up and overflow. The surplus water must have a subterranean outlet, probably toward the southeast, where the region is traversed by extensive breaks in the rocks, and abounds in excellent springs.

The color of the lake is deep blue, excepting along the borders, where it merges into various shades and tints of green. It is so transparent that even on a hazy day a white dinner plate 10 inches in diameter may be seen at a depth of nearly 100 feet. It contains no fish, but a small crustacean flourishes in its waters, and salamanders occur in abundance locally along the shore.

The level of the lake oscillates with the seasons. During the rainy winter it rises, and in the summer it falls. In August, 1896, observations were made for twenty-two days, and the lake sank at the rate of 1 inch for every five or six days, depending somewhat on the conditions of the weather. The Mazamas have established a water gauge, and it was hoped that an extended series of observations would be obtained, but the ice broke it off the next winter.

Mr. B. W. Evermann, of the United States Fish Commission, who visited the lake last summer, made some interesting observations of its temperature. At 1 p. m., August 22—

The temperature of the surface water was	60°
At a depth of 555 feet the temperature was	39°
At a depth of 1,043 feet the temperature was	41°
At a depth of 1,623 feet (on the bottom) the temperature was	46°

The increase of temperature with the depth suggests that the bottom may yet be warm from volcanic heat, but more observations are needed to fully establish such an abnormal relation of temperatures in a body of water.

Aside from its attractive scenic features, Crater Lake affords one of the most interesting and instructive fields for the study of volcanic geology to be found anywhere in the world. Considered in all its aspects, it ranks with the Grand Canyon of the Colorado, the Yosemite Valley, and the Falls of Niagara, and it is interesting to note that a bill has been introduced in Congress to make it a national park for the pleasure and instruction of the people.

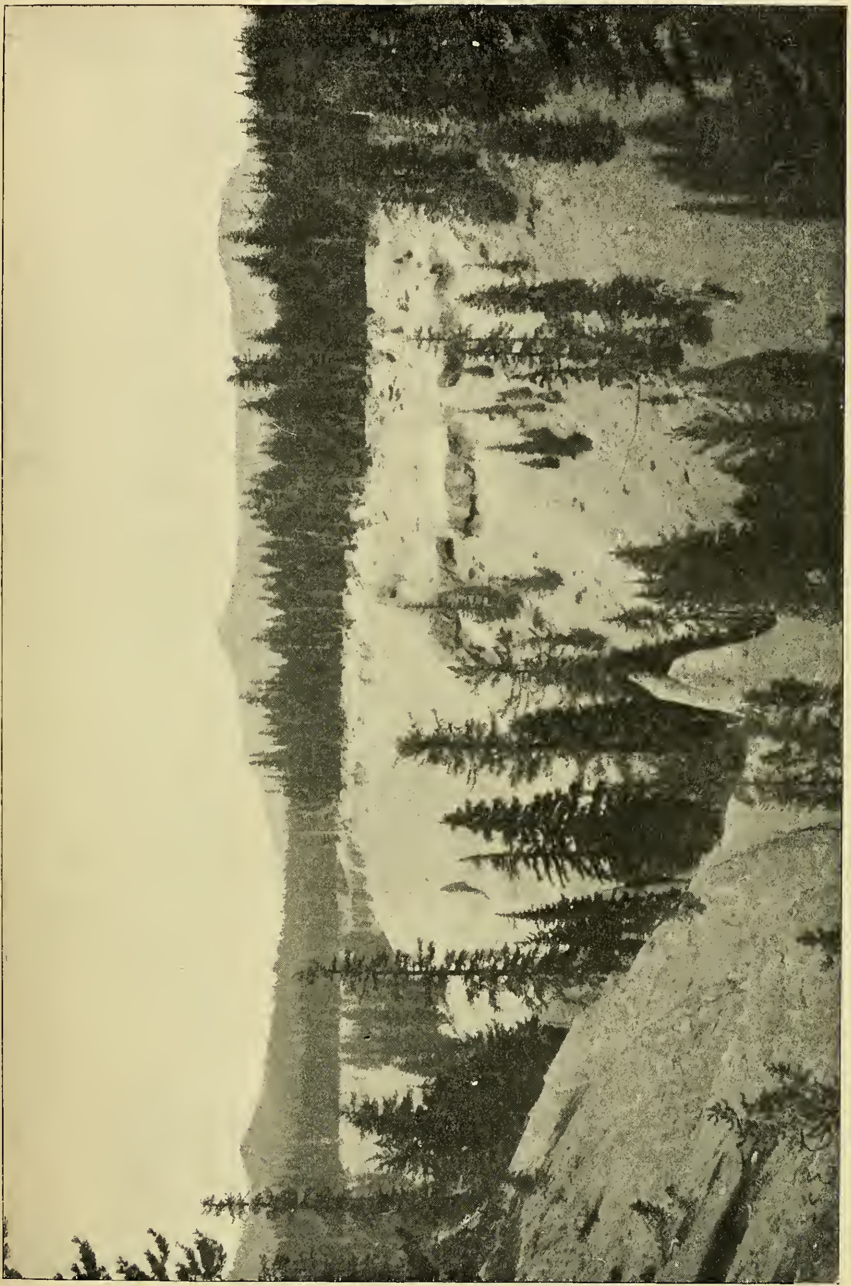


FIG. 2.—RIM OF CRATER LAKE IN THE DISTANCE, AS SEEN FROM THE SOUTH, ACROSS THE CANYON OF ANNA CREEK.

From a photograph by J. S. Diller.

FIG. 3.—CRATER LAKE, OREGON. WIZARD ISLAND, DEVILS BACKBONE. AND LLAO ROCK IN THE DISTANCE.

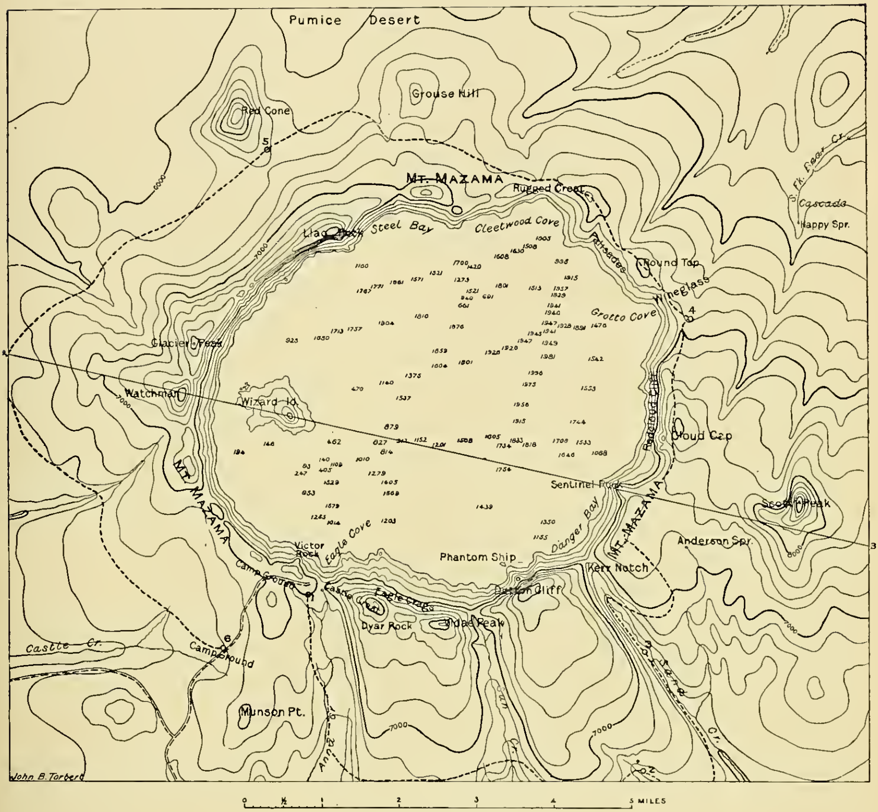
From a photograph by M. M. Hazeltine.

FIG. 4.—SOUTHWESTERN BORDER OF CRATER LAKE. VICTOR ROCK IN THE FOREGROUND. THE WATCHMAN AND GLACIER PEAK IN THE DISTANCE.

From a photograph by Maj. C. E. Dutton.

FIG. 5.—SOUTHERN BORDER OF CRATER LAKE. MOUNT SCOTT IN THE DISTANCE.

From a photograph by Maj. C. E. Dutton.



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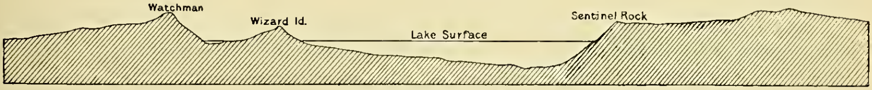

SECTION FROM A to B

(Reduced from U. S. Geological Survey special sheet.)

FIG. 6.—MAP OF CRATER LAKE.

(Soundings in feet.)

Feasible pack trail around the lake shown thus: --------, not blazed.
Camping places shown thus: o

FIG. 7.—GLACIAL MORAINE NEAR CAMP GROUND, CRATER LAKE.

From a photograph by J. S. Diller.

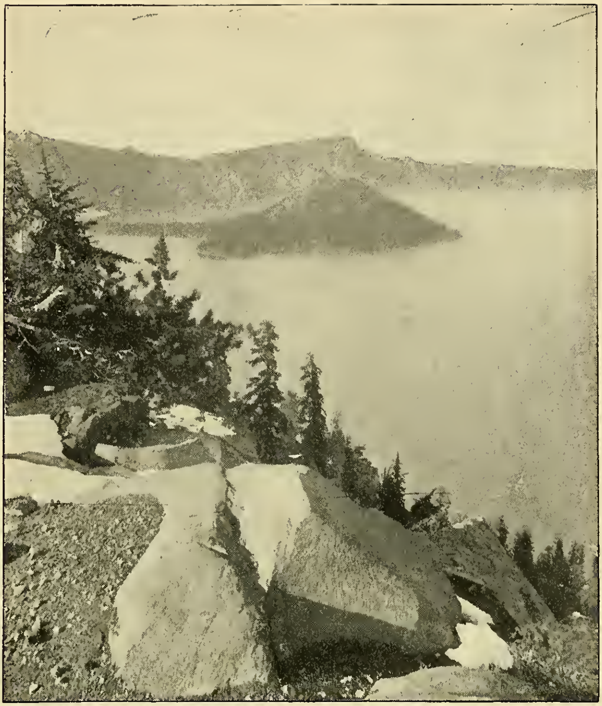

FIG. 8.—GLACIATED CREST OF RIM OF CRATER LAKE.

From a photograph by M. M. Hazeltine.

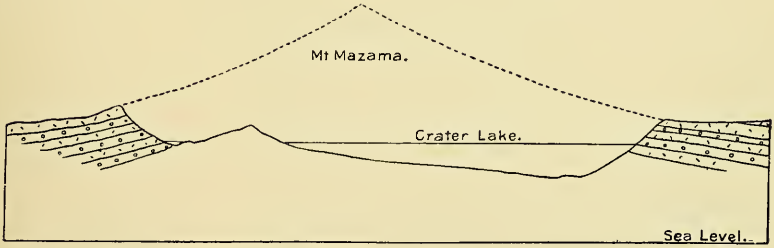

FIG. 9.—SECTION OF CRATER LAKE AND ITS RIM, WITH THE PROBABLE OUTLINE OF MOUNT MAZAMA. STRUCTURAL DETAILS GENERALIZED.

Vertical and horizontal scales the same.

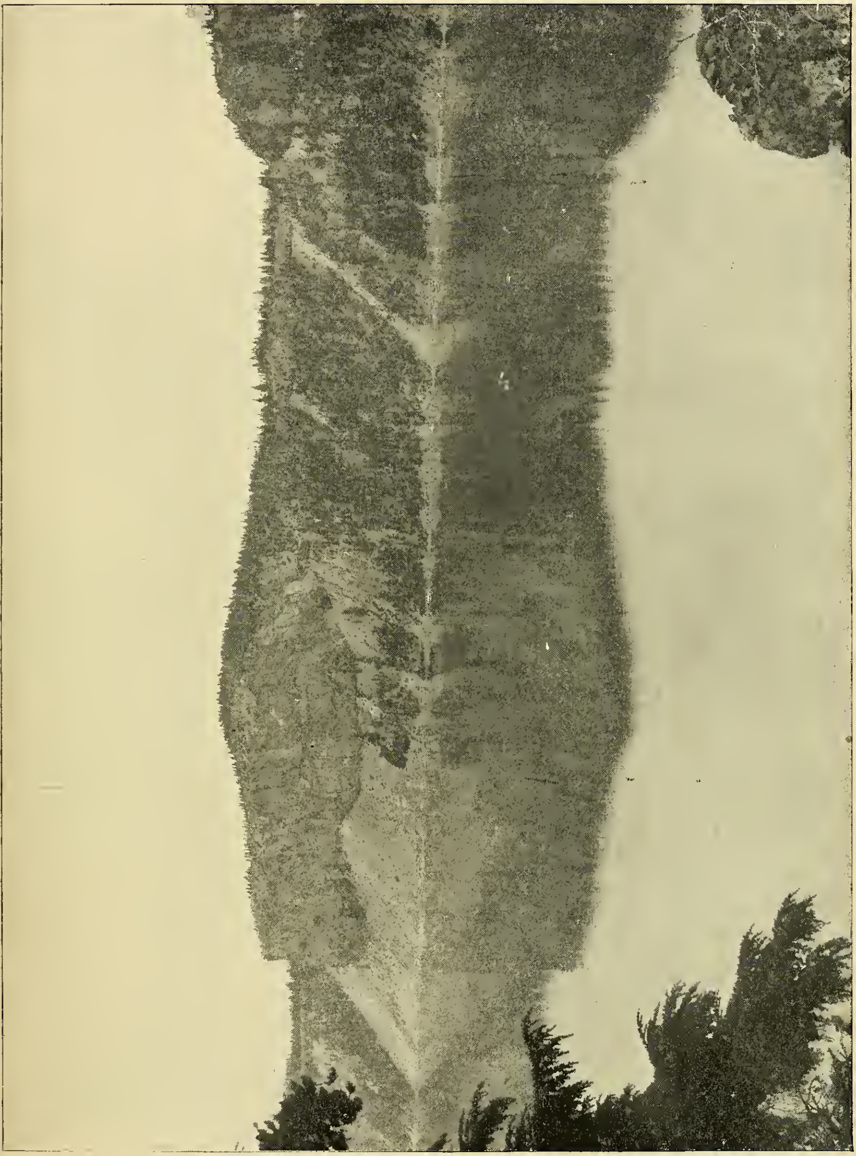

FIG. 10.—THE PALISADES, ROUND TOP, AND WINEGLASS SLIDE ON NORTHEAST BORDER OF CRATER LAKE.

From a photograph by J. S. Diller.

Fig. 11.—Crater Lake. The Devils Backbone and Llao Rock seen beyond Wizard Island. The Sharp Point of Mount Thielson Appears in the Distance.

FIG. 12.—SOUTHERN SHORE OF CRATER LAKE FROM KERRS NOTCH (LOOKING WEST).

From a photograph by J. S. Diller.

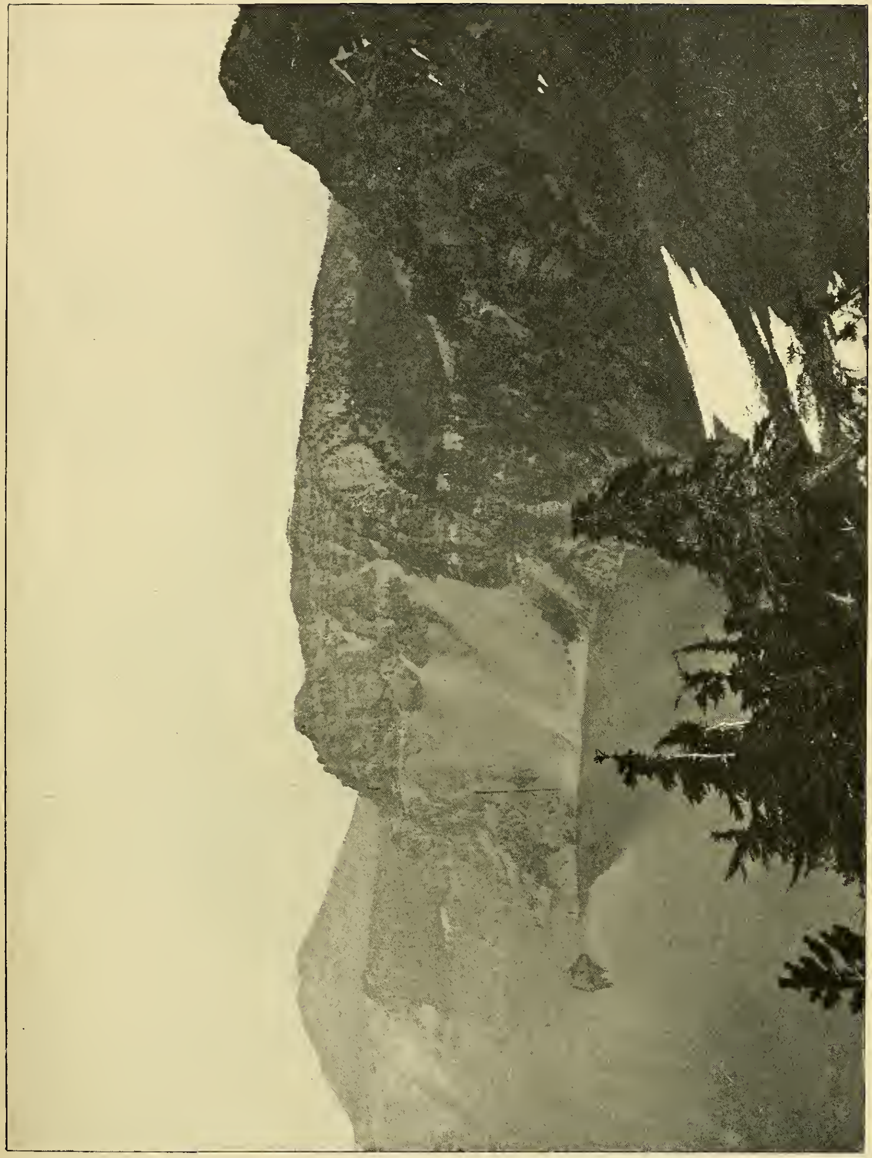

FIG. 13.—SOUTHERN SHORE OF CRATER LAKE FROM CASTLE CREST. MOUNT SCOTT AND PHANTOM SHIP ON THE LEFT; NOTCH AT THE HEAD OF SUN CREEK ON THE RIGHT.

From a photograph by H. B. Patton.

FIG. 14.—THE PHANTOM SHIP.

From photograph, by permission of C. C. Lewis.

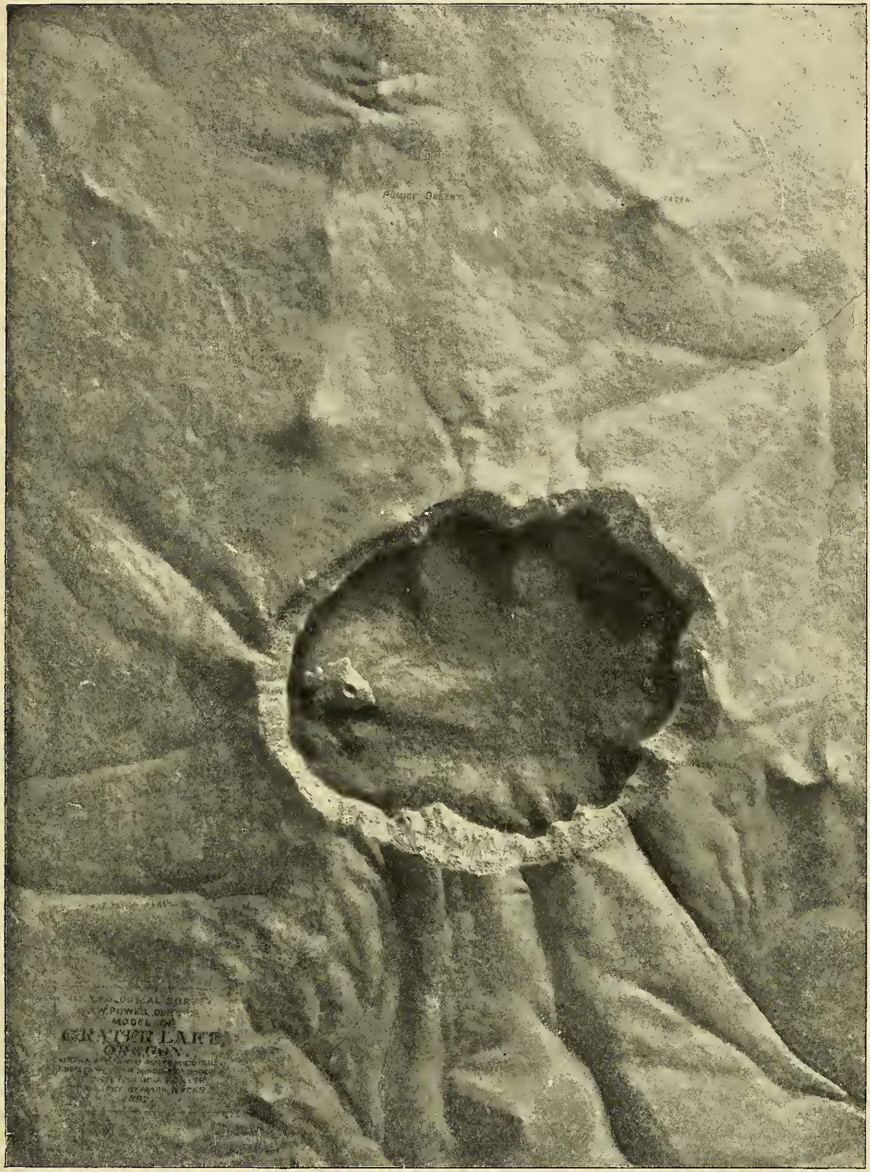

FIG. 15.—CRATER LAKE AS IT WOULD APPEAR FROM A HEIGHT OF OVER 20 MILES ABOVE IT.

Prepared from a photograph of a relief model in the U. S. National Museum.

FIG. 16.—WIZARD ISLAND, FROM THE WATCHMAN.
From a photograph by M. M. Hazeltine.

FIG. 17.—SNOWDRIFT IN THE CRATER OF THE CINDER CONE ON WIZARD ISLAND.
From a photograph by H. B. Patton.

Fig. 18.—Northwestern Portion of Lava Field on Wizard Island

From a photograph by H. B. Patton.

THE FUNCTION AND FIELD OF GEOGRAPHY.[1]

By J. Scott Keltie, LI

We meet this year in exceptional circumstances. Thirteen years ago the British Association met for the first time in a portion of the Empire beyond the limits of the British Islands. During these thirteen years much has happened of the greatest interest to geographers, and if I attempted to review the progress which has been made during these years—progress in the exploration of the globe, progress in geographical research, progress in geographical education—I could not hope to do it to any purpose in the short time during which it would be right for a president to monopolize the attention of the section. But we have, at the same time, reached another stage in our history which naturally leads us to take stock of our progress in the past. We have all of us been celebrating the sixtieth year of the glorious reign of the Sovereign, of whose vast dominions Canada and the United Kingdom form integral parts. The progress made during that period in our own department of science has been immense; it would take volumes to tell what has been done for the exploration of the globe. The great continent of Africa has practically been discovered, for sixty years ago all but its rim was a blank. In 1837 enormous areas in North America were unexplored, and much of the interior of South America was unknown. In all parts of Asia vast additions have been made to our knowledge; the maps of the interior of that continent were, sixty years ago, of the most diagrammatic character. The Australian interior was nearly as great a blank as that of Africa; New Zealand had not even been annexed. Need I remind you of the great progress which has been made during the period both in the North and South Polar areas, culminating in the magnificent achievement of Dr. Nansen? It was just sixty years ago that the great Antarctic expedition under Sir James Ross was being organized; since that, alas, little or nothing has been done to follow up his work. Sixty years ago the science of Oceanography, even the term, did not exist; it is the creation of the Victorian era, and may be said almost to have had its origin in the voyage of the *Challenger*, which added a new domain to our science and opened up inexhaustible fields

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[1] Address to the Geographical Section of the British Association for the Advancement of Science, Toronto, 1897, by J. Scott Keltie, LL. D., Sec. R. G. S., President of the section. From Report of the British Association, 1897.

of research. I have thought then that the most useful and most manageable thing to do on the present occasion will be to indicate briefly what, in my estimation, are some of the problems which geography has to attack in the future, only taking such glances at the past as will enable us to do this intelligibly.

It has been customary for the occupants of this chair to try to define the field of geography, and on occasions, in somewhat too apologetic language, to justify its existence as a section of a scientific association. I do not think this is any longer necessary. Even in England and America, during the last thirteen years, geography has done work enough to prove that she has a mission which no other department of research can fulfill. I say thirteen years, because that not only carries us back to the last Canadian meeting of the British Association, but to the year when the Royal Geographical Society undertook an inquiry into the position of geography at home and abroad, mainly with a view to the improvement of geographical education in England. During that time a good deal has been written as to the field and scope of geography, and a good many definitions given. But we really did not require to go to Germany to teach us as to the field and functions of geography. Sixty years ago, the then president of the Royal Geographical Society, Mr. William R. Hamilton, delivered the first presidential address ever given at that society, and his conception of the field and aims of geography was as exalted and comprehensive as the most exacting German geographer could wish. It is too long to quote here.[1]

It would be difficult to improve upon Mr. Hamilton's definition, and it shows that a correct conception of the wide and important field of geography is no new thing in England. He proceeded to indicate what remained to be done in the field of exploration, and I commend his address to anyone desirous of forming a conception of the vast progress that has been made since it was delivered sixty years ago. Since I am dealing with definitions, I may be permitted to quote that given by one so severely scientific as Gen. Sir R. Strachey in a course of lectures which he gave at the University of Cambridge in 1888, in connection with the establishment of a lecturership in geography in that university: "The aim of geographical science," he says, "is to investigate and delineate the various features of the earth; to study the distribution of land and sea, the configuration and relief of the surface, position on the globe, and so forth, facts which determine the existing condition of various parts of the earth, or which indicate former conditions; and to ascertain the relations that exist between these features and all that is observed on the earth. * * * I claim for geography," Sir R. Strachey says, "a place among the natural sciences as supplying the needful medium through which to obtain a connected and consistent conception of the earth and what is on it." He gives a list of the

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[1] Journal R. G. S., Vol. VIII, 1838.

various matters which, in his conception, it is the business of geography to deal with, and they are varied and important enough to satisfy the demands of the most exacting. "These are," he says, "the studies through which scientific geography will lead you, teaching you to view the earth in its entirety, bringing together the great variety of objects seen upon it, investigating their connection, and exploring their causes; and so combining and harmonizing the lessons of all the sciences which supply the key to the secrets of Nature."[1]

I think we may briefly define geography as the science of the topographical distribution of the great features of the earth's surface and of all that it sustains—mineral, vegetable, and animal, including man himself. In fact, man is the ultimate term in the geographical problem, the final object of which is to investigate the correlation between humanity and its geographical environment.

I may be pardoned for dwelling at some length on the function and field of geography. It is a subject that has been occupying the attention of geographers in England for some years, and it may not be without interest to our colleagues on this side of the Atlantic to know the conclusions which we have come to. Moreover, it seems necessary to arrive at some clear conception on the matter, with a view to the researches of the future. I say that the subject has been occupying our attention in England for some time; it has done so, I may say, as a result of the inquiry by myself on the part of the Royal Geographical Society to which I have referred. The object of that inquiry was mainly to collect information as to the position of geography in education at home and abroad. The report which I presented to the society attracted some attention, and whether as a result of that or not it is hardly for me to say, but certainly since that inquiry some twelve years ago the position of geography in England has considerably improved both in education and as a field for research. Better methods have been introduced in our schools; a much wider scope has been given to the subject; in many quarters teachers have shown themselves anxious to be guided in the right direction; and, above all, both Oxford and Cambridge at length consented to the establishment of lectureships in geography. A school of young geographers has grown up, consisting of men who have had a thorough university training in science and letters, and who are devoting themselves to the various branches of geography as a specialty. In this way the arid old text-books and characterless maps are being supplanted by others that will bear comparison with the best productions of Germany. Photography and lantern slides illustrating special geographical features are coming into use in schools; and in other directions appliances for use in education are being multiplied and improved. A British geographical literature is growing up, and if, as I hope, the progress be maintained, we shall be able to hold our own in geography with any country. The interest

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[1] Lectures on Geography delivered before the University of Cambridge, London, 1888.

in the subject has been extended by the foundation of geographical societies in various large centers; whereas thirteen years ago the only geographical society was that of London, there are now similar societies in Manchester, Newcastle, Liverpool, and Edinburgh, the last with branches in Glasgow, Dundee and Aberdeen. If this progressive movement is maintained, as there is every reason to hope it will be, the scientific and educational aspects of geography in Britain will be more nearly on a par with exploration in which our country has so long held the lead.

In the United States I found that the position of the subject in education was not much more satisfactory than it was in England. Since then there is reason to believe considerable progress has been made. One of the best text-books on physical geography, Hinman's Eclectic Physical Geography, is of American origin, while in the States, as in England, a school of scientific geographers has arisen which bids fair to give the subject a high place in that country. I fear, from what I can learn, that the position in Canada is not as satisfactory as it ought to be. It seems to me, then, that one of the great problems which geographers have to face in the future is the place which this subject is to hold in education, both as a body of information and as a discipline. We have been making progress, and if we persevere with intelligence and firmness and maintain the subject at the highest standard as a field of research, there can be little doubt of our success.

There is a prevalent belief that geoprapbers have nothing more to learn in Europe—that the old continent has been thoroughly explored. It is true that nearly every country in Europe has been or is being trigonometrically surveyed. Except some parts of the Balkan Peninsula and north of Russia the topography of the continent has been accurately mapped on scales and by methods sufficient at least for the purposes of the geographer. Yet there are districts in the Balkan Peninsula—for example, Albania—which are as vaguely known as central Africa. But it is a delusion to think that because a country has been fully mapped the occupation of the geographer is gone. It is only when a region at large is adequately mapped that the work of geographical research begins. The student, with a satisfactory map of a definite district as his guide, will find on the spot abundant occupation in working out its geographical details, the changes which have taken place in its topography, and the bearing of its varied features upon its history, its inhabitants, its industries. This kind of work has been in progress in Germany for over ten years under the auspices of the central commission for the scientific geography (Landeskunde) of Germany, with its seat at Stuttgart. Under the collective title of "Forschungen zur Deutschen Landes- und Volkskunde," a long series of monographs by specialists has been published, dealing in minute detail with one or more aspects of a limited district. Thus we have such memoirs as "The plain of the Upper Rhine and its neighboring

Mountains," by Dr. Richard Lepsius; "The towns of the North German Plain in relation to the configuration of the ground," by Dr. Hahn; "The Munich Basin: A Contribution to the physical geography of southern Bavaria," by C. Gruber; "The Mecklenburg Ridges and their relation to the Ice Age," by Dr. E. Geinitz; "The influence of the mountains on the climate of central Germany," by R. Assmann; "The distribution and origin of the Germans in Silesia," by Dr. K. Weinhold; "Mountain structure and surface configuration of Saxon Switzerland," by Dr. A. Hettner; "The Erzgebirge: An orometric-anthropogeographical study," by Dr. J. Burgkhardt; "The Thuringian forest and its surroundings," by Dr. H. Pröscholdt, and so forth. There is thus an inexhaustible field for scientific geography in its most comprehensive sense—a series of problems which may take generations to work out. In a less systematic way we have similar monographs by French geographers. One or two attempts, mainly by teachers, have been made in England to do similar work, but the impression generally produced is that the authors have not been well equipped for the task. I am glad to say that in England the Royal Geographical Society has initiated a movement for working out in a systematic fashion what one may call the regional geography of the British Islands on the basis of the 1-inch maps of the Ordnance Survey. It is a strange thing that the geography of the mother country has never yet been systematically worked out.

Taking the sheets of the Ordnance Survey map as a basis, it is proposed that each district should be thoroughly investigated, and a complete memoir of moderate dimensions systematically compiled to accompany the sheet, in the same way that each sheet of the Geological Survey map has its printed text. It is a stupendous undertaking, that would involve many years' work, and the results of which when complete would fill many volumes. But it is worth doing; it would furnish the material for an exact and trustworthy account of the geography of Britain on any scale and would be invaluable to the historian, as well as to others dealing with subjects having any relation to the past and present geography of the land. The librarian of the society, Dr. H. R. Mill, has begun operations on a limited area in Sussex. When he has completed this initial memoir, it will be for the society to decide whether it can continue the enterprise, or whether it will succeed in persuading the Government to take the matter up. I refer to work of this kind mainly to indicate what, in my conception, are some of the problems of the future which geography has to face, even in fully surveyed countries. Even were the enterprise referred to carried out, there would be room enough for special researches in particular districts.

But while there is an inexhaustible field in the future for geographical work in the direction I have indicated, there is no doubt that much still remains to be done in the way of exploring the unknown, or little known, regions of the globe. Let us briefly refer to the problems remaining to be solved in this direction. Turning to the continent of Asia, we find that immense progress has been made during the past sixty years. In the presidential address given sixty years ago, already referred to, Mr. Hamilton says of Asia: "We have only a very general knowledge of the geographical character of the Burman, Chinese, and Japan Empires; the innumerable islands of the latter are still, except occasionally, inaccessible to European navigators. Geographers hardly venture on the most loose description of Tibet, Mongolia, or Chinese Tartary, Siam, and Cochin China." Since then the survey of India, one of the greatest enterprises undertaken by any State, has been completed and is being rapidly extended over Burma. But I need not remind you in detail of the vast changes that have taken place in Asia during these years and the immense additions that have been made to our knowledge of its geography. Exploring activity in Asia is not likely to cease, though it is not to be expected that its inhospitable center will ever be so carefully mapped as have been the mountains of Switzerland.

The most important desiderata, so far as pioneer exploration in Asia is concerned, may be said to be confined to two regions.[1] In southern and central Arabia there are tracts which are entirely unexplored. It is probable that this unexplored region is in the main a sandy desert. At the same time it is, in the south at least, fringed by a border of mountains whose slopes are capable of rich cultivation, and whose summits the late Mr. Theodore Bent found, on his last and fatal journey, to be covered with snow. In exploration, as in other directions, it is the unexpected that happens; and if any traveler cared to face the difficulties—physical, political, and religious—which might be met with in southern and central Arabia, he might be able to tell the world a surprising story.

The other region in Asia where real pioneer work still remains to be done is Tibet and the mountainous districts bordering it on the north and east. Lines of exploration have in recent years been run across Tibet by Russian explorers like Prejevalsky, by Rockhill, Prince Henry of Orleans and Bonvalot, by Bower, Littledale, Wellby, and Malcolm. From the results obtained by these explorers we have formed a fair idea of this, the most extensive, the highest, and the most inhospitable plateau in the world. A few more lines run in well-selected directions would probably supply geography with nearly all she wants to learn about such a region, though more minute exploration would probably furnish interesting details as to its geological history.

The region lying to the north of the Himalayan range and to the south of the parallel of Lhasa is almost a blank on the map, and there is ample room here for the enterprising pioneer. The forbidden city of

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[1] For part of what follows with reference to Asia I am indebted to a valuable memorandum on the subject drawn up by the late Mr. Ney Elias.

Lhasa is at present the goal of several adventurers, though as a matter of fact we can not have much to learn in addition to what has been revealed in the interesting narrative of the native Indian traveller, Chandra Das. The magnificent mountain region on the north and east of Tibet furnishes a splendid field for the enterprising explorer. Mrs. Bishop recently approached it from the east, through Sze-chuen, and her description of the romantic scenery and the interesting non-Mongolian inhabitants leaves us with a strong desire to learn more. On the southeast of Tibet is the remarkable mountainous region, consisting of a series of lofty parallel chains, through which run the upper waters of the Yangtse, the Mekong, the Salwin, and the Irawady. This last-named river, recent exploration has shown, probably does not reach far into the range. But it will be seen by a glance at a map that the upper waters of the other rivers are carried far into the heart of the mountains. But these upper river courses are entirely conjectural and have given rise to much controversy. There is plenty of work here for the explorer, though the difficulties, physical and political, are great.

But besides these great unexplored regions, there are many blanks to be filled up in other parts of Asia, and regions which, though known in a general way, would well repay careful examination. There is the mountain track between the upper Zarafshan River and the middle course of the Sarkhab tributary of the Oxus, and the country lying between that and the Oxus. There is the great Takla-Makan desert in Chinese or eastern Turkistan, part of which has recently been explored by Russian expeditions and by that young and indefatigable Swedish traveler, Dr. Sven Hedin. It is now one of the most forbidding deserts to be found anywhere, but it deserves careful examination, as there are evidences of its once having been inhabited, and that at no very remote period. It is almost surrounded by the Tarim, and on its eastern edge lies Lob-nor, the remarkable changes in which have been the subject of recent investigation. As readers of Dr. Nansen's "Voyage of the Fram" will remember, the Siberian coast is most imperfectly mapped; of course it is a difficult task, but it is one to which the Russian Government ought to be equal. China has on paper the appearance of being fairly well mapped; but as a matter of fact our knowledge of its mountain ranges and of its great river courses is to a large extent extremely vague. All this awaits careful survey. In northeastern Manchuria and in many parts of Mongolia there are still blanks to be filled up and mountain and river systems to be surveyed. In the Malay Peninsula and in the great array of islands in the east and southeast of Asia—Sumatra, Borneo, the Philippines—much work still remains to be done. Thus for the coming century there will be abundance of work for explorers in Asia, and plenty of material to occupy the attention of our geographical societies.

Coming to the map of Africa, we find the most marvelous transformation during the last sixty years, and mainly during the last forty years, dating from Livingstone's memorable journey across the continent. Though the north of Africa was the home of one of the oldest civilizations, and though on the shores of the Mediterranean, Phœnicians, Carthaginians, Greeks, and Romans were at work for centuries, it has only been within the memory of many of us that the center of the continent, from the Sahara to the confines of Cape Colony, has ceased to be an unexplored blank. This blank has been filled up with bewildering rapidity. Great rivers and lakes and mountains have been laid down in their main features, and the whole continent, with a few unimportant exceptions, has been parceled out among the powers of Europe. But much still remains to be done ere we can form an adequate conception of what is in some respects the most interesting and the most intractable of the continents. Many curious problems still remain to be solved. The pioneer work of exploration has to a large extent been accomplished; lines have been run in all directions; the main features have been blocked out. But between these lines the broad meshes remain to be filled in, and to do this will require many years of careful exploration. However, there still remain one or two regions that afford scope for the adventurous pioneer.

To the south of Abyssinia and to the west and northwest of Lake Rudolf, on to the Upper Nile, is a region of considerable extent, which is still practically unknown. Again, in the western Sahara there is an extensive area, inhabited mainly by the intractable Tuaregs, into which no one has been able to penetrate, and of which our knowledge is extremely scanty. Even in the central Sahara there are great areas which have not been traversed, while in the Libyan desert much remains to be done. These regions are of interest almost solely from geographical and geological standpoints. But they deserve careful investigation, not only that we may ascertain their actual present condition, but in order, also, that we may try to discover some clews to the past history of this interesting continent. Still, it must be said that the great features of the continent have been so fully mapped during the last half century that what is required now is mainly the filling-in of the details. This is a process that requires many hands and special qualifications. All over the continent there are regions which will repay special investigation. Quite recently an English traveler, Mr. Cowper, found not far from the Tripoli coast miles of magnificent ruins and much to correct on our maps. If only the obstructiveness of the Turkish officials could be overcome, there is a rich harvest for anyone who will go to work with patience and intelligence. Even the interior of Morocco, and especially the Atlas Mountains, are but little known. The French, both in Tunis and Algeria, are extending our knowledge southwards. All the powers who have taken part in the scramble for Africa are doing much to acquire a knowledge of their territories. Germany, especially, deserves praise for the persistent zeal with which she has carried out the exploration of her immense territories in east and west

Africa. The men she sends out are unusually well qualified for the work, capable not simply of making a running survey as they proceed, and taking notes on country and people, but of rendering a substantial account of the geology, the fauna, the flora, and the economic conditions. Both in the French and British spheres good work is also being done, and the map of Africa being gradually filled up. But what we especially want now are men of the type of Dr. J. W. Gregory, whose book on the Great Rift Valley is one of the most valuable contributions to African geography ever made. If men of this stamp would settle down in regions like that of Mount Ruwenzori, or Lake Rudolf, or the region about Lakes Bangweolo and Tanganyika, or in the Atlas, or in many other regions that could be named, the gains to scientific geography, as well as to the economical interests of Africa, would be great. An example of work of this kind is seen in the discoveries made by a young biologist trained in geographical observation, Mr. Moore, on Lake Tanganyika. There he found a fauna which seems to afford a key to the past history of the center of the continent, a fauna which, Mr. Moore maintains, is essentially of a salt-water type. Mr. Moore, I believe, is inclined to maintain that the ancient connection of this part of Africa with the ocean was not by the west, as Joseph Thomson surmised, but by the north, through the Great Rift Valley of Dr. Gregory; and he strongly advocates the careful examination of Lake Rudolf as the crucial test of his theory. It is to be hoped that he, or someone equally competent, will have an opportunity of carrying out an investigation likely to provide results of the highest importance.

But there are other special problems connected with this, the most backward and the most repellent of continents, which demand serious investigation, problems essentially geographical. One of the most important of these, from the point of view of the development of Africa, is the problem of acclimatization. The matter is of such prime importance that a committee of the association has been at work for some years collecting data as to the climate of tropical Africa. In a general way we know that that climate is hot and the rainfall scanty; indeed, even the geographers of the Ancient World believed that central Africa was uninhabitable on account of its heat. But science requires more than generalities, and therefore we look forward to the exact results which are being collected by the committee referred to with much hope. We can only go to work experimentally until we know precisely what we have to deal with. It will help us greatly to solve the problem of acclimatization when we have the exact factors that go to constitute the climate of tropical Africa. At present there is no doubt that the weight of competent opinion—that is, the opinion of those who have had actual experience of African climate, and of those who have made a special study of the effects of that climate on the human constitution—is that though white men, if they take due precautions, may live and do certain kinds of work in tropical Africa, it will never be possible to colonize that part of the world with people from the temperate zone. This is the lesson taught by generations of experience of Europeans in India. So far, also, sad experience has shown that white people can not hope to settle in central Africa as they have settled in Canada and the United States and in Australia, and make it a nursery and a home for new generations. Even in such favorable situations as Blantyre, a lofty region on the south of Lake Nyasa, children can not be reared beyond a certain age; they must be sent home to England, otherwise they will degenerate physically and morally. No country can ever become the true home of a people if the children have to be sent away to be reared. Still, it is true our experience in Africa is limited. It has been maintained that it might be possible to adapt Europeans to tropical Africa by a gradual process of migration. Transplant southern Europeans to North Africa; after a generation or two remove their progeny farther south, and so on, edging the succeeding generation farther and farther into the heart of the continent. The experiment—a long one it would be—might be tried; but it is to be feared that the ultimate result would be a race deprived of all those characteristics which have made Europe what it is. An able young Italian physician, Dr. Sambon, has recently faced this important problem, and has not hesitated to come to conclusions quite opposed to those generally accepted. His position is that it has taken us centuries in Europe to discover our hidden enemies, the microbes of the various diseases to which northern humanity is a prey, and to meet them and to conquer them. In Africa we have a totally different set of enemies to meet, from lions and snakes down to the invisible organisms that produce those forms of malaria, anæmia, and other diseases characteristic of tropical countries. He admits that these are more or less due to heat, to the nature of the soil, and other tropical conditions, but that if once we knew their precise nature and modes of working we should be in a position to meet them and conquer them. It may be so, but this is a result that could only be reached after generations of experience and investigation; and even Dr. Sambon admits that the ultimate product of European acclimatization in Africa would be something quite different from the European progenitors. What is wanted is a series of carefully conducted experiments. I have referred to the Blantyre highlands; in British East Africa there are plateaus of much greater altitude, and in other parts of central Africa there are large areas of 4,000 feet and over above sea level. The world may become so full that we may be forced to try to utilize these lofty tropical regions as homes for white people when Canada and Australia and the United States become over populated. As one of my predecessors in this chair (Mr. Ravenstein) tried to show at the Leeds meeting some years ago, the population of the world will have more than doubled in a century, and about one hundred and eighty years hence will have quadrupled. At any rate, here is a problem of prime importance for the geographer of the coming century to attack; with so many energetic and intelligent white men all over Africa, it should not be difficult to obtain the data which might help toward its solution.

I have dwelt thus long on Africa because it will really be one of the great geographical problems of the coming century. Had it been as suitable as America or Australia, we may be sure it would not have remained so long neglected and despised by the European peoples as it has done. Unfortunately for Africa, just as it had been circumnavigated, and just as Europeans were beginning to settle upon its central portion and trying to make their way into the interior, Columbus and Cabot discovered a new world, a world as well adapted as Europe for the energies of the white races. That discovery postponed the legitimate development of Africa for four centuries. Nothing could be more marked than the progress which America has made since its rediscovery four hundred years ago and the stagnation of Africa, which has been known to Europe since long before the beginning of history. During these four hundred years North America at least has been very thoroughly explored. The two great nations which divide North America between them have their Government surveys, which are rapidly mapping the whole continent and investigating its geology, physical geography, and its natural resources. I need hardly tell an audience like this of the admirable work done by the survey of Canada under Sir William Logan, Dr. Selwyn, and his successor, Dr. George Dawson; nor should it be forgotten that under the lands department much excellent topographical work has been carried out by Captain Deville and his predecessors. Still, though much has been done, much remains to be done. There are large areas which have not as yet even been roughly mapped. Within quite recent years we have had new regions opened up to us by the work of Dawson and Ogilvie on the Yukon, by Dr. Bell in the region to the south of Hudson Bay, by the brothers Tyrrell in the Barren Lands on the west of the same bay, by O'Sullivan beyond the sources of the Ottawa, and by Low in Labrador. But it is not so long since that Dr. Dawson, in reviewing what remains to be done in the Dominion in the way of even pioneer exploration, pointed out that something like a million square miles still remained to be mapped. Apart from the uninhabitable regions in the north, there are, as Dr. Dawson pointed out, considerable areas which might be turned to profitable agricultural and mining account of which we know little, such areas as these which have been recently mapped on the south of Hudson Bay by Dr. Bell, and beyond the Ottawa by Mr. O'Sullivan. Although the eastern and the western provinces have been very fully surveyed, there is a considerable area between the two, lying between Lake Superior and Hudson Bay, which seems to have been so far almost untouched. A very great deal has been done for the survey of the rivers and lakes of Canada. I need hardly say that in Canada, as elsewhere in America, there is ample scope for the study of many problems in physical geography—past and present glaciation and the work of glaciers, the origin and régime of lake basins, the erosion of river beds, the oscillation of coast lines. Happily, both in Canada and the United States there are many men competent and eager to work out problems of this class, and in the reports of the various surveys, the transactions of American learned societies, in scientific periodicals, in separate publications, a wealth of data has already been accumulated of immense value to the geographer.

Every geologist and geographer knows the important work which has been accomplished by the various surveys of the United States, as well as by the various State surveys. The United States Coast Survey has been at work for more than half a century, mapping not only the coast but all the navigable rivers. The Lake Survey has been doing a similar service for the shores of the Great Lakes of North America. But it is the work of the Geological Survey which is best known to geographers—a survey which is really topographical as well as geological, and which, under such men as Hayden, King, and Powell, has produced a series of magnificent maps, diagrams, and memoirs of the highest scientific value and interest. Recently this Survey has been placed on a more systematic basis; so that now a scheme for the topographical survey of the whole of the territory of the United States is being carried out. Extensive areas in various parts of the States have been already surveyed on different scales. It is to be hoped that in the future, as in the past, the able men who are employed on this survey work will have opportunities of working out the physiography of particular districts, the past and present geography of which is of advancing scientific interest. Of the complete exploration and mapping of the North American continent we need have no apprehension; it is only a question of time, and it is to be hoped that neither of the Governments responsible will allow political exigencies to interfere with what is really a work of national importance.

It is when we come to Central and South America that we find ample room for the unofficial explorer.[1] In Mexico and the Central American States there are considerable areas of which we have little or only the vaguest knowledge. In South America there is really more room now for the pioneer explorer than there is in Central Africa. In recent years the Argentine Republic has shown a laudable zeal in exploring and mapping its immense territories, while a certain amount of good work has also been done by Brazil and Chile. Most of our knowledge of South America is due to the enterprise of European and North American explorers. Along the great river courses our knowledge is fairly satisfactory, but the immense areas, often densely clad with forests, lying between the rivers are almost entirely unknown. In Patagonia, though a good deal has recently been done by the Argentinian

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[1] I am indebted for much of the information relative to South America to a valuable memorandum by Sir Clements R. Markham and Col. G. E. Church.

Government, still in the country between Punta Arenas and the Rio Negro we have much to learn, while on the west coast range, with its innumerable fjord-like inlets, its islands and peninsulas, there is a fine field for the geologist and physical geographer. Indeed, throughout the whole range of the Andes systematic exploration is wanted, exploration of the character of the excellent work accomplished by Whymper in the region around Chimborazo. There is an enormous area lying to the east of the northern Andes, and including their eastern slopes, embracing the eastern half of Ecuador and Colombia, southern Venezuela, and much of the country lying between that and northern Bolivia, including many of the upper tributaries of the Amazon and Orinoco, of which our knowledge is of the scantiest. Even the country lying between the Rio Negro and the Atlantic is but little known. There are other great areas in Brazil and in the northern Chaco which have only been partially described, such as the region whence the streams forming the Tapajos and the Paraguay take their rise, in Mato Grosso. A survey and detailed geographical and topographical description of the whole basin of Lake Titicaca is a desideratum. In short, in South America there is a wider and richer field for exploration than in any other continent. But no mere rush through these little-known regions will suffice. The explorer must be able to use his sextant and his theodolite, his compass, and his chronometer. Any expeditions entering these regions ought to be able to bring back satisfactory information on the geology of the country traversed, and of its fauna and flora, past and present; already the revelations which have been made of the past geography of South America and of the life that flourished there in former epochs are of the highest interest. Moreover, we have here the remains of extinct civilizations to deal with, and although much has been done in this direction, much remains to be done, and in the extensive region already referred to, the physique, the traditions, and the customs of the natives will repay careful investigation.

The southern continent of Australia is in the hands of men of the same origin as those who have developed to such a wonderful extent the resources of Canada and the United States, and therefore we look for equally satisfactory results so far as the characteristics of that continent permit. The five colonies which divide among them the 3,000,000 square miles of the continent have each of them efficient government surveys, which are rapidly mapping their features and investigating their geology. But Australia has a trying economic problem to solve. In none of the colonies is the water supply quite adequate; in all are stretches of desert country of greater or less extent. The center and western half of the continent is covered by a desert more waterless and more repellent than even the Sahara; so far as our present knowledge goes one-third of the continent is uninhabitable. This desert area has been crossed by explorers, at the expense of great sufferings, in various directions, each with the same dreary tale of almost featureless sandy desert, covered here and there with Spinifex and scrub worse than useless. There are hundreds of thousands of square miles still unknown, but there is no reason to believe that these areas possess any features that differ essentially from those which have been found along the routes that have been explored. There have been one or two well-equipped scientific expeditions in recent years that have collected valuable data with regard to the physical characteristics, the geology and biology of the continent; and it is in this direction that geography should look for the richest results in the future. There remains much to be done before we can arrive at satisfactory conclusions as to the physical history of what is in some respects the most remarkable land area on the globe. Though the surface water supply is so scanty there is reason to believe that underneath the surface there is an immense store of water. In one or two places in Australia, especially in western Queensland, and in New South Wales, this supply has been tapped with satisfactory results; millions of gallons a day have been obtained by sinking wells. Whether irrigation can ever be introduced on an extensive scale into Australia depends upon the extent and accessibility of the underground water supply, and that is one of the geographical problems of the future in Australia. New Zealand has been fairly well surveyed, though a good deal remains to be done before its magnificent mountain and glacier system is completely known. In the great island of New Guinea both the British and the Germans are opening up the interiors of their territories to our knowledge, but the western and much larger portion of the island presents a large field for any explorer who cares to venture into its interior.

The marvelous success which has attended Dr. Nansen's daring adventure into the Arctic seas has revived a widespread interest in Polar exploration. Nansen may be said to have almost solved the North Polar problem—so far, at least, as the Old World side of the Pole is concerned. That someone will reach the Pole at no distant date is certain; Nansen has shown the way, and the legitimate curiosity of humanity will not rest satisfied till the goal be reached. But Arctic exploration does not end with the attainment of the Pole. Europe has done her share on her own side of the Pole. What about the side which forms the Hinterland of North America, and specially of Canada? To the north of Europe and Asia we have the scattered groups of islands Spitsbergen, Franz Josef Land, Novaya Zemlya, and the New Siberian Islands. To the north of America we have an immense archipelago, the actual extent of which is unknown. Nansen and other Arctic authorities maintain that the next thing to be done is to complete exploration on the American side, to attempt to do for that half of the North Polar region what Nansen has done for the other half. It may be that the islands which fringe the northern shores of the New World are continued far to the north; if so they would form convenient stages for the work of a well-equipped expedition. It may be that they do not go much farther than we find them on our maps. Whatever be the case it is important, in the interests of science, that this section of the Polar area be examined; that as high a latitude as possible be attained; that soundings be made to discover whether the deep ocean extends all round the Pole. It is stated that the gallant Lieutenant Peary has organized a scheme of exploring this area which would take several years to accomplish. Let us hope that he will be able to carry out his scheme. Meantime, should Canada look on with indifference? She has attained the standing of a great and prosperous nation. She has shown the most commendable zeal in the exploration of her own immense territory. She has her educational, scientific, and literary institutions which will compare favorably with those of other countries; her press is of a high order, and she has made the beginnings of a literature and an art of her own. In these respects she is walking in the steps of the mother country. But has Canada not reached a stage when she is in a position to follow the maternal example still further? What has more contributed to render the name of Great Britain illustrious than those great enterprises which for centuries she has sent out from her own shores, not a few of them solely in the interests of science? Such enterprises elevate a nation and form its glory and its pride. Surely Canada has ambitions beyond mere material prosperity, and what better beginning could be made than the equipment of an expedition for the exploration of the seas that lie between her and the Pole? I venture to throw out these suggestions for the consideration of those who have at heart the honor and glory of the great Canadian Dominion.

Not only has an interest in Arctic exploration been revived, but in Europe at least an even greater interest has grown up in the exploration of the region around the opposite pole of the earth, of which our knowledge is so scanty. Since Sir James C. Ross's expedition, which was sent out in the year 1839, almost nothing has been done for Antarctic research. We have here to deal with conditions different from those which surround the North Pole. Instead of an almost landless ocean, it is believed by those who have given special attention to the subject that a continent about the size of Australia covers the South Polar region. But we don't know for certain, and surely, in the interests of our science, it is time we had a fairly adequate idea of what are the real conditions. We want to know what is the extent of that land, what are its glacial conditions, what is the character of its geology, what evidence exists as to its physical and biological conditions in past ages. We know there is one lofty, active volcano; are there any others? Moreover, the science of terrestrial magnetism is seriously impeded in its progress because the data in this department from the Antarctic are so scanty. The seas around this continent require to be investigated both as to their depth, their temperature, and their life. We have here, in short. the most extensive unexplored area on the surface of the globe. For the last three or four years the Royal Geographical Society, backed by other British societies, have been attempting to move the Home Government to equip an adequate expedition to complete the work begun by Ross sixty years ago and to supplement the great work of the *Challenger*. But though sympathy has been expressed for Antarctic exploration, and though vague promises have been given of support, the Government is afraid to enter upon an enterprise which might involve the services of a few naval officers and men. We need not criticise this attitude. But the Royal Geographical Society has determined not to let the matter rest here. It is now seeking to obtain the support of public-spirited men for an Antarctic expedition under its own auspices. It is felt that Antarctic exploration is peculiarly the work of England, and that if an expedition is undertaken it will receive substantial support from the great Australasian colonies, which have so much to gain from a knowledge of the physical condition of a region lying at their own doors, and probably having a serious influence on their climatological conditions. Here, then, is one of the greatest geographical problems of the future, the solution of which should be entered upon without further delay. It may be mentioned that a small and well-equipped Belgian expedition has already started, mainly to carry out deep-sea research around the South Polar area, and that strenuous efforts are being made in Germany to obtain the funds for an expedition on a much larger scale.

But our science has to deal not only with the lands of the globe; its sphere is the whole of the surface of the earth, and all that is thereon, so far at least as distribution is concerned. The department of oceanography is a comparatively new creation; indeed, it may be said to have come definitely into being with the famous voyage of the *Challenger*. There had been expeditions for ocean investigation before that, but on a very limited scale. It has only been through the results obtained by the *Challenger*, supplemented by those of expeditions that have examined more limited areas, that we have been able to obtain an approximate conception of the conditions which prevail throughout the various ocean depths—conditions of movement, of temperature, of salinity, of life. We have only a general idea of the contours of the ocean bed and of the composition of the sediment which covers that bed. The extent of the knowledge thus acquired may be gauged from the fact that it occupies a considerable space in the fifty quarto volumes—the *Challenger* publications—which it took Dr. John Murray twenty years to bring out. But that great undertaking has only, as it were, laid down the general features of the oceanic world. There is plenty of room for further research in this direction. Our own surveying ships, which are constantly at work all over the world, do a certain amount of oceanic work, apart from mere surveying of coasts and islands and shoals. In 1895 one of these found in the South Pacific soundings deeper by 500 fathoms than the deepest on record, that found twenty years earlier by the *Tuscarora* to the northeast of Japan. The deepest of these new soundings was 5,155 fathoms. In the interests of science, as well as of cable laying, it is desirable that our surveying ships should be encouraged to carry out work of this kind more systematically than they do at present. This could surely be arranged without interfering with their regular work. We want many more observations than we now have, not only on ocean depths, but on the nature of the ocean bed, before we can have a satisfactory map of this hidden portion of the earth's surface and form satisfactory conclusions as to the past relations of the ocean bed with what is now dry land. I believe the position maintained by geologists, that from the remote period when the great folds of the earth were formed the present relations between the great land masses and the great oceans have been essentially the same; that there have no doubt been great changes, but that these have been within such limits as not to materially affect their relations as a whole. This is a problem which further oceanic research would go a long way to elucidate. That striking changes are going on at the present day, and have been going on within the human period can not be doubted. Some coast lines are rising; others are falling. Prof. John Milne, our great authority on seismology, has collected an extremely interesting series of data as to the curious changes that have taken place in the ocean bed since telegraphic cables have been laid down. The frequent breakages of cables have led to the examination of the suboceanic ground on which they have been laid, and it is found that slides and sinkings have occurred, in some cases amounting to hundreds of fathoms. These, it is important to note, are on the slopes of the continental margin, or, as it is called, the continental shelf, as, for example, off the coast of Chile. It is there, where the earth's crust is peculiarly in a state of unstable equilibrium, that we might expect to find such movements; and therefore soundings along the continental margins, at intervals of, say, five years, might furnish science with data that might be turned to good account.

As an example of what may be done by a single individual to elucidate the present and the past relations between land and sea, may I refer to an able paper in the Geographical Journal of May, 1897, by Mr. T. P. Gulliver, a pupil of Professor Davis, of Harvard, himself one of the foremost of our scientific geographers? Mr. Gulliver has made a special study on the spot, and with the help of good topographical and geological maps, of Dungeness Foreland on the southeast coast of Kent. Mr. Gulliver takes this for his subject, and works out with great care the history of the changing coast line here, and in connection with that the origin and changes of the English Channel. This is the kind of work that well-trained geographical students might undertake. It is work to be encouraged, not only for the results to be obtained, but as one species of practical geographical training in the field, and as a reply to those who maintain that geography is mere bookwork, and has no problem to solve. Professor Davis himself has given an example of similar practical work in his elaborate paper on "The development of certain English rivers" in the Geographical Journal for February, 1895 (Vol. V. p. 127), and in many other publications.

Another important problem to attack, and that in the near future, is that of Oceanic Islands. I say in the near future, because it is to be feared that very few islands now remain unmodified by contact with Europeans. Not only have the natives been affected, both in physique and in customs, but the introduction of European plants and animals has to a greater or less extent modified the native fauna and flora. Dr. John Murray, of the *Challenger*, has set a good example in this direction by sending a young official from the Natural History Museum to Christmas Island, in the Indian Ocean, one of the few untouched islands that remain, lying far away from any other land, to the south-east of the Keelings.

What islands are to the ocean, lakes are to the land. It is only recently that these interesting geographical features have received the attention they deserve. Dr. Murray has for some time been engaged in investigating the physical conditions of some of the remarkable lakes in the west of Scotland. Some three years ago my friend and colleague, Dr. Mill, carried out a very careful survey of the English lakes, under the auspices of the Royal Geographical Society. His soundings, his observations of the lake conditions, of the features on the margins of and around the lakes, when combined with the investigation of the régime of the rivers and the physical geography of the surrounding country, conducted by such accomplished geologists as Mr. Marr, afford the materials for an extremely interesting study in the geographical history of the district. On the Continent, again, men like Professor Penck, of Vienna, have been giving special attention to lakes, that accomplished geographer's monograph on Lake Constance, based on the work of the five States bordering its shores, being a model work of its kind. But the father of limnology, as this branch of geography is called, is undoubtedly Professor Forel, of Geneva, who for many years has been investigating the conditions of that classical lake, and who is now publishing the results of his research. Dr. Forel's paper on "Limnology, a branch of geography," and the discussion which follows in the report of the last International Geographical Congress, affords a very fair idea in short space of the kind of work to be done by this branch of the science. In France, again, M. Delebecque is devoting himself to a similar line of research; in Germany, Ule, Halbfass, and others; Richter in Austria, and the Balaton Commission in Hungary. I may also here refer appropriately to Mr. Israel C. Russell's able work, published in Boston in 1895, on "The Lakes of North America," in which the author uses these lakes as a text for a discourse on the origin of lake basins and the part played by lakes in the changes studied by dynamic geology. One of the best examples of an exhaustive study of a lake basin will be found in the magnificent monograph on Lake Bonneville, by Mr. G. K. Gilbert, and that on Lake Lahontan by Mr. Israel Cook Russell, published by the United States Geological Survey; the former is, indeed, a complete history of the great basin, the largest of the interior drainage areas of the North American continent. In the publications of the various surveys of the United States, as well as in the official reports of the Canadian lake surveys, a vast amount of material exists for anyone interested in the study of lakes; in addition, the elaborate special reports on the great lakes by the hydrographic department. Indeed, North America presents an exceptionally favorable field for limnological investigation; if carried out on a systematic method, the results could not but be of great scientific interest.

Rivers are of not less geographical interest than lakes, and these have also recently been the subject of special investigation by physical geographers. I have already referred to Professor Davis's study of a special English river system. The work in the English lake district by Mr. Marr, spoken of in connection with Dr. Mill's investigations, was mainly on the hydrology of the region. Both in Germany and in Russia special attention is being given to this subject, while in America there is an enormous literature on the Mississippi alone, mainly, no doubt, from the practical standpoint, while the result of much valuable work on the St. Lawrence is buried in Canadian official publications.

But time does not admit of my going further. I might have pointed out the wide and vastly interesting field presented by what the Germans call anthropogeography, dealing with the interrelations between humanity and its geographical environment. Geography, Mr. Mackinder has said, is the physical basis of history; it is, indeed, the physical basis of all human activity, and from that standpoint the field for geographical research is unbounded. But I can only hint at this. I have endeavored to indicate what are some of the leading geographical problems of the future; first, in order to show at this somewhat critical epoch how very much yet remains to be done, how many important lines of inquiry are open to the geographical student, and that the possibilities of our science are, like those of other departments of research, inexhaustible. My aim has also been to indicate by actual examples what, in the conception of British geographers at least, is the field of our subject. We need not trouble greatly about any precise definition so long as there is such a choice of work for the energies of the geographer. I trust I have been, to some extent at least, successful in the double object which I have had in view in this opening address in a country which presents so splendid a field to the practical geographer.

LETTERS FROM THE ANDRÉE PARTY.[1]

THE BALLOON EXPEDITION TO THE POLE—AN ACCOUNT OF THE START BY ANDRÉE'S FELLOW-VOYAGER, NILS STRINDBERG—LETTERS RELATING TO THE EXPEDITION FROM STRINDBERG'S FATHER.

On the 11th of last July, one Sunday afternoon, S. A. Andrée, with two companions, Nils Strindberg and Knut Fraenkel, ascended from Danes' Island in the balloon "Ornen" (The Eagle) and sailed away northward, hoping by this untried means to reach the North Pole. Daring even to foolhardiness as Andrée's project may well seem, it had been very coolly and prudently matured and systematically prepared for. Andrée was born in Sweden October 18, 1854, and is now, therefore, 43 years old. He is a carefully educated mechanical engineer and man of science. From 1886 to 1889 he filled a chair in the leading Swedish school of technology; he passed the winter of 1882–83 in Spitzbergen as a member of a Swedish meteorological expedition, directing experiments and observations in atmospheric electricity, and he has held for some years an important engineering post under the Swedish Government. In 1876, while on his way to America to serve the Swedish exhibitors at the Centennial Exhibition, he was impressed with the seeming regularity of the trade winds, and thus was led to consider the possibility of balloon voyages across the Atlantic. His coming to America augmented also in another way his interest in ballooning. In a little speech spoken by him into a gramophone, for use at a Swedish Aid Society's fair holding in Brooklyn while he was preparing for his journey to the Pole, Mr. Andrée said:

"It is a great pleasure for me to be able to contribute to the Swedish Aid Society's fair. I have been in America myself and have experienced how hard it is to be without work. I was glad many times to make my living by wielding a broom. In spite of that I have many pleasant recollections from that time, because I learned a great deal while staying there. It was there I met the old aeronaut John Wise, from Philadelphia, and it was there I got the first lesson in the manufacturing of balloons. For me is America, therefore, indeed memorable, and the Americans can rest assured that I should like very much, if I could, to visit them with my balloon via the North Pole."

Early in 1895 Mr. Andrée laid his ideas for a balloon expedition into the Arctic, then pretty well matured, before the Swedish Academy of

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[1] Reprinted from McClure's Magazine for March, 1898, by permission of the S. S. McClure Company.

Science. Later in the same year he presented them in England before the International Geographical Congress. He estimated that he would require for his project a little over $36,000. In time the money was provided, mainly by the generosity of Mr. Alfred Noble, who died, however, before Andrée could make his start; Baron Oscar Dickson, who died soon after the start; and the King of Sweden. Andrée had now been studying balloons with great care for some years. He had himself made a number of ascensions, and he had had some very thrilling and dangerous adventures. With the money he required made secure, he set about the construction of a balloon especially suited to his purpose.

THE BALLOON.

The "Ornen" was built by M. Lachambre, the well-known balloon maker of Paris, at an original cost of $10,000. The balloon proper was originally 97 feet through from top to bottom; and at the widest part 67¼ feet through from side to side; After the failure to make a start in 1896, Andrée decided to enlarge it, and it was carried back to Paris, cut in two at the middle, and an additional section inserted about 3¼ feet high. The perpendicular diameter was thus increased by about that much, but the horizontal diameter remained as before. By this enlargement the volume of the balloon was increased 10,600 cubic feet, becoming in all 170,000 cubic feet. It is made of silk—three thicknesses through the upper two-thirds, and two through the lower third, all varnished twice over, inside and out. Over all the seams are laid protecting strips, and to doubly insure tightness these were varnished at the edges, just before the start, with a varnish especially devised for this use. There are two valves about halfway up the balloon, nearly, but not quite, opposite each other; and there is a third at the bottom. The latter works automatically; the others are controlled by ropes attached to them on the inside and coming out of the balloon at the bottom beside the third.

The balloon is encased in a heavy netting of hemp, woven above, with much intricacy, of 384 separate ropes, and ending below in 48 "suspension" ropes, to which is attached what is known as the "bearing ring." This ring is a part of great importance; it is to the balloon much what the keel is to a ship. It is about 7½ yards in circumference, is made of wood, and is braced with cross-bars.

To the bearing ring is attached the car, or basket, by six ropes, each about 1⅕ inches in diameter. These ropes are knitted into the wall of the car, and fastened securely at the bottom of it. Above the car they are encircled and braced by five horizontal ropes, equidistant from each other, which thus form a series of guard rails. Above these, about 6½ feet from the roof of the car, is yet another; it is much shorter, and draws the suspending ropes into a circle of about half the diameter of that made by the lower ones.

The car is cylindrical in form, about 6½ feet in diameter and 5 in depth. It is of wicker, woven over a frame of chestnut wood. Iron and steel were avoided in its construction, lest they might disturb the action of the magnetic instruments with which the balloon is equipped. At one side, on the lower edge, the car is sheared, or beveled, away, in order that on landing it may strike more gently and not be overturned. Well up in the wall of the car are two small windows closed with glass, and near the bottom are two openings closed with wood, while through the roof is a trap door. The whole car is covered with tarpaulin.

The interior of the car is chiefly for rest and retirement. The place for work and observation is the roof. Here is erected a sort of swinging gallery, free at the bottom, so that it may remain horizontal under the tip of the balloon, and shielded somewhat from the weather by a curtain of tarpaulin. In this gallery were placed the scientific instruments: thermometers, barometers, cameras, and so on—a full equipment; and here two of the aeronauts would keep an outlook and manage the balloon, while the third took his rest in the car below. A sleeping bag (a hair mattress encased in reindeer skin) occupied the middle of the car; and all about, in ingenious compartments, were stored books, maps, instruments, toilet articles, kitchen utensils, arms, ammunition, and what not.

The main places of storage, however, were the bearing ring, which with its cross braces formed a sort of garret floor whereon were stowed various tools and implements, such as shovels, anchors, and reserve ropes; and the spaces between the forty-eight suspension ropes above the bearing ring. Securely hung in these spaces were forty-eight large, strong cloth sacks, divided into numerous compartments. In twelve were stowed sledges, boats, sail yards, and kindred articles; in thirty-six were stored provisions.

ANDRÉE'S PROVISIONS.

Andrée's store of provisions, since his fate became so much of a mystery, has grown to be a subject of great interest. Thousands of letters, from all parts of the world, have gone to the Academy of Science at Stockholm asking about it; and finally, in order to satisfy public curiosity, King Oscar of Sweden requested Dr. Beauvais of Copenhagen, head of the house that supplied Andrée, to make a report on the amount of provisions he carried. Dr. Beauvais has just reported as follows:

"The Andrée expedition has provisions for nine months. All the boxes in which the conserved food is kept were made of copper, as iron would have had a disastrous effect on the magnetic instruments carried by the expedition. To occupy as little space as possible they were made square instead of round. The food consists of every kind of steaks, sausages, hams, fish, chickens, game, vegetables, and fruit. If these provisions have been saved, together with the food which the explorers can procure through fishing and hunting, they have sufficient provisions to last them two years.

"The expedition is also furnished with a new kind of lozenges of concentrated lemon juice. This is the first time these have been used by Polar expeditions, and it is expected they will absolutely prevent every attack of scurvy.

"Finally, the expedition is provided with 25 kilos (about 55 pounds) of thin chocolate cakes, mixed with pulverized pemmican. To preserve this food against dampness it is packed in pergament, covered with stannine, a brittle metal composed of tin, sulphur, and copper, and inclosed in air-tight boxes. Nansen's expedition was also provided with this food, and it was found to be both nourishing and pleasant to the taste."

Even a means of cooking was not lacking from the outfit. A stove about 10 by 17 inches, heated by a spirit lamp, was carried along; and, in order to avoid the danger of using it near the gas of the balloon, it was so devised and placed that it could be lighted and operated hanging 25 feet below the roof of the car.

To aid in steering and controlling the balloon, Andrée devised an apparatus of sails and guide ropes—three sails, presenting to the wind when full spread a surface of 800 square feet; and three guide ropes, one about 1,017 feet long, another about 1,042 feet and the third about 1,205 feet. The ropes trail from the bearing ring, and are attached to it in such wise that they can be shifted from point to point; and by thus shifting them, the theory at least is that there can be a corresponding shift made in the course of the balloon. The sails are hung two from bamboo spars projecting from the bearing ring, and one above the bearing ring between the suspension ropes.

Andrée's first design was to sail in the summer of 1896. The balloon and all stores and appliances were conveyed to Danes' Island; a balloon house was erected, and engines set up for producing hydrogen gas and inflating the balloon. All, indeed, was made ready; but the south wind they wanted for the start did not come. They waited for it until the season had advanced too far for a safe venture, and then they came back to Sweden. In May, 1897, they returned, and by July 1 again had everything ready for a start. And again the south wind refused to come. They had to wait ten days for it. We have a very interesting view of the party at this trying time, as well as a full account of the work they had had to do in getting ready, in the following letter, written by Andrée's companion, Nils Strindberg, to his brother in New York and not before published:

LETTER FROM NILS STRINDBERG.

"Yes, now the folks at home believe us to be ascended. From Anna I had no letter, and papa was very doubtful about his letter reaching me. But alas! it is true that we have not yet departed. As you have probably heard through the papers or letters from home, we anchored the 30th of May in Virgo Harbor, after having been detained by the ice in Danes' Gate. It seems to have been an exceptionally mild winter. There is considerably less snow this year than last, which still was milder than the average winter. The balloon house stood when we arrived, but was so damaged by the winter storms that it was on the verge of collapsing. But one must remember that it was only calculated to remain for one summer. With the aid of tackle and buttresses it was soon fixed, and June 14 we brought the balloon from the *Virgo* to the balloon house. On the 16th the balloon was stretched out on the floor, which had been covered with thick coarse felt. The *Virgo* left Danes' Island on the 16th. And now we had our hands full to make the balloon tight and to inflate it. To make it tight we had to varnish all the seams on the outside as well as the inside. In order to varnish the inside the balloon is partly inflated with air by a large bellows, and the workmen crawl in through the lower opening. Svedenborg, Fraenkel, Machuron, and myself take turns in the superintending of the inside varnishing. The interior of the balloon is a very strange sight. It looks like a low vault of stone masonry. * * * There we were, eight men, each with a pot of varnish and a brush, and varnished every seam of the upper half of the balloon. The varnish makes the air very bad, and after some time one begins to feel a pain in one's eyes, as of onions.

"On Saturday, the 17th, at 7 o'clock in the morning, the hydrogen apparatus was started and put in connection with the balloon, and at 12 o'clock, midnight, between the 22d and 23d, it was inflated. Then it had to be tested as to its tightness and the principal holes fixed. This was done by a new method invented by Mr. Stake. It is simply to allow the few particles of hydrogen sulphide, which are always produced with the hydrogen, to accompany the hydrogen into the balloon. If pieces of muslin saturated with a solution of acetate of lead are put on the balloon, the smallest leakage may be discovered by the escaping hydrogen sulphide, which causes the muslin to turn black. This method proved to be very practical, and we discovered several small holes which could be fixed. During these operations one walks around on top of the balloon, which only yields imperceptibly. * * *

"After these preparations we have succeeded in getting the balloon in pretty good shape; at all events much better than last year. It loses daily about 45 kilos (a fraction over 99 pounds) in carrying capacity; but as we have possibilities of throwing out 1,700 kilos (about 3,748 pounds) of ballast, we will easily float for more than a month.

"We do not intend to start until we get favorable wind, to avoid being pushed right back to Spitzbergen by contrary winds. If we get the right wind we ought to be able to go some distance in these thirty days. With a fairly strong wind we will make from 10 to 20 knots an hour, and will reach the Pole, or a point near to it, in from thirty to sixty hours. Once having reached the northernmost point, we don't care where the wind carries us. Of course we would rather land in Alaska near the Mackenzie River, where we would very likely meet American whalers, who are favorably disposed toward the expedition. It would really be a glorious thing to succeed so well. But even if we were obliged to leave the balloon and proceed over the ice, we shouldn't consider ourselves lost. We have sledges and provisions for four months, guns, and ammunition; hence are just as well equipped as other expeditions as far as that is concerned. I would not object to such a trip. The worst thing is that the folks at home will feel uneasy if we don't appear in the fall, but are obliged to spend the winter in the Arctic regions. My body is now in such good condition, and I have got so accustomed to the Arctic life, that a winter up here don't seem terrible at all. One gets used to everything. But the best thing would be to come home in the fall. * * *

"Well, I hope we shall soon have favorable winds. On the 8th day of July we had a strong southerly wind, but then it was too strong. It was almost a gale, and it would have been impossible to ascend without damage to the balloon. Later it shifted over to the west too much. If we don't get a southerly wind before the 15th of July we intend to try with a southeasterly, to be carried north of Greenland, and there possibly utilize the south winds which, according to Lieutenant Peary, are prevalent during summer.

"Well, good-bye, now, brother; just wonder if we will meet next time in New York. Send my love to Uncle and Aunt Outad and the boy, also to the Ellnrod family. Tell them that nowadays I write to nobody but my fiancée. Got no time for more.

"Your brother, NILS.

"The *Lofoten*, which arrived this morning at 7 o'clock, has left already at 10; so this will have to go by the next mail."

THE START.

When the members of the party arose on the morning of July 11, they sent up a joyous cry of "A strong, steady wind from the south!" What followed this bestirring announcement has been very well described by one of the party, and we can not do better than to quote his account:

"After a short discussion on the morning of the 11th, Mr. Andrée and his companions decided to ascend as soon as possible. Now followed some hours of great activity. Everyone felt perceptibly the importance of the moment, and all demonstrated this in an excellent way. Through the roaring storm, which so powerfully pressed against the balloon house that it cracked and squeaked in all its joints, Mr. Andrée's powerful voice was heard, now from the outside, now from the inside, and now again from the top of the colossal building, giving orders and superintending the last preparations for this long-planned journey, which had cost so much effort and so much anxiety and for which so much was risked. All that was invested in the undertaking could still be lost at the very start.

"The wind is roaring, and the gigantic balloon pulls and pulls at its anchorage, sometimes with threatening force. Heavy clouds come tearing down from the mountain tops; a sudden gush of wind strikes the house, and it crashes more than ever. One of the poles at the upper balcony, to which canvas is fastened for protection against the wind, yields to the pressure and falls over the balloon, and might cause the whole expedition to come to naught, did not quick hands check it in its fall. The whole thing seems to hang on a hair. But Andrée does not seem at all excited. He takes in every detail of the preparations, and gives his orders, which are carried out rapidly and carefully.

"In about an hour's time the north wall of the house is torn partly down, and all hands are called to assist in raising and managing the balloon. Finally there is nothing left to do but attach the car—an extremely difficult job, as the raised balloon sways to and fro more than before. But even this is accomplished successfully, and now, about three and one-half hours after the work began, our three daring countrymen are ready to start on their hazardous journey. A few moments for the last farewells, and Andrée with his two companions, Nils Strindberg and Knut Fraenkel, jumps aboard the 'Ornen,' and orders are given to cut the retaining ropes. The captain of the *Svensksund*, Count Ehrensvard, proposes a 'long life' for Mr. Andrée, which is given with four hearty hurrahs. Andrée and his companions answer with, 'Long live old Sweden!'

"As the last ropes are loosened I hurry up a hill behind the balloon house to take photographs of the ascending balloon. Just as I reach my elevated position, the immense balloon slowly and majestically rises out of its prison. On account of its undulations the lower part catches on something connected with the house, but slips off again the next moment, and the balloon rises to between 600 and 700 feet, at the same time moving in a northeasterly direction out over Danes' Gate. But suddenly it drops down again, in a course straight toward the sea, being depressed by a current of air that has descended suddenly upon it from the mountain top, and also being somewhat pulled down by the catching of the guide ropes. The car touches the waves, but like a giant ball the balloon rebounds, and when some sand bags are thrown out (nine bags, each weighing about 42 pounds), it rises until it reaches a height of about 3,000 feet. Then flying free, it continues at the height of about 3,000 feet, first in a northeasterly direction over Danes' Gate and toward the southern cape of Amsterdam Island. This it passes, and then turns toward the north, keeping over the sound between Amsterdam Island and Fogelsang. After a while it again turns toward the northeast and passes the northern cape of Fogelsang. Then it disappears in a cloud. But in a short while it reappears in a north-northeasterly direction, between Fogelsang and Cloven Cliff, then changes toward the west, and finally disappears altogether—about an hour after the ascension."

LETTERS FROM STRINDBERG'S FATHER.

Nils Strindberg's brother in New York received from his father in Sweden a number of letters written about the time the expedition started and a little after that give interesting information regarding it and its members. We print here the important parts of these letters, no portion of which has been published before:

"On Saturday [May 8] we have a few of Nils's friends for dinner to say good-bye. But we are not able to have Andrée with us, because his mother died a few days ago from paralysis of the heart, and he is now down to her funeral."

"Nils was calm all the time [May 15] except when he was leaving the house, when he burst out weeping for a few moments. He is indeed a man, for he left the dearest he has on earth [his fiancée] to carry out a great idea, and therefore I do think we shall see him back again, after a successful trip. Andrée was as calm as a summer sea."

"It was so strange [when a picture of the ascension reached them]; all the time one could imagine the 'Ornen' soaring away over the ice and snow toward the unknown—to land where? and when? and how? And then?"

"The day after Anna [Miss Chaslier, Strindberg's fiancée] accompanied me into the city to meet Svedenborg. Of course it was interesting to hear eyewitnesses relate the story, although not much was told that had not been in the papers. Both Anna and myself had letters from Nils written the morning of the ascension day; calm and sure as always. It was Nils who called out, 'Long live old Sweden' when the balloon rose out of the house. The last words Andrée was heard to utter were 'What was that?' when the balloon caught somewhere for a moment. Svedenborg had saved, and presented to Anna, the sand bag Nils cut off at the start. I got another. Anna also got the pigeon, in a small cage, with the message. It was brought out in the country and well cared for; but when we moved to the city, she followed my advice and had it killed and stuffed—and soon she will have it back in flying position as a permanent souvenir from the dearest she has, poor thing."

"And so one has to go on and hope for a year at least; and even after that don't draw too unfavorable conclusions, for they may have long distances to walk before they reach inhabited places."

"At present I read Nansen's book with great interest, and in my thoughts I place 'the three' in the same or similar situations. Since they have rifles and sufficient ammunition and the necessaries for a journey over the ice and a stay over the winter, I suppose they can do it, although with difficulties to overcome."

"Andrée and Nils, whom I know best, are such characters that, if possible, they make the impossible possible; and they have surely intelligence enough to figure out the best way of getting out of their emergencies. Andrée's ideas and Nils's Anna are two mighty levers and self-protections, and the love of life will help along, too."

ACCOUNT OF THE START OF ANDRÉE, BY AXEL STAKE.[1]

I was the chemist of the expedition which fitted out Andrée for his North Pole journey. I made the gas which carried his balloon away to the north. I kept a diary of all the events that happened from the time the expedition was first assembled in Sweden until Andrée and his companions disappeared beyond Fogelsang on the northern horizon. From what I have seen printed in the papers both here and abroad I do not think that all of the happenings of the departure can be known to the public at large. For instance, it may not be known generally that Andrée was very reluctant to depart on his voyage on that rather memorable July 11. His own wish was to defer the start to the next day. I do not believe he would have gone on the 11th had he not been urged to go by his companions. He is very painstaking and careful as a rule, and in his middle age far more discreet than the youth of his companions allowed them to be. But Strindberg and Frankel had waited so long and hoped so much for a breeze from the south that they were eager to be off. They were afraid the breeze would die away and the expedition would be left stranded on Danes' Island, as it was in 1896.

I remember very well the morning of the 11th. Strindberg and I occupied the same cabin on board the Swedish gunboat which carried us up to the island. Strindberg came running to me that morning and awoke me in my bunk, crying, "The breeze! the breeze! We shall sail to-day. The wind is from the south." I laughed at him, for I did not believe it was possible. But when I came out on the deck I found that the preparations for the start had already begun. Andrée was doubtful. In his mind this southerly wind might be a false alarm. He thought they had better wait a day or so and see if it would continue. A conference was held on the vessel, after which, reluctant as yet, Andrée went ashore to the balloon house to see if the breeze was quite as strong there as it was on board the gunboat. During the morning he had been making meteorological observations, and the results added force to the pleadings of his companions. He came on board the vessel again, and a second conference was held. It was finally decided to go that day, and immediately the order to knock down the front section of the balloon house was given. This was at 10.30 a. m. At 2.30 in the afternoon the balloon sailed away.

Andrée went away with the impression that the balloon would float at least six weeks. Indeed, it was his idea in 1896 that he could keep in the air for a year or more if necessary, but the trouble we had with escaping gas soon dispelled this notion. I think that even after he found how impossible it was to confine the gas he overrated the time he would be able to keep afloat. The expert from the balloon factory

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[1] Published by McClure Syndicate, March 13, 1898. Not included in the magazine article.

and I made a minute calculation of how long the gas would remain in the bag, taking into consideration its slow escape through the minute interstices which we found it impossible to close up. Our calculation was that the balloon would remain afloat, barring accidents, not longer than fifteen days. The "Ornen" probably was the tightest balloon bag ever made, but we could not close up some of the holes. I invented a process for detecting the escape of the gas. After the balloon was inflated we spread long strips of sheeting saturated with acetate of lead over the top. The confined hydrogen sulphide as it escaped would, on coming in contact with the sheeting, cause the latter to become discolored. Thus the exact location of every hole could be ascertained.

Even so, although we varnished and revarnished the silk inside and out, we could not prevent the almost imperceptible holes. The greatest trouble was in the seams where the sections of the great bag were joined. The finest needle hole was sure to show a leak, even after the stitching had been done as neatly as possible. It may not be known that the successive varnishings could be done satisfactorily only on the upper part of the bag, against which the greatest outward pressure of the confined gas was exerted. Of course we would have done the lower part of the balloon more thoroughly, but we had not time. The aeronauts were eager to sail with the first good south wind, and we had to let them go. Strindberg had made some experiments to reduce the outflow of the gas, but they were without success. The constant smearing on of the gutta-percha which we used was really the best we could do. Our estimate of fifteen days' duration for the gas, small as it was, would provide for its retention twice the length of time that gas has ever before been confined in a balloon. I believe that no balloon heretofore has floated longer than a week.

Andrée was handicapped at the start by the loss of two-thirds of his drag ropes, upon which he depended to steer his balloon. The accident was a curious one, and while it could not have been foreseen, yet the conditions under which it happened might have been avoided if different arrangements had been made. The drag ropes of the balloon, which were about 1,000 feet long, were in three sections, and were joined together by metal screw couplings. The couplings could be screwed apart, the inference being, I suppose, that if Andrée wanted to do so he could unscrew and cast off any part of the drag rope. Why he could not just as easily have cut them apart I do not know. Now, when the balloon was ready to start, the drag ropes attached to the lower side of the basket were allowed to trail up over the top edge of the balloon house and down again to the beach along which they were trailed, so as to be clear of all obstructions and ready to follow the balloon out to sea when it rose out of its nest.

But the heavy weight of the ropes defeated this purpose. The part of the ropes which lay outside on the beach offered an immense friction, which the balloon seemed unable to overcome. Instead of following the bag out of the house, uncoiling as they went, the upper sections of the drag ropes twisted, and under the severe strain the couplings unscrewed. For a moment it seemed that the balloon would not get away; that the friction of the heavy ropes would hold her to the shore. Then, to our astonishment, the couplings parted and the airship darted upward.

The question whether or not the loss of these ropes would prevent Andrée from steering the balloon has been openly discussed. It is impossible to tell, of course, though he may have remedied the defect by putting out another drag rope composed of the rope which hung from the basket and which for the time being was used as ballast.

When the balloon rose out of the house some portion of it caught on the structure of the balloon house. Andrée was heard to exclaim "What was that?" Then we heard Strindberg crying, "Long live old Sweden!" A boat had pulled out from the shore, and as the bag tore away Andrée grabbed a speaking trumpet and shouted to those in the boat. From his motions every one believed he was trying to say something about the loss of the drag ropes, but no one could understand what he said, and as the balloon got farther and farther away, the difficulty of making himself understood became greater and greater.

Andrée lost much ballast and much gas before the balloon passed out of sight. After its first jump upward from the balloon house it was depressed toward the water by the air current coming down from the mountains behind us. It got so near the surface of the water that for a moment we wondered whether the expedition wasn't going to end right there. Nobody spoke, but everybody was filled with excitement all the more intense because no sound was uttered. Then the balloonists began throwing out ballast, 9 bags of sand, weighing about 378 pounds. After that the balloon went upward. It reached over 3,000 feet in height; then it went forward again. Later on it was depressed, evidently through the escape of gas, which the aeronauts permitted to flow through the valves. Finally, when it rose over Vogelsung, more ballast must have been dispensed with in order to accomplish the purpose.

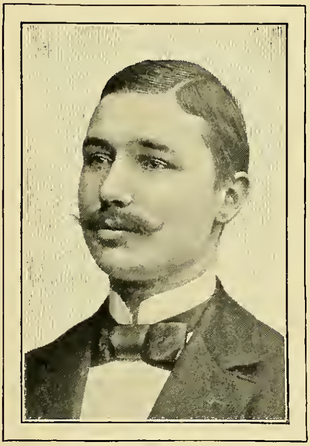
Of course it is impossible to surmise where the explorers are at present, if they have escaped the bad effects of Arctic exposure. It is naturally impossible for the balloon to have floated until this time, and in reasoning out any course of safety for the balloonists we must presuppose that they descended safely on some land. We know from the message received by means of the carrier pigeons that were shot in the rigging of the sealing vessel *Alken* in the vicinity of Spitzbergen, that Andrée did not continue northward. According to the dispatch he was headed in an easterly direction, after having gone 145 geographical miles to the north. He had already gone 45 miles to the eastward when the pigeon was released. Andrée started on a wind which carried his balloon along at the rate of 25 or 30 miles an hour. Had he continued northward at this rate he would have reached the Pole in less than two days. But I believe when these southern winds strike the northern ice, and become colder, they also become slower. Therefore, the rate of progress of his balloon would be greatly lessened as he proceeded northward. However, we know that he was going toward the east when last heard from.

If the balloonists managed to land safely on Franz Josef land, they may have been able to pull through the winter by erecting a hut and by hunting for food, as has been pointed out. It may be that they have gone too far north to sustain themselves in this manner. On the other hand, they may have descended into the ocean, although in the latter event they had one meager chance left open to them. It has been said that Andrée acted in an ill-advised manner when he placed his provisions in packages up in the ropes of his balloon rather than in the basket in which he and his companions were to live. It has been said that if the basket had been overturned the balloonists would have been spilled out, and the balloon, relieved of their weight, would immediately bound in the air and carry away with it their precious food. But Andrée's conclusions in this matter showed him to have been more far-sighted than his critics. His idea was that the balloon might descend into the sea, in which case he and his companions would be compelled to take to those very ropes themselves. Then, by cutting loose the basket beneath them, the balloon would rebound into the air and carry upward not only the aeronauts, but their precious food as well.

So it is all highly problematical. Under fortunate circumstances the explorers might exist several years in the Arctic regions. Everything that could be done to insure the success of the trip was done before they started. They were provided with every necessary of life; they had provisions, arms, ammunition, sledges, and a boat. They might, if they are on the mainland, gradually journey southward, in which case we shall hear of them before long. If they dropped into the ocean, they are lost. If they have reached the Polar cap and wrecked their balloon, they undoubtedly have found the great spot which so many have striven to find; but whether they will ever be able to tell its mysteries to the world is another question which I would rather not be asked to answer.

S. A. ANDRÉE.

From a photograph by G. Florman, Stockholm.



NILS STRINDBERG.

One of Andrée's two companions on the voyage.

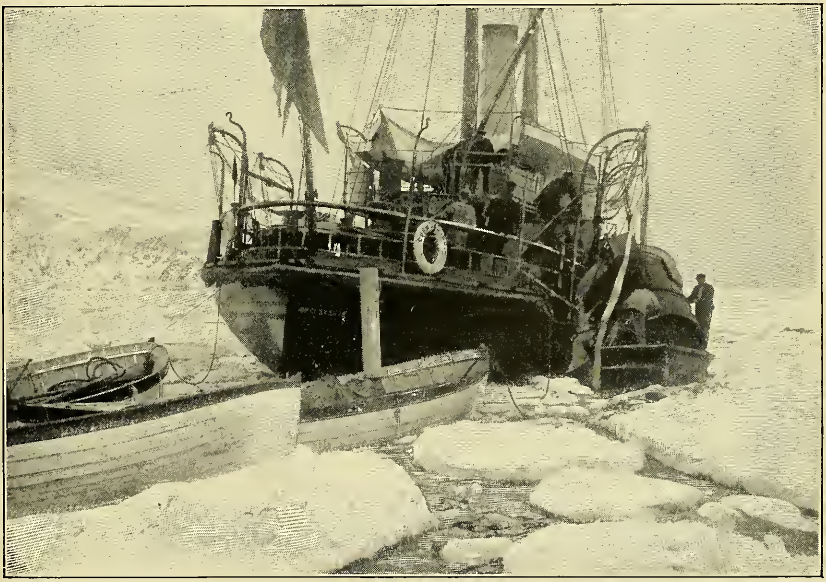
From a photograph by G. Florman, Stockholm.

KNUT FRAENKEL.

One of Andrée's two companions on the voyage.

DANES GATE. NEAR WHICH THE ASCENSION WAS MADE.

From a photograph by G. and H. Hasselblad, Göteborg, photographers of the Andrée Expedition.



UNLOADING THE BALLOON FROM THE SHIP AT DANES ISLAND.

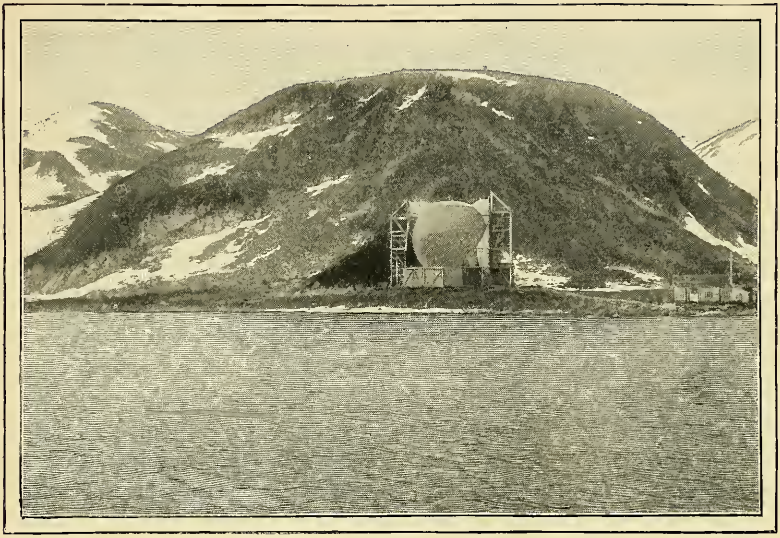
From a photograph by G. and H. Hasselblad, Göteborg, photographers of the Andrée Expedition.

SLEDDING THE BALLOON FROM THE SHIP TO LAND AT DANES ISLAND.

From a photograph by G. and H. Hasselblad, Göteborg, photographers of the Andrée Expedition.

LANDING THE BALLOON AT DANES ISLAND.

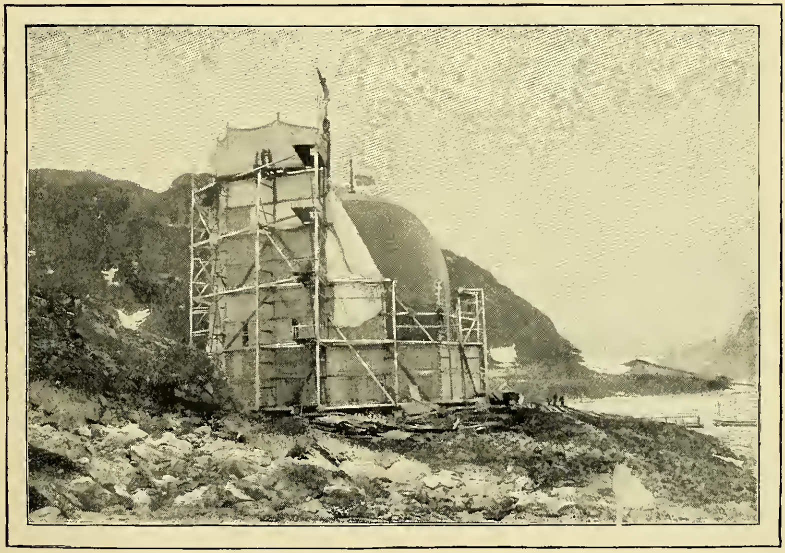
From a photograph by G. and H. Hasselblad, Göteborg, photographers of the Andrée Expedition.



VIEW OF THE BALLOON HOUSE AND THE BALLOON.

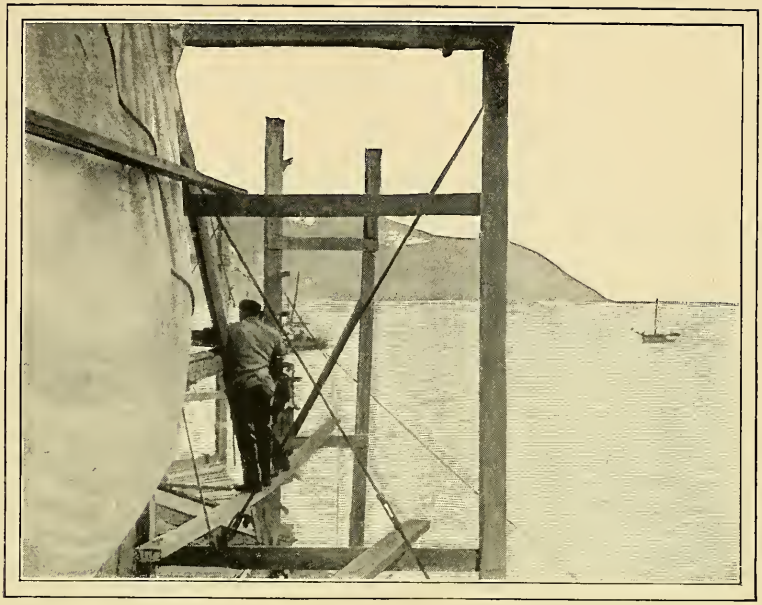
Part of the walls of the balloon house have been torn away, in order to let the balloon out at the ascension.

From a photograph by G. and H. Hasselblad, Göteborg, photographers of the Andrée Expedition.



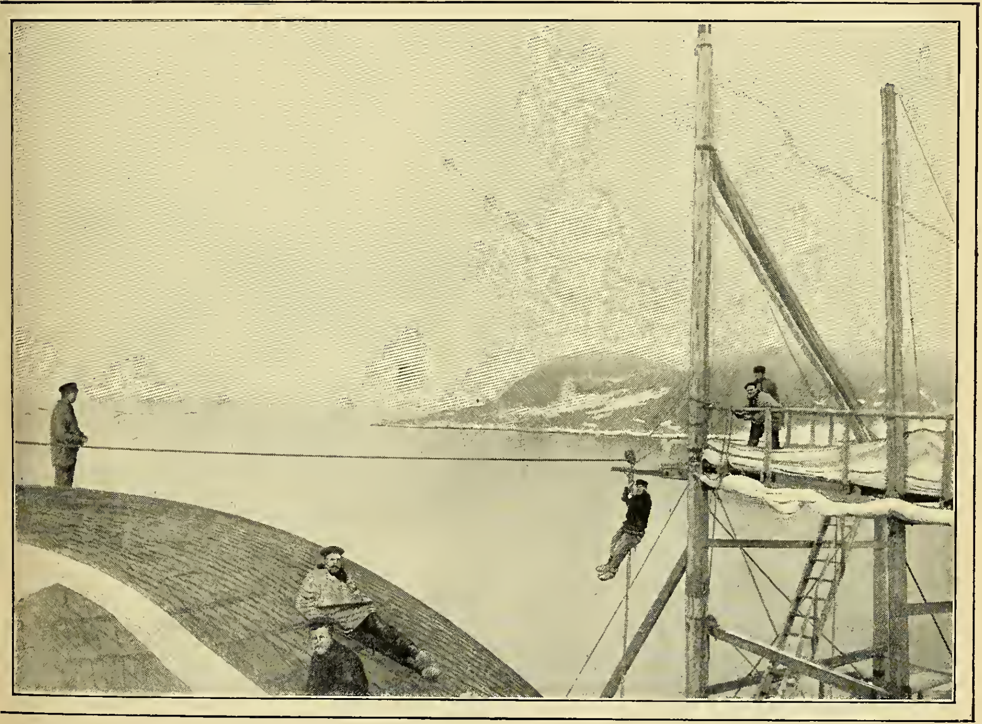
TAKING DOWN THE FRONT WALL OF THE BALLOON HOUSE.

From a photograph by G. and H. Hasselblad, Göteborg, photographers of the Andrée Expedition.



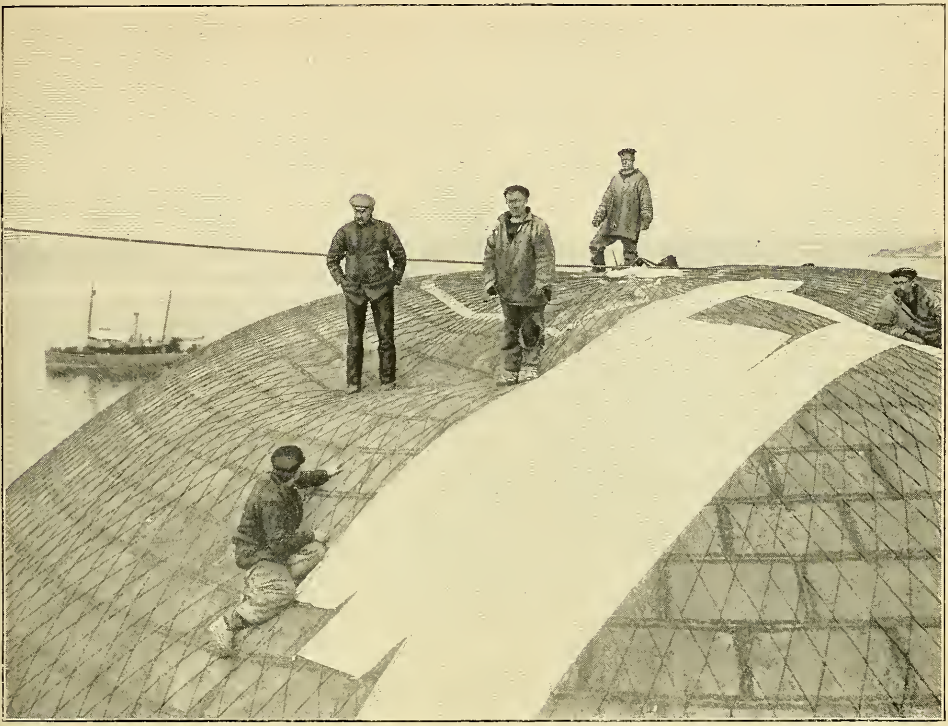
TAKING DOWN THE FRONT WALL OF THE BALLOON HOUSE.

From a photograph by G. and H. Hasselblad, Göteborg, photographers of the Andrée Expedition.



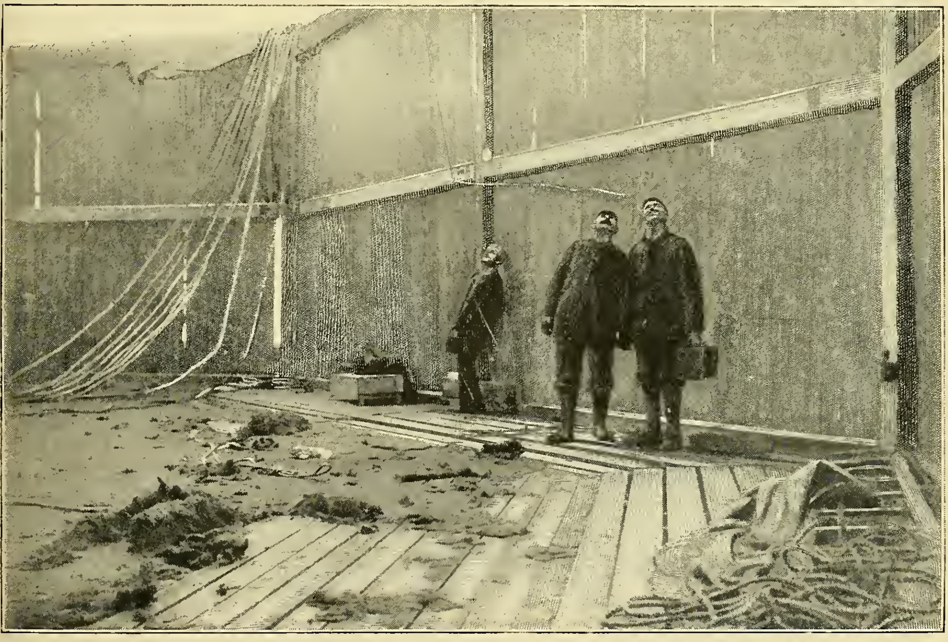
GETTING ON TOP OF THE BALLOON TO LOOK FOR LEAKS.

From a photograph by G. and H. Hasselblad, Göteborg, photographers of the Andrée Expedition.



Examining the Balloon for Leaks.

From a photograph by G. and H. Hasselblad, Göteborg, photographers of the Andrée Expedition.

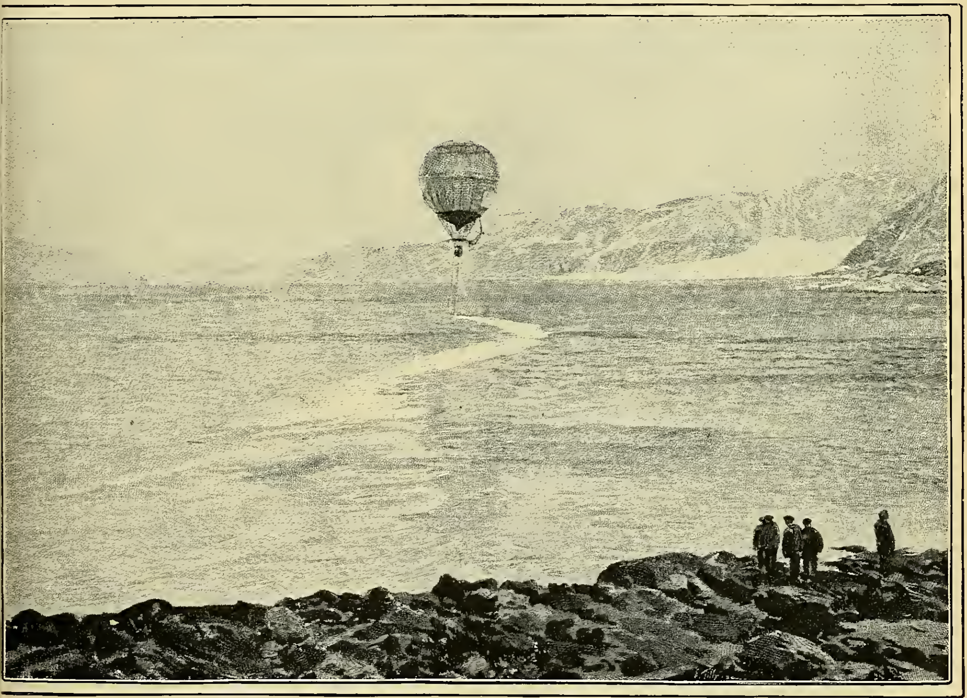


The Day of the Start. Inside the Balloon House after the Balloon has Risen.

From a photograph by G. and H. Hasselblad, Göteborg, photographers of the Andrée Expedition.

Watching the Balloon as it Sways under the Strong Blasts of Wind on the Day of the Start.

From a photograph by G. and H. Hasselblad, Göteborg, photographers of the Andrée Expedition.



The Start.

From a photograph by Mr. A. Machuron, who, as the representative of Mr. Lachambre, the maker of the balloon, accompanied Andrée to Danes Island and assisted him in making his start.

Reproduced from the Paris Illustration.

SCIENTIFIC ADVANTAGES OF AN ANTARCTIC EXPEDITION.[1]

Dr. MURRAY'S Address.

From a scientific point of view the advantages to be derived from a well-equipped and well-directed expedition to the Antarctic would, at the present time, be manifold. Every department of natural knowledge would be enriched by systematic observations as to the order in which phenomena coexist and follow each other in regions of the earth's surface about which we know very little or are wholly ignorant. It is one of the great objects of science to collect observations of the kind here indicated, and it may be safely said that without them we can never arrive at a right understanding of the phenomena by which we are surrounded, even in the habitable parts of the globe.

Before considering the various orders of phenomena concerning which fuller information is urgently desired it may be well to point out a fundamental topographical difference between the Arctic and Antarctic. In the Northern Hemisphere there is a polar sea almost completely surrounded by continental land, and continental conditions for the most part prevail. In the Southern Hemisphere, on the other hand, there is almost certainly a continent at the South Pole, which is completely surrounded by the ocean, and in those latitudes the most simple and extended oceanic conditions on the surface of the globe are encountered.

THE ATMOSPHERE.

One of the most remarkable features in the meteorology of the globe is the low atmospheric pressure at all seasons in the Southern Hemisphere south of latitude 45° S., with the accompanying strong westerly and northwesterly winds, large rain and snow fall, all round the South Polar regions. The mean pressure seems to be less than 29 inches, which is much lower than in similar latitudes in the Northern Hemisphere. Some meteorologists hold that this vast cyclonic system and low-pressure area continues south as far as the Pole, the more southerly parts being traversed by secondary cyclones. There are, however,

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[1] Address by Dr. John Murray, F. R. S., and subsequent speeches, delivered at a special meeting of the Royal Society, London, February 24, 1898. Printed in Nature, No. 1479, vol. 57, March 3, 1898.

many indications that the extreme South Polar area is occupied by a vast anticyclone, out of which winds blow toward the girdle of low pressure outside the ice-bound region. In support of this view it is pointed out that Ross's barometric observations indicate a gradual rise in the pressure south of the latitude 75° S., and all Antarctic voyagers agree that when near the ice the majority of the winds are from the south and southeast, and bring clear weather with fall of temperature, while northerly winds bring thick fogs with rise of temperature.

All our knowledge of the meteorological conditions of the Antarctic is limited to a few observations during the midsummer months, and these indicate that the temperature of the snow-covered Antarctic continent is even at that time much lower than that of the surrounding sea. The anticyclonic area at the South Pole appears therefore to be permanent, and when in winter the sea ice is for the most part continuous and extends far to the north the anticyclonic area has most probably a much wider extension than in summer. This is indicated by the southeasterly winds, which at times blow toward the southern point of the American continent in June and July.

All observations in high southern latitudes indicate an extremely low summer temperature. In winter we have no direct observations. The mean of Ross's air temperatures south of latitude 63° S. was 28.74° F., which is about the freezing point of sea water, and his maximum temperature was 43.5° F. Both Wilkes and D'Urville observed pools of fresh water on several icebergs, and, when sailing along the ice barrier, Ross saw "gigantic icicles depending from every projecting point of its perpendicular cliffs,"[1] so it is probable that extensive melting sometimes takes place.

In the latitude of the Antarctic Circle the air is frequently at or near the point of saturation, and precipitation takes place in the form of rain, sleet, snow, or hail. Most of the observations near the ice-covered land show, however, a much drier atmosphere, and in all probability precipitation over the Antarctic continent takes place in the form of fine snow crystals, such as is recorded in the interior of Greenland.

There would appear, then, to be good reasons for believing that the region of the South Pole is covered by what may be regarded practically as a great permanent anticyclone, with a much wider extension in winter than in summer. It is most likely that the prevailing winds blow out from the Pole all the year round toward the surrounding sea, as in the case of Greenland; but, unlike Greenland, this area is probably seldom traversed by cyclonic disturbances.

But what has been stated only shows how little real knowledge we possess concerning the atmospheric conditions of high southern latitudes. It is certain, however, that even two years' systematic observations within these regions would be of the utmost value for the future of meteorological science.

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[1] Ross, "Antarctic Voyage," Vol. I, page 237.

ANTARCTIC ICE.

From many points of view it would be important to learn something about the condition and distribution of Antarctic sea ice during the winter months, and especially about the position and movements of the huge table-shaped icebergs at this and other seasons of the year. These flat-topped icebergs, with a thickness of 1,200 or 1,500 feet, with their stratification and their perpendicular cliffs, which rise 150 or 200 feet above and sink 1,100 or 1,400 feet below the level of the sea, form the most striking peculiarity of the Antarctic Ocean. Their form and structure seem clearly to indicate that they were formed on an extended land surface, and have been pushed out over low-lying coasts into the sea.

Ross sailed for 300 miles along the face of a great ice barrier from 150 to 200 feet in height, off which he obtained depths of 1,800 and 2,400 feet. This was evidently the sea front of a great creeping glacier or ice cap just then in the condition to give birth to the table-shaped icebergs, miles in length, which have been described by every Antarctic voyager.

All Antarctic land is not, however, surrounded by such inaccessible cliffs of ice, for along the seaward faces of the great mountain ranges of Victoria Land the ice and snow which descend to the sea apparently form cliffs not higher than 10 to 20 feet, and in 1895 Kristensen and Borchgrevink landed on a pebbly beach occupied by a penguin rookery at Cape Adare without encountering any land ice descending to the sea. Where a penguin rookery is situated we may be quite sure that there is occasionally open water for a considerable portion of the year, and that consequently landing might be effected without much difficulty or delay; and further, that a party, once landed, might with safety winter at such a spot, where the penguins would furnish an abundant supply of food and fuel. A properly equipped party of observers situated at a point like this on the Antarctic Continent for one or two winters might carry out a most valuable series of scientific observations, make successful excursions toward the interior, and bring back valuable information as to the probable thickness of the ice-cap, its temperature at different levels, its rate of accumulation, and its motions, concerning all which points there is much difference of opinion among scientific men.

ANTARCTIC LAND.

Is there an Antarctic Continent? It has already been stated that the form and structure of the Antarctic icebergs indicate that they were built up on and had flowed over an extended land surface. As these bergs are floated to the north and broken up in warmer latitudes they distribute over the floor of the ocean a large quantity of glaciated rock fragments and land detritus. These materials were dredged up by the *Challenger* in considerable quantity, and they show that the rocks over which the Antarctic land ice moved were gneisses, granites, mica-schists, quartziferous diorites, grained quartzites, sandstones, limestones, and shales. These lithological types are distinctively indicative of continental land, and there can be no doubt about their having been transported from land situated toward the South Pole. D'Urville describes rocky islets off Adélie Land composed of granite and gneiss. Wilkes found on an iceberg near the same place bowlders of red sandstone and basalt. Borchgrevink and Bull have brought back fragments of mica-schists and other continental rocks from Cape Adare. Dr. Donald brought back from Joinville Island a piece of red jasper or chert containing Radiolaria and sponge spicules. Captain Larsen brought from Seymour Island pieces of fossil coniferous wood, and also fossil shells of Cucullæa, Cytherea, Cyprina, Teredo, and Natica, having a close resemblance to species known to occur in lower Tertiary beds in Britain and Patagonia. These fossil remains indicate in these areas a much warmer climate in past times. We are thus in possession of abundant indications that there is a wide extent of continental land within the ice-bound regions of the Southern Hemisphere.

It is not likely that any living land fauna will be discovered on the Antarctic continent away from the penguin rookeries. Still, an Antarctic expedition will certainly throw much light on many geological problems. Fossil finds in high latitudes are always of special importance. The pieces of fossil wood from Seymour Island can hardly be the only relics of plant life that are likely to be met with in Tertiary and even older systems within the Antarctic. Tertiary, Mesozoic, and Paleozoic forms are tolerably well developed in the Arctic regions, and the occurrence of like forms in the Antarctic regions might be expected to suggest much as to former geographical changes, such as the extension of the Antarctic continent toward the north, and its connection with, or isolation from, the northern continents, and also as to former climatic changes, such as the presence in pre-Tertiary times of a nearly uniform temperature in the waters of the ocean all over the surface of the globe.

MAGNETIC AND PENDULUM OBSERVATIONS, GEODETIC MEASUREMENTS, TIDES AND CURRENTS.

In any Antarctic expedition magnetic observations would, of course, form an essential part of the work to be undertaken, and the importance of such observations has been frequently dwelt upon by eminent physicists and navigators. Should a party of competent observers be stationed at Cape Adare for two years, pendulum observations could be carried out there and at other points within the Antarctic, or even on icebergs and on the interior ice cap. It might be possible to measure a degree on the Antartic continent or ice cap, which would be a most useful thing to do. By watching the motions of the icebergs and ice from land at Cape Adare, much would be learned about oceanic currents, and our knowledge of the tides would be increased by a systematic series of tidal observations on the shores of Antarctica, where we have at present no observations. The series of scientific observations here mentioned, and others that might be indicated, would fill up many gaps in our knowledge of the physical conditions of these high southern latitudes.

DEPTH OF THE ANTARCTIC OCEAN.

In regard to the depth of the ocean immediately surrounding the Antarctic continent we have at present very meager information, and one of the objects of an Antarctic expedition would be to supplement our knowledge by an extensive series of soundings in all directions throughout the Antarctic and Southern oceans. It would in this way be possible, after a careful consideration of the depths and marine deposits, to trace out approximately the outlines of the Antarctic continent. At the present time we know that Ross obtained depths of 100 to 500 fathoms all over the great bank extending to the east of Victoria Land, and somewhat similar depths have been obtained extending for some distance to the east of Joinville Island. Wilkes sounded in depths of 500 and 800 fathoms about 20 or 30 miles off Adélie Land. The depths found by the *Challenger* in the neighborhood of the Antarctic circle were from 1,300 to 1,800 fathoms, and farther north the *Challenger* soundings ranged from 1,260 to 2,600 fathoms. To the southwest of South Georgia, Ross paid out 4,000 fathoms of line without reaching bottom. In the charts of depth which I have constructed I have always placed a deep sea in this position; for it appears to me that Ross, who knew very well how to take soundings, was not likely to have been mistaken in work of this kind.

The few indications which we thus possess of the depth of the ocean in this part of the world seem to show that there is a gradual shoaling of the ocean from very deep water toward the Antarctic continent, and, so far as we yet know, either from soundings or temperature observations, there are no basins cut off from general oceanic circulation by barriers or ridges, similar to those found toward the Arctic.

DEPOSITS OF THE ANTARCTIC OCEAN.

The deposits which have been obtained close to the Antarctic continent consist of blue mud, containing glauconite, made up for the most part of detrital matters brought down from the land, but containing a considerable admixture of the remains of pelagic and other organisms. Farther to the north there is a very pure diatom ooze, containing a considerable quantity of detrital matter from icebergs, and a few pelagic foraminifera. This deposit appears to form a zone right round the earth in these latitudes. Still farther to the north the deposits pass in deep water, either into a Globigerina ooze or into a red clay with manganese nodules, sharks' teeth, ear bones of whales, and the other materials characteristic of that deep-sea deposit. Since these views, however, as to the distribution of deep sea deposits throughout these high southern latitudes are founded upon relatively few samples, it can not be doubted that further samples from different depths in the unexplored regions would yield most interesting information.

TEMPERATURE OF THE ANTARCTIC OCEAN.

The mean daily temperature of the surface waters of the Antarctic, as recorded by Ross, to the south of latitude 63° S. in the summer months, varies from 27.3° to 33.6°, and the mean of all his observations is 29.85°. As already stated, his mean for the air during the same period is somewhat lower, being 28.74°. In fact, all observations seem to show that the surface water is warmer than the air during the summer months.

The *Challenger* observations of temperature beneath the surface indicate the presence of a stratum of colder water wedged between warmer water at the surface, and warm water at the bottom. This wedge-shaped stratum of cold water extends through about 12° of latitude, the thin end terminating about latitude 53° S., its temperature varying from 28° at the southern thick end to 32.5° at the northern thin end, while the temperature of the overlying water ranges from 29° in the south to 38° in the north, and that of the underlying water from 32° to 35°. This must be regarded as the distribution of temperature only during the summer, for it is improbable that during the winter months there is a warmer surface layer.

In the greater depths of the Antarctic, as far south as the Antarctic circle, the temperature of the water varies between 32° and 35° F., and is not, therefore, very different from the temperature of the deepest bottom water of the tropical regions of the ocean. The presence of this relatively warm water in the deeper parts of the Antarctic Ocean may be explained by a consideration of general oceanic circulation. The warm tropical waters which are driven southward along the eastern coasts of South America, Africa, and Australia, into the great all-encircling Southern Ocean, there become cooled as they are driven to the east by the strong westerly winds. These waters, on account of their high salinity, can suffer much dilution with Antarctic water, and still be denser than water from these higher latitudes at the same temperature. Here the density observations and the sea-water gases indicate that a large part of the cold water found at the greater depths of the ocean probably leaves the surface and sinks toward the bottom in the Southern Ocean, between the latitudes of 45° and 56° S. These deeper, but not necessarily bottom, layers are then drawn slowly northward toward the tropics, to supply the deficiencies there produced by evaporation and southward-flowing surface currents, and these deeper layers of relatively warm water appear likewise to be slowly drawn southward to the Antarctic area to supply the place of the ice-cold currents of surface water drifted to the north. This warm underlying water is evidently a potent factor in the melting and destruction of the huge table-topped icebergs of the southern hemisphere. While these views as to circulation of oceanic water appear to be well established, still a fuller examination is most desirable at different seasons of the year, with improved thermometers and sounding machines. Indeed, all deep-sea apparatus has been so much improved as a result of the *Challenger* explorations, that the labor of taking salinity and all other oceanographical observations has been very much lessened.

PELAGIC LIFE OF THE ANTARCTIC OCEAN.

In the surface waters of the Antarctic there is a great abundance of diatoms and other marine algæ. These floating banks or meadows form primarily not only the food of pelagic animals, but also the food of the abundant deep-sea life which covers the floor of the ocean in these south polar regions. Pelagic animals, such as copepods, amphipods, mollusks, and other marine creatures, are also very abundant, although species are fewer than in tropical waters. Some of these animals seem to be nearly, if not quite, identical with those found in high northern latitudes, and they have not been met with in the intervening tropical zones. The numerous species of shelled Pteropods, Foraminifera, Coccoliths, and Rhabdoliths, which exist in the tropical surface waters, gradually disappear as we approach the Antarctic circle, where the shelled Pteropods are represented by a small *Limacina*, and the Foraminifera by only two species of *Globigerina*, which are apparently identical with those in the Arctic Ocean. A peculiarity of the tow-net gatherings made by the *Challenger* expedition in high southern latitudes, is the great rarity or absence of the pelagic larvæ of benthonic organisms, and in this respect they agree with similar collections from the cold waters of the Arctic seas. The absence of these larvæ from polar waters may be accounted for by the mode of development of benthonic animals to be referred to presently. It must be remembered that many of these pelagic organisms pass most of their lives in water of a temperature below 32° F., and it would be most interesting to learn more about their reproduction and general life history.

BENTHOS LIFE OF THE ANTARCTIC OCEAN.

At present we have no information as to the shallow-water fauna of the Antarctic Continent; but, judging from what we do know of the off-lying Antarctic Islands, there are relatively few species in the shallow waters in depths less than 25 fathoms. On the other hand, life in the deeper waters appears to be exceptionally abundant. The total number of species of Metazoa collected by the *Challenger* at Kerguelen in depths less than 50 fathoms was about 130, and the number of additional species known from other sources from the shallow waters of the same island is 112, making altogether 242 species, or 30 species less than the number obtained in eight deep hauls with the trawl and dredge in the Kerguelen region of the Southern Ocean, in depths exceeding 1,260 fathoms, in which eight hauls 272 species were obtained. Observations in other regions of the Great Southern Ocean, where there is a low mean annual temperature, also show that the marine fauna around the land in high southern latitudes appears to be very poor in species down to a depth of 25 fathoms, when compared with the number of species present at the mud line about 100 fathoms, or even at depths of about 2 miles.

In 1841 Sir James Ross stated that the animals he dredged off the Antarctic continent were the same as those he had dredged from similar depths in the Arctic seas, and he suggested that they might have passed from one pole to the other by way of the cold waters of the deep sea.[1] Subsequent researches have shown that, as with pelagic organisms, many of the bottom-living species are identical with, or closely allied to, those of the Arctic regions, and are not represented in the intermediate tropical areas. For instance, the most striking character of the shore-fish fauna of the Southern Ocean is the reappearance of types inhabiting the corresponding latitudes of the Northern Hemisphere and not found in the intervening tropical zone. This interruption of continuity in the distribution of shore fishes is exemplified by species as well as genera, and Dr. Günther enumerates eleven species and twenty-nine genera as illustrating this method of distribution. The following are among the species: Chimæra (*Chimæra monstrosa*), two species of dogfish (*Acanthias vulgaris* and *A. blainvilli*), the monkfish (*Rhina squatina*), John Dory (*Zeus faber*), angler (*Lophius piscatorius*), bellows-fish (*Centriscus scolopax*), sprat (*Clupea sprattus*). The genus by which the family Berycidæ is represented in the southern temperate zone (*Trachichthys*) is much more nearly allied to the northern than to the tropical genera. "As in the northern Temperate Zone, so in the southern, * * * the variety of forms is much less than between the Tropics. This is especially apparent on comparing the number of species constituting a genus. In this zone genera composed of more than ten species are the exception, the majority having only from one to five." " * * * *Polyprion* is one of those extraordinary instances in which a very specialized form occurs at almost opposite points of the globe, without having left a trace of its previous existence in, or of its passage through, the intermediate space."

Speaking of the shore fishes of the Antarctic Ocean, Günther says: "The general character of the fauna of Magelhaen's Straits and Kerguelen's Land is extremely similar to that of Iceland and Greenland. As in the Arctic fauna, Chondropterygians are scarce, and represented by *Acanthias vulgaris* and species of *Raja*. * * * As to Acanthopterygians, Cataphracti and Scorpænidæ are represented as in the Arctic fauna, two of the genera (*Sebastes* and *Agonus*) being identical.

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[1] Antarctic Voyage, page 207.

The Cottidæ are replaced by six genera of Trachinidæ, remarkably similar in form to Arctic types. * * * Gadoid fishes reappear, but are less developed; as usual, they are accompanied by *Myxine*. The reappearance of so specialized a genus as *Lycodes* is most remarkable."[1]

These statements with reference to shore fishes might, with some modifications, be repeated concerning the distribution and character of all classes of marine invertebrates in high northern and high southern latitudes. The *Challenger* researches show that nearly 250 species taken in high southern latitudes occur also in the Northern Hemisphere, but are not recorded from the tropical zone. Fifty-four species of seaweed have also been recorded as showing a similar distribution.[2] Bipolarity in the distribution of marine organisms is a fact, however much naturalists may differ as to its extent and the way in which it has originated.

All those animals which secrete large quantities of carbonate of lime greatly predominate in the tropics, such as Corals, Decapod Crustacea, Lamellibranchs, and Gasteropods. On the other hand, those animals in which there is a feeble development of carbonate of lime structures predominate in cold polar waters, such as Hydroida, Holothuroidea, Annelida, Amphipoda, Isopoda, and Tunicata. This difference is in direct relation with the temperature of the water in which these organisms live, carbonate of lime being thrown down much more rapidly and abundantly in warm than in cold water by ammonium carbonate, one of the waste products of organic activity.

In the Southern and Subantarctic Ocean a large proportion of the Echinoderms develop their young after a fashion which precludes the possibility of a pelagic larval stage. The young are reared within or upon the body of the parent, and have a kind of commensal connection with her till they are large enough to take care of themselves. A similar method of direct development has been observed in eight or nine species of Echinoderms from the cold waters of the northern hemisphere. On the other hand, in temperate and tropical regions, the development of a free-swimming larva is so entirely the rule that it is usually described as the normal habit of the Echinodermata. This similarity in the mode of development between Arctic and Antarctic Echinoderms (and the contrast to what takes place in the tropics) holds good also in other classes of Invertebrates, and probably accounts for the absence of free-swimming larvæ of benthonic animals in the surface gatherings in Arctic and Antarctic waters.

What is urgently required with reference to the biological problems here indicated is a fuller knowledge of the facts, and it can not be doubted that an Antarctic expedition would bring back collections and observations of the greatest interest to all naturalists and physi-

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[1] Günther, Study of Fishes, pages 282-290. (Edinburgh, 1880.)

[2] Murray and Barton. Phycological Memoirs of the British Museum, part 3. (London, 1895.)

ologists, and without such information it is impossible to discuss with success the present distribution of organisms over the surface of the globe, or to form a true conception of the antecedent conditions by which that distribution has been brought about.

CONCLUDING REMARKS.

There are many directions in which an Antarctic expedition would carry out important observations besides those already touched on in the foregoing statement. From the purely exploratory point of view much might be urged in favor of an Antarctic expedition at an early date; for the further progress of scientific geography it is essential to have a more exact knowledge of the topography of the Antarctic regions. This would enable a more just conception of the volume relations of land and sea to be formed, and in connection with pendulum observations some hints as to the density of the suboceanic crust and the depth of the Antarctic ice cap might be obtained. In case the above sketch may possibly have created the impression that we really know a great deal about the Antarctic regions, it is necessary to restate that all the general conclusions that have been indicated are largely hypothetical, and to again urge the necessity for a wider and more solid base for generalizations. The results of a successful Antarctic expedition would mark a great advance in the philosophy—apart from the mere facts—of terrestrial science.

No thinking person doubts that the Antarctic will be explored. The only questions are, When? and by whom? I should like to see the work undertaken at once, and by the British Navy. I should like to see a sum of £150,000 inserted in the estimates for the purpose. The Government may have sufficient grounds for declining to send forth such an expedition at the present time, but that is no reason why the scientific men of the country should not urge that the exploration of the Antarctic would lead to important additions to knowledge, and that, in the interests of science among English-speaking peoples, the United Kingdom should take not only a large but a leading part in any such exploration.

THE ANTARCTIC ICE SHEET: DUKE OF ARGYLL.

Scientific men generally feel, I think, that they do not need to give detailed reasons in connection with particular subjects of inquiry to justify their unanimous desire for an Antarctic expedition. It is enough, surely, for them to point out the fact that a very large area of the surface of our small planet is still almost unknown to us. That it should be so seems almost a reproach to our civilization. As to detailed reasons, it may almost be said with truth that there is hardly one of the physical sciences on which important light may not be cast by Antarctic exploration. Oceanic circulation, meteorology, magnetism, distribution of animal and vegetable life—not only in the present, but in the past—geology, mineralogy, volcanic action under special conditions—all of these are subjects on which the phenomena of the Antarctic regions are sure to bear directly.

If, however, I am asked to specify more particularly the question on which I look for invaluable evidence which can be got nowhere else, I must name, above all others, the most difficult questions involved in quaternary geology. Geologists are nearly all agreed that there has been, very recently, a glacial age—an age in which glacial conditions prevailed over the whole northern hemisphere to a much lower latitude than they prevail now. But geologists differ widely and fundamentally from each other as to the form which glacial agencies took during that period. In particular, many geologists believe in what they call an "ice sheet"—that is to say, in the northern world having been covered by an enormous mass of ice several thousand feet thick, which, as they assert, "flowed" over mountain areas as well as over plains, and filled up the bed of seas of a considerable depth. Other geologists disbelieve in this agency altogether. They deny that even such a body of ice ever existed; it could not possibly have moved in the way which the theory assumes. They affirm, also, that the facts connected with glaciated surfaces do not indicate the planing down by one universal sheet of enormous weight and pressure; but, on the contrary, the action of small and lighter bodies of ice, which have acted partially and unequally on different surfaces differently exposed.

We might have hoped that this controversy could be settled by the facts connected with the only enormous ice sheet which exists in the northern hemisphere, viz, that which covers the great continent of Greenland. But that ice sheet, enormous though it be, does certainly not do what the ice sheet of the Glacial Age is supposed to have done. That is to say, it does not flow out from Greenland, fill the adjacent seas, or override the opposite coasts, even in so narrow a sheet as Smith's Sound. But this evidence is negative only. In the Antarctic continent we have reason to believe that there is a larger ice sheet, and it certainly does protrude into the adjacent seas, not merely by sending out broken, floating fragments, but in unbroken ice cliffs of great height. Now, we want to know exactly under what conditions this protrusion takes place. Dr. Murray speaks of it as "creeping" seawards—a more cautious word than "flowing." But is it certain that it does even creep? May it not simply grow by accretion or aggregation till it reaches a depth of water so great as to break it off by flotation? Does it or does it not carry detritus when no detritus has been dropped on its surface? Or does it pick up detritus from its own bed? Or does it push foreign matter before it? Does the perfectly tabular form of the Antarctic icebergs indicate any differential movement in the parent mass at all; or does it not indicate a condition of immobility until their buoyancy lifts great fragments off? What is the condition of the rocks on which they rest? Is there any thrust upon the mass from the mountain ranges on which the gathering ground lies; or is the whole country one vast gathering ground from the continual excess of precipitation over melting? These questions, and a hundred others, have to be solved by Antarctic discovery, and until they are solved we can not argue with security on the geological history of our own now temperate regions. The Antarctic continent is unquestionably the region of the earth in which glacial conditions are at their maximum, and therefore it is the region in which we must look for all the information attainable toward, perhaps, the most difficult problem with which geological science has to deal.

SIR JOSEPH HOOKER'S VIEWS.

Dr. Murray's admirable summary of the scientific information obtainable by an organized exploration of the Antarctic regions, leaves nothing further to be said under that head. I can only record the satisfaction with which I heard it, and my earnest hope that it will lead to action being taken by the Government in the direction indicated.

Next to a consideration of the number and complexity of the objects to be obtained by an Antarctic expedition, what dwells most in my imagination is the vast area of the unknown region which is to be the field for investigation—a region which, in its full extension, reaches from the latitude of 60° S. to the southern pole, and embraces every degree of longitude. This is a very considerable portion of the surface of the globe, and it is one that has been considered to be for the most part inaccessible to man; I will, therefore, ask you to accompany the scientific explorer no farther than to the threshold of the scenes of his labors, that you may see how soon and how urgently he is called upon to study some of those hitherto unsolved Antarctic problems that he will there encounter.

In latitude 60° S. an open ocean girdles the globe without break of continuity. Proceeding southward in it, probably before reaching the Antarctic circle, he encounters the floating ice fields, which form a circumpolar girdle known as "The Pack," approximately concentric with the oceanic, interrupted in one meridian only, that south of Cape Horn, by the northern prolongation of Grahams Land. Pursuing his southward course in search of seas or lands beyond, after the novelty of his position in the Pack has worn off, he asks where and how the component parts of these great fields of ice had their origin, how they arrived at and maintain their present position, what are their rate of progress and courses, and what their influence on the surrounding atmosphere and ocean. I believe I am right in thinking that to none of these questions can a fuller answer be given than that they originated over extensive areas of open water in a higher latitude than they now occupy, that they are formed of frozen ocean water and snow, and that winds and currents have brought them to where we now find them; but of the position of the southern open waters, with the exception of the comparatively diminutive sea east of Victoria Land,[1] we know nothing, nor do we know anything of the relative amount of snow and ice of which they are composed, or of their age, or of the winds and currents that have carried them to a lower latitude.

The other great glacial feature of the Antarctic area is "The Barrier" which Ross traced for 300 miles in the seventy-eighth and seventy-ninth degrees of south latitude, maintaining throughout its character of an inaccessible precipitous ice cliff (the sea front of a gigantic glacier) of 150 to 200 feet in height. This stupendous glacier is no doubt one parent of the huge table-topped ice islands that infest the higher latitudes of the Southern Ocean; but as in the case of the Pack, we do not know where the Barrier has its origin, or anything further about it than that it in great part rests upon a comparatively shallow ocean bottom. It probably abuts upon land, possibly on an Antarctic continent; but to prove this was impossible, on the occasion of Ross's visit, for the height of the ship's crow's-nest above the sea surface was not sufficient to enable him to overlook even the upper surface of the ice. Nor do I foresee any other method of settling this important point, except by the use of a captive balloon, an implement with which I hope that future expeditions may be supplied. There were several occasions in which such an implement might advantageously have been used by Ross when near the Barrier, and more when it would have greatly facilitated his navigation of the Pack.

I have chosen the Antarctic ice as the subject upon which to address this most important meeting, not only because it is one of the very first of the phenomena that demand the study of the explorer, but because it is the dominant feature in Antarctic navigation, where the Pack is ever present or close by, demanding, whether for being penetrated or evaded, all the commander's fortitude and skill, and all his crew's endurance.

It may be expected that I should allude to those sections of Dr. Murray's summary that refer to the Antarctic fauna and flora; they are most important, for the South Polar Ocean swarms with animal and vegetable life. Large collections of these, taken both by the tow net and by deep-sea soundings, were made by Sir J. Ross, who was an ardent naturalist and threw away no opportunity of observing and preserving; but unfortunately, with the exception of the Diatomaceæ (which were investigated by Ehrenberg), very few of the results of his labor in this direction have been published. A better fate, I trust, awaits the treasures that the hoped-for expedition will bring back; for so prolific is that ocean that the naturalist need never be idle, no, not even for one of the twenty-four hours of daylight throughout an Antarctic summer,

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[1] I refer to the "pancake" ice, which, in that sea, on several occasions formed with great rapidity around Ross's ship, in lat. 76° to 78° S. in February, 1842, and which arrested their progress. Such ice, augmented by further freezing of the water and by snowfalls, may be regarded as a genesis of fields that, when broken up by gales. are carried to the north and contribute to the circumpolar pack.

and I look to the results of a comparison of the oceanic life of the Arctic and Antarctic regions as the heralding of an epoch in the history of biology.

THE PRACTICABILITY OF ANTARCTIC EXPLORATION.

Dr. Nansen said a great Antarctic expedition should be undertaken by the British nation. He confined his observations to the great importance of a land expedition in the Antarctic Continent. It would certainly be of the highest importance to have it in connection with a naval expedition, which would afford an excellent basis for such a land expedition. Dr. Murray had already mentioned the possibilities, and, perhaps, probabilities, that there was a large Antarctic continent covered by an ice cap. They did not quite know yet. It might be that there were large islands, and there might be sounds in between covered with floating ice. Whether that was so or not, it was certain there must be one or several huge ice caps inside this unknown territory in the south, and he felt certain that the exploration of these would give scientific information of the greatest importance. There were many problems to solve, and the only place they could try to solve them in was the polar regions. Greenland had already given them much information about the ice sheet, but Greenland was too small, when compared with the big ice sheets in the glacial packs. They should look to the much more extensive ice sheets which they might find in the unknown territory. He did not think it would be very difficult to reach the Antarctic Continent. They must remember they knew a great deal more about ice investigation than in the days of Ross. They had much better ships, and had steam, and were not afraid to push the ships into an ice pack. They knew that if they were exposed to pressure and some hard times, they had the means to get out of it again; and his opinion was that in the Southern Sea they were surrounded by much open water all round, and a ship would not run the risk of being shut up in ice as long as in the Arctic regions, where the seas were shut up by land round about. So far as he understood it, they would not run so much risk in that way in the south as in the Arctic. The ice generally opened in calm weather, and that was exactly when sailing vessels would not be able to make use of the opportunity to get in. So he thought with their modern steamships it would not be difficult to get into the Antarctic. It had been said that the ice sheet in the Antarctic continent was difficult to get at. It was difficult to ascend. Of course, when they went along the Barrier, as Ross did, it was difficult to get through, and probably the only way would be by captive balloons. He believed captive balloons would be of the greatest use for exploration in polar regions. With regard to the probable thickness of the ice sheet in the Antarctic, some put it at 2,000 feet, some at 10,000 feet, but he would rather put it at 20,000 feet. The height might present considerable difficulty to any land expedition.

This enormous ice sheet must have an important influence upon the climatology of the whole world, and valuable information might be obtained as to meteorological conditions through an Antarctic expedition. If such a great naval expedition as had been suggested were sent from this country, Norway would gladly join in the work and send out another expedition to take part in the land work, and it would be of the greatest importance if there could be international cooperation in these expeditions, because simultaneous observations could then be made in these Antarctic regions, and they could lay their plans in a more scientific way.

DR. NEUMAYER ON GRAVITY AND TERRESTRIAL MAGNETISM.

A gravity survey is, in connection with a thorough geographical survey of the Antarctic, one of the most urgent requirements of the science of our earth. There are no measurements of the gravity constant within the Antarctic region; indeed, they are very scarce in the Southern Hemisphere south of latitude 30° S., and they are so closely connected with the theory of the figure of our earth that it is hardly possible to arrive at any conclusive results in this all-important matter without observations within the Antarctic region. It is impossible to foretell what effect an exact gravity survey in that region might exert upon our views with regard to all physical elements which depend upon the radius of our earth. Apart from that consideration, we may hope for another important enlargement of the knowledge bearing upon the connection between terrestrial magnetism and gravity. Gravity observations have been so much simplified of late, by Von Herack's ingenious apparatus, that it does not offer a serious difficulty to multiply gravity determinations within the Antarctic region, so that we may well be able to speak of a "gravity survey." The all-important question of the distribution of land within the South Polar region is closely connected with it. The International Geodetic Permanent Commission expressed it as their conviction that a gravity survey within that region would be of the greatest benefit for higher geodetic theories.

The probable connection between gravity and terrestrial magnetism has already been referred to. But apart from this, a magnetic survey of the Antarctic region is of the greatest importance from other points of view. As, since the time of Ross, no other observations of the values of the magnetic elements have been made, we are perfectly ignorant of the values of the secular variations south of latitude 50°, though this information is urgently needed for the construction of trustworthy magnetic charts required in navigation. Of the situation of the southern magnetic pole, and of its motion during the last fifty years, we are equally ignorant, though the facts are so highly important according to Gauss's theoretical deductions.

Much as the mathematical theory of terrestrial magnetism has been developed, of the physical theory of that mysterious force in nature we are yet in perfect ignorance. This defect is certainly to some considerable degree caused by the want of our knowledge in higher latitudes. It seems as if the magnetic character of the South Polar region is such as would afford all facility for a sound investigation when compared with the magnetic conditions of the North Polar region. A glance at a magnetic map shows how entirely different is the distribution of the magnetic action in both polar regions.

There is the interesting fact to be noticed in the south that the two foci of total intensity are situated on the side toward the south of the Australian continent and nearly on the same meridian. The magnetic action which makes itself manifest by magnetic storms or disturbances reaches its highest degree likewise south of the Australian continent, whereas to the south of South America the storms become very scarce and of a similar magnitude to those in middle latitudes. This was most strikingly proved by the observations in Orange Bay and South Georgia during the period of international observations in 1882–83. Of course, the magnetic South Pole and the situation of the foci above mentioned are in close connection with these facts, but the reason of their distribution remains unexplained. A discussion of all observations on southern polar lights also shows a connection between their frequency and the maximum region of magnetic disturbance.

Though the examination of these few facts ought to prompt the institution of a vigorous examination of the South Polar regions, the series is far from being exhausted: there is the question of the geoid deformation, the phenomena of the tides, and the structure of the ice and its drifting.

The resolution of the Sixth International Geographical Congress that the present century should not be allowed to expire without unveiling the mysteries of the South Polar regions ought to be carried into effect. All scientific institutions and societies trust that such will take place without any further delay.

SIR CLEMENTS MARKHAM ON ANTARCTIC GEOGRAPHY.

I need scarcely say how fully I concur in every word that has fallen from Dr. Murray on the subject of the scientific results, and more especially of the geographical results of an Antarctic expedition.

It is sufficient to point out the vast extent of the unknown area, and that no area of like extent on the surface of the earth ever failed to yield results of practical as well as of purely scientific interest by its exploration.

But there is much more to be said in the present instance, because the little that we do know of the Antarctic regions points unerringly to the very great importance and interest of the results that are certain to attend further research.

The ice barrier, discovered by Sir James Ross, is known to be the source of the immense ice islands of the southern polar sea. But it has only been seen for a distance of 300 miles. It requires far more complete examination before any approach to an adequate knowledge can be obtained respecting the extent and nature of the supposed ice cap in its rear.

We know that the southern continent is a region of actual volcanic activity; but the extent, nature, and effect of that activity remain to be ascertained.

On the Antarctic Circle land has been reported at numerous points, south of Australia and the Indian Ocean, but it is unknown whether what has been seen indicates islets and rocks, or a continuous coast line.

Dr. Murray has pointed out that the whole southern continent is certainly not bounded by such an ice wall as was seen by Sir James Ross, and is not covered by an ice cap. But the extent alike of the ice cap and of the uncovered land is unknown.

We are ignorant of the distribution of land and sea, and of ice and water in summer, and of the causes which influence such distribution.

These are some of the geographical problems to be solved. The investigation of each one of them will lead to further discoveries as yet undreamt of, which must needs be of the deepest interest to geographers.

There are eminent men present who will no doubt refer to the results of Antarctic exploration as regards other branches of science. Combined together they make the discovery of the unknown parts of the Antarctic region the greatest and most important work that remains for this generation of explorers to achieve.

METEOROLOGY AND ANTARCTIC EXPLORATION.

Dr. Alexander Buchan stated that the remarks he was about to make would have exclusive reference to the first two paragraphs of Dr. Murray's address, under the heading of "The Atmosphere;" or, rather, more immediately to the relation between mean atmospheric pressure and prevailing winds. He supposed he had been asked to speak on this occasion, from the extensive and minute knowledge of the subject he had necessarily acquired in the preparation of the reports on atmospheric and oceanic circulation which were published as two of the reports of the scientific results of the voyage of H. M. S. *Challenger*.

The former of these reports, on atmospheric circulation, is accompanied by twenty-six maps, showing by isobars for each month and the year the mean pressure of the atmosphere, and by arrows the prevailing winds of the globe, on hypsobathymetric maps, or maps showing by shadings the height of the land and the depth of the sea; first on Gall's projection, and second on north circumpolar maps on equal surface projection. The isobars are drawn from mean pressures calculated for 1,366 places, and the winds from even a larger number of places, distributed as well as possible over the whole globe. It is also of importance to note that averages of pressure and prevailing winds are published with the report—an accompaniment to the maps of mean atmospheric pressure and prevailing winds of the globe not yet given in any other series of maps of mean pressure and prevailing winds.

This, then, is the work undertaken and published in these reports, which occupied seven years in preparing, as time could be spared from official duties. The result of the charting of the pressure and prevailing winds is this: Stand with your back to the wind, then the center of lowest pressure that causes the wind will be to the left in the Northern Hemisphere and to the right hand in the Southern Hemisphere, a relation well known as Buys Ballot's law. In charting the 1,366 pressures and the relative prevailing winds, no exception was found in any of the two hemispheres. This is one of the broadest generalizations science can point to.

Some years ago a theory of atmospheric circulation was published by the late Professor Ferrel, which, as it is not accordant with the broad results arrived at in the report of atmospheric circulation in the *Challenger* reports, calls for serious consideration on account of its bearing on any attempt proposed to be undertaken for the exploration of the Antarctic regions.

One of the more recent expositors of this theory is Professor Davis, of Harvard College, who, in his Elementary Meteorology, gives an admirable exposition of the results now arrived at by the various workers in meteorology, and of the opinions and theories promulgated by different meteorologists in different departments of the science. The book is largely used in secondary schools and colleges of the United States, and these views are all but universally held there, and are now spreading over other countries.

The following extract from Davis's book fairly represents these views as generally entertained:

> "The surface winds of the temperate latitudes and the high-level currents above them, sliding swiftly along on their steep poleward gradients, must all be considered together. They combine to form a vast aerial vortex or eddy around the pole. In the Northern Hemisphere this great eddy is much interrupted by continental high pressure in winter or low pressure in summer, and by obstruction from mountain ranges, as well as by irregular disturbances of the general circulation in the form of storms" (p. 110).

Now the facts of observation do not support the theory of the existence, at any season of the year, of a low barometric pressure, or an eddy of winds, round or in the neighboring regions of the North Pole. Observations do not show us any prevailing winds blowing homeward to the North Pole at any time of the year. Further, no low barometric pressure occupies the immediate polar region in any month; but, instead, the opposite holds good for the four months from April to July. In April and May the mean atmospheric pressure is higher in the region of the pole than it is anywhere in the northern hemisphere north of latitude 43° N.; and in June and July, also higher than it is anywhere north of latitude 55° N. Now the higher pressure in these four months necessitates the existence of upper currents in order to maintain this high pressure about the North Pole. These upper currents toward the pole are exactly opposed to the requirements of the theory which intimates that the upper currents in the region of the pole must necessarily blow, not toward but from, the pole.

The actual center in this hemisphere, north of the tropics, toward which the winds on or near the surface of the earth blow, is not the North Pole; but, in the winter months the low barometric depressions in the north of the Atlantic and Pacific, respectively, and in the summer months the low barometric depressions in the Eurasian and North American continents; and the sources out of which the prevailing winds blow, in the winter months, the high pressure regions in Siberia and North America; and in the summer months the high pressure regions lying northward of these continents, which, as already explained, are virtually the polar region itself. These are the facts in all regions where the winds, according to the theory, become winds blowing over the earth's surface.

As regards the southern hemisphere, Professor Davis states that—

> "In the southern hemisphere the circumpolar eddy is much more symmetrically developed." Again, "the high pressure that should result from the low polar temperatures is therefore reversed into low pressure by the excessive equatorward centrifugal force of the great circumpolar whirl; and the air thus held away from the polar regions is seen in the tropical belts of high pressure" (pp. 110, 111.)

The interpretation of this is that the remarkable low-pressure region of the southern hemisphere is continued southward to the South Pole itself, the pressure diminishing all the way; and that in the region of the South Pole the air currents poured thitherwards along the surface of the earth ascend, and thence proceed northward as upper currents of such enormous intensity and volume that they pile up in the tropical region of the southern hemisphere a mean sea-level atmospheric pressure about an inch and a half more than the sea-level pressure near the South Pole whence it starts. Now, to bring the matter to the business which this meeting of the Royal Society has taken in hand—if this theory be true and supported by the facts of observation, it is plain that no meteorologist could signify his approval of any scheme that could be proposed for exploring the Antarctic regions, it being obvious that these strong west-northwesterly winds, if they blow vortically round and in upon the pole, heavily laden as they necessarily would be with the aqueous vapor they have licked up from the Southern Ocean, would overspread Antarctica with a climate of all but continuous rain, sleet, and snow, which no explorer, however intrepid and enthusiastic, could possibly face.

But is this the state of things? Let it be at once conceded that, as far south as about latitude 55° S., the prevailing winds and the steadily diminishing mean pressures on advancing southward fairly well support the theory. South of this, however, southerly and southeasterly winds begin to increase in frequency until, from latitude 60° S. into higher latitudes, they become the prevailing winds. This is abundantly shown from the winds charted on the maps of the *Challenger* report, as well as from the unanimous experience of all those who have navigated this region from Ross to the present time. Thus the poleward-blowing winds from west-northwest in these summer months stop short at least 30 degrees of latitude from the South Pole.

These prevailing south-southeast winds necessarily imply, as has been shown in the case of the North Pole, the existence of a more or less pronounced anticyclone overspreading Antarctica; which in its turn necessarily implies the existence of upper currents from the northward, blowing toward and in upon the polar region to make good the drain caused by the surface out-blowing southeasterly winds. It may therefore be concluded that both the surface winds and the upper aerial currents are diametrically opposed to the requirements of this theory.

What is now urgently called for is a well-equipped Antarctic expedition to make observations which will enable meteorologists to settle definitely the distribution of atmospheric pressure and the prevailing winds of this great region. Were this done the position in the Southern Ocean of the great ring of lowest pressure that encircles the globe could be mapped out; and since it is toward this low-pressure ring that the wind-driven surface currents of the ocean flow, a contribution would thereby be made to oceanography of an importance that can not be overestimated, particularly as regards the great question of oceanic circulation.

SIR ARCHIBALD GEIKIE ON ANTARCTIC GEOLOGY.

Hardly anything is yet known of the geology of the Antarctic regions. By far the most important contributions to our knowledge of the subject were made by the expedition under Sir James Ross. But as he was unable to winter with his ships in the higher latitudes, and could only here and there with difficulty effect a landing on the coast, most of the geological information brought home by him was gathered at a greater or less distance from the land, with the aid of the telescope. Within the last few years several sealing vessels have brought home some additional scraps of intelligence, which only increase the desire for fuller knowledge.

As regards the land, merely its edges have here and there been seen. Whether it is one great continent or a succession of islands and archipelagos may possibly never be ascertained. We know that in Victoria Land it terminates in a magnificent mountain range with peaks from 10,000 to 15,000 feet high, but that elsewhere it is probably comparatively low, shedding its ice cap in one vast sheet into the sea.

The rocks that constitute the land are still practically unknown.

The dredgings of the *Challenger* expedition brought up pieces of granite, gneiss, and other continental rocks, and detritus of these materials was observed to increase on the sea floor southward in the direction of the Antarctic land. More recently several sealing vessels have brought home from the islets of Graham Land, to the south of the South Shetlands, pieces of different varities of granite, together with some volcanic rocks and fossiliferous limestones. So far as these rocks have been studied they do not appear to differ from similar rocks all over the globe. The granites have been found by Mr. Teall to be just such masses as might have come from any old mountain group in Europe or America.

Among the specimens sent to me by Captain Robertson, of the *Active*, from Joinville and Dundee islands, which form the northeastern termination of Graham Land, there was one piece of reddish jasper which at once attracted my attention from its resemblance to the "radiolarian cherts" now found to be so widely distributed among the older Paleozoic rocks, both in the Old World and in the New. On closer examination, this first impression was confirmed, and a subsequent microscopic study of thin slices of the stone by Dr. Hinde proved the undoubted presence of abundant radiolaria. The specimen was a loose pebble picked up on the beach of Joinville Island. We have no means of telling where it came from or what is its geological age. But its close resemblance to the radiolarian cherts so persistent in the Lower Silurian formations of the United Kingdom raises the question whether there are not present in the Antarctic regions rocks of older Paleozoic age.

It would be of the utmost interest to discover such rocks in situ, and to ascertain how far their fossils agree with those found in deposits of similar antiquity in lower latitudes, or whether, as far back as early Paleozoic time, any difference in climate had begun to show itself between the polar and other regions of the earth's surface.

Among the specimens brought home by Dr. Donald and Captain Larsen from Seymour Island, in the same region, are a few containing some half dozen species of fossil shells which have been named and described by Messrs. Sharmon and Newton, who suggest that they point to the existence of Lower Tertiary rocks, one of the organisms resembling a form found in the old Tertiary formations of Patagonia. Large well-developed shells of *Cucullæa* and *Cytherea* undoubtedly indicate the former existence of a far milder climate in these Antarctic seas than now prevails.

If a chance landing for a few hours on a bare islet could give us these interesting glimpses into the geological past of the South Polar regions, what would not be gained by a more leisurely and well-planned expedition?

But perhaps the geological domain that would be most sure to gain largely from such exploration would be that which embraces the wide and fascinating field of volcanic action. In the splendid harvest of results brought home by Sir James Ross one of the most thrilling features was the discovery of a snowy volcanic cone rising amid the universal snows of Victoria Land to a height of more than 12,000 feet, and actively discharging "flame and smoke," while other lofty cones near it indicated that they too had once been in vigorous eruption. Ross landed on one or two islands near that coast, and brought away some pieces of volcanic rocks.

If we glance at a terrestrial globe, we can readily see that the volcanic ring or "circle of fire," which nearly surrounds the vast basin of the Pacific Ocean, is prolonged southward into New Zealand. The few observations that have been made in the scattered islands farther south show that the Auckland, Campbell, and Macquarrie groups consist of, or at least include, materials of volcanic origin. Still farther south, along the same general line, Mr. Borchgrevink has recently (1894–95) made known the extension of Ross's volcanic platform northward to Cape Adare, the northern promontory of Victoria Land. He noticed there the apparent intercalation of lava and ice, while bare snowless peaks seemed still further to point to the continued activity of the volcanic fires. Some specimens brought by his expedition from Possession Island were found by Mr. Teall to be highly vesicular hornblende basalt, while one from Cape Adare was a nepheline tephrite. This region is probably one of the most interesting volcanic tracts on the face of the globe. Yet we can hardly be said to know more of it than its mere existence. The deeply interesting problems which it suggests can not be worked out by transitory voyagers. They must be attacked by observers stationed on the spot. Ross thought that a winter station might be established near the foot of Mount Erebus, and that the interior could easily be traversed from there to the magnetic pole.

But it is not merely in Victoria Land that Antarctic volcanoes may be studied. Looking again at the globe, we observe that the American volcanic band is prolonged in a north and south line down the western side of the southern continent. That it has been continued into the chain of the South Shetlands and Graham Land is proved by the occurrence there of old sheets of basalt, rising in terraces over each other, sometimes to a height of more than 7,000 feet above the sea. These denuded lavas may be as old as those of our western isles—Faroe, Iceland, and Greenland. But that volcanic activity is not extinct there has recently been found by Captain Larsen, who came upon a group of small volcanoes forming islets along the eastern coast line of Graham Land. It is tantalizing to know no more about them.

Another geological field where much fresh and important information might be obtained by Antarctic exploration is that of ice and ice action. Our northern hemisphere was once enveloped in snow and ice, and though for more than half a century geologists have been studying the traces of the operations of this ice covering, they are still far from having cleared up all the difficulties of the study. The Antarctic ice cap is the largest in the world. Its behavior could probably be watched along many parts of its margin, and this research would doubtless afford great help in the interpretation of the glaciation of the northern hemisphere.

To sum up: Geologists would hail the organization and dispatch of an Antarctic expedition in the confident assurance that it could not fail greatly to advance the interests of their science. Among the questions which it would help to elucidate mention may be made of the following:

The nature of the rocks forming the land of the Antarctic region, and how far these rocks contain evidence bearing on the history of terrestrial climates.

The extent to which the known fossiliferous formations of our globe can be traced toward the poles; the gaps which may occur between these formations and the light which their study may be able to throw on the evolution of terrestrial topography.

The history of volcanic action in the past, and the conditions under which it is continued now in the polar regions; whether in high latitudes vulcanism, either in its internal magmas or superficial eruptions, manifests peculiarities not observable nearer to the equator; what is the nature of the volcanic products now ejected at the surface; whether a definte sequence can be established from the eruptions of still active volcanoes back into those of earlier geological periods in Antarctic lands; and whether among the older sheets leaf beds or other intercalations may be traceable, indicating the prolongation of a well-developed terrestrial flora toward the South Pole.

The influence of the Antarctic climate upon the rocks exposed to its action; the effects of contact with ice and snow upon streams of lava; the result of the seaward creep of the ice cap in regard to any lava sheets intercalated in the ice. It is conceivable that portions of lava streams might be broken off by the onward motion of the ice which they overspread, and might thus be carried out to sea, intercalated in or capping icebergs.

The physics of Antarctic ice in regard to the history of the Ice Age in northern Europe and America.

ANTARCTIC FAUNA.

Although an ardent advocate of Antarctic exploration, Mr. Sclater acknowledged that, as regards the higher vertebrates, with which he was most conversant, there was little chance of the discovery of new forms of animal life in the South Polar continent. The Antarctic mammals and birds (of the latter of which about twenty species were known) were exclusively of marine forms. Not a single land mammal or land bird had been yet obtained in Antarctica. As regards the class of fishes and the marine invertebrates, the case was quite different, and great discoveries might be anticipated in these groups, where very little had yet been done. The most promising zoological subject of Antarctic exploration seemed to him, however, to be the further investigation of the extinct fauna. The few fossil remains already obtained indicated the former existence in the South Polar area of a very different climate from that which now prevailed there, and further researches on this point might lead to most important results.

Prof. D'Arcy W. Thompson said that all we knew of the deep-sea life of the Antarctic came from eight hauls of the dredge, which hauls were, by common consent of the naturalists of the *Challenger*, the most productive of the whole cruise. The fauna of every ocean urgently demanded further exploration, for we knew now no more about the fauna of the deep sea than was known a hundred years ago of the fauna of the shore. But the circumpolar fauna of the south, at the meeting of all the great oceans, presented problems of peculiar importance. He considered Dr. Murray's theory of a "bipolar fauna," closely akin both in the Arctic and Antarctic, as not proven; but he believed that there were many remarkable cases of continuous distribution, especially along the cold waters of the western American coast from the Antarctic into the North Pacific, and even to Japan. If the "bipolar hypothesis" were broken down, Antarctic exploration would lead to new generalizations, not less interesting, to take its place.

Admiral Sir William Wharton said that an Antarctic expedition must be under naval discipline. He hoped that such an expedition would not be far off, and he felt sure there would be a rush of officers and men to join it.

Sir John Evans, in briefly summing up the discussion, said it had maintained a high level, and that the meeting had been prolonged to an unprecedented hour in the Royal Society. All were agreed as to the immense advantages of an expedition, and he was sure it would find a warm advocate in the hydrographer to the Admiralty.

RECENT PROGRESS IN PHYSIOLOGY.[1]

By Michael Foster,

Secretary of the Royal Society.

We who have come from the little island on the other side of the great waters to take part in this important gathering of the British Association have of late been much exercised in retrospection. We have been looking back on the sixty years' reign of our beloved Sovereign and dwelling on what has happened during her gracious rule. We have, perhaps, done little in calling to mind the wrongs, the mistakes, and the failures of the Victorian era, but our minds and our mouths have been full of its achievements and its progress; and each of us, of himself or through another, has been busy in bringing back to the present the events of more than half a century of the past. It was while I, with others, was in this retrospective mood that the duty of preparing some few words to say to you to-day seemed suddenly to change from an impalpable cloud in the far distance to a heavy burden pressing directly on the back, and in choosing something to say I have succumbed to the dominant influence. Before putting pen to paper, however, I recovered sufficiently to resist the temptation to add one more to the many reviews which have appeared of the progress of physiology during the Victorian era. I also rejected the idea of doing that for which I find precedents in past presidential addresses, namely, of attempting to tell what has been the history of the science to which a section is devoted during the brief interval which has elapsed since the section last met; to try and catch physiology, or any other science, as it rushes through the brief period of some twelve months seemed to me not unlike photographing the flying bullet without adequate apparatus; the result could only be either a blurred or a delusive image. But I bethought me that this is not the first—we hope it will not be the last—time that the British Association has met in the Western Hemisphere; and though the events of the thirteen years which have slipped by since the meeting at Montreal in 1884 might seem to furnish a very

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[1] Address to the physiological section of the British Association for the Advancement of Science, Toronto, 1897, by Prof. Michael Foster, M. A., M. D., D. C. L., LL. D., secretary of the Royal Society, president of the section. From Report of British Association, 1897.

slender oat on which to pipe a presidential address, I have hoped that I might be led to sound upon it some few notes which might be listened to.

And, indeed—though perhaps when we come to look into it closely almost every period would seem to have a value of its own—the past thirteen years do, in a certain sense, mark a break between the physiology of the past and that of the future. When the association met at Montreal in 1884, Darwin, whose pregnant ideas have swayed physiology in the limited sense of that word, as well as that broader study of living beings which we sometimes call biology, as indeed they have every branch of natural knowledge, had been taken from us only some two years before, and there were still alive most of the men who did the great works of physiology of the middle and latter half of this century. The gifted Claude Bernard had passed away some years before, but his peers might have been present at Montreal. Bowman, whose classic works on muscle and kidney stand out as peaks in the physiological landscape of the past, models of researches finished and complete so far as the opportunities of the time would allow, fruitful beginnings and admirable guides for the labors of others. Brown-Sequard, who shares with Bernard the glory of having opened up the great modern path of the influence of the nervous system on vascular, and thus on nutritional, events, and who, if he made some mistakes, did many things which will last for all time. Brücke, whose clear judgment, as shown in his digestive and other work, gave permanent value to whatever he put forth. Du Bois Reymond, who, if he labored in a narrow path, set a brilliant example of the way in which exact physical analysis may be applied to the phenomena of living beings, and in other ways had a powerful influence on the progress of physiology. Donders, whose mind seemed to have caught something of the better qualities of the physiological organ to which his professional life was devoted, and our knowledge of which he so largely extended, so sharply did he focus his mental eye on every physiological problem to which he turned—and these were many and varied. Helmholtz, whose great works on vision and hearing, to say nothing of his earlier distinctly physiological researches, make us feel that if physics gained much, physiology lost even more when the physiologist turned aside to more distinctly physical inquiries. Lastly, and not least, Ludwig, who by his own hands or through his pupils did so much to make physiology the exact science which it is to-day, but which it was not when he began his work. I say lastly, but I might add the name of one who, though barred by circumstances from contributing much directly to physiology by way of research, so used his powerful influence in many ways in aid of physiological interests as to have helped the science onward to no mean extent, at least among English-speaking people—I mean Huxley. All these might have met at Montreal. They have all left us now. Among the peers of the men I have mentioned whose chief labors were carried on in the forties, the fifties, and the sixties of the century, one prominent inquirer alone seems to be left, Albert von Kölliker, who in his old age is doing work of which even he in his youth might have been proud. The thirteen years which have swept the others away seem to mark a gulf between the physiological world of to-day and that of the time in which most of their work was done.

They are gone, but they have left behind their work and their names. May they of the future, as I believe we of the present are doing, take up their work and their example, doing work other than theirs but after their pattern, following in their steps.

In the thirteen years during which these have passed away physiology has not been idle. Indeed, the more we look into the period the more it seems to contain.

The study of physiology, as of other sciences, though it may be stimulated by difficulties (and physiology has the stimulus of a special form of opposition unknown to other sciences), expands under the sunshine of opportunity and aid. And it may be worth while to compare the opportunities for study of physiology in 1884 with those in 1897. At this meeting of the British Association I may fitly confine myself, I was going to say, to British matters; but I feel at this point, as others have felt, the want of a suitable nomenclature. We who are gathered here to-day have, with the exception of a few honored guests from the Eastern Hemisphere, one common bond, one common token of unity, and, so far as I know, one only; I am speaking now of outward tokens; down deeper in our nature there are, I trust, yet others. We all speak the English tongue. Some of us belong to what is called Great Britain and Ireland, others to that which is sometimes spoken of as Greater Britain. But there are others here who belong to neither; though English in tongue, they are in no sense British. To myself, to whom the being English in speech is a fact of far deeper moment than any political boundary, and who wish at the present moment to deal with the study of physiology among all those who speak the English tongue, there comes the great want of some word which will denote all such. I hope, indeed I think, that others feel the same want too. The term Anglo-Saxon is at once pedantic and incorrect, and yet there is none other; and, in the absence of such a better term, I shall be forgiven if I venture at times to use the seemingly narrow word English as really meaning something much broader than British in its very broadest sense.

Using English in this sense, I may, I think, venture to say that the thirteen years which separate 1884 from to-day have witnessed among English people a development of opportunities for physiological study such as no other like period has seen. It is not without significance that only a year or two previous to this period, in England proper, in little England, neither of the ancient universities of Oxford and Cambridge, which, historically at least, represent the fullest academical aspirations of the nation, possessed a chair of physiology. The present professors, who are the first, were both appointed in 1883. Up to that time the science of physiology had not been deemed worthy, by either university, of a distinctive professorial mechanism. The act of these ancient institutions was only a manifestation of modern impulses, shared also by the metropolis and by the provinces at large. Whereas up to that time the posts for teaching physiology, by whatever name they were called, had been in most cases held by men whose intellectual loins were girded for other purposes than physiology, and who used the posts as stepping stones for what they considered better things, since that time, as each post became vacant, it has almost invariably been filled by men wishing and purposing at least to devote their whole energies to the science. Scotland, in many respects the forerunner of England in intellectual matters, had not so much need of change; but she, too, has moved in the same direction, as has also the sister island.

And if we turn to this Western Continent we find in Canada and in the States the same notable enlargement of physiological opportunity, or even a still more notable one. If the English-speaking physiologist dots on the map each place on this Western Hemisphere which is an academic focus of his science, he may well be proud of the opportunities now afforded for the development of English physiology; and the greater part of this has come within the last thirteen years.

Professorial chairs or their analogues are, however, after all but a small part of the provision for the development of physiological science. The heart of physiology is the laboratory. It is this which sends the life blood through the frame, and in respect to this, perhaps, more than to anything else, has the progress of the past thirteen years been striking. Doubtless on both sides of the waters there were physiological laboratories, and good ones, in 1884; but how much have even these during that period been enlarged and improved, and how many new ones have been added? In how many places, even right up to about 1884, the professor or lecturer was fain to be content with mere lecture experiments and a simple course of histology, with perhaps a few chemical exercises for his students. Now each teacher, however modest his post, feels and says that the authorities under whom he works are bound to provide him with the means of leading his students along the only path by which the science can be truly entered upon—that by which each learner repeats for himself the fundamental observations on which the science is based.

But there is a still larger outcome from the professorial chair and the physiological laboratory than the training of the student. These are opportunities not for teaching only, but also for research. And perhaps in no respect has the development during the past thirteen years been so marked as in this. Never so clearly as during this period has it become recognized that each post for teaching is no less a post for learning, that among academic duties the making knowledge is as urgent as the distributing it, and that among professorial qualifications the gift of garnering in new truths is at least as needful as facility in the didactic exposition of old ones. Thirteen years has seen a great change in this matter, and the progress has been perhaps greater on this side of the water than on the other, so far as English-speaking people are concerned. We on the other side have witnessed with envy the establishment on this side of a university, physiology having in it an honored place, the keynote of which is the development of original research. It will, I venture to think, be considered a strong confirmation of my present theme that the Clark University at Worcester was founded only ten years ago.

And here, as an English-speaking person, may I be allowed to point out, not without pride, that these thirteen years of increased opportunity have been thirteen years of increased fruitfulness? In the history of our science, among the names of the great men who have made epochs, English names, from Harvey onward, occupy no mean place; but the greatness of such great men is of no national birth; it comes as it lists, and is independent of time and of place. If we turn to the more everyday workers, whose continued labors more slowly build up the growing edifice and provide the needful nourishment for the greatness of which I have just spoken, we may, I will dare to say, affirm that the last thirteen years have brought contributions to physiology, made known in the English tongue, which, whether we regard their quantity or their quality, significantly outdo the like contributions made in any foregoing period of the same length. Those contributions have been equally as numerous, equally as good, on this side as on the other side of the waters. And here I trust I shall be pardoned if personal ties and affection lead me to throw in a personal word. May I not say that much which has been done on this side has been directly or indirectly the outcome of the energy and gifts of one whom I may fitly name on an occasion such as this, since, though he belonged to the other side, his physiological life was passed and his work was done on this side, one who has been taken from us since this association last met—Henry Newell Martin?

Yes; during these thirteen years, if we put aside the loss of comrades, physiology has been prosperous with us and the outlook is bright; but, as every cloud has its silver lining, so shadow follows all sunshine, success brings danger, and something bitter rises up amid the sweet of prosperity. The development of which I have spoken is an outcome of the progressive activity of the age, and the dominant note of that activity is heard in the word "commercial." Noblemen and noblewomen open shop, and everyone, low as well as high, presses forward toward large or quick profits. The very influences which have made devotion to scientific inquiry a possible means of livelihood, and so fostered scientific investigation, are creating a new danger. The path of the professor was in old times narrow and straight, and only the few who had a real call cared to tread it. Nowadays there is some fear lest it become so broad and so easy as to tempt those who are in no way fitted for it. There is an increasing risk of men undertaking a research, not because a question is crying out to them to be answered, but in the hope that the publication of their results may win for them a lucrative post. There is, moreover, an even greater evil ahead. The man who lights on a new scientific method holds the key of a chamber in which much gold may be stored up, and strong is the temptation for him to keep the new knowledge to himself until he has filled his fill, while all the time his brother inquirers are wandering about in the dark through lack of that which he possesses. Such a selfish withholding of new scientific truth is beginning to be not rare in some branches of knowledge. May it never come near us!

Now I will, with your permission, cease to sound the provincial note, and ask your attention for a few minutes while I attempt to dwell on what seem to me to be some of the salient features of the fruits of physiological activity, not among English-speaking people only, but among all folk, during the past thirteen years.

When we review the records of research and discovery over any lengthened period we find that in every branch of the study progress is irregular; that it ebbs and flows. At one time a particular problem occupies much attention; the periodicals are full of memoirs about it, and many of the young bloods flesh their maiden swords upon it. Then again, for a while it seems to lie dormant and unheeded. But quite irrespective of this feature, which seems to belong to all lines of inquiry, we may recognize two kinds of progress. On the one hand, in such a period, in spite of the waves just mentioned, a steady advance continually goes on in researches which were begun and pushed forward in former periods, some of them being of very old date. On the other hand, new lines of investigation, starting with quite new ideas or rendered possible by the introduction of new methods, are or may be begun. Such naturally attract great attention and give a special character to the period.

In the past thirteen years we may recognize both these kinds of progress. Of the former kind I might take, as an example, the time-honored problems of the mechanics of the circulation. In spite of the labor which has been spent on these in times of old, something always remains to be done, and the last thirteen years have not been idle. The researches of Hürthle and Tigerstedt, of Roy and Adami, not to mention others, have left us wiser than we were before. So, again, with the also old problems of muscular contraction, progress, if not exciting, has been real; we are some steps measurably nearer an understanding what is the exact nature of the fundamental changes which bring about contraction and what are the relations of those changes to the structure of muscular fiber. In respect to another old problem, too, the beat of the heart, we have continued to creep nearer and nearer to the full light. Problems again, the method of attacking which is of more recent origin, such as the nature of secretion and the allied problem of the nature of transudation, have engaged attention and brought about that stirring of the waters of controversy which, whatever be its effects in other departments of life, is never in science wholly a waste of time, if indeed it be a waste of time at all, since in matters of science the tribunal to which the combatants of both sides appeal is always sure to give a true judgment in the end. In the controversy thus arisen the last word has perhaps not yet been said, but whether we tend at present to side with Heidenhain, who has continued into the past thirteen years the brilliant labors which were perhaps the distinguishing features of physiological progress in preceding periods, and who in his present sufferings carries with him, I am sure, the sympathies if not the hopes of all his brethren, or whether we are more inclined to join those who hold different views, we may all agree in saying that we have, in 1897, distinctly clearer ideas of why secretion gathers in an alveolus or lymph in a lymph space than we had in 1884.

I might multiply such examples of progress on more or less old lines until I wearied you, but I will try not to do so. I wish rather to dwell for a few minutes on some of what seem to be the salient new features of the period under review.

One such feature is, I venture to think, the development of what may perhaps be called the new physiological chemistry. We always are, and for a long time always have been, learning something new about the chemical phenomena of living beings. During the years preceding those immediately recent, great progress, for which we have especially, perhaps, to thank Kühne, was made in our knowledge of the bodies which we speak of as proteids and their allies. But while admitting to the full the high value of all these researches and the great light which they threw on many of the obscurer problems of the chemical changes of the body, such, for instance, as the digestive changes and the clotting of blood, it could not but be felt that their range was restricted and their value limited. Granting the extreme usefulness of being able to distinguish bodies through their solution or precipitation by means of this or that salt or acid, this did not seem to promise to throw much light on the all-important problem as to what was the connection between the chemical constitution of such bodies and their work in the economy of a living being. For it need not be argued that this is an all-important problem. To-day, as yesterday and as in the days before, the mention of the word vitalism or its equivalent separates as a war cry physiologists into two camps, one contending that all the phenomena of life can, and the other that they can not, be explained as the result of the action of chemico-physical forces. For myself, I have always felt that while such a controversy, like other controversies, as I ventured to say just now, is useful as a stirring of the waters, through which much oxygen is brought home to many things and no little purification effected, the time for the final judgment on the question will not come until we shall more clearly understand than we do at present what we mean by physical and chemical, and may perhaps be put off until somewhere near the end of all things, when we shall know as fully as we ever shall what the forces to which we give these names can do and what they can not. Meanwhile the great thing is to push forward, so far as may be, the chemical analysis of the phenomena presented by living beings. Hitherto the physiological chemists, or the chemical physiologists, as perhaps they ought rather to be called, have perhaps gone too much their own gait and have seemed to be constructing too much a kind of chemistry of their own. But that, may I say, has in part been so because they did not receive from their distinctly chemical brethren the help of which they were in need. May I go so far as to say that to us physiologists these our brethren seemed to be lagging somewhat behind, at least along those lines of their science which directly told on our inquiries? That is, however, no longer the case. They are producing work and giving us ideas which we can carry straight into physiological problems. The remarkable work of Emil Fischer on sugars, one of the bright results of my period of thirteen years, may fully be regarded as opening up a new era in the physiology of the carbohydrates, opening up a new era because it has shown us the way how to investigate physiological problems on purely and distinctively chemical lines. Not in the carbohydrates only, but in all directions, our younger investigators are treating the old problems by the new chemical methods; the old physiological chemistry is passing away; nowhere, perhaps, is the outlook more promising than in this direction; and we may at any time receive the news that the stubborn old fortress of the proteids has succumbed to the new attack.

Another marked feature of the period has been the increasing attention given to the study of the lower forms of life, using their simpler structures and more difiuse phenomena to elucidate the more general properties of living matter. During the greater part of the present century physiologists have, as a rule, chosen as subjects of their observations almost exclusively the vertebrata; by far the larger part of the results obtained during this time have been gained by inquiries restricted to some half a dozen kinds of backboned animals; the frog and the myograph, the dog and the kymograph have almost seemed the alpha and the omega of the science. This has been made a reproach by some, but, I can not help thinking, unjustly. Physiology is, in its broad meaning, the unraveling of the potentialities of things in the condition which we call living. In the higher animals the evolution by differentiation has brought these potentialities, so to speak, near the surface, or even laid them bare as actual properties capable of being grasped. In the lower animals they still lie deep buried in primeval sameness; and we may grope among them in vain unless we have a clew furnished by the study of the higher animal. This truth seems to have been early recognized during the progress of the science. In the old time observers such as Spallanzani, with but a moderate amount of accumulated knowledge behind them and a host of problems before them, with but few lines of inquiry as yet definitely laid down, were free to choose the subjects of their investigation where they pleased, and in the wide field open to them prodded, so to speak, among all living things, indifferent whether they possessed a backbone or not. But it soon became obvious that the study of the special problems of the more highly organized creature was more fruitful, or at least more easily fruitful, than that of the general problems of the simpler forms; and hence it came about that inquiry, as it went on, grew more and more limited to the former. But an increasing knowledge of the laws of life as exemplified in the differentiated phenomena of the mammal is increasingly fitting us for a successful attack on the more general phenomena of the lowly creatures possessing little more than that molecular organization, if such a phrase be permitted, which alone supplies the conditions for the manifestation of vital activities. And though it may be true that in all periods men have from time to time labored at this theme, I think that I am not wrong in saying that the last dozen years or so mark a distinct departure both as regards the number of researches directed to it, and also, what is of greater moment, as regards the definiteness and clearness of the results thereby obtained. One has only to look at the results recorded in the valuable treatises of Verworn and Biedermann, whether obtained by the authors themselves or by others, to feel great hope that in the immediately near future a notable advance will be made in our grasp of the nature of that varying collection of molecular conditions, potencies and changes, slimy hitherto to the intellectual no less than to the physical touch, which we are in the habit of denoting by the more or less magical word protoplasm. And perhaps one happy feature of such an advance will be one step in the way of that reintegration which men of science fondly hope may ultimately follow the differentiation of studies now so fierce and attended by many ills; in the problems of protoplasm the animal physiologist touches hands with the botanist, and both find that under different names they are striving toward the same end.

Closely allied to and indeed a part of the above line of inquiry is the study of the physiological attributes of the cell and of their connection with its intrinsic organization. This is a study which, during the last dozen years, has borne no mean fruits; but it is an old study, one which has been worked at from time to time, reviving again and again as new methods offered new opportunities. Moreover, it will probably come directly before us in our sectional work, and therefore I will say nothing more of it here.

Still another striking feature of the past dozen years has been the advance of our knowledge in regard to those events of the animal body which we have now learned to speak of as "internal secretion." This knowledge did not begin in this period. The first note was sounded long ago in the middle of the century, when Claude Bernard made known what he called "the glycogenic function of the liver." Men, too, were busy with the thyroid body and the suprarenal capsules long before the meeting of the British Association at Montreal. But it was since then, namely, in 1889, that Minkowski published his discovery of the diabetic phenomena resulting from the total removal of the pancreas. That, I venture to think, was of momentous value, not only as a valuable discovery in itself, but especially, perhaps, in confirming and fixing our ideas as to internal secretion, and in encouraging further research.

Minkowski's investigation possessed this notable feature, that it was clear, sharp, and decided, and, moreover, the chief factor, namely, sugar, was subject to quantitative methods. The results of removing the thyroid body had been to a large extent general, often vague, and in some cases uncertain; so much so as to justify, to a certain extent, the doubts held by some as to the validity of the conclusion that the symptoms witnessed were really and simply due to the absence of the organ removed. The observer who removes the pancreas has to deal with a tangible and measurable result, the appearance of sugar in the urine. About this there can be no mistake, no uncertainty. And the confidence thus engendered in the conclusion that the pancreas, besides secreting the pancreatic juice, effects some notable change in the blood passing through it, spread to the analogous conclusions concerning the thyroid and the suprarenal, and moreover suggested further experimental inquiry. By those inquiries all previous doubts have been removed; it is not now a question whether or no the thyroid carries on a so-called internal secretion; the problem is reduced to finding out what it exactly does and how exactly it does it. Moreover, no one can at the present day suppose that this feature of internal secretion is confined to the thyroid, the suprarenal, and the pancreas; it needs no spirit of prophecy to foretell that the coming years will add to physiological science a large and long chapter, the first marked distinctive verses of which belong to the dozen years which have just passed away.

The above three lines of advance are of themselves enough to justify a certain pride on the part of the physiologist as to the share which his science is taking in the forward movements of the time. And yet I venture to think that each and all of these is wholly overshadowed by researches of another kind, through which knowledge has made, during the past dozen years or so, a bound so momentous and so far-reaching that all other results gathered in during the time seem to shrink into relative insignificance.

It was a little before my period, in the year 1879, that Golgi published his modest note, "Un nuovo processo di technica microscopica."[1] That was the breaking out from the rocks of a little stream which has since

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[1] Rendiconti del reale Istitulo Lombardo, Vol. XII, page 206.

swollen into a great flood. It is quite true that long before a new era in our knowledge of the central nervous system had been opened up by the works of Ferrier and of Fritsch and Hitzig. Between 1870 and 1880 progress in this branch of physiology had been continued and rapid. Yet that progress had left much to be desired. On the one hand the experimental inquiries, even when they were carried out with the safeguard of an adequate psychical analysis of the phenomena which presented themselves, and this was not always the case, sounded a very uncertain note, at least when they dealt with other than simply motor effects. They were, moreover, not unfrequently in discord with clinical experience. In general the conclusions which were arrived at through them, save such as were based on the production of easily recognized and often measurable movements, were regarded by many as conclusions of the kind which could not be ignored, which demanded respectful attention, and yet which failed to carry conviction. It seems to be risking too much to trust too implicitly to the apparent teaching of the results arrived at; something appeared wanting to give these their full validity, to explain their full and certain meaning by showing their connection with what was known in other ways and by other methods. On the other hand, during nearly all this time, in spite of the valuable results acquired by the continually improving histological technique, by the degeneration method, and by the developmental method, by the study of the periods of myelination, most of us, at all events, were sitting down, as our forefathers had done, before the intricate maze of encephalic structure, fascinated by its complexity, but wondering what it all meant. Even when we attempted to thread our way through the relatively simple tangle of the spinal cord, to expect that we should ever see our way so to unravel out the strands of fibers, here thick, there thin, now twisting and turning, and anon running straight, or so to set out in definite constellations the seeming milky way of star-like cells, so to do this as to make the conformation of the cord explain the performances of which it is capable, appeared to be something beyond our reach. And when we passed from the cord to those cerebral structures the even gross topography of which is the despair of the beginner in anatomical studies, the multiple maze of gray and white matter seemed to frame itself into the letters graven on the gateway of the city of Dis, and bid us leave all hope behind.

What a change has come upon us during the past dozen years, and how great is the hope of ultimate success which we have to-day. Into what at the meeting at Montreal seemed a cloudy mass, in which most things were indistinct and doubtful, and into which each man could read images of possible mechanisms according as his fancy led, the method of Golgi has fallen like a clarifying drop, and at the present moment we are watching with interest and delight how that vague cloud is beginning to clear up and develop into a sharp and definite picture, in which lines objectively distinct and saying one thing only reveal themselves more and more. This is not the place to enter into details, and I will content myself with pointing out as illustrative of my theme the progress which is being made in our knowledge of how we hear and how sounds affect us. A dozen years ago we possessed experimental and clinical evidence which led us to believe that auditory impulses sweeping up the auditory nerve became developed into auditory sensations through events taking place in the temporosphenoidal convolution, and we have had some indications that as these passed upward through the lower and middle brain the striæ acusticæ and the lateral fillet had some part to play. Beyond this we knew but little. To-day we can with confidence construct a diagram which he who runs can read, showing how the impulses undergoing a relay in the tuberculum acusticum and accessory nucleus pass by the striæ acusticæ and trapezoid fibers to the superior olive and trapezoid nucleus, and onward by the lateral fillet to the posterior corpus quadrageminum and to the cortex of the temporosphenoidal convolution. And if much, very much, yet remains to be done even in tracking out yet more exactly the path pursued by the impulses while they are still undeveloped impulses, not as yet lit up with consciousness, and in understanding the functional meaning of relays and apparently alternate routes, to say nothing of the deeper problems of when and how the psychical element intervenes, we feel that we have in our hands the clue by means of which we may hope to trace out clearly the mechanisms by which, whether consciousness plays its part or no, sounds affect so profoundly and so diversely the movements of the body, and haply some time or other to tell, in a plain and exact way, the story of how we hear. I have thus referred to hearing because the problems connected with this seemed, thirteen years ago, so eminently obscure; it appeared so preeminently hard a task, that of tracing out a path of an auditory impulse through the confused maze of fiber and cell presented by the lower and middle brain. Of the mechanism of sight we seemed even then to have better knowledge, but how much more clearly do we, so to speak, see vision now? So, also, with all other sensations, even those most obscure ones of touch and pain; indeed, all over the nervous system light seems breaking in a most remarkable way.

This great and significant progress we owe, I venture to say, to Golgi—to the method introduced by him; and I for one can not help being glad that this important contribution to science, as well as another contingent and most valuable one, the degeneration method of Marchi, should be among the many tokens that Italy, the mother of all sciences in times gone by, is now once more taking her right place in scientific no less than in political life. We owe, I say, this progress to Golgi in the sense that the method introduced by him was the beginning of the new researches. We owe, moreover, to Golgi not the mere technical introduction of the method, but something more. He himself pointed out the theoretical significance of the results which his method produced; and if in this he has been outstripped and even corrected by others, his original merit must not be allowed to be forgotten. Those others are many, in many lands; but two names stand out conspicuous among them. If rejuvenescent Italy invented the method, another ancient country, whose fame, once brilliant in the past, like that of Italy, suffered in later times an eclipse, produced the man who, above all others, has shown us how to use it. At the meeting at Montreal a voice from Spain telling of things physiological would have seemed a voice crying out of the wilderness; to-day the name of Ramon-y-Cayal is in every physiologist's mouth. That is one name, but there is yet another. Years ago, when those of us who are now veterans and see signs that it is time for us to stand aside were spelling out the primer of histology, one name was always before us as that of a man who touched every tissue and touched each well. It is a consoling thought to some of us elder ones that histological research seems to be an antidote to senile decay. As the companion of the young Spaniard in the pregnant work on the histology of the central nervous system done in the eighties and the nineties of the century must be named the name of the man who was brilliant in the fifties, Albert von Kölliker.

When I say that the progress of our knowledge of the central nervous system during the past thirteen years has been largely due to the application of the method of Golgi, I do not mean that it, alone and by itself, has done what has been done. That is not the way of science. Almost every thrust forward in science is a resultant of concurrent forces working along different lines; and in most cases at least significant progress comes when efforts from different quarters meet and join hands. And especially as regards methods it is true that their value and effect depend on their coming at their allotted times. As I said above, neither experimental investigation nor clinical observation nor histological inquiry by the then known methods had been idle before 1880. They had, moreover, borne even notable fruits, but one thing was lacking for their fuller fruition. The experimental and clinical results all postulated the existence of clear, definite paths for impulses within the central nervous system—of paths, moreover, which, while clear and sharp, were manifold and, under certain conditions, alternate or even vicarious, and were so constructed that the impulses as they swept along them underwent from time to time—that is, at some place or other—transformations or at least changes in nature. But the methods of histological investigations available before that of Golgi, though they taught us much, failed to furnish such an analysis of the tangle of gray and white matter as would clearly indicate the paths required. This the method of Golgi did, or rather is doing. Where gold failed silver has succeeded, and is succeeding. Thanks to the black tract which silver when handled in a certain way leaves behind it in the animal body, as indeed it does elsewhere, we can now trace out, within the central nervous system, the pathway afforded by the nerve cell and the nerve cell alone. We see its dendrites branching out in various directions, each alert to dance the molecular dance assigned to it at once by the more lasting conditions which we call structural, and the more passing ones which we call functional, so soon as some partner touch its hand. We see the body of the cell with its dominant nucleus ready to obey and yet to marshal and command the figure so started. We see the neuraxon prepared to carry that figure along itself—it may be to far distant parts, it may be to near ones—or to divert it along collaterals—it may be many, or it may be few—or to spread out at once among numerous seemingly equipollent branches. And whether it prove ultimately true or no that the figure of the dancing molecules sweeps always onward along the dendrites toward the nucleus, and always outward away from the nucleus along the neuraxon, or whatever way in the end be shown to be the exact differences in nature and action between the dendrites and the neuraxon, this at least seems sure, that cell plays upon cell only by such a kind of contact as seems to afford an opportunity for change in the figure of the dance—that is to say, in the nature of the impulse—and that in at least the ordinary play it is the terminal of the neuraxon (either of the main core or a collateral) of one cell which touches with a vibrating touch the dendrite or the body of some other cell. We can thus, I say, by the almost magic use of a silver token—I say magic use, for he who for the first time is shown a Golgi preparation is amazed to learn that it is such a sprawling thing as he sees before him which teaches so much, and yet when he comes to use it acquires daily increased confidence in its worth—it is by the use of such a silver token that we have been able to unravel so much of the intricate tangle of the possible paths of nervous impulses. By themselves, the acquisition of a set of pictures of such black lines would be of little value. But—and this I venture to think is the important point—to a most remarkable extent, and with noteworthy rapidity, the histological results thus arrived at, aided by analogous results reached by the degeneration method—especially by the newer method akin to that of Golgi, that of Marchi—have confirmed or at times extended and corrected the teachings of experimental investigation and clinical observation. It is this which gives strength to our present position; we are attacking our problems along two independent lines. On the one hand we are tracing out anatomical paths, and laying bare the joints of histological machinery; on the other hand, beginning with the phenomena, and analyzing the manifestations of disorder, whether of our own making or no, as well as of order, we are striving to delineate the machinery by help of its action. When the results of the two methods coincide, we may be confident that we are on the road of all truth; when they disagree, the very disagreement serves as the starting point for fresh inquiries along the one line or the other.

Fruitful as have been the labors of the past dozen years, we may rightly consider them as but the earnest of that which is to come; and those of us who are far down on the slope of life may wistfully look forward to the next meeting of the association on these Western shores, wondering what marvels will then be told.

Physiology, even in the narrower sense to which, by emphasis on the wavering barrier which parts the animal from the plant, it is restricted in this section, deals with many kinds of being, and with many things in each. But, somewhat as man, in one aspect a tiny fragment of the world, still more of the universe, in another aspect looms so great as to overshadow everything else, so the nervous system, seen from one point of view, is no more than a mere part of the whole organism, but, seen from another point of view, seems by its importance to swallow up all the rest. As man is apt to look upon all other things as mainly subserving his interests and purposes, so the physiologist, but with more justice, may regard all the rest of the body as mainly subserving the welfare of the nervous system; and, as man was created last, so our natural knowledge of the working of that nervous system has been the latest in its growth. But, if there be any truth in what I have urged to-day, we are witnessing a growth which promises to be as rapid as it has seemed to be delayed. Little spirit of prophecy is needed to foretell that in the not so distant future the teacher of physiology will hurry over the themes on which he now dwells so long, in order that he may have time to expound the most important of all the truths which he has to tell, those which have to do with the manifold workings of the brain.

And I will be here so bold as to dare to point out that this development of his science must, in the times to come, influence the attitude of the physiologist toward the world, and ought to influence the attitude of the world toward him. I imagine that if a plebiscite, limited even to instructed—I might almost say scientific—men, were taken at the present moment, it would be found that the most prevalent conception of physiology is that it is a something which is in some way an appendage to the art of medicine. That physiology is, and always must be, the basis of the science of healing, is so much a truism that I would not venture to repeat it here were it not that some of those enemies, alike to science and humanity, who are at times called antivivisectionists, and whose zeal often outruns, not only discretion, but even truth, have quite recently asserted that I think otherwise. Should such an hallucination ever threaten to possess me, I should only have to turn to the little we yet know of the physiology of the nervous system and remind myself how great a help the results of pure physiological curiosity—I repeat the words, pure physiological curiosity, for curiosity is the mother of science—have been, alike to the surgeon and the physician, in the treatment of those in some way most afflicting maladies, the diseases of the nervous system. No, physiology is, and always must be, the basis of the science of healing; but it is something more. When physiology is dealing with those parts of the body which we call muscular, vascular, glandular tissues and the like, rightly handled she points out the way not only to mend that which is hurt, to repair the damages of bad usage and disease, but so to train the growing tissues and to guide the grown ones as that the best use may be made of them for the purposes of life. She not only heals, she governs and educates. Nor does she do otherwise when she comes to deal with the nervous tissues. Nay, it is the very prerogative of these nervous tissues that their life is above that of all the other tissues, contingent on the environment, and susceptible of education. If increasing knowledge gives us increasing power so to mold a muscular fiber that it shall play to the best the part which it has to play in life, the little knowledge we at present possess gives us at least much confidence in a coming far greater power over the nerve cell. This is not the place to plunge into the deep waters of the relation which the body bears to the mind; but this at least stares us in the face, that changes in what we call the body bring about changes in what we call the mind. When we alter the one, we alter the other. If, as the whole past history of our science leads us to expect, in the coming years a clearer and clearer insight into the nature and conditions of that molecular dance which is to us the material token of nervous action, and a fuller, exacter knowledge of the laws which govern the sweep of nervous impulses along fiber and cell, give us wider and directer command over the molding of the growing nervous mechanism and the maintenance and regulation of the grown one, then assuredly physiology will take its place as a judge of appeal in questions not only of the body, but of the mind; it will raise its voice not in the hospital and consulting room only, but also in the senate and the school.

One word more. We physiologists are sorely tempted toward self-righteousness, for we enjoy that blessedness which comes when men revile you and persecute you and say all manner of evil against you falsely. In the mother country our hands are tied by an act which was defined by one of the highest legal authorities as a "penal" act; and though with us, as with others, difficulties may have awakened activity, our science suffers from the action of the State. And some there are who would go still further than the State has gone, though that is far—who would take from us even that which we have, and bid us make bricks wholly without straw. To go back is always a hard thing, and we in England can hardly look to any great betterment for at least many years to come. But unless what I have ventured to put before you to-day be a mocking phantasm, unworthy of this great association and this great occasion, England in this respect at least offers an example to be shunned alike by her offspring and her fellows.

THE FACTORS OF ORGANIC EVOLUTION FROM A BOTANICAL STANDPOINT.[1]

By Prof. L. H. Bailey.

The Survival of the Unlike.

We all agree that there has been and is evolution; but we probably all disagree as to the exact agencies and forces which have been and are responsible for it. The subject of the agencies and vehicles of evolution has been gone over repeatedly and carefully for the animal creation, but there is comparatively little similar research and speculation for the plant creation. This deficiency upon the plant side is my excuse for calling your attention, in a popular way, to a few suggestions respecting the continuing creation of the vegetable world, and to a somewhat discursive consideration of a number of illustrations of the methods of advance of plant types.

1. NATURE OF THE DIVERGENCE OF THE PLANT AND ANIMAL.

It is self-evident that the development of life upon our planet has taken place along two divergent lines. These lines originated at a common point. This common life-plasma was probably at first more animal-like than plant-like. The stage in which this life-plasma first began to assume plant-like functions is closely and possibly exactly preserved to us in that great class of organisms which are known as mycetozoa when studied by zoologists and as myxomycetes when studied by botanists. At one stage of their existence these organisms are amœba-like, that is, animal-like, but at another stage they are sporiferous or plant-like. The initial divergencies in organisms were no doubt concerned chiefly in the methods of appropriating food, the animal-like organisms apprehending their food at a more or less definite point, and the plant-like organisms absorbing food throughout the greater or even the entire part of their periphery. It is not my purpose to trace the particular steps or methods of these divergencies, but to call your attention to what I believe to be a fundamental distinction between the two lines of development, and one

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[1] Read before the American Philosophical Society May 1, 1896. Printed in the Proceedings of the American Philosophical Society, Vol. XXXV, 1896.

which I do not remember to have seen stated in the exact form in which it lies in my mind.

Both lines probably started out with a more or less well-marked circular arrangement of the parts or organs. This was consequent upon the peripheral arrangement of the new cells in the development of the multicellular organism from the unicellular one. A long line of animal life developed in obedience to this peripheral or rotate type of organization, ending in the echinoderms and some of the mollusks. This line long ago reached its zenith. No line of descent can be traced from them, according to Cope. The progressive and regnant type of animal life appeared in the vermes or true worms, forms which are characterized by a two-sided or bilateral, and therefore more or less longitudinal, structure. The animal-like organisms were strongly developed in the power of locomotion, and it is easy to see that the rotate or centrifugal construction would place the organism at a comparative disadvantage, because its seat of sensation is farthest removed from the external stimuli. But the worm-like organisms, "being longitudinal and bilateral," writes Cope, "one extremity becomes differentiated by first contact with the environment." In other words, the animal type has shown a cephalic or head-forming evolution in consequence of the bilateralism of structure. The individual has become concentrated. Out of this worm-form type, therefore, all the higher ranges of zootypic evolution have sprung, and one is almost tempted to read a literal truth into David's lamentation that "I am a worm and no man."

If, now, we turn to plants we find the rotate or peripheral arrangement of parts emphasized in all the higher ranges of forms. The most marked bilateralism in the plant world is among the bacteria, desmids, and the like, in which locomotion is markedly developed; and these are also among the lowest plant types. But plants soon became attached to the earth, or, as Cope terms them, they are "earth parasites." They therefore found it to their advantage to reach out in every direction from their support in the search for food. Whilst the centrifugal arrangement has strongly tended to disappear in the animal creation, it has tended with equal strength to persist and to augment itself in the plant creation. Its marked development among plants began with the acquirement of terrestrial life, and with the consequent evolution of the asexual or sporophytic type of vegetation. Normally the higher type of plant bears its parts more or less equally upon all sides, and the limit to growth is still determined by the immediate environment of the given individual or of its recent ancestors. Its evolution has been acephalic, diffuse, or headless, and the individual plant or tree has no proper concentration of parts. For the most part it is filled with unspecialized plasma, which, when removed from the parent individual (as in cuttings and grafts), is able to reproduce another like individual. The arrangements of leaves, branches, the parts of the flower, and even of seeds in the fruit, are thus rotate or circular, and in the highest type of plants the annual lateral increments of growth are disposed in like fashion; and it is significant to observe that in the compositæ, which is considered to be the latest and highest general type of plant form, the rotate or centrifugal arrangement is most emphatically developed. The circular arrangement of parts is the typical one for higher plants, and any departure from this form is a specialization, and demands explanation.

The point I wish to urge, therefore, is the nature of the obvious or external divergence of plant-like and animal-like lines of ascent. The significance of the bilateral structure of animal types is well understood, but this significance has been drawn, so far as I know, from a comparison of bilateral or dimeric animals with rotate or polymeric animals. I want to put a larger meaning into it by making bilateralism the symbol of the onward march of animal evolution and circumlateralism (if I may invent the term) the symbol of plant evolution. The suggestion, however, applies simply to the general arrangement of the parts or organs of the plant body, and has no relation whatever to functional attributes or processes. It is a suggestion of analogues, not of homologues. We may therefore contrast these two great lines of ascent, which, with so many vicissitudes, have come up through the ages, as Dipleurogenesis and Centrogenesis.

The two divergent directions of the lines or phyla of evolution have often been the subject of comment, but one of the sharpest contrasts between the two was made in 1884 by Cope, when he proposed that the vegetable kingdom has undergone a degenerate or retrogressive evolution. "The plants in general," he then wrote, "in the persons of their protist ancestors, soon left a free-swimming life and became sessile. Their lives thus became parasitic, more automatic, and, in one sense, degenerate." The evolution of the plant creation is, therefore, held to be a phenomenon of catagenesis or decadence. This, of course, is merely a method of stating a comparison with the evolution of the animal line or phylum, and is therefore of the greatest service. For myself, however, I dislike the terms retrogressive, catagenetic, and the like, as applied to the plant creation, because they imply intrinsic or actual degeneracy. True retrogressive or degenerate evolution is the result of loss of attributes. Cope holds that the chief proof of degeneracy in the plant world is the loss of a free-swimming habit, but it is possible that the first life-plasma was stationary; at any rate, we do not know that it was motile. Degeneracy is unequivocally seen in certain restricted groups where the loss of characters can be traced directly to adaptive changes, as in the loss of limbs in the serpents. Retarded evolution expresses the development of the plant world better than the above terms, but even this is erroneous, because plant types exhibit quite as complete an adaptation to an enormous variety of conditions as animals do, and there has been rapid progress toward specialization of structure. As a matter of fact, the vegetable world does not exhibit, as a whole, any backward step, any loss of characters once gained, nor any stationary or retarded periods; but its progress has been widely unlike that of the animal world and it has not reached the heights which that line of ascent has attained. The plant phylum can not be said to be catagenetic, but suigenetic. Or, in other words, it is centrogenetic as distinguished from dipleurogenetic.

The hearer should be reminded, at this point, of the curious alternation of generations which has come about in the plant world. One generation develops sexual functions, and the product of the sexual union is an asexual generation, and this, in turn, gives rise to another sexual generation like the first. In the lowest sex plants, as the algæ, the sexual generation—or the gametophyte, as it is called—generally comprises the entire plant body, and the asexual generation or sporophyte develops as a part of the fructifying structure of the gametophyte, and is recognizable as a separate structure only by students of special training. In the fungi, which are probably of catagenetic evolution, alternation of generations is very imperfect or wanting. In the true mosses the gametophyte is still the conspicuous part of the plant structure. It comprises all that part of the moss which the casual observer recognizes as "the plant." The sporophytic generation is still attached to the persistent gametophyte, and it is the capsule with its stem and appendages. In the ferns, however, the gametophytic stage is of short duration. It is the inconspicuous prothallus, which follows the germination of the spore. Therefrom originates "the fern," all of which is sporophytic, and the gametophyte perishes. With the evolution of the flowering plants the gametophyte becomes still more rudimentary, while the sporophyte is the plant, tree, or bush as we see it. The gametophytic generation is associated with the act of fertilization, the male prothallus or gametophyte developing from the pollen grain and soon perishing, and the female prothallus or gametophyte developing in the ovule and either soon perishing or persisting in the form of the albumen of the seed. The great development of the sporophyte in later time is no doubt a consequence of the necessity of assuming a terrestrial life; and with this development has come the perfection of the centrogenic form.

2. THE ORIGIN OF DIFFERENCES.

The causes which have contributed to the origin of the differences which we see in the organic creation have been and still are the subjects of the most violent controversy. Those persons who conceive these differences to have come into existence full formed, as they exist at the present time, are those who believe in the dogma of special creations, and they usually add to the doctrine a belief in design in nature. This doctrine of special creation receives its strongest support when persons contrast individual objects in nature. Certainly nothing can seem more unlike in very fundamental character than an insect and an elephant, a starfish and a potato, a man and an oak tree. The moment one comes to study the genealogies of these subjects or groups, however, he comes upon the astonishing fact that the ancestors are more and more alike the farther back they are traced. In other words, there are great series of convergent histories. Every naturalist, therefore, is compelled to admit that differences in nature have somehow been augmented in the long processes of time. It is unnecessary, therefore, that he seek the causes of present differences until he shall have determined the causes of the smallest or original differences. It is thus seen that there are two great and coordinate problems in the study of evolution—the causes of initial differences and the means by which differences are augmented. These two problems are no doubt very often expressions of the same force or power, for the augmentation of a difference comes about by the origination of new degrees of difference; that is, by new differences. It is very probable that the original genesis of the differences is often due to the operation of the very same physiological processes which gradually enlarge the difference into a gulf of wide separation.

In approaching this question of the origin of unlikenesses the inquirer must first divest himself of the effects of all previous teaching and thinking. We have reason to assume that all beings came from one original life-plasma, and we must assume that this plasma had the power of perpetuating its physiological identity. Most persons still further assume that this plasma must have been endowed with the property of reproducing all its characters of form and habit exactly, but such assumption is wholly gratuitous and is born of the age-long habit of thinking that like produces like. We really have no right to assume either that this plasma was or was not constituted with the power of exact reproduction of all its attributes, unless the behavior of its ascendants forces us to the one or the other conclusion. Inasmuch as no two individual organisms ever are or ever have been exactly alike, so far as we can determine, it seems to me to be the logical necessity to assume that like never did and never can produce like. The closer we are able to approach to plasmodial and unspecialized forms of life in our studies of organisms, the more are we impressed with the weakness of the hereditary power. Every tyro in the study of protoplasm knows that the amœba has no form. The shapes which it assumes are individual and do not pass to the descendants. To my mind, therefore, it is a more violent assumption to suppose that this first unspecialized plasma should exactly reproduce all its minor features than to suppose that it had no distinct hereditary power and therefore, by the very nature of its constitution, could not exactly reproduce itself. The burden of proof has been thrown upon those who attempt to explain the initial origin of differences, but it should really be thrown upon those who assume that life matter was originally so constructed as to rigidly recast itself into one mold in each succeeding generation. I see less reason for dogmatically assuming that like produces like than I do for supposing that unlike produces unlike.

I advanced this proposition a year ago in my Plant Breeding (pp. 9, 10), and I am now glad to find, since writing the above paragraph, that H. S. Williams has reached similar conclusions in his new Geological Biology. He regards mutability as the fundamental law of organisms, and speaks of the prevalent notion that organisms must necessarily reproduce themselves exactly as "one of the chief inconsistencies in the prevalent conception of the nature of organisms." "While the doctrine of mutability of species has generally taken the place of immutability," he writes, "the proposition that like produces like in organic generation is still generally, and I suppose almost universally, accepted. It therefore becomes necessary to suppose that variation is exceptional, and that some reason for the accumulation of variation is necessary to account for the great divergencies seen in different species. * * * The search has been for some cause of the variation; it is more probable that mutability is the normal law of organic action, and that permanency is the acquired law." I do not suppose that Professor Williams makes definite variation an inherent or necessary quality of organic matter, but that this matter had no original hereditary power, and that its form and other attributes in succeeding generations have been molded into the environment, and that the burden of proof is thrown upon those who assume that life matter was endowed with the property that like necessarily produces like. At all events, this last is my own conception of the modification of the streams of ascent.

In other words, I look upon heredity as an acquired character, the same as form, or color, or sensation is, and not as an original endowment of matter. The hereditary power did not originate until for some reason it was necessary for a given character to reproduce itself, and the longer any form or character was perpetuated the stronger became the hereditary power.

It is now pertinent to inquire what determined the particular differences which we know to have persisted. The mere statement that some forms became sessile or attached to the earth, and that others became or remained motile, is an assumption that these differences were direct adaptations to environment. Every little change in environment incited a corresponding change in the plastic organization; and the greater and more various the changes in the physical attributes of the earth with the lapse of time, the greater became the modifications in organisms. I believe, therefore, that the greater part of present differences in organisms are the result directly and indirectly of external stimuli, until we come into those higher ranges of being in which sensation and volition have developed, and in which the effects of use and disuse and of psychological states have become increasingly more important as factors of ascent. The whole moot question, then, as to whether variations are definite or multifarious, is aside from the issue. They are as definite as the changes in the environment, which determine and control their existence. More differences arise than can persist, but this does not prove that those which are lost are any the less due to the impinging stimuli. Those who write of definite variation usually construe the result or outcome of some particular evolution into a measure of the variation which is conceived to have taken place in the group. Most or all of the present characters of any group are definite because they are the survivals in a process of elimination; but there may have been, at various times, the most diverse and diffuse variations in the very group which is now marked by definite attributes. As the lines of ascent developed, and generation followed generation in countless number, the organization was more and more impressed with the features of ancestral characters, and these ancestral characters are the more persistent as they have been more constant in the past. But these characters, which appear as heredity or atavistic variations in succeeding generations, were no doubt first, at least in the plant creation, the offspring, for the most part, of the environment reacting upon the organism. As life has ascended in the time scale and has become increasingly complex, so the operation of any incident force must ever produce more diverse and unpredictable results. What I mean to say is that, in plants, some of the variations seem to me to be the resultants of a long line of previous incident impressions, or have no immediate inciting cause. Such variation is, to all appearances, fortuitous. It is, therefore, evident that the study of the effects of impinging environments at the present day may not directly elucidate the changes which similar conditions may have produced in the beginning.

While the steadily ascending line of the plant creation was fitting itself into the changing moods of the external world, it was at the same time developing an internal power. Plants were constantly growing larger and stronger or more specialized. The accumulation of vital energy is an acquired character the same as peculiarities of form or structure are. It is the accumulated result of every circumstance which has contributed to the well-being and virility of the organism. The gardener knows that he can cause the plant to store up energy in the seed, so that the resulting crop will be the larger. Growth is itself but the expression or result of this energy which has been picked up by the way through countless ages. Now, mere growth is variation. It results in differences. Plants can not grow without being unlike. The more luxuriant the growth, the more marked the variation. Most plants have acquired or inherited more growth force than they are able to use because they are held down to certain limitations by conditions in which they are necessarily placed by the struggle for existence. I am convinced that many of the members of plants are simply outgrowths resulting from this growth pressure, or, as Bower significantly speaks of them,[1] the result of an "eruptive process." The pushing out of shoots from any part of the plant body, upon occasion, the normal production of adventitious plantlets upon the stems and leaves of some

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[1] A Theory of the Strobilus in Archegoniate Plants, Annals of Botany, viii, 358, 359.

begonias (especially *Begonia phyllomaniaca*), bryophyllum, some ferns, and many other plants, are all expressions of the growth force which is a more or less constant internal power. This growth force may give rise to more definite variations than impinging stimuli do; but the growth force runs in definite directions because it, in its turn, is the survival in a general process of elimination. Many of the characters of plants which—for lack of better explanation—we are in the habit of calling adaptive, are no doubt simply the result of the eruption of tissue. Very likely some of the compounding of leaves, the pushing out of some kinds of prickles, the duplication of floral organs, and the like are examples of this kind of variation. We know that the characters of the external bark or cortex upon old tree trunks are the result of the internal pressure in stretching and splitting it. This simply shows how the growth force may originate characters of taxonomic significance when it is expressed as mere mechanical power acting upon tissue of given anatomical structure. This power of growth is competent, I think, to originate many and important variations in plants. I suppose my conception of it to be essentially the same as that of the bathmism of Cope, and the Theory of the Organic Growth, of Eimer.

We have now considered two general types of forces or agencies which start off variations in plants—purely external stimuli, and the internal acquired energy of growth. There is still a third general factor, crossing, or, as Eimer writes it, "sexual mixing." The very reason for the existence of sex, as we now understand it, is to originate differences by means of the union of two parents into one offspring. This sexual mixing can not be considered to be an original cause of unlikenesses, however, since sex itself was at first a variation induced by environment or other agencies, and its present perfection in higher organisms is the result of the process of continuous survival in a conflict of differences.

The recent rise of Lamarckian views seems to have been largely the result of an attempt to discover the vera causa of variations. Darwin's hypothesis of natural selection assumes variability without inquiring into its cause, and writers have therefore said that Darwin did not attempt to account for the cause of variations. Nothing can be further from his views. Yet some of our most recent American writings upon organic evolution repeat these statements. Cope, in his always admirable Primary Factors of Organic Evolution, writes that "Darwin only discussed variation after it came into being." Yet Darwin's very first chapter in his Origin of Species contains a discussion of the "Causes of variability," and the same subject is gone over in detail in "Variation of animals and plants under domestication." Darwin repeatedly refers the cause or origin of variation to "changed conditions of life," which is essentially the position maintained by the Lamarckians; and he as strenuously combats those who hold that definite variation is an innate attribute of life. "But we must, I think, conclude" * * * writes Darwin in the latter book, "that organic beings, when subjected during several generations to any change whatever in their conditions, tend to vary." He discussed at length the particular agencies which he considered to be most potent in inducing variability, and enumerated amongst other factors the kind and amount of food, climate, and crossing. "Changes of any kind in the conditions of life," he repeats, "even extremely slight changes, often suffice to cause variability. Excess of nutriment is perhaps the most efficient single exciting cause." Cope, in his discussion of the "Causes of variation," starts out with the proposition "to cite examples of the direct modifying effect of external influences on the characters of individual animals and plants," and he closes with this paragraph: "I trust that I have adduced evidence to show that the stimuli of chemical and physical forces, and also molar motion or use and its absence, are abundantly sufficient to produce variations of all kinds in organic beings. The variations may be in color, proportions, or details of structure, according to the conditions which are present." This is, in great part, the thesis to which Darwin extended the proofs of a most laborious collection of data from gardeners and stock-breeders and from feral nature. It has been the great misfortune of the interpretation of Darwin's writings that his hypothesis of natural selection has so completely overtopped everything else in the reader's mind that other important matters have been overlooked.

While the one central truth in the plant creation is the fact that differences arise as a result of variations in environment, there are nevertheless many exceptions to it. There are various types of differences which are merely incidental or secondary to the main stem of adaptive ascent. Some of these are such as arise from the cessation of the constructive agencies, and others are mere correlatives or accompaniment of type differences. As an example of the former, we may cite the behavior of the potato. By high cultivation and careful breeding, the plant has been developed to produce enormous crops of very large tubers, so heavy a crop that the plant has been obliged to spare some of its energy from the production of pollen and berries for the purpose of maintaining the subterranean product. It is evident that this high state of amelioration can be maintained only by means of high cultivation. The moment there is a let-down in the factors which have bred and maintained the plant, there is a tendency toward a breaking up and disappearance of the high-bred type. This is an illustration of the phenomenon of panmixia, as outlined by Weismann, except that the force which has ceased to act is human selection rather than natural selection. "This suspension of the preserving influence of natural selection," Weismann writes, "may be termed Panmixia." In his opinion, "the greater number of those variations which are usually attributed to the direct influence of external conditions of life are to be attributed to panmixia. For example, the great variability of most domesticated animals and plants essentially depends upon this principle." In other words, certain differences are preserved through the agency of natural selection, and certain differences are lost; if the organism is removed from this restraining and directing agency, all variations have the chance of asserting themselves. "All individuals can reproduce themselves," Weismann explains, "and thus stamp their characters upon the species, and not only those which are in all respects, or in respect to some single organ, the fittest." I am convinced that this term expresses a very important truth, and one which, as Weismann says, is particularly apparent in domestic animals and plants; but panmixia does not express an incident force. If new differences arise in consequence of the cessation of the directive agency of natural selection, it is because they were first impressed upon the organization by some unaccountable agency; or, if there is simply a falling away from accumulated characters, the residuary or secondary features which appear are probably the compound and often deteriorated result of various previous incident forces. In short, panmixia is a name for a class of phenomena, and it can not be considered as itself an original cause of variation. It is, to my mind, largely the unrestrained expression or unfolding of the growth-force consequent upon the removal of the customary pressure under which the plant has lived.

3. THE SURVIVAL OF THE UNLIKE.

The one note of the modern evolution speculations which has resounded to the remotest corner of civilization, and which is the chief exponent of current speculation respecting the origin and destiny of the organic world, is Spencer's phrase, "The survival of the fittest." This epigram is an epitome of Darwin's law of natural selection, or "the preservation, during the battle for life, of varieties which possess any advantage in structure, constitution, or instinct." In most writings, these two phrases—"natural selection" and "the survival of the fittest"—are used synonymously; but in their etymology they really stand to each other in the relation of process and result. The operation of natural selection results in the survival of the fittest. One must not be too exact, however, in the literal application of such summary expressions as these. Their particular mission is to afford a convenient and abbreviated formula for the designation of important principles, for use in common writing and speech, and not to express a literal truth. Darwin was himself well aware of the danger of the literal interpretation of the epigram "natural selection." "The term 'natural selection,'" he writes, "is in some respects a bad one, as it seems to imply conscious choice; but this will be disregarded after a little familiarity." This technical use of the term "natural selection" is now generally accepted unconsciously; and yet there have been recent revolts against it upon the score that it does not itself express a literal principle or truth. If we accept the term in the sense in which it was propounded by its author, we are equally bound to accept "survival of the fittest" as a synonymous expression, because its author so designed it. "By natural selection or survival of the fittest," writes Spencer, "by the preservation in successive generations of those whose moving equilibria happen to be least at variance with the requirements, there is eventually produced a changed equilibrium completely in harmony with the requirements."

It should be said that there is no reason other than usage why the phrase "survival of the fittest" should not apply to the result of Lamarckian or functional evolution as well as of Darwinian or selective evolution. It simply expresses a fact, without designating the cause or the process. Cope has written a book upon the Origin of the Fittest, in which the argument is Lamarckian. The phrase implies a conflict, and the loss of certain contestants and the salvation of certain others. It asserts that the contestants or characters which survive are the fittest, but it does not explain whether they are fit because endowed with greater strength, greater prolificness, completer harmony with surroundings, or other attributes. I should like to suggest, therefore, that the chiefest merit of the survivors is unlikeness, and to call your attention for a few minutes to the significance of the phrase—which I have used in my teaching during the last year—the survival of the unlike.

This phrase—the survival of the unlike—expresses no new truth, but I hope that it may present the old truth of vicarious or nondesigned evolution in a new light. It defines the fittest to be the unlike. You will recall that in this paper I have dwelt upon the origin and progress of differences rather than of definite or positive characters. I am so fully convinced that, in the plant creation, a new character is useful to the species because it is unlike its kin, that the study of difference between individuals has come to be, for me, the one absorbing and controlling thought in the contemplation of the progress of life. These differences arise as a result of every impinging force—soil, weather, climate, food, training, conflict with fellows, the strain and stress of wind and wave and insect visitors—as a complex resultant of many antecedent external forces, the effects of crossing, and also as the result of the accumulated force of mere growth; they are indefinite, non designed, an expression of all the various influences to which the passive vegetable organism is or has been exposed; those differences which are most unlike their fellows or their parents find the places of least conflict, and persist because they thrive best and thereby impress themselves best upon their offspring. Thereby there is a constant tendency for new and divergent lines to strike off, and these lines, as they become accented, develop into what we, for convenience sake, have called species. There are, therefore, as many species as there are unlike conditions in physical and environmental nature, and in proportion as the conditions are unlike and local are the species well defined. But to nature, perfect adaptation is the end; she knows nothing, per se, as species or as fixed types. Species were created by John Ray, not by the Lord; they were named by Linnæus, not by Adam.

I must now hasten to anticipate an objection to my phrase which may arise in your minds. I have said that when characters are unlike existing characters they stand a chance of persisting; but I do not desire to say that they are useful in proportion as they are unlike their kin. I want to express my conviction that mere sports are rarely useful. These are no doubt the result of very unusual or complex stimuli, or of unwonted refrangibility of the energy of growth, and not having been induced by conditions which act uniformly over a course of time, they are likely to be transient. I fully accept Cope's remark that there is "no ground for believing that sports have any considerable influence on the course of evolution. * * * The method of evolution has apparently been one of successful increment and decrement of parts along definite lines." Among domestic animals and plants the selection and breeding of sports, or very unusual and marked variations, has been a leading cause of their strange and diverse evolution. In fact, it is in this particular thing that the work of the breeder and the gardener is most unlike the work of nature. But in feral conditions, the sport may be likened to an attribute out of place; and I imagine that its chief effect upon the phylogeny of a race—if any effect it have—is in giving rise in its turn to a brood of less erratic unlikenesses. This question of sports has its psychological significance, for if the way becomes dark the wanderer invokes the aid of this ignis fatuus to cut short his difficulties. Sir William Thompson supposes that life may first have come to earth by way of some meteor, and Brinton proposes that man is a sport from some of the lower creation. It is certainly a strange type of mind which ascribes a self-centered and self-sufficient power to the tree of life, and then, at the very critical points, adopts a wholly extraneous force and one which is plainly but a survival of the old cataclysmic type of mind; and it is the stranger, too, because such type of explanation is not suggested by observation or experiment, but simply by what is for the time an insuperable barrier of ignorance of natural processes. If evolution is true at all, there is reason to suppose that it extends from beginning to finish of creation, and the stopping of the process at obscure intervals is only a temporary satisfaction to a mind that is not yet fully committed to the eternal truth of ascent. The tree of life has no doubt grown steadily and gradually, and the same forces, variously modified by the changing physical conditions of the earth, have run on with slow but mighty energy until the present time. Any radical change in the plan would have defeated it, and any mere accidental circumstance is too trivial to be considered as a modifying influence of the great onward movement of creation, particularly when it assumes to account for the appearing of the very capstone of the whole mighty structure.

Bear with me if I recite a few specific examples of the survival of the unlike, or of the importance, to organic types, of gradually widening differences. Illustrations might be drawn from every field of the organic creation, but I choose a few from plants because these are the most neglected and I am most familiar with them. These are given to illustrate how important external stimuli are in originating variation and how it is that some of these variations persist.

Let me begin by saying that a good gardener loves his plants. Now, a good gardener is one who grows good plants, and good plants are very unlike poor plants. They are unlike because the gardener's love for them has made them so. The plants were all alike in November; in January the good gardener's plants are strong and clean, with large dense leaves, a thick stem, and an abundance of perfect flowers; the poor gardener's plants are small and mean, with curled leaves, a thin hard stem, and a few imperfect flowers. You will not believe now that the two lots were all from the same seed pod three months ago. The good gardener likes to save his own seeds or make his own cuttings; and next year his plants will be still more unlike his neighbor's. The neighbor tries this seed and that, reads this bulletin and that, but all avails nothing simply because he does not grow good plants. He does not care for them tenderly, as a fond mother cares for a child. The good gardener knows that the temperature of the water and the air, the currents in the atmosphere, the texture of the soil, and all the little amenities and comforts which plants so much enjoy, are just the factors which make his plants successful; and a good crop of anything, whether wheat or beans or apples, is simply a variation.

And do these unlikenesses survive? Yes, verily! The greater part of the amelioration of cultivated plants has come about in just this way—by gradual modifications in the conditions in which they are grown, by means of which unlikenesses arise; and then by the selection of seeds from the most coveted plants. Even at the present day there is comparativaly little plant breeding. The cultivated flora has come up with man, and if it has departed immensely from its wild prototypes, so has man. The greater part of all this has been unconscious and unintended on man's part, but it is none the less real.

As an illustration of how large the factors of undesigned choice and selection are in the amelioration of the domestic flora, let me ask your attention to the battle of the seed bags. In the year 1890 the census records show for the first time the number of acres in the United States devoted to the growing of seed. I give the acreage of three representative crops, and these figures I have multiplied by the average seed yields per acre in order to arrive at an approximate estimate of the entire crop produced and the number of acres which the crop would plant. I have used low averages of yields in order to be on the safe side, and I have likewise used liberal averages of the quantity of seed required to plant an acre when making up the last column:

	Acres.	Average yield per acre.	Approximate crop.	Would plant—
		Pounds.	*Pounds.*	*Acres.*
Cabbage	1, 268	200	253, 600	1, 014, 400
Cucumber	10, 219	120	1, 226, 280	613, 140
Tomato	4, 356	80	368, 480	1, 473, 920

The last column in this table has particular interest because it shows the enormous acreage which these seeds, if all planted, would cover. We are now curious to know if such areas really are planted to these species, and if they are not, it will be pertinent to inquire what becomes of the seeds. Unfortunately, we have no statistics of the entire acreages of these various truck-garden crops, but the same census gives the statistics of the commercial market gardens of the country. Inquiry of seed merchants here has convinced me that about one-fourth of all the seeds sold in any year go to market gardeners. I have therefore multiplied the census figures of market gardens by four for the purpose of arriving at an estimate of the total acreage of the given crops in the United States; and I have introduced the last column from the above table for purposes of comparison:

	Acreage of market gardens.	Probable total acreage.	There are seeds enough to plant—	Difference.
			Acres.	*Acres.*
Cabbage	77,094	308,376	1,014,400	706,024
Cucumber	4,721	18,884	613,140	594,256
Tomato	22,802	91,208	1,473,920	1,382,712

It will thus be seen that there are enough cabbage seeds raised in this country each year, if the census year is a fair sample, to plant nearly three-quarters of a million acres more than actually are planted; about the same surplus of cucumber seeds, and a surplus of tomato seeds sufficient to plant over one and a quarter million acres. It is possible, of course, that the figures of actual acreage of these crops are too low; but such error, if it occur, must be much overbalanced by the large quantities of home-grown and imported seeds which are used every year. These startling figures would not apply so well to many other crops which are detailed in the census bulletin. For instance, the pease raised in this country would plant only about 46,000 acres, while there are over 100,000 acres actually grown; but this discrepancy is probably accounted for by the fact that the larger part of the seed pease are grown in Canada, and therefore do not figure in our census. There is a somewhat similar discrepancy in the watermelon, but in this crop the seeds are very largely home saved by the heavy planters in the South and West. I do not give these figures for their value as statistics, but simply for the purpose of graphically expressing the fact that many more seeds are raised by cultivators each average year than are ever grown into plants, and that the struggle for existence does not necessarily cease when plants are taken under the care of man.

What, now, becomes of this enormous surplus of seeds? Let us take a rough survey of the entire seed crop of any year. In the first place, a certain percentage of the seeds is laid aside by the seedsman as a surety against failure in the year to come. Much of this old stock never finds its way into the market and is finally discarded. We will estimate this element of waste as 20 per cent. Of the 80 per cent which is actually sold, perhaps another 10 per cent is never planted, leaving about 70 per cent which finds its way into the ground. These two items of loss are pure waste and have no effect upon the resulting crop. Now, of the seeds which are planted, not more than 75 per cent can be expected to germinate; that is, there is certainly an average loss of 25 per cent in nearly all seeds, and much more in some, due to inherent weakness, and 75 per cent represents the survival in a conflict of strength. We have now accounted for about half of the total seed product of any year. The remaining half produces plants; but here the most important part of the conflict begins. In the crops mentioned above much less than half of the seeds which are grown ever appear in the form of a crop. We must remember, moreover, that in making the estimate of the number of acres which these seeds would plant I have used the customary estimates of the quantity of seeds required to plant an acre. Now, these estimates of seedsmen and planters are always very liberal. Every farmer sows from five to twenty times more seeds than he needs. Some years ago I sowed seeds according to the recommendation of one of our best seedsmen, and I found that pease would be obliged to stand four-fifths of an inch apart, beets about twenty to the foot, and other vegetables in like confusion. I suppose that of all the seeds which actually come up not more than one in ten or a dozen, in garden vegetables, ever give mature plants. What becomes of the remainder? They are thinned out for the good of those which are left.

This simple process of thinning out vegetables has had a most powerful effect upon the evolution of our domestic flora. It is a process of undesigned selection. This selection proceeds upon the differences in the seedlings. The weak individuals are disposed of, and those which are strongest and most unlike the general run are preserved. It is a clear case of the survival of the unlike. The laborer who weeds and thins your lettuce bed unconsciously blocks out his ideas in the plants which he leaves. But all this is a struggle of Jew against Jew, not of Jew against Philistine. It is a conflict within the species, not of species against species. It therefore tends to destroy the solidarity of the specific type and helps to introduce much of that promiscuous unlikeness which is the distinguishing characteristic of domestic plants.

Let us now transfer this emphatic example to wild nature. There we shall find the same prodigal production of seeds. In the place of the gardener undesignedly molding the lines of divergence we find the inexorable physical circumstances into which the plastic organisms must grow, if they grow at all. These circumstances are very often the direct causes of the unlikenesses of plants, for plants which start like when they germinate may be very unlike when they die. Given time and constantly but slowly changing conditions, and the vegetable creation is fashioned into the unlikenesses which we now behold. With this conception let us read again Francis Parkman's picturesque description of the forest of Maine in his Half Century of Conflict.

> "For untold ages Maine had been one unbroken forest, and it was so still. Only along the rocky seaboard, or on the lower waters of one or two great rivers, a few rough settlements had gnawed slight indentations into this wilderness of woods, and a little farther inland some dismal clearing around a blockhouse or stockade let in the sunlight to a soil that had lain in shadow time out of mind. This waste of savage vegetation survives, in some part, to this day, with the same prodigality of vital force, the same struggle for existence and mutual havoc that mark all organized beings, from men to mushrooms. Young seedlings in millions spring every summer from the black mold, rich with the decay of those that had preceded them, crowding, choking, and killing each other, perishing by their very abundance; all but a scattered few, stronger than the rest, or more fortunate in position, which survive by blighting those about them. They in turn, as they grow, interlock their boughs, and repeat in a season or two the same process of mutual suffocation. The forest is full of lean saplings dead or dying with vainly stretching toward the light. Not one infant tree in a thousand lives to maturity; yet these survivors form an innumerable host, pressed together in struggling confusion, squeezed out of symmetry and robbed of normal development, as men are said to be in the level sameness of democratic society. Seen from above, their mingled tops spread in a sea of verdure basking in light; seen from below, all is shadow, through which spots of timid sunshine steal down among legions of dark, mossy trunks, toadstools and rank ferns, protruding roots, matted bushes, and rotten carcasses of fallen trees. A generation ago one might find here and there the rugged trunk of some great pine lifting its verdant spire above in the distinguished myriads of the forest. The woods of Maine had their aristocracy; but the ax of the woodman has laid them low, and these lords of the wilderness are seen no more."

In such bold and generalized examples as this the student is able to discern only the general fact of progressive divergency and general adaptation to conditions, without being able to discover the particular directive forces which have been at the bottom of the evolution. It is only when one considers a specific example that he can arrive at any just conclusions respecting initial causes of modification. Of adaptive modifications, two general classes have been responsible for the ascent of the vegetable kingdom, one a mere molding or shaping into the passive physical environments, the other the direct result of stress or strain imposed upon the organism by wind and water and by the necessities of a radical change of habit from aquatic to terrestrial life, and later on by the stimuli of insects upon the flowers. One of the very best examples of the mere passive ascent is afforded by the evolution of the root as a feeding organ; and a like example of development as a result of strain is afforded by the evolution of the stem and vascular or fibrous system. Our present flora, like our present fauna, is an evolution from aquatic life. The first sessile or stationary plants were undoubtedly stemless. As the waters increased in depth and plants were driven farther and farther from their starting points by the struggle for place and the disseminating influence of winds and waves, the plant body became more and more elongated. While the plant undoubtedly still absorbed food throughout its entire periphery, it nevertheless began to differentiate into organs. The area chiefly concerned in food gathering became broadened into a thallus, a constricted or stem-like portion tended to develop below, and the entire structure anchored itself to the rock by a holdfast or grapple. This holdfast, or so-called root, of most of our present seaweeds is chiefly a means of holding the plant in place, and it probably absorbs very little food. As plants emerged into amphibian life, however, the foliar portion was less and less thrown into contact with food, and there was more and more demand upon the grapple which was anchored in the soil. The foliage gradually developed into organs for absorbing gases and the root was forced to absorb the liquids which the plant needed. I do not mean to say that there is any genetic connection between the seaweeds and the higher plants, or that the roots of the two are homologous; but to simply state the fact that, in point of time, the holdfast root developed before the feeding root did, and that this change was plainly one of adaptation. Specialized forms of flowering plants which inhabit water still show a root system which is little more than an anchor, and the foliage actively absorbs water. The same environmental circumstances are thus seen to have developed organs of similar physiological character in widely remote times and in diverse lines of the plant evolution. "As the soil slowly became thicker and thicker," writes King in his book upon The Soil, "as its water-holding power increased, as the soluble plant food became more abundant, and as the winds and the rains covered at times with soil portions of the purely superficial and aerial early plants, the days of sunshine between passing showers, and the weeks of drought intervening between periods of rain, became the occasions for utilizing the moisture which the soil had held back from the sea. These conditions, coupled with the universal tendency of life to make the most of its surroundings, appear to have induced the evolution of absorbing elongations, which, by slow degrees and centuries of repetition, come to be the true roots of plants as we now know them." Some aquatic flowering plants are, as we have seen, still practically rootless, and they absorb the greater part of their food directly by the foliar parts; but the larger number of the higher plants absorb their mineral food by means of what has come to be a subterranean feeding organ and the foliar parts have developed into gas-absorbing organs and they take in water only when forced to do so under stress of circumstances.

But as a mere feeding organ the root requires no fibrous structure. It is still a holdfast or grapple and its mechanical tissue has developed enormously, along with that of the stem, in order to preserve the plant against the strain of the moving elements and to maintain its erectness in aerial life. When this self-poised epoch arrives, the vegetable world begins its definite and steady ascent in centrogenic form. While the animal creation leaves its centrogenic arrangement early in its own time scale, the plant creation assumes such arrangement at a comparatively late epoch in its time scale.

Perhaps the best illustration which I can bring you of the origin of the unlike by means of environmental conditions and the survival of some of this unlikeness in the battle for life, is the development of the winter quiescence of plants. What means all this bursting verdure of the liquid April days? Why this annually returning miracle of the sudden expansion of the leaf and flower from the lifeless twigs? Were plants always so? Were they designed to pass so much of their existence in the quiescent and passive condition? No. The first plants had no well-defined cycles, and they were born to live, not to die. There were probably no alternations of seasons in the primordial world. Day alternated with night, but month succeeded month in almost unbroken sameness age after age. As late as the Carboniferous time, according to Dana, the globe "was nowhere colder than the modern temperate zone, or below a mean temperature of 60° F." The earth had become wonderfully diverse by the close of the Cretaceous time, and the cycads and their kin retreated from the poles. Plants grew the year round; and as physical conditions became diverse and the conflict of existence increased, the older and the weaker died. So a limit to duration, that is, death, became impressed upon the individuals of the creation; for death, as seen by the evolutionist, is not an original property of life matter, but is an acquired character, a result of the survival of the fittest. The earth was perhaps ages old, even after life began, before it ever saw a natural death; but without death all things must have finally come to a standstill. When it became possible to sweep away the old types, opportunity was left for new ones; and so the ascent must continue so long as physical conditions, which are not absolutely prohibitive of life, shall become unlike.

Species have acquired different degrees of longevity, the same as they have acquired different sizes and shapes and habits—by adaptation to their conditions of life. Annual plants comprise about half of the vegetable kingdom, and these are probably all specializations of comparatively late time. Probably the greater part of them were originally adaptations to shortening periods of growth; that is, to seasonal changes. The gardener, by forceful cultivation and by transferring plants toward the poles, is able to make annuals of perennials. Now, a true annual is a plant which normally ripens its seeds and dies before the coming of frost. Many of our garden plants are annuals only because they are killed by frost. They naturally have a longer season than our climate will admit, and some of them are true perennials in their native homes. These plants are, with us, plur-annuals, and among them are the tomato, red pepper, eggplant, potato, castor bean, cotton, lima bean, and many others. But there are some varieties of potatoes and other plants which have now developed into true annuals, normally completing their entire growth before the approach of frost. It is all the result of adaptation to climate, and essentially the same phenomenon is the development of the annual and biennial flora of the earth from the perennial. An interesting example of the effect of climate upon the seasonal duration of plants is the indeterminate or prolonged growth of plants in England as compared with the same plants in America. The cooler summer and very gradual approach of winter in England develop a late and indefinite maturity of the season's growth. When English plants are grown in America, they usually grow until killed by fall frosts; but after a few generations of plants, they acquire the quick and decisive habit of ripening which is so characteristic of our vegetation. I once made an extended test of onions from English and American seeds (Bull. 31, Mich. Agric. College), and was astonished to find that nearly all of the English varieties continued to grow until frost and failed "to bottom," while our domestic varieties ripened up in advance of freezing weather. This was true even of the Yellow Danvers and Red Wethersfield, varieties of American origin, and which could not have been grown very many years in England. Every horticulturist of much experience must have noticed similar unmistakable influences of climate upon the duration of plants.

A most interesting type of examples of the quick influence of climate upon plants—not only upon their duration, but upon habit and structural characters—is that associated with the growing of "stock seed" by seedsmen. Because of uncertainties of weather in the Eastern States, it is now the practice to grow seeds of onions, lima beans, and other plants in California or other warm regions; but the plants so readily acquire the habit of long-continuing growth as to be thereafter grown with difficulty in the Northeastern States. It is, therefore, necessary that the seedsman shall raise his stock seed every year in his own geographical region, and this seed is each year sent to California for the growing of the commercial seed crop. In other words, the seed of California-grown onions is sold only for the purpose of growing onion bulbs for market, and is not planted for the raising of a successive crop of seed. This results in growing only a single generation of the crop in the warm country. Onion seed from stock which has been grown in California for several years produces onions which do not "bottom" well, much as I found to be the case with the English onion seed.

But many plants, in geologic time, could not thus shorten up their life history to adjust themselves to the oncoming of the seasons. They ceased their labors with the approach of the cold or the dry, tucked up their tender tissues in buds and resigned themselves to the elements. If a man could have stood among those giant mosses and fern forests of the reeking Carboniferous time, and could have known of the refrigeration which the earth was to undergo, he would have exclaimed that all living things must utterly perish. Consider the effects of a frost in May. See its widespread devastation. Yet, six months hence the very same trees, which are now so blackened, will defy any degree of cold. And then, to make good the loss of time, these plants start into activity relatively much earlier in spring than the same species do in frostless climates. This very day, when frosts are not yet passed, our own New York hillsides are greener with surface vegetation than the lands of the Gulf States are, which have been frostless for two months and more. The frogs and turtles, the insects, the bears and foxes, all adjust themselves to a climate which seems to be absolutely prohibitive of life, and some animals may actually freeze during their hibernation, and yet these April days see them again in heyday of life and spirits! What a wonderful transformation is all this! This enforced period of quiesence is so impressed upon the organization that the habit becomes hereditary in plants, and the gardener says that his begonias and geraniums and callas must have a "rest," or they will not thrive. But in time he can so far break this habit in most plants as to force them into activity for the entire year. These budding days of April, therefore, are the songs of release from the bondage of winter which has come on as the earth has grown aged and cold.

I must bring still one more illustration of the survival of the unlike, out of the abundance of examples which might be cited. It is the fact that, as a rule, new types are variable and old types are inflexible. The student of fossil plants will recall the fact that the liriodendrons, ginkgos, sequoias, sassafrasses, and other types came into existence with many species and are now going out of existence with one or two species. Williams has considered this feature, for extinct animal forms, at some length in his new Geological Biology. "Many species," he writes, "which by their abundance and good preservation in fossil state give us sufficient evidence in the case, exhibit greater plasticity in their characters at the early stage than in later stages of their history. A minute tracing of lines of succession of species shows greater plasticity at the beginning of the series than later, and this is expressed, in the systematic description and tabulation of the facts, by an increase in the number of the species." "When species are studied historically, the law appears evident that the characters of specific value * * * present a greater degree of range of variability at an early stage in the life period of the genus than in the later stages of that period." So marked is this incoming of new types in many cases that some students have supposed that actual special creation of species has occurred at these epochs. It should be said that there is apt to be a fallacy in observation in these instances, because the records which are, to our vision, simultaneous in the rocks may have extended over ages of time; but it is nevertheless true that some important groups seem to have come in somewhat quickly with many or several species and to have passed out with exceeding slowness.

To my mind, all this is but the normal result of the divergence of character, or the survival of the unlike. A new type finds places of least conflict, it spreads rapidly and widely, and thereby varies immensely. It is a generalized type, and therefore adapts itself at once to many and changing conditions. A virile plant is introduced into a country in which the same or similar plants are unknown, and immediately it finds its opportunity and becomes a weed, by which we mean that it spreads and thrives everywhere. Darwin and Gray long ago elucidated this fact. The trilobites, spirifers, conifers, ginkgos, were weed types of their time, the same as the composites are to-day. They were stronger than their contemporaries, the same as our own weeds are stronger than the cultivated plants with which they grow. After a time the new types outran their opportunity, the remorseless struggle for existence tightened in upon them, the intermediate unlikenesses had been blotted out, and finally only one or two types remained, struggling on through the ages, but doomed to perish with the continuing changes of the earth. They became specialized and inelastic; and the highly specialized is necessarily doomed to extinction. Such remnants of a vanquished host remain to us in our single liriodendron, the single ginkgo, and sassafras, and the depleted ranks of the conifers.

My attention was first called to this line of thought by contemplating upon the fact that cultivated plants differ widely in variability, and I was struck by the fact that many of our most inextricably variable groups—as the cucurbits, maize, citrus, and the great tribes of composites—are still unknown in a fossil state, presumably because of their recent origin. Many other variable genera, to be sure, are well represented in fossil species, as roses (although these are as late as the Eocene), pyrus, prunus, and musa; but absolute age is not so significant as the comparative age of the type, for types which originated very far back may be yet in the comparative youth of their development. The summary conclusions of a discussion of this subject were presented to the American Association for the Advancement of Science two years ago.[1] A modification of these points, as I now understand them, would run something as follows:

1. There is a wide difference in variability in cultivated plants. Some species vary enormously, and others very little.
2. This variability is not correlated with age of cultivation, degree of cultivation, or geographical distribution.
3. Variability of cultivated plants must be largely influenced and directed, therefore, by some antecedent causes.
4. The chief antecedent factor in directing this variability is probably the age of the type. New types in geologic time, are polymorphous; old types are monomorphous and are tending toward extinction. The most flexible types of cultivated plants are such as have probably not yet passed their zenith, as the cucurbits, composites, begonias, and the

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[1] Proc. A. A. A. S. 1894, 255; Botanical Gazette, XIX, 381.

like. The varieties of cereals, which are old types, are so much alike that expert knowledge is needed to distinguish them.

5. New types are more variable and flexible because less perfectly molded into and adjusted to the circumstances of life than the old types are. They have not yet reached the limits of their dissemination and variation. They are generalized forms.

The reader will please observe that I have here regarded the origin and survival of the unlike in the plant creation in the sense of a plastic material which is acted upon by every external stimulus, and which must necessarily vary from the very force of its acquired power of growth, and the unlikenesses are preserved because they are unlike. I have no sympathy with the too prevalent idea that all the attributes of plants are direct adaptations, or that they are developed as mere protections from environment and associates. There is a type of popular writings which attempts to evolve many of the forms of plants as a mere protection from assumed enemies. Perhaps the plant features which have been most abused in this manner are the spines, prickles, and the like, and the presence of acrid or poisonous qualities. As a sample of this type of writing, I will make an extract from Massee's Plant World:

> "Amongst the most prominent and general modes of protection of vegetative parts against the attacks of living enemies may be mentioned prickles, as in roses and brambles, which may either be straight, and thus prevent the nibblings of animals, or, in more advanced species, curved, thus enabling the weak stem to climb and carry its leaves out of harm's way. Spines that are sharp-pointed abortive branches, serving the same purpose as prickles, as in the common sloe or blackthorn (*Prunus spinosa*). Rigid hairs on leaves and stem, as in the borage (*Borago officinalis*) and comfrey (*Symphytum officinale*). Stinging hairs, as in the common nettles (*Urtica dioica* and *U. urens*). In these cases the stinging hairs are mixed on the leaves and stem with ordinary rigid hairs, of which they are higher developments, distinguished by the lower or basal swollen portion of the hair containing an irritating liquid that is ejected when the tip of the hair is broken off. Bitter taste, often accompanied by a strong scent, as in wormwood (*Artemisia vulgaris*), chamomile (*Anthemis nobilis*), and the leaves and fruit of the walnut (*Juglans regia*). Poisonous alkaloids, as in the species of *Strychnos*, which contain two very poisonous alkaloids, strychnine and brucine, in the root and the seeds; decoctions of species of *Strychnos* are used by the Javanese and the natives of South America to poison their arrows. Some of the species, as *Strychnos nux vomica*, are valuable medicines, depending on the strychnine they contain, which acts as a powerful excitant of the spinal cord and nerves; thus the most effective protective arrangements evolved by plants can be turned to account, and consequently lead to the destruction of the individuals they were designed to protect. Our common arum (*Arum maculatum*), popularly known as 'Lords and Ladies,' has an intensely acrid substance present in the leaves, which effectually protects it from the attacks of mammals and caterpillars, but not from the attacks of parasitic fungi, which appear to be indifferent to all protective contrivances exhibited by plants, nearly every plant supporting one or more of these minute pests, the effects of which will be realized by mentioning the potato disease, 'rust' and 'smut' in the various cereals, and the hop disease, all due to parasitic fungi."

Now, this is merely a gratuitous and ad captandum species of argument, one which is designed to please the fancy and to satisfy those superficial spirits who are still determined to read the element of design into organic nature. It does not account for the facts. These particular attributes of plants are specialized features, and it is always unsafe to generalize upon specializations. Each and every one of such specialized features must be investigated for itself. Probably the greater number of spinous processes will be found to be the residua following the contraction of the plant body; others are no doubt mere correlatives of the evolution of other attributes; and some may be the eruptions of the growth force; and the acrid and poisonous properties are quite as likely to be wholly secondary and useless features. The attempt to find a definite immediate use and office for every attribute in the creation is superficial and pernicious. There are many attributes of organisms which are not only useless, but positively dangerous to the possessor, and they can be understood only as one studies them in connection with the long and eventful history of the line of ascent.

The thought which I want to leave with you, therefore, is that unlikenesses are the greatest facts in the organic creation. These unlikenesses in plants are (1) the expressions of the ever-changing environmental conditions in which plants grow, and of the incidental stimuli to which they are exposed; (2) the result of the force of mere growth; (3) the outcome of sexual mixing. They survive because they are unlike, and thereby enter fields of least competition. The possibility of the entire tragic evolution lay in the plasticity of the original life-plasma. The plastic creation has grown into its own needs day by day and age by age, and it is now just what it has been obliged to be. It could have been nothing else.

THE LAW WHICH UNDERLIES PROTECTIVE COLORATION.[1]

By Abbott H. Thayer.

This article is intended to set forth a beautiful law of nature which, so far as I can discover, has never been pointed out in print. It is the law of gradation in the coloring of animals, and is responsible for most of the phenomena of protective coloration except those properly called mimicry.

Naturalists have long recognized the fact that the coloring of many animals makes them difficult to distinguish, and have called the whole phenomenon protective coloration, little guessing how wonderful a fact lay hidden under the name.

Mimicry makes an animal appear to be some other thing, whereas this newly discovered law makes him cease to appear to exist at all. The following are some examples of true mimicry. The screech owl, when startled, makes himself tall and slim, and with eyes shut to a narrow line simulates a dead stub of the tree on which he sits. Certain herons stretch their necks straight upward, and with head and green beak pointed at the zenith pass themselves off for blades of sedge grass. Certain harmless snakes spread their heads out flat, in imitation of their poisonous cousins, and rattle with their tails in the leaves. Many butterflies have stone or bark colored under sides to their wings, which make them look like a bit of bark or lichen when they sit still on a stone or tree trunk with wings shut over their backs.

The newly discovered law may be stated thus: Animals are painted by nature darkest on those parts which tend to be most lighted by the sky's light, and vice versa.

The accompanying diagram illustrates this statement. Animals are colored by nature as in A, the sky lights them as in B, and the two effects cancel each other as in C. The result is that their gradation of light and shade, by which opaque solid objects manifest themselves to the eye, is effaced at every point, the cancellation being as complete at one point as another, as in C of the diagram, and the spectator seems to see right through the space really occupied by an opaque animal.

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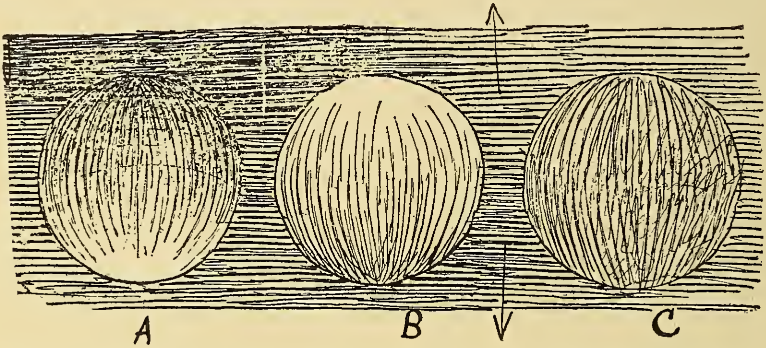
[1] Printed in The Auk, Vol. XIII, April and October, 1896.

Figure 1, of a ruffed grouse, shows this arrangement of color and light. This bird belongs to the class in which the arrangement is found in its simplest form, the color making a complete gradation from brown above to silvery white beneath, and conforming to every slightest modeling. For instance, it grows light under the shelving eyebrow and darker again on the projecting cheek.

When he stands alive on the ground, as in figure 2, his obliteration by the effect of the top light is obvious.

Writers say "he is so nearly like the color of his surroundings that you can not see him." Figure 3 is to show that they ascribe the concealment to the wrong cause. I merely took the bird shown in figure 2 and accurately tinted his under parts with brown to match his back, and in less degree tinted his sides, till I had reduced him to uniformity of color all over; but I did not, of course, change his upper surfaces at all. In short, I extended his "protective" color all over him.



Now observe the effect on replacing him in a life-like position. He is completely unmasked. The reader has but to compare the distance at which he can distinguish a bird in No. 2 and in No. 3, respectively, to see whether simple "protective coloration," as ordinarily defined, is the true cause of this concealment, or whether this compound gradation of color and light is the true cause.

Figures 4 and 5 show that his colors are powerless to conceal him in any position except the upright one which he holds when alive, and figures 6 and 7 do the same for the woodcock.

In figures 5 and 6, notwithstanding the fact that we have even the strongest "protective" colors toward us, the bird is by no means concealed.

The woodcock series corresponds to that of the ruffed grouse. Figure 8 shows a female on her nest, very difficult to find. In figure 9 the bird has been treated exactly as I treated the ruffed grouse in figure 3. Observe that she is essentially more conspicuous, though not a feather of her upper parts has been artificially painted. The reason of her visibility is that I have artificially extended her top colors down her sides, thereby destroying her counter gradation and forcing her solidity to manifest itself.

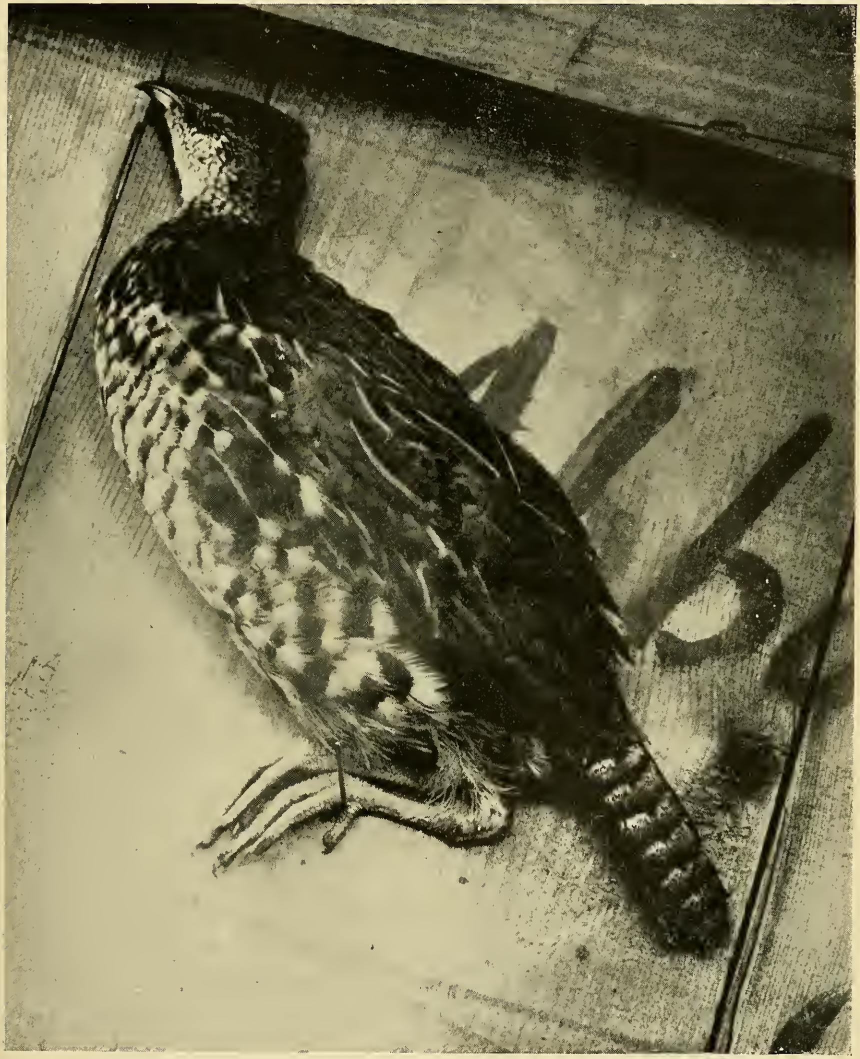


FIG. 1.—SIDE VIEW OF DEAD GROUSE TO SHOW COLOR GRADATION.

The reader, I think, must try these experiments for himself before he can believe that in figures 3 and 9 I tinted the under surfaces exactly as dark as the upper, and no darker. But I beg him to look at any horizontal branch in the woods which is either on the level of his eye or below it. He will see that, although it has exactly the color of its surroundings, it is not in the least concealed, because, being of uniform color above and below, like the birds after I had painted their under sides, it wears that universal attribute of a solid, namely, a gradation of shading from its light side to its dark side.

I leave to the reader the pleasure of discovering for himself that this principle of gradation in color is almost universal in the animal kingdom. In certain classes of birds and of flying insects, however, the principle gives place, more or less, to the device pointed out by Bates, namely, the employment of strong arbitrary patterns of color which tend to conceal the wearer by destroying his apparent continuity of surface. This makes, for instance, the mallard's dark-green head tend to detach itself from his body and to join the dark green of the shady sedge, or the ruby of the humming bird to desert him and to appear to belong to the glistening flower which he is searching. Yet many other cases of color applied apparently at random conform essentially to the law stated above. The dark patches are on top, the light ones beneath.[1] The dark breast mark, so widely used by nature on birds, usually has the effect of putting out a conspicuous and shining rotundity of some bright or light color, as in the meadow lark and the flicker, because it comes just where the breast, in its usual position, rounds upward and faces the sky. The dark collars of the males of most species of duck are absolute counter shading to the light from the sky, when the birds sit in their characteristic positions. For most female ducks nature uses the complete gradation, like that of grouse and sandpipers. Ground birds in general, such as grouse, sandpipers, and sparrows, are usually clothed throughout in colors graded according to this principle. But the males of many species of pheasant are notable exceptions to this last statement.

Now there is still one more very beautiful phenomenon to record. If the animal itself is obliterated by this mechanism of nature, for what useful purpose beyond considerations of sexual selections do his markings exist, since they are not obliterated? The answer is that the markings on the animal become a picture of such background as one might see if the animal were transparent. They help the animal to coalesce, in appearance, with the background which is visible when the observer looks past him. In many birds, for instance, those colors which would be seen by an enemy looking down upon them are laid on by nature in coarser and more blotchy patterns than are the colors on

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[1] I have proved, by experiments with painted decoys, that even brilliant top colors, however strongly contrasted to surroundings, scarcely tend to betray the wearer if his ensemble be a gradation from dark above to light below.

their sides, so that when you look down on them you see that their backs match the mottled ground about them; whereas, when you assume a lower point of view nearer their level, and see more and more of their sides, you find them painted to match the more intricate designs of the vegetation which is a little farther off, and which, from this new standpoint of the observer, now forms the background. In this latter position the head of the animal, being the highest part of its body, is seen against the most distant part of the background, whose details are still more reduced by perspective. To correspond with this reduction of strength in the more distant background, the details on the sides of the animal's head are likewise reduced in their emphasis, and like the more distant details are smaller in pattern.

It is a most significant fact that throughout the animal kingdom the highest development of the arrangement of color and light described in this article, and the highest development of the habit of standing or crouching motionless in full daylight to avoid discovery, seem to coincide very closely. For instance, gallinaceous birds, most waders, and the cat tribe have both the color arrangement and the standing or the crouching habit highly developed. Contrasted with these, for example, are the skunks and the bears. Neither of these quadrupeds has the gradation of color nor the standing or crouching habit. They are both nocturnal, and therefore do not need either gradation or crouching for concealment.

It is plain, then, that while nature undeniably completes the concealment of animals by pitching their whole color gradation in a key to match their environment, the real magic lies in the gradation itself from darkest above to lightest below, wherever this gradation is found. This is why it is so hard to see the partridge in the tree, the sandpiper on the mud, or the tiger crouching in the jungle.

FURTHER REMARKS ON THE LAW WHICH UNDERLIES PROTECTIVE COLORATION.

Since writing my article on protective coloration in the April Auk I have alighted on the means of still more complete ocular demonstration of the law of protective coloration.

I made some wooden eggs about the size of a woodcock's body, and provided them with wire legs to poise them 6 inches above the ground.

Most of these I colored in imitation of the color gradation of a grouse or hare—earth color above, to pure white beneath—while to two others I gave a coat of earth color all over, above and below; then set the whole like a flock of "shore birds" on the bare ground in a city lot.[1]

I then summoned a naturalist and let him begin at 40 or 50 yards to look for them. He saw immediately the two monochrome ones, but

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[1] To give the gradation its complete effect, the painting of the wooden eggs should be done after they are placed on the ground and of course by an artist.

PLATE II.

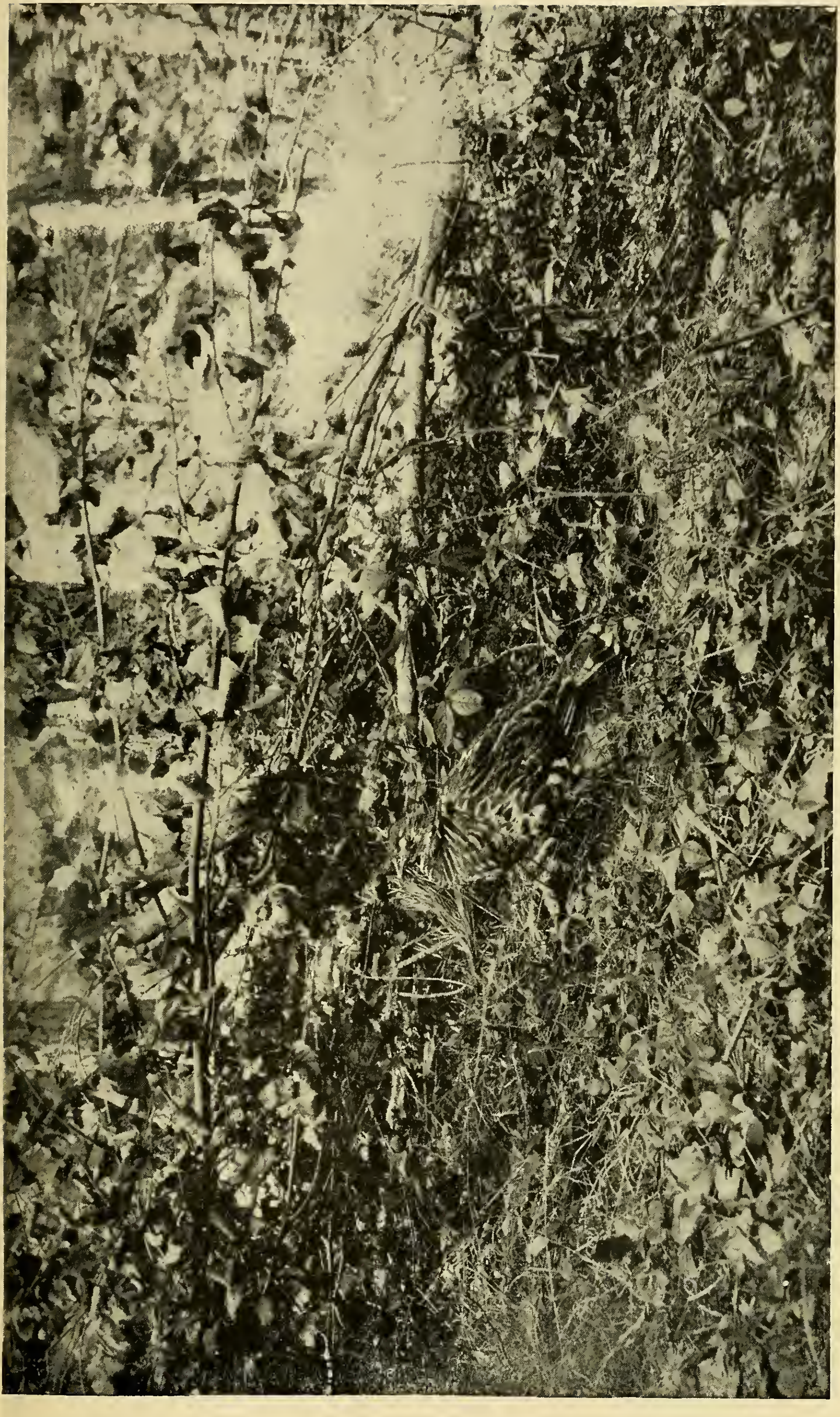

FIG. 2.—GROUSE POSED ON THE GROUND AS IN LIFE.

although told exactly where to look, failed to find any of the others until within 6 or 7 yards, and even then only by knowing exactly where to look.

I had also painted bright blue and red spots as big as a silver quarter of a dollar on the brown back of one of the graded eggs. These spots the naturalist saw, when we had come pretty near, though they only passed for details of the ground beyond the egg.

It was to this latter experiment that I alluded in a footnote when I said that brilliant top colors scarcely tend to interfere with the gradation's power. This statement does not apply, however, to creatures in which, as in a bluejay, the bright color so predominates as to form a silhouette shaped like the creature, but only when the bright pattern goes, as it were, its own way, not accompanying the animal's form.

Yet, even in the jay's case, his gradation down to white under throat and belly diminishes so greatly his conspicuousness in the dim forest shade that he may be suspected of great indebtedness to this arrangement of color as he skulks among the leaves. He must often be much helped, also, by the fact that whenever his gradation works its charm and denies his substantiality his blue is likely, at least, to appear to belong to whatever surface, far or near, forms his background for the beholder's eye at the moment; as, for instance, a bit of blue distance seen through the leaves. And often, when he is not concealed to this degree, his ghostly appearance still tends to cause the beholder to think him farther off than he is, which may be sometimes equivalent to concealment. The reader should compare a graded blue egg with one blue all over, both seen in deep woods. Let me urge the reader to understand these color phenomena, which are the open door into a new world of most charming study of special cases of protective coloration hitherto misunderstood.

One must remember that by far the greater part of the objects he espies as he walks are first caught sight of out of the side of his eye; and it is this faint seeing against which all this faint appearing is so potent in countless cases where the animal could not elude the direct eye. In my former article I omitted to emphasize the device of nature by which she accomplishes, in the only possible way, the bringing the top, sides, throat, and belly of an animal to the exact color of the surrounding earth, as well as to the same degree of darkness.

The animal's top is brown like the ground about him, and from this brown his color grades steadily colder till it becomes cold white on his under surfaces. The latter, being in shadow and bathed in a yellow reflection from the earth, has the exact color as well as degree of darkness of his top; since, obviously, earth-brown bathed in sky light equals sky light (color of the animal's belly) bathed in earth-yellow and shadow, i. e., brown.

This grading to white under surfaces is precisely what would result if daylight tended to brown animals' coats and its lack to bleach them.

And, from this, one might fancy the whole phenomenon to be the result of such browning and bleaching. But to those who believe in natural selection it must be obvious that the gradation's protecting power proves it a result of such selection. As to a bleaching and browning theory, many facts suggest that light does not tend to darken the coats of animals. Notice, for instance, the pale inhabitants of treeless regions, such as sandy beaches, etc., compared with wood dwellers. But this discussion is outside my present purpose.

As an epigrammatic lash to my entire thesis on protective coloration, it is important to say that no other conceivable arrangement of light and dark colors could effect the intrinsic unsubstantiality of appearance guaranteed by the gradation therein set forth.

Fig. 3.—Grouse Posed as in Fig. 2, but with Color Gradation Painted Out.

FIG. 4.—GROUSE ON SIDE, EXPOSING BREAST.

FIG. 5.—GROUSE ON SIDE, EXPOSING BACK.

FIG. 6.—WOODCOCK ON SIDE, EXPOSING BACK.

FIG. 7.—WOODCOCK ON SIDE, EXPOSING BREAST.

FIG. 8.—WOODCOCK ON NEST.

FIG. 9.—WOODCOCK ON NEST, COLOR GRADATION PAINTED OUT.

LIFE HISTORY STUDIES OF ANIMALS.[1]

By Prof. L. C. Miall, F. R. S.

It has long been my conviction that we study animals too much as dead things. We name them, arrange them according to our notions of their likeness or unlikeness, and record their distribution. Then perhaps we are satisfied, forgetting that we could do as much with minerals or remarkable boulders. Of late years we have attempted something more; we now teach every student of zoology to dissect animals and to attend to their development. This is, I believe, a solid and lasting improvement. We owe it largely to Huxley, though it is but a revival of the method of Döllinger, who may be judged, by the eminence of his pupils and by the direct testimony of Baer, to have been one of the very greatest of biological teachers. But the animals set before the young zoologist are all dead; it is much if they are not pickled as well. When he studies their development, he works chiefly or altogether upon continuous sections, embryos mounted in balsam, and wax models. He is rarely encouraged to observe live tadpoles or third-day chicks with beating hearts. As for what Gilbert White calls the life and conversation of animals—how they defend themselves, feed, and make love—this is commonly passed over as a matter of curious but not very important information; it is not reputed scientific, or at least not eminently scientific.

Why do we study animals at all? Some of us merely want to gain practical skill before attempting to master the structure of the human body; others hope to qualify themselves to answer the questions of geologists and farmers; a very few wish to satisfy their natural curiosity about the creatures which they find in the wood, the field, or the sea. But surely our chief reason for studying animals ought to be that we would know more of life, of the modes of growth of individuals and races, of the causes of decay and extinction, of the adaptation of living organisms to their surroundings. Some of us even aspire to know in outline the course of life upon the earth, and to learn, or, failing that, to conjecture, how life originated. Our own life is the thing of all

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[1] Address to the zoological section of the British Association for the Advancement of Science. Toronto, 1897, by Prof. L. C. Miall, F. R. S., president of the section. From Report of the British Association, 1897.

others which interests us most deeply, but everything interests us which throws even a faint and reflected light upon human life. Perhaps the professor of zoology is prudent in keeping so close as he does to the facts of structure, and in shunning the very attempt to interpret, but while he wins safety he loses his hold upon our attention. Morphology is very well; it may be exact; it may prevent or expose serious errors. But morphology is not an end in itself. Like the systems of zoology, or the records of distribution, it draws whatever interest it possesses from that life which creates organs and adaptations. To know more of life is an aim as nearly ultimate and self-explanatory as any purpose that man can entertain.

Can the study of life be made truly scientific? Is it not too vast, too inaccessible to human faculties? If we venture into this alluring field of inquiry, shall we gain results of permanent value, or shall we bring back nothing better than unverified speculations and curious but unrelated facts?

The scientific career of Charles Darwin is, I think, a sufficient answer to such doubts. I do not lay it down as an article of the scientific faith that Darwin's theories are to be taken as true; we shall refute any or all of them as soon as we know how; but it is a great thing that he raised so many questions which were well worth raising. He set all scientific minds fermenting, and not only zoology and botany, but paleontology, history, and even philology bear some mark of his activity. Whether his main conclusions are in the end received, modified, or rejected, the effect of his work can not be undone. Darwin was a bit of a sportsman and a good deal of a geologist; he was a fair anatomist and a working systematist; he keenly appreciated the value of exact knowledge of distribution. I hardly know of any aspect of natural history, except synonymy, of which he spoke with contempt. But he chiefly studied animals and plants as living beings. They were to him not so much objects to be stuck through with pins, or pickled, or dried, or labeled, as things to be watched in action. He studied their difficulties, and recorded their little triumphs of adaptation with an admiring smile. We owe as many discoveries to his sympathy with living nature as to his exactness or his candor, though these too were illustrious. It is not good to idolize even our greatest men, but we should try to profit by their example. I think that a young student, anxious to be useful but doubtful of his powers, may feel sure that he is not wasting his time if he is collecting or verifying facts which would have helped Darwin.

Zoologists may justify their favorite studies on the ground that to know the structure and activities of a variety of animals enlarges our sense of the possibilities of life. Surely it must be good for the student of human physiology, to take one specialist as an example of the rest, that he should know of many ways in which the same functions can be discharged. Let him learn that there are animals (starfishes) whose nervous system lies on the outside of the body, and that in other animals it is generally to be found there during some stage of development; that there are animals whose circulation reverses its direction at frequent intervals either throughout life (tunicata) or at a particular crisis (insects at the time of pupation); that there are animals with eyes on the back (*Oncidium*, scorpion), on the shell (some chitonidæ), on limbs or limb-like appendages, in the brain cavity, or on the edge of a protective fold of skin; that there are not only eyes of many kinds with lenses, but eyes on the principle of the pin-hole camera without lens at all (*Nautilus*) and of every lower grade down to mere pigment spots; that auditory organs may be borne upon the legs (insects) or the tail (*Mysis*); that they may be deeply sunk in the body, and yet have no inlet for the vibrations of the sonorous medium (many aquatic animals). It is well that he should know of animals with two tails (*Cercaria* of gasterostomum) or with two bodies permanently united (*Diplozoon*); of animals developed within a larva which lives for a considerable time after the adult has detached itself (some starfishes and nemertines); of animals which lay two (*Daphnia*) or three kinds of eggs (rotifera); of eggs which regularly produce two (*Lumbricus trapezoides*) or even eight embryos apiece (*Praopus*[1]); of males which live parasitically upon the female (cirripedes), or even undergo their transformations, as many as eighteen at a time, in her gullet (*Bonellia*); of male animals which are mere bags of sperm cells (some rotifera, some *Ixodes*, parasitic copepods), and of female animals which are mere bags of eggs (*Sacculina*, *Entoconcha*). The more the naturalist knows of such strange deviations from the familiar course of things the better will he be prepared to reason about what he sees, and the safer will he be against the perversions of hasty conjecture.

If a wide knowledge of animals is a gain to physiology and every other branch of biology, what opportunities are lost by our ignorance of the early stages of so many animals! They are often as unlike to the adult in structure and function as if they belonged to different genera, or even to different families. Zoologists have made the wildest mistakes in classifying larvæ whose subsequent history was at the time unknown. The naturalist who devotes himself to life histories shares the advantage of the naturalist who explores a new continent. A wealth of new forms is opened out before him. Though Swammerdam, Réaumur, De Geer, Vaughan Thompson, Johannes Müller, and a crowd of less famous naturalists have gone before us, so much remains to be done that no zealous inquirer can fail to discover plenty of untouched subjects in any wood, thicket, brook, or sea.

Whoever may attempt this kind of work will find many difficulties and many aids. He will, of course, find abundant exercise for all the anatomy and physiology that he can command. He will need the

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[1] Hermann von Jhering, Sitz. Berl. Akad., 1885; Biol. Centralbl., Bd. VI, p. 532–539 (1886).

systems of descriptive zoology, and will often be glad of the help of professed systematists. The work can not be well done until it is exactly known what animal is being studied. For want of this knowledge, hardly attainable one hundred and fifty years ago, Réaumur sometimes tells us curious things which we can neither verify nor correct; at times we really do not know what animal he had before him. The student of life histories will find a use for physics and chemistry, if he is so lucky as to remember any. Skill in drawing is valuable, perhaps indispensable.

If by chance I should be addressing any young naturalist who thinks of attending to life histories, I would beg him to study his animals alive and under natural conditions. To pop everything into alcohol and make out the names at home is the method of the collectors, but life histories are not studied in this way. It is often indispensable to isolate an animal, and for this purpose a very small habitation is sometimes to be preferred. The teacup aquarium, for instance, is often better than the tank. But we must also watch an animal's behavior under altogether natural circumstances, and this is one among many reasons for choosing our subject from the animals which are locally common. Let us be slow to enter into controversies. After they have been hotly pursued for some time, it generally turns out that the disputants have been using words in different senses. Discussion is excellent, controversy usually barren. Yet not always; the Darwinian controversy was heated, and nevertheless eminently productive; all turns upon the temper of the men concerned, and the solidity of the question at issue. One more hint to young students. Perhaps no one ever carried through a serious bit of work without in some stage or other longing to drop it. There comes a time when the first impulse is spent, and difficulties appear which escaped notice at first. Then most men lose hope. That is the time to show that we are a little better than most men. I remember as a young man drawing much comfort from the advice of a colleague, now an eminent chemist, to whom I had explained my difficulties and fears. All that he said was, "Keep at it," and I found that nothing more was wanted.

I greatly believe in the value of association. It is good that two men should look at every doubtful structure and criticise every interpretation. It is often good that two talents should enter into partnership, such as a talent for description and a talent for drawing. It is often good that an experienced investigator should choose the subject and direct the course of work, and that he should be helped by a junior, who can work, but can not guide. It seems to me that friendly criticism before publication is often a means of preventing avoidable mistakes. I am sorry that there should be any kind of prejudice against cooperation, or that it should be taken to be a sign of weakness. There are, I believe, very few men who are so strong as not to be the better for help. One difficulty would be removed if known authors were more generous in acknowledging the help of their assistants. They ought not to be slow to admit a real helper to such honor as there may be in joint authorship.

Among the most important helps to the student of life histories must be mentioned the zoological stations now maintained by most of the great nations. The parent of all these, the great zoological station at Naples, celebrated its twenty-fifth anniversary last April, so that the whole movement belongs to our own generation. How would Spallanzani and Vaughan Thompson and Johannes Müller have rejoiced to see such facilities for the close investigation of the animal life of the sea! The English-speaking nations have taken their fair share of the splendid work done at Naples, and it is pleasant to remember that Darwin subscribed to the first fund, while the British Association, the University of Cambridge, and the Smithsonian Institution have maintained their own tables at the station.[1] The material support thus given is small when compared with the subsidies of the German Government, and not worth mention beside the heroic sacrifices of the director, Dr. Anton Dohrn, but as proofs of lively interest in a purely scientific enterprise they have their value. Marine stations have now multiplied to such a point that a bare enumeration of them would be tedious. Fresh-water biological stations are also growing in number. Forel set an excellent example by his investigation of the physical and biological phenomena of the Lake of Geneva. Dr. Anton Fritsch, of Prag, followed with his movable station. There is a well equipped station at Plön among the lakes of Holstein, and a small one on the Müggelsee near Berlin. The active station of Illinois is known to me only by the excellent publications which it has begun to issue. France, Switzerland, Sweden, and Finland all have their fresh-water biological stations, and I hope that England will not long remain indifferent to so promising a sphere of investigation.

Biological work may answer many useful purposes. It may be helpful to industry and public health. Of late years the entomologist has risen into sudden importance by the vigorous steps taken to discourage injurious insects. I have even known a zoological expert summoned before a court of law in order to say whether or not a swordfish can sink a ship. I would not on any account run down the practical applications of biology, but I believe that the first duty of the biologist is to make science, and that science is made by putting and answering questions. We are too easily drawn off from this, which is our main business, by self-imposed occupations, of which we can often say nothing better than that they do no harm except to the man who undertakes them. There are, for example, a good many lists of species which are compiled without any clear scientific object. We have a better prospect of working to good purpose when we try to answer definite questions. I propose to spend what time remains in putting and

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[1] To this list may now be added the University of Oxford.

answering as well as I can a few of the questions which occur to any naturalist who occupies himself with life histories. Even a partial answer, even a mistaken answer, is better than the blank indifference of the collector who records and records, but never thinks about his facts.

The first question that I will put is this: Why do some animals undergo transformation while others do not? It has long been noticed[1] that as a rule fresh-water and terrestrial animals do not go through transformation, while their marine allies do. Let us take half a dozen examples of each:

Fluviatile or terrestrial, without transformation:	Marine, with transformation:
Crayfish,	Crab,
Earthworm,	*Polygordius,*
Helix,	*Doris, Æolis,*
Cyclas,	Oyster,
Hydra, etc.	Most hydrozoa, etc.

We get a glimmer of light upon this characteristic difference when we remark that in fresh-water and terrestrial species the eggs are often larger than in the allied marine forms A large egg favors embryonic as opposed to larval development. An embryo which is formed within a large egg may feed long upon the food laid up for it and continue its development to a late stage before hatching. But if there is little or no yolk in the egg the embryo will turn out early to shift for itself. It will be born as a larva, provided with provisional organs suited to its small size and weakness. Large eggs are naturally fewer than small ones. Does the size depend on the number or the number on the size? To answer in a word, I believe that the size generally depends on the number and that the number is mainly determined by the risks to which the species are exposed. At least so many eggs will in general be produced as can maintain the numbers of the species in spite of losses, and there is some reason to believe that in fresh waters the risks are less than in the shallow seas or at the surface of the ocean.[2] In most parts of the world the fresh waters are of small size and much cut up. Every river basin forms a separate territory. Isolation, like every other kind of artificial restriction, discourages competition and impedes the spread of successful competitors. In the shallow seas or

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[1] Darwin, Origin of Species, Chap. XIII; Fritz Müller, Für Darwin, Chap. VII.

[2] Indications are given by the survival in fresh waters of declining groups, e. g., ganoid fishes, which, when dominant, maintained themselves in the sea, and by the not uncommon case of marine animals which enter rivers to spawn. I do not attempt to count among these indications the supposed geological antiquity of fluviatile as compared with marine animals. Some marine genera are extremely ancient (*Lingula, Nucula, Trigonia, Nautilus*); a perfectly fair comparison is almost impossible, and great persistence does not necessarily imply freedom from risks. In the mollusca, which afford a good opportunity of testing the effect of habitat upon the number of the eggs, marine species seem to produce more eggs as a rule than fluviatile, and these many more than terrestrial species.

at the surface of the ocean conquering forms have a free course; in lakes and rivers they are soon checked by physical barriers.

A large proportion of animals are armor clad and move about with some difficulty when they have attained their full size. The dispersal of the species is therefore in these cases effected by small and active larvæ. Marine animals (whether littoral or pelagic) commonly produce vast numbers of locomotive larvæ, which easily travel to a distance. Floating is easy and swimming not very difficult. A very slightly built and immature larva can move about by cilia or take advantage of currents, and a numerous brood may be dispersed far and wide while they are mere hollow sacs, without mouth, nerves, or sense organs. Afterwards they will settle down and begin to feed. In fresh waters armor is as common, for all that I know, as in the sea, but locomotive larvæ are rare.[1] There is no space for effective migration. Even a heavy-armored and slow-moving crustacean or pond snail can cross a river or lake, and to save days or hours is unimportant. In rivers, as Sollas has pointed out, free-swimming larvæ would be subject to a special risk—that of being swept out to sea. This circumstance may have been influential, but the diminished motive for migration is probably more important. At least an occasional transport to a new area is indispensable to most fresh-water organisms, and very unexpected modes of dispersal are sometimes employed, not regularly in each generation, but at long intervals, as opportunity offers.

Early migration by land is nearly always out of the question. Walking, and still more flying, are difficult exercises, which call for muscles of complex arrangement and a hard skeleton. A very small animal, turned out to shift for itself on land, would in most cases perish without a struggle. There might be just a chance for it if it could resist superficial drying and were small enough to be blown about by the wind (infusoria, rotifera, and certain minute crustacea) or if it were born in a wet pasture, like some parasitic worms.

We can define two policies between which a species can make its choice. It may produce a vast number of eggs, which will then be pretty sure to be small and ill furnished with yolk. The young will hatch out early, long before their development is complete, and must migrate at once in search of food. They will, especially if the adult is slow moving or sedentary, be furnished with simple and temporary organs of locomotion, and will generally be utterly unlike the parent. The majority will perish early, but one here and there will survive to carry on the race.

Or the parent may produce a few eggs at a time, stock them well with yolk, and perhaps watch over them, or even hatch them within

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[1] *Dreyssensia* and *Cordylophora* are examples of animals which seem to have quite recently become adapted to fresh-water life and have not yet lost their locomotive larvæ. Many instances could be quoted of marine forms which have become fluviatile. The converse is, I believe, comparatively rare.

her own body. The young will in such cases complete their development as embryos, and when hatched will resemble the parent in everything but size.

Which policy is adopted will largely depend upon the number of the family and the capital at command. There are animals which are like well-to-do people, who provide their children with food, clothes, schooling, and pocket money. Their fortunate offspring grow at ease and are not driven to premature exercise of their limbs or wits. Others are like starving families, which send the children, long before their growth is completed, to hawk matches or newspapers in the streets.

In biology we have no sooner laid down a principle than we begin to think of exceptions. The exceptions may be apparent only; they may, when fully understood, confirm instead of disturbing the general principle. But this rarely happens unless the principle is a sound one. Exceptio probat regulam; it is the exception which tests the rule, to give a new application to an old maxim.

Parasites form one group of exceptions to our rule. Whether they pass their free stages in air, water, or earth, whether their hosts are marine, fluviatile, or terrestrial, they are subject to strange transformations, which may be repeated several times in the same life history. The change from one host to another is often a crisis of difficulty. Many fail to accomplish it. Those which succeed do so by means of some highly peculiar organ or instinct, which may be dropped as quickly as it is assumed. The chances of failure often preponderate to such an extent that an enormous number of eggs must be liberated. Even a brief parasitism may produce a visible effect upon the life history. The young *Unio* or *Anodon* attaches itself for a short time to some fish or tadpole. To this temporary parasitism is due, as I suppose, the great number of eggs produced, and a degree of metamorphosis unusual in a fresh-water mollusk.

The cephalopoda, which are wholly marine, and the vertebrates, whatever their habitat, very rarely exhibit anything which can be called transformation. Some few cases of vertebrate transformation will be discussed later. Cephalopods and vertebrates are large, strong, quick-witted animals, able to move fast, and quite equal in many cases to the defense of themselves and their families. They often produce few young at a time and take care of them (there are many examples to the contrary among cephalopods and fishes). They are generally able to dispense with armor, which would have indirectly favored transformation.

Echinoderms, which are all marine, develop with metamorphosis. There is an interesting exception in the echinoderms with marsupial development, which develop directly and give an excellent illustration of the effect of parental care.

Insects, which as terrestrial animals should lay a few large eggs, and develop directly, furnish the most familiar and striking of all transformations. I have already discussed this case at greater length than is possible just now.[1] I have pointed out that the less specialized insect larvæ, e. g., those of orthoptera, make a close approach to some wingless adult insects, such as the thysanura, as well as to certain myriopods. Fritz Müller seems to me to be right in saying that the larvæ of nonmetamorphic insects come nearer than any winged insect to primitive tracheates. The transformation of the bee, moth, or blowfly is transacted after the stage in which the normal tracheate structure is attained, and I look upon it as a peculiar adult transformation, having little in common with the transformations of echinoderms, mollusks, or crustaceans.

In the same way I believe that some amphibia have acquired an adult transformation. Frogs and toads, having already as tadpoles attained the full development of the more primitive amphibia, change to lung-breathing, tailless, land-traversing animals, able to wander from the place of their birth to seek out mates from other families, and to lay eggs in new sites.

Medusæ furnish a third example of adult transformation which seems to find its explanation in the sedentary habit of the polyp, which probably nearly approaches the primitive adult stage. But here the case is further complicated, for the polyp still proceeds from a planula, which is eminently adapted for locomotion, though perhaps within a narrower range. We have two migratory stages in the life history. Each has its own advantages and disadvantages. The planula, from its small size, is less liable to be devoured, or stranded, or dashed to pieces, but it can not travel far; the medusa may cross wide seas, but it is easily captured and is often cast up upon a beach in countless multitudes.

Adult transformation may be recognized by its occurrence after the normal structure of the group has been acquired, and also by its special motive, which is egg laying and all that pertains to it; the special motive of larval transformation is dispersal for food.

The reproduction of the common eel has been a mystery ever since the days of Aristotle, though a small part of the story was made out even in ancient times. It was long ago ascertained that the eel, which seeks its food in rivers, descends to the sea in autumn or early winter, and that it never spawns, nor even becomes mature, in fresh waters. The eels which descend to the sea never return, but young eels or elvers come up from the sea in spring, millions at a time. The elvers have been seen to travel along the bank of a river in a continuous band or eel-rope, which has been known to glide upward for fifteen days together. It was of course concluded that spawning and early development took place in the sea during the interval between the autumn and spring migration, but no certain information came to hand till 1896. Meanwhile this gap in our knowledge was a perplexity, almost a

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[1] Nature, December 19, 1895.

reproach to zoologists. The partially-known migration of the eel could not be harmonized with the ordinary rule of migratory fishes. We tried to explain the passage of marine fishes into rivers at spawning time by the supposition (a true supposition, as I think) that the river is less crowded than the shallow seas, and therefore a region in which competition is less severe. The river is to some migratory fishes what the tundras of Siberia are to some migratory birds, places comparatively free from dangerous enemies, and therefore fit for the rearing of the helpless young. But the eel broke the rule and cast doubt upon the explanation. The salmon, sturgeon, and lamprey feed and grow in the sea and enter rivers to spawn. The eel feeds and grows in rivers but enters the sea to spawn. What possible explanation could meet cases thus diametrically opposite?

This was the state of matters when Grassi undertook to tell us that part of the history of the eel which is transacted in the sea. When it leaves the river it makes its way to very deep water and there undergoes a change. The eyes enlarge and become circular instead of elliptical; the pectoral fins and the border of the gill cover turn black; the reproductive organs, only to be discovered by microscopic search before this time, enlarge. The eels, thus altered in appearance and structure, lay their eggs in water of not less than 250 fathoms depth. The upper limit of the spawning ground is nearly three times as far from sea level as the 100-fathom line, which we arbitrarily quote as the point at which the deep sea begins. The eggs, which are large for a fish (2.7 millimeters diameter), float, but do not rise. The young which issue from them are quite unlike the eels of our rivers; they are tapelike, transparent, colorless, devoid of red blood, and armed with peculiar teeth. A number of different kinds of such fishes had been previously known to the naturalist as Leptocephali. Günther had conjectured that they were abnormal larvæ, incapable of further development. Grassi has, however, succeeded in proving that one of these Leptocephali (*L. brevirostris*) is simply a larval eel; others are larvæ of congers and various murænoid fishes. He has with infinite pains compared a number of Leptocephali, and coordinated their stages, making out some particularly important ones by the direct observation of live specimens.

You will not unnaturally ask how Grassi or anybody else can tell what goes on in the sea at a depth of over 250 fathoms. His inquiries were carried on at Messina, where the local circumstances are very fortunate. Strong currents now and then boil up in the narrow strait, sweeping to the surface eggs, larvæ, and a multitude of other objects, which at ordinary seasons lie undisturbed in the tranquil depths. Further information has been got by dredging, and also by opening the body of a sunfish (*Orthagoriscus mola*), which at certain times of the year is taken at the surface and is always found to contain a number of Leptocephali. When a *Leptocephalus* has completed its first stage of growth it ceases to feed, loses bulk, and develops pigment on the surface of the body. At the same time the larval teeth are cast and the larval skeleton is replaced. Then the fish begins to feed again, comes to the surface, enters the mouth of a river, and, if caught, is immediately recognized as an elver or young eel. It is now a year old and about 2 inches long.

This history suggests a question. Are the depths of the sea free from severe competition? The darkness, which must be nearly or altogether complete, excludes more than the bare possibility of vegetation. A scanty subsistence for animals is provided by the slowly decomposing remains of surface life. When the dredge is sunk so low, which does not often happen, it may bring up now and then a peculiar and specially modified inhabitant of the dark and silent abyss. There can not, we should think, be more than the feeblest competition where living things are so few and the mode of life so restricted. Going a step further, we might predict that deep-sea animals would lay few eggs at a time, and that these would develop directly—i. e., without transformation. The risk of general reasoning about the affairs of living things is so great that we shall hold our conjectures cheap unless they are confirmed by positive evidence. Happily, this can be supplied. The voyage of the *Challenger* has yielded proof that the number of species diminishes with increasing depth, and that below 300 fathoms living things are few indeed.[1] Dr. John Murray gives us the result of careful elaboration of all the facts now accessible, and tells us that the majority of the abyssal species develop directly.[2]

We seem, therefore, to have some ground for believing that the depths of the sea resemble the fresh waters in being comparatively free from enemies dangerous to larvæ. The eel finds a safe nursery in the depths, and visits them for the same reason that leads some other fishes to enter rivers. It may be that the depths of the sea are safer than rivers, in something like the same degree and for the same reasons that rivers are safer than shallow seas. But we must be careful not to go too fast. It may turn out that deep recesses in the shallower seas—holes of limited extent in the sea bottom—enjoy an immunity from dangerous enemies not shared by the great and continuous ocean floor.[3]

After this short review of the facts I come to the conclusion that the general rule which connects the presence or absence of transformation with habitat is well founded, but that it is apt to be modified and even reversed by highly special circumstances. The effect of habitat may, for instance, be overruled by parasitism, parental care, a high degree of organization, or even by a particular trick in egg laying. The direct action of the medium is probably of little consequence. Thus the difference between fresh and salt water is chiefly important because it prevents most species from passing suddenly from one to the other.

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[1] Challenger Reports. Summary of Scientific Results (1895), pages 1430–1436.

[2] Nature, March 25, 1897.

[3] I am aware that other things affect the interests of animals and indirectly determine their structure besides danger from living enemies. So complicated a subject can only be discussed in a short space if large omissions are tolerated.

But the abyssal and the fluviatile faunas have much in common, as also have the littoral and the pelagic faunas. Relative density and continuity of population seem to be of vital importance, and it is chiefly these that act upon the life history.

In zoology, as in history, biography, and many other studies, the most interesting part of the work is only to be enjoyed by those who look into the details. To learn merely from text-books is notoriously dull. The text-book has its uses, but, like other digests and abridgments, it can never inspire enthusiasm. It is the same with most lectures. Suppose that the subject is that well-worn topic, the alternation of generations. The name recalls to many of us some class room of our youth, the crudely colored pictures of unlikely animals which hung on the walls, and the dispirited class, trying to write down from the lecture the irreducible minimum which passes a candidate. The lecturer defines his terms and quotes his examples; we have *Salpa*, and *Aurelia*, and the fern, and as many more as time allows. How can he expect to interest anybody in a featureless narrative, which gives no fact with its natural circumstances, but mashes the whole into pemmican? What student goes away with the thought that it would be good and pleasant to add to the heap of known facts? The heap seems needlessly big already. And yet every item in that dull mass was once deeply interesting, moving all naturalists and many who were not naturalists to wonder and delight. The alternation of generations worked upon men's minds in its day like Swammerdam's discovery of the butterfly within the caterpillar, or Trembley's discovery of the budding *Hydra*, which, when cut in two made two new animals, or Bonnet's discovery that an *Aphis* could bring forth living young without having ever met another individual of its own species. All these wonders of nature have now been condensed into glue. But we can at any time rouse in the minds of our students some little of the old interest if we will only tell the tale as it was told for the first time.

Adalbert Chamisso, who was in his time court page, soldier, painter, traveler, poet, novelist, and botanist, was the son of a French nobleman. When he was 9 years old he and all the rest of the family were driven out of France by the French Revolution. Chamisso was educated anyhow, and tried many occupations before he settled down to botany and light literature. In 1815 he embarked with Eschscholtz on the Russian voyage round the world, commanded by Kotzebue. The two naturalists (for Chamisso is careful to associate Eschscholtz with himself, and even to give him priority) discovered a highly curious fact concerning the salpæ, gelatinous tunicates which swim at the surface of the sea, sometimes in countless numbers. There are two forms in the same species, which differ in anatomical structure, but especially in this, that one is solitary, the other composite, consisting of many animals united into a chain which may be yards long. Chamisso and Eschscholtz ascertained that the solitary form produces the chain form by internal budding, while the chain form is made up of hermaphrodite animals which reproduce by fertilized eggs.[1] There is thus, to use Chamisso's own words, "an alternation of generations. * * * It is as if a caterpillar brought forth a butterfly, and then the butterfly a caterpillar." Here the phrase "bring forth" is applied to two very different processes, viz, sexual reproduction and budding. Chamisso's phrase, "alternation of generations," is not exact. Huxley would substitute "alternation of generation with gemmation," and if for shortness we use the old term, it must be with this new meaning. Subsequent investigation, besides adding many anatomical details, has confirmed one interesting particular in Chamisso's account, viz, that the embryo of *Salpa* is nourished by a vascular placenta.[2] The same voyage yielded also the discovery of *Appendicularia*, a permanent tunicate tadpole, and the first tadpole found in any tunicate.

Some ten years after the publication of Chamisso's alternation of generations in *Salpa*, a second example was found in a common jelly-fish (*Aurelia*). Not a few hydrozoa had by this time been named, and shortly characterized. Some were polyps, resembling the *Hydra* of our ponds, but usually united into permanent colonies; others were medusæ, bell-shaped animals which swim free in the upper waters of the sea. It was already suspected that both polyps and medusæ had a common structural plan, and more than one naturalist had come very near to knowing that medusæ may be the sexual individuals of polyp colonies.

This was the state of matters when an undergraduate in theology of the University of Christiania, named Michael Sars, discovered and described two new polyps, to which he gave the names, now familiar to every zoologist, of *Scyphistoma* and *Strobila*. In the following year (1830) Sars settled at Kinn, near Bergen, as parish priest, and betook himself to the lifelong study of the animals of the Norwegian seas. He soon found out that his *Scyphistoma* was merely an earlier stage of his *Strobila*. *Scyphistoma* has a *Hydra*-like body less than half an inch long and drawn out into a great number of immensely long tentacles. It buds laterally like a *Hydra*, sending out stolons or runners, which bear new polyps, and separate before long, the polyps becoming independent animals. In the midst of the tentacles of the scyphistoma is a prominence which bears the mouth. This grows upward into a tall column, the strobila, which is supported below by the scyphistoma. When the strobila is well nourished it divides into transverse slices, which at length detach themselves and swim away.[3] These are the

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[1] Brooks maintains that the solitary *Salpa*, which is female, produces a chain of males by budding, and lays an egg in each. These eggs are fertilized while the chain is still immature, and develop into females (solitary Salpæ). The truth of this account must be determined by specialists.

[2] Cuvier had previously noted the fact.

[3] Leuckart (Zeits. f. wiss. Zool., Bd. III, p. 181) remarks that elongate animals tend to divide transversely or to bud axially, while broad animals tend to divide longitudinally or to bud laterally. The question has been raised more than once whether the division of the strobila is not really a case of budding. Leuckart shows that budding and fission can not be separated by any definition; they pass insensibly into one another. (Wagner's Handb. d. Physiol., art. "Zeugung.")

ephyræ, which had been found in the sea before Sars's time, and were then counted as a particular kind of adult medusæ. They are small, flat discs with eight lobes or arms, all notched at the extremity. A pile of ephyræ is produced by the transverse constriction and division of the strobila in a fashion which reminds us of the rapid production of the animals in a Noah's ark by the slicing of a piece of wood of suitable sectional figure. It was thus ascertained that the scyphistoma, strobila, and ephyra are successive stages of one animal, but for a time no one could say where the scyphistoma came from, nor what the ephyra turned to. At length Sars, aided by the anatomical researches of Ehrenberg and Siebold, was able to clear up the whole story. The ephyra is gradually converted by increase of size and change of form into an *Aurelia*, a common jelly-fish which swarms during the summer in European seas. The aurelia is of two sexes, and the eggs of the female give rise to ciliated embryos, which had been seen before Sars's time, but wrongly interpreted as parasites or diminutive males. These ciliated embryos, called planulæ, swim about for a time and then settle down as polyps (scyphistomata). There is thus a stage in which aurelia divides without any true reproductive process, and another stage in which it produces fertile eggs. There is alternation of generations in aurelia as well as in salpa, and Sars was glad to fortify by a fresh example the observations of Chamisso, on which doubts had been cast.

It was not long before the alternation of generations was recognized in hydromedusæ also, and then the ordinary hydrozoan colony was seen to consist of at least two kinds of polyps, one sexual, the other merely nutrient, both being formed by the budding of a single polyp. The sexual polyp, or medusa, either swims away or remains attached to the colony, producing at length fertilized eggs, which yield planulæ, and these in turn the polyps which found new colonies.

Those of us who are called upon to tell this story in our regular course of teaching should not forget to produce our scyphistoma, strobila, and ephyra. The interest is greatly enhanced it they are shown alive. It is not hard to maintain a flourishing marine aquarium, even in an inland town, and a scyphistoma may be kept alive in an aquarium for years. budding out its strobila every spring.

Alternation of generations when first announced was taken to be a thing mysterious and unique. Chamisso brought in the name, and explained that he meant by it a metamorphosis accomplished by successive generations, the form of the animal changing not in the course of an individual life, but from generation to generation (forma per generationes, nequaquam in prole seu individuo, mutata). Sars adopted Chamisso's name and definition. Steenstrup a little later collected and discussed all the examples which he could discover, throwing in a number which have had to be removed again as not fairly comparable with the life histories of *Salpa* and *Aurelia*. He emphasized the alternation of budding with egg production and the unlikeness in form of the asexual and sexual stages. Like Chamisso, he carefully distinguished between development with metamorphosis and alternation of generations. All three naturalists, Chamisso, Sars, and Steenstrup, laid stress on this point. In an insect, they would have said, there is development with metamorphosis. The same animal passes from larva to pupa, and from pupa to imago. In *Aurelia* or *Salpa*, however, the animal which lays eggs is not the animal which buds, but its progeny. The cycle of the life history includes two generations and many individuals.

This view has spread very widely, and if we were to judge by what is commonly taught I think that we should recognize this as the doctrine now prevalent. It is, however, in my opinion, far inferior as an explanation of the facts to that adopted by Leuckart, Carpenter, and Huxley, who regard the whole cycle, from egg to egg, as one life history. Huxley and Carpenter, differing in this from Leuckart, do not shrink from calling the whole product of the egg an animal, even though it consists of a multitude of creatures which move about and seek their food in complete independence of one another. Rather than ignore the unity of the life history of *Aurelio* or *Salpa*, they would adopt the most paradoxical language. This attitude was forced upon them by the comparative method. They refused to study Aurelia, for example, as an animal apart. It had its near and its remoter relatives. Among these is the fresh-water *Hydra*, which develops without transformation, buds off other hydras when food is plentiful, and at length becomes sexually mature. Budding is here a mere episode, which may be brought in or left out, according to circumstances. The same individual polyp which buds afterwards produces eggs. The life history of *Salpa* can not be traced with equal facility to a simple beginning, for it presents points of difficulty on which the learned differ. In the polychæt worms, however, we find a beautiful gradation leading up to alternation of generations. We begin with gradual addition of new segments and increasing specialization of the two ends of the body, the fore end becoming nonreproductive and the hinder end reproductive. Then we reach a stage (syllis) in which the reproductive half breaks off from the fore part and forms (after separation) a new head, while the fore part adds new segments behind. In *Autolytus* the new head forms before separation, and many worms may cohere for a time, forming a long chain with heads at intervals. In *Myrianida* the worms break up first and afterwards become sexually mature. We should gather from these cases that alternation of generations may arise by the introduction of a budding stage into a development with transformation. The polyp or worm buds while young and lays eggs at a later time. The separation of the two processes of reproduction often becomes complete, each being restricted to its own place in the life history. As a rule the worm or polyp will bud while its structure is uncomplicated by reproductive organs. It is easy to propagate some plants by cutting one of the leaves into sections and making every section root itself and grow into a new plant, but we can seldom do the same thing with a flower. There may, therefore, be a distinct advantage to particular animals and plants in dividing the life history into two stages—an earlier budding and a later egg-laying stage.

The advantage to be drawn from budding is easily seen in those animals which find it hard to gain access to a favorable site. Thus a *Tænia*[1] is very lucky when it establishes itself in the intestine. Once there, it goes on budding indefinitely. It is harder to trace the advantage in the case of many polyps, though some (*Cunina*, etc.) admit of the same explanation as *Tænia*. There are yet other cases (some worms, salpæ, etc.) in which our ignorance of the conditions of life renders a satisfactory explanation impossible at present.

The budded forms often differ in structure from the budding forms which produce them, and many writers and teachers make this difference part of the definition of alternation of generations. I think that Leuckart has suggested a probable explanation in his essay of 1851,[2] which is still thoroughly profitable reading. He attributes the peculiarities of the larva mainly to the circumstance that it is turned out at an early age to shift for itself. In the budded forms there is no such necessity. The parent has established itself on a good site which commands a sufficiency of food. Until it has done this, it does not bud at all. The young which it produces asexually need not disperse in infancy, at least until crowding sets in. The tradesman who has founded a business puts his elder boys into the shop; perhaps the younger ones may be obliged to try their luck in a distant town. The budded forms, reared at the cost of the parent, may therefore omit the early larval stages at least, and go on at once to a later or even to the final stage. Thus the head of *Tænia*, when it has fixed itself in the intestine, produces sexual segments; the redia of *Distomum* produces cercariæ or more rediæ, omitting the locomotive embryo; the scyphistoma produces ephyræ. The saving of time must often be great, and the days saved are days of harvest. Think how much a tree would lose if in the height of summer it were unable to bud, and could only propagate by seeds. If the budded forms are sexual, while the budding forms are not, there is an obvious explanation of the difference in form. Even where there is no such fundamental difference in function, the circumstances of early life are very different, and may well produce an unlikeness upon which natural selection may found a division of labor.

No one who tries to trace origins can rest satisfied with Steenstrup's

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[1] This case is quoted by Leuckart.

[2] Ueber Metamorphose, ungeschlechtliche Vermehrung, Generationswechsel; Zeits. f. wiss. Zool., Bd. III. Equally important is the same author's treatise, Ueber den Polymorphismus der Individuen oder die Erscheinung der Arbeitstheilung in der Natur, Giessen, 1851.

account of alternation of generations. He makes no effort to show how it came about. Instead of considering alternation of generations as a peculiar case of development with metamorphosis, complicated by asexual reproduction,[1] he considers asexual reproduction as a peculiar case of alternation of generations.[2] He ignores all the facts which show that the alternation may have been gradually attained, an omission which is only excusable when we note that his treatise is dated 1842. He asserts dogmatically that there is no transition from metamorphosis to alternation of generations.

It is impossible to think much on this subject without falling into difficulties over the word generation. For my own part I believe that such words as generation, individual, organ, larva, adult can not be used quite consistently in dealing with a long series of animals whose life histories vary gradually and without end. Ordinary language, which was devised to meet the familiar and comparatively simple course of development of man and the domestic animals, is not always appropriate to lower forms, with complex and unusual histories. If we are resolved at all hazards to make our language precise and uniform, we either fall into contradictions, or else use words in unnatural senses.

Certain recent discussions render it necessary to point out that there can be no alternation of generations without increase by budding. If a single larva produces a single sexual animal, as when a pluteus changes to an echinus, there is development with transformation, but not alternation of generations.

It is, I think, of importance to be able to resolve so peculiar a phenomenon as alternation of generations into processes which are known to occur separately, and which may have arisen imperceptibly, becoming gradually emphasized by the steady action of the conditions of life. Every startling novelty that can thus be explained extends the application of that principle which underlies the theory of natural selection—I mean the principle that a small force acting steadily through a long time may produce changes of almost any magnitude.

The hydrozoa yield innumerable and varied examples of development with transformation and also of budding. They yield also the most admirable examples of division of labor. We have hydrozoan colonies, such as a budding *Hydra*, in which all the members are pretty much alike, but we soon advance to differentiation of the feeding and the reproductive members. In the siphonophora the colony becomes pelagic, and floats at the surface of the sea. Then the medusæ no longer break off and swim away, but are harnessed to the colony, and drag it along. The colony may contain feeding polyps, which procure and digest food for the rest; swimming bells, which are attached medusæ; perhaps a float, which is a peculiar kind of swimming bell;

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[1] This is a convenient short account of alternation of generations, but it will not apply to every case. In *Hydra*, for instance, there is an ill-defined alternation of generations, but no metamorphosis.

[2] Cf. Leuckart, loc. cit., page 183.

defensive polyps (which may be either batteries of nettling cells or covering organs); and reproductive individuals. As the individuals become subordinated to the colony, and lose essential parts of the primitive structure, they pass insensibly into organs.

The life histories of invertebrates abound in complications and paradoxes. Thus *Eucharis*, one of the ctenophors, becomes sexually mature as a larva, but only in warm weather. This happens just after hatching, when the animal is of microscopic size. Then the sexual organs degenerate, the larva, which has already reproduced its kind, grows to full size, undergoes transformation, and at length becomes sexually mature a second time.[1] There is often a striking difference between the early stages of animals which are closely related, or a strong adaptive resemblance between animals which are of very remote blood relationship. In the hydrozoa similar polyps may produce very different medusæ, and dissimilar polyps medusæ that can hardly be distinguished. There are insects so like in their adult state that they can only be distinguished by minute characters, such as the form and arrangement of the hairs on the legs, and yet the larvæ may be conspicuously different.[2] Annelids and echinoderms yield fresh examples of the same thing. In lepidoptera and sawflies the larvæ are very similar, but the winged insects quite different.[3] New stages may be added in one species, while closely allied species remain unaffected. In *Cunina* and the diphyidæ we get combinations which strain the inventive powers of naturalists even to name. Natural selection seems to act upon the various stages of certain life histories almost as it acts upon species.

But the history is not always one of growing complexity. Sometimes, for example, a well-established medusa stage is dropped. First it ceases to free itself, then the tentacles and marginal sense organs disappear, then the mouth closes. In the fresh-water Cordylophora the medusa is replaced by a stalked sac filled with reproductive elements or embryos. The lucernariæ present a single stage which seems to be polyp and medusa in one. *Hydra* has no medusa. It is not always clear whether such hydrozoa as these are primitive or reduced. Even the hydroid polyp, the central stage in the normal hydrozoan life history, may be suppressed, and certain medusæ in both of the chief groups develop direct from the egg or planula (*Pelagia*, *Geryonia*, *Ægina*, *Oceania*). There is no stage common to all hydrozoa except the egg. The same thing may be said of the tunicates.

The life history of many arthropods is to all appearance quite simple. There emerges from the egg a spider, scorpion, or centipede (in most chilopoda) which merely grows bigger and bigger till it is adult. But if, as in most crustacea, the circumstances of the species call for a

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[1] Chun Die pelagische Thierwelt, page 62 (1887).

[2] Some species of *Chironomus* are referred to.

[3] Baron Osten Sacken (Berl. Entom. Zeits., Bd. XXXVII, page 465) gives two cases of diptera, in which "almost similar larvæ produce imagos belonging to different families."

migratory stage, such a stage will be added. In certain decapod crustacea (*Penæus*, *Leucifer*) a nauplius and as many as five other stages may intervene before the final or adult stage. Some of these larval stages are common to a great many crustacea, but none, as we now think, belong to the original phylogeny. If a resting or a winged stage is wanted, it is supplied just as easily—witness the holometabolic insects. Here again, so far as we know, there is nothing absolutely new.[1] The stages which seem new are merely exaggerations for special purposes of sections of the life history, which were originally marked out by nothing more important than a change of skin and a swelling out of the body. Let us not suppose for a moment that it is a law of insect development that there should be larva, pupa, and imago, or that it is a law of crustacean development that there should be six distinct stages between the egg and the adult. Any of these stages may be dropped, if it proves useless—either totally suppressed, or telescoped, so to speak, into the embryonic development. Lost stages are indicated by the embryonic molts of some centipedes and spiders, *Limulus*, many crustacea, and *Podura*. The parthenogenetic reproduction of some immature insects, such as *Miastor*, shows a tendency to suppress later stages. Perhaps the wingless thysanura are additional examples, but here, as in the case of *Hydra* and *Lucernaria*, we do not certainly know whether they are primitive or reduced. It seems to be easy to add new stages, when circumstances (and especially parasitism) call for them. *Meloe*, *Sitaris*, and *Epicauta* are well-known examples. In some ephemeridæ the molts, which are potential stages, become very numerous, but as a curious exception to a very general rule, the last molt of all, which is usually so important, may be practically suppressed. The fly of an *Ephemera* may mate, lay eggs, and die, while still enveloped in its last larval skin.

Among the many cases of what one is inclined to call rapid adaptation to circumstances (the chief indications of rapidity being the very partial and isolated occurrence of remarkable adaptive characters) are those which Giard[2] has collected and compared, and which he refers to a process called by him pœcilogony. A number of very different animals[3] produce according to habitat, or season, or some other condition closely related to nutrition, eggs of more than one sort, which differ in the quantity of nourishment which they contain and in the degree of transformation which the issuing larva is destined to undergo. The analogy with the summer and winter eggs of *Daphnia*, etc., can not escape notice, and Giard connects with all these the pædogenesis of *Miastor* and *Chironomus*, and many cases of heterogony. For our immediate purpose it is sufficient to remark that the reproductive

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[1] Nirgends ist Neubildung, sonder nur Umbildung.—Baer.

[2] C. R. 1891, 1892.

[3] E. g. Crustacea (*Palæmonetes, Alpheus*), insects (*Musca corvina*, some lepidoptera and diptera), an ophiurid (*Ophiothrix*), a compound ascidian (*Leptoclinus*), etc.

processes and the course of development are as liable to vary for motives of expediency as the form of a leg or fin. The supposed constancy (the necessary constancy according to some naturalists) of the embryonic stages throughout large groups would not be hard to break down, if it were to be again asserted. Probably the doctrine is now totally abandoned; it belongs to that phase of zoological knowledge in which Meckel could declare that every higher animal passes in the course of its develment through a series of stages which are typified by adult animals of lower grade, and when an extreme partisan, far inferior to Meckel both in experience and caution, could affirm that the human embryo omits no single lower stage.

The tadpole larva, which is common in lower vertebrates and their allies, shows the influence of adaptation as strongly as any larva that we know. We may describe the tadpole as a long-tailed chordate, which breathes by gills and has a suctorial mouth disk, at least during some part of its existence. It is a cheap form of larva, when reduced to its lowest terms, requiring neither hard skeleton, nor limbs, nor neck, yet it can move fast in water by means of its sculling tail. Such a tadpole appears in many life-histories, and plays many parts. The tadpole is the characteristic tunicate larva, and in this group commonly ends by losing its tail, and becoming fixed for life. But *Salpa*, which is motile when adult, has lost its tadpole. *Appendicularia* has lost the normal adult stage if it ever had one, and its tadpole becomes sexually mature. The same thing seems to have happened to many amphibia, whose tadpoles acquire legs, become sexually mature, and constitute the normal adult stage. The lamprey, as Balfour and others have recognized, is another kind of sexually mature tadpole. Thus the tadpole may act as larva to a sea squirt, fish (*Acipenser*, *Lepidosteus*, *Amia*), or frog; it may also constitute the only remaining stage in the free life-history.

The lower and smaller animals seem to show beyond others the prevalence of adaptive features. They offer visible contrivances of infinite variety, while they are remarkable for the readiness with which new stages are assumed or old ones dropped, and for their protean changes of forms, which are so bewildering that many worms, for instance, can not as yet be placed at all, while many larvæ give no clue to their parentage. These lower and smaller animals show beyond others a tendency to multiply rapidly, and to break away from one another in an early stage. The tendency is so strong in the microscopic protozoa that it enters into the definition of the group. Fission, budding, alternation of generations, and spore formation (as in *Gregarina*) are ultimately due to the same tendency.

Weak animals are almost inevitably driven to scatter, and to make up by their insignificance, their invisibility, and their powers of evasion for the lack of power to resist. It is a great thing to a hydrozoan colony that if one polyp is bitten off, others remain, that no enemy can possibly devour all the medusæ liberated from one colony, or all the planulæ liberated from one medusa. Low organization gives very special facilities for extreme division. There are animals and plants which multiply greatly as a consequence of being torn to pieces or chopped small (chigoe, some fungi, etc.).

Small animals are usually short-lived. Many complete their life-history in a few weeks. Those which last for so long as a year are often driven, like annual plants, to adapt every detail of their existence to the changing seasons. The naturalist who explores the surface waters of the sea with a tow net soon learns that the time of year determines the presence or absence of particular larvæ. It is probably as important to an *Aurelia* as to a butterfly that it should tide over the storms of winter by means of a sedentary and well-protected stage. Any one who keeps scyphistoma in an aquarium will remark how small it is, how it creeps into crevices or the hollows of dead shells. But when the depth of winter is past it pushes out its strobila, which in spring liberates ephyræ. These rapidly enlarge, and by August have grown from microscopic disks to jelly-fishes a foot across.

The intelligence of many small animals is very low. They go on doing the thing that they have been used to do, the thing that has commended itself to the experience of many generations. They are governed by routine, by that inherited and unconscious power of response to external stimulus which we call instinct. But there are some notable exceptions. Of all small animals, insects seem to show the greatest flexibility of intelligence.

There is one large group of animals which is in striking contrast to nearly all the rest. Vertebrates, and especially the higher vertebrates, are usually big and strong. They rely upon skill, courage, or some other product of high organizations rather than upon numbers and fertility. Vertebrates swallow many other animals, together with their living parasites, but are rarely swallowed alive or fresh by invertebrates. This fact of nature has led to many consequences, among others to this, that many parasites which pass their earlier stages in the bodies of invertebrates only attain sexual maturity in a vertebrate host. The complexity of the structure of a vertebrate precludes the possibility of multiplication by breaking-up or budding, and they multiply only by egg-laying or strictly analogous processes. The higher vertebrates live so long that the accidents of a particular year or a particular season are not of vital importance. Hence seasonal transformation is almost unknown; the quadruped or bird may choose the warm months for rearing the family, or celebrate the pairing season by getting a new suit of feathers, or grow a thicker coat against the cold of winter, but that is all. No vertebrates perish regularly at the approach of winter, leaving only batches of eggs to renew the species in spring, nor is their structure profoundly modified by the events of the calendar (the frog is a partial exception). One minor cause of transformation, which affects the life history of many polyps, worms, and insects is thus removed. Vertebrates often take care of their young, and the higher vertebrates bring forth few at a time. For this reason, among others, they rarely afford examples of free larvæ. Such vertebrate larvæ as we do find conform to the vertebrate type. It is often impossible to predict what adult will develop from an invertebrate larvæ, but no one could hesitate to rank an *Ammocoetes*, a *Leptocephalus*, or a tadpole among the vertebrates.

It accords with this strength and mastery that vertebrates, and especially the higher vertebrates, should be more stable, more conservative, less experimental than other animals. They retain ancient structures long after they have ceased to be useful. The gill clefts, gill arches, and branchial circulation are good examples. Though not functional in sauropsida and mammalia, they never fail to appear in the course of the development. Yet the sauropsida and the mammalia are positively known to go back to the earliest secondary and late paleozoic times. Ever since the beginning of the secondary period at least, every reptile, bird, and mammal has continued to pass through a stage which seems obviously piscine, and of which no plausible explanation has ever been offered, except that remote progenitors of these animals were fishes. Could not natural selection, one is tempted to ask, have straightened the course of development during lapses of time so vast and have found out less roundabout ways of shaping the tongue bone and the ossicles of the ear? Either it costs nothing at all to pursue the old route, or it costs nothing which a higher vertebrate will ever miss. The second alternative seems to me the more likely. The sauropsida and mammalia, in comparison with other animals, are particularly well off, and like wealthy housekeepers they do not care what becomes of the scraps. It is, I fancy, different with many fishes, which show by their numerous eggs, the occasional presence of peculiar immature stages, and some other slight hints, that their life is a hard one.

The presence in the developing reptile, bird, or mammal of piscine structures which are no longer useful has been ascribed to a principle called recapitulation, and Haeckel lays it down as a fundamental biogenetical law that the development of the individual is an abbreviated recapitulation of the development of the race. If I had time to discuss the recapitulation theory, I should begin by granting much that the recapitulationist demands—for instance, that certain facts in the development of animals have an historical significance, and can not be explained by mere adaptation to present circumstances; further, that adaptations tend to be inherited at corresponding phases both in the ontogeny and the phylogeny. I am on my guard when he talks of laws, for the term is misleading, and ascribes to what is a mere general statement of observed facts the force of a command. The so-called laws of nature (a phrase to be avoided) may indeed enable us to predict what will happen in a new case, but only when the conditions are uniform and simple—a thing which is common in physics, but very rare in biology. I diverge from him when he says that "each animal is compelled to discover its parentage in its own development," that "every animal in its own development repeats this history, and climbs up its own genealogical tree." When he declares that "the proof of the theory depends chiefly on its universal applicability to all animals, whether high or low in the zoological scale, and to all their parts and organs,"[1] I feel persuaded that, if this is really so, the recapitulation theory will never be proved at all. The development, so far as it has yet been traced, of a *Hydra*, *Peripatus*, beetle, pond mussel, squid, *Amphioxus*, chick, or mammal, tells us very little indeed of the history of the races to which they belong. Development tells us something, I admit, and that something is welcome, but it gives no answer at all to most of the questions that we put. The development of a mammal, for instance, brings to light what I take to be clear proof of a piscine stage; but the stage or stages immediately previous can only be vaguely described as vertebrate, and when we go back farther still all resemblance to particular adult animals is lost. The best facts of the recapitulationist are striking and valuable, but they are much rarer than the thoroughgoing recapitulationist admits; he has picked out all the big strawberries, and put them at the top of the basket. I admit no sort of necessity for the recapitulation of the events of the phylogeny in the development of the individual. Whenever any biologist brings the word "must" into his statement of the operations of living nature I look out to see whether he will not shortly fall into trouble.

This hasty review of animal transformation reminds me how great is the part of adaptation in nature. To many naturalists the study of adaptations is the popular and superficial side of things; that which they take to be truly scientific is some kind of index making. But we should recognize that comparatively modern adaptations may be of vital importance to the species, and particularly luminous to the student because at times they show us nature at work.

I am accustomed to refer such adaptations to the process of natural selection, though if anyone claimed to explain them by another process I should, for present purposes, cheerfully adopt a more neutral phrase. There are, I believe, no limits to be assigned to the action of natural selection upon living plants and animals. Natural selection can act upon the egg, the embryo, the larva, and the resting pupa as well as upon the adult capable of propagation. It can even influence the race through individuals which are not in the line of descent at all, such as adults past bearing or the neuters of a colony. The distinction between historical and adaptive, palingenetic and cœnogenetic, is relative only, a difference not of kind but of degree. All features are

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[1] The quotations are from the late Prof. A. Milnes Marshall's address to Section D, Brit. Assoc. Rep., 1890, which states the recapitulationist case with great knowledge and skill.

adaptive, but they may be adapted to a past rather than to a present state of things; they may be ancient, and deeply impressed upon the organization of the class.

In biology facts without thought are nothing; thought without facts is nothing; thought applied to concrete facts may come to something when time has sorted out what is true from what is merely plausible. The reports of this association will be preserved here and there in great libraries till a date when the biological speculations of 1897 are as extinct as the ptolemaic astronomy. If many years hence some one should turn over the old volumes and light upon this long-forgotten address, I hope that he will give me credit for having seen what was coming. Except where the urgent need of brevity has for the moment been too much for scientific caution I trust that he will find nothing that is dogmatic or overconfident in my remarks.

THE ROYAL MENAGERIE OF FRANCE AND THE NATIONAL MENAGERIE ESTABLISHED ON THE 14th OF BRUMAIRE OF THE YEAR II (NOVEMBER 4, 1793).

By Dr. E. T. HAMY.[1]

GENTLEMEN: It was a hundred years ago, within a few days, that the National Convention, on a motion made by its committee on public instruction, founded the Museum of Natural History. It was the 10th of June, 1793; the proscribed Girondins were arousing the provinces, the Vendean bands had taken possession of Saumur after a bloody day, and the imperial army of Austria was bombarding Valenciennes and reducing Condé. Yet, in the midst of these frightful disasters, at a time when everything seemed irrevocably lost, there were found some indomitable men, like Joseph Lakenal and Daubenton, sufficiently resolute to brave the existing storm, sufficiently clear-sighted to prepare for the future. The decree, of which they were the joint authors, profoundly transformed the old Royal Garden of medicinal plants that Louis XIII had formally created. In a short time, thanks to the feverish activity of the corps of professors that carried on the rejunevated establishment, there was organized an extensive scheme of special instruction, embracing in its twelve courses the entire range of natural history and its applications; a large library was brought together, a menagerie was improvised, and at last the new galleries were ready to receive collections of every kind, found in convents or in the houses of émigrés, notably at Chantilly, at the Palais Royal, and at Saint-Victor.

These various developments of the new museum, which had been planned as far back as 1790 by a group of scientific men composed of Daubenton, Fourcroy, Thouin, Jussieu, and others, rapidly formed upon this foundation a sort of metropolis of natural sciences.

All of its institutions have been copied in the various countries of the globe, but one of them has caused the others to be almost forgotten. This one has remained up to the present time one of the best known and the most popular in France. It is at once suggested when we speak of the Jardin des Plantes: I refer to the menagerie of Étienne Geoffroy-Saint-Hilaire and of Frederic Cuvier.

It is concerning this celebrated institution that I wish to speak to you. It is its beginning that I propose to recall on the occasion of the

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[1] A discourse delivered at the general session of the Thirty-first Congrès des Sociétés Savantes. Translated from the Nouvelles Archives du Museum d'Histoire Naturelle, 3me Série, tome 5me, pages 1–15.

centennial of the foundation of the museum of which it constitutes one of the most important branches.

A menagerie, in the modern and scientific sense of the word, is above all a vast laboratory in which, under conditions which he himself determines, the naturalist comes to observe and experiment. He studies, in the animal whose external characteristics are already well known to him, the manifestations of intelligence or instinct, the degree of docility, the kind of alimentation, its endurance of captivity or of climate, everything that may by careful observation lead to an extension of the boundaries of human knowledge.[1] He studies also the modifications due to age and sex, and those which are due to a change of environment continued for a considerable time. He may, by appropriate unions, fix a useful or curious character; or he may, by crossing species or races, produce hybrids or mongrels, and thus approach a solution of great zoological problems which occasion much controversy at the present time.

By his side the artist will reproduce, with the pencil, the brush, the modeling stick, the forms and attitudes of the animals that are living before his eyes in the cages or in the paddocks, and when they finally die the anatomist comes to complete with his scalpel, and above all with his microscope, the descriptions and the comparisons of his predecessors, while the taxidermist seeks in studying the relief of the muscular masses the sure means of establishing for the museum collections the true forms of the animals.

Such is the course of affairs in modern scientific establishments of which the centenarian menagerie of the museum is the prototype.

I hardly need to say that it is only by gradual degrees, because of the slow progress of biology, that an establishment of so deeply a scientific character as this has been enabled to prevail.

The first centuries of our history knew no other collections of animals than those of the troops of savage beasts that the Romans, and after them the Franks, used in the arena.[2] Methodically starved, artfully irritated, these unhappy captives rushed at each other in furious combats, to the great joy of the brutal and blood-thirsty spectators.

The taste for combats of animals lasted quite a long time in France; the last of the Valois still had lion fights, and it was in one of these fights, ordered by Francis I, that the brave Sieur de Lorges[3] descended into the ring, cloak in hand and sword drawn, to pick up the glove which the lady of his affections had dropped among the beasts in order to test his valor.[4]

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[1] Cf. A. Milne Edwards, Museum d'histoire naturelle. La Ménagerie; rapport au ministre de l instruction publique. Paris, 1891.

[2] Rec. des Hist. des Gaules et de la France, Vol. II, page 243; Vol. III, page 87. Cf. Mem. Acad. Inscrip. et Belles-Lettres, Vol. X, page 360 et seq.

[3] François de Montgomery, sieur de Lorges.

[4] Oeuvres complètes de Pierre de Bourdeille, seigneur de Brantôme, published by Ludovic Lalanne, Vol. IX, Des dames (suite). Paris, 1876, pages 390-391.

These lions and other exotic animals, collected only to satisfy a vain love of display, were huddled together in some outbuilding belonging to the royal residence. Philip VI, in 1333, had obtained in the northeast corner of the old Louvre[1] a barn in which to keep his wild beasts. There was under Charles V some "oyseaulx et bestes estranges" at Conflans, an aviary and a menagerie at Tournelles, and the rue des Lions-Saint-Paul has preserved the remembrance of its noisy guests, who at that time lodged in an annex to the hotel of that name.[2]

With the fifteenth century more catholic tastes began to appear. Animals of far-away countries were sought for parks, and the duc de Berry, whom our regretted Luce had named the Curious, possessed, among other rare species at the chateau of Mehun-sur-Yèvre, a dromedary, a chamois, and an ostrich.

The last years of Louis XI were fraught with something better for practical zoology. In his gloomy manor of Touraine, where he was confined by the disorder that was soon to destroy him, the sad King attempted to enliven his solitude, and surrounded himself with new or little-known animals that he caused to be brought together from all parts. Commines explains these purchases made by his redoubtable master by the need which he felt of making people talk of him and of spreading abroad the good opinion which was prevalent concerning his health and his strength. But Louis XI knew, when he thought proper, how to direct his efforts to more effective ends than those which his councillor attributes to mere caprice, and the very choice of the animals brought to the royal menagerie—elk and reindeer from Scandinavia, horses and mules from Spain and Sicily, Spanish or Barbary dogs, Tunisian ostriches and falcons, canary birds and turtle doves from Africa—shows that it was something more than meaningless curiosity that animated the inhabitant of the manor at Plessis-les-Tours.

Louis XI had, as it seems to me, wider and more lofty views. I imagine that that great mind thought, in its isolation, that it might be possible to enrich the kingdom of France with some of those interesting and useful animals of which numerous and choice specimens were brought him at great expense from the south and north by Guillaume Moire, Gabriel Bertrau, Robert Sanze, and his other purveyors. Death surprised him in the midst of these efforts, and the only result of these attempts at acclimation was the acquisition of that gentle musician, the joy of the mansard, the popular singer from the Canaries, a quite unexpected legacy from the sad recluse of the manor of Plessis-les-Tours.[3]

Anne de Beaujeu had, it is said, all the tastes of her father; she was fond of living animals, preferring those that were odd and strange; thus

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[1] Sauval, Histoire et recherches des antiquités de la ville de Paris, Vol. I, page 365. A. Berty, Topographie historique du vieux Paris. Region du Louvre et des Tuileries, Vol. I, pages 124, 159. Mem. Soc. de Paris, Vol. VI, pages 103–107.

[2] Sauval, op. cit., Vol. II, page 282, etc.

[3] Cf. Jal., Dictionnaire critique de biographie et d'histoire, article Serins.

it was, in 1489, that she tried to obtain from Lorenzo de Medicis a giraffe which Malfota, envoy of the Sultan of Egypt, Kaitbai, had two years before brought to Florence. "C'est la beste du monde que j'ay plus grand désir de veoir," she pleasantly wrote to the prince, who had promised her by letter this curious animal. Lorenzo did not keep his word, and Anne had to content herself with seeing the giraffe—in a picture.

The princely courts of Italy vied with each other in maintaining rare and curious animals; it was one of the characteristic traits of the luxury of that epoch. "A great prince," writes Matarazzo, "ought to have horses, dogs, mules, hawks, and other birds, buffoons, singers, and animals from distant lands." And the great princes of France did as those of Italy, whom they wished to imitate in everything—they kept buffoons, singers, and animals. The menagerie was again established near the Louvre, and there were sent out to great distances, to Tunis, Fez, etc., special missions to bring back horses, greyhounds, camels, ostriches, a lion, an ounce, and a large number of birds intended for the chase and for ornament. A consul in Egypt sent young leopards, and there were also obtained bulls, bears, etc.

Natural history had just been revived in the west. Those who pursued it in France doubtless profited by the varied instruction which could be afforded by the royal collection, which grew richer every day. And yet one fine morning, the 21st of January, 1583, the entire menagerie disappeared in a lamentable catastrophe.

The sick mind of the last of the Valois, filled with strange visions, saw in a dream lions, bears, and dogs tearing his palpitating members. Henry III then went and partook of the sacrament with the Bonshommes of Nigeon, near Chaillot, and, returning to the Louvre, had all the lions, bulls, bears,[1] etc., killed with shots of arquebus. Thus ended, without any profit to science or art, that royal menagerie that might have served as a center for zoological studies in our country. It was more than a century before there arose anyone to continue the work of Pierre Gilles and Belon de Mans.

Henry IV cared little for wild animals. He only kept an elephant that had been given him, and all his collection of 1591 could be put upon the back of a horse.[2] Later, the Grand Seigneur having sent him a tiger that strangled one of his dogs, he disposed of the ferocious beast, which was exhibited for two sols, in the rue de la Harpe in May, 1607. Louis XIII, on the contrary, had, at his hunting lodge at Versailles, some animals, and especially birds, a collection that suggested later to his son the construction of the celebrated Menagerie du Parc, illustrated by the works of Perrault, Duverney, Oudry, and Desportes. It was in 1663 that Louis XIV commenced the first work upon this magnificent

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[1] Mémoires-Journaux de Pierre del'Estoile, Vol. II, Journal de Henri III. Paris, 1875, page 99.

[2] Ibid., Vol. VIII, 1880, page 297.

establishment, and as early as 1664 had the Nuncio Chigi and the Doge of Genoa visit the still-unfinished buildings.[1]

At that time the menagerie was reached by the left branch of the Traverse du Canal, leading from the Trianon. At the end of a fine avenue of trees there was an entrance to a first court, which led to a second one of an octagonal form, in the midst of which there arose the little chateau of the Dauphine, with its grand salon, its subterranean grotto, and two rich apartments. Around this radiated seven other courts, closed with grills united with termini representing "some subject of metamorphosis." There was the court of the Ostrich; the court of the Aviary, in which there was an aviary "of extraordinary beauty and magnificence;"[2] the court of Pelicans, with its reservoir quite filled with fish; then again the court of the Basin or of the Pond, the court of the Well, etc.; and beyond these symmetrical courts yet other courts, called those of the Stags, of the Lion, of the Fowls, cages for ferocious animals, an enormous dovecot that contained 3,000 pigeons, and at last a farmhouse, with its dependencies and various buildings that served as servants' quarters.

As early as 1671 the menagerie began to be filled with the most curious and varied animals. There was a certain Mosnier, or Le Mosnier, of Montpellier, who was the principal purveyor, while the officers of the royal navy, the consuls (particularly the one at Cairo), governors (like the one at Madagascar), sent in whatever they could find that was curious.

A single consignment, for example, that arrived in 1688 comprised 194 animals from the Levant—13 ostriches and 137 of those sultana fowls that we were trying vainly at that time to acclimate in France; a pelican, Egyptian geese, egrets, etc., and finally 6 goats from the Thebaide.[3]

The menagerie at Versailles then possessed several thousand animals more or less rare; an elephant, dromedaries, gazelles, a cassowary, and later a number of wild beasts which had been brought from Vincennes, then abandoned.

Oudry and Desportes took the portraits of the most curious of these foreign guests at Versailles, and the Louvre possesses an enormous collection of studies painted from nature by order of the King.

If an interesting animal died, Colson seized it for the museum, and Claude Perrault made of it the most minute dissections; Perrault, whom the scornful Boileau treated as a learned boaster, and who was one of the most erudite physicians of his age and one of the founders of comparative anatomy; Perrault, who, unappalled by the rigors of one of the most severe winters that France had ever known, studied the numerous victims that perished from cold in the cages at Versailles,

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[1] Dussieux, The Chateau de Versailles. Histoire et description. Versailles, 1881.

[2] Piganiol de la Force, Nouvelle description des châteaux et parcs de Versailles et de Marly, 1730, Vol. II, page 193, et seq.

[3] Comptes des bâtiments du roi, sous le règne de Louis XIV, published by M. J. Guiffery.

and who died at the age of 75 years a martyr to science from dissecting a dromedary that had died of a contagious disease.

Duverney succeeded Perrault; he also was an anatomist of the first rank, and the work of these two masters comprises documents of real value to-day.

The sudden death of the Dauphine, who, at the close of the reign of the grand monarch, was almost the only one who took any interest in the menagerie, caused a neglect of this fine establishment and its exotic inhabitants. Forty years later, when Rouille, minister of marine, offered to Louis XV a living bird of a new species, the court showed some desire to see again the establishment that had been abandoned. The Duc de Luynes, who then visited it (1750), found it worth making more use of, in good order, and with many animals.

It was no longer so when Louis XV, in his latter days, made a visit to that quarter. A sort of superintendent who had charge of the courts was then raising turkeys at the expense of the King. "Sir," said the monarch, "if that flock does not disappear I pledge my word that I will break you at the head of your regiment!"

Other not less crying abuses were introduced into the menagerie, forgotten at the foot of the park at Versailles. If we may believe Mercier,[1] a dromedary, a sober dromedary, such as is found in the deserts of Africa, cost the public every day six bottles of Burgundy wine. And the common people of Paris, returning on the evening of Whitsuntide by the boat from Sèvres, after having seen the princes, the procession of cooks, the park, and the menagerie, told the story of a Swiss guard who had petitioned for the place left vacant by the death of a dromedary.

These tales of the turkeys, the dromedary, and the Swiss guard certainly contributed in a large degree to excite the popular fury against the menagerie, which was pillaged from top to bottom during the days of October, 1789.

Seven years earlier (July, 1782) Buffon had tried unsuccessfully to transfer the last of its inhabitants to the Jardin du Roi, in which there were only a few aquatic birds. It required a number of unforeseen and peculiar circumstances to effect, one fine morning in November, 1793, a concentration in a corner of the Jardin des Plantes of a group of animals which formed the provisional menagerie of the new museum, a menagerie soon after made permanent.

After the devastations of 1789 there remained at Versailles five living animals that the pillagers had thought proper to leave alone—a lion of Senegal and its companion a Dalmatian hound, an Indian rhinoceros, a quagga from the Cape, and a bubalus sent by the Dey of Algiers. There had also been saved from the disaster a very beautiful crown pigeon from the Molluccas.

Couturier, the registrar general of the domains of Versailles, Marly,

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[1] Tableau de Paris, nouvelle éd., Amsterdam, 1782, Vol. IV, page 146.

and Meudon, wrote on the 19th of September, 1792, to Bernardin de Saint Pierre, who had been appointed intendant of the Jardin des Plantes two months and a half before, to inform him that the old menagerie was going to be destroyed,[1] saying that the minister had authorized him to send to the intendant anything he might want of the few animals that were still alive, and it seemed necessary for him to journey to Versailles. Bernardin set out, in fact, with Thouin and Desfontaines, and visited in their courts the subjects whose skins and skeletons had been offered him for mounting. He saw that a better use could be made of them, and appropriating as his own one of the most recent ideas of the council of officers of the Jardin du Roi, of the month of August, 1790, he proposed to transport what he calls a "public show" into "a place set aside for the study of nature, in the interests of sciences and the liberal arts, for scientists and for artists."

Such was the subject of the "Memoir on the necessity of uniting the menagerie with the national Jardin des Plantes at Paris," which appeared at the end of January, 1793. In it the author shows at length the services which an establishment of this kind might render, dissertating meanwhile, so as not to get out of the habit of it, on the influence captivity has on the character of living beings; on the sociability of the lion and the rhinoceros; on the interbreeding of wild and domestic animals; on the migration and acclimatization of animals; on the connection that ought to exist between a garden and a menagerie, etc. Then, after having easily refuted some objections that he himself raised, he concludes by proposing to take the animals, together with the cages they occupy, and to install the whole at the Nouveaux Convertis, that ancient monastery of which the maison Chevreul is the last vestige.

The Mémoire sur la Ménagerie was at the same time a request addressed to the convention: it helped perhaps to bring the Jardin des Plantes to the attention of some members of that assembly who were friends of science. But whatever we may say of that pamphlet, it was not that which a few days later started, in a strange and unexpected manner, the formation of the menagerie,[2] formally established twenty months later. I will recount the facts as they are given in the original documents.[3]

On the 13th of Brumaire of the year II (November 3, 1793) a decree was issued by the department of the police, signed by the administrators Baudrais and Soules, directing that the living animals then being exhibited on the Place de la Revolution and at some other places in Paris be immediately taken to the Jardin des Plantes, where they would be purchased, together with the cages which contained them.

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[1] They were going to make of it a breeding stable.

[2] Cf. Auge de Lassus, Jardin du Roi, Museum d'Histoire Naturelle, Rev. scient. Vol. LI, page 229, February 25, 1893.

[3] Archives nationaux et Archives du Museum.

The owners were also to receive an indemnity which would enable them "to get a living in some other manner."

Toussaint Charbonnier, commissary of police of the section of the Tuileries received the next day, the 14th of Brumaire (November 4), the first order of execution, and accompanied by the commissary of the civil committee for the section went to the Place de la Revolution. There, "on the left after leaving the Pont Tournant," he found in a booth the said Dominique Marchini, who was exhibiting a sea lion, a leopard, a civet cat, and a little monkey, and after having noted the observations of the said Marchini and those of his boy, Remi Amet, he conducted the animals and men to the committee and turned all of them over to the citizen corporal of the guard at the station of Rue St. Nicaise to be taken after the manner of a caravan to the Jardin des Plantes.

Great flutter was at the Museum, where no inquiries had been made, and where no one had been warned of the approach of these unexpected guests. The professor in charge of the Museum was a young man, 21 years old, appointed five months before, who was just beginning his career in both science and teaching, Étienne Geoffroy-Saint-Hilaire. Being a man of action, he rose to the occasion and began arranging the cages one after the other under the windows of the Museum, while awaiting the orders of the committee of public instruction.[1] This was his first menagerie.

Desfontaines, the secretary of the Museum, wrote next day to the president of the committee to ask instructions. "There is, under the galleries," he said, "a place where these animals can be provisionally lodged while we are preparing suitable quarters for them, and this place is even large enough to hold a greater number, if others are brought, and if the committee of public instruction should think proper to keep them. There is no doubt," adds the secretary, "but that a collection of living animals would be an advantage for the instruction of the public and for the progress of natural history, and that it would be the means of acquiring and multiplying, within the territory of the Republic, many useful species that now exist only in foreign countries. But it is left to the committee to consider, in its wisdom, whether these advantages can be made compatible with the present needs of the Republic." The four animals obtained from Marchini were to cost 12 livres per day, including the salaries of their keepers, and it was impossible to meet this expense from the funds of the establishment.

Desfontaines had not finished his letter before two other menageries arrived in their turn, that of Louzardi and that of Henry, containing a tiger cat, a white bear, two mandrill monkeys, two agoutis, two eagles, and a vulture. These were lined up with the animals of Marchini in the court of the establishment.

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[1] Is. Geoffroy-Saint-Hilaire, Vie. Travaux et doctrine scientifique. Étienne Geoffroy-Saint-Hilaire, Paris, page 49.

The committee of public instruction answered by a series of questions concerning the installation of the animals, their value, the daily expense they would involve, and even the purchase of adjoining land in case the convention should decide to form a menagerie. The professors redoubled their efforts to furnish without delay this very diversified information, while at the same time they decided to grant a daily indemnity to the proprietors of the confiscated animals. Their estimates were sent to the committee as early as the 17th of Frimaire (December 7, 1793), and their communication, giving details upon which it would be useless to enter here, ended by asking for the final possession "of all the materials and utensils belonging to the menageries of Versailles and Chantilly."

Then, while the committee was deliberating, the council of professors considered the most suitable means "for constructing temporary cages" and for transporting the poor animals from Versailles. The cages were finished the 16th of Ventose (March 3), and toward the end of Germinal the three survivors of the royal menagerie enjoyed the modest hospitality of the republican museum.[1]

The animals of the park at Raincy were put at the disposal of the Government by Crassous, a member of the convention (29th Germinal; 21st March). In short, when the citizens Billaud-Varennes, Barrère, and Priem (of la Marne) came to visit the museum to see with their own eyes what enlargements were necessary, Daubenton, who received them at the head of the professors, could show them a national menagerie already quite presentable.

The new institution was definitely established by the adoption of the report of Thibaudeau, read in the convention on the 21st of Frimaire of the year III (December 11, 1794), and Étienne Geoffroy, its founder, could then begin the works which have since immortalized his name.

In the course of the century, which it has just completed, the menagerie of the Museum of Paris has had some fine days. The arrival of ten cases (14th of Fructidor, year VI; August 31, 1796), escorted by fourteen guards, in which there was brought from Holland the animals and birds confiscated from the Stadhouder; the receipt of the male and female elephants from the same collection; the purchase of tigers, lynxes, and other carnivorous animals brought from London by Pembroke (1800); the arrival of a gnu, a zebra, etc., by the ships of Baudin (1804); the installation of the bear pits, in which dynasties of bruins, white or black, perform the same tricks before a crowd that is constantly renewed; the acquisition of the animals of King Louis, brother of Napoleon; the opening of the new houses for wild animals, which seemed in 1821 so finely arranged, and which appear to-day so mean and close; the arrival of the first hippopotamus, the first chimpanzee, the first gorilla—all these were marked events in the life of the establishment.

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[1] All these details are taken from the Proces-Verbaux of the Council of the Professors (Arch. du Museum, Proc. Verb. Reg. I, pass.).

Perhaps, however, none of these had the importance of the solemn entrance, on June 30, 1827, of dame giraffe into the good city of Paris.

Everyone wished to see her, all the newspapers were full of her, articles and songs were written about her, and fashion, that other dispenser of glory, used her forms and colors to make the giraffe dress, the giraffe hat, the giraffe comb. Nevers had polychromic crockery; Épinal, illuminated images that represented the celebrated visitor. Even politics meddled with her, and some amateurs possess in their collections a bronze medal upon which is seen the giraffe addressing the country in terms similar to the historic words used by the Comte d'Artois in 1814,[1] "Nothing is changed in France; there is only * * * one beast more." I need not explain why this medal quickly became very rare.

Giraffe, hippopotamus, chimpanzee, etc., all these animals assembled together, sometimes to the number of 1,300 to 1,400, have constituted a special school of instruction that for one hundred years has played a most important part. As Isidore Geoffroy-Saint-Hilaire wrote in 1860, if the menagerie had not existed and had not been enriched from the very first with a great number of rare species, Cuvier would never have been able at the beginning of our century to publish his Comparative Anatomy and to prepare in this way a new life for zoology and the birth of palæontology; and Étienne Geoffroy would not in his turn, twenty years later, have written his Philosophical Anatomy. I will add, that if it had not been for the menagerie Isidore Geoffroy himself, Blainville, Duvernoy, H.-Milne Edwards, P. Gervais, Gratiolet, and many others would not have brought together the materials for the memoirs with which they have enriched science.

Without the menagerie Frederic Cuvier, who was an aid there as early as 1805, would not have written his studies on the instinct and intelligence of animals, etc. Without the menagerie the remarkable studies of M. Alphonse-Milne Edwards would not have been concluded, and we would doubtless be unacquainted with the conditions of hybridization among the pithecoid apes, the equidæ, the bovidæ, etc.[2] Without the menagerie many species of herbivoræ and a number of useful birds would not be acclimated in our country, and the museum would not have been able to renew, in a degree, the great fauna of our forests.[3]

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[1] It is now known that this saying was ascribed to the Comte d'Artois by Beugnot. (Memoires, pages 112–114; Paris, 1886.)

[2] Hybrids have been obtained at the museum by crossing the magot with the macaque, the magot with the cynocephalus, the macaque with various pouched monkeys, the horse with the onager, the horse with the zebra, the zebra with the onager, the ass with the onager, the zebra with the ass, the yak with the cow (the male is infertile, the female fertile), etc.

[3] The names of some of the species acclimated in the Jardin are as follows: The onager and the sambur deer brought by Dussumier, the pig deer, the sika deer of Japan, the muntjak deer of China, the gnu, the mouflon of the Atlas Mountains, the Egyptian goose, the black swan, the emeu which we owe to Péron, the nandou, the Chinese crane introduced by Montigny, numerous pheasants, etc.

Finally, without the menagerie French art would not be able to add to its list some of the most illustrious names of modern times, that of Barye, for example, or of Fremiet, his successor.

The menagerie furnishes each year a great number of subjects to the scalpel of the anatomist, and those of you gentlemen who represent zoological studies in the provinces know to what an extent the museum has aided, thanks to its menagerie, the enrichment of public collections.

All these results have been obtained since 1793, in spite of quite unfavorable conditions, in confined quarters, badly protected against the rigors of winter, with limited means, and an insufficient force of employees. What a renewal of progress may we not hope, now that a rejuvenated staff, active and above all competent, makes its kindly influence felt everywhere in the museum, and the public administrators make known each year, by voting subsidies for long-wished-for improvements, their interest in the institution founded by the national convention.

Much has been done at the Jardin des Plantes for science and for the country during the century now ending; no less labor and devotion will be given in the century about to begin. And without doubt the chronicler who a hundred years hence shall take the place at this tribune that the kindness of the committee has to-day accorded to me will have the honor and the pleasure of celebrating before a select audience other great names and great events.

BOTANICAL OPPORTUNITY.[1]

By William Trelease.

In selecting a subject for the first presidential address before the Botanical Society of America, which you have done me the honor of requiring of me, I have deviated somewhat from the customary lines of such addresses, inasmuch as I have not attempted to present an abstract of recent general progress in botany, nor any results of my own investigation. Such topics, indeed, are more likely than the one I have chosen to interest an assemblage of specialists like this society; but as the society is supposed to have as a principal object the promotion of research, the present has seemed a fitting occasion to address, through the society, the large and growing number of young botanists who may be expected to look to this society for a certain amount of help and inspiration in the upbuilding of their own scientific careers; hence it comes that I have selected as my subject "Opportunity."

Let us for a moment compare the conditions under which scientific work is done to-day with those prevalent in the past. From a purely utilitarian, and, for a time, perhaps almost instinctive knowledge of plants and their properties, beginning, it may be, before our race can be said to have had a history, through the pedantry of the Middle Ages with their ponderous tomes, botany, almost within our memory, stands as the scientific diversion or pastime of men whose serious business in life was of a very different nature. Such training as the earlier botanists had was obtained as being primarily useful in other pursuits than pure research, though there is abundant evidence that the master often enjoined upon the pupil the possibilities of botanical study, and no doubt he stretched the limits of botanical instruction deemed necessary, just as is done to-day in technical schools, in the hope that the surplus might be so used as to increase the general store of knowledge; but, at best, training was limited, and research was recreation and relaxation.

But our predecessors, even the generation immediately before us, lived under conditions which made it possible for a man to hold high place in the business or professional world, to accumulate wealth in

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[1] Address of the retiring president, delivered before the Botanical Society of America, at Buffalo, N. Y., August 21, 1896. From the Botanical Gazette, Vol. XIII.

commerce, and at the same time to devote much time to the study of nature. To-day the man who is not entirely a business man is better out of business, and, with few exceptions, the man who is not entirely a student is little better than a dilettante in science. Concentration is the order of the day, and specialization is the lot of most men. But specialization, the keynote of progressive evolution, is always intimately associated with a division of labor. Fortunately, the men who enter and win in the great game of commerce and manufacture see in a more or less clear way that nearly every great manufacturing or commercial advance has grown out of a succession of obscure discoveries made by the devotee to pure science, often considered by him, indeed, only as so many more words deciphered in the great and mysterious unread book of nature, but sooner or later adapted and applied for the benefit of all men by the shrewd mind of a master in the art of money-making. To these men, successful in business, we owe it that to-day not only are some men able to devote their entire time to scientific research and the propagation of knowledge, but that their work is done under favorable conditions, and with a wealth of aids and adjuncts that would hardly have been thought of a generation ago.

Instead of a smattering of systematic botany and organography, given as an adjunct to chemistry, medicine, or engineering, the student who wishes may to-day equip himself for a life of research in botany, by a considerable amount of preparatory work in the lower schools, beginning, perhaps, even in the kindergarten, and by devoting the larger part of his undergraduate time in college to the elements of the subject in the broadest and, if he wish, technical scope, having the benefit of marvelously detailed appliances and a broad knowledge of general facts. If he can and will work for a higher university degree, thus equipped, he may delve into the depths of the most limited specialty, guided for a time by those who have already broken soil there, and left at last with a rich and unexplored vein for his own elaboration. With this training, if he be fortunate in securing a position offering opportunity for research, or if he enjoy independent means, he may hope for a lifetime of more or less uninterrupted opportunity for unearthing the wealth of discovery that lies just within his reach.

Considering the prevalent conditions, my subject naturally divides itself into two quite distinct parts: the opportunity of institutions and of individuals. We stand to-day, apparently, at a transition point. Most of the active workers of the present time are college professors, who have done the research work that has made their names known, during the leisure that could be found in the year's routine of instruction or during their long vacations, and with facilities nominally secured for class use, or, in many instances, like those of a generation ago, the private property of the investigator. Even when appreciated at something like its true value, their original work, for the most part, has been closely watched to prevent it from encroaching upon the first duty, class work; and in most cases the facilities that they have been able to bring together are in direct proportion to the number of students attracted to their departments, and therefore in inverse ratio to their own leisure for research. But, as I have already stated, the feeling is growing among men able to foster such enterprises, that research is a thing worthy of being promoted, and we have before our eyes the spectacle of a gradually unfolding class of institutions in which investigation is not only tolerated but expected, either as an adjunct to instruction, as in the greater number of colleges, as a concomitant of educational displays, as in botanical museums and gardens, or, at least nominally, as a basis for technical or economic research, as in several of the larger drug houses, and, notably, in various agricultural experiment stations and the national Department of Agriculture. Perhaps the time has not yet come when laboratories of botanical research can stand out quite alone and justify their existence without reference to other ends, the utility of which is more generally understood and conceded, but it seems safe to predict that the next decade will see their complete evolution.

Opportunity, for institutions, lies primarily in equipment, and secondarily in its use. The problem of equipment for research is a complicated and difficult one. So long as there were no laboratories specially designed for this purpose, it was natural that the instructional laboratory should be furnished with appliances for demonstration, and that these should be amplified, as far as possible, for the repetition of experiments, in the first place, and afterwards for their extension; and it is no doubt true that a number of the smaller educational laboratories are to-day over-equipped when account is taken of the possible use to which they can be put. With a specialization such as we now see in progress, it may be questioned whether the ordinary collegiate equipment can not be reduced in scope in many instances, with benefit to the institution, by releasing money often badly needed in other directions, either in the same or different departments. On the other hand, it is certain that the equipment of the broader research laboratories, whether connected with universities or independent, must be made much more comprehensive than any which to-day exists in this country.

Under the stimulus of the last two decades, botany has come to the front in most colleges as a study well calculated to develop the powers of observation and the reasoning faculties. Where it still occupies the place of a fixed study of a few terms' duration in a prescribed undergraduate course, it is evident that the necessary equipment of the department is expressible in the simplest terms: for each course, that which is needed to exemplify by the most direct object lessons the subject selected and enough general and collateral material and literature to complement the work. But the case is somewhat different when, as is now frequent, a considerable option is allowed the student in the courses taken for the baccalaureate degree. Here the temptation exists to secure equipment for the broadest possible series of electives, and it is too often yielded to for the best interests of the institution. However liberal one may be in the matter of electives, it is evident, in most instances, that the student can not afford to devote more than about one-half of his undergraduate time to a single study like botany, and in this time he can cover only a definite amount of ground. While there is a certain seductiveness in the perusal of long lists of electives in a college catalogue, the serious contemplation of them shows that few, if any, students can hope to take all of the courses of such a list, and as, for the most part, they are garnished out in an attractive form, there is likely to be embarrassment in the wealth of subjects, so that, if left to himself, the student is very likely to select a series of disconnected but pleasing fragments, rather than the proper links in an educational chain. Experience shows the wisdom of limiting the list of electives to those that there is reasonable probability that the student can take, and of making the list a consistent whole, fairly opening up the entire field of botany in such a manner as to pave the way for a piece of advanced thesis work at the end, and for specialization after graduation. So far as undergraduate instruction is concerned, where, as is usually the case, funds are limited, it is here desirable, as in the other instance, to limit the scope of the departmental equipment quite closely to the requirements of the courses offered. As the senior thesis work is almost certain to be a further study of some one of the subjects already elected, the provision for it, in nearly every instance, is easily and quickly effected by a comparatively inexpensive addition, in each case, to the standard library and laboratory equipment. Such research work as the head of the department and his assistants find time for, as well as such post-graduate work as may be undertaken, can then be provided for in the same manner, piece by piece, with the exception of the final touches, demanding the use of the largest reference libraries or collections, the provision for which is not likely to be far to seek in the stronger research centers within a very few years.

Great herbaria, broad reference libraries, and large stores of apparatus and living or preserved material, are possible only to few universities and to the still fewer institutions specially endowed for research, to which alone, indeed, they seem strictly appropriate. For the latter, every shade of breadth of foundation is possible, from the laboratory and library limited to the narrowest specialty, to the institution founded and equipped for research in any branch of pure or applied botany. Fairly perfect equipment of the former class it is possible to find here and there, to-day, but though the seed is sown in several places, the broadest institutions, in their entirety, are still to be developed.

No doubt the first requisite in any such institution is a library of scope comparable with its own. Whatever may be said against the prevalent nomenclature discussions, it must be admitted that they are having the effect of bringing to the front the half-forgotten work of many of our predecessors, some of which, at least, is well worthy of resurrection, and, incidentally, this is stocking our larger libraries with a class of books which have confessedly been too much neglected of late. Without for a moment losing sight of the fact that botany is a study of one branch of nature, an object-study, we must recognize that its prosecution beyond the merest elements is not only greatly promoted by but almost dependent upon a knowledge of what has already been done.

Where an institution is located in a literary or scientific center, closely associated with large general libraries, learned bodies, and the like, it is usually relieved of the necessity for purchasing and keeping up the long files of such serial publications as the journals, proceedings of societies, etc., of mixed contents, which prove expensive alike in cost, binding, and space which for a given subject are used but seldom, and which, nevertheless, are the most valuable part of a large reference library, since they are the hardest to duplicate. But where a botanical institution stands in absolute or comparative isolation, it must carry this burden in addition to that of maintaining a library of treatises on botany alone. And, moreover, no sooner is research begun in any direction, than the necessity of following up divergent threads running in many directions becomes evident; for so close and complex are the interrelations between things in organic nature, that no single subject can be pursued far without drawing in others at first sight having no possible bearing on it. After the serials, which from their expensiveness can be possessed by only the larger libraries, stand undoubtedly the general classics in the several subdivisions of botany, followed by the more restricted memoirs, and among these, for convenience of use, should be found, whenever possible, separates and reprints from the journals and series of proceedings, even when the latter are complete on the shelves.

Next to books, material preserving records, or available for study, forms the great foundation in any research institution. A generation ago, or even less, this expression would have been taken as synonymous with an herbarium, perhaps associated with a garden of greater or less extent; but to-day the most comprehensive of museum possibilities must be added, so greatly has the subject broadened and increased the needs. For a broadly planned institution, with ample means, no doubt the scope of the herbarium should be as great as that of the library, comprising every group of plants, representing a wide range of geographical distribution, the effects of cultivation, etc.; and, however limited they may be at first, such museum accessories as alcoholic material, large wood and fruit specimens, and sections for microscopic study are sure to accumulate quite as rapidly as they can be cared for suitably, and to prove in time a very important part of the equipment. Though some of the best botanical work has been performed entirely in the herbarium, there has long been a growing conviction that for certain groups of plants, even for purposes of description and classification, field observation is absolutely necessary, while it is self-evident that for all studies of biology living material is essential. Side by side with the herbarium, then, and virtually as a part of the same general collection, stands the experimental garden, with its greenhouses and other appliances.

While many of the most useful studies are made with but few aids beyond the library and collections referred to, there is a large class of subjects, now being closely followed by some of the keenest investigators, which demand a special instrumental equipment. However it may be with library and collections, there seems little doubt that, as a rule, apparatus should be obtained only as it is needed for direct use. Except for the rotting of the bindings observed in the libraries ot manufacturing cities and where illuminating gas is used, books, when once classified and indexed, are easily and cheaply kept in a usable condition. If a few simple rules are followed herbarium material is also preserved safely for generations at a very small cost; and even sections and specimens in fluid, if properly preserved in the first place, may be kept for many years without great deterioration. Instruments designed for research, as a general thing, represent a considerable sum of money, since, excepting microscopes, microtomes, and balances, they are rarely made in numbers allowing any great economy in the labor of manufacture. Each of them is also, unfortunately, with few exceptions, calculated for a restricted class of experiments and likely soon to be greatly modified. Apparatus, moreover, is usually of a delicacy of adjustment calling for the greatest care in handling it and the most perfect protection possible against rusting, etc., so that, as a general thing, a case of instruments 10 years old is merely a historical curiosity, in part entirely out of date, and for the rest so badly out of order as to be nearly or quite useless. Except for a few standard instruments, I think it is now generally recognized that this part of the facilities, however costly it may be, should be regarded as transient, perishable material, rather than a permanent equipment. The history of the most successful physiological laboratories, in which delicate apparatus is chiefly used, furthermore shows that the most important results, as a rule, are not obtained by the use of commercial instruments, but by simple apparatus designed by the investigator to meet the precise needs of the problem with which he is busied, and usually constructed by him or his laboratory mechanic at very little cost.

Although it seems comparatively easy to decide on the proper limits of library, herbarium, and instrumental equipment for a given institution, knowing its scope, situation, and resources, it is very difficult to arrive at as satisfactory a conclusion concerning the extent of the research garden. As a general thing such gardens are also intended to be useful in college work, or to afford pleasure and instruction to the public, so that they are likely to be heterogeneous almost of necessity, and usually they are made far too comprehensive. More than any other class of facilities, garden plants require constant and expensive attention if they are to be kept in usable condition; and, with all of the care that can be given them, they are forever performing the most inexplicable and unexpected gyrations with their labels, so that the collections grown in botanical gardens (because of their variety) are notoriously ill named, though it would naturally be supposed that they, of all collections, would be above suspicion in this respect.

My object being to speak of facilities for research, this rather than education or entertainment, I ought to pass by this part of the subject with a mere mention, but I can hardly dismiss it without comment. Where the only object is to supplement the facilities for undergraduate work, the scope of a garden can be very small or moderately large, according to the courses it is to help elucidate. It may be confined to what may be called a propagating bed for plants needed in quantity, either in season or out of season, for class use, to an exemplification of the natural affinities of plants, or to various other instructive synopses, representing medicinal plants, fiber plants, forage plants, fruits, vegetables, timber trees, nut trees, shade trees, carnivorous plants, climbing plants, the sleep of plants, pollination, dissemination, etc., or it may be devoted to several of these combined. If it is to be a pleasure ground as well, not only should the art of the landscape architect be invoked in the arrangement of the plants, but it is necessary to add collections of decorative shrubbery and a large variety of purely ornamental florists' forms of herbaceous plants. If research is added to its aims, the collection must be further augmented by specially selected groups cultivated from time to time as needed for study.

Unfortunately, few if any gardens are so richly endowed that they can cover, in a satisfactory manner, the entire field indicated, or even any large part of it. From what has been said of the peculiar difficulties pertaining to the maintenance of botanical gardens, it is evident that in no other line of securities, whether for pure research or not, is a wise restriction so necessary as here. Once properly prepared, a species is represented in the herbarium on one or more sheets of paper safely and economically stowed away in a pigeonhole; but in the garden it is a constant source of care and expense as long as it lasts. Hence it is possible for one of the larger herbaria to contain representatives of more than half of the 200,000 species, more or less, of phanerogams, and a considerable, if smaller, proportion of cryptogams, while it is absolutely impossible for anything like this number to be represented in a living state in the best garden. No doubt the local requirements of every institution will do more to influence the exact scope of its living collections than any theoretical considerations, but it is certain that in most cases the greatest usefulness combined with the minimum expenditure will be reached by adapting the synopses chosen to the chief aims of the institution as closely as possible, and very rigidly restricting the species cultivated to the smallest number capable of adequately expressing the facts to be shown. Perhaps it is safe to say that an institution able to maintain an herbarium of half a million specimens, representing one-fifth as many species, is doing exceedingly well if it has in cultivation at any one time 10,000 species of the higher plants, and there are very few gardens which actually grow half of this number, while no inconsiderable percentage of the plants cultivated are so deformed, distorted, dwarfed, and imperfect, as a general thing, that they can scarcely be said to represent the species whose name they bear either in appearance or technical characters.

This leads to the conclusion that not only class gardens but research gardens should be kept within reasonably narrow bounds so far as permanent planting is concerned, while allowing sufficient elasticity for rapid and ample temporary expansion in certain directions along which work is planned. This does not necessarily mean that any considerable amount of land not used in the permanent plantation need be reserved for special expansion. As a rule, the more important gardens are situated in or near large cities, and the high price of land alone would prevent such reservation in most instances; but the impure atmosphere of many of the larger cities is a further and even stronger reason for selecting, for any large experimental undertaking, a suitably located and oriented tract of farming land easily rented for one or several years at a relatively low figure.

Granting the wisdom of such temporary adjuncts to a research garden, a step further leads to a recognition of the possibility of securing the most varied climatic conditions by establishing branch gardens, located where particular kinds of study can best be carried on. In no other way can gardens be made to contribute to the fullest extent to the study of marine or seaside plants, alpines, or the great class of succulents, etc., characteristic of the arid regions of our Southwestern States and Territories, and in no other way, except in the field, can these groups be studied satisfactorily, even from the standpoint of the classificatory botanist.

Undoubtedly, too, the research institution of the future will count as a part of its legitimate equipment the provision, as needed, of very liberal opportunities for its staff to visit even distant regions for the study, in their native homes, of plants which can not be cultivated even in special gardens in such a manner as to be fully representative.

If the entire equipment here sketched in outline is not only appropriate but essential to the great centers of botanical investigation that are making their appearance as results of the specialization and division of labor that are now manifesting themselves in the endowment of research, it by no means follows that every institution, even of this class, should try to develop from the start on all of the lines which, intertwined, compose the complex tissue of botany. With ample means, the ideal development is that which, from the beginning, recognizes all branches as of value, and classifies and develops them alike in proportion to their relative importance. But to secure the greatest return for the money expended, it is desirable to equip fairly well before increasing the force of salaried men much beyond what is needed for the care and arrangement of the material accumulating. This principle, if followed out, almost forces an overdevelopment in the branches of special interest to the earlier employees—a departure from the ideal symmetry which is sure to be justified by the performance of more work in these hypertrophied specialties, with reference to the sum invested, than in other directions. From this may also be drawn the seemingly just inference that where the means are limited, it is far better to concentrate the entire equipment on the specialties of the persons who can use it than to allow them to work at a disadvantage through an effort, however commendable it may at first appear, to secure a symmetrical equipment.

With the evolution of centers of pure research will appear new problems. Just as the attendance of a large number of students in the botanical department of a college has heretofore been found to justify the acquisition of facilities beyond the power of their immediate use, it will be found that where research institutions exist, in close connection with a university of recognized standing, their equipment will be utilized more or less fully in postgraduate work done toward the acquisition of the doctor's degree, so that, like the undergraduate equipment, it will be more or less satisfactorily accounted for by the number of candidates for such degree; but with broadly grounded and well endowed research institutions not so situated, it is inevitable that as they take permanent form on the lines calculated to make them available for advanced research in any line of botany, they will sooner or later come to represent a very large sum of invested money, of which only a part is usefully employed at any given time, the remainder being held as a necessary but temporarily unproductive reserve. The same thing is seen, to a certain extent, in all large libraries and museums; but, unlike the general library, of interest to the entire reading public, or the collection of historical or political works, referred to by many people of ordinary intellectual attainments, the advanced equipment in botany, for the most part, is useful and interesting only to botanists, so that, while it may possess a passing interest for the general student, its serious use is limited to a very restricted class. How to increase this use to the maximum may well demand our best thought.

No doubt, just as many colleges now offer scholarships, making their advantages available to men who otherwise could not enjoy them, and some of our universities offer fellowships, opening their own post graduate courses or those of foreign universities to deserving students, the evolution of research institutions will witness some such provisions for enabling students who have partially completed pieces of research work to visit and utilize these centers without encroaching too far on the limited savings from the small salaries which, as a rule, are drawn by the botanists of the country. After all, however, the great opportunity of attainment for such institutions, whether or not connected with colleges or universities, lies in the performance of research work by their own employees, and while, except in a few instances already referred to, and notably in the national Department of Agriculture, to-day there is some hesitancy in recognizing the employment of a staff of investigators as a legitimate part of the maintenance expense of an establishment which does not use a large part of their time in instruction or necessary curator's routine, it is quite certain that within a very few years opinion will have so changed that a considerable number of salaried positions for research work in pure or applied botany will exist, and as these positions will compete with the professorships in the best universities, it seems probable that the salaries pertaining to them will be approximately those paid at the larger colleges.

In addition to bringing together facilities for research and rendering them easily accessible to competent investigators, and maintaining their own corps of workers, engaged in such study, institutions of research have no small field of usefulness opened up as publishers of the results of the work they have promoted. I shall have occasion later to speak of the means of publication from the standpoint of the student who is seeking to bring out his work in the best form, but it also demands consideration from the point of view of the institution. Much difficulty is experienced in looking up the literature of a subject because of the large number of journals, etc., in which references must be sought, and it is probable that at some time or other most workers have impatiently wished that publication could be confined to one or a few channels. Simple as this would render the bibliography of botany, it is obviously impossible, and the amount of work deserving or demanding publication is so great and so rapidly increasing as to leave no doubt that means of effecting the latter must be considerably augmented. To publish the results of good work well is no less commendable or helpful than to facilitate or perform such work. Nor is it less appropriate to an institution such as I have in mind. The object of publication being the adequate preservation and diffusion of a record of the results of research, however, it is easily seen that harm may be done by injudicious or ill-considered publication. While a volume of homogeneous contents may be so published almost anywhere as to accomplish its purpose, a serial publication ought to be started only when there is reasonable probability that it will persist for a considerable length of time. Granting this probability, a research institution with adequate funds forms one of the most satisfactory and effective agencies of publication, since it can place its proceedings or reports in all of the principal libraries of the world, a thing which the journals do not always accomplish; and not only can it thus amplify its field legitimately, but almost of necessity it must assume the duty of publication if it is to accomplish the greatest results possible from its direct investigation.

One has only to pass a short time in the library of one of the largest scientific institutions to be convinced that a great deal of activity is manifested in the botanical world. Each month and each week bring many additions to the literature of the science, and so numerous, varied, and widely scattered are these contributions that one feels the greatest hesitancy in publishing on even the most restricted subject, lest others should have antedated his discoveries. Yet, notwithstanding the variety and number of botanical publications, and the great progress which is undeniably made every year, it is a matter of frequent comment that the progress made is by no means so much greater than that of our predecessors as might be expected, considering the greater advantages under which work is prosecuted to-day. While it must be borne in mind that the seizing of the general features of a landscape is far easier than the working out of its detailed topography, that the outlining of the field of botany or of its principal divisions could not fail to proceed more rapidly, even under unfavorable conditions, than the elaboration of the details of the many specialties into which it is now broken up, so that less prompt and voluminous results are naturally to be expected now than a generation ago, there is reason to question whether the present returns can not be increased. How to secure the greatest possible results from the large number of trained men holding or soon to hold salaried positions, and from the large equipment in laboratories, libraries, herbaria, and gardens, is a subject deserving of the most careful study, whether viewed from the standpoint of the endower or administrator of an institution of education or research or from that of the botanist whose reputation is built up in the performance of the duties assigned to him in such an institution.

While there is every reason to expect large returns from the endowment of such independent departments of research, freedom from the duties of the class room, while leaving more time available for investigation, will not prove an unmixed blessing. I believe it to be the experience of the best investigators in this country that research is promoted by the necessity of imparting some or all of its results in the class room. In no other way, after specializing to the small field in which it seems necessary for most of us to confine ourselves, can one make sure of preserving the breadth of view needed for the investigation of even a limited specialty in the most successful manner. It must be admitted further that the power of application and concentration varies with different men, so that up to a certain point the interruptions introduced by limited teaching or looking after collections in many cases may give fresh zest to the pursuit of knowledge in the time remaining for research. And it may be that at this very point lies the greatest difficulty to be met and surmounted in the development and management of research institutions.

Though there is no doubt that some supervision and pressure are conducive to the performance of the greatest possible amount of investigation, as of other work, since they insure consistent planning and close application, it can not be overlooked that this is the extent to which scientific work can profitably be crowded. To require more of an investigator than that he shall be reasonably busy with thoughtfully planned study is and has always been antagonistic to the performance of his best work; and the requirement of some institutions that a bulletin shall emanate from each department at stated intervals, while it insures quantity in publication, generally does so at the expense of quality of attainment. As a rule, genius, which, left to itself, now and then leaps to the most unexpected accomplishments, is most effectively repressed by close supervision. It is tolerant of guidance, but not of the goad; and yet, on the whole, perhaps, both guided and driven, if this is done wisely, it accomplishes most, for in harness it becomes plodding research, which is dull, to be sure, but, if persevering, productive of cumulative results which become of incalculable importance. In fact, whether fortunately or unfortunately I shall not attempt to say, the world has come to recognize the slow but sure progress of research as in the main more desirable than the irregular and intermittent leaps of genius, though the two are closely akin—patient labor over endless facts, on the one hand, and broad observation and untrammeled thought, on the other.

If, everything considered, it is slow and persistent investigation, rather than sudden inspiration, to which we must look for the accomplishment of the greatest collective results in botany, it is equally true that the individual student is more likely to build his reputation on the summation of the small accomplishments of many days of close application than to arrive at some great discovery by a leap; and this, quite aside from the fact that the latter result is entirely impossible to many a man who in the other way may still hope to be of great usefulness. It has been said that there is a tide in the affairs of men, which, taken at the flood, leads on to fortune, and no doubt what is true in the military, literary, and commercial world is equally true in the smaller realm of science. In fact, I fancy that each member of my audience has in mind some one preeminent occasion which may have looked small or large at the moment, but the seizing or neglect of which he now sees marked a turning point in his scientific career. But, it will be seen, it is not of the one great opportunity that I would now speak. Improving it always has marked and always will mark the turning point of life, but unfortunately the bridge can not be crossed before it is reached, and great as the value of a true and wise friend's counsel then is, it can not be replaced by any generalities in advance; therefore it is to the countless lesser opportunities, repeated with almost every day that dawns for us, that I turn, in the hope that something helpful may be said of them, and in the firm belief that in them lies the making of any intelligent and indefatigable young man.

To the investigator, breadth of foundation is even more necessary than to the institution founded for his use, for while the latter should endure for centuries, and may be remodeled and improved at any time, he is limited to a single lifetime and can rarely in mid life or later repair the deficiencies of ill advised or defective training. Not only should his powers of observation be well developed, but he should be given more discipline in reasoning than is now customary, though the botanists of a generation ago counted among their number several men who are even more widely known as philosophers.

Equipped for the work, and enabled to use the material facilities that have been brought together against the day of his need, much depends on the early and wise formation of the investigator's plans. Except for the tasks set by a teacher, and really long contemplated by him and carried out by his intelligence, if through the eyes and hands of pupils, few pieces of valuable research are taken up on the spur of the moment, without previous thought on the part of the investigator. They are usually the outgrowth of reflection started, perhaps, by some casual observation or the remark of another, and turning and returning until it ultimately shapes itself into a definite plan. Simple as it may be in theory, few things are more difficult in practice than the formation and inception in early life, inexperienced, and often without certainty of the power of continuance for any great length of time, of a plan for a single piece of research work worthy of the devotion of a lifetime; and few and fortunate are the men, even among those who have outlined and entered upon such a task, who are not forced from the path by side issues, or whose lives are not unduly short. More commonly one must be content to choose several smaller subjects, for their own sakes somewhat closely related to one another, if possible, and to follow these up in succession. It is surprising how blind even the sharpest eyed among us are to all that does not directly interest us, and it is an equal surprise to see how quickly one's eyes open to things which he has once begun to think of and look for. If for no other reason than this, I would again urge breadth of early training, as giving the first impulse to many a series of special observations to be followed up in later life.

Once a subject is chosen, observations accumulate with surprising rapidity, and next to the selection of a subject nothing is so important as system in pursuing it. If we do not see it in ourselves, each one of us can see in others a great waste of energy, resulting from shiftless and ill-considered methods of procedure, by which the mind is so distracted and the memory so overloaded with unessentials and disassociated fragments that those which belong together are not matched, nor the missing bits, in plain view, gathered. How often do we have to return, time after time, and review partial work that we have had to dismiss temporarily from the mind, in which, meantime, has been lost the connection between the completed portion and the continuation awaiting our leisure. A phenomenal memory may enable one to work in this disjointed fashion without the production of scrappy results or the review of all that has been done each time that the task is resumed; but for those not so gifted, order and method are absolutely necessary, and next to a clear idea of the end aimed at I should place the immediate making of full and exact notes as their most essential part

Some years since I was privileged to assist Dr. Gray in collecting and republishing the botanical writings of Dr. Engelmann, and it was a matter of surprise to us both, as it has been to others, to see how voluminous these were. Had Dr. Engelmann devoted his entire life to botany, they would have been as creditable in quantity as in quality, but for the leisure-hour productions of a busy professional man they were truly marvelous. Some years later, when his herbarium and library having found a resting place at the botanical garden, in the development of which he had felt an interest for many years, it fell to my lot to arrange in form for permanent preservation Dr. Engelmann's manuscript notes, sketches, etc., I was far more surprised at the extent of these than I had been on collecting his printed works, for when mounted and bound they form sixty large volumes. In addition to their intrinsic value, these are of more than usual interest as showing the methodical manner in which Dr. Engelmann worked. On his table seems to have been always a bundle of plants awaiting study. As each specimen was examined its salient features were noted and sketched on the back of the ever-ready prescription blank. When interrupted he laid his unfinished sketch away with the specimen to resume his observation and complete his study at the first opportunity, without any doubt as to what had been seen in the first instance. And so from individual to variety, from variety to species, from species to genus, and from genus to family, his observations were preserved in memoranda that facilitated the resumption of interrupted work at any time and after any lapse of time. In no other way could the odd moments between the daily calls and occupations of a busy physician have contributed so much to botanical knowledge; in no other way could his seemingly small opportunity for investigation have been converted into a great one.

Almost as important as the early selection of a worthy subject for study and the adoption of a method insuring the preservation and use of even the most trivial information bearing on it, is the adoption of suitable library methods. The student whose specialty is small and little explored has mainly the task of observing and reasoning from the facts before him; but in the departments that have long been the subject of study, while a part of the work is already done to his hand, and the prospect is that he can go much further than on entirely new ground, the task of ascertaining and profiting by what his predecessors have done is often a difficult one. Not infrequently the literature of a subject is so scattered as to make it next to impossible to pass it all in review, and at best the task of finding the fragments is one calling for a special faculty. One or more attempts have been made to form general bureaus of scientific information, to which one need only turn if he would be possessed of references to the principal literature of any subject in which he chanced to be interested. Perhaps as library facilities accumulate at the great centers of research, some method may be found of supplementing them with the skill of expert librarians who shall be able and willing to carry the contents of the library, at least in skeleton form, to those who can not come to it; but the time has hardly yet come when any American library is complete enough in all branches to offer this aid with a reasonable chance of doing what it promises, or so manned as to make such assistance possible except at the sacrifice of more valuable direct research.

For the present, then, the investigator must be content to do his own delving into the literature of his predecessors. Fortunately, much of the earlier literature has been sought out by some of the writers on any branch that has been the subject of earlier study, so that, starting with a memoir of recent date, one is guided to others, each of which may bring further references, until, if he have access to the works, almost the entire earlier literature is unearthed. On the other hand, the most recent literature of a subject is always the most difficult to find and use. After a study has been gotten well under way, so that the student is keenly alert to every observation or published item in any way bearing on it, if he have access to a library receiving the principal current journals he is not likely to overlook any important publication on his specialty which then appears. As a rule, all of the larger papers, at least, are noticed in Just's Jahresbericht, generally not more than a year later than that for which the volume purports to be compiled; but as the Jahresbericht is always some three years in arrears, it is difficult to prevent notes extending over a period of this duration from being defective, at least for the earlier part of the time, and there is at present no means of removing this difficulty, though the plan proposed to zoologists a year ago, and, I suppose, tested during the present season, if successful, would be equally applicable to botany.

So far as the final result is concerned, perhaps the manner in which one's work is published is almost as important as the subject selected or the method adopted for its investigation. Alphonse de Candolle, in one of the most helpful treatises ever published in the hope of rendering botanical work methodical and productive,[1] lays a great deal of stress on the early selection of a form of publication for the results of each important study. This done, the work continually shapes itself to this end. Frequently there is much difficulty in securing the publication of a monograph or memoir in precisely the form and place desired by the author, but there is seldom an insuperable obstacle in the way of publishing any really meritorious work in about the manner wished, provided it is suitably prepared.

In general, it is desirable that works of a given class should be so published that in seeking one a reader is likely to learn of another. This appears less important for books than for shorter papers, since the arrangement of independently issued volumes in a library, and the fact that they are catalogued by authors, render it relatively easy to learn

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[1] La Phytographie, ou l'art de décrire les végétaux considérés sous différents points de vue. Paris, 1880.

of and have access to them; but even here one finds no little convenience in the recognition that a book by a given author on a given subject is quite likely to be listed in the catalogue of a certain publishing house. Smaller papers, which are usually published in the proceedings of some society, or in a scientific journal, may almost be said to be made or ruined by the place selected for their publication. Probably, as library facilities increase and are more thoroughly classified and subject indexed, this will become less true than it now is, though the underlying reason for it will remain. Usually a reader turns to the popular journals only when looking for popularized science, and is not likely to seek the original results of research there, so that such papers are nearly or quite lost for a long time if published in these journals. Except where they are chiefly devoted to digests and abstracts, few nominally general journals now exist which do not lean so strongly toward a specialty that one unconsciously classes them with it, notwithstanding the extraneous matter that they contain. While nothing once published is ever absolutely lost, all of this extraneous matter is likely to be overlooked by the persons most interested in the subjects considered. No small part of the present confusion and strife in botanical nomenclature arises from the comparatively recent unearthing of descriptions and names of plants published in such improbable or inaccessible places as to have escaped the attention of those whom they might have helped most, to be brought to light at a later date as great mischief makers. From now on, then, it may be concluded that a decreasing number of special papers are likely to be published in general journals, which will become more and more popular or bibliographic in their nature, with the exception that the necessarily slow differentiation of learned societies into special sections will for a long time cause the proceedings of many of the older to continue of the most miscellaneous character. Where papers are lengthy, though not adapted to publication in book form, such proceedings virtually offer the only means of printing them, and, except by the comparatively few botanists who enjoy the privilege of membership in purely botanical societies with publishing facilities, they must be accepted for the present, notwithstanding the attendant disadvantages. Shorter papers, however, can usually find room in the journals, and except in cases where they possess a temporary and exceptional value for the columns of a popular or general journal, or one devoted to another subject to which, in some manner they are relevant, they are best published in a periodical exclusively devoted to botany, and in most cases, in one devoted as closely as may be to their particular branch of botany, provided it have a fair general circulation, and especially, provided it reach the principal botanical libraries.

Especially in the earlier years of their work, writers are sometimes given to distributing their papers among a number of journals. Except for the purpose of specialization just referred to, this is usually a mistake. Knowledge that a certain student has published on a given subject is often first obtained through incidental reference, lacking every element of precision. The probability that all of his writings are to be found in one or a few journals or series of proceedings greatly simplifies the completion and use of such references, since the Royal Society's Catalogue, though perhaps more complete as to titles, is necessarily even further behind than the Jahresbericht. Where the subject of an earlier paper is again passed in review by the author, only the gravest necessity should lead to the selection of a new medium for the publication of the later paper.

Whether the medium of publication selected or accepted be a journal or the proceedings of a society, the possibility of having separates struck off for the mere cost of press work, paper, and stitching makes it possible for almost any paper to appear as an independent pamphlet, accredited, to be sure, to the journal from which it is an excerpt, but, like a book, necessitating author's citation in catalogues, and admitting of more ready arrangement in its proper place where the works of a library are disposed on the shelves according to subject. The time was when a pamphlet was considered of little value and quite certain not to be preserved, but one of the characteristics of the modern librarian is a great and growing appreciation of the value of this class of works, leading to their careful preservation.

No small part of the volume of M. de Candolle, already referred to, is devoted to very explicit and well-considered directions for preparing the record of one's observations for the press; and the general conclusion is reached, after a careful analysis of the subject, that the maximum value of any manuscript exists at the exact moment of its completion, indicating this as the most suitable time for its publication. Though it is probable that the publishing of any important work should not be unnecessarily delayed after it has been pushed to what the author considers completion, at least so far as he can carry it, there may be reasons in some cases for publishing a preliminary statement considerably in advance of the completion of the work. Neglecting the publication of an early abstract of unfinished work as a means of securing priority, too often a purely personal matter, I may say that such abstracts, coupled with a request for material or data, not infrequently bring to the advanced student the means of greatly increasing the completeness and value of his work.

Time does not permit me to go into a detailed analysis of the many ways in which an investigator may use his time so as to make it productive of important results for himself and others. Having passed in somewhat comprehensive, though hasty, review the main factors in the question, I desire in closing to repeat that for most of us the opportunity of life does not lie in a great and abrupt change of condition, but that it is composed of countless minor chances which are great only when viewed collectively. To see and use them calls for alert senses, a knowledge and use of the means of ascertaining what has already been done, and, by exclusion, something of what remains to be done, facilities adequate to the task in each case, and indomitable perseverance and ceaseless activity. Great as the value of facilities is, they are merely means to an end. They accomplish nothing themselves. Hence, though it is certain that the most voluminous and, perhaps, the most comprehensive results, and those resulting from the performance of coherent experiments extending through a long series of years, will come from the great centers of research, there is no reason why qualitative results equal to the best may not continue to come, as they have in the past, from isolated workers, to the rounding out and completion of whose studies the facilities of the larger institutions will be more and more applicable as the problems of equipment are worked out.

MESCAL: A NEW ARTIFICIAL PARADISE.[1]

By Havelock Ellis.

It has been known for some years that the Kiowa Indians of New Mexico are accustomed to eat, in their religious ceremonies, a certain cactus called Anhalonium Lewinii, or mescal button. Mescal—which must not be confounded with the intoxicating drink of the same name made from an agave—is found in the Mexican Valley of the Rio Grande, the ancestral home of the Kiowa Indians, as well as in Texas, and is a brown and brittle substance, nauseous and bitter to the taste, composed mainly of the blunt dried leaves of the plant. Yet, as we shall see, it has every claim to rank with haschisch and the other famous drugs which have procured for men the joys of an artificial paradise. Upon the Kiowa Indians, who first discovered its rare and potent virtues, it has had so strong a fascination that the missionaries among these Indians, finding here a rival to Christianity not yielding to moral suasion, have appealed to the secular arm, and the buying and selling of the drug has been prohibited by Government under severe penalties. Yet the use of mescal prevails among the Kiowas to this day.

It has indeed spread, and the mescal rite may be said to be to-day the chief religion of all the tribes of the southern plains of the United States. The rite usually takes place on Saturday night; the men then sit in a circle within the tent round a large camp fire, which is kept burning brightly all the time. After prayer the leader hands each man four buttons, which are slowly chewed and swallowed, and altogether about ten or twelve buttons are consumed by each man between sundown and daybreak. Throughout the night the men sit quietly round the fire in a state of reverie—amid continual singing and the beating of drums by attendants—absorbed in the color visions and other manifestations of mescal intoxication, and about noon on the following day, when the effects have passed off, they get up and go about their business, without any depression or other unpleasant aftereffect.

There are five or six allied species of cacti which the Indians also use and treat with great reverence. Thus Mr. Carl Lumholtz has

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[1] Reprinted from The Contemporary Review, January, 1898, by permission of Leonard Scott Publication Company.

found that the Tarahumari, a tribe of Mexican Indians, worship various cacti as gods, only to be approached with uncovered heads. When they wish to obtain these cacti, the Tarahumari cense themselves with copal incense, and with profound respect dig up the god, careful lest they should hurt him, while women and children are warned from the spot. Even Christian Indians regard Hikori, the cactus god, as coequal with their own divinity, and make the sign of the cross in its presence. At all great festivals Hikori is made into a drink and consumed by the medicine man, or certain selected Indians, who sing as they partake of it, invoking Hikori to grant a "beautiful intoxication;" at the same time a rasping noise is made with sticks, and men and women dance a fantastic and picturesque dance—the women by themselves in white petticoats and tunics—before those who are under the influence of the god.

In 1891 Mr. James Mooney, of the United States Bureau of Ethnology, having frequently observed the mescal rites of the Kiowa Indians and assisted at them, called the attention of the Anthropological Society at Washington to the subject, and three years later he brought to Washington a supply of mescal, which was handed over for examination to Drs. Prentiss and Morgan. These investigators experimented on several young men, and demonstrated, for the first time, the precise character of mescal intoxication and the remarkable visions to which it gives rise. A little later Dr. Weir Mitchell, who, in addition to his eminence as a physician, is a man of marked æsthetic temperament, experimented on himself, and published a very interesting record of the brilliant visions by which he was visited under the influence of the plant. In the spring of the past year I was able to obtain a small sample of mescal in London, and as my first experiment with mescal was also, apparently, the first attempt to investigate its vision-producing properties outside America,[1] I will describe it in some detail, in preference to drawing on the previously published descriptions of the American observers.

On Good Friday I found myself entirely alone in the quiet rooms in the Temple which I occupy when in London, and judged the occasion a fitting one for a personal experiment. I made a decoction (a different method from that adopted in America) of three buttons, the full physiological dose, and drank this at intervals between 2.30 and 4.30 p. m. The first symptom observed during the afternoon was a certain consciousness of energy and intellectual power.[2] This passed off,

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[1] Lewin, of Berlin, indeed, experimented with Anhalonium Lewinii, to which he gave its name, as early as 1888, and as he found that even a small portion produced dangerous symptoms, he classed it amongst the extremely poisonous drugs, like strychnia. He failed to discover its vision-producing properties, and it seems, in fact, highly probable that he was really experimenting with a different cactus from that now known by the same name.

[2] I pass lightly over the purely physiological symptoms which I have described in some detail in a paper on "The phenomena of mescal intoxication" (Lancet, June 5, 1897), which, however, contains no description of the visions.

and about an hour after the final dose I felt faint and unsteady; the pulse was low, and I found it pleasanter to lie down. I was still able to read, and I noticed that a pale violet shadow floated over the page around the point at which my eyes were fixed. I had already noticed that objects not in the direct line of vision, such as my hands holding the book, showed a tendency to look obtrusive, heightened in color, almost monstrous, while, on closing my eyes, afterimages were vivid and prolonged. The appearance of visions with closed eyes was very gradual. At first there was merely a vague play of light and shade which suggested pictures, but never made them. Then the pictures became more definite, but too confused and crowded to be described, beyond saying that they were of the same character as the images of the kaleidoscope, symmetrical groupings of spiked objects. Then, in the course of the evening, they became distinct, but still indescribable—mostly a vast field of golden jewels, studded with red and green stones, ever changing. This moment was, perhaps, the most delightful of the experience, for at the same time the air around me seemed to be flushed with vague perfume—producing with the visions a delicious effect—and all discomfort had vanished, except a slight faintness and tremor of the hands, which, later on, made it almost impossible to guide a pen as I made notes of the experiment; it was, however, with an effort, always possible to write with a pencil. The visions never resembled familiar objects; they were extremely definite, but yet always novel; they were constantly approaching, and yet constantly eluding, the semblance of known things. I would see thick, glorious fields of jewels, solitary or clustered, sometimes brilliant and sparkling, sometimes with a dull rich glow. Then they would spring up into flower-like shapes beneath my gaze, and then seem to turn into gorgeous butterfly forms or endless folds of glistening, iridescent, fibrous wings of wonderful insects; while sometimes I seemed to be gazing into a vast hollow revolving vessel, on whose polished concave mother-of-pearl surface the hues were swiftly changing. I was surprised, not only by the enormous profusion of the imagery presented to my gaze, but still more by its variety. Perpetually some totally new kind of effect would appear in the field of vision; sometimes there was swift movement, sometimes dull, somber richness of color, sometimes glitter and sparkle, once a startling rain of gold, which seemed to approach me. Most usually there was a combination of rich, sober color, with jewel-like points of brilliant hue. Every color and tone conceivable to me appeared at some time or another. Sometimes all the different varieties of one color, as of red, with scarlets, crimsons, pinks, would spring up together, or in quick succession. But in spite of this immense profusion, there was always a certain parsimony and æsthetic value in the colors presented. They were usually associated with form, and never appeared in large masses, or if so, the tone was very delicate. I was further impressed, not only by the brilliance, delicacy, and variety of the colors, but even more by their lovely and various textures—fibrous, woven, polished, glowing, dull, veined, semi-transparent—the glowing effects, as of jewels, and the fibrous, as of insects' wings, being perhaps the most prevalent. Although the effects were novel, it frequently happened, as I have already mentioned, that they vaguely recalled known objects. Thus, once the objects presented to me seemed to be made of exquisite porcelain, again they were like elaborate sweetmeats, again of a somewhat Maori style of architecture; and the background of the pictures frequently recalled, both in form and tone, the delicate architectural effects as of lace carved in wood, which we associate with the mouchrabieh work of Cairo. But always the visions grew and changed without any reference to the characteristics of those real objects of which they vaguely reminded me, and when I tried to influence their course it was with very little success. On the whole, I should say that the images were most usually what might be called living arabesques. There was often a certain incomplete tendency to symmetry, as though the underlying mechanism was associated with a large number of polished facets. The same image was in this way frequently repeated over a large part of the field; but this refers more to form than to color, in respect to which there would still be all sorts of delightful varieties, so that if, with a certain uniformity, jewel-like flowers were springing up and expanding all over the field of vision, they would still show every variety of delicate tone and tint.

Weir Mitchell found that he could only see the visions with closed eyes and in a perfectly dark room. I could see them in the dark with almost equal facility, though they were not of equal brilliancy, when my eyes were wide open. I saw them best, however, when my eyes were closed, in a room lighted only by flickering firelight. This evidently accords with the experience of the Indians, who keep a fire burning brightly throughout their mescal rites.

The visions continued with undiminished brilliance for many hours, and as I felt somewhat faint and muscularly weak, I went to bed, as I undressed being greatly impressed by the red, scaly, bronzed, and pigmented appearance of my limbs whenever I was not directly gazing at them. I had not the faintest desire for sleep; there was a general hyperæsthesia of all the senses as well as muscular irritability, and every slightest sound seemed magnified to startling dimensions. I may also have been kept awake by a vague alarm at the novelty of my condition, and the possibility of further developments.

After watching the visions in the dark for some hours I became a little tired of them and turned on the gas. Then I found that I was able to study a new series of visual phenomena, to which previous observers had made no reference. The gas jet (an ordinary flickering burner) seemed to burn with great brilliance, sending out waves of light, which expanded and contracted in an enormously exaggerated manner. I was even more impressed by the shadows, which were in all directions heightened by flushes of red, green, and especially violet.

The whole room, with its white-washed but not very white ceiling, thus became vivid and beautiful. The difference between the room as I saw it then and the appearance it usually presents to me was the difference one may often observe between the picture of a room and the actual room. The shadows I saw were the shadows which the artist puts in, but which are not visible in the actual scene under normal conditions of casual inspection. I was reminded of the paintings of Claude Monet, and as I gazed at the scene it occurred to me that mescal perhaps produces exactly the same conditions of visual hyperæsthesia, or rather exhaustion, as may be produced on the artist by the influence of prolonged visual attention. I wished to ascertain how the subdued and steady electric light would influence vision, and passed into the next room; but here the shadows were little marked, although walls and floor seemed tremulous and insubstantial. and the texture of everything was heightened and enriched.

About 3.30 a. m. I felt that the phenomena were distinctly diminishing—though the visions, now chiefly of human figures, fantastic and Chinese in character, still continued—and I was able to settle myself to sleep, which proved peaceful and dreamless. I awoke at the usual hour and experienced no sense of fatigue nor other unpleasant reminiscence of the experience I had undergone. Only my eyes seemed unusually sensitive to color, especially to blue and violet; I can, indeed, say that ever since this experience I have been more æsthetically sensitive than I was before to the more delicate phenomena of light and shade and color.

It occurred to me that it would be interesting to have the experiences of an artist under the influence of mescal, and I induced an artist friend to make a similar experiment. Unfortunately no effects whatever were produced at the first attempt, owing, as I have since discovered, to the fact that the buttons had only been simply infused and their virtues not extracted. To make sure of success the experiment was repeated with four buttons, which proved to be an excessive and unpleasant dose. There were paroxysmal attacks of pain at the heart and a sense of imminent death, which naturally alarmed the subject, while so great was the dread of light and dilatation of the pupils that the eyelids had to be kept more or less closed, though it was evident that a certain amount of vision was still possible. The symptoms came on very suddenly, and when I arrived they were already at their height. As the experiences of this subject were in many respects very unlike mine, I will give them in his own words: "I noticed first that as I happened to turn my eyes away from a blue enamel kettle at which I had been unconsciously looking, and which was standing in the fender of the fireplace, with no fire in it, it seemed to me that I saw a spot of the same blue in the black coals of the grate, and that this spot appeared again, farther off, a little brighter in hue. But I was in doubt whether I had not imagined these blue spots. When, however, I lifted my eyes to the mantelpiece, on which were scattered all sorts of odds and ends, all doubt was over. I saw an intensely vivid blue light begin to play around every object. A square cigarette box, violet in color, shone like an amethyst. I turned my eyes away and beheld this time, on the back of a polished chair, a bar of color glowing like a ruby. Although I was expecting some such manifestation as one of the first systems of the intoxication, I was nevertheless somewhat alarmed when this phenomenon took place. Such a silent and sudden illumination of all things around, where a moment before I had seen nothing uncommon, seemed like a kind of madness beginning from outside me, and its strangeness affected me more than its beauty. A desire to escape from it led me to the door, and the act of moving had, I noticed, the effect of dispelling the colors. But a sudden difficulty in breathing and a sensation of numbness at the heart brought me back to the arm-chair from which I had risen. From this moment I had a series of attacks or paroxysms, which I can only describe by saying that I felt as though I were dying. It was impossible to move, and it seemed almost impossible to breathe. My speedy dissolution, I half imagined, was about to take place, and the power of making any resistance to the violent sensations that were arising within was going, I felt, with every second.

"The first paroxysms were the most violent. They would come on with tinglings in the lower limbs, and with the sensation of a nauseous and suffocating gas mounting up into my head. Two or three times this was accompanied by a color vision of the gas bursting into flame as it passed up my throat. But I seldom had visions during the paroxysms; these would appear in the intervals. They began with a spurting up of colors; once, of a flood of brightly illuminated green water covering the field of vision, and effervescing in parts, just as when fresh water with all the air bubbles is pumped into a swimming bath. At another time my eye seemed to be turning into a vast drop of dirty water in which millions of minute creatures resembling tadpoles were in motion. But the early visions consisted mostly of a furious succession of colored arabesques, arising and descending or sliding at every possible angle into the field of view. It would be as difficult as to give a description of the whirl of water at the bottom of a waterfall as to describe the chaos of color and design which marked this period.

"Now also began another series of extraordinary sensations. They set in with bewildering suddenness and followed one another in rapid succession. These I now record as they occur to my mind at haphazard: (1) My right leg became suddenly heavy and solid; it seemed, indeed, as if the entire weight of my body had shifted into one part, about the thigh and knee, and that the rest of my body had lost all substantiality. (2) With the suddenness of a neuralgic pang, the back of my head seemed to open and emit streams of bright color; this was immediately followed by the feeling as of a draft blowing like a gale through the hair in the same region. (3) At one moment the color, green, acquired a taste in my mouth; it was sweetish and somewhat metallic; blue again would have a taste that seemed to recall phosphorus; these are the only colors that seemed to be connected with taste. (4) A feeling of delightful relief and preternatural lightness about my forehead, succeeded by a growing sensation of contraction. (5) Singing in one of my ears. (6) A sensation of burning heat in the palm of my left hand. (7) Heat about both eyes. The last continued throughout the whole period, except for a moment when I had a sensation of cold upon the eyelids, accompanied with a color vision of the wrinkled lid, of the skin disappearing from the brow, of dead flesh, and finally of a skull.

"Throughout these sensations and visions my mind remained not only perfectly clear, but enjoyed, I believe, an unusual lucidity. Certainly I was conscious of an odd contrast in hearing myself talk rationally with H. E., who had entered the room a short time before, and experiencing at the same moment the wild and extraordinary pranks that were taking place in my body. My reason appeared to be the sole survivor of my being. At times I felt that this, too, would go, but the sound of my own voice would establish again the communication with the outer world of reality.

"Tremors were more or less constant in my lower limbs. Persistent, also, was the feeling of nausea. This, when attended by a feeling of suffocation and a pain at the heart, was relieved by taking brandy, coffee, or biscuit. For muscular exertion I felt neither the wish nor the power. My hands, however, retained their full strength.

"It was painful for me to keep my eyes open above a few seconds; the light of day seemed to fill the room with a blinding glare. Yet every object, in the brief glimpse I caught, appeared normal in color and shape. With my eyes closed, most of the visions, after the first chaotic display, represented parts of the whole of my body undergoing a variety of marvelous changes, of metamorphoses or illumination. They were more often than not comic and grotesque in character, though often beautiful in color. At one time I saw my right leg filling up with a delicate heliotrope; at another, the sleeve of my coat changed into a dark green material, in which was worked a pattern in red braid, and the whole bordered at the cuff with sable. Scarcely had my new sleeve taken shape than I found myself attired in a complete costume of the same fashion, mediæval in character, but I could not say to what precise period it belonged. I noted that a chance movement—of my hand, for instance—would immediately call up a color vision of the part exerted, and that this again would pass, by a seemingly natural transition, into another wholly dissimilar. Thus, pressing my fingers accidentally against my temples, the finger-tips became elongated, and then grew into the ribs of a vaulting or of a dome-shaped roof. But most of the visions were of a more personal nature. I happened once to lift a spoonful of coffee to my lips, and as I was in the act of raising my arm for that purpose a vision flashed before my closed (or nearly closed) eyes, in all the hues of the rainbow, of my arm separated from my body, and serving me with coffee from out of dark and indefinite space. On another occasion, as I was seeking to relieve slight nausea by taking a piece of biscuit passed to me by H. E., it suddenly streamed out into blue flame. For an instant I held the biscuit close to my leg. Immediately my trousers caught alight, and then the whole of the right side of my body, from the foot to the shoulder, was enveloped in waving blue flame. It was a sight of wonderful beauty. But this was not all. As I placed the biscuit in my mouth it burst out again into the same colored fire and illuminated the interior of my mouth, casting a blue reflection on the roof. The light in the Blue Grotto at Capri, I am able to affirm, is not nearly as blue as seemed for a short space of time the interior of my mouth. There were many visions of which I could not trace the origin. There were spirals and arabesques and flowers, and sometimes objects more trivial and prosaic in character. In one vision I saw a row of small white flowers, one against the other like pearls of a necklace, begin to revolve in the form of a spiral. Every flower, I observed, had the texture of porcelain. It was at a moment when I had the sensation of my cheeks growing hot and feverish that I experienced the strangest of all the color visions. It began with feeling that the skin of my face was becoming quite thin and of no stouter consistency than tissue paper, and the feeling was suddenly enhanced by a vision of my face, paper-like and semitransparent and somewhat reddish in color. To my amazement I saw myself as though I were inside a Chinese lantern, looking out through my cheek into the room. Not long after this I became conscious of a change in the visions. Their tempo was more moderate, they were less frequent, and they were losing somewhat in distinctness. At the same time the feeling of nausea and of numbness was departing. A short period followed in which I had no visions at all, and experienced merely a sensation of heaviness and torpor. I found that I was able to open my eyes again and keep them fixed on any object in the room without observing the faintest blue halo or prism, or bar of glowing color, and that, moreover, no visions appeared on closing them. It was now twilight, but beyond the fact of not seeing light or color, either without or within, I had a distinct feeling that the action of the drug was at an end and that my body had become sober suddenly. I had no more visions, though I was not wholly free from abnormal sensations, and I retired to rest. I lay awake till the morning, and with the exception of the following night I scarcely slept for the next three days, but I can not say that I felt any signs of fatigue, unless, perhaps, on one of the days when my eyes, I noticed, became very susceptible to any indications of blue in an object. Of color visions, or of any approach to color visions, there was no further trace; but all sorts of odd and grotesque images passed in succession through my mind during part of the first night. They might have been the dreams of a Baudelaire or of an Aubrey Beardsley. I would see figures with prodigious limbs, or strangely dwarfed and curtailed, or impossible combinations such as five or six fish, the color of canaries, floating about in air in a gold wire cage. But these were purely mental images, like the visions seen in a dream by a distempered brain.

"Of the many sensations of which my body had been the theater during three hours, not the least strange was the feeling I experienced on coming back into a normal condition. The recovery did not proceed gradually, but the whole outer and inner world of reality came back, as it were, with a bound. And for a moment it seemed strange. It was the sensation—only much intensified—which everyone has known on coming out into the light of day from an afternoon performance at a theater, where one has sat in an artificial light of gas and lamps, the spectator of a fictitious world of action. As one pours out with the crowd into the street, the ordinary world, by force of contrast with the sensational scenes just witnessed, breaks in upon one with almost a sense of unreality. The house, the aspect of the street, even the light of day appear a little foreign for a few moments. During these moments everything strikes the mind as odd and unfamiliar, or at least with a greater degree of objectivity. Such was my feeling with regard to my old and habitual self. During the period of intoxication the connection between the normal condition of my body and my intelligence had broken—my body had become in a manner a stranger to my reason—so that now on reasserting itself it seemed, with reference to my reason, which had remained perfectly sane and alert, for a moment sufficiently unfamiliar for me to become conscious of its individual and peculiar character. It was as if I had unexpectedly attained an objective knowledge of my own personality. I saw, as it were, my normal state of being with the eyes of a person who sees the street on coming out of the theater in broad day.

"This sensation also brought out the independence of the mind during the period of intoxication. It alone appeared to have escaped the ravages of the drug; it alone remained sane during a general delirium, vindicating, so it seemed, the majesty of its own impersonal nature. It had reigned for a while, I now felt, as an autocrat, without ministers and their officiousness. Henceforth I should be more or less conscious of the interdependence of body and brain; a slight headache, a touch of indigestion, or what not, would be able to effect what a general intoxication of my senses and nerves could not touch."

I next made experiments on two poets, whose names are both well known. One is interested in mystical matters, an excellent subject for visions, and very familiar with various vision-producing drugs and processes. His heart, however, is not very strong. While he obtained the visions, he found the effects of mescal on his breathing somewhat unpleasant; he much prefers hasheesh, though recognizing that its effects are much more difficult to obtain. The other enjoys admirable health, and under the influence of mescal he experienced scarcely the slightest unpleasant reaction, but, on the contrary, a very marked state of well being and beatitude. He took somewhat less than three buttons, so that the results were rather less marked than in my case, but they were perfectly definite. He writes: "I have never seen a succession of absolutely pictorial visions with such precision and such unaccountability. It seemed as if a series of dissolving views were carried swiftly before me, all going from right to left, none corresponding with any seen reality. For instance, I saw the most delightful dragons, puffing out their breath straight in front of them like rigid lines of steam, and balancing white balls at the end of their breath! When I tried to fix my mind on real things, I could generally call them up, but always with some inexplicable change. Thus, I called up a particular monument in Westminster Abbey, but in front of it, to the left, knelt a figure in Florentine costume, like someone out of a picture of Botticelli; and I could not see the tomb without also seeing this figure. Late in the evening I went out on the Embankment and was absolutely fascinated by an advertisement of 'Bovril,' which went and came in letters of light on the other side of the river. I can not tell you the intense pleasure this moving light gave me and how dazzling it seemed to me. Two girls and a man passed me, laughing loudly, and lolling about as they walked. I realized, intellectually, their coarseness, but visually I saw them, as they came under a tree, fall into the lines of a delicate picture; it might have been an Albert Moore. After coming in I played the piano with closed eyes and got waves and lines of pure color, almost always without form, though I saw one or two appearances which might have been shields or breastplates—pure gold, studded with small jewels in intricate patterns. All the time I had no unpleasant feelings whatever, except a very slight headache, which came and went. I slept soundly and without dreams."

The results of music in the case just quoted—together with the habit of the Indians to combine the drum with mescal rites, and my own observation that very slight jarring or stimulation of the scalp would affect the visions—suggested to me to test the influence of music on myself. I therefore once more put myself under the influence of mescal (taking a somewhat smaller dose than on the first occasion), and lay for some hours on a couch with my head more or less in contact with the piano, and with closed eyes directed toward a subdued light, while a friend played, making various tests, of his own devising, which were not explained to me until afterwards. I was to watch the visions in a purely passive manner, without seeking to direct them, nor was I to think about the music, which, so far as possible, was unknown to me. The music stimulated the visions and added greatly to my enjoyment of them. It seemed to harmonize with them, and, as it were, support and bear them up. A certain persistence and monotony of character in the music was required in order to affect the visions, which then seemed to fall into harmony with it, and any sudden change in the character of the music would blur the visions, as though clouds passed between them and me. The chief object of the tests was to ascertain how far a desire on the composer's part to suggest definite imagery would affect my visions. In about half the cases there was no resemblance, in the other half there was a distinct resemblance, which was sometimes very remarkable. This was especially the case with Schumann's music, for example, with his Waldscenen and Kinderscenen; thus "The Prophet Bird" called up vividly a sense of atmosphere and of brilliant feathery bird-like forms passing to and fro, "A Flower Piece" provoked constant and persistent images of vegetation, while "Scheherazade" produced an effect of floating white raiment, covered by glittering spangles and jewels. In every case my description was, of course, given before I knew the name of the piece. I do not pretend that this single series of experiments proves much, but it would certainly be worth while to follow up this indication and to ascertain if any light is hereby thrown on the power of a composer to suggest definite imagery, or the power of a listener to perceive it.

It would be out of place here to discuss the obscure question as to the underlying mechanism by which mescal exerts its magic powers. It is clear from the foregoing descriptions that mescal intoxication may be described as chiefly a saturnalia of the specific senses, and, above all, an orgy of vision. It reveals an optical fairyland, where all the senses now and again join the play, but the mind itself remains a self-possessed spectator. Mescal intoxication thus differs from the other artificial paradises which drugs procure. Under the influence of alcohol, for instance, as in normal dreaming, the intellect is impaired, although there may be a consciousness of unusual brilliance; hasheesh, again, produces an uncontrollable tendency to movement and bathes its victim in a sea of emotion. The mescal drinker remains calm and collected amid the sensory turmoil around him; his judgment is as clear as in the normal state; he falls into no oriental condition of vague and voluptuous reverie. The reason why mescal is of all this class of drugs the most purely intellectual in its appeal is evidently because it affects mainly the most intellectual of the senses. On this ground it is not probable that its use will easily develop into a habit. Moreover, unlike most other intoxicants, it seems to have no special affinity for a disordered and unbalanced nervous system; on the contrary, it demands organic soundness and good health for the complete manifestation of its virtues.[1] Further, unlike the other chief substances to which it may be compared, mescal does not wholly carry us away from the actual world, or plunge us into oblivion; a large part of its charm lies in the halo of beauty which it

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[1] It is true, as many persons do not need to be reminded, that in neurasthenia and states of overfatigue, symptoms closely resembling the slight and earlier phenomena of mescal intoxication are not uncommon; but in such cases there is rarely any sense of well-being and enjoyment.

casts around the simplest and commonest things. It is the most democratic of the plants which lead men to an artificial paradise. If it should ever chance that the consumption of mescal becomes a habit, the favorite poet of the mescal drinker will certainly be Wordsworth. Not only the general attitude of Wordsworth, but many of his most memorable poems and phrases can not—one is almost tempted to say—be appreciated in their full significance by one who has never been under the influence of mescal. On all these grounds it may be claimed that the artificial paradise of mescal, though less seductive, is safe and dignified beyond its peers.

At the same time it must be remembered that at present we are able to speak on a basis of but very small experience, so far as civilized men are concerned. The few observations recorded in America and my own experiments in England do not enable us to say anything regarding the habitual consumption of mescal in large amounts. That such consumption would be gravely injurious I can not doubt. Its safeguard seems to lie in the fact that a certain degree of robust health is required to obtain any real enjoyment from its visionary gifts. It may at least be claimed that for a healthy person to be once or twice admitted to the rites of mescal is not only an unforgettable delight, but an educational influence of no mean value.

THE UNITY OF THE HUMAN SPECIES.[1]

By Marquis de Nadaillac.

All discoveries and prehistoric studies testify to the unity of the human species in all regions. While the fauna and flora vary from continent to continent, even from island to island, man rests always and everywhere the same. All human bones, however different in origin and epoch, have belonged to man the same as we. In vain have men sought to attach the skull of Neanderthal or that more recently discovered at Trinil, in the Island of Java, to a humanity different from ours. It has been recognized that the first is more modern than was supposed and that analogous types[2] have been found belonging to every epoch; while for the second, pompously decorated with the name of Pithecanthropus erectus, after having read without prejudice the remarkable study of Dr. Houzé,[3] one is forced to abandon the ambitious hopes which have been too lightly accepted. It is not from isolated fragments that one can resolve the question of the existence of a being intermediate between man and the anthropoids; and until we can obtain absolute and decisive proofs we have the right to reject the entire theory.

It is not alone by his bony structure that this identity of man in all time and in all regions is to be affirmed. In my long anthropological studies I have been more than once surprised to encounter everywhere the same manifestation of man's intelligence—the same creations due to his initiative. When we visit the prehistoric collections in our museums we are astonished to see everywhere the same forms and processes of work and labor, and these among peoples separated by broad oceans or by arid deserts.

The arrowheads of the Dakota, Apache, and Comanche Indians show such curious resemblance to those discovered on the borders of the Seine and Thames; the nuclei of Scandinavia compare well with

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[1] Translated from Revue des Questions Scientifiques, publiée par la Société Scientifique de Bruxelles. Deuxième série. Tome XII. October 20, 1897.

[2] I have already given several examples (Les premiers hommes et les temps préhistoriques, t. I, page 151). It would be easy to add others.

[3] Revue de l'Université de Bruxelles, 1896, Father Van den Gheyn has made an analysis, with his habitual talent, in the Revue des Questions Scientifiques, 1896, t. II, page 396.

those of Mexico,[1] and if one exchanges the hatchets or the knives of flint from Europe with similar objects from America it is difficult for even experts to separate them, however well versed they may be in petrography and prehistoric archæology,[2] and it will be extremely difficult to distinguish the races to which they belong.[3] Vogt says[4] this resemblance is so evident that one can easily confound the implements coming from entirely different sources. L'Anthropologie has just published a description of stone instruments recently discovered by P. Zumoffen in Phenicia. We can cite the same facts for Egypt, and even in countries of classical antiquity we may find a prehistoric period marked by productions analogous to our own regions.

What we have just said for the duration of the Paleolithic epoch can be repeated for the Neolithic and in the aurora of modern times. Everywhere chipped stone implements gave place to those of polished stone. The hardest rocks—jasper, jade, jadeite, nephrite, chloromelanite—from deposits in unknown regions, were polished by persevering labor and became ornaments of ceremony and parade.

It will be easy to persevere and find these comparisons. Pottery from widely separated regions is made in the same form and by the same processes of fabrication, and even with the same ornamentation. The spindle whorls in stone, bone, and pottery, found in settlements succeeding each other on the hill of Hissarlik, recall those of the Swiss lake dwellings. Those of Peru, Mexico, and even those in present use among the Navajoes, are the same as those preserved in our museums, whether they come from Italy, Germany, the south of France, or the north of Scandinavia.[5]

The bow and the sling belonging to the Paleolithic period have been found in all the countries then occupied. Their origin is unknown; they date from the beginnings of humanity; their invention was the first conquest of man, and they clearly mark his superiority over the animal; they affirm the victory of intelligence over brute force. The picks of deer horn were utilized in the mines of France and England, of Spain and Belgium, the same as for the working of copper in Lake Superior, jasper in Indiana, and petroleum in Ohio. A stone hammer found at the foot of the Asturias is the same as those coming from the most ancient American mines.[6]

In the Grecian Archipelago, as in the regions known in America by the name of the Far West, they placed their beams and posts of wood in the walls in order to avoid the dangerous effects of earthquakes. The houses of the Pueblos of New Mexico and Colorado recall, by their

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[1] Tyler, Anahuac, pages 98, 101.

[2] Sir W. Dawson, Fossil Man, page 121.

[3] Tylor, Early History of Mankind.

[4] Congrès des Naturalistes Allemands, Innsbruck, 1869.

[5] Wilson, Swastika, 1896, pages 96 et seq.

[6] American Antiquarian, May, 1889, Proceedings American Philosophical Society, No. 149, page 396. Simonin, La vie souterraine.

manner of construction, antique houses of Syria or Phenicia, and perhaps the modern habitations of the Caucasus.

This revival of ancient usages among peoples apparently such strangers to each other is not exceptional. The amentum[1] is found among the savages of New Caledonia, and their sling stones in steatite, still in use, do not differ from the sling stones of prehistoric times. This fact was apparent at the Exposition of Budapest on the occasion of the millennium of the Kingdom of Hungary. The river men of Theiss in fishing employ a staff of very peculiar form, such as is found at the mouth of the Volga. The sweep net used in Hungary is the same as that on the shores of the Caspian Sea. The bone awls constantly used by the shepherds of this country are analogous to those from the turf pits of Holland. The "strike-a-lights" are the same as the prehistoric arrowheads. The wooden hooks charged with pieces of metal that served to lift the fishing lines from the bottom resemble those of the lake dwellings of Switzerland.[2]

It would be easy to multiply examples, but I hasten to those still more striking.

Man has in all times cared for the remains of his dead. Numerous are the rites which belong to religion and sentiment and a belief in the future, though sometimes similar rites are attached to curious superstitions. With rare exceptions we can divide these rites into four kinds: inhumation, cremation, mummification, and the stripping of the flesh from the bones (décharnement). We will speak only of the latter.

The stripping of the flesh from the bones after death was practiced in Neolithic times, possibly at the close of the Paleolithic period, but it continued during the Bronze Age. We see it in the Middle Ages in certain parts of Europe, and it persists among certain races of savages and even among races which we consider more civilized.

"After the idea that I have obtained from savage races," says Dumont d'Urville, in speaking of the voyage of the *Astrolabe*, "the interment or inhumation will be only the provisionary state and give the necessary time to separate from the body its corruptible part, and the repose does not begin until the bones are deposited in the sepulcher of the ancestors." I do not know if so philosophic an idea has penetrated the intellect of the Australians or Indians, but it is certain that these men, entirely barbarous as we suppose them to be, braved fatigues and even dangers in order to accomplish their duty toward their fellows.[3] The ossuaries in all countries of Europe, and America as well,

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[1] The strap attached to the javelin with which to throw it.

[2] Mittheil. der Anthrop. Gesellschaft in Wien, 1896. "We are driven to the conclusion," says Buckle (Hist. of Civilization, Vol. I, p. 20), "that, the actions of men being determined solely by their antecedents, must have a character of uniformity; that is to say, must, under precisely the same circumstances, always issue to precisely the same results." But it is necessary to add that the antecedents must be the same, which has not been said by the eminent author aforesaid.

[3] M. Cartailhac has treated this subject fully in La France préhistorique, Ch. XVI. Paris, 1889.

are the sepulchers of a family, often of a tribe. At each new inhumation they make place for the skeleton by pushing away the bones of his predecessor. It is not possible to explain otherwise the mixture that is found among the bones reposing in the sepulcher of such a community.

The sepulchral grottoes of the Garenne and Misy are ossuaries in which are mingled the stripped bones of numerous generations.[1]

In the first of these grottoes, situated in the Department of Marne, the débris has been treated with a sort of respect due to an elevated sentiment, and possibly a desire that the dead should not return to trouble the living. The grotto measures 6 feet each way. It is paved with rude calcareous slabs. A staircase of six steps is at the entrance, and the rock which forms the ceiling is upheld by eleven pillars. The bones were arranged in a circle, and each pile, belonging to a separate individual, was surmounted by his skull. The funeral furniture indicated an epoch more ancient than the disposition of the sepulcher would lead us to suppose. It comprised a vase of rude pottery fabricated without the wheel, blades of flint and a collar of beads of shell and certain isolated unios and mussels.

The prehistoric shelter of Sandron, the trou du Frontal at Furfooz, the grottoes of Chaveau near Namur, are, according to MM. Fraipont and Tihon, neolithic ossuaries. M. Cartailhac is of the same opinion in regard to the grotto of Baumes Chaudes and that of Challes in Savoy. Other archæologists have the same opinion as to the grotto de l'Homme Mort and of Boundalaou in Aveyron.[2] The learned Professor Pigorini is of the same opinion in regard to certain neolithic sepulchers in Italy. Similar ossuaries have been recognized in Germany and Spain.[3]

In 1832, Bruzelius admitted that the bones from the sepulchral mound of Asa, in Scanie, Sweden, had been stripped of their flesh before burial,[4] and he compares the mode of burial with the modes in usage at Tahiti and in the kingdom of Siam. The same fact has been stated of New Zealand by the Baron de Düben of Luttra.[5]

I have said that this funeral rite, which so much wounds our sensibilities and sentiments, has endured for many centuries among even the most civilized races, and that it persists to our own day. One could cite many facts demonstrating this, but I will present only a few. The bones of St. Louis, who died in 1270, at Carthage, were brought to St. Denis while the flesh and viscera were deposited in the abbey at Montreal; it was the same thing for the Duke Leopold, of Austria,

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[1] Matériaux pour l'Histoire de l'Homme. 1881, page 178.

[2] These have been studied by Dr. Prunières and by M. Rivière (Assn. Franc. Cong. de Besançon, 1893). Among the objects found was a cylinder made of a piece of human femur (Acad. des Sciences, June 19, 1893). Was it an amulet or a trophy?

[3] Materiaux, 1885, page 299.

[4] Iduna. Vol. 9, 1832, page 285.

[5] Antiquarisk Fidskrift for Sverige, I, pages 255 et seq.

who died in Apulia; the flesh was interred at Mont Cassin and the bones were transported to his own country by his servants.[1]

The ossuaries of Palermo are celebrated. The dead, after complete dessication, were piously transported, and upon fête days their parents and friends did not forget to visit them.

Mérimée, in his notes of the voyage in the west of France, mentions the shrine of St. Herbot which belongs to the time of Louis XIV. The bones of the dead were, after a certain number of years' inhumation, gathered and deposited in the shrine. Mérimée adds that the constructions destined to the same usage are numerous in different parts of Brittany, and that no one of them antedates the Gothic period. At the present time in certain parts of Switzerland, after the disappearance of the flesh, they take the skull out of the earth or grave and place it in the parish church, inscribed with the name of the deceased and the date of his death. There are evidences of ancient customs where all the bones have been disposed in the same fashion.

If we traverse the ocean we will find similar conditions. Pigorini notes the same thing among the Tahitians, the New Zealanders, the aborigines of Fly River, the Papuans, the inhabitants of New Guinea, and other peoples. The profound respect of the dead is one of the characteristic traits of the Maories, says Quatrefages.[2]

The tribes remaining independent observe scrupulously the ancient rites. The dead body is tabooed; they lament during several days around the dead body before they confide it to the earth. At the end of a certain time they remove it from its temporary tomb, and the bones, carefully gathered, are carried to a cavern known only to the initiated and is extremely tabooed.

In America the examples are not less numerous or less interesting.[3] The same rite exists from the St. Lawrence to the Mississippi and in South America. The death of an Indian, recounted by Sir John Lubbock[4] is followed by particular ceremonies. When the flesh was detached from the bones they were suspended in the air on a bed of reeds or interlaced branches, that they might be dried by the sun and bleached by the rain. The most distinguished women of the tribe are charged with the duty of drying the bones. During the pious ceremony the Indians, covered with long mantles, their faces blackened with soot, march about, striking the earth with staffs to frighten off the valichus (evil spirits) from the corpse. The bones are then loaded upon the favorite horse of the deceased and carried to the sepulcher of his ancestors.

Ancient traditions of these customs have been transmitted from generation to generation. Every twelve years the Hurons celebrated the

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[1] Bonstetten, Essai sur les dolmens.

[2] Sur l'état actuel des Maoris restés indépendants. Revue d'Ethn., t. IV, 1885.

[3] Schoolcraft, History of the Indian Tribes of the United States.

[4] L'Homme avant l'histoire, trad. franc, page 440.

fête of the dead. Brebœuf has preserved the description of one of these solemnities at which he was present in 1634.[1] The four nations which formed the confederation of the Hurons placed their dead upon an elevated scaffolding; when the day of the fête approached, the sad remains were lowered to the earth and the coverings or envelopes removed. Each family recognized its own, stripped off the flesh still remaining, and, with caresses and embraces, showed the greatest tenderness. Then the bones were enveloped in rich furs and borne to the common ossuary, often a considerable distance, and which was to be reached by pathways known only to the members of the gens.

Among the Indians of Chiriqui, whenever a member of the family was dangerously ill, they brought the sukia, the doctor, or the sorcerer, whichever you would call him. If he gave no hope of recovery the relatives of the dying individual carried him to the neighboring forest, suspended his hammock from the trees, and abandoned him, depositing near by a gourd of water and some green plantains. At the end of a year, when decomposition had done its work, a member of the tribe returned to the forest and gathered and cleaned the bones, which were then taken to the huacas where reposed the bones of his ancestors.[2]

The burial of the bones after the removal of the flesh has, therefore, been a custom widely extended during the centuries. Cremation was more general than has been thought, and recent researches, if we accept the opinion of M. Cartailhac, was an excellent mode of removal of the flesh from the bones, and above all when the burning or cremation was only partial.[3]

But if the rite rested the same in principle among different peoples, the processes varied greatly. Among the Patagonians, for example, the flesh was often artificially removed from the bones. The stria on the bones made by flint knives was well known and is conclusive testimony of this. Often they were abandoned to natural decomposition, which produced all that could have been done by a provisional burial or by prolonged action of atmospheric influences. The Salivas, on the borders of the Orinoco, had recourse to a more expeditious method. They plunged the corpse into the river, retaining it by a cord. At the end of two days the flesh had entirely disappeared from the bones, having been devoured by the fish. The bones were then gathered in baskets and suspended from the roofs of houses. Among the Persians of the sect of Zoroaster inhumation and cremation were both prohibited; so the dead body was suspended in open air and abandoned. It thus became the prey of the vultures. The Parsees rigorously practice to this day the same rite, and the lugubrious Towers of Silence near Bombay contain piles of bones of successive generations, from which the flesh had been stripped by the birds of heaven. Father Lafitau[4]

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[1] Voyages de la Nouvelle France Occidentale dite Canada.

[2] Bul. Soc. Géog., 1885, page 445.

[3] Ass. Franc. pour l'avancement des Sciences. Nancy, 1886, t. I., page 169.

[4] Mœurs des sauvages comparées aux mœurs des premiers temps. Paris, 1723.

reports that the South Americans ate the bodies of warriors killed in combat and afterwards carried their bones in the guise of standards. This is also the custom among the Indians of North America. In their frequent migrations they transport with them the bones of their fathers who have thus perished.[1] At the present time the Banis, Chams, and Musulmen of Annam bury the bodies of their dead in a ditch or grave without a coffin. One or two years afterwards they gather the bones, which are placed on a small bier and carried to the common cemetery.[2]

M. Petrie believes that he has discovered the existence of a new race in Egypt. This race lived on the west bank of the Nile to the south of Abydos, and about 30 miles north of Thebes. The tombs contained, instead of mummies, skeletons in a crouching position. These, according to the learned Englishman, were the Lybians who invaded Egypt near the end of the ancient empire about three thousand years before our era. The Lybians are said to have eaten the flesh of their dead, partially at least. But the facts on which this saying is based may also be explained by the removal of the flesh in the open air prior to the definite burial.[3]

We will not pursue this lugubrious enumeration. We have said enough to show that this strange funeral rite is encountered in all parts of the globe. The identity of these conceptions of man, difficult to separate from the identity of its origin, is perhaps still more marked when we find that from the most ancient times the human bones were colored red before being deposited in their last resting place. The examples are numerous, and it is necessary to go into details.

The first discovery by which the curious fact was made known is due to M. Riviére.[4] In 1872 he discovered in the grotto of Baoussé-Roussé, near Mentone, the skeleton of an adult, the skull of which was covered with red patine due to a thin coat of sanguine. The skeleton had upon the head a string of small shells (nerites). By its side was a poniard made from the radius of one of the deer kind, about twenty canine teeth, also from the deer tribe, and other arms and ornaments precious and destined, without doubt, for the use of the dead in the new world into which he was about to enter. Among the animal bones scattered through the cavern were those of the cave tiger (*Felis spelæus*), the cave bear (*Ursus spelæus*), rhinoceros, and the hog (*Sus scrofa*), and still others belonging to the Quaternary epoch. Three other skeletons were found in the same cavern, or those adjoining. The bones of the adults were covered with peroxide of iron, which gave them a strong red color. Three skeletons of children were also brought to light, none of which bore any traces of color. We are, therefore, in presence of a rite perfectly characterized as being applied only to adults. M. Cartailhac has

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[1] Heckwelder, Indian Nations, pages 90 and 93.

[2] Rev. des Quest. Scient., 1897, t. I., page 684.

[3] S. Reinach, Chron. d'Orient, Revue Arch., 1895.

[4] L'Antiquité de l'homme dans les Alpes maritimes. Acad. des Sciences, April, 1872. Congres de Bruxelles, 1872.

concluded from these facts that the corpses were despoiled of their flesh probably by a rapid process, for nearly all the bones preserved their natural position and were still united by their tendons and ligaments.

The later discoveries have gone to confirm those of Baoussé-Roussé. M. Hardy found at Ramonden, in the commune of Chancelade, Dordogne, a grave containing skeletons, the bones of which were reddened by oligist.[1] They belonged to a man of about fifty-five or sixty years of age, 5 feet in height, with large head, strongly dolichocephalic, face large and high, orbits the same, strong under jaw, bones relatively long, and hands and feet large. The bones were stout and thick, with pronounced muscular attachments. The forehead and the capacity of the skull resembled those of the highest and most cultivated races.[2] This skeleton of Chancelade assuredly can not fill the gap which exists between man and other zoological groups.[3]

The right temporal region presented traces of a wound measuring 63 by 50 millimeters, of which the reparation was well defined, showing that the individual had survived the wound. With the skeleton was found a bâton de commandement in reindeer horn, on which had been engraved a representation of alca impennis, a pendant with the head of a mountain sheep, and seven small personages. The industry of the Madelainien epoch is here definitely characterized.

There was a fireplace or layer of ashes and cinders 14½ inches in thickness. At the base there was a small vein, colored like red brick, with peroxide of iron. Was it by contact with this small vein that the bones had taken their red tint, or is it not more probable that the ferruginous stratum had been applied after the removal of the flesh and the exposure of the bones? M. Hardy, who studied the conditions in place, pronounced in favor of the latter hypothesis, although he did not dissimulate the possibility that during an inundation the red ocher spread itself throughout the sepulcher and may have covered the skeleton, and so each of the bones become impregnated with the color.

Abbé Tournier and M. Charles Guillon excavated a cavern which had been occupied by man after the retreat of the quaternary glaciers near the village of Rossilon, department of Ain, known by the name

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[1] Féaux and Hardy, Acad. des Sciences, December 17, 1888; Cartailhac, La France préhistorique, page 116. Oligist is a combination of iron and oxygen.

[2] M. Hardy reported the cranial capacity at 1,710 cubic centimeters. The registrations of Broca, given in Revue d'Anthropologie in 1882 by Dr. Topinard, shows rarely, but still shows, a cranial capacity clearly more. The skull from Grenelle, belonging to the epoch of chipped flint, measures 1,552 cubic centimeters; the skulls of L'Homme Mort cube 1,606 cubic centimeters; one from the grotto of Baye, 1,554 cubic centimeters; that of Vauréal, 1,533 cubic centimeters. The last three belong to the Neolithic period. I know only one prehistoric skull, that found by M. Siret, which surpasses that of Chancelade. It measures 1,716 cubic centimeters.

[3] Dr. Testut, Reserches Anthropologiques sur le squelette quarternaire de Chancelade. Bul. Soc. Anth., 1890, pages 453, 454.

of Grotto of Hoteaux.[1] The fireplaces, that is to say, the ashes, covered an area of about 71 square yards, wherein have been found numerous worked objects. The flakes, scrapers, points, and gravers of flint, poniards and needles of bone, with divers other ornaments, principally of teeth and shell perforated for suspension, and engraved bones, among which was a baton de commandement of reindeer horn ornamented with a figure of a deer. Animal bones abounded. The animals belonged in general to the fauna of the later quaternary period. Those of the reindeer (*Cervus tarandus*) are found in the lower fireplaces or ash beds; those of the deer (*Cervus elaphas*) in the upper ones.

The skeleton of a young person, 15 to 18 years of age, was extended in the midst of the débris and ashes of the oldest fireplace.[2] The bones were covered with red ocher, and were in their natural position. MM. D'Acy and Boule, whose authority in such matters will not be denied, have classed this burial with the reindeer age. The sepulchers of Baoussé-Roussé, Raymonden, and Hoteaux, afford certain proofs that the custom of reddening human bones dates from the Paleolithic period and, possibly, we ought to assign to the same period the skeletons from the Grotte des Hommes near St. More (Yonne).[3] Further excavations have, unfortunately, given results not so complete nor conclusive as those just cited. Most of the débris found was in a fragmentary state and could not be determined. We have only recognized the human teeth and phalanges incrusted in calcareous concretions. Many of these bones have been colored, probably by the action of fire.[4]

The Neolithic period has furnished other and more interesting examples. All those interested in prehistoric anthropology know of the great discoveries of Judge Piette in Mas d'Azil, 1887-88. One must admire the science with which these excavations were directed, and the care and exactness with which our learned colleague has described them.[5]

If we follow from top to bottom the different strata of the deposit on the left banks of the Arise, where it enters the cavern (Mas d'Azil), we will find successively:

A. Blocks of stone, with a varying thickness of from 2½ to 6 feet, fallen from the roof.

B. A layer of ashes 2 feet in thickness containing vast numbers of snails (*Helix nemoralis*), and the bones of deer, wild boar, ox, and goat, and associated with them, flint blades and pieces finely worked, scrapers, points and burnishers of bone, walnuts and hazelnuts, acorns, seeds of the maple, vestiges of chestnuts, nuts of the prune, cherry,

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[1] Les hommes préhistoriques dans l'Ain. Boule, Anthropologie, 1895. D'Acy, La grotte des Hoteaux, Bul. Soc. Anth., June 6, 1895.

[2] The sixth of the series.

[3] Abbé Parat, Grotte des Hommes à Saint-More. Congress of Friburg.

[4] L'Abbé Parat, June 4, 1897.

[5] Bull. Soc. Anth., April 15-July 18, 1895.

and plum. This bed. says M. Piette, corresponds to the kitchen-middings.

C. A third bed or layer, about the same thickness as the preceding, contained similar implements and bones. We note, however, the presence of harpoons, perforated and of oval form; and also the numerous colored waterworn pebbles described by M. Piette.[1] It was in this layer that he found a portion of a human skeleton, the flesh of which had been removed and the bones reddened with peroxide of iron.[2] The striæ or lines on one of the femurs made by flint blades was apparent. The skull and small bones were missing. The long bones were placed in a pile along with the lower jaw. M. Piette is of the opinion that these bones were incontestibly contemporary with the colored pebbles, and that both belonged to the commencement of the Neolithic period.

In the session of the Society of Anthropology, Paris, July 18, 1895, our colleague showed several of the human bones gathered in the grotto of Mas d'Azil. The red color remained despite recent washing.

Such is a résumé of the communications of M. Piette to the Society of Anthropology.[3] Since then he has published still another paper in the same bulletins, wherein he says: "I have met in this formation two skeletons buried after having had the flesh removed and colored red by peroxide of iron." M. Boule found in the same layer a small pile of wheat grain, but which fell to dust as soon as touched. If the deposit belonged, as M. Piette thinks, to the beginning of the Neolithic period, it will be evident that man then knew to sow, cultivate, gather, and preserve wheat.

In 1880 Professor Pigorini, at the Congress at Lisbon, pointed out analogous facts. In a Neolithic tomb, quarried in the travertine near Anagni in central Italy, there was found the facial portion of a human skull colored red with cinnabar.[4] Two arrow points associated with these remains were similarly colored. The fact that the coloring was limited to these three objects nullifies the idea of its having been done by infiltration, and establishes the fact that it was due to a voluntary act of man.[5]

A short time after this M. de Rossi discovered the sepulcher of Sgurgola[6] and M. Incoronato described the skeleton which was brought to light. The frontal portion of the skull and upper jaw were painted a deep red, due to cinnabar.[7]

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[1] L'Anthropologie, 1896, page 385.

[2] The peroxide of iron associated with manganese was found in a deposit farther up the river.

[3] Bul., 1895, page 280. Bul., 1896, page 386.

[4] Atti della R. Acad. dei Lincei, 3e série, t. IV, page 187. Bol. Paleoth. Ital., 8e année, page 48. Matériaux, 1880, page 574.

[5] This skeleton, with the objects colored as described, is placed in its tomb restored to its original condition, and installed in the Kercheriano Museum, Rome, of which Professor Pigorini is director.—T. W.

[6] Near Rome.

[7] Rev. d'Anthropologie, 1889, page 606.

The tomb was a little niche dug or quarried in the travertine. It contained, along with the skeleton, a black hand-made pottery vase, some arrowheads, a stone hammer, and a bronze lance head.

The grotto of Arene-Candide near Finale-Marina (province of Genoa) has also furnished human bones sprinkled with iron oligist, due doubtless to the red patine with which it had been covered. M. Orsi, whose long excavations in Sicily have been so fruitful, excavated this and other caves, some with a diameter of from 6 to 10 feet, wherein had been laid from twenty to twenty-five dead of the Sikeles, the ancient inhabitants. It is evident that they were deposited after the flesh had been stripped from the bones. These bones, notably in the burial place of Lapaci, near Palermo, bore traces of red paint; and if one argues as to their small number, we should not forget that the color was not fixed by baking, like that on clay or pottery, and would not be preserved in the dry caves.[1]

In the Anthropological Congress at Lisbon M. Delgado announced analogous discoveries in the grotto of Furninha, a cave of Portugal, occupied by man from the most ancient times.

Professor Pigorini, who has studied this question with his habitual skill, believes that the skeletons were placed in their tomb after the flesh had been removed, and that the bones had been sometimes painted and colored with red ochre, other times with cinnabar or oligist. Our knowledge of the state of affairs points to that as the present conclusion.

Passing from the Mediterranean to the shores of the Black Sea we find the same funeral rite.

According to a communication of M. Antonovitch to the Congress of Vilna,[2] the application of red color to human bones is frequently seen in the countries belonging to the Russian Empire. The custom exists in Bessarabia, in New Russia, in Crimea, and the Ukraine, as well as in Poland, in the provinces of Kiew and Poltava, and in Siberia. This can be shown by examples.

Professor Wasselowski found in the Crimea two tombs containing—the first six skeletons, the other but one, all of which were colored red. Professor Grembler, of Breslau, after examination, attributed them to the Cimmeriens, who inhabited the Crimea in the time of Herodotus. The Cimmeriens exposed their dead in elevated places, to the end that the birds should devour the flesh. The bones, being thus cleaned, were painted with a mineral.

Three such tombs had been found in the Crimea. They are also said to have been found in Central Asia, but have not been sufficiently studied to be quoted.[3] The kourganes (Russian burial mounds) that contained human bones are generally poor, the excavations having furnished only a few pieces of pottery, some stone implements, and

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[1] G. Serrot, Un peuple oublié, Rev. des Deux Mondes, June 1, 1897.

[2] L'Anthropologie, 1894, page 72.

[3] L'Anthropologie, 1890, page 767.

occasional traces of bronze. The skeletons were stretched upon the back, the legs slightly drawn up. M. Antonovitch does not believe that the bones were cleaned before burial. He thinks that the corpses were covered with a layer of red ocher, and the soft parts becoming decomposed, the color impregnated the bones.

This same layer of ocher has been recognized by M. Ossowski in the kourganes of Ukraine, where it attained the thickness of half a centimeter. The discovery in one of these sepulchres of an amphora of Greek origin allows a great latitude in their date. According to M. Zaborowski they might vary from the second century B. C. to the third century A. D.[1]

At Kobrynowa, not far distant from the Dnieper and the Black Sea, a kourgane (burial mound) contained twelve graves, the bodies laid near the surface, but without regularity. The graves were trough-like in form, about 5 feet in length, 28 to 30 inches in width, and 20 to 25 inches in depth. The walls were of clay, well mixed, which had been originally sustained by joists and boards, all of which, however, had disappeared centuries ago. These tombs contained fifteen skeletons, all covered with a coat of red ferruginous paint of a variable thickness.[2]

M. Lygin anticipated M. Wasselowski in the Crimea. He had excavated numerous kourganes in the districts of Yalta and Theodosia. He tells us that the skeletons were sometimes extended at full length, sometimes drawn up, but that the bones were always colored red. In most of the tombs he found, near the skeleton, a small pottery vase filled with ashes. This is confirmed by M. Vassilowitz. These represented, according to one or the other of these explorers, a funeral rite which they were unable to understand.[3] Count Brobinski tells us of the result of his excavations, in 1887, of fifty-two kourganes in the Province of Kiew. The bones, and principally the skulls, had been colored red by the use of peroxide of iron, remnants of which were found near the skeletons. The tombs dated from the Neolithic period. The funeral furniture comprised implements of stone and of reindeer horn. Among the animal bones they found those of a rodent that had disappeared from the country centuries before.[4] M. Ossowski cites similar discoveries in Poland,[5] and M. Vitkowski does the same for the valley of Kitoi, Province of Irkutsk, Eastern Siberia. The dead were buried with the arms, implements, and ornaments destined for their use in the new life on which they were then entering. The corpses had been covered with a thick layer of red ocher mixed with sand. The decomposition of the flesh had aided the impregnation of the bones.

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[1] L'Anthropologie, 1890, pages 447, 448. Zaborowski, Du Dniester à la Caspienne. Bul. Soc. Anth., February 21, 1895.

[2] Zaborowski, l. c., page 126.

[3] L'Anthropologie, 1892, page 483.

[4] Soc. Anth. de Munich, 1888. Congrès de Moscou, 1893. Bul. Soc. Anth., 1895, page 126.

[5] L'Anthropologie, 1890, p. 446.

In all the Russian Empire, from the Caspian Sea to Poland, and from the Black Sea to the confines of China, there have been found in the tombs of antiquity human bones colored red. The question is whether this was the work of man after the destruction of the flesh, or was due to impregnation from the layers of ocher or cinnabar with which the corpses were covered. There was a variation according to the time and place, and both of them are believed to have been part of the same funeral rite. The date of these burials is more difficult to fix. The kourganes were for centuries the habitual mode of burial.[1] If they were posterior to the formation of the celebrated layer of black earth, they certainly dated from a time when stone alone was employed for arms and implements in the usages of life—that is to say, at the beginning of the Neolithic period, and, we might add, subject to a certain doubt, that the more ancient of them may have dated from the close of the Paleolithic period.

M. R. von Wenzierl tells us of a series of sepulchers near Lobositz, a small town on the Elbe.[2] One of the tombs or graves in the loess contained the skeleton of a woman extended on her back, the skull, subdolichocephalic, bore very apparent traces of a dark-red coloring matter. Bracelets of shells alternating with teeth of the dog and lynx were found on the limbs. The neighboring tombs have furnished a great number of pottery vases, often richly ornamented, polished stone hatchets, and stone hammers. M. von Wenzierl is of the opinion that these tombs belong to the Neolithic period. It is necessary to add that he does not mention any coloration of human bones, other than above cited.

An analogous discovery has been reported from Brunn (Moravia), but as no details have been given I can do no more than mention it.

I know of no similar coloration to have been found in England, whether in the alluvial, the caves, or the barrows (mounds).[3]

M. Dupont has found fragments of cinnabar in the caverns of the Lesse, in Belgium, but there is nothing to show whether it was destined for the toilets of the living or intended for the decoration of the dead.

Africa furnishes but little light upon this question. The difficulty of excavation is sufficient to explain a gap which probably the future may fill. Lieutenant Hannezo, of the Algerian sharpshooters, is pursuing interesting researches at Mahedia, Tunis.[4]

Mahedia is an ancient Phenician port which, judging from the number of burials, was important enough in ancient times. The Phenician tombs were utilized by the Romans, and the graves of the vanquished became the resting places of the victors. But of this the determination is difficult. A well of square or rectangular form served as an

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[1] Zaborowski, l. c.

[2] Zeitschrift für Ethnologie, 1895, page 49; L'Anthropologie, 1896, page 211.

[3] Possibly this may be owing to the failure of the seekers to either notice or report the fact.

[4] L'Anthropologie, 1892, page 160.

entrance. Steps on the side of the wall led to a door closed by a strong slab; the slab lifted and it was easy to enter a vast chamber, the walls of which, in volcanic tufa, were in ledges. On these ledges graves were cut as in the form of troughs. The floor of the tomb was covered with fragments of bone and pottery, and the two funeral rites, burial and cremation, were not infrequently encountered in the same tomb. Two corpses, or the remains thereof, one buried in a coffin of wood, the other cremated and in a cinerary urn, would be found placed almost together on the same ledge. In two tombs the explorers found fragments of skulls colored red on the outside by oligist. The bones were so decayed that they could not be measured. The capacity of the skulls was remarkably large. The well evidently belonged to the Phenicians, for this was their habitual mode of burial. Phenician and Roman lamps were found together. Despite this fact, Dr. Collignon, after a profound study of the bones, did not hesitate to attribute them either to the Phenicians or to the Liby-Phenicians.[1] Although this conclusion is supported by excellent arguments, it does not appear absolutely demonstrated. I content myself with the opinion that the bones and ashes gathered at Mahedia are of high antiquity.

If we traverse the Atlantic we find the same facts. Dr. Ten Kate, in a recent exploration in California, gives many examples of human bones colored red by oxide of iron. These abound in the island of the Holy Spirit (Espiritu Santo). M. Diguet reports analogous facts in Lower California. Some years ago there were presented to the Society of Anthropology at Paris a mummified head from a burial place in Bolivia.[2] The forehead and back of the head were painted red. The explorers declared the rite to have extended throughout the region.

It is scarcely possible to speak of America without recalling the discoveries of Dr. Marcano in the basin of the upper Orinoco.[3] Notably, he excavated the theretofore unknown cavern d'Ibi-Iboto. He gathered skulls of twenty-four males and twenty-five females. The forehead, he said, is retreating, the orbits are strong and powerful, the glabella voluminous, with heavy projecting eyebrows. Among the masculine skulls twelve were dolichocephalic, eight were mesaticephalic, and two only brachycephalic. The average cranial capacity was 1,375 cubic centimeters. The cephalic index of the female skulls were slightly greater than 80. Several skulls, representing both sexes, were colored red. Others, on the contrary, were without any trace of color. This, therefore, was not a funeral rite common to all the members of the tribe, and we do not know the circumstances which determined its adoption. It is difficult to fix the epoch of these burials. The objects in stone made default, while the pottery was abundant. The urns, often filled with ashes, were well made, the covers surmounted by

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[1] L'Anthropologie, 1892, page 163.

[2] Bulletin Societé d'Anthropologie, 1891, p. 124.

[3] Ethnographie précolombienne du Venezuela. Region des Raudals de l'Orénoque.

animal figures—monkeys and crocodiles. The tapirs were rudely executed. Other vases had Grecian decoration, like meanders, and combinations of lines entirely strange to the Indians of South America, and, if one is permitted a comparison, it is necessary to go as far north as Yucatan to find them.

Finally, the human bones colored red are encountered frequently in the ancient sepulchers of Australia,[1] and we know that this usage was general among a great number of tribes belonging to the Papuans and the Melanesian group.

The conclusion of this part of our study is easy. The accumulated proof renders it incontestable that the funeral rite of cleaning the bones and coloring them red was practiced in different countries widely separated by sea or desert. Thucydides says the history of a people is to be sought in their tombs. In the cases cited, the tomb has responded and has thrown a clear light on the earliest origin of the rite, and at the same time on the common origin of man. A question arising from these facts is, whether they relate to religious or funeral rites. But this is comparatively of small importance. It was surely a custom of the unknown ancestors of these peoples, transmitted from generation to generation. These facts do not allow us to say that primitive life was everywhere the same, nor that if the productions of men are everywhere the same, they are always to satisfy the same needs. In the strange rite that we have recounted, a rite which has required much thought and multiplied cares and which one can believe were strange to barbarous and nomadic races, it is not a question of similar needs growing out of similar creations. In order to find a solution it is necessary to seek higher and farther; it is the identity of the genius of man in all times and in all regions that should be inquired of, and it is only there that it can be found.[2]

The mysterious Swastika sign, born in undefined regions and rapidly extended over the entire world, goes to support this hypothesis. We will seek the lessons it teaches.

For a long time the Swastika (the croix gammée, a greek cross, with arms bent to the right at right angles) has been regarded as an Aryan sign, even *the* Aryan sign par excellence. From this, or from its apparent place of origin, the name Indian (East Indian) has been given it, a name difficult at present to maintain because of the daily discoveries of its diffusion or spread among absolute strangers to the Aryan race.[3]

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[1] Cartailhac, Matériaux, 1886, p. 441.

[2] J. McGuire, Classification and Development of Primitive Implements. Amer. Anthrop., July, 1896.

[3] The literature upon the Swastika has increased in late years until it has become a library. In 1889 Count Goblet d'Alviella made a communication to the Royal Academy of Belgium entitled La croix gammée, or Swastika. It has since been enlarged and published under the title La migration des Symboles, Paris, 1891. An English translation appeared with an introduction and note by Sir G. Birdwood. Among

It appears from the researches made during late years that the origin even of the Swastika sign appears to be contested. Thus we read in the work of Count Goblet d'Alviella,[1] one of those who has best studied the question:

> "The croix grammée (Swastika) appears from prehistoric times among the peoples originating in the valley of the Danube, who have respectively colonized the Troad and the north of Italy. It extends with the products of this antique culture, on one side, among the Greeks, Etruscans, Latins, Gauls, Germans, British, and Scandinavians; on the other side, to Asia Minor, Persia, the Indies, and to China and Japan."

Such is also the opinion of M. Salomon Reinach.[2] According to him the sign of the Swastika already represented in the city of Hissarlik, prior, according to all probabilities, to the thirteenth century B. C., did not penetrate the Indies until after that period.[3] He continues that one does not find the symbol in Egypt,[4] nor in Phœnicia, nor Assyria; while, on the other hand, it is frequent in northern Italy, in the valley of the Danube, in Thrace, in Greece, and on the western shores of Asia Minor. Thence comes his conclusions that we should seek in Europe for its origin.[5]

I do not pretend to contradict this, but the first discovery of the Swastika on the hill of Hissarlik determines that this was not its place of origin. Whence came this mysterious sign which we see at Troy? To what rite does it belong? Where did it originate? These are questions we would like to have answered. In the present state of our knowledge, the question is insoluble. One point excites my interest, that is the long persistence of the Swastika and its rapid diffusion throughout such different regions. I see in this an important argument

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recent publications were those of Michael Zmigrodzki, Zur Geschichte der Swastika, Brunswick, 1890, and Thomas Wilson, The Swastika, Washington, 1896, Eminent savants in all countries have been occupied with the question of its origin and signification, but it appears, nevertheless, that it is not yet entirely cleared, for Dr. Brinton writes: "It is easy to read into barbaric scratches the thoughts of later times, and we must acknowledge that something more than the figure itself is needed to prove its symbolic sense."

[1] La migration des Symboles. Revue des deux Mondes, May 12, 1889.

[2] Le mirage oriental, L'Anthropologie, 1895.

[3] M. Reinach afterwards recognized that the Swastika mentioned by Goblet d'Alviella on certain ingots of silver in the form of dominoes, serving as money, and also those with inscriptions in honor of Açoka, belonged to the third century B. C. L'Anthropologie, 1894, page 248.

[4] Flinders Petrie has found at Naukratis certain vases ornamented with the Swastika (Third Memoir Egyptian Exploration Fund), but this pottery appears to have been imported from Caria or from Cyprus. Stuffs ornamented with the same sign have also been discovered at Panopolis, Upper Egypt, but these have been attributed to Greek workmen who were numerous at Coptos, a neighboring village where Clermont Ganneau has recently discovered a Greek inscription. Acad. des Inscriptions, March 5, 1897 (Forrer, Die Gräber und Textilfunde von Achmim Panopolis).

[5] "As for India, everything induces the belief that the Swastika was there introduced from Greece, from the Caucasus, or from Asia Minor, by routes as yet unknown." Goblet d'Alviella, La migration des symboles, page 107.

in favor of the unity of the human species. This argument should be further presented and such facts produced as justify it.

An infant, the child of a savage, might amuse himself by tracing in the sand or on stone, or on the first object that came under his hand, squares, circles, crosses, and lines, making all imaginable angles; with progress the child can reproduce the images of his mind, the scenes that strike him most, even to bizarre figures which are due only to his imagination. He will not produce a sign as complicated as the Swastika unless he has, or has had, it before his eye, or unless it shall have been transmitted to him by his ancestors. It is puerile to explain its presence in so many and such widely separated regions by the theory of the identity of the psychologic state among human races which have the same rudimentary culture.

The mysterious Swastika[1] figured on the idols and spindle whorls[2] of the ancient Dardania, on the diadem of the daughters of Priam, and on the numberless objects from the early cities on the hill of Hissarlik,[3] in the sacred temples of India, as on the bas relief of Ibriz, attributed to the Hittites,[4] on Celtic funeral urns, and on the hut urns of Albano or Corneto, a curious imitation of the habitations of the living wherein they have piously deposited the ashes of the dead.[5]

We see the Swastika on the balustrades of the porticos of the temple of Athena at Pergamos, on the sculptured ceiling of the Treasury at Orchomenos, on the vases of Milo and Athena, those of Bologna, the ancient Felsina of the Etruscans,[6] of Cære (Cervetri),[7] Cumes,[8] Cyprus,[9] and on the pottery gathered at Königswalde on the Oder; on a golden

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[1] Sometimes the arms of the Swastika turn to the left, to which Prof. Max Muller says has been given the name Suavastika. [Mr. Virchand R. Gandhi reports that while studying an ancient Sanscrit philosophy, in the British Museum library, he found the word Suavastika in connection with Swastika.—T. W.]

[2] The number of these objects casts a doubt upon their use as spindle whorls only. They may have been religious objects, a sort of ex-voto, for example.

[3] Schliemann, Ilios, figs. 1873, 1911, and others.

[4] S. Reinach, Le mirage oriental, Anthropologie, 1893.

[5] Dennis, Cities and Cemeteries of Etruria, Vol. I, page 69; Vol. II, page 457. Dennis regards these urns as anterior to the Etruscan civilization. See also Annali Del. 'Inst. Romano, 1871, pages 239, 279.

Prof. H. W. Haynes, of Boston, is of opinion that these belong to the Iron Age. (Nation, January 24, 1889.)

Professor Heilbig, Guide to the Collection of Classic Antiquities in Rome, Vol. II, page 267; Pigorini, Bulletino Ethnologia Italiana, Vol. XII, page 262; Chantre, Necropoles Halstattiennes de Italie et de l'Autriche, Materiaux, Vol. XVIII, pages 3, 4.

[6] Gozzadini, Scavi Archæologici, Pl. IV.

[7] In a tomb at Cære there has been found a golden fibula with engraved Swastika. Greffi, Monumenti di Cære, Pl. VI, No. 1.

[8] At Cumes has been found the sign (Swastika) on pottery, buried at great depth, which mark the establishment of sepulchres at the most ancient periods, beneath the tombs of the Hellenic epoch, they in turn being under those of the Roman epoch. Alex. Bertrand (Arch. celtique et gauloise, page 45).

[9] Cesnola, Cyprus, its Ancient Cities, Tombs, and Temples, pls. 44 and 47.

fibula of the Museum of the Vatican, and on a copper fibula of the Royal Museum of Copenhagen. It is encountered in the most ancient paintings of the catacombs of Rome on the tunic of the Bon Pasteur,[1] and on the archbishop's chair of St. Ambrose at Milan, where it is associated with the Latin cross and the monogram of Christ; on the ancient sacred books of Persia, as well as on the coins of Arsacides and the Sassanides; on the most ancient Christian monuments of Scotland and Ireland, often accompanied with Ogam inscriptions;[2] on the Scandinavian runic books; in the Halstattien sepulchres of San Margarether or de Rovische,[3] and in the necropolis of Koban.[4]

Schliemann found it at Tiryns and at Mycenæ;[5] Cartailhac in the citanias, those strange fortified towns of Portugal, some of which date from Neolithic times;[6] Chantre in the tombs in Caucasus,[7] and the Russian archæologists on the bronze objects from their country in the Museum of Moscow.

The Swastika has been found in France, in the tumuli (mounds) of Haguenau, engraved on the cinctures of bronze.[8] It is perpetuated on objects posterior or strange to the Roman domination. For example, on those taken in the Frankish tombs opened at Colombe (Loire-et-Cher), on a funeral stèle at the Museum of Toulouse, on a vase at the Museum of Rouen,[9] on the cinctures, Gallo-Roman or Merovingian, near La Fère.[10] The Swastika also is found on a Celto-Roman altar erected at Ambloganna, in England by a Dacian legion in honor of Zeus or Jupiter.[11] On the right and left are two circles, rayed after the fashion of stars, which Gaidoz believes to be a representation of the sun.[12] The Laplanders still engrave the Swastika on their drums intended to be used in magic rites.

The Chinese decorate with it their standards, instruments of music, and their cannon.[13] The Japanese employ it as a mark on their pottery, and the Hindus paint it in red on their houses at the beginning of the New Year, and make it with flour or sacred rice upon a table or stand

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[1] Roller, Les Catacombes de Rome, Pls. VI, X, XXXII, XXXIX, LIV, LXXXVIII, XCIV.

[2] Dr. Graves, Bishop of Limerick, Proceedings Roy. Irish Acad. Ludvig Müller reports the same.

[3] Matériaux, 1884, pages 137, 139, 466, and fig. 84.

[4] Matériaux, 1888, page 352.

[5] Mycenæ, page 193.

[6] L'Espagne et le Portugal préhistoriques, figs. 410–412. Recently M. da Veiga has recognized the Swastika in the compartments of a mosaic found in Algarve. L'Anthropologie, 1891, page 222.

[7] M. Chantre assimilates these burials to those of Villanova, Halstatt, and Bismentova in upper Italy. Materiaux, 1881, pages 164, 165.

[8] De Mortillet, Album préhistorique, pages 98, 99, 100.

[9] De Mortillet, Album préhistorique, figs. 1257, 1247.

[10] Moreau, Album de Caranada.

[11] Goblet d'Alviella, La migration des symboles, page 65.

[12] Le dieu gaulois du soleil et la migration des Symboles.

[13] The letter of Gordon to Schliemann, Ilios, page 352.

when entering a house or church as a sign of good luck or good wishes, or the occasion of a wedding or fête.[1]

The diffusion of a sign so complicated as the Swastika throughout all time and in all countries is something to be remarked, and of which we should recognize the importance. Our astonishment is doubled when we find the same symbol among the Ashantes on the western coast of Africa,[2] and then see it figured in America among the most ancient civilization of which we have any knowledge. By what migration has it crossed the Atlantic, by what migrations has it penetrated such distant countries and appeared among races of men so different? And if, as we believe, all these representations are due to an indigenous art, either Indian or African, where did they obtain their model? Our ignorance on these points is complete, and the most we can do is to give a résumé of the principal known facts.

The Swastika has been found engraved on a shell from a mound in Tennessee which contained 32 human burials,[3] on plates (five) of copper from the mounds of Chillicothe, Ohio,[4] a stone hatchet from Pemberton, N. J., on an Arkansas vase in the National Museum, on a silver ornament, the authenticity of which appears incontestable, and which was shown in 1887 at the reunion of the Association Française at Toulouse.[5]

Nordenskiöld cites numerous examples of the Swastika now engraved in straight lines, other times indicated by dots, among the cave dwellers of Mesa Verde, and the same is done by Max Müller in Yucatan and Paraguay, while other savants have found it under the huacas of Peru and among savage tribes of Brazil, where the triangular pieces of pottery, sometimes bearing the mysterious Swastika sign, often form the only dress of the women.[6]

We find it in the paintings of the Navajos[7] and on the ornaments of the Pueblo Indians, while the Sac Indians of the Southwest wear it on

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[1] It has been contended by some persons that the triskelion was an evolution from or to the Swastika. The triskelion consists of three human legs bent at the knee and joined at the thigh. It is found on the Lycian coins about 480 B. C., and thence was carried by Agathocles to Sicily. (Barclay Head, Coins of the Ancients, Pl. XXXV.) It is also found on a vase from Agrigentum. (Waring, Ceramic Art in Remote Ages, pl. 42.) Newton explains how the symbol (triskelion) is found on the arms of Sicily, and also those of the Isle of Man. (Athenæum, September, 1892.) The Duke of Athol, proprietary of the Isle of Man, sold in 1765 his right to the Crown of England, but because he had been its sovereign he kept the triskelion in his coat of arms.

[2] It is not possible to admit, says Count Goblet d'Alviella (Migration des Symboles, page 108), that this has been spontaneously conceived and executed. Of all a priori hypotheses, this is certainly the most difficult to accept.

[3] III Annual Report, Bureau of Ethnology, fig. 140.

[4] XII Annual Report, Bureau of Ethnology. Other similar discoveries have been made in Ohio.

[5] Compte rendu, I, page 284.

[6] Wilson, Swastika, Report U. S. Nat. Mus., 1894, Pl. XVIII.

[7] Wilson, l. c., Pl. XVII.

their collars and garters on occasion of their religious fêtes, although it is not possible that they should know the sense which is attached to it,[1] and the Wolpis paint it on their dance rattles.[2]

I have omitted to treat of numerous figurines ornamented with the Swastika in the hope to find an explanation of this mysterious symbol. We find it engraved on a figure of Buddha in the United States National Museum,[3] on the base of a bronze Buddha from Japan, and on a vase in the Kunsthistorische Museum of Vienna, where it figures on the breast of Apollo.[4] Astarte bears it on her arms and shoulders,[5] Adonis on his arms, a follower of Aphrodite, on her robe,[6] a centaur from Cyprus on his right shoulder.[7] In a rude representation of Apollo directing the car of the sun it is found on the wheels of the chariot.[8] A female statue in lead found at Troy wears a triangular covering over the vulva, the center of which bears a Swastika.[9] Numerous cinctures or girdles worn by women bore this same Swastika sign. Does this not indicate that it may have been regarded as an emblem of the generative forces of nature?

But we will not venture further in our researches for the signification of a sign so obscure as is the Swastika. Probably (and the figurines just mentioned give this hypothesis a semblance of truth) it was a religious emblem, an amulet consecrated by the varied superstitions of man, as is the hand with the fingers raised a survival of an ancient Chaldean symbol which is worn to-day by the Italians, as is the little pig by the Parisians.[10] Was it dedicated to the living sun; to Zeus or Baal; to Astarte or to Aphrodite; to Agni, the god of fire; or to Indra, the god of rain; or, still further, to Vishnu or to Siva, the Hindu representatives of creation and destruction? All these hypotheses are possible; more than this, all of them are probable, for the signification of Swastika has singularly varied according to the time and to tradition.[11] Those persons who in the actual state of our knowledge pretend to formulate general conclusions are sadly in error.

I approach the end of my task. By the side of the similarity of the

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[1] Wilson, l. c., Pls. XV and XVI. (Nevertheless these Indians recognize it as a sign of good luck and give it a corresponding name.—T. W.)

[2] Rev. d'Ethnographie, 1885, No. 1.

[3] Wilson, l. c., Pl. I.

[4] Goblet d'Alviella, l. c., Pl. I.

[5] Bul. Soc. d'Anth., 1888, page 676.

[6] This statuette was found in 1887 in a Greek tomb. Bul. Soc. d'Anth., 1888, page 677.

[7] Cesnola, Salaminia, page 243.

[8] Cesnola, idem.

[9] Schliemann, Ilios, fig. 226.

[10] W. W. Rockhill (Diary of a Journey through Mongolia and Tibet, 1891-92) cites the Tibetan who had a Swastika tatooed on his hand.

[11] Sewell (Indian Antiquary, July, 1881) presents innumerable hypotheses to which the Swastika has given rise. To cite but one: Mr. Cunningham, a distinguished savant, believes the Swastika to have been a monogram.

anatomic structure of man in all times and of all races, I have sought to place the similarity of his genius, as proved by the identity of his conceptions. The ossuaries which contain the remains of his predecessors, the custom of coloring his bones red after they had been denuded of their flesh, the mysterious sign to which we have given the name Swastika, and other conceptions, other almost universal creations, which it would be easy to add, all tend toward the confirmation of the knowledge given to us by the earliest arms, the first tools and implements of flint, and the most ancient pottery. We believe it impossible to misapprehend or mistake the multiplied proofs that flow from modern researches, all of which affirm with an irrefutable eloquence the unity of the human species.

RECENT RESEARCH IN EGYPT.[1]

By W. M. Flinders Petrie, D. C. L., LL. D., Ph. D.

Discoveries come so incessantly and the point of view so often changes in the ever widening interests of Egyptian history that each year puts out of date a great part of what has been written. Any general work on Egyptian history or art needs revision every few months, so thickly have new subjects and new standpoints come before us lately. We propose here to show what great changes have arisen in our ideas during the last three years, taking each age in historical order.

During all this century in which Egyptian history has been studied at first hand, it has been accepted as a sort of axiom that the beginnings of things were quite unknown. In the epitome of the history which was drawn up under the Greeks to make Egypt intelligible to the rest of the world, there were three dynasties of kings stated before the time of the great pyramid builders; and yet of those it has been commonly said that no trace remained. Hence it has been usual to pass them by with just a mention as being half fabulous, and then to begin real history with Senefern or Khufu (Cheops), the kings who stand at the beginning of the fourth dynasty, at about 4000 B. C.

The first discovery to break up this habit of thought was when the prehistoric colossal statues of Min, the god of the city of Koptos, were found in my excavations in his temple. These had carvings in relief upon them wholly different from anything known as yet in Egypt, and the circumstances pointed to their being earlier than any carvings yet found in that country. In the same temple we found also statues of sacred animals and pottery which we now know to belong to the very beginning of Egyptian history, many centuries before the pyramids, and probably about 5000 B. C. or earlier.

The next step was the finding of a new cemetery and a town of the prehistoric people, which we can now date to about 5000 B. C., within two or three centuries either way. This place lay on the opposite side of the Nile to Koptos—that is to say, about 20 miles north of Thebes. At first we were completely staggered by a class of objects entirely

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[1] Reprinted from the Sunday School Times, February 19, 1898, by permission of the publishers.

different from any yet known in Egypt. We tried to fit them into every gap in Egyptian history, but found that it was impossible to put them before 3000 B. C. Later discoveries prove that they are really as old as 5000 B. C. They show a very different civilization from that of the Egyptians, whom we already know—far less artistic, but in some respects even more skillful in mechanical taste and touch than the historical Egyptians. They built brick houses to live in, and buried their dead in small chambers sunk in the gravels of the water courses, lined with mats, and roofed over with beams. They show several points of contact with the early Mediterranean civilization, and appear to have been mainly north African tribes of European type. Their pottery, in its patterns and painting, shows designs which have survived almost unchanged unto the present day among the Kabyles of the Algerian Mountains. And one very peculiar type of pottery is found spread from Spain to Egypt, and indicates a widespread commercial intercourse at that remote day. The frequent figures upon the vases of great galley ships rowed with oars, show that shipping was well developed then, and make the evidences of trading between different countries easy to be accepted.

All of the above belongs to the age probably before 4700 B. C., which is the age given for the first historical king of Egypt by the Greek history of Manetho. A keystone of our knowledge of the civilization is the identification of the tomb of Mena, the first name in Egyptian history, the venerated founder of all the long series of hundreds of historic kings. This tomb, about 15 miles north of Thebes, was found by some Arabs, and shown to M. De Morgan, the director of the Department of Antiquities. It was a mass of about thirty chambers, built of mud brick and of earth. Each chamber contained a different class of objects, one of stone vases, one of stone dishes, one of copper tools, one of water jars, etc. And among the things are carvings of lions and vases in rock crystal and obsidian, large hard-stone vases, slate palettes for grinding paint, pottery vases, and, above all, an ivory tablet with relief carvings which show the names of the king.

Besides this, M. Amelineau has found sixteen tombs of this same general character at Abydos, which we can hardly now doubt belong to the early kings of the first three dynasties, and some four or five have been actually identified with the names of these kings in the Greek history.

So now instead of treating the first three dynasties as half fabulous and saying that Egyptian art and civilization begin full blown at 4000 B. C., we have the clear and tangible remains of much of these early kings back to 4700 B. C., and a stretch of some centuries of the prehistoric period with a varied and distinctive civilization, well known and quite different from anything later, lying before 4700 B. C. To put the earlier part of this to 5500 B. C. is certainly no stretch of probability.

Coming down now into the historical period, a cemetery at Deshasheh (about 80 miles south of Cairo) was thoroughly worked out last year, and we preserved all the skeletons and measured them. This belonged to the middle of the age of the pyramid builders, about 3500 B. C. To my great surprise, I found that two entirely different systems of treating the bodies were followed then. While many bodies were wrapped up and buried in the usual Egyptian style, nearly half of the bodies were more or less cut to pieces, and some had been elaborately dissected, stripped of all their flesh, and then wrapped up bone by bone separately in cloth. Thus there were two entirely different customs of funerals existing side by side. Yet, on measuring the bodies, there proved to be no distinct difference between them. The population was completely fused and unified as to ancestry, but had kept up entirely different customs in different levels of society.

We now pass entirely from these early times, with their fascinating insight into the beginnings of things, long before any other human history that we possess, until we reach down to what seems quite modern times in the record of Egypt, where it comes into contact with the Old Testament history. On clearing out the funeral temple of King Merenptah I found in that the upper half of a fine colossal statue of his, with all the colors still fresh upon it. As this son of Rameses the Great is generally believed to be the Pharaoh of the exodus, such a fine portrait of him is full of interest. Better even than that—I found an immense tablet of black granite over 10 feet high and 5 feet wide. It had been erected over two centuries before and brilliantly carved by an earlier king, whose temple was destroyed for materials by Merenptah. He took this splendid block and turned its face inward against the wall of his temple and carved the back of it with other scenes and long inscriptions. Most of it is occupied with the history of his vanquishing the Libyans, or North African tribes, who were then invading Egypt. But at the end he recounts his conquests in Syria, among which occurs the priceless passage: "The people of Israel are spoiled; they have no seed." This is the only trace yet found in Egypt of the existence of the Israelites, the only mention of the name, and it is several centuries earlier than the references to the Israelite and Jewish kings in the cuneiform inscriptions of Assyria. What relation this has to our biblical knowledge of the Israelites is a wide question, that has several possible answers. Without entering on all the openings, I may here state what seems to me to be the most probable connection of all the events, though I am quite aware that fresh discoveries might easily alter our views. It seems that either all the Israelites did not go into Egypt or else a part returned and lived in the north of Palestine before the exodus that we know, because we here find Merenptah defeating Israelites at about 1200 B. C. Of his conquest and of those of Rameses III in Palestine there are no traces in the biblical accounts, the absence of which indicates that the entry into Canaan took place after 1160 B. C., the last war of Rameses III. Then the period of the judges is given in a triple record—(1) of the north, (2) of the east of Jordan, (3) of Ephraim and the west; and these three accounts are quite distinct and never overlap, though the history passes in succession from one to another. Thus the whole age of Judges is but little over a century. And to this agree the priestly genealogies stretching between the tabernacle and temple periods.

Leaving now all the monumental age, we come lastly to the evidences of the Christian period, preserved in the papyri or miscellaneous waste papers left behind in the towns of the Roman times. Last winter my friends, Mr. Grenfell and Mr. Hunt, cleared out the remains of the town Behnesa, about 110 miles south of Cairo. There, amid thousands of stray papers, documents, rolls, accounts, and all the waste sweepings out of the city offices, they found two leaves which are priceless in Christian literature—the leaf of Logia, or sayings of Jesus, and the leaf of Matthew's Gospel.

The leaf of the Logia is already so widely known that it is needless for me to describe it. But I would rather call attention to some obvious conclusions to which it awakes us and which render more clear our grasp of the history of the Gospels. Every great teacher surrounded by disciples has, in the natural order of things, been commemorated first by notebooks of his sayings, compiled by his nearest followers. The "Memorabilia" of Socrates, the "Teaching" of Epictetus, the "Table Talk" of Luther, are the most obvious examples of this. And to suppose that the record of the sayings of Jesus would be less attended to, less affectionately noted down, less treasured and preserved, is against all probability. It is à priori almost certain that collections of sayings must have been made. Where, then, have they gone? A leaf of such a collection has now turned up, showing that such did exist. But we can see, when our eyes are thus opened, that a whole handbook of classified sayings has come down to us in the form of what we call the Sermon on the Mount, the fifth, sixth, and seventh chapters of Matthew's Gospel. That is really the kernel of the whole Gospel. To that has been added a narrative for the sake of those who in later years were not familiar by hearsay with all that went on, and an introduction has been put before it to explain the circumstances. But the Sermon on the Mount has all the character of a contemporary handbook in its structure and nature, and the tone of it is entirely different from all the narratives written years after the events.

Another point to which this mass of papyri opens our eyes is the importance of the scribe and account keeper. In the East, down to the present time, the village scribe does all the business and is incessantly writing for people from morning to night. It is his profession. So it was in Roman times, and far the greater part of the writings that remain were written by the scribe and tax gatherer. When such a man left his profession his habits of life could not all change; he would naturally continue to write. And amid the group of fishermen and peasants gathered about Jesus it is impossible to doubt that Matthew, the tax gatherer, ready of pen all his life, would continue his old habit and be the natural scribe of the new way. John, the only other personal disciple among the gospel writers, certainly wrote far later, when all his ways had changed from the days of his fisher life on the Sea of Galilee. Hence there appears no rival to the obvious position of Matthew as the first recorder of the sayings of his Master compiled in the Sermon on the Mount.

The leaf of Matthew's Gospel is of great interest in the literary history of the Gospels. Hitherto we have had no manuscripts older than the second great ecclesiastical settlement under Theodosius. Now we have a piece two ages earlier—before the first settlement of things under Constantine at the council of Nicæa. Here, in the middle of the third century, we find that the beginning of the Gospel, the most artificial, and probably the latest, part, the introductory genealogy and account of the Nativity was exactly in its present form. This gives us the greatest confidence that the Gospel as we have it dates from the time of the great persecutions.

Such are some of the astonishing and far-reaching results that Egypt has given us within three years past. All the most important ages of history seem to suddenly stand out with a vividness and clearness which has hardly any parallel in the history of discovery. What may not three years more show us?

DENDERA, KENA, CAIRO, EGYPT.

A STUDY FROM THE OMAHA TRIBE: THE IMPORT OF THE TOTEM.[1]

By Alice C. Fletcher,[2]
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In this study of the significance of the Omaha totem, the aim will be to set forth, as clearly as possible, first, what these Indians believed concerning their totems, and, secondly, what these totems stood for in the tribal structure.

There will be no attempt in this paper to treat the subject of totems in a world sense. The experience of many years of research within a limited area has shown the writer that close, careful studies of the various tribes and races of the two hemispheres are as yet too few to afford sufficient evidence for a final summing up from which to deduce points held in common, or the equally important lines of divergence, found in the beliefs and customs involved in the use of totems.

It is proper to call attention at the outset to a few of the perplexities of a research at first hand in a matter as recondite as that under consideration. There is the difficulty of adjusting one's own mental attitude, of preventing one's own mental atmosphere from deflecting and distorting the image of the Indian's thought. The fact that the implications of the totem are so rooted in the Indian's mentality that he is unconscious of any strangeness in them, and is unable to discuss them objectively, constitutes a grave obstacle to be overcome. Explanations of his beliefs, customs, and practices have to be sought by indirect rather than by direct methods, have to be eliminated from a tangle of contradictions and verified by the careful noting of the many little unconscious acts and sayings of the people, which let in a flood of light, revealing the Indian's mode of thought, and disclosing its underlying ideas. By these slow processes, with the analysis of his songs, rituals, and ceremonies, we can at last come upon his beliefs concerning nature and life, and it is upon these that the totem is based.

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[1] The vowels in the Indian words have the continental sound. *n* is the nasal n; p̈ = a sound between b and p; ẗ = a sound between d and t; z̈ = a sound between z and th; th = th in thither; dh = th in the; *h* = the German sound of ch as in bach; ĕ = e in met.

[2] A paper read before the section of anthropology of the American Association for the Advancement of Science, at the Detroit meeting, August, 1897.

There were two classes of totems known among the Omahas: The personal, belonging to the individual, and the social, that of societies and gentes.

The personal totem.—The question first to arise is, How did the individual obtain his totem? We learn that it was not received from an ancestor, was not the gift of any living person, but was derived through a certain rite by the man himself.

In the Legend of the Sacred Pole of the Omahas, which has been handed down from generations, and which gives a rapid history of the people from the time when "they opened their eyes and beheld the day" to the completed organization of the tribe, we are told: "The people felt themselves weak and poor. Then the old men gathered together and said: Let us make our children cry to Wa-k̇o*n*′-da. * * * So all the parents took their children, covered their faces with soft clay, and sent them forth to lonely places. * * * The old men said, You shall go forth to cry to Wa k̇o*n*′-da. * * * When on the hills you shall not ask for any particular thing * * * whatever is good, that may Wa-k̇o*n*′da give. * * * Four days and nights upon the hills the youth shall pray, crying, and when he stops, shall wipe his tears with the palms of his hands, lift his wet hands to heaven, then lay them on the earth. * * * This was the people's first appeal to Wa-ko*n*′-da."

This rite, called by the untranslatable name No*n*′-zhi*n*-zho*n*, has been observed up to the present time. When the youth had reached the age of puberty he was instructed by his parents as to what he was to do. Moistened earth was put upon his head and face, a small bow and arrows given him, and he was directed to seek a secluded spot upon the hills and there to chant the prayer which he had been taught and to lift his hands, wet with his tears, to heaven and then to lay them upon the earth; and he was to fast until at last he fell into a trance or sleep. If in his trance or dream he saw or heard anything, that thing was to become the special medium through which he could receive supernatural aid. The ordeal over, the youth returned home to partake of food and to rest. No one questioned him, and for four days he spoke but little, for if within that time he should reveal his vision it would be the same as lost to him. Afterwards he could confide it to some old man known to have had a similar manifestation, and it then became the duty of the youth to seek until he should find the animal he had seen in his trance, when he must slay it and preserve some part of it (in cases where the vision had been of no concrete form symbols were taken to represent it). This memento was ever after to be the sign of his vision, his totem, the most sacred thing he could ever possess, for by it his natural powers were to be so reenforced as to give him success as a hunter, victory as a warrior, and even the power to see into the future.

Belief concerning nature and life.—The foundation of the Indian's faith in the efficacy of the totem rested upon his belief concerning nature

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and life. This belief was complex and involved two prominent ideas: First, that all things, animate and inanimate, were permeated by a common life, and second, that this life could not be broken, but was continuous.

The common life.—The idea of a common life was in its turn complex, but its dominating force was conceived to be that which man recognized within himself as will power. This power which could make or bring to pass he named Wa-k̇o*n*′-da.

The question arises, Did the Omaha regard Wa-k̇o*n*′-da as a supreme being? There is no evidence that he did so regard the power represented by that word, nor is there any intimation that he had ever conceived of a single great ruling spirit.

Anthropomorphism.—The word Wa-k̇o*n*′-da appears to have expressed the Indian's conception of immanent life, manifest in all things. Growing out of this conception was a kind of anthropomorphism; the characteristics of man were projected upon all nature; the Rock, in the rituals, was addressed as "Aged One!" sitting with "furrowed brow" and "wrinkled loins;" the Tree lived a double life in the Indian's fancy, as did the Water, the Fire, the Winds, and the Animals. This duality can be recognized in myths, in legends, in rituals, and in the paraphernalia of ceremonies, in which there is a constant confusion of the external aspect and the anthropomorphic conception. All things were distinct from man, but in the subtle bond of a common life, embodying the idea of will, or directive energy, they were akin to him, and could lend him the aid of their special powers, even as he could help or hinder his fellow-men.

Will power.—We trace the Omaha's estimate of his own will power in the act called Wa-zhi*n*′-dhĕ-dhĕ (wa-zhi*n*, directive energy; dhĕ-dhĕ, to send), in which, through the singing of certain songs, strength could be sent to the absent warrior in the stress of battle, or thought and will be projected to help a friend win a game or a race, or even so to influence the mind of a man as to affect its receptivity of the supernatural. Aside from the individual practice of this power there was, so to speak, a collective energy exercised by the Ho*n*′-he-wa-chi society in the act of Wa-zhi*n*′-a-gdhe (wa-zhi*n*, directive energy; a-gdhe, to place upon), where the members so fixed their will upon an obnoxious person as to isolate him from all helpful relations with men and animals and leave him to die. A similar ability to aid or to injure was imputed to the elements and all natural forms. The Winds could bring health to man; the Stone insure him long life; the Elk could endow the pursued with speed, and the Hawk make the warrior sure to fall upon his enemy. But it is to be noticed that, while man's own will was believed to act directly, without intervening instrumentality upon his fellows, the supplementing of man's powers by the elements and the animals was obtainable only after an appeal to Wa-k̇o*n*′-da in the rite of the vision.

The appeal.—The prayer, which formed a part of the rite of the vision, was called Wa-k̇o*n*′-da gi-ko*n*. Gi gi-ko*n*′ is to weep from loss, as that of kindred; the prefix "gi" indicates possession. Gi-ko*n* is to weep from want of something not possessed, from conscious insufficiency, and the longing for something that could bring happiness or prosperity. The words of the prayer, "wa-k̇o*n*′-da dhe-dhu wah̲-pa′-dhi*n* a-to*n*′-he," literally rendered, are "wa-k̇o*n*′-da," "here needy I stand." (A-to*n*-he is in the third person and implies the first, as "he stands," and "I am he," a form of speech used to indicate humility.) While this prayer has been combined with many rites and acts, its inherent unity of name and words has been preserved through generations of varied experience and social development of the people.[1]

Wa-k̇o*n*′-da was a vague entity to the Omaha, but the anthropomorphic coloring was not lacking in the general conception. The prayer voiced man's ever-present consciousness of dependence, was a craving for help, and implied a belief in some mysterious power able to understand and respond to his appeal. The response came in a dream, or trance, wherein an appearance spoke to the man, thus initiating a relation between them, which was not established until the man, by his own effort, had procured a symbol of his visitant, which might be a feather of the bird, a tuft of hair from the animal, a black stone, or a translucent pebble. This memento or totem was never an object of worship; it was the man's credential, the fragment, to connect its possessor with the potentiality of the whole species represented by the form seen in his vision, and through which the man's strength was to be reenforced and disaster averted.

Basis of the efficacy of the totem.—The efficacy of the totem was based upon the Omaha's belief in the continuity of life—a continuity which not only linked the visible to the invisible and bound the living to the dead, but which kept unbroken the thread of life running through all things, making it impossible for the part and the entirety to be disassociated. Thus one man could gain power over another by obtaining a lock of his hair, which brought the man himself under his influence. In the ceremony of the first cutting of the child's hair the severed lock which was given to the Thunder god placed the life of the child in the keeping of the god. Again, when a man's death had been predicted—by one gifted to see into the future—the disaster could be averted by certain ceremonies, which included the cutting off a lock of hair from one side of the head and a bit of flesh from the arm on the opposite side of the body and casting them into the fire. By this sacrifice of a part the whole was represented, the prediction fulfilled, and the man permitted to live. From the ritual of the Corn, sung when the priest distributed the kernels to indicate that the time for planting had come, we learn that these kernels were the little portions which would draw

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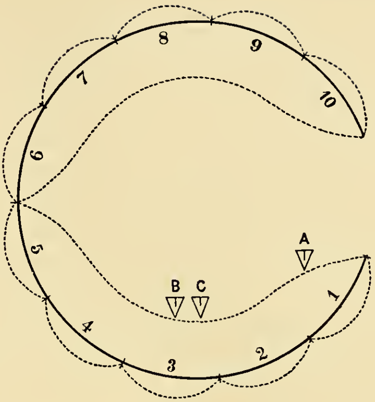
[1] This prayer can be seen on page 136, song No. 73, of vol. 1, No. 5, of the Archæological and Ethnological Papers of the Peabody Museum, Harvard University.

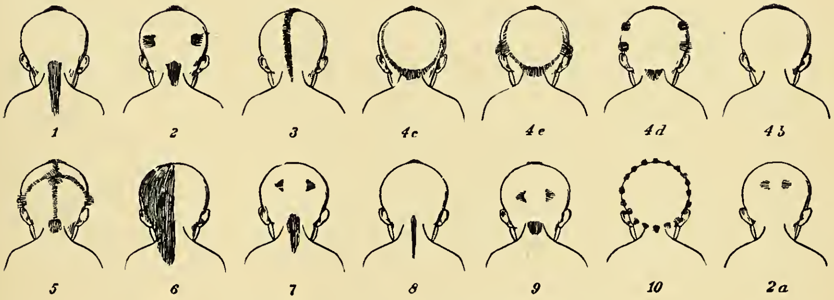


THE OMAHA TRIBAL CIRCLE, OR HU′-DHU-GA.

To*n*′-wo*n*-gdho*n*, or gentes:

1. We-zhi*n*-shtĕ.
2. I*n*-ke′-tha-bĕ.
3. Ho*n*′-ga.
4. Dha′-ẗa-da.
5. Ko*n*′-z̈e,
6. Mo*n*′-dhi*n*-k̈a-ga-*h*ĕ.
7. T̈e-thi*n*′-dĕ.
8. T̈a-p̈a′.
9. I*n*-gdhe′-zhi-dĕ.
10. I*n*-shta′-thu*n*-da.

A. The Sacred Tent of War.
B. Tent of the Sacred White Buffalo Skin
C. Tent of the Sacred Pole.



TOTEMIC CUT OF THE OMAHA BOYS' HAIR.

(The numbers refer to the gentes as marked on the tribal circle, or Hu′-dhu-ga.
The letters to subgentes.)

No. 1 is typical of the head and tail of the elk. No. 2 symbolizes the head, tail, and horns of the buffalo. No. 2*a*—the children of this subgens and those of the Ni-ni′-ba-ẗo*n* subgens of other gentes have their hair cut alike; the locks on each side of the bared crown indicate the horns of the buffalo. No. 3 represents the line of the buffalo's back as seen against the sky. No. 4b stands for the head of the bear. No. 4c figures the head, tail, and body of small birds. No. 4d, the bare head, represents the shell of the turtle; and the tufts, the head, feet, and tail of the animal. No. 4e pictures the head, wings, and tail of the eagle. No. 5 symbolizes the four points of the compass connected by cross lines; the central tuft points to the zenith. No. 6 represents the shaggy side of the wolf. No. 7 indicates the horns and tail of the buffalo. No. 8 stands for the head and tail of the deer. No. 9 shows the head, tail, and knobs of the growing horn of the buffalo calf. No. 10 symbolizes reptile teeth. The children of this gens sometimes have the hair shaved off so as to represent the hairless body of snakes.

to themselves the living corn. In the ritual sung over the Sacred Buffalo Hide prior to the hunt the same idea is present—that in the continuity of life the part is ever connected with the whole, and that the Sacred Buffalo Hide was able to bring within reach the living animal itself.

Limitation in totems.—The totem opened a means of communication between man and the various agencies of his environment, but it could not transcend the power of its particular species; consequently all totems were not equally potent. Men who saw the Bear in their visions were liable to be wounded in battle, as the bear was slow of movement, clumsy and easily trapped, although a savage fighter when brought to bay. Winged forms, such as the Eagle, having greater range of sight than the creatures which traveled upon the ground, could bestow upon the men to whom they came in the dream the gift of looking into the future and foretelling coming events. Thunder gave the ability to control the elements and the authority to conduct certain religious rites.

Despite the advantages to be derived from the possession of certain totems, the inculcations given when the youth was instructed in the rite of the vision, and taught the prayer he was to sing, forbade him to ask for any special gift or the sight of any particular thing. He was simply to wait without fear and to accept without question whatever Wa-k̇o*n*′-da might vouchsafe to send him. No man was able to choose his personal totem, but it was the general belief of the people that the powerful animals and agencies were apt to be drawn toward those who possessed natural gifts of mind and strength of will.

Nature of the totems.—The totems of the Omahas referred to animals—the Bear, the Buffalo, the Deer, the Birds, the Turtle, and Reptiles; to the Corn; to the elements—the Winds, the Earth, the Water, and Thunder. There was nothing among them which in any way represented the human family, nor was there any trace of ancestor worship. The relation between the man and his totem did not lie along the line of natural kinship, but rested upon the peculiarities in his theory of nature, in which the will and ability to bring to pass, which he was conscious of within himself, he projected upon the universe which encompassed him. The rite of the vision was a dramatization of his abstract ideas of life and nature, and the totem was the representation of the vision in a concrete form.

THE SOCIAL TOTEM AND WHAT IT STOOD FOR IN THE TRIBE.

We have thus far seen the influence of the totem upon the individual. We are now to trace it as exerted upon groups of people, in the Religious societies, in the Ṫo*n*′-wo*n*-gdho*n* or gens, and in the development and organization of the tribe.

Religious societies.—The totem's simplest form of social action was in the Religious societies, whose structure was based upon the grouping together of men who had received similar visions. Those who had seen the Bear made up the Bear society; those to whom the Thunder or Water beings had come formed the Thunder or the Pebble society. The membership came from every kinship group in the tribe. Blood relationship was ignored, the bond of union being a common right in a common vision. These brotherhoods gradually developed a classified membership with initiatory rites, rituals, and officials set apart to conduct the ceremonials.

The function of the totem in the societies was intermediate between that of the individual totem and the totem in its final social office, where it presided over an artificial structure, in which natural conditions were in part overruled and the people inevitably bound together. In some of the tribes of the linguistic group to which the Omahas belong, where the political structure of the gens is apparently weak and undeveloped, the Religious societies exist and are powerful in their organization. This fact, with other evidence which can not be detailed here owing to its complex nature, together with the similarity traceable between the rituals and ceremonies of these Religious societies and those incident to the inauguration of gentile and tribal officers, makes it seem probable that the training and experience derived from the working of these earlier societies had taught the leaders among the Omahas and their close cognates certain lessons in organization, by which they had profited during the formative period of the artificial social structure of the Ṫo*n*′-wo*n*-gdho*n* or gens.

The Ṫon′-won-gdhon.—The word Ṫo*n*′-wo*n*-gdho*n*, means a place of dwellings, where kindred dwelt together. There were ten Ṫo*n*′-wo*n*-gdho*n* u′-zhu—dominant, ruling Ṫo*n*′-wo*n*-gdho*n*, or gentes, in the Omaha tribe. These gentes practiced exogomy, and traced their descent only through the father. Each gens had its particular name, which referred directly or symbolically to its totem, which was kept in mind by the practice of tabu. There was also a set of names peculiar to each gens, all having the same reference, one of which was bestowed upon each child; an Omaha's gentile name, therefore, would at once reveal his kinship group or gens. This name was proclaimed at the time of the ceremony attendant upon the cutting of the first lock of hair. After this ceremony the child's hair was cut in a fashion to symbolize the totem of its gens, and each spring, until it was about 7 years of age, this peculiar trimming of the hair was repeated. The teaching of this object lesson, so placed before the children, was reenforced by their training in the strict observance of the special tabu of their gentes, holding ever before them the penalties for its violation of blindness, physical deformity, and disease.

There were religious rites peculiar to each gens in which the members did homage to the special power represented by the gentile totem. In these ceremonies the hereditary chiefs of the gens were the priests. It is easy to see why the totem was never forgotten—why its sign was borne through life, and at last put upon the dead, in order that they

The Last Keeper of the War Tent.

ight be at once recognized by their kindred, and not wander as they assed into the spirit world.

Office of the totem in the gens.—In the early struggle for existence the vantages accruing from a permanent kinship group, both in resisting ggression and in securing a food supply, could not fail to have been erceived; and, if the people were to become homogeneous and the actice of exogomy continue, some expedient must have been devised by which permanent groups could be maintained and kinship lines be defined. The common belief of the people, kept virile by the universal ice of the rite of the vision, furnished this expedient—a device which could be understood and accepted by all—the concrete sign of the vision, the totem of the leader, he whose abilities and prowess evinced supernatural favor, and won for his followers success and plenty.

From a study of the minutiæ of the customs and ceremonies within the gens, it is apparent that their underlying purpose was to impress upon the people the knowledge and the duties of kindred, and that one of the most important of these duties was the maintenance of the union the gens. This union of kindred we find to have been guarded by the agency of the totem. The name of the gens, the personal names of its members, and the practice of tabu—obligatory upon all persons, except the hereditary chiefs, while they were officiating in the gentile rites pertaining to the totem—indicate a common allegiance to a supernatural presence believed to preside over the gens by virtue of its relation to the common ancestor. These rites did not imply ancestor worship, but were a recognition of the special power represented by the totem. We also find that the gentile totem and its rites did not interfere with a man's freedom in seeking his personal totem, nor of his use of it when desiring help from the mysterious powers. The gentile totem gave no immediate hold upon the supernatural, as did the individual totem to its possessor; outside the rites already referred to, it served solely as a mark of kinship, and its connection with the supernatural was manifest only in its punishment of the violation of tabu. Briefly stated, the inculcation of the gentile totem was that the individual belonged to a definite kinship group, from which he could never sever himself without incurring supernatural punishment.

Social growth depended upon the establishment of distinct groups, and the one power adequate for the purpose was that which was believed to be capable of enforcing the union of the people by supernaturally inflicted penalties. The constructive influence of the totem is apparent in the unification of the Ṫo*n*′-wo*n*-gdho*n* or gens, without which the organization of the tribe would have been impossible.

The influence of the religious societies upon the gens.—In the religious societies the people were made familiar with the idea that a common vision could create a sort of brotherhood. This fraternity was recognized and expressed by the observance of rites and ceremonies—in which all the members took part—setting forth the peculiar power of the totem. The influence of this training in the religious societies is traceable in the structure of the gens, where the sign of a vision, the totem, became the symbol of a bond between the people, augmenting the natural tie of blood relationship in an exogomous group. We find this training further operative in the establishment of rites and ceremonies in honor of the gentile totem, which bore a strong resemblance to those already familiar to the people in the societies. In the gens the hereditary chief was the priest, and this centralization of authority tended to foster the political development of the gens.

Related totems.—Certain fixed habits of thought among the Omahas growing out of their theories and beliefs concerning nature and life—upon which the totem was based—present a curious mixture of abstractions and anthropomorphism, blended with practical observations of nature. Thus, in the varied experiences of disintegration and coalescing during past generations, composite gentes came into existence through the supposed affinity of totems. Out of the ten Omaha gentes, three only observe a single tabu; the other seven were composed of subgroups, called T̈o*n*′-wo*n*-gdho*n* u-zhi*ń*ga (u-zhi*ń*ga, a small part), each of which had its own special tabu, obligatory upon its own members only, and not upon the other subgroups of the gens. While there was no common totem in a composite gens, the totems of the subgroups which formed such gens had a kind of natural relation to each other; the objects they symbolized were more or less affiliated in the natural world, as, for example, in the Mo*n*′-dhi*n*-k̇a-ga-ẖĕ gens (the earth makers), where the totems of the subgroups represented the earth, the stone, and the animals that lived in holes in the ground, as the wolf.

The relation between the totems of composite gentes is not always patent; it frequently exists because of fancied resemblances, or from a subtle association growing out of conditions which have sequence in the Indian mind, although disconnected, and at variance with our own observation and reason.

The totem in the tribal organization.—The families within a gens pitched their tents in a particular order or form, which was that of a nearly complete circle, an opening being left as an entrance way into the inclosed space. This encampment was called by the untranslatable name, Hu′-dhu-ga. When the entire tribe camped together, each of the ten gentes, while still preserving its own internal order, opened its line of tents and became a segment of the greater tribal Hu′-ghu-ga, in which each gens had its fixed, unchangeable position, so that the opening of the tribal Hu′-dhu-ga was always between the same two gentes. Both these gentes were related to Thunder. That upon the right, as one entered the circle, was the I*n*-shta′-thu*n*-da—flashing eye—known as the Thunder gens or people. To a subgroup of this gens belonged the right of consecrating the child to the Thunder god; in the ceremony of cutting the first lock of hair; another subgroup kept the ritual used in filling the Sacred Tribal Pipes. On the left of the entrance camped the We′zhi*n*-shtĕ—a symbolic name probably meaning the representatives of anger. The We′-zhi*n*-shtĕ were Elk people, having in charge the Sacred Tent of War, in which the worship of Thunder, as well as all rites pertaining to war, of which Thunder was the god, took place.

It would lead too far afield to follow at great length the interrelations of the gentes; or the dominance of position and leadership in tribal rites and ceremonies conceded to certain gentes; or to indicate the scars left upon the Hu′-dhu-ga by the breaking away of groups of kindred; or the devices used to keep intact an ancient form and order. The point to be borne in mind is, that the position of the gentes in the tribe, and the interlacing of their functions, were regulated by the ascription of different powers to their totems; and that the unification and strengthening of the tribal structure, as in the unification and strengthening of the gens, depended upon the restraining fear of supernatural punishment by the totemic powers.

In this rapid review of Omaha beliefs and customs connected with the totem, many observances have not even been mentioned, and of those indicated the details have had to be omitted in order to keep strictly within the limits of our subject, but the fundamental ideas which have been briefly considered will be found to underlie all rites and ceremonies within the tribe.

Linguistic evidence as to the totem.—We turn now to the language for further evidence as to the import of the totem.

The name of the concrete sign of the vision is Wa-*h*u′-bĕ, a sacred thing. The word is applied to sacred objects other than the totem, such as the sacred pole, the sacred tents, the sacred tribal pipes, etc.

The name of a religious society always included the name of the manifestation of the vision of its members. For instance, the Bear society was called Wa-tha′be i′-dha-ĕ-dhĕ—literally rendered, the Bear with or by compassion—that is, those upon whom the Bear had compassion. I′-dha-ĕ-dhĕ implies that this compassion, this pity, was aroused by a human being making a personal appeal, either by his destitute appearance or the moving character of his supplication. Usage forbade the application of this word to any emotion excited by animal life; it could only express a feeling between man and man, or between man and the manifestation of Wa-k̇o*n*′-da. It did not represent an abstract idea, as of a virtue, but a feeling awakened by direct contact with need. In the prayer already cited as a part of the rite of the vision, the man makes a direct appeal to Wa-k̇o*n*′-da ("Wa-k̇o*n*da! here needy I stand"), and reference to this act is made in the employment of the word i′dha-ĕ-dhĕ in the term designating the Religious societies.

The name of a gens indicated its totem, or the characteristic of the group of totems in a composite gens. When the people of a gens were spoken of in reference to their totem, the word i′-ni-ka-shi-k̇i-dhĕ was used immediately following that of the totem. For instance, the Thunder people—the I*n*-shta-thu*n*-da gens—were called I*n*-gdha*n*-i′-ni-ka-shi-ki-dhĕ—i*n*-gdha*n*′, thunder; i′-ni-ka-shi-k̇i dhĕ is a composite word, meaning, they make themselves a people with—that is, with thunder they make themselves or become a people. The We′-zhi*n*-shtĕ gens, the Elk people, were called O*n*-po*n*-i′-ni-ka-shi-ki-dhĕ—o*n*-po*n* elk—with the Elk they make themselves a people. The word i′-ni-ka-shi-k̇i-dhĕ clearly indicates the constructive character of the totem in the gens.

The set of names which belonged to each gens referred to the sign or totem of a family group. These names were called ni′-ki-e, spoken by a chief, or originated by a chief. The word ni′-ki-e points to the formative period when means were being devised to transform the family into a distinct political group; it argues a central authority, a man, a chief. The individual names which he bestowed allude solely to the power behind the chief, the manifestation of his vision represented by his totem, in the favor of which he and his kindred had made themselves a people, i′-ni-ka-shi-k̇i-dhĕ.

The Osage equivalent of the Omaha word i′-ni-ka-shi-k̇i-dhĕ is zho′i-ga-ra, meaning associated with. The Otoe word used for the same purpose is ki′-gra-jhĕ, they call themselves.

The word for tribe, u-k̇i′-tĕ, when used as a verb, means to fight, to war against outside enemies, indicating that the need of mutual help impelled the various *Ṫon′*-wo*n*-gdho*n* (gentes) to band together for self-preservation; but the order of their grouping was, as we have seen, controlled by their totems.

Summary.—In the word for tribe, in the formation of the gens within the tribe, and in the rite which brought the individual into what he believed to be direct communication with Wa-k̇o*n*′-da, we trace the workings of man's consciousness of insecurity and dependence, and see his struggles to comprehend his environment, and to bring himself into helpful relations with the supernatural. And we find in this study of the Omaha totem that while the elements, the animals, and the fruits of the earth were all related to man through a common life, this relation ran along discrete lines, and that, his appeal for help once granted, relief could only be summoned by means of the Wa-*h*u′-be, the sacred object, the totem, which brought along its special line the desired supernatural aid.

It is noteworthy that the totems of individuals, as far as known, and those of the gentes, represented the same class of objects or phenomena, and, as totems could be obtained in but one way—through the rite of the vision—the totem of a gens must have come into existence in that manner, and must have represented the manifestation of an ancestor's vision, that of a man whose ability and opportunity served to make him the founder of a family, of a group of kindred who dwelt together, fought together, and learned the value of united strength.

A NEW GROUP OF STONE IMPLEMENTS FROM THE SOUTHERN SHORES OF LAKE MICHIGAN.

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INTRODUCTORY.

The first long chapter in the history of human effort and progress is written in stone, and more especially in the simple forms of implements shaped by fracture of brittle stone. Our knowledge of these earlier phases of human activity would be very meager save for the fact that the ruder peoples of to-day are found practicing similar forms of art. Observations among these peoples give us a multitude of clues as to the first steps in culture, while survivals of primitive processes in some of our modern trades have afforded no little aid. Experimental shaping, though rarely taken up seriously, has also proved a fruitful source of information, and a fuller knowledge of the properties of the varieties of stone has led to the better appreciation of the varied phenomena of the stone-shaping arts. The body of information secured through all of these sources is further enforced by recent studies of the refuse of our native American shop sites. The careful analysis of the stone-flaking art by Professor Holmes has done much to place the whole subject on a scientific footing.[1] His work, however, has dealt more especially with the great family of implements of "leaf-blade" genesis, while those shaped more directly from flakes have received less attention, and it is with the purpose of developing more fully this branch of the subject that the present study is undertaken. Both shop refuse and the designed products of the flaking art are abundantly represented in the region about the southwest shore of Lake Michigan, though the implements are not good examples of aboriginal skill in shaping because of the absence of minerals especially suited to flaking.

Before passing to the consideration of the shaped-stone products of this section the character of the ground and the manner of occurrence of the materials employed require a brief description.

The region studied.—The region studied begins in northern Cook County, Ill., and continues southward into Indiana. It is a succession

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[1] W. H. Holmes, Natural History of Flaked Stone Implements, Memoirs of the Congress of Anthropology, 1893, page 122.

of narrow sandy ridges covered with forest trees, which extend around the head of the lake. Alternating with these are marshes which in former years were frequently covered with water for a considerable period of the winter and spring, but displayed for the rest of the year a heavy growth of flags and grasses. The sharply defined ranks of oak woods regularly divided by meadows are still a characteristic of the lake shore landscape.

Cities and towns now occupy much of the area, and artificial drainage has changed the marshes for the most part into cultivated fields. The ridges were at different times in the past the beaches of Lake Michigan; three are well marked and continuous, while many intermediate ones are mere sand bars. The width of this zone of shore formations is variable. In northern Lake County, Ind., it is 7 or 8 miles; across Chicago, from the present beach to Summit, it is about 14 miles, this being the greatest width; at Evanston it is 4 miles, and between this place and Winnetka, 5 miles farther down, the beaches fade out into the present shore line. Similar areas of shore country occur at other points about the Great Lakes. One of them, a narrow detached district between the cities of Waukegan, Ill., and Kenosha, Wis., has been repeatedly visited by the writer for purposes of study. It is a mile wide and displays all the characters of the larger region about the head of the lake. It is limited on the west by an old shore line of clay bluffs.

Aboriginal remains.—Along the sand ridges old hearths and scattered relics of aboriginal life mark sites formerly occupied by camps and villages. In certain localities such remains are continuous for miles, and are traceable in wind-swept places, where the sand drifts away from the heavier objects, leaving them exposed on the surface. The objects are such as characterize similar sites throughout the United States. They consist of groups of hearthstones cracked and broken by fire; fragments of pottery of the kind made and used by the tribes of the Eastern and Middle States; net weights; the refuse of stone shaping, and implements and ornaments of stone and sometimes of copper. Articles formed of less durable substance are exceptional. An occasional gun flint or iron axe connects certain sites with the period of French and English trade. A few of the graves give like evidence in silver crosses and ornaments introduced by the Jesuits. However, on all the sites the refuse of stone flaking is constant, and represents a purely aboriginal phase of art.

In 1853 Professor Lapham recorded the abundant evidence of the former manufacture of articles of flint near Kenosha.[1] Similar observations at Evanston were made some years ago by the present writer (1884).[2] Chert refuse was collected at that time illustrating successive

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[1] Antiquities of Wisconsin. I. A. Lapham, Smithsonian Contributions to Knowledge. Vol. VII, page 6, 1855.

[2] Science. Vol. III, page 273, 1884.

stages of the flaking work. This material was placed in the museum of the Northwestern University at Evanston and was arranged in a series, beginning with the water-worn pebble from the beach and ending with the nearly completed but broken implement.[1]

The present study is limited to a heretofore unnoticed group of flaked products, which differ wholly from those of flint and chert, and much time has been given to their collection and separation from other allied phenomena.

For a part of the material here described I am indebted to Mr. F. H. Lyman, of Kenosha, who has been familiar with the sites of flaking for many years. For cooperation in collecting the refuse I wish especially to acknowledge my indebtedness to Mr. W. C. Wyman and Mr. E. F. Wyman, of Evanston.

THE TRAP FLAKE SERIES.

The district between Waukegan and Kenosha, bordering the lake, and more especially certain sites about the village of Benton, furnish a numerous series of shop products in trap rock, comprising flaked cobblestones, flakes, and specialized forms.

The total amount of this material from a number of sites was collected for examination. All forms were gathered with equal care, in order that the relative proportion of waste and designed products might be compared. The amount of such material is not great when compared with the refuse of flaking in quarries, and it is doubtful whether the use of intractable rock like trap would under any circumstances have given rise to extensive quarrying. A few well-defined shops were found near the beach where the refuse of shaping trap cobblestones is mixed with camp-site remains.

Thirty-two sites located along the sand ridges which extend northward through the marsh from Benton Station to the Wisconsin State line, a distance of 5 miles, have contributed material. All of the sites are small, and marked usually by one hearth. In places they occur at intervals of a few hundred feet. The most productive in trap refuse were chosen for collecting. In addition to material found on the camp sites, I have included in my studies local collections furnishing specialized forms obtained by others on these sites.

The material.—The trap rock occurs throughout the region in the shape of cobblestones, varying greatly in size. In color, structure, and composition it is fairly uniform when examined by ordinary methods; commonly slate blue or gray when weathered; compact, crystalline, and finely granular, giving a slightly uneven fracture. There is nothing remarkable either in its weight or hardness.

Subjected to light blows it readily yields a white powder, as it does also to friction. It is, however, the power of resistance to fracture

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[1] Report of Curator, museum, Northwestern University, 1884.

under repeated blows that especially distinguishes this rock—a quality that made it the favorite material for hammer stones throughout the region. Tough, and merely battered by ordinary hammering, it is still capable of flaking under heavy blows, and with a characteristic effect when certain conditions of the mass worked are present, large and symmetrical flakes resulting. While the fracture is not as clean cut as that produced in flaking flint and similar materials, it is nevertheless of the same general character.[1]

The flaked cobblestones.—The mixed beach gravels east of the shaping sites afford a supply of well-rounded cobblestones, those used averaging perhaps 4 inches in diameter and 1½ inches in thickness. The clay bluffs west of the sites supply the same rock in angular masses, but these are not represented in the refuse under consideration, though many were carried to the camp sites for a variety of uses. The amount of refuse of flaking varies with different sites. In some places a single piece only has been flaked, while again as many as twenty or thirty must have been worked up. Occasionally a group of four or five well-selected cobbles occurs in connection with small quantities of refuse, as if a part only of the stock gathered had been worked up. The number of flakes removed from a single stone was never great. Many blows were struck without producing flakes, the point subjected to percussion exhibiting only a gray mark or depression where the surface crumbled under the shock. Such marks commonly occur singly or in groups at the usual points of impact, but in cases extend quite around the stone.

A successful blow, one producing the desired flake, is represented by a notch in the margin of the resultant facet from which slight furrows and ridges radiate for a short distance over the face of fracture. (See Plate I.) The notch is often light gray or even white in fresh-looking specimens; these percussion marks usually occur singly, but instances of two or even three notches in one facet are met with. The facet is commonly circular or oval in outline and often of considerable size, reaching in cases 4 or 5 inches in diameter; many, as a matter of course, are small and some are irregular in contour. A single facet sometimes

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[1] I am greatly indebted to Mr. W. J. Stebbins for the exact identification of this rock. A number of flakes submitted to him for analysis gave the following result: "Under the microscope, sections of each of the rock specimens submitted were found to be diabase, in each case composed of oligoclase, augite, chlorite, calcite, and iron. That they have been subjected to much weathering and wearing from water action is evident from their outward appearance. The most noticeable character brought out by the microscope is their decomposition. The oligoclase is much clouded and altered, though a few crystals show clearly defined polysynthetic twinning. The augite is almost entirely decomposed to calcite and chlorite, and the biotite of the original diabase has all been changed to chlorite and iron. Calcite is found only in very small scattered grains. The rock is very tough, fine grained, and holocrystalline." A few of the flakes and cobblestones show a laminated structure; they comprise less than 1 per cent of the whole. This character has not played an important part in fracture except in this small proportion.

occupies nearly the whole of one surface of the stone, while two or three facets may occur within this space. Commonly where a flake has been detached, whether of large or small size, further flaking has not been carried on in the same quarter, but has been repeated at intervals of about a fourth or a third or even half the distance around the stone.

Two and three faceted stones are about equally represented and comprise the greater part of the flaked cobblestones. (Plate I.) Those with one facet are in fair proportion, while four and five faceted stones are less numerous. A limited number have more than five facets. The order of the letters *a*, *b*, *c*, *d* is the supposed order of flaking.

Halves of flaked cobblestones, mainly the result of fractures running through the shorter diameter, occur in the proportion of one to every four or five of the unbroken specimens; in extent of flaking, as in other characters, they usually correspond with the stones just described. Other broken products, in a variety of forms, represented in the refuse by split or shattered portions of the faceted stones, by large inner flakes and by fractured outer flakes, suggest that some cobblestones have been wholly reduced to flakes and splinters. (Plate I, fig. 8.) These products are limited in number, and are not easily separated from the flakes proper. An idea of their relative frequency is gained only from the occurrence of similar results in experimental flaking when attempts are made to secure from a single stone all possible outer flakes.

The flakes.—On the various sites studied the flakes are much more numerous than the other trap products, and the excess of outer flakes is particularly noticeable. In most cases the smooth convex surface of the original stone forms one entire side of the flake, while a less convex and rougher surface, showing the characteristic fracture of the rock, forms the opposite side. The two surfaces meet in a continuous edge extending around the flake. The thicker end or margin is marked by a percussion notch in the edge, like that found in the nucleus facet, or flake bed, and by similar furrows radiating from it for a short distance over the face of the fracture.

Beyond the radiating furrows, and nearer the opposite margin, the greatest convexity of the face of fracture occurs and extends across the flake in a curve having its center in the notch. The degree of convexity of this face depends upon the length of the flake; the greater the length the nearer the approach to a plane surface; a concave flake is rarely met with. The waterworn face has its greatest convexity near the margin that received the blow, to which it descends more abruptly for about one-third of the circumference; the remaining two-thirds present an edge like that of a knife, the surfaces meeting acutely. The edge does not lie wholly in one plane, but approaches nearly that position; although apparently sharp, it is uneven to the touch. The main characteristics of the flakes are easily made out in the photographs and drawings presented.

Of the irregular flakes the greater number are also from the outside of the cobblestone and owe their lack of symmetry to the overlapping of previously detached flakes. Inner flakes are for the most part rough splinters of small size, but a few large flakes appear from the inside of the stone, lacking the waterworn surface except perhaps in a limited portion of the margin. Other facts relating to the form of the flake are developed in experimental flaking, of which an account is given later on.

A study of the fracture and outline of these flakes serves to establish nothing beyond the fact that they are artificial, and their number and association with the camp site might lead to the conclusion that they are flaking waste. Close examination, however, makes it apparent that they are often more than this. They exhibit modifications, some slight while others are more decided, that point to extensive and systematic use on the one hand, and to designed modification on the other, and I wish to lay particular stress upon the fact that here, at least, flakes were made for definite purposes.

Use of unmodified flakes.—There is abundant evidence that flakes were employed as implements on all of the sites examined. Similar use of "teshoa" flakes has been noticed by Mr. H. C. Mercer,[1] but, so far as I have learned, by few other writers.

"Let us only insist," says this writer, "that here were six cases where the stone chippers' object was to make flake knives, and that at these sites one distinct process has been added to those already studied and classified as illustrative of prehistoric life in America." * * * "And can we easily help regarding this chip knife next only to the hammerstone that made it as the type of the most venerable of all stone implements?"

Modifications of the edge and surfaces, due to wear, occur in numerous cases, and several kinds of use are readily distinguished. The sharp edge is often dulled to roundness or even to flatness, and is polished or striated in a variety of ways. It is also chipped, notched, and battered.

Of 228 flakes thus modified 81 have the edge smoothed and striated by use. (Plate III, figs. 1 and 2.) Sometimes the amount of wear is such that more or less of the edge is replaced by a narrow rounded or nearly flat surface. In a few instances the striæ are oblique and show a variety of relations both to the direction of the edge and the plane in which it lies. It is commonly the lateral edges that are most worn, and, especially in broad flakes, the striæ run at right angles to the edge. The whole circumference or only a small part of it may show these characters. Flakes showing these modifications have an average long diameter of about 3 inches.

In the few instances where the striations are directed obliquely to the edge there is no other mark of use. In cases where the striations

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[1] "Pebbles chipped by modern Indians as an aid to the study of Trenton Gravel Implements." H. C. Mercer. Proc. A. A. A. S. 1892, page 287.

are directed obliquely to the plane of the edge the modification is generally in that part of the circumference near the percussion notch, where the edge is like that of a half sphere. The oblique striæ run into the rough surface of the flake, as shown in Plate III, fig. 7.

Of the 228 specimens studied, 166 have the edge altered by chipping incident to use. Few or many chips have been broken from the edge, and commonly from what was its sharpest portion. Besides the chipping, an added loss is seen, in such cases, in the rounding off and often in the polishing of angles or prominences of the edge, whether of the original flake or where it has been altered by the chipping. In some specimens the facets left by chipping are of different ages; some are little worn where they meet the edge of the flake, while others are almost obliterated. Chipping of the edge is apt to be most marked in heavy flakes, and especially in those having well-developed marks of use. Occasionally the chipping has worked a notch in the edge to which most of the wear is limited. Where the chipping is greatly developed the edge is roughly blunted, presenting a battered appearance. The chipping here referred to is the result of use. and is not designed.

The simple rounded, often polished edge, is present in some part of the circumference of 211 out of the 228 specimens examined. The rounding is often the only modification of the edge. (Plate III, fig. 6.) The abrasion in cases extends over chipped portions and irregularities.

Modifications of the face of fracture in the nature of smoothly worn and striated portions, distinct from changes incident to wear at the edge, occur in 87 flakes. (Plate III, figs. 3 and 4.) These vary from a slight reduction of the roughness of the stone to a marked grinding of the whole surface. The middle of the face is always more worn than parts nearer the circumference. Striæ are usually short, but some extend nearly the length of the flake and cross others at various angles. Sometimes a groove is worn in the face of the stone corresponding in direction with the greater number of scratches. All save 9 of the 87 specimens show striation. In these 9 only the more prominent irregularities have been touched by the grinding. In a few cases the striæ are arranged in broken circles around the center of the flake and more or less concentric. indicating a circular movement of the stone in use.

The changes thus described are readily distinguished from changes produced by natural agencies, chemical or mechanical. Wear produced by water and ice action is of a totally distinct nature, and alterations of the surface by decay operate alike on all surfaces, giving no hint of the specialization which comes about through use.

Uses illustrated.—The trap flake was used in various ways, some of which are illustrated in Plate V. As a scraper, *a*, it was held perpendicularly to the surface operated upon, or approximately so, and moved in the direction indicated by the arrow; with downward pressure the rounding and striation of the edge could be produced· an oblique movement would produce the oblique striations.[1]

In the operation represented in *b* the flake is held obliquely and the edge is pushed forward, with its fractured face directed downward toward the surface treated, as indicated by the arrow. This operation is not unlike that of planing, and accounts for modifications in which striations run over the rough surface of the flake, which was generally beneath. The divergence of the striæ from the line of the long axis of the flake coincides with the natural outward tendency of the movement, and with the theory that the operator was right-handed. Flakes modified in this way are usually characterized by a rounded upper end, making them convenient for holding in the palm of the hand. The results of incising wood and leather, and especially of incising with a sawing motion, were the same. Chopping and grinding gave characteristic results.

In the operation represented in *c*, the flake is held in much the same manner as in the first instance, but is drawn in the direction of the edge, as indicated by the arrow, making a cutting tool. The modification shown in Plate III, *f*, is thus produced, the character of the substance incised naturally having much to do with the rapidity of wear. Scraping as already shown may produce a similar effect. Held again in much the same way, repeated chopping blows would give the chipped edge with rounded angles superimposed shown in *d*.

Pressure in all cases is an important part of the operation, whether of scraping or cutting, and in the experiments it was productive of more or less chipping of the edge in addition to the abrading effects.

In *e* the flake is held by the fingers of the right hand with the rough surface directed downward. If moved in this position back and forth in a straight line or in a circle or ellipse over the surface subjected to its action, a variety of the effects observed in the specimens may be produced.

Again, if held in the palm of the left hand, *f*, the rough surface of fracture uppermost, the flake may answer the purpose of a whetstone, the object ground being moved back and forth in the direction of the arrows. A specialized flake (*b*, Plate VI) is represented as the object ground. In this manner the trap flake would take on exactly the modification that is found in certain of the flake tools.

Specialized flakes.—Many of the trap-flake tools have been designedly specialized by means of flaking, pecking, and grinding. Degrees of elaboration occur extending from a simple alteration of the serviceable edge to a more complete working out of forms characteristic of the

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[1] Materials of several kinds were operated upon by the author to test the rapidity of wear and the nature of the effect upon fresh flakes. Wood, stag horn, bone, hide, and stone were cut, chopped, and ground. The effect when the scraping acts had been carried on for some time was a smooth and polished edge, without striations, except where the material worked was itself uneven in texture.

ordinary elaborated edge tools of the vicinity. The simplest alteration is found in the resharpened edge of flakes which have seen service. The result is clearly apparent when produced by slanting blows near the edge from one side only, a result not readily duplicated by any of the acts of utilization, such as chopping, scraping, etc. The large flake (*a*, Plate IV) illustrates the effect of designed flaking, and (*b*) the flaking caused by chopping, both results being easily verified by experiment.

Another example of the flaked edge, not, however, the result of slanting blows, is found in the used scraper (Plate III, *g*). This specimen is shown principally because of its well-marked striations. A scraping edge shaped by flaking is not common, and is never as sharp or uniform as the natural edge, although more durable for rough usage.

Hatchet shapes.—A very common form of implement in this region is a notched flake (Plate VI) resembling a hatchet. The notches are worked in the lateral edge of the flake nearer its thicker end. Sometimes their presence is indicated by a slight battering of the edge only, but for the most part they are deeply flaked from both sides, the roughness of the cleft being reduced by a few bruising blows. The thinner margin of the flake is the edge of the implement and is frequently ground on the rough side, but to a moderate extent only. Three inches may be given as an average length of these objects. They were probably used chiefly for scraping and incising, as the marks of use correspond to those of the unspecialized flakes which have been so used.

The notched forms pass by degrees into others, showing more extensive alterations of the natural outline. The notches are enlarged by flaking and bruising until only the thick and thin portions of the circumference remain unchanged. This completes the elaboration of form for a large proportion of the series; but bruising of the thick end into a rounded head or poll and grinding of the thin edge have occasionally produced still more finished implements. Many examples recall the familiar chopping knife of the modern kitchen, while specimens having a greater length and relatively less width are well suited for use in the hand without hafting. Many may be described as broad hatchets with a flaring edge, and these pass into the narrower and more conventional shapes.

Celt shapes.—A more uniform reduction of the sides of the implement has produced a well-represented series that may well be classed with celts (Plate VII). Instead of concave lateral outlines, the sides are approximately straight. The edge of the tool remains unmodified, or is reduced to a straighter line and greater thickness by grinding. The opposite end is bluntly pointed or rounded.

The original percussion notch, or the lines radiating from it, can usually be discovered at the side rather than at the head of the implement, the long axis of which corresponds to the width of the flake. In cases where both the notch and lines have been obliterated, the curve of the convex surfaces, as shown in the characteristic profile of the flake, also serves to locate the point at which the initial blow fell.

Grinding has served more in this class of implements than in others as a method of shaping. It was applied to the edge of the rough surface for a space of half an inch on the average, and at such an angle as to remove a considerable portion of the thin, natural edge. At the same time it served to bring the edge more exactly into the plane of the implement.

The waterworn surface was rarely treated, as grinding, even in slight amount on the convex side, would carry the edge farther from the median plane of the implement.

The functions of these celt forms, as indicated by worn edges, were the same as those of the simple flakes. A length of 3½ inches may be taken as an average for these implements.

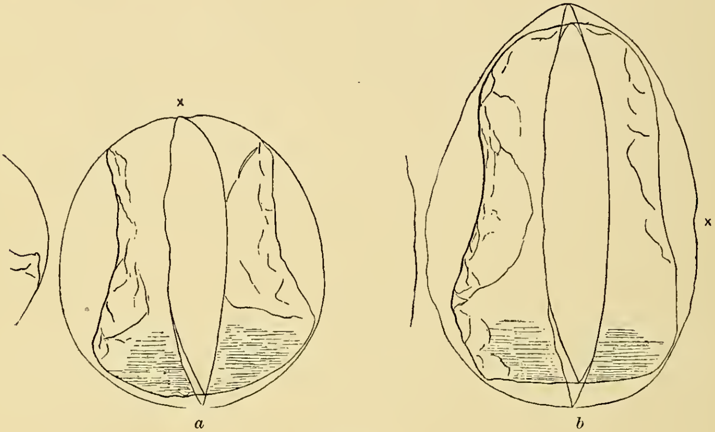

Fig. 1.

In fig. 1 two implements are sketched within the outlines of flakes that might readily have served as blanks for such specializations. The outlines have been traced from common forms of outer flakes selected as far as practicable to correspond in proportions with the included implements. In each case the section through the long axis shows the nearly unaltered flake section still present in the implement. Elaborated outlines are thus clearly seen to be specializations of the flake. The length of the flake and the length of the implement correspond in the specimen *a*, while in *b* the width of the flake has become the long axis of the implement. This is shown by the location of the percussion notch (*x*) and in the character of the curves of the section. An oblique diameter often became the long axis of the implement when the blank offered some irregularity of thickness or outline. Alignment of the edge by grinding is shown in both specimens.

Rejectage.—Rejectage which has necessarily occurred during specializing operations is not particularly abundant. The number of rejects obtained from the sites amounts to twenty-nine only, including all partly flaked forms which may be recognized as such. About twelve halves may be added to these; halves of used implements are not included.

Although limited in number, there are sufficient rejects to show that flakes have been discarded at all stages of shaping. The beginnings of designed forms appear in the single roughly flaked notch or in the roughing out of one side by slanting blows. In such examples the cause of rejection is apparently often to be found in too great a loss of the edge at a point probably designed to remain unaltered. Transverse and oblique fractures have also in some cases been the cause of failure, dividing the unfinished implement at the narrow portion or notch or detaching the smaller end of the partly flaked celt. Some rejections have resulted from attempts to bring the desired edge into proper alignment by flaking rather than by the more usual and slower method of grinding.

In Plate VIII the forms which are intermediate between the blank flake and the final product are presented as instances of partial elaboration. The simple flake is the first term of the series, its blade characters having reached their full development with one effectual flaking blow upon a waterworn stone. The unbroken cobblestone and the nucleus precede the flake in the series presented.

Designed shaping of the cobblestone.—Specimens are not wanting with which to arrange a series of flaked cobblestones corresponding to the Piny Branch rejectage series[1] and series from other shops in the United States where pebbles and bowlders have been flaked into blank blades.

Some of the cobblestones have the characters of the one-faced turtle back or reject, and additional trimming and straightening of the edge is seen in a limited number of specimens. Twenty-six rejects and eleven halves and fragments constitute the total number of specimens intermediate between the faceted stones described above and the celt forms.

Just how many of the flaked cobblestones of the refuse were designed originally to pass into some of these more advanced forms and proved to be failures before assuming their intended characters, is a question that can not be definitely answered.

The working out of the celt outline (*f*, Plate IX) may be considered to begin with *c*, Plate II, and to be well advanced in *d* and *e*, Plate IX, which are the only forms of the particular kind in all the refuse. The final form is also one of three examples appearing in local collections. It is difficult to say whether the evidences of grinding, seen in *d*, *e*, and *f*, Plate IX, are due to intentional shaping or to the effects of use. Flaking of both sides of the cobblestone is poorly represented. A fully developed leaf blade, with a sharp edge all around, is, from the

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[1] American Antiquarian, November, 1896. "Manufacture of picked-abraded stone implements," W. H. Holmes.

nature of the material, not a possible outcome of the process; and the specimens shown in *d* and *e*, Plate IX, do not have the meaning in the Benton refuse that exactly similar forms have been demonstrated to have in the Piny Branch rejectage.

Tabulated statement.—The following table will aid in comparing the various classes of trap products. It shows clearly that the flake was much more extensively used than the nucleus.

	Stones flaked.		Flakes.
Total number of cobblestones and forms derived from cobblestones.	250	Total number of flakes and forms derived from flakes.	1,051
Stones described as nuclei (85.6 per cent).	213	Outer and faceted outer flakes corresponding to the flakage of the stones described as nuclei (80.5 per cent).	847
		Irregular flakage, splinters, and spawls of small size (19.5 per cent).	204
Stones corresponding to the nuclei, bearing marks of use.	11	Large and medium-sized outer flakes bearing marks of use (21.6 per cent).	228
Use of edge	3	Use of edge	211
Use of surface	8	Use of surface	87
Flaked stones subjected to special flaking (shaping).	37	Flakes subjected to reshaping (8 per cent).	89
Rejects corresponding to early generalized stages of shaping.	27		
Rejects of celt-like outline	7	Rejects of imperfect specializations	41
Completed celt form	(?) 1	Completed specializations	48
Used edge	(?) 1	Used edge (3.4 per cent)	36
Total number of used implements	12	Total number of used implements (25 per cent).	264

The used and shaped flakes comprise nearly all the large outer flakes and about an equal number of medium size, while a very few of the smaller flakes became tools. Of the total flakage 21.6 per cent are used flakes, and 3.4 per cent specialized and used flakes. Twenty-five per cent is therefore the proportion of used implements. On the other hand, there are eleven used faceted cobblestones and one specimen that may be classed as a completed and used celt, less than 5 per cent of the total number of flaked stones. Two of the used faceted stones used for grinding have been illustrated (*e*, Plate I, and *b*, Plate II). The remaining six show very slight flaking. Of the three specimens showing use only one was flaked, and that but slightly, around the margin. Similar specimens, but unused, are tabulated with the rejectage. It appears, therefore, that nearly all of the Benton series of implements are flakes or of flake origin.

Of the 1,051 flakes found a little more than 80 per cent, including the used and the shaped flakes, are such as must have been struck from cobblestones from which not more than five flakes have been derived. The other 20 per cent are such as would originate in further shaping of the flakes or of the cobblestones.

The amount of flakage and the number of cobblestones flaked are about rightly proportioned, and as the collections represent a number of sites this relation can be relied upon as evidence that waste and products are fairly accounted for. As already stated, the greatest care was taken to collect all visible trap waste on the sites studied. Later visits to the sites have yielded little additional material. The prevalence of outer flakes seems to indicate designed flake production rather than the blocking out of blades, especially as it is associated with general use and specialization of outer flakes. There are excellent reasons for assigning nearly all flaked cobblestone nuclei to the undesigned waste, though there may be occasional instances of used and shaped nuclei.

Methods of shaping.—Thus far in the description of flaking operations direct percussion has been the method of working implied. In the chert flaking of the region trap hammer stones have been used, other stones rarely serving this function. Hundreds of sites yield trap pebbles battered in portions of the circumference and associated with chert flakes and rejects. That similar hammers may have served at Benton in the flaking of trap is possible; as elsewhere, they are present in the mixed refuse of the sites, but chert flakes and rejectage are likewise present. There is, however, a distinction to be drawn between two classes of hammer stones on the Benton sites—a distinction which might easily result from the different properties of the two materials flaked, the trap being only of medium hardness and remarkably tough; the chert hard, but brittle. From the great number of trap hammers in certain localities associated with chert refuse only, the incidental effect upon the hammer in shaping that material is shown to be nearly constant, many light or moderate blows with the softer rock upon hard, flinty angles having produced finely battered portions of its margin. Distinct from these are a small number of stones on the Benton sites which have been subjected to few and exceedingly hard blows, producing coarse and irregular battering (Plate II, *b*). In size and shape such battered stones were like those selected for flake making by the aborigines.

Experiments in flaking.—When flaking is attempted experimentally with fresh stones from the beach, blows of sufficient force to flake a trap cobblestone mar the trap hammer in precisely the same manner as the rougher specimens of the refuse have been marred. The blow necessary to flake trap smashes a chert stone into innumerable pieces which have no resemblance to flakes. Flaking of the hammer rarely occurs in working chert experimentally, and from investigations on many sites it is clear that it rarely occurred in aboriginal manufacture.

On account of the force required in flaking trap bowlders, it proved to be a difficult matter to hold the stone which was to be flaked in the hand. Both hands were numbed by repeated shocks and often contused by glancing strokes, the perpendicular blow upon the edge permitting little chance of escape for the fingers of either hand in case of a deviation in direction on the part of the hammer. The rebound likewise caused uncertainty of grasp. Thick gloves improved matters somewhat, but flaking was seldom successful, and large flakes seemed to be altogether impossible results. The failure by free-hand methods led to other experiments. The stone was made to stand on edge by sinking it partly into compact sand or sod. The blow was then directed upon one of the usual points chosen for flaking, the hammer being held lightly, so that in case of a glancing blow it might be allowed to leave the hand and strike the ground at the side of the stone operated upon. With this greater freedom of action it developed that flakes were produced, but more often from the hammer than from the stone struck. This result was made nearly constant by using the smaller or thinner stone as the hammer when a difference of weight or thickness existed. Thus, without change in the operation other than transferring the design of flaking from the stone struck to the stone which gave the blow, choosing flaking points in its circumference rather than in the circumference of the stone upon the ground, flakes were successfully reproduced.

The three sketches given in Plate X illustrate the operation and the manner of revolving the stone held in the hand as successive flakes were made.

All the forms of nuclei and flakes characteristic of the Benton trap refuse resulted from extended experiments, and more commonly from the stone employed as the hammer. It was observed that, unless fresh stones often replaced those experimented with, battering and fracture carried them beyond common effects found in the aboriginal refuse.

Specialization of the flakes was pursued far enough to show that there was little difficulty in applying the ordinary methods of shaping, the flake being held in the hand and slanting blows from a hammer stone delivered along its edge. A combination of flaking and battering soon produced the desired outlines. Grinding with the rough surface of flakes and nucleus facets soon gave the desired edges, the grinding surfaces taking on characters similar to those observed in the abrading tools shown in Plate III.

It is not claimed that the aboriginal method of flake making is certainly determined by these experiments, but the operation is not without precedent among primitive peoples. Two instances in which the stone flaked served as the hammer have been reported, one in Australia, where flakes were made,[1] and one in Tasmania,[2] in which the edge of the stone was sharpened by the removal of flakes.

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[1] Mackie's Geol. and Nat. Hist. Repertory, 1867, vol. 1, page 258. T. Baines "On the Flint Flakes in the Drift and the Manufacture of Stone Implements by the Australians." Also quotation in Evans's Ancient Stone Implements, page 25, from another account by the same author. Anthropological Review, Vol. IV, page civ.

[2] Operation cited by F. H. Cushing. "The Arrow." Anthropologist, Vol. VIII, page 328.

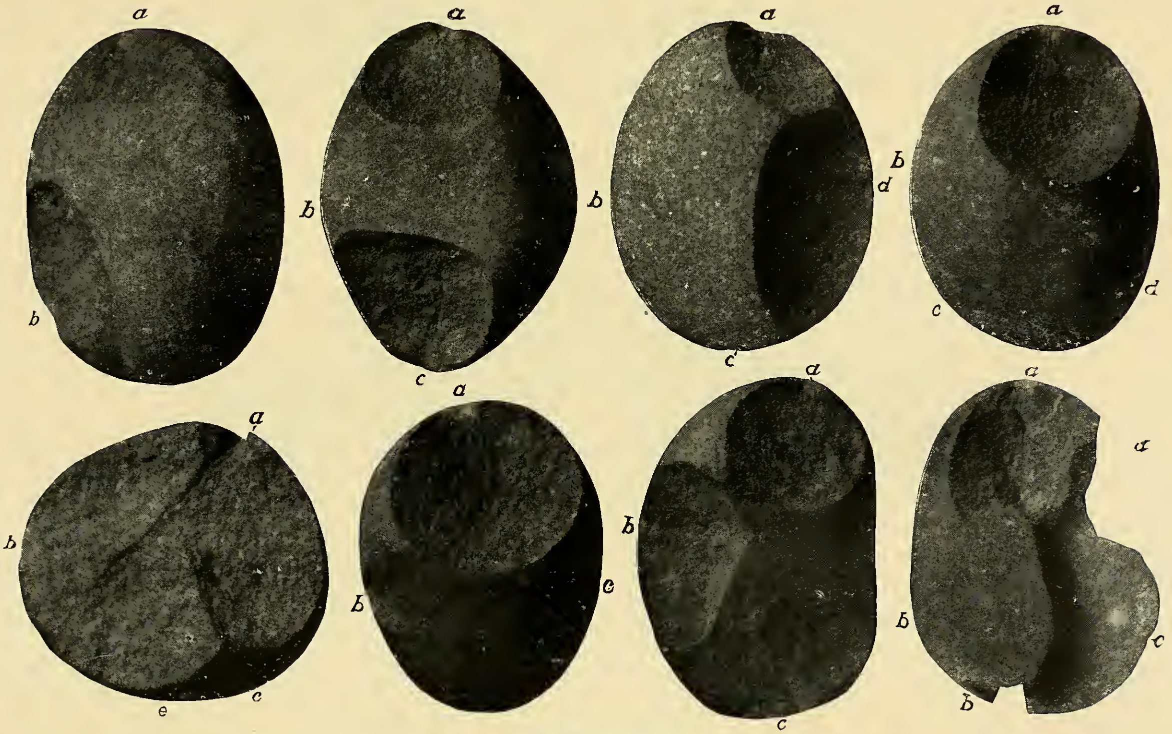
EXPLANATION OF PLATE I.

FLAKED COBBLESTONES.

(One-half actual size.)

Nos. 1–7. Two and three-faceted stones of average size. The points subjected to blows are indicated by the letters *a b c d*, the order supposed for successive strokes in each case. No. 4 presents a second facet on its opposite surface. No. 8. Partly restored five-faceted stone; stone reduced to flakage.

1. Stone subjected to blows at two points, *a* and *b*; flaking at each.
2. Stone subjected to blows at three points, *a b c*; flaking at *a* and *c*; the mark of a blow at *b* without flaking.
3. Stone subjected to blows at four points, *a b c d*; flaking at *a* and *d*; marks of blows at *b* and *c* without flaking.
4. Stone subjected to blows at four points, *a b c d*; *c* and *d* irregularly placed; flaking at *a* and *b*; flake at *b* running out on the opposite surface; marks of blows at *c* without flaking; marks of blows for some distance along the circumference at *d*, but without flaking.
5. Stone subjected to blows at three points, *a b c*; yielding flakes of fair size in each case; *c* last flake, running out on previously flaked surface; an example of the inner flakes of the refuse; the remnant of the cobblestone is worn smooth and striated on the flaked surface.
6. Stone subjected to blows at three points, flaking in each case.
7. Stone flaked at three points; irregular surface of fracture.
8. Stone subjected to blows at five points, *a′ b′* and *a b c*; flaking at all points; stone reduced to flakage; relative order of *a b c* that of the letters; *c* probably last blow, yielding a faceted inner flake like *c* of No. 5 and destroying the stone; *a′ b′* small flakes running out on the opposite surface and possibly attempts at flaking previous to *a b c*.



FLAKED COBBLESTONES FROM SHORES OF LAKE MICHIGAN.
(See explanation of plate, opposite.)


a
b

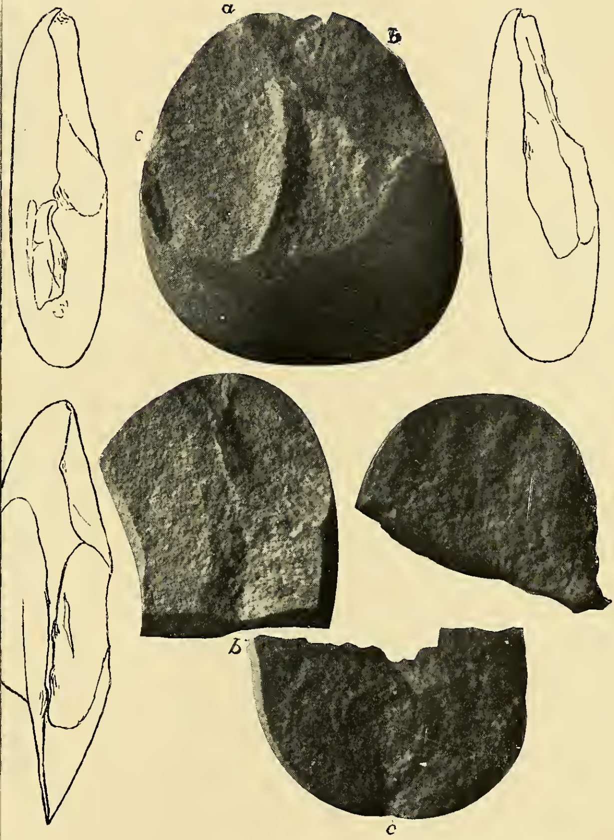
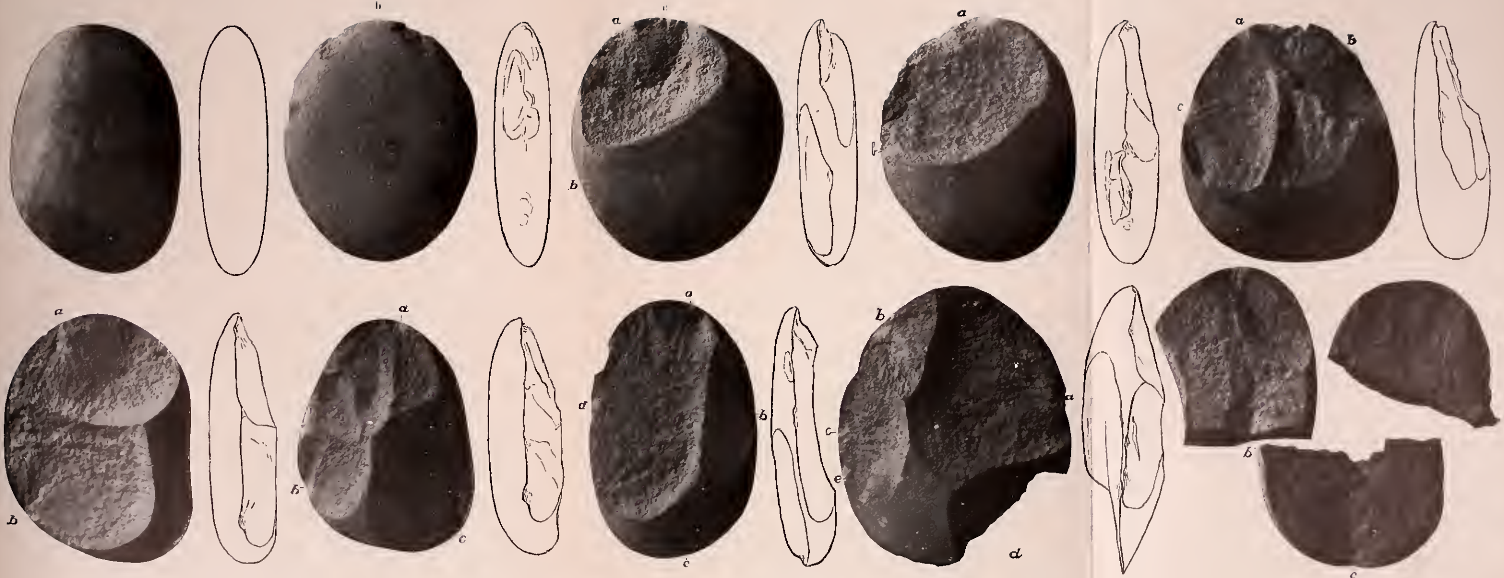

a
b
c
b
c

PLATE II.



FLAKED COBBLESTONES FROM SHORES OF LAKE MICHIGAN, SHOWING PROGRESS IN MANIPULATION.

(One-half actual size.)

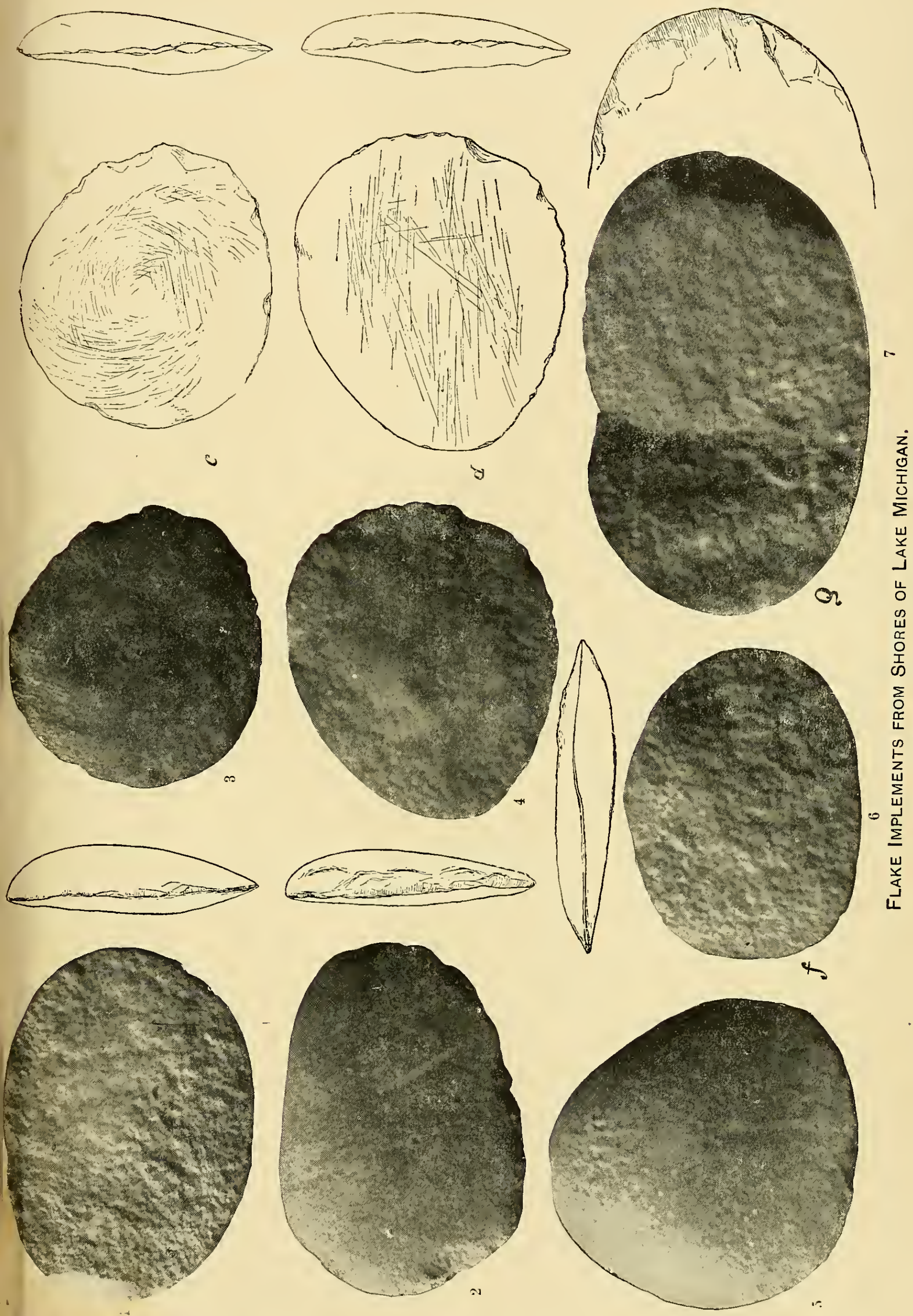
EXPLANATION OF PLATE III.

USED FLAKES.

(One-half actual size.)

Outer flakes of common sizes, showing various modifications of the edge and surface. The sketches show details more clearly.

1. Worn and striated edge throughout the greater part of the circumference, but lesser curves more worn. (Pl. V, fig. *a.*) Small portion of rough surface ground and striated.
2. Edge worn, chipped and striated for greater part of circumference; degree of wear greater than in No. 1. Surface not ground.
3. Edge worn and chipped. Surface much ground and striated in broken circles. (Right hand, Pl. V, fig. *e.*)
4. Edge worn and slightly chipped. Surface much ground and striated in straight lines across the flake. (Pl. V, fig. *f.*)
5. Worn and chipped edge. Surface not ground.
6. Edge worn without striations. (Pl. V, fig. *c.*)
7. Flaked edge (possibly designed). Wear mainly of the character of grinding, the striations running on to the surface. Right hand. (Pl. V, fig. *b.*)



FLAKE IMPLEMENTS FROM SHORES OF LAKE MICHIGAN.

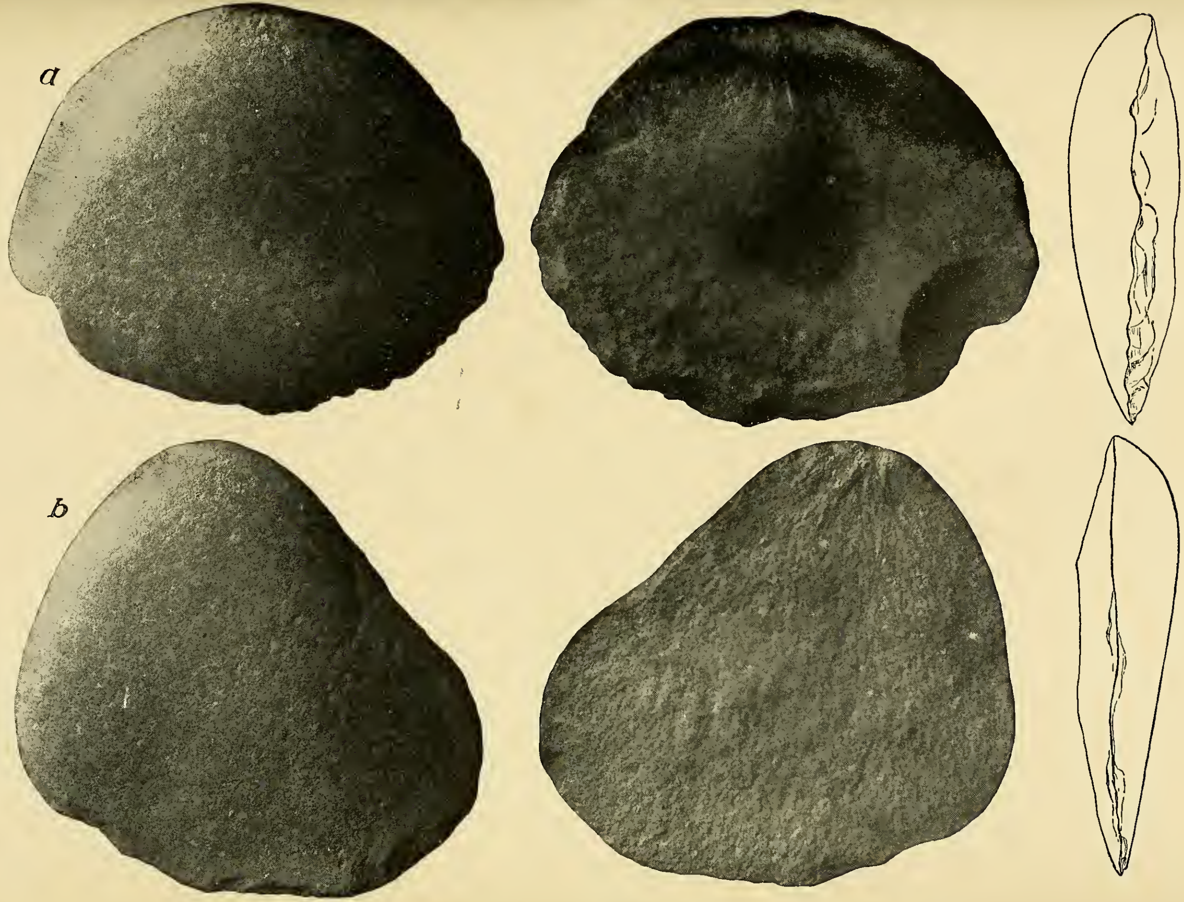
(See explanation of plate, opposite.)

EXPLANATION OF PLATE IV.

Large used Flakes.

(One-half actual size.)

1. Edge flaked (a designed alteration), much worn and blunted. Surface also much worn by grinding.
2. Edge chipped and blunted from use only. Surface worn by grinding.

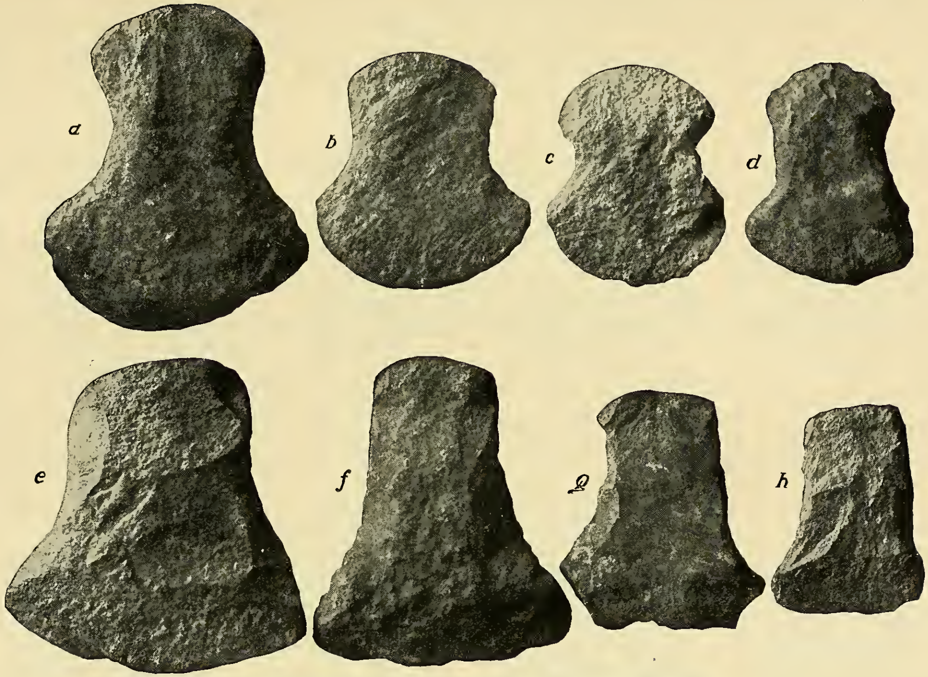


FLAKE IMPLEMENTS FROM SHORES OF LAKE MICHIGAN.

(See explanation of plate opposite.)


a
c
e

MANNER OF USING FLAKE IMPLEMENTS.



STONE IMPLEMENTS FROM SHORES OF LAKE MICHIGAN.

SPECIALIZED FLAKES, ONE-HALF ACTUAL SIZE. HATCHET-LIKE IMPLEMENTS.

Average specialized implements representing the usual degree of working, range of form, and size. In every case the reverse side is a part of the waterworn surface of the original cobblestone. With possibly two exceptions all have used edges.

STONE IMPLEMENTS FROM SHORES OF LAKE MICHIGAN.

Celts made from flakes. (One-half actual size.)

EXPLANATION OF PLATE VIII.

Progressive Stages of Shaping, Benton Flake Series.

(One-half actual size.)

The cobblestone passes into the nucleus; the larger flakes by simple alterations of the edge become specialized tools.

A. The cobblestone, natural state of material used; specimen from a Benton site, one of a group of five similar stones, some of which had been flaked and marred by blows.

B and *B'*. Nucleus. Opposite sides of a faceted cobblestone flaked from the usual points. If flaked in the order of the letters *a b c d* the first points subjected to blows yielded the small flakes *a* and *b* which represent designed products but failures to attain proper size for convenient use or further shaping, and therefore rejects of implement making.

c A successful flake, the characteristic blank for the smaller notched implements.

c' Reject; discarded after one notch had been worked in the lateral edge.

c'' Completed form having both notches worked and moderate degree of grinding at the edge.

d A successful flake; the form commonly worked into a celt.

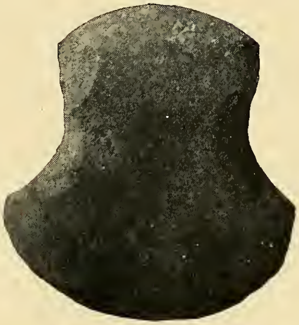
d' Reject: long margins partly worked.

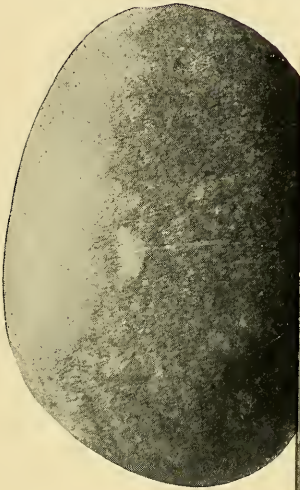
d'' Reject similar to *d'*; edge also flaked into alignment, an unusual procedure.

d''' Completed form: sides finished with moderate degree of battering; no grinding in this case, as edge is strong and nearly in proper alignment with plane of implement.

PLATE VIII.

c''





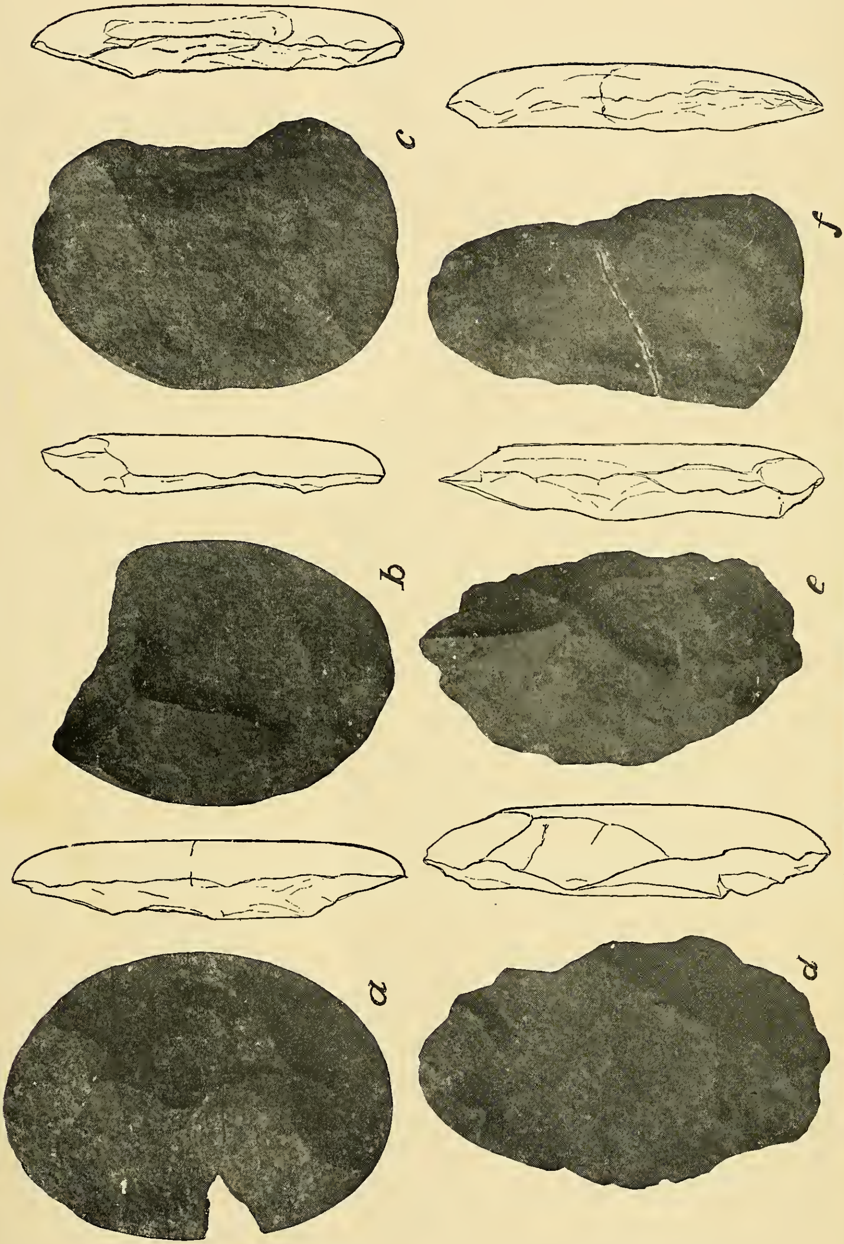
A

d''

d'''

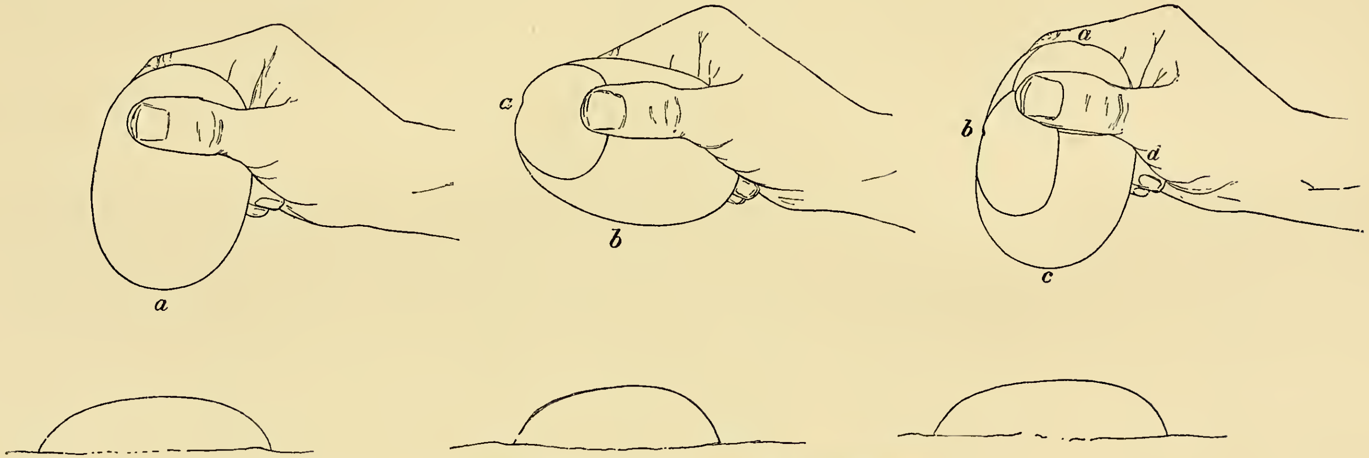
PROGRESS OF SHAPING FROM FLAKE TO HATCHET AND CELT.

See explanation of plate opposite page. (One-half actual size.)



STONE IMPLEMENTS FROM SHORES OF LAKE MICHIGAN.

(One-half actual size.)



STONE IMPLEMENTS FROM SHORES OF LAKE MICHIGAN.

Method of flaking.

A PRELIMINARY ACCOUNT OF ARCHÆOLOGICAL FIELD WORK IN ARIZONA IN 1897.[1]

By J. WALTER FEWKES.

My field work in the summer of 1897 opened with an examination of Kintiel, a ruin about 25 miles north of Navajo, Arizona. Extensive excavations were made at Four-Mile Ruin, near the town of Snow Flake, south of Holbrook, and at Pinedale, not far from the northern border of the Apache Reservation. A preliminary reconnoissance of the ruins of Pueblo Viejo, on the Upper Gila south of the White Mountains, closed the archæological work of the season.

I was accompanied throughout the summer by Dr. Walter Hough, of the National Museum, and was joined at Four-Mile Ruin by Mr. F. W. Hodge, of the Bureau of Ethnology of the Smithsonian Institution. Both of these gentlemen rendered most valuable aid. and contributed much to the success of the explorations.

I left Washington on the 17th of June, 1897, and returned on October 16th of the same year, being absent in the field about three months. Nearly a thousand ancient objects were collected from the various localities visited. As in former years, the majority of the specimens were mortuary pottery, but in addition to this material I brought back many notes, photographs, and plans of rooms and ruins. A visit to the Hopi Indians, in August, enlarged my knowledge of their Snake Dances.

OBJECT OF THE FIELD WORK IN 1897.

The primary aim of my explorations in the summer of 1897 was a continuation of the work of previous summers, viz: To follow the migration of the southern families of the Hopi, along the Little Colorado, and its tributaries, to their sources in the White Mountains. In the preceding summer my work extended as far south as Chaves Pass and Winslow, about 30 miles from the latter town. The main result of that exploration was the determination of the southern extension of a zone of Tusayan pottery. My first effort, in 1897, was to discover the breadth

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[1] While the cost of this expedition was defrayed from the appropriation of the Bureau of Ethnology of the Smithsonian Institution, this preliminary account seems to possess such popular interest that it has been deemed desirable to give it early publication here.

of this zone or to find its eastern limits. I therefore began work on a ruin in as nearly as possible the same latitude as Walpi, to determine the relation of its antiquities with those of Tusayan. It was found expedient to choose the ruin called Kintiel for this study, which choice took me into a region with archæological characters different from those on which I had formerly worked.

While the line of demarcation between the Hopi and Zuñi zones of ruins is not a sharp one, and ancient pottery of the one grades imperceptibly into that of the other, as might be expected, we can say that there are two parallel sections of country extending north and south, which are achæologically different, separated by the boundary of New Mexico and Arizona. In the Arizona section there is a marked architectural similarity of the ruins from the Colorado River to the Gila, and a difference from the Zuñi belt which extends across New Mexico from San Juan River, southward. The variations in the ceramic art of these two sections, in prehistoric times, were not as marked as that between the modern Hopi and Zuñi, the survivors of the inhabitants of these ancient pueblos. The plan of work in 1897, was to gather more data than were hitherto available bearing on these differences.

KINTIEL AND NEIGHBORING RUINS.

A ruin which is designated on many recent maps by the name Pueblo Grande is situated about 25 miles north of Navajo, one of the small stations on the Atlantic and Pacific Railroad. This ruin is called by the Navajo Indians, Kintiel (broad house), and is one of the largest in New Mexico or Arizona. It is claimed by Zuñi traditionalists as a former home of some of their ancestral clans.

Up to within a few years Pueblo Grande, or Kintiel, was one of the best preserved ruins in the southwest, and one of the largest west of those in Chaco Canyon. Its walls were at that time almost entire, but since then most of them have been torn down to yield building material for the house of a trader, who has built his store in about the center of the old pueblo, and dug out the old spring in the middle of the ruin. Little attention has been paid to this ruin by archæologists and no attempt at elaborate excavations has been made.

The destruction of the walls of Kintiel rendered it impossible for me to add anything to the description of them given[1] by Mr. V. Mindeleff, and my attention was therefore mainly turned, as soon as I saw the mutilation of the ruin, to the character of the material that could be brought to light by excavations. A cemetery was soon discovered on the eastern side of the northern section, through which extensive trenches were dug extending from the outer walls of the ruin to the periphery of the mounds. It was found that the same custom of burying the dead just outside the outer wall of the pueblo prevailed here as at Sikyatki, Homolobi, and the ruin at Chevlon Fork, but that the

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[1] Eighth Annual Report of the Bureau of American Ethnology.


C
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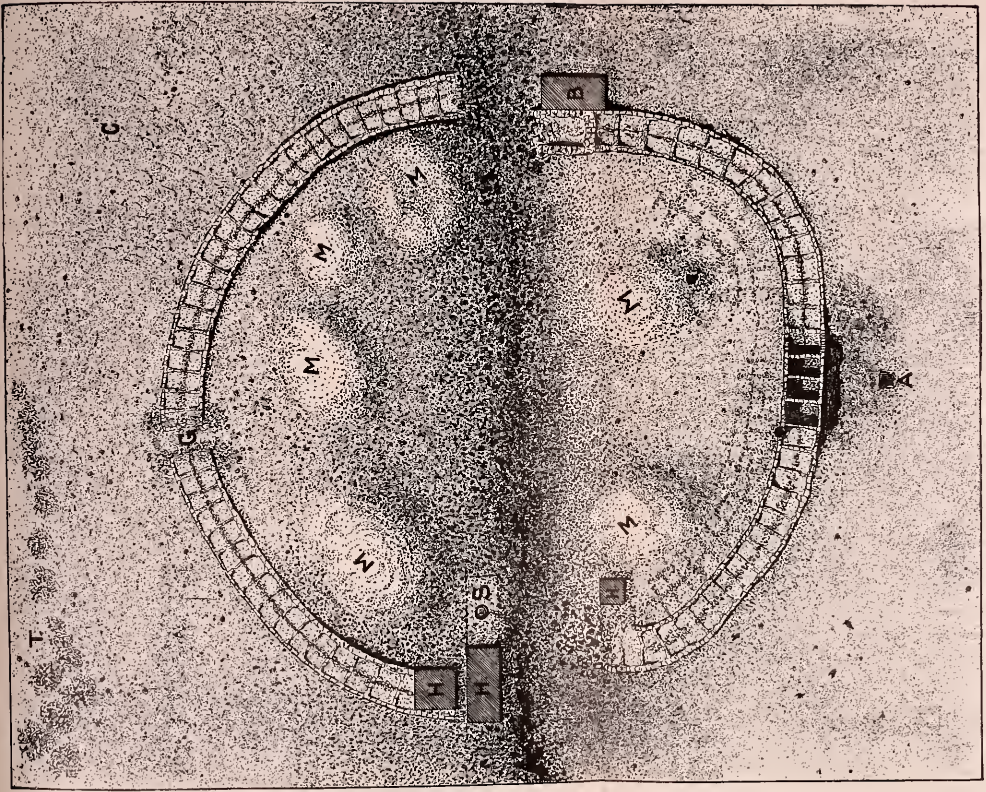


GROUND PLAN OF KINTIEL.

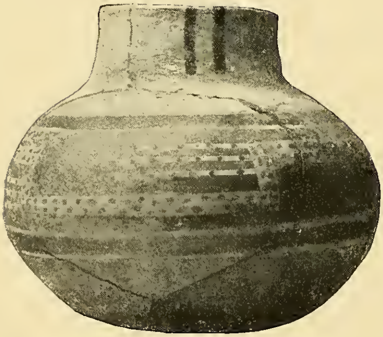
PLATE II.

CUP.
No. 176811.

COILED VASE.
No. 176910.

GLOBULAR BOWL.
No. 176912.

TWO-HANDLED BOWL.
No. 176936.



DECORATED VASE.
No. 177234.

ORNAMENTED ROUGH BOWL.
No. 177148.

KINTIEL AND FOUR-MILE-RUIN.

number of interments at this point were by no means so large as the size of the pueblo led me to suspect they should have been.

The pottery exhumed from the cemeteries at Kintiel was very different from that of any ruin yet explored near the Hopi pueblos, and resembled very closely in character and ornamentation, ceramic ware from the ruins near Zuñi, which evidence substantiates the claim of the Zuñis, that Kintiel was constructed by one of their clans or phratries. A large proportion of the pottery belongs to the group called white, with black decoration, and there were representatives of the group of red ware, but the yellow ware, so characteristic of Tusayan, appears to be almost wholly absent. The pottery, as a rule, is coarsely made, and the decoration, as a general thing, simple. It is more closely related to the Cliff House pottery of the San Juan River on the north and Tulerosa Ruins to the south than to that of any ancient ruin from Moki southward.

One needs but make a superficial comparison of the pottery of Kintiel with that of Sikyatki, or other Hopi ruins, to see how inferior it is in character and decoration. This is true in more general comparisons, for, with the exception of white pottery with black-line decoration, the ancient Hopi ceramic ware is superior to any other in our Southwest. The accompanying plates indicate the general character of the pictography of Kintiel.

A cairn, filled with stones and rock concretions, was found southeast of the ruin Kintiel, not far from its outer wall. Of the many strange shapes which the stones in this cairn assumed the most striking were three stone fetiches, carved to resemble animals. There were likewise a number of stone balls, some spherical, others of ovoid and cylindrical shapes. A small, flat, oval stone disk was likewise found in this collection.

In digging out the spring, from which no doubt the ancient people of Kintiel derived their water supply, the resident trader found stone steps still used in descending to the water. The spring had been walled in, and was protected from hostile invasion by its position inside the old pueblo.

Taylor Spring is situated about 7 miles north of Navajo,[1] on the road from that station to Kintiel, and near it there are two small ruins. These ancient habitations are both reduced to low mounds, one of which is circular, the other rectangular; but neither shows any signs of walls standing above the surface of the ground. The former mound lies a few hundred feet north of the spring; the latter about a quarter of a mile off the road in the same direction. Both of these ruins are promising places for study, but I was obliged to pass them by with only a cursory examination.

There are extensive ruins near Tanner Spring, about 20 miles west

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[1] In the immediate neighborhood of Navajo Springs there are several ruins, some of considerable size.

of Kintiel, which, from reports, I should judge would repay systematic study.

A ruin known by a Navajo name, Kinna Zinde, was photographed and described several years ago by Mr. Victor Mindeleff. It lies a few miles north of Kintiel and, I should judge, is now in about the same condition as at the time of his visit. A hurried examination showed that it is a small, well-preserved ruin, walls of which still rise two stories high with many of the beams still in situ. An old ladder,[1] or rather the poles of one, with notches in which the rungs were formerly tied, has not been moved from its old place. From the scarcity of pottery shards about the foundation of Kinna Zinde, it would appear that the house had not been inhabited many years when it was abandoned. The base of the walls is slightly elevated above the neighboring plain, and when seen from one side, as shown in Mindeleff's photograph, the ruin resembles a round tower. The walls on the opposite side from which his view was taken are rectangular, and there are partitions in the inclosure as if this part was once divided into several rooms. There are evidences of at least two stories in the inclosures of the walls. The character of the stone masonry is the same as that of Kintiel, and there is no reason to doubt that it was built about the same time.

RUINS SOUTH OF HOLBROOK NEAR PINEDALE AND SNOWFLAKE.

A short distance east of Holbrook, Arizona, the Little Colorado River takes a southeasterly course, and near this turn it is joined by a tributary, which rises in the foothills of the White Mountains almost due south of the junction. I was informed that there were several large ruins on the banks of this tributary, which led me to extend my archæological researches into this part of Arizona.

There are evidences of former occupation of the country immediately about Holbrook, and several unbroken specimens of pottery and many fragments of the finest ware have been collected within a mile of the town. I was, however, unsuccessful in a search for ruins of any large pueblo in the neighborhood, and it would appear either that the aboriginal dwellings in this locality were very small, temporary halting places of migratory clans, or constructed of such perishable material that it has left no commensurate mounds to mark their former presence.

Evidences of occupation by Indians were obtained from the top of Woodruff Butte, that prominent mountain of pyramidal shape which is visible from Holbrook in a southerly direction. Two beautiful stone pendants, in the form of birds, and an ancient pipe of white stone, with numerous turquoise and shell beads, were found on top of this butte by Mr. Webb, of Pinedale. Other objects of Indian manufacture are

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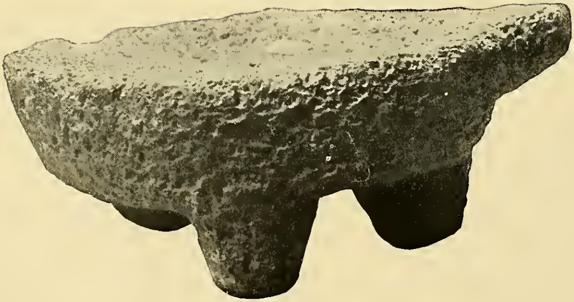
[1] This ladder was mentioned by Mr. F. H. Cushing, who visited Kinna Zinde several years ago.

STONE BIRD.
No. 177898.

STONE BIRD.
No. 177898.

COPPER BELL.
No. 177804.



METATE.
No. 177471.

BONE IMPLEMENT.
No. 176964.

KINTIEL AND FOUR-MILE-RUIN.

known to me, from the summit of Woodruff Butte, but I have never visited the elevation, which is conspicuous for miles around.

Small ruins in the petrified forest[1] were examined, and yielded a few interesting objects.

PINEDALE RUINS.

The settlement called Pinedale is situated, as its name signifies, among the pine trees, in a small valley high up on the sides of the foothills near the northern edge of the Apache Reservation. It lies on one of the tributaries of the Little Colorado River, near its source, and is approached by a rough road which branches from the Fort Apache military road a few miles south of the settlement called Taylor.

The largest of the ruins near Pinedale is situated just beyond the town near an unfinished (1897) stone schoolhouse. It is divided into two parts, which are separated by the road. The portion on the right has a rectangular form, composed of a single series of square rooms inclosing a plaza. Tall pine trees of great age grow from the soil, covering the floors of several of these rooms, which indicate the great antiquity of the buildings. The part of the ruin to the left of the road, on the opposite side from the schoolhouse, is more concentrated than the former, and was apparently more densely populated when inhabited. It also has a square form, but with smaller inclosed plaza than the other ruin. Our excavations were confined mostly to the eastern slope of this section, where many ancient burials were found and a few fine objects, mostly mortuary pottery, exhumed. About fifty specimens were collected at the Pinedale ruins, but none of these were different from the objects from other ruins along the Little Colorado Valley. Among the smaller specimens may be mentioned numerous objects made of bone, as awls, bodkins, scrapers, gouges, and tubes, fragments of deer antlers, metates, grinding stones, and spear points. The color of the pottery was as a rule either red with black decorations, or so-called black and white, and was closely related to that found at Four-Mile Ruin,[2] lower down the valley.

FOUR-MILE RUIN.

The ruined pueblo which I have called Four-Mile Ruin is situated 4 miles from Snow Flake and about 2 miles west of Taylor, Arizona. It lies on Pinedale Creek, a small tributary of Silver Creek, which flows into the Little Colorado. Before my visit nothing had been written about this important ruin, as it was known only to a few people in the neighboring towns. My attention was called to it by Mr. Brimhall, of Taylor, but I had little hope of finding much of interest there on my first

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[1] A large ruin not far from Adamana, on the road to the forest, has attracted the attention of several tourists.

[2] Several ruins were discovered near Stott's ranch a few miles west of Pinedale. A few specimens were taken from the largest of these, in front of the cabin.

visit. The collections made at Pinedale not having been of sufficient size to enable me to draw conclusions regarding the arts of the people who formerly lived on this tributary of the Little Colorado, I camped at Four-Mile Ruin on my return from the mountains, intending to make a brief reconnoissance. While the results were at first very discouraging, I was later rewarded with one of the most instructive collections yet obtained in Arizona.

The site of the Four-Mile Ruin is a bluff overlooking the creek. This elevation rises abruptly on the west side, but slopes more gradually to the east to the surrounding plain. On the north and south sides the rise is also abrupt from plains which were probably originally the farms of the aboriginal inhabitants. The ancient cemeteries were discovered on these two sides, but had apparently been washed away on the eastern section.

Four-Mile Ruin was evidently formerly a pueblo of considerable size, certainly as large as modern Walpi, which contains about 300 inhabitants. It was similar in form to that at Chevlon Fork, but was somewhat more extended.[1] There was no evidence of a central plaza in this pueblo, which was a solid mass of houses, probably of pyramidal shape, crowning a natural hillock and extending along a crest of a hill. Excavations at the highest point of the ruin showed the existence there of three well-marked stories, possibly a fourth, and we were able to lay bare a floor of hardened clay. Some of the wooden floor joists, very much decayed, projected from the side walls, and cooking vessels in fragments were thrown out from a level about 6 feet below the surface of the ground.

Early in my work at Four-Mile Ruin it was discovered that the creek had encroached on one side of the mounds, and good evidence was found that a portion of the fields north of the ancient pueblo had been washed away by frequent freshets in the rainy season. These inroads were particularly clearly marked in the northern and western sections. An examination of the bank of the creek north of the bluff, about 200 yards from the ruin, revealed a human bone projecting from the bank, which led to extensive excavations at that point. This was the site of an extensive cemetery, which continued under the bed of the stream, or, rather, the stream in freshets had washed away the superficial soil, so that the graves were very shallow, often only a few inches deep. This part of the river bed, which was only occasionally flooded, was stony and scantily covered with a growth of sage bush. The roots of these plants grew from skeletons, near which were accompanying bowls and other offerings which are customarily deposited with the dead. The base of the northern slope of the ruin at the level of the plain was found to be another burial place, containing many skeletons, deeply

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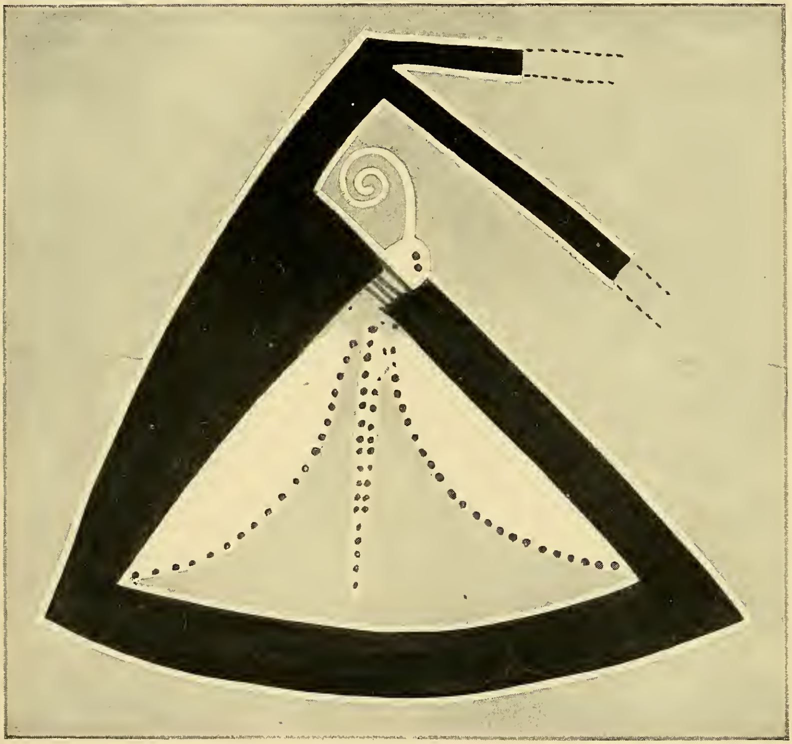
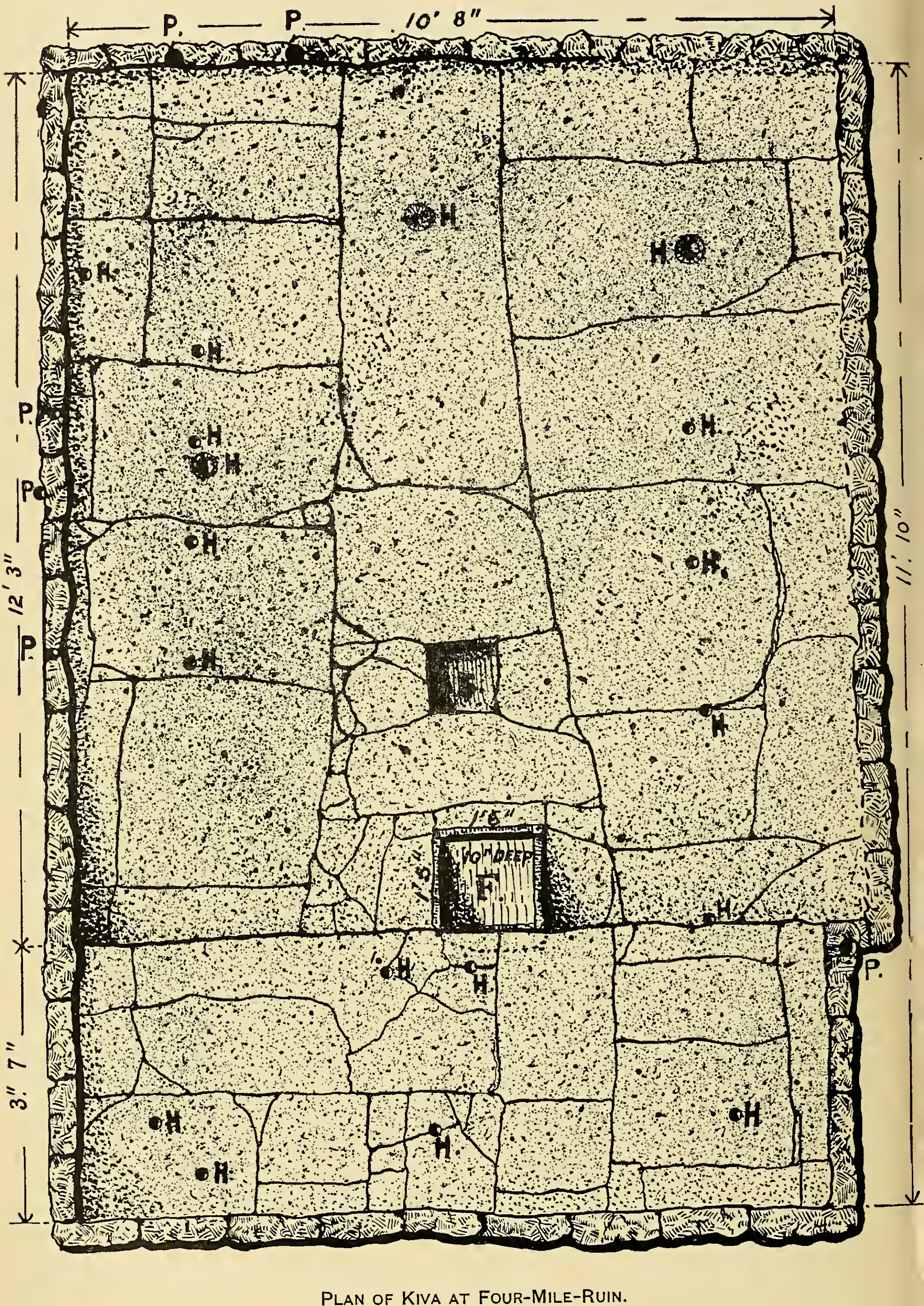
[1] For want of space several small ruins discovered near Four-Mile Ruin are not described. The mounds of an ancient pueblo near Shumway are of considerable size.

SMALL SAUCER.
No. 177131.



BUTTERFLY DESIGN—FOOD BOWL.
No. 177110 .

FOUR-MILE-RUIN.



PLAN OF KIVA AT FOUR-MILE-RUIN.

covered by soil. This burial place yielded a far greater number of skeletons and mortuary objects than the south cemetery, possibly because more extended excavations were made at that point than at others where the interments were as a rule deeper.

One of the most interesting rooms excavated at Four-Mile Ruin was situated on the north side, where a deep gulley had been furrowed in the soil by the rains. This room was excavated to the floor, by which important architectural details of construction, in common with a Hopi kiva, were revealed. It measured 10 feet 5 inches by 16 feet 10 inches, and at one end there was a banquette 4 feet wide raised 1 foot 7 inches above the main floor. The upright walls were plastered with adobe, still smooth, and blackened with soot, and at intervals very much decayed vertical logs were found still in place. The situation of these upright supports reminded me of a similar architectural feature in the construction of the buildings of Pueblo Viejo, which are described later in this report.

The floor was paved with large flat stone slabs, nicely fitted together. Several of these pavement stones were perforated by one or more perfectly round holes, sometimes beveled, an inch or two in diameter. When these perforated stones were first found I was so struck with their resemblance to the symbolic orifices in the floors of Tusayan ceremonial rooms that I regarded them as such, but their number and distribution, shown in the accompanying plan, would throw doubt on this identification. A rectangular shrine made of four stone slabs set on their edges occupied a position between the banquette and the fireplace. This receptacle measured 1 foot 5 inches by 1 foot 6 inches, and was about 10 inches deep. Its position and contents suggested the shrine in the middle of the floor of the kiva at Awatobi, which I have elsewhere[1] described.

We found in our excavations at Four-Mile Ruin worked stones which gave rise to considerable speculation. The first specimen was dug out of the ground, a few feet from the surface, in one of the cemeteries. Later another specimen was exposed in a deep gulley in the side of the mounds just above the site of the burial place, and in excavating the room, which has been called a kiva, five other specimens were discovered on the floor of the room. From the relative positions of the seven objects there is no doubt that the two specimens found outside the room had been washed out of the chamber, and that originally all seven were part of the furniture of the room. These forms were half ovoid in form, with one side smooth, the curved surface carefully trimmed into shape. They were of hard stone, and the work of pecking them into shape had evidently been an arduous one. Their sizes were uniformly about that of a ceremonial helmet, slightly larger than the human head. On one side near the rounded pole there was a shallow

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[1] Smithsonian Report for 1895.

pit or depression about the diameter of the thumb, which was present in every specimen.

It was customary for the ancient Hopi to use ceremonial masks made of agave fiber which was plaited on "forms." Some of the oldest helmets now in use were made in this way, and Mr. H. R. Voth has secured one or more modern specimens of these "forms" from Oraibi.

On my visit to Oraibi I examined the helmet forms collected by Mr. Voth, and found them to be of the same material and shape as the ovoid stones from the Four-Mile Ruin, save that the Oraibi specimens were somewhat smaller and destitute of the pit in one side to which I have referred.

One of the best copper bells which I have yet found in Arizona ruins was taken from a grave at Four-Mile Ruin. This bell closely resembles those from the Gila Valley ruins, but is larger and better preserved than that from Chaves Pass, elsewhere[1] described.

In the graves of the north cemetery I also found a gourd rattle, painted green and red. The impression of feathers on the surface of this rattle indicated that it was formerly ornamented with feathers, as is often true of rattles in use in the modern pueblos.

As in former years, the majority of specimens obtained from the preceding ruins were pottery, and include the several classes peculiar to old pueblos. A small number of decorated vessels belonged to that division called black and white, or white with black line decoration, and only about one per cent could be referred to the group called yellow ware, which characterizes true Hopi ruins. The largest number belong to the group called red ware, with black and white decorations, and there were a few pieces typical of the Gila and Salt River valleys. About the same relative proportion of rough and coiled ware was found in the ruins excavated in 1897 as in previous years, and as a rule the forms of these vessels were identical. One of the most symmetrical specimens was a globular jug made of red ware with black decoration. This jug had a long neck and a graceful handle, ornamented with highly conventionalized figures of birds drawn in a glossy black.

Two of the bowls from Four-Mile Ruin were ornamented with human figures which are strikingly different from any that have yet been found on ancient Arizona pottery. On the head of one of these there was represented a radiating crown of feathers, recalling certain headdresses worn in ceremonial dances among the modern pueblos. There was represented on each elbow of another figure a conventionalized feather, which I have also seen in figures on bowls from Sikyatki.

One of the best reptilian figures was drawn on a food bowl of chestnut color almost identical with the pottery from Chevlon Ruin, excavated in 1896. There were numerous figures of birds, the majority of which were highly conventionalized, but having little in common with the elaborate bird designs of Sikyatki. In no instance was the decoration as carefully drawn as the pictography of the last-mentioned ruin.

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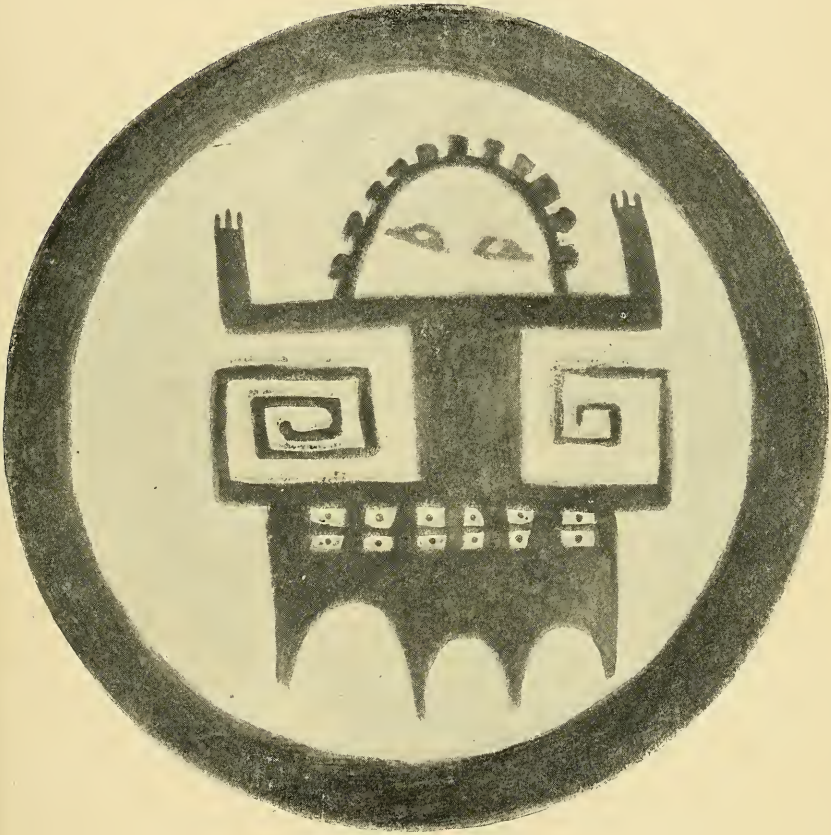
[1] Smithsonian Report for 1896.



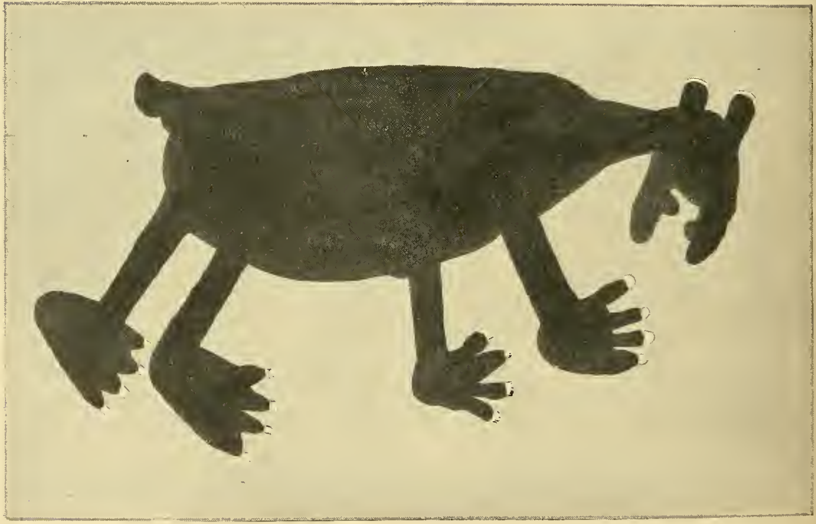
HUMAN FIGURE—FOOD BOWL.
No. 177061.

FOUR-MILE-RUIN.

UNKNOWN REPTILE—FOOD BOWL.
No. 177099.

FOUR-MILE-RUIN.

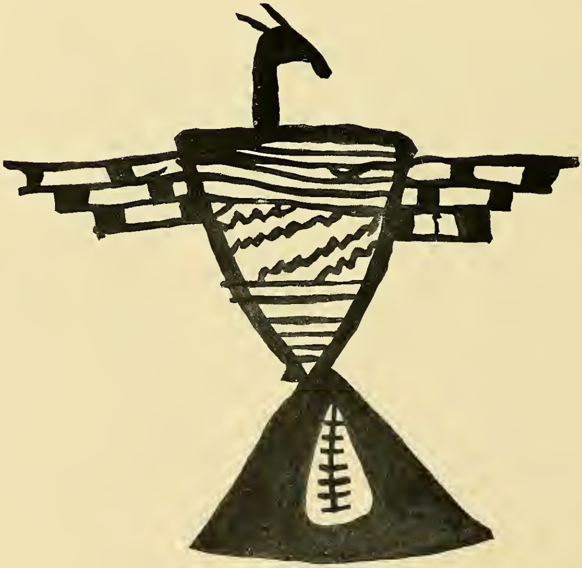
BIRD DESIGN—EXTERIOR FOOD BOWL.
No. 177378.



BEAR DESIGN—EXTERIOR FOOD BOWL.
No. 176999.

FOUR-MILE-RUIN.

BIRD DESIGNS—FOOD BOWL.
No. 177170.



BIRD DESIGN—FOOD BOWL.
No. 177173.

FOUR-MILE-RUIN.

The only insects found figured on pottery from Four-Mile Ruin were the butterfly and the dragon fly. Flower decorations were wanting. The typical forms of feathers characteristic of Hopi ruins were not observed in the southern ruins.

The large food basins of red ware were ornamented on the exterior with many highly complicated geometrical designs, and a larger number of figures of animals were found on the exterior of vessels in 1897 than in previous years.

One of these was the figure of the paw of some large plantigrade animal, possibly a bear. The complete animal, of which this was probably one of the tracks, was delineated on the exterior of a second food basin. The conventionalized figures of two birds joined by the tails decorated the exterior of several food bowls.

RUINS IN THE PUEBLO VIEJO VALLEY.

If one follows a meridian south from the Moki Reservation he will find that it strikes the Gila River not far from the mouth of the San Pedro. It crosses the Little Colorado near the mouth of Chevlon Fork and runs about a half degree west of Snow Flake and Pinedale, which are situated over two-thirds the distance from Sikyatki to the Gila. This north and south line crosses a great mountainous watershed which separates the drainage of the Gila and the Little Colorado, and passes through sections of Arizona presenting all the different kinds of geological environment inhabited by pueblo peoples. I will call this north and south line an archæological meridian to which to refer variations in the character of ceramics. The differences in soil at places on this line have profoundly affected the character of pottery found in the different localities through which it passes.

In examining large collections of ancient pottery made at Sikyatki, Awatobi, Chevlon, Four-Mile Ruin, and Pinedale one can trace, step by step, the gradual modifications in the character of ceramic ware over 120 miles of that distance. The beautiful yellow pottery of Sikyatki, with sporadic examples of red, black, and white, gradually loses its predominance and is replaced by red ware, which is most abundant in the Little Colorado ruins. Bowls made of a rough coiled ware, with a glossy black inner surface, unknown at Sikyatki, begin to appear and increase in relative numbers as we go south. Last of all, as we ascend the northern slope of the hills fringing the White Mountains, while still on the banks of tributaries of the Little Colorado, a singular kind of pottery, typical of the Gila basin, appears for the first time.

Having detected in ruins north of the White Mountains the sporadic appearance of Gila Valley pottery, I was anxious to examine the ruins on the Apache Reservation intermediate between those of the head waters of the Little Colorado and Gila rivers. Not being prepared to cross the reservation for this purpose, I therefore went around the White Mountains to a valley south of the Apache Reservation, called

Pueblo Viejo, which, as will be seen by consulting the map, is about due south of the ruin near Taylor and not far east of the meridian already referred to.

In the year 1846 a detachment of United States troops, known as the "Army of the West," under General Kearny, made what was officially called a military reconnoissance from Fort Leavenworth to San Diego, Cal. Lieutenant Emory, of the topographical engineers, and Captain Johnston, who lost his life in an action with Californians at San Pasqual, were attached to the expedition, and their journals were published in 1848,[1] in compliance with a resolution of the Senate. These oft-quoted "Notes" are of great value to the student of the antiquities of southern Arizona.

On the 28th of October the "Army of the West" camped on the Upper Gila River bank opposite a high mountain, indicated on their map as Mount Graham. For three days they traveled along the valley now called the Pueblo Viejo, and Emory and Johnston were probably the first Americans to call attention to the abundant evidences of ancient habitations which this valley contains. Their accounts of the ruins have remained for fifty years the best that we have of the antiquities of this interesting region. Emory was particularly struck with the amount and character of broken pottery about the ruins, and suggested that the shards were broken pipes once used "to convey water." He found fragments of agate and obsidian, and mentions the existence of both circular and rectangular rooms. Stone implements and metates made of lava rock also attracted his attention. Johnston records having picked up a perforated marine shell and a stone painted red,[2] which "may have been used as a foot of an idol." He also mentions both circular and rectangular rooms, and figures several fragments of decorated pottery.

Both Emory and Johnston seem to have recognized that the walls of the ancient buildings were built of earth, with foundations of water-worn bowlders. From their descriptions I am led to believe that the ruins which they mention are those that can still be seen at the little Mexican settlement at Buena Vista and at Pomeroy's farm, between Solomonville and Safford.

The material used in the construction of these houses was such that it weathered rapidly, with the result that in a short time after desertion the house walls were leveled to the ground, and nothing remained but mounds of earth to mark the sites of ancient settlements. These mounds, rising a few feet above the general surface of the land, are conspicuous for some distance on account of the poverty of vegetation upon them. They are generally slightly higher than the level of the surrounding plain, and as the water can not be made to flood

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[1] Notes of a Military Reconnoissance from Fort Leavenworth, in Missouri, to San Diego, in California, including parts of the Arkansas, Del Norte, and Gila Rivers.

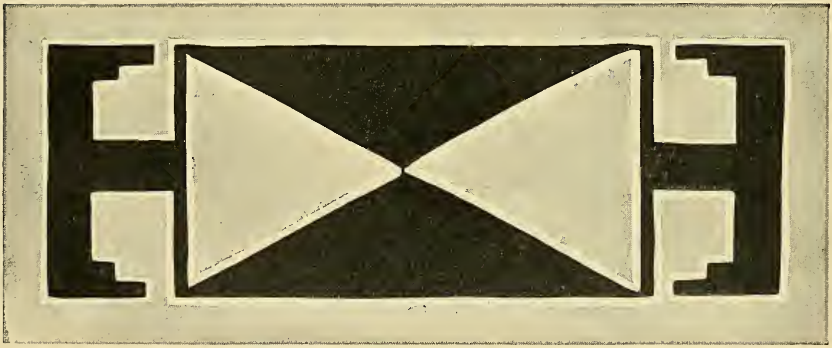
[2] I found at Epley's ruin a painted piece of pottery in the form of a moccasin.

HEART-SHAPED BOWL.
No. 177102.

BIRD FIGURE—FOOD BOWL.
No. 177203.

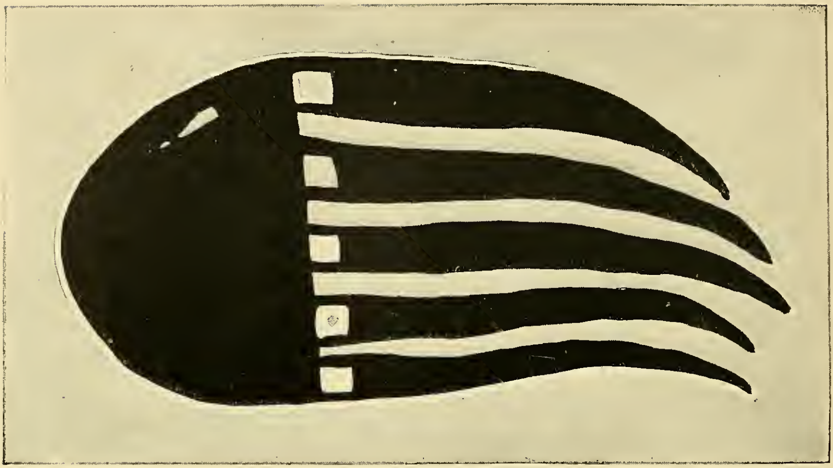
FOUR-MILE-RUIN.

CLOUD EMBLEM—FOOD BOWL.
No. 157352.



GEOMETRICAL DESIGN—EXTERIOR FOOD BOWL.
No. 177000.

FOUR-MILE-RUIN.



Bear's Paw—Exterior Food Bowl.
No. 177277.

Twin Birds—Exterior Food Bowl.
No. 176888.

PINEDALE.

them by means of irrigating ditches, their surface often shows above the cultivated fields of corn or alfalfa. Naturally the white farmer, seeking to bring them under cultivation, endeavors to plow them down to the level of the rest of his field, and this reduction can readily be accomplished in a few years, as the clay is not very hard. As a result a large number of the ancient mounds of Pueblo Viejo have disappeared, and the sites of many old habitations are even now difficult to find. In the same way the burial mounds have likewise been leveled, and it is only by chance—digging a ditch or excavating a cellar—that the wealth of aboriginal objects below the surface of the ground is discovered.

A house wall built of earth, if not kept in repair, may last many years, as those of Casa Grande, or they may rapidly sink to the ground under the erosive action of rains. The old buildings of the Pueblo Viejo have disappeared, and in late years all traces of them have been obliterated by the farmers of the valley. The overflow of the Gila River, and the great torrents of water which have inundated the valley, redistributing the soil, no doubt contributed to the destruction and concealment of the mounds, especially those near the river banks.

I have not found any ruined house cluster in Pueblo Viejo built on an artificial platform for protection from these floods, but it is possible that they may be later found in this region, as they are said to occur elsewhere in the valley.

The desertion of Pueblo Viejo by the sedentary home-builders who formerly lived there can be traced to the inroads of the Apaches. The manner of life of the original inhabitants in the plain so exposed them to attack that with the advent of a vigorous nomad stock they rapidly melted away, fleeing, no doubt, to the inaccessible canyons north of the valley or withdrawing from their farms to their kindred along the Aravaypa and San Pedro.

The houses of Pueblo Viejo valley were constructed of stone and earth, of which the latter predominated. The rocks employed were river washed, rounded stones, and not angular fragments, like those used elsewhere. The ancient builders adopted the building material at hand. Evidently small bowlders, such as are found in Pueblo Viejo, could not be used in constructing a wall of any height without using much more adobe than was necessary with angular rocks, and there was a great abundance of clay ready for use. The result was that while stones were used for foundations and for strengthening the walls, clay gave the walls form and finish. In the weathering of these structures, the foundation row of stones still remain in their original positions, where they are now exposed in almost every mound. Such a row of foundation stones extends across a street in Solomonville, near Mr. Kelly's printing office. The earth walls and the stones they contained were still further strengthened at intervals by upright cedar logs, which helped to support the roof. These logs, when the earth wore out between them, would remain, and several old residents of the valley remember seeing these logs projecting from the mounds twenty years ago. They were mentioned by both Emory and Johnston. Señor Montoya, who lives in a modern adobe house at Buena Vista, told me that when he first settled there several of the mounds were staked out by rows of upright cedar posts, marking the positions of the former walls. These posts were used as fuel, and although no fragments remain above ground, there are many indications of them buried in the soil. The method of supporting an earth wall by means of upright posts was used at Four-Mile Ruin, and is conspicuous in the walls of one of the rooms at that ruin. Similar vertical stumps have likewise been found by me in several other ruins, showing that this method of construction was a common one in widely separated localities. It has been noticed lower down the Gila, in the Mancos and Chelly canyons, and appears to have been in use in the middle of the sixteenth century, for it is mentioned by Castaneda in his account of the "Coronado expedition."

I saw no evidence in my examination of the walls that they were made of blocks of clay, sun dried before they were set in position, nor of the rammed earth or pisé work, said to be so evident in Casa Grande. I am inclined to believe that the builders were familiar with both these architectural methods, but that in the Pueblo Viejo they adopted a simpler one. A foundation row of stones was first laid, and upright logs were driven at intervals along the lines of foundation. The intervals between the logs, which were large, were then filled in with stones and clay taken from neighboring flats. Possibly one object of the line of foundation stones was to prevent undermining by water and other agencies the action of which would be greatest at the level of the ground. The stones thus presented an effectual resistance to erosion at the weakest part of the wall—its foundation.

The compact dwelling called a pueblo, in which clans are huddled together, is not represented in the ancient habitations of Pueblo Viejo. The clusters of houses were approximated, but were not joined, being separated by courts, reservoirs, or irrigation ditches. We find something similar to this in the localization of gentes in different quarters of the Pueblo Walpi, where there is a separation of the houses of different clans, where the tendency has been to consolidation, but in the Pueblo Viejo ruins the houses of different clans were isolated from each other. All the rooms were rectangular in form, or nearly so, and although circular depressions were noted in several ruins, no evidence was seen that these were formerly inhabited rooms. The nearest approach to a circular room is the depression at Buena Vista, which was too small to be referred to any of the circular rooms mentioned by Emory and Johnston.

The chambers at Epley's ruin, which we excavated, were filled with adobe denuded from the upper part of the walls. Although the room

Spiral Design.

Four-Mile-Ruin.

Unknown Design—Food Bowl.
No. 177126.

Four-Mile-Ruin.

was filled with clay, it had not become so hard that the walls of the rooms could not be easily distinguished on account of their superior compactness.

The large circular or elongated oval depressions in the immediate neighborhood of some of the house mounds have been identified as the sites of former reservoirs. Other more irregular depressions mark the places from which earth had been excavated by the ancient builders, and it is possible that these depressions were also utilized as water holes by the ancients. The reservoir at Buena Vista is one of the largest that was discovered, yet no irrigating ditches leading into it were distinctly traced. A depression full of water, to the right of the road as one approaches Epley's ruin, is said to have existed there before the Mexicans excavated adobe from the adjacent lands, and may possibly be the remains of an ancient reservoir.

There is abundant evidence that the ancient people of the Pueblo Viejo Valley led the water from the Gila River over the plain by means of canals for purposes of agriculture, for in many places the depressions marking the old ditches may be traced for considerable distances. These signs are well marked near San José and Buena Vista, where the surface of the land has been least changed by the plow. I have been informed by some of the older residents that when they came into the country, before the Montezuma and San José irrigation ditches had been constructed, the ancient aqueducts were much more conspicuous than they are to-day, and that sections of the modern ditches follow the course of the ancient waterways.

We found the slopes of the hills marked out with lines of stones, arranged in rectangular forms of great regularity, extending over many acres of ground. Some of these, especially on the foothills near San José, were very extensive. These lines of stones are regarded, not as remains of house walls, but as boundaries of ancient terraced gardens. If this interpretation be a correct one, it appears that the ancient inhabitants of Pueblo Viejo cultivated not only the bottom lands, slightly elevated above the Gila River, but also the side hills, which white farmers have not yet brought under cultivation. It is probable that these terraced gardens were not irrigated by ditches from the river, but that water was carried to them by the natives in ollas from some neighboring reservoir at the foot of the hills.

As no remains of houses were discovered near these terraced farms it would seem that the ancients cultivated lands some distance from their dwellings. Good examples of these terraced gardens exist near the Solomonville slaughter house, and from that place along the side of the San Simon Valley. The best examples that I examined were not far from San José. There is a close resemblance between these lines of bowlders and the so-called bowlder sites in the Verde Valley, which bear every evidence of having been ancient gardens. There is little débris about them, such as would accumulate if they were parts of houses which had been inhabited any length of time. The great areas covered by these lines of bowlders, both in the Verde Valley and especially in the Pueblo Viejo, would militate against their being house sites, for if we regard them as house walls we would have to suppose a continuous pueblo a half a mile square without a single fragment of pottery near by to indicate former habitation.

RUIN NEAR SAN JOSÉ.

The plain near the Mexican settlement, San José, shows evidence of a large ancient population, and it is no rare thing for farmers to dig up pottery and other prehistoric objects when preparing their farms for cultivation. There still remains, on the right bank of the irrigating ditch opposite where the San José arroya enters it, a section of a symmetrical mound, through which run ancient walls. This mound is so situated that it protects a neighboring cornfield from overflow at times of freshets in the arroya, for when the San José is flooded the water is turned from it by this mound into the canal. A considerable part of the mound has thus already been worn away, leaving exposed a section 20 feet high, revealing its artificial character. In an examination of this section we easily traced the course of the walls, and many fragments of pottery were exposed from the base to the top of the mound. It would have been an easy and profitable work to dig away this whole hill, but the owner strongly objected to my doing so, since it would destroy the embankment which turned the water from his field. It is only a question of a few years when this mound will be wholly washed away, as every considerable storm eats deeper and deeper into its sides, and spreads the soil of which it is composed over the neighboring valley or washes it down to the Gila. A morning's work in the embankment revealed a small vase and one or two other objects. Several pieces of pottery were said to have been taken from it by natives of the village of San José, which is only a short distance away.

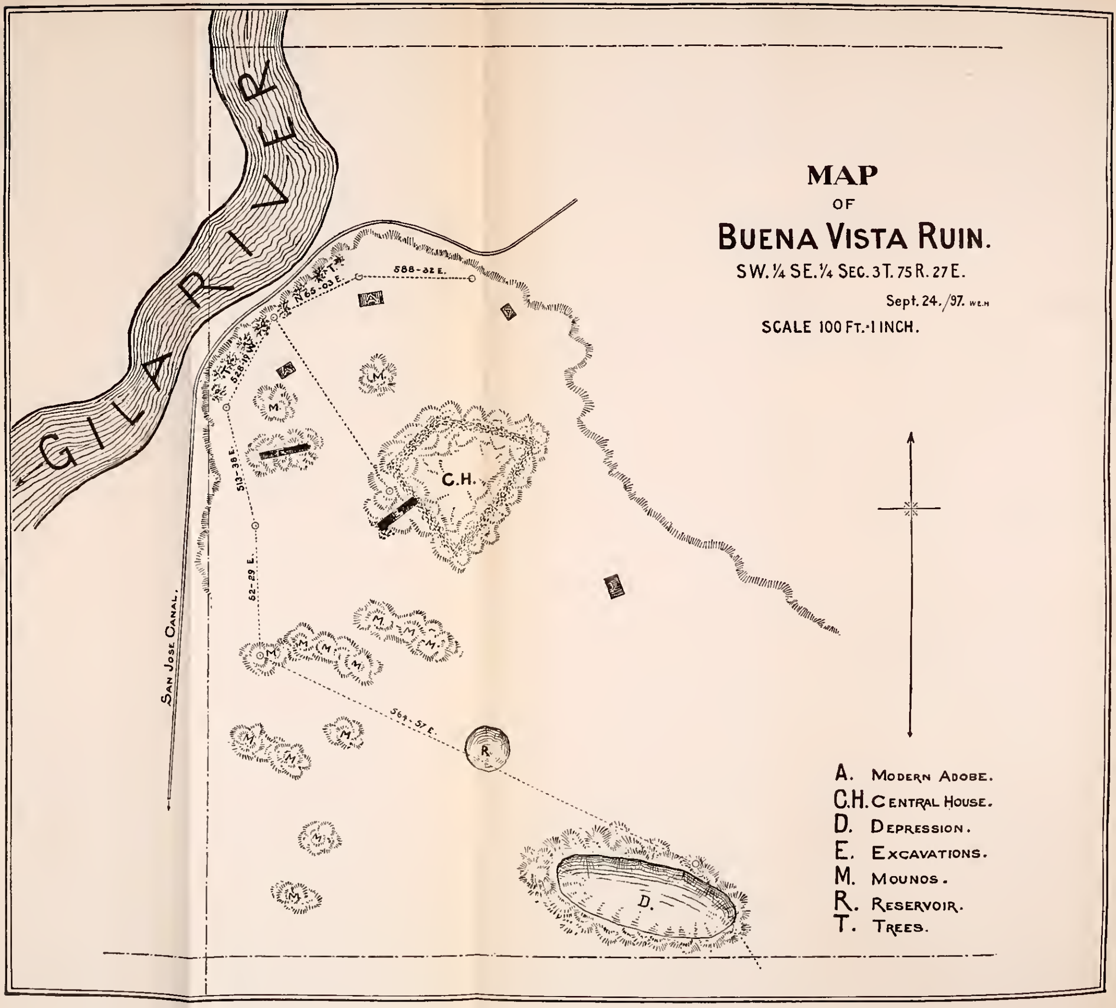
BUENA VISTA RUIN.

A few miles higher up the river beyond San José the valley narrows, and about two miles from the settlement there is a cluster of four or five modern adobe homes called Buena Vista. They are situated on a bluff about 20 feet above the Gila, with a beautiful view of the Bonita and neighboring mountains. This cluster of adobes is built on the site of a large ruin, commonly called the San José ruin, which may have given the name San José or Pueblo Viejo to that hamlet.

This ruin is one of the best preserved of any that was visited in the valley, as there has been no great disturbance of the soil on its site, and the mounds, as a rule, have not been leveled, as is generally the case with those near Solomonville, Safford, Thacher, and Pima. I have therefore made a map of the distribution of the mounds, but it is far from satisfactory as it does not include all that are found in the cluster.

SUN EMBLEM—FOOD BOWL.
No. 177058.

FOUR-MILE-RUIN.



MAP
OF
BUENA VISTA RUIN.
SW. ¼ SE. ¼ SEC. 3 T. 7S R. 27 E.
Sept. 24./97. W.E.H.
SCALE 100 FT.-1 INCH.

A. MODERN ADOBE.
C.H. CENTRAL HOUSE.
D. DEPRESSION.
E. EXCAVATIONS.
M. MOUNDS.
R. RESERVOIR.
T. TREES.


MAP
OF
NA VISTA RUIN.
/4 SE.¼ SEC. 3 T. 7S R. 27 E.
Sept. 24./97. W.E.H.
CALE 100 FT.=1 INCH.
GILA
SAN JOSE CANAL.
A. MODERN ADOBE.
C.H. CENTRAL HOUSE.
D. DEPRESSION.
E. EXCAVATIONS.
M. MOUNDS.
R. RESERVOIR.
T. TREES.

Indeed, probably no two observers would agree as to the limits of the place, as detached home mounds can be detected almost all the way from Buena Vista to San José. The bluff on which the ruin of Buena Vista stands is a natural one, and commands a view of the Gila River, between which and the mound is a low ridge of sand which forms a bank of the San José Canal. The Gila is fringed with trees, which also line the edge of the bluff. North of the bluff there is a wide river bottom, now cultivated, and, no doubt, the site of ancient farms. The ruin itself is covered with a growth of mesquite bushes, cacti, and other thorny vegetation, but the soil is more rocky than the rich alluvium of the river bottom.

The large central ruin of the Buena Vista cluster is trapezoidal in shape, formed of a number of rooms with a central inclosure or plaza, which is now used as a corral. The walls of these rooms were built of stones, or at least stone walls are all that now remain. In one or two parts of this large structure the character of the masonry can be made out, but as a rule the walls have so fallen that it is very difficult to determine the size and arrangement of the chambers that formerly composed this building. This stone inclosure may be likened to the great central structures that characterize the Gila Valley ruins. There is evidence that earth was combined with stone in its construction, but naught now remains but the rows of stones which supported, possibly strengthened, the walls. This was a citadel or place of refuge, and from an architectural standpoint resembled more nearly than any other building the great stone ruins north of the White Mountains, at Chaves Pass, or on the Little Colorado.

The greater part of the terrace upon which the Buena Vista ruin was built was covered with small, rounded mounds, isolated or forming low ridges, bearing evidences of artificial origin.

RUINS NEAR SOLOMONVILLE.

The excavations of the mounds on Epley's farm, a short distance from Solomonville, rewarded me with fair results. Some of these mounds had been opened by Mexicans for adobe, and the many specimens of ancient handiwork found by them have been scattered and lost. Work had likewise been done upon them by Mr. B. B. Adams, who greatly aided me in my studies in the valley. Epley's ruin is rapidly being brought under cultivation, and in a few years traces of the ancient mounds will wholly disappear. Already so many have been leveled that I found it next to impossible to get a good idea of the arrangement of the house clusters in this locality. There was formerly a many-chambered house, indicated by a high mound, just back of Epley's residence, which appeared to occupy the same relationship to smaller neighboring dwellings that the large inclosure at Buena Vista does to the mounds in its neighborhood. We dug to the lowest floor of this house cluster and found it made of a thick layer of hard adobe plastered on a base of cobblestones. The upright walls, separating the rooms, were very smooth and thick. Excavations in this mound revealed little of archæological value, and no house burials were found.

The rooms north of this central mound had been thoroughly dug out by the adobe seekers, and it was said that pottery of fine make had been taken from this place. The surface of the ground in the immediate neighborhood was strewn with worked stones, metates, grinding stones, and fragments of decorated pottery left there by the workmen. Among the objects thus abandoned were some of the finest metates that I have ever seen, and it would not be extravagant to state that there were over fifty rubbing stones in sight on the surface of one of these mounds. The more valuable objects and all whole pieces of pottery had been removed by the Mexican workmen.

The road from Solomonville to San José bisects Epley's ruin with mounds on both sides, connected by sections of walls visible in the roadbed. There are large quantities of broken pottery on the surface of all elevations in the immediate neighborhood. The cluster of mounds on the farm adjoining Epley's belongs, no doubt, to the same composite settlement, and between both these places and the Gila River there are many small hillocks indicative of former habitations. In fact, the whole country from Solomonville to San José is thickly dotted with mounds, as if formerly densely populated.

RUINS NEAR THACHER AND SAFFORD.

There were several house clusters near the present village, Thacher, although at present little evidence remains above ground of their former position. Perhaps the best preserved mounds are at Mr. L. Place's farm, especially in the cut of the road near his house, where traces of walls can yet be seen. They are being rapidly leveled, and in a few years will disappear completely. Several ancient ollas have been found while excavating irrigation ditches in this neighborhood.

There are two large ruins at Safford which are worthy of note. One of these is on Mr. Peter Anderson's farm, but the mounds have been leveled and are hard to trace. The ruin at Beebe's place is well marked, and many beautiful specimens of ancient pottery have been taken from this locality. The mounds indicate that the ruin here was very extensive, and although considerable excavation has been carried on much of it has been unsystematic. The original form of the ancient cluster of houses has been well-nigh obliterated, but profitable results are possible, especially in the lowlands surrounding the mounds.

From an examination of Emory's "Notes" and plan of his march it seems probable that he camped not far from this ruin on October 28th, 1846. A visit to the mounds in this vicinity was disappointing, for most of them had been plowed down to the level of the surrounding plain. There was, however, a small elevation in the midst of a cornfield which probably belonged to the collection referred to by Emory.

This mound lies not far from the left bank of the Gila, a few miles from Solomonville.

The general appearance of the mounds on Mr. D. Olney's ranch is practically unchanged save by superficial excavations in some of the largest. The land, as a rule, has not been cultivated since the occupation of the valley by the whites, and is covered with a desert vegetation which formerly characterized the whole valley. Although not unlike the ruins along the banks of the river, it is less changed than they are, and affords a good type of the ruins of the higher lands. There is a large spring near the mounds, and patches of cultivated land, possibly the same farms as those once tilled by the Indians.

A number of house burials were found in the rooms at Epley's ruin, but they were almost invariably the skeletons of infants, the bones of which showed no signs of cremation. These skeletons were found in large ollas, of coarse coiled ware, and were sometimes accompanied with decorated bowls or small ornamented vases. These burials were, as a rule, found deeply covered with soil, near or under the floor, in the vicinity of fireplaces. The earth in their immediate neighborhood was loosely packed, in marked contrast to a harder soil filling the rest of the room.

Proofs that the ancient people of Pueblo Viejo burned their dead are many and decisive. The existence of large cemeteries just outside the pueblo walls, which is so common in the Little Colorado ruins, was not detected, but there were many hillocks of ashes, indicating pyral mounds.

The Gila Valley ruins are characterized by a large centrally placed house, or fortress, and clustered about it many dwelling houses, unconnected with each other, apparently habitations of clans. It would seem that each family had its own pyral mound, and that there were several places in each cluster of mounds where the dead were buried. Consequently the interments of the incinerated bones were more scattered, as each family had its own cemetery. The calcined bones were placed in vases, over which was luted a pottery disk or bowl, and the whole buried in a neighboring mound. Near the cinerary olla, in which the cremated bones were placed, there were deposited other pieces of pottery, some broken, others entire.

Most of the pottery obtained from this valley came from the ruins near San José and Buena Vista or from Epley's farm, the site of my most important excavations. This latter locality has yielded many specimens in the past decade, as it has been a favorite place for digging adobe. In the course of their work Mexican laborers have found there a large number of pottery objects, which they have either given away or sold to collectors. We obtained a few pieces from them by purchase, others were presented, and several were dug up. The majority were large ollas of coarse ware, coiled or indented, similar to those now made by Pimas or Papagoes, save that their exteriors were rarely smooth. These were generally found buried in the rooms, and not a single entire specimen of these large ollas was found.

The majority of the specimens from these two ruins were food bowls, made of rough coiled ware, with a glossy black interior. This is also a common kind of pottery from the tributaries of the Little Colorado and is especially abundant at Four-Mile Ruin, near Taylor, and Pinedale. Sporadic specimens of this peculiar ware have been likewise found at Homolobi and the ruin near the mouth of Chevlon Fork, excavated in 1896.

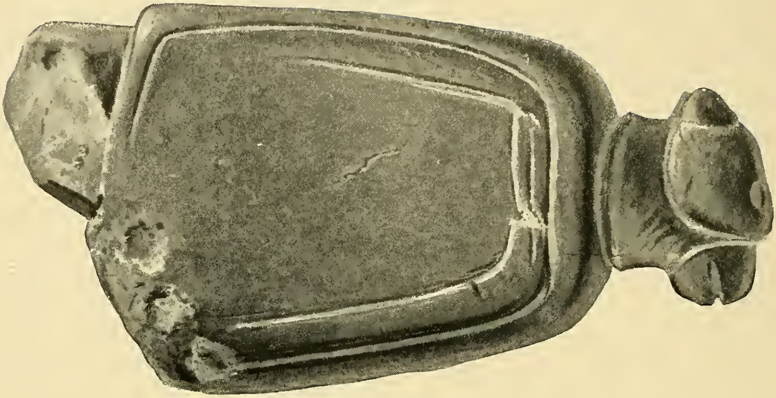
The characteristic decorated ware of the Pueblo Viejo ruins is similar to that from the Salado River, near Tempe. It has a gray color, with black and red decorations, but is not glazed, and ordinarily is not as glossy as the red ware of the Little Colorado. Specimens of this ware have been found at Pinedale, Four-Mile Ruin, Chevlon, and Homolobi, the relative proportion diminishing as the ruin is situated more and more remote from the Gila River. A few specimens of white and black ware and a limited number of plain red bowls were found, but the characteristic yellow ware of Tusayan was not represented. It will thus be seen that as we go south from Sikyatki, in Tusayan, the yellow ware is gradually replaced by decorated red pottery, and that the glossy black food bowls, with rough, undecorated exteriors, begin at Homolobi, are more numerous at Four-Mile Ruin, and most abundant in the Pueblo Viejo. The gray, black, and red ware also increases as we go south.

All typical forms of pueblo pottery appear in the Pueblo Viejo. Among these may be mentioned vases, jars, food bowls, slipper jars, and the like. The geometrical decorations are similar to those on pottery objects from ruins in northern Arizona.

None of the ceramic objects which were collected in the Pueblo Viejo were adorned with figures of animals or human beings, and this rule seems to hold in all the Gila Valley ruins. This may be regarded as an indication either of great age or of a primary state of pueblo art.

It has been said that the custom of making vessels in the forms of birds and other animals is of recent date among the Pueblos, but, as already shown, such a statement is not accurate. I have described and figured clay vessels in the form of birds from Homolobi, Chevlon, and other ruins on the Colorado Chiquito. Equally fallacious, also, is the statement that ancient vessels were not made with organs of the human body in relief. A vase from a cave in the Nantack's is good confirmatory evidence bearing on this point.

There is another instructive specimen in the collection from Pueblo Viejo which shows human features in relief. An intelligent Mexican laborer brought me, for sale, a jar which he claimed to have dug from the ruins near San José. I was disposed at first to doubt his statement, but later gathered such convincing proof of his veracity that there was no doubt of the antiquity of the vessel. The jar is made of red ware, identical with many other specimens from this region, and apart from its unusual decoration there is no reason to question that it was found in this valley. The remarkable thing about this vessel is



CEREMONIAL STONE SLAB.

No. 177578.

CEREMONIAL STONE SLAB.

No. 177575.

PUEBLO VIEJO.

the existence of human faces, with organs in relief, found on the upper or smaller of the two globular parts of which it is composed.

My attention was called soon after my arrival in Solomonville to a small head made of clay, which reminded me of certain similar objects from Old Mexico. As in all excavations on the Little Colorado I had never found more than the rudest imitation of a human head in clay, I was startled by the discovery of objects of this kind so well made and so Mexican in appearance. A report of a similar head on a dipper handle from the Beebe Ruin, near Safford, was confirmatory of my suspicions, but later, in the excavations at Buena Vista, a well-made figure of a human face and head was taken from a small mound of ashes and other débris.

The stone implements from the ruins in Pueblo Viejo were particularly fine, and include all the more common kinds. In fact, almost every stone found in Epley's ruin bore traces of having been worked, chipped, or in some other way pecked or dressed into an implement. There were many metates, mostly made of a volcanic rock, the grinding surface of which was so worn down that the sides stood 4 or 5 inches in relief. A most exceptional form of metate was made of lava and had three stumpy legs. This is a well-known Mexican form, which has never been found in northern Arizona.

Several worn quartz crystals, which had evidently been prized by their owners, were found in the rooms which were excavated. It is supposed that they were used in the ancient ritual. A considerable number of perfectly spherical stones, that show marks of chipping and dressing, recall the weapons of offense used by the ancient Pueblos. It is possible that some of the smaller of these were covered by a skin, and, when attached to a stick, were used as a maul.

My attention was called, soon after my arrival in the Pueblo Viejo, to small rectangular slabs of stone about the thickness of the sole of a shoe, flat on one face, rounded on the opposite, and grooved about the edge. Later several of these sole-shaped stones were collected. They have never been found in northern ruins, but are common in the ruins along the Gila and Salt rivers. These slabs were used in ancient ceremonials, and the Hopi, who have legends referring to them, still use stone objects, with which they may be compared, in certain kiva rites.

The ordinary form of these objects is simply rectangular, with a groove on the rim and a slight depression on one face, with a border which is ornamented with incised parallel lines. One of the best of these slabs had the head and tail of a bird carved on the ends.

This specimen was found near Mr. L. Place's ranch at Safford, and was dug out of an irrigating ditch not far from his house. From the large number of these stone objects from the Gila Valley ruins, I judge that they were in common use in ancient worship.

A number of grooved stones identical with those mentioned in previous reports from more northern Arizona ruins were likewise found in the Pueblo Viejo. As a rule, however, they were more elaborately made than those from the north. Their use is apparently identical, viz, to smooth, by rubbing, wooden sticks or arrows. Similar stones are at present used in the Hopi pueblos in the manufacture of prayer sticks.

SACRIFICIAL CAVE IN THE GRAHAM MOUNTAIN.

Both the Graham and Bonita mountains have many caves of considerable size which were formerly used for sacrificial and other purposes. One of these I will designate Adams Cave, from my friend Mr. B. B. Adams, of Solomonville, who guided me to it, and assisted me in many ways during my work in Pueblo Viejo.

This cave lies on the northern slope of Mount Graham, near a sawmill, south of Thacher. The entrance is difficult to find, and is said to have been discovered by following an Apache Indian who used it as a hiding place.

Descending the cave by a perpendicular passageway, through which one had to lower himself as if in a well, one enters a large chamber, from which horizontal passages drifted in different directions. One of the largest of these extended about 100 feet into the side of the hill. At its end the cave is enlarged and the floor covered with prayer sticks, examples of which are shown in the accompanying cut. I gathered possibly a peck of these sticks, but many bushels were left behind. The sides of the rock at this point, as elsewhere, show signs of smoke, and the smooth surface of the passages indicate frequent visits of the Indians to their shrine.

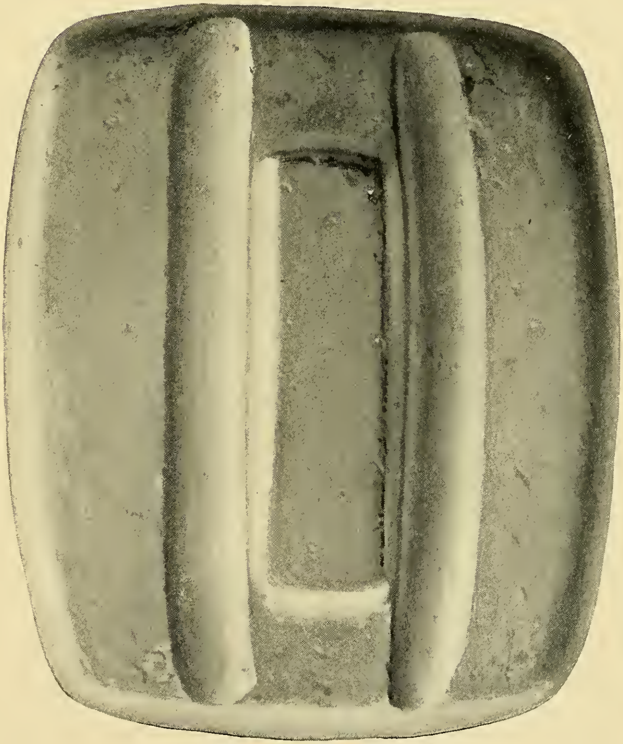
Following another and narrower cleft we came upon a place where there was a similar collection of prayer emblems on the ground. In order to enter this passage one was forced to swing himself to the floor by means of a rock artificially wedged in between the two walls. The upper surface of this wedge was worn smooth by human hands which had grasped it many times. At one place, where the two walls of the cleft approached so closely that it was impossible to pass between them without rubbing the body on either side, the rock surface had been worn smooth by constantly passing human bodies.

Fragments of basketry were found with the prayer sticks, one of the best of which was a small basket plaque. I was much surprised not to find any objects of pottery in this cave, but my examination was necessarily a hurried one, and no excavations were made. According to Mr. Adams, the only person in Solomonville who had visited this cave, pottery had never been found in it.

OBJECTS FROM A SACRIFICIAL CAVE IN THE NANTACKS.

The mountainous region north of the Pueblo Viejo is designated on maps as the Natanes Plateau, portions of which are called by cowboys, the Nantacks. It is rough and broken, cut with deep canyons and mountain streams, and forms the northern part of Graham County,

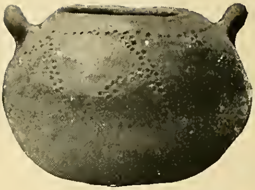
UNKNOWN STONE OBJECT.
No. 177677.



ARROW POLISHER.
No. 177569.

PUEBLO VIEJO.

INDENTED BOWL.
No. 177458.



SMALL AMPHORA.
No. 177463.

DECORATED SLIPPER JAR.
No. 177533.

HUMAN EFFIGY VASE.
No. 177519.

EFFIGY VASE.
No. 177332.

PUEBLO VIEJO.

adjoining the southern border of the Apache Reservation. Early last summer my attention was called by letters to discoveries made by young men from Pima in a cave in that region, and as soon as I was settled in Solomonville I visited Pima to inspect the collection that had been procured by them from this region.

I found that it had been divided into about four equal parts, and two of these parts, which were almost complete could be purchased. Recognizing the value of the objects offered for sale, they were obtained at a reasonable rate. The original collection contained not far from one hundred and fifty specimens of pottery, and many shell beads, obsidian arrowheads, and turquoises. All these objects were taken from one cave, and were evidently ancient.[1]

The majority of the specimens in this collection were flat clay disks, and platters, of undecorated red ware, and a few globular bowls of the same color. These objects were found on benches of rock in the cave, where water was continually dripping upon them. As the water contained lime in solution a film of calcareous matter had covered most of the pottery objects, but unfortunately this deposit had, in most instances, been rubbed off, although in a few cases it still remained. The objects removed were said to be only a portion of those seen in the cave, the broken fragments having been left behind.

The turquoises and shell beads, which no doubt had been placed in the platters, were strewn about the floor of the cave, some of them still remaining where they were deposited. The red ware from this cave is identical with that at present made by the Pimas and Maricopas, and the same as that from the ruins along the Little Colorado River.

One of the most exceptional forms of pottery found in this cave was a gray vase with head, body, and arms of a female human figure in relief. The vase is made of coarse paste and is undecorated with the exception of parallel white lines on the cheeks, which recall markings on the helmets of certain Pueblo Katcinas. The treatment of the figure in relief on this vase betrays Mexican influence.

Among other objects from this cave a clay cylinder covered with projecting knobs, and a globular jar decorated with rows of small pits, were the most striking. Several of the vessels had holes about the rim as if for suspension, and all were small, as is the case with bowls used for sacrificial purposes. In fact, the whole collection bore evidence that it was sacrificial in nature, and from the small size of the individual specimens we could hardly suppose they ever had any other use.

CONCLUSIONS.

There is little to guide us from historical sources regarding the age of the ruins in this valley. In 1697 the valley of Pueblo Viejo was destitute of sedentary people, being overrun by Apaches, for in that year

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[1] From what could be learned there is little doubt that this cave has many likenesses to the sacrificial cave in the Graham Mountains, already described.

Bernal led an expedition down the San Pedro to its junction with the Gila, where he turned west without entering Pueblo Viejo. A mission called Victoria is indicated on Kino's map of 1710, at the mouth of the San Pedro, but east of that no missions appear to have been founded, nor does Kino appear to have traveled in that direction. All that region was called Apacheria, and probably long before the beginning of 1700 the sedentary people had been forced out of the valley by hostiles.[1]

The ruins in Pueblo Viejo are similar to those near Tempe and along the banks of the Gila and Salado. From what is left of the houses and the arrangement of the clusters into villages, their mode of construction, irrigating ditches and reservoirs, I should judge that the former inhabitants of the two regions were of similar culture if not the same stock. The pottery of the upper and lower Gila ruins can not be distinguished, being identical in color, texture, and decoration. Both people cremated their dead, and likewise, in rare instances, practiced intramural burial.

The nearest agricultural people to Pueblo Viejo of which we have any account in early Spanish writings were the Sobaipuri Indians, a Piman tribe then living on the San Pedro. They appear to have occupied this range in 1540, and the names of their rancherias and number of inhabitants were given by Kino in 1679. Although they lived on or near the sites of some of the ruins they do not seem to have regarded these houses as the work of their people. They had an intimate knowledge of Zuñi and Tusayan, and told Father Garces that the Mokis built some of the old buildings on the Gila.

The Sobaipuri, like the Pimas, were an agricultural people, living in rancherias which were often clustered together, irrigating their farms, and raising corn, melons, and cotton. It is possible that their old range was the Pueblo Viejo, out of which they had been driven by hostile Apaches.

At the time of the visit of Marcos of Nizza there were trade relations between Cibola (Zuñi) and the Sobaipuri, and some of the latter accompanied Estevan to Cibola. Coronado, in 1540, met a Cibolan Indian among the Sobaipuri and gathered information from him concerning Cibola and other "kingdoms." It appears that in the middle of the sixteenth century there were cordial trade relations between the settlements on the Little Colorado River and those of the San Pedro and Gila, which would indicate visits back and forth and the existence of trails between the two sections. At that time the Apaches were not strong enough to prevent communication between the people of the Gila and the upper tributaries of the Little Colorado.

When Bernal descended the San Pedro with Padre Kino in 1697 they were accompanied by Coro, a Sobaipuri chief, and some of his

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[1] Probably Arivaypa Canyon was one of the last strongholds of the sedentary people. The people of that region intermarried with the Apaches.

people. Kino recorded that when they arrived at the Gila they found mounds which indicated ancient dwellings, but what information the Sobaipuri gave him about these mounds is not mentioned in the diary, and almost a century later the same people ascribed ruins on the Gila to the Mokis.

Up to the advent of the Apaches there was considerable trade between the Mokis and Sobaipuris, which was fostered by fairs (ferias) which the Hopi came as far as the valley of the San Pedro to attend. They appear to have visited Taibanipita, an ancient settlement near the modern city, Tombstone. But the Apaches in their inroads cut off communication between the Sobaipuri and Mokis by occupying a ford by which the ancient trail crossed the Gila River. There is abundant archæological evidence to show that trade relations between the people of the Little Colorado and Gila basins existed in prehistoric times, and the Mokis were acquainted with the tribes south of the mountains.

A peculiar kind of pottery which is common in the Pueblo Viejo Valley and in the southern ruins of the Colorado Chiquito is a coiled ware with rough decorated exterior. The inner surface is a glossy black. This kind of pottery is unknown at Sikyatki, is common at Four-Mile Ruin, and has been found at Homolobi and Chaves Pass.

The characteristic Gila pottery, which is a light gray ware with black decoration on a red base, has been found by me at Four-Mile Ruin, near Taylor, but it is absent in ruins farther down the Colorado Chiquito at Homolobi and Chevlon.

The decoration of pottery with human or animal designs, which was so prominent a feature north of the Mogollons in the Little Colorado watershed, has disappeared south of the mountains and its place taken by figures in relief. The geometrical patterns were, however, similar in the two regions.

The symbolism on the decorated pottery of the Pueblo Viejo ruins is the same as that farther down the Gila, and remotely related to that of the Little Colorado and its tributaries.

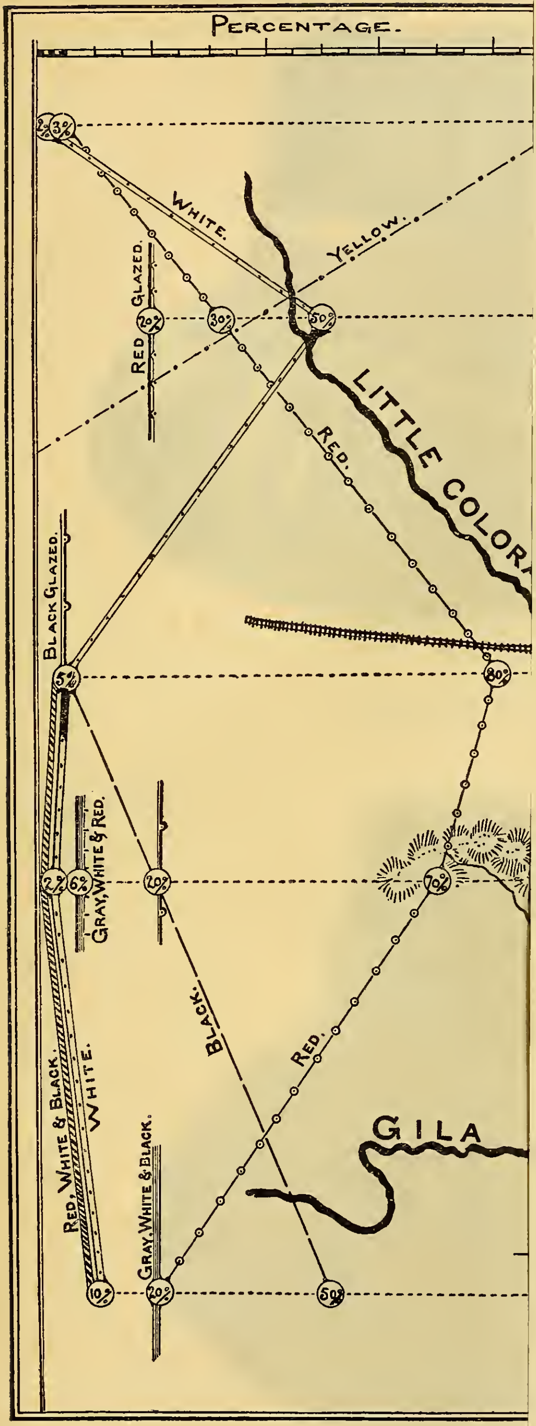


Diagram showing Relative

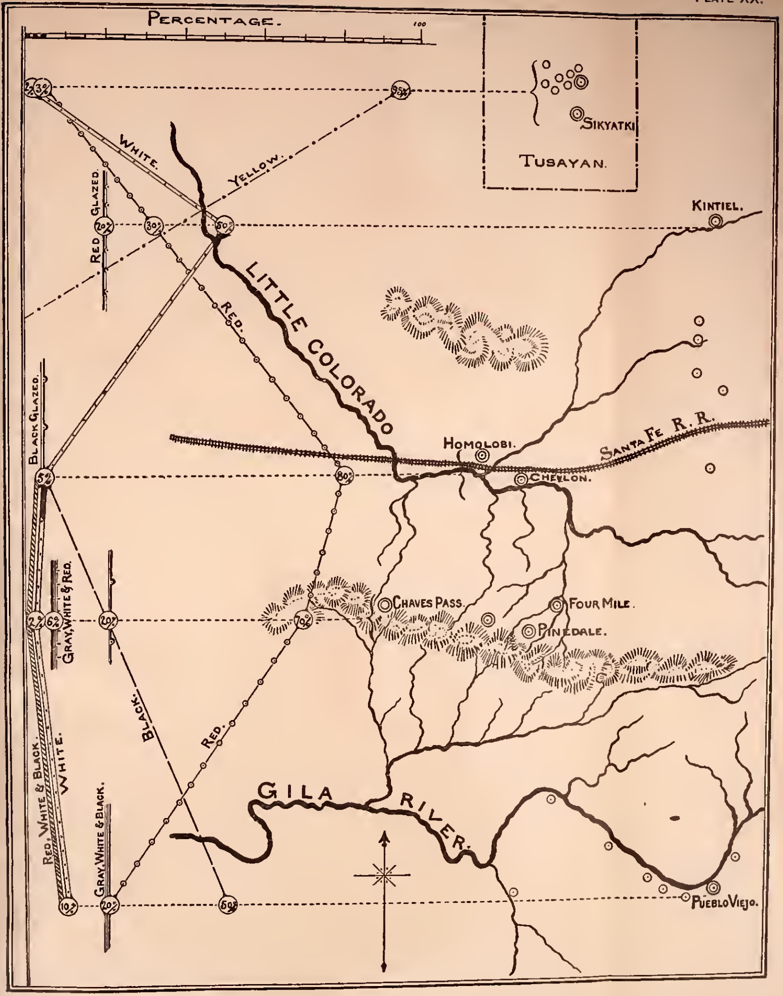


DIAGRAM SHOWING RELATIVE DISTRIBUTION OF ANCIENT POTTERY.

CINERARY VASE.
(No. 177171.)

VASE.
(No. 177160.)

FOOD BOWL WITH HUMAN FIGURE.
(No. 177293.)

FOOD BOWL WITH FIGURE OF MASKED DANCER.
(No. 177864.)

THE BUILDING FOR THE LIBRARY OF CONGRESS.

By Bernard R. Green.

The Library of Congress was founded by the act of April 24, 1800, entitled, "An act to make further provision for the removal of the Government of the United States." The sum of $5,000 was appropriated "for the purchase of such books as may be necessary for the use of Congress at the said city of Washington and for fitting up a suitable apartment for containing them and placing them therein."

The act also provided "that the said books shall be placed in one suitable apartment in the Capitol in the said city, for the use of both Houses of Congress, and the members thereof." "An act concerning the library for the use of both Houses of Congress," approved January 26, 1802, has continued to the present time as the fundamental law of "The Library of Congress."

The Library, comprising not more than 3,000 volumes, occupied a room in the Capitol until August 25, 1814, when it shared the fate of the building by being burned on that day by the English in the war of 1812. A month later Ex-President Jefferson offered his private library of about 6,700 volumes on such terms as the people might accept, and Congress, after much debate, purchased it for $23,950. It was placed in a room in the building used by the Post-Office Department, but temporarily taken for the occupation of Congress. In 1816 Congress removed to a brick building erected on Capitol Hill for its use pending the restoration of the Capitol. The Library was transferred to the same building, where it remained until 1824, when it was finally returned to the Capitol and placed in the west hall, which later became the central of the three halls occupied by the Library until it was transferred to the new special building in 1897. For seventy-three years, therefore, it occupied the same accommodations in the Capitol.

In 1851 the number of volumes had increased to 55,000, but on December 24 of that year some 35,000 of them were destroyed by fire communicated by a defective flue, to which the woodwork then comprising the fitting and shelving of the library was exposed. The interior of the three halls referred to was then reshelved on the alcove plan in three and four tiers, with galleries wholly cast in iron, by the Architect of the Capitol, Thomas U. Walter.

In 1860 75,000 volumes had been accumulated. By act of April 5, 1866, the library collected by the Smithsonian Institution, amounting to about 40,000 volumes, was transferred to the Library of Congress, which thereafter grew rapidly, reaching in 1870, when the national copyright law went into effect, 197,668 volumes and 30,000 pamphlets.

The operation of the copyright law so increased the rate of growth that the accommodations furnished by the three halls and two or three adjacent small interior office rooms soon became quite inadequate for shelving the books and performing the work of the Library and copyright office.

In his annual report of 1871 the Librarian, Ainsworth R. Spofford, briefly called attention to the need for more space, and in the following year reported on the subject at length, earnestly setting forth the imperative necessity for more room, and even the construction of a special building. At that time the collections amounted to 246,345 volumes and 45,000 pamphlets. The subject was soon taken up by Congress and considered through its committees from that time until, by the act of April 15, 1886, the present site, one-quarter of a mile south of east from the Capitol, was selected, its acquisition by the United States provided for, and the construction of a building authorized.

During this long period of discussion many schemes for attaining the desired end, including a variety of plans for enlarging and occupying the Capitol and many different sites in the city of Washington, were considered. Several times did the legislation reach an advanced stage and fail through the pressure of more absorbing interests. Finally the law referred to adopted sketch plans that had been prepared by Messrs. Smithmeyer & Pelz, a firm of Washington architects, but it fixed no limit of cost, nor did it specify the materials of construction or character of execution of the design other than to stipulate that the building should be fireproof. A commission, composed of the Secretary of the Interior, the Librarian of Congress, and the Architect of the Capitol, was designated to conduct the construction of the building. The site, comprising two city squares—nearly 9 acres, within the city building lines and with the included streets—was purchased of the private owners, the ground cleared of some seventy buildings occupying it, and by the summer of 1888 about one-half of the foundation footings for the building were laid. During that year, however, Congress became dissatisfied with the progress that had been made and the uncertainties involved in the operation of the inadequate original law, and accordingly, on October 2, modified it and lodged the entire control of the work, including the preparation of new plans at a limited cost, in the hands of Brig. Gen. Thomas Lincoln Casey, Chief of Engineers of the United States Army. He immediately placed the writer in local charge. On March 2, 1889, Congress enacted that the building should be erected at a total cost of $6,500,000, including previous expenditures, according to a plan that had been prepared and submitted by General Casey, pursuant to the previous act of October 2, 1888. This

LIBRARY OF CONGRESS. VIEW FROM CAPITOL.

plan was based on that adopted by the original act, and provided a building of similar form, dimensions, and architecture. The project embodied the principal materials of construction and a detailed estimate of the cost.

Under these auspices operations were begun in the spring of 1889 where the operations had left off the year before, and the construction thence proceeded without interruption until the building was finally completed, in the spring of 1897. It is 470 feet in length by 340 feet in width, having three stories and a subbasement, and fronts west—toward the Capitol. Each of the six pavilions contains an attic. The accompanying illustrations show the floor plans of the three principal stories and views of the interior and exterior of the building. The first guiding principle of the design to accommodate a nation's library was that an ample general reading room should be located within the mass of the books, to insure their easy reach and access. The next was ample light and air, combined with security and latitude for future growth of the collections and their uses; then that all necessary accommodations should be provided for the constant work involved in the care, use, and increase of the Library and its several departments, including suitable halls for the exhibition of the curiosities and varieties of bibliography and the graphic art.

In planning the building it was also recognized that its peculiar importance and functions, as well as its location near the Capitol, demanded a monumental treatment throughout, and consequently the most substantial and durable construction, and a high order of fine art. In endeavoring to meet all the utilitarian requirements of a national library in the present generation it was found that library science was in a state of rapid evolution, and that the future of the subordinate departmental locations and needs was not sufficiently defined to enable special provisions to be made for them during the construction of the building. As a collection of books, however, to be housed in shelves, and used by readers, the problem of shelving and reading room was much more clearly presented, although book shelving, on a scale of millions of volumes, had not hitherto approached a stage of perfection.

Out of these conditions the plan of the building was evolved. First, a reading room of octagonal plan, to accommodate 250 to 300 readers, with ample light from the sky through clearstory windows at a high angle, is placed in the center of the building. Adjoining this on three sides, stacks of book shelving are located, the main entrance being on a fourth side. Four alternate octagonal sides have wings to the fronts of the building, and the other four sides open into as many large courts. The cross plan thus formed is inclosed by four fronts, forming on the whole a rectangular plan, and containing spaces for the subordinate departments of the library, for the expansion of the stacks of book shelving as future needs will require, and for the exhibition halls, special reading rooms, staff offices, etc. For the reason given the spaces in the front sections of the building are generally not subdivided, but are so disposed as to admit of any desirable subdivision of a more or less permanent character that experience may indicate. At the same time, the arrangements of windows, ventilation, and communication, and the strength of floors are such that any part or the whole of any of the long halls may be conveniently occupied by shelved books, while the corner pavilion rooms are favorable for staff work, reading, etc., and contain also lavatories and stairways.

The building is less exposed and better guarded by having but two entrances, a double or two-story one in front, and a single one in rear. The first is for the general use of the public and the other especially for the delivery and dispatch of freight. The front or main entrance opens into a great staircase hall, 132 feet wide by 122 feet deep, extending the full height of the pavilion through the first and second stories, constructed of veined white Italian marble in richly ornamented design, the vaulting of the lower story in marble mosaic, and that of the upper story in painted decoration. This hall has a full basement on the ground floor which serves as the entrance hall from the porte cochère.

The great reading room or main rotunda opens directly from the stair hall. It is surrounded by two tiers of alcoves shelved for books, and surmounted by a gallery 35 feet above the floor, wherefrom visitors, not desiring to read, may view the rotunda. Special students will find quiet spaces in the alcoves. The reading room has a clear diameter of 100 feet, with hemispherical dome whose crown is 125 feet above the floor. The dome lantern and semicircular clearstory windows of 32 feet diameter above the gallery furnish the light. A few small windows open into some of the alcoves from the courts. By means of translucent glass the direct rays of the sun are sufficiently softened in the southerly windows. The catalogue and attendants' counter and desk of circular form are placed in the middle of the floor, and the readers' tables are arranged around this in three concentric circles. The central desk is in communication with the several stories of the book stacks at all times by means of pneumatic tubes for written messages and speaking, electrical annunciators, and endless-chain book carriers operated by electric motors. The side walls are composed of an alternation of narrow brick piers and plate glass openings extending the full height of the building, admitting ample light from the courts throughout the stack.

An elevator, stairway, and the endless-chain book-carrier are located in the middle of the stack. The windows are fixed and air-tight, all air for warming and ventilation being passed up through the stack from the subbasement by natural draft or fans, and discharged at the roof. Each of the larger stacks will hold 800,000 volumes, and the smaller one about 175,000. This shelving and that of the reading room alcoves will together accommodate about 2,000,000 volumes, which is the present capacity of the shelved portion of the building. The capacity of the large halls above noted as available for extensions of the shelv-

PLATE II.

LIBRARY OF CONGRESS. VIEW FROM SOUTHEAST—REAR ENTRANCE.

ing is about 2,500,000 volumes, making the total ultimate capacity of the building 4,500,000 without encroaching on the exhibition halls or any of the spaces needed for the work of the Library or for special reading rooms. The small book stack is devoted to the Smithsonian collection.

From a point in the basement just below the center of the reading room books of any size are transported in a few minutes to the Capitol and returned by means of an underground endless cable driven by an electric motor. The Capitol terminal of this apparatus, located in a small room east of the statuary hall, is in further communication with the Library by means of a pneumatic message tube, electric bells, and a telephone.

Every part of the interior of the Library building, excepting the basement of the rotunda and stair hall, receives daylight most abundantly through some two thousand windows and skylights, and this is favored by the great width of the four courts and the reflecting quality of the light cream-colored enameled bricks with which their walls are faced.

Some of the larger hall spaces are not yet assigned and occupied by the Library, the collections having but recently been placed in the building and not yet fully arranged, but in the main the first or Library story will contain the executive offices of the Library proper, the private reading rooms for members of Congress, and the cataloguing, manuscript, periodical, and Smithsonian rooms, together with the Toner collection and Washingtoniana, and perhaps the music and map rooms; that is to say, the first floor will be the Library floor proper, not only for the great public reading room before described, but for all principal collections, executive offices, and the main entrance hall of the Library. At least one-half of the second or upper story, which is 29 feet in clear height, is to be occupied for the exhibition of graphic art and curiosities of bibliography and literature. The first story has a height of 21 feet, floor to floor. The basement story, on the ground floor, is for the offices of superintendence and maintenance of the building and grounds and disbursements for the whole Library, the extensive copyright department, reading room for the blind, book binding, mailing, packing, receiving and delivering, storage, etc. This story is 14 feet high from floor to floor. Each of the six pavilions—being one at each corner of the building, and two in the east and west fronts—contains an attic story well ventilated and lighted and suitable for safe storage and various kinds of subordinate work. In that of the west center pavilion there is a convenient restaurant for the accommodation of the Library attendants and visitors.

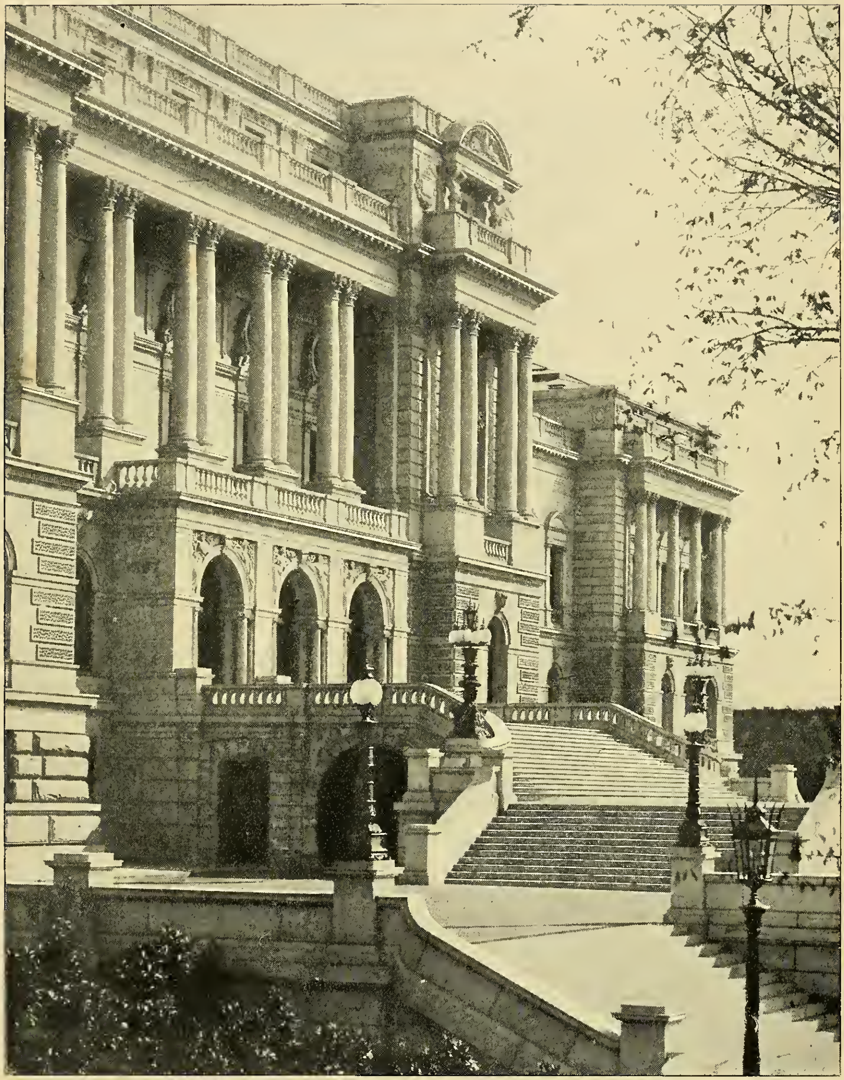
The cellar, or subbasement, extends throughout the area of the building and is occupied by the warming and ventilating apparatus and most of the machinery. The boiler battery, steam pumps, coal vaults, ash hoist, etc., are located under the east approach outside the building. No fires are required within the building and all discomforts of the proximity of the boiler room are avoided. There are sixteen

60-horsepower horizontal shell tubular boilers, and a coal storage capacity of 3,000 tons. The steam operates the hydraulic pumps for elevator power and water supply, the air compressor for the pneumatic-tube system, and the dynamo engines for electric lighting and power apparatus, the exhaust from which is passed through eight sets of tubular tanks for heating the water required for warming the building. By circulation of this hot water through cast-iron coils of 3-inch pipe, inclosed in chambers of brick distributed throughout the cellar, to which it is carried and returned by pipes, fresh air from outside the building is warmed and passed by natural draft up wall flues to the respective stories and apartments above. The air thus taken into the building and circulated through the rooms is discharged at the roof through other wall flues.

A complete system of electric lighting is provided for night service, including both the building and surrounding grounds; also means of internal communication between all parts of the building through cables of electric wires laid in the floors, with frequent points for access and connection without disturbing the permanent structure.

The foundations of the building are of hydraulic cement concrete, 6 feet deep in ground which is a mixture of clay and sand of very uniform character. The cellar walls are of hard red brick; the exterior face of the superstructure of a fine grained light blue granite from Concord, N. H.; the stone of the rotunda and the trimmings of the court walls a light blue granite from near Woodstock, Md.; the facing of the court walls enameled brick from Leeds, England; and the backing and interior walls as well as all of the vaulting of the basement and first stories are of hard red brick. Most of the floors that are flat ceiled are of terra cotta, and this material also forms the covering and filling of the roofs and main dome, of which the supporting members are of rolled steel in beams, girders, and trusses. All of the floors are leveled up with concrete and surfaced with tiles, terrazzo, or mosaic in the public spaces, while in the office and working rooms they are covered with a carpet of southern pine boards.

The most important of the strictly useful features of the building are the book stacks, of which the design is largely original. The problem was new, not only through the capacity to be provided but the numerous other conditions to be met, such as light, ventilation, adjustability to several uses, communication, immunity from fire, cleanliness, durability, and simplicity. It was also necessary that rapid mechanical transmission of books between the shelving and the reading room should be provided, coupled with a quick and reliable means of communication, both written and oral. Existing libraries lacked nearly all of these qualities and appointments and it behooved the designers of the new building to take advantage of modern materials and mechanical resources and devise the best possible system of shelving and mechanism to meet all the requirements. This was quite successfully accomplished. Having prepared the courts of the building and the walls of the



LIBRARY OF CONGRESS. MAIN ENTRANCE—WEST FRONT.

stacks, as above described, a slender, rolled steel framework of continuous columns and horizontal deck members was introduced, dividing the stack into nine stories of 7 feet each. The decks are covered with white marble slabs and contain narrow openings in front of the shelf ranges for communication, admission of light, and circulation of air. In each story on either side of every column a simple vertical cast-iron wing is attached, having the necessary teeth and lugs to receive the shelves and permit of their ready adjustment to any interval desired. The shelf is of a gridiron form, being a set of parallel bars of ∩ section, forming an extremely stiff and smooth shelf, capable of receiving anything that any shelf may be called upon to carry while its weight does not exceed that of white pine wood. The shelves may all be removed from any bay or series of bays on a deck to make a passageway or insert any convenient piece of furniture, such as a card catalogue, desk, or cabinet of any kind. The shelf length is uniformly 38 inches in all the stacks and the shelves are therefore quite generally interchangeable. Thus, by indefinite multiplication of the few simple elements described, a stack of almost any required dimensions and capacity may be constructed. Proper anchorage of the horizontal deck bars in the surrounding walls is requisite for lateral stability of the loaded stack, and some other constructive details must be observed, but it is a simple thing to make a stack of twenty or more stories or tiers in height and of almost any other dimensions.

The book carrier is a pair of parallel, endless chains, running in a vertical shaft in the middle of the stack; thence in a horizontal duct in the cellar to a point below the central desk of the reading room, where it turns upward and ascends vertically to the delivery outlet at the desk. A series of equidistant book trays, eighteen in number, are suspended between the chains. The machine runs continuously and automatically takes on and delivers books of the size of a quarto or less at its reading room terminal and at each of the stack stories. The speed of the carrier is about 100 feet per minute.

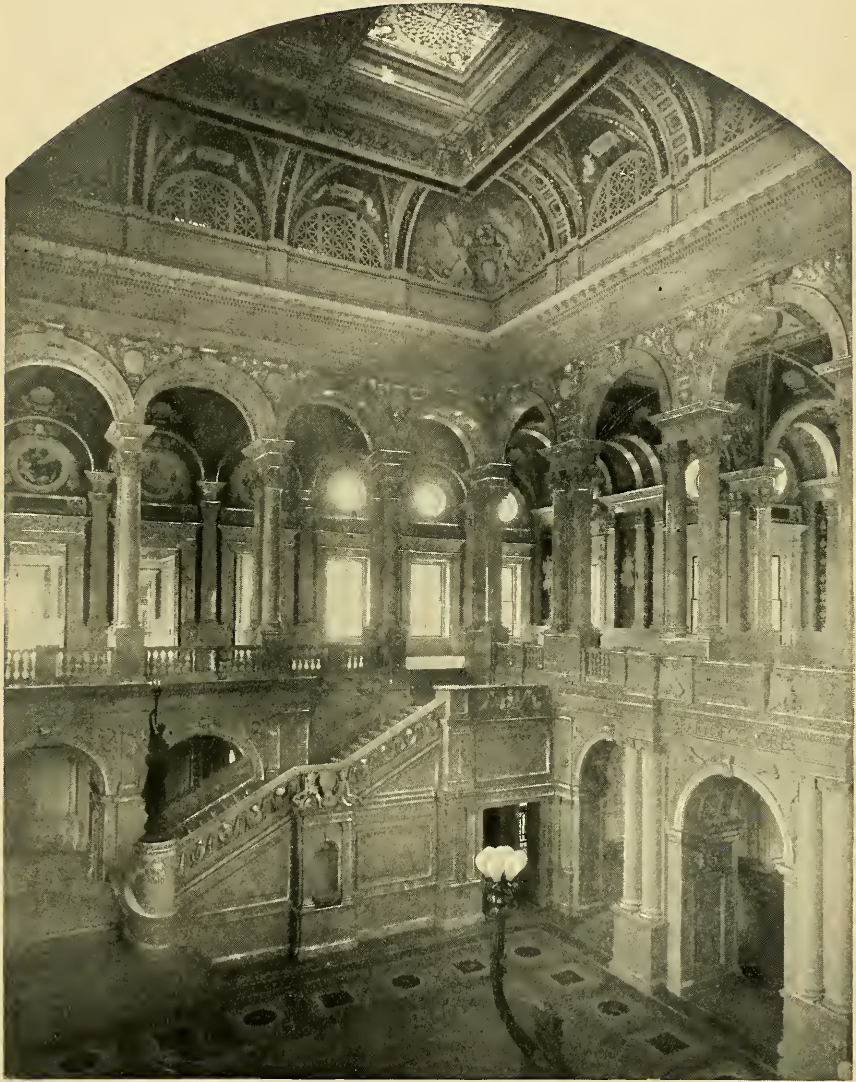
The pneumatic message tube is also convenient as a speaking tube.

The great rotunda or public reading room of the building, the main staircase hall or foyer, the private reading rooms for the members of Congress, the Librarian's office, the corridors communicating with these, and the exhibition halls as well as many portions of the exterior walls, especially the west main pavilion, have received a good degree of artistic treatment and embellishment, but all within strict architectural requirements. Some forty sculptors and mural painters, about equally divided in numbers, furnished the principal works of art under the architects' supervision and direction. Many appropriate quotations and names are inscribed on the walls in the architectural tablets, freizes and panels, adding to the general impressiveness and interest of the building.

In all ways and from all points of view the library building is eminently instructive as an example of good design, good appointment for its great purpose, good building and good administration in the execution, and therefore the more appropriate to house the nation's library.

The unusual success of the undertaking under Government auspices is almost wholly due to the selection of a known competent, sturdy, and faithful individual such as General Casey was, and giving him the sole charge directly under Congress without an executive superior liable to interfere and cause delays. The work went on quietly, but with energy, and was completed within the originally estimated time and well within the legal limit of cost.

The total cost of the building was $6,344,585.34—that of the site, $585,000—complete records of the work are preserved in drawings, photographs, correspondence, and accounts. Mr. Paul J. Pelz, of the original firm of architects, was employed to assist as architectural designer, remaining until the drawings of the building as a whole were completed, when he was succeeded by Edward P. Casey, architect, whose work comprised chiefly the design and supervision of all the more artistic as well as many of the important fundamental features of the interior. The writer designed the construction throughout, the book stacks and much of the apparatus, and supervised everything.



LIBRARY OF CONGRESS. STAIR HALL AT MAIN ENTRANCE.

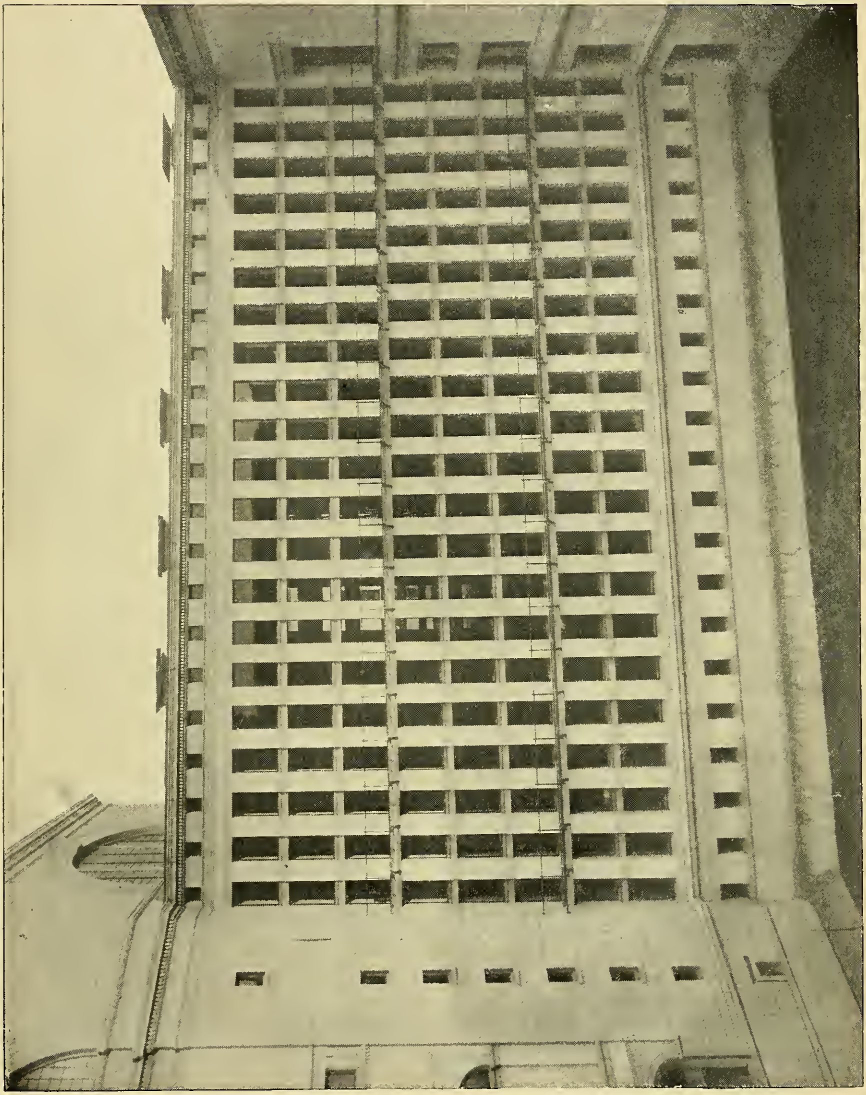
LIBRARY OF CONGRESS. READING ROOM.

LIBRARY OF CONGRESS. READING ROOM GALLERY—SHOWING STATUE OF PROFESSOR JOSEPH HENRY.

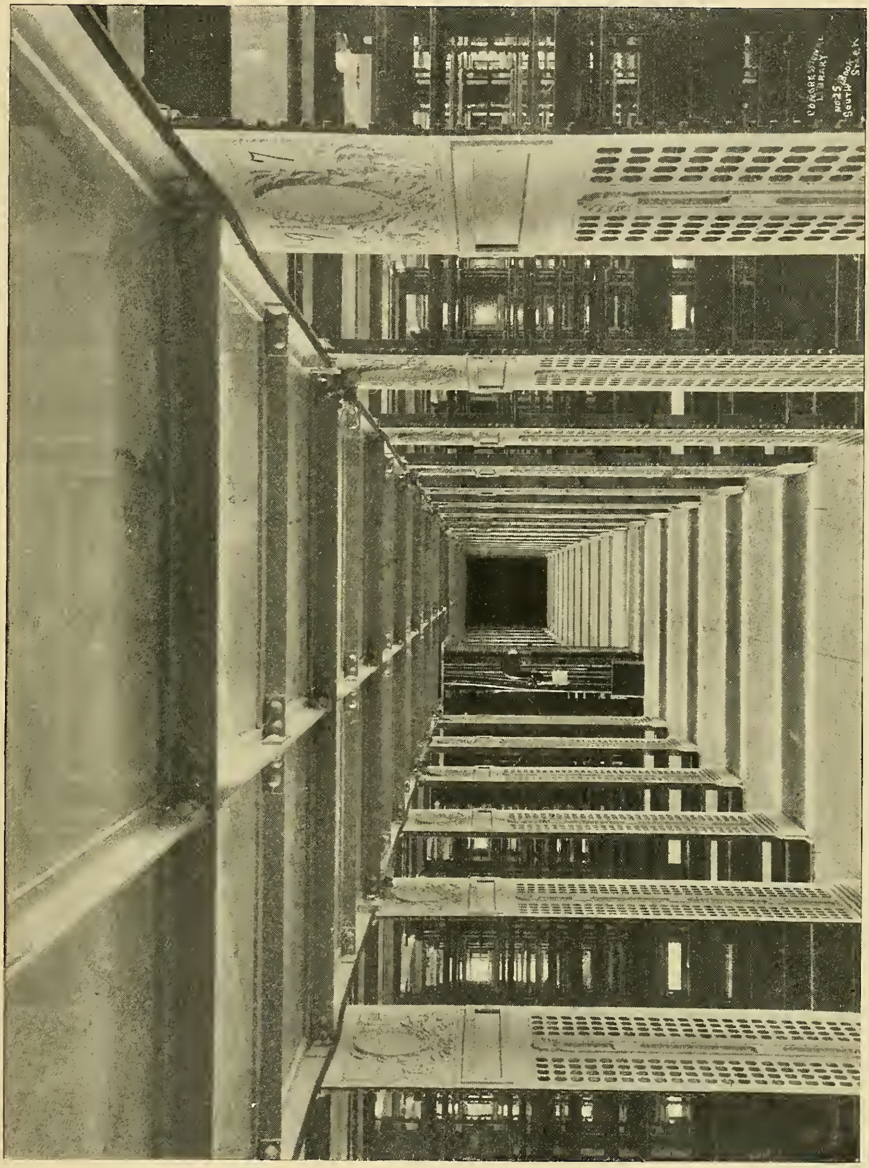
LIBRARY OF CONGRESS. READING ROOM GALLERY.

LIBRARY OF CONGRESS. SOUTHWEST COURT.

LIBRARY OF CONGRESS. SOUTHWEST COURT.

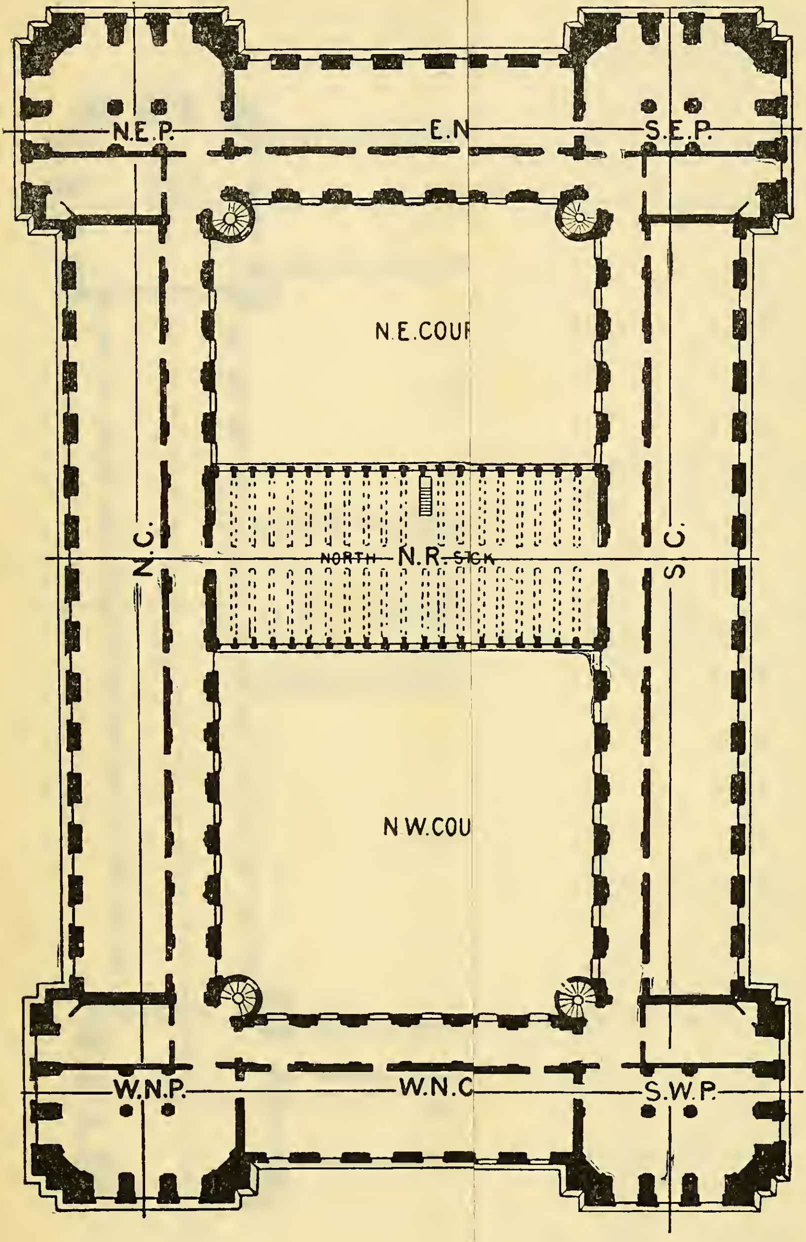


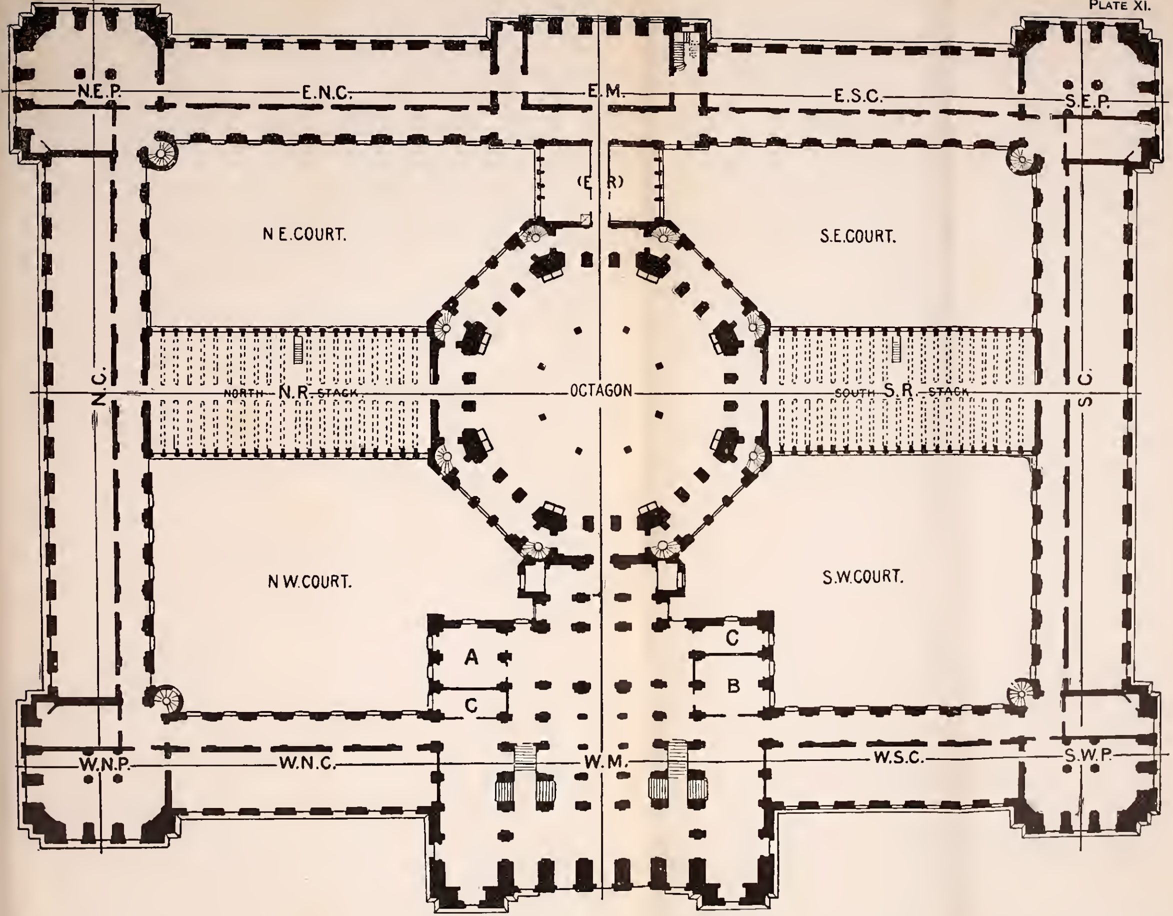
LIBRARY OF CONGRESS. SOUTH BOOKSTACK—SOUTHWEST COURT.



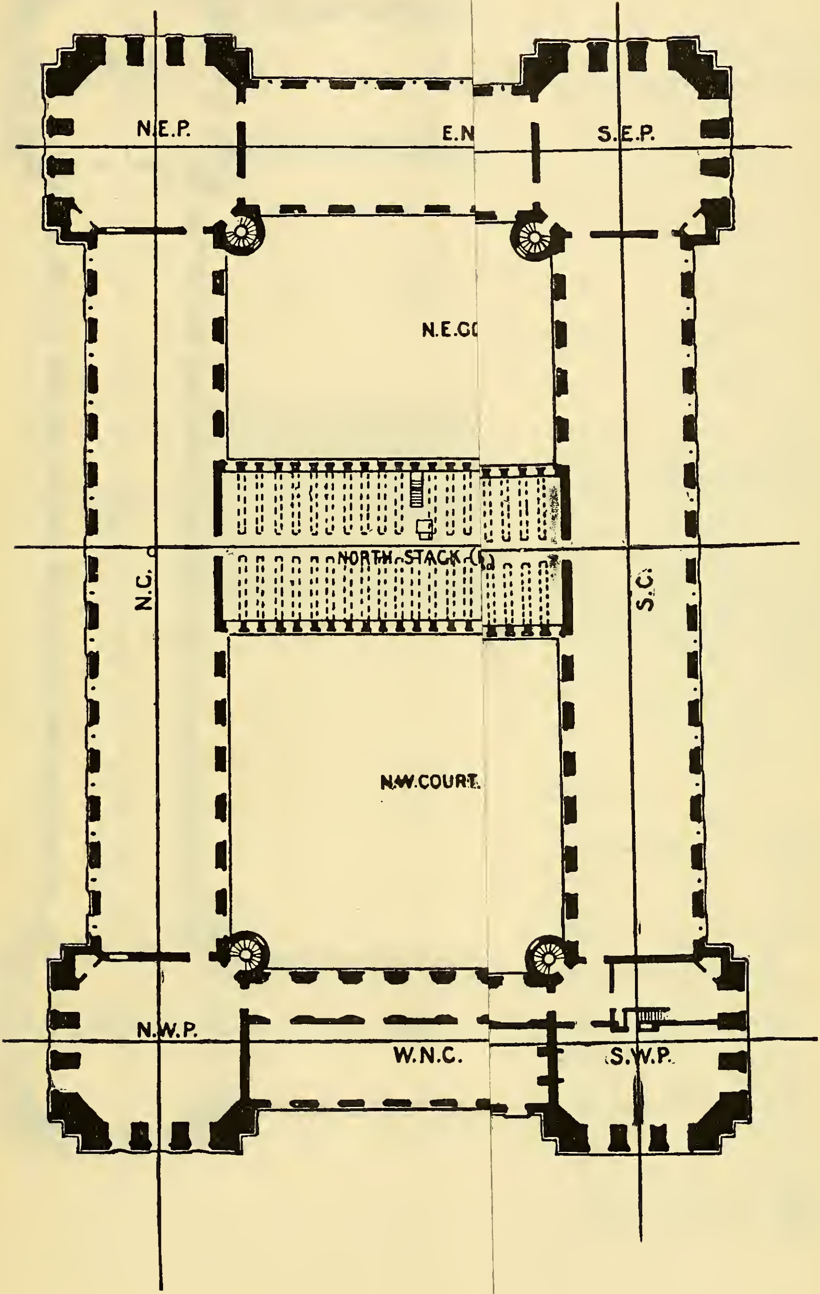
LIBRARY OF CONGRESS. A TIER OF SHELVING—SOUTH BOOKSTACK.

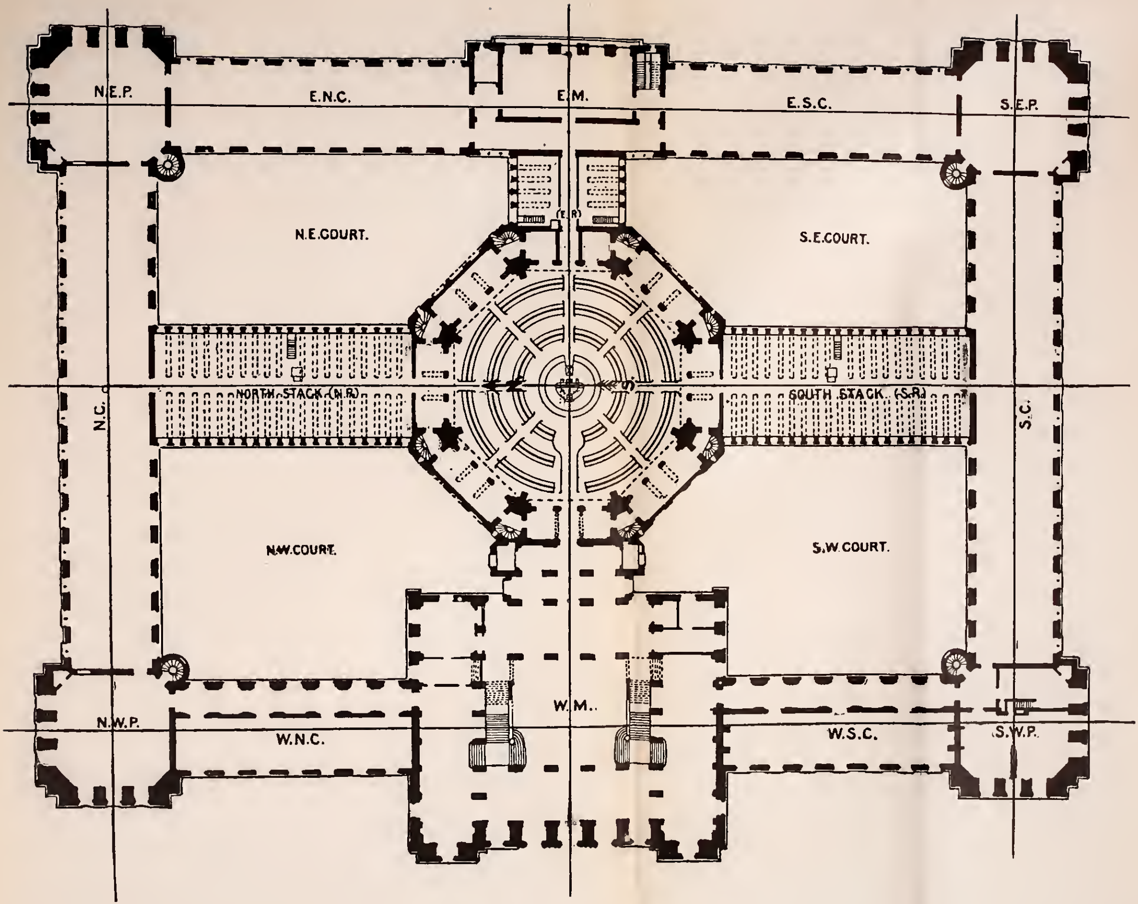
PLATE XI.



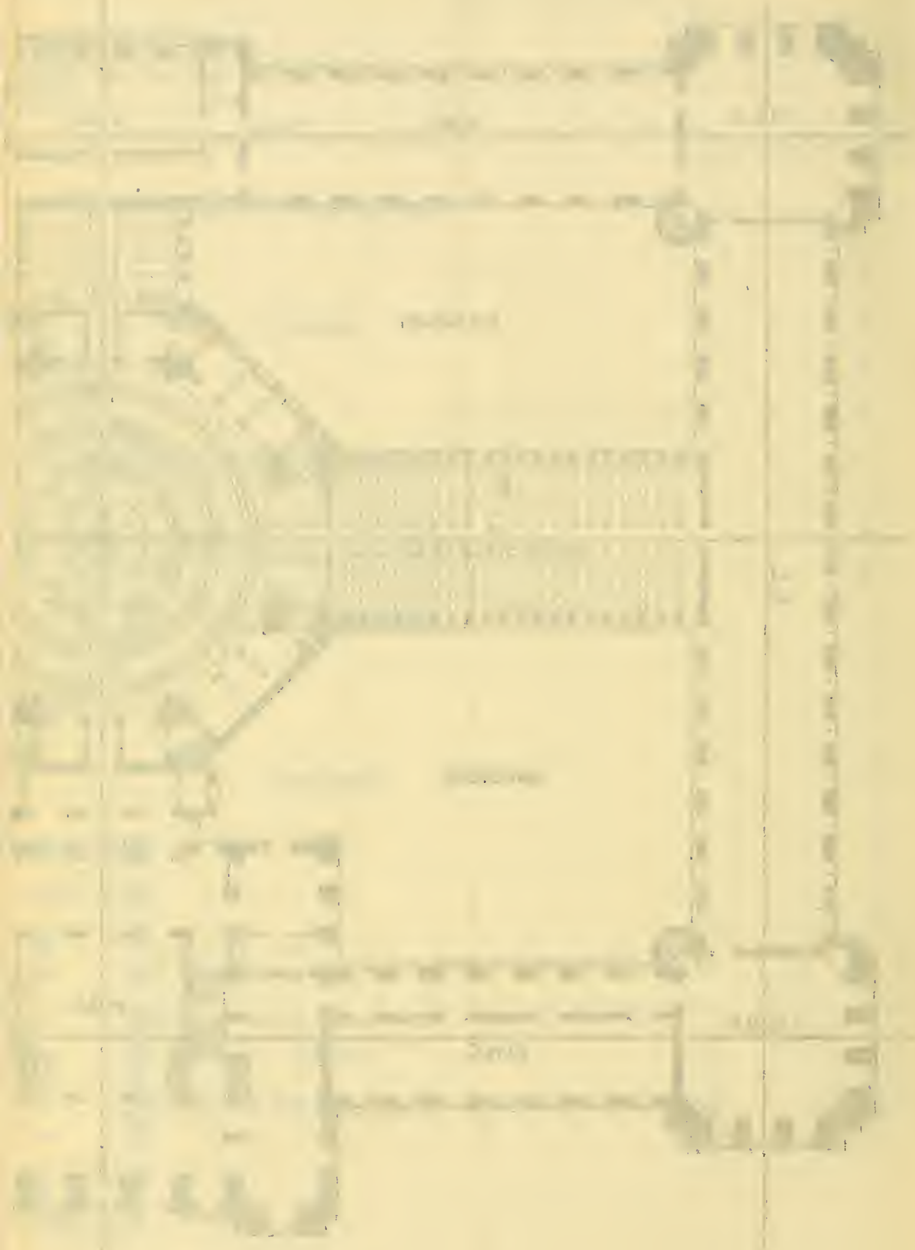


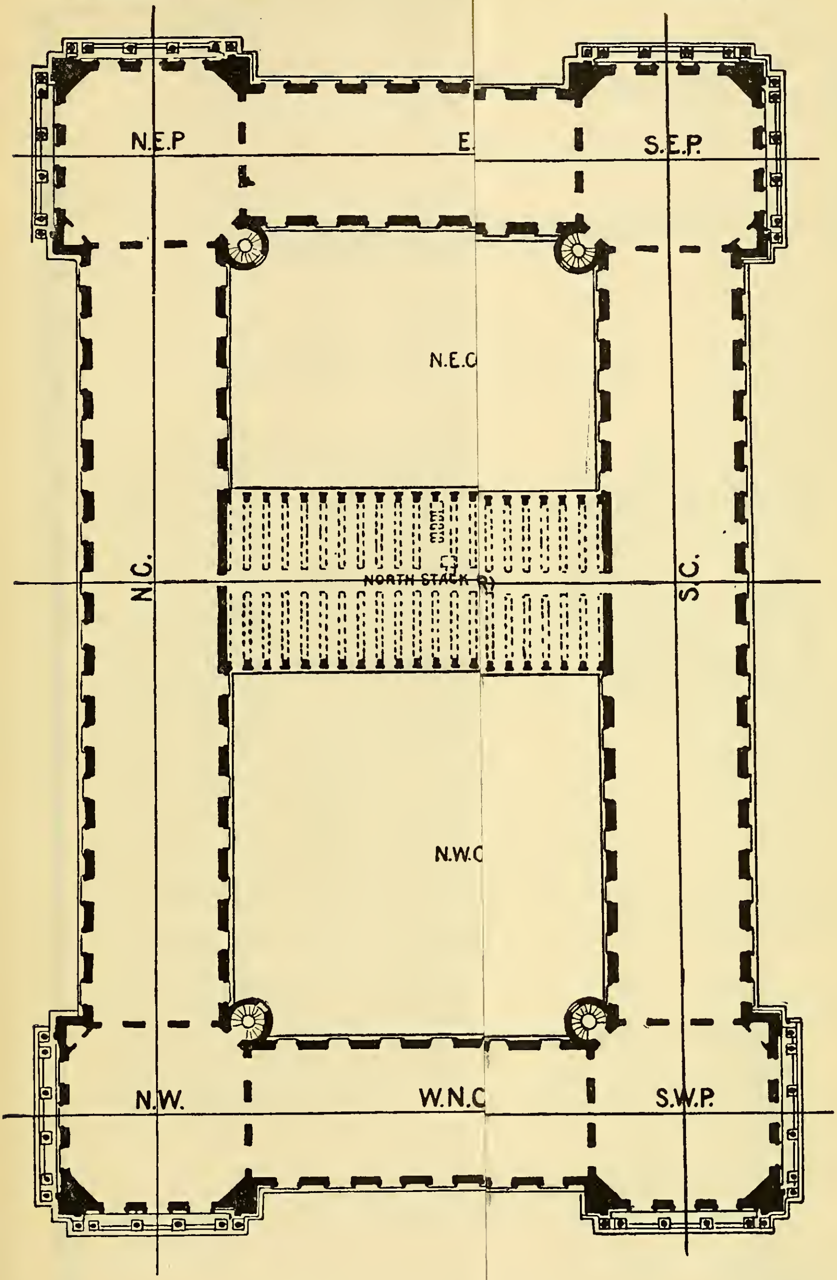
Plan of Basement Floor of Library Building.


N.E.P.
E.N
S.E.P.
N.E.C
NORTH STACK (5)
N.C.
S.C.
N.W.COURT.
N.W.P.
W.N.C.
S.W.P.



PLAN OF FIRST STORY OF LIBRARY BUILDING.




N.E.P
E.
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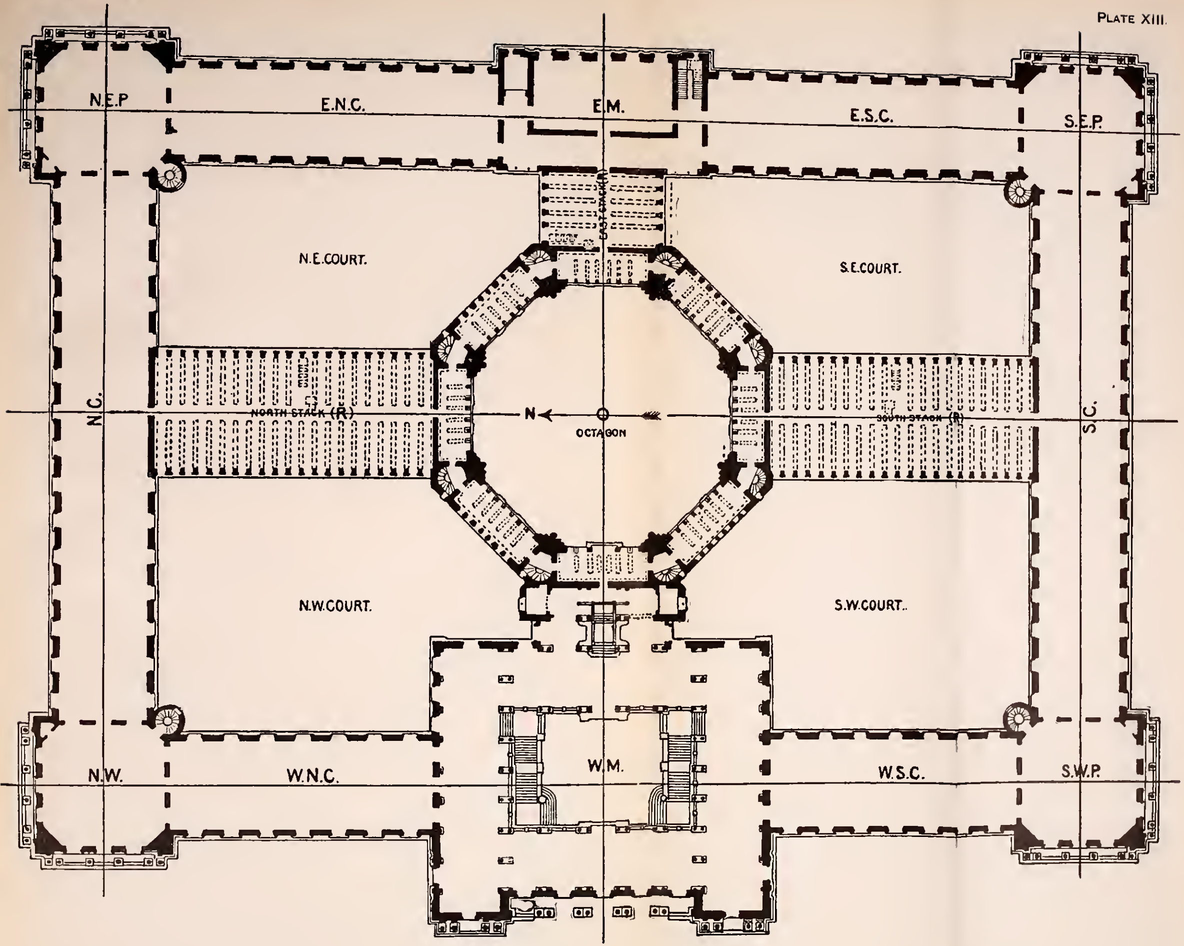
NORTH STACK
S.C.
N.W.C
N.W.
W.N.C
S.W.P.



PLAN OF SECOND STORY OF LIBRARY BUILDING.

GEORGE BROWN GOODE.

Born February 13, 1851. Died September 6, 1896.

It had been the intention of the Institution to place here a biography of its late Assistant Secretary, Dr. George Brown Goode, but on account of his special charge of the Museum it has been thought better to transfer this to the Museum volume, where an account of his scientific labors can be found at length. The Secretary, however, does not wish to leave without remark the page which contains this reference to one whose life was intimately associated not only with the Museum, but with the parent Institution, of which he was, as it were, a son, and where he was so greatly loved and trusted by all.

S. P. LANGLEY,

Secretary of the Smithsonian Institution.

FRANCIS AMASA WALKER.

By George F. Hoar and Carroll D. Wright.

I.—EXTRACTS FROM ORATION BY SENATOR GEORGE F. HOAR.[1]

We have come to pay a public debt, so far as we can pay such debts, to a public benefactor. Massachusetts has no orders of knighthood, no robes, or star, or jeweled ribbon of Bath or Garter, no coronet or crown for those who have served her. She gives them no title of rank or nobility. She endows them with no lands or castles to leave to those who bear their name. Their children must begin again, for themselves, by the side of the humblest and poorest, with no advantage but the stimulant of the father's example, and the feeling, if they be of noble mind, that the State holds a pledge of them. We have no mausoleum, or cathedral, or abbey, like that—

> "Where Death and Glory a joint Sabbath keep,"

to which Nelson looked forward on the morning of his death and his immortality. We do not consign the dust of the men we delight to honor to sleep the last sleep among rows of warriors and walks of kings. A simple Well done! coming from the heart of the people, takes, for us, the place of crown, and coronet, and rank, and title, and broad lands, and dome, and arch. Yet we should not be here to honor General Walker unless we knew that he would have preferred what we have to bring—the esteem and love of Massachusetts, with the conscience that he deserved them, to all other honor, or wealth, or glory.

I do not purpose, in what I have to say to-night, to speak of Francis Walker as a man of great and remarkable original genius, as that word is commonly used. Whether he possessed or lacked that undefinable quality, it is not that which brings us here. It is because he was an admirable example, perhaps the best example Massachusetts has to offer in late years, of a complete and rounded citizenship. The excellence and variety of his work grow upon you as you study it. He was a more useful man and a safer guide than any man, with

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[1] From oration by Senator Hoar at the memorial meeting at Boston Music Hall, October 14, 1897.

rare exception, that we have had of great and remarkable original genius. He was wise, industrious, conscientious, patriotic, faithful, and brave. He had an independent intellect rendered prudent by a great modesty. He administered with remarkable success the Indian Bureau, the Census Bureau twice under vastly different conditions, and a great college. He began his service in the Union Army as a noncommissioned officer. He left it a general officer. He was severely wounded. He was six weeks in Libby prison, where he met his brother. He was twice promoted for gallantry in the field. He was a favorite aide of three famous generals. He was frequently praised for soldierly quality in the reports of his superiors and in general orders. He was a model adjutant-general. He saved the day in an important battle. He was our foremost writer and thinker on political economy in a generation constantly called upon to reconsider and to apply economic laws. He contributed to science our best example of the scientific temper. He was a profound thinker. He was a successful teacher. He was a lover, inspirer, and leader of youth. He concealed great power by quietness in his work. He did, for nearly forty years, bravely and faithfully, what his generation much needed to have done. He was a constant spectator and critic of what other men were doing in other departments of human activity. Yet he never lost his kindness of heart or his brave hopefulness. He loved his country, his Commonwealth, the town where he was born, and the city where he lived. He was perfect in the relation of son, brother, husband, and father. He was unselfish. He was a constant, loving, and faithful friend. He had no ignoble ambitions. His objects in life were public, not personal. The ends he aimed at were—

"his country's,
His God's, and truth's."

Every step in his life was upon a stair of honor. His sound brain and athletic frame could bear great labor without fatigue. He had a thoroughly healthy and robust intellect, capable of being directed upon any of the pursuits of life, or any of the affairs of State in any department of the public service. He exhibited a varied and sound mental capacity which made it sure that he could have attained distinction as a metaphysician, or a mathematician, as a linguist, as an advocate, as a lawyer, as a legislator, as a judge, as he did in fact attain it as a soldier, as a teacher, as an administrator, as a statistician, as a writer and reasoner upon abstruse doctrines of political economy. As Mark Hopkins said of his pupil, President Garfield, "there was a large general capacity applicable to any subject, and sound sense. What he did was done with facility, but by honest and avowed work. There was no alternation of spasmodic effort and of rest, but a satisfactory accomplishment in all directions of what was undertaken." * * *

He was born in Boston July 2, 1840. He descended in the eighth generation from Capt. Richard Walker, of Lynn, a man active in the church and in the town, a member of the Honourable Artillery Company of London, and one of the first members of the Ancient and Honorable Artillery Company of Boston. The Walkers in every generation were good farmers and soldiers and important men in the town. His great grandfather, Phineas Walker, who moved to Worcester County, and built a house on land where his descendants now dwell, served in the old French war under Wolfe; was a captain in the Revolution, and was at the taking of Ticonderoga with Ethan Allen. His grandfather, Deacon Walter Walker, a blacksmith and farmer, moved to North Brookfield in 1800. There his children were born. He was the principal citizen of the town. His wife, Priscilla Carpenter, originally spelled Charpentier, was of French Huguenot descent. General Walker was of Huguenot descent on his mother's side likewise. This is a strain of which we have many delightful examples. The character and quality of our New England women do not often get into the town records. But it is noticeable that so far as we have accounts, certainly for three generations, the women from whom he descended were remarkable for intellectual vigor and great public spirit. General Walker told the story at the Commercial Club of his going home unexpectedly on leave of absence at the darkest time of the war, in the fall of 1862, when almost every household in Massachusetts was in mourning, and officers were resigning, and desertions frequent. He came into the house unseen early in the morning, and found his mother with an open Bible in her lap—not reading, but with her gaze fixed toward the south, thinking of her absent sons.

"Instead of the glad welcome I had expected came the quick, sharp question, 'You haven't left the Army, have you?'" His mother was one of a family of four sisters, all women of commanding presence and character, having, as Mr. Spencer, the North Brookfield pastor, says, "an intense regard for righteousness." * * *

He began to study Latin when he was 7 years old. He fitted for college at Leicester Academy and at Lancaster. He was graduated from Amherst at 20, where he got two prizes for extempore speaking. He studied law in Worcester.

He enlisted as a private in the Fifteenth Massachusetts Volunteers, a regiment never excelled on the face of the earth for soldierly quality. Soon after his enlistment he was made sergeant-major. Very soon he was commissioned as captain and assistant adjutant-general. He was promoted to be major, then to be lieutenant-colonel, then brevetted general. He served on Couch's staff and Warren's, and was chief upon Hancock's. Two of his promotions were for gallant conduct on the field. His name receives honorable mention in the reports of many battles, including Williamsburg, Fair Oaks, Malvern Hill, Fredericksburg, Chancellorsville, Bristow Station, Wilderness, Spottsylvania, Petersburg, and Reams Station. He was severely wounded at Chancellorsville. General Couch reported that at Fair Oaks he "made a daring personal reconnoissance, and had his horse shot under him." He was a model adjutant-general; and to be a model adjutant-general demands high intellectual and literary capacity. General Warren says of him in his report of the campaign: "He is fully acquainted with his office duties, so important to the operation of an army corps. He is willing and gallant on the field."

He was by Hancock's side at Reams Station and received honorable mention in Hancock's report. He rode into the enemy's lines in the darkness and was captured. He made a gallant dash for liberty and swam the Appomattox River, but was taken again by the enemy as, exhausted by the last stroke for which his strength held out, he gained the shore. He was six weeks in Libby prison. He was made brigadier-general for gallant conduct at Chancellorsville, by Hancock's express request. * * *

Walker did not come back from the war spoiled for the duties of civil life. I would not speak unkindly of those to whom that happened. Commonly they could not help it. The cause of it, in many cases, was physical and not moral. A malarial fever or a wound often takes out of a man the courage and steadfastness needed for common business who never would flinch before the enemy or the hardship of a campaign life. The anxiety, the hope and fear, the constant peril, the din of battle, the thunder of the cannon, and the shouting—I do not see how the sound of shot and shell could ever afterwards get out of their ears—how could they not spoil the boy for the quiet duties of the farm and the shop? It was the sacrifice he made for his country, a sacrifice even more costly than life itself. But to Walker, as to many another good soldier—as to Grant, as to Devens, as to Garfield, as to McKinley—the military training was the very source and inspiration of civic greatness. * * *

Walker was 24 years old when he came home from the Army, broken down by imprisonment and hardship. It took several years to get back his health. But he set himself to work at once. His purpose was to go to work wherever he could find work to do. His path in life seems to have been determined by opportunity and not by inclination. He was employed as a teacher of Latin and Greek in Williston Seminary for more than three years. From March 15, 1868, to January 15, 1869, he was an assistant editor of the Springfield Republican, and wrote, he says, about two-thirds of the editorial matter. He went from Springfield to Washington. From that day to his death he led two busy and crowded lives. One was the life of a practical administrator of large affairs; the other the life of a theorist, an industrious student and thinker in the closet. He administered in succession a variety of important executive offices. He was the author of a great number of books, addresses, and papers on interesting and living questions. * * *

He taught Latin and Greek at Williston from 1865 to 1868.

Assistant editor of the Springfield Republican in 1868.

Professor of political economy and history in Sheffield Scientific School, at Yale, from 1873 to 1881.

Ten years trustee of Amherst.

President of the Massachusetts Institute of Technology from 1881 till his death.

He was appointed deputy special commissioner of the revenue in the service of the United States January 15, 1869.

He was then made Chief of the Bureau of Statistics, and held that office until he was appointed Superintendent of the Ninth Census in 1870.

He became Commissioner of Indian Affairs.

Superintendent of the Ninth Census until he was appointed Superintendent of the Tenth Census, April 1, 1879. He held that office till 1881.

He was chief of the bureau of awards at the Centennial Exposition in 1876.

Member of the school committee of New Haven from 1877 to 1880.

Member of the board of education of Connecticut, 1878 to 1881.

United States Commissioner at the Monetary Conference at Paris in 1878.

Member of the Massachusetts board of education from 1882 to 1890.

Lecturer in Johns Hopkins University, 1877 to 1879.

Lecturer in Harvard University in 1882, 1883, and 1896.

Chairman of the Massachusetts Topographical Survey Commission, 1884 to 1890

Member of the school committee of Boston, 1885 to 1888.

Member of art commission of Boston from 1890 till he died.

Member of park commission of Boston, 1890 to 1896.

Chairman of the Massachusetts board of the World's Fair Managers, 1892 to 1894

Trustee of the Boston Public Library in 1896.

President of the American Statistical Association from 1882 until his death.

Vice-president of the National Academy of Sciences.

He helped to found the International Statistical Institute, of which he was an honorary member and président-adjoint.

President of the American Economic Association from 1885 till 1892.

President of the Society of Arts.

Vice-president of the Military Historical Society of Massachusetts from 1891 to his death.

Vice-president of the American Society for the Promotion of Profit Sharing from 1892 till his death.

Vice-president of the Society of Naval Architects and Marine Engineers from 1892 till his death.

An officer of the French Legion of Honor.

Perhaps this enumeration may seem tedious; yet it is by no means complete. It is peculiar to this man that the bare, naked catalogue of his useful deeds is too long to leave room for comment if the sketch of his life is to be within moderate bounds. The offices he filled have no glamour about them. When filled by a great man they are great offices; when filled by an insignificant man they are insignificant. He gave dignity to every place he held.

The ease with which he passed from one field of service to another, which we seldom find among eminent men of other countries, and which, indeed, the structure of their society renders impossible, was characteristic of him, even as compared with his own countrymen. His intellect never seemed jaded or strained. He worked rapidly, but he rarely gave the impression of being hurried in his intellectual work and never of being tired. He had light without heat, although quite capable of lofty enthusiasm and deep affection, as witness his eulogies on Hancock and Devens.

He did his work with a quietness which concealed its power. He was not a little, rattling steam engine that goes puffing and smoking and spitting along, with the clang of machinery and the smell of oil. He was an electric current. It was conveyed by a slender and almost invisible wire, exerting its force without heat or noise or odor, but it never failed to do its work and to carry its load.

Every one of these offices he administered with signal success. You can not find an instance of any duty undertaken by him which was not faithfully and thoroughly discharged. You can not find an instance of bad or superficial intellectual work. He received more honorary degrees in this country and abroad than any other American left alive when he died. * * *

General Walker brought the result of this varied experience and the fame of these achievements to the service of the Institute of Technology in the autumn of 1881. It was a time of great discouragement here. There had been barely 200 graduates up to 1878. The number of students had fallen within six years from 348 to 188—almost one-half. Professor Tyler says of the friends of fifteen years earlier: "Some were disheartened and some were dead." The very existence of the institute seemed for a time in peril. It had been seriously proposed to merge its identity with one of the departments of Harvard University. I should do injustice alike to an admirable and devoted faculty and to the liberality of this community if I were to attribute the change which these sixteen years have shown to any one man; but surely he was born under a fortunate and auspicious star, of whom it can be said that he touched nothing that did not spring into new life and rejoice in new health when he touched it. Nil tetigit quod non inspiravit.

He was selected by President Rogers as the fittest person to be found in the country to carry out his comprehensive plans. Leland Stanford, as Mr. Stanford himself told me, tried afterwards with all his might to get him to do the same thing for his university. I do not lay great stress on the fact that in his time the roll of students increased from 302 to 1,198, or that you had, when he began, 39 teachers, and when he died, 153. That is the story of many of the schools and colleges of the country, certainly of the North, during the same period. General Walker's lot was cast at the time of the marvelous growth of the country in wealth and power. He was here for fifteen years out of thirty, in which the country gained thirty-eight million in population, and in which its valuation grew from seventeen thousand million to thirty thousand million, and in which it became the foremost manufacturing nation in the world, and the richest nation in the world.

The endowment of institutions of learning has been going on all over the North and West. Contributions to the cause of higher education in this country, which have been published in the newspapers, and were large enough to attract the notice of the statisticians of the Bureau of Education, have amounted, in the last twenty-five years, to nearly two hundred million dollars.

His work was done in Massachusetts and in Boston, and that means that he had behind him as generous and liberal a community as ever existed on the face of the earth—a community specially alive to the value of institutions like this. It would be a dishonor to the great old presidents, the Josiah Quincys, the James Walkers, the Mark Hopkinses, the Woolseys, the Jeremiah Days, and the Timothy Dwights, who administered our colleges in the days of their poverty, to measure their success either by money or by the number of their students. General Walker's title to gratitude is that in his time this institution grew toward the zenith as well as toward the horizon; it is that while the number of students grew, the efficiency and fitness for their life's work, to which their diplomas certified, grew in like proportion; it is that the personal character of the president exerted an ennobling and enlarging influence on the character and intellect of the pupil. Never, if we may trust the testimony of those who know best, was it surpassed in this country, unless possibly in the single instance of Mark Hopkins. He understood every need of the institution. The school felt his touch in every nerve. He brought his great administrative abilities to bear in every department and upon the smallest detail, as will be seen if you read his reports. He was a lover of young men. He sympathized with them in their studies, in their athletic sports; he had a kindly tolerance for their foibles and faults. The pupils and the younger teachers of the Institute of Technology loved the president, and the president loved them. * * *

He had inherited a spirit of independent thinking and an aptness for economic study. His father, a successful merchant, without college training, had been attracted to such studies by the desire to understand the laws on which the success of his own business depended. He became deeply interested in the currency, wages, and freedom of commercial intercourse. He was a thinker and investigator of great independence; early an antislavery man, and an influential advocate of cheap postage and sound banking. He was everywhere respected and had much influence in his day. During the war he saved Secretary Chase from a grave error, the evil of which we could not easily have gotten rid of. Mr. Chase had purposed to issue an adulterated silver coinage, to make the value far above the true value, and had prepared a bill and sent a letter to the Senate and the committee of the House of Representatives recommending it. This he showed to Amasa Walker, who was then in Washington, and gave up the plan in consequence of his sharp expostulation.

In dealing with this part of General Walker's life I am afraid some of my audience will think it is as if I were to deliver an address on Shakespeare and were to begin by saying that I would not undertake to estimate Shakespeare's rank as a dramatic poet. But the time has not come for a final opinion as to General Walker's rank in the department of political economy. I should be presumptuous if I were to undertake to measure the permanent value of his contributions to this science, if that mean that we are to affirm in confidence what among his conclusions are to stand the test of time and to be accepted hereafter as settled truth. One thing we may say, Walker thoroughly understood the limitations of his science and of his own intellect. He brought to these discussions, where usually there is so much heat and arrogance and anger, a sweet and gentle personal quality; but it will be a great mistake if we let this quietness of tone and modesty of spirit mislead us into thinking that he lacked either earnestness of conviction or depth of investigation, or that he was not able to defend himself with vigor and spirit against any antagonist. Other men have prepared themselves for affairs by studying political economy. He prepared himself for studying political economy by a large and intimate knowledge of affairs at first hand. The fact that he maintained always so large a relation to practical life was, in my judgment, his great advantage as a public leader over other men who have been eminent economists. How many other writers on political economy would have been fit to administer a technical school? How few others are ever trusted by their fellow-citizens either with public responsibilities or with the management of important private affairs? * * *

The great celebration of the three hundredth year of the University at Dublin, in 1892, where the universities of Europe, of America, of Africa, of Australia, of India, and of New Zealand were represented, was a scene of the kind never approached in Europe for dignity and splendor, unless we except the tercentenary of Leyden or of Edinburg, the five hundredth anniversary of Heidelberg, or the eight hundredth anniversary of the University of Bologna. Seventy-one honorary degrees were conferred upon men famous in church and state, in science and literature. The degree of Doctor of Laws, the highest degree of all, was conferred upon five persons only—two Englishmen, one German, one Frenchman, and one American. The American was President Walker. Edinburg conferred upon him a like honor in 1896. * * *

He was a great inspirer of economic thought. There was no rashness about him. He discussed the economic problems for which his generation was ready. He boldly took by the throat the delusions which had gained a strong hold upon the thought of men who went before him. His training as a statistician had taught him always to keep close to his facts. To use his own words in giving advice to a scholar, he kept himself superior to partisan dictation, and to the seductions of theory.

He rejected the doctrine of laissez faire. He held the state to be a moral being. It was meet that its great forces should do what the individual can not do for himself. It was not only that the state should be great in wealth and empire, but that it should produce a noble growth of men. Every economic law was lacking in comprehension that left this out.

To him political economy was an applied science. It was a science to be applied forthwith for the benefit of his own generation, and his own country. There was no alteri seculo about him. He was not content to keep a mighty magnet in his museum, holding its weight till a later generation should accidentally discover and make useful the resources of its power. Still less could he tolerate for an instant that arrogant and disdainful spirit with which some men who undertake, with little title, to represent science in this country, sneer at any attempt to make use of the forces she reveals to us for the service of mankind. Someone said, the other day, that science was becoming a hod carrier. I do not see why the term "hod carrier" should express the relation rather than the term "benefactress." I do not see, either, that there is anything degrading in the thought that the knowledge of the learned man enables him to lift the burden beneath which humanity is bowed and bent. I do not know that science is exempt from the divine law, "He that is greatest among you, let him be the servant of all." If the great forces of the universe perform all useful offices for man, if the sunshine warm and light our dwellings, if gravitation move the world and keep it true to its hour, nay, if it keep the temple or cathedral in its place when the hod carrier has builded it, I do not see why it should not lend its beneficent aid to him also. Our illustrious philosopher advised his countryman to "hitch his wagon to a star." The star will move no less serenely on its sublime pathway when the wagon is hitched to it. I do not know that any archangel or goddess, however resplendent the wings, has yet been constructed or imagined without feet. I do not know that any archangel, however glorious, has ever been created or imagined without sympathy with suffering humanity.

I think perhaps this exalted, let me rather say this divine, sympathy for common humanity is the peculiar characteristic of the men who have been devoted to scientific pursuit in this country. Each of the great republics of the world has stood for a great ideal—the Greek for beauty, the Roman for law, the Hebrew for religious faith. So the American stands, I will not say for utility, for that word has bad associations, but rather for ennobling humanity and raising from the dust its humblest and coarsest members. I will not undertake to dispute whether this be of itself and alone the highest ideal to which states have been consecrated. I certainly do not think, highly as I esteem him, that Benjamin Franklin was the loftiest example either of philosophy or statesmanship. But the realization of our ideal is the condition of all the others. There can be neither beauty, law, love, nor spiritual life for a people in abject poverty or condemned to struggle for existence under a perpetual and unremitting drudgery. It is the praise and the glory of the great leaders of American science—it is the glory of Francis Walker—that the truths they have discovered and the laws they have expounded have been the benignant harbingers of light, knowledge, and happiness to the mass of mankind. A soul living in abject poverty is but the soul of a brute, though in human form.

Where in this gallery the works of our friend will be hung, on what shelf his books will be found, we will not to-night undertake to prophesy. But he will be among the masters. He will have a chapter or a page in the most compact history of science. His pithy sentences will be quoted. Better than that; his example of human interest in the welfare of man, and of a deferential regard for his opponents, will be followed. He will be remembered as having helped to teach the haughty, arrogant philosopher, the presumptuous and self-confident debater, a more excellent way. * * *

Lord Erskine says that though the word "gentleman" can not be defined, it is the quality to which England owes everything that she is. Perhaps our conception of it and that of the Englishman may not be in all respects the same. But certainly Frank Walker possessed that attractive something, consisting partly of grace of behavior, and chiefly of quality of heart, which makes the character of the gentleman. In those countries where rank and birth count for much, rank and birth are essential to the gentleman. We count them for nothing. But I do not know that the republican gentleman can better be defined than by taking the description which, when the age of chivalry was not yet gone, Chaucer gave of his young knight, the flower of English chivalry:

> "Curteis he was, lowly and servisable."

Courtesy, modesty, and service! We can find its examples in New England farmhouses as well as in baronial halls.

Our English brethren, when they speak of a great-hearted, honest, and brave man, who does nothing mean, and will not give in, like to say he is thoroughly English. With all the faults of the English character, they have an honest right to say it. But we have a right to say of this man that there was not a trait in his intellectual or moral character, there was not an action of his life, there was not an emotion of his soul, that was not intensely American. He loved his country. Under great and varied responsibilities he did what an American citizen ought to do, and was what an American citizen ought to be. You can hardly think of a place in the Republic where there was a man's work to do in which, if you had put Frank Walker, he would not have done it well.

He edited a paper in his youth, and he thought at one time of making that his profession. As I have said, he was all his life a constant spectator and critic. But as I said just now, he never lost his temper of a brave hopefulness. He never lost his love of country. While he had many friendships abroad, and had a reputation there which he highly valued, he was an American from the crown of his head to the sole of his feet. To him the word "countryman" was a title of endearment. He would accept no honor or respect for himself which implied any dishonor or want of respect for his country. Perhaps the crowning literary distinction of his life was the degree conferred on him at Dublin at the tercentenary celebration of that famous university. He devoted his speech of acknowledgment to eulogy of the Irish Catholic soldiers who had been his comrades in the war for the Union. Indeed, he was always a good stander-by. No comrade or friend need ever fear that his good name would suffer from any hostile critic in Francis Walker's presence.

He had none of the spirit which can find nothing but evil and degeneracy and decay in our national life—a spirit which does more than any other to create what it thinks it finds and to bring to pass its own prophecies. As nothing can be more injurious to a child than to teach him that he is base and worthy to be despised, nothing can be more injurious to a nation than to teach it such a lesson. The boastful, braggart, self-asserting temper which is sometimes found in a nation whose place in the world, whose title to the respect of mankind, is not yet assured, bad as it is, is infinitely less harmful than the spirit of an indiscriminate detraction. The taking for granted that honesty and courage and nobility are the motives of public conduct tends to elevate public conduct to a higher standard. We may be willing, in admiration for his mighty genius, to submit to Carlyle's note of despair and contempt. Yet I believe Emerson has inspired a hundred heroes where Carlyle has inspired one. The satirist's scourge, the critic's sneer, or the pessimist's whine or moan never exalted a nation and never saved a soul. It is not that these men are possessed by an enthusiasm for a lofty ideal. If it were we might bear with them. They have neither enthusiasms nor ideals. Whatever other qualities these wailers may have, they have neither faith, hope, nor charity.

It is remarkable that a man among whose great titles to distinction were those of author and soldier should be so indifferent to fame and praise.

> "Lyke as a ship that through the ocean wyde
> By conduct of some star doth make her way,"

he was constant to one bright and blazing star—the star of duty—

> "Of whose true fixed and resting quality
> There is no fellow in the firmament."

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II.—EXTRACTS FROM ADDRESS BY HON. CARROLL D. WRIGHT.[1]

When a man passes through the supreme agony of his existence, leaving only the clay embodiment of his soul, the mind, after the first shock of personal loss, turns from the contemplation of the individual embodiment to the life, character, and achievements of the real man.

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[1] Extracts from an address delivered at the regular quarterly meeting of the American Statistical Association, held in Huntington Hall, Massachusetts Institute of Technology, Friday evening, April 16, 1897, 8 p. m. Hon. Horace G. Wadlin, vice-president of the association, presided. Many friends and acquaintances of General Walker were present by invitation. Printed in Quarterly Publications of the American Statistical Association, new series, No. 38 (Vol. V), June, 1897.

The tear of affection has been shed, and we approach the life of our friend with bowed head, ever the attitude of reverence and respect. We can not, as personal friends, forget the magnificent presence of General Walker, nor do we wish ever to have the memory of his kindness, his cordial support, his active cooperation in everything that makes for the upbuilding of humanity, as viewed from the personal standpoint, effaced. No eulogy, no tribute, however glowing, can do justice or fully express the influence of personal relations. Such tributes must come from the heart, which can not express itself fully and satisfactorily through the medium of speech. Speech can recount the lives of men, their deeds, their professions, their worth to the public, and thus in a measure build a tomb to their greatness. The epitaph to goodness must always appear less than the truth. * * *

To my mind the life we commemorate to-night was a great orchestra, all its parts and instruments attuned to harmonious results. The versatility of General Walker's character, the great variety of his successful duties, make this simile emphatically expressive. We who have watched him through the last quarter of a century of his life know well the full meaning of this. There were no discords under his magic leadership; the orchestra ever rendered, obedient to his direction, the great symphony of his life. Here, with the allegro of the soldier, in that brisk, sprightly movement carried to the presto, he comes in his professorial experience to the adagio, where, with grace and beauty and diligent execution, he brings out the finest traits of his character, the closing movement of his life being an andante, with an even, graceful, onward progression; but through it all we discern the theme, the motif of his life. There was music in General Walker's life, as we contemplate all these movements in it, and this music makes it to us, who knew him, to all who will know him better as time goes on, a grand symphony indeed. But, in exact terms, who was this man, and what did he do? Why do we come here to-night to pronounce our eulogy? * * *

An address could be devoted to each branch of his work and yet fall far short of a comprehensive analysis. His public addresses alone would constitute a valuable collection of material for the use of students, for he took as much pains in preparing them as in producing his more voluminous works. So the temptation to take up some particular line which appeals, perhaps, more strongly than any other to individual taste must be resisted and in this presence chief attention devoted to his work as an economist and as a statistician, regretfully leaving all the rest for other men and for other occasions.

It is difficult to separate in the case of General Walker the economist from the statistician. He was each in turn, and both always; yet chronologically he was an economist before he was a statistician. On the other hand, the foundation of his great reputation abroad was rather as a statistician than as an economist, while in this country I think he was better known as an economist. It was natural that he should at a very early age turn his attention to economic considerations. In view of his familiarity with his father's life, and impressed by his strong, original, and far-seeing mind, it would have been strange if the keen, receptive qualities of the younger man were not affected. The elder Walker, as already intimated, was an honored and trusted authority in the perplexing problems of political and social science. * * *

There are two classes of political economists—those belonging to what is popularly known as the orthodox school and those adhering to the so-called new school of political economy. General Walker did not belong exclusively to either. He upheld the theorist's views, and yet he understood the pressing influences of environment, of the constitution of human nature, in affecting human economic conditions; he did not hesitate to attack well-grounded assumptions and theories or to uphold those which had not the weight of the culture of the past to support them; he was orthodox enough to insist that ethics could not displace economics; he was just and fair enough to recognize that economics could not displace ethics, and his well-balanced mind taught him that, economic conditions once established, the relationship of men under them became ethical. This fair-mindedness, of course, subjected him to attacks from both schools. When his invaluable work on wages (one of the most positive contributions to economic and social science that has been published in the last half century) appeared, in which he made his brave and democratic attack upon the settled wage-fund theory of the great economists of Europe, he called down upon himself attacks which might have staggered a less courageous fighter, but with his human and humane instincts Walker kept on his course. He placed manhood at the center of his economic system. He recognized not only the power but the good of the organization of labor, and insisted that wages were to be determined, not by any arbitrary rule under which wage earners have no voice and no concern, and are mere physical elements of production, but by conditions of production, and that the receivers of them had some rights; for, as he expressed it, no class is fit to be the trustee of the interests of any other class. He did not occupy the position in America that must be given to Émile de Laveleye, of Belgium, although there is much in common in their writings; he can be classed more truthfully, probably, with Leslie and Marshall, of England. He recognized that political economy must deal with vital things; that it has something more to do with the world's affairs than to teach the accumulation of wealth. * * *

His chief studies as an economist have related to wages, theory of distribution, money, and social economics. Adopting the historical method, President Walker very naturally turned to statistics for his arguments and illustrations, and, while many economists may differ with him in many of his positions, the general student feels that he came nearer the truth than many of his contemporaries. He did not waste his efforts in quarreling over definitions; his economic work was something grander and broader. He undertook to announce principles and to draw deductions which, whether new or old, could be clearly backed by fact and reason. He was not worried about definitions. * * *

In his discussions on wages and distribution, while merciless in destroying the popular views of simple cooperation, thus showing his power as a theorist who understood conditions as well, he did not hesitate to advocate, not as a solution, not as a full remedy for labor difficulties, but as an alleviation of them, the principles of profit sharing and industrial partnership, and to this end he was, from its inception, one of the vice-presidents of the American Society for the Promotion of Profit Sharing.

This and various other positions did not give him the highest reputation as a theoretic economist, although in other lands he stood higher in this respect than in his own country. His strong grasp of whatever subject he was discussing, and the striking and interesting way in which he presented it, added force to his views. At the same time his interest in all good works, his attitude in educational matters, and his warm-hearted personality brought him strength with the masses, while he held his own as a theorist. He had that rare faculty of treating matters, both scientifically and popularly, in such a way that neither the scientists with whom he was associated nor the common people who admired him could claim him for their own. Impregnated, it may be, with the hard-headed doctrines of Sumner, which admit of no collateral distinctions in economic science, he could still see, feel, and comprehend the ever-existing struggle of humanity toward a higher standard. Yet no man was more rigid and unyielding in his teaching. He never allowed himself, optimist that he was, to proclaim any sentimental doctrines, for he knew well that their adoption would simply result in increasing the difficulties which he was so anxious to avoid, in intensifying the struggle which he was so desirous of softening. This scientific attitude enabled him to attack some of the modern isms, and even to destroy some latter-day views of life. * * *

His knowledge of history and his familiarity with statistical science, or the scientific method of statistics, were his powerful allies as an economist. With this equipment he vitalized political economy, and thus stood, with all fair and due consideration of the splendid abilities of other economists, at the head. No one was jealous of him, even though attacking him. * * *

It was very natural, when the professors of political economy and others organized the American Economic Association, that General Walker should be chosen as its leader. It was an honor due him and a choice which honored the association as well. Modest in his acceptance of the trust, he always urged that other men should take the lead, and yet he was continued as its president from 1885 to 1892. His counsel in guiding the course of the association in its formative period, his addresses at the opening of each recurring session, and his reputation at home and abroad did more to give the association its great impetus and to establish its right to exist than any other single influence.

His works, his doctrines, his public positions growing out of them, contribute to make him the foremost figure among the political economists of America.

As a statistican the life work of our friend comes nearer to us in this association than any other feature perhaps. For nearly a generation General Walker's name has been associated with the statistics of the United States Government. His first experience was as Deputy Commissioner of Internal Revenue and Chief of the Bureau of Statistics of the Treasury Department. His experience here, however, was so brief that he did not have the opportunity of stamping his individuality upon the work of the office. In 1870 he was appointed Superintendent of the Ninth Census, and he entered upon the work of that office with all the enthusiasm of his nature and the consecration of his science as a political economist. Like every other statistician in this country who has been called to take charge of statistical enterprises, General Walker came to his work untrained, yet with a vast advantage over every other man in the country who has undertaken like work. He was a political economist by inheritance, by endowment, and by painstaking equipment, and so an economist adopting the historical method, he had been a student of the statistics of other countries and of this, as far as they existed.

As a census taker he was obliged, in almost every expansive respect, to blaze the way. The censuses of the past had been of a fragmentary nature, those of 1850 and 1860 being creditable departures from the old-time Federal enumeration. It was General Walker's ambition to have the census of 1870 taken on a broader basis than that of any preceding it. The country was approaching its interesting centennial period. In 1876 there was to be a great exposition of the resources, the wealth, and the character of our people. The General foresaw all this, and his desire was to make the census of 1870 encyclopedic in its nature, comprehending all the great features of social and economic conditions. He wanted to show not only the character, the composition, the movement, and the growth of the people, but the character and composition of its industries, its transportation systems, its accumulation of wealth, its sociological elements—everything, in fact, that goes to make up a great and growing nation. In this he had Gen. James A. Garfield, then in the House, as an able coadjutor. General Garfield was chairman of the House Committee on the Ninth Census, and he made a deep study of the census methods of the world. With the aid of General Walker and the advice of Dr. Edward Jarvis, at that time president of this association, he projected a system of census taking on a broad and comprehensive basis. The bill * * * was defeated, the result being that General Walker was obliged to take the census of 1870 in accordance with the law of 1850. Nevertheless, he made it, through the support he gained from legislators and the Administration, by far the most scientific census which this country had yet seen, and it gave him at once a leading position among the statisticians of the world. He was hampered all through it by the clumsy methods of the past. His experience, however, gave him not only the courage but the power to insist in the Tenth Census, that of 1880, upon the adoption of more scientific methods. Coming into that census with splendid equipment, General Walker had no great difficulty in inducing Congress to broaden its scope. He adopted new methods of enumeration; he got rid of the old, bungling methods of collecting facts through the United States marshals, and secured the power to appoint an army of enumerators, properly supervised in districts comprehending a certain number of enumeration districts. The census of 1870 was confined to the few topics authorized by the law of 1850, as already stated—population, vital statistics, wealth, and industry—the publications of that census being comprised in four volumes, including a compendium, in one volume. The results of the Tenth Census, however, comprised twenty-two volumes and a compendium (in two parts). General Walker's ambition of 1870 to make a centennial contribution of facts found ample field in 1880. The enthusiasm of the period, perhaps, aided him greatly, but by this vast work he was enabled to show to the country the leading facts relative to its population, its manufactures, and its agriculture, and, in addition, its agencies of transportation; its valuation, taxation, and public indebtedness; statistics of its immense systems of transportation, its newspapers, and its shipbuilding. He also presented elaborate reports upon the forest trees of North America, the petroleum and building-stone industries of the country, the technology of the precious metals, the mining laws and industries of the country; also a technical report upon the the water power of the United States, a magnificent volume upon wages, with reports upon the prices of necessaries of life, trade societies, strikes and lockouts, and other features.

When it is considered that the census of 1880 was undertaken on such an expansive plan as that outlined, and had to be carried out by a temporary and an untrained force, under the necessity of fighting off those thirsty for position, the magnitude of the work involved seems simply appalling. But General Walker was a great organizer. His experience in bringing together bodies of troops, in superintending the movement of all the impedimenta of a great army, in collecting materials for attack and defense, in handling forces in the field in active conflict, his training as a scientific economist—all these experiences constituted him par excellence the one to carry out the plans of the Tenth Census. He did not carry the work through to completion, in one sense. Called to the presidency of the institute here in Boston, he left the census in the middle of 1881, so far as his official connection with it was concerned, but until the last volume appeared, in 1888, General Walker exercised a supervisory relation to it all. All the text passed through his hands.

While this vast work is not, of course, of uniform merit, on the whole it must be considered as the greatest, most elaborate, and most valuable contribution to the body of statistics furnished by any country up to its date. It has been criticised, attacked, and abused; it has been lauded and indorsed, but so far as I know it has never yet had a severer critic than General Walker himself.

In the various analyses accompanying the innumerable tables he always took pains to point out the weak spots. He put his downright integrity into this work, as into every other to which he put his hand. He has been blamed for launching the Government upon an exhaustive census, but it must be understood that it was never General Walker's idea that a census so comprehensive should be repeated. It was the centennial period, as I have said, and he wanted to exhibit this country in all its vast, comely proportions through the statistical method. In this he succeeded. When it was done he was in hopes that the Government would establish a scientific census, profiting by the experience of the past, and giving to the world the results of work conducted by a trained and well-qualified force. * * *

With the cessation of his duties as Superintendent of the Tenth Census, and his voluntary duties as general supervisor after his resignation, General Walker's active statistical experience came to a close, but his interest in official statistics never flagged. He was an honorary member of the Royal Statistical Society of England. He was in England and France at the time of the celebration of the jubilee year of the society. The proposition was advanced that out of that jubilee celebration of the Royal Statistical Society there be established an international statistical institute. There had been many attempts to create an international statistical body, taking the form of statistical congresses held from some time in the fifties, for twenty or thirty years. The results of these congresses were of no great importance. It was therefore the view of the principal statisticians of the world that some regular organization could be of great service in harmonizing international statistics for comparative purposes. General Walker gave his warm adherence to the project. The International Statistical Institute was organized, and he was made one of its vice-presidents. In 1893, when the institute met in Chicago, he was created honorary member and président-adjoint, presiding at the deliberations of the institute during that session. In 1882 he was elected president of this American Statistical Association, and he served it devotedly until his death. I have been informed that he was present at every meeting of the association. His last service in the interest of statistical science and of statistical progress was rendered in Washington on Thursday, December 31 last, only a few days before his death, which occurred on the 5th of January. It was on the occasion of the opening meeting of a series to be held by the Washington members of the association. President Walker was delighted at the prospect, and, at great inconvenience to himself, was present and made an address. This address is typical of the man. It should be read by every legislator, State or Federal, in the country. He brought out most clearly and graphically how vast sums of money have been spent by the Federal Government in training men for the Army and the Navy, through the maintenance of the Military and Naval academies, but that not even the interest of $10,000 had ever been expended by the Government in training men for its great census service—a service which has cost tens of millions of dollars, and yet is entered upon every ten years with an untrained, unscientific, and unequipped corps of clerks and officers. This he deprecated most severely, and pointed out the advantages that would accrue if the Government would establish a permanent census office, and devote itself to the assiduous collection, analysis, and publication of statistical information; that the expenditure of such vast sums in the publication of statistical matter was not warranted unless great pains was taken to secure accurate data to begin with.

This was the last public utterance of General Walker, and those who heard him were impressed, not only with the dignity, the character, and the force of his address, but, alas! with the fact that he was overworked, and that soon we might expect—we did not anticipate how soon—his work to overcome him.

It may be that we shall conclude that the most monumental work undertaken by President Walker was the administration of the affairs of the Massachusetts Institute of Technology. I have spoken of his equipment for that position. His experience at Williston and at Yale confirmed him in his estimate of a sound, practical education. Never ignoring the classics, always broad enough to appreciate and foster all that belongs to a college of the liberal arts, he, nevertheless, understood more thoroughly than most educators fifteen years ago the real necessity of the most complete scientific education. He therefore entered upon his duties in the institute with more than the ardor of an educator, for he had but little to do with teaching as such. His breadth of mind enabled him to understand the needs of the institute, and his great administrative abilities made him familiar at all times with all the features of the various curricula. His innovations were of the very greatest help to the young man seeking to equip himself for his life contests. He never believed in giving diplomas to men who had simply stayed the required time in the institution over which he presided; but when a young man was not competent to take the full course, or through ill health or other adverse conditions was obliged to drop out of some of the departments, he gave him every opportunity to secure a special certificate in some one branch. By this means he has sent out into the world men often without a diploma to be sure, but thoroughly competent to take charge of work intrusted to them. We all know many of these young men—we know their success; we know their loyalty to President Walker. He was their friend always, and at all times ready to aid with his advice and assist materially with his recommendations. While carrying the student roll of the institute from 302 to 1,198 during his incumbency of the presidency, he raised the standard of the school in like proportion. The graduates of the institute are found everywhere, and, so far as my own knowledge is concerned, always with the same result as to their standing and their efficiency.

It must be admitted that as an educator in the very highest sense President Walker had no superior, and with that rare faculty of acquiring a personal relationship to each student his influence has never been surpassed, except it may be in the case of Mark Hopkins.

How can I sum up the life of General Walker? His works, his service as a public officer, his devoted life as an educator, his brilliant career as a soldier—all these appeal to one; and yet they in themselves, taken all together, making, as they do, a magnificent monument to his memory, planned, erected, completed by himself, do not fully answer the question. One must have known him personally, have known his devoted adherence to principle, have realized and felt his courageous action at all times in order to completely appreciate and understand him. How many knew him! How many miss him! The Loyal Legion knew him—knew him as a brave soldier, a man of capacity in the field, a man of resources in planning, and a man of courage in carrying out his plans; a man who won the commendation, the admiration, the affection of his comrades in arms, whether of low or of high rank. The institutions of learning with which he was connected knew him; students knew him as a man who loved them, who believed in their sports, who entered into their ambitions and aspirations, who guided them with his advice. He once wrote to a young man that he never wished to shut the door on anyone who was seeking an education, and was always willing to give another trial to a young man who had failed. You and I know that as an educator he won their affection. They felt at liberty to approach him personally, to call upon him, to visit him. I know how the men who came from under his guidance here in the institute stand in the world, how their services are sought, how their preeminent attainments secure for them at once a living position.

The economists and statisticians of the country will miss him; they knew him, knew his worth, knew his integrity in all that interests them, knew the value of his advice. The philanthropic and benevolent institutions of this State and city knew him and will miss him, for he was ever ready to give his thought, his sympathy, and his presence to all good enterprises, and his kindly sympathy with those who had fallen along the track of life was warm and devoted. * * *

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