BULLETIN

OF THE

GEOLOGICAL SOCIETY

OF

AMERICA

VOL. 31

JOSEPH STANLEY-BROWN, *Editor*

NEW YORK
PUBLISHED BY THE SOCIETY
1920

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* Elected by the Council, December 31, 1919, to fill out the unexpired term of Barrell, deceased.

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ILLUSTRATIONS

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(14 plates; 7 figures.)

PUBLICATIONS OF THE GEOLOGICAL SOCIETY OF AMERICA

Regular Publications

The Society issues annually, in four quarterly parts, a single serial octavo publication entitled Bulletin of the Geological Society of America, the edition being 700 copies. A small supply of authors' separates of the longer articles is kept for sale by the Secretary at the prices quoted in each volume.

The Bulletin is sold at the uniform price of ten dollars ($10.00) per volume, with a discount of twenty-five (25) per cent to Fellows of the Society, persons residing elsewhere than in North America, and public and institutional libraries; carriage extra. Subscriptions are payable in advance. Regular subscribers within the United States of America and its possessions receive their parts, postage paid, as issued. Forty (40) cents per volume must be added to the subscription price to cover postage to other countries in the Postal Union.

The price of the index to volumes 1-10 is $2.25; to volumes 11-20, $3.50, and to volumes 21-30, $3.75; carriage extra. No reduction is made to dealers. Orders should be addressed to the Secretary, whose address is care of the American Museum of Natural History, New York, N. Y., and drafts and money orders should be made payable to *The Geological Society of America.*

Description of the Published Volumes

Volumes.	Pages.	Plates.	Figures.
Vol. 1, 1889	593 + xii	13	51
Vol. 2, 1890	622 + xiv	23	63
Vol. 3, 1891	541 + xi	17	72
Vol. 4, 1892	458 + xi	10	55
Vol. 5, 1893	655 + xii	21	43
Vol. 6, 1894	528 + x	27	40
Vol. 7, 1895	558 + x	24	61
Vol. 8, 1896	446 + x	51	29
Vol. 9, 1897	460 + x	29	49
Vol. 10, 1898	534 + xii	54	83
Index to volumes 1–10	209	..	..
Vol. 11, 1899	651 + xii	58	37
Vol. 12, 1900	538 + xii	45	28
Vol. 13, 1901	583 + xii	58	47
Vol. 14, 1902	609 + xii	65	43
Vol. 15, 1903	636 + x	59	16
Vol. 16, 1904	636 + xii	94	74
Vol. 17, 1905	785 + xiv	84	96
Vol. 18, 1906	717 + xii	74	59
Vol. 19, 1907	617 + x	41	31
Vol. 20, 1908	749 + xiv	111	35
Index to volumes 11–20	422	..	..
Vol. 21, 1909	823 + xvi	54	109
Vol. 22, 1910	747 + xii	31	66
Vol. 23, 1911	758 + xvi	43	44
Vol. 24, 1912	737 + xviii	36	60
Vol. 25, 1913	802 + xviii	28	47
Vol. 26, 1914	504 + xxi	27	41
Vol. 27, 1915	739 + xviii	30	55
Vol. 28, 1916	1005 + xxii	48	102
Vol. 29, 1917	679 + xix	22	67
Vol. 30, 1918	644 + xiii	15	40
Index to volumes 21–30	325	..	..
Vol. 31, 1919	450 + xviii	14	7

PARTS OF VOLUME 31

	Pages.	Plates.	Figures.	Price to Fellows.	Price to Public.
Number 1	1–246	1– 7	2	$3.00	$4.50
Number 2	247–338	8–14	5	2.00	3.00
Number 3	339–400	....	...	1.50	2.25
Number 4*	401–450	....	...	1.00	1.50

REPRINTS FROM VOLUME 31

Reprints.	Pages.	Plates.	Figures.	Price to Fellows.	Price to Public.
Proceedings of the Thirty-second Annual Meeting of the Geological Society of America, held at Boston, Massachusetts, December 29-31, 1919. E. O. HOVEY, *Secretary*	1–176	1–7	1–2	$2.00	$3.00
Proceedings of the Fifteenth Summer Meeting, held in conjunction with the Sixteenth Annual Meeting of the Cordilleran Section, University of California and Stanford University, August 3, 4, and 5, 1915. J. A. TAFF, *Secretary*	177–184	....	....	.10	.15
Proceedings of the Seventeenth Annual Meeting of the Cordilleran Section, held at San Diego, California, August 10, 1916. J. A. TAFF, *Secretary*	185–186	....	....	.10	.15
Proceedings of the Eighteenth Annual Meeting of the Cordilleran Section, held at Stanford University, California, April 6 and 7, 1917. J. A. TAFF, *Secretary*	187–190	....	....	.10	.15
Proceedings of the Nineteenth Annual Meeting of the Cordilleran Section, held at Pasadena, California, June 19-22, 1919. G. D. LOUDERBACK, *Acting Secretary*	191–196	....	....	.10	.15
Proceedings of the Eleventh Annual Meeting of the Paleontological Society, held at Boston, Massachusetts, December 30-31, 1919. R. S. BASSLER, *Secretary*†	197–230	....	....	.45	.65
Minutes of the Ninth Annual Meeting of the Pacific Coast Section of the Paleontological Society. C. STOCK, *Secretary*	231–232	....	....	.10	.15
Earth sciences as the background of history. J. C. MERRIAM	233–246	....	....	.15	.25
Discoidal structure of the lithosphere. B. WILLIS	247–302	8–13	1–2	.75	1.05
Oscillations of level in the belts peripheral to the Pleistocene ice-caps. R. A. DALY	303–318	14	....	.20	.30

* Preliminary pages and index are distributed with number 4.

† Under the brochure heading is printed PROCEEDINGS OF THE PALEONTOLOGICAL SOCIETY.

Reprints.	Pages.	Plates.	Figures.	Price to Fellows.	Price to Public.
Extent and thickness of the Labrador ice-sheet. A. P. Coleman..	319–328	....	1–3	$0.10	$0.15
Quantitative methods of estimating ground-water supplies. O. E. Meinzer......................	329–338	....	....	.10	.15
The teaching of historical geology as a factor conditioning research. J. C. Merriam........	339–350	....	....	.15	.25
Structural and petrographic geology. J. F. Kemp............	351–356	....	....	.10	.15
Teaching of geology as related to research. W. E. Gregory......	357–362	....	....	.10	.15
American paleontologists and the immediate future of paleontology. C. Schuchert...........	363–374	....	....	.15	.25
The teaching of historical geology. H. E. Cleland................	375–382	....	....	.10	.15
Fossils as aids in teaching stratigraphy, or applied paleontology. S. Weller....................	383–388	....	....	.10	.15
The teaching of paleontology. E. W. Berry.....................	389–394	....	....	.10	.15
Value and use of stages in development in teaching paleontology. R. T. Jackson................	395–400	....	....	.10	.15
Researches on sedimentation. T. W. Vaughan..................	401–410	....	....	.10	.15
Physical and geographic criteria in the study of sedimentary deposits. E. W. Shaw..........	411–418	....	....	.10	.15
Chemical researches on sediments. H. E. Merwin................	419–424	....	....	.10	.15
Diagenesis in sedimentation. C. Schuchert...................	425–432	....	....	.10	.15

Irregular Publications

In the interest of exact bibliography, the Society takes cognizance of all publications issued wholly or in part under its auspices. Each author of a memoir receives 30 copies without cost, and is permitted to order any additional number at a slight advance on cost of paper and presswork; and these reprints are identical with those of the editions issued and distributed by the Society; but the cover bears only the title of the paper, the author's name, and the statement [Reprinted from the Bulletin of the Geological Society of America, vol. —, pp. —, pl. — (Date)]. Contributors to the Proceedings and "Abstracts of Papers" are also authorized to order any number of separate copies of their papers at a slight advance on cost of paper and presswork; but such separates are bibliographically distinct from the reprints issued by the Society.

Regular Editions

Pages	233–246,		50	copies.	June	30, 1920.
"	247–302,	plates 8–13,	200	"	"	30, 1920.
"	303–318,	" 14,	520	"	"	30, 1920.
"	319–328,		100	"	"	30, 1920.
"	329–338,		150	"	"	30, 1920.
"	339–350,		150	"	September	30, 1920.
"	351–356,		250	"	"	30, 1920.
"	357–362,		120	"	"	30, 1920.
"	363–374,		300	"	"	30, 1920.
"	375–382,		350	"	"	30, 1920.
"	383–388.		150	"	"	30, 1920.
"	389–394,		50	"	"	30, 1920.
"	395–400,		100	"	"	30, 1920.
"	401–410,		200	"	November	30, 1920.
"	411–418,		50	"	"	30, 1920.
"	419–424,		550	"	"	30, 1920.
"	425–429,		200	"	"	30, 1920.
"	433–440,		550	"	"	30, 1920.

Special Editions‡

Pages	13– 25,	plate	1,	450	copies.	March	31, 1920.
"	26– 64,	"	2,	120	"	"	31, 1920.
"	65– 80,	"	3,	50	"	"	31, 1920.
"	81– 88,	"	4,	350	"	"	31, 1920.
"	88– 97,	"	5,	100	"	"	31, 1920.
"	97–100,	"	6,	350	"	"	31, 1920.
"	100–110,	"	7,	250	"	"	31, 1920.
"	117–120,			150	"	"	31, 1920.
"	141–144,			150	"	"	31, 1920.
"	144–148,			350	"	"	31, 1920.
"	165–176,			100	"	"	31, 1920.
"	197–232,*†			200	"	"	31, 1920.

* Bearing on the cover
PROCEEDINGS OF THE PALEONTOLOGICAL SOCIETY.
[Reprinted from the Bulletin of the Geological Society of America, vol. ——, pp. ——, pls. ——, (Date)].

† Under the brochure heading is printed PROCEEDINGS OF THE PALEONTOLOGICAL SOCIETY.

‡ Bearing imprint [From Bull. Geol. Soc. Am., Vol. 30, 1918.]

CORRECTIONS AND INSERTIONS

Contributors to volume 31 have been invited to send corrections and insertions to be made in their papers, and the volume has been scanned with some care by the Editor. The following are such corrections and insertions as are deemed worthy of attention:

Page 189, line 11 from bottom, *omit* word "Mountains."

" 352, lines 11 and 12 from the top *should read* of disjointed fish bones, which the student discovered, because of the six pairs of jaws, represented a half dozen skeletons.

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
VOL. 31, PP. 1-196, PLS. 1-7 MARCH 31, 1920

PROCEEDINGS OF THE THIRTY-SECOND ANNUAL MEETING OF THE GEOLOGICAL SOCIETY OF AMERICA, HELD AT BOSTON, MASSACHUSETTS, DECEMBER 29-31, 1919.

EDMUND OTIS HOVEY, *Secretary*

CONTENTS

Session of Monday, December 29

The Thirty-second Annual Meeting of the Geological Society was held December 29-31, 1919, in Boston, Massachusetts, by invitation of the Geological Society of Boston; the business and scientific sessions were held in the Rogers Building of the Massachusetts Institute of Technology; the annual subscription dinner took place at the Hotel Vendome.

The first general session of the meeting was called to order at 9.30 o'clock a. m. by President John C. Merriam, and the annual report of the Council was called for. This was presented as follows:

REPORT OF THE COUNCIL

To the Geological Society of America, in thirty-second annual meeting assembled:

The regular annual meeting of the Council was held at Baltimore, Maryland, in connection with the meeting of the Society, December 27-28, 1918. A special meeting was held in New York City, April 7, 1919.

The details of administration for the thirty-first year of the existence of the Society are given in the following reports of the officers:

Secretary's Report

To the Council of the Geological Society of America:

The Secretary's annual report for the year ending November 30, 1919, is as follows:

Meetings.—The proceedings of the annual general meeting of the Society, held at Baltimore, Maryland, December 27-28, 1918, have been recorded in volume 30, pages 1-142, of the Bulletin, and of the Paleontological Society, pages 143-188, of the same volume.

Membership.—During the past year the Society has lost eight Fellows by death—Joseph Barrell, George F. Becker, Charles H. Hitchcock, Alexis A. Julien, Lawrence M. Lambe, Louis V. Pirsson, G. Sherbourne Rogers, and J. C. da Costa Seña. The names of the nineteen Fellows elected at the Baltimore meeting have been added to the printed list. The present enrollment of the Society is 10 correspondents and 407 Fellows. Eighteen candidates for Fellowship are before the Society for election and several applications are under consideration by the Council.

Distribution of the Bulletin.—There have been received during the year six new subscriptions to the Bulletin. Three subscriptions have been canceled, seventeen have not yet been renewed, and one has been reinstated. The number of volumes sent out to subscribers is now 123. Five volumes are distributed gratis to the Library of Congress, the American Museum of Natural History, and the government geological surveys of the United States, Canada, and Mexico. During the year 43 volumes were sent out to foreign exchanges.

The irregular distribution of the Bulletin during the past year has been as follows: Complete volumes sold to the public, 71; sold to Fellows, 1; sent out to supply delinquents, 1, and deficiencies, 1; brochures sold to Fellows, 10; sold to the public, 72; sent out to supply delinquents, 54, and deficiencies, 20. Index to volumes 1-10 sold to the public, 2; Index 11-20, 1.

Bulletin sales.—The receipts from subscriptions to and sales of the Bulletin during the past year are shown in the following table:

Bulletin Sales, December 1, 1918–November 30, 1919

	Complete volumes.			Brochures and parts.			Grand total.
	Fellows.	Public.	Total.	Fellows.	Public.	Total.	
Volume 1...	....	$30.00	$30.00	....	....	....	$30.00
Volume 2...	....	30.00	30.00	$3.00	$0.39	$3.39	33.39
Volume 3...	....	30.00	30.00	....	....	....	30.00
Volume 4...	....	30.00	30.00	....	....	....	30.00
Volume 5..	....	22.50	22.50	....	2.97	2.97	25.47
Volume 6...	....	30.00	30.00	....	....	....	30.00
Volume 7...	....	22.50	22.50	....	....	....	22.50
Volume 8...	....	22.50	22.50	....	.54	.54	23.04
Volume 9...	....	22.50	22.50	....	1.38	1.38	23.88
Volume 10...	....	30.00	30.00	....	1.00	1.00	31.00
Volume 11...	....	15.00	15.00	....	....	....	15.00
Volume 12...	....	15.00	15.00	....	....	....	15.00
Volume 13...	....	15.00	15.00	....	....	....	15.00
Volume 14...	....	15.00	15.00	.15	1.00	1.15	16.15
Volume 15...	....	15.00	15.00	.20	....	.20	15.20
Volume 16...	....	15.00	15.00	1.80	.60	2.40	17.40
Volume 17...	....	....	....	....	1.05	1.05	1.05
Volume 18...	....	....	....	....	....	....	....
Volume 19...	....	7.50	7.50	....	.30	.30	7.80
Volume 20...	....	22.50	22.50	....	....	....	22.50
Volume 21...	....	15.00	15.00	....	....	....	15.00
Volume 22...	....	30.00	30.00	....	....	....	30.00
Volume 23...	....	22.50	22.50	....	2.75	2.75	25.25
Volume 24...	....	30.00	30.00	....	11.78	11.78	41.78
Volume 25...	....	37.50	37.50	....	5.16	5.16	42.66
Volume 26...	....	22.50	22.50	....	2.25	2.25	24.75
Volume 27...	....	30.00	30.00	7.90	11.47	19.37	49.37
Volume 28...	....	37.50	37.50	....	78.23	78.23	115 73
Volume 29...	7.50	90.00	97.50	....	1.09	1.09	98.59
Volume 30...	....	855.00	855.00	....	....	....	855.00
Volume 31...	....	127.50	127.50	....	....	....	127.50
Volume 32...	....	7.50	7.50	....	....	....	7.50
Total....	$7.50	$1,695.00	$1,702.50	$13.05	$121.96	$135.01	$1 837.51
Index 1–10..	....	4.50	4.50	....	....	....	4 50
Index 11–20..	....	3.50	3.50	....	....	....	3.50
Total....	$7.50	$1,703.00	$1,710.50	$13.05	$121.96	$135.01	$1,845.51

Charged on 1919 account, but not yet received....................	$192.39
Receipts for the fiscal year..................................	$1,653.12
Previously reported..	23,313.92
Total receipts to date.................................	$24,967.04

Expenses.—The following table gives the cost of administration and of Bulletin distribution during the past year:

EXPENDITURES OF SECRETARY'S OFFICE DURING THE FISCAL YEAR ENDING NOVEMBER 30, 1919

Account of Administration

Printing and stationery	$164.50	
Postage	72.24	
Telegrams, telephone, and cable	22.85	
Badges (used at Baltimore meeting)	26.75	
Council meeting in April (sundries)	11.25	
Express	3.66	
Binding 3 volumes	8.25	
Wrapping paper	3.29	
Sundries connected with Baltimore meeting	3.00	
Addressograph plates	.93	
Group portrait (clerical work, postage, printing)	82.80	
Total		$399.52

Account of Bulletin

Printing	$78.50	
Extra clerical help	65.00	
Express	16.74	
Postage	11.78	
Collection of checks	1.90	
Telegram	.40	
Loss on foreign exchange	.26	
Addressograph plates	.15	
Total		174.73
Grand total		$574.25

Respectfully submitted,

EDMUND OTIS HOVEY,
Secretary.

TREASURER'S REPORT

To the Council of the Geological Society of America:

The Treasurer herewith submits his annual report for the year ending November 30, 1919.

The membership of the Society at the present time is 10 correspondents and 407 Fellows, of whom 321 pay annual dues. Nineteen new members were elected at the last annual meeting, all of whom qualified, as well as one elected in 1918 who had not qualified, making a total of twenty new members. There have been eight deaths during the present year, two Life Members, leaving 88 Life Members at present on the roll. Five Fellows resigned and two were dropped for non-payment of dues. When the books closed for the year, 21 members were delinquent in the payment of dues—2 for 4 years, 1 for 3 years, 5 for 2 years—and are therefore liable to be dropped from the roll, and 13 for 1 year.

RECEIPTS

Balance in the treasury, December 1, 1918		$1,660.48	
Fellowship fees, 1917 (3)	$30.00		
1918 (11)	110.00		
1919 (302)	3,020.00		
		3,160.00	
Initiation fees (20)		200.00	
Interest on investments		1,115.00	
Interest on deposits in Baltimore Trust Company		95.26	
Bond of Saint Louis and San Francisco Railroad Company redeemed		1,000.00	
From Brazilian Government for geological map by Branner		710.00	
Collection charges added to checks		.46	
Received from Secretary:			
Sales of publications	$1,653.12		
Payment of postage and express	11.25		
Collection of checks	.84		
Binding	2.75		
Authors' separates	111.76		
Group portrait	72.00		
Authors' corrections	26.37		
Printing for Paleontological Society	17.80		
		1,895.89	
			$9,837.09

EXPENDITURES

Secretary's office:		
Administration	$316.72	
Bulletin	174.73	
Group portrait	82.80	
Salary	1,000.00	
		$1,574.25
Treasurer's office:		
Expenses	$87.25	
Clerk	100.00	
		187.25
Collection of foreign checks		1.02

Publication of Bulletin:			
Printing	$2,552.35		
Engraving	181.03		
Editor's allowance	250.00		
		2,983.38	
Purchase of one Florida Central and Peninsular Ext. 6 per cent bond		1,020.17	
Branner map of Brazil		710.00	
		$6,476.07	
Balance in Baltimore Trust Company		3,361.02	
			$9,837.09

Respectfully submitted,

EDWARD B. MATHEWS,
Treasurer.

EDITOR'S REPORT

To the Council of the Geological Society of America:

The following tables cover statistical data for the thirty volumes thus far issued:

ANALYSIS OF COSTS OF PUBLICATION

Cost.	Average—Vols. 1-25.	Vol. 26.	Vol. 27.	Vol. 28.	Vol. 29.	Vol. 30.
	pp. 759. pls. 42.	pp. 525. pls. 27.	pp. 757. pls. 30.	pp. 1027. pls. 48.	pp. 698 pls. 22	pp. 657 pls. 15
Letter press	$1,807.41	$1,076.22	$1,684.67	$2,128.15	$1,483.37	$2,477.16
Illustrations	327.04	171.69	378.30	484.37	321.07	917.56
Paper	....	231.00	416.00	698.00	416.00	379.61
Total	$2,134.45	$1,478.91	$2,478.97	$3,310.52	$2,220.44	$3,773.33
Average per page	$2.83	$2.81	$3.27	$3.23	$3.18	$5.75

CLASSIFICATION OF SUBJECT-MATTER

Volume.	Areal geology.	Physical geology.	Glacial geology.	Physiographic geology.	Petrographic geology.	Stratigraphic geology.	Paleontologic geology.	Economic geology.	Official matter.	Memorials.	Unclassified.	Total.
	Number of Pages.											
1	116	137	92	18	83	44	47	-----	60	4	4	593+xii
2	56	110	60	111	52	168	47	9	55	1	7	662+xiv
3	56	41	44	41	32	158	104	-----	61	15	1	541+xii
4	25	134	38	74	52	52	14	-----	47	32	2	458+xii
5	138	135	70	54	28	51	107	-----	71	14	9	665+xii
6	50	111	75	39	71	99	1	-----	63	25	4	538+x
7	38	77	105	53	40	21	123	4	66	28	13	558+x
8	34	50	98	5	43	67	58	14	79	8	----	46+x
9	2	102	138	-----	44	28	64	16	64	12	----	60+x
10	35	33	96	37	59	62	68	28	84	27	17	534+xiii
11	65	110	21	10	54	31	188	7	71	60	46	651+xii
12	199	39	55	53	24	98	5	5	70	2	----	538+xi
13	125	17	13	24	28	116	42	4	165	32	29	583+xii
14	48	47	48	59	183	118	22	1	80	14	1	609+xi
15	26	124	3	94	36	267	-----	-----	77	17	3	636+x
16	64	111	78	30	102	141	19	-----	67	22	15	636+xiii
17	49	161	41	84	47	294	27	-----	71	9	2	785+xiv
18	16	164	141	5	29	246	5	-----	68	40	3	717+xii
19	106	108	29	66	30	155	32	-----	56	15	20	617+x
20	43	54	35	29	37	45	303	8	60	3	132	749+xiv
21	72	234	75	48	85	70	106	1	111	11	10	823+xvi
22	23	54	28	28	23	403	74	-----	63	49	1	747+xii
23	75	52	126	108	19	145	134	-----	66	32	1	758+xvi
24	18	57	96	57	49	160	106	23	133	53	3	737+xviii
25	34	211	54	32	156	9	175	-----	108	9	22	802+xviii
26	-----	72	23	11	56	90	148	-----	54	44	6	504+xxi
27	1	59	125	31	146	20	271	2	73	24	5	739+xviii
28	25	273	70	69	78	200	55	39	94	110	14	1005+xxii
29	3	107	62	15	127	169	64	-----	73	57	21	679+xix
30	160	3	41	9	5	36	205	16	73	59	50	644+xiii

Respectfully submitted,

JOSEPH STANLEY-BROWN, *Editor.*

The foregoing report is respectfully submitted.

THE COUNCIL.

December 29, 1919.

On motion, the report was laid upon the table until Tuesday morning.

ELECTION OF AUDITING COMMITTEE

A. C. Lane, H. F. Cleland, and E. W. Berry were elected a committee to audit the Treasurer's accounts.

ELECTION OF OFFICERS

The Secretary declared the election of officers for 1920 as follows, the ballots having been canvassed and counted by the Council in accordance with the By-Laws:

President:

I. C. WHITE, Morgantown, West Virginia

First Vice-President:

GEORGE P. MERRILL, Washington, D. C.

Second Vice-President:

WILLET G. MILLER, Toronto, Canada

Third Vice-President:

F. B. LOOMIS, Amherst, Massachusetts

Secretary:

EDMUND OTIS HOVEY, New York City

Treasurer:

E. B. MATHEWS, Baltimore, Maryland

Editor:

JOSEPH STANLEY-BROWN, New York City

Councilors (1920-1922):

T. W. VAUGHAN, Washington, D. C.
GEORGE F. KAY, Iowa City, Iowa

ELECTION OF FELLOWS

The Secretary announced the election in due form of the following Fellows, the ballots having been canvassed and counted by the Council:

WILLIAM NOEL BENSON, B. A., D. Sc., Professor of Geology and Mineralogy, University of Otago, Dunedin, New Zealand.

ELEANORA FRANCES BLISS, A. B., A. M., Ph. D., Associate Geologist, United States Geological Survey, Washington, D. C.

WILLIAM BOWIE, B. S., C. E., M. A., United States Coast and Geodetic Survey, Washington, D. C.

ARTHUR FRANCIS BUDDINGTON, Ph. B., Sc. M., Ph. D., Instructor in Geology, Brown University, Providence, Rhode Island.

FRANK RINKER CLARK, B. S., Geologist, United States Geological Survey, Washington, D. C.

Joseph Augustine Cushman, S. B., Ph. D., Associate Geologist, United States Geological Survey; Associate Curator, Boston Society of Natural History, Sharon, Massachusetts.

William Barton Emery, B. A., Ph. D., Geologist, Ohio Oil Company, Casper, Wyoming.

Wilbur Garland Foye, B. A., M. A., Ph. D., Associate Professor of Geology, Wesleyan University, Middletown, Connecticut.

Eugene T. Hancock, B. S., Geologist, United States Geological Survey, Washington, D. C.

Albert O. Hayes, B. Sc., M. Sc., Ph. D., Geologist, Geological Survey of Canada, Ottawa, Canada.

Oliver Baker Hopkins, A. B., Ph. D., Geologist, United States Geological Survey, Washington, D. C.

Bertrand Leroy Johnson, S. B., Associate Geologist, United States Geological Survey, Washington, D. C.

Edwin Russell Lloyd, A. B., B. A., Associate Geologist, United States Geological Survey, Washington, D. C.

Marjorie O'Connell, A. B., A. M., Ph. D., Expert in Invertebrate Paleontology, care of American Museum of Natural History, New York, N. Y.

Joseph T. Pardee, Geologist, United States Geological Survey, Washington, D. C.

Harry Justin Roddy, B. S., M. S., Ph. D., Dean, Department of Natural Science; Professor of Geology, State Normal School, Millersville, Pennsylvania.

Ransome Evarts Somers, B. A., M. A., Ph. D., Assistant Professor of Economic Geology, Cornell University, Ithaca, New York.

Chester Wesley Washburne, A. B., Consulting Geologist, 66 Liberty Street, New York.

REPORT OF THE PHOTOGRAPH COMMITTEE

The Secretary reported that N. H. Darton, who has served for many years as the Photograph Committee of the Society, had turned over to his custody the Society's collection of photographs, with the statement that changes in his office made it no longer practicable for him to care for it. Mr. Darton further suggested that the committee be discharged. The report was accepted and the committee discharged, with thanks to Mr. Darton.

NECROLOGY

After announcements were made by the Secretary regarding the program of the meeting and other matters, the President called for the necrology of the year, brief oral tributes being given, leaving the customary obituaries for publication in the Bulletin. The Society lost eight Fellows by death during the year 1919, namely, Joseph Barrell, George F. Becker, Charles H. Hitchcock, Alexis A. Julien, Lawrence M. Lambe, Louis V. Pirsson, G. Sherbourne Rogers, and J. C. de Costa Seña.

MEMORIAL OF GEORGE FERDINAND BECKER [1]

BY ARTHUR L. DAY

With the death of Dr. George Ferdinand Becker on April 20, 1919, the Geological Society of America loses an ex-President and one of its original Fellows. He died at his home, in Washington, at the age of seventy-two years.

Any memorial of Dr. Becker's service to geology or to the Society in which he was most active throughout his career must begin with a sincere tribute of respect and honor to one of the last of that splendid group of pioneer geologists which was brought together by the famous Survey of the Fortieth Parallel, the founders of the United States Geological Survey. Clarence King, the central figure of the group, has long since gone from among us; but his three distinguished collaborators, Emmons, Hague, and Becker, have but lately finished their tasks and left to other hands the great problems which they so courageously mapped out.

Like most of the pioneer thinkers, now unhappily very few in number, Dr. Becker was by necessity the master of several fields of scientific research. He possessed an excellent working knowledge of mathematics, physics, chemistry, and geology, and used all these with the greatest freedom and effectiveness through all of his work. With the possible exception of Gilbert, there was no man of his time in the Washington geological world who possessed greater versatility in discussion or such breadth of view. In consequence of this, no problem was ever brought to his attention, whether in private council or in the more formal atmosphere of the Geological Society, to which he was not at once ready to contribute some fertile suggestion or some novel viewpoint.

It has sometimes been said, and I think rightly, that his formal papers, both in oral presentation and in print, lost something of their effectiveness through an assumption that his listeners and readers were equally well equipped in all of these sciences, and it is certainly true that his most important contribution to theoretical geology, if it may be so called ("Finite homogeneous strain, flow, and rupture of rocks," 1893), was twice republished with popular or field illustrations in place of the rigid mathematical demonstration, because the conclusions would not "clinch" in their mathematical form.

That this versatility was strenuously cultivated by him, rather than an accidental product of early studies, is shown by the fact that the scientific man whom of all his contemporaries he most admired was Lord Kelvin,

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[1] Manuscript received by the Secretary of the Society March 9, 1920.
Presented by title before the Society December 29, 1919.

George F. Becker 1910

a man always distinguished for extraordinary breadth of view. He by no means always shared Kelvin's conclusions, particularly in his investigation of the age of the earth and kindred subjects, but his admiration for the pertinence and breadth of Kelvin's reasoning was outspoken.

Dr. Becker's death, just at the close of the great war, in which he took the keenest interest throughout, recalls to mind an even more personal quality. He was practically without fear and often expressed the impulse of his heart, namely, that, next to being a student of the interior of the earth, he would choose to be an army officer in the field. Dr. Becker visited the Philippines in 1898 with the United States forces under command of General (then Major) J. Franklin Bell and saw a considerable portion of the Philippine insurrection of that year. In Major Bell's official report to Major General McArthur, commanding the Second Division of the Eighth Army Corps, the following interesting paragraph occurs under date of February 11, 1898:

"I have reserved for the end of this letter mention of the exceedingly gallant and courageous conduct of Prof. George F. Becker, United States Geologist, because in accordance with his idea of his duty he insisted on accompanying me into this fight (Galoocan) and remained with the company, much of the time mounted, throughout the entire engagement. He was as cool and collected as if he were pursuing geological investigations in his study, encouraged the men behind whom he was standing, and rendered other valuable services which required him to pass, mounted, immediately in rear of the entire line. I am sorry that, not being a soldier, he cannot receive the reward which his courage and gallantry has entitled him to."

With but a single interruption of two years (1892-1894), Dr. Becker was a "Geologist in Charge" or chief of division in the United States Geological Survey from its establishment, in 1879, to the time of his death, a period of almost forty years. In this position, which he preferred to any other which the Survey offered, he found opportunity to initiate the many new directions of research with which his name stands inseparably associated, and he was spared much of the dull administrative routine of Washington departmental life which he particularly abhorred. Indeed, it may be said of Dr. Becker that the advancement of these various lines of geophysical research was the dominating purpose of his life. Many times during the last twenty years I have heard him say, with that intensity of expression so characteristic of him when in conversation upon the subject he loved, that the study of the interior of the earth was the only thing really worth while.

Though born in New York City (January 5, 1847), Dr. Becker's early life was spent in Cambridge, Massachusetts, where his preference for natural science rather than sports brought him early into contact with such men as Benjamin A. Gould, Jeffries Wyman, Benjamin Pierce, the elder Agassiz, and other distinguished contemporaries, and gave directive impulse to his earier studies. He was graduated from Harvard in 1868 and went abroad at once, taking advanced degrees at Heidelberg in 1869 and at Berlin (Royal School of Mines) in 1871. Neither did he neglect the practical side while abroad, for he was accustomed to speak with some pride of having "begun life" as a "puddler" while still in Germany.

Upon the completion of his education he went to California, partly in pursuit of health, which in early life appears not to have been rugged, and partly from interest in mining and metallurgy, which was his major subject of study while abroad, and became instructor in those subjects at the State University at Berkeley. There he came in contact with Mr. Clarence King, who was then engaged upon the survey of the fortieth parallel.

Mr. King's strong and inspiring personality, aided perhaps by the personal influence of his two younger associates, Messrs. Emmons and Hague, evidently attracted Dr. Becker strongly, for he became deeply interested in the geological problems developed during that survey, and one of them, the Comstock lode, later became the subject of what is, perhaps, Dr. Becker's best-known geological memoir, "Geology of the Comstock lode and Washoe district," 1882.

In 1879, when Mr. King was invited by Congress to organize the United States Survey and to become its first director, Dr. Becker was among the first called to Mr. King's side, and here we encounter almost immediately the pioneer quality of Dr. Becker's mind. Notwithstanding the utilitarian demands of the times and the purposes (then utilitarian also) of the Survey, namely, to discover and record the mineral resources along the line of the newly opened transcontinental railroad (Union Pacific) and adjacent territory, we find Dr. Becker seeking out two physicists (Dr. Carl Barus and Dr. William Hallock) to be his assistants and initiating the first of the geophysical studies which thereafter became his chief interest. The details of his plan as conceived at that time are nowhere formulated, but he evidently had as an immediate purpose a study of the origin and growth of ore bodies, and I think, even at the outset, he had already in his mind a systematic physical and chemical study of the formation of igneous rocks. At all events, the first publications to issue from the laboratory, soon after established in one of the towers of the Smithsonian Institution, had to do with the physical instruments, if they may be thus collectively described, necessary to such a task. I refer to the development of apparatus for the measurement of the high temperatures involved and of a trustworthy scale in which to express and compare them, of appropriate means for determining and expressing rigidity, viscosity, and other determinative qualities.

The program was evidently directed to a quantitative study of igneous rocks, but did not then reach so far. Nearly ten years were consumed in the preparation of the weapons for the attack, which from any viewpoint, whether physical or geological, was at that time a herculean task. Then the political upheaval of 1892 intervened to put an end to the undertaking through the familiar Washington method—the discontinuance of appropriations.

In consequence of this Dr. Becker returned perforce to a routine of Survey work, being occupied mainly with the location and development of the mineral resources of the country.

Notwithstanding the fact that he had enjoyed a rare training for just this kind of activity and had opportunity to work in a region of altogether exceptional interest (California to Colorado), the task failed to hold his interest, and he reverted constantly to the necessity for a strict and more comprehensive application of physical law and method to all genetic problems of geology.

In the "Finite homogeneous strain, flow, and rupture of rocks" (1893), which is perhaps the finest product of Dr. Becker's analytical genius, we recognize a splendid attempt to define and formulate in precise terms some of the relations in the science of "rock mechanics." This was a magnificent task of pioneer quality and of extraordinary difficulty, but was not immediately fruitful, because clothed in somewhat abstruse mathematical form. The paper is destined to exert a considerable influence upon future geological thought (geodynamics).

His papers on schistosity and slaty cleavage were severely criticised, but in many instances his critics failed completely to understand the rigorous methods which he developed for dealing with these rather abstruse problems. His paper on the age of the earth was severely criticised by Barrell. Doctor Becker apparently had no intention of replying to Barrell, although the discovery just before his death of large quantities of helium in natural gas served, I believe, to convince him that his estimate of the age of the earth was not seriously in error.

In 1900 Dr. Becker was able to reestablish his (geophysical) laboratory with the writer as his assistant. The work was resumed substantially where Barus and Hallock had left it in 1892, but with the advantage of more appliances, and proceeded with less interference. The first paper, "The isomorphism and thermal properties of the feldspars," appeared in 1904, and contains an introduction written by Dr. Becker, in which the purpose and progress of his thought in this direction is briefly but clearly set forth.

In this year also the Carnegie Institution of Washington came to the aid of the undertaking and increased both its scope (to include chemistry) and its resources. In 1907 a separate and more appropriate laboratory building was provided by the same institution, and here he carried out (in collaboration with Mr. Van Orstrand) the experimental work on schistosity, elasticity, and diffusion which occupied the closing years of his life. The major portion of the results of this later activity is still unpublished.

He had outlined several papers such as "Elastic after-effect," "Notes pertaining to the probability integral," "Applications of capillarity to oil problems," and a "Treatise on geophysics."

It is greatly to be regretted that the paper on "Elastic after-effect" was not published during Dr. Becker's lifetime. It represents a vast amount of painstaking experimental work by Mr. Van Orstrand and his associates, using the most refined methods known to quantitative physics, and offers, I think, more elaborate as well as more comprehensive data on elastic after-effect than have been obtained hitherto by any investigator. It is obviously unfair, not to say inexcusably presumptuous, to forecast the results of the unpublished work of another, but I cannot forbear directing attention to this paper, when it may appear, because of the value of the data in throwing light upon the vexing subject of the fundamental definition of "solid" and "liquid" and the possibility of placing a boundary between these two states of matter which shall be theoretically sound. The classical definitions of these states of matter were conceived at a period when elastic deformation was much less precisely measurable than now, when the phenomena of plasticity formed a *terra incognita,* and when the study of the thermal relations between these two states had not been extended to such inert substances as the silicates. The consequent confusion in our conception of these states, according as they happened to be viewed from the mechanical or thermal standpoint, was a problem which interested Dr. Becker profoundly and one to which he devoted himself most assiduously during several of the later years of his life.

Dr. Becker enjoyed a wide acquaintance and received many honors, both at the hands of his colleagues and of foreign learned bodies. He was elected to the National Academy of Sciences in 1901; also the Presidency of the Geological Society of America in 1914.

In thought and manner Dr. Becker was a true pioneer, absolutely fearless, impatient of limitations, quick to get at the heart of a problem, direct and vigorous in its prosecution, and with untiring spirit, even under the strain of protracted illness, which clouded the closing years of his life.

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MEMORIAL OF GROVE KARL GILBERT[1]

BY W. C. MENDENHALL

Grove Karl Gilbert was born at Rochester, New York, on May 6, 1843. He died at Jackson, Michigan, May 1, 1918, five days before his seventy-fifth birthday anniversary.

He himself considered that his career as a geologist began in 1869, with his appointment as assistant to Dr. Newberry on the Ohio State Survey. It ended on April 22, 1918, nine days before his death, when he transmitted to the United States Geological Survey, for publication, two completed chapters of a projected larger work on the Basin Range problem. Thus, in his last days, he returned to one of his earliest major subjects, completing, as it were, one of those rhythmic cycles which had furnished the theme of one of his most thoughtful papers.

Gilbert's half century of active professional life spans a highly productive period in the development of American geologic science and of organizations for the advancement and application of that science, and to this development he contributed in a masterly and constructive way. He began as a museum worker and became a State Survey assistant. He then joined in succession two of the great Federal exploratory surveys, and when they were merged in the present United States Geological Survey he became a member of its staff and continued to be the wise and trusted counselor of each of its directors until his death. Especially throughout the formative period of Powell's directorship was Gilbert's counsel most intimate and potent. A broad foundation of methods and policies was then laid on which the organization stands today.

Meanwhile, with varied advisory and administrative responsibilities, many of them none too congenial to a man whose tastes were essentially those of the student and investigator, he contributed widely and always authoritatively to the solution of the problems of the time in geology and cognate subjects.

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[1] Manuscript received by the Secretary of the Society June 20, 1919.

NOTE.—The writer is deeply indebted to many relatives and friends of Mr. Gilbert for unpublished biographical material. Especial mention should be made of Mr. A. M. Gilbert, San Francisco, California, who has given the freest and most generous access to his father's records; Mrs. Emma Gilbert Loomis, Jackson, Michigan, who has supplied facts and incidents not available from any other source; Mr. H. W. Henshaw, associate on the Wheeler Survey and friend since 1872; from his letters I have quoted freely; Miss Alice Eastwood, San Francisco, California, long an intimate friend of the Gilberts. It is hoped that the rich biographical material may somewhere be more adequately used than is practicable in this brief sketch.

Yours gratefully,
G. K. Gilbert

Gilbert's bibliography contains about 400 titles, many of them, of course, abstracts, reviews, administrative reports, and other brief miscellaneous papers, over 100 being contributions to popular encyclopedias; but there are a dozen or more master papers which represent distinct advances in the principles or the philosophy of our science. We get some conception of the range of Gilbert's mind by a mere random recital of a few of the topics upon which he has written:

Archeology	Glaciation	Paleontology
Bibliography	Graphics	Pictographs
Biography	Ground waters	Physiography
Chess	Hypsometry	The soaring of birds
Correlation problems	Intrusion	Simplified spelling
Earth origin	Isostasy	Surveying
Erosion	Meteorology	Topography,
Exploration	Nomenclature	both lunar and terrestrial
Geologic time		

John Gilbert, Jr., accompanied by his wife and his sons Thomas, John, and Giles settled in Dorchester, Massachusetts, in 1630. He was followed later by an older son, Jonathan B. Gilbert, from whom Grove Karl Gilbert is descended.

G. K. Gilbert's great-grandfather served through the Revolutionary War as quartermaster and as captain of a troop of light horse. His grandfather, John Gilbert, was born in New Hartford, Connecticut, but moved to New York and became a tool-maker in Clinton and Le Roy. In Clinton, Grove Sheldon Gilbert, father of G. K., was born on August 5, 1805. He married Eliza Stanley on November 30, 1826. Of him his daughter, Emma Gilbert Loomis, writes:

"He was a portrait painter and painted in Rochester for over 50 years. Charles Elliott once said of him that he painted the best head in America. He was a man of remarkable character and varied attainments. In his work he strove earnestly and always to attain an ideal—that of a fine picture—but was never satisfied. Fame and success appeared to be within reach, but were never reached, for 'the picture was the goal.' "

Here, at Rochester, Grove Karl, the youngest of three children, was born, on May 6, 1843. The family had little means and lived in a small house called the "Nutshell"—a name under which it is frequently referred to in the Gilbert journals.

Karl, as the lad was called in the family and among his playmates, appears to have passed a normal, busy, and studious boyhood. Many years later, when asked by his own son as to what he did when a boy, he replied quizzically, "I studied a good deal when not working." There is one verdict available to us from a teacher at Tonawanda, New York, where the lad of ten, as an incident in a long visit to an aunt living there, attended for a time a school conducted by George R. Barker. Upon his departure Mr. Barker wrote as follows to Grove Sheldon Gilbert:

"It affords me much pleasure to make to you a commendatory report of your son Karl's conduct. During the four months he has been in my school his deportment has been unexceptionable, and he has been a most faithful, industrious, and attentive student, meriting in every way my highest approbation. It is with much regret that I part with so exemplary a scholar. I am sure that a boy of Karl's ingenuousness of character can not fail to find friends wherever he may go. May success crown all his efforts."

This glowing opinion is supported by that of the aunt with whom Karl was then visiting. She wrote to his father as follows on October 4, 1853:

"He (Karl) advances famously with his studies. . . . I believe he could learn double the number of lessons if required."

The lessons at that time, as listed by the lad himself in a letter to his father, included Latin and French, as well as English, grammar, spelling, arithmetic, and reading.

So the years passed in enthusiastic study and in wholesome play. The Genesee River was only a block from the Gilbert home, and that fact had its due influence upon the boy's activities. He and a playmate from the neighborhood together built two or three small rowboats and Karl became a skilled oarsman. He is reported to have won at least one regatta, by default, in a boat of his own building, the owners of the other craft in his class withdrawing, for they had seen the *Great Western*, as Karl called his tiny craft, in action and did not relish a contest with it.

The skill in rowing and the enjoyment of it, acquired in this boyhood environment, lasted throughout Gilbert's life; for in late years, after impaired health had placed limits upon his physical activities, one of his favorite recreations was rowing or canoeing with a congenial companion on the Potomac or on Lake Cayuga. Doubtless, too, the boyhood sports on the Genesee River made possible that efficiency in watermanship which contributed so much to the success of the remarkable trip of Lieutenant Wheeler and associates in 1871 up the lower canyons of the Colorado River to the mouth of Diamond Creek. A reading of the narratives of that adventure reveals the substantial part played by Gilbert and the respect which his skill as well as his spirit commanded.

Karl Gilbert graduated from the Rochester High School in 1858, at the age of 15. "He is remembered as a quiet, modest boy with pleasant manners, very kindly disposition, and of very even temper."[2]

From the high school he entered the University of Rochester, graduating with the degree of A. B. in 1862, at the age of 19.

The college work involved sacrifice on the part both of the boy and his family. He had to go rather shabbily dressed, was unable to join much in the social life that offered, and yet found himself in debt upon graduation. He is reported by his classmate, Dr. Aaron Clark, to have been quiet and studious, one who did well whatever he had to do, but as especially good in mathematics and Greek. Indeed, he was selected to deliver the Greek oration at commencement. His college record indicates a preponderance of Greek, Latin, and mathematics, with some English and a very minor proportion of science. There was but one term of geology, one of chemistry, one of astronomy, and two of zoology. In this curriculum, typical of the arts courses of the day, there is naturally little indicative of the future bent of the man. But mathematics appealed even then to his love of precise methods and he never lost his interest in problems of that type. He himself has stated that during his college career he was much interested in mathematics and engineering, but not particularly interested in geology.[3]

Before Gilbert graduated, Henry A. Ward became professor of geology and natural history at Rochester and there founded that unique institution known as Cosmos Hall. This was an event of great interest to all of the students, but especially to Gilbert, and was destined to become an important factor in his career.

After graduation, Gilbert went to Jackson, Michigan, where he attempted to teach in order to pay off his college debt; but he was neither happy nor successful as a teacher, not being well equipped temperamentally to deal with those unruly pupils, indifferent to study, who make up a proportion of the students in every public school. So he gave up the school when the term was but two-thirds completed and returned to Rochester. There, soon afterward, he entered Cosmos Hall, where he remained until he joined the Ohio Survey, under Dr. Newberry, on July 1, 1869.

[2] H. L. Fairchild: Grove Karl Gilbert. Proceedings of the Rochester Academy of Sciences, volume 5, 1919.

[3] Personal communication to J. P. Buwalda.

Cosmos Hall "was devoted to the assemblage and preparation of scientific material for museums of natural history. . . . Its work was performed largely by young men of congenial tastes, who there acquired the practical experience which commended them later to the trustees of larger responsibilities. It thus served incidentally as a training school in the natural sciences. . . ." [4]

Among the graduates of this institution were a number of men who have since become distinguished in various departments of natural history. Gilbert spoke of himself "somewhat proudly" as "senior alumnus" of this unique establishment. Professor Ward was a remarkable personality, an enthusiastic collector, a man with a broad knowledge of science and a wide acquaintance with scientific men here and abroad. He is described as a most interesting and inspiring companion and a skillful raconteur with an inexhaustible fund of humorous stories.

The five years spent in intimate association with Ward at the very beginning of his career undoubtedly aroused Gilbert's latent scientific bent, served as a postgraduate scientific course, and developed his strong inherent tendency toward system and accuracy in the consideration and discussion of natural phenomena.

Gilbert was not a geologist, nor yet a scientist, when he joined Professor Ward's establishment; but while there he prepared and published his first paper, a well written popular article on "The American mastodon," which appeared in *Moore's Rural New Yorker* on March 2, 1867. In Prof. James Hall's [5] description of the "Cohoes mastodon," Gilbert is credited with the measurements and comparisons of the parts of the skeleton which he had excavated and mounted in the State House at Albany. To this paper he also contributed a signed chapter on the circumstances of the deposition of the skeleton, in which an interesting estimate of 35,000 years is given for the period that has elapsed during the recession of Cohoes Falls from the mastodon pothole to its present position. Professor Fairchild fixes the date of the field-work on which this essay is based as 1866, when Gilbert was 23 years old, but says the paper may have been completed as much as two years later.[6] The date of publication is 1871 and of transmittal 1868.

Gilbert's interest in geologic research, stimulated by this problem, led him to seek an appointment as assistant on the Second Geological Survey

[4] G. K. Gilbert: Memoir of Edwin E. Howel. Bull. Geol. Soc. Am., vol. 23, 1912, p. 30.

[5] Twenty-first Annual Report of the New York State Museum of Natural History, 1871. pp. 99-148.

[6] H. L. Fairchild: Grove Karl Gilbert. Proceedings of the Rochester Academy of Sciences. vol. 5, 1919.

of Ohio, for which appropriations were made by the State legislature in 1869. He went to Columbus, called upon Governor Hayes, and was informed that the regular appointments would be given to residents of the State; but he also learned that Dr. Newberry was among those under consideration as State Geologist. Gilbert then called upon Dr. Newberry and was offered a position as volunteer assistant, with $50 per month from the State toward the payment of expenses. This offer was accepted, and Gilbert thus came into association with a group of men, with Newberry at their head, many of whom later came to recognized leadership in American geology. In addition to Gilbert, other members of this group were Irving, Newton, Winchell, and Orton.

Gilbert began his work as a volunteer on July 1, 1869. That summer and the next were spent in field-work in the northwestern part of the State, while the succeeding winters were passed in part at least in New York City, where Dr. Newberry occupied the Chair of Geology and Paleontology at the Columbia School of Mines. Here Gilbert assisted Newberry in various ways. He made drawings of fossils for reproduction in the Ohio reports which won generous praise from his chief. He aided in the preparation of some of Dr. Newberry's lectures. Here, too, doubtless he prepared his own reports on the geology of the Maumee Valley and the counties of northwestern Ohio.

Through Dr. Newberry he became acquainted with members of the famous New Haven group of geologists, his journal recording a journey which the two made there together on Fabruary 23, 1870, when he met Professors Silliman, Marsh, Norton, and Blake. He even undertook to prepare some drawings for Professor Marsh, in addition to his work for Dr. Newberry.

He appears to have taken full advantage of the general opportunities which residence in New York gave him. The theater and lectures on a variety of topics attracted him. The catholicity of his tastes as well as the tendency to maintain a careful balance between opposing theories are indicated by the two lectures which he attended on January 9, 1870, one by Henry Ward Beecher, entitled "Request of the disciples for more faith;" the other by George Francis Train, on "Old fogies of the Bible."

The direct products of his own work while with the Ohio Survey are the county reports, the surface geology of the Maumee Valley, and some shorter papers, in which advance publication was given to the more important general conclusions resulting from the field-work. In these reports he described clearly the series of beaches around the western end of Lake Erie and gave first published recognition to the old southwestern outlet of the lake through the Maumee and Wabash valleys; although, with the same fairness and care that always distinguished him, he gave full credit to local observers for prior recognition of the phenomenon. Of the maps accompanying the final report in which this outlet and the beaches with which it connects are shown Dr. Fairchild says:[7]

"These fine maps are the first ever made in delineation of ancient lake beaches and correlation with the controlling outlet."

He also explained for the first time the peculiar drainage relations of the Maumee and its tributaries, the Saint Joseph and Saint Marys rivers, attributing them to control by lobate moraines left by the glacial tongue that occupied the valley of the Maumee at one stage of its retreat. This conception, at that time new and perhaps radical and startling, seemingly came to Gilbert clearly and definitely while engaged upon his field-work out of Toledo, for his journal, under date of November 10, 1870, contains this interesting entry: "Invented the moraine hypothesis for Saint Jo and Saint Marys rivers." Gregory,[8] referring to the paper in which this concept is discussed, credits Gilbert with the first clear and unequivocal recognition of either terminal or recessional moraines in the United States.

Gilbert left Rochester in April, 1871, for San Francisco, on his first assignment to the Wheeler Survey. He was then 28 years old, his career was definitely chosen, and his period of preparation may be regarded as completed. There is no hint that in his college days he had looked forward to a scientific career. His father was an artist; his grandfather a mechanic, who in 1824 invented a rotary steam-engine. His grandmother, Eunice Barnes Gilbert, was the daughter of a wooden-clock maker who connected a clock in his shop with a dial in the living room below and was considered very ingenious. His great-grandfather was an officer in the Revolutionary War and others of the family served honorably in the Colonial wars as well. There is no definite scientific trend discernible in the family, but there is artistic and mechanical skill, honor, patriotism, and high ideals.

I think we must look primarily to the influence exerted by the two magnetic and enthusiastic personalities with whom young Gilbert was

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[7] H. L. Fairchild: Grove Karl Gilbert. Proceedings of the Rochester Academy of Sciences, vol. 5, 1919.

[8] H. E. Gregory: A century of geology. Steps of progress in the interpretation of land forms. American Journal of Science, vol. 46, July, 1918, pp. 104-132.

early thrown for the direction of his life into scientific channels. Prof. Henry A. Ward gave the initial impulse. The five or six years spent in the enthusiastic atmosphere of Cosmos Hall, with the ever-growing collections, the occasional opportunities for field excursions and observations, the measuring and mounting of specimens, the increasing familiarity with rocks and fossils and geologic material generally, the chance to test the powers of constructive work given by the Cohoes studies, the sense of capacity that must have come, for the work was well done—these doubtless went far toward determining a career. Then there was sought and found the opportunity in Ohio—a new and broader field, new problems, new associates, another inspiring leadership in Dr. Newberry. Dr. Newberry's old home was in Cleveland, and during the existence of the Ohio Survey he spent the summers in the State and was often in Cleveland. Gilbert was on terms of intimacy with the family there, so that summer and winter during these two years, from 1869 to 1871, the relations between the two were very close.

I can imagine that these two enthusiasts in geologic science, Ward and Newberry, were greatly attracted by the clearness and fairness and mental power of their assistant, and that they offered every encouragement in the direction of a scientific career. Certain it is that the first associations of chief and assistant developed in each case into firm friendships, for Professor Ward and Dr. Newberry always sought Gilbert when in later years they came to Washington, and he in turn always looked forward to a visit with them when in New York or Rochester.

Henry W. Henshaw, lifelong friend and associate of Gilbert, writes [9] that once in after years, when they had gone to New York together, Gilbert especially to see Newberry, "We called on him in his hotel one Sunday morning rather early and found him in bed. When he came down we sat at the breakfast table with him while he ate, and then walked over to his quarters in Columbia College, where he and Gilbert proceeded to discuss geologic problems at a great rate."

It was through Dr. Newberry that Gilbert was selected as geologist to the Wheeler Expedition. Before the Civil War Newberry had been attached as geologist and naturalist to the Williamson, the Ives, and the Macomb expeditions to the Far West, under the auspices of the War Department. His work on the Sanitary Commission throughout the war had necessarily increased his acquaintance with the personnel of the military establishment. It was but natural, therefore, that Lieutenant

[9] Personal communication.

Wheeler should appeal to him to nominate a geologist for the new western work. He recommended Gilbert, who was at once appointed.

Gilbert was 28 years old when his appointment as geologist with the Wheeler Expedition transferred his field of activity to the West. and although he was later to return and again to take up problems connected with the history of the Great Lakes, upon which he had touched surely and skillfully while with the Ohio Survey, the western fields and their problems were now to command his attention and his energies exclusively for the next decade.

Gilbert's connection with the Wheeler Survey lasted from the spring of 1871 until December, 1874. Three of the summers were utilized in extensive and rapid reconnaissances, under conditions that were none too favorable for geologic work, and the fourth summer, that of 1874, was devoted to the completion of the resulting reports. On the first of these trips Gilbert left Rochester on April 21, 1871, arriving at San Francisco May 2.

The reconnaissance of that year carried him southward across central Nevada, through Death Valley and into Owens Valley, California, then eastward to the Colorado River at old Fort Mohave, above the present site of Needles. From this point began that remarkable boat trip up the lower canyons of the Colorado to Diamond Creek. At Diamond Creek the journey was resumed by pack train, and continued southeastward through central Arizona to Camp Apache, then down the valley of the Gila to its junction with the Colorado at Yuma. From Yuma the party went by boat down the Colorado, and through the Gulf of California and the Pacific to San Francisco, where it arrived on January 3, 1872.

Gilbert's work during the following summer was confined to western and central Utah, where he had his first introduction to Lake Bonneville and to northern Arizona. During the summer of 1873 headquarters were at Fort Wingate, and Gilbert's field was western New Mexico and eastern Arizona from Fort Wingate south.

The geologic results secured by the Wheeler Survey appear in volume III of the reports of that organization and in the atlas. Gilbert's written contributions are represented by two papers, the first containing the results of his work in 1871-1872 in Utah, Nevada, California, and western Arizona, and the second his eastern Arizona and western New Mexico discussions, products of the work of 1873.

The Basin Range and plateau provinces were discriminated, the characteristics of each set forth, the fault-block theory of the origin of the first enunciated, none too definitely, perhaps, and Lake Bonneville was named and its general features were described. Thus the opening chapters were written on two of Gilbert's principal contributions to the development of geologic science.

The association with the Wheeler Survey had a significance in Gilbert's life, however, other than the direct opportunity that it gave him to become acquainted with the inspiring problems awaiting solution in the West. It brought him into contact with a group of brilliant men deeply engrossed in geologic and geographic exploration, and into an environment that to a young geologist and geographer must have been one of the most stimulating of any in the world at that time.

Near the end of January, 1872, at the end of his first field season with the Wheeler Expedition, Gilbert went to Washington, if not for the first time, at least for his first protracted stay. Lieutenant Wheeler's headquarters were there, and there Gilbert established himself for the winter. He and his close friends and cogeologists on the Wheeler Survey, Archibald R. Marvine and Edwin E. Howell, clearly found the opportunities and associations in Washington at that period much to their liking.

Professor Baird was then Secretary of the Smithsonian. Major Powell, with the prestige of his Grand Canyon trip still fresh upon him, was in the city for the winter; the Philosophical Society, organized on March 13, 1871, was the common meeting place for scientific discussion, the informal adjourned sittings holding in the minds of the members quite as valuable a place as the more dignified, formal gatherings, where the prepared papers were delivered.

Gilbert was assiduous in his attendance at the formal and presumably also at the adjourned meetings. He was soon on terms of intimacy with Baird, Newcomb, Hilgard, Harkness, Abbey, and others. It is highly probable, too, that he first met Major Powell and Lieutenant Dutton at one of these meetings. We know at least that by April he was calling at the Powell home.

It was inevitable that these two should have much in common. Both were geological explorers; they had seen contiguous and in part identical fields; they were both inspired by the magnificence of the phenomena there displayed and by an intellectual enthusiasm for the solution of the new problems encountered. Powell, with his originality and remarkable fertility in ideas, must have recognized a splendid foil in Gilbert's steadiness of thought and accuracy of observation, and Gilbert in turn must have been greatly stimulated by the Major's brilliancy in conception and in generalization. So in the new circle of valued and valuable friends formed in the winter and spring of 1872 none grew into a closer intimacy than that with Powell and none was destined to have more influence upon Gilbert's future.

During the succeeding winter seasons of 1872-1873 and 1873-1874, each following a summer of field-work for the Wheeler organization, the intimacy between Powell and Gilbert grew. Gilbert did not go West during the summer of 1874, but remained in the East, completing his Wheeler reports. That autumn, November 10, he married Miss Fannie Porter, a sister-in-law of Archibald Marvine, and on December 2 he entered into a contract with Powell and began work for his organization. The official association thus initiated was maintained until the Major's resignation from the Directorship of the Geological Survey, in 1894.

Gilbert's work was now to be done under more favorable auspices. The conditions for geologic observation could not be of the best when the geologist was attached to a party whose movements were necessarily controlled by other considerations than his needs. Gilbert sets forth the disadvantages under which he labored on the Wheeler expeditions in a prefatory note to a limited edition of his Wheeler Survey papers issued in 1876. He says:

> "The observations that form the basis of these reports were hurried in the extreme. The writer, for the most part, accompanied field parties which were especially equipped for rapidity of movement and were crowded to the utmost. Moreover, in a country almost unmapped the demand for geographical information was more urgent than that for geological, and all plans and routes were accordingly and with propriety shaped to give the topographer the best opportunities consistent with rapidity of movement, while the geologist gleaned what he could by the way. To study the structure of a region under such circumstances was to read a book while its pages were turned quickly by another, and the result was a larger collection of impressions than of facts."

The conditions of military control, too, were less favorable to technical publication and to informal presentation of results than civilian control, particularly if that civilian control is itself scientific, and therefore sympathetic. These conditions, together with the now established friendship between Gilbert and Powell, make it very easy to understand why Gilbert late in 1874 transferred his services from Wheeler to Powell.

Under the auspices of the Powell Survey, Gilbert returned in 1875 to Utah. That summer was spent in the region of the Aquarius Plateau, the Water Pocket fold, and the Henry Mountains, while the succeeding summer was devoted entirely to the Henry Mountains study.

The summer of 1877 was spent in irrigation studies for Powell, and that of 1878 to designing and executing a triangulation system in the Plateau Province as a base for topographic surveys.

The period of the Powell Survey was thus one of diversified activities for Gilbert, but it was, nevertheless, very productive from the geologic and physiographic standpoint. The Henry Mountains report, which defined and fixed the laccolite conception of structure and which contains also the remarkable chapter on "Land sculpture," was completed early in 1877. The greater part of this chapter had been published in the *American Journal of Science* for July and August, 1876, under the title "The Colorado Plateau Province as a field for geological study."

Powell's "Lands of the arid region," issued in 1879, contains two chapters by Gilbert, one on water supply and one on the irrigable lands of the Salt Lake drainage system, and various brief papers were published on the Wasatch and on Bonneville features. Gilbert's part in the topographic work of the Powell Survey, now remembered by so few, is appreciatively outlined by Captain John H. Renshaw in a letter prepared as one of those which it was intended that Gilbert should receive on his 75th birthday anniversary. I quote.

> "I venture to say that few men . . . know of this example of your versatility—how you planned and executed the scheme of triangulation connecting the old Gunnison base with the provisional base at Kanab, and later in the season had the latter base remeasured with a most ingenious apparatus designed by yourself and constructed under your personal direction; how, during the following office season, the whole scheme was adjusted and computed and stands today as a standard."

With the abolition of the preexisting surveys in 1879 and the creation of the United States Geological Survey, Gilbert became one of its six senior geologists and remained an honored member of its staff until his death.

But, beginning almost immediately, science began to pay the penalty that is usually paid when men of established wisdom and ability become members of large organizations. Gilbert was withdrawn more and more completely from personal research and utilized in the general problems of organization and administration. For the first two years he retained headquarters in Salt Lake, continued his Bonneville studies, and administered the Division of the Great Basin; but with the appointment of Powell as Director in March, 1881, Gilbert was recalled to Washington. Powell desired to have close at hand the wise and balanced counsel of his coexplorer and trusted adviser; hence the period from 1881 to 1892 is a period in which Gilbert's duties were largely administrative. In 1883 the field-work of the Great Basin Division was closed. In 1884 Gilbert was placed in charge of the Appalachian Division of Geology. On January 1, 1889, the Division of Geologic Correlation was created and Gilbert placed at its head, and on July 1 he became Chief Geologist—a position which he retained until August, 1892.

Naturally this general period is not one prolific in personal studies. To be sure, the Bonneville monograph, which Gilbert himself in a personal letter has called his *magnum opus*, was completed during this interval and appeared in 1890; but it is the fruit of earlier investigations. Some of his best brief philosophical papers, like the first presidential address before the American Society of Naturalists, on "The inculcation of scientific methods by example," belong to this period; but on the whole his contributions are indirect and appear largely in the work of others, whom he counseled and inspired, and in the general advance made by the Federal Survey toward a leading position among organizations of its class.

He had much to do with the planning and inauguration of the Survey's bibliographic work, whose annual volumes are now such useful tools in the hands of investigators. He had no small part in the adoption of the principles of nomenclature and cartography set forth in the Tenth and Twenty-fourth Annual Reports and forming the basis of the Survey's geologic map-work. As head of the Division of Geologic Correlation he was responsible for the selection of the authors of the correlation bulletins and for their preparation.

As a clear analyst of problems and a master of scientific presentation, as well as a sympathetic critic, he helped many an author from mediocrity to respectability. That which Gilbert[10] said of Powell may well be said also of Gilbert:

> "The work which he inspired, and to which he contributed the most important creative elements, I believe to be at least as important as that for which his name stands directly responsible."

It is probable that, had he so desired, Gilbert might have become Director of the Geological Survey during this period; but his judgment of himself and his capacities was as clear, cool, and detached as though he had no personal interest whatever in the matter, and he decided that his most useful field was that of research, not that of administration. Powell's interest in the Bureau of Ethnology and in the philosophical investigations in which he was engaged had very largely replaced his personal interest in geological problems, so that he undoubtedly had it

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[10] G. K. Gilbert: John Wesley Powell. Annual Report of the Smithsonian Institution, 1902, pp. 633-640.

in mind to relinquish the directorship of the Geological Survey long before he actually did so.

Of that situation and Gilbert's attitude Mr. Henshaw writes:

"I think it was early in the 80's (I have no means of recalling the exact year) when he came to me (we were on our way to a meeting of the Philosophical Society) with a request for advice: for, as he said, probably none of his associates knew him better than I. Powell, he stated, wished to resign the position of Director of the Geological Survey in his favor, so that he himself might be free to devote all of his time and effort to building up the Bureau of Ethnology, of which at the time I had subordinate charge. . . . The occasion was a difficult one for me, but I knew Gilbert too well to doubt for a moment that what he wanted was that perfect frankness which concealeth nothing and is possible only between tried friends. I told him in substance that while no one who knew him could doubt for a moment that the future of the Geological Survey would be safe in his hands, nevertheless I felt sure that administration was not his strong forte; that, aside from the thousand and one vexatious details of daily administrative life, which in time I felt sure he would come to abhor, the necessity of a certain amount of political work would prove a heavy strain upon him, and I urged that if no feeling of duty was involved, and if he felt free to make his own choice, I should advise him to hesitate long before giving up his own selected field, so full of promise and in which he had already gained so enviable a place. He assented to what I said, and added that he had come to much the same conclusion."

In 1888 Gilbert made his only trip to Europe. In September of that year he attended the International Geologic Congress at London and visited Scotland, Ireland, the Isle of Wight, and Paris.

In 1892, with the sweeping reduction in Survey appropriations and the reorganization consequent thereto, Gilbert, doubtless with great relief, relinquished his position as Chief Geologist, and with it the greater part of his administrative responsibilities.

Another, and the final, phase of his career, marked by the gradual resumption of personal research, begins with this date. The period from 1892 to 1918, a period of lessening administrative duties and, for the last nine years, one of precarious health and reduced physical energy, is a productive period of varied scientific activities, with a proportion of new researches, but with also a tendency to recur to earlier problems, in order to develop them more fully than was possible when first they were realized.

Southeastern Colorado was a field of investigation to which about three years were devoted. The problems were structural, stratigraphic, and economic, and the results appear as folios, which contain some original features, and some papers on water-supply and on fire-clay.

As by-products of this areal work we have the papers on "Sedimentary measurements of Cretaceous time," "Tepee buttes," a joint paper by Gilbert and Gulliver, and the description of a new laccolite locality.

The Niagara River and Great Lakes studies, which had been carried on intermittently prior to 1894, were resumed more systematically thereafter, and there appeared the National Geographic Monograph on "Niagara Falls and their history," the "History of the Niagara River," published in the Sixth Annual Report of the Commissioners of the State Reservation at Niagara, and the paper in the Eighteenth Annual Report of the Survey on "Recent earth movement in the Great Lakes region," with a number of less formal discussions of phases of the problems, presented in other ways.

The Coon Butte study eventually became the basis for the stimulating presidential address before the Geologic Society of Washington, on the "Origin of hypotheses," a paper to be compared with the earlier address on the "Inculcation of scientific method by example."

The Coon Butte investigation also led Gilbert to a study of the craters of the moon, and this to the delivery of that most fascinating address on "The moon's face," which was presented before the Philosophical Society of Washington in December, 1892.

Meanwhile Gilbert was serving geology in an international capacity as chairman of the Commission of the International Geological Congress on Geological Bibliography. He also continued the work represented in the Tenth Annual Report of the Survey in the capacity of chairman of a new Survey Committee on Geologic Nomenclature and Classification, whose results appear in the Twenty-fourth Annual.

In 1899, following a short excursion to Mexico, there came the opportunity for studies of problems connected with glaciation, afforded by the Harriman Expedition, followed by the sumptuous reports of that expedition, the one on "Glaciers and glaciation" being by Gilbert himself.

He was led also to review the evidences for and against the "Fault block" theory of the origin of the Basin Ranges, by the presentation of Spurr's [11] opposing views before this society in 1900. Gilbert revisited the field in 1901, with his companion in so many similar studies, Willard D. Johnson, but the loss of a part of the records of that work led to the postponement of its presentation. Field-work was resumed and extended in very recent years, and just before his death Gilbert submitted for publication a report, incomplete as a whole, but complete as to cer-

[11] J. E. Spurr: Origin and structure of the basin ranges. Bull. Geol. Soc. Am., vol. 12, 1901, pp. 217-270.

tain chapters, in which his mature views are presented and the whole conception more comprehensively stated than before. It may be said in passing that additional evidence strongly confirms the conclusions reached in the Wheeler Survey reconnaissance.

Following the Harriman Alaskan Expedition, Gilbert turned again to the Pacific coast. In 1903 he undertook some investigations of the glaciation and morphology of the Sierra, from which several valuable short papers resulted. In 1904 the California Miners' Asociation petitioned the President of the United States to instruct the Geological Survey to make a general study of the problem of the transportation of debris in its relation to mining, agricultural, and transportation interests. The work was undertaken and Gilbert assigned to the problem. He particularly welcomed the assignment for the opportunity it would give him to apply quantitative and experimental tests to the qualitative and deductive conclusions which he had reached in his chapter on "Land sculpture," in the Henry Mountains report. A laboratory was established at Berkeley, field studies were carried out in the Sierra, along the California streams, and about San Francisco Bay, and the results were about ready for presentation when the first physical breakdown came, in 1909.

Gilbert's most obvious anxiety at that time was the fear that the work which he had done might be lost, and it was a source of supreme satisfaction to him when the two papers, Professional Papers Numbers 86 and 105, in which these results were presented, had been completed.

The first of these papers is a record of experimental data and a mathematical and physical discussion of them, too abstruse for the majority of geologists to read; the general problem is treated in the second volume as a quantitative discussion of certain physiographic processes and the effects of those processes upon the rivers, valleys, and harbors of the region, and thus upon human industry.

While the debris investigations were under way central California was shaken by the earthquake of April, 1906. Gilbert, in Berkeley at the time, took an active part in the seismological studies which followed, and, as was practically always true when he was engaged upon a general problem, a number of papers, in this case generally brief, resulted.

With the completion of the earthquake studies and the report upon the debris investigations he returned to the familiar field of the "Basin ranges," with results already recorded. With the completion of the most important chapters of his review of this problem, he must have taken keen satisfaction in the realization that little of the work that he had undertaken remained unfinished.

In sheer balanced mental power, Gilbert was probably unsurpassed by any geologist of his time. Fundamental among the qualities of his mind were self-knowledge and self-control. These qualities he possessed in a degree equaled by few. That mind which he knew and controlled so well was a quiet, efficient, powerful instrument, which functioned perfectly. Thus he was the very antithesis of the brilliant, temperamental, erratic genius. He recognized both his powers and his limitations, and did not undertake that which he was not equipped to do. When he had entered on the study of a problem, he brought to bear on it a rare capacity for discriminating observation. Few significant facts escaped him, but he was rarely diverted from the path of progress toward a correct solution by the mass of facts that are not significant. Having gathered his facts, he weighed them and marshaled them in proper order, and so presented them that the solution at once became obvious. Thus, in his writings Gilbert never seems to be, and in truth never is, supporting a theory. He puts all theories of which he can conceive to the test of fact, indifferent as to which, or as to whether any, survive. It is the truth, and the truth only, that he seeks.

Finally, in addition to that self-knowledge and self-control which directed his activities into appropriate fields, that patience and capacity in observation which, given sufficient opportunity, assured the mastering of available facts, and that logical power which analyzed and interpreted them with infallible precision, Gilbert also developed the art of presentation to such a point that his papers are models of scientific exposition. Clearness in presentation with him, as with others who master the art, is primarily a result of clear thinking; but in his case at least the clear thinking was applied as directly to the problem of presentation as to the scientific problem itself. Interestingly enough, Henshaw [12] states that "early in his career he was not a ready writer and found some difficulty in satisfying himself as to the literary quality of his reports. As a result, however, of care and diligent labor, he soon acquired a singularly simple and lucid style, which later distinguished all his communications to his fellows and the public."

In personal characteristics Gilbert was direct, frank, simple, and unconventional, but calm and dignified, very even and serene in temper, very kindly and considerate, but in nowise demonstrative. He was the

[12] Personal communication.

least introspective and the least reminiscent of men, at least so far as he revealed his thoughts to others. He lived essentially in the present, and while his primary interest, of course, was in his work, he also exhibited a lively interest in the general affairs of the world, about which he was always well informed.

He bore his personal bereavements and adversities philosophically and without complaint, and maintained always a serene and cheerful attitude toward the problems of life—an attitude thus epigrammatically expressed to a friend at the moment greatly depressed by a series of misfortunes: "There are two sorts of troubles about which it is not worth while to worry—the troubles that we can help and those that we can not help." He had a strong distaste for controversy, and his own writings and his own relations remained singularly free from it, during a period when scientific writings too often were marred by personalities. His fairness and generosity in matters of scientific precedence rendered it impossible for even the most ebullient of contemporaries to find any plausible basis for personal recrimination.

Gilbert's amusements were very simple and wholesome. He disliked formal functions of all kinds, but greatly enjoyed the company of congenial friends. He was fond of walking, of rowing and canoeing, but did not care to be alone and usually invited some friend to join him in the excursion. He was also fond of cards, and billiards became one of the recreations of his later years. Of these Mr. Henshaw writes:

"He gave to the mastery of games much the same care and attention that distinguished his scientific work. He was especially fond of euchre and whist, but knew and liked to play all manner of games with cards. . . . Powell, too, was a lover of cards, and from the early 70's on a card game at least once a week was an established custom with us. . . . I think it was chiefly on Powell's account that Gilbert took up billiards, and in this, as in all other things he elected to do, he soon came to be surprisingly proficient, considering that he was no longer young when he began to play. . . . Toward the end of his life, after the day's work was done, he could usually be found in the billiard-room of the Cosmos Club, forgetful of all problems but the ones presented by this fascinating game."

He derived much pleasure from guessing and rhyming games, and on occasion amused himself by composing ingenious limericks or more ambitious rhymes.

Reading aloud to a group of friends was one of his favorite diversions, the purpose of the readings being relaxation and entertainment rather than information. A good book or a good short story seems usually to have been selected. Samuel Crothers is mentioned as one of his favorites, and numerous friends have made the acquaintance of Clarence King's "Mountaineering in the Sierra Nevada" at his suggestion or as a result of attendance at these readings.

Gilbert's family relations were ideal, for he was a most faithful and devoted husband and father. Mrs. Gilbert died in 1899, after a long period of invalidism, during which she received the tenderest and most solicitous care. Their eldest child, a daughter and a great favorite of her father, died of diphtheria at the age of seven. Two sons survive—the older, Archibald Marvine Gilbert, being a distinguished engineer of San Francisco, California. An elder sister, Mrs. Emma Gilbert Loomis, to whom her brother was greatly devoted, now lives in Jackson, Michigan. After the death of his wife, Gilbert made his home, when in Washington, with Dr. C. Hart Merriam,[13] who thus aptly summarizes the character of his lifelong friend:

"An authority in many fields and yet one who never assumed authority; a leader in science and yet one who never assumed leadership; neither power nor glory did he seek, but the satisfaction of contributing his share to the sum of human knowledge."

Honors

Master of Arts, University of Rochester	1872
LL. D., University of Rochester	1898
LL. D., Wisconsin University	1904
LL. D., University of Pennsylvania	1908
Member National Academy of Sciences	1883
President American Society of Naturalists	1885
President Geological Society of America	1892 and 1909
President Philosophical Society of Washington	1892
President Cosmos Club	1894
President Geological Society of Washington	1895 and 1908
Wollaston Medal, London Geological Society	1899
President American Association for the Advancement of Science	1900
President National Geographic Society	1904
President Association of American Geographers	1908
Walker Grand Prize, Boston Society of Natural History	1908
Hubbard Medal, National Geographic Society	1909
Charles P. Daly Medal, American Geographic Society	1910

Foreign member, London Geological Society.
American Academy, Rome; Royal Society of London.
Honorary member, Geographic Society of Berlin.
Corresponding member, Bavarian Royal Academy of Sciences.
Corresponding member, Geographic Society of Leipsig.

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160. Barrois, Charles.
161. Black Hills.
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166. Cache Valley.
167. Carboniferous period.
168. Chamberlin, T. C.
169. Cienega.
170. Cincinnati group.
171. Coastal Plain.
172. Coast Range.

173. Continents.
174. Colorado Desert.
175. Cordillera.
176. Corniferous limestone.
177. Crater Lake.
178. Cretaceous period.
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198. Geology.
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202. Greensand.
203. Hague, Arnold.
204. Hall, James.
205. Hamilton group.
206. Hayden, F. V.
207. Homotaxis.
208. Huronian series.
209. Hydrography.
210. Iceberg.
211. Infusorial earth.
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213. Joints.
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219. Landslip.
220. Laurentian Mountains.
221. Laurentian system.
222. Le Conte, Joseph.
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247. Novaculite.
248. Old Red sandstone.
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250. Orton, Edward.
251. Owen, Richard.
252. Pacific Ocean.
253. Paleozoic era.
254. Permian series.
255. Phillips, J.
256. Physical geography.
257. Physiography.
258. Pikes Peak.
259. Plains, Great.
260. Pleistocene period.
261. Pliocene period.
262. Pothole.
263. Potsdam sandstone.
264. Proterozoic era.
265. Quaternary.
266. Quicksand.
267. Rainier, Mount.

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269. Sand.
270. Sandstone.
271. San Francisco Mountain.
272. Seismograph.
273. Sevier Lake.
274. Shale.
275. Shaler, N. S.
276. Sierra Nevada.
277. Silurian period.
278. Sink hole.
279. Surveys, geological.
280. Taconic system.

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281. Taylor, Mount.
282. Terrace.
283. Tertiary era.
284. Triassic period.
285. Valleys.
286. Vanuxem, Lardner.
287. Vesuvius.
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MEMORIAL OF CHARLES HENRY HITCHCOCK [1]

BY WARREN UPHAM

The geologist whom we honor and remember with gratitude in this memorial, the explorer who mapped and described the rock formations of Vermont, Maine, New Hampshire, and Hawaii, gave more than sixty

[1] Manuscript received by the Secretary of the Society December 29, 1919.
Presented in abstract before the Society by J. W. Goldthwait, December 29, 1919.

C H Hitchcock

years to observations, studies, and writings for the advancement of this branch of science. Like the descent of the mantle of the translated Hebrew prophet to his favorite disciple and successor, it may be truly said that the geologic devotion and rare discernment of the father, Edward Hitchcock, State Geologist of Massachusetts and Vermont, were inherited by the son and increased during his very long service in the States of northern New England and in the distant islands of the central Pacific Ocean.

Charles Henry Hitchcock was born in Amherst, Massachusetts, August 23, 1836, and died in Honolulu, Territory of Hawaii, November 5, 1919. The founders of the family in America were two brothers from England, the ancestor of this line being Luke Hitchcock, who came in 1695 and settled at Wethersfield, in Connecticut. From him the subject of this sketch was in the seventh generation, and he was equally distant from his maternal ancestor, John White, who similarly was an English immigrant settler of Canton, in Massachusetts. He was the sixth child of Edward Hitchcock, Professor of Chemistry and Natural History in Amherst College, 1825-45, later its president, from 1845 to 1854, and his wife, Orra White. This mother had such classical learning and scientific and artistic talent that "she could read the Greek Testament and calculate eclipses. . . . She prepared with her own hands many of the numerous illustrations in her husband's reports and also diagrams for the lecture-room. She took indefatigable pains with the education of her children, placing their moral and religious welfare first."

The father, in addition to the duties of his professorship at Amherst, conducted the geological survey of Massachusetts in 1830 to 1841, its final report being a massive quarto volume. From 1835 he was largely occupied during many years with collection and description of the fossil footmarks of the sandstone beds in the valley of the Connecticut River. He was greatly interested in the proper interpretation of the early chapters of Genesis and led the way to the general belief that geology is not at variance with the Bible.

In childhood and early youth, under home influences of these studies and discussions, the boy Charles acquired keen powers of observation, with eagerness to explain and theorize, which caused him to be called "the young philosopher." A biographic sketch published in 1898 noted his training in the home, school, and college:

"He seemed to be fonder of his father than the other children, and was never so happy as with him. Through this constant intercourse Charles became absorbed in his father's pursuits, and grew up into a knowledge of geology from nature and from verbal explanations, a more satisfactory method than that of learning from books; and he was associated with his father in all his geological work from the time when he was first old enough to be of service. Thus, before 1856 he was acquainted, from inspection, with the terraces and reputed beaches and drift phenomena of all western Massachusetts; he had handled every specimen of a footmark in the Appleton Cabinet, and by 1861 was the principal assistant on the Vermont Survey, having prepared for the press the greater part of the matter of the report. He had enjoyed the best educational advantages of his day, having completed the classical and preparatory courses of Williston Seminary and been graduated thence in 1852, then graduated from Amherst College in 1856, a short time before his twentieth birthday."

The life work of Edward Hitchcock was begun in the Christian ministry, as a Congregational pastor, before he heard and obeyed other calls to duty as a geologist and leader in education. With like inclinations his son Charles, next after the college course, took up theological studies during a year at Yale and two years in the Andover Seminary, looking forward to be a pastor; but the lure of geologic field-work, which he began in the summer of 1857, on the survey of Vermont, changed his plans for life to investigations and teaching in geology.

Charles was an assistant geologist of Vermont four years, under direction of his father, the State Geologist, to the close of the survey. For its report, published as two volumes and comprising nearly 1,000 pages, he had charge of the parts relating to the stratigraphic geology, the glacial and modified drift, the measurement and delineation of thirteen sections crossing the State from east to west, and the compilation of the geological map.

In 1861 he was appointed State Geologist of Maine, in which service he spent two summers in field-work and prepared two reports of progress, published in connection with the report of the secretary of the Board of Agriculture. This survey included a general reconnaissance of Maine, with discovery of large areas of Upper Silurian and Devonian formations.

For Amherst College he was curator of the museum and lecturer on zoölogy from 1858 to 1865, and for Lafayette College was non-resident professor of geology and mineralogy from 1866 to 1870.

During a year in 1866-7 he studied at the Royal School of Mines, London, made researches of the crustacea and trilobites in the British Museum, and visited Switzerland for examination of its glaciers.

In 1868 he received appointments as State Geologist of New Hampshire and Professor of Geology and Mineralogy in Dartmouth College. This State survey occupied ten years, with publication of three quarto volumes and an atlas. His active professorship at Dartmouth continued forty years, to 1908, and later was extended through his life by the title of professor emeritus.

On leave of absence from Dartmouth for parts of each year from 1870 to 1896, Professor Hitchcock was lecturer in geology for Mount Holyoke College. In 1880-81 he additionally taught geology and zoölogy at Williams College and in the Virginia College of Agriculture and the Mechanic Arts.

He received the degree of M. A. in course at Amherst in 1859, the honorary degree of Ph. D. from Lafayette College in 1870, and that of LL. D. from Amherst College in 1896.

Since 1856 he was a member and fellow of the American Association for Advancement of Science, during half a century a nearly constant attendent at its meetings and participant in its proceedings, and in 1883-4 was a vice-president, in charge of its section of geology. He was a member of local scientific societies in Portland, Maine; Boston, New York, Philadelphia, and St. Louis, and also of the Imperial Geological Institute of Vienna. He took a leading part in the organization of the Geological Society of America, and earlier prepared reports and maps for several meetings of the International Congress of Geologists.

Between 1860 and 1870 Professor Hitchcock engaged largely as a mining geologist in examination and estimation of the quantity and value of mineral deposits for mining companies, traveling in Nova Scotia, New Brunswick, Quebec, the New England States, New York, New Jersey, Pennsylvania, and southward to Alabama. Subsequently he examined the phosphate beds of South Carolina, Florida, and Redonda Island of the West Indies, the gold fields of eastern Oregon, the Chalcedony Park of Arizona, the Grand Canyon of the Colorado, and the Yosemite and Yellowstone parks. In 1883, 1886, and 1898 he visited the Hawaiian Islands for studies of their volcanoes.

The geological survey of New Hampshire may justly be regarded as his most prominent work. The formations are principally crystalline schists and igneous rocks, and the areas best studied in detail are the White Mountains and the Ammonoosuc mining district. Each of the sections previously surveyed across Vermont was continued east through this State, and the abundant rock specimens collected were placed on exhibition with sectional drafts of the stratigraphy in the Dartmouth College museum, a duplicate series being placed in the American Museum of Natural History, New York City.

Connected with this survey, a meteorological station was established on the summit of Mount Washington. J. H. Huntington, principal assistant in the field-work and description of Coos County, the most northern and largest in the State, had charge of the mountain observatory during its first winter, of 1870-71. Daily statements of the weather conditions on this highest peak of New England were sent by telegraph to the newspapers. The series of observations there was extended through many years by the United States Signal Service, being found very significant for weather predictions, which were soon afterward begun from comparisons of the telegraphic reports of many observers throughout the country.

Volume I of "The Geology of New Hampshire," published in 1874, treats of the physical geography, climatology, the fauna, flora, and scenery, with the history of the survey, and of explorations among the White Mountains. Ten chapters are by Professor Hitchcock, and nine by assistants of the survey or specialists in natural history. The second volume, on the stratigraphic geology, published in 1877, has eight chapters written mainly by Charles H. Hitchcock, and two chapters, with parts of two others, by J. H. Huntington. Volume III was issued in 1878, having three parts. Under the title for Part III, Surface Geology, the glacial drift is described by Professor Hitchcock, and the modified drift by the present writer, who was his assistant during the second half of this survey, 1874-78. Part IV, entitled Mineralogy and Lithology, is by George W. Hawes; and Part V, Economic Geology, by Professor Hitchcock.

Among early endeavors to portray the geology of the entire United States, a very noteworthy map was compiled in 1872 by Charles H. Hitchcock and W. P. Blake, for the Ninth Census, and for R. W. Raymond's report of mineral resources. On a much larger scale, of twenty-five miles to the inch, Professor Hitchcock in 1881 issued his United States geological map for class-room use in schools and colleges. For this map he consulted every work that had been printed on the geology of this country, and obtained the privilege of using much unpublished information collected by geologists in States and Territories where surveys had not been carried to completion.

In the progress of the New Hampshire survey, a relief map or model of that State was constructed under the direction of Professor Hitchcock, having a scale of one mile to the inch horizontally and a thousand feet to the inch vertically. Its length was thus about fifteen feet, and Mount Washington was shown with a height of slightly more than six inches. Copies of this relief model were placed in the museum of Dartmouth College, in the State House at Concord, and in the American Museum of Natural History, the last being colored geologically. County and township boundaries, villages, cities, railroads, rivers, and lakes were delineated, but not the areal geology.

Nearly twenty years later, between 1894 and 1898, Professor Hitchcock conducted further field-work, by which the surveyed geologic sections crossing New Hampshire were increased to eighteen, with collection of about 5,000 rock specimens. Improved drawings of the profiles, colored geologically, were prepared for the new Butterfield Museum of the college, and another large relief map of the State was made, on the same horizontal scale of a mile to an inch, but with a vertical scale of a half mile to an inch, the White Mountains being thus represented as about two inches high. This map has geological coloration like the sectional profiles.

In development of knowledge of the Ice Age and its drift formations, Professor Hitchcock made very important studies. On the Vermont Survey he mapped the terraces of valley drift bordering the Connecticut River, which were again the chief subject of a chapter by the present writer for the Survey of New Hampshire. In 1868 he gave a lecture in New York and Brooklyn, in which he asserted that the hilly and ridged drift deposits called the backbone of Long Island for its entire length are the terminal moraine of the continental ice-sheet. Ten years later I examined and described this moraine and its extension eastward on Marthas Vineyard and Nantucket. Its western course has been traced across New Jersey, Pennsylvania, Ohio, and other States, to Wisconsin, Minnesota, the Dakotas, and onward to the Rocky Mountains and the Pacific coast. Smoothly rounded and oval hills of the glacial drift, which in 1876 Professor Hitchcock described and named lenticular hills in New Hampshire, are since called drumlins, from their name in Ireland, where many such hills were earlier mapped. Long ridges of gravel and sand, deposited in ice-walled channels of streams during the final melting and departure of the ice-sheet, were described by G. F. Wright in the third volume of the New Hampshire Survey, being then called kames, as in Scotland, for which later their Irish name, eskers, has come into general use.

Glacially transported boulders were found by Professor Hitchcock in 1875 on the summit of Mount Washington, about 1,000 feet above the former recognized upper limit of the glacial drift, proving that for some relatively short time the great ice-sheet entirely enveloped this mountain. The name Champlain was given by him to fossiliferous marine clays in the Saint Lawrence Valley and adjoining Lake Champlain, which overlie the glacial drift and show that this part of our continent was somewhat depressed below its present height when the ice-sheet was melted away. Hence the closing stage of the Ice Age is named the Champlain stage or epoch.

Professor Hitchcock removed in 1908, when he had completed forty years in the service of New Hampshire and Dartmouth College, to Honolulu, the principal city of the tropical Hawaiian Islands, to which he had previously made several visits. Here his last eleven years were passed, excepting occasional returns in summers to the United States. His latest extended geologic work was published there in 1909, a very interesting book of 314 pages, with more than fifty plate illustrations, entitled "Hawaii and its Volcanoes." "The object of this work," as noted in its preface, "is to describe correctly the phenomena connected with the discharges of molten lava from the two great Hawaiian volcanoes, Kilauea and Mauna Loa. The greater part of the text presents the statements of visitors to their borders, descriptive of what they saw, set forth in chronological order."

In June, 1862, he married Martha Bliss Barrows, daughter of Prof. E. P. Barrows, of Andover, Massachusetts. To this union two sons and three daughters were born, of whom the first, Arthur Charles, died in infancy, and the second son, Edward White, died at the age of nearly six years. Maria Porter, the eldest daughter, married to Frederick Allen, has died. The first wife of Professor Hitchcock died in February, 1892, and he married her sister, Charlotte Malvina Barrows, in September, 1894, who resides, with the two surviving daughters, Martha Barrows and Alleine Lee, in Honolulu.

Honored, revered, beloved friend, with whom I have had intimate association through fifty years, first under your teaching at Dartmouth and later as your assistant in the survey of my native State, I have now to say, gratefully, Farewell! Your life, devoted to science, education, and Christian service of the State, the college, and the wide world, has been an inspiration, a beneficent guidance and example, to your hundreds of students and to all workers in geology.

The following list of Professor Hitchcock's publications was compiled by himself, up to the year 1907, for his successor, Prof. J. W. Goldthwait, and for the libraries of Dartmouth and Amherst Colleges:

Bibliography

1855. Extra-Terrestrial excursions. Amherst Collegiate Magazine, 4 pages.
Impressions (chiefly tracks) on clay. American Journal of Science, series 2, volume XIX, 6 pages.

1857. Fractured ledges of slate in Vermont. Proceedings of the American Association for the Advancement of Science, Montreal meeting, 5 pages.

1858. Section from Greenfield to Shelburne Falls, Massachusetts. Proceedings of the Boston Society of Natural History, 3 pages.

1859. Catalogue of rocks and minerals in the Massachusetts State Collection; a revision of the catalogue published by E. Hitchcock in the Final Report on the Geology of Massachusetts, and prepared under his direction. Published by the Secretary of the Board of Agriculture, 65 pages.

So-called talcose schist of Vermont. Proceedings of the American Association for the Advancement of Science, Springfield meeting, 8 pages.

Marks of ancient glaciers on the Green Mountain range in Vermont and Massachusetts. Idem, 7 pages.

Lake ramparts in Vermont. Idem, 3 pages.

1860. Distorted pebbles in conglomerates near Newport, Rhode Island. Proceedings of the Boston Society of Natural History, volume VII, page 208.

Geology of the Island of Aquidneck. Proceedings of the American Association for the Advancement of Science, Newport meeting, 25 pages. A geological map illustrating the paper was distributed at this meeting.

Synchronism of coal beds in New England and Western United States coal basins. Idem, 6 pages.

Review of Religion of Geology. Bibliotheca Sacra, volume XVII, 35 pages.

Elementary Geology, by Edward Hitchcock and C. H. Hitchcock. Ivison, Phinney & Co., New York, 430 pages; revised in 1868.

1861. Letter on Red sandstone of Vermont. American Journal of Science, series 2, volume XXXII.

Geological map of the United States, by Edward Hitchcock and C. H. Hitchcock. Published on a wall map of the United States by Robert Persall Smith, Philadelphia.

Report on the geology of Vermont, by Edward Hitchcock, Albert D. Hagar, Edward Hitchcock, Jr., and C. H. Hitchcock. Two volumes, 988 pages, Claremont, New Hampshire; having 473 pages prepared by C. H. Hitchcock.

1862. Preliminary report upon the natural history and geology of the State of Maine. 478 pages, Augusta, Maine; having 266 pages prepared by C. H. Hitchcock.

Notes on the geology of Maine. Proceedings of the Portland Society of Natural History, volume I, 14 pages, with a map.

Catalogue of the mammals and birds of Maine. Idem, 7 pages.

Fossils of the Potsdam group in North America. Idem, 4 pages.

1863. Description of Carpolithes Lescurii. Idem, 2 pages.

Second annual report of the natural history and geology of Maine. 447 pages, 226 being by C. H. Hitchcock.

Review of Antiquity of Man, by Sir Charles Lyell. North American Review, October, 32 pages.

Visitors' guide to the cabinets of Amherst College. 120 pages; reprinted in 1868 and 1875.

Reports to Saint Flavien Mining and Smelting Company, Canada. 5 pages.

Report to Saint Francis Mining and Smelting Company, Canada. 5 pages.

Report to Port Hood and Boston Coal Mining Company, Cape Breton. 7 pages.

Report to Cape Breton Coal Mining Company. 7 pages.

1864. Report upon Missisquoi copper mine, Canada. 7 pages.

Report to Hatley Hill Copper Mining Company, Canada. 4 pages.

Letter concerning coal lands in Elk County, Pennsylvania. 9 pages.

Report to Logan Mining and Smelting Company, Canada. 7 pages.

Report to Clark Mining and Smelting Company, Canada. 8 pages, with a map.

Antimony minerals in Canada. American Journal of Science, series 2, volume XXXVII, 1 page.

1865. Albert coal of New Brunswick. Idem, volume XXXIX, 7 pages.

Report to Peoples' Mutual Coal Company, Pennsylvania. 4 pages.

Report to Golconda Gold Mining Company, Canada. 13 pages, with a map.

Report to Nicolet Antimony Company, Canada. 4 pages.

Report to Homowack Oil and Mineral Company, New York. 2 pages.

Report to New York and Pennsylvania Petroleum, Coal and Iron Company. 6 pages.

Report to Amazon Oil and Mining Company, Kentucky. 12 pages.

Supplement to the Ichnology of Massachusetts, by Edward Hitchcock, published by the Legislature of Massachusetts. 96 pages, with 20 plates; having 60 pages, including the catalogue, prepared by C. H. Hitchcock.

The geological distribution of petroleum in North America. Press report of the Nottingham meeting of the British Association for the Advancement of Science, 8 pages.

1866. Petroleum in North America. Idem, abstract of proceedings, 3 pages; also reprinted in the Geological Magazine, 1867.

Description of Tarsodactylus expansus. Annals of New York Lyceum of Natural History, volume VIII, 2 pages.

1867. Relations of geology to theology. Bibliotheca Sacra, in two numbers, 77 pages.

Geology of Andover, Massachusetts. Proceedings of the Essex Institute of Natural History, volume V, 3 pages.

Geology of Vermont. Proceedings of the American Association for the Advancement of Science, Burlington meeting; abstract, 3 pages.

Winooski marble of Colchester, Vermont. Idem; abstract, 1 page.

Explanation of the geological map of Maine. Idem; abstract, 1 page.

Distortion and metamorphism of pebbles in conglomerates. Idem; abstract, 4 pages.

Report on property of the Green Mountain Mining Association. Written in 1865; 2 pages.

Review of Billings' Anticosti Catalogue. Geological Magazine, volume IV, 3 pages.

1868. Relations of Science to the Bible. A part of Bond's edition of Kitto's Illustrated History of the Bible.

Geological history of Long Island, read before the Lyceum of Natural History, New York, and the Long Island Historical Society, Brooklyn, in which it is claimed that the backbone of Long Island is part of the terminal moraine of the continental ice-sheet. This paper was preserved for many years in the custody of the Lyceum, to insure the priority of suggestion.

1869. Geology of Dixville Notch. Published in New Hampshire Patriot.

Letter to President Smith, Dartmouth College, concerning the collections made by the Geological Survey. In report of the College of Agriculture. 2 pages.

Note on supposed fossil footmarks in Kansas. American Journal of Science, series 2, volume XLVI, 2 pages.

First annual report upon the geology and mineralogy of New Hampshire. Manchester, New Hampshire, 36 pages, with a map.

1870. The Calaveras skull. Engineering and Mining Journal.

Second annual report upon the geology and mineralogy of New Hampshire, 37 pages, one map.

Geological map of New York. Compiled for Atlas of Asher and Adams.

Elevation of mountains. The Dartmouth, volume IV, 4 pages.

The geology of Vermont. Folio, 5 pages; only 25 copies circulated.

The distribution of maritime plants in North America a proof of oceanic submergence in the Champlain period. Proceedings of the American Association for the Advancement of Science, Troy meeting; abstract, 7 pages.

1871. The geology of Massachusetts. In Walling's Official Atlas, folio; 7 pages, with map.

Report of the geological survey of the State of New Hampshire for 1870. Nashua, 82 pages.

Mount Washington in Winter. Boston, Massachusetts, Chick and Andrews; 363 pages, including 110 pages prepared by C. H. Hitchcock.

Geological explorations in the Rocky Mountains. Geological Magazine, 1 page.

Review of White's Geology of Iowa. Idem, 5 pages.

Review of geological reports of Michigan and Ohio. Idem, 4 pages.

1872. Norian rocks in New Hampshire. American Journal of Science, series 3, volume III, 5 pages.

Recent geological discoveries among the White Mountains. Proceedings of the American Association for the Advancement of Science, Dubuque meeting, 17 pages.

Explanation of the new geological map of New Hampshire. Idem, 2 pages.

Report of the geological survey of the State of New Hampshire for the year 1871. Nashua, 56 pages, and map.

Report upon marble at Pittsford, Vermont. 13 pages.

Notice of the geology of New Hampshire. Geological Magazine, 3 pages.

Geological map of the United States, in Atlas of Asher and Adams.

Description of geological map of the United States. Report of Ninth Census of United States, 3 pages.

Sketch of the gorilla. In Johnston's Natural History, volume I, 2 pages, with a full-page illustration.

1873. Report of the geological survey of the State of New Hampshire for the year 1872. Nashua, 15 pages, one plate.

Classification of the rocks of New Hampshire. Proceedings of the Boston Society of Natural History, 8 pages.

Geological map of the United States. New edition in Mineral Resources of the United States west of the Rocky Mountains, Fifth Annual Report of R. W. Raymond, Commissioner of Mining Statistics.

Geological history of Winnipiseogee Lake. Proceedings of the American Association for the Advancement of Science, Portland meeting, 12 pages.

Geology of Portland, Maine. Idem, 13 pages.

Note upon the Cretaceous strata of Long Island. Idem, 2 pages.

Geology of the northwest part of Maine, by C. H. Hitchcock and J. H. Huntington. Idem, having 2 pages by C. H. Hitchcock.

Footprints in the rocks. Popular Science Monthly, August, 14 pages.

Relations of Geology to Agriculture. In report of the Board of Agriculture of New Hampshire, 6 pages, with a map.

1874. Helderberg rocks in New Hampshire. American Journal of Science, series 3, volume VII, 24 pages.

The geology of New Hampshire. Part I, forming volume I, by several authors. Concord, New Hampshire, 668 pages, 47 plates. Several chapters were issued separately.

The world before the introduction of life. Popular Science Monthly, March, 19 pages.

An examination paper: the Long Island boulder.

The Age of Ice. Popular Science Monthly, April, 16 pages.

Geological map of the United States, compiled by C. H. Hitchcock and W. P. Blake. In Walker's Physical Atlas, with 2 folio pages of text, by C. H. Hitchcock.

The coal measures of the United States. In Walker's Physical Atlas, 3 folio pages, with map.

The physical history of New Hampshire. Address before the Dartmouth Scientific Association on retiring from the presidential office; reprint from volume I, Geology of New Hampshire, 39 pages.

1875. The lithology of New Hampshire. Preliminary catalogue of 250 typical specimens; 20 pages.

Catalogue of mineral localities in New Hampshire, by G. W. Hawes. 15 pages; information furnished by C. H. Hitchcock.

Existence of glacial action upon the summit of Mount Washington, New Hampshire. Proceedings of the American Association for the Advancement of Science, Detroit meeting; 5 pages.

Cambrian and Cambro-Silurian rocks in the Champlain valley. Proceedings of the Boston Society of Natural History, volume XVIII, 3 pages.

1876. Lenticular hills of glacial drift. Idem, volume XIX, 5 pages.

Atlantic system of mountains. Appalachia, volume I; abstract, 3 pages.

Geology of the White Mountains. Idem, 7 pages.

The century of geology in the United States. In The Independent, New York, September 21 and 28; 2 folio pages.

The relations of the geology of New Hampshire to that of the adjacent territory. Chapter I of volume II, Geology of New Hampshire.

Sketch of United States geology, accompanying geological map in O. W. Grey's Atlas of the United States; Philadelphia, 2 folio pages.

1877. Glacial markings among the White Mountains. Appalachia, volume I, 4 pages.

Physical history of New Hampshire. In report of the State Board of Agriculture, 15 pages.

On the White Mountains. Bulletin of the Essex Institute, volume IX, 3 pages.

Geology of New Hampshire. Part II, forming volume II, by several authors. 693 pages, 40 plates.

Report of Gardner Mountain Mining Company. 2 pages.

Helderberg rocks in New Hampshire. American Journal of Science, series 3, volume XIII; one page.

Topography and geology of New Hampshire. In State Atlas, Comstock and Kline, New York; 12 folio pages.

The earlier forms of life. Popular Science Monthly, June; 16 pages.

1878. The geology of New Hampshire, Part III. 386 pages, 7 plates, and 64 figures in the text; with Warren Upham, joint author.

The geology of New Hampshire, Parts IV and V. 365 pages, 13 plates; with George W. Hawes, joint author.

Atlas of the geology of New Hampshire. Large folio, 17 sheets.

The geology of the Ammonoosuc gold field. Extracted from the Geology of New Hampshire; 121 pages, with 2 maps.

1879. Recently discovered cupreous veins at Bluehill, Maine. Read at the Saratoga meeting of the American Association for the Advancement of Science; printed in the Boston Advertiser, September.

Resumption of the Maine Geological Survey. Printed in The Maine Farmer.

The Loraine group. Science News, volume I; 2 pages.

The glacial period in eastern America. Geological Magazine, June; 3 pages.

Geology of New England. In Macfarland's American Geological Railway Guide, New York, D. Appleton and Co.; 11 pages. Reprinted in a later edition.

The Dodge farm in Lyman, New Hampshire. Report to W. C. Johnson; 10 pages, and map.

Report to the Atlantic Copper Mining Company, Bluehill, Maine. 2 pages.

1880. Report to New England and Oregon Mining Company. Concord, New Hampshire; 15 pages.

Report to Maine Tin Mining Company. 1 page.

Report on alluvial gold mine in Oregon. Typewritten.

Tin ore at Winslow, Maine. Engineering and Mining Journal, October 2.

Report on the Tin Mining Company, Winslow, Maine. 1 page.

Gold and silver mining in northern New England. Address before the Bullion Club in New York; printed in the Maine Mining Journal.

New glacial observations [upon Mount Washington]. Among the Clouds, August 7.

Circular announcing preparation of a geological atlas of the United States. 2 pages.

Geological report on the property of the Shelburne Mining Company, Coos County, New Hampshire. Portland, Maine; 9 pages, dated August 24, 1880.

1881. Text accompanying the geological map of the United States, published by Julius Bien, New York. 30 pages. The base is the 1879 edition of the Centennial Map of the United States Land Office.

North America in the Ice Age. Popular Science Monthly, December; 13 pages.

Report to the Commonwealth Mica Mining Company, New Hampshire. 2 pages.

1882. Report on the property of the Holston Salt and Plaster Company, Virginia. 16 pages.

Report on the gypsum lands of William Frazer, situated in Smyth County, Virginia. 13 pages.

Brief answers to questions concerning the salt and gypsum in Virginia.

Several short contributions to Appleton's and Johnson's Encyclopedias.

The glacial flood of the Connecticut River valley. American Association for the Advancement of Science; abstract, 4 pages.

The crystalline rocks of Virginia compared with those of New England. American Institute of Mining Engineers; abstract, 4 pages.

1883. The geological position of the Philadelphia gneisses. American Institute of Mining Engineers; abstract, 4 pages.

The early history of the North American continent. Address as vice-president of Section E, Geology, American Association for the Advancement of Science. 13 pages.

1884. Geological sections across New Hampshire and Vermont. Bulletin of the American Museum of Natural History, New York: 25 pages, 3 plates.

Description of geological sections crossing New Hampshire and Vermont. (Somewhat different from the preceding.) In report of the secretary, Board of Agriculture, New Hampshire; 31 pages, 3 plates.

Identification of the Green Mountain gneisses in eastern New England. American Association for the Advancement of Science; abstract, 1 page.

Physical features of New Hampshire, from Encyclopedia Britannica. 2 pages.

Forsythe gold and silver mine in Bath, New Hampshire.

Statistics and notes about the building stone industry, collected for the Tenth Census of the United States, volume X; chiefly for New Hampshire and Vermont.

1885. The crystalline rocks of Alabama. American Journal of Science, October; 6 pages.

Geology of Cheshire County, New Hampshire. Child's Gazetteer; 8 pages.

Slides in the White Mountains, New Hampshire. Science, July 31 and October 9; 3 pages.

Geology of Maine. Folio, 4 pages, with map, from the State Atlas.

The geology of northern New England, being a special edition of the sketches heretofore cited of Maine, Vermont, and New Hampshire. Folio, 20 pages, 3 maps.

1886. Geology of Grafton County, New Hampshire. Child's Gazetteer; 10 pages.

Letter from Honolulu, Hawaiian Islands. Science, July 23; 3 pages.

The geological map of the United States. American Institute of Mining Engineers; 24 pages, descriptive of a map prepared to illustrate schemes of coloration and nomenclature, recommended by the International Congress of Geologists.

1887. The late eruption from Kilauea. Science, February 25; 6 pages.

Genesis of the Hawaiian Islands. American Association for the Advancement of Science; abstract, 1 page.

Geological history of North America, in Appleton's Physical Geography.

Compilation of a treatise on botany, typewritten for classes in college.

1888. Report H., of the subcommittee on the Quaternary and Recent, for the International Congress of Geologists. 12 pages.

Letter to geologists, calling a meeting to organize an American Geological Society. American Geologist; 1 page.

Note to paper of A. Wanner upon footmarks at York, Pennsylvania. American Association for the Advancement of Science; abstract, 1 page.

Date of the publication of the report upon the geology of Vermont. Proceedings of the Boston Society of Natural History; 5 pages.

1889. Recent progress in ichnology. Proceedings of the Boston Society of Natural History, volume XXIV; 11 pages, read December 19, 1888.

Conglomerates in New England gneisses, a letter addressed to Alexander Winchell. American Geologist, April; 3 pages.

Field studies of hornblende schist. American Association for the Advancement of Science; abstract, 1 page.

Report upon coal and iron land near Morrison Station, Georgia. 3 pages.

The same, reprinted in prospectus of the New England company, Dade County, Georgia.

1890. Report to Horatio Adams upon kaolin near Columbia, South Carolina; 4 typewritten pages.

The use of the terms "Laurentian" and "Newark" in geological treatises. American Geologist, April; 6 pages.

Review of characteristics of volcanoes, Dana, in Sunday School Times.

Review of Greenland ice-fields and life in the North Atlantic, Wright and Upham, in Sunday School Times.

Review of Wright's Ice Age in North America. Bibliotheca Sacra, January; 22 pages.

The Redonda phosphate. Bulletin of the Geological Society of America; 3 pages.

1892. The Green Mountain anticlinal. Science, December; 2 pages.

Terminal moraines in New England. American Association for the Advancement of Science; abstract, 3 pages.

Studies of the Connecticut valley glaciers. Bulletin of the Geological Society of America; 2 pages.

Article on geology. International Cyclopedia, Dodd, Mead and Co.

1893. Report on the Fellows mica property, Orange, New Hampshire. 1 page.

Glaciation of the White Mountains, New Hampshire. Bulletin of the Geological Society of America; abstract, 3 pages.

Typewriter edition of mineralogy for students, revised from Hatch.

1894. Legend on geological map of the United States, prepared for the University of Texas.

Ancient eruptive rocks in the White Mountains. American Geologist; abstract.

High level gravels in New England. Bulletin of the Geological Society of America; abstract, 1 page.

1895. Divisions of the ice age in the United States and Canada. American Geologist; 6 pages.

Paleozoic terranes in the Connecticut valley. Bulletin of the Geological Society of America; abstract, 3 pages.

Biographical sketch of Edward Hitchcock. American Geologist; 16 pages, with portrait.

Biographical sketch of Edward Hitchcock. Popular Science Monthly, 8 pages, with portrait.

Champlain glacial epoch. Bulletin of the Geological Society of America; abstract, 2 pages.

Review of Geikie's ice age. Science, April 12.

The Connecticut sandstone group. Science, January 18.

1896. Note on the stratigraphy of certain homogeneous rocks. Bulletin of the Geological Society of America; abstract, 1 page.

Review of Greenland ice-fields, etcetera, Wright and Upham. Science, April 17.

Remarks upon the petrographic excursion, Rochester meeting. Bulletin of the Geological Society of America; 1 page.

The geology of New Hampshire. Journal of Geology, Chicago. 18 pages.

Geology of the White Mountains. From White Mountain Glimpses, by F. N. Kneeland; 24 pages.

Gotham's cave of fractured rocks in northern Vermont. Proceedings of the American Association for the Advancement of Science; abstract, a half page.

1897. Biographical sketch of W. W. Mather. American Geologist. 15 pages, with portrait.

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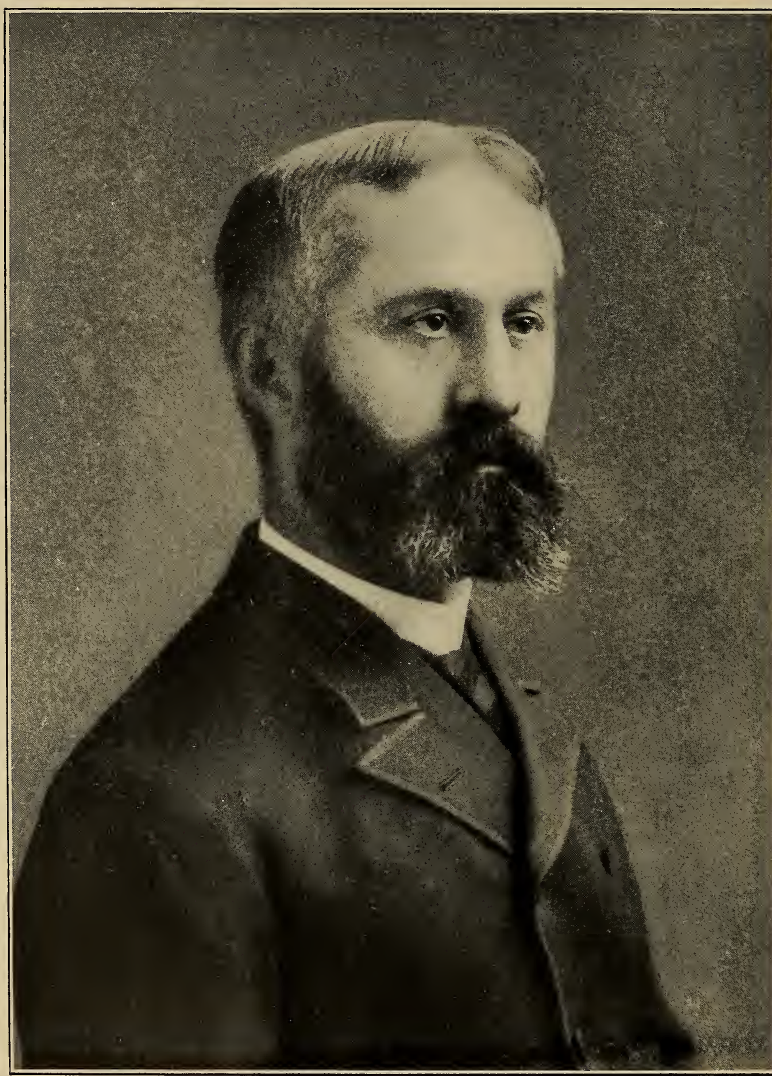

Alexis A. Julien

MEMORIAL OF ALEXIS ANASTAY JULIEN [1]

BY JAMES F. KEMP

In the loss of Alexis Anastay Julien May 4, 1919, the Fellowship of the Geological Society was diminished by one of the pioneer American petrographers, if indeed he was not actually the very first of American-taught users of the polarizing microscope. His report on the lithology of 259 rocks from the Huronian and Laurentian of the Upper Peninsula for volume II of the Michigan Geological Survey, published in 1873, is, so far as known to the writer, the earliest contribution in which this indispensable aid to investigation was employed in this country.[2] In the subsequent years of the decade of the seventies, those American students who followed microscopic petrography in the more advanced courses of our colleges were trained upon sets of slides and collections of rocks emanating from Dr. Julien's laboratory. In these latter days of the universal employment of thin sections we may hark back in respectful and appreciative memory of one who blazed a pioneer trail through regions then unexplored.

Dr. Julien was born in New York, February 13, 1840. His father, Pierre Joseph Denis Julien, had come to America from Lourmarin, Provence, France; while his mother, Magdalene Cantine, was a member of an old Huguenot family, long settled in Ulster County, New York. The parents educated their son at the Mount Washington Collegiate Institute in New York, and from it, in 1856, entered him as a sophomore in Union College, where he graduated in 1859, salutatorian of his class. He was elected to Phi Beta Kappa, and, having manifested the special interest in chemistry which he never lost during his later years, he was appointed assistant in the chemical laboratory the following year, under Prof. Charles F. Chandler.

In July, 1860, Alexis Julien became chemist of a company exploiting the guano deposits of the island of Sombrero, in the West Indies, and passed the next four years in its service. He had peculiar advantages for the study of the natural history of the lime phosphates and of the reactions produced by waters descending through bird guano to encounter limestones lower down. The year following his return to New York we

[1] Manuscript received by the Secretary of the Society.
Presented in abstract December 29, 1919.

[2] The same volume (volume II of the Michigan reports) contains, in Appendix C, a report by C. E. Wright on a collection of rocks, using the microscope, but the investigations were carried on at Freiberg, Saxony, with the aid of Professors Von Cotta and Kreischer.

find the results of his observations published in the American Journal of Science, as cited below in the first contribution of his bibliography. While on the Key of Sombrero he also studied the birds and shells and sent collections to the Smithsonian Institution. He likewise kept meteorological observations for the Smithsonian's records, maintaining thus the most southern of its stations. In 1862 he made a geological survey, for the Swedish Government, of the islets around the island of Saint Bartholomew, West Indies, and forwarded his report to the governor, Carl Ulrich. In 1863 he received from the King of Sweden the gold medal which is given by that monarch to those whose work deserves it—"Illis quorum meruere labores." He returned to New York in 1864 and prepared for publication the results of his observations in the West Indies. Union College gave him the degree of A. M. in the same year.

In 1865 he was appointed assistant in charge of the quantitative laboratory, in the recently established School of Mines of Columbia College, to which his old chief at Union, Dr. Charles F. Chandler, had been called. In the same year he became a member of the New York Academy of Sciences, then the Lyceum of Natural History, and was an active worker in it all the rest of his life. In the last five years of the sixties microscopical petrography, first developed by H. Clifton Sorby, in England, had its chief nurture and expansion under Ferdinand Zirkel, then in Vienna. By 1872 Alexis Julien had mastered its methods of work and undertook the studies for the Michigan Survey mentioned in the opening paragraph above. In 1875 he undertook a similar engagement for Prof. W. C. Kerr, State Geologist of North Carolina, and spent three successive summers in the field. A very detailed report resulted, which has been the subject of revision in recent years, so as to bring it within the means of publication of the North Carolina Survey, and is stated to be now in process of issue.

Although attached to the Department of Chemistry, Dr. Julien thus became more and more drawn away from chemical research by his interest in the microscopic study of rocks and in the investigation of geological phenomena. In 1880 he published one of his most important and most widely quoted papers, "On the geological action of the humus acids," in the Proceedings of the American Association for the Advancement of Science. The paper was of unusual value in bringing to the attention of observers the work of this little appreciated agent in the weathering of rocks.

In 1881 he reverted to his early studies of the guano deposits of the

West Indies, and visited the islands of Bonaire, Curaçoa, and Aruba, West Indies, making a study of the guano resources and of their general geology. At the commencement of 1882 the degree of Doctor of Philosophy was conferred upon him by New York University.

During this period Dr. Julien was also preparing an extremely important report for the Tenth United States Census, on "The durability of building stones in New York City and vicinity," which is published in volume X of its reports. This larger report was suggested by several shorter contributions, cited in the bibliography below, and in the end led to the investigation of the causes and cure of the alarming disintegration of New York's priceless relic of antiquity, the Egyptian obelisk, one of the features of Central Park. In these early years of the decade of the eighties, Dr. Julien became interested, along with his friend the late H. Carrington Bolton, in the curious musical note given out by certain beach-sands under the pressure of footsteps, a note which has attached to them the name "singing sands." A very extensive collection of sands from, one might almost say, all over the world was made for investigation and several contributions resulted on their microscopic characters. Soon afterward Dr. Julien took up the study of the two species of sulphide of iron, pyrite, and marcasite, especially with regard to their decomposition, and several papers were published which have been much quoted. While many geologists were more or less thoroughly convinced of the organic nature of *Eozoon canadense,* Dr. Julien's petrographic studies led him very early to the conclusion that it was a purely inorganic product of contact metamorphism; but his interest became excited in the development of serpentine, and continued active all the rest of his life. Years afterward he brought out the importance of brucite, deweylite and some minor transition products, and serpentine became the subject of his later labors. He had prepared a manuscript embracing the results of many years of reading and investigation, which was unfortunately destroyed in a fire which consumed his home, just a week before his death.

Having become transferred in 1898 from the Department of Chemistry in Columbia University to the Department of Geology, at the time the university removed to its present site, he entered into the discussions which were active in the meetings of the officers and students of the latter. Among these the recasting of the analyses of rocks was one. Prompted by his studies of the serpentines, Dr. Julien was led to apply the methods of recasting to the analyses of a variety of doubtful species of minerals and with extremely fruitful results, which are set forth in his paper on "The determination of mineral constitution through recasting of analyses" (1908).

In the above review the endeavor has been made to emphasize the subjects to which Dr. Julien made contributions of especial importance. Perusal, however, of his bibliography will show a number of other topics on which he wrote. His interest was especially keen in microscopic work and he was one of the original founders of the New York Microscopical Society, in 1880. He was one of the original members of the American Society of Naturalists at its establishment, in 1883. In 1878 he was elected honorary member of the Louisville Microscopical Society and in 1889 was made Fellow of the Royal Microscopical Society. He became Fellow of the Geological Society of America in May, 1889, in the second year of its organized activities.

Upon retirement from active university work, at the age of seventy, Dr. Julien vigorously continued his scientific work and was busy with his writings up to his final illness. The disastrous fire which destroyed his home, as mentioned above, played havoc with the results of years of work. It is tragic that they could not have come to issue while yet he was able to see the fruition.'

In 1882 Dr. Julien married Annie Walker Nevius, daughter of the late Peter J. Nevius, of New York City, and still living at their home of recent years in South Harwich, Massachusetts. To Mrs. Julien the writer is greatly indebted for incidents in the life of her husband, which have supplemented a personal friendship of nearly forty years.

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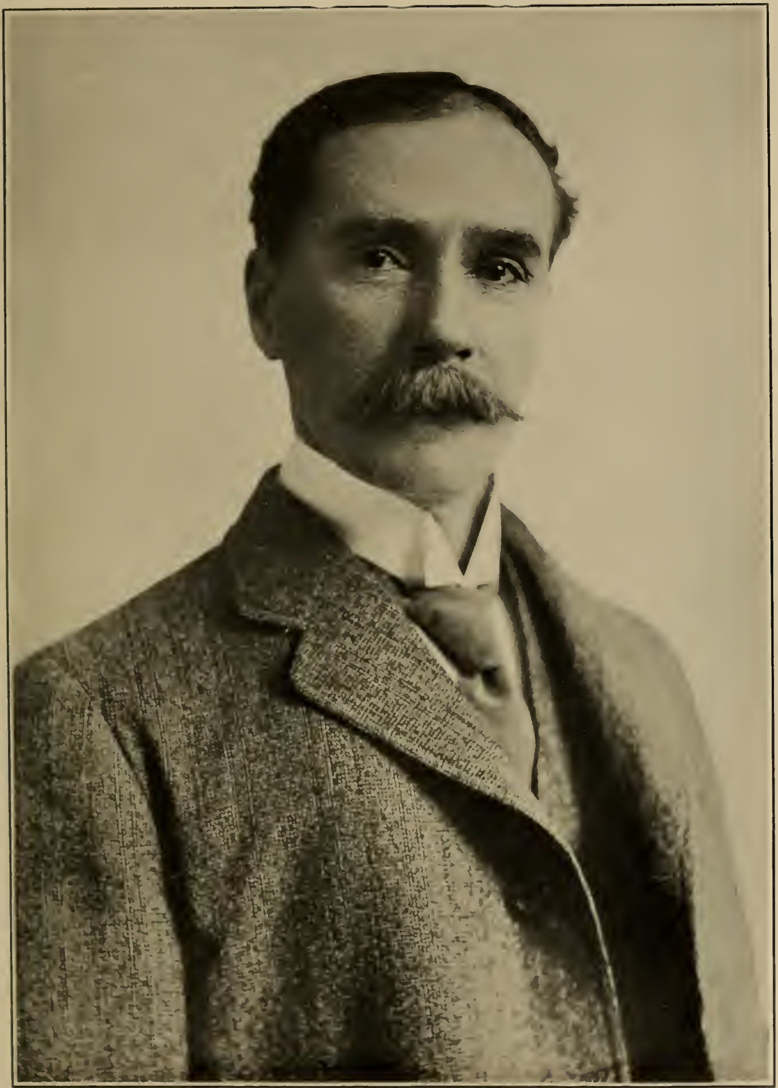
MEMORIAL OF LAWRENCE M. LAMBE [1]

BY E. M. KINDLE

The death of Lawrence M. Lambe occurred on March 12, 1919, at his home in Ottawa, after a brief illness which culminated in pneumonia. He was the Vertebrate Paleontologist of the Geological Survey of Canada.

Mr. Lambe, the son of William B. Lambe, advocate, of Montreal, and

[1] Manuscript received by the Secretary of the Society January 23, 1920. Presented in abstract before the Society December 29, 1919.



Lawrence M. Lambe.

Margaret Morris, daughter of the late Hon. William Morris, of Montreal, was born in Montreal, August 27, 1863. He was one of a family of five sisters and two brothers. William B. Lambe, the father of Lawrence, was an Englishman who came to Canada when a young man. His mother was of Scotch descent. As a boy he enjoyed the advantages which the abundant means of his parents was able to furnish.

The early education of Mr. Lambe was received at private schools. He graduated from the Royal Military College at Kingston, Ontario, in 1883. His training in a military school was no doubt in part responsible for the interest which for some years he took in military matters. At one time he held a commission as lieutenant in the Governor General's Foot Guards. Mr. Lambe was married in 1902 to Miss Mabel Maud Schreiber.

Lambe's college training was taken with a view to entering the profession of civil engineer. He secured shortly after his graduation from college a position with the engineers of the mountain division of the C. P. R. It is most probable that he would have remained a civil engineer but for the fact that an attack of typhoid fever compelled his return home. Although offered after his recovery another position on the engineering staff of the C. P. R., he preferred an appointment to the Canadian Geological Survey.

Much of Mr. Lambe's training in zoölogy and paleontology was acquired chiefly through his association with that keen naturalist and paleontologist, Dr. J. F. Whiteaves. This association began when Lambe, at the age of twenty-two, received his first appointment to the Canadian Geological Survey as artist and assistant to Dr. Whiteaves. At a considerably later period he studied with Dr. H. F. Osborn at Columbia University. Concerning this period of Mr. Lambe's career, Dr. Osborn writes as follows:[2]

"When I was appointed, in April, 1900, on the Geological Survey of Canada as paleontologist, to succeed Prof. Edward D. Cope, I chose Mr. Lawrence M. Lambe as my chief associate and I immediately engaged with him in the study of the fauna of the Belly River, which was published in 1902 (see Osborn Bibliography, 1902, page 217). He afterward came to Columbia University and took my full course in vertebrate paleontology."

In a letter to Dr. George M. Dawson, accepting the direction of the Canadian Survey's work in vertebrate paleontology, Dr. H. F. Osborn took pains to point out that Lambe was to have credit for all of the work which he did, and he planned for the vertebrate work to be taken over by

[2] Letter to E. M. Kindle, December 23, 1919.

Lambe as rapidly as his increasing knowledge and discrimination would permit. Concerning publication, Dr. Osborn expressed his wishes as follows:

"I desire also to make an arrangement regarding publication which will be entirely fair to Mr. Lambe, because the lion's share of the actual work must necessarily fall upon him both of collecting, arranging, and working up the fossils. For this, together with whatever writing he does, he should receive the fullest credit." [3]

In May, 1902, Dr. Osborn wrote the Director of the Canadian Geological Survey, intimating that he wished to relinquish the position of honorary vertebrate paleontologist and entrust the entire work to Mr. Lambe. The vertebrate paleontological work of the United States Geological Survey, which Dr. Osborn had also assumed at the request of Dr. Walcott, and the consequent heavy draft on his time, was probably the immediate occasion of his desire to entrust the continuation of the Canadian work to Lambe at that time.

The letters of Dr. Osborn to the Director of the Canadian Survey, written during Mr. Lambe's studies at Columbia, express a high opinion of Lambe's work. In December, 1903, Dr. Osborn recommended the appointment of Mr. Lambe as vertebrate paleontologist of the Canadian Survey, in a letter to the Minister then in charge of the Survey, which follows:

"I have had the pleasure during the last few years to succeed the late Prof. Edward D. Cope as (honorary) paleontologist of the Geological Survey of Canada. My duties on the United States Geological Survey and in this institution [American Museum of Natural History] have become so pressing that I have been obliged to resign the position. I have had associated with me on the Canadian Survey Mr. Lawrence M. Lambe, and I have formed a high opinion of his character and ability. I strongly recommend him to your consideration as vertebrate paleontologist.

"Great discoveries have been made in the Northwest Territory, and will be made in the future; and it appears to be of the utmost importance for the progress of science in general, the advancement of the Canadian Survey, and of the great museum which has been approved by your government, that vertebrate paleontology should form a special department of research and be cared for by an able specialist.

"I am convinced that Mr. Lambe will fill this position with great credit to the government, and that his appointment will meet scientific approval everywhere." [4]

Lambe's appointment as vertebrate paleontologist of the Canadian Geological Survey followed shortly after this recommendation was made.

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[3] Letter to George M. Dawson, May 1, 1900.

[4] Letter to the Hon. Clifford Sifton, M. P., December 11, 1903.

Lawrence Lambe's work in this capacity has fully justified the opinion expressed by Dr. Osborn in advising his appointment.

Analysis of Lambe's publications shows three stages of his development as a scientific worker. His first three papers dealt with living marine sponges. His contributions to zoölogy all relate to sponges and extend over a period of thirteen years, beginning in 1892. His first contribution to invertebrate paleontology appeared in 1896, four years after he had begun publishing on sponges. Two years later his first paper on vertebrate fossils was published. His papers published since 1900 relate, with few exceptions, to vertebrate paleontology, the subject with which his name in recent years has been chiefly associated. Lambe's most important work on invertebrate fossils relates to the corals. For a short period after the death of Dr. J. F. Whiteaves, the determination of all of the paleontological collections of the Canadian Geological Survey fell to Mr. Lambe—a task which few paleontologists could have ventured to undertake. After 1910, Lambe was able to devote his energies exclusively to vertebrate paleontology. He had, too, during the later part of his career, the good fortune to have the assistance of the Sternbergs, who collected for him a wealth of dinosaur and other material from the Alberta Cretaceous.

Lambe's interest centered in the office elaboration and description rather than in the collection of fossils. Himself an accomplished artist, he took the greatest care in supervising the execution of the drawings which illustrate the remarkable series of fossils which he has described during the last eight years. Among these were the first specimens of horned dinosaurs which had ever been found showing the character of the skin. The vertebrate fauna described by Lambe included many enormous heavy-boned reptilian creatures of most fantastic appearance. One of these, which bears the name of *Styracosaurus albertensis,* possessed a skull six feet in length. The top of the skull extended backward from the great hooked mandibles, expanded like a shield over the neck, where it was bordered by six powerful horns projecting from its posterior margin.

Among the important papers which he prepared in recent years were those describing the Triassic fishes of the Rocky Mountains. We are also indebted to him for important contributions to our knowledge of the Devonian fishes of New Brunswick. But it is with the wonderfully rich and varied vertebrate fauna of the Red Deer River valley of Alberta collected by the Sternbergs that Lambe was chiefly occupied in recent years. His various papers dealing with the Cretaceous faunas of the West show admirable illustrations of many of the bizarre creatures of the

Canadian Cretaceous. Several new genera were described from the Alberta material. Among these new genera are the following: *Styracosaurus, Centrosaurus, Chasmosaurus, Gryposaurus, Stephanosaurus, Cheneosaurus, Edmontosaurus, Enophocephalus, Gorgosaurus,* and *Stegoceras.* Some of his work on the Cretaceous fauna was still in manuscript form at the time of his death. The definition of a new subfamily of vertebrates was included in one of his unpublished manuscripts.

Mr. Lambe was a member of the Rideau Club at Ottawa. He was elected a Fellow of the Royal Society of Canada in 1901 and was a member of various other societies.

Lawrence Lambe belonged to that small group of men who find in their work their greatest pleasure. Paleontological work was to him indeed a labor of love. The little worries of life seemed never to penetrate his optimistic temperament. His friends will long remember the cheery smile and kindly word with which he always greeted them. Lambe accomplished much toward revealing Canada's early vertebrate life, and wherever such knowledge is cherished his passing will be deeply regretted.

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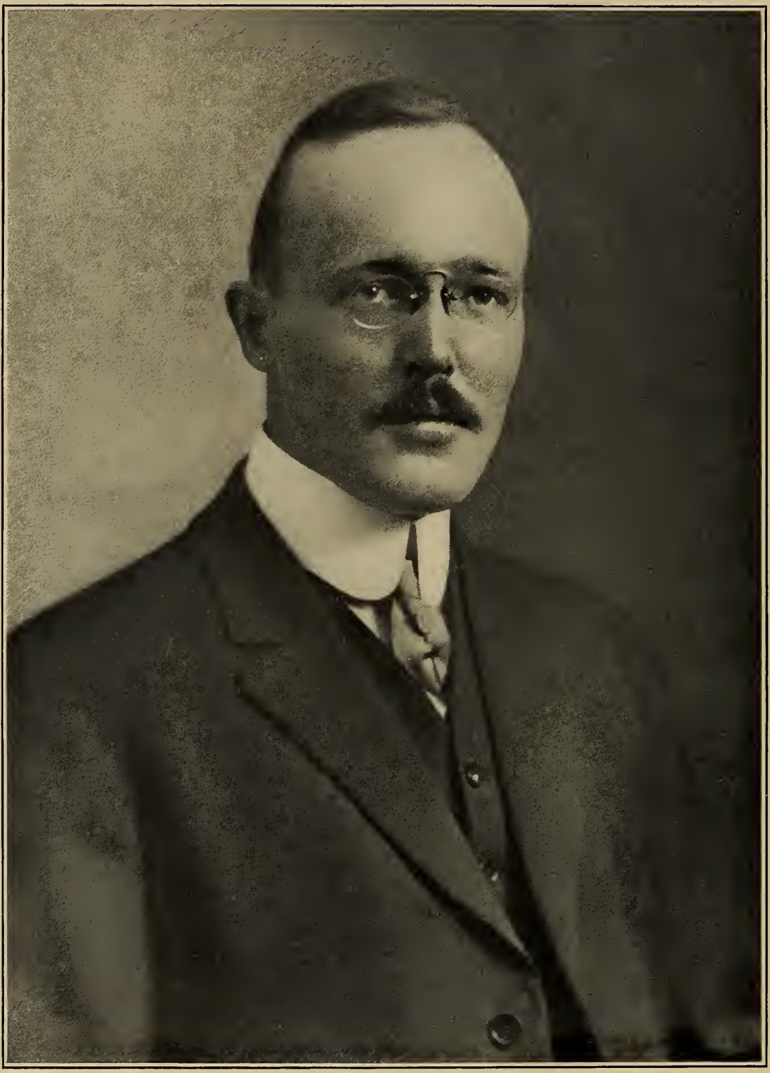

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MEMORIAL OF GAILLARD SHERBURNE ROGERS [1]

BY J. F. KEMP

A fortnight before the annual meeting of the Society news was received of the accidental drowning of Gaillard Sherburne Rogers, on No-

[1] Manuscript received by the Secretary of the Society December 29, 1919.
Presented in abstract before the Society December 29, 1919.

vember 18, while crossing the bar at the mouth of the Mulatas River, Colombia. Dr. Rogers had not quite completed his first full year as Fellow and his loss lessens the membership by one who had already accomplished much and who gave promise of increasingly important contributions to geology, as greater and greater maturity and experience were attained.

Dr. Rogers was born in New York City, March 21, 1889, and was thus in his thirty-first year. He was the son of the late Benjamin Tappan and Charlotte Kennedy Rogers, and was a lineal descendant of the Rev. Nathaniel Rogers, who settled in Ipswich, Massachusetts, in 1636, and of John Rogers, president of Harvard College in 1684. Sherburne Rogers, as he was known to his friends, was prepared for college in the Trinity School, New York City, and while yet a school-boy informed the writer of his desire and ambition to become a geologist. He was therefore personally advised and guided during his course in Columbia College (A. B., 1908), and as a graduate student later on in Columbia University (A. M., 1909; Ph. D., 1911). He was a high-rank man in college and was elected to Phi Beta Kappa. The same qualities secured for him the Sigma Xi key while a graduate student. He was assistant in the Department of Mineralogy during his graduate studies. In these years also he began his contributions to the literature of the science, as the bibliography below will show. His paper on the bedrock or partly inferential bedrock bottom of the Hudson River opposite New York adds in permanent form the conclusions of the geologist to the records obtained by the engineers. His dissertation for his doctorate was a careful study of the petrographical and chemical relationships of the complex Cortlandt series of intrusive rocks on the Hudson. The corundum deposits associated with them were shown to be due to inclusions of mica schist. On receiving his degree and passing with a high record the civil service examinations, Sherburne Rogers was appointed to the United States Geological Survey and remained in its service up to his untimely death. He worked on coal areas in eastern Montana, in his early field-work, but was not content with areal mapping. His active mind was pondering the source of the shales, and as a result we have the paper on the petrology of the associated sediments in the Journal of Geology in 1913. The paper forms one of the early contributions to the microscopic study and interpretation of the sediments, now a subject of such prominent interest. Later on Dr. Rogers was assigned to special problems in oil geology, such as the waters of the California oil fields, about whose composition and distribution he reached important conclusions; the relations of sulphur to the gravity of California oils; and the application of his conclusions on associated waters to the problem of the salt domes in the Gulf Coastal Plain. His experience also with gas wells in Ohio and in the mid-continent field during the open season of 1918 prepared him for writing the interesting paper on helium in the National Geographic Magazine and the bulletin now in press with the United States Geological Survey.

Early last autumn he obtained leave of absence from the Survey in order to undertake exploration work for oil in the Mulatas River region of Colombia. He had successfully completed his engagement and was on his way out in a native boat when, on crossing the bar, the accident happened which, to our inexpressible sorrow, ended a career of unusual promise.

Sherburne Rogers was a man of broad training and view. The careful selection of his college courses gave him this characteristic all through his life. Of accurate scholarship and a patient and faithful observer, he brought to bear on his problems reasoning ability of a high order.

On March 20, 1914, he was married to Marie Willoughby, of Washington, D. C.

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MEMORIAL OF CHARLES RICHARD VAN HISE [1]

BY C. K. LEITH

The keynote of Van Hise's scientific work, as I see it, was an implicit faith and belief in the existence of an ordered universe, governed by

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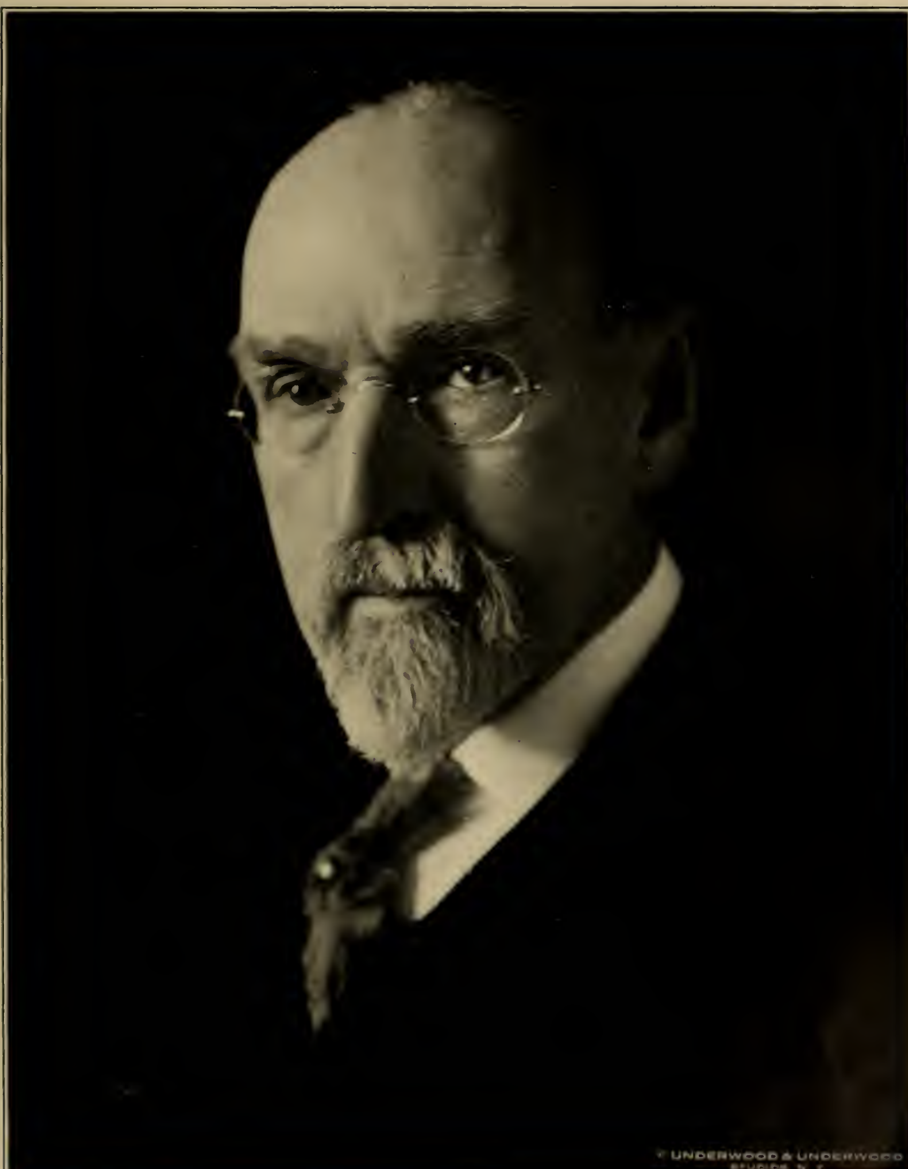

Charles R Van Hise

definite and ascertainable laws. His thought and effort were directed toward the discernment of these laws, and details of observation were of interest to him only as a means to this end. While he appreciated the necessity for careful descriptive work and for the assembling of miscellaneous information, his own main interest frankly did not lie in this direction. Yet he was indefatigable in searching for details necessary to establish basic principles and he was able to use and hold an astonishing mass of them when they fitted into the general story. We find this manner of approach to scientific problems illustrated in each of his principal lines of investigation.

Many geologists had worked in the Lake Superior iron and copper region and had brought together a large number of detailed observations. After having absorbed and added to these by years of painstaking field study, Van Hise proceeded to build them into the first definite and connected story of the region and to present this story simply and broadly with its background of general principles. Even the layman was then able to see some order in apparent chaos. The principles worked out by Van Hise have become the basis for the work of other investigators in this field. There is to this day scarcely a bit of geologic study, drilling, underground development, or other exploration in the Lake Superior region that does not take these general principles into account. Not only has the telling of the general story had a profound effect on the local investigation and mining of Lake Superior, but as a result of it the Lake Superior region has become almost a classical area for pre-Cambrian study the world over, it being one of the few places where light has been thrown on the history of the pre-Cambrian, or oldest rocks. The Lake Superior region being really the southern margin of the great pre-Cambrian area of Canada, its geologic history and principles have a marked influence on the investigation of the vast areas of the far north.

The vistas through ancient geologic history opened in the Lake Superior region led naturally to the consideration of other pre-Cambrian areas of North America. After wide travel and observation, Van Hise published a general account of the pre-Cambrian geology of North America, which for the first time brought together simply and broadly the general features of pre-Cambrian history and established lines of correlation and comparison. Some of these generalizations have proved too sweeping, others seem well established, but in any case they have stimulated efforts to get at the fundamentals of pre-Cambrian history. I would mention especially the opening of the field of pre-Cambrian study by Van Hise's view that pre-Cambrian rocks and history, from earliest known vestiges, indicate the same behavior and conditions of development as in later times, that Hutton's conception of uniformitarianism applies to the pre-Cambrian, and that there is no evidence of an "original" earth shell.

In the structural aspects of geology, Van Hise was not content with the mere observation and platting of dips, strikes, faults, joints, and other common structures, but he saw in them expressions of great earth movements, governed by mechanical laws, caused by stresses originating in the basic conditions of the origin and development of the globe. This phase of Van Hise's work attracted some attention, especially as presented in his "Principles of pre-Cambrian Geology," but the geologic profession as a whole has not yet learned to employ the application of mechanical principles to structural geology in any large and effective way. Further investigation and application of these principles have naturally resulted in some modification, but it was Van Hise that furnished much of the impetus to this quantitative method of attack.

In the investigation of old rocks it is necessary to deal with rock alterations of all sorts—physical, mineralogical, and chemical. Descriptive details had become so voluminous that the subject was regarded as one of the most difficult of geologic problems. Here again Van Hise saw in these multifarious alterations the action of certain definite physical and chemical laws, and when expressed in terms of these general laws the story became much more simple, definite, and understandable. His sound early training in physics and chemistry here finds its best application. It is interesting to recall that he began his work in the field of metamorphism before there was general recognition of simple processes like induration of sediments and of the fact that they could be turned into schists. Van Hise characterized his great monograph on metamorphism as "an attempt to reduce the phenomena of metamorphism to order under the principles of physics and chemistry, or, more simply, under the laws of energy." While the book contains an astounding mass of detail, throughout Van Hise is consistent in his attempt to develop the general laws. The success of this effort, directly and indirectly, can not be questioned, even though later results have required the modification of some of Van Hise's conceptions. He was one of the most active in urging the kind of attack on the problem so successfully inaugurated by the Geophysical Laboratory and others, and he eagerly welcomed every contribution from these sources, whether it confirmed or disproved some of his earlier generalizations.

Ore deposits in themselves did not particularly interest Van Hise, so far as their study was confined to descriptive detail or economic considerations. When, however, he began to see in an ore body the evidence of some general principle of ore deposition, he took up eagerly and comprehensively the study of ore deposits and developed certain principles of ore deposition, through the action of ordinary ground waters, which became the basis of wide discussion and much observation. Geologists are by no means agreed on this difficult subject; but, whether agreed or not, one can not go very far in the study of ore deposits without having to consider the principles presented by Van Hise. It is unfortunate that so many geologists have assumed that Van Hise ascribed the origin of *all* ore bodies to the kind of processes he emphasized. This was distinctly not his view. His purpose was rather, in view of the strong tendency to connect all ore deposition with vulcanism, to show the application of another great set of factors, which were unquestionably important in the deposition of many important groups of ore bodies and which were often ignored by geologists working principally on ore deposits undeniably related to vulcanism.

The eager search for fundamental laws in the physical field was paralleled in later life by a similar method of approach to the broad subject of mineral and other natural resources in their human relationships, as expressed in his "Conservation" and in his "Concentration and control." It is unfortunate that he did not live to complete his work on "Mineral resources and the history of civilization," which is an attempt to formulate certain principles which have controlled the interrelation of civilization with natural resources. He presented a paper on this subject in 1909 to the International Geological Congress in Sweden, and after that time he wrote and lectured on the subject at odd times, with the result that a volume was approaching completion at the time of his death. The conception is a great one and Van Hise had approached the subject with his usual incisiveness. I hope that it may be possible within a reasonable time to find means to complete this work, not only as a memorial to Van Hise, but as a scientific contribution of great significance in these times of world change.

In each of the fields I have mentioned, Van Hise was in a sense a pathfinder, advancing the frontiers of geologic knowledge with unsurpassed energy, industry, and enthusiasm. His far-reaching contributions during a comparatively short period of years were vital and significant. He was one of the pioneer geologists, like Gilbert, Chamberlin, and Powell, who set broad lines of geologic investigations for years to come.

Van Hise's outstanding contribution to the future development of the science of geology is perhaps the emphasis he laid on the quantitative application of physical and chemical laws. While he obtained large results by the application of these laws, even more important was the impetus he gave to this particular method of attack. Like other sciences, geology must grow out of the purely descriptive and qualitative stage into a more exact science. Van Hise saw this at a time when few American geologists were interested in such development.

Van Hise's method of scientific attack explains some of his characteristics as teacher and administrator. His constant effort was for positive and simple results, thought out long in advance and based on the broadest principles. Daily routine and detail interested him only in this relation. I have sometimes thought that critics of his administration in certain stages of University of Wisconsin history were paying too much attention to current incidents and were not, as Van Hise was, looking forward to an ultimate outcome. His purpose was simple and definite—to make the University of Wisconsin (a State institution) of broader and more fundamental service to the State. During the early part of his presidency his attempts to extend the scope of the university and bring it closer to the State were opposed in many quarters and there were political storms in which he was bitterly attacked. Long before the end of his administration, however, these vicissitudes had been passed and both the president and the university had won a position of influence and respect with all factions in the State. The university became a mecca for educational specialists from all parts of the world interested in the particular kind of university development which Van Hise so steadfastly advanced.

In the class-room he sought for results by inspiring the student, not by disciplinary methods. The success of this plan is testified to by the number of professionally successful geologists who date their choice of profession to moments in the class-room when Van Hise touched their imaginations.

The effort to get at fundamentals, so well shown by Van Hise's scientific record, was closely related to certain other qualities and methods of work which I will indicate very briefly.

There was almost a complete lack of petty jealousy or animosity in his make-up. He never felt that any field of investigation was overcrowded; the more in it, the better. For him, no one had ever "skimmed the cream" from any scientific opportunity. His thought was always that all the knowledge available was to be regarded as the starting point for further effort toward ascertaining fundamental laws. He often said, "The man has not yet lived who can adequately describe a grain of sand," and he was apt to be impatient with anybody who complained of lack of opportunity for investigation. In his administrative work, all factors were regarded objectively as a means toward an end, and it apparently never entered his mind that differences of opinion, even when they amounted to petty criticism, were to be taken personally. As he often expressed it, it was "all a part of the game," and any man who allowed personal considerations to influence his judgment or to affect his temper failed in playing the game.

Van Hise believed strongly in the use of working hypotheses as a means of assembling and evaluating facts of observation and arriving at final conclusions or the formulation of principles. He often cited the fact that a rock specimen would make the same impression on the retina of a child as on that of a trained geologist, and that it was necessary in surveying a complex set of phenomena to bring to bear on it all of the scientific principles and experiences available. In a bit of complex field-work, his method was, after a preliminary size-up of the facts, to formulate some hypothesis. When adverse facts appeared, he instantly and cheerfully abandoned his hypothesis—there was no pride of ownership—but almost as quickly he formulated a new one to cover the new facts. He applied to an unusual extent the principle of "multiple working hypotheses" so ably used and advocated by Professor Chamberlin. For this reason a long piece of investigative work under his guidance never degenerated into a routine piece of observation. For those of us who worked with him, it was necessary to be constantly alert to every conceivable aspect of the situation, in order that we might be able to bring the facts to bear for or against the working hypotheses likely to be "sprung" on us.

Van Hise's method of work left little room for accident or chance. Believing firmly, as he did, that all matters are controlled by ascertainable laws, he was inclined to ascribe failure in any reasonable task, no matter from what cause, to the lack of a sufficiently wide and deep consideration of the problem. Even weather and health were considered as factors, to be thought out as clearly as camp or laboratory equipment. This does not mean that he was lacking in human sympathy in distress—all who knew him will testify to his warm heart—but it did mean that within limits he was unsparing of himself and associates in the matter of getting results. It was far easier to make extraordinary efforts to accomplish these results in some way than to go back to Van Hise with any story, however plausible, in which the end had been defeated by causes which could possibly have been foreseen by the best use of intellect.

Finally, I would refer to one of Van Hise's qualities less generally known, but an essential part of his character and philosophy. He had not only a passionate desire to ascertain truth, but the keenest appreciation and love of its fine expression, whether in the form of prose, poetry, painting, music, or sculpture. Seldom did he take any extensive trip without carrying with him some well selected volume of verse, usually verse interpreting nature. Tennyson's "In Memoriam," with its wonderful expression of geologic principles, was a favorite, and those of us fortunate enough to work with him are not likely to forget the evenings spent about the camp-fire listening to those beautiful words read over and over again by Van Hise in tones of deep inspiration.

In his implicit belief and faith in an ordered universe under supreme control, his passionate desire to understand its make-up, and his love of adequate and beautiful expression of its essence, Van Hise was one of the most deeply religious men I have known.

Chronology

Born at Fulton, Wisconsin, May 29, 1857; died at Milwaukee, Wisconsin, November 19, 1918.

B. M. E., University of Wisconsin, 1879, B. S. 1880, M. S. 1882, Ph. D. 1892; LL. D., University of Chicago, 1903; Yale, 1904; Harvard, 1908; Williams, 1908; Dartmouth, 1909.

Instructor in metallurgy, University of Wisconsin, 1879-1883; assistant professor, 1883-1886; professor, 1886-1888; professor of mineralogy, 1888-1890; professor of Archean and applied geology, 1890-1892; professor of geology, 1892-1903; president, 1903-1918.

Non-resident professor of structural geology, University of Chicago, 1892-1903.

Assistant geologist, United States Geological Survey, 1883-1888; geologist in charge of Lake Superior Division, 1888-1900; geologist in charge of Division of pre-Cambrian and Metamorphic Geology, 1900-1908; consulting geologist, 1909-1918.

Consulting geologist, Wisconsin Geological and Natural History Survey, 1897-1903; president Board of Commissioners, 1903-1918.

Chairman of Wisconsin State Board of Forestry, 1905-1918.

Chairman Wisconsin Conservation Commission, 1908-1918.

Member of National Conservation Commission, 1908-1918.

Trustee of Carnegie Foundation for the Advancement of Teaching, 1909-1918.

Member of National Academy of Sciences, Washington Academy of Sciences, Scientific Society of Christiania, Royal Swedish Academy of Science, Geological Society of America (President in 1907), Wisconsin Academy of Sciences, Arts, and Letters (President, 1893-1896), American Association for the Advancement of Science (vice-president of Section E, 1901; President, 1916-1918), American Institute of Mining Engineers, National Geographic Society, Philosophical Society, New York Academy of Sciences, Boston Society of Natural History, and others.

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TITLES AND ABSTRACTS OF PAPERS PRESENTED BEFORE THE MORNING SESSION AND DISCUSSIONS THEREON

The scientific program of the meeting was begun with a paper entitled

FRAMEWORK OF THE EARTH

BY WILLIAM MORRIS DAVIS

(Abstract)

Opportunity for international correlation of geological investigation is offered in connection with the further study of the "framework of the earth," as already well developed in "Das Antlitz der Erde," by Suess, and in its French translation, "La Face de la Terre," by de Margerie. As a means of promoting such investigation, a summary of the points to which attention is most needed would be helpful, and bibliographies of recent studies on each element of the framework, published at intervals of a few years, would be an additional aid. The suggestion was made that our Society invite de Margerie to prepare such bibliographies for us to publish; and also that we urge de Margerie, as the best qualified man, to prepare a critical summary of the points in "La Face de la Terre" which, in his judgment, need revision and extension. Such a summary would be an invaluable guide to future investigators.

The paper was presented without notes, with maps and diagrams. It was discussed by Prof. Charles Schuchert.

DISCOIDAL STRUCTURE OF THE LITHOSPHERE [1]

BY BAILEY WILLIS

Presented by title in the absence of the author.

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[1] Presented before the Society on December 27, 1918, under the title "Structure of the Pacific Ranges, California." Abstract published in Bull. Geol. Soc. Am., vol. 30, pp. 84-86.

CONSTITUTION OF THE EARTH'S INTERIOR

BY L. H. ADAMS [1]

(*Abstract*)

Recent measurements of the true compressibility of rocks, combined with information gained from a study of the propagation of earthquake waves through the earth, lead to certain conclusions concerning the nature of the earth's interior, especially in regard to the question of the continuity or discontinuity of the earth at great depths.

Presented without notes, with lantern-slide illustrations.

EARTH'S RADIUS OF MOLAR REPOSE

BY CHARLES R. KEYES

(*Abstract*)

In a rotating spheroid of moderate rigidity the geometrical radius and the radius of no strain are not coincident. One is a straight line; the other a section of a parabolic curve the focal coefficient of which varies with the rate of revolution. In the case of the earth, the behavior of the zone of rock-flow is that of a homogeneous body under hydrostatic pressure. In the zone of rock fracture the characteristics are those of a heterogeneous mass. Cumulative stress is relieved through flexure, rupture, and shear. It gives rise to all those tectonic structures which are commonly accounted for upon the hypothesis of a shrinking nucleus. Tangential compression and mountain genesis thus appear to be directly initiated without reference to cooling globe or hydrostatic compensation in the earth's interior.

Presented by title in the absence of the author.

OSCILLATIONS OF LEVEL IN THE BELTS PERIPHERAL TO THE PLEISTOCENE ICE-CAPS

BY REGINALD A. DALY

(*Abstract*)

The record of wave erosion in Maine during the late Wisconsin submergence is relatively weak. One of the suggested reasons is the low power of the waves then beating on the shores. The small size of the waves is in turn explained by assuming uplift of the continental shelf during Wisconsin time, the land thus formed preventing access of waves from the open Atlantic. Subsidence of the shelf during the later isostatic rise of the glaciated region exposed the Maine coast once more to the full force of the Atlantic.

This hypothesis is supported by Fernald's botanical discoveries in eastern North America and by the results of Barrell's studies on isostasy. It needs, however, to be tested by the facts observed elsewhere in the belt marginal to the ice-cap of the Wisconsin stage, in the corresponding belts around the pre-

[1] Introduced by H. E. Merwin.

Wisconsin ice-caps, and around the ice-caps of other continents. Questions arise as to the possibility of evidence from the warped strands of the lakes ancestral to the existing Great Lakes, the submarine channel of the Hudson River, the "deeps" of the Susquehanna River, the abandoned channels of the Ohio Valley, and the belt marginal to the Scandinavian ice-cap. These questions are raised as means of advertising a problem which clearly needs, for definite solution, cooperative attack by many specialists.

Presented without notes.

RECENT WORLD-WIDE SINKING OF OCEAN LEVEL

BY REGINALD A. DALY

(*Abstract*)

In the Gulf of Saint Lawrence, along the New England coast, in Florida, and in Samoa field observations have shown that the sealevel has recently fallen about 20 feet. In each region the shift of level has been practically uniform. This regional uniformity and the accordance of the shifts registered in four mutually distant areas have prompted explanation by a general fall of ocean level to the extent of about 20 feet. Evidence from the British Isles, the Atlantic seaboard south of New England, the West Indies, South America, New Zealand, Australia, and the Pacific islands seems to strengthen the explanation offered for the facts personally observed in the field.

Presented without manuscript.

Discussion

Dr. E. O. Hovey cited observations on a 15-20-foot raised beach noted at many places on the Greenland coast from Cape York to Etah.

SUBMERGENCE AND POST-GLACIAL UPLIFT IN NEW HAMPSHIRE

BY JAMES WALTER GOLDTHWAIT

(*Abstract*)

Recent field-work in the southeastern part of New Hampshire affords distinct evidences of the late glacial, or "Champlain," marine submergence and reelevation of that district. Delicate yet unmistakable shoreline features, such as curved bars built between drumlin islands and short-hooked spits attached to the rear ends of these islands, occur not only in Stratham and Dover, where they are nicely shown by contours on the new Dover quadrangle, but also in North Hampton, Kensington, and Seabrook, where earlier and less accurate topographic sheets offer no suggestion of their presence. Outwash aprons associated with water-laid moraines in the Dover region and several cliffed drumlins agree with the better evidence of the raised beaches in fixing the amount of the postglacial uplift here at about 150 feet.

From the coastal district inland the old marine and estuarine deposits give way to disconnected valley trains and local wash plains whose levels appear

to lie above the marine plane and show discordances such as would be expected in drift-blocked river valleys. Profiles of some of the State highways bring out this feature.

Facts collected in the Merrimac and Connecticut valleys are considered in relation to the foregoing data, together with data secured by the writer in the provinces of Quebec, New Brunswick, Nova Scotia, and elsewhere, between 1906 and 1916, and an attempt is made to determine by means of isobases the probable amount of uplift of the interior of New Hampshire since the Glacial period.

Presented without manuscript, with the aid of a map.

Discussion

Prof. C. W. Brown: A great contrast exists between the number, magnitude, and size of pebbles or boulders of modern beaches and those of the two well marked stratified clay sea-deposits at 91 feet on Mount Desert Island, Maine, and the well defined cliff and fine gravel offshore beach at 210 feet elevation. These great differences indicate either less continuous wave action or more offshore protection at the time of the formation of the high level beaches.

Remarks were also made by Prof. R. A. Daly.

The Society adjourned for luncheon about 12.30 o'clock and reconvened at 2.10 p. m., with President Merriam in the chair.

TITLES AND ABSTRACTS OF PAPERS PRESENTED BEFORE THE AFTERNOON SESSION AND DISCUSSIONS THEREON

FEATURES OF THE SHIFTS OF LAND AND SEA-LEVEL IN THE ATLANTIC AND GULF COASTAL PLAIN DURING PLEISTOCENE AND POST-PLEISTOCENE TIME

BY THOMAS WAYLAND VAUGHAN

(*Abstract*)

This paper described some of the features of the fluvial and coastal marine terraces of the Coastal Plain, with particular reference to earth-movement and eustatic shift of sealevel.

Read from manuscript.

Discussion

Dr. W. T. Lee: In eastern Virginia there is one conspicuous wave-formed terrace lying at the foot of a sea-facing scarp. This terrace is approximately 20 feet above sealevel and is composed of horizontally bedded sand and clay. Beds of oyster shells in these strata indicate that sealevel was once about 20 feet higher than now, with reference to the land. In the same region peat beds indicating cypress swamps are penetrated by wells at least as low as 60 feet below sealevel. A tempting explanation is oscillation of sealevel.

Remarks were also made by Prof. R. A. Daly.

FIELD EXPERIMENT IN ISOSTASY

BY C. W. BROWN

(*Abstract*)

A common experience in engineering when a railroad fill is placed upon swamp or mud, is a resulting isostatic readjustment of material about fill, which may correspond in part to some of the phenomena attending surficial change of load on the earth.

Presented without notes, with lantern-slide illustrations.

Discussion

Prof. W. M. Davis pointed out that the railroad embankment and the adjoining uplifted tidal flats described by Professor Brown were not altogether homologous with isostatic highlands and lowlands of the earth. The isostasy of the earth's crust involves the following changes of conditions: Given a lowland which is in isostatic adjustment—that is, its mass to a depth of about 70 miles is such that any column of it is of a standard value. Now, if that lowland suffers deformation, so that it becomes a mountainous highland averaging two miles in elevation, any column of it will then be 72 miles high above the level of compensation; yet this column will have no greater mass—that is, it will weigh no more—than did the 70-mile column before uplift. Still further, the highland will in time be worn down to a lowland by the removal of its extra height of two miles. Nevertheless, after this removal the remaining 70-mile column will still have the same standard mass or weight as the original 70-mile column and as the mountainous 72-mile column. The best explanation of this anomalous condition is due to the late George F. Becker, who suggested that in the change of the initial lowland to the mountainous highland the gain of height is largely due to lateral crushing, whereby the whole mass is so shattered that the empty spaces of its shattered structure will be about 3 per cent of its volume, as a result of which its height of 70 miles will be increased to a height of 72 miles, but its mass will be the same as before. Then, during the erosion of the extra two miles at the top of the column, the empty spaces beneath will be filled up by new material, presumably from deep within the earth; and, thus resolidified, the original weight of the column will be restored.

POST-GLACIAL RIVER CHANGES IN RHODE ISLAND AND CONTINENTAI TILT

BY C. W. BROWN

(*Abstract*)

Decided diversions of river courses at river mouths in unconsolidated material in similar directions indicate continental uplift with relief of ice-load.

Presented without notes, with lantern-slide illustrations.

Discussion

Prof. J. B. Woodworth remarked that he found that the streams described on the west side of Narragansett Bay displayed adjustments to ice-fronts marked out by sand-plains. As for the hypothesis of telling, if the streams on the west were diverted into northeast courses and the seekock on the east side cuts a northwest course, at right angles to the first described, he is left in doubt as to the direction of the telling, regarded as a feature of continental extent.

Vice-President H. E. Gregory took the chair.

CALCINATION VOLCANOES

BY WILLIAM HERBERT HOBBS

(Abstract)

The paper offered an explanation of the manner of formation of such peculiar craters as the volcanic Reis, the Steinheim crater, Coon Butte, and the Zuni Salt Lake. This explanation is found in the peculiar properties of limestone in contact with lavas under different pressures.

Presented without notes.

RELATIONS OF FAULT-BLOCK MOUNTAINS TO FOLDED CHAINS

BY J. B. WOODWORTH

(Abstract)

The sequence of geological events in the mountain-built tracts of North America exhibits such dependence of fault-block mountains (or basin range structure) on antecedent folded chains as to warrant the statement that the two contrasted types of geological structures are causally related in the problem of mountain-building. The structure of the Appalachian province of eastern North America, that of the Lake Superior region, and the geological section from the Yellowstone Park westward across southwestern Montana are appealed to as illustrating the schematic development in time and place of mountain folds and subsequent normal faulting. The place occupied by dominantly acid intrusions in the episode of folding and of dominantly basic eruptives in the episode of faulting are pointed out, together with the position of non-marine sediments of the graywacke type associated with coal or vegetal deposits in the evolution of the geological structure. The occurrence of red beds in the closing stages of faulting and other correlations are briefly stated, including the place of unconformities (peneplanes) in the actual order.

Presented without notes.

Discussion

Prof. W. H. Hobbs: I would suggest a slight change in the use of terms "underthrust" for "overthrust" and an abandonment of the term "tensional stress" in connection with normal faulting. The mechanics of the folding process indicate that the active force in the case is from below and in front of a rising anticline, not from above and behind. Some years since, in a study of the changes brought about in railroads and bridges at the time of earthquakes, I showed that in the cases of all known earthquakes from which data were available the normal faulting which accompanied the quakes corresponded to a reduction of area of the surface affected.

Prof. W. M. Davis suggested that the contrast between folding and faulting described by Professor Woodworth might possibly be two phases of one phenomenon, the folding being deep seated and the faulting being superficial. The areas described as folded had been greatly denuded, so that their deep-seated structure was revealed, although their original surface structure was lost. The faulted areas had been less eroded; their surface structure is still visible, while their deep-seated structure is buried. This suggestion was based on one of Gilbert's statements regarding the contrast between the folded Appalachians and the faulted Basin ranges, the former being regarded as of deep-seated origin, revealed by great erosion, while the latter are of superficial origin, not yet much eroded.

Prof. J. B. Woodworth replied, that if normal faulting is the surface expression of deeper-seated folding, the phenomena ought to be found as extemporaneous structures in the same region and not in the sequence set forth in the scheme presented in this paper.

STRUCTURE OF SOME MOUNTAINS IN NEW MEXICO

BY NELSON H. DARTON

(*Abstract*)

In determining the stratigraphy of the red beds in New Mexico and obtaining data for a geological map of the State, the author has determined the structure of the various mountain ranges. Numerous types are represented, comprising tilted mesa blocks, anticlinal uplifts in some cases faulted, volcanic piles and cones, early paleozoic ridges, and laccoliths. Physiographic features are in part dependent on structure and in part independent of it, excepting so far as to determine the altitude and distribution of hard and soft rocks.

Presented by title in the absence of the author.

NATURE OF PALEOZOIC CRUSTAL INSTABILITY IN EASTERN NORTH AMERICA

BY CHARLES SCHUCHERT

Read from manuscript.

STRATIGRAPHY AND DIASTROPHISM OF WESTERN NEWFOUNDLAND

BY CHARLES SCHUCHERT AND CARL O. DUNBAR

Presented by title by request of the authors.

LARGE FAULT IN WESTERN NEW YORK *

BY GEORGE H. CHADWICK

CONTENTS

Introductory Remarks

The faulted Paleozoic region of New York State has hitherto been regarded as confined to the Mohawk Valley [1] and the areas north [2] and east.[3] No considerable fault has been recorded west of Little Falls,[4] except one in the vicinity of Trenton Falls.[5] An unrecorded (or the same?) fault probably exists along the line of Stebbins Creek,[6] northeast of Clinton, New York,

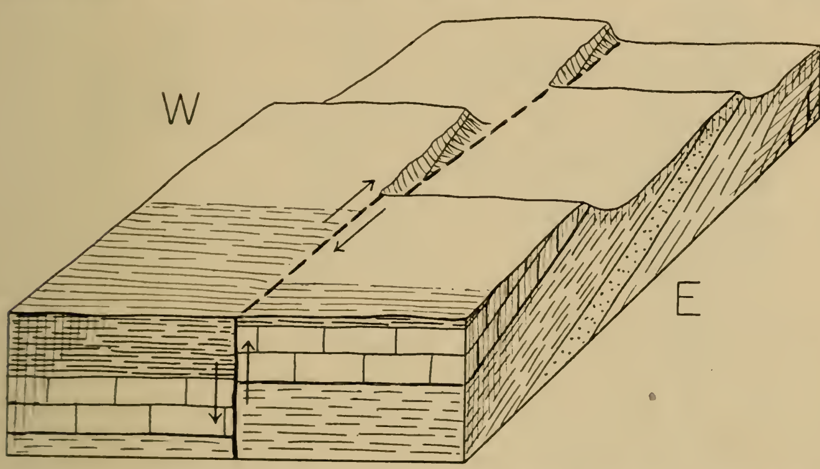

Figure 1.—*Diagram showing actual Vertical, but seemingly horizontal, Displacement of gently sloping Strata*

which, like the Trenton fault, differs from the rule of the Mohawk Valley displacements by having its downthrow on the west side.

A similar downthrow on the west characterizes the fault to be described, but it shows itself ostensibly as a northward shove of the downthrow side

* Manuscript received by the Secretary of the Society December 30, 1919.

[1] Darton: 14th Rept. N. Y. Geology, 1894, pp. 33-54, pl. 1.

[2] Cushing: Bull. 191, N. Y. State Museum, p. 53, and geologic map; Bull. 95, pp. 403-412.

[3] Cushing and Ruedemann: Bull. 169, N. Y. State Museum, p. 104, etc.

[4] Cushing: Bull. 77, N. Y. State Museum, p. 38, and geologic map.

[5] Miller: Bull. 126, N. Y. State Museum, pp. 23-26.

[6] Compare Vanuxem, Rept. on 3d Dist. N. Y., pp. 85-86, with map, pl. 12, in Newland and Hartnagel, Bull. 123, N. Y. State Museum, which shows the iron-ore bed shifting a hundred feet on the contours in crossing this creek.

because of the gentle southerly dip of the strata. The block diagram, figure 1, illustrates how this would occur.

The Field Evidence

The New York State geologic map of 1901 shows the Niagara limestone escarpment jogged northward in the manner of figure 1 for a distance of about three miles, at Clarendon and Holley, Orleans County, New York, and the Onondaga (Corniferous) scarp similarly affected two miles east of Batavia,[7] where crossed by the New York Central main line. These two offsets (see map, figure 2) are in line with each other and with a discordance of the stratigraphy at Linden and the valley of Dale, Wyoming County, New York, recognized by the writer in 1918.

The relations at Linden are as follows: The strata involved are the Upper Devonian (Portage and Genesee) formations, whose thicknesses[8] are approximately:

	Feet	
Nunda sandstone	150	
Gardeau shales and flags	300	(285)
Hatch shales	150	(125)
Rhinestreet black shale	90	(60)
Cashaqua olive shale	80	
Middlesex black shale	20	
West River dark shale	30	
Genundewa limestone	1	
Geneseo[9] black shale	25	

The total thickness between the Nunda sandstone and the Genundewa limestone is, therefore, 670 (600) feet. The falls at Linden are over Geneseo shale capped by Genundewa limestone (1060 contour), each with characteristic fossils; but in the ravine a mile west the base of the Gardeau beds is found at only 1,264 feet above tide resting on the darker Hatch shales, and the Nunda sandstone appears at only 1,490 feet in the road north from Vernal, seven-eighths mile southwest from the preceding. These figures, which check with other outcrops, harmonize with the proper thickness of the Gardeau, but demand either an excessive drop or a remarkable thinning of the strata in the first mile west of Linden. The discrepancy is not less than 100 feet.

That there is really an abrupt drop here is demonstrated three miles south up the same valley (locally "the dale"). On the west side of this valley, a mile and a half north of Dale, the Nunda sandstone at 1,385 feet above tide caps a 200-foot cascade, which exposes only the Gardeau down to the mouth of the ravine. Directly across the dale, on the east side, the dark

[7] Compare the corresponding jogs in the Albion moraine and the Warren beaches on Leverett's map, pl. iii, Mon. xli, U. S. Geol. Survey, pp. 702, 768-9.

[8] Luther: Bull. 172, N. Y. State Museum. See under each formation.

[9] The name "Genesee" is in duplicate use for the group (including the West River) and for the part beneath the Genundewa limestone, which is under water at Hall's type locality. To avoid confusion, the variant *Geneseo* may be given to the latter, which is 84 feet thick in the fall on Fall Brook, Geneseo, N. Y.

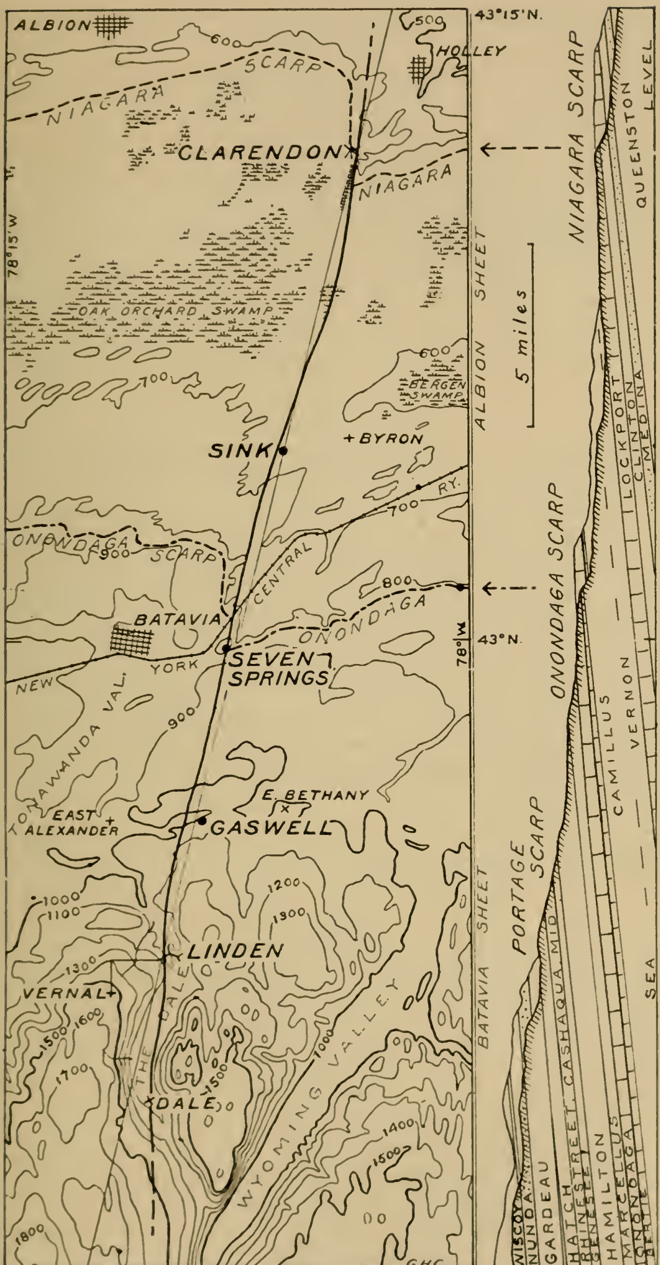

FIGURE 2.—*Map and Section of Clarendon-Linden Fault Area in western New York*

Map based on United States topographic sheets with limestone scarps adapted from New York State geologic map. Geologic profile (vertical exaggeration 12 1/3 times) on east of fault with portions of downthrow block appearing behind. Main lines of five other railways, not shown, cross this area.

Hatch shales rise to 1,255 feet in the gullies and no Nunda sandstone appears on the hilltops in outcrops up to 1,500 feet or over. An excessive west dip, at least 100 feet per mile, is required to explain this without faulting, and the downthrow is on the west, as in the broken limestone scarps to the north.

An earlier observation on this line of disturbance was made by Mr. D. W. Williams, geologist for the Dominion Natural Gas Company, and his assistants, who recognized the westward plunge in the Centerfield (a Hamilton) limestone [10] halfway between East Alexander and East Bethany (see map) and located an exploratory drilling on it. Five miles north, near Batavia, according to Mr. H. P. Woodward, are the locally famous "Seven Springs." The latest development along this line is an infall sink in the Salina shales near Byron, New York, investigated by the writer for the Director of the State Museum, which may or may not have genetic connection with the supposed fault.

Nature and Origin

This displacement is, of course, an unexpected phenomenon in the flat-lying Paleozoic strata of western New York. It is 135 miles west of the nearest known large faults, at Clinton and Trenton. An abundance of small, apparently superficial faults, often with accompanying buckling, proves to exist between Rochester and Lake Erie, but the only other one found of mappable size, offsetting formations, is a broad overthrust discovered by D. W. Williams in the Laona-Shumla beds near Smiths Mills, Chautauqua County, with a throw of 25 feet and a heave of possibly 100. Scarcely any square mile is wholly free from these small displacements—either buckles, normal faults, or flat thrusts—but they are easily ascribed to surface agents or to removal of subjacent salt or gypsum, etcetera. The Clarendon-Linden fault involves beds far below the salt, belongs in another category, and is clearly of tectonic origin. Its isolation makes it unique in New York geology.

EXPERIMENTAL STUDIES ON THE FORMATION OF JOINTING PLANES

BY FRED E. WRIGHT

(*Abstract*)

Slabs of polished plate-glass one to two inches thick, if chilled rapidly, develop fracture cracks which have all the characteristics of platy and columnar jointing in lava-flows; the strain phenomena and consequent jointing cracks can be studied on these plates in actual process of development and photographed in polarized light with a moving-picture camera. The fracture planes formed under these conditions of rapid cooling are entirely different from those which appear on rapid heating, which produces curved fracture planes not unlike the warped exfoliation surfaces of natural rocks.

An analysis of accurate measurements of the strains set up in glass plates for different temperature gradients leads to interesting conclusions, which are significant in the general theory of fracture phenomena in rocks.

Presented with illustrative experiments, without notes.

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[10] Defined: Clarke and Luther, Bull. 63, N. Y. State Museum, pp. 17, 19, 50.

STRUCTURAL FEATURES OF THE SOUTHERN OUACHITA MOUNTAINS, OKLAHOMA

BY CHARLES W. HONESS [1]

(*Abstract*)

A map of the areal geology of one thousand square miles of country lying chiefly in the folded Paleozoics of McCurtain County, Oklahoma, was presented, together with appropriate charts and structural cross-sections.

The stratigraphy is the same as that in west-central Arkansas and involves the following section:

Trinity sand (Carboniferous).
Unconformity.
Jackfork sandstone (Carboniferous).
Stanley shale (Carboniferous).
Arkansas Novoculite (Devonian).
Missouri Mountain slate (Silurian).
Blaylock sandstone (Silurian).
Polk Creek shale (Ordovician).
Bigfork Chert (Ordovician).
Womble shale (Ordovician).
Blakely sandstone (Ordovician).
Mazorn shale (Ordovician).
Crystal Mountain sandstone (Ordovician?)
Unconformity.
Collier shale (Cambrian).

The structure is likewise similar to that in west-central Arkansas, and is essentially a series of recumbent, plunging anticlines and synclines accompanied by thrust and normal faults.

The sediments are predominantly shales, slates, and sandstones embracing two formations of chert and all are considerably metamorphosed. There is almost no limestone in the series.

The present brief report, while it comes as a result of four years of field-work and study in the area under consideration, can not be considered as a summary of results and conclusions. It is more properly a report of progress and a statement of the problems.

Presented without notes.

DISCUSSION

DR. H. D. MISER: Several geologists have made reconnaissance trips into the area described by Mr. Honess, but he is the first to map in detail the rock formations. Structure as indicated by his map shows many points in common with the structure of the part of the Ouachita Mountain region in Arkansas. One of the most noticeable structural features in the area described by him is the high angle that the eastward or southeastward trending axis of many

[1] Introduced by C. P. Berkey.

folds make with the axis of the principal anticlinorience, which trends northeast.

Remarks were also made by Prof. Charles Schuchert.

HEART MOUNTAIN OVERTHRUST, NEAR CODY, WYOMING

BY D. F. HEWETT

(Abstract)

Investigations in 1919 in northwestern Wyoming show that this overthrust, first recognized by Dake in 1916, is more extensive than first suspected and is a dominant structural feature of the region. In addition to the Heart Mountain block numerous remnants form the summits of McCullock Peak, 16 miles southeast. These remnants are 28 miles east of the westernmost outcrop of the thrust block and show that the displacement on the fault is at least that distance. The thrust probably took place during the Upper Eocene period as the Paleozoic limestones that make up the thrust block overlie Lower Eocene beds and were deeply eroded before the deposition of the tuffs and breccias of the Akoroka Mountains (Neocene).

Presented with lantern-slide illustrations.

Adjournment for the day was taken about 5.10 o'clock p. m.

PRESIDENTIAL ADDRESS

The evening session was held in Huntington Hall of the Rogers Building, where the Society and its guests gathered to listen to the address, entitled "Earth sciences as the background of history," by President John C. Merriam.

The reading of the presidential address was followed by an illustrated lecture by J. W. Pack,[1] entitled "The wonders of Bryce Canyon, Utah."

ANNUAL SMOKER

The two foregoing addresses were followed by the customary annual subscription smoker in the Rogers Building.

Session of Tuesday, December 30

The Society was called to order by President Merriam at 9.10 o'clock a. m., and, after announcements by the Secretary and the local committee had been made, the Annual Report of the Council was, on motion, taken from the table, adopted and ordered printed.

[1] Introduced by James F. Kemp.

REPORT OF THE AUDITING COMMITTEE

The report of the Auditing Committee was called for, and the chairman reported that it had found the Treasurer's accounts correctly cast and properly vouched for.[2] The report was accepted.

SYMPOSIUM ON RESEARCHES IN SEDIMENTATION

The participants in the Symposium and the titles of their papers are as follows:

RESEARCHES IN SEDIMENTATION

BY T. WAYLAND VAUGHAN

THE VALUE OF TERRESTRIAL ORGANISMS IN THE INTERPRETATION OF SEDIMENTS

BY J. P. BUWALDA[1]

MECHANICAL RESEARCHES IN THE INVESTIGATION OF SEDIMENTS

BY EUGENE WESLEY SHAW

CHEMICAL RESEARCHES IN THE INVESTIGATION OF SEDIMENTS

BY H. E. MERWIN

DIAGENESIS IN SEDIMENTATION

BY CHARLES SCHUCHERT

These papers are to be published in full in the Bulletin.

The symposium subject was discussed further by Messrs. R. A. Daly, W. H. Twenhofel, J. A. Holmes (a visiting civil engineer), E. M. Kindle, F. E. Wright, M. W. Twitchell, D. F. Hewett, R. W. Sayles, J. B. Woodworth, L. D. Burling, G. R. Mansfield, and John C. Merriam.

After the close of the symposium the following papers were presented:

TITLES AND ABSTRACTS OF PAPERS PRESENTED BEFORE THE MORNING SESSION AND DISCUSSIONS THEREON

PRELIMINARY RESULTS OF A STUDY OF THE SAN FRANCISCO BAY SEDIMENTS

BY GEORGE D. LOUDERBACK

(*Abstract*)

The work was done in cooperation with the United States Fish Commission,

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[2] Since the meeting the Secretary has received the following report from E. W. Berry, of the Auditing Committee, under date of January 26, 1920:

"According to the instructions of the Council, I have examined the securities belonging to the Geological Society of America in the safe-deposit box of the Treasurer at the Baltimore Trust Company and found them to be correct as listed by the Treasurer and to have the coupons attached."

[1] Introduced by H. E. Gregory.

and both dredge and core samples were taken by the staff of the United States Steamer *Albatross*. Some preliminary charts and physical data have already been published in a University of California Publication, Zoology, in volume 14, number 1, 1913. Bottom samples vary from black mud, which is especially abundant in the southern part of the bay, to coarse boulders in the Golden Gate. Part of this latter region appears to be kept practically free from sediment by current action, and to have bedrock exposed. Parts of the bay show rhythmic banding in thin layers, as seen in core samples; some parts show thicker uniform layers, and some parts material not well sorted. The present horizon of deposition varies in depth below sealevel from 0 to 378 feet, and if preserved in the geological series would probably give the appearance of a marked disconformity, although the wide and deep "eroded" channel is contemporaneous with the deposits at higher levels now forming on both sides. Considerable areas where mud is now depositing formerly showed sand on which oysters and other shellfish lived. The cause of the change is not understood. In core samples from these areas alternations of sand and mud were found, and the appearance of a sand layer gave rise immediately, as it were, to a colony of oysters that disappeared with the first appearance of a mud layer.

The distribution of types of sediment is the reverse of that ordinarily given in general descriptions of sediments, in that the muds are deposited inshore, often up to and including the beach, while sands are deposited farther from the land and out into the ocean.

Presented without notes.

Discussion

Dr. E. W. Shaw: The fact that sedimentary deposits show infinite variety in mechanical constitution—no two specimens are exactly alike and there is almost no semblance of natural grouping—does not seem to me a strong argument against classifying and naming. This argument would not be advanced against the sorting of oranges or against the sizing of coal and the naming of the various separates. Incidentally, the names that have become popular seem to indicate that to be useful and used names must be mnemonic, whether or not they are sensible. Pea coal does not resemble peas in size or shape any more than in color.

Lateral variations of strata, such as Professor Louderback describes, raise a question concerning the interpretation of ancient sediments. To what extent is the fact that present deposits seem to show much greater variability than ancient deposits due to unusual present conditions, and to what extent to mistake in correlation of ancient strata and formations? Present deposits seem to vary from place to place, ancient deposits from time to time. How often can we say of ancient strata, as we can of the present deposit at San Francisco, "This stratum represents the various products of erosion of a certain fraction of an epoch. Here is the gravel, there the sand, and beyond is the mud. Here is the silica, there are the oxidized and hydrated silicates, and somewhere else are the carbonates, sulphates, chlorides, etcetera."

A third point concerns original structures. The bottom of San Francisco Bay, like other present surfaces of deposition, is undulating. The deposit formed in any particular century has marked basins, monoclines, and anticlines that are not due to deformation after deposition. Through differential settling, other structural features are developed, and two strata 500 or 100 feet apart may not be at all parallel.

TILL ARGILLITES (PELLODITES), PRE-CAMBRIAN, PERMIAN, AND PLEISTOCENE

BY ALFRED C. LANE

(*Abstract*)

Glacial deposits are now generally accepted from the late Precambrian, Permian, and Pleistocene, and the till is generally recognized by having fragments unassorted in size, shape, and composition (sometimes striated). Correlative to the till, as Sayles has shown, are glacial brick-clays or till-argillites largely composed of rock-flour particles, rhythmically banded and not unassorted in size, but relatively so in shape and composition, the chemical composition tending to approach that of the average rock and to be relatively unleached. Illustrative analyses of rocks near Boston were given.

Presented without manuscript.

Adjournment for luncheon was taken at 12.30 o'clock.

After the noon recess the Society divided into three sections, to facilitate the presentation of the papers on the program.

The main body was called to order at 2.20 o'clock p. m., President Merriam in the chair, and the program was resumed.

TITLES AND ABSTRACTS OF PAPERS PRESENTED BEFORE THE AFTERNOON SESSION OF TUESDAY

MISSISSIPPIAN TUFF IN THE OUACHITA MOUNTAIN REGION

BY H. D. MISER

(*Abstract*)

Tuff of Carboniferous age occurs near the base of the Stanley shale, in the Ouachita Mountain region in Polk County, Arkansas, and McCurtain County, Oklahoma. There are three, and possibly four or five, beds of it, ranging in thickness from 6 to 85 feet. All of them are very similar in lithologic character. The lowest bed is the thickest and most widely distributed; it has been mapped in detail in De Queen quadrangle, lying mostly in Arkansas, and to it the name Hatton tuff lentil has been applied, for the reason that the best known exposure is in a cut of the Kansas City Southern Railway one-half mile south of the village of Hatton. The tuffs are compact, massive, and tough; are generally homogeneous, except for the presence of numerous chloritic "pellets" that lie parallel with the bedding, and in color are dark gray

with a greenish tinge. Feldspars which occur in grains 7 millimeters or less in diameter are generally more abundant than the other minerals, which include muscovite, magnetite, pyrite, zircon, apatite, chlorite, calcite, and kaolin. Much of the tuff, however, consists of devitrified and silicified volcanic glass, of which a large part is ordinary volcanic ash and represents fragments of the walls of bubbles, though some of the larger fragments of glass are fluidal. The original rock from which the tuff was derived was probably near a rhyolite or quartz latite.

None of the tuff beds have yielded fossils, so their assignment to the Mississippian series is based upon the relations of the Stanley shale to overlying and underlying rocks, whose ages have been determined by fossils.

The conclusion is that the tuff had a southern source. This source was probably on or near a large land area that existed in Louisiana and eastern Texas not only during the Mississippian epoch, but during the greater part of the Paleozoic era.

Read in full from manuscript.

DISCUSSION

Dr. SIDNEY POWERS: Evidence of the existence of a land-mass in east-central Texas in Paleozoic time is found in well records. Precambrian schist and granite are found directly beneath the cretaceous beds south and east of the Llano Burnet region. Ordovician limestones overlie the Precambrian granite in the Red River district in Cooke, Montague, and Clay counties, and they are in turn overlain directly by Pennsylvanian sediments.

Remarks were also made by Prof. Charles Schuchert, with reply by the author.

TYPES OF ROCKY MOUNTAIN STRUCTURE IN SOUTHEASTERN IDAHO

BY GEORGE R. MANSFIELD

(Abstract)

The paper gives additional data regarding the Bannock overthrust, described by R. W. Richards and the author in 1912, and discusses briefly a number of types of mountain structure recognized in seven quadrangles (part of the western phosphate field) in southeast Idaho. The types include elongate and curved zigzag folds; eroded fan folds, some of which are broken by faults; transverse folds, one of which has developed into a transverse thrust-fault; horst and graben structure, strikingly cross-faulted; and noteworthy unconformities. Mention is made of the supposed conditions attending the deformation.

Presented without manuscript, with lantern-slide illustrations.

DISCUSSION

Dr. G. W. STOSE: The author states that the great overthrust fault-planes were flat and not deep, which is in accord with conclusions I have reached in regard to such faults. The question is raised whether the normal faulting, which cuts and offsets the thrust blocks, was simply due to a readjustment in the thin overthrust mass, or was it a later and distinct episode of faulting under different conditions? If the latter, compressive stresses which gave rise to the great overthrusts must have had relief and tension conditions set in.

Dr. G. R. MANSFIELD replied: The normal faulting is thought to be associated with extrusion of basalt, as indicated in one of the views shown, and this was outpoured upon the greatly eroded surface of the folded formations. The normal faulting is thus believed to be of incidentally later date than the folding and thrusting.

AGE OF THE SCARP-PRODUCING FAULTS OF THE GREAT BASIN

BY GEORGE D. LOUDERBACK

(*Abstract*)

The methods used in determining the age of the faults were outlined and the relations of the scarps to the Tertiary sediments, lavas, and erosion surfaces discussed. A comparison was made of various scarps which have been held by certain writers to be of different age, and also of different parts of certain scarps that have been referred to different periods of activity. The conclusion was reached that the present scarps originated in comparatively recent times, their beginnings probably being not later than middle Pliocene and possibly post-Pliocene.

Presented without notes.

FAULT SYSTEM AT THE SOUTHERN END OF THE SIERRA NAVADA, CALIFORNIA

BY JOHN P. BUWALDA [1]

(*Abstract*)

The Sierra Nevada[2] are prolonged in southern California by the Tehachapi Mountains, which extend southwestward and join the Coast Ranges at Tejon Pass. While the Sierras are probably a simple tilted block farther north, their extreme southern end and the Tehachapi Mountains show acute deformation along both flanks and considerable faulting of Tertiary date within the range. The fractures appear to have occurred in at least two distinct periods. Thrust-faulting characterized the earlier period. Both the stresses which uplifted the Sierra and those which folded the Coast Ranges seem to have participated in deforming the southern Sierra region between Kern River and Tejon Pass.

Read from manuscript, illustrated by lantern slides.

Discussed by Messrs. A. M. Bateman, Eliot Blackwelder, W. M. Davis, and J. E. Spurr, with reply by the author.

Former President W. M. Davis was called to the chair.

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[1] Introduced by H. E. Gregory.

[2] The term "Sierra Navada" is properly plural.

EXTENT AND THICKNESS OF THE LABRADOR ICE-SHEET

BY A. P. COLEMAN

(Abstract)

It is shown that the Labrador ice-sheet did not cover some thousands of square miles in the northeast of Labrador and did not cross the Shickshock Mountains in Gaspé. Estimates are formed of its thickness at certain points and conclusions are drawn as to the bearing of these facts on the theory of isostasy.

Presented without notes and illustrated with lantern slides.

DRUMLINS AT LAKE PLACID

BY WARREN UPHAM

(Abstract)

In a visit last summer at Lake Placid, in the central part of the Adirondack Mountains, my observations of the glacial drift included the mapping of a group of a dozen drumlins, a phase of till accumulation not previously reported in that region. These smoothly oval drift hills, from 50 feet to nearly 150 feet in height above the adjacent Mirror Lake and Lake Placid, occupy much of the area of Lake Placid village and the extensive grounds of the Lake Placid Club. Westward they are prominent one to two miles from this village, beside and south of the roads leading to Saranac Lake. Their absence around Saranac Lake and generally throughout the region of the Adirondacks, with their elevation, 1,900 to 2,000 feet above the sea, give to this group exceptional significance in its testimony concerning the conditions and time of formation of this class of drift deposits.

Signal Hill, between the southern arm of Lake Placid and the northwest side of Mirror Lake, is a typical drumlin, its graceful outline being well seen as it is approached on the steamer cruise around the former lake. Thence a series of similarly massive drumlins continues a mile southward, forming a connected ridge a third to a half of a mile wide. From the rounded summit of Signal Hill a descent of about 15 feet is made before rising to the next broad hilltop, which may be named Stevens Hill for its large summer hotel, the Stevens House. On the nearly level top of the most southern drumlin in this series is another great hotel, the Grand View House, opposite to the south end of Mirror Lake.

The altitude of Lake Placid, as stated in a note beneath its topographic map for the United States Geological Survey, is 1,859 feet above the sea. A narrow and flat tract of modified drift, 5 to 10 feet above this lake, adjoins the eastern foot of Signal Hill, separating Lake Placid from Mirror Lake, the latter having an altitude of 1,857 feet. Above these lakes the drumlins thus described attain heights of about 120 to 135 feet, Signal Hill being noted as 1,981 feet above the sea. Stevens Hill is estimated as 1,990 feet; the depression next south, 1,960 feet; the third drumlin summit, 1,975 feet; next a depression to 1,965 feet; and the Grand View Hill, 1,980 feet. The principal street of Lake Placid village, parallel with the Mirror Lake shore, extends

along the eastern side of these drift hills, which are merged together, excepting the slight hollows between their successive broadly rounded crests.

East of the central part of Mirror Lake and north of the principal buildings of the Lake Placid Club, a drumlin about a half mile long from north to south, with a width of a quarter of a mile, rises 100 feet above the lake. Its northern part is wooded, and the southern part is farming land. Within a third of a mile thence to the south and southeast are three smaller and lower drumlins, having heights of about 40, 50, and 20 feet, in their order eastward from the south end of Mirror Lake. The first and second, mainly wooded, are close south of the road leading east from the Lake Placid Club, and the third is on cultivated land at the northeast side of this road.

The outlet of Lake Placid, flowing south two miles from its southwestern arm or bay to the Chubb River, crosses a tract of glacial drift, with many boulders along the stream course, which intersects this group of drumlins. On the farms next west, and lying south of the road from the Stevens House to Saranac Lake, are three drumlins, the most northern being named Ne-an-doc Hill, about 2,010 feet above the sea. The others, adjoining this on the southeast and southwest, are 30 to 40 feet lower. Each of the three has a length of about a third of a mile, trending south-southeastward; but all before described trend from north to south, excepting Signal Hill, which trends southwesterly.

Between a quarter of a mile and one mile southwest of the preceding, Chubb Hill, a large drumlin at the south side of the road from Lake Placid railway station to Saranac Lake, rises nearly 150 feet above this road, its wooded top being about 2,000 feet above the sea. This oval hill trends from north to south, but its contour and height are not well represented on the Saranac quadrangle sheet of the United States Geological Survey.

These twelve drumlins, found within an area that measures about two and a half miles from east to west and slightly more than a mile from north to south, consist of till, so far as observed, upon all the surface and in sections seen beside roads, on lake shores, and in digging wells. The extensive deposit of glacial drift, containing frequent boulders up to 6 or 8 feet in diameter and rarely larger, probably reaches to a depth of about 250 feet beneath the crests of the drumlins. Its thickness is indicated by the maximum depth of Lake Placid, which is 125 to 150 feet in its eastern arm, about a mile north from Signal Hill. Such sounding is there found between Buck Island and the eastern shore, near the only precipitous rock cliff that borders the lake through all its extent.

Rock formations, only thinly overspread with drift, adjoin Lake Placid and form its islands and all its shore, except that the glacial drift attains great thickness, as here described, at its southern end, also inclosing Mirror Lake. Southward from this group of drumlins, glacial drift and modified drift cover a tract about two miles wide and extend nearly five miles southward, traversed by the lower part of Chubb River and from south to north by the West branch of the Ausable River. Neither drumlins nor marginal moraines are observed on this tract, which in part the West branch has channeled and terraced.

Surrounding this plentiful drift and the remarkable group of drumlins, the high rock ranges of the Adirondack Mountains rise to altitudes from 2,000 feet to nearly 3,500 feet higher, including McKenzie and Street Mountains, Mount Whiteface, the Sentinel Range, and Mount Marcy, the last being the highest peak of this great mountainous region. These summits were enveloped by the continental ice-sheet at its time of greatest thickness, and during its final melting the drumlins were amassed and molded by glacial currents beneath the weight of a relatively thin remnant of the waning ice-fields.

Presented by title in the absence of the author.

EXTREMES OF MOUNTAIN-GLACIER EROSION

BY WILLIAM HERBERT HOBBS

(*Abstract*)

A comparative study of different glaciated mountain districts as illustrating stages of the erosion cycle of mountain glaciation. The Glacier National Park appears to represent an extremely late stage. If the Big Horn Range and the Alpine Highland be taken to represent respectively the *grooved* or *channeled* upland and the *fretted* upland and correspond to the stages of youth and maturity, the Glacier National Park may be described as a *monumented* upland, and to represent the old-age stage of the cycle. The relation in position of the erosion residuals in such a monumented upland to the glacial cirques is quite different from that of the *horns* in a district like the Alps.

Presented by title in the absence of the author.

DISPERSION OF STONES IN THE DRIFT, IN NEW HAMPSHIRE

BY JAMES WALTER GOLDTHWAIT

(*Abstract*)

A State-wide survey of road materials for the State Highway Department in 1917 and 1919, in which attention was directed particularly to the study of the distribution of various types of ground moraine and gravel, has afforded opportunity to collect a mass of information regarding the paths followed by fragments of native rock during the Pleistocene glaciation of New Hampshire. The percentages of different types of rock have been counted in samples of ground moraine and gravel from more than 250 localities; and these data, when plotted on the geological map of the State, show clearly the relation of the drift stones to the parent areas and to the course of the Wisconsin ice-sheet as recorded by striæ. By this means one reaches the conclusions that: (*a*) there is a strong prevalence of strictly local material in the drift; (*b*) great heterogeneity characterizes those gravels and tills which were fed from a number of small and diverse rock areas, while simplicity of composition characterizes those deposits which lie on or immediately southeast of wide belts of rock; (*c*) there are great differences in the amount of stony material and in the average size of the stones in the drift, according to the massive or fissile character of the parent rocks and the durability of the fragments during transportation; (*d*) percentage counts of drift stones

are useful as a guide to the bedrock geology of districts where actual outcrops are scarce or wholly absent.

The two dominant factors in this study—areal distribution of the bedrock and direction of glaciation—explain why some parts of New Hampshire are fortunate and others are unfortunate in the road-making quality of their gravels and till deposits.

Presented with the aid of maps and diagrams.

President Merriam resumed the chair.

LATE PLEISTOCENE CHANGES OF LEVEL IN NORTHERN MANITOBA, CANADA[1]

BY W. A. JOHNSTON

(*Abstract*)

The paper presents the results of field-work done during parts of 1917 and 1918. Determinations of the altitudes above sealevel of the raised beaches of glacial Lake Agassiz were made at numerous localities in northwestern Manitoba, and of the marine beaches along the line of the Hudson Bay Railway to supplement those already made by Messrs. J. B. Tyrrell, D. B. Dowling, and William McInnes in this region. It is thought that the new data, considered in conjunction with the well known work of Mr. Warren Upham and Mr. Frank Leverett in the southern part of the Lake Agassiz basin, may throw some further light upon the question of the character and amount of the late Pleistocene changes of level in this part of Canada.

The results of the work confirm, in general, Upham's view, that the southern part of the region was uplifted earlier than the northern part and was not greatly affected by the later uplift of the northern part, but indicate also that the changes of level were related to two or more domes or centers of uplift in different parts of the region. It was also found that the highest Lake Agassiz beach in the area north of Duck Mountain, in northwestern Manitoba, has a gradual descent toward the north, amounting to about 60 feet in a distance of 25 miles, though north and south of this area the beach has a gradual ascent toward the north. The true significance of this fact in relation to the question of the character of the late Pleistocene changes of level has not been satisfactorily determined, but possible interpretations are offered.

Presented by title in the absence of the author.

GLACIAL HISTORY OF THE COLUMBIA RIVER IN THE BIG BEND COUNTRY

BY OSCAR E. MEINZER

Presented by title in the absence of the author.

[1] Published by the permission of the Directing Geologist, Geological Survey of Canada.

CERTAIN ASPECTS OF GLACIAL EROSION IN ALASKA

BY W. O. CROSBY

(Abstract)

Describes and discusses examples, believed to be typical, which tend to localize glacial overdeepening and to show that valley broadening is by far the more important phase.

Presented by title by request of the author.

FURTHER DISCUSSION OF THE AFTONIAN GRAVELS AND THEIR RELATION TO THE DRIFT-SHEETS IN THE REGION ABOUT AFTON JUNCTION AND THAYER, IOWA

BY GEORGE FREDERICK KAY

(Abstract)

The gravels near Afton Junction and Thayer, in Union County, southwestern Iowa, are so well known to students of Pleistocene geology, and their Aftonian age has been so generally accepted, that one may well hesitate to state that a restudy of these famous exposures and other exposures in the same region has revealed evidence which seems to justify further discussion of the origin and relationships of these gravels, and to warrant question being raised with regard to former interpretations.

Ever since the year 1895, when Dr. T. C. Chamberlin made reference in Geikie's Great Ice Age to the interesting characteristics of these gravels, and interpreted them to be kamelike deposits closely associated with the drift on which the gravels lie and overlain by a later drift-sheet, many glacial geologists of America and of Europe have visited the locality. Some have come merely to see the type sections of the two oldest drifts, now known as the Nebraskan drift and the Kansan drift, separated by the gravels which years ago were named the Aftonian interglacial gravels; others have come to study carefully the characteristics of the drifts and gravels and their interrelationships. The most important contributions dealing with these gravels and related deposits have been made by Chamberlin, Bain, and Calvin.

The purpose of this paper is to show that the gravels of Union County, which for so many years have been called the Aftonian interglacial gravels, do not constitute a distinctive stratigraphic horizon separating the Nebraskan drift from the Kansan drift, which is the generally accepted interpretation, but that these gravels are lenses and irregularly shaped masses of gravels within a single drift, the Kansan, or, if in two drifts, the Nebraskan and the Kansan; it is not possible by means of the gravels to differentiate the two drifts.

It is shown also in this paper that, although the gravels in the vicinity of Afton Junction and Thayer can not be used to establish the presence of two drifts, there is other evidence in the region which makes it clear that the two oldest drift-sheets are present, and that they are separated in age by a

very long interglacial epoch. At several places near the village of Afton and also west of Osceola, about 20 miles east of Afton, there are fine outcrops of Nebraskan gumbotil, below which is Nebraskan drift and above which is Kansan drift. Moreover, in Dodge township, Union County, there is a splendid section of peat lying on Nebraskan drift and overlain by Kansan drift. This peat bed was described years ago by Savage. The gumbotil and the peat are now the significant evidence of Aftonian time.

If the interpretations of the writer are correct, the Aftonian gravels of Union County have lost much of their former significance, and the type sections, particularly the Grand River section, can no longer be referred to as sections in which it is possible to study the two oldest drift-sheets separated by Aftonian gravels.

Presented without notes, with lantern-slide illustrations.

The session adjourned about 5.30 o'clock p. m.

Sectional Sessions

Two additional sections were formed Tuesday afternoon for the reading of papers. The first was presided over by former President A. P. Coleman, with D. F. Hewett acting as secretary.

The following papers were presented:

WINDROW FORMATION, AN UPLAND GRAVEL FORMATION OF THE DRIFTLESS AND ADJACENT AREAS OF THE UPPER MISSISSIPPI VALLEY

BY F. T. THWAITES AND W. H. TWENHOFEL

(Abstract)

The Windrow formation consists of widely separated patches of quartz and chert gravels, with associated limonite deposits, which occur on the tops of high elevations throughout the driftless area of the upper Mississippi Valley and the adjacent glaciated area to the west and south. The chert pebbles contain fossils, of which the youngest are certainly of Silurian age. The authors have concluded that this formation was river-deposited at a time far antedating the Pleistocene and not necessarily connected with peneplanation, and that no definite age can be assigned.

Presented without manuscript by W. H. Twenhofel.

CORRELATION OF THE CENOZOIC FORMATIONS OF THE GREAT BASIN

BY JOHN C. MERRIAM

Presented by title by request of the author.

CERTAIN DIVERSE INTERPRETATIONS OF PLEISTOCENE IN THE DAKOTAS

BY JAMES E. TODD

(*Abstract*)

Ever since the first study of the Pleistocene deposits of the Missouri River in the Dakotas the opinion has been held persistently that that stream holds its course through that region on account of the diverting influence of the continental ice-sheet as it pushed in from the north and northeast.

Dr. A. G. Leonard, State Geologist of North Dakota, came to a different conclusion, from his studies in the western part of the State. His conclusions were published briefly in the Bulletin of the Geological Society of America, volume 27. His view was given more in detail in the Bismarck folio in 1912. He concludes, first, that the Missouri River has flowed in approximately its present course in North Dakota ever since the middle of the Tertiary, and that the ice-sheet had little effect upon its course. Second, its valley had already been noted down to the present plain of drainage, and that it was partly filled during the latter part of the Wisconsin stage of the Pleistocene. It is the purpose of this paper to consider his reasons and show what conclusions the present knowledge of the subject calls for.

The first significance of a fact that he considers decisive is that glacial boulders were found near Bismarck resting on bedrock at the bottom of the present river channel, 125 feet below the surface and 80 feet below the surface of the river. The locality was on a river terrace rising 40 feet above the river. It apparently was formed as a terrace of Apple Creek where it joined the Missouri River, and apparently in its maximum flow, during the latter part of the formation of the Altamont moraine, which is so approximately developed a few miles east of that point. He concludes that the river had cut down to the depth where the boulders were found before the formation of the moraine, that the boulders were part of the early portion of the deposit, and that since that time the valley has been filled at least 50 feet. This, it may be remarked in passing, would require that the river was relatively 80 feet lower than at present, which is considerably lower than any conditions of this channel farther toward the south or east; for, according to the conclusions of South Dakota geologists, the Missouri at this point was much higher than at present, making Dr. Leonard's conclusion directly opposed to that just stated.

He apparently comes to this conclusion by ignoring or underrating the depth of scour exerted by a flooded river. Cases will be cited showing that this scour, to a depth of 130 feet below the stream, has frequently existed. Another fact is the occurrence of till or boulder clay 30 feet or more in thickness resting upon Cretaceous clay. The argument which he draws from this is shown to be inconclusive, as is also a point which he considers proved, that the broad, mature valley north of Manden is preglacial. The strip of country between the Altamont moraine and the margin of the drift, 25 to 100 miles farther west, seems to have been occupied, both in North and South Dakota,

by temporary lakes and short connecting channels. This will be discussed at some length in the paper.

Presented by title in the absence of the author.

TERTIARY FORMATIONS OF PORTO RICO

BY BELA HUBBARD[1]

(*Abstract*)

An outline is given of the results of an extensive field study of the Tertiary formations of northwestern Porto Rico, made for the New York Academy of Sciences in the summer of 1916, and of nearly two years of stratigraphic and paleontologic study in the laboratory. The Tertiary formations of the type localities in northwestern Porto Rico are described, and correlated with the other Tertiary formations in Porto Rico, the Antilles, and the South Atlantic States. New evidence is presented bearing upon the Tertiary history of the Antillean region and some unsettled questions regarding the age of the formations.

Presented in condensed form from notes.

Discussed by Dr. Marjorie O'Connell and Prof. A. W. Grabau.

TYPE SECTION OF THE MORRISON FORMATION

BY WILLIS T. LEE

(*Abstract*)

The section at Morrison, Colorado, the type locality of the Morrison formation, presents an interesting problem in correlation. The lower part of the rocks originally included in this formation prove to belong to an older formation (Sundance), and the upper part is traceable laterally into beds which have been called "Lower Dakota" by some geologists and "Purgatoire" by others. The upper part contains fossil plants which F. H. Knowlton shows in an accompanying paper are of Dakota type. What shall be done with a formation which at its type locality contains beds of Jurassic age at the base and at the top beds containing fossil plants indistinguishable from those of the Dakota flora?

Presented without notes.

Discussed by Messrs. W. H. Twenhofel, T. W. Vaughan, and A. W. Grabau, with replies by the author.

DISCUSSION

Prof. W. H. TWENHOFEL: The conclusions reached by Dr. Lee are extremely interesting and also particularly pleasing, as I independently have

[1] Introduced by Charles P. Berkey.

reached essentially similar conclusions in respect to the Kansas "Dakota" and the marine Kiowa and Mentoo beds of the same State.

In southern Kansas the sediments considered Lower Cretaceous begin with the terrestrial Cheyenne sandstone, in which there are fossil leaves of dicotyledons belonging to species which also occur in the "Dakota" sandstone. The Cheyenne sandstone is succeeded by the marine Kiowa shales, above which are other terrestrial sediments containing fossil plants like those in the Cheyenne sandstone.

In central Kansas the series begins and ends with terrestrial sediments. There are three horizons containing marine fossils which are separated by two horizons of terrestrial sediments in which are fossil leaves. The marine fossils are of the same general character as those in the Kiowa shales. The uppermost marine horizon and the overlying and underlying terrestrial sediments are those known as the "Dakota."

It is quite obvious that all of these strata are of the same general time of deposition, and if the Kiowa shales and the equivalent marine deposits are of Lower Cretaceous or Comanchean age, then the "Dakota" sandstones are of the same age.

Dr. T. W. Vaughan mentioned the necessity of critical studies of Washita flora of southern Kansas and comparison of it with the so-called Dakota flora of north Texas, and that plans for conducting this investigation have been made.

Former President John M. Clarke took the chair.

FURTHER STUDIES ON THE JURASSIC OF CUBA

BY MARJORIE O'CONNELL

(*Abstract*)

During the summer of 1919 Mr. Barnum Brown continued his stratigraphic work in Cuba and made extensive collections from the Jurassic rocks, obtaining a large and valuable series of ammonites which were submitted to the author for study. In addition to this material, a considerable number of ammonites were received from Dr. Rorg, of Havana, from the study of which the existence of the Upper Cortlandian in western Cuba has been established, in addition to the Upper Oxfordian, the discovery of which was announced at the Baltimore meeting of the Society.

A preliminary paleogeographic map of Cuba, Mexico, and adjoining regions for Upper Jurassic time was presented and a list given of additional species of ammonites not heretofore reported from Cuba.

Presented without notes.

Discussed by Dr. T. W. Vaughan, with reply by the author.

MIDDLE ORDOVICIAN OF VIRGINIA AND TENNESSEE

BY PERCY E. RAYMOND

(*Abstract*)

Strata of Middle Ordovician age outcrop on the western sides of a series of parallel fault-blocks in Virginia and Tennessee, and in passing from block to block a different development is found in each. Very considerable differences in lithology and fauna in the various blocks have led to an interpretation in which each block is thought to have been a separate trough, with its own peculiar life and sediments.

Recent investigations in the field, made with the aid of the Shaler Memorial Fund, have led the writer to the belief that there is much physical and faunal evidence in favor of the older view, that the observed differences are due to distance from the shore, depth of water, and other local conditions. Sections and fossils which illustrate a transition from one "trough" to another will be described and a summary of the probable sequence of events presented, showing the possibility of continuous deposition in the area from early Chazy to late Trenton time, with a single emergence of short duration during and after the Lowville.

Presented by title by request of the author.

NOTES ON THE ORISKANY AND HELDERBERG IN VIRGINIA

BY R. J. HOLDEN

(*Abstract*)

Southwest of the New River those formations which may include the Oriskany and Helderberg have relatively meager development. The members of the Oriskany and Helderberg and their distribution are unknown.

Northeast of the New River these formations have a greater development, particularly the Helderberg. The Oriskany is represented by the Monterey sandstone. This is a definite lithologic unit—a calcareous sandstone. It is sharply limited above by a knife-edge contact with the Romney shale and less sharply limited below by the Lewistown limestone. The Helderberg is included in the upper part of the Lewistown limestone. The Lewistown is a thick formation and divisible into several units. The upper member is named the Longdale limestone. This latter has two lithologic phases—a flint-free limestone above, with a thickness of 30 to 50 feet, and a flinty limestone below, with a thickness of 20 to 30 feet. Below the Longdale there is a member named the Craigsville limestone. This is a pure limestone and has a thickness of about 150 feet.

The Longdale limestone is correlated with the Becraft, and the Craigsville limestone is correlated with the New Scotland.

Presented by title by request of the author.

SIGNIFICANCE OF THE MIDDLE SILURIC IN AMERICAN AND EUROPEAN GEOLOGY

BY AMADEUS W. GRABAU

(Abstract)

The classification of the Siluric is considered, and the reasons for regarding the Salina as the only American representative of the Middle Siluric are given. The status of the Shawangunk conglomerate is considered and the evidence for an extensive hiatus between the Salina and the Bertie, which latter is of Upper Monwan age, is discussed. In Europe, only Lower and Upper Siluric beds are known corresponding to the Niagaran and the Monwan respectively of the American classification. The Middle Siluric is everywhere represented by a hiatus.

Presented without notes.

GEOLOGY OF THE PORTSMOUTH BASIN, MAINE AND NEW HAMPSHIRE

BY ALFRED WANDKE [1]

(Abstract)

This basin is a downfaulted block bounded by gneisses of an unknown age, and consists of a series of fine-grained sediments, probably Upper Carboniferous in age, which are cut by sub-alkaline and alkaline intrusives. The easterly striking sediments, occurring in tight folds overturned to the southeast, have been grouped into two formations—a lower one, the Berwick gneiss, and an upper one, with the Kittery quartzite and Eliot phyllite as lower and upper members. The intrusives are made up of dikes which range in composition from diabase through to the alkaline varieties, such as the tinguaites and paisanites, and of subjacent bodies, which include both sub-alkaline and alkaline types. Elevated shorelines, delta deposits upon marine clays, and reworked glacial drift indicate a Quaternary marine transgression. Submerged tree stumps indicate a relatively recent drowning of the shoreline.

Presented with lantern slide illustrations, without notes.
Discussed by Prof. C. W. Brown.

PALEOZOIC SECTIONS SOUTH OF JAMES BAY

BY MERTON Y. WILLIAMS

(Abstract)

The Paleozoic outcrops exposed in the beds of the Mattagami, Abitibi, and Moose rivers indicate the presence of red shale and basal arkose, which probably represent the Queenston shale and older beds of the Ordovician system; shales (red and gray, with interbedded dolomites) and gypsum, evidently of

[1] Introduced by R. A. Daly.

Salina age (Silurian), and limestones and gray and black shales, with thin limestones near top and bottom, which are to be correlated with the Onondaga limestone, the Tully limestone, the Huron shale, and the Portage shale, of the Devonian system of New York State. Thin sheets and narrow dikes of trap cut the Onondaga and older sediments.

Presented by title in the absence of the author.

ARRANGEMENTS FOR ADDITIONAL GEOLOGICAL SURVEYS IN THE WEST INDIES

BY THOMAS WAYLAND VAUGHAN

(Abstract)

The paper gave a comprehensive statement regarding the arrangements now in force for geological surveys of the Dominican and Haitian republics.

Read from manuscript.

GEOLOGICAL RECONNAISSANCE OF THE DOMINICAN REPUBLIC

BY CHARLES WYTHE COOKE

Presented by title in the absence of the author.

At the second section formed for the afternoon—E. W. Shaw, chairman, and C. N. Fenner, secretary—the following program was offered:

DIFFERENTIATION BY DEFORMATION

BY N. L. BOWEN

(Abstract)

The problem was raised of the possible effects of deformation of an igneous mass during crystallization. Suggestions were made as to how such action may *aid* in the production of mono-mineralic masses of extreme purity with particular reference to anorthosites. The possibility was considered that some peridotite dikes may be merely constricted parts of basaltic dikes, and that composite dikes are a likely complementary manifestation. The origin of primary banding, of border facies, and of the so-called Atlantic and Pacific families of igneous rocks was considered in this connection.

Presented without notes.

Discussed by Messrs. R. A. Daly, A. C. Lane, Whitman Cross, and J. E. Spurr.

GEOLOGY OF THE KATMAI REGION, ALASKA, AND THE GREAT ERUPTION OF 1912

BY CLARENCE N. FENNER

(Abstract)

During the summer of 1919 a party of three from the Geophysical Laboratory cooperated with the expedition sent by the National Geographic Society to the Katmai region, and made geological and chemical studies of the phenomena connected with the great eruption of a few years ago and of the fumarolic activity still going on. These studies have not yet been fully worked up and only a preliminary report can be made, but an account was given of some of the more interesting geological features. This includes the general geology of the district and the arrangement of the volcanic chain; the nature of the lava-masses which have built up the volcanoes as contrasted with the material blown out recently; the so-called mud-flow in the Valley of Ten Thousand Smokes and its probable origin; fumarolic activity; the extruded plug of Novarupta; the present form of the crater of Katmai and its relation to the great eruption; the stratified ash which covers the region.

Read from manuscript, with lantern slide illustration.

Discussed by Messrs. J. E. Spurr, Lawrence Martin, and A. H. Brooks.

ANORTHOSITE-GABBRO IN NORTHERN NEW YORK

BY WILLIAM J. MILLER

(Abstract)

A body of Precambrian anorthosite-gabbro 4½ miles long, in Saint Lawrence County, New York, exhibits a number of features of special interest. Much of the rock is a normal dark medium-grained to coarse-grained massive gabbro, but it commonly varies to anorthosite-gabbro, and even to nearly white anorthosite, with only 5 to 1 per cent of dark minerals. From facies with no foliation there are all gradations into those which are very highly foliated. The variants in composition and structure are mostly in zones or bands more or less sharply separated from each other. All of the principal variations in composition, structure, and texture may be observed within a distance of a few rods.

One zone of the anorthosite-gabbro, a rod wide and about 200 feet long, is highly fractured by curving cracks, which are filled in part by dikes of nearly pure plagioclase anorthosite and in part by dikes of hornblendite. These filled cracks show a remarkable festooned arrangement.

Dikes of many kinds, with and without sharp contacts, cut the anorthosite-gabbro. These include white dikes consisting chiefly of plagioclase and scapolite, hornblendite, hornblende-rich gabbro, pegmatite (acidic to basic), and granite.

The gabbro is surrounded mostly by a younger granite, and along the borders between the two there are some fine displays of injection gneisses. Dikes of pure pyroxenite also cut this bordering granite.

In this paper the origin and significance of the above mentioned features of the anorthosite-gabbro are discussed,

Read from manuscript, with lantern slide illustration.

Discussed by Messrs. N. L. Bowen, C. N. Fenner, and W. G. Foyé.

EXPERIMENTS ILLUSTRATING DEVELOPMENT OF IGNEOUS ROCK TEXTURES

BY FRED E. WRIGHT

(*Abstract*)

The crystallization of certain low melting organic compounds, such as thymol, menthol, guiacol, benzophenon, and others, can be readily studied under the petrographic microscope during the cooling down of the melted mixtures. The order of crystallization of the several components, the crystallization of the eutectic mixtures, and other attendant phenomena can be witnessed in process of formation. Experiments of this kind, which illustrate the development of textures analogous to those found in rocks, should be of educational value.

Presented by title by request of the author.

FELDSPARS AS INDICATORS OF SEDIMENTARY OR IGNEOUS ORIGIN OF GNEISSES AND SCHISTS[1]

BY EDWARD STEIDTMANN

CONTENTS

Facts on which the Use of Feldspars as Indicators of the sedimentary or igneous Origin of the parent Materials of Gneisses and Schists is based

Feldspars appear to have value as indicators of the sedimentary or igneous origin of the parent materials of certain gneisses and schists, in view of the following considerations:

In an igneous rock the plagioclase feldspars represent a narrow range of composition. Acid and basic plagioclase feldspars rarely occur intermingled in an igneous rock, excepting in certain zonal growths. Orthoclase generally occurs with acid plagioclase, rarely with basic varieties. The feldspars of sediments, however, are not controlled by the laws governing those of igneous rocks. Mixture of all kinds of feldspars are possible in sedimentary, but not in igneous rocks. So generally recognized is this law of association of feldspars in igneous rocks that it forms the basis of one of the best known methods

[1] Manuscript received by the Secretary of the Society December 19, 1919.

of the microscopic determination of plagioclase in igneous rocks, namely, the statistical method of finding the maximum equal extinction angle in the zone perpendicular to 010. This method is based on the assumption that the plagioclase feldspars of a slide are all of the same composition.

It is reasonable to assume that the law of association of feldspars characteristic of igneous rocks also holds true of most schists or gneisses derived from igneous rocks, from considerations which follow. The feldspars of a schist or gneiss derived from igneous rocks may have the following sources:

(*a*) Primary crystallization from a magma—the case of primary gneisses.

(*b*) Recrystallization, granulation, slicing, and rotation of the primary feldspathic material of the igneous rocks—processes which leave the composition of the feldspars the same as in the original igneous rocks.

Dynamic metamorphism commonly causes a loss of lime, but this does not necessarily imply a change in the composition of the feldspars, but a change in the lime-bearing ferromagnesian minerals to those containing less lime, such as the change from augite to amphibole or biotite. In the saussurization of basic igneous rocks, however, to quartz, albite, or sodic micas, all stages of this decomposition are generally visible. In some cases the dynamic metamorphism of acid igneous rocks destroys the feldspars, one of the principal products of decomposition being mica.

(*c*) Recrystallization of the weathered igneous rock. The feldspars in the recrystallized portion would be chiefly residual feldspars. The weathered material is generally deficient in bases; hence its recrystallization would probably fail to produce feldspars. In the case of basic igneous rocks, carbonation is sometimes an important process of weathering. It is, therefore, conceivable that recrystallization of the weathered basic igneous rock might produce limy feldspars. Their occurrence, however, would be no radical violation of the law of association of feldspars characteristic of igneous rocks, since the primary feldspars of such rocks are basic. It is conceivable that their relation to carbonates and their distribution might indicate their secondary nature. Regional weathering rarely extends to a depth of more than 100 feet; hence its relation to the unweathered portions might be observed in the field. As yet, no schists derived from weathered igneous rocks have been positively identified; hence this proposition is only of theoretical interest.

(*d*) Recrystallization of hydrothermally altered igneous rocks. In a broad way, the results of hydrothermal alteration are similar to weathering. Such changes generally destroy the feldspars. The feldspars present in the schist and gneisses derived from them may be (1) residual, (2) recrystallized from the altered product, or (3) may have been introduced by the thermal solutions. It is conceivable that the feldspars due to introduction or recrystallization may violate the law of association of igneous rocks. Their secondary nature, however, might be recognizable. As in the preceding cases, schists and gneisses of this type have not as yet been identified.

The source and characteristics of the feldspars of gneisses and schists of sedimentary origin are chiefly as follows:

1. They may represent the primary feldspars of feldspathic sediments—recrystallized, granulated, sliced, and rotated. If the sediments were derived from a single kind of igneous rock, the feldspars present will show the same

laws of association as the feldspars of an igneous rock. Extensive feldspathic sedimentary deposits are generally derived from many kinds of rocks; hence their feldspars can not be expected to show the same characteristic association as those of igneous rocks.

2. Recrystallization of non-feldspathic materials, muds, etcetera. If original material was uniform in composition, it is probable that the feldspars developed may show the limited range of composition of those which occur in igneous rocks. The contrary, however, is to be expected in most cases.

3. The feldspars may have been introduced by magmatic or other thermal solutions before schistification. This may or may not cause a violation of the rules governing the association of feldspars in igneous rocks. Such an introduction may be expected to be local rather than regional. Its local secondary character might be recognized if looked for.

How Feldspars may be used as Indicators of the parent Materials of Gneisses and Schists

The conclusions to be drawn from the preceding general considerations are:

1. Schists and gneisses whose feldspars show the same laws of association as those of igneous rocks are probably of igneous origin. The conclusion is strengthened if the rocks in question are of great areal extent and thickness.

2. Schists and gneisses whose feldspars do not show the laws of association peculiar to igneous rocks are almost certainly of sedimentary origin. Great areal extent and thickness of the rocks in question add to the validity of the conclusion.

Detailed studies of thin sections made for the purpose of testing out the use of feldspars as a method of distinguishing the sedimentary or igneous origin of schists and gneisses have been in progress by the writer for some time and are sufficiently advanced to warrant the belief that they may prove valuable in some cases. Obviously, the statistical method of identifying the plagioclase feldspars can not be used in this study, since it is based on the assumption that the composition of the plagioclase feldspars of a slide is uniform. Instead the method of measuring the angle between the optic plane and the trace of 010 in sections perpendicular to X or Z has been found to be a very useful method.

Application of the Feldspar Test to the Coutchiching Schist south of Rainy Lake, Minnesota

One of the rocks which the writer has studied in considerable detail by this method is a fine to medium grained gray gneiss or schist which maintains a remarkably uniform habit over hundreds of square miles between Lake Vermilion and Rainy Lake of northern Minnesota. On Rainy Lake the planes of schistosity of this material dip to the northward under Keewatin greenstones, into which it appears to grade, the gradational phase being no more than about 10 feet wide. Both greenstone and the gray schists are intruded by an Archean granite batholith whose outcrops are more conspicuous in the schist area than in that of the greenstones. The schist area presents such a mixture of gray schist and intrusive granite that it is difficult to map them separately; yet there is no evidence of marked interaction between the gray schist and granite. Lawson has mapped this gray schist as Coutchiching along the Minnesota coast and believes that it is of sedimentary origin, chiefly because of its banded character. The writer's statements apply only to Lawson's Coutchiching of Minnesota and not to his Coutchiching in general.

The banding is generally faint, being due to a slight concentration of small brown biotite flakes. Rarely it consists of seams rich in brown biotite up to one-half inch thick. Sometimes the schistosity of these bands is diagonal to the banding—a trait which would seem to indicate that the banding antecedes the schistosity.

One of the outstanding characteristics of the rock is its uniformity of grain, mineral composition, and general field appearance. The mineral constituents are uniformly brown biotite, orthoclase, oligoclase, and quartz, the schistosity being due mainly to the parallelism of the mica. The concentration of the mica in parallel laminæ is the cause of both the faint and prominent gneissic structure. The ratio of feldspar to quartz is about the same as in a typical granite.

The writer believes that this rock is of igneous origin, because of its uniformity over great areas and because its feldspars conform rigidly to the law of association characteristic of the feldspars of igneous rocks. It is suggested that the banding, where prominent, may be due to an original tuffaceous or to a rhyolitic flow strucure. Some of the samples taken from this formation near the contact of the Keewatin greenstones on Rainy Lake are undoubted rhyolites. The fineness of grain of the schists as a whole suggests that originally they were rhyolitic, glassy, felsitic, tuffaceous rocks.

Presented by title in the absence of the author.

TECTONIC CONDITIONS ACCOMPANYING INTRUSION OF BASIC AND ULTRA-BASIC IGNEOUS ROCKS

BY W. N. BENSON

(Abstract)

The investigation of the great serpentine belt of New South Wales revealed to the writer a mass of ultrabasic rock extending intermittently for over two hundred miles, with a breadth of generally only a small fraction of a mile. It occupied a well marked zone of faulting and its intrusion was clearly dependent on the movements which determined also the principal structures of the region. In the consideration of this relationship the writer determined to put to an independent test Suess' generalization, that the "green rocks form sills in dislocated mountains that sometimes follow the bedding planes and sometimes the planes of movement." For this purpose he has made a study of the geological history of every region containing ultrabasic rocks throughout the world the literature of which was accessible to him. He has also had the opportunity of examining on the field numerous occurrences in Great Britain, in Europe, and in New Zealand, of examining the petrological collec-

tions of many workers on these rocks, and of personal discussion with them of the problems involved. The work became enlarged during its progress by the addition to it of the consideration of rocks classed in the spilitic suite by Messrs. Flett and Dewey and other rock types not considered by Suess among the green rocks.

Considering all these occurrences, there appear to be certain types of conditions of development so frequently repeated that they seem to correspond to fundamental processes in the development of igneous rocks. We may provisionally classify these as follows:

1. The complexes of a generally laccolitic form (sometimes lopolithic) represented by the Duluth, Sudbury, and Bushveld complexes. The extended studies of the last of these seem to render difficult of application the view that they result simply from the gravitational differentiation of a magma intruded at a single act, and to favor instead the view that they result from a succession of intrusions of increasing acidity, extending over a long period of time. These are developed where the lateral pressure is not great and the successive fractions invaded are not geographically separated from one another to any great extent.

2. In the Cordilleras, where lateral pressure is more noteworthy, the separation of the successive fractions is a marked feature, and the more usually concordant character of the basic and ultrabasic intrusions, as contrasted with the strongly transgressive granitic intrusions, is clear. The microscopic strucures of the basic and ultrabasic portions of such complexes do not seem to lend support to the suggestion that the rocks did not crystallize from a magma of the composition of the rock which they now form, but were a collection of sunken crystals, with just sufficient magma mingled with them to allow of viscous flow. Rather they seem to be successive intrusions of diverse magmas, the differentiation having occurred, not in the upper portion of the crust, but in some deep-seated reservoir below. The phenomena of hybrid rocks occur between closely succeeding intrusions of diverse composition.

3. In Alpine regions, where the effects of lateral thrust are very great indeed, the sill form becomes most marked in the more basic intrusions, which may occur in the forward thrust portion of great overthrust foldings. Here occur what Steinmann has termed ophiolitic rocks, but the writer does not concur with Steinmann in his conception of them. After a detailed consideration, he is of the opinion that they consist in part of flows and shallow intrusions of a somewhat spilitic nature in the folded strata, and in part of intrusions that occurred during the subsequent period of overthrust, and were injected in large measure along the plane of overthrusting, and may indeed have locally extruded from the front of the thrust-plane, to be subsequently overridden by the advancing crust-flake, under which the continued flow of magma produced coarse-grained intrusive rocks. In the absence of detailed information concerning the peculiar association of serpentines, gabbros, basalts, and agglomerates in the Cretaceous folded rocks that surround the massif of peninsular India, it is impossible at present to state the relationship of these intrusions to the green rocks of the more folded Alpine regions. The

absence of any noticeable evidence of dikes or necks of basic rocks traversing the structures below the planes in which these Alpine green rocks are found makes it difficult to consider them as flows that have been poured out from fissures or vents near where they are now found. All these rocks, with the exception of those of spilitic characters, are more or less free from any alkaline characters. Even in the case of the nepheline-syenites of the Pilandsberg, the former assumption of consanguinity between these and the rocks of the Bushveld complex is weakened, though not completely disproved, by the more recent studies.

These are the types of eruptive rocks produced in periods of some regional orogenic activity, of which the first series, the laccolitic, shows the least evidence of lateral pressures. This leads to a fourth group, that of the great dolerite-sills, such as those of the Karoo, in which there is little or no evidence of crust movement accompanying the intrusion. In these sills gravitational differentiation is sometimes in evidence, though rare, as in the case of the Palisades on the Hudson and the laccolitic enlargements of the Karoo sills in northeast Cape Colony. In such rocks there is no evidence of alkaline characters. In certain regions, particularly in the Western Isles of Scotland, extensive fissure eruptions of basaltic rocks which have very slight alkaline characters (shown by the alkaline character of the zeolites and the occasional presence of nepheline-bearing rocks and ouachitite) have been strongly folded about centers of plutonic intrusion where a succession of strongly calcic olivine-bearing rocks and gabbros with granophyre and granite have invaded the basalts. These show no clear evidence of differentiation *in situ* gravitationally, for the granite frequently lies beneath the basic rocks which it invades. A regional series of intrusions of basalt dikes and dolerite sills into the basalts closed the igneous activity.

On the other hand, associations of this type lead us to more alkaline occurrences, of which several divisions may be recognized. The stable regions of the earth's crust, which have not been subjected to such subsequent folding (which are continental areas rather than geosynclinal), such as the Bohemian Mittelgebirge and other plateau areas, have been the site of the development of rocks of the essexitic-theralitic sequence, among which the picrites form a series of basic sills. Not infrequently such extrusion was associated with block-faulting, but not with folding. The rocks of the spilitic series, as defined by Flett and Dewey, are also developed in areas in which there was an absence of folding forces during the period of eruption. They occurred, however, in geosynclinal areas that were undergoing steady subsidence in the period prior to an orogenic movement. Studies of thick albitized sills and of the large masses of associated albitic rocks (keratophyres) made by the writer and others do not support the hypothesis that their features result from the action of resurgent or even juvenile water, concentrating the alkalies in the upper portion of the magmas, when in their present position, but indicate a composition for the immediate parent magma of spilites and keratophyre more sodic than that of normal basalts, though the possible effectiveness of resurgent water in adding to the pneumatolytic activity of such intrusions is not excluded. In the development of rocks of this group shallow intrusions of picrite are frequently present, and sometimes, as, for example, in the Devonian igneous series in northwest Germany, and perhaps in the fourchite of Arkansas, the characters of the spilitic and essexitic-theralitic divisions of alkaline rocks are somewhat intermingled. In general, however, these rocks, formed in geosynclines, have been subjected to subsequent folding and are greatly disturbed. The history of the region subsequent to the development of the igneous rocks, by showing the nature of the crust conditions in the region, geosynclinal or continental, is permissible in some degree as evidence of the tectonic conditions at the period of the intrusion of the rocks.

A final group is the very uniform series of mica-peridotite dikes of alnoitic characters that occur in the regions west of the Appalachian Mountains and in the Indian coal fields, and are represented by melilite-basalts in South Africa. These are probably a very special group of dikes forming under conditions allied to those which accompanied the formation of essexitic-theralitic or foyaitic rocks (with which they seem to be associated in Arkansas)—that is, they were formed in practically unfolded regions, with perhaps fracturing and faulting of the crust, but not tangential pressure.

This general survey leads to the conclusion that those later processes giving rise to the evolution of igneous rocks have in all probability a much less important effect in those solidified magma basins, which are now exposed by erosion, than they had in the deeper subcrustal reservoirs, in which the physico-chemical processes described by Smyth and Bowen, together with some gravitational differentiation emphasized by Daly, may have been very effective; but the writer concurs with Dr. Harker in urging the effectiveness of lateral pressure in the separations of the magma, the portions richer in alkalies, and generally those portions which under Bowen's scheme would form the later consolidations from the magma, being moved toward the direction of least lateral pressure.

In regions where there is no great lateral pressure over broad areas the plutonic rocks are generally the normal sub-alkaline series of gabbro-granitic rocks, but where lateral pressures are pronounced, there is a tendency for the region where the pressure is least to become enriched in alkalies. The total volume of alkaline rocks, as Professor Daly has shown, is so small in comparison with the rocks that do not show alkaline features that it is not possible to distinguish with any certainty the products of a normal gabbro magma from those of a magma rather impoverished in alkalines which have been strained off toward areas of local enrichment in alkalies. Hence the gabbros and granites of the laccolitic complexes are indistinguishable chemically from those of the Cordilleran and Alpine types. This straining of the magma in response to lateral pressure, leading to lateral differentation, may be either of a regional or of a local character. In the former case, the characteristics of the petrographic provinces produced may be more or less clear, but in the latter the differentiation may be incomplete and lead to no great geographical separation as a whole, though locally specialized types may occur, thus causing development of those complexes of associated alkaline and sub-alkaline rocks in which the affinities of the complex and of some of its members can not be sharply defined into one or other of the two groups which, in Dr. Harker's phrase, are the petrographical poles toward which differentiation tends. The existence of regions of incomplete petrographical specialization, which are sometimes considered to be evidence against the truth of the conception of the tectonic control of the primary differentiation of the original (possibly basaltic) magma into the secondary magmas of special petrographical provinces, appears to the writer to be, on the contrary, a necessary product of such primary differentiation.

In the present paper, however, the writer has not laid the stress on this conclusion as to general petrogenesis and the major acts of differentiation such as appears in this abstract, but in the absence of an intensive study of the occurrence of the granites and alkaline syenites, etcetera, he has refrained from discussing at length such conclusions as are here indicated, though the evidence of the basic intrusive rocks, which alone has been studied in detail, appears to point in this direction. The bulk of the paper is given up to a review, country by country, throughout the world, of the occurrence of basic and ultrabasic rocks, as it appears in the literature available, and it is offered with the hope that it may serve, when associated with the detailed study of the conditions during the development of other types of rocks, in forwarding the knowledge of petrogenesis, which has been so advanced by the illuminating generalizations put forward during the last thirty years.

Presented by title in the absence of the author.

MICHIGAN METEOR OF NOVEMBER 26, 1919

BY WILLIAM HERBERT HOBBS

Presented by title in the absence of the author.

ANNUAL DINNER

The evening of Tuesday, December 30, was devoted to the annual subscription dinner of the Geological Society of America and the Paleontological Society and their guests, in which there were 156 participants. President John C. Merriam acted as toastmaster, and addresses were made by Messrs. I. C. White, Frank D. Adams, D. W. Johnson, James F. Kemp, A. H. Brooks, R. A. Daly, John M. Clarke, Lawrence Martin, G. D. Louderback, and H. S. Washington.

Session of Wednesday Morning, December 31

The Society, meeting in joint session with the Paleontological Society, was called to order at 9.20 o'clock a. m. by President John C. Merriam. The Secretary informed the body that the Council, having learned of the financial straits in which the *Geological Magazine* of England found itself by reason of conditions arising from the war, had voted to underwrite forty new subscriptions to the famous periodical for the year 1920, relying upon the Society to take them up.

STANDARD OF FELLOWSHIP

Acting under instructions from the Council, the Secretary read the following minute as its interpretation of Article III, paragraph 1, of the Constitution:

Whereas questions have been raised concerning standards of eligibility for Fellowship in the Geological Society of America, be it

Resolved, That it is the sense of the Council that the Society should carefully adhere to uniform standards of eligibility. The Constitution states that "Fellows shall be persons who are engaged in geologic work or in the teaching of geology," and that "the object of this Society shall be promotion of the science of geology in America." The Council believes that there should be no discrimination against a man engaged in commercial geology, but instead if he shows scientific spirit and constructive ability—if he contributes and will presumably continue to contribute toward the advance of the science, he is eligible for Fellowship. In accordance with present aims and the established policy, the attempt should be made to discriminate solely on the basis of scientific fruitfulness—on contribution to geology. The skillful application of geology to economic ends and the mere assembling and presentation of facts without suggestions as to their meaning are not regarded as important contributions to the science, and this applies to men engaged in all branches of geology. The Society desires men who build rather than those who merely accumulate building material.

President Merriam then called Vice-President H. E. Gregory to the chair to preside over the joint symposium on the teaching of geology and paleontology.

SYMPOSIUM ON THE TEACHING OF GEOLOGY AND PALEONTOLOGY

After introductory remarks by the chairman the following papers were read:

TEACHING OF HISTORICAL GEOLOGY AND PALEONTOLOGY AS FUNDAMENTAL TO HISTORY, THE HUMANITIES AND SCIENCE

BY JOHN C. MERRIAM

PETROLOGY AND STRUCTURAL GEOLOGY

BY JAMES F. KEMP

President Merriam resumed the chair.

PHYSIOGRAPHY AND GENERAL GEOLOGY

BY HERBERT E. GREGORY

AMERICAN PALEONTOLOGISTS AND THE IMMEDIATE FUTURE OF PALEONTOLOGY

BY CHARLES SCHUCHERT

The four following papers, comprising a symposium on the teaching of geology, are to be printed in full in the Bulletin:

GENERAL TEACHING PROBLEM OF PALEONTOLOGY

BY HERDMAN F. CLELAND

TEACHING OF PALEOBOTANY

BY EDWARD W. BERRY

TEACHING OF INVERTEBRATE PALEONTOLOGY

BY STUART WELLER

Presented by title in the absence of the author.

VALUE AND USE OF STAGES OF DEVELOPMENT IN TEACHING

BY ROBERT T. JACKSON

Presented by title.

The subject was discussed further by Messrs C. W. Brown, J. M. Clarke, W. M. Davis, G. F. Kay, A. C. Lane, and E. B. Mathews.

On motion, it was voted that the Society recommend the Council to appoint a Standing Committee on Geological Instruction, with the object of extending and improving the teaching of geology.

VOTE OF THANKS

A hearty vote of thanks to the Geological Society of Boston, the Massachusetts Institute of Technology, and the Boston Society of Natural History was passed in recognition of hospitalities extended and facilities furnished in connection with the meeting.

Adjournment for luncheon was taken at 12.40 o'clock p. m.

TITLES AND ABSTRACTS OF PAPERS PRESENTED BEFORE THE AFTERNOON SESSIONS AND DISCUSSIONS THEREON

The afternoon was devoted by the Council to a business meeting, while the Society met in two sections for the consideration of papers.

One section, under the chairmanship of I. C. White, gave consideration to the following papers:

CLIFT ISLANDS IN THE CORAL SEAS

BY W. M. DAVIS

(*Abstract*)

The Marquesas Islands, Tutuila, in Samoa, and Tahiti, in the Society group, are all of volcanic origin and all have strongly clift shores; but as the cliffs are interrupted by embayed valleys and are fronted by lagoon floors, or platforms, at a depth of 20 or more fathoms, it follows that the cliffs were cut back by waves during the same time of higher stand of the islands as that in which the new embayed valleys were eroded by their streams. The clift islands of the Marquesas group are exceptional in being free from reefs, although nearer the equator than the abundant atolls of the Paumotus, which stand about 300 miles to the southwest.

Tutuila is fringed by a rather strong reef, but the outer edge of its remarkable platform is not surmounted by a barrier reef. Tahiti has a barrier reef as well as fringing reefs, and its embayments are nearly all filled with delta flats, while the embayments of the other islands are not filled. It is inferred from these contrasts that the date of submergence of the islands can not have been contemporaneous, and hence that they were submerged by local subsidence and not by a universal rise of ocean level.

Presented without notes.

Discussed by Prof. R. A. Daly, with reply by author.

EROSIVE CLUES TO THE HIGH PLATEAUX OF UTAH

BY CHARLES R. KEYES

(*Abstract*)

When the high plateaux of eastern Utah were first made subject of especial description, the possibility of a definite geographic cycle in land sculpture was one of the earth conceptions not yet even faintly adumbrated. Omission of such consideration of fundamental importance necessarily led to curious aberration in the treatment of the phenomena presented.

Following closely the rather unscientific Powellian policy of geological saisissement, Dutton entirely missed the larger physiographic significance of his basic observations on the prodigious amount of erosion which the Utah region had manifestly undergone in relatively recent geologic times. He easily fell into speculation along other lines.

Since the elevations of the high plateaux are about the same as those of the crest of the Rockies, the two are often closely compared. The terre pleins of the plateaux and the summit plain of the Cordillera are both about 12,000 feet above the sea. The usual inference is that through epeirogenic uplift the

tops of all belong to the same former baselevel of erosion. There are now ample evidences indicating that these two highland plains are not separated parts of a once continuous peneplain.

It appears that the terre pleins of the high plateaux and of the Mesa de Maya, on the other side of the Cordillera, are widely removed tracts of the same Miocene peneplain which once doubtless extended completely over the Rocky Mountain region. The summit plain of the Rockies, however, seems to be much older—Comanchean in date. It is continually made fresh, because it is being rapidly exhumed by the peeling off of the Dakotan sandstone which everywhere reposes upon it.

Presented by title in the absence of the author.

MOUNTAIN PEDIMENTS: A DISCUSSION OF THE EROSION OF DESERT RANGES

BY KIRK BRYAN [1]

(*Abstract*)

Many of the mountains in the Papago country of southwestern Arizona are bordered by plains cut in rock which superficially resemble alluvial slopes. These plains are eroded by weathering in place and by corrasion of running water. An analysis of the processes of erosion of mountain slopes and of the rock plains at their bases is presented. The term pediment, with a series of qualifying adjectives, is proposed for the rock plains. The present state of dissection of most of the pediment areas is described and present conditions of erosion compared to those of the past. Criteria are suggested for interpretation of the history of desert valleys characterized by pediments on the borders.

Presented without notes.

Discussed by Prof. W. M. Davis and Dr. J. L. Rich, with reply by author.

SOME FEATURES OF STREAM DEVELOPMENT AND OF GLACIATION IN THE CATSKILL MOUNTAINS

BY H. E. MERWIN

(*Abstract*)

The capture by the Schoharie of upper branches of the Delaware which drained the unsymmetrical valleys of the northeastern Catskills was considered. An explanation of the "cloves" and deep notches was offered. Observations concerning the height of glaciation were given, and certain glacial deposits were described with reference to local glaciation.

Discussed by Dr. J. L. Rich and Prof. W. M. Davis, with reply by the author.

[1] Introduced by H. E. Gregory.

UNICLINE: A TERM PROPOSED FOR MONOCLINAL RIDGES OF EROSION

BY AMADEUS W. GRABAU

(*Abstract*)

The term monocline is used at present in two ways: The geologist understands by monocline a simple structural fold. This is the original and proper use of the term. The physiographer has used the term for the ridges produced by erosion of anticlines—that is, for "monoclinal ridges of erosion," including hog-backs. For this type of ridge, a part of an anticline, the term *unicline* is proposed.

Discussed by Dr. F. F. Hintze and Prof. W. M. Davis.

The second section was called to order at 2.20 o'clock by A. P. Coleman, acting as chairman. The following program was offered:

USE OF GEOLOGY ON THE WESTERN FRONT

BY ALFRED H. BROOKS

(*Abstract*)

The application of geology to military problems first developed during the World War. The British were the first to make use of geology in determining the water supply for army use, while the Germans appear to have been the first to recognize the need of geologic knowledge in military mining. During 1916 geologic staffs were organized in both the British and German armies. That of the British Expeditionary Force included four geologists, but their work was supplemented by that of a large number of officers of the mining troops who were more or less trained in geology. It is not yet definitely known how many geologists were employed professionally in the German army, but there were probably over a hundred. There were no geologic officers in the French army, but some of the French engineers, notably those in the water-supply service, made use of geology. In September, 1918, a geologist was attached to the staff of the chief engineer of the Belgian army. No information is at hand regarding the use made of geology by the Italian or Austrian armies.

In September, 1917, a geologic section was established as a part of the staff of the chief engineer of the American Expeditionary Force. At the close of hostilities there were eight geologic officers with the American army and authorization had been granted for eight more.

The principal applications of geology to military problems as developed during the war are as follows:

1. *Field fortifications* (trenches, dugouts, and mines).—Depth and permeability of soil and subsoil; physical character of bedrock; depth of ground water.

2. *Maneuvering of infantry, artillery, and tanks.*—Physical character of soil and subsoil during wet and dry seasons and after heavy shelling; character of streams with reference to fords and bridges; descriptions of topography and map interpretation.

3. *Water supply.*—Resources of surface and underground water.

4. *Transportation.*—Road metal, railroad ballast, stability of talus slopes; depth of soil and weathered material; river crossings, etcetera.

5. *Construction.*—Raw materials for concrete, building stones, etcetera; stability of slopes; depth to bedrock, etcetera.

Presented without manuscript.

Discussed by Messrs. Lawrence Martin, Frank D. Adams, and H. F. Cleland.

Discussion

Prof. Lawrence Martin pointed out that the continuation of interest in geology on the part of the American army administration may be maintained by pointing out (*a*) the excellent work of Colonel Brooks and his assistants in France; (*b*) the mistakes made during the war by all the Allies, through failure to use geologists adequately from the beginning, and (*c*) the extent to which the enemy found geologists indispensable.

Following the period when the Germans located water at Saint Mihiel with a witchhazel twig, they increased their geological service until in and following January, 1918, each of their 13 armies on the western front is believed to have maintained 22 geological workers. Each Armee Oberkommando had a *Geologen Gruppe*, with a geologist (commissioned officer) as chief. Each of the three general Kommandos, or army corps, had *Geologenstelle* with one geologist (officer or beamter) as chief and three geologengehilfen. Each of the three divisions in a corps had one geologengehilfe. Thus a German army had four geologists and 18 assistants. Some of the assistants were geological students from the German universities; some were teachers or technical men; toward the end they were trained in special six-week courses in military geology.

SURVEY OF ROAD MATERIALS IN NEW HAMPSHIRE

BY JAMES WALTER GOLDTHWAIT

(*Abstract*)

Few States make use of such diverse materials in road construction as New Hampshire, because of (*a*) the widely differing physical conditions in different parts of the State—particularly climate, grades of roads, and ground water; (*b*) extreme differences in traffic, both as regards the amount and the kind; and (*c*) the great variety of materials locally available, owing to the diversity of bedrock geology and to glaciation.

While economy and efficiency demand, on the one hand, an increased use of native materials, the policy of the State Highway Department, which aims to build roads of a fairly permanent character, calls for great care in the choice of the material, or of two or more materials which may be successfully combined. In order to discover what road materials are available, in New Hampshire, and how they are distributed, the writer was engaged by the State Highway Commissioner to make a geological reconnaissance of the State, and to supplement this with such detailed local studies as the field-work or

important construction projects might suggest. This was done during the summers of 1917 and 1919, in close cooperation with (*a*) the several division engineers of the department, whose experience with materials has afforded many practical suggestions and problems for research, and (*b*) the testing engineer of the State Highway Laboratory, who is studying the composition and physical properties of several hundred samples of rock, gravel, till, sand, and clay collected to represent types from all parts of the State.

The object of this paper is to indicate some of the ways in which such a survey, when combined with laboratory study, enables the highway engineer to judge whether or not he may expect to find suitable material near the project, and when several different kinds of material are available, how to chose among them.

Presented without manuscript.

CARBONIFEROUS SALT AND POTASH DEPOSITS OF EASTERN CANADA

BY ALBERT O. HAYES

(*Abstract*)

Stratified salt deposits of early Mississippian (Windsor) age occur at Malagast, Cumberland County, Nova Scotia. The salt occurs as stratified sedimentary deposits, and was proved in 1918 by a shaft to lie at 85 feet depth, and drilling operation indicated a thickness of several hundred feet. The shaft and mine workings have already proved a commercial body of salt; also an interbanded zone of sylvite and other potash-bearing minerals which promise to be of commercial value.

A revision of the geology of the Carboniferous system in New Brunswick indicates the Windsor horizon to underlie large areas hitherto supposed to belong to an older series.

Presented by title in the absence of the author.

NATURAL GAS DECLINE CURVE

BY ROSWELL H. JOHNSON

(*Abstract*)

The natural gas decline curves which have so far been published are decline curves of pressure and have ordinarily been of single wells. In the meanwhile there have been published an extensive collection of oil-well decline curves and there has been considerable speculation as to the formula of these curves. It seems desirable, therefore, that a natural gas volume decline curve should be published.

There are a number of difficulties that have so far deterred the construction of these curves:

1. Gas wells are never allowed to produce freely, and their decline curve, therefore, is largely determined by the history of the back pressures to which the wells have been subjected.

2. There are seasonal variations of the extent to which the well is drawn upon.

3. There is interference from later drilled wells.

4. It is difficult to get periodical volume determinations.

In spite of these difficulties, however, the accompanying curve is presented. It was constructed by the segmental method, namely, determining the rate of decline at different parts of the curve by the rate shown in wells of various sizes, assuming the validity for practical purposes of Lewis and Beal's "Law of Equal Expectations."

The curve is based on wells in Cowanshanock Township, Armstrong County, Pennsylvania.

The curve is shown drawn on quadrillé paper and also on logarithmic and arithlog paper, to indicate the nature of the curve. It is to be seen that the curve approaches more nearly the petroleum curve than might have been expected, from the pressure curves that have heretofore been published.

Mr. W. I. Moyer and Mrs. Fisher Bossler have cooperated in the construction of these curves.

Presented by title in the absence of the author.

QUANTITATIVE METHODS OF ESTIMATING GROUND-WATER SUPPLIES

BY OSCAR E. MEINZER

(*Abstract*)

This paper relates only to ground water, or phreatic water—that is, water in the zone of saturation. It is not concerned with the water that occurs above the water-table. It relates not to the quantities of water stored in the earth, but to the rate of replenishment of the ground-water supply, on which conservative developments must be based.

Four principal groups of methods are used to determine the annual recharge or "safe yield" of ground water: The *intake, discharge, water-table,* and *underflow* methods. The first of these consists in measuring the quantity of surface water that seeps into the earth and percolates into the zone of saturation; the second in measuring the ground water that is discharged through springs or by evaporation from soil and plants; the third in observing the fluctuations in the water-table, which represent filling or emptying of the ground-water reservoir; the fourth, like the gaging of surface streams, in measuring the flow of ground water at selected cross-sections. In arid regions plants of certain species habitually utilize water from the zone of saturation. For such plants the name phreatophyte, meaning a "well plant," is proposed. The water-table methods are best adapted to regions, such as California, which have well defined rainy and dry seasons. The three principal methods—intake, discharge, and water-table—are entirely independent of each other and can be used as checks upon one another.

Presented by title in the absence of the author.

SOME RESULTS OF DEEP DRILLING IN THE APPALACHIAN OIL AND GAS FIELDS

BY I. C. WHITE

(*Abstract*)

A discussion of the stratigraphic, geologic, temperature, and other results obtained by deep-well drilling in Pennsylvania and West Virginia, and also giving summary records of two of the deepest wells in the world (7,386 and 7,579 feet) from West Virginia and two very deep wells (7,248 feet and still drilling) in Pennsylvania.

Presented without manuscript.

Discussed by Prof. A. C. Lane, who said in part:

Prof. A. C. Lane: These wells add information from the economic geologist that we could not otherwise obtain. For instance, the rate of increase of temperature, the geothermal gradient, seems somewhat greater at the greater depths. This may be accounted for by varying diffusivity of strata and in other ways, but if a general phenomenon, it suggests that the mean surface temperature has increased since the last ice age, and that the temperature wave thereby started down has not reached a depth of 7,000 feet.

GEOLOGY OF THE ORE DEPOSITS OF KENNECOTT, ALASKA

BY ALAN M. BATEMAN

(*Abstract*)

The unique copper deposits of Kennecott, Alaska, present many peculiar and interesting features, and the origin of the great masses of chalcocite has long been a puzzle. Their deciphering involves the origin of fractures unusual in form; of a peculiar kind of primary mineralization unparalleled in other deposits; of a source of metals not customarily considered, as well as agents of transportation but seldom referred to. No conclusions can be reached without carefully weighing the primary or secondary origin of the chalcocite, and much of interest is added regarding the distribution of oxidation and ground water. It is with these problems that the paper deals, and the salient facts and conclusions will be briefly presented.

The ore deposits occur in the Upper Triassic Chitistone limestone, which conformably overlies the basaltic lava-flows comprising the Nikolai greenstone, of presumed Triassic age. The formations have been folded into a gently pitching major anticline, one flank of which has been eroded, and the other crinkled, faulted, and fractured. In these fractures circulated warmed solutions from which copper minerals were deposited, partly as cavity fillings, but largely as massive replacement of the limestone. No gangue minerals were introduced and the walls are unaltered. Aside from the dolomite limestone country rock, the ore consists almost entirely of the elements, copper and sulphur, with traces of iron, arsenic, and silica. Oxidation has produced

copper carbonate. The sulphide minerals are chiefly chalocite and covellite, with traces of bornite.

The copper is believed to have been derived from the greenstone by warmed circulating waters, perhaps of meteoric origin. The chalcocite was, with the exception of a little covellite, the latest sulphide to be deposited. It is considered to have been formed from the warm solutions and to be primary in origin.

At a later period oxidizing surface penetrated the ore bodies beyond the lowest workings, a maximum distance of 1,500 feet beneath the surface, and converted part of the copper sulphides into copper carbonate. The oxidation of the sulphide was pervasive and feeble; never absent, yet nowhere complete.

The advent of the Glacial Period arrested the deep oxidation by the freezing of the oxidizing waters, and they have remained frozen until this day. Thus, the oxidation was preglacial and the present ground water is a frozen one.

Postglacial oxidation has been negligible, but mechanical disintegration of the ore cropping during this period has been sufficiently vigorous to produce a sulphide talus-slope ore body and an ore body of broken sulphide debris in a glacier, which is now about to be mined.

Presented in abstract from notes.

CONDITIONS AT VESUVIUS IN 1919

BY HENRY S. WASHINGTON

Presented without manuscript.

SWEET GRASS HILLS, MONTANA

BY JAMES F. KEMP AND PAUL BILLINGSLY

(Abstract)

The Sweet Grass Hills are the most northerly of the laccolithic mountain groups lying east of the Rocky Mountains in Montana. They are the only remaining group whose petrography has not been worked up in considerable detail. They were the object of a hasty visit by Dr. George M. Dawson in the seventies, when the region was a dangerous Indian country. Specimens then collected were determined shortly after by F. D. Adams, and fifteen years or more later three specimens of special interest were described by W. H. Weed and L. V. Pirsson. The present writers spent a week together in the hills in May, 1918, reviewing earlier work by the junior writer, and have been aided by further details from George M. Fowler, a geologist, resident for a year in the region, in charge of oil exploration. The petrographic details have been subsequently worked out by the senior writer.

The three separate and striking groups of laccoliths constituting the three buttes are described and illustrated and their relations to the sedimentary rocks are shown. Excellent sections of the sedimentaries to the depth of 2,000 feet have been afforded by four wells sunk for oil. The laccolithic centers are surrounded by sills and dikes in great numbers.

There is notable range in variety in the laccoliths, but the rocks are, on the

whole, intermediate between typical trachytes and andesites, as recognized by F. D. Adams from a few specimens in 1876. Among the dikes and sills there is a tendency to shade off toward the basalts without developing typical and markedly basic types. A greatly contrasted variety, less frequently seen in both sills and dikes, is the minette, already described by Weed and Pirsson from a specimen gathered by Dr. Dawson from an outlying dike to the north of East Butte. One sill of minette is packed with inclusions of Precambrian rocks brought up from a great depth. One or two pipes have afforded still different varieties of igneous rocks. Contact zones have been developed by the main laccolith of East Butte from the Madison limestone of the Carboniferous with attendant deposits of magnetic and specular iron ore.

Presented without manuscript by the senior author.

CUMBERLAND FALLS, KENTUCKY, METEORITE

BY ARTHUR M. MILLER

(Abstract)

The paper describes the phenomena and incidents connected with the meteoric fall in McCreary County, Kentucky, near the Falls of the Cumberland, on the McCreary-Whitley County line, at midday, on April 9, 1919.

Recognizing that the event chronicled in the local newspapers of southeastern Kentucky as an earthquake was a falling meteorite, the writer immediately took steps to obtain information concerning it with a view to determining its trajectory and obtaining as much of the meteorite itself as possible. The methods employed in this investigation are recounted. A summary of the facts obtained is as follows:

The meteor in its fall was seen or heard throughout an area of about 22,700 square miles, extending from Neubert, 12 miles south of Knoxville, Tennessee, on the south, to Lexington, Kentucky, on the north; and from Rogersville, Tennessee, on the east, to Jamestown, Kentucky, on the west. In that part of this region lying in Tennessee the sky was clear and the meteor, shining with a light which exceeded in brightness that of the sun, was seen descending toward the northwest. In that portion of Kentucky near where it fell the sky was cloudy, and the inhabitants were advised of its presence only by the deafening sounds produced by the concussions. Farther north in the area the sky was again clear, and people both saw the descending body in the south and heard the detonations as it neared the earth. The horizontal component in its trajectory is computed to be north 31 degrees west. In its passage over Kentucky till it fell it paralleled on the east side the line of the Queen and Crescent Railroad.

In this stretch the signal-tower men along the line of railroad heralded its coming ahead by telephone, after the manner of keeping track of an "extra." At Coal Creek, Tennessee, a little east of south of Cumberland Falls and 45 miles distant, a telegraph operator saw it disappear to the northwest at 12.21 p. m. (daylight-saving schedule time for this meridian). At Tatesville, Kentucky, about 13 miles northwest of Cumberland Falls, it was heard by a towerman at 12.27. The telegraph operator at Danville, 50 miles north of

Tatesville and 58 miles northwest from Cumberland Falls, heard the sounds three minutes later. Professor Downing, of the Department of Astronomy, University of Kentucky, at Lexington, Kentucky, heard the sounds at 12.35, and immediately after felt the building tremble. Lexington is 85 miles distant from Cumberland Falls.

All the pieces recovered to date fell at Sawyer Post-Office, in McCreary County, longitude 84 degrees 20 minutes, latitude 36 degrees 53 minutes. They number 50 in all, ranging in size from less than an ounce up to 5¼ pounds. The total weight is about 52 pounds. It is the opinion of Mr. W. H. Morgan, postmaster at Sawyer, through whom all the pieces were obtained, that the main mass went on farther north. In this the writer concurs, and he predicts that if this main mass is ever found, which is very unlikely, on account of the ruggedness and unsettled condition of the country, it will be in that portion of Pulaski County a little to the north of the Cumberland River and a short distance below the mouth of the Rockcastle River. Cumberland Falls is close to Sawyer Post-Office. On account of its being a well known place, it is proposed as a proper designation for this meteoric fall.

Presented from notes.

CUMBERLAND FALLS, KENTUCKY, METEORITE

BY GEORGE P. MERRILL

(Abstract)

The paper refers but briefly to the phenomena of the fall, which have been described sufficiently by Prof. A. M. Miller, and dwells particularly on the composition and structure of the stone, which is a breccia composed of fragments of a coarse, light gray enstatite mingled with those of a dark gray chondritic material. The coarseness of crystallization of the original stone and the present structural peculiarities are accounted for on the supposition that the detrital matter, derived from the disintegration of previously consolidated rock-masses of at least two distinct types, accumulated on the surface like an ordinary terrestrial volcanic tuff or breccia. Subsequently the beds were deeply buried, and through crustal movement compressed into their present condition. This supposition carries with it the supposition that this meteorite is but a spawl from a very much larger mass, one of sufficient size to have been subject to such crustal movements as are incidental to mountain-making and which find their terrestrial counterpart only in regions of great disturbance, as in our southern Appalachians. How large such a mass should be it is impossible to say. That it must have been of planetary dimensions is thought to be a safe assumption. In fact, that the fragments are direct evidence of the destruction of some preexisting planet is regarded as a legitimate conclusion.

Presented without manuscript, with the aid of lantern-slide illustrations.

Discussed by F. R. Van Horn.

PHENOL RED INDICATOR AS AN AID TO THE GEOLOGIST

BY EDGAR T. WHERRY

(*Abstract*)

Phenol red is a brilliant indicator, used in the chemical laboratory for testing reaction. It is yellow when acid, orange-yellow at the neutral point, and violet-red when alkaline; solutions of calcium bicarbonate, and even of calcium carbonate, produce the alkaline color. It can be carried into the field in dry form and dissolved in water as needed, and may prove useful to the geologist in the following ways: Demonstrating that a given natural water is "hard" or "soft," the former condition reacting alkaline, the latter acid, toward the indicator; this may enable the source of waters to be determined and help in tracing contacts, fault-lines, etcetera. Ascertaining whether a given rock is calcareous or not, a little rock powder is scraped off with a knife or rock fragment and shaken in water, when, if the rock is calcareous, enough will dissolve to affect the indicator and produce the alkaline color.

Presented with illustrative experiment.

THE TERNARY SYSTEM $Fe_2O_3 — SO_3 — H_2O$

BY H. E. MERWIN AND E. POSNJAK

(*Abstract*)

The formation of hematite, of the hydrated ferric oxides, of several ferric sulphates which occur as minerals, and of other precipitates not definitely crystalline, and variable in composition like certain natural materials, are considered.

Presented by the senior author without manuscript, with lantern-slide illustrations.

BEARING OF EXPERIMENTAL CHEMICAL DATA ON THE FORMATION OF SMITHSONITE

BY THOMAS L. WATSON

(*Abstract*)

Smithsonite, which corresponds to the normal carbonate of zinc and is one of several oxidized ores of the metal, is an important ore mineral in zinc deposits of certain localities, both in this country and abroad. The mineral is most abundant in limestones and dolomites and their residual decayed products. It is usually associated with other oxidized ores of zinc and some other metals, especially iron and lead, and to some extent copper and silver. Smithsonite is a secondary mineral, regarded at present as having formed in the zone of oxidation by descending waters acting on the corresponding sulphide, sphalerite.

The present paper reviews the synthesis of zinc carbonate; discusses the conditions under which normal zinc carbonate (smithsonite) and the basic zinc carbonate (hydrozincite) are formed, as based on analytical chemical data, and discusses in light of the experimental data some of the more important districts in which zinc carbonate ores occur. That limestones and dolomites hydrolyze, the former more readily than the latter because of its greater solubility, is well known; also that normal carbonates of the alkalies and alkali earth metals produce basic zinc carbonate when added to a solution of zinc sulphate, while the corresponding bicarbonates produce normal zinc carbonate. It has been further shown that excess CO_2 changes the basic carbonate of zinc into the normal carbonate. These reactions are in agreement with the field investigations by Loughlin of some of the oxidized zinc ores in metalliferous deposits of the western United States. The conclusion is reached that the proportion of CO_2 present or its bicarbonate radical equivalent is the controlling factor in determining which one of the two zinc carbonates will form—smithsonite or hydrozincite.

Presented by title in the absence of the author.

CONTACT-METAMORPHIC DEPOSIT AT THE MOUNTAIN LAKE MINE NEAR SALT LAKE CITY, UTAH

BY AUSTIN F. ROGERS

(*Abstract*)

A detailed petrographic study of a copper-bearing forsterite-magnetite zone at the contact between a granodiorite and limestone. Microscopic examination of thin sections and polished surfaces proves that the minerals were formed in stages one after the other. The sequence of the minerals furnishes a fairly complete history of the deposit. The appearance of tremolite and antigorite and in other deposits sericite, chlorite, talc, and anthophyllite at a late hydrothermal stage is emphasized, for then we have criteria by which the hypogene ore minerals can be distinguished from the supergene ore minerals. Microscopic investigation of the ores of smaller mines and prospects is very important on account of the frequent absence of geological data.

Presented without manuscript, with lantern-slide illustrations.

NEW AND RARE MINERALS FORMED IN LIMESTONE BY CONTACT METAMORPHISM

BY ARTHUR S. EAKLE

(*Abstract*)

The crystalline limestone deposit at Crestmore, Riverside County, California, is one of the best and most remarkable examples of localized metamorphism known. In a single quarry there have been exposed, almost daily, zones or bands of new associations of minerals, consisting of new and rare species in

the blue calcite and mixed with the abundant green vesuvianite. Tons of these associations can be collected at the time of their exposure, but the company has paid no attention to the scientific value of the minerals, and consequently much instructive material has disappeared completely, since the metamorphism of the limestone of this quarry shows such a localization that it is seldom that the same association is encountered a second time.

Metamorphism of the whole mass of limestone and remetamorphism of the northern half of the deposit has taken place, and there appear to have been three general agents of metamorphism. The intrusion of the granodiorite which underlies the limestone hills changed the original beds into marble, and this is seen in the white crystalline marble of the south hill. Following this intrusion came dikes of monzonite and coarse pegmatite, which intruded the deposit in its northern half only and brought about a recrystallization of the marble and formation of such contact minerals as vesuvianite and garnet. These dike intrusions were accompanied or followed by ascending hydrothermal solutions, and it is due to these solutions that most of the rare or new minerals have been formed. Some of these new minerals are still under investigation.

Presented by title.

Register of the Boston Meeting, 1919

Adams, F. D.
Anderson, Robert
Ashley, G. H.
Barton, G. H.
Bassler, R. S.
Bateman, A. M.
Blackwelder, E.
Boutwell, J. M.
Bowen, N. L.
Brooks, A. H.
Brown, Chas. W.
Burling, L. D.
Burwash, E. M.
Chadwick, G. H.
Clarke, J. M.
Cleland, H. F.
Cobb, Collier
Coleman, A. P.
Cooke, C. Wythe
Crosby, W. O.
Cross, Whitman
Daly, R. A.
Davis, W. M.
De Golyer, E.
Eakle, Arthur S.
Fenner, C. N.
Foerste, A. F.
Ford, W. E.
Goldthwait, J. W.
Gordon, C. E.
Grabau, A. W.
Granger, Walter
Graton, L. C.
Gregory, H. E.
Hartnagel, C. A.
Hewett, D. F.
Hintze, F. F.
Hobbs, W. H.
Hovey, E. O.
Hunt, Walter F.

Huntington, E.
Hyde, J. E.
Iddings, J. P.
Jackson, Robert T.
Johnson, D. W.
Johnson, R. H.
Kay, G. F.
Kemp, J. F.
Kindle, E. M.
Knight, C. W.
Kraus, Edward H.
Kümmel, H. B.
Kunz, G. F.
Lane, Alfred C.
Lee, W. T.
Little, H. P.
Loomis, F. B.
Louderback, George D.
Lull, R. S.
Mansfield, G. R.
Martin, G. C.
Martin, Lawrence
Mathews, Edward B.
Merriam, John C.
Merrill, G. P.
Merwin, H. E.
Miller, A. M.
Miller, B. L.
Miller, W. J.
Miser, H. D.
Moore, E. S.
Palache, Charles
Penrose, R. A. F., Jr.
Perkins, G. H.
Perry, J. H.
Phillips, A. H.
Price, W. Armstrong
Raymond, P. E.
Rice, W. N.
Rich, J. L.
Richardson, C. H.
Rogers, A. F.
Sayles, R. W.
Schuchert, Charles
Shaw, E. W.
Shimer, H. W.
Sinclair, W. J.
Spurr, J. E.
Stoller, J. H.
Stose, G. W.
Talbot, Mignon
Twenhofel, W. H.
Twitchell, M. W.
Ulrich, E. O.
Van Horn, F. R.
Van Ingen, Gilbert
Vaughan, T. W.
Walker, T. L.
Warren, Charles H.
Washington, H. S.
Westgate, L. W.
Wherry, E. T.
White, I. C.
Wolff, J. E.
Woodman, J. M.
Woodworth, J. B.
Wright, Charles W.
Wright, Fred E.

Fellows-elect

Foye, Wilbur G.
Washburne, Chester W.

There were also 104 visitors who registered.

OFFICERS, CORRESPONDENTS, AND FELLOWS OF THE GEOLOGICAL SOCIETY OF AMERICA

OFFICERS FOR 1920

President:

I. C. White, Morgantown, W. Va.

Vice-Presidents:

George P. Merrill, Washington, D. C.
Willet G. Miller, Toronto, Canada
F. B. Loomis, Amherst, Mass.

Secretary:

Edmund Otis Hovey, American Museum of Natural History, New York, N. Y.

Treasurer:

Edward B. Mathews, Johns Hopkins University, Baltimore, Md.

Editor:

J. Stanley-Brown, 26 Exchange Place, New York, N. Y.

Councilors:

(Term expires 1920)

Herbert E. Gregory,[1] New Haven, Conn.
R. A. Daly, Cambridge, Mass.

(Term expires 1921)

William S. Bayley, Urbana, Ill.
Eugene W. Shaw, Washington, D. C.

(Term expires 1922)

T. W. Vaughan, Washington, D. C.
George F. Kay, Iowa City, Iowa

[1] Elected by Council, December 31, 1919, to fill out the unexpired term of Joseph Barrell, deceased.

MEMBERSHIP, 1919

CORRESPONDENTS

Barrois, Charles, Lille, France. December, 1909.
Brögger, W. C., Christiania, Norway. December, 1909.
Capellini, Giovanni, Bologna, Italy. December, 1910.
De Geer, Baron Gerhard, Stockholm, Sweden. December, 1910.
Geikie, Sir Archibald, Hasslemere, England. December, 1909.
Heim, Albert, Zürich, Switzerland. December, 1909.
Kayser, Emanuel, Marburg, Germany. December, 1909.
Kilian, W., Grenoble, France. December, 1912.
Teall, J. J. H., London, England. December, 1912.
Tietze, Emil, Vienna, Austria. December, 1910.

FELLOWS

* Indicates Original Fellows (see article III of Constitution)

Abbe, Cleveland, Jr., 563 Eldert Lane, Brooklyn, N. Y. August, 1899.
Adams, Frank Dawson, McGill University, Montreal, Canada. Dec., 1889.
Adams, George I., 17 San T'iao Hutung, Peking, China. December, 1902.
Alden, William C., U. S. Geological Survey, Washington, D. C. Dec., 1909.
Aldrich, Truman H., 1026 Glen Iris Ave., Birmingham, Ala. May, 1889.
Allan, John A., University of Alberta, Strathcona, Canada. December, 1914.
Allen, R. C., Rockefeller Building, Cleveland, Ohio. December, 1911.
Ami, Henry M., Strathcona Park, Ottawa, Canada. December, 1889.
Anderson, Frank M., State Mining Bureau, 2604 Aetna St., Berkeley, Calif. December, 1902.
Anderson, Robert V., 47 Parliament St., London, S. W., England. Dec., 1911.
Arnold, Ralph, 923 Union Oil Building, Los Angeles, Calif. December, 1904.
Ashley, George Hall, State Capitol, Harrisburg, Pa. August, 1895.
Atwood, Wallace Walter, Harvard University, Cambridge, Mass. Dec., 1909.
Bagg, Rufus Mather, Jr., 7 Brokaw Place, Appleton, Wis. December, 1896.
Bain, H. Foster, care Thos. Cook & Son, Rangoon, Burma. December, 1895.
Baker, Manley Benson, School of Mining, Kingston, Ontario. Dec., 1911.
Baldwin, S. Prentiss, 2930 Prospect Ave., Cleveland, Ohio. August, 1895.
Ball, Sydney H., 71 Broadway, New York City. December, 1905.
Bancroft, Joseph A., McGill University, Montreal, Canada. December, 1914.
Barbour, Erwin Hinckley, University of Nebraska, Lincoln, Neb. Dec., 1896.
Barton, George H., Boston Society of Natural History, Boston, Mass. August, 1890.
Bartsch, Paul, U. S. National Museum, Washington, D. C. December, 1917.
Bascom, Florence, Bryn Mawr College, Bryn Mawr, Pa. August, 1894.
Bassler, Ray Smith, U. S. National Museum, Washington, D. C. Dec., 1906.
Bastin, Edson S., U. S. Geological Survey, Washington, D. C. Dec., 1909.
Bateman, Alan Mara, Yale University, New Haven, Conn. December, 1916.
Bayley, William S., University of Illinois, Urbana, Ill. December, 1888.
Beede, Joshua W., 404 West 38th St., Austin, Texas. December, 1902.
Benson, W. N., University of Otago, Dunedin, New Zealand. Dec., 1919.
Berkey, Charles P., Columbia University, New York, N. Y. August, 1901.

BERRY, EDWARD WILBER, Johns Hopkins University, Baltimore, Md. Dec., 1909.
BEYER, SAMUEL WALKER, Iowa Agricultural College, Ames, Iowa. Dec., 1896.
BLACKWELDER, ELIOT, 317 Railway Exchange Bldg., Denver, Colo. Dec., 1908.
BLISS, ELEANORA F., U. S. Geological Survey, Washington, D. C. Dec., 1919.
BOUTWELL, JOHN M., 1323 De la Vine St., Santa Barbara, Calif. Dec., 1905.
BOWEN, CHARLES F., U. S. Geological Survey, Washington, D. C. Dec., 1916.
BOWEN, N. L., Queen's University, Kingston, Ont., Canada. December, 1917.
BOWIE, WILLIAM, U. S. Coast and Geodetic Survey, Washington, D. C. December, 1919.
BOWNOCKER, JOHN ADAMS, Ohio State University, Columbus, Ohio. Dec., 1904.
*BRANNER, JOHN C., Leland Stanford, Jr., University, Stanford Univ., Calif.
BRANSON, EDWIN BAYER, University of Missouri, Columbia, Mo. Dec., 1911.
BRETZ, J. H., University of Chicago, Chicago, Ill. December, 1917.
BRIGHAM, ALBERT PERRY, Colgate University, Hamilton, N. Y. December, 1893.
BROCK, REGINALD W., Univ. of British Columbia, Vancouver, B. C. Dec., 1904.
BROOKS, ALFRED HULSE, U. S. Geological Survey, Washington, D. C. Aug., 1899.
BROWN, BARNUM, American Museum of Natural History, New York, N. Y. December, 1910.
BROWN, CHARLES WILSON, Brown University, Providence, R. I. Dec., 1908.
BROWN, THOMAS CLACHAR, Laurel Bank Farm, Fitchburg, Mass. Dec., 1915.
BUDDINGTON, A. F., Brown University, Providence, R. I. December, 1919.
BUEHLER, HENRY ANDREW, Rolla, Mo. December, 1909.
BURLING, LANCASTER D., Geological Survey of Canada, Ottawa, Canada. December, 1917.
BURWASH, EDWARD M. J., University of Manitoba, Winnipeg, Canada. December, 1916.
BUTLER, BERT S., U. S. Geological Survey, Washington, D. C. December, 1912.
BUTLER, G. MONTAGUE, College of Mines, Tucson, Arizona. December, 1911.
BUTTS, CHARLES, U. S. Geological Survey, Washington, D. C. December, 1912.
CALHOUN, FRED HARVEY HALL, Clemson College, S. C. December, 1909.
CALKIN, FRANK C., U. S. Geological Survey, Washington, D. C. Dec., 1914.
CAMPBELL, HENRY D., Washington and Lee Univ., Lexington, Va. May, 1889.
CAMPBELL, MARIUS R., U. S. Geological Survey, Washington, D. C. Aug., 1892.
CAMPOS, LUIZ FILIPPE G. DE, Geological Survey of Brazil, Rio de Janeiro, Brazil. December, 1917.
CAMSELL, CHARLES, Geological Survey, British Columbia Branch, 510 Pacific Building, Vancouver, B. C. December, 1914.
CAPPS, STEPHEN R., JR., U. S. Geological Survey, Washington, D. C. Dec., 1911.
CARMAN, J. ERNEST, Ohio State University, Columbus, Ohio. December, 1917.
CARNEY, FRANK, 208 S. Chatauqua St., Wichita, Kans. December, 1908.
CASE, ERMINE C., University of Michigan, Ann Arbor, Mich. December, 1901.
CHADWICK, GEORGE H., University of Rochester, Rochester, N. Y. Dec., 1911.
CHAMBERLIN, ROLLIN T., University of Chicago, Chicago, Ill. December, 1913.
*CHAMBERLIN, T. C., University of Chicago, Chicago, Ill.
CLAGHORN, CLARENCE RAYMOND, 22 Dover Road, Wellesley, Mass. Aug., 1891.
CLAPP, CHARLES H., Montana School of Mines, Butte, Mont. December, 1914.
CLAPP, FREDERICK G., 120 Broadway, New York, N. Y. December, 1905.
CLARK, BRUCE L., Bacon Hall, Univ. of California, Berkeley, Calif. Dec., 1918.
CLARK, F. R., U. S. Geological Survey, Washington, D. C. December, 1919.
CLARKE, JOHN MASON, Albany, N. Y. December, 1897.

CLELAND, HERDMAN F., Williams College, Williamstown, Mass. Dec., 1905.
CLEMENTS, J. MORGAN, 20 Broad St., New York City. December, 1894.
COBB, COLLIER, University of North Carolina, Chapel Hill, N. C. Dec., 1894.
COLEMAN, ARTHUR P., Toronto University, Toronto, Canada. December, 1896.
COLLIE, GEORGE L., Beloit College, Beloit, Wis. December, 1897.
COLLIER, ARTHUR J., U. S. Geological Survey, Washington, D. C. June, 1902.
CONDIT, D. DALE, U. S. Geological Survey, Washington, D. C. December, 1911.
COOK, CHARLES W., University of Michigan, Ann Arbor, Mich. Dec., 1915.
COOKE, C. WYTHE, U. S. Geological Survey, Washington, D. C. Dec., 1918.
COSTE, EUGENE, 2208 Amherst St., Calgary, Alberta, Canada. Dec., 1906.
CRAWFORD, RALPH DIXON, 1050 Tenth St., Boulder, Colo. December, 1916.
CROOK, ALJA R., State Museum of Natural History, Springfield, Ill. Dec., 1898.
*CROSBY, WILLIAM O., Massachusetts Institute of Technology, Boston, Mass.
CROSS, WHITMAN, U. S. Geological Survey, Washington, D. C. May, 1889.
CULVER, GARRY E., 310 Center Ave., Stevens Point, Wis. December, 1891.
CUMINGS, EDGAR R., Indiana University, Bloomington, Ind. August, 1901.
*CUSHING, HENRY P., Western Reserve University, Cleveland, Ohio.
CUSHMAN, J. A., Sharon, Mass. December, 1919.
DALY, REGINALD A., Harvard University, Cambridge, Mass. December, 1905.
DANA, EDWARD SALISBURY, Yale University, New Haven, Conn. Dec., 1908.
*DARTON, NELSON H., U. S. Geological Survey, Washington, D. C.
*DAVIS, WILLIAM M., 31 Hawthorne St., Cambridge, Mass.
DAY, ARTHUR LOUIS, Geophysical Laboratory, Washington, D. C. Dec., 1909.
DAY, DAVID T., 1333 F St. N. W., Washington, D. C. August, 1891.
DEAN, BASHFORD, Columbia University, New York, N. Y. December, 1910.
DE GOLYER, E. L., 65 Broadway, New York City. December, 1918.
DEUSSEN, ALEXANDER, 504 Stewart Bldg., Houston, Texas. December, 1916.
DE WOLF, FRANK WILBRIDGE, Urbana, Ill. December, 1909.
DICKERSON, ROY E., 114 Burnett Ave., San Francisco, Calif. December, 1918.
*DILLER, JOSEPH S., U. S. Geological Survey, Washington, D. C.
D'INVILLIERS, EDWARD V., 518 Walnut St., Philadelphia, Pa. December, 1888.
DODGE, RICHARD E., Dodge Farm, Washington, Conn. August, 1897.
DRAKE, NOAH FIELDS, Fayetteville, Arkansas. December, 1898.
DRESSER, JOHN A., 701 Eastern Townships Bank Bldg., Montreal, Canada. December, 1906.
*DUMBLE, EDWIN T., 2003 Main St., Houston, Texas.
EAKLE, ARTHUR S., University of California, Berkeley, Calif. December, 1899.
ECKEL, EDWIN C., Munsey Building, Washington, D. C. December, 1905.
EMERY, WILSON B., Casper, Wyoming. December, 1919.
*EMERSON, BENJAMIN K., 152 Crescent Ave., Leonia, N. J.
EMMONS, WILLIAM H., Univ. of Minnesota, Minneapolis, Minn. Dec., 1912.
*FAIRCHILD, HERMAN L., University of Rochester, Rochester, N. Y.
FARRINGTON, OLIVER C., Field Museum of Natural History, Chicago, Ill. December, 1895.
FENNEMAN, NEVIN M., University of Cincinnati, Cincinnati, Ohio. Dec., 1904.
FENNER, CLARENCE N., Geophysical Laboratory, Washington, D. C. Dec., 1911.
FISHER, CASSIUS ASA, 705 First Natl. Bank Bldg., Denver, Colo. Dec., 1908.
FOERSTE, AUGUST F., 129 Wroe Ave., Dayton, Ohio. December, 1899.
FORD, WILLIAM E., Sheffield Scientific School, New Haven, Conn. Dec., 1915.
FOYE, W. G., Wesleyan University, Middletown, Conn. December, 1919.

Fuller, Myron L., 157 Spring St., Brockton, Mass. December, 1898.
Galpin, Sidney L., 630 Park Ave., Ames, Iowa. December, 1917.
Gane, Henry Stewart, Wonalancet, New Hampshire. December, 1896.
Gardner, James H., 626 Kennedy Building, Tulsa, Okla. December, 1911.
George, Russell D., University of Colorado, Boulder, Colo. December, 1906.
Gill, Adam Capen, Cornell University, Ithaca, N. Y. December, 1888.
Glenn, L. C., Vanderbilt University, Nashville, Tenn. June, 1900.
Goldman, Marcus Isaac, U. S. Geol. Survey, Washington, D. C. Dec., 1916.
Goldthwait, James Walter, Dartmouth College, Hanover, N. H. Dec., 1909
Gordon, Charles H., University Library, University of Tennessee, Knoxville, Tenn. August, 1893.
Gordon, Clarence E., Massachusetts Agricultural College, Amherst, Mass. December, 1913.
Gould, Charles N., 1218 Colcord Bldg., Oklahoma City, Okla. Dec., 1904.
Grabau, Amadeus W., Columbia University, New York, N. Y. December, 1898.
Granger, Walter, American Museum of Natural History, New York, N. Y. December, 1911.
Grant, Ulysses Sherman, Northwestern Univ., Evanston, Ill. Dec., 1890.
Grasty, John Sharshall, Box 458, Charlottesville, Va. December, 1911.
Graton, Louis C., War Industries Bldg., Washington, D. C. December, 1913.
Gregory, Herbert E., Yale University, New Haven, Conn. August, 1901.
Greene, Frank Cook, 30 North Yorktown St., Tulsa, Okla. December, 1917.
Grimsley, George P., 16 York Court, Baltimore, Md. August, 1895.
Grout, Frank F., University of Minnesota, Minneapolis, Minn. Dec., 1918.
Gurley, William F. E. R., University of Chicago, Chicago, Ill. Dec., 1914.
Halberstadt, Baird, Pottsville, Pa. December, 1909.
Hancock, E. T., U. S. Geological Survey, Washington, D. C. December, 1919.
Harder, E. C., 1111 Harrison Building, Philadelphia, Pa. December, 1918.
Harris, Gilbert D., Cornell University, Ithaca, N. Y. December, 1903.
Harrison, John Burchmore, Georgetown, British Guiana. June, 1902.
Hartnagel, Chris A., Education Building, Albany, N. Y. December, 1913.
Hastings, John B., 1480 High St., Denver, Colo. May, 1889.
*Haworth, Erasmus, University of Kansas, Lawrence, Kans.
Hayes, Albert O., Geological Survey of Canada, Ottawa, Canada. Dec., 1919.
Hennen, Ray V., West Virginia Geol. Survey, Morgantown, W. Va. Dec., 1914.
Hershey, Oscar H., Crocker Building, San Francisco, Calif. December, 1909.
Hewett, Donnel F., U. S. Geological Survey, Washington, D. C. Dec., 1916.
Hice, Richard R., Beaver, Pa. December, 1903.
*Hill, Robert T., 612 American Exchange Bldg., Dallas, Texas.
Hills, Richard C., Denver, Colo. August, 1894.
Hinds, Henry, Sinclair Oil and Gas Company, Tulsa, Okla. December, 1912.
Hintze, Ferdinand Friis, 580 Corona St., Denver, Colo. December, 1917.
Hobbs, William H., University of Michigan, Ann Arbor, Mich. August, 1891.
*Holbrook, Levi, P. O. Box 536, New York, N. Y.
Holden, Roy J., Virginia Polytechnic Institute, Blacksburg, Va. Dec., 1914.
Holland, William Jacob, Carnegie Museum, Pittsburgh, Pa. December, 1910.
Hollick, Arthur, N. Y. Botanical Garden, New York, N. Y. August, 1898.
Hopkins, O. B., U. S. Geological Survey, Washington, D. C. December, 1919.
Hopkins, Thomas C., Syracuse University, Syracuse, N. Y. December, 1894.

HOTCHKISS, WILLIAM OTIS, State Geological Survey, Madison, Wis. Dec., 1911.
*HOVEY, EDMUND OTIS, American Museum of Natural History, New York, N. Y.
HOWE, ERNEST, Litchfield, Conn. December, 1903.
HUBBARD, GEORGE D., Oberlin College, Oberlin, Ohio. December, 1914.
HUDSON, GEORGE H., Plattsburg Normal School, Plattsburg, N. Y. Dec., 1917.
HUNT, WALTER F., University of Michigan, Ann Arbor, Mich. December, 1914.
HUNTINGTON, ELLSWORTH, 222 Highland St., Milton, Mass. December, 1906.
HUSSAKOF, LOUIS, American Museum of Natural History, New York, N. Y. December, 1910.
HYDE, J. E., Western Reserve University, Cleveland, Ohio. December, 1916.
IDDINGS, JOSEPH P., Brinklow, Md. May, 1889.
JACKSON, A. WENDELL, 9 Desbrosses St., New York, N. Y. December, 1888.
JACKSON, ROBERT T., Peterborough, N. H. August, 1894.
JAGGAR, THOMAS AUGUSTUS, JR., Hawaiian Volcano Observatory, Territory of Hawaii, U. S. A. December, 1906.
JEFFERSON, MARK S. W., Michigan State Normal College, Ypsilanti, Mich. December, 1904.
JEFFREY, EDWARD C., Harvard University, Cambridge, Mass. December, 1914.
JOHANNSEN, ALBERT, University of Chicago, Chicago, Ill. December, 1908.
JOHNSON, B. L., U. S. Geological Survey, Washington, D. C. December, 1919.
JOHNSON, DOUGLAS WILSON, Columbia University, New York, N. Y. Dec., 1906.
JOHNSON, ROSWELL H., 306 State Hall, University of Pittsburgh, Pittsburgh, Pa. December, 1918.
JOHNSTON, WILLIAM ALFRED, Geological Survey, Ottawa, Canada. Dec., 1916.
KATZ, FRANK JAMES, U. S. Geological Survey, Washington, D. C. Dec., 1912.
KAY, GEORGE FREDERICK, State Univ. of Iowa, Iowa City, Iowa. Dec., 1908.
KEITH, ARTHUR, U. S. Geological Survey, Washington, D. C. May, 1889.
*KEMP, JAMES F., Columbia University, New York, N. Y.
KEYES, CHARLES ROLLIN, 944 Fifth St., Des Moines, Iowa. August, 1890.
KINDLE, EDWARD M., Victoria Memorial Museum, Ottawa, Canada. Dec., 1905.
KIRK, CHARLES T., Box 1592, Tulsa, Okla. December, 1915.
KIRK, EDWIN, U. S. Geological Survey, Washington, D. C. December, 1912.
KNIGHT, CYRIL WORKMAN, Toronto, Ontario, Canada. December, 1911.
KNOPF, ADOLPH, U. S. Geological Survey, Washington, D. C. December, 1911.
KNOWLTON, FRANK H., U. S. National Museum, Washington, D. C. May, 1889.
KRAUS, EDWARD HENRY, University of Michigan, Ann Arbor, Mich. June, 1902.
KÜMMEL, HENRY B., Trenton, N. J. December, 1895.
*KUNZ, GEORGE F., 401 Fifth Ave., New York, N. Y.
LAHEE, FREDERIC H., 4031 Holland Ave., Dallas, Texas. December, 1917.
LANDES, HENRY, University of Washington, University Station, Seattle, Wash. December, 1908.
LANE, ALFRED C., Tufts College, Mass. December, 1889.
LARSEN, ESPER S., JR., U. S. Geological Survey, Washington, D. C. Dec., 1914.
LAWSON, ANDREW C., University of California, Berkeley, Cal. May, 1889.
LEE, WILLIS THOMAS, U. S. Geological Survey, Washington, D. C. Dec., 1903.
LEES, JAMES H., Iowa Geological Survey, Des Moines, Iowa. December, 1914.
LEITH, CHARLES K., University of Wisconsin, Madison, Wis. Dec., 1902.
LEONARD, ARTHUR G., State University of North Dakota, Grand Forks, N. Dak. December, 1901.
LEVERETT, FRANK, Ann Arbor, Mich. August, 1890.

LEWIS, JOSEPH VOLNEY, Rutgers College, New Brunswick, N. J. Dec., 1906.
LIBBEY, WILLIAM, Princeton University, Princeton, N. J. August, 1899.
LINDGREN, WALDEMAR, Massachusetts Institute of Technology, Cambridge, Mass. August, 1890.
LISBOA, MIGUEL A. R., Caixa postal 829, Ave. Rio Branco 46-V, Rio de Janeiro, Brazil. December, 1913.
LITTLE, HOMER P., Colby College, Waterville, Maine. December, 1918.
LLOYD, E. R., U. S. Geological Survey, Washington, D. C. December, 1919.
LOGAN, WILLIAM N., Indiana University, Bloomington, Ind. December, 1917.
LOOMIS, FREDERICK BREWSTER, Amherst College, Amherst, Mass. Dec., 1909.
LOUDERBACK, GEORGE D., University of California, Berkeley, Cal. June, 1902.
LOUGHLIN, GERALD F., U. S. Geological Survey, Washington, D. C. Dec., 1916.
LOW, ALBERT P., Department of Mines, Ottawa, Canada. December, 1905.
LULL, RICHARD SWANN, Yale University, New Haven, Conn. December, 1909.
LUPTON, CHARLES T., 611 17th St., Denver, Colo. December, 1916.
MCCALLIE, SAMUEL WASHINGTON, Atlanta, Ga. December, 1909.
MCCASKEY, HIRAM D., Central Point, Oregon. December, 1904.
MCCONNELL, RICHARD G., Geological and Natural History Survey of Canada, Ottawa, Canada. May, 1889.
MACDONALD, DONALD F., Sinclair Oil Company of Louisiana, Incorporated, Shreveport, La. December, 1915.
MACFARLANE, JAMES RIEMAN, Woodland Road, Pittsburgh, Pa. August, 1891.
MCINNES, WILLIAM, Geological and Natural History Survey of Canada, Ottawa, Canada. May, 1889.
MCKELLAR, PETER, Fort William, Ontario, Canada. August, 1890.
MANSFIELD, GEORGE R., 2067 Park Rd., N. W., Washington, D. C. Dec., 1909.
MARBUT, CURTIS F., Bureau of Soils, Washington, D. C. August, 1897.
MARSTERS, VERNON F., 316 Rialto Bldg., Kansas City, Mo. August, 1892.
MARTIN, GEORGE C., U. S. Geological Survey, Washington, D. C. June, 1902.
MARTIN, LAWRENCE, University of Wisconsin, Madison, Wis. December, 1909.
MATHER, KIRTLEY F., Denison University, Granville, Ohio. December, 1918.
MATHEWS, EDWARD B., Johns Hopkins University, Baltimore, Md. Aug., 1895.
MATSON, GEORGE C., U. S. Geological Survey, Washington, D. C. Dec., 1918.
MATTHES, FRANCOIS E., U. S. Geol. Survey, Washington, D. C. Dec., 1914.
MATTHEW, W. D., American Museum of Natural History, New York, N. Y. December, 1903.
MAYNARD, THOMAS POOLE, 1622 D. Hurt Bldg., Atlanta, Ga. December, 1914.
MEAD, WARREN JUDSON, University of Wisconsin, Madison, Wis. Dec., 1916.
MEINZER, OSCAR E., U. S. Geological Survey, Washington, D. C. Dec., 1916.
MENDENHALL, WALTER C., U. S. Geol. Survey, Washington, D. C. June, 1902.
MERRIAM, JOHN C., University of California, Berkeley, Calif. August, 1895.
MERRILL, GEORGE P., U. S. National Museum, Washington, D. C. Dec., 1888.
MERWIN, HERBERT E., Geophysical Laboratory, Washington, D. C. Dec., 1914.
MILLER, ARTHUR M., State University of Kentucky, Lexington, Ky. Dec., 1897.
MILLER, BENJAMIN L., Lehigh University, South Bethlehem, Pa. Dec., 1904.
MILLER, WILLET G., Toronto, Canada. December, 1902.
MILLER, WILLIAM JOHN, Smith College, Northampton, Mass. December, 1909.
MISER, HUGH D., U. S. Geological Survey, Washington, D. C. December, 1916.
MOFFIT, FRED HOWARD, U. S. Geological Survey, Washington, D. C. Dec., 1912.
MOLENGRAAF, G. A. F., Technical High School, Delft, Holland. December, 1913.
MOORE, ELWOOD S., Pennsylvania State College, State College, Pa. Dec., 1911.

MUNN, MALCOLM JOHN, Clinton Bldg., Tulsa, Okla. December, 1909.
*NASON, FRANK L., West Haven, Conn.
NEWLAND, DAVID HALE, Albany, N. Y. December, 1906.
NEWSOM, JOHN F., Leland Stanford, Jr., University, Stanford University, Calif. December, 1899.
NOBLE, LEVI F., Valyermo, Calif. December, 1916.
NORTON, WILLIAM H., Cornell College, Mount Vernon, Iowa. December, 1895.
NORWOOD, CHARLES J., State University, Lexington, Ky. August, 1894.
O'CONNELL, MARJORIE, 1939 Daly Ave., New York, N. Y. December, 1919.
OGILVIE, IDA HELEN, Barnard College, Columbia University, New York, N. Y. December, 1906.
O'HARA, CLEOPHAS C., South Dakota School of Mines, Rapid City, S. Dak. December, 1904.
OHERN, DANIEL WEBSTER, 515 W. 14th St., Oklahoma City, Okla. Dec., 1911.
OLIVEIRA, E. P. DE, Geol. Survey of Brazil, Rio de Janeiro, Brazil. Dec., 1918.
OSBORN, HENRY F., American Museum of Natural History, New York, N. Y. August, 1894.
PACK, ROBERT W., American Exchange National Bank Bldg., Dallas, Texas. December, 1916.
PAIGE, SIDNEY, U. S. Geological Survey, Washington, D. C. December, 1911.
PALACHE, CHARLES, Harvard University, Cambridge, Mass. August, 1897.
PARKS, WILLIAM A., University of Toronto, Toronto, Canada. Dec., 1906.
*PATTON, HORACE B., 911 Foster Building, Denver, Colo.
PECK, FREDERICK B., Lafayette College, Easton, Pa. August, 1901.
PENROSE, RICHARD A. F., JR., 460 Bullitt Bldg., Philadelphia, Pa. May, 1889.
PERKINS, GEORGE H., University of Vermont, Burlington, Vt. June, 1902.
PERRY, JOSEPH H., 276 Highland St., Worcester, Mass. December, 1888.
PHALEN, WILLIAM C., U. S. Bureau of Mines, Washington, D. C. Dec., 1912.
PHILLIPS, ALEXANDER H., Princeton University, Princeton, N. J. Dec., 1914.
POGUE, JOSEPH E., 111 W. Washington St., Chicago, Ill. December, 1911.
PRATT, JOSEPH H., North Carolina Geol. Survey, Chapel Hill, N. C. Dec., 1898.
PRICE, WILLIAM A., JR., Johns Hopkins University, Baltimore, Md. Dec., 1916.
PRINDLE, LOUIS M., U. S. Geological Survey, Washington, D. C. Dec., 1912.
PROUTY, WILLIAM F., Univ. of North Carolina, Chapel Hill, N. C. Dec., 1911.
*PUMPELLY, RAPHAEL, Newport, R. I.
RANSOME, FREDERICK L., U. S. Geol. Survey, Washington, D. C. August, 1895.
RAYMOND, PERCY EDWARD, Museum of Comparative Zoölogy, Cambridge, Mass. December, 1907.
REEDS, CHESTER A., American Museum of Natural History, New York, N. Y. December, 1913.
REGER, DAVID B., Box 816, Morgantown, W. Va. December, 1918.
REID, HARRY FIELDING, Johns Hopkins University, Baltimore, Md. Dec., 1892.
REINECKE, LEOPOLD, 120 Broadway, New York City. December, 1916.
RICE, WILLIAM NORTH, Wesleyan University, Middletown, Conn. August, 1890.
RICH, JOHN LYON, Chamber of Commerce Bldg., Albuquerque, New Mexico. December, 1912.
RICHARDSON, CHARLES H., Syracuse University, Syracuse, N. Y. Dec., 1899.
RICHARDSON, GEORGE B., U. S. Geol. Survey, Washington, D. C. Dec., 1908.
RIES, HEINRICH, Cornell University, Ithaca, N. Y. December, 1893.

Riggs, Elmer S., Field Museum of Natural History, Chicago, Ill. Dec., 1911.
Robinson, Henry Hollister, Hopkins Hall, New Haven, Conn. Dec., 1916.
Roddy, H. J., State Normal School, Millersville, Pa. December, 1919.
Rogers, Austin F., Stanford University, Calif. December, 1918.
Rose, Bruce, care of Whitehall Petroleum Corporation, 47 Parliament St., S. W. I., London, England. December, 1916.
Rowe, Jesse Perry, University of Montana, Missoula, Mont. December, 1911.
Ruedemann, Rudolf, Albany, N. Y. December, 1905.
Rutledge, John Joseph, McAlester, Okla. December, 1911.
St. John, Orestes H., 1141 Twelfth St., San Diego, Calif. May, 1889.
Sales, Reno H., Anaconda Copper Mining Company, Butte, Mon. Dec., 1916.
Sayles, Robert Wilcox, Harvard University, Chestnut Hill, Mass. Dec., 1917.
*Salisbury, Rollin D., University of Chicago, Chicago, Ill.
Sardeson, Frederick W., Univ. of Minnesota, Minneapolis, Minn. Dec., 1892.
Savage, Thomas Edmund, University of Illinois, Urbana, Ill. December, 1907.
Schaller, Waldemar T., Great Southern Sulphur Co., Inc., New Orleans, La. December, 1918.
Schofield, S. J., Geological Survey of Canada, Ottawa, Canada. Dec., 1918.
Schrader, Frank C., U. S. Geological Survey, Washington, D. C. Aug., 1901.
Schuchert, Charles, Yale University, New Haven, Conn. August, 1895.
Schultz, Alfred R., Hudson, Wis. December, 1912.
Scott, William B., Princeton University, Princeton, N. J. August, 1892.
Seaman, Arthur E., Michigan College of Mines, Houghton, Mich. Dec., 1904.
Sellards, Elias H., University of Texas, Austin, Texas. December, 1905.
Shaler, Millard K., 66 Rue Des Colonus, Brussels, Belgium. Dec., 1914.
Shannon, Charles W., Oklahoma Geol. Survey, Norman, Okla. Dec., 1918.
Shattuck, George Burbank, Vassar College, Poughkeepsie, N. Y. Aug., 1899.
Shaw, Eugene W., U. S. Geological Survey, Washington, D. C. Dec., 1912.
Shedd, Solon, State College of Washington, Pullman, Wash. December, 1904.
Shepard, Edward M., 1403 Benton Ave., Springfield, Mo. August, 1901.
Shimek, Bohumil, University of Iowa, Iowa City, Iowa. December, 1904.
Shimer, Hervey Woodburn, Massachusetts Institute of Technology, Cambridge, Mass. December, 1910.
Siebenthal, Claude E., U. S. Geol. Survey, Washington, D. C. Dec., 1912.
*Simonds, Frederick W., University of Texas, Austin, Texas.
Sinclair, William John, Princeton University, Princeton, N. J. Dec., 1906.
Singewald, Joseph T., Johns Hopkins University, Baltimore, Md. Dec., 1911.
Sloan, Earle, Charleston, S. C. December, 1908.
Smith, Burnett, Syracuse University, Skaneateles, N. Y. December, 1911.
Smith, Carl, Box 1136, Tulsa, Okla. December, 1912.
*Smith, Eugene A., University of Alabama, University, Ala.
Smith, George Otis, U. S. Geological Survey, Washington, D. C. Aug., 1897.
Smith, Philip S., U. S. Geological Survey, Washington, D. C. Dec., 1909.
Smith, Warren Du Pré, University of Oregon, Eugene, Ore. December, 1909.
Smith, W. S. Tangier, 640 Tennyson Ave., Palo Alto, Calif. June, 1902.
*Smock, John C., Hudson, N. Y.
Smyth, Charles H., Jr., Princeton University, Princeton, N. J. Aug., 1892.
Smyth, Henry L., Harvard University, Cambridge, Mass. August, 1894.
Somers, R. E., Cornell University, Ithaca, N. Y. December, 1919.

Soper, Edgar K., 120 Broadway, New York City, Room 3101. December, 1918.
Speight, Robert, Christ Church, Canterbury College, New Zealand. Dec., 1916.
Spencer, Arthur Coe, U. S. Geological Survey, Washington, D. C. Dec., 1896.
*Spencer, J. W., 2019 Hillyer Place, Washington, D. C.
Springer, Frank, U. S. National Museum, Washington, D. C. December, 1911.
Spurr, Josiah E., c/o Engineering and Mining Journal, 10th Ave. and 36th St., New York, N. Y. December, 1894.
Stanley-Brown, Joseph, 26 Exchange Place, New York, N. Y. August, 1892.
Stanton, Timothy W., U. S. National Museum, Washington, D. C. Aug., 1891.
Stauffer, Clinton R., Univ. of Minnesota, Minneapolis, Minn. Dec., 1911.
Stebinger, Eugene, Jr., U. S. Geological Survey, Washington, D. C. Dec., 1916.
Steidtmann, Edward, University of Wisconsin, Madison, Wis. Dec., 1916.
Stephenson, Lloyd W., U. S. Geol. Survey, Washington, D. C. Dec., 1911.
*Stevenson, John J., 215 West 101st St., New York, N. Y.
Stoller, James Hough, Union College, Schenectady, N. Y. December, 1917.
Stone, Ralph Walter, U. S. Geological Survey, Washington, D. C. Dec., 1912.
Stose, George Willis, U. S. Geological Survey, Washington, D. C. Dec., 1908.
Stout, Wilber, Geological Survey of Ohio, Columbus, Ohio. December, 1918.
Swartz, Charles K., Johns Hopkins University, Baltimore, Md. Dec., 1908.
Taber, Stephen, University of South Carolina, Columbia, S. C. Dec., 1914.
Taff, Joseph A., 781 Flood Building, San Francisco, Cal. August, 1895.
Talbot, Mignon, Mount Holyoke College, South Hadley, Mass. Dec., 1913.
Talmage, James E., 47 E. So. Temple St., Salt Lake City, Utah. Dec., 1897.
Tarr, William Arthur, University of Missouri, Columbia, Mo. Dec., 1917.
Taylor, Frank B., Fort Wayne, Ind. December, 1895.
*Todd, James E., 905 Missouri Ave., Lawrence, Kans.
Tolman, Cyrus Fisher, Jr., Leland Stanford, Jr., University, Stanford University, Calif. December, 1909.
Tomlinson, Charles Weldon, 714 Ideal Bldg., Denver, Colo. December, 1917.
Trowbridge, Arthur C., 602 West 190th St., New York City. December, 1913.
*Turner, Henry W., Mills Building, San Francisco, Calif.
Twenhofel, William H., University of Wisconsin, Madison, Wis. Dec., 1913.
Twitchell, Mayville W., State Geological Survey, Trenton, N. J. Dec., 1911.
Tyrrell, Joseph B., Confederation Life Bldg., Toronto, Canada. May, 1889.
Udden, Johan A., University of Texas, Austin, Texas. August, 1897.
Ulrich, Edward O., U. S. Geological Survey, Washington, D. C. Dec., 1903.
Umpleby, Joseph B., University of Oklahoma, Norman, Okla. Dec., 1913.
*Upham, Warren, Minnesota Historical Society, Saint Paul, Minn.
Van Horn, F. R., Case School of Applied Science, Cleveland, Ohio. Dec., 1898.
van Ingen, Gilbert, Princeton University, Princeton, N. J. December, 1904.
Van Tuyl, Francis M., Colorado School of Mines, Golden, Colo. Dec., 1917.
Vaughan, T. Wayland, U. S. Geol. Survey, Washington, D. C. August, 1896.
Veatch, Arthur Clifford, 260 Riverside Drive, New York City. Dec., 1906.
*Vogdes, Anthony W., 2425 First St., San Diego, Calif.
*Wadsworth, M. Edward, School of Mines, Univ. of Pittsburgh, Pittsburgh, Pa.
*Walcott, Charles D., Smithsonian Institution, Washington, D. C.
Walker, Thomas L., University of Toronto, Toronto, Canada. Dec., 1903.
Warren, Charles H., Massachusetts Institute of Technology, Boston, Mass. December, 1901.
Washburne, C. W., 66 Liberty St., New York, N. Y. December, 1919.

Washington, Henry Stephens, Geophysical Laboratory, Washington, D. C. August, 1896.
Watson, Thomas L., University of Virginia, Charlottesville, Va. June, 1900.
Weaver, Charles E., University of Washington, Seattle, Wash. Dec., 1913.
Weed, Walter H., 29 Broadway, New York, N. Y. May, 1889.
Wegemann, Carroll H., 1129 Pennsylvania St., Denver, Colo. Dec., 1912.
Weidman, Samuel, Wisconsin Geological and Natural History Survey, Madison, Wis. December, 1903.
Weller, Stuart, University of Chicago, Chicago, Ill. June, 1900.
Westgate, Lewis G., Ohio Wesleyan University, Delaware, Ohio. Aug., 1894.
Wherry, Edgar T., Bureau of Chemistry, Washington, D. C. Dec., 1915.
White, David, U. S. National Museum, Washington, D. C. May, 1889.
*White, Israel C., Morgantown, W. Va.
Wieland, George Reber, Yale University, New Haven, Conn. December, 1910.
Wilder, Frank A., North Holston, Smyth County, Va. December, 1905.
*Williams, Edward H., Jr., Woodstock, Vt.
Williams, Ira A., Oregon Bureau of Mines and Geology, 417 Oregon Bldg., Portland, Ore. December, 1905.
Williams, Merton Yarwood, Geological Survey, Ottawa, Canada. Dec., 1916.
Willis, Bailey, Leland Stanford, Jr., University, Calif. December, 1889.
Wilson, Alfred W. G., Department of Mines, Ottawa, Canada. June, 1902.
Wilson, Morley Evans, Geological Survey, Ottawa, Canada. December, 1916.
Winchell, Alexander N., University of Wisconsin, Madison, Wis. Aug., 1901.
*Winchell, Horace Vaughn, First National Society Bldg., Minneapolis, Minn.
*Winslow, Arthur, 131 State St., Boston, Mass.
Wolff, John E., Harvard University, Cambridge, Mass. December, 1889.
Woodman, Joseph E., New York University, New York, N. Y. Dec., 1905.
Woodward, Robert S., Carnegie Institution of Washington, Washington, D. C. May, 1889.
Woodworth, Jay B., Geological Museum, 38 Oxford St., Cambridge, Mass. December, 1895.
Wright, Charles Will, Ingurtosu, Arbus, Sardinia, Italy. December, 1909.
Wright, Frederic E., Geophysical Laboratory, Carnegie Institution, Washington, D. C. December, 1903.
*Wright, G. Frederick, Oberlin Theological Seminary, Oberlin, Ohio.
Ziegler, Victor, Colorado School of Mines, Golden, Colo. December, 1916.

CORRESPONDENTS DECEASED

Credner, Herman. Died July 22, 1913.
Michel-Lévy, A. Died September, 1911.
Rosenbusch, H. Died January 20, 1914.
Suess, Edward. Died April 20, 1914.
Tschernyschew, Th. Died Jan. 15, 1914.
Zirkel, Ferdinand. Died June 11, 1912.

FELLOWS DECEASED

* Indicates Original Fellow (see article III of Constitution)

*Ashburner, Chas. A. Died Dec. 24, 1889.
Barlow, Alfred E. Died May 28, 1914.
Barrell, Joseph. Died May 4, 1919.
Beecher, Charles E. Died Feb. 14, 1904.
*Becker, George F. Died April 20, 1919.
Bell, Robert. Died June 18, 1917.
Bickmore, Albert S. Died Aug. 12, 1914.
Blake, Wm. Phipps. Died May 21, 1910.
Bowman, Amos. Died June 18, 1894.
Brown, Amos P. Died Oct. 9, 1917.
Buckley, Ernest R. Died Jan. 19, 1912.
Cairnes, D. D. Died June 14, 1917.

*CALVIN, SAMUEL. Died April 17, 1911.
CARPENTER, FRANK R. Died April 1, 1910.
*CHAPIN, J. H. Died March 14, 1892.
CLARK, WILLIAM B. Died July 27, 1917.
*CLAYPOLE, EDWARD W. Died Aug. 17, 1901.
*COMSTOCK, THEO. B. Died July 26, 1915.
COOK, GEORGE H. Died Sept. 22, 1889.
*COPE, EDWARD D. Died April 12, 1897.
CASTILLO, ANTONIO DEL. Died Oct.28,1895.
*DANA, JAMES D. Died April 14, 1895.
DAVIS, CHARLES A. Died April 9, 1916.
DAWSON, GEORGE M. Died March 2, 1901.
DAWSON, SIR J. WM. Died Nov. 19, 1899.
DERBY, ORVILLE A. Died Nov. 27, 1915.
DRYSDALE, CHAS. W. Died July 10, 1917.
DUTTON, CLARENCE E. Died Jan. 4, 1912.
*DWIGHT, WM. B. Died Aug. 29, 1906.
EASTMAN, CHAS. R. Died Sept. 27, 1918.
*ELDRIDGE, GEORGE H. Died June 29, 1905.
*EMMONS, SAMUEL F. Died March 28, 1911.
FONTAINE, WM. M. Died April 30, 1913.
*FOOTE, ALBERT E. Died October 10, 1895.
*FRAZER, PERSIFOR. Died April 7, 1909.
*FULLER, HOMER T. Died Aug. 14, 1908.
*GILBERT, GROVE K. Died May 1, 1918.
GIROUX, N. J. Died November 30, 1891.
HAGUE, ARNOLD. Died May 14, 1917.
*HALL, CHRISTOPHER W. Died May 10,1911.
*HALL, JAMES. Died August 7, 1898.
HATCHER, JOHN B. Died July 3, 1904.
*HAY, ROBERT. Died December 14, 1895.
HAYES, C. WILLARD. Died Feb. 9, 1916.
*HEILPRIN, ANGELO. Died July 17, 1907.
HILGARD, EUGENE W. Died Jan. 8, 1916.
HILL, FRANK A. Died July 13, 1915.
*HITCHCOCK, CHAS. H. Died Nov. 7, 1919.
*HOLMES, JOSEPH A. Died July 13, 1915.
HONEYMAN, DAVID. Died October 17, 1889.
*HOWELL, EDWIN E. Died April 16, 1911.
*HOVEY, HORACE C. Died July 27, 1914.
HUNT, THOMAS S. Died Feb. 12, 1892.
*HYATT, ALPHEUS. Died Jan. 15, 1902.
IRVING, JOHN D. Died July 26, 1918.
JACKSON, THOMAS M. Died Feb. 3, 1912.
*JAMES, JOSEPH F. Died March 29, 1897.
JULIEN, ALEXIS A. Died May 7, 1919
KNIGHT, WILBUR C. Died July 28, 1903.
LACOE, RALPH D. Died February 5, 1901.
*LAFLAMME, J. C. K. Died July 6, 1910.
LAMBE, L. M. Died March 12, 1919.
LANGTON, DANIEL W. Died June 21, 1909.
*LE CONTE, JOSEPH. Died July 6, 1901.
*LESLEY, J. PETER. Died June 2, 1903.
LOUGHRIDGE, ROBT. H. Died July 1, 1917.
MCCALLEY, HENRY. Died Nov. 20, 1904.
*MCGEE, W J. Died September 4, 1912.
MARCY, OLIVER. Died March 19, 1899.
MARSH, OTHNIEL C. Died March 18, 1899.
MELL, P. H. Died October 12, 1918.
*MERRILL, FRED. J. H. Died Nov. 29, 1916.
MILLS, JAMES E. Died July 25, 1901.
*NASON, HENRY B. Died January 17, 1895.
*NEFF, PETER. Died May 11, 1903.
*NEWBERRY, JOHN S. Died Dec. 7, 1892.
NILES, WILLIAM H. Died Sept. 12, 1910.
*ORTON, EDWARD. Died October 16, 1899.
*OSBORN, AMOS O. Died March, 1911.
*OWEN, RICHARD. Died March 24, 1890.
PENFIELD, SAMUEL L. Died Aug. 14, 1906.
PENHALLOW, DAVID P. Died Oct. 20, 1910.
PIRSSON, L. V. Died Dec. 8, 1919.
*PLATT, FRANKLIN. Died July 24, 1900.
PETTEE, WILLIAM H. Died May 26, 1904.
*POWELL, JOHN W. Died Sept. 23, 1902.
*PROSSER, CHAS. S. Died Sept. 11, 1916.
PURDUE, A. H. Died Dec. 12, 1917.
ROGERS, G. S. Died Nov. 18, 1919
*RUSSELL, ISRAEL C. Died May 1, 1906.
*SAFFORD, JAMES M. Died July 3, 1907.
*SCHAEFFER, CHARLES. Died Nov. 23, 1903.
SEELY, H. M. Died May 4, 1917.
SEÑA, J. C. DA COSTA. Died June 20, 1919.
*SHALER, NATHANIEL S. Died Apr. 10, 1906.
SUTTON, WILLIAM J. Died May 9, 1915.
TARR, RALPH S. Died March 21, 1912.
TIGHT, WILLIAM G. Died Jan. 15, 1910.
*VAN HISE, C. R. Died Nov. 19, 1918.
WACHSMUTH, CHAS. Died Feb. 7, 1896.
WESTON, THOMAS C. Died July 20, 1910.
WHITE, THEODORE G. Died July 7, 1901.
*WHITFIELD, ROBT. P. Died April 6, 1910.
*WILLIAMS, GEORGE H. Died July 12, 1894.
*WILLIAMS, J. FRANCIS. Died Nov. 9, 1891.
*WILLIAMS, H. S. Died July 31, 1918.
WILMOTT, ARTHUR B. Died May 8, 1914.
*WINCHELL, ALEX. Died Feb. 19, 1891.
*WINCHELL, NEWTON. Died May 1, 1914.
WRIGHT, ALBERT A. Died April 2, 1905.
YEATES, WILLIAM S. Died Feb. 19, 1908.

Summary

Correspondents	10
Original Fellows	35
Elected Fellows	386
Membership	431
Deceased Correspondents	6
Deceased Fellows	111

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
VOL. 31, PP. 177-184 MARCH 31, 1920

PROCEEDINGS OF THE FIFTEENTH SUMMER MEETING, HELD IN CONJUNCTION WITH THE SIXTEENTH ANNUAL MEETING OF THE CORDILLERAN SECTION, UNIVERSITY OF CALIFORNIA AND STANFORD UNIVERSITY, AUGUST 3, 4, AND 5, 1915.

J. A. TAFF, *Secretary*

CONTENTS

Session of Tuesday, August 3

The first session was called to order at 10 a. m. Tuesday, August 3, in Bacon Hall, University of California, by C. F. Tolman, Jr., chairman of the Cordilleran Section, in the absence of President Arthur P. Coleman, of the Geological Society. After announcements in regard to proposed excursions, the dinner, and the official program, the following papers were read in the order given:

EPIGENE PROFILES OF THE DESERT

BY ANDREW C. LAWSON

Read in full from manuscript.

Discussion

In reply to a question by Professor Holway, Dr. Lawson explained that the profile limit of the pan-fan stage in the desert profile was reached only occasionally. Changes of climate usually interrupt the pan-fan stage.

Prof. Bailey Willis cited instances in desert regions of China and Patagonia to show that the pan-fan stage is rarely reached.

Dr. Lawson is of the opinion that the Great Basin region is the ideal region to illustrate instances cited.

The paper is illustrated by four section diagrams and photographs of examples in the region of Needless, San Bernardino County, California.

THE BAJADAS OF THE SANTA CATALINA MOUNTAINS, ARIZONA

BY C. F. TOLMAN, JR.

Presented in full extemporaneously.

Discussed by Sidney Paige, who cites experiences in the same region.

In response to an inquiry, Professor Tolman defines the bajada as coalescing fans that let down from the mountain base to the bolson. Bajada should apply only to surface forms and not to other dimensions.

Discussed by Professor Lawson, who compared the bajada material to fanglomerate.

ORIGIN OF THE TUFAS OF LAKE LAHONTAN

BY J. C. JONES

Presented extemporaneously.

Discussion was deferred.

The session adjourned at 12.10 p. m. for luncheon.

Seventy-five members and visitors were present.

A short business session was held to consider the election of officers of the Cordilleran Section for the ensuing year.

Moved by Dr. Lawson and voted, that the Secretary call for a nominating ballot, through correspondence, for election of officers at a meeting to be called for the purpose.

The Section adjourned to convene in the afternoon, following a meeting of the Paleontologic Section.

The afternoon session was called to order by Vice-President E. O. Ulrich, of the Geological Society, at 4.20 p. m., when the following papers were read and discussed:

SOME PHYSIOGRAPHIC FEATURES OF BOLSONS

BY H. E. GREGORY

SCULPTURING OF ROCK BY WINDS IN THE COLORADO PLATEAU PROVINCE

BY H. E. GREGORY

Read in full from manuscript by C. F. Tolman, Jr.

Discussion

C. F. Tolman, Jr., named types of protected surfaces in desert regions: (1) Vegetation; (2) Coarse gravel and boulders, making pavement; (3) Caleche and surface soil cements; (4) Desert pavements, angular gravel, forming mosaic; and (5) Clay in bottoms of playas.

Horace B. Patton stated that wind is abrading rather than transporting, and cited deep holes that have been formed by wind.

E. E. Free referred to salt cements in deserts as protection against wind erosion.

Erasmus Haworth and J. C. Ray discussed protecting effects of alternating coarse and fine, hard and soft, strata.

J. C. Jones discussed deposition by winds.

At 4.45 the session adjourned, to meet at Stanford University, August 4, at 10 a. m.

Session of Wednesday, August 4

This session was held at Stanford University. At 10 a. m. the meeting was called to order by Chairman C. F. Tolman, Jr. Dr. A. C. Lawson was called to the chair and the following papers were read:

SOME CHEMICAL FACTORS AFFECTING SECONDARY SULPHIDE ORE ENRICHMENT

BY S. W. YOUNG

Presented in full.

Discussion

C. F. Tolman, Jr., called attention to the fact that H_2S is a great mineralizing agent and not a precipitant.

J. C. Ray stated that high temperature veins carry H_2S.

J. E. Wolf commended the work of Tolman and Young indicating that past work in sulphides should be revised.

RÔLE OF COLLOIDAL MIGRATION IN ORE DEPOSITS

BY JOHN D. CLARKE

Presented in full extemporaneously.

Discussion

Dr. Lawson challenged the statement that alkaline solutions come from magmas.

Dr. Bastin suggested that silica may play a part, bearing minerals in colloidal state.

SOME EXAMPLES OF PROGRESSIVE CHANGES IN THE MINERAL COMPOSITION OF COPPER ORES

BY C. F. TOLMAN, JR.

Presented in full extemporaneously.

Discussion

A. C. Lawson questioned the propriety of too definite conclusions being drawn on interpretations of rock sections.

At 12.30 p. m. the session adjourned for luncheon, to convene after meeting of the Paleontological Society.

The afternoon session was called to order by Chairman C. F. Tolman, Jr., at 4.30 p. m. and the following paper was read:

SERICITE, A LOW-TEMPERATURE HYDROTHERMAL MINERAL

BY A. F. ROGERS

Presented in full extemporaneously.

At 5 p. m. the session adjourned, to meet at the University of California at 10 a. m. August 5.

ANNUAL DINNER

The annual dinner was held jointly by the Geological, Paleontological, and Seismological Societies, at the Engineers' Club, San Francisco, at 7.30 p. m.

Session of Thursday, August 5

The Society was called to order at 10 a. m., in Bacon Hall, University of California, by Chairman C. F. Tolman, Jr., and the following papers were read:

PHYSIOGRAPHIC CONTROL IN THE PHILIPPINES

BY WARREN D. SMITH

Read in full from manuscript.

Discussed by R. A. Daly, A. C. Lawson, C. F. Tolman, Jr., and Wm. H. Hobbs.

In reply to inquiry, Dr. Smith stated that the valleys were V-shaped and contained no soil.

ORIGIN OF THE BASINS WITHIN THE HAMADA OF THE LIBYAN DESERT

BY WILLIAM H. HOBBS

Presented extemporaneously.

THE LIMITED EFFECTIVE VERTICAL RANGE OF THE DESERT SAND-BLAST, BASED ON OBSERVATIONS MADE IN THE LIBYAN DESERT AND IN THE ANGLO-EGYPTIAN SUDAN

BY WILLIAM H. HOBBS

Presented extemporaneously.

DIASTROPHISM OF THE PACIFIC COAST

BY R. S. HOLWAY

Presented in full extemporaneously by the author and supplemented by

NOTES ON MOUNT LASSEN ERUPTIONS

BY J. S. DILLER

Discussion by J. C. Jones and others.

At 12.15 p. m. the session adjourned for luncheon.

The afternoon session was called to order by Chairman C. F. Tolman, Jr., at 2 p. m. and the following papers were read:

GEOLOGY OF PORTIONS OF WESTERN WASHINGTON

BY CHARLES E. WEAVER

Presented in full.

Discussion by John P. Buwalda.

THE PROBLEM OF THE TEXAS TERTIARY SANDS

BY E. T. DUMBLE

Read in full.

Discussion by Alexander Deussen and Dr. Matthew.

PISOLITES AT SAN ANTONIO, TEXAS

BY ALEXANDER DEUSSEN

Read in full.

Discussion by Messrs. Matthew, Clarke, and Turner.

GEOLOGIC AGE OF THE COAL CREEK BATHOLITH AND ITS BEARING ON SOME OTHER FEATURES OF THE GEOLOGY OF THE COLORADO FRONT RANGE

BY HYRUM SCHNEIDER

Presented in full.

Discussion by Messrs. Patton, Haworth, and Bastin.

OCCURRENCE OF FLOW-BRECCIAS IN COLORADO

BY H. B. PATTON

Presented in full.

Discussion by E. Haworth.

GEOLOGY OF A PORTION OF THE SANTA YNEZ RIVER DISTRICT, SANTA BARBARA COUNTY, CALIFORNIA

BY W. S. W. KEW

Read in full.

SOME INTERESTING CHANGES IN THE COMPOSITION OF THE SALTON SEA

BY A. E. VINSON

Read by the author.

EXAMPLES OF SUCCESSIVE REPLACEMENT OF EARLIER SULPHIDE MINERALS BY LATER SULPHIDES AT BUTTE, MONTANA

BY J. C. RAY

Read by the author.

General discussion was had on all papers bearing on ore deposits by Messrs. Paige, Rogers, Schneider, Bastin, and Tolman.

The session adjourned at 5.15 p. m., to be resumed in the evening.

The evening session was called at 8.10 p. m. and the following papers read:

STRUCTURE OF THE SOUTHERN SIERRA NEVADA

BY JOHN P. BUWALDA

Read in full.

Discussion by Messrs. Tolman, Chamberlain, and Holway.

The remaining papers on the program were read by title, as the authors were not present.

At 9.10 p. m. the final session adjourned.

Register of the California Meeting

Arnold, Ralph
Bastin, E. S.
Berry, E. W.
Branner, J. C.
Chamberlain, R. T.
Clapp, Charles H.
Daly, R. A.
Deussen, Alexander
Dumble, E. T.
Eakle, Arthur S.
Fairbanks, H. W.
Hobbs, Wm. H.
Haworth, Erasmus
Hershey, Oscar H.
Keyes, Charles R.
Lawson, A. C.
Matthew, W. D.
Merriam, J. C.
Osborne, H. F.
Paige, Sidney
Patton, H. B.
Schuchert, Charles
Sellards, E. H.
Sinclair, Wm. F.
Smith, W. S. T.
Stanton, T. W.
Stone, R. W.
Taff, J. A.
Tolman, Jr., C. F.
Turner, H. W.
Weaver, Charles E.
White, I. C.
Willis, Bailey
Wolff, J. E.
Ulrich, E. O.

J. A. Taff,
Secretary.

Minutes of Special Meeting to canvass Ballots for Officers

A special meeting of the Cordilleran Section was called to meet at Stanford University, November 20, 1915, for the purpose of canvassing the ballot for officers.

The section was called to order by the chairman and the purpose of the meeting stated.

A. C. Lawson and J. A. Taff were appointed a committee to canvass the vote that had been assembled by mail, upon notification to all the members, pursuant to an order of the section at the regular meeting, August 3.

The following-named officers were declared elected:

Chairman, C. F. Tolman, Jr.
Secretary, J. A. Taff.
Councilor, Charles E. Weaver.

Thereupon the session adjourned.

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
Vol. 31, pp. 185-186 March 31, 1920

PROCEEDINGS OF THE SEVENTEENTH ANNUAL MEETING OF THE CORDILLERAN SECTION, HELD AT SAN DIEGO, CALIFORNIA, AUGUST 10, 1916.

J. A. Taff, *Secretary*

CONTENTS

Session of August 10

The seventeenth annual meeting of the Cordilleran Section was called to order by the Secretary, in the High School Building, at 2 p. m., in conjunction with that of the Pacific Division of the American Association for the Advancement of Science, August 10, 1916. In the absence of the chairman, Dr. George D. Louderback was elected to the chair. Twenty-five members and visitors were present.

All of the papers on the program were presented in order, as follows:

AGE OF THE ELLENSBURG FORMATION OF EASTERN OREGON

BY DR. J. C. MERRIAM

Read in full.

Discussion by Mr. Taff, Dr. Louderback, and General Vogdes.

SOME INDICATIONS OF CLIMATIC ZONES IN CALIFORNIA DURING LOWER EOCENE TIME

BY ROY E. DICKERSON

Read in full.

RELATIONSHIP OF TERATORNIS TO OTHER REPORTED BIRDS

BY LOYE H. MILLER

Read in full.

RECENT EXTENSION OF THE DISTRIBUTION OF MARINE WOOD BORERS IN SAN PABLO BAY

BY ALBERT L. BARROWS

Read in full.

Discussed by Mr. L. H. Miller, Dr. Merriam, and Dr. Louderback.

At 3.30 p. m. the session adjourned.

J. A. Taff,
Secretary.

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
VOL. 31, PP. 187-190 MARCH 31, 1920

PROCEEDINGS OF THE EIGHTEENTH ANNUAL MEETING OF THE CORDILLERAN SECTION, HELD AT STANFORD UNIVERSITY, CALIFORNIA, APRIL 6 AND 7, 1917.

J. A. TAFF, *Secretary*

CONTENTS

SESSION OF APRIL 6

The eighteenth annual meeting of the Cordilleran Section of the Geological Society of America was held at Stanford University, in conjunction with that of the Pacific Division of the American Association for the Advancement of Science, April 6 and 7, 1917. The first session, April 6, was called to order at 10 a. m. by Chairman C. F. Tolman, Jr. After announcement in regard to the annual dinner, to be held at Stanford Union at 6 p. m., and the business meeting for 1.45 p. m., the following papers were read:

STRUCTURE OF THE LITHOSPHERE

BY BAILEY WILLIS

Presented extemporaneously.

DISCUSSION

Dr. A. C. LAWSON expressed appreciation and asked for experimental data of a positive kind as to movement on plains of schistosity, calling attention to the fact that schistose rocks sometimes break as easily across as with lines of schistosity.

Dr. WILLIS explained, illustrating by use of sheared wood blocks, showing movement with and not across grain of wood.

Dr. C. F. TOLMAN cited studies of schistose structure in region of Tucson, Arizona, illustrating gneissic structure in granite at contact between older and younger masses.

GEOLOGY AND STRUCTURE OF THE SAN BERNARDINO MOUNTAINS

BY F. E. VAUGHAN

Presented extemporaneously.

Discussed by Bailey Willis, who compared structure of San Bernardino Mountains to that encountered in China.

At 12.15 p. m. the session adjourned for luncheon, to resume in business session at 1.45 p. m., 44 members and visitors being present.

ELECTION OF OFFICERS

The business meeting was called to order by Chairman C. F. Tolman, Jr., at 1.50 p. m. Election of officers of the section for the ensuing year was announced as first object. Messrs. Bailey Willis, A. C. Lawson, and J. A. Taff were appointed as a committee to canvass the ballot. The result was declared by the chair as follows:

President, HENRY LANDES.
Secretary, CHARLES E. WEAVER.
Councilor, WARREN D. SMITH.

APPOINTMENT OF COLOR SCHEME COMMITTEE

Dr. Bailey Willis presented the problem of a color scheme for use in the preparation of geologic maps in Pacific region publications. He pointed out the lack of harmony in colors used to represent geologic units in Pacific Coast State and other publications.

A motion was made and voted that a committee of three be appointed to consider a color scheme and report at the December meeting. It was suggested that the standard colors adopted by the United States Geological Survey be considered. In age distinctions use the spectrum order of colors.

Discussed by Drs. Lawson, Tolman, and Merriam.

The Chairman appointed the following committee: Bailey Willis, J. C. Merriam, and J. A. Taff.

At 2.20 p. m. the regular afternoon session was resumed and the following papers were read:

GEOLOGIC CROSS-SECTION ACROSS THE CASCADE MOUNTAINS AND PUGET SOUND BASIN, WASHINGTON

BY CHARLES E. WEAVER

Presented in full extemporaneously.

Discussed by Bailey Willis, J. C. Merriam, George D. Louderback, and A. C. Lawson.

GEOLOGY AND MINERAL RESOURCES OF INYO COUNTY, CALIFORNIA

BY A. C. WARING

Read from manuscript.

FOOTHILL COPPER BELT OF THE SIERRA NEVADA MOUNTAINS, CALIFORNIA

BY C. F. TOLMAN, JR.

Presented in full extemporaneously.

NATURAL HISTORY OF MINERALS

BY AUSTIN F. ROGERS

Presented in full extemporaneously.

LADD MANGANESE MINE—NOTES ON THE OCCURRENCE OF MANGANESE IN CALIFORNIA

BY G. C. GESTER

Presented in full.

At 6 p. m. the session adjourned, 41 members and visitors being present.

Session of April 7

Following the session of the Seismological Society, the Cordilleran Section was called to order at 11.30 a. m. and the following papers read:

ANNOUNCEMENTS

Announcements were made in regard to the organization and plans of the Research Committee of the National Defense organization by Dr. J. C. Merriam, chairman, the various affiliated societies of the Pacific Division of the American Association for the Advancement of Science, including the members of the Le Conte Club, to participate.

BRECCIAS OF THE MARIPOSA FORMATION IN THE VICINITY OF COLFAX, CALIFORNIA

BY E. F. DAVIS

Read by title.

RADIOLARIAN CHERTS OF THE FRANCISCAN GROUP, CALIFORNIA

BY E. F. DAVIS

Read from manuscript.

Discussion by C. F. Tolman, J. A. Taff, and W. S. T. Smith.

OCCURRENCE OF THE ORE DEPOSITS OF THE SANTA FE DISTRICT, MINERAL COUNTY, NEVADA

BY C. W. CLARK

At 12.30 p. m. the session adjourned for luncheon.

An afternoon session was held, beginning at 2 p. m., when further discussion was had on the paper entitled

FOOTHILL COPPER BELT OF THE SIERRA NEVADA, CALIFORNIA

BY C. F. TOLMAN

At 2.30 the section adjourned *sine die.*

Register of the Meeting of the Cordilleran Section

Branner, J. C.
Gill, A. C.
Lawson, A. C.
Louderback, G. D.
Merriam, J. C.
Smith, W. S. T.
Taff, J. A.
Tolman, Jr., C. F.
Turner, H. W.
Willis, Bailey

J. A. Taff,
Secretary.

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
VOL. 31, PP. 191-196 MARCH 31, 1920

PROCEEDINGS OF THE NINETEENTH ANNUAL MEETING OF THE CORDILLERAN SECTION, HELD AT PASADENA, CALIFORNIA, JUNE 19-22, 1919.

GEORGE D. LOUDERBACK, *Acting Secretary*

CONTENTS

SESSION OF THURSDAY, JUNE 19

The nineteenth annual meeting of the Cordilleran Section of the Geological Society of America was held, in conjunction with the Pacific Division of the American Association for the Advancement of Science, at Throop College of Technology, Pasadena, California, on June 19 and 20, 1919.

The meeting was called to order at 10.20 a. m., June 19. In the absence of the regular officers, Dr. B. L. Clark was elected temporary chairman and G. D. Louderback Acting Secretary.

ANNOUNCEMENTS

The Acting Secretary reported that, due to the absence of the Secretary in Central America, the chairman had appointed him Acting Secretary to prepare the program and make arrangements for the Pasadena meeting.

It was voted that the Friday meeting should be called at 9.30 a. m., in Room 205, Pasadena Hall. It was also voted to postpone the business meeting to Friday noon.

The following paper was then presented:

STRATIGRAPHIC AND FAUNAL RELATIONSHIPS OF THE MEGANOS GROUP, MIDDLE EOCENE

BY BRUCE L. CLARK

(Abstract)

The data presented in this paper give the results of the writer's investigation on this newly recognized division of the Eocene since the publication of the first announcement of its discovery.[1]

The most important points brought out in the paper are:

1. Beds referred by the writer to the Meganos group have a wide distribution throughout the State. In all the principal sections they lie unconformably below beds containing a typical Tejon fauna. The principal general Eocene localities studied in the State are on the south side of Mount Diablo, north of Coalinga, the type section of the Tejon, at the south end of the San Joaquin Valley, and the Simi Valley, which is thirty miles northwest of the city of Los Angeles. In all these localities, a maximum distance of about five hundred miles apart, beds of Tejon age were found unconformably above those referred to the Meganos. In some localities the difference in dip between the Meganos and the Tejon is more than fifteen degrees and in almost every case is accompanied by a marked difference in strike.

2. The faunal evidence supports the conclusion that there was a big time hiatus between the deposition of the beds of these two horizons.

3. The writer's conclusion is that the marine beds in the foothills of the Sierra Nevada, as at Table Mountain, near Oroville, which beds have been referred to the Ione formation—the *Siphonalia sutterensis* zone of Dr. R. E. Dickerson, supposed by him to represent the uppermost Tejon—belong to the Meganos epoch of deposition. The Ione as originally described is undoubtedly a composite group.

DISCUSSION

Discussion by W. D. Smith, —— Buell, and B. L. Clark was particularly directed to comparisons between California and Oregon stratigraphy and also

[1] Meganos Group, a newly recognized division in the Eocene of California. Bull. Geol. Soc. Am., vol. 29, 1919, pp. 281-296.

to the indications of the relative length of Tertiary time. Smith was of the opinion that observations in the tropics indicated that the Tertiary was shorter than ordinarily considered. Clark held that a study of the Pacific Coast stratigraphic column indicated that the Tertiary is very long, representing a real era rather than simply a period.

SYMPOSIUM ON PROBLEMS OF THE NORTH PACIFIC OCEAN

The meeting adjourned at 11.25 a. m., after decision that no afternoon meeting should be held, so that the members might have an opportunity to attend the symposium on "Problems of the North Pacific Ocean," held under the auspices of the Western Society of Naturalists.

Session of Friday, June 20

The meeting was called to order at 9.50 a. m. by the temporary chairman. The following papers were presented:

STRUCTURE OF THE COAST RANGES OF CALIFORNIA

BY BAILEY WILLIS

(*Abstract*)

According to the interpretation here presented, the typical structure of the Coast Ranges and of the Sierra Nevada is the rotated mountain block. The block commonly presents a long slope, which is a more or less steeply tilted surface of erosion, often a peneplain. Tilting is an effect of rotation of the block on a curved under surface. The rotation is a movement which may be described as a highly inclined upthrust fault and which is an effect of compression due to horizontal pressure. The paper describes specific cases and indicates that the upthrust fault in fact frequently exists where normal faults have been assumed.

Discussion

Discussion by W. D. Smith in relation to tidal stress, by Buell on the magnitude of the movements, by Louderback in regard to the apparent lack of correspondence between actual observations on schistosity and lava occurrences with the theoretical deductions. Clark discussed the relation to the thrusting along the Great Valley of California and several miles to the west. He questioned how this was possible with faulting as explained by the author and also inquired concerning the Santa Lucia thrust. The author replied, explaining the relationship of this theory to the various other phenomena mentioned.

At 11.05 the meeting adjourned to permit the members to take part in the meeting of the Seismological Society of America.

Session was resumed at 11.35 and the following paper was presented:

CENOZOIC HISTORY OF THE GROUND SLOTH GROUP

BY CHESTER STOCK

(*Abstract*)

The gravigrade edentates afford an interesting example of mammalian evolution during the Cenozoic in America. They are intimately associated with the Tertiary and Pleistocene histories of North and South America and indicate former land connections between the two continents, with evidence as to the periods of such connections. Genera of the ground sloth group, known from the Miocene, Pliocene, and Pleistocene, arrange themselves in phylogenetic series illustrating principles of paleontology.

Discussion by Messrs. Buell and Bailey Willis.

The meeting adjourned at 12.18.

BUSINESS MEETING

A business meeting of the Cordilleran Section was called at 12.25.

The following delegates to the Affiliation Committee were chosen: G. D. Louderback and W. D. Smith.

NOMINATION OF OFFICERS

On account of the small number of Fellows present, it was decided that instead of electing officers for the coming year the meeting should consider itself a Nominating Committee, and that the Acting Secretary should send to the members of the Cordilleran Section ballots for a vote by mail for officers for the year 1919-20.

The following names were chosen:

For Chairman, George D. Louderback.
For Secretary, A. F. Rogers.
For Councilor, C. F. Tolman, Jr.

The regular meeting of the afternoon for presentation of papers was called to order at 2.15.

EXHIBIT OF LANTERN SLIDES

BY WARREN D. SMITH

Mr. Smith exhibited a series of lantern slides illustrating various features of Oregon geology and physiography and called attention to various problems in Oregon geology.

The following papers were then presented:

OIL POSSIBILITIES ALONG THE SOUTHERN MARGIN OF THE BLACK HILLS, SOUTH DAKOTA

BY ROY R. MORSE [1]

(*Abstract*)

Domed structure on the southern margin of the Black Hills uplift is discussed. Oil-bearing strata of the not remote Wyoming fields are present. Certain localities of their outcrop in the Black Hills are saturated with oil. Results of drill tests here confirm the conclusion that such "favorable indications" are not alone sufficient. By analogy with the structural position of producing fields, it is apparent that the relation between local structure and regional structure is, at the points tested, unfavorable. The relations between minor structures, such as domes or terraces, and the major structural features of a province, with the resultant conditions of gathering ground for oil and water, are here, as elsewhere, of prime importance in the search for accumulations of commercial value. Assuming uniform porosity conditions, the perhaps too-often assumed necessity of perfect "closure"—that is, "domes"—is dependent upon the position of the local structure with respect to the major structure of the region. The position of many important fields, both upon "open and closed" structures, illustrates this feature.

STRATIGRAPHIC AND AGE RELATIONS OF THE SCARP-PRODUCING FAULTS OF THE GREAT BASIN

BY GEORGE D. LOUDERBACK

(*Abstract*)

The discussion is based in part on recent results of paleontological studies in the Tertiary formations of the Great Basin and of California, in part on the relation of the faulting to the volcanoes of the Sierra Nevada and the Great Basin, and in part on the physiography of the various fault-scarps.

Discussion

W. D. Smith made comparison with conditions in Oregon; Bailey Willis made comparison with conditions in Shantung, in the Danube region, and in the Bernese Alps.

Permission was granted to present a brief paper, entitled

MIOCENE FISHES OF CALIFORNIA

BY J. Z. GILBERT

(*Abstract*)

Dr. Gilbert described the work in which he had been engaged in the various localities at which such fish remains had been found and the general paleon-

[1] Introduced by G. D. Louderback.

tologic and geologic results. He asked for the cooperation of all present in calling his attention to new localities or good material they might find.

The section adjourned at 5. p. m.

Session of Saturday, June 21

EXCURSION TO MOUNT WILSON

The day was given over to an excursion to Mount Wilson, on which all of the visiting scientists were guests of the Mount Wilson Solar Observatory and were given free access and guidance through the scientific shops of Pasadena and the various observatory buildings on top of the mountain.

Session of Sunday, June 22

EXCURSION TO RANCHO LA BREA

On Sunday an excursion was made to the Rancho La Brea for the examination of the beds from which abundant paleontological material had been taken, and later the Museum of History, Science and Art was visited for an examination of collections made at Rancho La Brea, including some carefully mounted skeletons.

Register of the Meeting of the Cordilleran Section

Fellows:

Clark, B. L.
Louderback, George
Smith, W. D.
Vogdes, A. W.
Willis, Bailey

Visitors and other geologists taking part:

Buell, ——
Gilbert, J. Z.
May, A.
Morse, R. R.
Phelps, R. W.
Stock, Chester
Townley, S. D.
Woodford, A. O.

Altogether the attendance was as follows:

Thursday morning	20
Friday morning	40
Friday afternoon	20

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
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PROCEEDINGS OF THE PALEONTOLOGICAL SOCIETY

PROCEEDINGS OF THE ELEVENTH ANNUAL MEETING OF THE PALEONTOLOGICAL SOCIETY, HELD AT BOSTON, MASSACHUSETTS, DECEMBER 30-31, 1919.

R. S. BASSLER, *Secretary*

CONTENTS

SESSION OF TUESDAY, DECEMBER 30

The eleventh annual meeting of the Society was called to order by President Jackson at 10 a. m., December 30, in the Rogers Building of the Massachusetts Institute of Technology. After welcoming the Society to Boston, President Jackson called upon the Secretary to present the report of the Council.

REPORT OF COUNCIL

To the Paleontological Society, in eleventh annual meeting assembled:

The formal meetings of the Council during the past year have been limited to one at Baltimore, following the tenth annual meeting, and one held just before the present session. Matters of business not discussed at these meetings have been transacted by correspondence, as heretofore. A résumé of the Council's administration for the eleventh year of the Society is presented in the following reports of the Secretary and Treasurer:

SECRETARY'S REPORT

To the Council of the Paleontological Society:

The Secretary's annual report for the year ending December 27, 1919, is as follows:

The proceedings of the tenth annual meeting of the Society, held at Baltimore, Maryland, December 28, 1918, are printed in volume 30, pages 143-164, of the Bulletin of the Geological Society of America.

The preliminary ballot for officers and the announcement that the eleventh annual meeting of the Society would be held at Boston, as a guest of the Geological Society of Boston, was issued March 19, 1919.

The Council voted that the dues of the members in the military service of the United States abroad should be remitted during this period. The Council also voted approval of the issuance of a war record card to the members, so that their activities may be preserved in the records of the Society.

The nomination of Dr. T. Wayland Vaughan as representative of the Society in the National Research Council for the year ending May 1, 1920, was approved by the Council. At the Council meeting just concluded, the nomination of Dr. Vaughan to the same position for the three-year period ending May 1, 1923, was recommended.

Membership.—During the year the Society has lost four of its members by death: Mr. C. A. Waring, of the State Mining Bureau of California, well known in the field of invertebrate paleontology of the Pacific coast; Mr. Lawrence M. Lambe, vertebrate paleontologist of the Canadian Geological Survey; Dr. Joseph Barrell, eminent geologist and teacher, of Yale University, and Mr. Victor Lyon, city engineer of Jeffersonville, Indiana, and long an energetic collector and student of fossils in the celebrated Falls of the Ohio area.

One member has resigned during the year and ten new members are under consideration for election. At the election just concluded two of our members became Fellows of the Geological Society of America. The result of these various changes leaves the total number of members at the end of 1919 as 194.

Pacific Coast Section.—The ninth annual meeting of the Pacific Coast Section of the Paleontological Society was held at Pasadena, California, June 20, 1919, in conjunction with the Cordilleran Section of the Geological Society of America and the Seismological Society of America, with Prof. G. D. Lauderback as presiding officer. During the presentation of the papers on geology and paleontology Prof. B. L. Clark presided.

The following officers for the section were elected for the ensuing year:

President, John C. Merriam.
Vice-President, Earl L. Packard.
Secretary, Chester Stock.

Several papers on paleontology were presented, and following the program the members made an excursion to the asphalt deposits at Rancho La Brea, and later the Museum at Los Angeles was visited.

The minutes of this section are printed on pages 231 to 232 of this Bulletin.

Respectfully submitted, R. S. Bassler,
Secretary.

Washington, D. C., *December* 30, 1919.

Treasurer's Report

To the Council of the Paleontological Society:

The Treasurer begs to submit the following report of the finances of the Society for the fiscal year ending December 26, 1919:

RECEIPTS

Cash on hand December 13, 1918		$622.38	
Membership fees (1917)		6.00	
Membership fees (1918)		24.00	
Membership fees (1919)		225.00	
Membership fees (1920)		3.00	
Interest, Connecticut Savings Bank		19.74	
			$900.12

EXPENDITURES

Treasurer's office:			
Treasurer's allowance	$25.00		
Postage	7.00		
		$32.00	
Secretary's office:			
Secretary's allowance	$50.00		
Office expenses	39.91		
Clerical help	25.00		
		114.91	
Geological Society of America:			
For printing programs, etcetera	$17.80		
For printing separates	111.65		
		129.45	
			276.36
Balance on hand December 26, 1919			$623.76
Net increase in funds			$1.38
Outstanding dues (1918), 2		$6.00	
Outstanding dues (1919), 7		21.00	
			27.00

Respectfully submitted,

R. S. Lull,
Treasurer.

New Haven, Connecticut, *December* 26, 1919.

APPOINTMENT OF AUDITING COMMITTEE

The appointment of a committee to audit the Treasurer's accounts was in order following the reading of his report. The chairman selected P. E. Raymond and R. M. Field for this purpose.

ELECTION OF OFFICERS AND MEMBERS

The results of the ballots for the election of officers for 1920 and of new members were then announced, as follows:

OFFICERS FOR 1920

President:

F. B. LOOMIS, Amherst, Mass.

First Vice-President:

E. C. CASE, Ann Arbor, Mich.

Second Vice-President:

RALPH ARNOLD, Los Angeles, Calif.

Third Vice-President:

E. M. KINDLE, Ottawa, Canada

Secretary:

R. S. BASSLER, Washington, D. C.

Treasurer:

RICHARD S. LULL, New Haven, Conn.

Editor:

W. D. MATTHEWS, New York City

NEW MEMBERS

CHARLES L. CAMP, American Museum of Natural History, New York City.
THOMAS HENRY CLARK, South Weymouth, Mass.
HORACE N. CORYELL, Department of Geology, Columbia University, New York City.
GEORGES CROZEL, 17 Chemin des Celestins, Oullins (Rhone), France.
BELA HUBBARD, Queens College, Kingston, Ontario.
GEORGE S. HUME, Geological Survey of Canada, Ottawa, Canada.
MALCOLM R. THORPE, Peabody Museum, New Haven, Conn.
KATHERINE E. H. VAN WINKLE, 126 Kelvin Place, Ithaca, N. Y.

The President then brought up the following two nominations for membership which had received the approval of the Council, but which had arrived too late for the printed ballot:

Ralph W. Chaney, Old Science Hall, Iowa City, Iowa. Engaged in study of paleobotany. Proposed by F. H. Knowlton and R. S. Bassler.

Charles E. Decker, University of Oklahoma, Norman, Oklahoma. Engaged in study of stratigraphy and invertebrate paleontology. Proposed by Charles Schuchert and R. S. Bassler.

On motion and by unanimous vote, the Secretary was instructed to cast the ballot of the Society for the election of Messrs. Chaney and Decker.

NEW BUSINESS

The recommendation of the Council that Dr. T. Wayland Vaughan be nominated to succeed himself as the representative of the Society on the National Research Council for the three-year term ending May 1, 1923, was on motion and vote, now duly approved by the members.

The Secretary then presented briefly the results of the war record cards issued earlier in the year, which showed that an unusually large percentage of the members had been either in active military service or had been teaching in the several training corps.

President Jackson now called for the report of the Williston Memorial Committee. This was presented by W. K. Gregory, who outlined the scope of a proposed memorial volume in honor of Professor Williston and discussed plans for financing the project.

The next matter of business was a motion by P. E. Raymond to the effect that a committee be appointed to prepare a list of not more than 1,000 North American index fossils for use in teaching paleontology.

On vote, this was carried, and President Jackson appointed J. C. Merriam, Charles Schuchert, Gilbert Van Ingen, E. W. Berry, and P. E. Raymond as the committee for this purpose.

NECROLOGY

Memorial addresses in tribute to the lives of the four members of the Society who passed away during the year were then presented, as follows:

Chester Stock gave an account of the life of Mr. C. A. Waring and enumerated his researches in the field of the invertebrate paleontology of the Pacific Coast.

R. S. Lull touched on the many activities of Prof. Joseph Barrell, but brought out particularly his researches on paleontologic subjects.

R. S. Bassler spoke of his pleasant personal experiences with Mr. Victor W. Lyon, of Mr. Lyon's researches upon fossil echinoderms, and of his early geological work with his father, Mr. Sidney S. Lyon.

PRESENTATION OF PAPERS ON PALEONTOLOGY

At 11 a. m., with the conclusion of the business meeting, the reading of papers was commenced by the presentation of a short discussion by each of the authors of an interesting occurrence of a fossil horse in New York State. The specimens on which this discovery was based were exhibited and the subject was discussed by F. B. Loomis and the authors.

SUPPOSED FOSSIL HORSE FROM THE LATE PLEISTOCENE FOUND AT MONROE, ORANGE COUNTY, NEW YORK

BY JOHN M. CLARKE AND W. D. MATTHEW

(*Abstract*)

In the State Museum at Albany is part of the skeleton of the mastodon from a postglacial peat-bog in Monroe County, New York. This find was investigated by Dr. Clarke, and with the mastodon bones were found a lower jaw, femur, etcetera, of a horse and part of the femur of a bear. The horse shows no characters to distinguish it from the domestic species, but the state of preservation of the bones indicates, although not positively, that it is not of recent introduction and is coeval with the mastodon. The bear femur appears to be too large for the black bear, *Ursus americanus*, and accords in size with the grizzly, *U. horribilis*, but it is not further determinable.

If these fossils are of the same age as the mastodon, they indicate (1) that native species of *Equus* survived the Wisconsin glaciation in eastern North America; (2) that a larger species of bear, perhaps the grizzly, existed at the same time and place.

Several very interesting restorations of Devonian marine life now on exhibition at the New York State Museum were then described by Dr. Clarke and illustrated with colored lantern slides.

RECENT RESTORATIONS OF FOSSIL INVERTEBRATES

BY JOHN M. CLARKE

Our use of the term "fossils" was discussed in the next paper and suggestions for its proper usage were offered. The paper was discussed by President Jackson and Dr. John M. Clarke.

THE "GOOD USE" OF THE TERM "FOSSIL"

BY RICHARD M. FIELD

For the benefit of the visiting paleontologists, Dr. Raymond described the local collections of fossils and announced arrangements for personally conducted tours through the various museums of Boston and vicinity.

PALEONTOLOGICAL COLLECTIONS IN THE VICINITY OF BOSTON

BY PERCY E. RAYMOND

(*Abstract*)

The Boston Society of Natural History has a small but interesting series of the fossils of New England, among which is one of unusual importance, the type of *Paradoxides harlani.*

There is no public exhibition of fossils at the Massachusetts Institute of Technology, but a number of collections may be consulted there, particularly the Pleistocene shells from about Boston.

At the Museum of Comparative Zoology at Harvard there are very large collections of invertebrate fossils and excellent series of some groups of vertebrates, especially fishes. Noteworthy among the invertebrates are the following: Schary and Barrande collections from the Paleozoic of Bohemia; Schultze collection from the Devonian of the Eifel; Haeberlein collection from Solenhofen; Dyer, Day, Taylor, and Walcott collections from the Ordovician and Silurian of America; Wachsmuth collection of crinoids and a very large collection of European cephalopods and other mollusca. Among the vertebrates are mounted specimens of moas and edentates, some unique birds from the Tertiary, amphibians and reptiles from the late Paleozoic of Texas and Scotland, etcetera.

The increasing use and the value of such microscopic organisms as the foraminifera in stratigraphic work was then described. The paper was discussed by Dr. John M. Clarke, with replies by the author.

VALUE OF FORAMINIFERA IN STRATIGRAPHIC CORRELATION

BY JOSEPH A. CUSHMAN

(*Abstract*)

The foraminifera have not been greatly used in correlation work in America; workers have been lacking and collections few. In recent years, however, the United States Geological Survey has accumulated many collections from the Coastal Plain region, Panama, and the West Indies which are rich in fossil foraminifera.

A study of some of these has already proved that the foraminifera are excellent for use in stratigraphic correlations, and further study will probably result in numerous detailed faunas which can easily be distinguished from one another.

The author of the next paper presented a discussion of the importance and significance of the various species of the gastropod genus *Orthaulax,* illustrating his remarks by lantern slides.

STRATIGRAPHIC SIGNIFICANCE OF ORTHAULAX

BY C. WYTHE COOKE

(*Abstract*)

Three species of *Orthaulax* have been described. The genotype, *O. inornatus* Gabb, which has been supposed to be restricted to the Oligocene, was recently rediscovered in beds of basal Miocene age (Baitoa formation) in Santo Domingo at what may be the type locality. It is known also from the Tampa "silex beds" (Oligocene). The stratigraphic range of *O. gabbi* Dall appears to be very nearly the same as that of *O. inornatus*, but the two species have never been found in the same beds. *O. gabbi* comes from the Chipola marl of Florida and from the Culebra formation of Panama. *Orthaulax pugnax* (Heilprin) is found in the Tampa "silex beds," the lower part of the Chattahoochee formation near Bainbridge, Georgia; the Antigua formation, and in the Culebra formation of Panama. Its known range is Middle and Upper Oligocene.

The species of *Orthaulax* at Aguadilla, Porto Rico, referred to by Dr. Maury under the nude name of *aguadillensis*, is abundant in beds supposed to bé of Miocene age, at Palo Copado, Azua Province, Dominican Republic, and it occurs also on the Island of Saint Croix. Its range is probably Miocene and Oligocene.

A Silurian horizon, apparently new to the Boston Basin, was indicated in the next paper, which was illustrated by the original specimens forming the basis of the determination.

PRESENCE OF UPPER SILURIAN SANDSTONE IN ESSEX COUNTY, NORTHEASTERN MASSACHUSETTS

BY A. F. FOERSTE

(*Abstract*)

In 1893 John H. Sears, curator of geology at the Peabody Academy of Science, in Salem, Massachusetts, published his Geological Map of Essex County, Massachusetts, in the Bulletin of Essex Institute. On the legend accompanying this map the areas colored yellow and numbered 18 are stated to consist of lime slate, quartzite, and sandstone, thus indicating the presence of clastic rocks. In the hope of finding fossils, the present writer, during the summer of 1894, visited the area southwest of Rowley and also that extending from Middleton northeastward toward Topsfield. Somewhere in the latter area specimens of *Leperditia* were found in a relatively soft quartzite or sandstone, but the writer was unable to identify the species or to refer them to a definite horizon. In 1918 the best specimens were submitted to Dr. R. S. Bassler, of the United States National Museum, and identified by him as belonging to the *Leperditia alta* group, thus suggesting the presence of strata of Cayugan or upper Silurian age.

As far as may be determined from memory, the specimens of *Leperditia* were found in a quartzitic rock forming an exposure about 10 square yards in area along the upper part of a hilly area, somewhere north of Howe station. From Howe station the main road to Topsfield starts off northward. About a mile from Howe station another road branches off southeastward for half a mile, and then curves northward across Nichol Brook. On the writer's copy of Sears's map the statement north 20° west occurs on the northwest side of the crossing of this branch road over Nichol Brook, and somewhere in this neighborhood the *Leperditia* are believed to have been found.

Evidently it is highly desirable that the *Leperditia* locality should be rediscovered. An attempt made during the Christmas holiday week of 1919 failed dismally, not even the quartzite being found. Under these circumstances, the writer no longer being situated favorably for further search, it has been deemed best to leave to others the task of rediscovering the *Leperditia* locality, and the only specimen left from the original collection made by the writer has been deposited with Prof. Percy E. Raymond, of Harvard University.

The *Leperditia* locality lies about 17 miles east of north from Boston. The desirability of rediscovering this locality has been increased by the discovery by Mr. Arthur Keith, in 1915, of numerous specimens of *Camarotœchia* and of a few other fossils about 10 miles northeast of the *Leperditia* locality, immediately south of Glen Mills, at the corner where the road to Rowley branches off eastward. Here the fossils occur at the base of a volcanic mud flow. Their age is regarded tentatively as low in the Devonian.

The great variation in plate structure in the cystid genus *Holocystites* and related genera was recalled to the members by the author of the next paper and the reason for their variation was explained. Doctors O'Connell and Jackson, Ulrich, and Foerste took part in the discussion following the paper.

INTERCALATION OF THECAL PLATES IN HOLOCYSTITES IN CONNECTION WITH THE CRITERIA UPON WHICH SPECIES CAN BE DISTINGUISHED

BY A. F. FOERSTE

(*Abstract*)

The genus *Holocystites* was founded by Hall on his species *Holocystites cylindricus*. This species is described by Hall as consisting of at least six encircling rows of hexagonal plates, each row containing eight plates, the plates of successive rows alternating with each other. In addition to these six encircling rows, there is at least one additional row at the base, and at the top of the theca there is a series of circum-oral plates. The number of species in the Racine of Wisconsin and northern Illinois, which are strictly congeneric with *Holocystites cylindricus*, includes at least *Holocystites abnormis* and *Holocystites alternatus*.

Holocystites alternatus is of special interest, since it departs from the pri-

mary simplicity of structure of the genotype in the presence of more or less numerous intercalated plates, resulting in a considerable variety of patterns in the mature specimens. Young specimens of this species, however, probably were quite typical in appearance. This is suggested by the fact that if the margins of all plates are reduced at the same rate, the smaller plates will disappear first, and then those next in size, until only the primary plates remain. These primary plates in that case, moreover, will then be arranged in transverse rows, eight in a row, in such a manner that if the plates were brought closer together those in successive rows would alternate with each other, and their interference on growth would tend to produce hexagonal outlines, as in *Holocystites cylindricus*.

Between the first and second transverse rows beneath the circum-oral plates, intercalation frequently is confined to single plates, inserted at the angles of the primary plates, resulting in pentagonal outlines in the intercalated plates. Additional intercalation at the apex of the latter results in the truncation of the latter, producing an hexagonal outline, the second intercalated plate being triangular. Similar simplicity of structure may occur between the second and third transverse rows of plates, and even between the third and fourth rows, counting downward from the top.

Beneath the third transverse row, in some specimens, intercalation may take place at three successive intervals, resulting in a more or less complicated pattern of small plates. Much less frequently complicated patterns of small plates occur also between the second and third transverse rows of plates and even between the first and second rows.

The primary plates of each transverse row usually remain in contact with each other laterally or are separated by two intercalated plates, one of which has wedged in from above and the other from below. In other words, additional plates appear in much greater numbers between the transverse rows of primary plates than between the adjacent primary plates of the same row. The growth of the animal within the theca appears to have produced tension chiefly in the axial direction, resulting, in *Holoycstites alternatus*, in frequent intercalation of plates, especially toward the base in some specimens. This resulted in a theca which was conspicuously elongated, especially toward the base.

In typical *Holocystites abnormis* a similar elongation of the theca was produced by the elongation of the individual primary plates without the addition of intercalated plates.

If *Holocystites scutellatus*, *Holocystites ovatus*, and *Holocystites winchelli* are to be regarded as genuine species of *Holocystites*, it is evident that considerable variation in the plate diagrams occurs also in each of these species, accompanied more or less with intercalation of additional plates.

It then becomes evident that the discrimination of species of *Holocystites*, in the presence of numerous specimens, is likely to prove a matter of more or less difficulty, especially if specific differences be established chiefly on the general appearance of the plate diagram and the form of the theca as a whole. In such cases it is probable that a much safer guide will be found in the ornamentation of the individual plates of the theca, such as a central boss, radiating ridges, the presence, size, and arrangement of the granules, and the like.

Further field-work in the stratigraphy of Anticosti Island during the past summer, leading to a revision of the lower part of the section, was then explained to the Society by the author, whose remarks were discussed by Messrs Schuchert, Vaughan, Ulrich, and Chadwick.

REVISION OF THE ANTICOSTI SECTION

BY W. H. TWENHOFEL

(*Abstract*)

The second formation of the Anticosti section is renamed the Vaurial from splendid exposures on the Vaurial River. The strata of Charleton Point, previously considered the type locality of this formation, have been proven to belong to the English Head formation. The discovery of this fact greatly increases the size of the English Head fauna and shows it to have a stronger Richmond aspect than was suspected. Evidence has been discovered indicating an uplift of the Saint Lawrence region prior to the time of English Head deposition and a second period of uplift at the close of Ellis Bay time or the close of the Ordovician.

Luncheon time having arrived, the Society adjourned for an hour.

At 2 p. m. the afternoon session was opened with an appreciation by E. M. Kindle of the life and work of Mr. L. M. Lambe, late vertebrate paleontologist of the Canadian Geological Survey. A memorial of Mr. Lambe is presented on pages 88 to 97 of this Bulletin.

REPORT OF THE AUDITING COMMITTEE

The committee now announced that the Treasurer's accounts of the receipts and expenditures of the Society were found to be correct as read. It was then voted by the members that the report be accepted.

Vice-President Van Ingen now took the chair and the Society listened to the presidential address, which was illustrated with lantern slides.

STUDIES IN VARIATION AND A PROPOSED CLASSIFICATION OF VARIANTS

PRESIDENTAL ADDRESS BY ROBERT T. JACKSON

Following the presidential address, the vertebrate paleontologists withdrew and formed a section for the presentation of their own papers. The minutes of this section are printed on pages 222 to 225.

The first paper on the general program was an interesting account, illustrated by lantern slides and drawings, of the peculiar fossil remains known as *Serpulites*. The paper was discussed by Messrs. Clarke, Foerste, Bassler, and the author, who presented diverging views as to the nature of these doubtful organisms.

HYDROZOAN AFFINITIES OF SERPULITES SOWERBY

BY W. ARMSTRONG PRICE

(*Abstract*)

Serpulites MacLeay (erroneously attributed to Murchison by writers), a Paleozoic genus, commonly provisionally placed with the tubicolous annelids. and to which have been referred conical tubes of a variety of form and composition, is restricted to chitinous tubes with marginal ridges appearing upon the flattened test. Several species formerly placed in the genus *Enchostoma* Miller are transferred to *Serpulites*.

A branching, tubular, plantlike organism from the Devonian of Paraná, Brazil, identical in the structure of individual tubes with tubes of *Serpulites*, appears to be closely related to the latter, as has been pointed out by J. M. Clarke.

Ruedemann has enumerated points of resemblance between *Serpulites* and *Conularia*, among which are the possession by the young of both forms of terminal disks for attachment and the possession by both of longitudinal ridges, which he interprets in the case of *Conularia* and *Serpulites* as thickenings of the test.

An unflattened test referred to *Serpulites* from the Pennsylvanian of West Virginia is described, which shows two internal, cylindrical bodies, composed of galena and other crystalline materials, lying beneath the marginal regions where the "ribs," or ridges, are seen upon flattened specimens of *Serpulites*.

The appearance of these marginal mineral deposits can not be considered to have a direct connection with the marginal ribs, if the latter are thickenings of the insoluble chitinous test. The suggestion has been offered that they may be replacements of internal structures composed of soluble materials, but no explanation of the presence of such problematical bodies is apparent. It is considered possible that the animal possessed two internal canals within the body tissue, and that, in the case of the specimen described, mineral sulphides and other salts were deposited in the canals in the presence of decaying organic matter. In the case of flattened specimens which show marginal welts in the flexible test, it is suggested that flattening took place before the complete decay of the animal body, and that the canals were more rigid than the intervening portion of the animal body.

If the latter interpretation should be confirmed by further investigation, the relation of *Serpulites* to the annelids would appear remote. The branching habit of the tubes and of the marginal ribs of the organism from Paraná suggest a possible relationship with the Hydroids and Graptolites, in which case canals can be more readily understood. It is possible that the significance of the internal bodies may be ascertained by a further study of unflattened tests. These are found chiefly in limestone.

Following this paper Dr. Kindle gave a preliminary account of his work in the basin of the Lower Mackenzie River and described the salient faunal and stratigraphic features of the section, about 5,000 feet thick, ranging in age from Middle Silurian to Upper Devonian. Professor Schuchert, in discussing this paper, called attention to the important stratigraphic and paleogeographic results of Dr. Kindle's work in this far northern area.

PALEOZOIC SECTION OF THE LOWER MACKENZIE RIVER

BY E. M. KINDLE

The last paper of the afternoon was an account of the Iowa Devonian echinoderms, given by the author and illustrated by numerous well selected lantern slides. It was discussed by Professor Jackson, who expressed his appreciation of the detailed studies of the author upon Iowa Devonian fossils.

ECHINODERMS OF THE IOWA DEVONIAN

BY A. O. THOMAS

(*Abstract*)

The investigations of Hall, White, Whitfield, Barris, Wachsmuth and Springer, and others have made known at least sixteen species of echinoderms collected in Devonian formations within the borders of Iowa.

The descriptions of the known forms are widely scattered through the literature; in this paper these are brought together and a few of the species are redescribed in the light of new material. A number of undescribed species in the Calvin Collection, together with several secured by the author during a study of the Iowa Devonian for the Iowa Geological Survey, brings the total number of species up to thirty. Four classes of echinoderms are represented, the most abundant being the crinoids. A brief analysis of the fauna follows.

The cystids are represented by two species of the genus *Strobilocystites*, one of them being new and differing from the other, *S. calvini* White, in having unbranched ambulacra, nodose ambulacral plates, and smooth thecal plates. These cystids occur in the Cedar Valley limestones and are relatively rare.

The blastoids include one pentremite, *Pentremitidea subtruncatus* (Hall), and three species of *Nucleocrinus*. Two of the latter are described by Barris in the Proceedings of the Davenport Academy of Science, and the third is new. The new species has a large calyx, which in cross-section is sharply stellate, and its very narrow ambulacra are elevated upon the longitudinal keel-like edges.

The crinoids, especially the camerates, are very common and in places their remains make a crinoidal limestone. Of the Melocrinidæ there are five species of *Melocrinus;* these are *M. nodosus* Hall, from near Iowa City; *M. tiffanyi* Wachsmuth and Springer, from Solon; a new species with extremely nodose plates from Brandon, and another with a large bowl-shaped cup and smooth plates from Rockford. *Dolatocrinus triadactylus* Barris, *Stereocrinus triangulatus* Barris, and a new *Stereocrinus* from Littleton with stout spines on the costals complete this family. The Batocrinidæ are abundantly represented by the well known *Megistocrinus farnsworthi* White. *M. latus* Hall and *M. nodosus* Barris are much less common. The Hexacrinidæ include the type and only specimen of *Hexacrinus occidentalis* Wachsmuth and Springer from

Davenport and a new, fairly large species from the Cedar Valley near Rockford. In the latter the two primibrachs are separate—a point which proves to be an exception in this rare North American genus. *Arthracantha*, which is found in the Hamilton, Portage, and Chemung of the Lake Ontario region, is represented by fragmentary remains from the Independence shale near Brandon.

The Flexibilia are rare. A new species assigned to *Dactylocrinus* is founded on a well preserved specimen from the Lime Creek shale at Bird Hill. It is close to *D. concavus* (Rowley) from the Craghead Creek shale near Fulton, Missouri, but has a granulose surface. The only other flexible crinoid is *Taxocrinus interscapularis* Hall from Buffalo.

Two species of Inadunata, *Synbathocrinus matutinus* Hall and *Deltacrinus barrisi* (Worthen), have been described from beds at Buffalo and Davenport respectively. A new form of *Decadocrinus* from Vinton, preserving the crown, and some imperfect specimens of *Cyathocrinus* and of a doubtful *Lecanocrinus* from the Lime Creek shale conclude this order.

The class Echinoidea contain very unique remains and they are abundant in certain zones in the Lime Creek shale. Unfortunately, the plates and spines of the tests are dissociated, but are still in such close proximity that little error, it is felt, is made in referring the separate parts to their approximate position in the test. Moreover, the various species have not been found commingled, but at different horizons and localities. Three genera, two of them new and the other hitherto unknown, from North America are recognized. They belong to the family Archæocidaridæ. *Devonocidaris* has small, remarkably thin, and fragile interabulacral plates, bearing a central tubercle and a few secondary tubercles; a scrobicule is fairly well defined and a basal terrace is present. Spines long and acicular. Teeth, braches, and other parts of the lantern well preserved. *Xenocidaris*, described by Schultze from the Devonian of Germany, is represented by a considerable number of trumpet- or club-shaped spines, whose slender shafts terminate distally in flat or concave apices surrounded by a marginal coronet of blunt spinules. These spines are specifically distinct from the Eifel species. The third genus is named *Nortonechinus*. Its heavy interambulacral plates are highly imbricate, indicating that the test had considerable flexibility. The primary spines are stout and they expand distally, until over the greater part of the test they are rendered polygonal by mutual contact; their apical faces are flat. These spines formed a protective covering over the test much as to the expanded spines of *Colobocentrotus atratus* Brandt of our modern seas. Ambulacral plates and the parts of the lantern are also preserved.

At 5.30 the Society adjourned for the day. In the evening the members participated in the annual dinner with the Fellows of the Geological Society of America.

SYMPOSIUM ON TEACHING OF PALEONTOLOGY

SESSION OF WEDNESDAY, DECEMBER 31

At 9 a. m. the Society met in general session with the Geological Society of America, to participate in the symposium on the teaching of geology and paleontology. The papers in this symposium by members of the Paleontological Society were as follows:

Charles Schuchert: American paleontologists and the immediate future of paleontology.

J. C. Merriam: The teaching of paleontology as fundamental to history, the humanities, and science.

H. F. Cleland: The general teaching problem of paleontology.

E. W. Berry: The teaching of paleobotany.

Stuart Weller: The teaching of invertebrate paleontology.

R. T. Jackson: The value and use of stages of development in teaching.

CONTINUATION OF PALEONTOLOGIC PAPERS

With the conclusion of the symposium, at 12.30 p. m., the Society adjourned for luncheon, convening again at 2 p. m., to continue the reading of papers, in two sections. Vice-President Van Ingen was chairman of the Section of Invertebrate Paleontology, while Mr. Granger continued as chairman of the Vertebrate Section.

In the Invertebrate Section the first paper of the afternoon was an account of bibliographic catalogue of Cambrian fossils of the world under preparation by the author, who explained the various phases of the work.

BIBLIOGRAPHIC STUDIES OF THE CAMBRIAN

BY CHARLES E. RESSER

(*Abstract*)

Because of the scattered nature of Cambrian data, much of which is inaccessible to many workers, it seemed advisable to work out and publish a summary of all available information. Such a work was begun several years ago and is now nearing completion. It comprises a bibliography of all papers on the Cambrian and a bibliographic synomony of the fossils, in the first part. The second part will consist mainly of a summary description and discussion of each Cambrian formation, taken up in a geographic order and with a list of the fossils. A correlation table of all formations will also be included.

Intercontinental correlations and the faunal similarities and identities of the Middle Cambrian rocks of Newfoundland and Great Britain were next presented by the author, who illustrated his remarks by charts. Discussed by E. O. Ulrich.

CORRELATION OF THE MIDDLE CAMBRIAN OF NEWFOUNDLAND AND GREAT BRITAIN

BY B. F. HOWELL

(Abstract)

Recent discoveries in Great Britain and southeastern Newfoundland indicate that the faunal succession of the Paradoxides beds of those two regions is remarkably similar. The three faunas occurring in Newfoundland, which are characterized by *Paradoxides bennetti*, *P. hicksi*, and *P. davidis*, are represented in Great Britain by the faunas characterized by *P. groomi*, *P. hicksi*, and *P. davidis*. A great many of the British species are represented in Newfoundland by nearly or quite identical forms with very similar ranges. As most of the Scandinavian faunas are represented in the British sections, and as most or all of the Massachusetts and New Brunswick faunas occur in Newfoundland (the Massachusetts *Paradoxides harlani* fauna, at the bottom of the Newfoundland section, the New Brunswick faunas, somewhat higher up), the relations between the northeastern North American and the northwestern European beds are now fairly well understood. Much work still remains to be done in both Great Britain and Newfoundland, and when this is completed we shall probably be able to correlate the beds of the two countries in even greater detail than at present.

The recent discoveries do not appear to throw much new light on the problems of the place or places of origin and the migrations of the Paradoxides faunas, but they do seem to prove conclusively that there was an easy route of migration between the British and Newfoundland regions during a large part of known Paradoxides time.

A third contribution dealing with the Cambrian followed, consisting of a description of the Upper Cambrian stratigraphy of Wisconsin, the faunal relations of the various formations, and the paleogeography of the time. This paper, presented by the author and illustrated by diagrams and charts, was discussed by P. E. Raymond.

CAMBRIAN FORMATIONS AND FAUNAS OF THE UPPER MISSISSIPPI VALLEY

BY E. O. ULRICH

New views on the body structure of the trilobites, resulting from the author's recent researches, were then explained and illustrated by lantern slides.

TRILOBITES AS ANCESTORS

BY PERCY E. RAYMOND

(Abstract)

When the appendages of modern Arthropoda are compared with those of trilobites, it is found that the latter represent a simple type from which it is possible that all others were derived. Analysis shows that the phyllopodan

limb is not primitive, and that the modern Branchiopoda are not so nearly allied to the trilobites as the Copepoda are. Relationships to the Arachnida, Diplopoda, Chilopoda, and Insecta are pointed out. A renewed study of the bodily form of the trilobite shows that it is not so specialized as has been supposed, and that it is capable of modifications which allow a consideration of these animals as the possible ancestors of the other Arthropoda.

The results of studies on the formation of coquina followed, with the author's remarks illustrated by lantern slides. Discussed by Dr. T. Wayland Vaughan, with replies by the author.

ORIGIN OF THE "BEACH ROCK" (COQUINA) AT LOGGERHEAD KEY, TORTUGAS

BY RICHARD M. FIELD

(*Abstract*)

Through the kindness of Dr. Alfred G. Mayer, the writer was enabled to visit the Tortugas group for the purpose of studying the conditions of sedimentation on and near the "coral" reefs. Among other studies, an attempt was made to discover the origin of the "beach-rock" or cemented shell-sands which occur between high and low tides. By means of a stand-pipe and pump, it was found that during heavy rains a shell key acts like a reservoir, and that the meteoric water (to which an appreciable amount of humus acid is added as it passes through the surface layers) dissolves $CaCO_3$ on its way down through the loose shell sands. The ground water, which lies just above the salt or brackish water zone, was found to contain 40 per cent more $CaCO_3$ in solution, or colloidal suspension, than the normal sea water. This concentrated solution of $CaCO_3$ has a strong cementing value, and is probably an important factor in the formation of the "beach-rock" where the ground water flows out through the beach sands, between tides.

A curious similarity of foraminifera in the Upper Vicksburgian of Mississippi and the recent waters of the Indo-Pacific region formed the subject of the next paper.

FORAMINIFERAL FAUNA OF THE BYRAM MARL

BY JOSEPH A. CUSHMAN

(*Abstract*)

A study of the foraminifera of the marl at the type station Byram, Mississippi, has shown a fauna consisting of nearly seventy (70) species. Nearly half of these are undescribed; of the others most of them are either now living or represented by a closely allied species in the shallower waters of the Indo-Pacific region. Some of the species are also found in the Mint Spring marl and in the Red Bluff clay, the lower members of the Lower Oligocene.

Dr. Vaughan next presented a digest of his stratigraphic studies on certain parts of the West Indies and Central America, illustrating his remarks by lantern slides.

STRATIGRAPHY OF THE VIRGIN ISLANDS OF THE UNITED STATES AND OF CULEBRA AND VIEQUES ISLANDS

BY THOMAS WAYLAND VAUGHAN

(*Abstract*)

The following are the conclusions expressed in this paper:

(1) The presence of shoal water deposits of Upper Cretaceous age, in Saint Croix and in the islands on the Virgin Bank from Saint John to Porto Rico and in Porto Rico, shows that the major tectonic axis of this part of the West Indies antedates Upper Cretaceous time, because there was an antecedent basement on which these deposits were laid down. I have recently suggested that these major trends may be even as old as late Paleozoic.

(2) During Upper Cretaceous time it is probable that most, perhaps all, of the areas now occupied by land were under water, and that there was considerable volcanic activity is proven by the water-laid tuffs and lava flows which are interbedded with the shoal-water calcareous sediments.

(3) In early Tertiary, probably Eocene, time there was mountain-making by folding, which in places was so intense that the stratified rocks were left in an almost vertical position, and both the sediments and the older igneous rocks were highly metamorphosed. There were also intrusions of diorite, dolerite, and granite, and probably the extrusion of some volcanic rocks. West of the Virgin Islands, there was during later Eocene time extensive submergence in Santo Domingo, Haiti, and Cuba, as is attested by the Upper Eocene limestones now above sealevel in those areas.

(4) The episode of mountain-making was followed in the Virgin Islands by one of prolonged subaerial erosion, and the production of the Virgin Bank apparently may in large part be assigned to this period of the history of the region. It seems that the axial islands on the Virgin Bank and the Central Sierras of Porto Rico, from its east to its west end, have continuously stood above the water since the close of Cretaceous deposition. In Saint Croix by Middle Oligocene time erosion had proceeded far enough to reduce almost to baselevel the tightly, steeply folded strata of the mountains.

(5) In Middle Oligocene time a large part of Saint Croix was submerged and, with slight fluctuations, remained under water until some time during the Miocene. Although both the northern and southern, but not the axial, parts of western Porto Rico were submerged in Middle Oligocene and probably in Lower Oligocene time, the eastern end of Porto Rico and the axial islands of the Virgin Bank west of Anegada Island were not submerged. The age of the limestone on Anegada Island is not known. These facts mean that there was differential movement, the movement being greater toward the west than in the central part of the bank. In Lower Miocene time the northern shore of Porto Rico east of San Juan was submerged, as were also the southern shore and eastern end of Vieques Island; both the northern and the southern edges of the bank were submerged probably by marginal down-flex-

ing. Although there are corals in the exposed sediments of Oligocene and Miocene age, and corals were therefore constructional agents during those epochs, their work, as compared with that of other agents, was of minor importance. If the work of these organisms in forming deposits concealed under water can be evaluated by their work in deposits exposed to view, the conclusion would be drawn that they played only a minor rôle in the formation of the Virgin Bank. There is as yet no evidence showing intense deformation during later Oligocene time in the Virgin Islands and Porto Rico, such as is known to have taken place in Santo Domingo.

(6) Subsequent to early Miocene time there has been uplift, greater along the axis of Porto Rico and the Virgin Bank than on the flanks, bringing Miocene and older Tertiary sediments, in places where they are present, above sealevel. The Tertiary sediments are tilted and gently flexed, but they have not been deformed as the Upper Cretaceous deposits. It is about this time that the land connections permitting migration of land animals from Anguilla to Porto Rico, Haiti, and Cuba seem to have existed. Saint Croix seems to have been connected with Anguilla, Saint Martin, and Saint Bartholomew.

(7) The period of high stand of land was followed by an episode of block-faulting, such as I have several times described recently. By faulting, Anegada Passage between the Virgin Bank and Anguilla was produced, and the islands assumed very nearly the outlines and arrangements of today.

(8) Subsequent to the episode of faulting, there was emergence of the land and terracing of the margins of the Virgin Bank, followed by submergence. In places in Porto Rico and along the Cordilleras reef, which extends eastward from the northeast corner of Porto Rico, there has been local emergence due to differential crustal movement.

(9) The living coral reefs on the Virgin Banks are growing on an extensive flat in a period of geologically Recent submergence. This flat is geologically an old feature. Its origin, in large part at least, may reasonably be attributed to the long period of erosion following early Tertiary mountain-making.

There was then presented a stratigraphic paper transferred from the program of the Geological Society of America:

GEOLOGIC RECONNAISSANCE IN SANTO DOMINGO [1]

BY C. WYTHE COOKE

(Abstract)

During the spring and early summer of 1919 a preliminary reconnaissance of Santo Domingo was made for the Dominican Government by T. W. Vaughan, D. D. Condit, C. P. Ross, and the writer, under the direction of Dr. Vaughan. The central mountain system was crossed by three routes and as much of the areas north and south of it was examined as was feasible in the time allotted. Many fossils, chiefly from the Tertiary formations, were collected, and have been distributed to specialists for study. The data obtained, al-

[1] Published by permission of the military governor of the Dominican Republic.

though incomplete, are sufficiently exact to block out the major features of the topography and geology.

The Cordillera Central, a rugged mountain system, extends from the eastern end of the island westward into Haiti. It is flanked on the north by the Cibao Valley, a fertile valley extending from Samaná Bay to Manzanilla Bay. The Cordillera Setentrional, containing some peaks 4,000 feet high, lies between the Cibao Valley and the Atlantic. South of the Cordillera Central a coastal plain borders the Caribbean as far west as Calderas Bay. The Aźua Plain and the great Valley of San Juan separate the Cordillera Central from the mountains of the Sierra de Neiba and the Sierra de Martín García. Lake Enriquillo, 144 feet below sealevel, occupies part of the depression called the Enriquillo Basin, between the Sierra de Neiba on the north and the Sierra de Bahoruco on the south. Little is known of the southern peninsula.

The basal complex, consisting chiefly of metamorphic and igneous rocks, is similar to the corresponding formations of Cuba, Porto Rico, and the Virgin Islands. It makes up a large part of the Cordillera Central and Samaná Peninsula. Rocks of known Cretaceous age occur in the southern part of the Cordillera Central, between Santo Domingo City and Azua, and in the faulted front range of the Cordillera Setentrional near Demajagua.

The Tertiary formations and their supposed equivalents are named in the correlation table. All the names of Dominican formations in the table are new except the Cercado formation and the Gurabo formation, which were very recently proposed by Dr. Maury.[2] Lower and Middle Eocene sediments appear to be lacking in Santo Domingo, but from Upper Eocene upward there is a nearly complete sequence. Limestone of Upper Eocene age is widely distributed on both sides of the Cordillera Central, but appears to be most extensively developed in the mountains of Barahona Province. Two formations of Oligocene age in the north have been given names, and rocks of the same age have been found also in the south. The Lower and Middle Miocene are represented by the thick series of conglomerates, silts, and coralliferous limestones comprising the Yaque group, and the upper Miocene, not at present known elsewhere in the West Indies, appears to have left its trace in the gypsum, salt, and clastic sediments of the Las Salinas formation. The Las Matas gravels are supposed to be of Pliocene age, but no fossils have been found in them.

Pleistocene deposits—gravels, soft limestones ("caliche"), and raised coral reefs and beaches—are widely distributed around the island.

In addition to rocks of igneous origin included within the basal complex, there is a considerable variety of eruptive and dike rocks, tuffs, and agglomerates, the youngest of which is not older than Pleistocene. These late igneous rocks are most conspicuous in the Province of Azua.

Faults are numerous and outline many of the topographic features. Folding, except in the basal complex, is less important.

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[2] C. J. Maury: Science, new series, volume 50, December 26, 1919, page 591.

Tentative Correlation Table of the Tertiary Marine Formations of the Dominican Republic

by C. W. Cooke, J. A. Cushman, and T. W. Vaughan

American Time Subdivisions		Santo Domingo North Side	South Side	Other American equivalents	European Time Subdivisions
Pliocene			Las Matas gravels	Panama, Jamaica, Cuba, and Costa Rica	Sicilian Astian Plaisancian
Miocene	Upper		Las Salinas formation	Not known elsewhere in West Indies. Yorktown and Duplin formations of Virginia and North Carolina	Pontian Sarmatian Tortonian
	Middle	Yaque Group: Valverde clay Mao adentro limestone Gurabo formation (*Sconsia lævigata* zone)	Yaque group	La Cruz marl of Cuba and Calvert, Choptank, and Saint Marys formations of Maryland and Virginia	Helvetian
	Lower	Yaque Group: Cercado formation (*Aphera islacolonis* zone) Baitoa formation and Bulla conglomerate	(Divisible)	Bowden marl of Jamaica Chipola marl of Florida; Eastern Porto Rico; Saint Croix	Burdigalian
Oligocene	Upper	Cevicos limestone	East of Baní and elsewhere	Anguilla; Saint Croix; Tampa	Aquitanian Chattian
	Middle	Tabera formation[3]		Antigua; Saint Croix; Bainbridge, Georgia	Rupelian
	Lower			Vicksburg group	Lattorfian
Eocene	Upper	Limestone at Demajagua and elsewhere	Limestone in Sierra de Neiba, Sierra de Bahoruco, and elsewhere	Saint Batholomew Island; Plaisance Island of Haiti;[4] Ocala Island; Jackson formation	Ludian Bartonian

[3] May include lower Oligocene. [4] Vaughan, unpublished.

A paper explaining the laboratory method of teaching paleobotany at Brown University was then delivered by the author, who illustrated her remarks with copies of a laboratory manual prepared by her for the purpose and distributed among the members.

NOTES ON THE TEACHING OF PALEOBOTANY

BY MARION D. WESTON[1]

A study of the growth of the arms in the cystid genus *Caryocrinites*, illustrated by lantern slides, was then presented by the author under the title

METHOD OF APPEARANCE OF ADDITIONAL ARMS ON INCREASING AGE IN CARYOCRINITES

BY A. F. FOERSTE

(*Abstract*)

The youngest known specimens of *Caryocrinites ornatus* Say from the Rochester shale of New York possess three subtegminal food grooves, each terminating in a single arm. Older specimens may possess 14 or more well developed biserial arms at the ends of a corresponding number of branches of the subtegminal food groove system. From this it is evident that the food grooves branch within the theca, beneath the tegmen, and that there is some method by which the food grooves pass through the theca and reach the surface. On examining each articulating surface at which an arm was attached to the theca, this is found to rest on three small plates at the margin of the tegmen, the passage for the food groove being at the junction of the three plates. A second passage, much smaller in size and apparently representing the axial canal, is located a short distance farther down, between the two lower plates forming the articulating surface.

Few specimens retain the arms. Usually only the articulating surfaces for their attachment are observed.

In addition to the articulating surfaces for the attachment of the arms, practically all specimens, both old and young, possess small depressions in their immediate vicinity. At the base of each of these depressions there is a small passage located between two of the marginal plates of the tegmen and connecting with the food-groove system. The larger of these depressions frequently are marked along the median line by a single low ridge, arranged in a radial direction and suggesting the former attachment here of some small arm or armlike appendage. The smaller depressions show no evidence of articulation with anything.

The earlier of these depressions usually appear in accordance with a readily recognized system. In the youngest known specimens, with only three distinct arms, one depression makes its appearance on the sinistral side of each of

[1] Introduced by R. S. Bassler.

the three primary arms; later an additional depression is found on the dextral side of each arm. Each depression is connected with the food-groove system by a pore passing between two or three of the marginal plates of the tegmen. At first, the depressions show no evidence of articulation with anything. Later, as they grow in size, the articulating ridge mentioned in the preceding paragraph makes its appearance. Still later, articulating surfaces for the attachment of full-sized arms are found where younger specimens show only depressions, but in the meantime additional small depressions appear along some of the neighboring sutures between the more or less marginal plates of the tegmen, and the latter, in time, also developing articulating ridges and then articulating surfaces, until 14 or more articulating surfaces for full-sized arms are present.

In this process it is probable that well developed arms with biserially arranged plates were accompanied by much smaller arms with plates in their more initial stages; and the still younger arms, at their first appearance exterior to the theca, may have been free from protective plates altogether.

Since even the largest and most mature specimens of *Caryocrinites* show depressions in addition to the articulating surfaces for fully developed arms, it is possible that some of the later developed arms functioned genitally and did not serve merely for the conveyance of food.

The argument for the recognition of the Paleocene as a distinct epoch of the Tertiary was presented briefly by the author.

STATUS AND LIMITS OF THE PALEOCENE

BY W. D. MATTHEW

(*Abstract*)

The term Paleocene has been revived by several vertebrate paleontologists in recent years to cover the faunal zones previously known as Basal Eocene. Upon evidence of the vertebrate faunas it is entitled to rank as a distinct epoch, coordinate with the Eocene and Oligocene. It includes the Puerco, Torrejon, Fort Union, and probably certain less known vertebrate faunas in this country, and the Cernaysian in France. Its upper limit is marked by the first appearance of the principal modern orders of mammals and of certain modern groups of reptiles simultaneously in Western America and in Western Europe. The lower limit is more doubtfully fixed by the first appearance of placental mammals. The evidence of marine invertebrates and of plants does not at present appear to support the distinction of the Paleocene as a separate epoch. It is possible, however, that it covers the gap between the Cretaceous and Tertiary insisted upon by many stratigraphers and paleobotanists, and there are other possible interpretations that might reconcile the evidence. The writer believes that the epoch may also prove to include the Lance and certain other dinosaur-bearing formations, and that it may belong rather to the Cretaceous than to the Tertiary period. No final conclusions are in order until the evidence in different fields has been satisfactorily reconciled.

STUDY OF THE LIFE PROCESSES IN FOSSILS

BY R. S. BASSLER

The last paper of the session was a short discussion of the methods of recognizing the life processes in fossils, and of the importance of taking these factors into account in the discrimination of species. The remarks were based mainly upon the cyclostomatous bryozoa, where species exactly alike externally show by their ovicells, or organs of reproduction, that they belong far apart.

The following papers were read by title:

SPONGES OF THE MIDDLE CAMBRIAN

BY CHARLES D. WALCOTT

INORGANIC EVIDENCES OF DISCONFORMITIES IN LIMESTONE

BY RICHARD M. FIELD

At 5.30 the Society adjourned.

Minutes of Sectional Meeting of Vertebrate Paleontology

The section convened at 3.30 p. m. Tuesday, December 30, at the conclusion of President Jackson's address, with Walter Granger as chairman and Messrs. Matthew, Gregory, Camp, Lull, Loomis, Troxell, Thorpe, Chaney, Sinclair, Stock, and Buwalda present.

The following papers were then presented, the first one, on paleobotany, being included in this section on account of relationship to the vertebrate faunas of the same region:

FURTHER DISCUSSION OF THE ECOLOGICAL COMPOSITION OF THE EAGLE CREEK FLORA

BY RALPH W. CHANEY

(Abstract)

The Eagle Creek flora of the Columbia Gorge region is made up largely of dicotyledonous arboreal forms, and the flora as a whole bears a strong resemblance to that now occupying the northeastern United States. Its most notable feature is a mixture of mesophytic and xerophytic types, the former dominating in number of species, the latter in number of individual leaves. This mixture is interpreted as being due to a varied topography during the Eagle Creek epoch—an interpretation which suggests a bajada mode of origin for the Eagle Creek formation.

NEW MOUNTS IN THE PRINCETON GEOLOGICAL MUSEUM

BY WILLIAM J. SINCLAIR

(*Abstract*)

Lantern slides illustrating mounted skeletons of the Bridger Eocene creodont *Mesonyx obtusidens* and the White River Oligocene cursorial rhinoceros, *Hyracodon nebrascense*, were exhibited. These mounts, of the most modern panel type, are of interest because they embody the only practically complete skeletons of both animals yet known. The material is not new. The *Mesonyx* skeleton was found in 1885 and the *Hyracodon* skeleton ten years later, and both have been described at length by Prof. W. B. Scott. The new mounts bring out the highly cursorial character of these animals. In bodily proportions, *Mesonyx* closely resembles a modern wolf, differing, of course, in the shape of the head and in the structure of the feet, while *Hyracodon* approaches, in its limb structure, some of the larger three-toed horses of the Upper Oligocene and would, undoubtedly, have been monodactyl, like the modern horse, had its line persisted.

STUDY OF THE ENTELODONTS

BY EDWARD L. TROXELL

(*Abstract*)

A summary of the features of several new species of entelodonts in the Marsh Collection. It is argued that the wear of the teeth indicates a complex movement of the mandible, and that this is made possible by the use of the peculiar processes from the jaw and molar arch, which are so characteristic of the family.

MOUNTED SKELETON OF MOSCHOPS CAPENSIS BROOM

BY WILLIAM K. GREGORY

(*Abstract*)

This dinocephalian reptile from the Permian of South Africa is represented in the American Museum collections by the remains of numerous skeletons, all more or less incomplete, found in one locality by Dr. Broom in 1910. When sorted according to size and age by Dr. Broom, it was found that three of the individuals afforded knowledge of the principal elements of the adult skeleton. The missing parts of one of these have since been carefully restored and the whole animal has been mounted for exhibition. Remarks were made on other dinocephalian genera and on the characters of the group as a whole.

SMALL MAMMALS IN THE MARSH COLLECTION AT YALE UNIVERSITY

BY EDWARD L. TROXELL

(*Abstract*)

Three Oligocene specimens of unusual interest were illustrated by lantern slides and briefly discussed. *Hypisodus* sp.? is a very small deer with many

very progressive characters, but of uncertain taxonomic position. *Ictops dakotensis* and *Palæolagus haydeni* are represented by skulls and skeletal parts which, because of their perfection, will add to our knowledge of these tiny creatures.

TERTIARY ARTIODACTYLS FROM THE MARSH COLLECTION

BY RICHARD S. LULL

(Abstract)

Numerous remains of a small horned antelope, apparently new to science, collected by the Yale Museum expedition of 1914 in western Nebraska, are described, together with a novel method of combining the mounted skeleton of the animal with the plastic restoration in the flesh. No characters appear which debar this form from direct ancestry with the existing prong-buck, *Antilocapra*. A new species of *Blastomeryx*, contemporaneous with the above, of which the type skull was collected by Professor Marsh in 1872, is also described.

At 5.30 p. m. the section adjourned for the day.

On Wednesday, December 31, the sectional meeting was resumed at 2.30 p. m. and the following papers were presented:

OLIGOCENE EQUIDÆ IN THE MARSH COLLECTION

BY JOHN P. BUWALDA

(Abstract)

A preliminary statement indicating that the Oligocene Equidæ material in the Marsh collections, derived from the John Day region of eastern Oregon and from the Great Plains, and consisting of several complete skulls and a considerable quantity of teeth and partial jaws, represents about half of the described species of *Mesohippus*, *Miohippus*, and *Archeohippus*. At least two forms are recognized as new. The genus *Mesohippus* appears to grade insensibly into the genus *Miohippus* in the material studied.

PAWNEE CREEK BEDS OF COLORADO

BY F. B. LOOMIS

(Abstract)

This paper takes up the position of the Pawnee Creek Beds in the series and particularly points out that they should be divided into a lower bed, "Pawnee Creek," and an upper level, which is of Pliocene age.

NEW SPECIMEN OF THE PLEISTOCENE BEAR ARCTOTHERIUM FROM TEXAS

BY W. D. MATTHEW

(Abstract)

A specimen found by C. H. Sternberg, during the past season, in the Rock Creek formation, Briscoe County, Texas, two miles north of the famous

Equus quarry discovered by J. W. Gidley. It consists of the hinder half of the skeleton, very perfectly preserved, which exceeds in size the largest living brown bears. The specimen has recently been acquired by the American Museum.

NOTHROTHERIUM SHASTENSE, A PLEISTOCENE GROUND SLOTH OF NORTH AMERICA, WITH REMARKS ON THE MEGALONYCHIDÆ

BY CHESTER STOCK

At 4 p. m. the section adjourned and resumed meeting wth the general Society.

Register of the Boston Meeting, 1919.

Anderson, Robert
Bassler, R. S.
Bell, W. A.
Burling, L. D.
Buwalda, John P.
Chadwick, G. H.
Chaney, R. W.
Clarke, J. M.
Cleland, H. F.
Cooke, C. W.
Coryell, H. N.
Cushman, J. A.
Field, R. M.
Foerste, A. F.
Galloway, J. J.
Goldring, W.
Gordon, C. E.
Grabau, A. W.
Granger, Walter
Gregory, W. K.
Hartnagel, Chris
Haynes, W. P.
Howell, B. F.
Hyde, Jesse
Jackson, R. T.
Jeffrey, E. C.
Kindle, E. M.
Lee, W. T.
Loomis, F. B.
Lull, R. S.
Matthews, W. D.
Merriam, J. C.
O'Connell, M.
Price, W. A.
Raymond, P. E.
Resser, C. E.
Schuchert, C.
Shimer, H. W.
Sinclair, W. J.
Stock, C.
Talbot, M.
Thomas, A. O.
Thorpe, M. R.
Troxell, E. T.
Twenhofel, W. H.
Twitchell, M. W.
Ulrich, E. O.
Van Ingen, G.
Vaughan, T. W.

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CORRESPONDENT DECEASED

Koken, E., died November 24, 1912.

MEMBERS DECEASED

Calvin, Samuel, died April 17, 1911.
Clark, William B., died July 27, 1917.
Barrell, Joseph, died May 4, 1919.
Derby, Orville A., died November 27, 1915.
Eastman, Charles R., died September 27, 1918.
Fontaine, William M., died April 30, 1913.
Gill, Theodore N., died September 25, 1914.
Gordon, Robert H., died May 10, 1910.
Harper, George W., died August 19, 1918.
Hawver, J. C., died May 15, 1914.
Lambe, Lawrence M., died March 12, 1919.
Lyon, Victor W., died August 17, 1919.
Prosser, C. S., died September 11, 1916.
Seely, Henry M., died May 4, 1917.
Waring, Clarence A., died November 4, 1918.
Williams, Henry S., died July 31, 1918.
Williston, Samuel W., died August 30, 1918.

MEMBERS-ELECT

Camp, Charles L., American Museum of Natural History, New York City.
Chaney, Ralph W., Old Science Hall, Iowa City, Iowa.
Clark, Thomas H., South Weymouth, Mass.
Coryell, Horace N., Columbia University, New York City.
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Hubbard, Bela, Queens College, Kingston, Ontario.
Hume, George S., Milton, Ontario, Canada.
Thorpe, Malcolm R., Peabody Museum, New Haven, Conn.
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Minutes of the Ninth Annual Meeting of the Pacific Coast Section of the Paleontological Society

Chester Stock, *Secretary*

The ninth annual meeting of the Pacific Coast Section of the Paleontological Society was held in conjunction with the Cordilleran Section, Geological Society of America, and the Seismological Society of America, at Pasadena, California, June 20, 1919. The societies participated in the third annual meeting of the Pacific Division, American Association for the Advancement of Science. The meeting of the societies in paleontology, geology, and seismology was called to order by Prof. G. D. Louderback at 9.30 a. m., in Room 205, Throop College of Technology. Prof. B. L. Clark presided while several of the paleontological and geological papers were given.

ELECTION OF OFFICERS

The following officers were elected for the ensuing year:

President, John C. Merriam, University of California.
Vice-President, Earl L. Packard, University of Oregon.
Secretary, Chester Stock, University of California.

TRIBUTE TO C. A. WARING

It was moved and carried that the Secretary record in the minutes the regret of the death of Mr. C. A. Waring, a member of the Paleontological Society and lately associated with the State Mining Bureau of California, and to express appreciation of the work and accomplishments of Mr. Waring in the field of invertebrate paleontology of the Pacific coast.

READING OF PALEONTOLOGICAL PAPERS

The following paleontological papers were then read:

STRATIGRAPHIC AND FAUNAL RELATIONS OF THE MEGANOS GROUP (MIDDLE EOCENE), CALIFORNIA

BY B. L. CLARK

CENOZOIC HISTORY OF THE GROUND SLOTH GROUP

BY CHESTER STOCK

(*Abstract*)

The gravigrade edentates afford an interesting example of mammalian evolution during the Cenozoic in America. They are intimately associated with the Tertiary and Pleistocene histories of North and South America and indicate former land connections between the two continents, with evidence as to periods of such connections. Genera of the ground sloth group, known from the Miocene, Pliocene, and Pleistocene, arrange themselves in phylogenetic series illustrating principles of paleontology.

Mr. J. Z. Gilbert spoke concerning some recent investigations of the fossil fishes of California.

Following the presentation of the program the meeting adjourned.

LUNCHEON AND EXCURSIONS

Members of the Paleontological Society partook of luncheon served, through the courtesy of the Throop College of Technology, to the Pacific Division, American Association for the Advancement of Science, on Thursday and Friday, June 19 and 20, 1919. Members of the Society also attended a dinner of the Western Society of Naturalists on Friday evening, June 20, 1919, at the Maryland Hotel, Pasadena.

Under the direction of Dr. Frank S. Daggett, Director of the Museum of History, Science, and Art, in Los Angeles, California, an excursion was made to the Pleistocene asphalt deposits of Rancho La Brea. Later the visiting members returned to the museum and viewed the collections of vertebrate remains secured from the asphalt beds.

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
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EARTH SCIENCES AS THE BACKGROUND OF HISTORY[1]

PRESIDENTIAL ADDRESS BY JOHN C. MERRIAM

(Delivered before the Society December 29, 1919)

CONTENTS

Introduction

The present day of great concepts is witnessing expansion of our vision so as to include in each personal relation the wider contacts of community, state, and race. We attempt with shifting view point to visualize the complicated political, economic, and psychologic factors determining attraction and repulsion of the major human groups. Most of us now consider national politics in terms of their international significance, and see great problems of the people projected against the background of interlocking world relations. It is clear that this enlarged vision must for the future be the normal view. Unless by some political cataclysm civilization breaks down completely, we shall retain our close connection with other nations, and cannot return to the kind of isolation possible in the age before space was narrowed by electricity and steam, and extensive international trade pointed the way to more efficient use of the world's resources.

With the widening of our mental vision, space relation is not the only element to change. We see needed readjustment of national boundaries and regrouping of peoples brought about by reason of conditions antecedent to present time by long or short periods. More clearly than ever before is it necessary for us to view world affairs, and in them our own connections, by the perspective of space plus time. It may be that by reason of selfish political and economic motives we find here and there

[1] Manuscript received by the Secretary of the Society January 24, 1920.

expression of judgment giving evidence of geographical and historical blindness, but such is not now the normal sight of the average man.

We have, moreover, come to see that history, or depth of view, is not merely a continuity with which we must reckon, but that there appears in it the evidence of calculable movement, subject to law, and reaching forward to express itself in the trend of present and future affairs.

Concept of History

The concept of history as generally accepted has undergone extraordinary changes in recent years. History as read and taught has expressed only in part the broader relations of events with the view to indicating their true bearing on the present. In its origin as a constructive science much of history was concerned with the emotional side of national propaganda. In varying measure, in our own and in other countries, it has been the instrument of propagandists, aiming to promote a nationalistic spirit which makes for the strength of one group without regard to right relations outside. Fortunately, we *do* find here and there interpretations which have clearly stated the continuity of events, their real relations and significance in the world sense, and their proper trend.

Not less insufficient than the use to which history has been put is in many instances the structure of the account presented. Continuity has not always been the fundamental factor. Descriptions of events in series, but unrelated, have often formed the basis for discussion, and fundamental laws or scientific principles have not always played an important part.

Reaction against the incomplete view of historical study is in some measure due to application in human affairs of the hypothesis of evolution or development growing out of the fundamental historical sequence of geology as presented by Lyell and applied in the broad biological concept of Darwin. Assuming that man remains on a constant level, representing the type as created, human history might show indefinite fluctuations of movement; or it might be cyclic, each cycle representing approximately the same plane of development. According to the evolution hypothesis, the trend of the living world would be toward the more specialized, or more complicated, or more advanced. Although it might be cyclic, each cycle would represent a higher stage, and the path would be spiral. According to the developmental or evolutional interpretation, every part of a historic sequence is related to every other part, and each feature of past series contributes somewhat to the interpretation of the present. This concept gives us for every portion of historic succession a formula, through which, with a certain degree of accuracy, the line may be projected beyond the present. Viewed in this light, history becomes not merely a teacher by comparison or by analogy, but interprets the development of present conditions, and also furnishes a key to the future.

Rarely has the range of historical account included all influences actually involved. Largely by reason of the fact that the world is so complicated, there is no connected statement which shows the happenings as a whole with their interlocking connections. All records are merely pieces, or pieces of pieces, limited to one phase of the subject, limited to one portion of the world, and limited to a small space of time without relation to what precedes or follows. True world history scarcely exists.

Analysis of the elements composing the fabric of history, considered in its enormous complication and as a world problem, shows that there can be no question regarding the need for every element of knowledge which may be brought to bear, for interpretation of our present situation and requirements, and for guidance of mankind in decisions on those greater problems demanding for their proper settlement a foresight reaching over periods including more than a single generation. We need light with increasing brilliance and in many dark corners.

Present world questions will be solved in part by men who trade and men who study those who trade, in part by those who do politics and those who rule, and those who study rulers and politicians. But the only view that can show us where we are and whither we ride is one that, with other items, includes at least the outlines of the path over which we have come.

Purpose of Paper

The point of my story in this paper is that the farther back we see the path clearly, and the better we know our progress over it, the more certain we are to eliminate the minor curves and determine the true direction and the rate of speed to expect.

I am suggesting that the deepest view of history is desirable for purposes of most fundamental decisions; that, no matter how far back this vision leads us, if it continues to add to knowledge of what we are by showing us how we came to be, it is desirable and should be secured.

Contribution of the Sciences to History

The sciences especially concerned with historic sequence are astronomy, geology, geography, paleontology, biology, and anthropology. Astronomy, with its broad conceptions of stellar evolution, concerns us because it discusses the origin and early history of our planet. Geology and geography deal directly with the earth. Paleontology, representing biological history, must go to geology for its record. Anthropology has, as one of its most important phases, the history and origin of man.

The field of the astronomer, with its myriad bodies of the heavens, presumably represents wide range in stage of development of the stellar systems within our view. Yet, with all our information as to the stages through which these bodies may proceed in their evolution, there is but little positive evidence on which we may depend. We may note modifications in the surface of the sun or in the clouds of Jupiter, or we may observe the varying brightness of the stars, but there is little in these variations which we can consider as more than incidental fluctuation. Our knowledge of evolution of the stellar universe must depend largely upon comparisons of stars of various types, or of groups of stars and nebulæ which we assume to represent incipient stellar systems. The nebular hypothesis, which has served as the type of evolution of the solar system and as a basis for an interpretation of the origin of the earth, is called in question to such an extent as to be no longer acceptable to a large group of astronomers. The planetesimal hypothesis, developing similar world systems out of spiral nebulæ, seems also to suffer under recent criticism. For practical comparisons in study of world evolution, we appear to have one of the most important sources of information in the history of our own planet. For the universe in the large we can prove little more than that there is shown a process of development for which infinite time is required and in which definite cycles are determined.

Our greatest scientific contributions to study of history and of origins have come through geological and biological investigations. Geology is the greatest of all historical sciences. From comparative and experimental studies alone biology makes large contribution, but its distinctly historical phase lies in the field of paleontology, in which the life record is read from the geological book. To geology and biology, furnishing together the life records, anthropological history must be added, reaching back, as it does, into geological history and expressing the beginnings of our account of human life and activity in terms of geology and paleontology.

For the purposes of this paper, geological history may be divided roughly into two portions, one represented in the known section of stratified rocks formed through the piling up of sediments and by the outwelling of molten material spread on the surface or squeezed into the strata. An earlier period expresses in a more doubtful manner the partly astronomic history of the earth antecedent to the record presented by the lowest or earliest known strata.

The astronomic period of our earth's history is a subject for investigation by astronomer, physicist, and geologist. As yet the results of studies in this region are in large part of a speculative nature. The field furnishes one of the most attractive opportunities in science for further investigation. Although this phase of the problem has in it very much of fascination, the contributions are as yet of such a nature as to contribute little toward the objects of the present discussion. I shall therefore refer to geologic history only in terms of the distinct record extending to the lowest known strata in the second chapter of the account.

The length of the period which remains after elimination of the earlier, or astronomic, period may be very short, measured against the total age of the earth. We know that the lowest strata, wherever we find them, rest upon rocks which have been molten and in their molten state have destroyed the basement upon which the oldest known stratified rocks now rest. We admit, therefore, that not only have we lost the record before the earliest strata were formed, but that the earliest strata themselves have disappeared. The record remaining is, however, by no means brief in terms of human understanding. Few recent estimates have suggested that the section comprises less than two hundred thousand feet of strata, or that the time involved measures less than one hundred million years. This time may not be long compared to the entire age of the earth, and may not be more than a moment compared with the age of our solar system, but it furnishes all that we require for purposes of interpretation of human history.

Reduced to their simple terms, the geological data of the stratified record give us a history of the accumulation of sediments, of movements of the earth's crust, of the making of continents and ocean basins, of volcanic activities, and of climatic changes. In this evidence is included a history of the forming of mountains, and the wearing down of mountains and continents by the processes of erosion to form the sediments spread over great basins of accumulation.

This history presents, as its first significant lesson, the fact of the instability of the earth's crust and the evidence that throughout geologic time, as we know it, the surface has shown diversity of form dependent upon vertical movements of large magnitude. By offering the opportunity for erosion forces to act, the movements which have produced continents and mountain ranges have also been responsible for accumulation of the sediments washed into the deeps to form the strata from which our record is read. Also intimately related to the succession of crustal movements is the history of igneous activity evidenced from time to time in the great extrusions of molten material forming successions of lava flows intercalated in the sedimentary series. The history of climate furnished us through a great variety of data gives evidence of almost continuously fluctuating conditions in the physics of the atmosphere ranging between high and low humidity, and between temperatures comparable to those of the glacial periods and the climate of tropical or subtropical regions of the present day. The salient features of climatic history are the continuous change and the evidence of comparatively slight range of temperature for the earth as a whole within the span of geologic time as known.

Earth history, as we see it, shows from the most remote periods to the present constantly varying surface conditions dependent upon an unstable crust; continents and mountains arise only to be subject to the steady grind of erosion, wearing them away and spreading the debris over the seas. Always do we find land areas and seas, but with much variation as to size and form; always with temperature near that of the present, though fluctuating from warmer and more humid to climates like that of the Glacial Period.

Within the whole span of geological history and its continuous changes recorded, the phases of purely physical history presented do not show us in any of their various aspects definite progression or trend which may be described as an evolutionary process. It was once our practice as geologists to place emphasis on the gegological history of the earth as the continuation of a graded or evolution series based on the succession of stages described in the nebular hypothesis. According to this view, we seemed to see in climatic evolution a gradual movement away from the conditions of the primitive heated earth and toward the present temperature of a cooling sphere. We once thought we saw the early atmosphere fit only for lower organisms and later cleared and purified for the higher types of life. With better understanding of climatic history, it comes out more and more distinctly that while the earth's climate fluctuated continuously, there is no clear evidence of definite progression through a series of stages dependent on gradual cooling of a once highly heated globe.

So in other phases of purely physical history we have worked out what seemed at first to be evolution series, which have all proved finally to be nothing more than cycles that may be represented by somewhat variable formulæ. As nearly as we can determine, the physical history of the earth within the span of time represented by our legible record has been so nearly stabilized as to show little or no variation which may not be considered merely as fluctuation.

As evidencing a continuously changing evolution series, the most extraordinary record of all history is that included in the paleontologic record of life, running down through the whole story of geology, practically to the earliest strata.

Not only do we find the character of the earlier stratified rocks indicating atmospheric and climatic conditions similar to those now obtaining on the earth, but we find these rocks containing traces of living forms such as now are fitted to these climatic conditions. Throughout the whole stretch of the strictly geologic record, conditions in temperature and humidity evidently kept within the range permitting development of living forms. The period in which life came to be on this earth is represented by a chapter now destroyed.

The life record is, to be sure, fragmentary, but in many groups it is extraordinarily full. Although there is much to be desired, out of the long series of events, certain features in the evolutionary sequence are so clear as to be unavoidable. We find this record showing: (1) that life has been in almost continuous state of change. From top to bottom of the geologic section, in no two great groups of strata do we find that the assemblage of living forms represented are the same; (2) we know the life of each stage to exhibit closer resemblance to that found in strata immediately above and to that immediately below than to the life representation of the more remote divisions, and (3) we note that the series of forms with certain common characters, but differing in grade of specialization, generally trend toward greater specialization from earlier toward later time. The way in which the changes took place may not always be evident, and the paleontologist may admit his ignorance of the cause, but the fact of more or less rapidly changing definitely specializing series of presumably connected or related types seems reasonably clear. The evidence, taken in its entirety, furnishes strong support for the view that life of all these stages is related, that the life of each stage is derived or modified from that of a preceding stage, and that the whole series indicates the continuity of life from earliest to latest time.

Unlike the sequence in purely geologic history, we have here continuity and continuous progress in a definite direction. We have, however, noted that there is probably close relation between the continuous fluctuations of the progressing living world and the fluctuations of earth climate and earth crust. Movements of the crust producing change of topography and variation in distribution of land and water, taken with changes of climate, must have had important influence in keeping the currents of life moving. A dead earth without crustal movement and with uniform climate might have limited greatly the possibility of biological evolution.

The fluctuations in physical conditions on the earth in geologic time have, therefore, great significance in consideration of the larger problems of earth history.

It is not my purpose to bring into review, or to discuss, the tremendous field for evolutionary studies in the groups of animals and plants whose history we find represented in the rocks. One after another these have been considered by specialists in various fields. In all cases, the laws of which I have just spoken find expression, whether this be in the evolution of the nautilus, dinosaur, or elephant. Given lapse of time and change of environment, and the old goes out the new comes in, the unspecialized gives way to the specialized. As the ages go by, in each successive step, almost without exception, we find a higher level of life, representing greater intelligence, greater efficiency, and greater progress.

The most interesting of all the series of fossil forms represented in the geological record, and the most interesting in the first instance because the series begins well back in the geological record, is that succession giving us the beginnings of the race of man. The earliest known traces of human beings represent a normal part of the life of the earth in a period so remote from the present that our calculations must be in terms of eons rather than of millenniums. We find that since these first man-like forms appeared great crustal movements have changed the face of the earth, and that the climate has shifted back and forth many times through relatively wide ranges of temperature. We know also in this period a long procession of living generations of animals other than man passing through the ages and disappearing.

We find the first remains of humans more beast-like than any living race, approaching ape-like forms both in skeleton and skull, and meeting the requirements of the missing link. We find this first stage followed by others still different from man of the present day, but approaching more nearly to the modern type. The laws applicable to the evolution of other groups apply to man. We note the same relation of physical change in man to lapse of geologic time, to climatic and crustal change, and to other factors in the physical history of the earth. So far as the evidence goes, it meets the requirements of those who assume the emergence of man from the animal in the manner in which innumerable other organic types have arisen in the long life record as we know it.

Through still later stages of the geological and paleontological record man advanced in intelligence and culture, his environment gradually approximated present conditions in both physical and biological factors, and we record the history of these stages partly in terms of archeology, which in turn merges into history based on written records.

Through the evidence of archeology, paleontology, and geology we see human history extended back stage by stage until we go from history to prehistory, where in ages remote and in environments strange we find man already widely distributed over the earth, varying as to kind and culture and advancing as to ideas. With this view there seems no escape from recognizing that not merely the foundations of history, but the greater part of the human span, falls within a realm the approach to which has been largely by investigators concerned with the problems of earth science and using the methods developed for this field of study.

The present paper is addressed to the relation between this material, obtained from the earlier segment of history which has been briefly outlined, and that which comes within humanistic study based on modern man. You may, perhaps, urge as in Huxley's remark concerning the significance of information obtained through a "medium": that, whether or not we are truly dealing with "a message from beyond," there may not be in what we learn anything worth attention. It may be thought that remoteness means by definition diminution of value and interest, and that events of ancient history diminish in importance as the square of the distance, or at a more rapid rate. At present my only answer would be that what is first is commonly, if not always, fundamental, though fundamental characters may be overshadowed by superficial.

It is not my purpose to give detailed application of present and future use, for the facts of history seen in the outlines of the longer span secured by study of earth sciences. I may, however, illustrate by one or two examples.

Of the many elements in the problem of world government which now confronts us, there seems to me every reason to believe that race as a fundamental factor is inferior to no other involved in consideration of unity in organization. Assuming that culture, speech, economic interest, and political organization may temporarily overshadow it, in the last analysis we may not avoid reckoning with this factor, not merely in consideration of the organization of the greater groups of human beings, but also in the relations of slightly separated types. The fact that we may refuse to consider it does not prevent its acting as a continuously operating element, which remains while prices go up and down, political parties come and go, and national units group themselves in this way or that.

Race is the product of evolution in a changing environment, the conditions of which have been determined by factors of geological significance. As a relatively simple illustration, the history of the original Americans is a tangled web in which there is inextricably woven the story of great continental and climatic changes and of vast intercontinental migrations of plants and animals. The history of European and Asiatic races is of like order. The relation between Africans and Caucasians or Africans and Mongolians is dependent on similar conditions reaching into remoter periods and still more difficult of interpretation.

The Balkans represent the fault-line of Europe, because this is a region of overlapping races and subraces, conditioned in their history by extraordinarily complicated migrations determined and directed in part by physical features and climatic changes. Although the Balkans present a problem of the greatest difficulty in the racial and political sense, they place before us a study simple of aspect and significance compared with the larger race questions which we have now to encounter in world government, the difficulties of which we shall not improbably see in larger measure as the centuries pass.

Shall we in attempting to solve these incalculably complicated questions look only at the present balance of trade, the dominance of particular political parties, the present grouping of social elements, or the present military strength of the nations involved; or shall we, realizing the vastness and the complexity of the difficulty, bring to the light every element concerned, scrutinizing with especial care those factors which seem to be fundamental and more clearly of permanent significance. Unless the larger or broader view is taken, I feel that we shall fall short of the interpretation of humanity needed, in order to fit the nations of the world together into one great unity in which each people supplements the needs of the others, and thus gives to every group, as well as to every individual, that freedom to develop its own peculiar talent and grow into that fullest usefulness which we assume to be the natural right of all.

The question of race just described is only one phase of the historical problem, in which the background represented by the field now occupied mainly by earth sciences becomes of real significance.

In passing I may only mention two other examples illustrating the relation of historical data from earth sciences to affairs of life of today. I believe I am correct in stating that earthquakes are by most persons considered as extraordinary happenings, without relation to the normal order of events with which we have acquaintance. The geologist, however, recognizes them as the natural corollary of crustal movements. Regarding continuance of such movements, he must believe that the only basis for considering that crustal activities have ceased is to assume some extraordinary intervention definitely holding back forces which if unfettered would result in further crustal disturbance and in earthquakes. Such disturbances have affected the earth since the beginning of our geological record. The geologist who views the history of crustal movement considers that there is no reason for believing that the crust is now stabilized, and that we may expect other movements and other earthquakes. We know fairly the physical laws that govern earthquakes. We can prepare to meet them in such a way as to eliminate most of the dangers incident to their action, but it will take the passing of another generation before we reach a stage in which the clear lessons of earth history bearing on interpretation of these phenomena will become the basis of common practice, such as dictates the precautions which have made it possible for us to build in the summer against the rains of autumn and the snows of winter. Many of us still build as if the last earthquake suddenly ended the series measuring back for tens of millions of years.

Still more difficult may it be for us to make use of the lessons of prehistoric history relating to our adjustment to biological environment. In America we live largely on plants and animals of Old World origin, not because the abundance of these types is so much greater than that of America, but because man has lived a longer time in the Old World, and within the period of his early history, reaching back to past geological periods, he has experimented intentionally or accidentally with Old World plants and animals for a longer time than has been given to contact with the native life of America. There are many who do not recognize this relation to the world of undomesticated organisms about us, and seem to feel that some plants and animals were predetermined to domestication, while others can never serve us.

Left to chance, as during past millenniums, we may in time develop a series of useful American plants and animals corresponding to those of the Old World; or, recognizing the significance of the historical explanation of our relation to domestication, we may by active and carefully directed research secure results comparable to those of a long period of casual or accidental contact, and obtain a great variety of wild forms for use to meet human needs. Such an example of possibilities seems to be found in the development of the desert rabbit-brush as a source of rubber. An investigation was undertaken as an emergency problem during the World War, when there loomed before us the possibility that submarine dominance would eliminate all possible rubber importations. Recent studies by Hall and Goodspeed have shown the presence of 300,000,000 pounds of rubber in the desert region of the West. At present prices it is not available. In an emergency it might be a factor of first importance contributing to defense of the nation. Future research may also show possibility of large use of this supply through cultivation of the wild stock, thus making the desert an important area of production.

History shows us that sufficient understanding of the natural world about us brings large contribution to human comfort and efficiency; but, in spite of the lesson before us, many feel that the day of discovery of species most useful to man is past.

Returning to the larger view of our problem, the value of ancient history depends on our breadth of interest. If we are to deal only with matters of limited personal or national significance, only for immediate ends, and without reference to other generations; if our democracy is circumscribed in space and time, then lack of perspective and of fundamental laws in history may not be felt. If, on the other hand, we see the impending necessity of full understanding of the world's needs in their present relations and future complications, it behooves us to increase the range of human knowledge and of our comprehension of all factors entering into the problems. To most of us it appears that these great questions require the widest and deepest possible range of human understanding and the labor of generations for their satisfactory adjustment. The world statesman of the future must not only be trained to larger and higher vision, but he must have available an organization of knowledge, perfect in its simplicity and infinite in its detail, covering every interpretable phase of the intricate human problem. As we approach the preparation for this task, we recognize at once the limits of the human mind and of human life, and accomplishment seems realizable only through operation in an altruistic democracy, making possible intellectual cooperation covering a wider range of experience than can be available to the individual mind.

If, in consideration of the larger problems suggested, we assume that man was created as we find him and destined to no higher plane, the sequence of history is of little value. If, however, the evolutionary view of life be correct, the continuity of history becomes of great importance, and origins, however far back, interpret the present. Should we consider that man in his environment is the product of a long series of changes determined by laws laid down in the record of the earth sciences, we have reason to consider, as bearing on the present, every fact leading from the past. In the interpretation of this record we shall view history feeling assured that nothing on the earth or in life stands still, and that this movement means continuous lifting of the plane of the more complex and more progressive.

In the lines which have been read it has been my purpose to indicate the extension of history backward into the earth sciences, and to point out the significance of this sequence as a continuity presenting in its formula an expression of the present. One may not leave the subject without referring also to the possibility of extending this continuing series from ancient geologic time into the future through a span comparable to the past we know.

To one who views the story of the world as presented through the medium of the earth sciences, it must seem unnatural to conceive of the physical and biological forces now in operation as ceasing to act before lapse of many periods like those which we have viewed. Unless there intervene some extraordinary force beyond the reach of our understanding, the laws which have so long defined the course of nature must continue operation. Without the addition of any power beyond the spring of action furnished by laws now working, the clock of the universe must go for almost infinite ages before it runs down.

Just as we are not able to conceive of crustal movements ceasing, so long as we are subject to physical forces like those now controlling nature, so when we visualize the history of life in the broadest sense we are unable to understand how the biological world, if it continues to be, and if it continues in the environment of physical change, can do other than go on to greater extremes of specialization, to greater range of complication, to greater comprehension, and to greater intelligence. If man of the future continues to maintain the relation between mental and biological which has obtained in the past stages of his evolution, there is reason to believe that he may reach to heights of mental ability, of comprehension, of intellect, of understanding, greater than those yet known. What the ultimate goal will be no one may yet see; without fundamental change of governing laws, the movement must go on.

The Geologist's Rôle of Interpreter

One does not expect a geologist to state his own views in philosophy or in phrases aiming at the deeper human understanding, and yet there seems reason for feeling that the wider outlook of science in all of its phases lifts us up to the identical viewpoint from which the philosopher and the poet obtain their comprehensive vision. Unlike the philosopher, we do not reach backward or forward to interpret the final unity and purpose of Nature; nor can we, like the poet, picture in words with fullness of meaning the view which opens to us; but the kind of a landscape which we see and the training of the eye which sees it give to our picture a measure of reality which its stupendous magnitude does not lessen.

Of all favored men, it is the geologist and paleontologist only who see the panorama of ages unrolled in fullest length and in truest reality. To them this record is not written in doubtful hieroglyphs and symbols. It represents the imprints of living feet that have never ceased to advance in unbroken procession over a trail that winds upward through the ages. From one glimpse at footprints on these sands of time, a poet, in the person of Longfellow, gave to all generations a Psalm of Life, which has found response in an ever-widening circle of human hearts. Longfellow's poem, suggested by the antiquity of the print of a foot upon the Connecticut sandstone, was based upon a splendid lesson of analogy. He emphasized for us the idea that the influence of each life may reach out undreamed distances through space and time to make the forlorn and shipwrecked take heart again.

Pointing in the same direction, but of infinitely deeper meaning than the lines of the poet, is the reality of the story, the sermon, the poem which the geologist sees, and which must of necessity reach its recognition through his eyes and its expression through his voice. The footprints and the stages of the path on which they appear are to us not merely evidences of an unending influence; they are tangible proofs of progress from eon to eon which might well help a forlorn world to take heart once more. We may not understand the method by which betterment has come, but we see the stages of its movement, and realize that, whatever struggles the future may have in store, we shall always be credited with a margin of safety when we risk ourselves in the cause which makes for uplift in the widest sense.

Without assuming more than is involved in the field of his daily work, the geologist stands before the world as the interpreter of one view of a great truth fundamental to human interest and belief. This truth should not be overemphasized; it should not be underestimated. It was in large measure this depth of view that stimulated Darwin to his great constructive work, giving biology and the whole range of human thought his progressive evolution. It stands as the background out of which history emerges and against which its interpreted movement must always be projected. The world needs now, as never before, a wide and deep view, and a full understanding of all that may concern mankind. The student of earth sciences was once a contributor to the wider philosophy of nature. It may be his duty now to make sure, not only that his influence is felt in advancement of material welfare, but that he serve also to point out the lesson of the foundations of the earth, and to show that strength may still come from the hills.

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DISCOIDAL STRUCTURE OF THE LITHOSPHERE[1]

BY BAILEY WILLIS

(*Presented in abstract before the Society December 27, 1918*)

CONTENTS

ADVANCED SUMMARY

The following discussion seeks to determine inductively the structural character of the lithosphere at depths beyond the reach of observation.

The mechanical state of the outer shell of the lithosphere is discussed on the basis of Adams' experiments with rocks under confining pressures,

[1] Manuscript received by the Secretary of the Society December 15, 1919.
Entered in printed program under the title "Structure of the Pacific ranges of California" and included in outline in the oral presentation of that topic, but subsequently expanded to the present article.

which demonstrate that pressure increases the internal friction and consequently the absolute strength of solid rocks. The conclusion reached is that the rate of increase of absolute strength is very great in the first few miles below the surface; that below 10 miles the gain decreases rapidly, but there is, nevertheless, a continued increase in strength with increasing depth under normal conditions of pressure and temperature in the lithosphere. Pressure alone can not promote mobility. High temperature in excess of the normal is essential to mobility or fusion.

The absolute strength of rock does not, however, increase as rapidly as the load with increasing depth. The relative strength of rock—that is, the ratio of the absolute strength to the superincumbent load—therefore decreases with increasing depth. The relative strength thus becomes equal to 1 at 40 miles or less below the surface, and below that depth is a fraction; that is to say, the rock is potentially crushed.

According to this analysis, the "zone of flowage" lies much deeper than was estimated by Van Hise. Furthermore, emphasis is laid upon the fact that the flow of a solid takes place either by recrystallization or by shearing, and thus differs markedly from liquid flow. No matter how slight the *relative* strength of the absolutely strong rock may be, it can not transmit pressures hydrostatically, and the assumption of hydrostatic pressure, which underlies many discussions of the mechanical state of the lithosphere, including isostasy, is an assumption of conditions which can exist only if the rock be molten.

In order to analyze the relative effects of temperature and pressure, a comparison is made between the moduli of thermal expansion and the moduli of compression of rocks, plate-glass being taken as a representative substance. Quantitative values are not determinable, since the moduli at high temperatures and pressures are unknown. It is shown, however, that heat and pressure, in constant and unrelaxing opposition, maintain the rock in a sensitive elastic state, such that it responds instantly by change of volume to any variation of pressure or temperature.

Isostasy is discussed as an essential working hypothesis. Among the several forms which the hypothesis has assumed during the past thirty years, that one is preferred which was formulated by Gilbert and which postulates isostatic equilibrium among large masses, but recognizes effective rigidity of the crust as the condition of support of smaller irregularities of the earth's surface.

The demonstrated strength of the crust puts out of consideration the effects of erosion and deposition as a cause of deformation.

Hayford's mathematical deductions are believed to be inconsistent with the facts of geologic history. Exception is taken to the assumption of uniform density distribution in a vertical column, and to that of hydrostatic pressures below the zone of compensation. The existence of a zone of compensation is, nevertheless, regarded as established by his researches. It is defined as a zone of solid rock of great strength and of variable thickness, which passes, at its lower limit, into the asthenosphere of Barrell. The anomalies of gravity which have been demonstrated by the researches of Hayford and Bowie are considered to be those superficial effects of unequal attractions exerted by bodies of different densities which lie near enough to produce notable anomalies. The anomalies are not regarded as unique, however, nor confined to the isostatic shell. Following Gilbert, nucleal heterogeneity is regarded as highly probable. The fact that its effects are not evident is attributed to the law of averages among large numbers of small differences, to the masking of the deep-seated anomalies by the asthenosphere, and to their distance from the observer on the surface.

The development of a certain structure in the lithosphere is attributed to recrystallization of rocks under non-uniform stress, the result being a foliation which is oriented by the stress. Although erosion and deposition are not regarded as capable of producing mass movement or "undertow," they are considered competent to set up a stress within the elastic limit of the lithosphere which does determine the direction taken by foliation, provided a rise of temperature or other external force causes recrystallization. The orientation of the elastic stress, and consequently of the foliation, is approximately vertical in the unloaded mass, but is horizontal in the loaded mass and curves from beneath the latter upward into the former. Thus, under any sinking, overloaded area there develops a curved foliation which gives rise to a disk-like structure. Disks obviously correspond with heavier bodies, whereas the lighter bodies, in which foliation is predominantly vertical, may be called interdisks.

An analysis of continental structure and of oceanic basins leads to the conclusion that the continental platforms and the underbodies of the ocean consist of heavier and lighter masses corresponding to disks and interdisks. The inference regarding curved foliation, which throughout the previous argument is attributed to elastic stresses set up by erosion and deposition, is extended to broad oceanic depressions by consideration of the stresses which would be produced in the underbody, if the deeps resulted from subsidence of heavy masses.

Throughout the argument the structural relation between foliation and the course taken by rising igneous masses is used as a means of recognizing the original foliation of the rocks. The argument suggests that the igneous material which is erupted in the margins of the continents, around the ocean basins, comes from beneath the latter. In order to test this suggestion by an independent investigation, a study was made of the conditions of fusion of basic and acid igneous rocks, and the conclusion is reached that basic rocks would melt under conditions of dry melting while the acid rocks were yet very far from reaching a melting temperature. Eruptions of basic rocks from deep-seated sources to the surface would follow; a disturbance of isostatic equilibrium would result, and the heavy masses, seeking their appropriate level, would sink and form depressions which would become ocean basins. The line of thought thus suggests an origin of ocean basins and explains the occurrence of basic eruptions in their margins.

Introduction

THESIS

The lithosphere is characterized by a structure or structures imposed upon it by the forces of gravitation, heat, and chemical attraction. An analysis of the stresses resulting from imperfect equilibrium of heterogeneous masses indicates that the resulting structures, whose form is determined by the orientation of foliation in metamorphic schists, are disk-like bodies—that is, that the outer shell of the lithosphere has a discoidal structure.

ESSENTIAL CONCEPTS

For the purpose of this discussion, the lithosphere is defined as that rigid, elastic layer of the solid earth in which the elements are combined to form minerals and minerals are aggregated to form crystalline rocks. Two other states of rock are recognized, sedimentary and molten, but they are regarded as incidental rather than as essential to the lithosphere.

Crystalline rocks may be classified, according to their texture, as granular and foliated. The granular texture originates by cooling of a molten magma under balanced stresses in all directions. The foliated texture develops when crystallization or recrystallization takes place in the presence of unbalanced or non-uniform stresses. The nature and distribution of stresses in the lithosphere must then determine the nature and distribution of the crystalline masses of which it is composed.

The lithosphere is heterogeneous as to density, and there is a tendency toward isostatic equilibrium between the heavier and lighter masses. The attainment of equilibrium is, however, opposed by rigidity, and any approach to equilibrium is more or less effectively canceled by erosion and deposition. Equilibrium has, therefore, been least complete during periods of general planation, while more nearly attained during epochs of continental uplift and mountain growth, such as the present is, for instance. In the long retrospect of the history of the lithosphere, states of strain due to notable departures from isostatic equilibrium have been more prolonged than the alternating states of approximate equilibrium relatively free from strain.

Provisionally, for convenience of discussion, the depth of the zone of compensation is taken as in the neighborhood of 120 to 150 kilometers (75 to 100 miles), as indicated by the studies of Barrell, following Hayford and Schweydar.[2] In the discussion referred to, Barrell gave definite form to the concept of a zone of weakness, which he called the "asthenosphere," and which should underlie the zone of compensation. Gilbert had previously discussed the "horizon of mobility."[3] Chamberlin and Lunn had shown how such a zone might result from the outflow of internal heat and its concentration at a level where the constant pressure would be overcome by the rising temperature and local fusion would result.[4]

I accept the concept of the asthenosphere as an inevitable consequence of the conditions postulated by Chamberlin. I prefer, however, to use round figures, such as 100 miles, to express the probable depth at which the zone of compensation passes into the upper part of the asthenosphere, because a precise figure, such as 122 kilometers, for example, gives an idea of definiteness to a transition zone, which must be by its very nature indefinite and variable.

For the purpose of this discussion, then, the lithosphere is taken to be a shell of solid, rigid, elastic crystalline rock of indefinite depth. It comprises:

(*a*) An outer shell, approximately 120 to 150 kilometers thick, the zone of isostatic compensation or the isostatic shell.

(*b*) A zone of local and temporary, but periodic, fusion, extending from the bottom of the isostatic shell to a depth of at least 1,000 kilometers, more or less, and equivalent to the asthenosphere of Barrell.

Within the lithosphere, as above defined, there is a mass which it is convenient to call the centrosphere, in order to indicate that nothing is postulated as to the state and constitution of the interior of the earth

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[2] J. Barrell: The strength of the earth's crust. Chicago Jour. of Geol., vol. xxii, 1914, p. 680; also ibid., vol. xxiii, 1915, p. 428.

[3] G. K. Gilbert: Interpretation of anomalies of gravity. U. S. Geol. Survey, Professional Paper 85 C, 1913, pp. 34-35.

[4] T. C. Chamberlin: Geology, vol. 1, p. 629. Hypothesis of 1909.
H. C. Lunn: Geophysical theory under the planetesimal hypothesis. Carnegie Institution of Washington, Publication 106.

beyond the fact that it is heavy and behaves like an elastic solid. This discussion deals only with a relatively superficial shell.

Mechanical State of the isostatic Shell

EFFECT OF PRESSURE

During the past dozen years the experimental researches of Dr. F. D. Adams, of Montreal, and also of the Carnegie Geophysical Laboratory have greatly increased the available data which are helpful in seeking to comprehend the state of rock under high pressure and at high temperatures, and it is now possible to speculate somewhat more surely than we previously could regarding the general conditions. The limitations of experiment are such, however, that it is necessary to extrapolate beyond them to a degree which renders inferences suggestive rather than conclusive. It is with a full appreciation of this qualification that the following discussion is offered.

Since the publication of Heim's studies of alpine structure[5] geologists have recognized that solid rocks "flow" under the unequal pressures which develop in the earth. Since rocks do not flow at the surface, and since the pressure which may cause them to flow is an effect of their own weight, it has been assumed that a condition of plasticity corresponding to flowage develops at some depth in the earth where the weight of the superincumbent column is sufficient to overcome the rigidity of the rock.

The terms plasticity and flow involve the idea of mobility, and even when used with the distinct statement that the flowing rock is a rigid solid, they persist in suggesting ease of movement. Gilbert was, so far as I know, the first to postulate that rock may grow stronger, may become more rigid under pressure than it is under surface conditions, and that even where it moves it may present greater resistance to distortion than does rock as we know it. He expressed this view in discussing the conditions of experiments on folding strata in 1887, and the concept which I then gained from him was subsequently phrased as follows:[6]

"The crushing strengths of stones . . . probably increase with depth in the earth's crust and in an unknown ratio; but it is not likely that the increment of strength is as great as the increment of pressure. . . . At five miles below the surface the pressure exceeds the maximum resistance of the rocks at the surface, and at ten miles the pressure is more than double the resistance. This means that somewhere between five and ten miles beneath the surface the weight of the superficial crust is sufficient to crush its support.

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[5] Albrecht Heim: Mechanismus der Gebirgsbildung, 1878.

[6] Bailey Willis: Mechanics of Appalachian structure. U. S. Geol. Survey, 13th Ann. Report, 1891-92, p. 237.

But crushing is not possible within the earth's mass in the way in which we see it at the surface. To crush is to separate into incoherent particles, and irresistible confinement, itself due to the pressures which are greater than coherence, holds any deep-seated rock-mass to its coherent volume. In this condition, confined under pressures greater than its crushing strength, a substance may be said to be latently plastic. The cohesion between its particles is unimpaired; fracture or crushing into separate grains is impossible for want of space; but change of form may be induced by a sufficient disturbing force, and such change is plastic flow."

Van Hise and Hoskins subsequently estimated the depth at which, under the most favorable conditions for strength, a small cavity might remain open as 10,000 to possibly 12,000 meters—that is, below that depth the stress-difference due to the superincumbent load would be greater than could be permanently resisted and the rock would flow. This estimate was based on the assumption that the strength and elastic properties of rocks under great pressure are comparable with those of rocks under surface conditions.

Adams and Bancroft,[8] by their brilliant experimental researches, have demonstrated the accuracy of Gilbert's postulate and have shown that rocks do increase in strength under pressure. The gain in strength is, moreover, at a rate far greater than would have been supposed, and the zone of flow must lie correspondingly deeper than was estimated, except as heat may modify the viscosity of rocks.

Unfortunately, the experiments are insufficiently numerous to yield quantitative data by which the strength of rock at any desired depth might be determined. They do, however, suggest limiting values, which, with a full appreciation of their approximate character, are indicated in plate —.

The nature of Adams' experiments is shown in the figure taken from his paper (figure 1, page 602). Pressure applied to the rock column forced it to expand against the resistance of the steel jacket in which it was inclosed. According to Adams:

"The pressure which was applied to the (rock) column effected two results. It overcame the pressure (or resistance) exerted upon the sides of the column by the inclosing tube of steel, and it overcame the internal friction developed within the rock during its change of shape."[9]

The resistance of the steel tube was determined by an independent experiment, in which tallow was substituted for the rock, and it was thus

[7] L. M. Hoskins: Flow and fracture of rocks as related to structure. U. S. Geol. Survey, 16th Ann. Report, 1896, pp. 858-859.

C. R. Van Hise: Principles of pre-Cambrian geology. Ibid., pp. 591-593.

[8] F. D. Adams and J. A. Bancroft: On the amount of internal friction developed in rocks. Jour. of Geol., vol. xxv, 1917, pp. 597-637.

[9] Ibid., p. 606.

made possible to ascertain the internal friction by subtracting the resistance of the steel cylinder from the total pressure.

To apply these results to the case of the lithosphere, let us consider the pressure applied to the ends of the rock specimen as a tangential or horizontal stress, and conceive the resistance of the steel cylinder to be replaced by a vertical rock column which would exert the same pressure. Then, in order that the latter should be raised in the manner and to the degree that the steel cylinder was bulged, the differential horizontal stress must be sufficient to overcome, not only the weight of the rock column, but also the internal friction or flow-strength of the rock specimen.

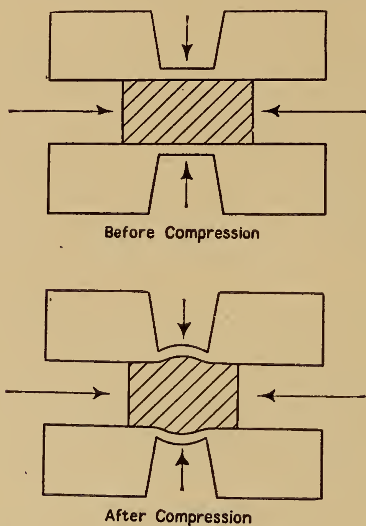

FIGURE 1.—*Longitudinal Section through steel Cylinder with Wall 0.33 centimeter and inclosing one of the Rock Columns.*

Adams used steel jackets having walls 0.25 centimeter thick in one set of experiments, and in a second set 0.33 centimeter thick. The former exerted a resistance of 26,685 pounds to the square inch, and was therefore equivalent to the load of 4.2 miles of rock of specific gravity 2.8. The steel cylinder, 0.33 centimeter thick, resisted with a pressure equivalent to 37,359 pounds, or a load of 5.8 miles of rock.

The above values are those given by Adams for the stage of experiment "where the regular column shows a diametrical increase of 0.05 inch (or 6.35 per cent), that is, when the deformation is well under way, and after which it becomes proportional to the tangential pressure." Plotting the numerical results given by Adams for successive stages of the experiments on granite, one obtains curves which show very marked deformation before the steel had developed a resistance equal to the values given above, and if one were looking for the precise point at which the rock began to flow, somewhat less depths than 4.2 and 5.8 miles would probably be chosen; but I prefer to take the values deduced by Adams as representing a state of established flow.

Granite confined in a steel jacket 0.25 centimeter thick flowed under a total pressure per square inch equivalent to a column of rock 25.8 miles high, the total being made up of two parts, namely, 4.2 miles corresponding to the resistance of the jacket and 21.6 miles representing the internal friction of the granite.

Similarly, the same rock confined in a jacket 0.33 centimeter thick flowed under a total pressure per square inch equivalent to a column 29.1 miles high, consisting of 5.8 miles for the resistance of the steel and 23.3 for the internal friction of the rock.

This same granite has at the surface a crushing strength sufficient to support a column of rock only about 5 miles high. Between the surface and a depth of 5.8 miles there is, accordingly, as demonstrated by experiment, a very rapid increase in internal friction or resistance to deformation by crushing or flow.

To visualize this increase, let us plot an arbitrary curve (plate 8) whose vertical ordinates shall be 0, 4.2, and 5.8 miles, corresponding to the load under which the granite exhibits a resistance to flow equivalent to 5, 21.6, and 23.3 miles, and let these values be the abscissas of the curve. We thus obtain the curve LMN.

In the absence of experimental data with which to prolong the curve below 5.8 miles, we may speculate as follows:

The increase in internal friction under increasing pressure may reasonably be attributed to the compression of the rock, whose particles and molecules are thereby brought into closer proximity. The cubic compressibility of rock follows Hooke's law of a direct ratio of increase per unit of increment of load within the elastic limit; but in approaching that limit there is a tendency toward a decrease in the rate of compression, as may be seen in the results of Adams' work on compressibility.[10] Lunn's curves for pressure density and temperature within the earth demonstrate the same change, but in a higher degree.[11] Accordingly, any increase of internal friction which, by postulate, is a function of compression should show a similar decrease in rate with greater loads. Now, if the increase in internal friction were to proceed at a constant rate below the depth of 5.8 miles, the curve would assume the slope of the straight line MNF, making an angle 43 degrees with the horizontal. By the above reasoning that line represents an improbable maximum of increase of strength with depth, and the true curve falls within it.

If the curve of increasing internal friction bends downward, as seems probable, at such a rate that its abscissas grow at a less rate than its ordinates, it will somewhere intersect the diagonal of 45 degrees at which

[10] F. D. Adams and E. G. Coker: An investigation into the elastic constants of rocks, more especially with reference to cubic compressibility. Carnegie Institution of Washington, Publication 46, 1906.

[11] Chamberlin and Salisbury: Geology, vol. i, p. 566.

the abscissa and ordinates are equal. At that point the resistance which the granite offers to flowing is equal to the load resting upon it. The granite should, therefore, flow into any very small or temporary opening.*

There is an experiment on the closing of a small cavity in granite which appears to correspond closely to the preceding postulate (figure 2).[12] In this experiment a cylinder of Westerly granite 0.5 inch in diameter was confined in a steel cylinder and so compressed that it sheared or flowed into a cavity 0.5 inch in diameter. The steel cylinder in this experiment did not yield. The pressure, applied to the end of the granite column over its entire area, was transmitted throughout the column and affected its whole mass. There were two holes, the one vertical along the axis of the column, the other transverse at one side. When subjected to sufficient pressure the granite powdered or sheared off in minute flakes on the periphery of the vertical and horizontal holes.

Referring to the results on Westerly granite, experiments 357 and 358, Table III, of the article referred to, we may quote Adams as follows:

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* Professor Hoskins has commented on this statement as follows:

"There is needed a strict definition of the word 'strength' as applied to a rock or other material.

"The resistance which a material offers to flow does not depend on one single quantity or factor, but must obviously be influenced by the whole stress-condition existing in the material. To specify completely the stress-condition requires three quantities, namely, the values of the normal stresses on the three principal planes at the point under consideration.

"In the experiments of Adams two of the principal stresses were kept equal, while the third was made greater and greater up to the point at which the flow occurred; any general conclusions drawn from his results are applicable only under similar restrictions. This fact must be remembered in interpreting the curves OMNP and OK on plate 8.

"Assuming that the location of the point Q by extrapolation is valid, what is its meaning? Merely this: that if the granite were under a stress-condition such that two of the principal stresses were equal, each having a value equivalent to the weight of a 40-mile column of the rock, then the third principal stress would have to have double this value in order that flow should result."

With this understanding I am agreed. I have phrased what seems to me to be the same concept in the statement contained in the summary as to mechanical state, that "a non-uniform stress equivalent in pressure to the weight of a column of rock 40 miles high will cause movement."

Professor Hoskins further states his view that "there does not seem to be justification for the statement that granite should therefore flow into any opening." He argues that "the stress-condition in the neighborhood of an empty cavity must be wholly different from that on which the curve is based. So long as the cavity remains empty, one of the three principal stresses has to be zero at the wall of the opening."

If the substance surrounding the hole were the ether of pure mathematics, or possibly if it were water, the above argument would apply; but solid granite obeys a law of shear, which produces wedges that crowd each other in the movement that represents flow. The stress-conditions around an opening are therefore radically different from those which would exist in a fluid, as appears from the results of Adams' experiment on the pressure required to close a small cavity. See the following paragraphs.

[12] F. D. Adams: An experimental contribution to the question of the depth of the zone of flow in the earth's crust. Chicago Jour. of Geol., vol. xx, 1912.

"In the case of the granite the holes when filled are closed by what appear to be minute fragments of granite detached from the walls. In the case of experiment 358, after the removal of the steel the vertical hole as seen from either end was still open and was unaltered in size or shape for a distance of .08 and .24 inch respectively. Beyond that, however, it was blocked up. On removing the steel so as to expose the extremities of the transverse hole, it was found that one end of this hole was completely filled up, no trace of the opening remaining. The locus of the hole was occupied by what seemed to be part of the rock, finer in grain than the rest and which looks as if it were a

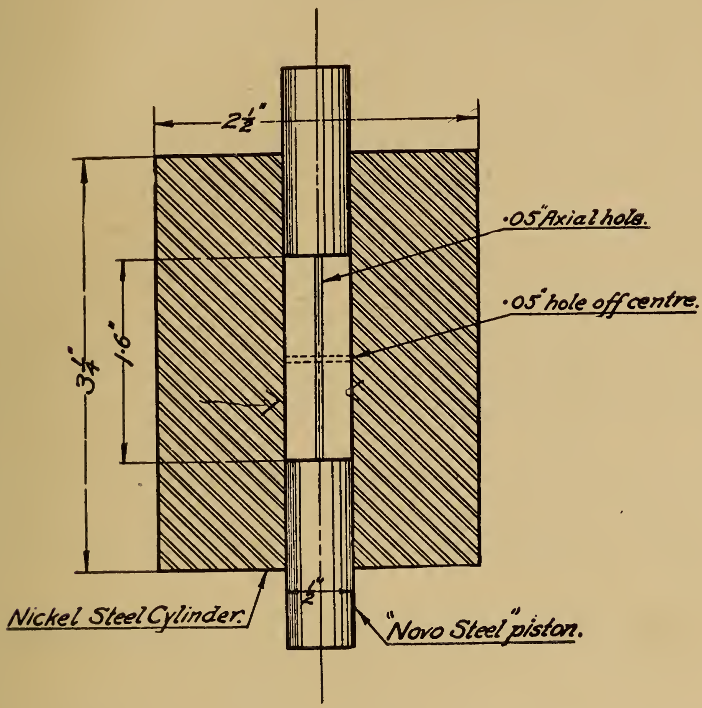

FIGURE 2.—*Diagram showing the Column of Rock with a vertical and transverse Hole drilled through it, inclosed in a heavy Tube of nickel Steel*

perfectly compacted mass of powdered granite. The other extremity of the hole had also been completely closed, although an outline marking its original position could be seen; it was filled with finely granular material, clearly crushed granite, imbedded in which were a few relatively larger fragments, giving to the whole the appearance of a breccia."

In experiment 358 the pressure on the granite amounted to 222,500 pounds to the square inch, and thus corresponded to a depth of 35 miles in the earth. In experiment 357 the pressure was 200,000 pounds to the square inch, or 31 miles depth, and only the vertical hole was partly filled by rock powder. The transverse hole remained unaltered. In both experiments the pressures were maintained 2½ months. There can, therefore, be no question but that the very great pressure pervaded the entire mass and all the granite was involved in the movement that was shown by the local shearing. In other words, the granite approached a state of flow at a pressure equivalent to 31 miles, and yielded at a pressure equivalent to 35 miles of depth below the surface of the earth.

As has been pointed out by Professor Hoskins, in conference, this result is not exactly comparable with those obtained by compressing the granite in a steel cylinder which, though it yielded, nevertheless continued to resist. The resistance raised the internal friction or flow-strength of the granite. If in experiments 357 and 358 there had been substituted for the small hole a steel jacket offering a resistance equivalent to the pressure of 31 or 35 miles of load, the internal friction of the granite would have been even higher than it was.

The problem thus raised related to the relative values of the stresses exerted in the case of flow into a small central cylindrical hole, as compared with those developed by outward flow against the surrounding steel jacket. To what extent does the centripetal movement toward an open cavity develop increased internal friction, which may be regarded as the equivalent of the confining jacket toward which the movement is centrifugal?

King, discussing Adams' experiments with the steel jacket, says:[13]

> "It follows from Tresca's theory that the rock, when stressed under these ideal conditions, will commence to break down or flow *simultaneously* throughout its entire volume."

It is evident from the manner in which both the vertical and transverse holes were filled in experiment 358, cited above, that this condition of simultaneous rock flow existed when the pressure on the end of the specimen equaled the weight of a column of granite 35 miles high. Rock moving under these conditions would seem to be subject to the familiar law that a constriction of cross-section increases friction. Now the centripetal movement of the rock constituted, in any cross-section vertical to the hole, a flow of 99 areal units toward one unit, the hole having an area of but 1 per cent of the cross-section of the cylinder. It is impossible that this should occur without a very notable increase in friction.

On the other hand, in analyzing the stresses in the specimen which expands inside a steel jacket, King determined "that the average longi-

[13] L. V. King: On the mathematical theory of the internal friction and limiting strength of rocks. Chicago Jour. of Geol., vol. xxv, 1917, p. 642.

tudinal pressure at the center (of the bulge) is somewhat less than that of the ends by amounts which increase considerably with the harder rocks."[14]

Now one might readily assume that the internal friction in a rock forced to flow toward an open cavity would be lower than that developed in the same rock when forced to flow and in flowing to overcome a confining stress; but the above considerations indicate that the friction may also depend upon the constriction or expansion of the cross-section. If so, the two types of experiments which have been described may yield comparable results.

I saw no way of making the comparison, however, until Professor Durand made the suggestion which developed the following method.

In the experiments with the yielding steel jacket the total pressure applied to the end of the rock specimen overcame the internal friction of flow of the rock and also the resistance of the steel jacket. In the experiments with an unyielding steel jacket the total end pressure overcame the internal friction of flow throughout the mass, plus the added friction due to centripetal crowding. We have for granite three experiments, two with the yielding steel jacket and one with the unyielding jacket and open cavity, in all of which flow was well established. They are: (1) the experiment with a steel jacket 0.25 centimeter thick, equivalent to a load of 4.2 miles, which developed an internal rock strength equal to 21.6 miles; (2) the similar experiment with the jacket 0.33 centimeter thick, equivalent to a load of 5.8 miles, which developed a rock strength of 23.3 miles; and (3) the experiment number 358, in which granite flowed toward a central cavity under a total pressure of 35 miles. The total load applied to the end of the cylinder in each of these cases is: for (1), 4.2, 21.6, 25.8 miles; for (2), 5.8, 23.3, 29.1 miles; and for (3), 35 miles. We also know that at the surface a load of about 5 miles would crush the granite.

Plotting the total end pressures 5, 25.8, 29.1, and 35 as ordinates, and the known strength of the granite, 5, 21.6, and 23.3 as abscissas, we obtain three points on a curve which may be prolonged to cut the abscissa corresponding to 35 miles. (See plate 8.) The length of that abscissa, JK, will then represent the average internal friction or strength of the rock under the total end pressure of 35 miles, and the difference between JK and the length corresponding to 35 miles will represent the added internal friction due to centripetal movement.

Since the values thus obtained depend upon the curvature of a line whose true curve is not definitely known, the result is arbitrary, but

[14] Op. cit., p. 655.

approximately correct. The prolongation of the curve gives a probable value of 25.8 miles for the average internal friction and of 9.2 miles for the added friction or resistance. The point K corresponds in position to these figures.

The values of 25.8 miles and 9.2 miles may be plotted as in plate 8 to fix the point P, which is a point on the curve of internal resistance of granite to flow when confined by the corresponding load. The position of P may be changed by a slight variation of curvature in the diagram from which its ordinates are derived. It is, therefore, not of high value. But based as it is on what seem the more reasonable assumptions, it indicates a probability that the curve LMNP will continue to bend downward and may be extended, approaching a straight, in such manner as to intersect the diagonal of 45 degrees at the point C, at a depth of 40 miles below the surface.

This result may be interpreted to mean that, at approximately 40 miles below the surface, the column of rock of specific gravity 2.8 would equal in weight the crushing strength or internal friction of granite at surface temperature. Below 40 miles the curve which represents the increase of internal friction under the above stated conditions may be extended, as shown on the diagram, to D, but its form is hypothetical.

The preceding inference is to be qualified by the effect of heat, which is discussed in a subsequent paragraph.

To pursue the inquiry a little further, we may consider the relation of the strength of the granite at any depth to the superincumbent load and, using a familiar engineering term, may call the ratio of strength to load the factor of safety. Thus, where the load is 4.2 miles and the strength of granite is known by experiment to be 21.6 miles, the factor of safety is 5.1. Similarly, at 5.8 miles it is 4, and, by estimate, at 40 miles it is 1. (See the curve RST, plate 8.)

Below the level at which it becomes 1 the factor of safety is a fraction—that is, an excess of stress, which is a corresponding fractional part of the overlying load, will cause the granite to flow. But, even though that be so, the *absolute strength* of the granite at surface temperatures, as shown by the abscissas of the curve of internal friction, continues to increase with depth.

The *relative* strength of the granite, expressed by the factor of safety or the ratio of strength to load, can become zero only if the numerator becomes zero. This latter would be equivalent to assuming that the internal friction of the granite becomes zero, or that the granite is a fluid—that is, is molten. But the normal increase of temperature with depth is not sufficient to overcome the pressure and melt the granite.

Hence it follows that, however small the ratio which expresses the relation of strength to load may be, it will always be a finite quantity.

Pressure, which increases internal friction, can not destroy solidity or promote mobility.

EFFECT OF HEAT

The preceding discussion deals with the problem of the strength of a cold lithosphere. The lithosphere being hot, the concept of rigidity and great strength which has been suggested must be somewhat qualified by the weakening effects of heat.

The surprisingly high values of rigidity and strength which Adams has demonstrated are effects of exceedingly great pressures. In the lithosphere there are proportionately high temperatures, which oppose expansion to compression, and appeal is commonly made to this effect of heat to justify the assumption of flow of solid rock under stresses which would otherwise be incompetent to produce movement. Comparison and expansion are thus placed in the balance, and it is a question which outweighs the other.

The question is rigidly conditioned by the rates of increase of pressure and temperature with depth in the lithosphere, which we may accept with the values given by Adams[15] as being in accord with the most reliable available data. They are expressed by the straight lines marked pressure and temperature in plate 9.

The moduli of cubical compression of rocks, as determined by Adams, vary from four to ten million, granites being the most compressible and basic intrusives the least so. Adams says:

> "The cubic compressibility, D, of the earth's crust must lie between the values given for the granites and the basic intrusives, approaching one or the other of these values according to the proportion in it of one or the other of these classes of rocks. If we take the average of the values obtained from these two classes of rocks as represented by the seven granites and the five basic intrusives, the value obtained for D is 6,353,500. This, as will be seen, differs little from the value of D obtained for plate glass, which is 6,448,000."

A search for the moduli of thermal expansion of rocks has not resulted in finding satisfactory data. The modulus of cubical expansion of quartz (SiO_2) was determined by Kopp as .000039 to .000042, by Pfaff as .00003840 for 1 to 100 degrees centigrade, and by Fizeau as .00003619 for 40 degrees centigrade. Hornblende, according to Pfaff, has a modulus of .00002845 for 1 to 100 degrees centigrade.[16] These, however, are indi-

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[15] F. D. Adams: Op. cit., 1912, p. 99.

[16] F. W. Clarke: Constants of nature. Smithsonian Misc. Collections, vol. 14, no. 289, 1876.

vidual minerals, and the difference between quartz and hornblende is such as to show that the modulus for granite, for instance, would depend largely upon the predominance of one or another mineral. To this must be added the well known fact that the moduli of linear expansion of crystals vary greatly in different directions, and consequently the expansion of a granite or gneiss would depend on the orientation of the crystals. Thus, in approaching the general problem of thermal expansion in the lithosphere, we are obliged to accept an average modulus, and with the available data it seems that the best approximation to the general fact will be reached by taking the modulus of expansion of glass, which we can compare with the modulus of compression of the same substance. In making this comparison I am not unaware that the expansion of fused silica, silica glass, is exceedingly small, while that of crystalline quartz is high; and by analogy it may be that the expansion of glass is notably lower than that of crystalline silicates. But this possibility does not appear to offset the advantage of comparing the expansion and compression of one and the same substance.

Du Long and Petit determined the cubical expansion of glass as follows:

0 to 100 centigrade	.0000258
100 to 200 centigrade	.0000275
200 to 300 centigrade	.0000307

These results indicate an increasing rate of free expansion with rising temperature. By free expansion we mean expansion under atmospheric pressure. A similar increase was determined by Fizeau for the linear expansion of quartz, measured successively at 10 degree intervals from 20 to 50 degrees centigrade, and both parallel to and perpendicular to the major axis. It seems reasonable, therefore, to assume that the three determinations given above for glass represent a change in rate, which continues with further rise in temperature. We proceed according to this suggestion, although clearly the rate of increase of the modulus of expansion is indeterminate and extrapolation is no better than guessing in the right direction.

The effects of cubical compression and cubical expansion of plate-glass are represented in plate 9 under the conditions of pressure and temperature existing in the lithosphere to a depth of 40 miles, using the data just recited and extrapolating arbitrarily beyond them. The curves are hypothetical and the resultant has only a suggestive qualitative value.

D, the modulus of cubical compression, whose reciprocal gives the decrease in volume of a cubic inch of material for a pressure of one pound per square inch applied on every side, is taken at the value determined by Adams, 6,448,000. If the compression followed Hooke's law, it would then be expressed by the straight line which is so designated. Since, however, it is more probable that the rate of compression diminishes with increased state of compression and higher pressures, the actual amount of cubical compression is presumably more nearly that indicated by the curve OLM. This curve represents compression at surface temperatures.

The expansion of volume due to rise of temperature, under a pressure of one atmosphere, may be represented by plotting the three values determined by Du Long and Petit for 100, 200, and 300 degrees centigrade. The resulting curve, arbitrarily prolonged, gives the line ONP.

The curve OLM, representing compression, is concave downward, whereas ONP, representing expansion, is concave upward. The former is minus, the latter is plus. The difference of their abscissas, as NL, would represent the resultant effect. Down to 40 miles they are, as drawn, divergent, but they would converge at some greater depth. Their divergence indicates increasing density, and the convergence the opposite. Hence the density and strength of rock would become less after passing a certain maximum.

We are thus landed in a conclusion which contradicts the unquestionable fact that density increases continuously with depth, and the contradiction is directly due to the assumption that the effects of pressure and heat are independent. It is clear that pressure, the dominant force under the conditions of gravitation and heating in the lithosphere, controls thermal expansion. The curve ONP may be flattened to the straight OQ, or more probably may be reversed to the curve OR. The latter is necessary under normal relations, if we accept the results reached by Lunn[17] for the increases of pressure, density, and temperature on the assumption that the increase of density varies as the square root of the increase of pressure, as postulated by Laplace.

While it is thus clear that compression controls and limits thermal expansion under normal relations of pressure and heat in the lithosphere, it is equally evident that temperature affects the degree of compression.

Assuming that the curves OLM and OR indicate respectively the degree of compression and expansion, their difference, OST, represents the resultant of the two. The actual degree of compression or expansion of any mass in the lithosphere is thus that state which is determined by the equilibrium of the effects of pressure and heat at any instant. The forces of pressure and heat are all-pervading, ever present, and constantly

[17] A. C. Lunn: Geophysical theory under the planetesimal hypothesis. Carnegie Institution of Washington, Publication 106, 1908, p. 200. Also in Chamberlin and Salisbury's Geology, Am. Sci. series, advance course, vol. 1, 1909, pp. 564-566.

opposed. In the intensely elastic rock they are instantly active and responsive to the slightest change in their relative values. *The lithosphere is vibrant, more vibrant than the most sensitive musical string.*

This concept, which expresses the contest that persists in all bodies subject to gravitation and heat, needs to be emphasized, because rocks as we know them are dull and lifeless, and it is difficult to conceive of them as constituting a sphere which is intensely alive.

Let us now consider the effect of heat upon the strength of rock, which is expressed by the curve LMNPQR of plate 8. Drawn on the assumption of surface temperature, that curve exaggerates the minimum strength which it is supposed to represent. The true minimum would fall within it, and yet not very far within it, if we may accept the relations of compression and expansion, as shown on plate 9, as approximately correct. The effects of expansion due to normal temperatures in the isostatic shell are relatively small as compared with the effects of compression.

In this connection it is convenient to consider the effects on the strength of rock of an abnormal rise of heat sufficient to produce fusion. Let us assume that the rise of temperature is due to a rise of heat from below. Fusion, beginning below, will at some level reduce the internal friction practically to zero. Let this be represented by the point Z, plate 8, whose abscissa is zero. At some point X the effect of the abnormal temperature will be negligible—that is, the rock will have its normal strength. Between X and Z the strength will vary as represented by the hypothetical curve XYZ.

The curve XYZ represents a transition between two states of rock, in both of which rock will flow. The resistance which the rock offers to deformation is, however, very different, being in the one case practically zero, in the other equal to the weight of a column of rock many miles high. Even though it be true, as it is, that the internal viscosity of molten rock is not quite zero, it closely approaches zero as compared with the enormous absolute strength of cold rock under high pressure.

Recalling the relative weakness of cold rock in relation to vertical shear at depths below that at which the factor of safety is reduced to 1, we see how seriously local melting reduces the resistance of the lithosphere to shear. Failure and subsidence are inevitable as melting is approached or occurs, unless heated rock be hermetically sealed in.

The two states of flow contrast in their capacity to transmit stress. The molten rock, with internal friction near zero, will transmit any stress, however slight, in all directions equally, according to the laws of hydrostatics. The solid rock will transmit any stress in the direction in which the stress is exerted. It will fail in the direction of maximum shear. The solid rock is capable of stress orientation. The molten rock is not.

The deformation of molten rock obeys the laws of flow under hydrostatic pressure. The deformation of solid rock under non-uniform stress beyond the elastic limit proceeds by shearing. Although an excessive stress will be transmitted throughout the mass in all directions, the law of shear will control deformation, and any displacement will take place on planes at 45 degrees to the instantaneous stress[18] (plate 10).

SUMMARY AS TO THE MECHANICAL STATE

In conclusion, we may draw from the preceding qualitative survey of the data on the mechanical state of the lithosphere, the following inferences:

1. The internal friction, or the strength which rocks oppose to stresses tending to deform their masses, increases with pressure. The increase is very rapid in the outer 10 miles of the isostatic shell: is less rapid below that; but, in spite of the normal rise of temperature, it continues at a rate which may reasonably be assumed to be related to the increase of density.

2. With reference to the support of the load resting upon any unit mass of rock within the lithosphere, the strength of rock does not increase as fast as the superincumbent weight. The quotient of the strength divided by the load diminishes in such manner that it reduces to 1 at a depth of about 40 miles, or less making allowance for rise of temperature. At that depth a non-uniform stress, equivalent in pressure to the weight of a column of rock, say, 40 miles high or somewhat less, will cause mass movement. No less stress will. Below that depth an even greater non-uniform stress is required, but the ratio to the superincumbent load becomes less than one.

3. The strength of rock in the isostatic shell under normal conditions of temperature is so great that the stresses set up by erosion and sedimentation, whose maxima do not exceed an unloading or loading by an amount of 6 miles of rock, are far within the elastic limit and are, therefore, incompetent to produce mass deformation.

4. Since the elastic volume of the lithosphere is determined by the balance between compression and thermal expansion, and since the opposed forces are instantly and constantly active, the lithosphere is very sensitive and responsive to stresses within the elastic limit.

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[18] B. Willis: Mechanics of Appalachian structure. U. S. Geol. Survey, 13th Ann. Rept., 1891-1892. See plates xciii and xciv and the explanation thereof.

Isostasy as a working Hypothesis

The recognition of heterogeneity as a characteristic of the earth requires that one should postulate either extreme rigidity or a tendency toward isostatic equilibrium, to account for the inequalities of surface which the earth presents. The working hypothesis on which this discussion is based assumes isostasy in the terms in which it was stated by Gilbert.

"Mountains, mountain ranges, and valleys equivalent to mountains exist generally in virtue of the rigidity of the earth's crust; continents, continental plateaus, and oceanic basins exist in virtue of isostatic equilibrium in a crust heterogeneous as to density." [19]

Since Gilbert wrote that interpretation of the hypothesis of isostasy, which he and Dutton had developed in friendly discussion and of which Dutton has become the recognized author, the extraordinarily elaborate and precise mathematical studies of Hayford have demonstrated the truth of the general theory, but have at the same time cast it in a rigid special form which, as it appears to me, is inconsistent with the variety and variable character of geologic facts.

It seems extremely improbable that isostatic compensation is everywhere within the United States nearly complete, as is computed by Hayford and Bowie, it being demonstrated that the processes of degradation and aggradation, and also those of molten intrusion and extrusion, tend constantly to modify the conditions of equilibrium. A coincidence of nearly complete equilibrium within narrow limits would be an accidental and temporary state on which no general theory of the stresses in the earth's crust could be based. This view was stated some years ago,[20] and I do not find that the doubt raised by geologic reasoning has yet been answered by geodetic observation and computation.

In the meantime Adams' researches have demonstrated the very great strength of the crust, and have rendered more difficult the explanation of isostatic adjustments.

We are thus led to reconsider the fundamental assumptions of the Hayford concept of isostasy, and there are two which appear to be inconsistent with geologic facts. The first relates to the distribution of density in any vertical column. The second is the assumption of hydrostatic pressures at and below the level of compensation.

The distribution of density assumed by Hayford postulates uniform

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[19] G. K. Gilbert: Strength of the earth's crust. Bull. Geol. Soc. Am., vol. 1, 1890, p. 25.

[20] Bailey Willis: What is terra firma? Smiths. Rept., 1910, pp. 391-406 (402-403).

differences of density between heavier and lighter masses from the surface to the bottom of the zone of compensation. In order that the density differences should be uniform, each column must consist of the same rock or of similar sequences of unlike rocks many times repeated. The scheme is too artificial to be generally real.

Chamberlin suggested that density differences be assumed to decline with depth.* This assumption limits the vertical heterogeneity of the lithosphere, which must necessarily result from the geologic processes of metamorphism and igneous intrusion, to a special case. It also is artificial, a concession to the limitations of mathematical analysis.†

A comparison between the Hayford and Chamberlin assumptions is, nevertheless, significant. Whereas by certain methods of calculation the uniformly distributed density differences assumed by Hayford gave 76 miles as the depth of the zone of compensation, the same methods applied to the Chamberlin assumption gave 178 miles as that depth. Since these two artificial concepts yield results which differ by 250 per cent, it is reasonable to conclude that actual differences in the distribution of density in unlike columns may also greatly affect the depth of the zone of compensation.

Gilbert has definitely shown that this must be so. In his last contribution to the subject he concludes:

> "The same moderate assumptions as to variation of density which Hayford and Bowie apply to horizontal relations in discussing isostatic compensation yield, if applied to vertical relations, departures in gravity intensity of the same order of magnitude as the outstanding anomalies, after making allowance for isostatic compensation."

His final statement is:

> "At present the map (of gravity anomalies) seems to express chiefly an effect of heterogeneity in the nucleus and an effect of irregularity in the vertical distribution of densities within the crust." [21]

When thus interpreted the map of gravity anomalies refutes the assumption of uniformly distributed densities, on which it is based. It throws the field open to any rational assumption regarding distribution; and the range of possible assumptions is broadened by recognition of the probability that the depth of compensation is different for different distributions of density. The problem is taken out of the straight-jacket of

* T. C. Chamberlin: Review. Chicago Jour. of Geol., vol. xv, 1907, p. 75.

† T. C. Chamberlin: The mathematics of isostasy. Am. Jour. Sci., 4th series, vol. xlix, 1920, pp. 312-313.

[21] G. K. Gilbert: Interpretation of anomalies of gravity. Part C, Professional Paper 85, U. S. Geol. Survey. Contributions to general geology, 1913, pp. 30-31 and 37.

mathematical physics and restored to the conditions of its natural environment. They impose certain limitations, among which is the requirement that the solidity of the earth be recognized as the controlling condition.

The second erroneous assumption of Hayford's hypothesis is that "at and below the depth of compensation the condition as to stress of any element of mass is isostatic—that is, any element of mass is subject to *equal pressures from all directions as if it were a portion of a perfect fluid.*" [22] This interpretation of the term "isostatic" is restated in all the later writings of Hayford and Bowie, and is accepted by Barrell, although the latter recognized the artificial character[23] of the assumption that isostasy is equivalent to hydrostatic balance.

There is a difference between solid flow and liquid flow, to which attention is called on a preceding page (265). In the case of liquid flow there is no orientation of stress, for under hydrostatic pressure stress is equal and uniform in all directions. The movement of the fluid occurs in threads or currents which are incapable of transmitting compressive stress in a definite direction. They bend aside. On the other hand, they respond directly to a tensile stress or lessened resistance, flowing toward the point or area of least resistance. This is readily seen in colored liquids.

In the case of solid flow there is definite orientation of stress, which obeys the law of shear. I was struck with this fact when making experiments on folding and thrusting in solid plastic substances, in 1886-1887. The more homogeneous the solid and the lower the ratio of its viscosity or internal friction to the applied pressure, the more perfect the shearing. Applying this observation to solid flow in the lithosphere, shearing should, and no doubt does, become more perfect as the ratio of strength to load, the factor of safety, decreases—that is, as depth increases.

Movements which take place in obedience to the law of shear are definitely oriented and persistent in direction. The shearing surface originates as a plane. There may be one or an infinite number of parallel planes. The movement results from a compressive stress, which is the resultant of all the stresses acting on the element of mass, less the internal friction.

At and below the depth of compensation, shearing stresses in solid rock may be *equilateral,* if the resistances on all sides be equal, but they

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[22] J. F. Hayford: The geodetic evidence of isostasy. Proc. Wash. Acad. of Sci., vol. viii, 1906, p. 28.

[23] J. Barrell: The strength of the earth's crust. Chicago Jour. of Geol., vol. xxii, 1914, pp. 662-663.

can not be hydrostatic, as if the solid "were a portion of a perfect fluid."

That this conclusion is correct is proved by the distinction between granular and schistose rocks. The former, having cooled from a molten condition, crystallized under hydrostatic stress. Their components are not definitely oriented. The latter, which, according to theory, observation, and experiment, are known to have recrystallized in a solid state under non-uniform stress, exhibit definite orientation of the component crystals.

There is, furthermore, a distinction between liquid and solid flow in regard to the intensity of stress in various directions. Hydrostatic pressure is uniform in all directions, and can develop only in a perfect fluid—that is, in a medium whose internal resistance is practically zero. When the internal resistance or viscosity becomes a significant factor, it neutralizes a portion of any stress tending to produce flow. All resultant stresses must, therefore, be less intense.

Darwin, Hayford, Love, and Barrell have all recognized this fact in calculating the stresses within the lithosphere. The term "hydrostatic" as used in this connection, therefore, needs qualification.

In spite of the fact that the lithosphere is a rigid solid, there must be mobility to permit adjustments of equilibrium to take place in such a manner as to produce that approach to isostasy which exists. Now, mobility may be conceived to be either a permanent or a temporary condition. If it be permanent, the mobility of the rock may be compared with that of pitch, for instance; but if temporary the mobility is that of a crystalline substance dissolving and recrystallizing momentarily and little by little, just above and below the critical temperature and pressure.

The concept of permanent mobility underlies Hayford's statement that, for reasons detailed by him, "it appears probable that under the stress differences within the earth due to the weight of continents and mountains, the material must slowly yield, the continents slowly sink downward and the ocean bottoms rise." [24] Time is the factor appealed to in this argument to give competency to relatively small stresses to overcome relatively great strength of rock-masses and to induce movement.

Permanent mobility, notwithstanding high rigidity, was postulated by Willis in 1907, and the concept of temporarily increased mobility was introduced to account for the periodicity of diastrophic effects.[25] The very great strength of the lithosphere since demonstrated by Adams gives

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[24] J. F. Hayford: The earth a failing structure. Phil. Soc. of Wash., Bull. XV, 1907, p. 63.

[25] B. Willis: Research in China. Carnegie Institution of Washington, Publication 54, vol. 2, 1907, p. 130.

to the postulate of temporary mobility greater importance, even, than it then seemed to have.

It was suggested in the work referred to that periodic rises of temperature, favoring recrystallization and fusion, were a cause of temporary mobility; and this view I still hold.

Gilbert in 1913 presented an argument for recognition of a "relatively mobile layer separating a less mobile layer above from a nearly immobile nucleus," and qualified the concept as follows:

"It is not necessary to suppose that the degree of mobility at the horizon of mobility is that of a liquid at the surface. When such mobility is attained by any but the densest rocks, eruption takes place. It is not necessary to think of the degree of mobility as uniform, either from place to place or from time to time. Its place variation will naturally be coordinate with that of rock types, and its time variation coordinate with epochs of elevation and subsidence. Neither should the depth of maximum mobility be thought of as uniform." [26]

Gilbert did not assign a cause of mobility.

Barrell in 1915 gave the ideas of his predecessors more definite form, and defined the "asthenosphere," or zone of weakness, as that zone "in which flowage is conceived as taking place with but little expenditure of energy, by a ready recrystallization at the temperature of primary crystallization of magmas." His discussion of the subject is exhaustive, and his definition of the zone of mobility is that which most completely satisfies the conditions of current knowledge of geophysics.[27]

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[26] G. K. Gilbert: Interpretation of anomalies of gravity. U. S. Geol. Survey, Professional Paper 85 C, 1913, p. 35.

[27] J. Barrell: The strength of the earth's crust. Chicago Jour. of Geol., vol. xxiii, 1915, pp. 425-429.

Note.—It is apparently essential to a true record that I should correct a misstatement made, no doubt, through misapprehension of my meaning, by Barrell in his review of Research in China (Science, new series, volume 29, 1909, pages 257-260) and repeated in his later articles (Journal of Geology, volume xxii, 1914, pages 672-683; American Journal of Science, volume xlvi, 1918, page 166; ibid., volume xlviii, 1919, pages 301-302). He attributes to me an acceptance of "undertow," due to erosion, as an adequate cause of horizontal crustal movements, saying:

"Willis in 1907 and Hayford in 1911, overlooking Dana's objections, have attempted to make a lateral isostatic undertow the cause of all horizontal movements in the crust, adopting the mechanism of Dutton."

In order that his statement should correctly interpret mine, it would be necessary to replace the words *"lateral isostatic undertow"* with *lateral underpush due to recrystallization, energized by heat and oriented by isostatic stress.*

I quote my original statement, as follows (Research in China, pages 130-131):

"The periodicity of diastrophic effects may be attributed to four conditions which, after a prolonged interval, unite in accentuating the difference between stress and resistance. The first is molecular rearrangement, by virtue of which rocks recrystallizing under stress take on the mineralogical constitution and particular structure which occupies least space. The second is the gradual accumulation of stress through excessively slow but unceasing movement in suboceanic sectors. It is conceived that there is very

Mobility may be, furthermore, a condition of a mass or of a molecule. Evidently, if mobility be a condition temporarily induced in rock by a rise of temperature which first produces effects of recrystallization, it will be at first a molecular condition. But it may pervade an entire mass, if the rise of temperature affects a sufficient number of molecules to such a degree as to cause general recrystallization, followed possibly by fusion. Barrell expresses the concept of magma diffused through the solid rock-mass. Mechanical conditions would, it seems to me, result in a thinly sheeted, lenticular distribution of newly formed crystals and of the ultimate melt.

Nucleal heterogeneity is a concept introduced by Gilbert into the theory of isostasy in his last paper.[28] Whereas Hayford and Bowie implicitly assume perfect isostatic adjustment to be permanently established in the nucleus, Gilbert recognizes reasons for the existence of masses differing as to density and not necessarily in balance among themselves. His view is based on the independence observed between crustal activity and latitude, on the lack of relation between crustal activity and "things visible." For explanation he appeals to "primordial heterogeneity of earth material, a heterogeneity which gives diversity to the flow of heat energy and to physical and chemic changes of crustal regions."

While agreeing with Gilbert in his argument for "primordial hetero-

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slow spreading toward the periphery and consequent accumulation of stress. The resisting mass is, furthermore, unloaded by erosion, and herein lies a third important disturbing effect, though not a primary cause of diastrophism. The fourth condition of periodic movement inheres in the storage of heat in an outer shell, as deduced by Chamberlin, the effect being generally to lower the internal resistance of the masses and promote the tendency to plastic flow and locally to induce special conditions of melting and extrusion. More or less effective combinations of these effects appear to be inherent in the heterogeneous structures of the lithosphere, and to be adequate to account for the diversities of local diastrophism, as well as the universality of deformation during the great epochs. . . .

"If the molecular rearrangements occur in the zone of contact of two adjacent masses, one of which is denser than the other, the net result of all the movements will be from the denser toward the lighter under the directive influence of the heavier. There will thus be established a stress or tendency to flow which in course of time may become sufficient to overcome the resistance of the solid rock and occasion actual flow from the suboceanic toward the subcontinental mass. It is conceivable that this molecular process is only one of several which may give rise to a similar movement, but in view of the rearrangement of minerals in the development of flow cleavage in deep-seated rocks, recrystallization is a process of notable potency and appropriate orientation."

Even though Barrell misunderstood these statements, he later came to recognize the potency of recrystallization as a mode of flow and named it, along with rising temperature, as a principal cause of movement in the asthenosphere:

"A fourth mode of rock flowage is by recrystallization. It is the chief factor, as Van Hise has shown, in the deformation of crystalline foliates. It is thought that this is the method by which the asthenosphere yields, and that a readiness of recrystallization under unbalanced stresses of a permanent nature is the cause of the weakness of the asthenosphere." (Strength of the earth's crust, page 428.)

[28] Op. cit., 1913, p. 35.

geneity," I am unable to follow him in the statement that it appears "quite possible that underflow in a mobile layer might affect a practically perfect adjustment for differences of density above the layer, so as to bring crustal densities and crustal relief into harmony, and yet *leave uncompensated the differences in density of the nucleus.*" The part of the statement which I have italicized appears to overlook the fact that the mobile layer would adapt itself to any underlying inequalities of gravitative attraction, not as readily, but in the same manner as the ocean does, and that at the upper surface of the mobile layer the intensity of gravitation would become, by adjustment, everywhere the same.

This conclusion agrees with the concept of Hayford and Bowie as to the uniformity of gravitative stresses at the base of the zone of compensation, but it recognizes diversity of constitution, and therefore inequality of local stresses, below that level.

It is necessary to push the consideration of the heterogeneous region below the zone of compensation further. To what degree may it be regarded as a firm, immobile foundation? To what depth does it partake of the character of a mobile layer in which adjustments occur?

The asthenosphere, according to Barrell, who bases his estimates on the work of Chamberlin and Lunn, corresponds with the outer zone in which the heat conductivity of rocks is lower than in the nucleus, and there is consequently a periodic banking up of heat energy. The depth of this outer zone is about 800 miles.[29] Now, immobility would be possible at constant temperature, but it is impossible in the presence of accumulating heat energy. It has been shown on a previous page that the lithosphere must be intensely vibrant, readily responsive to changes of temperature. There must be corresponding changes of density, and in a heterogeneous mass they would be localized according to conductivity. Similarly, the major effects of fusion and movement of magmas must produce local changes in equilibrium.

Thus the asthenosphere presents the concept of a deep zone, in which molecular and molar changes of state may introduce unbalanced stresses at any time; and within that zone non-uniform stresses must be very effective in producing molecular or molar movements. It is difficult to escape the conclusion that the asthenosphere is the site of activities which may and do periodically disturb the isostatic balance of overlying masses.

We are thus led to question the meaning of the term "depth of compensation." According to Hayford, it is the depth below sealevel at which the isostatic compensation is complete. If we accept the arbitrary

[29] T. C. Chamberlin and R. D. Salisbury: Manual of geology, vol. i, 1909, p. 566.

assumption of sealevel as a reference datum, the definition may stand. It is then equivalent to the statement: there is a depth at which isostatic compensation is complete, and this depth shall be called the depth of compensation. If, however, we raise the question, Is the depth of compensation uniform? we must answer, No, not according to geologic and physical evidence. It probably ranges through several hundred miles and may involve a large part of the globe.

Is isostatic equilibrium nearly perfect, as is indicated by the geodetic calculations? Probably not, for those calculations rest on the assumption of an artificial scheme of density distribution, and on assumed conditions of hydrostatic pressure which can not exist, except where there is local and temporary melting.

What, then, is the significance of the figure of 76 miles, calculated by Hayford, or of 60 miles, more recently published by Bowie, as the mean depth of compensation? Is there any basis for believing that it approximately defines the base of the isostatic shell? Even though the mathematical demonstration is too precise, I believe there is such a basis and that the general conclusion regarding isostasy is not invalidated. My reasons are as follows:

It is probable that heterogeneous masses in the lithosphere and nucleus are of much greater horizontal dimensions than vertical dimensions. Thus, a lens of basalt 10 miles thick and but 100 miles in diameter would be an exceedingly thick lens. One to one hundred or one thousand would seem more probable ratios. If this be so, we may say that the lithosphere is thin bedded as to density. The number of variations of density is, then, large in any column of considerable depth.

Second, the differences of density existing between masses at or near the same level in the lithosphere are relatively not great. A difference of 10 per cent would be large.

If these conditions exist in columns, say, 700 miles high, extending from the base of the isostatic shell through the asthenosphere to the base of the latter, it is probable, under the law of averages when applied to numerous small differences, that any two columns will have nearly equal average densities. The resulting inequalities of gravitative stress would be correspondingly small.

Furthermore, the mean radius of attraction of these differences, as observed from points on the surface, would be 400 miles. Their effect on the observations of gravity would be relatively slight.

By contrast, similar differences of gravity occurring in the outer 100 miles of the crust, in the isostatic shell, would produce correspondingly large anomalies of gravity.

Since the outermost crust, from the surface down to a depth of 40 miles, is relatively strong as compared to the load of its own mass, no adjustment of isostatic equilibrium is likely to occur, except below 40 miles from the surface. In the neighborhood of 40 miles the weight of the superincumbent column equals the strength of the rock, and thence downward increasingly exceeds it. Below 40 miles from the surface the temperatures rise above 2,000 degrees centigrade and minerals presumably approach a state of unstable equilibrium. The conditions below 40 miles, therefore, favor adjustment toward uniform stresses, such as define the depth of compensation.

Hence it seems reasonable to conclude, on geological and physical grounds, that there is a zone of adjustment below 40 miles and extending to the base of the asthenosphere; that imperfections of equilibrium in the deeper asthenosphere are less apparent on the surface than similar imperfections in the upper part of the asthenosphere and isostatic shell, and that the latter are chiefly compensated in the zone which lies between 40 and 100 miles below the surface.

Furthermore, the present geologic period corresponds with unusual orogenic activity, which must have reduced departures from isostatic equilibrium to a minimum. It is, therefore, not surprising that refined geodetic investigations should demonstrate an existing state which is a close approach to complete isostasy, or that they should place the depth of compensation at 76 or 60 miles.

Development of the discoidal Hypothesis

DISTRIBUTION OF HETEROGENEOUS MASSES

This section treats of the size of masses which differ as to density, and of their distribution within continental areas or oceanic underbodies.

If a dike of basalt of density 3 cuts a granite of density 2.6, there is a certain distribution on a small scale of masses whose density differs more than 12 per cent. The differences of density which are indicated by isostatic compensation are, as a rule, under 5 per cent; and yet no one would ascribe an isostatic effect to the basalt dike in granite, because of its insignificant size.

The illustration may serve to indicate that heterogeneous masses which may be competent to produce a tendency toward isostatic compensation in the lithosphere must be of notable dimensions. Barrell, by an exhaustive analysis of the evidence bearing on the strength of the earth's crust, arrived at the conclusion that—

"although the relations of continents and ocean basins show with respect to each other a high degree of isostasy, there is but little such adjustment within areas 200 to 300 kilometers in diameter. Individual mountains and mountain ranges may stand by virtue of the rigidity of the crust. Even under level plains equally great loads are permanently borne, loads produced by wide-spread irregularities of density not in accord with the topography above. Isostasy, then, is nearly perfect or is very imperfect, or even non-existent, according to the size and relief of the area considered." [30]

Barrell's statement is a confirmation, after the consideration of the data accumulated during twenty-four years, of that which was formulated by Gilbert as a result of the studies inspired by Lake Bonneville. Gilbert stated as a working hypothesis that—

"Mountains, mountain ranges, and valleys of magnitude equivalent to mountains exist generally in virtue of the rigidity of the earth's crust; continents, continental plateaus, and oceanic basins exist in virtue of isostatic equilibrium in a crust heterogeneous as to density." [31]

Gilbert's studies of Lake Bonneville led him to estimate the residual strain which the earth's crust could bear without yielding as equivalent to that which would be imposed by an excess of weight of 400 to 600 cubic miles of rock. In the discussion which followed his paper, Chamberlin asked to what area he would limit that cubic mass. Gilbert replied:[32]

"That raises a question I have not answered to my own satisfaction. It seems clear to me that the imposition of a long, narrow ridge will be no more effective in producing deformation than a small portion of the same ridge; but it is not clear whether a broad lens of added matter will be as effective as a compact lens of the same weight."

The problem raised by Chamberlin's question may be regarded as that of the length, breadth, and thickness of a slab of the earth's crust which would support the weight of 400 to 600 cubic miles of rock. With any ordinary slab the resistance to bending is a simple function of the thickness of the slab; but, as we have seen in the preceding section, the strength which the isostatic shell opposes to vertical weight is great only in the outer 10 or 20 miles of the crust. Below that its relative strength rapidly lessens. This gives to the horizontal dimensions a peculiar importance. A greater load on a narrow base would be supported more rigidly than a less load on a wide base, provided the proportions were

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[30] J. Barrell: The strength of the earth's crust. Chicago Jour. of Geol., vol. xxii, parts i, ii, and iii, 1914; part iii, 1915, pp. 656-7.

[31] G. K. Gilbert: Strength of the earth's crust. Bull. Geol. Soc. Am., vol. 1, 1890, p. 25.

[32] Ibid., p. 26.

such that the thickness of the slab involved in carrying the load did not in the one case exceed, say, 10 miles, or in the other approached 30 miles. This is but a statement of the general relation between the resisting shear and the resisting moment of flexure, but in the case of the earth's crust the inequality between the two is exaggerated by the weight of the crust itself.

The problem may be capable of mathematical treatment, but in the judgment of the writer mathematical estimates of this character are too artificial to be of great value. Geologic investigations afford a safer guide, and to them we turn.

It is to be observed that all discussions of isostasy deal with the earth's surface as it is, in this very mountainous stage of relief, when, according to gravity studies, isostasy is nearly perfect. In the progress of elevation from the previous condition of less perfect adjustment, which probably was least perfect toward the close of the Cretaceous period, intrusions of heavy igneous rocks and mechanical displacements of large masses have undoubtedly introduced anomalies of gravity, details which confuse the evidence. To illustrate: The Columbia basalt flows in Washington and Oregon constitute a very large mass of heavy rock which has been extruded from some locus below the surface. If that locus was vertically below the present basalt plateaus, there has been no change in the mean density of the underlying column, although the intensity of gravity at the surface has been increased; but if the original seat of the basalt was to one side of the present area of occurrence, if the basalt has been moved diagonally upward, then there has been a considerable addition to the gravitative attraction of the column on which it now rests. It will appear in this discussion that there is reason to postulate the diagonal movement of magmas from their deep-seated sources. Herein lies a possible cause of change in the isostatic relations of areas penetrated by intrusive and extrusive rocks, a cause which has particularly affected the margins of lighter masses. The point which it is here desired to make is that gravity observations of continental areas do not in themselves, without geologic correction, afford a satisfactory means of analyzing the continent into its permanent heavier and lighter elements.

If the specific gravity of a large mass, an element of the lithosphere, be regarded as an original and, within narrow limits, a constant character, which has persisted during recorded geologic history, and if differences of density of large masses be regarded as a controlling condition of the major features of relief of the earth's surface, then the effects of the upward and downward movements of the lighter and heavier masses respectively should be recognizable throughout geologic history. If the masses be of subcontinental size, their individual movements may have been obscured, to some extent, by the movements of the continent as a whole; but the individuality of the larger subcontinental elements should appear in the record as a more or less pronounced tendency on the part of the heavier individuals to lie relatively lower, and on the part of the lighter individuals to stand relatively higher, each with reference to the other. The evident effect would be that the former would become more deeply buried beneath continental and marine sediments, and that the latter would be more deeply eroded.

Let us call the heavier masses negative elements of the lithosphere, the lighter masses positive elements; then we may say: A negative element is indicated as underlying an area which has been the scene of long-continued or repeated deposition, taking the geologic record as a whole, whereas the positive element is presumably the underbody of an area which has more commonly been subject to erosion than to deposition.

The preceding conclusion was stated in 1907 in the reverse order of argument. Isostasy was not so firmly established then on geodetic and geologic opinion as it now is, and the geologic evidence of sediments *versus* unconformities was appealed to as demonstrating the existence of negative and positive continental elements, whose diverse tendencies were interpreted in terms of isostatic balance.[33] Observational evidence of the existence of isostatic balance to the degree determined by Gilbert and Barrell is now practically conclusive and requires, in the opinion of the writer, the recognition of continental elements which differ as to density.

In plate 11 is given an analysis of North America into its negative and positive elements on the basis of sediments and unconformities. It thus rests primarily on the facts of geology and presents the geologic argument independently of the isostatic. It might be modified in outline by comparison with the distribution of gravity anomalies, but were there material contradiction the geologic evidence, where definite, should, in my opinion, be given greater weight, because the evidences of deposition and unconformity are unequivocal and represent an original, persistent condition, whereas the evidences of gravity anomalies are capable of several interpretations and may correspond to recent changes only.

A study of Asia, according to the view of continental structure here presented, yielded as a result the distinction of positive and negative elements.[34] South America appears to comprise at least four positive

[33] Bailey Willis: A theory of continental structure. Bull. Geol. Soc. Am., vol. 18, 1907, pp. 389-412.

[34] Bailey Willis: Research in China. Carnegie Institution of Washington, Publication No. 54, vol. ii, 1907, pp. 115-123 and plate 8.

elements, the Colombian, Guianan, Brazilian, and Patagonian, with surrounding negative elements. The Andes may represent a crushed positive element or elements. Europe, Africa, and Australia exhibit groupings of areas which have been either deeply buried or deeply eroded, and may be interpreted accordingly in terms of heterogeneous structure.

Thus, on geologic evidence it appears conclusively that the structure of continents is complex as to the distribution of density in the underbody.[35] The magnitude of masses of approximately like density throughout each mass ranges from one hundred to many hundred kilometers, measured horizontally. The least horizontal dimension is determined, probably, not altogether by variations of density, but chiefly by the bending or shearing moment of the slab or beam which can support the gravitational strain to which its own weight and any excess load or deficiency of load subject it. This least horizontal dimension is a function of the depth or thickness of the slab. Barrell's researches, already cited, appear to indicate that the least horizontal dimension of a mass which may respond isostatically to gravitational stress is between 100 and 300 kilometers. Bodies smaller than this would not demonstrate their existence by independent movements which would be recorded by erosion or deposition, though their presence may be detected by observations of gravity.

In the underbodies of ocean basins the distribution of masses which differ as to density can not be traced on geologic evidence. We are thrown back on the general theory of isostasy and on determinations of the intensity of gravity. Since oceanic waters protect their basins from superficial disturbing activities, except that of deposition in the littoral regions, we might expect that isostatic balance would be more nearly complete throughout oceanic underbodies. It appears, however, as will be argued in a subsequent section, that the great deeps are loci whose underbodies are especially liable to fusion, and that the molten material flows from under them, leaving them occasionally defective as to gravity. Hecker's observations of the intensity of gravity on the oceans appeared to demonstrate that the suboceanic masses are close to isostatic equilibrium, except for negative anomalies over certain deeps and positive anomalies on volcanic islands; but his methods of observation have been challenged and it is wise to withhold judgment as to their value. They were calculated, furthermore, according to Hayford's assumption of the

[35] The term *underbody* has come, in the writer's thought, to indicate the mass underlying any area, not including the superficial veneer of rocks in the zone of clastic deformation. It is used to designate the column extending from, say, 10 kilometers below the surface down to the bottom of the lithosphere.

sealevel surface as a reference datum, and this has been questioned by MacMillan, who argues that "a true isostasy must be based on a level 9,000 feet below the sea." [36] These doubts, however, touch only the degree of perfection of isostatic adjustment at the present time. They do not invalidate the general hypothesis that the relief of the earth's surface has always approximated that which the larger elements of the lithosphere tend to assume because of differences of density. That hypothesis remains the one which most nearly satisfies geodetic and geologic evidence and is therefore the safest guide. Accepting, then, the depths and shallows of the ocean basins as an approximate expression of the distribution of heterogeneous masses, we may analyze the underbodies of the oceans accordingly.

Groll's bathymetric maps constitute the latest presentation of the available data as to the distribution of ocean depths, which, by hypothesis, express the distribution of denser and lighter suboceanic materials. It is not necessary here to describe what may better be seen in the maps themselves. Plate 12 gives a reproduction of the north and south central Atlantic region, adapted from Groll, and brings out the four major basins, together with many minor ones, which constitute the deeps of the ocean. The map also illustrates clearly the winding central ridge and its branches, which divide the basins and occasionally widen out into broader shallows.

Our knowledge of the depths of the Pacific and Indian oceans is much less complete than that of the Atlantic, but where soundings are numerous differences of depths are characteristic, and it is evident that the deeps of these two oceans are of the same general order of magnitude as those of the Atlantic.

Thus it seems safe to generalize that the underbodies of the oceans consist of elements of suboceanic horizontal dimensions which, according to the hypothesis of isostasy, indicate differences of density by the greater or less depth which their surfaces have assumed.

The general postulate with which we proceed is, then, that the underbodies of continents and of oceans consist of masses of subcontinental or suboceanic dimensions which differ in density; that these masses are recognizable within continental areas by their tendencies to stand relatively high or relatively low, as recorded by geologic history; that by analogy the masses composing the underbodies of the ocean have shown similar tendencies during the same prolonged history.

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[36] W. D. McMillan: On the hypothesis of isostasy. Chicago Jour. of Geol., vol. xxv, 1917, pp. 105-111.

DEPARTURE FROM AVERAGE DENSITIES

Considering the major differences in density of the lithosphere, we contrast the underbody of a continent as a whole with that of an ocean as a whole. Minor differences also are recognizable, and, according to the maps, the separate masses within a continent are lighter bodies lying in a somewhat heavier general mass, whereas the distinctive elements beneath the oceans are heavy bodies lying in a somewhat lighter general mass. The details of form are interpretations of partial knowledge and are subject to change, but the relations seem to indicate that there is a continental underbody of average density for the continent and an oceanic underbody of greater average density for the ocean, while within each there are masses which depart from the average toward greater lightness on the continent and toward greater density beneath the ocean.

It is obvious that where isostasy is imperfect the resultant stresses set up in the lithosphere will be of moderate degree in the marginal zones of juxtaposed masses of average continental density and average sub-oceanic density, whereas greater stresses will develop where extremes of density characterize neighboring masses.

The preceding statements deal with the horizontal distribution of densities. The vertical distribution, which is equally important, has been discussed in a preceding section. Geologically, any assumption of uniform densities or of uniformly varying densities from the surface down is unreal. The igneous rocks within reach of observation vary widely in density. The average density of the earth as a whole greatly exceeds the average density of surface rocks. The processes of igneous intrusion and extrusion tie the superficial masses of rock to their deep-seated sources. The heterogeneity exhibited in the visible masses must characterize all the shell which has been subject to igneous activity, and that shell certainly comprises not only the zone of compensation, but also the asthenosphere, or zone of fusion. We cannot avoid the conclusion that vertical heterogeneity exists to great depths.

DIRECTION OF ELASTIC STRESS

We turn to the consideration of elastic stresses which are produced in the lithosphere by the weight of the rocks themselves. A familiar treatment of the subject, based on the assumption that isostatic equilibrium is initially perfect and extended to the condition of imperfect equilibrium, is given by Barrell.[37] He, however, introduces an artificial condi-

[37] J. Barrell: Strength of the earth's crust. Chicago Jour. of Geol., vol. xxii, 1914, pp. 655-670.

tion in accepting the postulate of hydrostatic pressures beneath the isostatic shell, as was assumed by Hayford and also by Love, to simplify the mathematical calculation of stresses. Barrell recognized the artificiality of the postulate, but employed it, nevertheless, in his diagrams. In the following discussion we adhere to the assumption of a solid earth, except as fusion is temporary and local, and we must therefore recognize that stresses beneath the isostatic shell can not in general be hydrostatic.

For the general case, let it be assumed that there are two adjacent columns, one of lighter, the other of denser material. In each column, at any level below the upper surface, there is a vertical pressure proportioned to the height of the column above that level, and there must develop a corresponding shearing stress, by virtue of which the column will tend to spread laterally. If the columns be juxtaposed, their lateral stresses will be mutually opposed; if equal, they will balance; if unequal, there will remain an unbalanced stress difference. It is with this unbalanced lateral stress difference that we have to deal.

As an initial case, assume that the surfaces of the two columns are on a level. Then, at any depth below the surface, the column of denser material will weigh more than the same column of lighter material, and the lateral stress difference will be from the heavier toward the lighter column. This condition may have existed beneath a peneplain.

If the surface of the lighter column should lie lower than that of the heavier, the above condition would simply be exaggerated. The lateral stress difference from the heavier toward the lighter would be increased.

The case which commonly occurs and which is assumed in the discussion of isostatic equilibrium is that in which the surface of the lighter column stands above that of the heavier column. It is represented in plate 13.

At the level of the top of the heavier column its lateral stress is zero, while that of the lighter column is a finite quantity. The stress difference is, therefore, from the lighter toward the heavier. With every foot below that level the weight of the heavier column, and consequently its lateral stress, gains faster than the corresponding pressures in the lighter column. The former will eventually equal the latter and the stress difference will be zero.

In plate 13 let the shaded triangle represent the lateral stresses and the black triangles the stress differences. If the weights of the columns be equal at the depth AC, the lateral stresses will be equal and the stress difference will be zero at that level. Above the level of zero stress difference, the stress from the lighter toward the heavier column at any level is greater than the opposed stress by the width of the black triangle, whereas below the level of zero stress difference the pressure from the heavier toward the lighter column is the greater by the width of the lower black triangle.

The position of the level of zero stress difference depends on two variables: the ratio of densities of the two columns and the difference of level of their upper surfaces. In any particular case the density ratio is assumed to be fixed, but the relative levels of the upper surfaces vary in course of geologic changes.

When the two upper surfaces are on the same level the position of zero stress difference coincides with that level, because the lateral stresses there are both equal to zero.

As the upper surface of the lighter column rises above that of the heavier, the position of zero stress difference sinks lower. The greater the altitude of the lighter column above the heavier, the deeper the level of no stress difference.

If we take the level of zero stress difference as the base of the two columns, above which their heights are measured, and consider the ratio of their heights, we may compare the latter with the ratio of their densities. Since the lateral stresses can be equal only under equal weights, other things being the same for both columns, the ratio of heights must be inversely as the ratio of densities. In columns of indefinite extension downward there will always be a depth at which the weights will be equal, or at which the superincumbent columns will be in equilibrium with one another. This is the condition of isostasy.

Theoretically, isostatic equilibrium is always complete and perfect for any difference of surface heights, at some lower level at which the weights of the columns are equal. If the difference of surface heights changes, the level of equilibrium rises or sinks and all stresses in the columns and between the columns are modified. In mobile material the readjustment of stresses to the new conditions of equilibrium proceeds rapidly. In rigid material the readjustment is delayed, and residual unbalanced stresses persist during a period which is the more prolonged as the stresses are relatively small as compared with high rigidity.

In the case of the earth, changes in the weight of adjacent columns, such as by erosion and deposition of sediment, proceed slowly, but the stresses set up are very small and the rigidity of the earth is very high. There is, therefore, a lag in the adjustment, which results in residual stresses. The latter accumulate during periods of general planation and are relieved during epochs of mountain growth.

In the actual state of the lithosphere it appears that the level of no horizontal stress difference, coinciding with the bases of columns of equal weight, lies at a depth of 60 to 100 miles below sealevel, according to the calculations of Hayford and Bowie. On the assumption that the depth is everywhere the same, the figure of 76 miles best satisfies the observations of the deflection of the vertical and of the intensity of gravity and is commonly referred to as the depth of the zone of compensation. This is, however, an exact mathematical conclusion based on assumptions of the distribution of gravity which are artificial, and it is therefore not a geologic fact.

The preceding considerations lead to the recognition of the isostatic shell, an outer layer of rock whose upper surface is the uneven surface of the earth and whose under surface is that continuous, but warped, level at which the weights of unlike columns are equal and the horizontal stresses are balanced.

Any change in the levels of the upper surface changes the position of the under surface, and thus modifies the stresses throughout the isostatic shell.

Beneath the isostatic shell differences of density, if they persist as geologic reasoning shows they should, must occasion differential lateral stresses which, since the columns lie below a surface at which the overlying loads are equal, must always be directed outward from the denser toward and into the lighter column.

In the solid mass below the isostatic shell, as in the isostatic shell itself, stresses are oriented as to direction. They can become hydrostatic only when some part of the mass is locally melted.

It is desirable to consider the resultant stresses in case the relative heights of adjacent columns are modified by geologic change. The dominant and general case is that of erosion of uplands and burial of lowlands or sea-bottoms.

In that change of surface levels, whatever may have been the position of the level of zero stress difference, it is gradually raised. As it passes upward the lateral stresses change, those from the unloaded column decreasing and from the loaded column increasing. At any point which passes below the level of zero stress difference, the direction of lateral stress difference changes from toward the loaded to toward the unloaded column.

At the same time certain elastic stresses are set up by the unloading and loading. They are vertical in direction and represent the effort of the columnar mass, which may be regarded as an elastic spring, to accommodate itself to the changing conditions of load. In the unloaded column the vertical stress is upward, in the loaded it is downward.

The resultants of these horizontal and vertical stresses are inclined at angles, which are determined by the relative intensities and to which definite values can not be assigned, the component stresses being indeterminate and variable; but in general the direction of the resultants is fixed and they form a circuit.

Let the level of no stress difference in any vertical section be taken as the axis of an ellipse; then the resultants of the horizontal and vertical stresses may be regarded as forming the ellipse itself. Their direction will be upward in the unloaded column and outward toward the loaded column above the axis. In the loaded column the resultants will tend downward and from it inward below the axis.

To assist in visualizing these forces, assume the position of an observer looking northward along the Atlantic coast of the United States. The elastic stress circuit will then be oriented clockwise around an axis which lies beneath the coastal plain or continental shelf, according to local conditions.

The depth at which the axis of no horizontal stress difference lies depends on the completeness of isostatic equilibrium. If the equilibrium is perfect according to the currently accepted form of the isostatic theory, the center lies at a depth of 76 miles. Where the unloaded column is deeply eroded, and therefore exhibits a defect of mass, the axis should rise toward or even to the surface. It may have approached the extreme upper position possible during the Jurassic-Cretaceous peneplanation of eastern North America. On the other hand, it might locally have been depressed below 76 miles by the outflow of the Triassic traps, if the weight of the lava-sheet were added to that of the column of lighter material at a time when it was otherwise in isostatic equilibrium with its surroundings.

These considerations indicate that the resultant elastic stresses vary in position and intensity with geologic changes, but their general clockwise relation in a circuit, as presented to an observer looking north on the Atlantic coast, is permanent. On the Pacific coast of North America the general direction of resultant stress would be anticlockwise to an observer looking north.

The elastic stress circuit in the solid lithosphere represents the unbalanced non-uniform stress which must direct any mass movement or molecular movement, provided that conditions arise which make such movement possible.

EFFECTS OF ISOSTATIC STRESS ON FOLIATION

Uniformly stressed molten rock, crystallizing under hydrostatic pressure, assumes a holocrystalline structure like that of granite. Unequally stressed solid rock, recrystallizing under unequal stresses, takes on a foliated or schistose structure, with the longer axes of the crystals or the laminæ of foliation oriented in the direction of least stress.[38] Van Hise, in 1896, first formulated the latter proposition in terms of rock flowage:

"The secondary structure of a rock which is deformed by plastic flow develops in the plane normal to the greatest pressure, and this structure is true cleavage."

Becker contended that secondary structure must develop in the shearing planes or planes of maximum tangential stress, which would stand at 45 degrees or at greater angles to the line of force, as stated by him in 1893.[39]

It is important to recognize that the controversy which ensued dealt with two distinct processes, both of which affect deep-seated rocks, but under different conditions. Van Hise's concept of flowage by recrystallization involved a physico-chemical process of molecular or particular solution and redeposition in response to stresses within the elastic limit. Becker's "maximum tangential strain" is a mechanical effect, capable of producing actual shear only when in excess of the elastic strength of the rock. Since, in the present discussion, we are dealing with stresses within the elastic limit, we are treating the case described by Van Hise. His views, originally based on most extended and thorough research in the field and laboratory, have been fully confirmed by the writers referred to above. When a rock recrystallizes under non-uniform pressures or stress, the crystals develop in a plane at right angles to the maximum pressure or in the direction of least stress.

In the preceding paragraphs we have considered the unbalanced stresses within the elastic limit, set up by erosion and deposition and directed as already described, upward in the unloaded mass, downward in the loaded mass, and horizontally or diagonally between them, forming a circuit. Nowhere, presumably, are these stresses strictly vertical or

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[38] C. R. Van Hise: Principles of pre-Cambrian geology. U. S. Geol. Survey, 16th Ann. Report, 1896, p. 639.

C. R. Van Hise: Metamorphism and rock flowage. Bull. Geol. Soc. Am., vol. 9, 1898, pp. 296-328.

C. K. Leith: Rock cleavage. U. S. Geol. Survey, Bull. 239, chap. vi, 1905, pp. 107-118.

G. F. Becker: Experiments on schistosity and slaty cleavage. U. S. Geol. Survey, Bull. 241, 1904.

F. E. Wright: Schistosity by crystallization. Am. Jour. of Sci., fourth series, vol. xxii, 1906, pp. 224-230.

C. K. Leith: Structural geology, 1913, pp. 16-21.

C. K. Leith: Metamorphic geology, 1915, p. 176.

[39] G. F. Becker: Finite homogeneous strain, flow, and rupture of rocks. Bull. Geol. Soc. Am., vol. 4, 1893, p. 50.

horizontal, but they will approach those directions and may be discussed accordingly.

These stresses, in the writer's judgment, will produce no effect of movement in the masses other than a very slow elastic recovery, unless external forces occasion recrystallization. The most competent external force is heat, which increases the speed of chemical reaction as the powers of 2 with each rise of 10 degrees centigrade in temperature, within the range of temperatures commonly employed.

A rise of temperature may conceivably result from the gathering of heat energy, conducted from within the earth, as postulated by Chamberlin, more rapidly through the denser interior than through the outer shell, and therefore accumulating in the latter;[40] or it may be due to the intrusion of rocks melted at a deeper level. It is to be noted that heat may also be developed by movements, but we are now concerned with masses which, as a whole, are at rest, although subject to non-uniform pressure.

The effect of non-uniform pressure on melting or solution is far greater than that of uniform pressure and is itself greatly augmented by a rise of temperature. Johnston and Adams, by their investigation of the physical and chemical behavior of solids under high pressures, demonstrated that non-uniform pressure "always lowers the melting point and raises the solubility, and by amounts which are many times greater than the corresponding changes with uniform pressure." They emphasize further the effects of high temperature, which they cite as an agency which is coordinate with non-uniform pressure, saying:

> "Even with unequal pressure, the effect of change of temperature is so very important that the two factors must be considered simultaneously." [41]

In the geologic case, typified by the underbodies here considered, any rise of temperature will first affect the deeper-seated levels in which the direction of maximum stress is approximately vertical and that of least stress is approximately horizontal. In the underbody of a depressed mass, which is under added load, and therefore under elastic compression, the direction may be truly horizontal. In the direction from the loaded to the unloaded the influence of the elastic relief due to unloading will form a resultant, which will trend upward and will approach or reach verticality in the unloaded mass.

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[40] Chamberlin and Salisbury: Manual of Geology, vol. i, p. 629. Hypothesis of 1909.

[41] John Johnston and L. H. Adams: On the effect of high pressures on the physical and chemical behavior of solids. Am. Jour. of Sci., 4th series, vol. xxxv, 1913, pp. 206-253. Conclusions, p. 251.

If, owing to a rise of temperature, recrystallization occurs under these stresses, the foliation will be oriented accordingly and will develop a curved structure, bending upward from under the loaded mass toward the surface of the unloaded mass.

The preceding is a conclusion on which rest all further deductions regarding the structure of border zones subject to erosion and deposition of sediment. It should therefore be examined with care. To reconsider:

(*a*) It is assumed that a group of bodies, heterogeneous as to density, has attained a state of isostatic equilibrium and consequently was affected throughout by balanced stresses. It is certainly doubtful whether the hypothetical equilibrium has ever been reached, but a state approaching it doubtless has been, and any residual strains existing in the masses would represent the preceding movements toward adjustment. They would therefore be in the direction of flow, upward in the lighter, downward in the heavier element, and from the heavier toward the lighter below. These are the directions of elastic stress due to erosion. It follows, therefore, that any unbalanced residual strain in a system tending toward isostatic equilibrium, but simultaneously subject to transfer of load, is oriented in the direction of the elastic stress concurrently set up by erosion and deposition.

(*b*) It is assumed that erosion and deposition do set up elastic stresses in the direction described. It being demonstrated by experiment that rocks under atmospheric pressures are approximately as elastic as glass[42] (E, or Young's modulus of elasticity for plate-glass, 10,500,000; for granites, 5,685,000 to 8,295,000; for basic intrusives, 9,000,000 to 15,650,000); it being determined by seismic observations that elastic vibrations are transmitted in the lithosphere rapidly, as through steel; and the isostatic shell being in a state of vibrant sensitiveness to stress, as shown in the preceding discussion, there can be no question but that the underbodies transmit the elastic stresses due to slow unloading and loading.

(*c*) The adequacy of the non-uniform elastic stresses to direct the orientation of crystal growth is perhaps open to question. In Wright's experiments a cube of wollastonite glass, for example, was weighted and heated to a state of viscosity at which crystallization began. "It was then in a state of fair rigidity and capable of supporting a certain amount of unequal strain." The amount of strain is not given, but it evidently was within the elastic limit. In the case of foliation in the lithosphere,

[42] F. D. Adams and E. G. Coker: An investigation into the elastic constants of rocks, more especially with reference to cubic compressibility. Am. Jour. of Sci., 4th series, vol. xxii, 1906, pp. 121-122.

let it be pointed out that the non-uniform stress is not supposed to *cause* recrystallization. That is the work of rising heat energy. With regard to the elastic stress it is postulated only that in a state where the temperature of the minerals is rising to the point of solution or of melting, as in Wright's experiment, it is sufficient to cause a mineral to pass into solution or to melt in the locus of maximum pressure before it does so in that of least pressure. Recrystallization in a form better adapted to the environment is assumed to occur. The system is, therefore, close to equilibrium, and a very small difference of stress would probably be effective.[43]

(*d*) It may with reason be regarded as probable that the elastic stress, which accumulates very slowly, is steadily converted into a permanent strain, which, being within the elastic limit, is equivalent to a separation of the molecules in the direction of elastic stress. The energy thus taken up is potentialized in the mass and, as argued by Van Hise, is available for service, as, for instance, to promote chemical reaction.[44] The direction of this force would be the same as that of the original stress, and it would therefore have the same effect in orienting recrystallization.

Thus it appears not unreasonable to assume that the elastic stress circuit, which constitutes a condition which has pervaded the heterogeneous lithosphere since its earliest development, has directly or indirectly acted to orient foliation in rocks in process of recrystallization. This is particularly true of deep-seated rocks in the zone where the weight of the load exceeds the strength of rock and high temperatures are liable to variation.

DISTRIBUTION OF FOLIATION

In the preceding discussion a certain rôle is assigned to the elastic stress set up in the lithosphere by loading and unloading due to erosion and deposition. That rôle consists in orienting foliation in a vertical plane, in a curve which rises from under the loaded mass into an adjacent unloaded mass. Clearly, if this be the case, the outcrop of the foliation in a horizontal plane—that is, its strike—should conform to the outlines of the loaded and unloaded masses. The outlines may be straight or curvilinear, but, in the nature of the case, since we are dealing with more or less deformed rounded bodies, they will commonly be curved.

Suess traced the winding *leitlinien,* or directrices of the continents, in his great survey of the face of the earth. He linked in one chain, which

[43] John Johnston and Paul Niggli: The general principles underlying metamorphic processes. Chicago Jour. of Geol., vol. 21, 1913, p. 206.

[44] C. R. Van Hise: Treatise on metamorphism. U. S. Geol. Survey, Monograph 47, 1904, pp. 46-47 and 690-692.

may serve as an illustration, the Atlas, Apennines, Alps, and Carpathians, around the depressions of the western Mediterranean, the Adriatic and Po, and the Hungarian plain. The chain extends eastward across Asia into the Malay Peninsula and the curve of Sumatra and Java. Its course lies around, between, and in the margins of basins, some of which have been obliterated in the Tertiary deformations, while others still persist. Some are small and intercontinental, others are oceanic. The local structural phenomena are among the most complex known to geology, and it is not the purpose of this article to discuss the facts or the mechanics of their development, but they are more readily brought into accord with mechanical principles if we recognize that the winding directrices of the ranges are the outcrops of foliation, which is curved also in any vertical section.

The directrices traced by Suess are based on the strikes of the axes of folds, on the strike of schistosity, and on the trends of intrusive rock-masses. The three are intimately related, and schistosity is the fundamental one, for schistosity in deep-seated rocks guides the folding of sediments and the course of intrusives. From this relation results the parallelism which is so generally observed and which may be utilized in the present discussion to trace the effects of the relation between foliation and the postulated elastic stress.

As a case in point, let us take up the zone of metamorphic rocks of the eastern Appalachians from Newfoundland to Georgia. It is 3,000 kilometers (2,000 miles) long and from 50 to 300 kilometers wide. It is a continental feature. As an element of the continent it has persistently stood relatively high and is one of the original nuclei, which has from the earliest known geologic periods to the present formed a land between the Atlantic basin and the epicontinental seas or lowlands of the interior. It is therefore a mass which has been deeply eroded and in which, if erosion directs foliation in the underbody, the dip of the exposed schists should be steep and the strike of the schists should conform to the limits of the eroded area, which, on the east, are the limits of the Atlantic basin. Furthermore, intruded igneous bodies should have a similar orientation.

The required relations of dip and strike exist throughout the belt.[45]

The original foliation—at least, the most ancient foliation—characterizes the Carolina gneiss, which is the matrix into which younger igneous rocks have been intruded. The earlier intrusions are pre-Cambrian;

[45] See Geologic Map of North America. U. S. Geol. Survey, Professional Paper 71, 1910, in pocket; also published separately; and folios of the Geologic Atlas of the United States, No. 192, Eastport, Maine; No. 149, Penobscot Bay, Maine; No. 83, New York City; No. 162, Philadelphia; No. 70, Washington, D. C.; and No. 151, Roan Mountain.

others, in New England at least, occurred during the middle Paleozoic; still others invaded the entire belt from Newfoundland to Georgia in the late Paleozoic; the latest eruptions are of Triassic age. They all show the same general control of orientation. Even though many masses are rounded, the distribution of such masses and the directions taken by the offshoots from them coincide with the foliation of the gneiss and correspond to the parallelism of the foliation, the lenticular intrusions, and the outline of the Atlantic deep. As a phenomenon which has persisted from pre-Cambrian time to the present, the parallelism of structure and oceanic outline is an impressive fact.

Along the Pacific coast of the Americas there is an enormous development of batholiths associated with ancient and Mesozoic rocks and following the continental border with marked persistence. The linear form of the batholiths, regarded as intrusions, demonstrates the existence of an earlier foliation parallel to their present trend. For North America we know from the evidence of Algonkian and Cambrian sediments and their fossils that there was land in the Pacific border region during those periods.[46] It became submerged beneath epicontinental seas, to a greater or less extent at different periods and emerged as a whole in the late Mesozoic, the uplift accompanying the intrusion of the great batholiths.

Along the Pacific as along the Atlantic border, the controlling structure was developed in the continental margin during an early geologic period, when erosion had removed an upper layer down to the metamorphosed and granitic rocks of the pre-Cambrian—that is, when the zone had been unloaded and adjacent areas had been loaded.

In general terms, then, though without the same persistence of land conditions, the Pacific coast shows a relation between a deeply eroded surface and the outcrop of steeply dipping schists, whose orientation is what it should be if it were controlled by the elastic stress resulting from unloading.

If the bearing of the place relations of foliation to erosion and loading be regarded as favorable to the recognition of a causal relation, it becomes pertinent to push the inquiry to the further test of more detailed studies. This has been done for the Coast ranges and Sierra Nevada of California, and the deductions from the facts of local orogeny are entirely in harmony with the inferences from general relations.

The characteristic mountain form of the Pacific ranges is a tilted or rotated block. If its under surface be conceived to be one of curved

[46] C. D. Walcott: Cambrian. U. S. Geol. Survey. Correlation Papers. Bull. 81, 1891, pl. ii (section).

Ibid., Evolution of early Paleozoic faunas in relation to their environment. In outlines of geologic history, Willis and Salisbury, 1910, pp. 28-29.

foliation, the rotation finds a ready explanation. The mechanics of movement, uplift, and vulcanism constitute an accordant group of phenomena under this hypothesis.[47]

SUBOCEANIC CONDITIONS OF FOLIATION

In the preceding section the stress which is regarded as competent to orient foliation is due to the unloading and loading effects of erosion. The area beneath which the stress develops is evidently the elevated land which is eroded and any hollow in which the sediments are deposited. The latter may be continental or suboceanic. If the loaded belt be submarine, it is commonly narrow as compared with the extent of the basin. Beyond it lie great expanses of the ocean floor, where the loading is insignificant. Within these areas lie the greater deeps, and therefore, by postulate of isostasy, the greater densities. What of the orientation of foliation in these extensive regions where erosion and sedimentation are absent or negligible?

Any answer to this question, in order to be consistent with the basal hypothesis of the discussion, must be framed on the assumption that the underbodies of the deeps have reached their present position in the effort to attain isostatic equilibrium. If, at any stage of development of the ocean basins, the underbodies stood too high with reference to the surrounding masses, they must have sunk to their present depressed positions. The stresses developed before and during subsidence must have been directed horizontally outward, and in the adjacent masses upward. Any adjustment by recrystallization of the solid substance would, therefore, result in an accordant foliation.

It would appear that this reasoning is conclusive, provided the underbodies of the deeps have assumed their low positions by sinking from higher ones. If, however, they have risen from lower ones or tend to do so, the resultant stresses and foliation would be directed upward within them and would be directed outward toward them in the neighboring masses, since the latter would have sunk as the deeps rose till equilibrium was reached.

The latter postulate does not accord readily with any process of gravitative accumulation of the denser and lighter masses. It, the postulate of an original overdeepened basin, seems in general less probable than that of original overburdened areas; and this inference is confirmed if we consider the distribution of igneous outcrops, which are grouped around rather than conspicuously in ocean basins. The latter would be

[47] To be discussed in an article on the mechanics of the Pacific ranges, to appear in the Chicago Journal of Geology.

the case if the planes of foliation led inward and upward beneath the deeps.

Thus it seems we may reasonably conclude that suboceanic foliation is oriented in an approximately horizontal attitude, but rises in the broad marginal regions to join the curve that extends up into the eroded continental areas.

The preceding statement, which applies to ocean deeps in general, should not be understood as excluding the occurrence of overdeepened hollows. On the contrary, overdeepening would be likely to result under certain conditions as a local effect.

Thus, suppose that the support of a suboceanic body be melted and pressed outward toward the surface. If the foliation be horizontal or gently inclined, the melt would escape laterally and the body would sink. Overdeepening must result, if isostatic equilibrium existed previously. However, a comparison of the volume of extruded igneous rocks with the areas of ocean deeps indicates that overdeepening due to their extrusion can be of notable amount only if narrowly localized. The Tonga Deep would seem to offer a case in point, but the Pacific, Indian, and Atlantic deeps in general are too extensive.

The horizontal diameters of an area which might thus assume a disk-like structure under an ocean basin are not indicated by the preceding analysis. It is clear, however, that one possible limit is a continental margin along which the foliation would rise from under the loaded zone to the coast. Another limit is the divide, so to speak, between two disk-like masses, both of which, together with the intermediate zone, are submerged. Such a divide would correspond, for instance, with the mid-Atlantic ridge.

DISKS AND INTERDISKS

In a preceding section, in discussing the departures from average densities, it was suggested that we may recognize an average density of the oceanic underbody and also an average density of the continental underbody, which is lighter; and that within each of these there are bodies which are in the former case heavier, in the latter case even lighter. It is not supposed that the distinctions as to density are in any way precise. They are merely relative, but they exist if the postulate of isostatic equilibrium be true.

Curved or discoidal schistosity has now been deduced as a structure rising from beneath a heavier into a lighter body. The underbodies considered in the discussion have been those which give the more striking illustrations, those of continents as contrasted with those of ocean basins;

but there is nothing in the controlling conditions to limit the argument or conclusion to those contrasts. Differences of density within a continental underbody or within an oceanic underbody may have similar effects, provided the initiative activity of internal heat shall reach the area.

In the analysis of continental structure we have distinguished positive and negative elements, and we would anticipate that the former would exhibit steeply dipping schistosity, which by hypothesis should curve outward beneath the negative areas. The latter constitute the continental matrix, as it were. They are commonly confluent. A section across a negative area from one positive element to another should show a structure which would be convex downward—that is, would exhibit a synclinal foliated structure; but a section along the axis, which winds between positive elements, would transect nearly horizontal foliation.

The oceanic underbody contrasts with that of the continent in that the average density is less than the density of the surrounded masses. The contrast may be stated thus: Within continents the heights are isolated; beneath oceans the deeps represent isolated bodies. The foliation in the oceanic underbody should, then, be flat under the deeps and should curve upward around them in the surrounding matrix.

Let us call any mass which is characterized by foliation that is flat or convex downward a *disk,* and let us designate any mass in which the foliation rises to a steeply dipping attitude by the term *interdisk*. We may then say: In both continents and oceanic underbodies disks represent the heavier, negative elements; interdisks represent the lighter, positive elements. Thus, within either a continental or a suboceanic mass as a whole the terms disk and interdisk have the same significance as to relative density of the parts. But if we contrast the continental with the oceanic the disks of the former are lighter even than the interdisks of the latter, and extremes of unlike densities exist between suboceanic disks and continental interdisks.

In the continents the disks in general are confluent and constitute the matrix in which the interdisks are isolated, whereas in the oceanic underbody the interdisks in general are continuous and the disks are separated.

There is obviously no sharp boundary between disk and interdisk, either in fact or by definition. There is in each of them a characteristic attitude of foliation, but between them the change in attitude is gradual.

Suboceanic Structure

PROBLEM AND PREMISES

In the preceding discussion the fact that igneous rocks are extruded along lines parallel to the outlines of oceanic basins is cited as evidence of their relation to oceanic underbodies and is a basis for the argument that those underbodies have a discoidal structure. It is desirable to approach this relation and conclusion independently from a different angle. If the eruptives do originate chiefly beneath the oceans, there is a reason for their predominance in the suboceanic masses, and that reason must be known, if the hypothesis is to advance beyond the purely speculative stage. We are thus confronted with the problem, What is the constitution of the oceanic underbodies? Do they consist in large part of those basic rocks which are the deep-seated equivalents of the basalts and andesites that are so copiously erupted about their margins?

HYPOTHETICAL ALTERNATIVES

Isostatic theory finds it convenient to assume the affirmative in response to the preceding question, because basic rocks are heavy and so also are, by hypothesis, the low-lying underbodies; but this does not explain why.

That subtheory of the planetesimal hypothesis which assumes that the infalling matter was dust, and that in its passage through the atmosphere it was so sorted as to establish the existing differences of density in oceanic and continental underbodies[48] assigns an original cause for the distribution and leaves only the subsequent processes of segregation to be explained. But that subtheory is a special postulate of a particular hypothesis and, brilliant and logical as it is, does not occupy the field to the exclusion of alternatives.

Among alternative hypotheses to account for the character of the oceanic underbodies is the postulate that the planetesimals gathered into knots and formed masses of notable size whose differences of density are perpetuated in the existing features of the globe; but the suggestion is undemonstrable, and there are dynamic consequences possible as a result of impact of bodies of suboceanic size which suggest prudence. That the heat of impact may have produced local melting is not, however, an extreme postulate. There is a tendency in certain current speculation to return to a hypothesis of a more or less molten globe, to appeal to gravitational sorting in the molten magma as a process which resulted in the

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[48] T. C. Chamberlin: The origin of the earth, 1916, pp. 193-200.

separation of basic and acid magmas, and to infer a coarse stratification of earth matter into a basaltic shell under and within a granite shell.[49] Bowen and Harker have recently discussed the settling down of heavier crystals in a molten magma, and the former has given us a clear analysis, which amounts almost to demonstration, of the process of gravitative separation; but many will agree with Harker that the requisite conditions of a prolonged state of fluidity in masses of sufficient size have not commonly existed in intrusive bodies such as are exposed to observation. The process, if it existed, was a deep-seated one in general, and the magnitude of the results which may be attributed to it is a subject of speculation.[50]

It is not necessary in the present discussion, and certainly it is not desirable in the absence of proof, to adopt any one of the major hypotheses in this field of speculation. Acid and basic magmas appear intruded separately at the surface. The separation may have been original or, by differentiation of a complex magma, subsequent to their inclusion in the earth body. When the earth attained its present size they may have been distributed in large or small bodies. They may have been intimately interrelated in layers, lenses, and dikes, or they may have been separated into basaltic and granitic shells. Any or all of these possibilities may have been realities. From the point of view of isostasy, the problem is: How could basic and acid rock-masses assume such relations in the isostatic shell as to give rise to the ocean basins and continents?

ORIGIN OF OCEAN BASINS

In presenting a possible solution of this problem of the origin of the ocean basins based on selective fusion, I rest on the following postulates regarding the conditions under which rocks melt in the depths of the lithosphere:

1. Dry melting, or melting, in the absence of water, water vapor, or other catalyzers, differs from melting in their presence, it being well established that the catalyzers promote solution.

2. Although the weight of evidence is in favor of the conclusion that volcanic vapors are original constituents of the magmas, it is not unreasonable to assume that there are large masses of rock in the lithosphere which are so nearly free from vapors that when heated they melt as dry

[49] R. A. Daly: Igneous rocks and their origin, 1914, pp. 159-164.
J. Barrell: The evolution of the earth and its inhabitants, 1919, pp. 33 et seq.

[50] N. L. Bowen: The later stages of evolution of the igneous rocks. Chicago Jour. of Geol., vol. xxiii, Supplement, 1915.
A. Harker: Differentiation in intercrustal magma basins. Chicago Jour. of Geol., vol. xxiv, 1916.

rocks melt in laboratory experiments. Such masses may never have occluded water vapor, or they may have lost their more volatile constituents by repeated heating and kneading.

3. The observed conductivities, specific heats, densities, and melting points of basic rocks, represented by basalt, and of acid rocks, represented by gneiss, may be taken as a basis of inference regarding the behavior of the two kinds of rock when subjected to rising temperature in the asthenosphere—that is, at depths of 50 to 800 miles below the surface.

To give form to the hypothesis, let it be assumed that there is a mass of basic rock in contact with acid rock, both in a solid state, in the asthenosphere. Let the temperature be rising by the conduction of heat from below and, to establish uniform conditions to begin with, suppose that an isogeotherm passes through both kinds of rocks.

Now the temperature to which each kind of rock will be raised by the same amount of heat depends on the conductivity, the specific heat, and the density of that rock. The following values may be taken as a basis of estimate:[51]

Characteristic	Basalt.	Gneiss.	Notes.
Conductivity (k)	0.00317	0.000578	k at 0° C.; temperature coefficient for 1° C. is, for gneiss, minus 0.002803; for basalt, plus 0.00001.
Specific heat (c)	0.199	0.214	At temperatures ranging from 20° to 200° C.
Density (d)	3.0	2.6	At the surface, under one atmosphere pressure.
Melting point	1,200	1,300 to 1,500	

The formula for the temperature to which a supply of heat will raise a unit of volume in a unit of time is

$$t = k/cd$$

Inserting the above values, we obtain: for basalt, $tb = 0.0052$; and for gneiss, $tg = 0.0014$. That is to say, the basalt will heat up nearly four times (3.7 times) as fast as the gneiss.

This being so and the melting temperature of basalt in a dry melt being lower than that of gneiss under atmospheric pressure, it follows that the times required to melt them with a given supply of heat are at least as 1 : 4.

Assuming special cases, we may conclude: (1) If a body of basalt

[51] Landholt, Börnstein, and Meyerhoffer: Tabellen, 1913.

underlies a mass of gneiss and is subject to heating from below, the heat will not be conducted away by the gneiss as fast as it is supplied through the basalt. It must therefore accumulate and raise the temperature of the basalt until the basalt melts. Since pressure tends to restrain the basalt from melting, the critical temperature will first be reached where the pressure is least—that is, on top.

(2) If a body of basalt lies above a mass of gneiss or is isolated in it, it can receive heat only as fast as heat is conducted through the gneiss, and it will melt sooner than the gneiss only because its melting point is lower.

(3) A dike or dikes of basalt extending from a basaltic mass upward through gneiss would serve as heat conduits and might conduct the heat away so rapidly that the gneiss would not reach the melting point.

These considerations bring us to the question of structure. It is postulated that holocrystalline rocks exist in the lithosphere only in so far as they have, because of youth or peculiar local conditions of stress and temperature, escaped the effects of metamorphism. The prevailing structure of deep-seated rocks is believed to be a foliated one; hence it is appropriate to use the term gneiss rather than granite to designate the acid rock of the lithosphere. The attitude of the foliation may be conceived to be vertical, horizontal, or inclined.

In addition to foliation, shearing planes or shearing zones demand recognition. They must necessarily develop in all parts of the lithosphere in which the ratio of strength of rock to superincumbent load is less than 1. They must develop in response to gravitative stress, and their general attitude should be approximately at 45 degrees to the vertical.

In case of intrusion of the gneiss by molten magma, the liquid rock should be guided in its course by the foliation and by the shearing planes. There would result a structure which I propose to call a *bonded* structure.

The term bonded is borrowed from architecture in the use which signifies tied together by interpenetration of integral parts. The intrusive and the intruded rocks are interbedded along the foliation, and the lenses of the intrusive are connected by cross-cutting bodies, dikes, along the shearing planes.

The origin of a bonded structure is obvious. Hypothesis presupposes a large mass of basic rock underlying a mass of gneiss. The basic rock, being subjected to a rising temperature, melts at and immediately below its contact with the gneiss and thus becomes lighter than the solid gneiss. When the molten mass attains sufficient extent and depth, the gneiss will tend to bow down into it, pressing the liquid magma out at the margins between the foliation planes. Escape of the magma must gradually occur and an actual bowing down of the roof must follow, resulting in tension in the lower layers of the gneiss. The development of shearing planes and the intrusion of dikes along them would ensue. Thus a bonded structure of gneiss intruded by basic magma would be built up.

Assuming that the intrusive cools, it is evident from the discussion of melting conditions that on reheating it must melt first. We may say, once an intrusive, always an intrusive. Thus the sheets and dikes of basic rock will extend themselves by repeated melting and intrusion. On reaching the surface the dike becomes the feeder of a flow, and in one or several epochs of eruption builds up a mass of heavy basic rock.

The essential point upon which emphasis should be laid is that the higher conductivity and lower melting point of basalt make inevitable the remelting of basaltic masses and the repeated extrusion of the magmas along the same conduits. Two conditions may arise to stop or divert the outflow. The first is the exhaustion of the magma basin. When all the basic magma had been pressed out the gneiss would close on itself and basic eruptions would cease. The second condition is horizontal, inclined, or vertical faulting, which may displace a dike and occasion the opening of new channels to possibly remote exits.

Now the extrusion of heavy masses to or near to the surface produces a disturbance of isostatic equilibrium. As Gilbert has shown, the surface gravity is thereby intensified. The underlying gneiss would be subjected to excessive vertical load, which would produce lateral, outward horizontal stress and readjustment by recrystallization, either during the epoch of intrusion or during a later heating. The readjustment requires a lowering of the surface and the development of a corresponding depression.

If now we review the postulates and steps of the above outlined process, it is clear that the magnitude of the depression depends upon the size of the original basalt mass and the repetition of epochs of melting. It is not unreasonable to assign to the original mass of basic magma diameters approaching those of the oceanic deeps, which are parts of the ocean basin, and thus to suggest the origin of the basins themselves.

The basic magma rises, according to hypothesis, from the asthenosphere, that is from depths of 60 to 800 miles below the surface. A direct path along vertical foliation planes might be established in the early stages of the rise of the magma, but it would be replaced by inclined shearing planes and horizontal foliation during subsequent stages. An important group of shearing surfaces would be peripheral and inclined outward from below upward. The magmas following up those surfaces would form the outer limits of the complex bonded structure, which would thus have the form of an inverted truncated cone, the apex being turned toward the center of the earth.

The development of horizontal foliation due to overloading at the surface and the formation of marginal shearing surfaces in process of subsidence would direct later extrusions toward the margins and would widen the cones, particularly in their upturned bases. They would therefore depart widely from a linear, conical shape. The elements would be in general irregular curves, formed of zigzag details, as the magma passed from dike to sheet and from sheet to dike, spreading outward and upward. It is evident that the process would tend to broaden an ocean basin at the expense of the continental platform, by invading the margins of the latter with heavy rocks. I regard the Columbia lava flows and others of a similar extensive character as invasions of this nature.

There are probably limitations of oceanic spreading. There are certainly offsetting processes due to compression; but it is not proposed to treat them here.

The object of this immediate section is to show reason why the deeper oceanic underbodies should be regarded as the sources of those eruptives which appear at the surface so commonly around their margins. The reason is found, according to the argument, in the origin of the basin, which is regarded as having developed in consequence of the rise of basic magmas and the subsidence of their cooled masses as the overloaded gneissic shell flowed out from under them, chiefly by recrystallization.[52]

In conclusion, the reader is invited to read over the advanced summary in which the several concepts presented in the article are stated without argument and stripped of details.

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[52] NOTE.—It is but just to my friend, Joseph Barrell, and to myself to call attention to the parallelism which exists between the hypothesis of the origin of ocean basins here set forth with that which is stated in his posthumous paper, "Evolution of the earth" (November, 1916, page 42). We agree as to the rise of basic magmas and the resulting conditions of isostatic adjustment. We differ as to the cause of the melting of the magma, which he ascribes to the heat generated by radioactivity. Our paths of thought converge to a common point, yet our thinking had been independent. We had not conferred on the subject and my manuscript was in essentially its present form when I read his printed page, in September, 1919.

Explanation of Plates

Plate 8.—*Curves of absolute Strength and relative Strength of Rock from the Surface to 100 Miles below it, at surface Temperature*

The curves in plate 8 represent hypothetically the absolute and the relative strength of granite at surface temperature, but at depths ranging from the surface down to 100 miles below it.

The *absolute strength* is deduced from Adams' experiments on the increase of internal friction or crushing load under high pressures. The result is represented by the abscissas of the curve LMNPQR.

L is a point on the surface. The strength is equivalent to a crushing load of 5 miles of granite. M and N are points determined by Adams' experiments on cylinders of granite inclosed in yielding steel jackets, which gave for a confining pressure equivalent to burial at 4.2 miles below the surface a strength equivalent to a load of 21.6 miles, and similarly for 5.8 miles burial a crushing load of 23.3 miles. P is a point deduced from Adams' experiment on a perforated granite cylinder inclosed in an unyielding steel jacket. From P to R the curve is hypothetically extended, Q being adopted from a study of Adams' experiments as the probable depth at which the strength would equal the load, 40 miles.

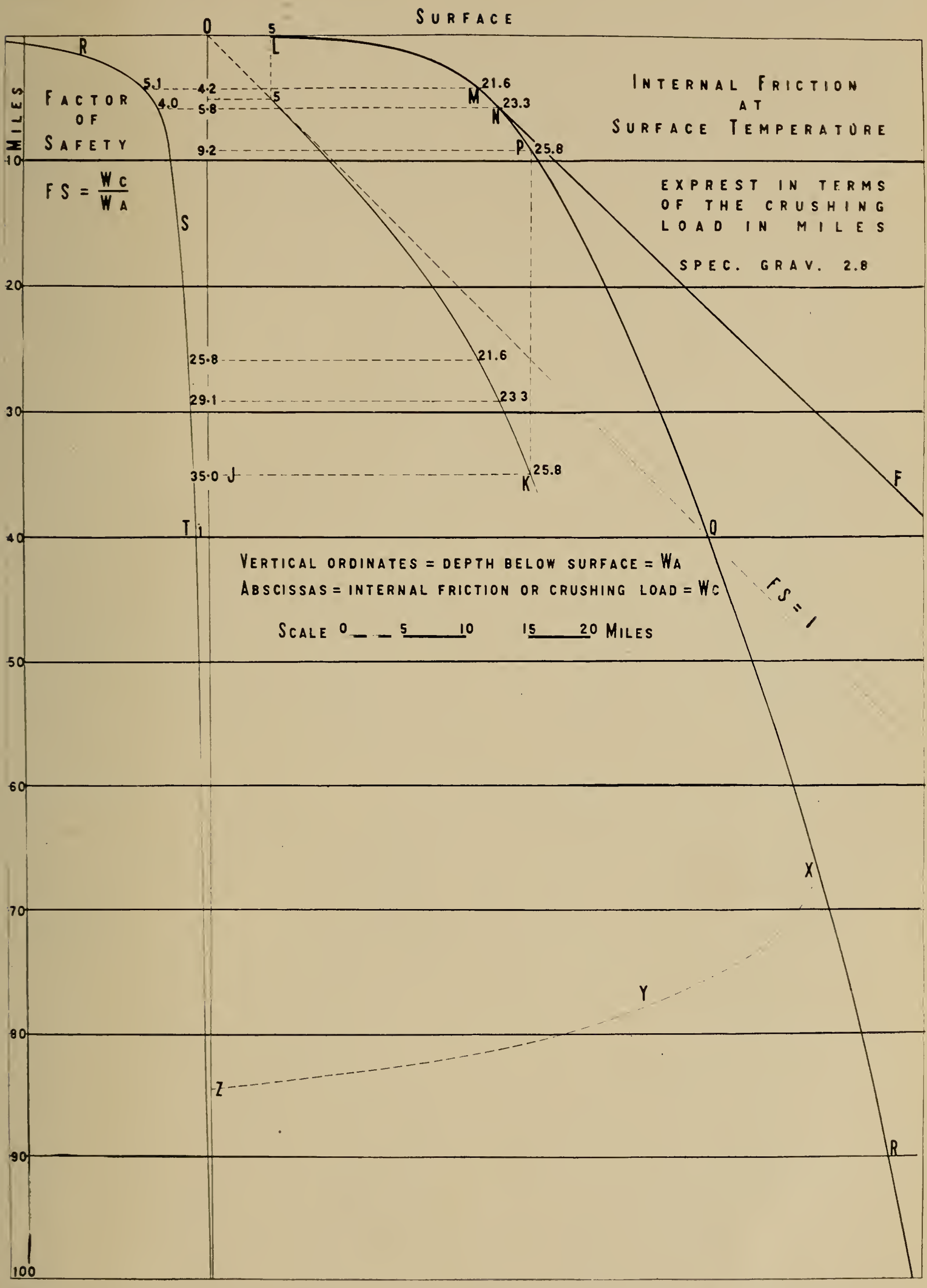
The curve XYZ expresses the loss of strength due to rise of temperature, on the assumption that melting reduces the internal friction of the rock to zero, which is only approximately true. The curve has no quantitative value.

The relative strength of granite, as compared to the load, is shown by the curve RST and its prolongation. The abscissas of the curve, plotted to the left of the axis, represent the ratio of the crushing strength to actual load, Wc/Wa. This may be called the factor of safety. At the surface it is infinite. At the depth of 4.2 miles it is equal to 21.6/4.2 or 5.1. Similarly, at 5.8 miles it is 4. At 40 miles it is by deduction equal to 1. Below that depth it is a fraction, but it can not reduce to zero unless the temperature be raised to the melting point. Pressure alone can only increase the absolute strength, even though the relative strength becomes less than 1—that is, at uniform temperature the absolute strength increases with increasing pressure, even though the rock is potentially crushed.

Plate 9.—*Diagram of Compression and thermal Expansion of Rock between the Surface and a Depth of 40 Miles*

On the right and left of plate 9 are shown depths and corresponding pressures and temperatures in the lithosphere down to 40 miles. Pressure and temperature also are expressed by the straight lines so marked, according to the scale at the bottom of the diagram. The rates of increase accepted are those adopted by Adams (specific gravity, 2.8; temperature increase, 1° C. for every 32.9 meters).

The diagram is designed to express the antagonistic effects of pressure and heat on the volume of rocks. Plate-glass is taken as a representative of rocks in its behavior toward compression and expansion. The curve marked "Hypothetical compression" represents the supposed effect of pressure in reducing the volume of a rock at uniform temperature. The curve marked "Hypo-



CURVES OF ABSOLUTE STRENGTH OF ROCK FROM THE SURFACE TO 100 MILES BELOW IT, AT SURFACE TEMPERATURE

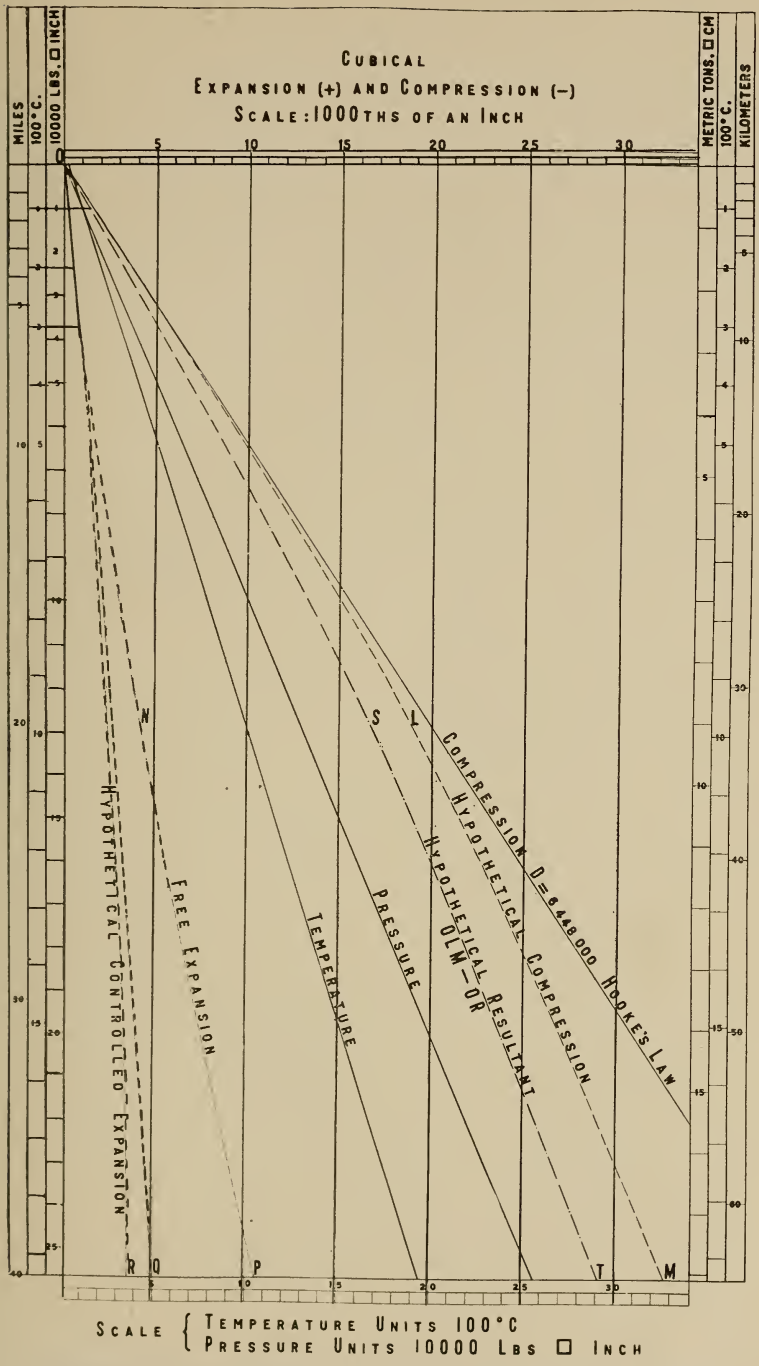


DIAGRAM OF COMPRESSION AND THERMAL EXPANSION OF ROCK BETWEEN THE SURFACE AND A DEPTH OF 40 MILES

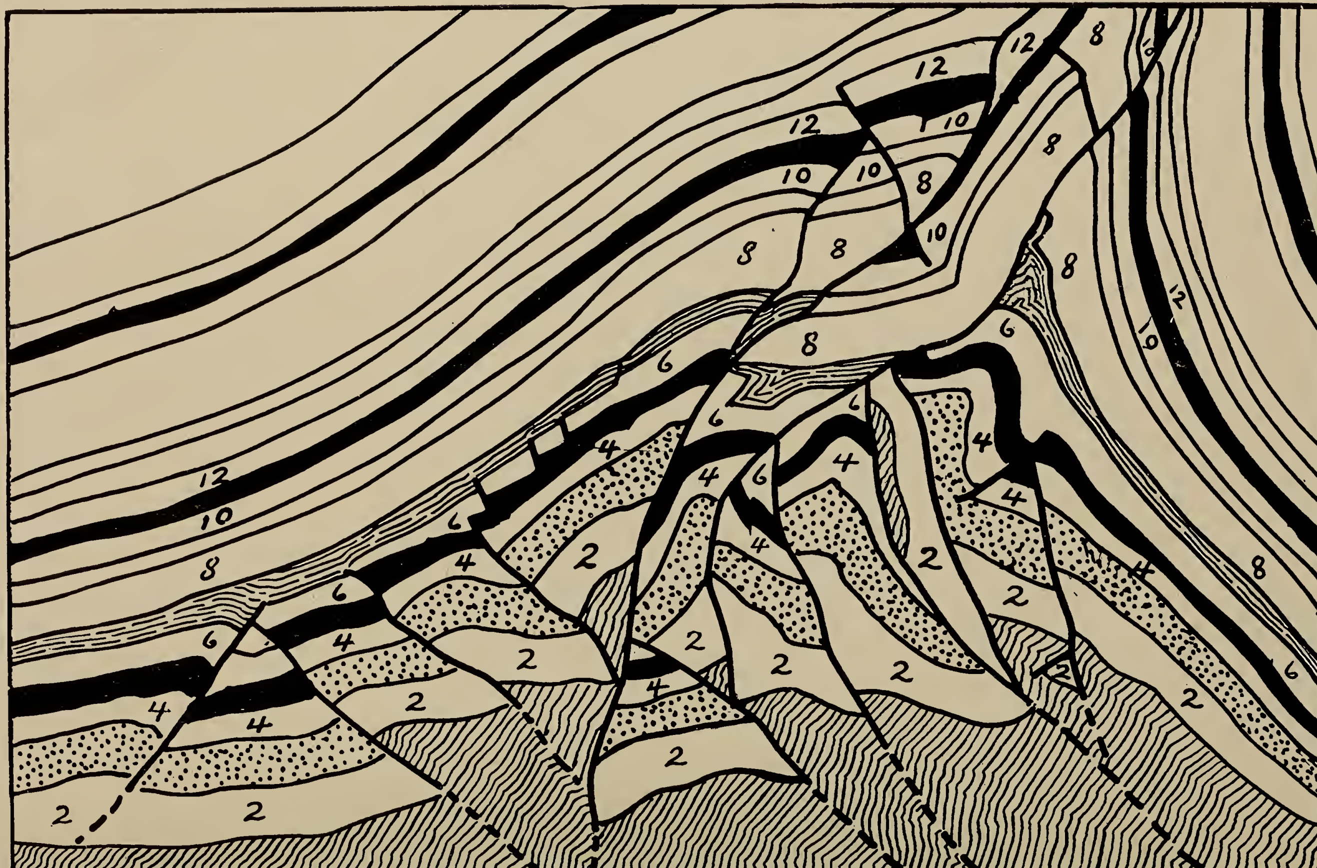
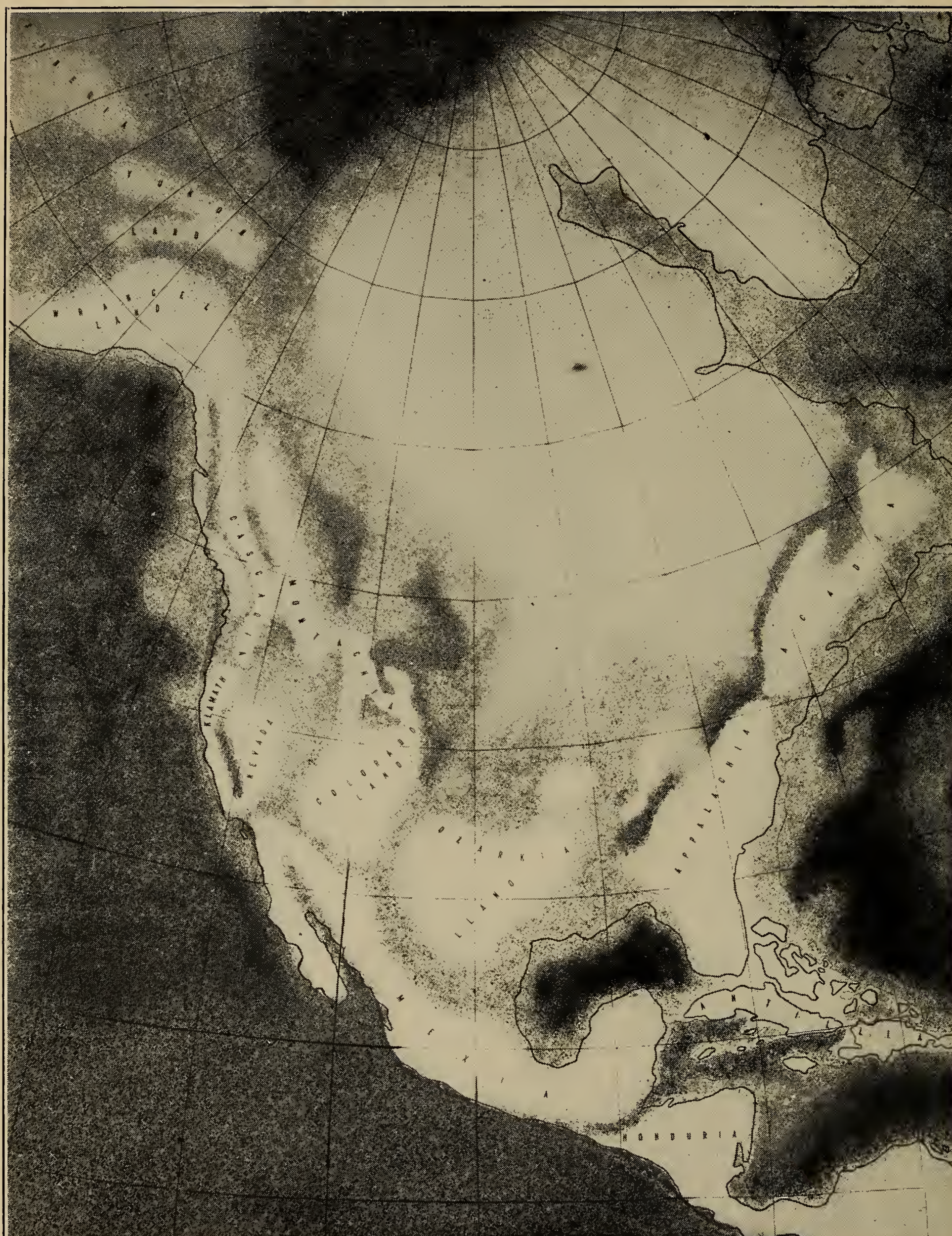


ILLUSTRATION OF DEFORMATION OF A SOLID IN THE ZONE OF FLOWAGE, SOLID FLOW BEING ACCOMPANIED BY SHEARING



POSITIVE AND NEGATIVE ELEMENTS OF NORTH AMERICA thetical controlled expansion" represents the effect of heat in terms of cubical expansion under increasing pressure. The "Hypothetical resultant" is the difference between the two, and is subject to instantaneous change if either pressure or temperature vary.

Plate 10.—*Illustration of Deformation of a Solid in the Zone of Flowage, solid Flow being accompanied by Shearing*

Plate 10 represents shearing in substances of the consistency of butter at 20° C. The effect was produced by compressing a model composed of layers of wax. mixed with thick Venice turpentine to soften it or with plaster of Paris to harden it.

The lower layers, in which shearing is conspicuous, were soft and flowed under the load of shot which rested on them and under the pressure which was applied at the right. When a plate-glass covering the face of the model cracked this soft material flowed out through the cracks to a height of half an inch. It nevertheless sheared on planes at 45° to the stress. During the movement the earlier shear planes were revolved. The later ones at the left retain their initial orientation. The harder upper layers, above number 8, yielded by flexure rather than by shear. (See Experiment J, Plates XCIII and XCIV, Thirteenth Annual Report, U. S. Geological Survey, 1889.)

Plate 11.—*Positive and negative Elements of North America*

Plate 11 represents the continental platform of North America and the oceanic deeps adjoining it, the whole being shaded so that the darker areas correspond with depressions and the lighter ones with elevated tracts. Beneath the oceans the shading represents the actual relief. On the continental platforms the shading represents a hypothetical relief which would exist if there had been no erosion, neither degradation nor aggradation, since the beginning of the Paleozoic. The relief which would exist if that assumption were true is deduced from the sedimentary record, according to the algebraic sum of sediments and unconformities, expressed in epochs, for each separate area. Where erosion has been constantly or commonly active the corresponding area would now stand high. Where sediments have commonly been deposited the surface under them had sunk low. The continent is thus divided into masses that have tended to stand high or to sink relatively low.

The tendency which the several areas have exhibited throughout known geologic history is, under the hypothesis of isostasy, attributed to their relative lightness or to their greater density. The lighter masses are shown as white areas. They are the positive elements of the continent. The heavier masses are shown in medium and dark shades. They are the negative elements of the continent. The former are the interdisks, the latter the disks, according to the definitions of those terms under the discoidal hypothesis.

The names given to the positive elements are those employed by Willis (1907) or by Schuchert (1908) to designate masses in a similar locality. Likeness of outline is not to be expected or striven for in the present state of paleogeography. Mexia, Klamath Land, Wrangell Land, and some others are new names.

PLATE 12.—*Bathymetric Map of the Atlantic Ocean*

Plate 12 is a copy of the bathymetric map of the Atlantic Ocean, by Dr. M. Groll, 1912. The shading represents different depths, and emphasis is placed on depths greater than 5,000 meters, in order to bring out the major deeps.

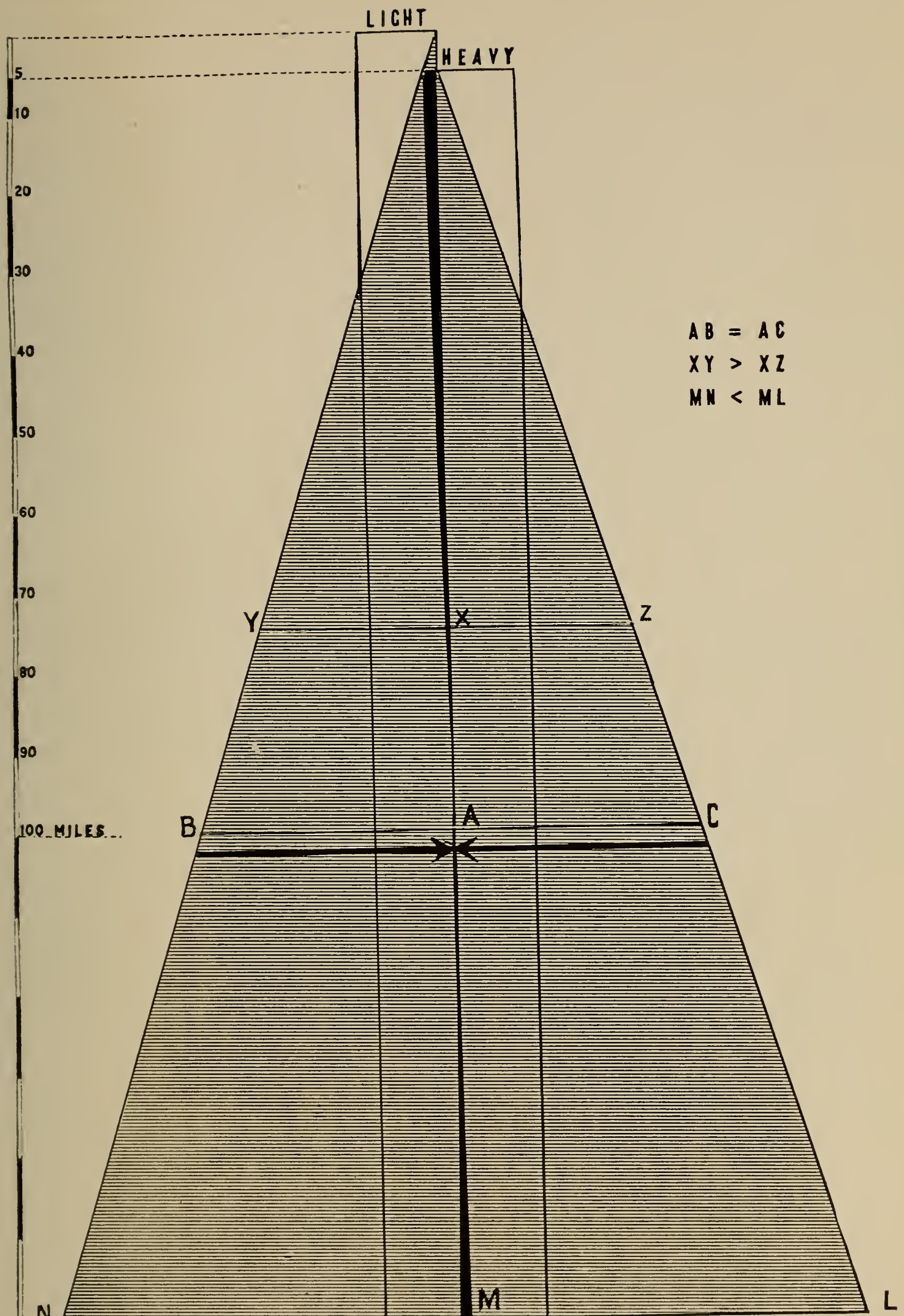
The darker areas represent disks, the lighter areas interdisks, under the discoidal hypothesis.

PLATE 13.—*Horizontal Stresses and stress Differences between lighter and heavier Columns*

The diagram in plate 13 represents two columns (outlined under the words "Light" and "Heavy") which are supposed to be in isostatic balance with each other at the depth of 100 miles and to differ 5 per cent in density. The light column is 100 miles high and the heavy column 95 miles. Each column presses against the other with a stress which is arbitrarily represented by the horizontal shading. By hypothesis the lateral stresses are equal at the depth of 100 miles, where the two columns are in equilibrium. AB is therefore equal to AC. At any level above the level BC, such as YZ, the stress from the lighter column toward the heavier is the greater; thus XY is greater than XZ. At any level below BC the reverse is true—that is, MN is less than ML.

Whatever the difference in surface level of the two columns, that we assume, there will always be a level corresponding to BC, at which the weights of the columns will be equal; and the relations of lateral stress above and below any such level of equilibrium will be those shown in the diagram.

BATHYMETRIC MAP OF THE ATLANTIC OCEAN



HORIZONTAL STRESSES AND STRESS DIFFERENCES BETWEEN LIGHTER AND HEAVIER COLUMNS

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
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OSCILLATIONS OF LEVEL IN THE BELTS PERIPHERAL TO THE PLEISTOCENE ICE-CAPS[1]

BY REGINALD A. DALY

(Presented in abstract before the Society December 29, 1919)

CONTENTS

A Test of Isostasy

The theory of isostasy is widely favored as an explanation of recent uplifts in the glaciated tracts of North America and Europe. If the earth's crust sinks after a heavy, extensive load of ice is put on it, and rises after that ice melts; if the crust so behaves each time that one and the same region is glaciated and deglaciated; if similar behavior were proved in separate areas—eastern Canada, the British Isles, Scandinavia, British Columbia, Patagonia, and Antarctica—the theory would almost become certainty. As a matter of fact, field studies have been largely confined to but three of the greater ice-cap areas and in each case to uplift following the latest deglaciation. Complete testing of the isostatic theory after the manner described is evidently much more difficult than the partial test so far applied.

[1] Manuscript received by the Secretary of the Society December 29, 1919.

The sinking under load is in small part due to purely elastic compression, a shortening of the earth's radii below the ice-cap, effected without lateral flow of material. In part the sinking is referable to subsurface lateral flow, to what may be conveniently called plastic deformation of the globe. Since the viscosity of the subcrustal material is everywhere high in absolute measure, deformation of the loaded surface by subsurface flow entails an upward bulge of the country just outside the sinking area. The evaluation of such a marginal bulge and of the corresponding marginal sinking after deglaciation would help to show the relative importance of the two modes of crustal yielding. As Barrell has noted, the relative size of each marginal bulge would also give some idea concerning the degree of resistance to subsurface flow. For brevity these marginal movements will be referred to as isostatic, although of course the upwarped areas are not isostatically adjusted.

The full, quantitative solution of the problem must be long postponed. Curiously enough, its attack on the qualitative side has been seldom made or even mentioned in the copious literature dealing with Recent uplifts in deglaciated regions.

The delay is the more remarkable in view of Jamieson's clear, though brief, statement of the principle. He seems to have been the first to announce the hypothesis of depression under ice-load and elevation consequent on ice-melting. Seventeen years later he wrote:

> "It seems likely that there might be not only a slight sinking of the ice-covered tract, but likewise a tendency to bulge up in the region which lay immediately beyond this area of depression, just as we sometimes see in the advance of a railway embankment, which not only depresses the soil beneath it, but also causes the ground to swell up farther off." [2]

Glacial loading and unloading represent Nature's gigantic experiments, perhaps the most useful ever made, to test the isostatic theory quantitatively. The present paper is offered as a contribution to those queries and records which are preliminary to the solution of the quantitative problem. Significant as the problem is in itself, its solution may turn out to be of principal aid in working out the paleogeography and recent history of wide belts of the earth's surface.

Field-work on the New England coast, following the season of the year 1900, spent on the shores of Labrador and Newfoundland, led the

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[2] T. F. Jamieson: Geol. Mag., vol. 21, 1865, p. 178; vol. 9, 1882, p. 461.

Cf. J. Barrell: Am. Jour. Sci., vol. 40, 1915, p. 13.

W. B. Wright, in The Quaternary Ice Age, London, 1914, p. 406, has given an excellent survey of the subject so far as it concerns the areas uplifted after ice-melting, and puts welcome stress on shifts of ocean-level because of glaciation and deglaciation; but, like most other writers, does not discuss crustal movements outside the isobases for zero.

SKETCH MAP PRESENTING DATA BEARING ON OSCILLATIONS OF LEVELS

Relative positions of the limit of glaciation (*toothed line*); isobases off New England (zero, 100-foot, 200-foot); submarine channel of the Hudson River (HC); Whittlesey hinge-line (WHL); Ohio River (1 and 5) and its branches, Kentucky (2), Big Sandy (3), Kanawha (4), Monongahela (6), Youghiogheny (7), and Conemaugh (8); Susquehanna River (9). Scale: 1:11,100,000, or 1 inch to 175 miles.

writer to the hypothesis of plastic deformation which he has lately found to have been already formulated by Jamieson. Certain facts observed in New England seem to be explicable only on the assumption that there has been offshore subsidence in Recent time. If that subsidence was correlative with the post-Glacial uplift of northern New England, the assumption enters the domain of Jamieson's hypothesis. If his speculation is well founded, similar subsidence in the belt surrounding the former ice-cap west of New England should be expected. Can the value of his speculation be tested by those expert in the geology of the long belt involved? The question can be satisfactorily answered only by the pooling of conclusions reached by many specialists. The present writer must be content with listing examples of specific or local tests, which, however, must be applied by those more competent.

Evidence of isostatic Movements off the New England Coast

ISOBASES

During recent years the writer has studied, at intervals, the emerged coastal strip from Boston to Boothbay Harbor, Maine. In agreement with Tarr, Woodworth, and Laforge, he believes that the highest post-Glacial shoreline is located on Cape Ann at about 80 feet above the present sealevel. It is a few feet lower at Magnolia, Massachusetts, but rises rapidly to the northward, reaching a height of nearly 300 feet near Boothbay Harbor, 115 statute miles from Magnolia. Katz has determined its height as 155 feet at Stratham, New Hampshire, and as 300 feet at Pownal, Maine, 26 miles west of Boothbay.[3] According to Katz, the shore structures "indicate that the postglacially uplifted surface has been tilted 5 to 6 feet per mile in a direction north 40° east of south, and that the lines of equal elevation approximately parallel the shore of the Gulf of Maine." These results accord with De Geer's isobases for the region, but are contrasted with the recent conclusions of Fairchild, whose isobases portray a post-Glacial uplift of this coastal belt which is 200 to 300 feet too great.[4]

WEAK WAVE EROSION IN THE EMERGED ZONE

Though the emerged belt of New England shows the well known features—wave-washed rock slopes, raised beaches, sand and clay plains—the

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[3] R. S. Tarr and J. B. Woodworth: Bull. Museum Comp. Zoology. Cambridge, Mass., vol. 42, 1903, p. 181.

F. J. Katz: Proc. Washington Acad. Sci., vol. 8, 1918, p. 410.

[4] G. De Geer: Proc. Boston Soc. Nat. Hist., vol. 25, 1892, p. 454.

H. L. Fairchild: Bull. Geol. Soc. Am., vol. 29, 1918, p. 202; vol. 30, 1919, p. 614.

writer has been specially impressed by the comparative feebleness of the waves which worked on this coast before and during the recent uplift. At the higher levels the bedrock was locally fretted and roughened, largely by frozen spray, but notable cliffing is exceedingly rare. Even the till of drumlins, which when submerged directly faced the sea on the east and south, has been little affected by erosion. An example is seen in Pigeon Hill, Cape Ann. At the existing sealevel many cliffs cut in granite and other hard rocks, to say nothing of the visible destruction of drumlins, show the power of the open Atlantic. Sea-chasms eroded along trap-dikes are well widened out at present sealevel, but rapidly narrow upward. Good illustrations are seen at Cape Neddick, Maine, and at Magnolia, about 500 yards southwest of Normans Woe Rock. At the latter locality the glaciated surfaces of the dikes are almost intact at and near the highest strand-level; the same dikes are the loci of notable sea-chasms at present sealevel.

Constructional forms in the emerged belt give similar testimony. The raised beaches, bars, and spits are small, poorly developed in many instances, and usually not composed of well assorted materials. In all these respects the contrast with corresponding embankments at present sealevel is striking. Moreover, the pebbles of the higher embankments are usually not well rounded, having preserved much of their original angularity.

A detailed account of the foregoing observations is hardly necessary, since several authors have already stressed the same facts regarding the emerged belt, from Boston to Eastport, Maine.[5] On the other hand, the facts are subject to different interpretations.

The prevailing explanation has been well expressed by Stone:

> "When we compare the ragged and uneven cliff of erosion at the present beach with the still moutonnéed ledges at higher levels, it becomes evident that the sea has stood at or near its present position many times as long as at any higher level. At the higher elevations the surf had time to erode the till from the more exposed shores, but it had not time to form a cliff of erosion in the solid rock before a change of level transferred the wave-action to higher or lower rock. In other words, the changes of level of the sea were relatively rapid." [6]

A similar view is taken by Bastin for the areas covered by the Rockland, Penobscot Bay, and Eastport folios of the United States Geological Survey.

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[5] G. H. Stone: Monograph 34, U. S. Geol. Survey, 1889, pp. 44-52; Penobscot Bay Folio, U. S. Geol. Survey, 1907, p. 12; Rockland Folio, 1908, p. 10; Eastport Folio, 1914, p. 11.

[6] G. H. Stone: Monograph 34, U. S. Geol. Survey, 1899, p. 44.

Students of the glaciated area are also in fair agreement as to a prolonged lag in the uplift of the continental part of it, though there may have been some uplift before the Labrador ice-cap disappeared. A similar lag is evident in the uplift of Scandinavia after its deglaciation. De Geer holds that the crustal sinking in Scandinavia continued even after the ice had melted off from southern Sweden.[7]

Probably New England had lost its general ice-covering by the time Atlantic water commenced to wash the hills around Ottawa, Canada, at elevations now 600 or more feet above sea. Incidentally, it may be observed that this lag in uplift is not consonant with the explanation by purely elastic resilience, for uplift so caused should have begun immediately after unloading.

Meanwhile, however, the ocean-level was doubtless rising because of the return of water to the ocean through melting—a rise which more and more masked the local sinking of sealevel because of the weakening of the gravitative pull exerted by the diminishing glacier. The coast region may have been thus drowned to the extent of 150 to 200 feet.[8]

Another considerable part of post-Wisconsin time was occupied by the gradual uplift.

Clearly, therefore, the highest strand in Maine could have been beaten by waves during but a small portion of the period elapsing since the Wisconsin ice began to disappear. The upper part of the emerged belt was washed first by an advancing sea and then by a retreating sea. The total time involved was not a great many thousands of years. Under the circumstances, strong and continuous terraces would not be expected. Supplemented by the postulate of eustatic shifting of sealevel, the prevailing explanation of the weakness of the strand-marks is unquestionably correct in principle. Yet it is not the full explanation.

From Belle Isle Strait to Nachvak Bay, on the northeast coast of Labrador, a distance of 600 miles, the evidences of energetic wave-action in the emerged belt are much stronger than in the emerged belt of New

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[7] W. A. Johnston: Memoir 101, Geol. Survey of Canada, 1917, p. 30.

W. Upham: Monograph 25, U. S. Geol. Survey, 1895, pp. 235 and 499.

F. B. Taylor: Monograph 53, U. S. Geol. Survey, 1915, pp. 330, 506-7.

G. De Geer: Bull. Geol. Soc. Am., vol. 3, 1892, p. 65, and Compte Rendu, Cong. géol. internat., Stockholm, vol. 2, 1910, p. 849.

Brögger believes that the Christiania region began to sink to an ultimate amount of 240 meters when the Scandinavian ice-cap began to melt. His evidence is by no means convincing. Moreover, he did not consider the effect of ice-melting in raising general sealevel. Norges geol. Unders., no. 31, 1901, pp. 690-1.

[8] Johnston is inclined to make the eustatic rise yet greater at Ottawa, Canada. With W. B. Wright, he ascribes to this movement the late Glacial submergence of Norway, which Brögger had attributed to crustal sinking. The Quaternary Ice Age, London, 1914, p. 414.

England. The contrast appears in the degree of cliffing and fretting of hard rocks, in the number and development of beaches, and in the size, assortment, and rounding of beach cobbles. The uplift was doubtless synchronous, or nearly so, with the uplift of Maine, though somewhat greater in average amount. Hence the greater intensity of wave-action in Labrador is hardly ascribable to longer exposure of its emerged belt to the waves. Any difference in the terranes of the two regions makes the contrast all the more noteworthy, for the Labrador coast is much poorer in glacial drift and other weak material than the Maine coast. Nor can appeal be made to different degrees of storminess of the adjacent seas. The only remaining variable in the problem is the strength of the waves, as this is controlled by the wave fetch. The Labrador coast during submergence seems to have been exposed to the full fury of the Atlantic. If the Gulf of Maine were largely landlocked at the same time, the waves beating on its shores must have been much weaker than the waves of the Labrador coast. As a matter of fact, they seem to have been decidedly weaker than the waves which constructed the superb boulder beaches on Hogland Island, in the Gulf of Finland, and at other places in the nearly landlocked Yoldia Sea of Europe. One may well doubt that the elevated strands of Maine betoken wave power as great as that characterizing the waves of Lake Superior at its higher late Glacial levels.

All observers have found the highest strand-level of Maine to be poorly marked in the usual ways. Nevertheless, evidence is not wanting that the sea stood near the highest level for a comparatively long time. The well known clay plains of the emerged tract are chiefly developed in the long bays that existed during the submergence. The larger bays, inclosed by shores of till or bedrock, were filled with the clay and sand to heights of 100 to 200 feet or more above present sealevel. These outwash deposits have maximum widths of several miles and maximum depths approaching 200 feet. The sediments were laid down when the sea was at or not far below its highest level. There seems, indeed, to be no evidence that uplift began until the clays and sands of the old bays had been largely deposited.

In this connection a recent memoir on the Newington moraine, stretching from Newburyport, Massachusetts, to Portland, Maine, bears significant conclusions. Its authors state that all the clays and sands of the coastal plains and bay fillings must be "regarded as a stratigraphic unit that was deposited uninterruptedly during a period perhaps beginning before the Newington substage, but certainly continuing through and after it." They note that the ice-front stood in the sea during the Newington substage and continue:

"Although the wide, level plains of marine clay were deposited in situations open to the disturbing influence of river, tidal, and storm currents, the clay is characteristically uniform in composition and texture and in the attitude of its bedding. Therefore the clay must have been deposited in quiet water, namely, in this instance, water whose surface was considerably above the levels up to which the clay plains were built and in consequence probably deep enough to submerge the moraine." [9]

During so long a time the waves of the open Atlantic should have made an easily read record at or near the highest strand-level, instead of the obscure markings actually found.

CONCLUSION

Thus the details of the highest strands, as well as the general character of the emerged belt in New England, seem to indicate that the rollers of the open Atlantic did not pound the coast during most of the period of submergence, although they may have done so during the last quarter of the uplift. In the field several hypotheses were entertained in explanation. It is conceivable that, if the ocean were locally frozen during most of the period of uplift, strong wave-action would be prevented. That this was not the dominant condition is suggested by the phenomena of the Labrador coast, where annual freezing is likely to have been more prolonged than along the New England coast. Similarly, the possibility of protection of the land by an offshore tongue of the great ice-cap was considered, but dismissed for good reasons, which need no recounting on the present occasion. Jamieson's idea seemed much more promising than any other, and later study has confirmed the impression gained in the field. If his hypothesis is in line with the truth, the outer part of the continental shelf should have been forced up by the weight of the Labrador ice-cap. The Gulf of Maine would then have become a nearly landlocked arm of the ocean, elongated parallel to the existing coastline and so narrow that its waves could attain the energy of a lake but not that of the open ocean. Extending, theoretically, from Cabot Strait or farther northeast to New Jersey, this bulge of new land would include Georges Bank, and it may for convenience be called Georges Bank Land.

Because of the narrowness of the shelf off northeastern Labrador, similar moderate bulging would not there form dry land; hence the emerged belt of Labrador shows the effects of wave-action stronger than those in the New England belt.

This speculation was seen at once to harmonize with Fernald's view that the flora of Newfoundland is best explained by assuming a former more or less continuous land-mass of the coastal plain type between New

[9] F. J. Katz and A. Keith: Professional Paper No. 108 B, U. S. Geol. Survey, 1917, p. 27.

Jersey and Newfoundland. It was then found that Barrell had adopted the same hypothesis as a result of his deep study of the strength of the earth's crust, founding his case on general principles of geology and on the field-work of Fernald and Woodworth.[10] Where so many lines of evidence converge, initial speculation should rise to the status of a working hypothesis. If it be true, the extraglacial belt west of Massachusetts might be expected also to have undergone recent oscillations of level. Among the consequences of peripheral upwarping might be: special trenching by rivers in the marginal bulge; special alluviation in the strip of relatively low land adjacent to the zero isobase; diversion of rivers from valleys which had been too heavily alluviated because of the upwarping; and temporary reversal of some streams. Among the consequences of subsidence following the peripheral upwarping might be: new local reversal of drainage, and appropriate tilting of lake strands which were established before the subsidence was completed.

Evidence from the marginal Belt west of New England

SCOPE OF THE DISCUSSION

The application of these criteria is not a simple matter. According to the hypothesis, each glacial stage—Kansan, Illinoisan, and Wisconsin—was accompanied by a double oscillation of levels. The effects must be associated in the field more or less complexly. One of the first steps toward understanding the hypsometric changes involved is the mapping of the zero isobases for each glacial stage. Practically nothing has been done in locating the pre-Wisconsin isobases. Existing maps of the Wisconsin isobase for zero do not agree. Taylor suggests that in the Great Lakes region this line may coincide with the Whittlesey hinge-line, while De Geer and others have located it considerably farther south.

Obviously the further testing of Jamieson's hypothesis, as applied to the long stretch from New England to Montana, must await the deliberate conclusions of many specialists who in the future make observations with his idea in mind. Perhaps some geologists working in this part of the marginal belt have considered it, but there seems to be no published record of their findings. Rather to illustrate the kind of questions involved than to appear to answer them, the writer offers the following notes, which refer to: the testimony of lake strands; the submarine channel of the Hudson River; the "deeps" of the Susquehanna River; the abandoned channels of Ohio Valley and farther west; the testimony of Europe (see plate 14).

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[10] M. L. Fernald: Rhodora, vol. 13, 1911, p. 109, and Am. Jour. Sci., vol. 40, 1915, p. 17.

J. Barrell: Am. Jour. Sci., vol. 40, 1915, p. 13.

ABANDONED LAKE STRANDS

At first sight the old shorelines of the Great Lakes appear to negative the hypothesis of marginal downwarping since the Wisconsin stage of glaciation. Southwest of the Whittlesey hinge-line, Leverett and Taylor find the abandoned strands to be almost perfectly horizontal. For example, in Michigan alone their sections show horizontality for the Whittlesey shore through a distance of 65 miles. Assuming their zero isobase and assuming also that they have correctly identified the beaches, the Jamieson hypothesis is decidedly not favored by their field observations. These would point rather to the hypothesis of purely elastic deformation if the rise to the northward is isostatic; or to the assumption of practically perfect fluidity for the subcrustal material, if the isostatic adjustment took place through "plastic" deformation.

Since both deductions are antecedently improbable, the search for evidence of a marginal bulge south of the Great Lakes should not be given up. According to Leverett and Taylor, the Whittlesey hinge-line is only approximately placed on the map, and they note the uncertainty that it does really represent the zero isobase. The older Maumee beaches in the "area of horizontality" show some slope to the southwest, and all of that slope may not be due to ice attraction, as held by the authors quoted.[11] If so, the zero isobase is somewhat south of the mapped Whittlesey hinge line. The lake strands may thus lie outside the critical area where such levels might give evidence of crustal downwarping. The delicacy of the hypsometric problem is illustrated in Brögger's map of the analogous isobases crossing the west coast of Denmark. Twenty-two kilometers northeast of the zero isobase the uplift was only 2.5 meters and a subsidence of equal amount is indicated at about the same distance southwest of the zero isobase. If correct in principle, Brögger's map suggests that the hinge area in America may be broad and characterized by but very slight departures from true horizontality. Moreover, the strand-lines in uplifted Ontario now show broad, almost perfectly level "treads," separated by areas of tilted beaches. Is the "area of horizontality" adjoining the Whittlesey hinge-line in a similar relation, the true isobase for zero being southwest of this region of old strands?

On the other hand, the strands of the late Wisconsin Lake Passaic in New Jersey are rather strongly tilted in the direction demanded by Jamieson's hypothesis. In a distance of not more than 25 miles the tilt measures 67 feet and it is in a southerly direction.[12] According to Wood-

[11] F. Leverett and F. B. Taylor: Monograph 53, U. S. Geol. Survey, 1915, pp. 348, 377, plate xx.

[12] R. D. Salisbury: Final Rept. State Geologist of New Jersey, vol. 5, 1902, p. 224.

worth, the zero isobase is north of New York City; Leverett and Taylor are inclined to the same view. If it be correct, the deformation of the Passaic basin would be a striking proof of downwarping in a belt outside the area of Recent uplift.

A parallel case is found in former Lake Missoula of northwestern Montana. This late Glacial lake lay in the belt peripheral to the Cordilleran ice-cap and owed its origin to damming by powerful tongues of ice flowing southward from British Columbia. According to Campbell, the highest strand of Lake Missoula has been tilted strongly to the northwestward, showing "a depression of the earth's crust in that direction since the breaches were formed, or a rise in the surface to the southeast. Such a movement is also indicated by the recent canyon cut by Clark Fork between Missoula and the mouth of Saint Regis River."[13] The highest lake strand south of Missoula is now at the 4,200-foot contour, and not far upstream from Pend d'Oreille Lake is at or near the 3,500-foot contour. The average tilting is thus nearly 6 feet to the mile for an air-line distance of 120 miles. Is this Recent crustal warping due to isostatic collapse of the temporary marginal bulge, which had been forced up by the weight of the Cordilleran ice-cap?

SUBMARINE HUDSON CHANNEL

Merrill, Woodworth, and others have shown that the land near the mouth of the Hudson River was higher at some post-Glacial stage than it is now.[14] Long ago, Dana, Upham, and others explained the submarine channel of the Hudson across the continental shelf by the erosion of this river when it had a lower baselevel. The depth of the channel is probably too great to be attributed to corrasion when the ocean-level fell because of abstraction of water to form the Pleistocene ice-caps. On the other hand, the channel may have been cut as the continental shelf rose, to form Georges Bank Land. In that case the cutting could have been begun by an extended Hudson River (seeking the new Glacial baselevel of a sinking ocean surface), to be continued in the soft material of the shelf after the upwarping began. For a time, then, the lower Hudson would have been in the class of antecedent rivers. Subsequent rise of general ocean-level, followed by downwarping, would account for the final drowning of the valley (see plate 14).

In common with other hypotheses, this one encounters the difficulty of

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[13] M. R. Campbell and others: U. S. Geol. Survey Bull. 611, 1915, footnote on pp. 134-136. See map opposite p. 144 and also J. T. Pardee, Jour. Geol., vol. 18, 1910, p. 376.

[14] F. J. H. Merrill: Am. Jour. Sci., vol. 41, 1891, p. 460.
J. B. Woodworth: Bull. 84, New York State Museum, 1905.

explaining the great depth of the channel, nearly 5,000 feet, at the outer edge of the continental shelf. Upwarping to so great an amount is improbable; some other cause must be sought to complete even a speculation concerning the origin of the channel. According to Fuller, the excavation of the deep rock gorge of the Hudson at and above New York City, like that of the channel out to sea, is to be referred to an early stage of the Glacial period.[15] He thinks the cutting was essentially fluviatile, and it is hard to credit any other cause with the original development of the channel. But, taking together the gorge above New York City and the whole channel in the continental shelf, we have a trough that has the look of a rather typical fjord depression. Is it possible that the original antecedent part of the valley of the formerly extended Hudson River was occupied by an effluent glacier as far as the edge of the shelf? Modern glaciers are cutting deep troughs below sealevel, matching in principle the fjords of Norway. If the Montauk or other pre-Wisconsin ice extended farther seaward than the Wisconsin moraine, such a tongue might have reached so far out. Later filling by glacial outwash and by marine agencies would tend to fill the old trough, especially along its inner part. The outer part would long escape aggradation. The shallow trench inside the 100-fathom line might be explained as due to etching by the Hudson when extended during the latest (Wisconsin) glaciation.

The question as to whether the submarine channel may be used as a test of the Jamieson hypothesis is seen to include many problems. Baffling as these are, they are no more troublesome than the assumption of a Pleistocene emergence of this part of North America to the extent of 5,000 feet or more. A combination of moderate crustal warping, glacial excavation, and river-cutting is surely complex; but no simple theory is likely to account for the remarkable channel of the Hudson.

SUSQUEHANNA "DEEPS"

Another recent change of river régime in the peripheral belt is found in the line of "deeps" along the channel of the Susquehanna River near the Maryland-Pennsylvania boundary. Mathews has given a good description of them.[16] Within a distance of about 25 miles the broad, flat rock-floor of the river is interrupted by six long spoon-shaped depressions. These "deeps" vary in length from 4,000 feet to nearly 2 miles and in maximum depth from 50 to 130 feet, measured below the general floor of the channel. The bottoms of five of the holes are 10 to 30 feet below sealevel. The "deeps" usually show a "downstream sag in the bottom

[15] M. Fuller: Professional Paper 82, U. S. Geol. Survey, 1914, pp. 60, 218, 220.
[16] E. B. Mathews: Bull. Geol. Soc. Am., vol. 28, 1917, p. 335.

profile." Many vertical pot-holes were found in the walls of the "deeps," enforcing the conclusion that the river current was very powerful during the excavation of these remarkable depressions (see plate 14).

Mathews believes that the cutting of the "deeps" must be "post-Talbot in age," continuing well into the Recent period, and that it was induced by increase of the river's volume or by constriction of its volume. Constriction of the channel seems to have been a necessary condition, because the "deeps" are much narrower than the general rock-floor of the channel and are all located near the east bank of the river. On the other hand, mere increase of volume is not likely to account for the forms and localization of the "deeps." If, however, the river's velocity were considerably greater a short time ago, the "deeps" might have been then excavated, in a way analogous to the cutting of channel grooves by torrents.

Higher velocity for the stream might have been produced by general lowering of sealevel when the Wisconsin ice-cap was formed; but, since the rock-floor of the Susquehanna is intact below the most southerly "deeps," this cause alone seems inadequate for the deep excavation. A more probable supposition is that the river gradient was temporarily steepened by crustal uplift. The writer is, therefore, inclined to connect this peculiar drainage history with the general hypothesis of marginal warpings connected with Wisconsin glaciation. If the peripheral bulge had its crest in Pennsylvania, the river's volume may have been smaller, but its velocity must have been greater than it is now, after the bulge has subsided. The volumes of rock removed, the recency and great depth of the excavations, and the details of form all seem to accord with the assumption that the "deeps" were made on the southerly slope of such an upwarp.

DRAINAGE CHANGES IN THE OHIO VALLEY AND FARTHER WEST

The foregoing suggestions of marginal warping have to do with the Wisconsin stage of glaciation. West of the Hudson River the corresponding isobase for zero has not been definitively mapped; hence local criteria for the truth of Jamieson's hypothesis are not easily devised in the larger part of the peripheral belt. However, in western Pennsylvania and beyond, there are not wanting evidences of widespread and rather drastic rearrangements of drainage during the pre-Wisconsin part of the Glacial period. Are these changes genetically associated with crustal upwarps marginal to one or more of the pre-Wisconsin ice-caps?

In the basin of the Ohio the changes are registered in valleys now abandoned by the rivers that cut them and in many detrital terraces perched high above the existing streams. The rivers known to have been affected include the Kanawha, Allegheny, Monongahela, Youghiogheny, Kiskiminitas, Guyandot, Big Sandy, Kentucky, and the Ohio itself.

The most numerous cases of diversion are found along the north-flowing streams (see plate 14).

Campbell explained the stream diversions by postulating many, temporary ice-dams. Summarizing his view, he wrote:

"The lower courses of the Allegheny, Monongahela, Kanawha, Guyandot, Big Sandy, and Kentucky rivers are characterized by abandoned channels, which generally range from 100 to 200 feet above the present streams. Generally these channels are deeply covered with silt, but sometimes the rock-floor is only partially obscured by a thin layer of sand and gravel. The streams which have forsaken these valleys have sought new routes, along which they have carved deep channels through the upland topography. Teay Valley, in West Virginia, is perhaps the most noted example, but the old channels at Carmichael and Masontown, on the Monongahela River, and opposite Parker, on the Allegheny River, are also well known.

"No reason has been assigned for the abandonment of these channels; they can not be considered as 'ox-bows,' and they are all beyond the limit of glacial ice. The present hypothesis seeks to explain them through the breaking up of river ice and the formation of local ice-dams which were of sufficient height to force the water over the lowest divide in the rim of the basin and which persisted long enough for the stream to intrench itself in its new position." [17]

Campbell's explanation would be more acceptable if any existing river were known to have dammed itself by ice for any of the long periods of time demanded by his hypothesis. On the other hand, diversion through excessive alluviation is well illustrated by existing streams; Shaw and Munn prefer this cause for the abandonment of valleys in the Ohio basin. Their statement for the type cases observed along the lower Allegheny may be quoted:

"The high terraces [remnants of the floors of ancient valleys that had steep sides but broad flat bottoms] developed as a unit through the overloading of the Allegheny in early glacial time and the later redissection of its deposits. The overloaded condition of the Allegheny was probably due to several causes, among which the following may be mentioned as being more or less effective: First, an actual increase in load derived from (*a*) material fed more or less directly by the glaciers; (*b*) débris from the cutting of new gorges across old divides; (*c*) material brought after the ice melted by streams in the glaciated area as they cut new valleys. Second, a decrease in velocity and carrying power, produced by (*a*) the attraction of the ice mass . . . (*b*) crustal deformation, due to the weight of the ice; (*c*) the crossing of divides or ice barriers, each of which would check the velocity and cause deposits for a short

[17] M. R. Campbell: Bull. Geol. Soc. Am., vol. 12, 1901, p. 462. See folios 69, 72, 82, 94 of the U. S. Geol. Survey for Campbell's fuller statement.

distance upstream. Third, a possible but not probable decrease in volume, arising from a change in climate. . . .

"In any case the aggradation of the Allegheny caused every tributary to build up to a gradient over which it was just able to carry its load. The coarseness, slope, and other characters of the deposit indicate that the tributaries built up as rapidly as the overloaded master stream. As the stream beds rose they reached the heights one after another of the lowest places in divides between small tributaries, and at such times and places the currents of the rivers were divided and the cols occupied. When final redissection began the rivers took the channels momentarily most desirable, or, if there was little difference in desirability, used both for a time; thus many parts of valleys were abandoned." [18]

The explanation by Shaw and Munn is thus seen to embody elements which had been suggested by Tight and others. In his work on similar drainage changes in Ohio, Tight mentions also crustal warping as a conceivable factor, but rejects it. If the changes were due to warping, "it would be necessary to assume that after the modifications were well established an exactly opposite warping took place." Such movement "in exactly the reverse order seems extremely improbable." [19]

However, according to Jamieson's general hypothesis, just such a reversal of crust movements in the Ohio Valley ought to be seriously considered. Its probability depends on the relation of the Ohio Valley to the zero isobase for the isostatic movement caused by the associated (Kansan?) ice-cap. Presumably this line lay a little farther south than the zero isobase for crust movements connected with the last glaciation. If the marginal bulge of the earlier glacial stage were located in or near the "Great Valley" of the Appalachian chain, the southern headwaters of the Ohio would have been stimulated and their detrital loads increased. Aggradation in the lower stream courses, outside the upwarped belt, would thus result from a cause which is additional to the causes listed by Shaw and Munn. With sufficient aggradation, the rivers would locally find, on solid rock beyond their valley walls, channels lower than their own alluvial plains. This local abandonment of the old channels would cease when the Great Valley belt subsided as a result of deglaciation. Most of the new channels would, however, be permanent.

Even from the brief review given, it is obvious that the testing of Jamieson's hypothesis in the Ohio Valley portion of the marginal belt is an exceedingly complicated problem. The writer's only excuse for venturing so far into speculation is to open the subject for discussion by those more familiar with this extensive field. Drainage peculiarities of some

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[18] E. W. Shaw and M. J. Munn: Burgettstown-Carnegie Folio (no. 177), U. S. Geol. Survey, 1911, p. 9.

[19] W. G. Tight: Professional Paper No. 13, U. S. Geol. Survey, 1903, pp. 93, 97.

kind must be registered therein, if Jamieson's version of the isostatic theory is correct. Can special studies disentangle the effects of climatic changes and isostatic changes in the Ohio basin?

Similar, likewise unanswered questions arise in connection with recent diversion of rivers in the marginal belt farther west. The Platte River is an example.[20] Have some rivers been reversed in their directions of flow because of crustal warpings due to glaciation and deglaciation? Was the Columbia River temporarily diverted into the course of the Grand Coulée, State of Washington, because of a marginal bulge across the Columbia Valley below the northern end of the spectacular coulée? The prevailing explanation by the damming action of the Okanagan glacier ought not to have the field all to itself. Is the Recent tilting of this region, shown by systematic displacement of stream courses running east and west, possibly connected with marginal warps of the earth's crust around the Cordilleran ice-cap, as expected on Jamieson's hypothesis?[21]

Evidence from Europe

Since the preceding pages were written, it was found that Munthe, in explanation of recent changes of level in the belt marginal of the Scandinavian ice-cap, has also adopted Jamieson's hypothesis. His own observations, compiled with those of Brögger, De Geer, and others, led Munthe to assume peripheral sinking in the south Baltic region to the extent of at least 125 meters since the glaciated area on the north began to rise. He estimates the correlative subsidence near the outer edge of the continental shelf off Norway to be about 200 meters. He writes:

"From what has been said above, it is evident that the opinion first expressed by T. F. Jamieson (1891) and later (1892) confirmed by myself may to a certain extent be correct, namely, that a movement of land within the central regions of the glaciated northern Europe also caused a gradual movement in the opposite direction within the peripheric parts, and *vice versa*."[22]

In this connection reference may be made to Brögger's map of negative (subsidence) isobases crossing Denmark, and to the recent sinking of the fresh-water "Sanddiluvium" (Eemian series) in Holland to levels below sea, deepening from 83 meters at Deventer to 200 meters at Amsterdam.[23]

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[20] U. S. Geol. Survey Bull. 612, 1915, p. 18 and references.

[21] J. T. Pardee: Bull. 677, U. S. Geol. Survey, 1918, p. 50.

[22] H. Munthe: Geol. Fören. Stockholm Förhandl., vol. 32, 1910, pp. 1197-1293, with full bibliography. Reprinted as Guidebook No. 25, Cong. géol. internat., Stockholm, 1910.

[23] W. C. Brögger: Norges geol. Unders., no. 31, 1901, pp. 681, 683; no. 41, 1905, map. F. E. Geinitz: Die Eiszeit, Braunschweig, 1906, p. 98.

Of course, sedimentary loading must also be considered as a possible cause of recent subsidence in Holland.

Munthe's able summary of the case in northwestern Europe is clearly of great significance for American geologists. Jamieson's hypothesis attains new dignity, as it is seen to have been quite independently adopted to correlate and explain many facts observed in the belts peripheral to two different ice-caps.

Summary

Many lines of evidence seem to support Jamieson's suggestion that the earth's crust has been plastically deformed by glacial loading and, in the reverse way, by unloading through the melting of ice-caps. Study of the recently emerged zone in New England and of the specific distribution of plants and animals in Newfoundland indicate the probable existence of a late Glacial to Recent bulge of land near the edge of the continental shelf. If that bulge ("Georges Bank Land") were flattened because the glaciated area on the northwest rose in consequence of unloading (ice-melting), one may reasonably expect field evidences of similar peripheral subsidence west of New England. Deformed lake strands, the submarine channel of the Hudson River, the "deeps" of the Susquehanna River, and Pleistocene drainage rearrangements of the marginal belt west of New Jersey offer relevant topics for discussion. In no case can the evidence be regarded as final, for none can be discussed intelligently without making at least one unproved assumption. A leading and general difficulty lies in the uncertainty as to the position of the zero isobase corresponding to each subsidence and each uplift, respectively induced by glaciation and deglaciation. Jamieson's hypothesis thus leads to many questions without answers; precisely for that reason it has value if it stimulates further field-work by experts. Especially because of Munthe's positive results in Baltic lands, the hypothesis can not fail to be seriously and actively entertained in America.

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EXTENT AND THICKNESS OF THE LABRADOR ICE-SHEET[1]

BY A. P. COLEMAN

(*Presented before the Society December 30, 1919*)

CONTENTS

INTRODUCTION

The Labrador ice-sheet did not cover the whole of eastern America north of New York, since the Torngat region, in northeastern Labrador, and the Shickshock Mountains, in Gaspé, escaped and remained as driftless areas. On the other hand, in places the ice-sheet passed beyond the present eastern edge of the continent, covering part of the shallow sea bottom along the coast of Labrador and occupying most, if not all, of the present Gulf of Saint Lawrence. On the western side the ice-sheet at its greatest extent filled the basins of Hudson and James bays, thus meeting the Keewatin sheet. Just how far it extended southwest is not quite certain, its relations to the Patrician sheet described by Tyrrell being still only imperfectly known. The island of Newfoundland is believed to have been an independent glacial center, adjoining the Labrador sheet on the east.

It is proposed in this paper to define the eastern limits of the Labrador sheet, referring specially to the driftless areas; to estimate the thickness of the sheet at various places, and to discuss the relation of the ice burden to postglacial raised beaches and to the theory of isostasy.

[1] Manuscript received by the Secretary of the Society February 30, 1919.

Glacial Features of the Torngats

That the summits of the Torngat Mountains, in northeastern Labrador, were not glaciated has been known for many years and has been referred to by Bell, Low, Daly,[2] and others. During the summers of 1915 and 1916 the present writer studied parts of the region in some detail and made sure that the Labrador ice-sheet never crossed the region of moun-

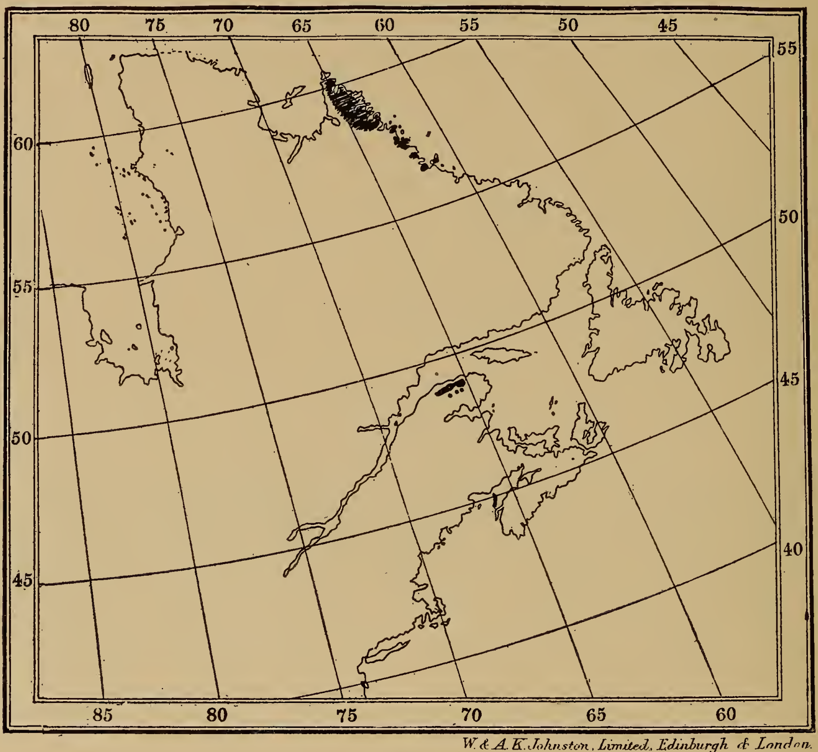

Figure 1.—*Eastern Edge of the Labrador Ice-sheet*

Driftless area shaded.

tains and tableland called the Torngats, though the eastern side of the range supported many large local glaciers which carved or deepened the valleys and left behind some magnificent fiords.

Most work was put upon Nakvak fiord and its surrounding mountains, some of which reach altitudes of 5,000 feet or over. Nakvak fiord itself

[2] Bell: Geol. Survey of Canada, 1882-1884, pp. DD 11-17.

Daly: Geol. of northeast coast of Labrador. Bull. Mus. Comp. Zool., Harvard, vol. xxxviii, 1902.

is 30 miles long, and its valley, occupied by a river and a fiord-like lake, continues 15 miles farther west, cutting across the highest part of the mountains. It did not, however, furnish an outlet for the Labrador ice to the sea, as has been supposed, but was occupied by a long local glacier fed by numerous lateral glaciers from north and south. At the head of the fiord the ice probably stood 2,000 or 2,100 feet above the sea, its surface sinking to about 1,400 feet at the outlet, so that there was a gradient of about 20 feet per mile. The highest distinct evidence of glacier-work observed in the region is at 2,650 feet in a side valley, where a cirque lake is dammed by morainal blocks.

The summits of most of the mountains are flat or rolling and are covered with loose blocks that show no effects of ice-work. In places, for instance, blocks of diabase rest upon a dike, while blocks of gneiss lie undisturbed on both sides.

For 200 miles south of Cape Chidley, the northeast point of Labrador, the mountains seem to have formed an effectual barrier to the great ice-sheet; but just north of Hebron, at the fiord called Iterungnek, it may have reached the sea. Some islands near the mouth of the fiord are well glaciated, though on Maidment Island, 10 miles out to sea, there are no signs of ice-action, showing that land ice did not reach so far.

Johannesberg, north of Hebron, rising to 2,300 feet, was a nunatak elevated several hundred feet above the ice-sheet, and its flat summit is covered with loose stones weathered *in situ*. At Hebron itself beautifully polished and striated surfaces from which boulder-clay is now being stripped give conclusive evidence of glacial work on a large scale.

For 30 miles to the south the comparatively low country has been ice-shaped; but the Mugford Mountains rose above the ice, though transported blocks are found up to 2,000 feet. A gap of 35 miles separates the Mugford nunataks from the next ones to the south, at the Kiglapait and Aulatsivik Mountains, near Nain. Proofs of glacier-work are found on all the shore of Labrador to the south, though a few high summits near Hamilton Inlet may have been nunataks.

Newfoundland seems to have been covered with a local ice-cap, though the island of Twillingate, to the north, shows no signs of land ice action. However, blocks transported by floating ice may be found up to 270 feet on the island.

Ice Boundary in the Gulf Region

The boundary of the ice to the south is somewhat uncertain. The low island of Anticosti was apparently covered, and there is clear proof of the work of glacial ice on the Magdalen Islands[3] in the form of sandy till with well striated stones, up to an elevation of 105 feet. Above this hills of loose basaltic blocks rise to 360 feet in places, showing no evidence that a great ice-sheet ever passed over them. The islands seem too small and low to have formed an independent glacial center, and it is probable that the thin southeastern margin of the Labrador ice-sheet inclosed them without crossing the hilltops. If so, the thickness of the sheet at this point can not have been more than about 200 feet. Whether the ice, perhaps with its edge afloat, reached Cabot Straits between Newfoundland and Cape Breton is uncertain.

FIGURE 2.—*Map of Gaspé in the Ice Age*

Driftless area shaded.

It was thought by Chalmers that the eastern end of Prince Edward Island was unglaciated; but I have found undoubted till with well striated stones at Souris, proving that land ice covered at least the lower ground.[3] The central part of the island has not furnished evidence of the action of land ice, but the west end is more or less covered with boulder-clay containing blocks derived from the mainland. There are no hills rising much above 311 feet (Wiltshire station, highest railway point on the

[3] J. W. Goldthwait: Geol. Survey of Canada. Museum Bull. No. 14, 1915.
A. P. Coleman: Glacial history of Prince Edward Island and the Magdalen Islands. Roy. Soc. Canada, 1919.

island), so that one would hardly expect a local ice center, relations being much the same as in the Magdalen Islands.

The evidence is conflicting as to whether Nova Scotia was covered by the Labrador ice-sheet or not.[4] Kame deposits and boulder-clay occur on the western side of the province, suggesting the margin of a great ice-sheet, but in some places, as near Pictou, there are erratics which seem to have been transported northward instead of southeast, as would be expected, and Chalmers' conclusion that most of the glaciation of the peninsula came from local centers may be correct.

The Gaspé driftless Area

While the margin of the Labrador ice-sheet crossed the floor of the present Gulf of Saint Lawrence and swept southwestward over the lower part of Nova Scotia, the highest part of the Province of Quebec, the Gaspé Peninsula, was never covered by continental ice, but formed a local center of glaciation. The backbone of the peninsula is formed by the

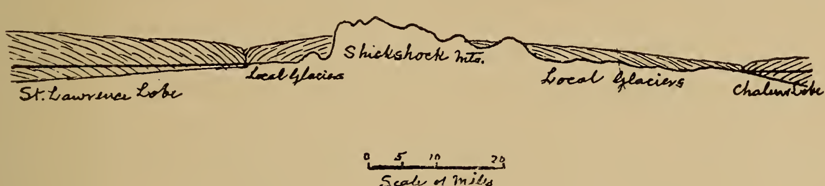

FIGURE 3.—*Section across Gaspé in the Ice Age*

Average thickness of ice, 1,300 feet.

Shickshock Mountains, rising from 2,000 to 4,300 feet above the sea, and the higher parts of the range, above 3,000 feet, show no evidence of glaciation, and may be described as a driftless area of a similar kind to that of the Torngats, in northeastern Labrador, though on a smaller scale. The flat or rolling tops of the range are formed of a sheet of weathered blocks of local origin with no foreign boulders, while glacial deposits, usually thin and sometimes lacking, spread outward from the higher parts of the mountains to the sea on each side. The most elevated proofs of ice-work on the north, or Saint Lawrence, side are found at Lac aux Americains, at 2,300 feet, while there is evidence that ice from the Shickshocks carried morainic material over the tops of mountains 2,000 feet high on the southern side.

[4] Geol. Survey of Canada. Sum. Rept., Part F, 1918, pp. 20 and 24.

One must imagine the Labrador ice as parting into two lobes on the col just west of Gaspé, where the elevation is only 751 feet on the Intercolonial Railway, one lobe occupying the Saint Lawrence Valley and the other the valley of Matapedia River and the Bay of Chaleur. Beyond the end of Gaspé the two lobes, augmented by local glaciers from the Shickshocks, met once more and continued as a single sheet to the margin beyond the Magdalen Islands, as described above.

The driftless area of the higher Shickshocks is about 50 miles long by 10 miles wide, with an area of 500 square miles; and the region which escaped the Labrador sheet, though largely covered by local ice, is 150 miles in length by about 70 in breadth, with an area of 10,000 square miles, in round numbers.

Thickness of the Ice-sheet

The Shickshocks give an opportunity to estimate the thickness of the ice-sheet. At the col 751 feet high, just west of the peninsula of Gaspé, the ice can not have risen more than 3,000 feet above present sealevel in the Saint Lawrence or it would have submerged the lower Shickshocks, of which there is no evidence. Drift boulders have been found on Carlton Mountain, 100 miles to the southeast, at 1,270 feet; so that the ice at that point must have risen at least somewhat above that level—say, to 1,500 feet. If one allows 10 feet of slope per mile to account for the flow of the ice, this gives 2,500 feet for the surface at the col—a very probable estimate. The thickness of the ice would be 2,500 feet, 750 feet, or 1,750 feet. If one assumes the specific gravity of glacier ice to be about one-third that of average rock, this would mean a load equivalent to 583 feet, which corresponds exactly to Fairchild's determination of the highest marine level at Sayabec, a few miles to the southeast.

It is possible also to reach an approximation of the thickness of ice in a north and south section through Tabletop, the highest part of the Shickshocks. On the north side of the range no boulder-clay containing Archean stones, and therefore formed by the Saint Lawrence lobe of the main sheet, has been found more than half a mile inland; and it may be supposed that the local glaciers or ice-sheet which carried blocks of granite and serpentine northward from the mountains met the Saint Lawrence ice near the present shore. The highest undoubted evidence of glacier-work on the north side of Tabletop is a moraine at the outlet of Lac aux Americains, 2,300 feet above sea. The glacier which built the moraine must have risen somewhat above this level. Assuming the slope of the local glacier to be 20 feet per mile for 12 miles to the present shore, the ice there would have a thickness of about 2,000 feet, and presumably the Saint Lawrence lobe met it at that level, but itself sloped gently upward toward the glacial center to the north. On the south side of the Shickshocks there is proof, in the form of boulder-clay containing Tabletop granites and also serpentines, near the Federal zinc mine, that a glacier crossed the summit of mountains reaching 2,000 feet or a little over, and drift blocks of the same kind with some boulder-clay extend to the mouth of Cascapedia River, on the Bay of Chaleur. From the height to which erratics have been found on Carlton Mountain, as mentioned before, the elevation of the Chaleur ice-lobe at this point may be put at about 1,250 feet. In the way suggested, the probable surface level of the ice from the middle of the Saint Lawrence to the middle of the Bay of Chaleur, a distance of 120 miles, may be inferred; and, allowing for about 20 miles of mountain with little or no load of ice, the average thickness works out to 1,300 feet. One-third of this is 430 feet, the equivalent load of average rock. Fairchild's map shows the isobase of 400 feet passing close to this line of section, which accords sufficiently well with the result.

Thickness of the Ice in the Adirondacks

No other mountains of eastern Canada were high enough to project above the Labrador sheet at its maximum, but a brief visit was paid to the Adirondacks in New York to study ice relations, and Mount MacIntyre (5,112 feet) was climbed. The valleys show powerful effects of glaciation, and erratics as well as sandy till were found in the ascent up to a height of 4,300 feet, where pebbles and small boulders of quartzite and granite occur, clear proof of glacier action, since Mount MacIntyre consists of anorthosite passing into gabbro.

Probably more extended search would disclose ice-borne materials at a higher level, and in any case it is probable that the ice reached some hundreds of feet above the lowest portion of the glacier charged with stones. The top of the mountain consists of large and small blocks of native rock, some weighing many tons, piled loosely and suggesting relationships due to weathering only. It seems to me very improbable that a great ice-sheet could have passed over the mountain top without dislodging these stones, and it is likely that Mount MacIntyre, Mount Marcy (5,344 feet), and a few other peaks a little lower were nunataks rising a few hundred feet above the ice.

If we suppose the ice to have reached 500 feet above the highest known morainic materials, its surface would have been 4,800 feet above sealevel and 2,800 feet above the adjoining valleys, which are at about 2,000 feet. Supposing that the ice filled the valleys to 4,800 feet, and that the mountain masses occupied half the total volume below that level, the additional load of ice would average 1,400 feet. One-third of this is about 470 feet, while Fairchild's map of isobases gives the old marine level in the Adirondacks as about 600 feet. However, the group of highest peaks does not occupy much space, and the ice over the rest of the region, with its much lower mountains, would average thicker and thus supply the deficiency in load equivalent to 130 feet of rock.

I have had no opportunity to study the White Mountains with respect to glaciation, but Goldthwait believes that the ice-sheet passed over them. If so, it must have been far thicker than would be expected if the Shickshocks to the northeast and the Adirondacks to the west were not entirely covered. Is it possible that local glaciers could account for the facts in regard to the White Mountains?

Relations to Isostasy

It is of much interest to determine the limits within which inequalities of load can be sustained by the earth's crust without adjustment by bending or faulting, and the Gaspé region may be considered from this point of view. A belt of mountains 50 miles long by about 12 miles wide was scarcely at all ice-covered and over double that length and breadth there appear to have been only small local glaciers or ice-sheets, while in the valleys to the south and north there was a thickness of from 1,250 to 2,300 feet. If there was perfect adjustment to the relief from load, the borders of the peninsula should have risen from 400 to 800 feet while the mountain axis remained at its old level. The uniformly graded channels of the rivers, which are undoubtedly preglacial, show that no such differential adjustment occurred. The grade of Sainte Anne and Cap Chat rivers to their forks, well within the mountains, is so uniform that boats are poled all the way up with scarcely an interruption, their lower course being almost as rapid as the upper part. This is even more markedly the case on Cascapedia River, flowing 45 miles south from the Shickshocks to the Bay of Chaleur. There is a fairly rapid current all the way, but scows with several tons of freight are towed the whole way by a team of horses.

The excellent grading of the rivers and the almost complete absence of lakes or floodplains where they enter the sea prove that the region rose as a whole, although the relief from load was almost confined to the north and south and amounted to little or nothing at the center.

One may draw the conclusion that the earth's crust can sustain inequalities of load amounting to 1,500 or 2,000 feet of ice, or the equivalent weight of rock, where the width of the region in question is not greater than 75 miles, the load decreasing from a maximum at the two sides to nothing in the center.

The lack of differential elevation in the Gaspé Peninsula may be contrasted with the doming up of Newfoundland after the Ice Age, as indicated by Tyrrell and Fairchild. Newfoundland is considered to have been an independent glacial center, and the isobases are so mapped as to show a doming of the interior of 400 feet as compared with the edges. This may be interpreted as meaning that Newfoundland bore an ice-cap 1,200 or 1,500 feet thick, diminishing in all directions outward—conditions exactly opposite to those in Gaspé, where the central mountains carried little or no ice, though local glaciers radiated out from them.

Newfoundland has diameters of about 300 miles by 200, and a land area of 42,000 square miles, and it seems that a portion of the earth's crust of those dimensions does not change its level as a whole, but undergoes differential elevation to correspond to the varying relief from load; or, to put it in another way, the earth's crust is not strong enough to resist isostatic adjustments where the area affected is 200 miles wide, but can support the difference of load where the width is only 75 miles, as in Gaspé.

The case of the Torngat region, in northeastern Labrador, is apparently similar to that of the Gaspé Peninsula, though it has been studied much less thoroughly. The breadth of the unglaciated core of mountain and tableland is probably 50 or 60 miles, but 30 miles of the eastern side of the Torngat Range is riddled with fiords and deep valleys, occupying probably half the surface. As these were once filled with great local glaciers reaching in places 2,000 feet in thickness, the amount of depression of the region, about 250 feet, is readily accounted for. There is no evidence that the glacier-laden coastal region has risen more than the unglaciated tableland, perhaps 20 or 30 miles wide toward the west.

The suggestion of De Geer that the pre-Cambrian shield rose, as contrasted with stationary Paleozoic terranes to the south, receives no support from the actual changes of elevation shown by beach levels. The highest actually measured elevation, 690 feet at Kingsmere, north of Ottawa, is on Archean territory; but the beach levels diminish to 225 feet, still on the pre-Cambrian, at Komaktorvik Fiord, in the north of Labrador. To the southeast of the glacial center raised beaches occur on Paleozoic territory beyond the Saint Lawrence at 430 feet and continue for 350 miles before sinking to sealevel. It is evident that the change of level is not connected with the character of the bedrock, which is, after all, a superficial thing, the granitoid gneisses of the Archean really underlying the Paleozoic sediments and probably at no very great depths.

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QUANTITATIVE METHODS OF ESTIMATING GROUND-WATER SUPPLIES [1]

BY OSCAR E. MEINZER

(Presented before the Society December 31, 1919)

CONTENTS

INTRODUCTION

The water that comes from beneath the surface of the earth has from time immemorial been regarded by man as a mysterious boon—cold, clear, and presumably pure; wonderfully refreshing to the hot and weary wayfarer; essential, perhaps, to the very life of the community, yet without cost and generally regarded as inexhaustible; coming from some dark orifice in the earth, its source unknown and unknowable; a great blessing freely and mysteriously bestowed on man by some benevolent but capricious Providence. When, very rarely, in times of unusual drought, this mysterious blessing, which was regarded to be perennial and inexhaustible as the air itself, was cut off and the spring or well went dry, the occurrence was a calamity, in the face of which man stood helpless in fear and awe.

When at last the geologist appeared, with his methods for beholding what is beneath the earth's surface, concealed from ordinary eyes, some of the mystery of these hidden waters began to disappear, and it became possible to give a rational explanation of their occurrence and origin and

[1] Manuscript received by the Secretary of the Society December 29, 1919.
Submitted by permission of the Director of the U. S. Geological Survey.

to outline in general terms the sources of the subterranean streams. This encroachment on the realm of mystery seemed almost sacrilegious, and has, naturally enough, been regarded by the ordinary man with a good deal of skepticism. However, water supplies are so necessary for nearly all undertakings that no one can claim the vision of a prophet in this respect without being compelled to prophesy. Thus, perforce, the science of ground-water hydrology has been developed, and its development has taken place under the wholesome stimulus of practical men, who insist on specific predictions, and then immediately proceed to sink wells which will definitely test these predictions.

The progress in developing methods for estimating quantities of ground-water has resulted largely from the demand for information required for the complete utilization of the ground-water resources in the arid regions. In humid regions it is seldom desired to pump out of wells all the available water that is in the earth, but in many irrigation districts that is precisely the problem. Fortunately, the conditions are more favorable for quantitative studies in arid than in humid regions.

The study of water supplies differs from a study of other mineral deposits, in that it relates to dynamic, not static, conditions; it relates to processes that are going on in the earth at the present time, not alone to processes of past geologic ages. We have very little interest in mineral reserves. The practical question with which we have to deal is not as to the quantity of ground-water that lies below an area, but as to the rate of replenishment, for conservative developments must be based on the rate at which the ground-water can be withdrawn year after year, for generations to come, without depleting the supply.

It should be understood that this discussion relates only to the water in the zone of saturation—that is, to the *ground-water,* meaning the basal, or bottom, water, also called *phreatic water,* which in Greek means well water. It does not relate to the subsurface water that fails to get down to the water-table, which would be a very different subject.

Four Groups of Methods

IN GENERAL

Four principal methods, or groups of methods, are used to determine the annual recharge or "safe yield" of ground-water. These may be called the intake, discharge, water-table, and underflow methods. The first of these consists in measuring the quantity of surface water that seeps into the earth and percolates into the zone of saturation; the second, in measuring the ground-water that is discharged through springs or by evaporation from soil and plants; the third, in observing the fluctuations in the water-table, which represent filling or emptying of the ground-water reservoir; the fourth, like the gaging of surface streams, in measuring the flow of ground-water at selected cross-sections.

INTAKE METHODS

Intake methods are applicable chiefly with respect to influent streams in arid regions, especially the mountain streams that flow out over gravelly alluvial fans, where they lose water rapidly.

If nearly the entire supply comes from one stream, as in the San José district of Santa Clara Valley, California,[2] the recharge can be ascertained by establishing a gaging station where the stream enters the valley, and one or more gaging stations farther downstream.

In an area such as Big Smoky Valley, Nevada,[3] however, which receives 54 perennial mountain streams and the flood discharge of innumerable dry canyons, the problem is more complex. In the latter part of April, 1915, measurements showed that the discharge of 33 of the streams entering Big Smoky Valley amounted to 163 second-feet, and it was estimated that the aggregate discharge of the 54 perennial streams was about 170 second-feet. Similar measurements were made in July and September, and a daily record for several months was obtained on one of the streams. Certain of the streams were investigated with respect to seepage losses. For example, in April, Belcher Creek flowed 6.25 second-feet at the mouth of its canyon and 4.73 second-feet at a point farther down the valley, thus losing 1.52 second-feet in the intervening distance. On the basis of the evaporation area afforded by the creek and the known rate of evaporation, the loss by evaporation was calculated to be only 0.02 second-foot, leaving for ground-water recharge a flow of 1.50 second-feet, which is at the rate of 1,086 acre-feet a year. On the basis of similar studies for other creeks and for this creek at other seasons, it was estimated that the perennial streams contribute to the ground-water supply at an average rate somewhere between 15,000 and 30,000 acre-feet a year. The contributions by floods from dry canyons, by underflow through the gravel deposits in the canyons, by precipitation in the valley, and by percolation from the bedrock could not well be measured, though the data obtained indicates that these contributions amount to several tens of thousands of acre-feet a year.

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[2] W. O. Clark: Ground-water for irrigation in the Morgan Hill area, California. U. S. Geol. Survey Water-supply Paper 400 *e*, 1917.

[3] O. E. Meinzer: Geology and water resources of Big Smoky, Clayton, and Alkali Spring valleys, Nevada. U. S. Geol. Survey Water-supply Paper 423, 1917.

As methods dealing with intake give no information as to the unavoidable losses from the ground-water reservoirs, the results obtained by these methods must be used with caution where the question of safe yield is involved.

DISCHARGE METHODS

Water is discharged from the zone of saturation by two very distinct processes—by the flow of springs and by evaporation from soil and vegetation. To estimate the discharge by these diverse processes, entirely different methods are required.

Most areas yield two kinds of run-off: direct run-off, which is water that has not been beneath the surface since it fell as rain or snow, and ground-water run-off, which is derived either from definite springs or from general seepage along effluent streams. Commonly the two kinds of run-off are mingled, and neither can be measured separately. However, we know that in times of flood the extra water is nearly all direct run-off, and that in the intervals between storms the direct run-off diminishes and the streams carry a constantly increasing proportion of water derived from subterranean sources. It is doubtless possible to reach a fairly close approximation as to the ground-water run-off of a basin by studying the fluctuations of stream discharge as shown in a hydrograph, and making correlative observations on precipitation, melting of snow, capacity and yield of ponds, lakes, and reservoirs, total quantity of stream water in the basin at specific stages, turbidity of the stream water, chemical composition of stream water as compared with ground-water, fluctuations of water levels in wells, and fluctuations in the flow of springs that are available for measurement. Such a study is at present being made by A. J. Ellis of the Pomperaug drainage basin, in Connecticut. This method is more generally applicable in humid than in arid regions, and hence is not of as much practical value as some other methods. It does not, of course, give total discharge of ground-water, for it takes no account of the discharge from the zone of saturation through plants and evaporation from soil, which in humid regions is very difficult to estimate, though certainly large.

Where the zone of saturation is near the surface, ground-water may be discharged either through capillary rise and evaporation from the soil or through absorption by the roots of plants and subsequent transpiration. The height to which water in perceptible quantities will rise above the water-table by capillarity depends on the texture of the deposit. It is greater in fine-grained than in coarse-grained materials, but is not commonly as much as 10 feet.

In arid regions plants of certain species habitually utilize water from the zone of saturation. For such plants the name *phreatophyte* (meaning a well-plant) has been proposed. Salt grass (*Distchlis spicata*), rabbit brush (*Crysothamnus graveolens*), big greasewood (*Sarcobatus vermiculatus*), a certain type of mesquite (*Prosopis* ——), samphire (*Spirostachys occidentalis*), rye grass (*Elymus condensatus*), yerba mansa (*Anemopsis Californica*), and birch, sycamore, and willow trees are among the most common phreatophytes in the arid West.

According to observations by C. W. Riddell in Steptoe Valley, Nevada, the water-table is generally within 20 feet of the surface where greasewood predominates, within 15 feet where rabbit brush predominates, and within 7 feet where salt grass predominates. Other observations seem to indicate that a certain type of mesquite will send its roots to a depth of as much as 50 feet to find water.

Except for tracts containing plants of uncertain significance, the areas of ground-water discharge can be shown on a map with considerable accuracy. Thus, it was found that in Big Smoky Valley ground-water is discharged from a total area of 130,000 acres, exclusive of the greasewood belt, and in a number of other Nevada valleys there are discharge areas of comparable size, large parts of which are bearing luxuriant vegetation. Such information, of itself, gives valuable clues as to the rate of discharge from an aquifer and as to the quantity of ground-water annually available. This was strikingly impressed on me recently, when I went from some of the relatively well watered valleys of east-central Nevada, with their large areas of luxuriant phreatophyte vegetation, directly to the more arid Mohave and Colorado deserts of southeastern California, where, as a rule, the areas of discharge are much smaller and have less luxuriant vegetation. In a valley having 130,000 acres of ground-water discharge there may be a question as to whether the annual water supply is sufficient to irrigate 50,000 acres or only 5,000 acres, but it is certain that there is a substantial irrigation supply. On the other hand, in a desert valley, with only a few hundred acres of stunted vegetation that is discharging ground-water, irrigation developments should be made with caution, even though wells of satisfactory yield are obtained.

A very valuable series of experiments on the rate of discharge of ground-water from soil and vegetation in Owens Valley, California, was made several years ago by Charles H. Lee.[4] In experiments made in tanks filled with soil supporting a growth of salt grass, he found that the

[4] Charles H. Lee: An intensive study of the water resources of a part of Owens Valley, California. U. S. Geol. Survey Water-supply Paper 294, 1912.

annual discharge amounted to 43 inches in depth of water where the depth to the water-table was 1.3 feet, 30 inches where the depth was 2.9 feet, 23 inches where the depth was 4.5 feet, and 8 inches where the depth was 4.9 feet. By applying these and similar results to the segment of Owens Valley which is investigated, he estimated the average discharge of ground-water by evaporation from soil and by transpiration from native vegetation to be between 93 and 114 second-feet.

The rate of discharge depends on three factors: (1) Evaporativity of the atmosphere, (2) depth to the water-table, (3) character of soil and vegetation. In the field survey both the areal variations and the seasonal fluctuations in depth to the water-table must be taken into account. As this method gives the surplus actually discharged by nature, it comes nearer than some of the other methods to giving the quantities that can be withdrawn for human use.

WATER-TABLE METHODS

The third group of methods, based on water-table fluctuations, are especially well adapted to conditions such as those in California, where the year is divided into a rainy season, when nearly all the recharge takes place, and a dry season, when the heavy withdrawals are made through evaporation and transpiration and by pumping for irrigation. The rise of the water-table in the rainy season represents filling of the aquifer, and the average annual increment to the ground-water supply can be computed by multiplying the average annual rise by the percentage of available pore space and multiplying this product by the area of the water-table of the given aquifer. If the rise is given in feet and the area in acres, the annual increment is expressed in acre-feet.

Good examples of the application of this method are afforded by the recent work of W. O. Clark on the Niles Cone[5] and in the Morgan Hill area,[6] in California. The Niles Cone was under investigation during 1912-1913, an unusually dry year, and 1913-1914, an unusually wet year. On the basis of measurements at about 125 wells, it was estimated that in 1912-1913 the average rise of the water-table was less than 2 feet and the total recharge, exclusive of the loss during the period of rise, was only 2,600 acre-feet, but that in 1913-1914 the average rise was 11.3 feet and the recharge 59,000 acre-feet. In the Morgan Hill area the annual fluctuation of the water-table in 1914 to 1916 was found to range in different

[5] W. O. Clark: Ground-water resources of the Niles Cone and adjacent areas, California. U. S. Geol. Survey Water-supply Paper 345 *h*, 1915.

[6] W. O. Clark: Ground-water for irrigation in the Morgan Hill area, California. U. S. Geol. Survey Water-supply Paper 400 *e*, 1917.

parts of the area from 10 to 45 feet. The available pore space was estimated at about 12 per cent. This gave a total recharge of 34,000 acre-feet, exclusive of loss during the period of rise.

The most uncertain factor in this method is the percentage of available pore space—that is, the *specific yield,* sometimes called the effective porosity. There is urgent need for the development of practical methods of determining this quantity. It is well known that the specific yield of a deposit may be very different from its total water content or porosity. In clean gravel, which gives up nearly all of its water by gravity, the two are nearly the same, but silt and clay may have high porosities and yet yield little or no water. What is needed is determinations not of porosity, but of specific yield. Reliable determinations of specific yield can not be made by draining small samples, because, as is well known, the amount of water that drains per unit volume from a small sample is less than the amount that drains per unit volume from a large body of the same kind of material. The principle involved could be illustrated by means of a very long capillary tube filled with water. If such a tube were held upright, it would drain down to the level determined by its capillary range, but if it were broken up into small pieces each piece would hold all of its water.

Although no methods for determining specific retention and specific yield have been well developed and standardized, examination of the literature on the subject shows that at least seven different methods for the determination of these quantities have been suggested. I can not take time here to discuss these methods. Briefly stated, they consist of the following procedures: (1) Draining high columns of saturated materials in the laboratory;[7] (2) saturating in the field a considerable body of material situated above the water-table and above the capillary fringe, allowing it to drain while reasonably protected from evaporation, and then determining its water content and porosity;[8] (3) collecting samples immediately above the capillary fringe of the water-table after the water-table has gone down an appreciable distance, as it commonly does in summer and autumn, and determining its water content and porosity;[9] (4) ascertaining the volume of sediments drained by heavy pumping, a

[7] F. H. King: Principles and conditions of the movements of ground-water. U. S. Geol. Survey, 19th Ann. Rept., part 2, 1899, pp. 86-91.

[8] F. J. Alway and G. R. McDole: Relation of the water-retaining capacity of a soil to the hygroscopic coefficient. U. S. Dept. Agr., Jour. of Agr. Research, vol. 9, 1917, pp. 27-71.

[9] A. J. Ellis and C. H. Lee: Geology and ground-waters of the western part of San Diego County, California. U. S. Geol. Survey Water-supply Paper 446, 1919, pp. 122 and 123. This method was applied by C. H. Lee.

record being kept of the quantity of water that is pumped;[10] (5) ascertaining the volume of sediments saturated by a measured amount of seepage from a natural or artificial stream; (6) making indirect determinations in the laboratory with small samples by ascertaining the moisture equivalent—that is, the percentage of water held against a centrifugal force 1,000 times as great as the force of gravity;[11] and (7) making mechanical analyses of the water-bearing material and computing therefrom the specific retention and specific yield.[12]

It will be noted that the fourth and fifth methods are essentially combinations of the water-table method with the discharge and intake methods of determining recharge, with a shift in the factor that is the unknown quantity. In the Morgan Hill area Clark calculated the average specific yield by applying the laboratory results obtained by King to the local deposits as revealed by 72 well logs. This calculation gave an average specific yield of a little over 12 per cent. Later he had opportunity to obtain a check on this figure by means of an entirely independent method. In a pumping test made by the Bay Cities Water Company 1,643 acre-feet of water were withdrawn, and, according to the records of the lowering of the water level in a series of observation wells situated within a few miles of the pumped wells, the total volume of sediments drained was 14,195 acre-feet, giving a specific yield of 11.6 per cent as against 12 per cent estimated by the other method. As Clark states in his report, "the close agreement is, of course, accidental, but the fact that the two methods lead to the same general result is probably significant."

In the highly developed San Bernardino Valley, in California, Mendenhall[13] used a skillful modification of the ordinary water-table method in which he eliminated specific yield as a factor. He obtained data on the total annual pumpage and also found that in years having about 2½ inches more of precipitation than the average there was no net rise or fall of the water-table. On the assumption that the recharge varies in proportion to the precipitation, he calculated that about 115 second-feet may be withdrawn without permanently reducing the reserve supply. Such a method is, of course, applicable only in areas having extensive pumping developments.

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[10] W. O. Clark: Ground-water for irrigation in the Morgan Hill area, California. U. S. Geol. Survey Water-supply Paper 400 *e*, 1917, pp. 84-86.

[11] L. J. Briggs and J. W. McLane: The moisture equivalent of soils. U. S. Dept. Agr. Bur. Soils Bull. 45, 1907, p. 22.

[12] L. J. Briggs and H. L. Shantz: The wilting coefficient for different plants and its indirect determination. U. S. Dept. Agr. Bur. Plant Industry Bull. 230, pp. 68 and 73.

[13] W. C. Mendenhall: The hydrology of San Bernardino Valley, California. U. S. Geol. Survey Water-supply Paper 142, 1905, pp. 56-65.

The water-table method, more than any other, gives information on the important question of the increase in the rate of recharge with increased use. In southern California the United States Geological Survey has maintained a series of measurements of water levels in 100 to 200 wells for 15 years, with the record for one well extending back 27 years. The records of these wells show that the depleted supplies in the ground-water reservoirs of that highly developed region were to an encouraging extent restored in recent wet seasons, the recharge in some places during these wet seasons being greater than would have been physically possible if the reservoirs had already been nearly full. It was found that the annual fluctuations were much less in Salinas Valley than in Santa Clara Valley. This fact, however, does not mean a smaller supply available for recharge, but, on the contrary, means that the supply in Salinas Valley is so abundant that the ground-water reservoir is in all seasons kept nearly full.

UNDERFLOW METHODS

The underflow methods consist of estimating the rate at which water is percolating through a selected cross-section. They involve three factors: (1) the area of the cross-section through which percolation is taking place, (2) the velocity of the percolating waters, and (3) the porosity, or, rather, perhaps, the specific yield, of the sediments. The greatest uncertainty is doubtless in the last factor.

The rate of movement of ground-water was formerly estimated from pumping tests and later by the use of salt or dyes, the salt being detected in the downstream wells by chemical tests. The more practical and reliable electrolytic method for measuring velocities was devised by Charles S. Slichter[14] in 1901.

The underflow methods have a rather restricted use for making determinations of recharge and safe yield. Except where the water percolates along a definite course, such as is afforded by the alluvium of a stream valley, the conditions are too complex for its successful application. Even in stream valleys it does not generally give maximum yield. For example, the deposit of coarse, clean sand and gravel that underlies the Arkansas Valley for hundreds of miles of its course is more important as a reservoir than as a channel for ground-water. If the water-table is lowered throughout this valley by heavy pumping, there will be recharge in every locality, by rainfall and by percolation from the river and from the porous deposits underlying the uplands adjacent to the valley. This

[14] C. S. Slichter: Field measurements of the rate of movement of underground water. U. S. Geol. Survey Water-supply Paper 140, 1905.

widely distributed recharge will insure a large continuous yield, provided the pumping plants are widely and regularly distributed. The rate at which this body of water is creeping downstream—less than a mile in a year—has really little to do with the total recharge or safe yield.

Conclusion

Estimates of ground-water recharge do not, and probably can not, approach in accuracy the measurements of surface streams. The estimates that have been made, though doubtless of much practical value, express degrees of magnitude rather than definite quantities.

There are, however, two very encouraging features of this work: The first is that we have methods that are fairly dependable and applicable. The work of the future is to refine these methods and to apply them in sufficient detail, rather than to devise new ones. The two lines along which additional research are most needed relate to the habits of phreatophytes and their rates of transpiration and to the specific yields of different kinds of rocks. The second encouraging feature is that the three main methods—intake, discharge, and water-table—are absolutely independent of one another. In many areas two, or even all three, of these methods can be applied, and in this way checks can be obtained on the accuracy of the work. Thus, the discharge investigations in Owens Valley were checked by the intake method, and thus at the present time two entirely independent sets of observations to determine recharge are being made in Santa Clara Valley—one on water-table fluctuations and the other on seepage losses of streams.

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CONTENTS

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BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
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THE TEACHING OF HISTORICAL GEOLOGY AS A FACTOR CONDITIONING RESEARCH[1]

BY JOHN C. MERRIAM

(Presented before the Society December 31, 1919)

CONTENTS

Function of the educational Institution in Development of Research

Discussion of the teaching of any research subject involves preliminary consideration of the position of the educational institution in relation to that of other agencies concerned with the advancement of research. Viewed from the standpoint of one surveying the work of investigation as now expressed in this country, we may divide our greater research agencies tentatively into five groups.

Without assuming to present a complete or exact classification, these are: (1) research of practical application in engineering laboratories; (2) governmental bureaus and laboratories; (3) research foundations; (4) museums and allied institutions; (5) educational institutions. To these five a complete statement would add several of lesser magnitude, among which a very potent force is found in effort of individuals working privately, as has been done to the great advantage of science by many pioneers in investigation.

In order to make clear the position of educational institutions with relation to the other four kinds of research agencies, it is necessary to give an approximate definition of each type.

[1] Manuscript received by the Secretary of the Society August 12, 1920.
This paper is one of a series composing a symposium on the teaching of geology and paleontology.

(1) The expression of research referred to as "practical application in engineering laboratories" includes use of science in development of economic interests in the great variety of ways in which investigation contributes to the good of mankind. The words "engineer" and "science" are here used in the widest sense, covering the appliers of knowledge secured by investigation. The operations of this group might be illustrated by the constructor of railways, the builder of aeroplanes, or the dentist. The work of the engineer in all of the fields in which he operates may, unfortunately, be carried on by rule-of-thumb application without consideration of the special merits of each case. The true engineer we all recognize as one who views each problem as a new subject for special study. In a large measure, his judgment must be based upon previous experience with similar studies, but his greatest success comes through realization of the fact that each bridge to be built, whether it be intended to cross a river or only to reach from one tooth to another, presents a special problem not identical with any previously considered case, and that failure to see the individual peculiarities may mean inability to make full use of the principles which are his instruments. The successful engineer is continuously engaged in the application of research methods.

In a still larger sense does the engineer concern himself with research problems by consideration of questions which are not merely specific applications, but involve principles which must be better understood before he is able to proceed. The dentist recognizes that knowledge of microscopic structure of the tooth is of fundamental importance in his treatment of tissues, if this work is to have value in a degree of permanence measured in years or tens of years. The railroad builder realizes that not all rock foundations give real stability to a railway bed, and that an understanding of the material through which he cuts may determine the ultimate value of his constructive work. These investigations in engineering inquiry we often designate as research in applied science. They differ from those in so-called pure science only in the fact that the research of the engineer is specifically directed, and by nature of the inquiry is rather narrowly limited, whereas the real solution of the problem may lie in a rather remote field. The railway builder may find the answer to his engineering questions in special phases of chemistry or petrography which were not included in the curriculum of his training course.

Even with the limitations which are set in investigations designed to meet specific needs in restricted fields of applied science, we must recognize that the every-day operations of great laboratories conducted by far-seeing corporations are developing some of the most significant advances in fundamental science of today. The student of pure science must always keep in close contact with these special researches, both to be helpful and to receive from the engineer the great wealth of data which should be incorporated into the organized body of fundamental science.

(2) Government institutions, as exemplified by the federal bureaus and laboratories of the United States, represent a field which is in some respects intermediate between that of engineers who apply and that of the special students of pure science concerned only with the principles of their subjects. The laboratories of government departments exist for the special purpose of contributing for the benefit of the community. It is necessary that they serve as sources of information for practical applications and for interpretation of the principles of science to the great group of inquiring engineers throughout the country.

Consideration of scientific problems relating to specific community needs leads the government bureau to undertake far-reaching and fundamental investigations in the broadest fields of applied science. Such researches, by reason of the wide range of interests covered, may extend farther than the studies of the engineer or the corporation. As institutions which stand for a continuing people, the government bureaus should be able to undertake inquiries from which results might first become available to later generations. It is unfortunate that budget requirements and responsibilities of political parties tend to limit us in handling of projects which should be continued for long periods or with large funds, for the expenditure of which immediate returns may not be visible. It is presumably true that all science has its application in one form or another, but exceptional vision is required in organization of government work to make it clear that every phase of each investigation undertaken represents efficient application of science for real needs. By reason of its practical limitations, the government organization may lose opportunity for consideration of certain critical problems the settlement of which would ultimately be of great advantage to the state.

(3) Research foundations, with ample resources, freedom of choice in selection of objectives, and with trained men of vision directing their researches, have given opportunity not otherwise available for exhaustive investigation of fundamental problems and groups of problems without regard to the time required in the study and without reference to immediacy of pressure for application. These institutions have in some measure covered the fields for basic investigation which the corporation engi-

neer and the government bureau could not readily reach. The efficiency attained by these foundations, the vision with which their problems have been selected, and the great contributions which they have made to science, to human thought, and to application of science in every-day life rank among the greatest achievements of American science.

(4) The great museums of America have been strongholds of research in the natural sciences. Their function has generally involved the special study of wide or narrow geographic regions to which they are related through circumstances governing their origin. The museums have also served a most important purpose as educators in natural history, supplementing in a vital way the work of the schools and universities. Through interpretation of science to the great public, the museums have greatly assisted in the effort to make knowledge and reason the basis of our community judgment and to give research the fullest opportunity to serve the people.

In organization of purely research projects the museums have contributed a large share of the material upon which the advance of American natural history has been based.

The work of these institutions is in general characterized by peculiarly close relation to public welfare, both in effective educational work and in the support of fundamental investigations for the sake of their human interest. The museums fill a most important place in the scheme of our research development.

(5) The educational institutions of America, as represented by the universities and colleges, have always had a large place in the advance of knowledge in all its phases and in its application. Their range of operation in constructive scholarship has been as wide as the limits of learning and its use.

In schools of engineering and agriculture, research has been largely on specific problems of application not differing from those of the engineer's laboratory or the government bureau. Here, as in the departments of fundamental science, the researches have also ranged into all phases of description, organization, interpretation, and analysis in special phases of science for which no immediate application is considered. These activities have been financed in some part by the universities and in part from the pockets of the professors. Considerable support has also come from business interests, from government institutions, and from research foundations.

The university or college includes constructive work as a necessary part of its regular program for at least four reasons, which may be stated as follows:

(*a*) Investigation is an indispensable means of keeping the faculty in a position to present the most fundamental and most advanced knowledge through its teaching.

(*b*) Training in creative or constructive work is one of the most important phases of teaching and can be carried out successfully only through actual experience of the student.

(*c*) The state will naturally depend upon the institution of higher learning as an exceptionally organized group of constructive experts prepared to consider urgent questions requiring investigation.

(*d*) As a body representing a wide range of closely interlocking subjects having continuous relation to research in one form or another, the university affords unusual opportunity for correlation of knowledge on questions in new fields of thought.

In considering the first reason (*a*) we must realize that, even if the universities be assumed to exist only for teaching, they are expected to present the most advanced thought, and we can not keep them in a position of leadership in understanding and in training without a faculty continuously setting forth the best in thought and experience in every subject. This condition can be maintained either by continuous research on the part of the faculty or by continuous renewing of the membership of the faculty. Continuous replacement of individuals is impossible, as the institution is a great and complex instrument in which the parts can be kept in proper adjustment only through long contact. It therefore becomes necessary for the faculty to keep its position by continuous growth of its members. If this process is merely imitative, the teacher is not an authority. The only way in which he can be assured of growth is by working in his specialty. This constructive operation involves intimate knowledge of the fundamentals of his subject and definition of the limits and relationships of his chosen field of study.

More than this, the function of teaching in an educational institution does not concern alone the retailing of facts already assembled; it must include that kind of understanding of the subject which will prepare the student for his task as a leader in the future. To become such a leader the student must look beyond our present knowledge and experience with the expectation of accomplishing things which have never before been done. No good instructor can avoid recognizing this need of his students. No teacher who sees this requirement can fail to make a serious effort to determine the direction of advance in constructive use of his subject, if for nothing more than to point out to students the trend of the path and the preparation necessary for those by whom it will be extended to new fields of usefulness. It is hardly possible for the instructor to obtain a clear view of future development in his subject without intimate personal relation to the most advanced work in progress.

From the point of view of the student, training in constructive work or in development of creative imagination, suggested in point (*b*), must be considered of importance at least equal to the securing of information or the disciplining of the mind to habits of work. As in no other type of mental attitude, this involves the acquiring of a distinct love of the work and understanding of its purpose. It is not conceivable that the university will neglect this extraordinarily important aspect of the student's preparation for future activity, or that it will expect the student to proceed without guidance. If this particular phase of educational activity is not to be eliminated, it places upon the instructor the requirement that he stand before his students as an unmistakable representative of creative work and as illustrating in his personal attainment the end or purpose of his effort. Evidence of any other attitude on the part of the instructor will make useless whatever attempt he may make to serve as a leader or adviser in the field of constructive study.

The third contribution of value (*c*) furnished by research related to education concerns the immediate use of the results of this study by the community. While the university is naturally assumed to be primarily an educational institution, it has been made clear that without continuing research it can neither provide adequate instruction nor maintain its leadership in the educational work required. Constructive problems in all departments of investigation must be continuously the subject of successful handling, and the results of this work will be products of the first importance to the community. It is natural that to such an institution the whole people will look for the appearance of new ideas of broadest significance and of practical value. It is to be expected that the state will depend upon the university for information and will expect it to furnish the necessary knowledge and the constructive ability required in meeting new situations that make necessary the building of new plans of thought for community use. The contributions made by research in these institutions will generally tend to concern fundamental subjects and to group themselves on the more indefinite areas along the borders of knowledge, but it is frequently these broader principles which offer the largest opportunity for real addition to the sum of immediately useful information.

The fourth reason (*d*) for including research as a part of the necessary program of an institution of higher learning involves one of the distinguishing characteristics of the university. By reason of the extraordinary scope of interests represented in such a body, one might expect the unusual opportunity for contacts of investigators in related fields to produce new combinations of formulæ, and through these the opening of new fields of discovery. No other organization presents the same wide range of subjects represented by leaders of thought who are normally investigators. To these conditions the university adds an unusual freedom of opportunity for choice of materials or combination of materials to be used in investigations, as also the stimulating influence of a continuous stream of students with new inquiries and new ideas. In no other type of institution engaged in investigation are the chances greater for contribution in fields representing either new groupings of subjects or areas which have thus far remained untouched by the workers of all organized departments of knowledge.

For all of the reasons that have been presented, research has now an established place in institutions for higher learning. The position of constructive work in the universities is clearly not accidental, but relates to the generic characters of these institutions.

To the university, viewed as the highest training school, investigation becomes as necessary for natural activity as eating and assimilating are to continued effectiveness of the biological organism. The research so necessary to continuance of adequate instruction we come to recognize as a normal part of the life of the institution, and we look to this kind of an organization in the course of its growth to produce much of value in the forefront of discovery and construction.

The university fails of its mission in creative work in many instances because, of all the types of institutions, it is the most imperfectly financed for this phase of the work which it should naturally conduct. With the clear requirement that, to keep its position in the first line of advanced thought, it must consist of men of the best type in the professions, the university is often financed almost exclusively for teaching and administration without reference to research, and it is assumed that the constructive work so necessary to development of the faculty and students will be cared for in other ways. Beyond funds for purchase of books, departments with large salary rolls for instruction often show almost nothing for constructive work. The ultimate result of this policy must be failure to attain the full measure of efficiency. Under unfavorable conditions potential leaders in the faculty will either find support of their greatest contributions to knowledge outside the institution or, failing in this, they will burn out like a lamp producing feeble light by consuming a wick to which no oil is fed.

The university and college, then, take their places with other groups of research agencies of the country as institutions caring for the initial training of nearly all investigators, and particularly given to wide range of researches among a great variety of fundamental subjects. Their activities in constructive work will often run parallel with those of other kinds of organizations; but breadth of interest, wide range of contact, unusual freedom of relationship, and spontaneity will always be among their characteristics.

Functions of Teaching

Having defined in some measure the position of the educational institution with reference to research, one naturally inquires further concerning the more intimate relation of the methods of teaching to research in the institution. This discussion may be introduced by a brief analysis of the function of instruction as we may interpret it for the institution of higher learning.

The principal objects of teaching in a university or college, as understood by many of us, may be considered as of four kinds: (1) informational, (2) organization and presenting of perspective, (3) stimulative, (4) constructive.

(1) Informational teaching is a descriptive process concerning the transmission of facts which may or may not be available elsewhere. There are many cases in which the inadequacy of existing information and the position of the instructor as an expert justify the presentation of data which may not be obtained otherwise. The reiteration of statements already set down in text-books or in other literature easily available may in many cases be of importance because of the touch of personal relation in the presentation. On the other hand, a large part of the information transmitted in purely informational courses may be dangerous to the student, as it inhibits personal effort to go to the available sources for knowledge; and dangerous to the instructor, because it takes his time from other kinds of instruction which may be more valuable.

(2) Organizational instruction begins with masses of fact available and teaches their grouping and use as more or less complicated organized bodies. It represents in some respects a higher use of knowledge and involves a more difficult kind of presentation.

One of the important phases of instruction consists in that type of organization which permits one to see knowledge in the simplest form of statement, and at the same time to realize the infinity of detail and the position of details with reference to the larger principles involved. Not too many individuals recognize the importance of organization of information and the impossibility of effective use of knowledge without understanding the interrelation of its elements. Not a few individuals accumulate vast bodies of detailed information without being able ever to arrange them in such a way as to serve as other than an impediment to advance by reason of clogging of the machinery through the pressure of disorganized and unrelated materials. Others see each subject in its simplest form, and may serve as guides to indicate the direction in which larger movements will go, but are limited in the use of their judgment by reason of lack of relation between general principles and the details through which the principles must in their application be ultimately expressed. One fundamental principle of university instruction involves the larger organization which makes possible a view showing us knowledge perfect in the simplicity of the interlocking parts and infinite as to the details of expression.

(3) The second view of university instruction, which we have just discussed, gives the factors of discipline and order as the most fundamental and significant. Contrasted with this idea, we have the considerations of interest and pleasure in the inquiry into a given subject developed through stimulation in the statement of the instructor. While it may be true that many students are not worth while, by reason of their constitutional limitations in relation to the subject which they approach or with reference to the particular instructor with whom they come in contact, there can be no doubt that a great number of students also fail because of not having set before them, through the stimulation of clear presentation, the really fundamental ideas basic to subjects in which they might naturally be interested.

(4) Developing the power to do constructive or creative work constitutes the highest object of the student. The ability of the instructor to bring out this quality of mind is the greatest gift. The desire to construct generally arrives most readily through participation in constructive work, and involves the idea of research as a normal part of the program of the university. This view can be presented or suggested in realistic manner only by those who are engaged in actual research. In other words, the institution which fails to give proper opportunity to develop its faculty in constructive work fails to secure the results of highest type in its teaching.

In the statements that have been presented up to the present stage of this paper it has been my purpose to make clear that teaching, as expressed in colleges and universities, serves its highest purpose as training when it develops in the student the ability to see with a true perspective, and to use his subject as a means for constructive effort. At the same time it has become evident that the educational institution is not only the most important molding influence for all future investigators, but that it is itself one of the greatest types of creative institutions, occupying a position distinguished by breadth of interest and by opportunity for contribution to the basic elements of science. Considering its largest usefulness, there is every reason for holding that, whatever else may be included in a university program, the interests of the teacher, the taught, and the community demand that a knowledge of needs and objects for future constructive work and an understanding of the mode of operation of the creative mind shall always be dominant features.

Special Significance of Geology in Teaching Idea of Continuity in Research

What has been stated regarding the problem of the educational institution as a whole applies without amendment to the fields of instruction in geology in all of its phases. I have presented the problem in this general form in order that in further discussion we may more readily express the relation of the teaching of geology to that in other subjects. My desire is to emphasize the point that teaching conducted in such a way as to fail of stress on the constructive element expresses a condition less advanced than that of full leadership and promises for the future a result corresponding to what *has been* accomplished rather than what *should be* done. Geologists have always been a constructive group of scientists, and we may trust that in the further progress of educational work in this field the example of research activities of our distinguished predecessors may serve to establish the method for future advances.

In addition to consideration of the relation of teaching to research expressed in geology, along with all other phases of scientific inquiry, I wish to direct attention to a peculiar relation between the historical phase of geology and other aspects of this science, as also the relation of this aspect of geology to other sciences. It is stated only briefly as indicating an influence of the teaching of this subject upon the general development of research.

Great advances in knowledge are not infrequently made by sudden flashing up of genius opening the way into a new field. Some hold that these individual efforts, unrelated to other researches, represent the normal method of advance of science. Important as these individual contributions may be, careful study indicates that a very large part of the general forward movement is made possible by the fitting of moderate individual contributions into a larger scheme in which the relations of all the parts are known. Through an understanding of the continuity or interrelationship of the facts in the whole field of knowledge it becomes possible to obtain the widest meaning and largest value of each fact. The mind that sees these interrelationships—perhaps through the occasion of having brought a new element into the scheme—is the one that makes the greatest and most useful contributions. This idea of continuity is one which needs large cultivation in science. We may approach it from various directions. One of the most important and most effective means of coming to recognize it is by the historical view. History, interpreted in the truest sense, gives a feeling of broad connections in time and space and origin not excelled in any other field and equaled in few. I am convinced that the wider outlook given by this kind of contemplation is one of the most desirable fundamental elements in the education of a becoming investigator in any subject. We find historical geology giving the grandest of all strictly historical or continuity views. I am confident that the proper expression of this subject in the education program would exert an influence of wide extent contributing to development of a better understanding of the idea of continuity in science and greatly advancing the interests of research in all fields. I believe that a certain responsibility rests upon the geologist to see that this wider view is sought with increasing interest, and I feel assured that we have large opportunity to exert an influence upon the development of connected constructive thought in the whole education scheme of the great institutions of learning.

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
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STRUCTURAL AND PETROGRAPHIC GEOLOGY [1]

BY JAMES F. KEMP

(Read before the Society December 31, 1919)

CONTENTS

Examples of teaching Methods

EARLIER METHODS AS EXEMPLIFIED BY PROFESSOR AGASSIZ

In that priceless human document, "Autobiography of Nathaniel Southgate Shaler," the author tells us of his entry, in 1859, into the laboratory of Prof. Louis Agassiz and of the methods of instruction which then and there prevailed. Professor Agassiz gave him a rusty tin basin and seated him at a small pine table in front of a window in a greatly crowded room, 15 feet wide by 30 feet long. His neighbors were Alpheus Hyatt, F. W. Putnam, A. E. Verrill, E. S. Morse, Richard Wheatland, and Caleb Cook.

Professor Agassiz supplied him with a small fish, taken from a bottle of old alcohol, whose fragrance is described by our author as a "stench." To his inquiry, "What shall I do?" the Professor replied, "Find out what you can without damaging the specimen. When I think you have done the work I will question you." The new student was, furthermore, sternly bidden to talk with no one else concerning it, and not to read anything relating to fishes without permission. In an hour young Shaler considered himself prepared to report, but the Professor paid no atten-

[1] Manuscript received by the Secretary of the Society January 5, 1920.
This paper is one of a series composing a symposium on the teaching of geology and paleontology.

tion to him. Nor did the latter concern himself with his new pupil all that day, nor the next, nor for a full week.

Shaler rose to the occasion, understood the game, and buckled down to know that fish as he had never known anything before. On the seventh day the Professor came over and said, "Well?" Whereupon the student unburdened himself for an hour, receiving, however, in reply, the laconic sentence, "That is not right." Thereupon another week, ten hours a day, was spent in the study of the fish, with results which surprised even the student and satisfied the master. There came, however, no word of praise or approval, but the fish was replaced with half a peck of disjointed fish bones, which the student discovered because the six pairs of jaws represented a half dozen skeletons. The reassembling of the parts occupied the youthful Shaler for two months, and in the end gave him a sense of mastery and power, with which he passed on to more extended reading and more varied work.

The method of instruction just outlined is severe and furnishes a novitiate under which Shalers, Hyatts, Putnams, Verrills, and Morses will stand up. It makes a wonderful appeal to initiative and independence, but of course we realize that it would not do for general classes. I was once in a position to observe a professor who was teaching zoology keep a class of college juniors the entire winter term studying drawing, dissecting, and sectioning the common hard clam. The months passed and these future lawyers, teachers, doctors, business men, and clergymen surely knew, with great detail and accuracy, the mantle, the sinus, the big liver, the beaks, the cardinal processes, the hinge-line, etcetera, of *Venus mercenaria;* but of protozoans, cœlenterates, arthropods, reptiles, birds, mammals; of evolution, development, ancestry, and all the great questions regarding life on the earth, they heard nothing. Their horizon was bounded by a quohog, and a rebellious and disgusted group of young people shook the dust of zoology off their feet, never to return to it.

THE MODERN METHOD

We owe our classes a review and general survey of our subject at the outset. A lecture course, rightly given, is a short cut to the experience of generations. Of course, fundamentals *must* be learned. No one can read who does not know his letters. Technique *must* be taught and good methods of observing and recording. No young man should start in today's stage of geology as if he were the first geologist who ever worked. In 1884, in trying to study microscopic petrography by myself, I recall spending weeks floundering around with interference figures and tests with the mica plate; whereas, if I could have appealed to an experienced worker, I could have learned in half an hour what I could do and what I could not do with the haphazard slices of a thin-section. I might then have pushed on to something else.

We teachers may well give careful and thorough instructions at the outset. The instruction should be conveyed with the clearness and conciseness and, so far as possible, with the elegance of a well expressed mathematical demonstration, inculcating thereby precise habits of thought. But when once we have grounded our beginners and have given them a comprehensive survey of the field, we can hold them reasonably and justly to more independent work.

Having established these premises, it is of certain features of more advanced instruction that I desire especially to speak. In the law the student is now taught the fundamental principles, and then carried farther in large part by the "case system"—that is, the professor assigns the student a case, exactly as it would come up in practice. The student consults the records and prepares his brief, which is then tried out before the critical eye of the teacher and before the others of the class. In medicine the student is not alone trained in the laboratory and lecture-room, but attends clinics, in which he sees operations performed by the most experienced and skillful surgeons or treatment given by the older men, expert in diagnosis.

Fifteen years ago, with mining students in their last year and with graduate students looking forward to future work in geology, I was moved to develop a little course on the lines of the "case system" of the law schools and the clinics of the schools of medicine—that is, from personal experience in the field and with the problems which confront the civil and the mining engineers, and from many conversations and discussions with friends in active practice, I marshaled in order a series of "cases," enough to fill two hours a week for a term. I endeavored to set the stage for each case as nearly in a way true to life as I could, and to faithfully place before the students the situation as it had confronted the geologist or engineers.

We took up folds and their bearings on many areal and mining problems—not alone the usual and typical cases of the text-books, but the unexpected things which underground work sometimes reveals and which may be very confusing. We passed to faults and raised the questions how were they solved—where and by whom. What did the responsible man do and how did he come out? Some of the most interesting questions in geology hinge on these remarkable displacements; and while they sometimes behave as the ordinary rules in the text-books prescribe, they also often confront us in underground work with surprises which only experience reveals. There are cases of great interest involving joints, dikes, sheets, the chilled border of intrusives, the contact zones, the lenticular form of sediments and of ore bodies, the behavior of underground waters, and many other subjects.

The course was an endeavor to connect up geology with life, and, I may truthfully add, it aroused interest among the members of the class in an unexpected degree. Discussions among themselves and with the instructor followed without end, and the young engineers or future geologists went out into the practice of their profession not entirely as if into an unexplored and uncharted wilderness. We may not unreasonably hope that even those who entered the engineering professions retained a goodly proportion of the true scientific spirit, since it is no discredit to any branch of scientific work to be useful. Accuracy and care in observation and soundness and skill in interpretation are demanded by both.

PETROGRAPHIC INTERPRETATION AS TAUGHT BY PROFESSOR BERKEY

The use of the word interpretation brings up another line of instruction and discussion, which has been developed in an impressively successful way by Prof. Charles P. Berkey, for whom, on account of enforced absence in the southwest, I am substituting in this symposium. Professor Berkey has developed a course which deals with problems in petrography. Now, the matter of interpretation is one of the most important parts of the training of investigators after they have passed their novitiate in a subject. Dr. Berkey has applied this principle in carrying his students in petrography a stage beyond the systematic exposition of rocks as covered in the usual course. Students in this branch must necessarily be trained on good, fresh typical specimens and sections, and must thereby establish in their minds types and standards. Later on, however, when they go into the field and are confronted with weathered, altered, or recrystallized exposures, they have to deal with rocks which are entirely different; yet they must determine and interpret such rocks, whether they write a scientific paper or apply what they have learned in the service of an employer. Wall rocks of ore bodies, for example, are seldom in a fresh condition and are usually so altered by thermal or other processes as to be very different things from the specimens and sections studied in the petrographic laboratory. A young observer, even though carefully trained, may find himself face to face with obscure and difficult problems, of whose very existence he had not previously known.

Moreover, ores are extremely interesting and fruitful subjects for microscopic study. It may be of vital importance, in connection with development in depth, to know whether an ore is original and primary or whether it is the product of secondary enrichment; whether, in other words, it may continue far down in the earth or whether it will change in character a relatively short distance below the surface. Two ores, chalcocite and argentite, are of extreme interest in this respect and both have from time to time called for interpretation.

Dr. Berkey has thus developed a branch of work called "Problems in applied petrography," and has now no less than 150 actual cases which he may pass out to his students with the same data and in exactly the same form as they have come to him or to me or to a few of our friends in the field. After a student has studied, thought over, and interpreted his case, his results are often discussed for the benefit of the others in the class, and are then checked up by the work of older observers on the same material. The interest aroused in young, vigorous, and inquiring minds is impressive and the line of attack has proved to be one of exceptionally stimulating character. A successful teacher, indeed, must have not only an inquiring and investigating mind himself, but if he maintains good and inspiring relations with his students, he must make them sharers in his work and experiences. This relation is the one with which to follow up fundamental instruction if we are to train investigators.

Importance of full personal References to the Work of prominent Geologists

There is one other side which I believe to be also very important. A teacher should speak often, freely, and appreciatively of the men who have done the geological work in the past and who are doing it today. The personal appeal of actual service well performed is very strong, and when a subject, such, for instance, as the inspiring theme of Lake Superior geology, is discussed with an advanced class, it is well to tell the young people of the men who deciphered it; when and how they came into the field, and what they each accomplished. It was rough work. There was often thick brush; there were old lumber jobs burned over; second growth almost impenetrable; swamps, black flies, gnats, and mosquitoes.

Not every one knows, as I learned once when talking with our lamented fellow-member, President Van Hise, that after his first summer's work he swung himself on the train for home and said to himself, "Thank Heaven, I never will have to go back into that country again." Hardships seem easier if the beginner knows that he is only the last member added to a long and honorable procession. Books such as "Shaler's autobiography"; the "Life and letters of Josiah Dwight Whitney"; "Life and letters of Peter and Susan Lesley"; "Life of James Dwight Dana," and the memorial volume issued to the finest spirit produced by American geology, Clarence King, by his fellow-members in the Century Association in New York. Latest of all, the incomparable "Reminiscences" of our venerable past President, Raphael Pumpelly, whom I describe as venerable, although I well know that he has partaken of the spring of eternal youth.

These works are no less important in a course which will stimulate investigators than is the last word in magmatic differentiation, in metamorphism, or in the isostatic equilibrium of the earth.

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
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TEACHING OF GEOLOGY AS RELATED TO RESEARCH[1]

BY HERBERT E. GREGORY

(Read before the Society December 31, 1919)

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Introduction

During the past three years it has fallen to my lot as a member of the Committee on Geology and Geography and of the Committee on Educational Relations of the National Research Council to make a study of the strength of the present geological brotherhood and of the ability of educational institutions to supply the needed recruits.

Discussion of the fundamental Facts

This examination of the status of geology in America reveals two fundamental facts:

1. A surprisingly small number of colleges and universities offer courses in geology.

2. A relatively low value is placed on geology, both as an educational factor and as a practical contribution, by college presidents, deans and faculty; by men of affairs; by Army officers; and by civil engineers.

Statistics and comments supporting these conclusions were given at the Baltimore meeting[2] and need not be repeated. They do not compose an alluring picture. The position of geology in college and university curricula is relatively less favorable in 1920 than in 1900 or even in 1890,

[1] Manuscript received by the Secretary of the Society October 6, 1920.
This paper is one of a series composing a symposium on the teaching of geology and paleontology. It is a careful condensation of the paper as presented at the meeting.
[2] Bull. Geol. Soc. Am., vol. 30, 1920, pp. 81-82.

and in a number of institutions is absolutely weaker, as measured by size of faculty or by number of students or by both. Some fifteen institutions in which biology, physics, and chemistry are well thought of have eliminated geology from the course of study or reduced it to harmless proportions since 1870. Geology has disappeared almost entirely from secondary schools and physical geography is getting up speed to follow. The history of geology as a teaching subject is more like astronomy—once universally taught, now almost absent from schools of all grades—than like biology or chemistry, which have shown a consistent growth. The appeal to geology is now primarily for economic contributions and of late mainly for assistance in making money out of oil. This growing appreciation of the subject is gratifying and should be encouraged, but the relatively low valuation given science as a means of training minds and of teaching essential truths is worthy of serious attention. The present conditions are favorable for strengthening the position of geology; the attitude of investors and of business men is sympathetic; and the war, with the consequent unsettled condition of the world, has increased faith in science as a whole.

Discussion of the Functions of Geology

The function of geology may be viewed from three standpoints:

1. Should a knowledge of geology be the possession of the few—the well preserved treasure of a selected group of men and women who write papers to be read within a restricted circle; whose reward consists of the approval or envy of colleagues trained to understand the intricacies of a particular subdivision of the subject? If so, geology in America has been successful.

2. Is the essential function of geology to find new truths and to restate old truths which may or may not bear on the happiness and welfare of man? If so, we have succeeded, for only a few choice minds are necessary to develop theories, and it is probable that there are now within the United States a half dozen abundantly capable geologists to one in the days of Dana, Hitchcock, and Rogers. But the great development of research institutions and the setting aside of men for special investigations has taken place since 1890. Relatively, geological research has not kept pace with the advance of other sciences.

3. Is geology a medium of thought, a method of training minds for use in any activity—a body of fact and principles and methods, acquaintance with which will raise the level of the intellectual life of a community? In other words, has its teaching a significance which justifies its incorporation in school and college courses? In academic parlance, is it

an "essential subject," like history and English and mathematics, or a "subsidiary subject," like Russian, oriental history, and archeology? Those who regard geology as a luxury or "side issue"—as a mildly desirable feature of a college course—will find satisfaction in the study of college catalogues. That educational authorities take this view is shown by the facts that about one-half of 1 per cent of high-school students are studying geology; that 43 per cent of 512 colleges and universities, including all the strong institutions, offer no geology; that in 173 of the 294 colleges which include geology in their curriculum the subject is taught by instructors primarily interested in other subjects; that in 48 institutions, including some whose enrolment is numbered in thousands, the geological faculty consists of one man. In only two colleges and universities of which I have knowledge geology ranks in the minds of the faculty with physics, chemistry, and biology.

The Future of Geology in the educational Field

Those of us who believe that geology is not only an entrance to unexplored fields and a means of developing the world's material resources, but that it also has a message for all thoughtful people, are not satisfied with the position in which our chosen subject finds itself. Research is essential to the life of geology and should be financially supported and encouraged in every possible other way. In my opinion, the support most urgently needed is to be furnished by young men of ability who will devote their life to science. Without funds, research is hampered, but without trained men it ceases to exist. Teaching, therefore, becomes perhaps the most significant factor in conditioning research, and it deserves the serious attention of all those interested in the intellectual welfare of the country.

To geologists outside as well as inside educational circles, the number of men studying geology and the amount and quality of the instruction received is a matter of primary importance. From a continuous supply of well taught students the exceptional investigator comes.

An examination of the present position of geology in colleges, government institutions, and in the public mind suggests ways for increasing and improving the crop of students who are to supply future geologic needs.

A. In educational institutions considerable improvement is possible.

1. It is not improbable that colleges and engineering schools without geology may see their way to including this subject in the curriculum if the significance of its teachings is properly presented by administrative

officers and faculty, by interested individuals, educational associations, and scientific bodies.

2. In colleges where geology is now taught systematic effort should result in securing more time for the subject. The curriculum allowance for field and laboratory work in geology is commonly much less than that considered necessary for chemistry and biology.

3. Change in the method of teaching elementary geology would, I believe, place the subject in a stronger position. In most institutions it is now placed with "informational" or "descriptive" courses, which are weighted lightly by committees on courses of study; but the subject is admirably adapted for exercises in logical presentation, formulation of hypotheses, and constructive criticism. In my opinion, the method now in vogue in many institutions—nomenclature and facts from a text-book, supplemented by more facts presented through lantern slides—needs thorough revision. The newer text-books bring facts and established principles up to date and include new illustrations and diagrams, but in method they are substantially like those of a half century previous. It is a mistake to assume that college undergraduates are unable or indisposed to work on problems.

4. To an undesirable extent, the graduate student is treated as an advanced undergraduate, who selects defined courses and whose attainments are measured in standards of time and recorded marks. Emphasis on information in lectures and a failure to distinguish "disciplinary" from "mechanical" in laboratory exercises is discouraging to originality and initiative.

5. Cooperation among universities, whereby each institution should emphasize some particular branch of the subject and encourage interchange of students, affords obvious advantages to the young man in training for a professional career. There is a need also of a carefully organized field school of geology, attendance at which should be required of all students who intend to devote their lives to this subject. Such a school, I believe, could be adequately financed and efficiently managed by a group of strong institutions in combination with the United States Geological Survey and the State surveys.

B. The public should be given a better understanding of the purpose and scope of geology.

1. There is a large field for geological literature designed for the intelligent layman. For the public the popular newspapers and magazines replace lectures, laboratory work, and printed transactions. The widely recognized significance of geologic truths creates an atmosphere favorable

for geological research. To teach the public is as necessary as to teach the selected group. This popular education has proceeded far in medicine and in chemistry, with the result that funds and personnel for research are relatively easy to obtain. In geology the impression seems to prevail that the preparation of an interesting article for the press, or of an elementary text-book, is a task for inferior minds.

2. The attitude of the public toward the teacher is unfavorable for the development of research workers. As an aid in procuring a larger and better crop of scientists, geologists are justified in magnifying the value of the teacher. It seems somewhat incongruous that honorary degrees and membership in learned societies are denied to the man whose chief qualification is eminence in teaching.

3. More men and better men would doubtless choose geology for a career if the idea were prevalent that interpretating the history of the earth was a field of activity which justified the use of the full powers of the best-trained minds. Many men prefer "service" to "wealth," and are attracted by congenial though difficult tasks, provided those tasks appear likely to contribute to the welfare of their fellow-men. When geology is commonly looked upon as a "man's job" the recruits will not be wanting.

C. Geological societies may advance research by giving thought to the improvement of teaching. The Geological Society of America was founded for "the promotion of the science of geology in North America," but the training of geological workers appears to have been considered outside the scope of its interests. Proposals that the teaching of geology receive some attention at our annual meetings found little favor with the program committee until the war had revealed the insufficiency of workers and the apathetic attitude of the public. With the belief that teaching of geology is an important conditioning factor in research, I am recommending to the Society the appointment of a standing Committee on Geological Instruction.

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
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AMERICAN PALEONTOLOGISTS AND THE IMMEDIATE FUTURE OF PALEONTOLOGY[1]

BY CHARLES SCHUCHERT

(Read before the Society December 31, 1919)

It is well that we take stock of the men and women in America who are interested in paleontology, to see how many there are of us, what we are doing in spreading the knowledge of our subject, and how well this is being done. As to this personnel, we are greatly aided by a study of the membership of the Paleontological Society, taking the list of 1918. The Society then had 190 members, and this number probably includes all of the American paleontologists but three, and these three, curiously, are actively interested in the science. The list shows 12 members in Canada, 1 in Mexico, and 177 in the United States. Of the 184 living members, about 93 are connected with teaching institutions, about 60 with geological surveys and museums, and about 31 are outside of either of these two groups. From these figures we learn that there are but few actual amateurs, meaning by this term those who are interested in paleontology, but are not directly connected with some institution of a geologic, paleontologic, or museum nature.

Now, let us look into this membership in a totally different way and see how many of the 184 are actually working paleontologists. By a working paleontologist the speaker would understand, in this connection, one whose livelihood is more or less concerned with paleontological work and who either teaches the subject, does research work in it, or is employed in husbanding the collections of a museum or survey from the scientific standpoint. You will see, therefore, that all geologists are excluded, even though they are interested in paleontology and make use of fossils through a paleontologist, and also all workers with fossils in surveys, museums, and universities who do nothing in paleontology from the

[1] Manuscript received by the Secretary of the Society February 2, 1920.
This paper is one of a series composing a symposium on the teaching of geology and paleontology.

standpoint of publishing scientific papers. With these limitations, we may say that there are about 112 working paleontologists in North America, including the three who are not members of the Society. Of these, 8 are women—1 Canadian and 7 Americans—all of whom are interested in invertebrate fossils and 4 of whom are actively concerned in research publication. Of the total 112, 7 are Canadians, all of them invertebratists, and the remaining 105 are living in the United States. From these figures it is apparent that Canada is backward in paleontology.

Of the 112 more or less active American paleontologists, 74 are concerned with invertebrate fossils and stratigraphy, 30 are vertebratists, and 8 are paleobotanists. We should ask ourselves next how many of these workers are actually and constantly forwarding our science by original research, with results seeing the light of day through the printing-press, thus eliminating the sporadic workers and also those whose work is not of a constructive kind. The speaker fully realizes that into such a verdict personal opinion enters largely, and with this understanding he would say that, even though his viewpoint seems to him liberal, there are but 37 invertebratists, 17 vertebratists, and 4 paleobotanists who meet these qualifications. In other words, there are at best but 58 leading American paleontologists.

After an analysis of the visible results of the three divisions, we must accord the first place to the vertebratists as being in the healthiest condition scientifically. This division, with its 17 active workers, well balanced with young, middle-aged, and older men, has a coherence and an esprit de corps that are contagious when one gets among them. If there is a weakness anywhere, it is the fact that in all of Canada there is not at present a vertebrate paleontologist. It is true that the science of vertebrate paleontology can be successfully taught in but few places, because of the extraordinary expense involved in getting the necessary material for demonstration and study; and yet it is more or less successfully taught today at Columbia, Yale, Princeton, California, Michigan, and Chicago universities; and let us hope that in the near future it may be taught also at McGill and, as it surely will be, at Toronto.

Let us next see where the vertebratists are located. Not a single one is *actually* at work on either a national or State survey; and this anomaly clearly needs rectification. In our universities there are 17 vertebratists, using the term now in its broadest sense; 15 are at work in museums, and 12 of these are either in the American Museum of Natural History, the Carnegie Museum, or the United States National Museum. As there are but 7 invertebratists in all American museums, however, contrasted with the 15 vertebratists, we see illustrated here, what is so well known, that museum administrators and the public are far more interested in an elephant and a dinosaur than in an oyster or a coral.

The output in publication of the vertebrate workers is large and of the greatest import. It is, moreover, varied in its relation to paleozoology, zoology, evolution, and stratigraphy. In spreading their knowledge they are wonderfully active in presenting it, not only in the way of scientific publications, but even more so in museum exhibitions and in popular books that are "good sellers." Think of the wonderful vertebrate exhibitions of the American Museum of Natural History, the United States National Museum, Yale University, the Carnegie Museum, and the Los Angeles Museum; and of such readable books as "Men of the Old Stone Age," "The Age of Mammals," "The Origin and Evolution of Life," "A History of Land Mammals in the Western Hemisphere," "Animals of the Past," "Animals before Man in North America," "Organic Evolution," "Water Reptiles of the Past and Present," and "American Permian Vertebrates." The upwelling of American vertebrate paleontology came with the great pioneers, Joseph Leidy, Edward Drinker Cope, and Othniel Charles Marsh. Today the spirit of their work is perpetuated mainly by Henry Fairfield Osborn, William Berryman Scott, William Diller Matthew, John Campbell Merriam, and Richard Swann Lull. And is there anywhere in the world a better or more united band than that of the American Museum, in cooperation with Columbia University? This is an eminently constructive group, one for all of us to "sit up and take notice" of, and to strive not only to emulate but to surpass. A healthy rivalry is what is needed in paleontology; for, after all, in this world, based as it is on the struggle for existence, the golden nuggets of paleontologic progress will be won only through friendly interaction.

Next allow me to diagnose the condition of the paleobotanists. With Wieland, we agree that "what paleobotany most needs is men. The dearth of men conversant with fossil plants, not merely in America, but taking the world over, is to be deplored." The American clan numbers only 8, and, what is most important, but 4 of these can be said to be active. Think of the further fact that in all of our American and Canadian universities, and some of them are greatly endowed, there is but one paleobotanist who can be said to be teaching the science successfully. To be sure, the problem is to a certain extent a practical one, for after a student has been trained he expects to be placed so that he can make a living, and it is true that not many paleobotanists can be employed as such on our national surveys and in our universities. Think, however, of the many institutions that teach historical geology; and what more natural than that paleontologists or those trained in paleontology and stratigraphy should be the teachers of this subject? The income so necessary for life can therefore be made out of a related subject, enabling at least some of these teachers to do research in one of the divisions of paleontology.

In all of Canada there has long been no active paleobotanist, and the coal fields of the Dominion are calling for at least two such workers. However, we seem to see one bud that will soon break forth into productivity, and let us hope that he may be a worthy follower of Sir William Dawson, who did so much to develop not only the paleobotany, but even more the geology and paleontology of eastern Canada. In our own country no new man has entered paleobotany in a dozen years. We know, however, two budding paleobotanists, and are glad to note that their elders say both give promise of much good work.

Another bad feature is that almost the whole output of paleobotanic study nowadays relates to Mesozoic and Cenozoic times, since the only leading worker on Paleozoic plants has, through the force of circumstances, gone into administration. What a loss this is for American paleobotany and stratigraphy! And all the more so since the European leaders in Paleozoic botany are so few and so rapidly going on to their reward. We pray that the United States Geological Survey will return this man to the field which he has so eminently made his own.

There should in the near future be found places for at least six additional paleobotanists—two in the Geological Survey of Canada, at least one at some Canadian university, at least one on the newly organized State survey of Pennsylvania, and two more in the universities of this country. Then, and not until then, can we say that American paleobotany is in a healthy state. But, even if our hopes are realized, the science of paleobotany will not properly and completely function until there be more than the single worker who looks at fossil plants from the structural side. Paleobotanists, like invertebrate paleontologists, are too much the followers of the scientific fashion of the day, devotees of the time value of fossils.

American paleobotanists, let me add, have not yet taken up seriously the great desideratum of popularizing their science, as can also be said of the invertebrate workers. We need books for the intellectual layman—and by layman here is meant not only the educated person, but the paleontologist and geologist as well—setting forth in easy language and in interesting style the grander features of the extraordinary plant worlds of the later Paleozoic and the earlier Mesozoic. What more interesting or stranger flora has there ever been than that of the Coal Period?

In imagination, the ancient floras should be presented as if living, associated with the animals of their times and contrasted with those of today. The modern faunas and floras would then stand out in their marvelous progression. We understand that one such book is in the works, and that we are to have from Doctor Wieland a volume presenting the cycad floras in all of their splendor. In the same way, the invertebrate workers should popularize their studies, and here historical geology is an inviting field. Think of what could be done with trilobites and eurypterids, the wonders of cephalopod development, the corals and coral reefs of geologic time, the peopling of the seas and oceans, the ancient climates as indicated by the fossils and sediments, and the rise and decline of animals in geologic time. Book publishers nowadays are on the lookout for these popular books, one of which has gone into thousands of copies. A more admired monument a paleontologist can not build for himself.

Now, to come to the condition of American invertebrate paleontology, we count 74 devotees in this division of our science, 8 of whom are women; in fact, no other division has "lady paleontologists." At present, Mexico has no visible student of fossils of any kind, and one of the two it did have is now living in this country, working privately as a petroleum geologist. In Canada there are 7 paleontologists, all invertebratists. Of the 74 invertebrate workers, 71 are in the Paleontological Society; but a close analysis shows that only 37 of them are leaders in the sense of our previous definition of the word. Of the 74, 30 are located in colleges or universities, 14 on national and 7 on State surveys, 7 in museums, and the remainder, 16 in number, are following paleontology more or less as a hobby. In this last class, however, we note several distinguished names.

The first glaring fact that needs to be pointed out, and it is a discomfiting one, is that we invertebrate paleontologists are fashionable almost to the last man, in that about 90 per cent of our work is of a chronogenetic character. In other words, almost all of our recent research has in view primarily the discerning of the development of the earth's varying surface phenomena. We are, therefore, almost wholly dominated by the geologic aspect of our science. Twenty-five years ago we prided ourselves on having an active school deeply interested in ontogenesis and phylogenesis, with at least a half dozen workers clustered around Alpheus Hyatt. He and Charles E. Beecher have gone on, four of their students have almost completely gone over to the chronogenetic field, and one is struggling alone, having neither the assistance of an institution to father his work nor the adequate sympathy of his associates. It is true that in our American universities there is no place as yet for pure paleontology, unless it be of the spectacular type; nor is there as yet adequate recognition of paleontology of any kind in our universities, and the reason for this will be pointed out later. At the present moment, however, I would like to drive this fact deep into the minds of American invertebrate paleontologists, that our work is lopsided, and that the splendid realm of pure paleontology, biogenesis as studied from the fossil evidence, is almost completely neglected. We all know that paleontology is the biology of all time, while botany and zoology are but the studies of the organic terminals of some of the life that has lived through a thousand million years. The theory of evolution was made possible through the chronogenetic record as discerned by the paleontologists and through the phylogenies of living plants and animals. Nor can the distribution of modern floras and faunas be determined without a study of the life of the past, or the rate or speed of evolution and the origin of many organic trends be understood without a knowledge of the climates of the past. The fact that American invertebratists and paleobotanists are doing so little along these lines is a blot on our science—all the more so because of the good example set us by Hyatt and his school, and by the vertebratists. There is no continent with a better array of well preserved fossils or a longer geologic sequence of them. Let me, therefore, ask you young men of today, which of you will become the future high priests of biogenesis as based on invertebrate paleontology and paleobotany?

The vast field of Mesozoic invertebrate paleontology is husbanded today by but two American leaders, and one of these has largely gone into administration. Paleozoic and Cenozoic paleontologic stratigraphy go forward with leaps and bounds, but Mesozoic paleontology lags far behind. Not a single one of the many, many States and provinces whose territory consists mainly of Mesozoic strata now has a paleontologist, and New Jersey and Kansas alone among them have in the past given this aspect of the work adequate attention.

At the outset of my remarks it was pointed out that there are about 112 paleontologists in America recognized as such. Now let us see where their workshops are. In the colleges and universities there are 50, in the public museums 25, on the national and State surveys 23, and 17 are following paleontology as a side issue. The vertebratists are almost wholly in teaching institutions (17) and museums (15), while the invertebratists are placed in the main in the colleges (30), surveys (21), and museums (7). These figures bring out some marked shortcomings, and the lesser one is that only 25 are working in museums and but 23 on the national and State surveys. In the United States there are about 38 State or university State surveys, and yet on all of them there are but 9 official paleontologists, and 7 of these are located in three places. Five of these paleontologists are mainly absorbed in geologic problems, and in but one State is paleontology fostered in an adequate way. This is in the grand survey of New York State, organized in 1843 by James Hall, first a paleontologist and secondarily a geologist, and now continued on an even grander scale by John M. Clarke. It should be pointed out here, moreover, that our State surveys are almost wholly dependent upon the United States Geological Survey, the New York State Survey, and the university surveys of Maryland and Illinois for their chronogenetic evidence. In this we see clearly that the great majority of States are not doing their duty toward American stratigraphy and historical geology. Think of the State surveys of 25 to 50 years ago and of the good paleontologic work done by them, nobly led by New York and more or less heartily seconded by Illinois, Ohio, Pennsylvania, Minnesota, Iowa, Indiana, Missouri, Wisconsin, Michigan, and California. Where are they today in paleontology? It is true that applied geology must first be taken care of, so that the natural resources of our land can be rapidly developed; but it is not at all necessary, nor is it a healthy viewpoint, that the whole of the work of State surveys should be for practical or mapping purposes. We know that there are more States than those mentioned which publish some paleontology, but it is almost wholly of the nature of a restatement or adaptation of what has been known before. We should add here that not only the State surveys, but the national surveys as well, tend altogether too much to the practical. The grander ideals of paleontology and geology are too much lost sight of in these times of intense commercialism.

The greatest shortcoming in American paleontology at present, however, is that our science is dominated by the geologists, and largely by the geologists of the universities; for, after all, it is in these institutions that most of the geologists and paleontologists of America are made, or at least educated. This charge may be resented in certain quarters, and therefore we will present the evidence upon which it is based.

The United States Bureau of Education tells us that there are of colleges and universities in this country not less than 450, and that undergraduate geology is taught in about 288 of them and graduate geology in 38. We learn, further, that geology in some form is taught in every State in the Union, and that there are about 465 instructors in the subject, of whom it appears certain that over 100 are not geologists, but are in the small institutions where one man teaches all the natural sciences. But, even if we should say that there are but 365 teachers of geology, the fact remains that but 50 of these are paleontologists. The geologists, in other words, dominate in the ratio of 7 to 1. There are 75 institutions having two or more instructors of geology, and yet we can discern, with the most liberal count, but 18 of these in which a paleontologist is the head or chairman of the geological department. In university leadership, therefore, the geologists exceed us 4 to 1.

In 11 of the Southern States geology is taught in 58 higher institutions by 58 instructors, and in but one do we find a paleontologist, and he is not active as such. In the 13 Western States geology is taught in 41 institutions by 59 instructors, and here we find the seemingly good representation of 8 paleontologists, but still we are outnumbered 7 to 1. In 12 Eastern States geology is taught in 75 institutions, with a staff of 150 instructors, and among them are 23 paleontologists—a ratio of 6 to 1. In the 13 Central States more geology is taught than in any other section of our country—in 115 institutions by 185 instructors. From this we see that geology has attained its widest activity in the central portion of our country, the region of simplest geology, but of greatest natural resources; and yet even here we can find but 16 teaching paleontologists—a ratio of 1 paleontologic to 11 geologic instructors. This lack of paleontologists is all the more remarkable when one remembers that the Central States are a paleontological paradise and the region of grandest interpretative stratigraphy. Most of our invertebrate workers are bred and taught here; and yet the geologists outnumber us 11 to 1.

In line with this overwhelming testimony that the geologists unduly rule the paleontologic outlook, though not intentionally and, above all, not domineeringly, we see that in one of our large universities historical geology is taught by a geologist, and the single paleontologist in a large family of geologists is kept politely but firmly in the background. In still another one a geologist pushes paleontology into the background, and when students go to him for advice as to taking a major in paleontology, he almost always asks scornfully, "Why do you want to go into paleontology?" On the other hand, when a paleontologist is chairman of the geological department in a university, and there are a few such, we do not see the department filled up with paleontologists. The condition is rather quite the contrary, and we can firmly state that the paleontologist usually has a wider and better balanced outlook than the average geologist.

We need today a large body of paleontologists alone to enable us to perpetuate and to increase the knowledge of paleontology. We need many more to apply our information to geology for practical purposes, and a greater number still to teach historical geology, paleontology, and organic evolution in our colleges. Only too often is the course in historical geology made "as dry as dust" by a geologist who knows nothing directly of biology or paleontology, or by some inexperienced young man upon whom the teaching of the course is shunted. The subject must be taught by those not only having the knowledge of organisms, but also with the very spirit of paleontology. Think of the three decades of Harvard students who felt that their education was incomplete without the course in paleontology and evolution given by Nathaniel Southgate Shaler. And when this great teacher passed away, his students commemorated him by giving to Harvard an endowment that yields each year about $2,500, to be devoted to research work in geology and paleontology. This is, moreover, not the only brilliant case of successful paleontologic teaching, for we know that at California Merriam attracts to himself each year several hundred undergraduate students in paleontology, and that he is turning out more professional students of the subject than almost any other teacher. And Lull in his course in organic evolution at Yale, which is largely based on paleontology, has each year upward of a hundred undergraduate men. If the geologists will only turn historical geology and evolution over to the paleontologists, we are convinced that in the great majority of cases they will never again have the chance to kill these courses by making them "as dry as dust."

If the paleontologists of America will justly and adequately assert themselves, it is certain that our clan at the close of another decade will number at least 160 active workers instead of 112. New colleges are arising and many an old one is enlarging. In addition, there are being founded in many places local museums, and some of these will need a paleontologist, because he can, if necessary, husband the biological collection as well. At present, however, there are not enough of us to fill the demand. Our colleges and universities are being depleted of students and instructors at a frightful rate. Not only are most of our students of geology and paleontology being absorbed by the mining companies and the petroleum barons, but these all-powerful interests are now invading the ranks of our assistant professors and professors. Truly our heads are dizzy at the salaries offered—rarely under $5,000 and often $10,000, with exploration in foreign lands, and in at least one case a man was offered $20,000 by two different companies within a week. Under these circumstances, it is a question who can resist mammon, even knowing that capitulation to it means the loss of an honored university position and a research career. In this way the young life of our teaching institutions and of the United States Geological Survey is taken away from us, and unless we are very careful to meet the crisis by higher salaries and the chance to do more research work, the sciences of geology and paleontology will be greatly retarded. The danger is not yet fully felt in our universities, because the older men are not wanted in the active, plunging world of petroleum; but ten years hence where will be the experienced teachers, with a general fund of knowledge, to take the places of the leaders of today? In these times of extraordinarily rapid scientific progress, any one out of the going for five years will hardly be able to catch up with the leaders when he returns to the fold. Let us hope that the universities will rise to the demands of the time. The struggle for the making and holding of good teachers and research workers in geology and paleontology will be even more difficult during the next decade than it has ever been. On the other hand, we should be proud of the high stand the earth sciences have attained and the recognition we now have that geologists and paleontologists really do know something which can be turned into dollars.

Let me sound one more warning, and this relates to patriotism and the ascendancy of the American geologist and paleontologist. If the lure of petroleum and valuable metals prove irresistible to any American geologist or paleontologist, let us hope that he will serve wholly American commercial interests. Foreign syndicates have a great respect for American mining and petroleum geologists, and they have already taken many a one from us, and among them are some of our leaders in geology. They are now dangling great salaries before others of our young men, but let us hope that the patriotism of the latter will keep them from being led into these "foreign entanglements." [2]

It is all very well to point out the weaknesses of our science and its workers, but we must not stop here; rather should we also try to suggest lines of work and ideals that if followed will lead paleontology into greater effectiveness and build up an esprit de corps that will be irresistible. Many pieces of desirable work, and even some along more or less new lines, can easily be pointed out by any of the leaders in paleontology; but what is really wanted above all at the present time is a medium that will constantly direct our attention to the new results attained, to be attained, and to be striven for. To meet this need, we want a paleontologic journal, broad and liberal in its outlook, with a spirit of good fellowship toward all paleontologists, geologists, zoologists, and botanists. We need a journal the numbers of which shall appear from

[2] When this paragraph was read at the Boston meeting, the "patriotism" here intended was misunderstood by one member. My sentiments are those expressed by Director George Otis Smith in his address on "The Public Service Opportunity of the Oil Geologist" (Bull. Amer. Assoc. Petrol. Geologists, vol. 4, 1920, p. 15) as follows: "The American geologist, who, on the signing of the Armistice, realized that the hour had struck for the severance of professional relations with an allied nation and returned home to add his efforts to the new endeavor under his country's flag, saw clearly his duty."

four to six times a year, to spread the news of what paleontologists are doing everywhere, and of what geologists and biologists, chemists and physicists, are doing that reacts on our work, and to contain short but comprehensive articles bringing together the things already attained and to be attained. This will not be a journal primarily for the publication of technical paleontologic articles, but will give its support to a movement seeking for a patron or patrons to establish an endowment for the publication, under the auspices of the Paleontological Society, of a Paleontologia Americana. Therefore let us soon start and establish such a journal, which will at the same time be the official organ of this Society.

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
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THE TEACHING OF HISTORICAL GEOLOGY[1]

BY HERDMAN F. CLELAND

(*Read before the Society December 31, 1919*)

CONTENTS

HANDICAPS IN THE TEACHING OF HISTORICAL GEOLOGY

The first instruction in paleontology which most students receive is in elementary historical geology. The exception to this rule is afforded by those alert young men who become interested in the fossils of their home region and learn about them from State and government reports without first having received preliminary instruction. Some of our most eminent paleontologists had their interest aroused in this way and are true products of their environment. J. M. Clarke, Charles Schuchert, Edward O. Ulrich, and Charles D. Walcott are examples. Notwithstanding these notable exceptions, most persons who obtain a preliminary knowledge of paleontology acquire it in a first course in historical geology, and it is on the teaching of this subject that I am to speak.

There are few courses in the teaching of which the instructor is so severely handicapped as in the one under discussion, and the most serious obstacle lies in the lack on the part of the students of a preliminary

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This paper is one of a series composing a symposium on the teaching of geology and paleontology.

knowledge of the classification of the plants and animals which form the basis for an important part of the work. Under the most favorable conditions, the student may have had one or more courses in biology, but in these courses he learned more about cells, heredity, and evolution than about forms and their classification. One will be conservative in stating that not one student in fifty who elects geology possesses a knowledge of a classification of plants and animals that will be really helpful to him in his study of historical geology. Such important Paleozoic animals as brachiopods, trilobites, bryozoans, crinoids, cystoids, graptolites, and old-style corals are given bare mention, if mentioned at all, in courses in elementary biology. Consequently, biology as a prerequisite to historical geology is of little value. It is because of this ignorance of those invertebrates which must necessarily be discussed that every teacher of historical geology heaves a sigh of relief when the Paleozoic is completed or, at least, when the Pennsylvanian is reached.

As a result of the student's ignorance of biological classification, he has a vocabulary hurled at him, almost as soon as he begins his study of historical geology, which discourages the earnest and frightens the dull, and it is not until the Cambrian, Ordovician, and Silurian are passed that the class begins to find solid ground. I am assuming that the course is largely confined to the study of the classes and orders of animals and plants, and that the student is not required to learn much about species and genera. I think we too often fail to realize that even group names, such as brachiopod, gastropod, and trilobite, do not immediately call up a definite image to most students until quite late in the course, and consequently when the teacher lectures about them or brings them into his discussions he is not fully comprehended. Until each group studied does call up a definite image to the student, he can not, of course, become interested in the life of the past. An extreme example will illustrate this difficulty. A well known geologist, an excellent teacher, was asked by a student, during the study of the reptiles of the Mesozoic, if these animals had skins when they were alive or if they were always skeletons.

As the time allotted to historical geology in most colleges and universities is three hours a week for one semester, it is evident that the teacher can not devote many lectures and laboratory exercises to the teaching of classification and the descriptions of animals and plants. One of two ways of teaching this necessary classification is used: the teacher either gives preliminary lectures or laboratory work on the entire classification, and thus quickly gets it out of the way before going on with the substance of the course, or he takes it up a little at a time, as the subject requires. When this latter method is followed he spends a disproportionate time on the Cambrian and Ordovician, since in the discussion of these two periods most of the invertebrates are studied for the first time. I am assuming that the subject is taught in the conventional way.

Another handicap to the successful teaching of historical geology in many educational institutions is that the teacher is not a paleontologist and has not had adequate training in the science. Moreover, as his major interest is in some other branch of geology, he seldom gives an interesting course in this subject.

Even the stratigrapher may be somewhat at a disadvantage in teaching this elementary course, because if he does not hold himself in check he is likely to find himself spending too much time on the details of paleontology and stratigraphy. To him the specific names of certain fossils, because of their great value in the correlation of certain formations, may seem more important than anything else and he may smother his class in technicalities.

The ideal gateway to paleontology is such a course as that outlined in Shimer's "Introduction to the Study of Fossils." To teach historical geology to students who had mastered such a course would be a pleasure. The objection to such a prerequisite is that few students would elect it, with the resultant practical elimination of elementary historical geology—one of the most valuable cultural subjects offered in college curricula.

What should be Emphasized in an elementary Course in historical Geology?

In preparing this paper I found myself repeatedly asking: What would you want to learn from a course in historical geology if you could take but one course and would never again have an opportunity to make a further study of the subject? In other words, what should a study of historical geology contain for a very large proportion of the students who elect it? This is a fair question because very few students go on with paleontology. This being true, our attention should be given to the ninety and nine rather than to the one.

Our answer might be somewhat as follows:

(1) I would want to have a knowledge of the proofs and progress of evolution as shown by the life of the past.

(2) I would want to understand the effects of physical conditions in bringing about the modifications of the form and structure of organisms.

(3) I would want to acquire a general knowledge of the succession of life—that is, to learn how time after time in the history of the world great classes of animals and plants predominated and then either disappeared never again to return or ever after took a subordinate place.

(4) I would want to be shown how the sedimentary series was built up, how it varied in composition, and why; how the ages of the strata are determined, and how the work of correlation is accomplished.

(5) I would want to learn about the climates of the past and how this knowledge is obtained from a study of fossils and other evidence.

(6) I would want to carry away with me a general idea of the evolution of the continents.

Topics such as these are the ones which, it seems to me, should be emphasized in a cultural course, such as is offered in all of our institutions of higher learning.

What should not be Emphasized?

The most difficult part of historical geology for the student, as has been said, is the Paleozoic, because he knows so little about the invertebrates found in the rocks of this group and has little interest in them. Am I a heretic if I ask, "Why should he?" These fossils are interesting largely because of their value in chronology and correlation and because of the lessons they teach of evolution. But these first year students do not have a sufficient knowledge of the structure and characteristics of these animals to appreciate or understand their evolutional value and few will have acquired such a knowledge when the course is passed. These are the facts. Why not face them?

One reason that students are required to spend so much time on invertebrates is that we are to some extent following the method of teaching in geology which used to be employed in the teaching of human history. Not many years ago the student in history was required to learn the number of men engaged in each battle, the names of the generals, the number of men killed and wounded in each engagement, the line of march, and other easily forgotten statistics, whereas the bearings of these events upon the progress of civilization were too often merely touched upon or omitted. Our method of teaching geology is largely inherited from the time when fossils were regarded as of value largely because they are indicators of chronology. This is shown in the treatment of the Paleozoic as given in all text-books of geology.

The Answers of eight Teachers to the Question: How should the Paleozoic be presented to Students in the first year Course?

Some months ago, before I was asked to speak on this subject, I wrote to seven paleontologists and have since spoken to one other, who are also successful teachers of general geology, asking, among other questions, how, in their opinion, the Paleozoic should be presented to students in a first year course. The answers are interesting and are worthy of the careful consideration of those who teach this subject. Two of these teachers were of the opinion that the discussion of the animals and plants of the Paleozoic should be by periods rather than by eras—that is, the life of the Paleozoic should be treated from the stratigraphic rather than from the biologic point of view. They believe the subject has, on the whole, been properly presented ever since geology became a science. As one says: "Doubtless a student would get a better idea of the animal or plant groups by having each discussed as a unit, but he would fail to grasp the geological significance of the group." Two of the eight teachers favored grouping together the life under two divisions, the late and early Paleozoic, perhaps under such heads as Eo-Paleozoic and Neo-Paleozoic. This they considered desirable because for beginners there is hardly enough difference between the Cambrian and Ordovician faunas, for example, to make it worth while to point out distinctions. On the other hand, the changes between the Cambrian and Permian are rather too great to go unnoticed until the end of the discussion of the Paleozoic era. Four advocated the gathering together of the life of the Paleozoic in one place and the physical history in another. There is much to be said for this last suggestion, but the danger of losing the geological significance of the group is great. Moreover, with this presentation the student might become confused because of the large number of periods which would necessarily be discussed with each class of animal or plant.

Suggested Methods of Teaching

CLASSIFICATION OF THE PERIODS

All things considered, it seems desirable to change our method of presenting the material for the study of this difficult era, and my belief at present is that beginning students would have less trouble and would learn more of value if the animals and plants were grouped together under earlier, or Eo-Paleozoic, and later, or Neo-Paleozoic. If this were done, much repetition could be avoided and the descriptions and classification of Paleozoic plants and animals could be made much easier and more interesting, while time would be left for the acquiring of a knowledge of the subjects mentioned a few minutes ago.

RELATIVE EMPHASIS TO BE PLACED ON THE STUDY OF VERTEBRATES AND INVERTEBRATES

There is an interesting inconsistency in the argument that the invertebrates of the Paleozoic should be discussed by periods, and that much time should be spent on them. This is shown in the discussion of the Mesozoic and Cenozoic in text-books of geology. I think no one will deny that the invertebrates of all eras are of nearly equal rank as index fossils and for the lessons of evolution that they teach. Nevertheless, they are given relatively little attention in the discussion of the Mesozoic and Tertiary. The explanation for this difference of treatment is to be found in the fact that the vertebrates are more interesting, more important, and present more striking lessons in evolution. Nevertheless, some teachers spend so much time on the Paleozoic invertebrates that they are obliged to give an inadequate amount of time to the study of the Mesozoic and Tertiary and of the great lessons that are there taught.

LABORATORY AND FIELD-WORK

Some laboratory work should, of course, be required of all students who elect historical geology. If our educational institutions were all situated in fossiliferous regions, as is Cornell University, the University of Cincinnati, and the University of Texas, the teaching of paleontology and stratigraphy would be simplified. Prof. G. D. Harris, perhaps more than any one else, has made much of this fortunate situation. Even the first year student wants to know the names of the fossils he himself discovers, the age and the name of the rock in which they occur, and how it was formed. Unfortunately, most institutions of learning are not so favorably situated and it is necessary for the teacher to use labeled specimens in the laboratory. Fossils, rather than casts, are necessary. Theoretically, the cast of a well preserved fossil, or a restoration, is better than an imperfect fossil; but, as every teacher has learned, the elementary student can not be convinced of this fact. He wants the real thing and loses interest if he does not have it. Consequently, where historical geology is taught there should be a set of fossils for laboratory use. More should be made of lantern slides than has hitherto been done, and especially is this desirable in a study of the vertebrates. There are many instructive and striking restorations of which slides can be made. Unfortunately, a great deal of time is required to get this material together, and this Society would do a great kindness to teachers of paleontology if such a set were made available.

PALEOGEOGRAPHY

I fear to express an opinion on the teaching of paleogeography for fear that you will think that I have left all hope behind; but I am looking for difficulties and for the best way out of them. In order to apply the Taylor efficiency methods in our teaching as it is employed in manufacturing, it is first necessary to find the faults in our systems. No topic in historical geology is more abused than that of paleogeography. This is true because most teachers do not realize how uncertain are the boundaries of the lands and seas on the published paleogeographic maps. The result is that students are sometimes required to learn a great deal that is based on very uncertain evidence or much that will later be changed, or which may already have been abandoned by those who are working on the subject.

There are two suggestions for the teaching of paleogeography to elementary students that are at least worth our consideration. One is that the paleogeography of a single period should be chosen as a type and should be thoroughly studied. In this way the student would get a better grasp of the changes in geography that have taken place in a single period, and from this as a type could form a better idea of the paleogeography of other periods. A second suggestion is that the regions of general submergence should be emphasized. The method usually adopted is to require the student to learn the boundaries between the seas and lands of each period as given in the most recent paleogeographic maps. As in the study of invertebrates, the teacher must put himself in the place of the student and ask himself, "Is all of the work required worth the student's time and effort?" If it is found that some of it is not, it should, of course, be omitted or rearranged. It is evident that the paleogeography of the region in which the college or university is situated should be emphasized. One obstacle in selecting the material to be presented is that few geologists have an extended knowledge of paleogeography, and consequently do not know what is hypothetical and what is based on fact.

Conclusions

I do not wish to touch on a controversial subject, but one can not fairly dismiss the topic before us without doing so. I refer to the seemingly growing practice of permitting a student to elect either physical geology or physiography as a first course, and later allowing the same student to choose as a second course whichever of these two he had not taken. Any one who has looked over the text-books of physical geology and physiography must be impressed with the large amount of material in the one which is duplicated in the other. Certainly one-half, possibly three-fourths, of the subject-matter is the same in the two. It is not my purpose to discuss the advisability of eliminating the one or the other of these courses, but to ask you to think about this question: Would not every student, without exception, receive vastly greater benefit from a year's course in physical geology and historical geology, or physiography and historical geology, than from a semester course in physical geology and a second course in physiography, or *vice versa?* The answer seems obvious. It is to be hoped that every effort will be made by teachers to prevent the present tendency to divorce historical from physical geology. Such a separation would be most unfortunate, because a student who has had physical geology and has not had historical geology has been deprived of a conception of time, of the progress of life, of evolution, of the growth of continents, and of other subjects which every educated man should have. The principal reasons that physical geology and historical geology are not offered as a single-year course in all of our colleges and universities appear to be two: First, historical geology does not draw as large electives as physical geology or physiography, because it is a more difficult subject for the student to acquire; and, second, most teachers of general geology have had little training in paleontology, and consequently slight this subject for one in which they are more interested. Some of the blame for this shyness of the student in electing historical geology, as has been stated, should be placed on the teachers and text-book writers, who have been making the course too largely a test of memory, and who have been requiring the class to learn a great deal that, for the student who takes the course for its cultural value, is, frankly, not worth his time and effort. Nevertheless, the subject as now taught, with all its imperfections, is well worth the time of any student. We are, I think, passing through a transitional stage, from which the subject will soon emerge as one of the broadest, most valuable, most interesting, and most cultural that will be offered in colleges and universities.

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FOSSILS AS AIDS IN TEACHING STRATIGRAPHY, OR APPLIED PALEONTOLOGY [1]

BY STUART WELLER

(Read before the Society December 31, 1919)

The importance of the subject of applied paleontology is, of course, due to the fact that long experience has demonstrated that the only satisfactory data for establishing a geological chronology are to be gathered from a study of the successive life forms in the earth's strata, and the chronological relations of the various rock strata with which the geologist comes into contact in his field studies are a consideration of the highest importance to him. It must be recognized in this connection that both the paleontology and the stratigraphy, from the standpoint of the geologist, are but adjuncts to historical geology, and the ultimate purpose of applied paleontology is to throw light upon the succession of events that have taken place in the course of the earth's history.

Before discussing the proper methods to be pursued in the training of students in applied paleontology, the necessary qualifications of one who is to devote himself to such work must be considered. It is almost axiomatic that the usefulness of one's knowledge of fossils in the solution of problems in stratigraphy and historical geology is in distinct relation to his familiarity with numbers of fossil forms, to his knowledge of their known stratigraphic positions and their faunal associations, and in his ability to properly discriminate genera and species.

As greater and greater refinement is demanded in stratigraphy, greater and greater refinement is constantly being demanded in the discrimination of both genera and species, and larger numbers of these forms are continually being recognized, described, and named, so that the amount of desirable knowledge for the paleontologist is assuming enormous proportions. The magnitude of this body of desirable information is typified

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This paper is one of a series composing a symposium on the teaching of geology and paleontology.

in the two volumes constituting Bulletin 92 of the United States National Museum, in which no less than 1,342 pages are devoted to a bibliographic index alone of the known Ordovician and Silurian fossils in America. No tabulation of the actual number of genera and species recognized in these volumes is given, but there are probably somewhere near 8,000 species recorded. More than twenty years ago a list of 3,754 species of American Carboniferous invertebrate fossils were recorded,[2] and this list could be much amplified at this time. Probably 20,000 American Paleozoic fossils have been described and named, with the descriptions scattered through a body of literature to be found in hundreds of volumes. With the Mesozoic and Cenozoic forms added to these, the number suggested above would be very greatly increased, though probably not doubled.

The actual task of one who is by profession an applied paleontologist is not unlike, in many ways, the work of a student of the literature of an unknown and forgotten language and a language that has been recorded by word symbols rather than by a phonetic alphabet. The fossil species constitute the symbols in this literature and their association in faunules and faunas are the sentences and paragraphs. The literature as a whole is one great record of a continuous history, but the pages in this record are not assembled in regular order, as they might be in a volume in a library, but are scattered over the whole world; some of them are fairly complete, but many are mere scraps and fragments, while others have been lost beyond recovery or have not yet been found. The assembling and piecing together of this great historical work and the translating of the history so recorded into our written or spoken language are no small task. Furthermore, the bringing together of this paleontologic record is still far from complete. Pages of the record here and there, belonging to many different chapters, have been read in a more or less satisfactory manner, but new information, which is being constantly accumulated, makes necessary the re-reading of many of the pages already deciphered.

The teacher who seeks to arouse a thoroughgoing interest in the subject of applied paleontology has a most difficult task. He labors at a disadvantage in teaching a subject that is in the making, and although this very fact may lend vast interest to the work when the student has progressed far enough to appreciate the possibilities for original research, it makes it far more difficult to present to beginners. It is highly desirable that the student should become familiar with as large a number as possible of fossil genera and species, which are the words of the lan-

[2] Stuart Weller: Bull. U. S. Geol. Survey, No. 153, 1898.

guage in which the story he is to read is recorded; yet it would be as absurd to confine the work of a class to this sort of thing as it would to use the Century Dictionary as a text-book in a course in English. Just the names of fossils are discouraging, with their Greek and Latin roots, with which most students nowadays are not familiar. In general, I tell my students to make no attempt at memorizing names, but to give their attention to becoming intimately acquainted with the forms themselves; then the names will take care of themselves, just as the names of our intimate friends attach themselves to the persons without special effort on our part. Yet this learning of the names of fossils along with the forms must not be neglected; it is the vocabulary work in the course, and, as in learning the vocabulary of a spoken or written language, every addition to our working word list makes it easier to read the record and to add other units in the vocabulary.

The actual study of the principles of organic evolution does not properly come into the immediate field of work of the applied paleontologist, but every bit of new data concerning evolutionary laws, especially in so far as some application of them can be made in connection with the fossil forms, is of utmost interest and practicability. The student of fossils is actually working with the results of evolutionary processes, and these results, exhibited in the progressively changing characteristics of various groups of fossil organisms and correlated with the passage of geologic time, are not only of the greatest value for chronological purposes, but always serve to give life to the subject, in a class of students. The progressive changes exhibited in the sutural modifications among the cephalopods or the progressive changes shown by the changing plications of the shell in the genus spirifer always arouse interest among students and serve to help fix various fossil forms and names in the memory of the individual student. Progressive changes of various sorts, some conspicuous and others more or less obscure, and all possible of correlation with the passage of geologic time, may be pointed out in practically all groups of fossils. Such facts always appeal to the student, for he feels that they may be of practical service to him in his geological work, even though he should not retain the names of all the genera and species involved. As a fact, however, the appreciation of all such characteristics serve to impress the names of the fossil forms on his mind unconsciously.

One of the most essential qualifications for a student of applied paleontology is the ability to discriminate between various fossil forms, and especially between forms that are closely allied, and the training necessary to develop this faculty is highly important. This means the development of the power of observation in connection with small, very commonly almost microscopic, features and the recognition of minor but constant differences and resemblances among fossil forms. This sort of observational ability is very different from that of the student in many other branches of geological science, where the objects to be observed are of the magnitude of mountains, hills, valleys, and rivers, and demands a very special sort of training. The only way to develop this sort of ability is in connection with laboratory work upon the fossils themselves and actual practice in the identification of genera and species with the aid both of the more general and some of the special literature. Such work as this, when conducted systematically with material representing the several phyla and classes of organisms commonly found as fossils, and accompanied by some lectures and by informal conferences, accomplishes several different, important results.

In the first place, the student becomes familiar with the broader features of the classification of organisms into phyla, classes, orders, etcetera. Secondly, he gains some appreciation of the sorts of characters in the various groups that are commonly used for the separation of individuals into species and the species into genera. Thirdly, in handling material in this manner from different geological formations and from different localities, he comes to appreciate something of the various conditions of preservation of fossils and the different appearances which the same form may assume in different situations. Fourthly, he gains some knowledge of and some experience in the use of paleontological literature; and, lastly, he is constantly adding to his paleontological vocabulary.

It is perhaps an open question as to just what contact between the student and the fossil specimens should be first established. Whether he should first approach the specimens in their geological relations—that is, as faunal assemblages—or whether they should be approached first as organisms, in their taxonomic relations. As a matter of fact, I have had numerous students who have come into the subject from each of these directions, and both contacts must be made sooner or later. My experience has been that the first point of contact makes little or no difference in the final outcome, but in general I prefer the geological contact first, because that can usually be made more interesting to the average student.

Several purposes must be accomplished in the training of students in applied paleontology. First, a general interest in fossils themselves must be aroused. Undoubtedly the very best stimulus in this direction is that afforded by collecting specimens in the field. If this interest has been aroused in this manner before the student has entered upon his college work, he is indeed fortunate and his instructor is relieved to that extent. If the interest is to be newly aroused, the field excursion to good fossil beds is of first importance; but, as this is not always a possibility, the interest can be first aroused in the laboratory in some cases. Second, a paleontological vocabulary—that is, a knowledge of fossil forms and their correct names—must be acquired, or at least a beginning must be made in that direction. Third, the power of observation and the power of discrimination must be developed to a high degree. The only sure means of accomplishing these results is through laboratory study of the fossils themselves, under proper supervision, accompanied, if it is in any manner possible, by field excursions for the purpose of collecting fossils which may be used as the basis for study in the laboratory. I doubt whether any paleontologist has ever been encouraged to enter into this field of activity through listening to lectures alone. Lectures may, or perhaps should, accompany the laboratory work, at least during the early period of training, although informal consultation with the instructor is perhaps more effective than set lectures. It is the handling and the study of the specimens themselves, however, that really counts.

Furthermore, the material placed in the hands of students should be good material, and although good specimens—perhaps choice specimens—may be injured occasionally or perhaps lost, this should be no excuse for withholding such material from student use. The making of a real paleontologist is well worth the loss of an occasional specimen. If a student is allowed to handle only the junk which accumulates in all museums, he will never develop an enthusiasm for paleontology which he must acquire to become a successful and a productive worker in this field.

It is only when a student has made considerable acquisitions along the lines which have been indicated that he is really ready to enter upon the work which demands the actual application of his paleontological knowledge, and only rarely does one progress to this stage as an undergraduate student.

The sort of work already outlined may well be considered as being suitable for advanced undergraduates, although graduate students may commonly enter courses in which such work is carried on. The later work, however, is strictly of a postgraduate character. This will consist primarily of individual work devoted to assigned problems in research upon special collections. It is highly desirable that the materials to be studied in such problems be collections which the student himself has gathered or has had some field connection with, for such contact always adds a personal interest in the problem being attacked, which is of the greatest value to the student. Such problems involve the actual application of the paleontological knowledge which has been acquired in the earlier work of the student and will actually be an investigation in some chapter of the geological history which has not yet been completely read. He will be working out an interpretation of a little scrap of the scattered paleontological record, to be added to the already known portion of the fascinating story.

In conclusion, let me again insist that the real way to impart knowledge of fossils and paleontology is through the agency of the laboratory and the field. The lecture has its part to play, but this is entirely secondary to the actual contact with the objects themselves with which the paleontologist deals.

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
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THE TEACHING OF PALEOBOTANY[1]

BY EDWARD W. BERRY

(*Read before the Society December 31, 1919*)

CONTENTS

INTRODUCTION

It is difficult to formulate the most desirable content of a course in paleobotany, since so much depends upon the environment and opportunities for the subsequent career of the student. The difficulties of combining the biological treatment with the geological perspective are perhaps greater in paleobotany than in the sister science of paleozoology. Shall paleobotany be considered the handmaiden of botany or of geology? It would be easy to formulate such a course for an ideal world, but in a world of pragmatists and when confronted with the necessity of a living wage for the neophyte the problem is not so simple. Probably the graduate in paleobotany will locate with the United States Department of Agriculture or some experiment station, where he will be assigned to the study of strawberry rot or potato scab, or, if the geological call is stronger than the botanical, the United States Geological Survey will take him on and assign him to land classification work. At any rate, the chances of opportunities in his chosen subject are few indeed.

RELATIONS TO GEOLOGY AND BOTANY

In the United States, and to a less extent in Austria and Germany, paleobotany has been the foster-child of geology and has hence been more

[1] Manuscript received by the Secretary of the Society February 2, 1920.
This paper is one of a series composing a symposium on the teaching of geology and paleontology.

intimately associated with the latter science, while in Britain, and to a less extent in France, it has been a foster-child of botany. Neither relationship has resulted in a fully rounded-out child. In the other countries of the world paleobotany is still a foundling. I should like to insist that plants are still plants, even though fossil, and that paleobotany is the botany of all time, while botany is the botany of but a fraction of geologic time, namely, the present. This seeming inversion results from the "fundamental, experimental, genetical" state of the biological sciences at the present time and the unfashionableness of displaying any acquaintance with plants or animals.

Students desiring instruction in paleobotany are either from botanical departments with no knowledge of earth history and but slight knowledge of plants, or they are from geological departments and also with no knowledge of plants. One generally gets some of both. This being true, shall the emphasis be placed on the historical side, and the succession of floras that have clothed the earth be given in so much detail that the student comes forth a stratigraphic paleobotanist, or shall structure, rather than form and habit, govern the method of presentation?

In England, where the chief workers in paleobotany have been professors of botany in universities or colleges, the work with fossil plants has been almost entirely anatomical and morphological and quite without geological perspective or geological results.

It has always seemed to me that any young man with good eyesight, an average brain, and reasonable industry can do satisfactory anatomical work. If you object that he can not interpret his results, I am inclined to consider it a blessing in disguise, in the present state of our knowledge. On the other hand, a student who can visualize the epic of bygone life and appreciate the relations of his fossil to its fellows and to its physical environment is indeed a *rara-avis*.

Since it can not be hoped to train a student so that he will step forth from his *alma mater* fully equipped as a stratigraphic paleozoologist or paleobotanist, it has seemed to me more desirable to subordinate stratigraphic details (easily acquired in practice) and to give a broad and philosophical treatment. Without presuming that my own experience is necessarily a guide to others, I can only say that I have tried both methods and find the latter much more profitable. The framework of the discussion is phylogeny. We start as near the source of the stream of life as is possible, with the pre-Cambrian bacteria, and trace the rise, the evolution, and the radiation of the great groups amid the changing environments of the past. Histological and morphological aspects are treated as fully as knowledge permits, in emphasizing that fossil plants are not merely medals of creation, but are still more—organisms with an intensely interesting story to tell and one that can contribute much to our knowledge of past climate, topography, geography, and paleozoology. Modern plants are the end products of age-long evolution; their broader taxonomic relations and distribution are meaningless without a knowledge of their extinct forebears. Time and place and space are emphasized for the benefit of the botanical tyro, since there are even some paleobotanists who do not know that the world was not made in the six days of the Pentateuch—at least, a million years is but as a day to them—while form and structure are emphasized for the benefit of the geological tyro.

Beginners, and some who are not beginners, attach much faith to taxonomy, both geologic and biologic. We doubtless must have classifications as well as language; but classifications are utilitarian and not objective. Time was and is continuous, and so is geological history. Nature knows no dead-line between geological systems, nor does she furnish any pigeonholes or Dewey system for filing plants and animals.

Syllabus of Subject-matter

Life is as continuous as time, and I can not believe that it has ever been shortcircuited, despite the seeming plausibility of the views which would ascribe times of active mutation (De Vriesian sense), such as the time of early radiation of the flowering plants, followed by long intervals during which natural selection was operative.

With this apology for the seeming synopticalness of what follows, I append the abstract of a syllabus of the ground I consider it desirable to cover in the teaching of paleobotany.

Introductory.
- Historical—The pre-scientific period, the time of Diluvial hypotheses, and the Modern period.
- Methods of preservation of fossil plants.
- General principles—Evolution, Adaptive radiation, Race periods, Recapitulation, Conservative organs and organisms.
- Relation to other sciences—Botany, geology, paleoclimatology, paleoecology, paleogeography.

The evolution of plants.
- Evidence of pre-Paleozoic plants.
- The pre-chlorophyllic stage of evolution.
- The algal stage.
- The terrestrial stage.
    - Origin of seeds.
    - Secondary thickening.
- Thallophyta.

- Algæ.
- Fungi.

Bryophyta.

Pteridophyta.
- Cœnopteridæ.
- Hydropteridæ.
- Eusporangiatæ.
- Leptosporangiatæ.

Arthrophyta.
- Sphenophyllales.
- Pseudoborniales.
- Protocalamariaceæ.
- Calamariaceæ.
- Equisetales.

Lepidophyta.
- Bothrodendraceæ.
- Lepidodendraceæ.
- Sigillariaceæ.
- Lycopodiales.
- Isœtales.
- Psilotales.

Pteridospermophyta.

Glossopteris—Gangamopteris floras and province.

Cycadophyta.
- Origin.
- Williamsoniales.
- Cycadeoidales.
- Cycadales.

Coniferophyta.
- Cordaitales.
- Ginkgoales.
- Taxales.
- Araucariales.
- Pinales.
- Gnetales.

Angiospermophyta.
- Origin (time, place, manner).
- Relations of monocotyledons and dicotyledons.
- Evolution of vegetative body and of floral organs.
- Mesozoic beginnings.
- Eocene modernization.
- Neogene distribution.
- Pleistocene history.
- Evolution of herbaceous temperate families.

Relation of Student and Teacher

DISCUSSION BY EDWARD B. MATHEWS

The most effective time for influencing the student to undertake a life of research in geology is during the junior and senior years of college, for these are the years in which young men are beginning to think seriously of their life work. To win one's devotion to research in geology involves two distinct steps: First, the acceptance of the scholastic life, with its placing of spiritual rewards above the material; and, second, the choice of geology as the field of activity.

At the age of unfolding manhood the aims are particularly idealistic, ambitious, and optimistic. The student is straining at the leash to do something worth while and takes less account of the costs than the man of greater maturity. He is more than ready to devote himself to the task of increasing human knowledge, if properly enthused by his teacher's portrayal of the joy of discovery and the spiritual and social rewards of idealistic work well performed. However, unless equipped with private means, he must be assured of the likelihood of sufficient financial return to supply a wholesome, though simple, life for himself and his prospective family. This assurance comes from a knowledge of the economic status of the scholar and the degree of well being displayed by his instructors.

If his beloved teachers, whom he regards as leaders, present the appearance of men worsted in life's battle, or complain of the economic stress where they should be expressing in word and deed the joys of the scholastic life, the student will doubt the economic fact that men of ability and industry get rewards as teachers and investigators commensurate, on the average, with those received in other lines of activity. Such rewards can neither be expressed by dollars nor bought with money. If the student measures his prospective returns in money and is looking for the highest financial reward, he is not temperamentally equipped for the life of an investigator and should not be diverted from his normal line of development. These facts should be enforced by collegiate professors through precept and example, if the embryonic investigator is to be drawn into the life work which will yield him his most enjoyable rewards.

Given the student who is willing to forego the material ends for the sake of the rewards of a teacher and investigator, the next question is his selection of geology as his field of activity. Here again the responsibility rests upon the collegiate professor of geology, who must show not only the opportunities and joy of geological work, but the relative economic status of geologists among scholars. Humdrum, routine teaching of the subject, with a flood of facts presented without perspective and without enthusiasm for geological investigations, dulls the imagination of the prospective scholar and causes his attention to be attracted to some other field of study more alluringly presented by another instructor.

The economic status of the geologist is as good or better than that of an investigator in other lines. This is shown by a recent investigation of the professional incomes of geologists in universities and State surveys, which indicates that they receive on an average from 50 to 100 per cent more than the salary of corresponding college and university professors. This comes about from the fact that geologists have knowledge which is commercially valuable, and are able to do consultant work either during term time or in the summer vacations. Moreover, the industrial demand for geologically trained men is greater than the supply. During the last century many men have gone into commercial geology and mining engineering, but a canvass of the names in our various societies and in the bibliographies of geological literature shows that the annual supply of adequately trained investigators in American geology seldom exceeds twenty.

To build up a corps of geologists embued with a love of the science and a desire to investigate in order that the bounds of the subject may be extended by additions to our present knowledge, it is necessary that collegiate professors of geology, in their contact with students deciding upon their vocation, shall display the riches of the subject with contagious enthusiasm and impress upon them that the work of a geological worker is worth while and is rewarded by a satisfaction of the scholar's yearnings and a financial support that will yield a comfortable, though far from luxurious, living.

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VALUE AND USE OF STAGES IN DEVELOPMENT IN TEACHING PALEONTOLOGY[1]

BY ROBERT TRACY JACKSON

(Read before the Society December 31, 1919)

In the study of paleontology, one of two main objects may be the end to be attained: First, where the fossils are studied as assemblages of organic forms characteristic of certain beds and formations and from them is gathered the key to unravel stratigraphy and build up the historical record of the rocks. This important aspect of paleontology is perhaps the leading line of paleontologic work at the present time, at least among students of invertebrate fossils. Without decrying this work in the slightest, it must be remembered that stratigraphy is an application of paleontology and is not the whole subject. Its bearing is somewhat comparable to the study of geographical distribution to the student of recent forms of animals and plants.

The second object of paleontologic study is where fossils are studied from the point of view of the structure, development, and systematic affinities of the fossils themselves. This second method of study seeks to give a definite knowledge of the organisms and of their morphologic and systematic relations to other fossils in the faunas or floras of the past and to the living organisms of the present day. Whatever the object to be attained, it seems that at least in the beginning a student should follow this second method in order to obtain an intelligent knowledge of fossils. As Huxley wrote:[2]

"Already indications are not wanting that the vast multitude of fossil Arthropods, Mollusks, Echinoderms and Zoophytes now known will yield satisfactory evidence of the filiation of successive forms when the investigations of palæontologists are not merely actuated by the desire to discover time-marks and to multiply species, but are guided by that perception of the importance

[1] Manuscript received by the Secretary of the Society February 16, 1920.
This paper is one of a series composing a symposium on the teaching of geology and paleontology.
[2] Anatomy of Invertebrate Animals, London, 1877, page 687.

of morphological facts which can only be conferred by a large and thorough acquaintance with anatomy and embryology. But under this aspect the palæontology of the *Invertebrata* has yet to be created."

If the simpler and ancestral types of our living animals are to be known, it is to the faunas of the past, or the fossils, that we must look for them. If fossils are to be studied with intelligence, we must constantly compare them with living forms, where, with the soft parts still in place, we come to have a full appreciation of the relation of soft and hard parts that can not be attained from a study of the fossil alone. In other words, a combined study of living and fossil forms gives the fullest understanding of each and gives a rounding out of our knowledge of a group of animals that it is impossible to get by a study based only on either the fossil or the recent. A course of study based on such principles should appeal to the purely paleontological student and also to the zoölogical student as well; for it would be helpful to them in future work, whether that were in the line of the fossil or the living organisms. Such was the method of work laid down by the great master, Alpheus Hyatt, and followed by those who got their inspiration from him.

In an address of this nature, with many other speakers and limited time, only a few points can be touched on, and briefly at that. The value of stages in development exists, it is believed, through all organisms and also more or less throughout the life of all organisms, but here consideration will be given only to a few invertebrates. The adult of any given type presents a certain complexity of structure differing greatly with the type. How did it come to be what it is, and what are the meanings of the structural details in relation to associated forms? The adult came to be what it is by growth from the young, and during this growth characters are progressively added until the full species features are attained, and may even be continued up into the old age of the individual. Such additions of characters, or stages in development, are of first importance from the light they throw on structure, on morphological relations, and on genealogical affinities.

In Protozoa, as unicellular organisms, it has been thought that stages in development do not exist. However, J. A. Cushman has shown abundantly, in studies of recent and fossil Foraminifera, that there is a development of the test in which, by the addition of chambers and ornamentation of the same, a series of stages is presented quite as truly as may be seen in the higher groups, such as mollusks.

In the compound fossil corals, Pleurodictyum and Favosites, the late C. E. Beecher showed that the young has a simple cup, and during growth other cups were added progressively, with pore connections similar to the structure in adult Aulopora. Points of possible budding, where buds did not develop through mechanical lack of space, are indicated by the pores in the outer wall of the corallites of Favosites.

In his extensive studies of fossil and recent Brachiopoda, Beecher showed that from the development of the several groups we could gather a full understanding of the origin and structure of the shell from its early protegulum stage. He compared the protegulum with the adult of Paterina, from the Cambrian, as an archaic radicle. He elucidated greatly the structures of the deltidium, deltidial plates, and calcareous supports of the arms from their developmental stages, and, moreover, built up a satisfactory classification of the class, Brachiopoda, based on the ontogeny of representative types. The shell of the brachiopod being an external structure, much can be gathered as regards later stages from studying the lines of growth of the test, unless they have been destroyed by mechanical wear, or resorption in the enlargement of the pedicle opening.

Paleozoic Echini, with their numerous columns of interambulacral plates and often numerous columns of ambulacral plates as well, have been difficult to bring into accord with modern Echini, which have but two columns in each of the said areas. Jackson has definitely shown that, from a study of the test, passing from the ventral border dorsally and including a study of the young as well, new columns of plates are added progressively in the interambulacrum from one column ventrally (represented by only a single plate in all but Bothriocidaris) to two or more, up to fourteen columns dorsally, according to the type in hand. In the ambulacrum with two columns ventrally as the primitive condition, passing dorsally they may increase to four, six, etcetera, up to twenty columns of plates in an area in extreme forms. These changes in paleozoic Echini (when resorption has not removed any plates ventrally) may even be observed on a single well preserved specimen by studying the zones of plates from the ventral portion of the test and passing progressively dorsally to the mid-zone and finally to the apical disk, in which zone the new, last added plates appear.

The stages in development of both living and fossil Echini elucidate the structure of the adult and point to Bothriocidaris of the Ordovician, with a single column of plates in each interambulacrum and two columns of high plates in each ambulacrum as a primitive archaic radicle. The development of the ambulacrum in the Palæchinidæ demonstrates the genetic relations of the genera in that large family. Throughout the Echini stages were the main basis of an attempted natural classification of the group.

The test of a sea-urchin, while superficial, is really an internal skeleton, being covered by living tissue, so that the plates are capable of growth, or resorption, throughout life. Usually, however, the plates have suffered no material change in position or shape during growth, and by their position and number indicate marked stages. Dorsally in Echini, the young, last added plates, both interambulacral and ambulacral, may show youthful characters which, as localized stages in development, can be compared with characters seen in young specimens or in adults of more primitive types.

In the Pelecypoda, Jackson showed that the larval shell, or prodissoconch, of Ostrea, Avicula, Pecten, and allies, both in the structure of the hard and soft parts, points toward the ancient genus Nucula as a radicle. The stages in development of the shell of Avicula and Pecten show a series of changes which can be directly compared with the characters of the adults of ancient fossil forms which are allied to and presumably are ancestral forms of these genera. The development of the shell of Perna shows its aviculoid origin in the external characters of the test in the young and also in the development of a series of cartilage pits on the hinge-line from the youthful and also the primitive character of a single cartilage pit.

The Cephalopoda were the subject of a lifelong study by Professor Hyatt, and on them he based a large part of his philosophical conclusions on biology. In this group, stages in development are abundantly shown in the progressive coiling or uncoiling of the shell, all through the nautiloids and ammonoids. In nautiloids, taking a form which has in the adult a close-coiled shell like Coeloceras or Nautilus, we find that the young has first a straight shell for a very brief period, like Orthoceras; then an arcuate shell, like Cyrtoceras; then an involute stage, like Gyroceras; then it becomes continuously more involute until the adult form is attained. In the septal sutures of Ammonites, stages are strongly emphasized, the earliest septa being relatively simple, like early fossil Goniatites, and as one passes from the apex of the cone representing the young to the last built portion of the shell the sutures become progressively more complex until full differential characters are attained. In addition to the above, characters of the siphuncle, ornamentation of the test, and characters of the aperture all come in for their share in yielding stages in development to the student.

In Pelecypoda and Cephalopoda, as also in Gastropoda, the shells are for the most part external structures, and, as in the Brachiopoda, youthful stages may be studied by following lines of growth of the shell where these have not been destroyed by erosion. This is particularly true of the coiled Cephalopoda, where by breaking back the shell one can get youthful characters very perfectly preserved at the apical portion of the test, which has been protected from wear by the involute coiling.

Studies which Beecher made on the development of the Trilobita opened up our knowledge of the group so as to make it a new thing. Here, as the animal molted the test at each period of growth, we have to get the actual young and individuals preserved at just the right period of growth in order to obtain successive stages in development. In the early protaspis stage the body consists of a cephalon and pygidium, with no thoracic segments and, except in accelerated types, with no eyes or free cheeks visible from the dorsal side. Later, thoracic segments appear, one at a time, with successive molts, the eyes and free cheeks travel in, ornamentation and other characters appear progressively until full species features are attained. Primitive trilobites (Ptychoparia, Sao) show a relatively slow development and specialized types (Acidaspis, Arges) an accelerated development. This same principle holds widely with primitive types, which retain stages in development with remarkable pertinacity. On the other hand, specialized types have such quick development that stages are often run together or telescoped, or even may be skipped altogether. As a result of his beautiful studies, Beecher made out a classification of the Trilobita based on ontogeny—a great achievement to attain in so difficult a group.

Stages in development as applied to fossil animals, or, better still, to a combined study of fossil and recent, open up a field of research which is almost unlimited, and the work that has been done in this line, especially by the Hyatt school of investigators, shows that this method of study throws great light on the important lines of anatomy, morphology, and phylogeny.

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RESEARCHES ON SEDIMENTATION [1]

BY THOMAS WAYLAND VAUGHAN

(*Read before the Society December 31, 1919*)

CONTENTS

Purpose of this Symposium

The purpose of this symposium is not to give an inventory of present information on sedimentation, nor is it to lay before you the definite solution of specific problems; but it is rather to discuss certain needs in the light of what we now know, and to consider ways and means of filling those needs. Instead of "symposium," perhaps "conference" would be a more appropriate name, for geologists should confer regarding this matter, and it is hoped that discussion will be full, and that those here will give others the benefit of many suggestions.

Object of Researches on Sediments

Since geologists generally will admit that one of the objects of the investigations of sedimentary rocks and processes of sedimentation is to aid in understanding the history of the earth, it is pertinent to ask how such researches can help in the solution of problems of that kind, and the inquiry may begin by asking about any sedimentary rock certain questions:

1. What is the rock, what are its constituents, and what are their

[1] Manuscript received by the Secretary of the Society February 2, 1920.
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This paper is one of a series composing a symposium on sedimentation.

physical characteristics? The answer to this question would include information on the chemical and mineralogic composition of the rock, the sizes and the percentage of the different sizes of the particles composing it, the shapes of the particles, the percentage of pore space, and the percentage of particles of different kinds.

2. What were the sources of the constituents of the rock? The answer to this question would tell whether the constituents are clastic, organic, or chemical in origin; or, if the constituents have been derived from more than one source, as usually is the case in sedimentary rocks, what the proportion of each kind is according to its source. In the case of clastic constituents, information would be given on nature of the parent rock, where it was, its topographic features and relations, and by what processes it was disintegrated. In the case of constituents of organic origin, the kinds of organisms, the relative importance of each kind, and the relative importance of the total organic constituents would be ascertained. In the case of chemically deposited constituents, the relative importance of the different constituents and the processes by which they were formed would be made known.

3. How were the constituents brought to the place of their deposition? The answer to this question would let us know whether the transporting agent was wind, flowing water, ice, or gravity without the help of these agents, or whether two or more agents acted jointly. We should also know the velocity of the transporting agents and their capacity for moving material.

4. What were the factors that caused deposition; what were the agencies that caused the particular arrangement in beds, laminæ, etcetera, exhibited by the deposit? Deposition may be caused by checking the velocity of a transporting agent, by the flocculation of particles, or by crystallization from a supersaturated solution. If this question were answered, we should know not only the part played by each of these processes, but should also know how currents were checked, the processes involved in the flocculation of the particles, and the nature of the solutions from which crystallization took place. Information would also be given on rates of deposition.

5. Under what conditions not necessarily factors in origin, transportation, and cause of deposition did deposition take place? The answer to this question would comprise information on the chemical composition of the water, in the case of aqueous sediments; the depth of the water; the relations to land areas—distance from the land, the topographic features of the land, and the climate of the land; and whether the deposit was made on a stationary, rising, or sinking basement.

6. What changes has the sediment undergone after its deposition and what are the causes of those changes?

If all these questions regarding any one geologic formation could be answered fully, we should have made a considerable advance in interpreting the history of the earth, for such information could be applied in interpreting other formations; but it is safe to say that very few of the questions can be answered for any formation; it is also safe to say that some of the questions can not now be answered for any geologic formation, because the answers depend on deductions from premises that must be, but have not yet been, inductively established. In the investigation of such subjects as sediments the general method of modern science must be followed—that is, we must first build premises inductively, and then by deduction from those premises interpret phenomena that can not now be observed.

Modern Sediments

In order to understand sediments, both modern and ancient must be studied, but a reliable basis for interpreting ancient sediments can be obtained only through a study of sediments now in process of formation.

Researches on modern sediments should cover the following kinds:

1. Continental deposits. These embrace (*a*) colluvial, fluvial, lacustrine, spring, and progradation deposits in cold, temperate, and torrid climates of humid regions; (*b*) eolian, talus slope, and water-laid deposits in desert plain and mountainous regions and in desert basins; clastic, organic, and chemical deposits in inclosed lakes; (*c*) glacial deposits.

2. Great lake deposits.

3. Marine deposits. (*a*) Deposits, mostly detrital, formed under diverse conditions, at the edges of coastal plains and off steep shores, as follows: In protuberant deltas, in bays, and in areas between embayments, in each major climatic zone; (*b*) deposits, mostly organic and chemical, on submarine plateaus, in lagoons, and on reefs, in tropical and subtropical waters; (*c*) the origin and distribution of the deeper-water marine sediments. A few subjects of importance in the study of marine sediments are the relations between certain abyssal deposits in the ocean (particularly red clay and manganese nodules) and the physico-chemical condition of ocean water; changes in outline and position of shorelines; direction of the movement of littoral drift; influence of depth, distance from shore, etcetera, in determining the character and continuity or lack of continuity of deposits; the effect of different degrees of salinity of the

water; the ratio of wave amplitude to symmetrical ripple-mark amplitude; the bathymetric and thermometric ranges of marine organisms.

This statement regarding modern sediments is not intended to be exhaustive; its purpose is to indicate what the scope of researches on such deposits should be. The aim of these researches is not only to understand present processes, it is also to discover and to make available for use criteria to be applied in the interpretation of ancient sediments.

Ancient Sediments

Researches on ancient sediments should include, besides seeking the general information needed for all sediments, studies of the lateral variations of geologic formations, the causes of cementation, the formation of concretions, the development of secondary stratification planes, replacements, vein-fillings, salt domes, etcetera—in fact all diagenetic changes of sedimentary rocks should be studied. The origin of dolomite is an important problem. Among important economic geologic resources that offer fields for further research are deposits or accumulations of coal, oil, gas, phosphate, potash, nitrate of soda, iron ores, manganese, etcetera. No attempt should be made to distinguish between researches into products of ascertained economic value and researches into products that are not commercially important, because knowledge of both kinds of products is necessary for understanding geologic history.

From the brief statements that have been made, it is obvious that an adequate knowledge of sediments requires diversity of research in the fields of physical, chemical, physico-chemical, and biologic sciences.

Inadequacy of some earlier Investigations of Sediments

Although many valuable researches have been made on sediments, the statement that no sedimentary deposit is adequately understood will probably not be contradicted. The reason of this is that the results of an insufficient number of lines of research have been brought to bear on the problem of the origin of any particular sedimentary formation. The inadequacy of several researches will be briefly considered.

Certain deposits are practically 100 per cent organic remains, as, for instance, those on the coral reef of Murray Island, Australia. Knowledge of the chemical composition of the deposits, of what groups of organisms contributed material to them, and of the proportion contributed by each group of organisms is valuable; but this information alone is not adequate, for the material has been subjected to sorting by moving water, with the result that very nearly all particles below a certain size have been removed. Knowledge of the transporting power of water of a certain density at certain rates of motion is necessary for understanding the sizing of the deposit. The part played by other factors, physical and physico-chemical, also needs to be known.

Mechanical analyses of deposits are indispensable in the study of sediments, but they alone will not supply all the information needed for the interpretation of the history of any deposit. The same may be said of other physical features of sediments, such as shapes of particles, relative amount of pore space, size of pores, and total surface of particles per unit volume, all of which is necessary information. These physical determinations do not take into consideration other factors, chemical and biologic, that have played rôles in the origin of most sediments.

Very valuable researches have been conducted on the chemistry and physical chemistry of sediments and sedimentary processes, but they do not solve the problem of the origin of any sedimentary formation. Clarke and Wheeler have shown that the difference in the chemical composition of the skeletons of marine invertebrates and calcareous algæ is the determining factor of the difference in the chemical composition of certain marine sediments, but Professor Clarke himself emphasizes, in conversation, the inadequacy of such analyses alone to solve problems of sedimentation. With reference to investigations in physical chemistry: Should we know precisely the relations of CO_2 in sea-water to depth, temperature, and other physical factors in the ocean, and to the CO_2 content of the air above the ocean, we should not solve the problems presented by any sediment, but without such knowledge the origin of certain deposits can not be understood. The reason of the failure of this information to solve the problem is because it concerns only one of a number of factors involved in producing any particular sediment.

In order to understand the history of certain sedimentary rocks, it is necessary to know the causes, in many instances of physico-chemical nature, that have produced changes in the sediments. Among such changes are silicification, phosphatization, and dolomitization. But a complete knowledge of the processes whereby such changes were brought about will not suffice, because the origin of the unchanged sediment needs to be explained, and it may have undergone other changes than one of those mentioned.

Other instances of valuable chemical and physico-chemical researches might be given and the reason of their inadequacy for the interpretation of sediments indicated, but those mentioned seem sufficient to illustrate the point in mind.

Researches such as those enumerated, and the number might be greatly increased, have given insight into some of the factors and principles involved in the origin and subsequent changes of sedimentary rocks. One important result of these investigations is to make clear the complexity of the phenomena; another important result is to make it clear that an adequate basis for interpreting sediments will not be established until all the needed lines of research have been brought to bear in proper coordination on the different classes of sedimentary deposits.

Suggestions as to the Means of furthering Researches on Sedimentation

The discussion so far has presented the object of researches on sedimentation in their geologic bearing; it has indicated the range of phenomena that should be covered; and it has pointed out that certain valuable researches have not been completely satisfying in their results. An attempt will now be made to bring forward some suggestions for increasing activity in this field of scientific endeavor and for providing additional agencies. The factors entering into the origin and the deposition of sediments and into the changes that take place in them after their deposition are so diverse and are so widely distributed that it is impracticable for any one man, any small group of men, or any organization of moderate size, especially one whose activities are areally limited, to cover the field. All possible activity needs to be encouraged and all possible agencies should be utilized.

According to the opportunities of individuals and organizations, three categories of researches may be recognized, as follows: (1) Researches on specific phenomena; for instance, Gilbert's "Transportation of debris by running water" and Harder's "Iron-depositing bacteria and their geologic relations"; (2) researches covering particular areas, such as Thoulet's study of the deposits in the Gulf of Lyons, Kindle's studies of the Great Lakes, and the work being done in San Francisco Bay by the geologists of the University of California; (3) complex investigations, requiring the cooperative and coordinate researches of physicists, analytical chemists, physical chemists, and biologists (including bacteriologists). There is no good instance of such an investigation, for there has been no institution established for conducting such researches. I attempted a piece of work of this kind in the study of the marine bottom deposits of Florida and the Bahamas, but, although valuable data were procured, the results obtained are inadequate. Sir John Murray had a similar ideal in mind in investigations conducted at the Granton Marine Laboratory, and he assuredly accomplished important results, but he did not attain the desired goal.

According to agencies already existing whereby researches on sediments might be conducted, the following categories can be recognized: (1) National institutions; (2) State institutions; (3) privately endowed institutions; (4) universities.

Of the national institutions of the United States those that should be mentioned are the Geological Survey, the Bureau of Fisheries, the Coast and Geodetic Survey, certain bureaus in the Department of Agriculture, and the National Museum. The Geological Survey should cover a wide range of phenomena, and that a subsection on sediments has been established in it is at least promising. The Bureau of Fisheries is systematically collecting bottom samples, and as part of its oceanographic work it has inaugurated a survey of the hydrogen-ion concentration in the sea, in addition to acquiring data on the temperature and salinity of the water. The Coast and Geodetic Survey collects bottom samples. In the Department of Agriculture the Bureau of Soils has made for me alone hundreds of mechanical analyses of sediments, and the Bureau of Plant Industry has greatly helped, particularly through the work of Kellerman on calcium-carbonate-precipitating bacteria. The United States National Museum receives the samples of modern sediments collected by the different governmental agencies, but it has no staff for the study of them. Although the Federal Government has done and is doing much to further knowledge of sediments, it is not doing so much as might reasonably be expected of it. The Geological Survey of Canada, through Doctor Kindle, is making valuable contributions to knowledge of sediments in the Great Lakes and in some of the marine waters of Canada. This work should be encouraged and extended. If practicable, the help of others of the Canadian institutions should be enlisted in the prosecution of such investigations.

Of the State institutions the State geological surveys would naturally be expected to render much service. They have made important contributions and it is hoped others will follow. State surveys should be able to conduct researches on specific phenomena and on particular areas, such as a more accurate analysis of the physical characteristics of formations and the variations of formations in all three dimensions. It may also be practicable for them to conduct researches on special types of deposits that are well exemplified within the State boundaries, such as eolian, fluviatile, lacustrine, embayment, and beach deposits. Other kinds of deposits might be mentioned, but those named will serve as samples. It is suggested that it might be well for each State survey to take into consideration the kinds of researches it could profitably undertake and make arrangements for the prosecution of at least some of them.

Of the privately endowed institutions, the possibilities of certain departments of the Carnegie Institution of Washington come immediately to mind. The Geophysical Laboratory has already put students of sediments under obligations to it through the researches of John Johnston, H. E. Merwin, and E. D. Williamson. Perhaps further researches into problems in sedimentation may be done by it. The Department of Botanical Research has issued a volume of much geologic value in its monograph on Salton Sea. Further researches of this kind are needed, and Doctor MacDougal should be urged to do all he can toward making known the geologic history of the inclosed lakes of the West, including the part plants play in the deposits forming there. Much work has been done on sediments in connection with the Department of Marine Biology, largely in cooperation with the United States Geological Survey and other national institutions, and other work of value is in progress. The work that is being done should be encouraged. Mention should also be made of the studies on diatoms by Dr. Albert Mann, who is now an associate of the Carnegie Institution of Washington.

Besides the researches conducted by at least three departments of the Carnegie Institution, it may be possible to arrange for more work on sediments at some of the marine laboratories. There are, for instance, the Scripps Laboratory of the University of California and the laboratory to be established in connection with the Bishop Pauahi Museum of Honolulu.

There are many problems in sedimentation that can be advantageously attacked in the universities; there are both problems dealing with specific phenomena and those dealing with particular areas. The University of California has set a good example by its studies of San Francisco Bay. At Harvard Professor Sayles has published an important memoir on seasonal deposition in aqueo-glacial sediments, but Harvard might do more and attempt work on Boston Bay and the Gulf of Maine. There are other aqueous sediments in this part of the country deserving critical study, and the sand-dunes of Cape Cod are worthy of monographic treatment. There are accessible to every university opportunities for research that will not soon be exhausted. Some universities are favorably situated for the study of the physical characteristics of geologic formations, and as instances of such work Goldman's paper on the Upper Cretaceous sediments of Maryland and a research by Prof. Leonard P. Dove, of the

University of North Dakota, on the Upper Cretaceous shales of that State may be mentioned. The studies of Prof. J. Claude Jones on the lakes of Nevada constitute an instance of another kind of research.

I will mention a special research of particular importance now in progress. Prof. Shiro Tashiro, of the University of Cincinnati, is trying to ascertain the physico-chemical factors controlling the formation and determining the chemical and mineralogic composition of the skeletons of marine organisms. This is really carrying one step farther the researches of Clarke and Wheeler, of the United States Geological Survey, and already there is promise that we shall soon have a more accurate basis for deducing the physical and chemical conditions under which certain deposits containing marine organisms were formed. Broadly synthetic work, such as that of Barrell, is of value and more of it is needed. The speakers who will follow me will make other suggestions regarding researches in universities. The possibilities are great. May our universities see fit to put a certain amount of their energy into studies of problems of sedimentation.

An Institution for the Study of Sedimentation

I think it will be admitted that all the kinds of researches indicated for the different existing agencies that might be utilized are really needed, and as much as possible should be done to accomplish the desired ends, but I will recall that, under a category described as "complex investigations requiring the cooperative and coordinate researches of physicists, analytical chemists, physical chemists, and biologists (including bacteriologists)," it was stated that no example of a satisfactory piece of work of this kind could be given. The reason for this is that there has been until now no institution in which a comprehensive, coordinated study of sediments and processes of sedimentation could be made. Valuable, more or less incoherent work, considered from the viewpoint of the interpretation of sediments, has been done by field geologists, oceanographers, engineers, students of soils, analytical chemists, physical chemists, and biologists, but the attempts of investigators to bring to bear on particular problems the results of researches along many lines are few indeed. The reasons of the paucity of such attempts are twofold: The first is that the object of the individual investigator usually has been to solve specific problems and not to consider sediments in a comprehensive way. The second is that there has been no institution or place wherein all the lines of research needed for the solution of problems offered by any sediment could be prosecuted. Without the existence of a definitely organized institution, the only way the results of multiple-lined research can be directed to the solution of a particular problem is either by the compilation of the results of dissociated studies or by enlisting the cooperation—as a rule, the voluntary cooperation—of specialists. Under a system of voluntary, mostly unremunerated, cooperation, it is usually—in fact, nearly always—impracticable to hold a group of specialists together until the desired result is assured. The fundamental basis of continuity of work, which is definite association with an established institution, is lacking. Under present circumstances, the desirability of the establishment of an institution for the coordinated study of sediments and sedimentary processes in the broadest practicable way is obvious.

An attempt is now being made to bring about the realization of this aim, and the members of the committee that I have the honor to represent trust that we shall have the support of the geologists of the country.

Conclusion

In conclusion, I wish to emphasize the purpose of the investigation of sediments as a part of the history of the earth. Such work does more than supply the foundation for stratigraphic geology—it includes stratigraphic geology. It tries to reconstruct the geography of the earth for each of the successive periods of the earth's history since sedimentation began; to ascertain the boundaries of land and sea; to trace the rises and the sinkings of the continents, the growth and the destruction of mountains, and the waxings and the wanings of the seas; to reconstruct the climates and the physical features of the lands of the past and to populate their surfaces with the life that then existed; to bring before our vision the seas of the ages gone by, so that we shall see what was happening in them and know the old oceans, with their currents, their temperatures, their depths, the composition of their waters, and the organisms that inhabited them. It tries to bring back the reality of what was long ago and follow the changes that have taken place on earth.

These investigations are the counterpart of researches into the physical constitution of the earth and into the forces that express themselves in the raising and lowering of continents, the building of mountains, and in the intrusion and extrusion of certain parts of the earth's crust as molten matter. If we would know the earth, the two kinds of research must advance hand in hand.

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PHYSICAL AND GEOGRAPHIC CRITERIA IN THE STUDY OF SEDIMENTARY DEPOSITS[1]

BY E. W. SHAW

(Read before the Society December 31, 1919)

CONTENTS

EXTENT OF THE FIELD

Apparently by common consent the present symposium covers the study of the formation of sedimentary deposits from the immediate or even remote origin of the component materials to the destruction of the deposit—that is, not only sedimentation, or the process of deposing sediment, but also the nature of the ancestral rocks and the complex processes of weathering, plucking, transportation (direct, interrupted, repeated, and reversed), deposition (usually repeated), diagenesis, and all the other processes in the entire cycle of transformation from igneous or metamorphic rocks to metamorphic or even igneous rocks. It includes the multitude of physical processes, agents, and results involved in the formation of a clastic deposit; and most sedimentary rocks are to some degree clastic. The field thus overlaps that of erosion, particularly with regard to transportation.

[1] Manuscript received by the Secretary of the Society February 14, 1920. Published by permission of the Director of the U. S. Geological Survey. This paper is one of a series composing a symposium on sedimentation.

Objects of Research

The main reason for studying the physical or any other characters of sedimentary deposits is to acquire knowledge, useful for practical and scientific purposes, on two general subjects, namely, paleogeography and present geography. We are all striving to meet the definite need of a better understanding of both the history of the earth and the present activities of natural forces. The two subjects are complementary and to a certain extent mutually dependent, but full knowledge of one will not supply or relieve the need of knowledge of the other. Even the old dictum of the present being the key to the past must be used with caution, for the conditions of the present were not exactly duplicated during former epochs.

The studies should obviously include many of direct economic bearing, some of probable economic bearing, and a few of no evident practical use. Studies of the physical properties of present-day sedimentary deposits and of the physical processes involved in their formation and modification are applicable in problems of river and harbor improvement; valuable deposits of gravel, sand, clay, salts, greensand, guano, etcetera; culture of sea foods; water supplies and sanitary problems; military problems; the silting up of reservoirs; swamp and desert reclamation; hydraulic engineering projects; and land title and political boundary questions, such as often depend on the processes of accretion and avulsion. The economic value of the study of ancient sediments lies in the determination of the origin, mode of accumulation, and extent of various kinds of mineral deposits, including underground water supplies, and the relation of structure and texture to bearing value and durability. The results have other economic applications that serve very properly as an added attraction to the investigator.

Concerted Efforts

Perhaps rapid progress in sedimentary rock petrology is retarded not because of lack of able investigators, or lack of interest, or lack of material with significant features, but because of the difficulty of making progress that can be coordinated. When a geologist goes to the field and studies certain sedimentary formations, he notes the kinds of rocks, makes a more or less accurate and expressive description of their physical character, their stratification, color, thickness, granularity, etcetera, but a large number of more or less obvious features are undescribed because the descriptions are of no apparent value. What a difference there would be in procedure if the geologist knew that if he observed and noted, for example, the precise tint of a stratum or the details of mechanical constitution, he might be able (1) to recognize the stratum when other criteria are not available; (2) to decipher some part of its history; (3) to discover features of economic value; (4) to record a possibly useful observation in a place where it is available. Under present conditions, when we turn to a note book for data on a certain point we often are convinced either that the notes are inadequate or that they are clogged with observations of little significance.

It is evident that conditions could be greatly improved and progress rendered more facile by coordination and centralization. So long as each investigator is left to work independently, the advance of the science will, though real, be relatively slow. Some investigators will reason from the established principles of geology, physics, and chemistry, and describe some of the kinds of events and products that we may expect to find, pointing out deposits that may furnish examples and opening up large fields of investigation. Paleontologists will study fossils and, with the help of data concerning character and distribution of certain deposits, will add to our knowledge of paleogeography. Oceanographers, engineers, and others will give us new information about the beds of seas, lakes, and streams.

But what would be the outcome if all these activities were encouraged and coordinated? One result would be the study of the extensive collections of sea-bottom materials that have been carefully made by the United States Coast and Geodetic Survey and the United States Bureau of Fisheries and preserved by the National Museum. Perhaps some man or group would demonstrate what can be done in some particular field, such as classification of rocks, paleogeography, study of deep-well drill cuttings, and thus stimulate other men and organizations.

Classes of Investigation

Since there seems to be general agreement that research in sedimentation should occupy a prominent place on the program of advance in science, the question arises: What shall be the nature of these investigations and, particularly at the present moment, what shall be the nature of the physical investigations? Some of the ends to be sought are mentioned in preceding paragraphs and in other papers.[2]

[2] J. A. Udden: Mechanical composition of clastic sediments. Bull. Geol. Soc. Am., vol. 25, no. 4, December, 1914, pp. 655-744.

E. W. Shaw: Present tendencies in geology: Sedimentation. Jour. Wash. Acad. Sci., vol. 9, no. 17, October, 1919, pp. 513-521. The other papers in the present symposium.

The physical investigations that have been considered seem to fall into the following general classes:

1. Field and laboratory studies of the action of present oceans, lakes, streams, glaciers, wind, gravity, and frost, and the various products of these agencies.

2. The application of physical and geographic criteria to the study of geologic strata and formations—their extent, thickness, stratification, mechanical and mineralogic constitution, and color, and variations in these physical characters.

We need especially to know (1) more about the mechanical constitution of various kinds of strata and the extent to which their origin is recorded therein; (2) more about the areal extent and variations of specific ancient and modern strata, particularly those that are more or less closely related to each other; and (3) more about the physics and physical chemistry of the forces that affect sediments before, during, and after deposition. Organized and coordinated efforts in these studies should obviously bring forth many new facts and principles that would be acquired slowly, if at all, by individual effort.

Mechanical Analyses and their diagrammatic Representation

Studies of the mechanical constitution of sediment can be, and are being, made by individuals, but the results are of little value because the equipment and methods are not standardized. The work is handicapped not so much because of the impracticability of a high degree of precision as for lack of equipment giving separates of approximately known range in size that bear a uniform ratio to each other and constitute analyses that are comparable. Proximate analyses made by an experienced man are comparable with each other, but the results of two analysts may not be comparable, unless the methods of the two are carefully standardized at the beginning. For this reason it would seem desirable to establish at first a single central laboratory where most collections would be examined and reports made on them.

What is the value of a mechanical analysis or of a set of mechanical analyses? It depends, for one thing, on the care used in collecting the material. The sample for each analysis should, to meet most purposes, represent only a small fraction of an inch of thickness and should come from an area of a very few square inches. The object is, of course, to get grains deposited under one set of conditions.

Aside from the determination of commercial values of deposits and the furnishing of identification characters, perhaps the main value in mechanical analyses lies in their promise of furnishing a record of some events in the earth's history. Udden[3] and others have already pointed out some significant features and relationships, such as the high perfection of sorting of wind-blown and beach materials and the significance of position and magnitude of secondary maxima in the diagrammatic representation of the analyses. Groups of analyses of the same or closely related materials furnish characteristics not made clear and reliable by single analyses.[4]

Mechanical analyses are most easily used when plotted in diagrams, of which there are three common types—direct, cumulative, and logarithmic. Each type has its special uses, and it will probably be found desirable to use all three for most sets of analyses. The composite cumulative plot or monogram for a group of closely related samples seems especially likely to display diagnostic features and thereby furnish reliable data concerning the origin of the deposit.

However, what can be done with analyses plotted in any form is still largely unknown. The field is, for certain definite reasons, to be looked on as one of great promise. Several significant features have already been discovered and the variety in analyses is so great and of such a nature that it is almost certain that other significant features can be discovered. We need additional and more detailed and standardized analyses of material deposited under various known conditions.

Reference Collection

If a central laboratory were established the need would immediately arise for a representative collection of samples. Certainly a portion of each of the specimens analyzed should be preserved and, if possible, a portion of each fraction separated in analysis. Also, it would seem to be highly desirable to assemble a reference collection of sedimentary rocks that would be representative, both geographically and lithologically. True, every deposit seems to differ from every other and a collection containing a specimen of each variety may be impracticable, but if we had a dozen well selected specimens from every county in the country, might it not be well for the collection to be of the same order of magnitude as collections of fossils, minerals, or igneous rocks? The specimens should, of course, be large enough to supply some working material; some specimens should weigh two or three pounds, others need weigh only a few ounces. Generally a thimbleful of fine sand or silt is sufficient for an analysis.

[3] J. A. Udden: Loc. cit.

[4] E. W. Shaw: The significance of sorting in sedimentary deposits. Bull. Geol. Soc. Am., vol. 28, no. 4, pp. 925-932.

Need of Classification and Nomenclature

The collection, together with mechanical analyses, might furnish a foundation for that goal that seems at times hopelessly inachievable, a satisfactory classification and nomenclature of sedimentary rocks. These are, is seems to the writer, very essential foundation stones for the upbuilding of any science. It is a mistake to say that it does not matter whether or not you have a name for a thing or a diagram or an index where its name can be placed in an expressive position, or even that it matters not what the name is. What would have been the advance in mineralogy or paleontology without names, and what further advance might they not have realized if all the names had been carefully fashioned handles, easy to grasp, retain, and wield! Numerous examples might be cited of the coming to grief of an overzealous student of physiology, histology, or ecology who thought he could dispense with taxonomy and nomenclature. The gumbotil has been observed and more or less well described over and over again, but, whether or not it is a matter of cause and effect, it seems to be a fact that progress in understanding the deposit dates from its christening. (There is a rather complex bit of psychology involved that can not be further discussed here.)

Areal Surveys

Areal studies of ancient sediments have already been carried as far as or farther than those of present-day deposits, notwithstanding the fact that they are on the whole less accessible, considerable portions being unexposed and not yet reached by the drill. The work on ancient sediments has been done largely through the individual enterprise of such men as Salisbury, Lee, Barrell, Willis, and many paleontologists; and, as Blackwelder[5] has emphasized, much more can be done if facilities are afforded and encouragement offered.

Detailed areal surveys of ocean, lake, and river bottoms are few and of small extent, and some of the results have been achieved by engineers who either do not publish their observations or else report them in documents not often consulted by geologists. Excellent material on the bed of portions of the Mississippi, for example, can be culled from the United States Engineers' reports on river improvement. Other classes of investigators have made numerous observations on the sediment that is accumulating in certain restricted areas, but have not carried the work to the point of constructing an areal map. The work done by Vaughan and

[5] Eliot Blackwelder: Memorandum to T. W. Vaughan quoted in "Present tendencies in geology: Sedimentation," E. W. Shaw, Jour. Wash. Acad. Sci., vol. 9, no. 17, October, 1919, pp. 513-521.

others, for example, among the Florida Keys, by Louderback and others in San Francisco Bay, and that by the Bureau of Fisheries constitute excellent and very full preliminary requisites and fall barely short of areal surveys. One can scarcely foresee the nature and value of the results of well organized expeditions that have such surveys as their primary object.

Laboratories and Equipment

Since one of the main fields of laboratory work will probably be mechanical analyses, and since there is a peculiar need for standardization of such analyses, it would seem desirable to plan for one central fully equipped establishment. More or less laboratory work can, of course, be profitably undertaken anywhere, but a recognized leader would bring about a rate of progress that would not otherwise be realized. After standards are well established, it may be that all kinds of work can be carried on profitably at various independent places, but at present the great need is for standardized screens, standardized centrifuge, and standardized microscope work. Most tests made with commercial screens are worth little (1) because only the number of wires to the inch is known, (2) because in many screens the wires are not uniformly spaced, (3) because different methods will give widely different results with the same set of screens, and (4) because the sizes of openings in the successive screens do not bear a fixed ratio to each other.

It would be relatively easy to devise for sands and gravels a standard set of field screens that would give results of fair accuracy. The principal shortcoming would arise out of differences in method of use, but this might be largely obviated by screening directions. Whether the openings should be round or square has not been settled to the satisfaction of all, but in any case they should certainly bear a fixed ratio one to another, and this ratio should be some root of 2 or 10.

Some of the Problems

Among the problems awaiting attention are many concerning the source and destiny of eroded materials. Sometimes we know not whence the deposit came. For example, the Pottsville formation of southern Illinois is thick and contains quartz pebbles. Does this prove orogenic movement and near-by mountains? There is almost certainly no adequate source for the pebbles within hundreds of miles.

Again, the Mississippi Valley loess is commonly thought to have come from the floodplains of the Mississippi and some of its tributaries, but preliminary computations indicate that the volume of the loess is too great to harmonize with this inference. At other times we do not know where the material has gone when removed from eroded areas. More often we know of several possible sources for parts of several related clastic deposits.

In many questions isostasy and gravity anomalies are involved. Can we determine the approximate thickness of light, unconsolidated sedimentary deposits through determinations of intensity of gravity? Is the sinking of the great sedimentary basins due to loading alone or in considerable part to some other cause? Loading will, no doubt, cause subsidence, and subsidence will, no doubt, lead to deposition, but how can either take place without the other as a cause? The process would seem to be self-accelerating, if it could only be started. Does great thickness show orogenic movement near by? May it not be produced in other ways, as by the filling of a basin, as by shore drift—a basin that is gradually sinking without the accompaniment of mountain-making?

What are the peculiarities, particularly as to mechanical constitution, of those clastic deposits ordinarily spoken of as chemical and organic, though the material after precipitation has been transported, broken up, and sorted? To what extent were the so-called estuarine deposits laid down in estuaries?

What are the conditioning causes and mode of development of bedding planes and of joints? What is the exact nature of cementation of sand, and why, except where quartzite develops, does it usually proceed only to a limited extent and then apparently cease? Why are oil sands less well cemented than other sands? Was the oil present soon enough to check cementation, or did it select uncemented sand because of larger pores, or has it some way of disintegrating sandstones? What part does capillarity play in the formation of an oil or a gas pool?

What are the controlling factors of recrystallization, replacement, the growth of concretions, nodules, and geodes? What are the effects of various pressures in the physical chemistry and geophysics of the sedimentary basins? Why are formations generally so uniform in coarseness, and how do coarse materials get far from shore? To what extent must we modify such conclusions as that when a formation becomes finer in grain the direction of increasing fineness coincides with the direction from the old shore or from the source of sediment?

In conclusion, let it be stated that enough has already been accomplished through individual initiative to show that valuable economic and other scientific results are to be had through the study of the physical processes involved in the formation of sedimentary deposits, and that organized attack promises to bring about much more rapid progress in the science.

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
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CHEMICAL RESEARCHES ON SEDIMENTS[1]

BY HERBERT E. MERWIN

(*Read before the Society December 31, 1919*)

CONTENTS

GENERAL CONSIDERATIONS

The chemical problems growing out of studies of sedimentary rocks must, even more than other problems, be viewed from many angles. Each such problem is likely to lead to several highly specialized studies in restricted chemical fields. The mere statement of any one of these special problems will often require an extended study of its general chemical, geological, and biological setting. Because of the special nature of these chemical problems, no general outline for their study needs presentation here, but it may be worth while to present some viewpoints from which the problems may be seen.

From the standpoint of physical chemistry many of the problems of sedimentary rocks are especially difficult, because the solutions concerned may be very dilute and very complex; the solid phases are often ill-defined physically, or are stable at temperatures so little above the ordinary that heat to speed up reactions can not be effectively applied; or unstable forms may appear and persist indefinitely.

An important chemical consideration respecting the consolidation or recrystallization of sedimentary rocks is the effect of differential stress. Adjacent grains are partially separated by films of liquid. These films are so thin in places that they are able to transmit stresses from grain to

[1] Manuscript received by the Secretary of the Society March 3, 1920.
This paper is one of a series composing a symposium on sedimentation.

grain; on the other hand, they do not greatly impede the migration of ions. Material which is thus and otherwise forced into solution by unequal stresses can be readily deposited on parts of the grains which are less stressed or in open pores. Cementation, accompanied by diminution of pore space, results, and smaller grains tend to disappear.

Diffusion of the constituents of one crystal into another crystal which is in intimate contact with it has been proposed from time to time as a factor in petrogenesis, but I have seen no adequate discussion of the mechanism. Diffusion could hardly be expected unless the diffusing material can have a definite place in the crystal structure into which or through which it diffuses. Thus, dolomite and siderite might interdiffuse to form mix-crystals of intermediate composition. Diffusion should be slower in hard than in soft minerals and should increase with rising temperature.

A crystal which is forming or growing in a complex solution concentrates and holds at its surface certain of the constituents of the solution. The smaller the crystal the greater this tendency. Commonly in cases of very slightly soluble substances the nuclei of incipient crystals become covered to such an extent with this (adsorbed) material that definite crystals do not grow, but the nuclei and adsorbed material become aggregated into "amorphous" or fibrous minerals. More or less of the surrounding solution is usually enmeshed in the aggregates and is later partly replaced by other matter. The elucidation of the genetic relationships of materials of this character, such as chert, bauxite, glauconite, hydrated oxides of iron and manganese, garnierite, phosphate rocks, etcetera, will require much cooperative study.

In some cases slightly soluble substances may form nuclei which do not at once become sufficiently aggregated as to be held by such filters as are ordinarily used in water analysis. Such material may be transported to considerable distances and finally be precipitated quite otherwise than if it had been in true solution. Silica especially should be considered from this viewpoint.

The formation of nuclei does not take place until a certain supersaturation, varying greatly under different conditions, has been reached. Such supersaturation may be so great that the solution becomes saturated with respect to a second crystalline substance, and the second substance may appear and continue to crystallize to the exclusion of the first, which is the more stable. The crystal structure which is to result from the growth of the nuclei as compared with the structure of the ions and the complex atomic groups of the solution must help to determine whether stable or unstable phases appear.[2] Both may be forming at the same time, but the relative rates of growth may be overwhelmingly in favor of the less stable form. As soon as active growth of the deposit has ceased the supersaturation which allowed the growth of the unstable form may be reduced so as to permit the more stable form to continue its slow growth at the expense of the less stable form. The factors controlling the rates of formation of substances (besides the well known temperature effect) are little known. Numerous reactions involving liquids and gases are known to be greatly speeded up by the presence of certain substances, called catalysts, which furnish no material to the substances which are formed. Almost nothing is known about that action of catalysts which finally results in increasing the rate of growth of crystals from solutions.

Some illustrative Problems

DOLOMITE

Knowledge of the chemistry of dolomite goes scarcely farther than inferences from the relations of this mineral as revealed from geological studies. It is apparent that in some localities calcium carbonate reacts with sea-water to produce dolomite; furthermore, the sea is, and probably has been, the only adequate source of magnesia for the great dolomite formations. Many of the dolomites do not bear evidence of being replacements of limestones. Thus arises the question whether or not they formed as primary deposits through direct precipitation, either chemical precipitation or precipitation through the intervention of minute dolomite-producing organisms of unknown characteristics. Is dolomite and not calcium carbonate the stable phase in presence of sea-water under the various conditions of temperature of the oceans? Would a slightly higher ratio of magnesia to lime in sea-water cause either a chemical precipitation of dolomite or a rapid alteration of calcium carbonate to dolomite? Or is dolomite one of those substances which at ordinary temperatures forms very slowly, in solutions in which it is stable, unless a catalyst is present? Is the general similarity but greater complexity of the crystal structure of dolomite as compared with calcite a dominating cause of the slow reaction of calcite to form dolomite?

Inasmuch as rates of reaction are about doubled by increases of tem-

[2] The terms "stable" and "unstable" are used here in the ordinary sense, which disregards the effects of the adsorbed layer of foreign material on the surface of the nuclei. In reality this layer is not in general a permanent thing, for it is subject to the jostlings of the swiftly moving ions of the solution, which are tending thus to break it down and allow additions to be made to the nuclei.

perature of 10° centigrade, and as dolomite has been formed artificially at somewhat elevated temperatures, it seems not an unpromising laboratory task to attempt—possibly by extrapolation of a series of quantitative results at elevated temperatures—to answer some of the geologists' questions concerning dolomite. The fact that both dolomite and the common forms of calcium carbonate usually crystallize well adds much to the hope of getting definite results.

OCEANIC RED CLAY

The oceanic red clay has been considered to be nearly the end product of the chemical action of sea-water on materials sinking through great depths of the ocean and resting on the bottom. The sources and means of distribution of parent materials have been discussed, the clays have been analyzed chemically by different methods with different objects in view, they have been studied microscopically—chiefly from the viewpoint of the biologist—and certain well founded conclusions have been reached; yet it seems probable that when critical studies of the structures of the complex granules of the clay have been made by means of the petrographic microscope, after suitable chemical treatments, more silica of organic origin and less decomposed pumice will be found than has hitherto been supposed. The solubility relations of the elements most sparingly present in sea-water and comparatively abundant in the red clay need much study, and the chemical effects of the working over of constituents of the mud in the alimentary tracts of animals should not be neglected.

THE PRIMARY ROCK PHOSPHATES

Geological relationships indicate that the extensive bedded phosphate rocks were formed on sea floors either by direct deposition or by replacement of oolitic or other limestones. Microscopical studies seem not to have settled the question of replacement for any particular deposit; but the more general chemical problems involved need not take this question into consideration.

At present no formation of phosphate comparable in purity with older deposits is known to be taking place. In fact, no source of phosphorus adequate for such deposits under any probable method of concentration is evident from the most recent water analyses, with the possible exception of water bodies in inclosed basins such as Lake Van, Armenia, which covers many thousand square miles and may, like certain well known lakes containing highly saline sulphate and carbonate waters, be relatively rich in phosphorus. In ocean water phosphoric oxide is remarkably low, but lime, the other essential constituent of phosphate rock, is abundant. If the phosphorus in a 1,000-foot column of sea-water were precipitated as tricalcium phosphate a layer less than half a millimeter thick would be produced. The lime in a similar column would give, if sufficient P_2O_5 were available, a layer about 30 centimeters thick. Practically all of this lime could be precipitated rapidly from sea-water as phosphate if the sea-water were mingled with such highly phosphatic waters as were previously mentioned. If the phosphatic waters contained high sulphate, mixtures of calcium phosphate and calcium sulphate would be expected.

Many other problems involving chemical factors, some of them of long-standing interest, might be reviewed, but they would only further illustrate how necessary it is that the problems of sedimentary rocks be attacked from many directions.

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DIAGENESIS IN SEDIMENTATION[1]

BY CHARLES SCHUCHERT

(*Read before the Society December 31, 1919*)

Doctor Vaughan has asked me to say something of the problems that have arisen in my studies of sedimentary formations, and the kinds of research that are needed in their solution. I will dwell on but one phase in sedimentation, but this is a most important one, though seldom taken up seriously by students of stratigraphy or by oceanographers. I refer to diagenesis, or the chemical and physical changes that sediments undergo during and after their accumulation, but before consolidation takes place. Diagenesis, therefore, has nothing to do with the later metamorphic alterations superimposed by orogeny and intrusive rocks. It is strange that this subject is so rarely studied, since it was as long ago as 1888 that Gümbel directed attention to these changes and coined the word. On the other hand, Walther in 1893 devoted a long chapter to diagenesis in his "Einleitung in der Geologie" (pages 693-711), and yet we rarely see the word employed by European or American stratigraphers.

It is well known that the igneous rocks are the primary sources for all sedimentary formations, and something is known as to the kinds of material into which they break down under the influence of temperature, water, grade, and climate. The end-results—the muds, sandstones, and limestones—make up the stratified formations, and they may later undergo another process of weathering, and so be reworked and redeposited. But in detail we know next to nothing as to what the exact end-products are that develop under the many varying kinds of climate. What is even more important, almost nothing is known in regard to the very decided chemical changes wrought on them directly or indirectly by the organic world. The climate that is impressed on the rock detritals and the organic deposits, as recorded in our geologic formations, is also hardly ever discerned by stratigraphers. In all of this we see that at the very source

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This paper is one of a series composing a symposium on sedimentation.

of the rock materials we know but little in regard to the destructive or katamorphic changes and the constructive or anamorphic alterations of the sedimentary formations. Finally, when the sediments come to rest in their basins of accumulation, they are again subjected not only to the variable chemistry and temperature of the water, or more rarely the air, but also to the physical and chemical work resulting from organic decay. The enormous quantity of chemical work done by the organisms, not only of the land, but even more of the water-bodies and chiefly of the seas, is not at all appreciated by geologists. The paleontologist often sees the resultant diagenetic changes, but he is usually too busy interpreting the fossils gathered from the sediments to study the rocks themselves. The diagenetic changes are therefore rarely studied by any one, but it is the paleontologist who must take up this line of research, which requires also a knowledge of chemistry, physics, and mathematics.

A good beginning has been made by the staff of the Dry Tortugas Biological Station of the Carnegie Institution of Washington, but their work as yet relates wholly to the calcareous deposits. Then, too, it is in a region of warm waters, so that the diagenesis of cool and cold waters remains almost wholly unstudied. On the other hand, the chemical laboratory of the United States Geological Survey has also done much good work in ascertaining the nature of the hard parts of invertebrates, and whether the calcareous structures are in the form of calcite or aragonite. Their results are important to stratigraphers and paleontologists, since it is well known that aragonitic structures are rarely preserved in fossils. Aragonite is the first mineral to change in the diagenesis of marine sediments.

All life is ceaselessly undergoing chemical change and is constantly throwing off materials that affect other materials, both organic and inorganic; and all life is destined to die, resulting in bacterial decomposition products that are either taken up by the sediments or are transformed by them. A great amount of the organic acids is used up on the land, but much is also washed into the seas, where still more is made by the decomposition of the marine organisms. To bring out the stupendous amount of organic material and the chemical changes involved in its death and decay, let me direct your attention to a few figures which will visualize to you the quantities of life.

It is well known that the kinetic energy of the sun is at the basis of all life. We are told that on an average about 4,000,000 horsepower per square mile is received by that portion of the earth's surface exposed not too obliquely to the sun's rays; and yet Horace Brown has shown that the plants store up less than 2 per cent of the sun's energy received by the earth. This energy has built up the plants through the carbon dioxide of the air and the water of the ground, and, according to H. Wagner, the amount of living substance, very largely plants, if spread equally over the surface of the earth, would cover it to a depth of one-fifth of an inch. In general, the biosphere is much thicker and locally the depth of it rises into feet.

Mr. W. L. McAtee estimated that an acre of woodland soil, about one inch thick, near Washington, D. C., contained of easily seen organic remains far more than 1,000,000 animals and over 2,000,000 fruits and seeds. In meadow or grass land of the same latitude, the animal remains rise to the incredible sum of about 13,000,000 and the seeds to nearly 34,000,000 per acre. In warmer climates these figures would rise and in cooler ones decrease. In these estimates no account is taken of the plants rooted in the ground, nor of the errant animals; so that our statements of the numbers of the macroscopic life are far below the actual amount in these areas. The invisible organic world is equally important, for K. F. Kellerman tells us that a fertile soil has from 15,000,000 to 300,000,000 bacteria to the ounce of soil. Even though from 10,000 to 100,000 individuals may be placed side by side in the length of one inch, they regenerate the soils and help to make possible the growth of plants and animals. "In spite of their small size, they are concerned with every phase of our daily life, and by their incredible numbers and ceaseless activity overcome their apparent insignificance." Many of the decomposition bacteria double their number in a few hours of time when their environment is favorable. Other kinds of bacteria occur in the sea-water and in the bottom muds and oozes. At the bottom of the shallow seas there may be as many as 285,000 individuals in one cubic centimeter of mud. Besides the decomposition bacteria, there are many other important kinds, like the denitrifying bacteria that throw down carbonate of lime, and the iron- and sulphur-making bacteria that are also very abundant in favorable places.

The microscopic plants are but one element leading to rock disintegration and rock alteration. Equally important are the easily seeable plants and animals, and their quantity is overwhelming. The number of known kinds of living plants and animals is now nearly 800,000, and the sum will eventually run into the millions, and of many species there are billions upon billions of individuals, and in general we can say that most of the organisms pass through the life cycle in about one year. At the bottom of the deeper oceans occurs the transparent, gelatinous, dead organic slime known as bathybius (Huxley at first regarded it as a sort of organic Urschleim and a primitive moner), which resembles protoplasm and which in places is said to be more than 30 feet thick. This is the result of organic decay and is in a state of change. From all these facts we get some insight into the enormous organic chemistry of nature, and thus become aware of the magnitude of its reaction on the inorganic materials of which our sedimentary formations are composed.

This interaction of organic chemistry on the marine and fresh-water deposits produces in the main the cements that bind the sandstones and mudstones. Through the living organisms are produced the limestones, and through organic decay their change into the magnesian limestones and the dolomites. The carbonaceous materials of the stratiform rocks, including the kerogen out of which petroleum develops, are also of organic origin. The coal beds are but the accumulation of land plants more or less decomposed and chemically altered. Denitrifying bacteria are necessary to the formation of some kinds of limestone deposits, while other kinds of bacteria lead to the formation of iron ores and bring about the marcasite and pyrite nodules of black shales. But why do the changes cited, and many other transformations as well, go on in one place and not in another? Why are some limestones crystalline, and others, which are 97 per cent carbonate of lime, amorphous? Why are some of the dolomites coarsely crystalline or cavernous, and others not so at all? Why are some dolomites rich in the molds of fossils, and yet in others every trace of the organisms is removed by the diagenetic changes? These are but a few of the changes wrought by the decomposition products of organisms.

Even though we can mention many diagenetic changes, we have as yet no clear idea of the processes and hardly any at all as to the depth, clarity or turbidity, temperature, or the agitation or streaming nature of the waters in which these alterations go on. When we know more of these conditions, to be discerned in the lakes and seas of today or experimentally developed in our laboratories, then we will also be able to tell the depth and nature of the waters and the climates under which the deposits were accumulated. Then diagenesis will be another stepping-stone toward a visualizing of the actual physical and organic worlds of the geologic past.

Finally, let me ask, when do muds, sands, and lime deposits that are permanently under a water cover become cemented into solid rock? Calcareous muds back of the Florida Keys, and up to 20 feet thick, are still soft and rest uncemented on the hard and eroded Pleistocene limestones. It was out of these muds that the "railway over the seas" was built. It is therefore evident that some marine deposits may remain unconsolidated for thousands of years, and yet in some of the Ordovician intraformational limestone breccias we know that the bedded lime muds, up to one inch thick, became sun-cracked and hardened to rock in the course of at most a few months. Apparently, then, drying out brings on quick solidification through cementation, but if formations remain constantly wet, when do they become shale and limestones? These are some of the problems to be answered by the rising school of stratigraphers.

Discussion

G. R. Mansfield: In a recent economic study of New Jersey greensands I had occasion to make a number of borings and to collect samples of greensand from the top to the bottom of the deposit at a number of places. Mechanical analysis, including wet and magnetic separations and sizing operations, were carried out. Some of the material contains much "clay" or colloidal matter, which analysis shows contains about as much potash as the actual glauconite grains. It seems probable analyses, not yet completed, will show this substance to be colloidal or amorphous glauconite. Some of this material may have been formed by mechanical wear of the glauconite grains, but some may represent amorphous glauconite not yet formed into actual grains. The grains themselves show little resemblance to foraminifera, and it seems likely that some grains may have been formed or may now be forming from the amorphous glauconite. I have been unable to identify such grains or to detect them in formation. This phase of the problem might appropriately be considered by the physical chemist.

M. W. Twitchell called attention to the need of further investigations of the microscopic life found in sediments—diatome, foraminifera, radiolaria, etcetera, determining characteristic or diagnostic forms or groups of forms. This is especially important to the student of well samples, well records, correlation of well sections, etcetera.

W. H. Twenhofel: Before sedimentary rocks can be studied and described so that a reader may obtain the most from what is written, there must be precision of definition. Sands are described as fine, medium, and coarse grained. The terms mean different things for every student and different things for the same student at different times. The color of rocks is given as drab, buff, gray, etcetera, and no other than the person who applied the color has any clear conception of what the term means, and even he is apt to have forgotten when his memory of its occurrence becomes dim.

Sediments, both those non-forming and those consolidated into rocks, should be studied from the point of view learning to what extent the organisms living in and on the sediments change with their character. Observations made on the Paleozoic rocks of Anticosti, Gotland, and elsewhere have convinced me that this is one of the most fundamental factors to be considered in correlation and stratigraphy. I suspect that a number of our formations which are considered to bear a vertical relation to each other or to have been deported in distinct basins are laterally related to each other, with the faunal differences merely due to environmental conditions.

The organic contents of sedimentary rocks should be studied to learn if they were deported as they grew or were rolled about after death and before deposition. Are the shells in natural position or are they upside down? Are shells present which were broken and then healed? Information of this character appears to me to be extremely valuable in the interpretation of the sediments in respect to the conditions and places of deposition.

The question of paleogeography appears to me to be closely tied up with the dolomite and flint problem. The "Niagaran" rocks of Wisconsin, Illinois, etcetera, are in large part at least composed of dolomite and contain an abundance of flint nodules. Rocks of the same age and same general conditions on the island of Anticosti, Gotland, and Esthonia contain neither dolomite nor flint. Similarly, the Richmond strata of Wisconsin are filled with flint; yet there is none in the Richmond deposits of Anticosti or Esthonia. To interpret these differences, I think we need the assistance of the paleogeographer and facts relating to paleogeography.

E. M. Kindle: Professor Twenhofel in his remarks dwelt forcibly on the inadequacy and uncertainty of meaning of the color nomenclature used by geologists. I would call attention in this connection to the elaborate and exhaustive scheme of color shades and nomenclature which has been worked out and used by zoologists. Mr. Ridgway's volume on this subject furnishes the illustrations and nomenclature for more than one thousand different shades of color. I believe that a less elaborate color scheme would be adequate to the needs of most geologists and suggest that a satisfactory one could be selected from Ridgway's color manual by a committee of geologists. This is one of the things which should be undertaken in connection with the initiation of the more precise and exhaustive study of sediments now under consideration.

Doctor Vaughan has suggested in his excellent presentation of plans for researches in sedimentation the cooperation of various groups of geologists. He has mentioned the universities and the State Geological Surveys among the groups which should be represented in this cooperation. There is, I think, one other group of men who should be invited to cooperate in the investigations which it is proposed to take up. I refer to engineers in charge of dredging operations and the construction and maintenance of harbor structures. Although not geologists, there is probably no group of men more vitally interested in sedimentation than those engineers to whom is entrusted the work of keeping our harbors open and of certain depths. I know from personal association their keen interest in many of the problems we propose to take up.

It is most probable that one of the practical byproducts of the proposed researches in sedimentation will be the accumulation of much data of vital importance to those engineers who are intimately concerned with the problems of inshore sedimentation.

L. D. Burling: The problem of researches in sedimentation is fundamental and so large that I shall confine my remarks to the particular suggestion made by Mr. Shaw as to the collection of rock specimens illustrating the processes, types, and results of sedimentation. If my own experience is typical, and it is probably no more than this, we all have in our own collections numbers of specimens which may have larger importance and wider significance than we now think. A start in the direction of following out the suggestion made by Mr. Shaw could be made by the assembling of this scattered material into the larger museums or in the hands of special workers in the problems involved. Certainly there is just as much reason for assigning the name of type to a specimen used as the basis of a description of a new type of oolite, and for its preservation in a museum, as there is in the case of a fossil.

Among my own collections is a specimen of limestone permeated along bedding planes by series of tabular dolomite crystals which may even entirely replace the limestone in particular localities. The general drift toward the hypothesis that dolomitization is essentially contemporaneous with deposition may receive confirmation or refutation as a result of the careful study of such a specimen as the one described.

J. B. Woodworth called attention to the necessity of understanding the laws of contrast in coloration of rocks. He also called attention to Newberry's doctrine of ternary succession of strata in marine formations, the recognition of which has since been found in the Comanchean of Texas and the Upper Cretaceous of the Great Plains by Lee.

D. F. Hewett: In considering the materials that make up sedimentary rocks, many geologists overlook the direct contributions from volcanic sources. This is largely due to the obscurity of the evidence of the origin. Close examination recently of numerous specimens of sediments from the Mesozoic and Tertiary sections of northeastern Wyoming showed that volcanic materials are common and locally make up 5 to 10 per cent of large thickness of sedimentary rocks. In this work bentonite has been proved to be a decomposed volcanic ash, and as it is known to occur in many zones throughout the sediments of the Colorado and Montana epochs in Utah, Colorado, Wyoming, Montana, and South Dakota, it gives evidence of volcanic activity not previously suspected during these periods. It may be noted that any sediments which contain especially fresh angular feldspar in excess of the quartz should be closely examined as to possible volcanic origin.

Several years ago Prof. O. C. Lawson, in the examination of some peculiar clays in the Shinarump group (Juro-Trias) of southern Utah which contained from a trace to twelve cents' worth of gold, was unable to explain the presence of gold in the material. Through the courtesy of B. S. Butler, who was recently able to examine this clay, there can be little doubt that it is a variety of bentonite and a much decomposed volcanic ash.

The recognition of igneous material in sediments obviously indicates simultaneous volcanic activity in near-by lands or possible seas. It may serve to identify the early stages of batholithic intrusions, for at present we are dependent on the identification of the ages of deformed beds, and this is rarely possible in a satisfactory degree.

R. W. Sayles: Although De Geer proved to the satisfaction of those with him at the International Geological Congress in Stockholm in 1910 that the alternating coarse and fine layers found in Sweden were of a seasonal nature, to find out what actually takes place at the bottom of an active glacial lake in the course of years is of the greatest importance. An apparatus which will collect the year's deposit and yet allow for the action of bottom currents will be necessary. To obtain a core of the bottom sands will also be necessary. To accomplish these results must be the aim of all those interested in this problem. In addition to work in deposits now forming, an examination of the slates and shales from marine formations must be made to find out whether any or many of them show seasonal characters. Physical, mineralogical, and chemical analysis has an extremely important bearing on the whole problem.

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OIL GEOLOGY IN RELATION TO VALUATION[1]

BY RALPH ARNOLD

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CONTENTS

INTRODUCTION

It is intended in this paper to discuss the relationship which exists between oil geology and the valuation of oil properties, or, more properly speaking, oil deposits, as the natural assets only will be primarily referred to. As introducing the subject, it may be asserted as obvious that the fundamental law of supply and demand governs the valuation of oil properties, the same as it does all other commodities. The causes which affect the demand are largely artificial and need not be taken up here; the factors governing supply and production are primarily natural or geologic and as such properly come under the head of geology—oil geology, to be more precise. The relationship existing between the natural and artificial elements, as affecting oil production, are so intimate that it will be impossible to discuss the one without mentioning the other—in fact, oil geology and oil technology are so closely related that any one

[1] Manuscript received by the Secretary of the Society April 22, 1919.

essaying to be an oil geologist without a working knowledge of the technical side of the business is pretty sure to meet with failure in commercial work. One need look back only a few years—a very few years—to a time when oil men would have laughed with scorn at the statement that there was any connection whatever between geology and the oil industry. Today every important oil and gas company in the world has its corps of oil geologists, and on their opinion rests the investment of most of the millions of dollars which annually go into prospecting and development work.

Oil geology and technology are comparatively new sciences, largely developed within the last ten years. To the geologists and engineers who have specialized on and developed these subjects, the author wishes to render acknowledgment for the information on which this paper is based.

Factors Governing the Valuation of Oil Properties

CLASSIFICATION OF THE FACTORS

The factors governing the production of oil, and hence the valuation of oil properties, may be discussed conveniently under four headings: (1) character of the oil, (2) quantity and rate of production, (3) cost of production and transportation, and (4) the certainty with which all of these factors may be predicted.

CHARACTER OF OIL

The quality of any commodity is usually one of the determining factors of its value. In the case of oil, this is preeminently so. Roughly speaking, the value of oil, and hence of the property producing it, is inversely in proportion to the specific gravity of the fluid. This is true, not because this particular physical characteristic of oil measures its desirability, but because it indicates inversely, in a general way, the relative contents of the more valuable derivatives which may be obtained from it through distillation. Oils of low specific gravity contain the naphtha, gasoline, and other desirable light distillates, while the high gravity oils are composed largely of constituents such as fuel oil, which are of lesser value. There are, of course, exceptions to this rule—oils with special properties which make them unusually valuable—but in general the specific gravity is the ruling factor. As illustrating this point, it is to be noted that in all quotations of value the gravity—usually recorded in the Beaumé scale in this country—always is mentioned and is the only physical property entering into the quotation.

QUANTITY AND RATE OF PRODUCTION

Obviously, the amount of recoverable oil in any property is the pre-eminently dominant factor governing valuations. It furnishes the one great element of chance which makes the oil industry the greatest gambling game in the world. Second only in commercial importance to the quantity of oil in the property is the rate at which the fluid may be recovered. These two factors, quantity and rate of recovery, are dominated to a large extent by the geology of the deposit; hence the importance of the work of the geologist as applied to valuation.

The quantity of oil in any deposit depends on the structure of the container, extent of the deposit, both laterally and vertically, and on the character and porosity of the reservoir rock. The rate of production and the quantity of "recoverable oil," as distinguished from "contained oil," is dependent on the above factors and, in addition, on hydrostatic, hydraulic, and gas pressure (which in turn are dependent on depth below the surface, etcetera), on the viscosity of the oil and the connection that exists between the oil and associated waters.

The relationship between valuation and such factors as the lateral extent of deposit are too obvious to merit mention. Structure enters largely into the value of oil properties; in many instances it is the dominant factor of accumulation, and therefore of value, while in others its potency may be shared with lithology. Domes and anticlines and their various modifications are usually the more favorable structures for oil, although fault zones and unconformities play an important rôle in its accumulation. The value varies with the position of the property on the structure, usually inversely with the distance of the property from the centers of accumulation. The influence of vertical extent is in connection with the fact that the production varies with the intake surface of the well, and this in turn is in proportion to the depth of producing rock through which the well penetrates. Likewise the pressure under which the oil exists, its viscosity, and the degree and type of porosity of the container have a definite relation to the percentage of the contained oil that may be recovered and the rate at which it may be produced. Production varies with the pressure and inversely with the viscosity. Coarse-grained reservoir rocks or oil-yielding formations with large pores or cavities give up more of their contents than those with small cavities. These factors have a direct bearing on the rate of production, and therefore on the present value of the deposit, for it is obvious that a certain quantity of oil that may be recovered quickly is worth more at any given time than a similar quantity requiring a longer time for recovery. As illustrating this point, consider the relative value of an acre of ground in one of the salt domes of Texas, where the presumptive recoverable oil is 50,000 barrels per acre, and a similar area in the Kern River field in California, where the same production per acre may be expected. Assume the oil to be the same grade and value per barrel, which, in reality, it may be. In the first case, the great bulk of the oil could be recovered in the first two years of operation; in the second, twenty years would elapse before the oil would become even approximately exhausted.

Of course, there are artificial factors governing the rate of production, such as the depth of the well (which in reality is a natural factor, as it is dependent on the structure), size of hole, character of pump and other production equipment used, efficiency of management, etcetera; but these may be considered as secondary, as compared with the factors with which the geologist has to deal.

WATER ASSOCIATED WITH OIL

The influence of the water situation in any field on the values in the field may be said to be of equal rank with that of the quantity of recoverable oil and rate of production; for, in a measure, the two latter are often directly dependent on the former. In nearly all producing areas water exists above and below the oil reservoir and underlying or adjacent to the oil in the reservoir. The first is known as "top" water, the second as "bottom" water, and the third as "edge" water. Besides these simple cases, there are many special ones in various fields. As long as these waters remain isolated from the oil, no harm is done, but as soon as the water begins to show in the well, either through faulty shutting off above or below the producing horizon or through infiltration on account of the exhaustion of the oil and the resultant "following up" of the water, production and likewise values are affected. Often the effect is almost instantaneous; in others gradual. The harmful effects of top or bottom water may be remedied through manipulation, but when edge water shows it means the beginning of the end of that well and eventually of the pool. In some cases water may act advantageously for the production, and therefore for values, but these cases are rare. Innumerable cases of the harmful effect of water on production and values might be cited. As illustrating the few exceptions, there are some parts of the Kern River field, California, and the Bradford pool, Pennsylvania. In the former, natural waters have helped flux the heavy oil into the wells, while in the latter water has been artificially forced into certain wells to drive the oil along to other wells, higher up on the dip of the formation.

As illustrating the importance of the water problem, it may be stated that the most effective and usually the first practical work undertaken by the Government and State technical bureaus in the oil fields is the effort to overcome water difficulties. Over 2,000 barrels daily have been added to the production of the Mid-continent field within the last few months, directly as a result of the effort of the petroleum division of the United States Bureau of Mines in shutting off offending waters. The annual value of the oil thus conserved is $1,825,000, or enough to finance the oil division for many years. Other examples of a like nature might be cited from California and elsewhere among the producing States.

No problem is usually given so little consideration in opening up new fields as the water problem—none is usually as important eventually. Water is the specter of all fields. When the production comes from a few large wells, as in Mexico, the specter looms large and its hand may cut down millions in value instantaneously; when numerous wells contribute to the total yield, the grim shadow is less ominous, but is, nevertheless, as certain to levy its toll as in the fields of larger wells. Within the same week that this paper was started the great Portrero de Llano well, No. 4, of the Pearson's, in Mexico, changed suddenly from oil to an emulsion of oil and hot water. From a producer of 35,000 barrels of oil per day, valued at $17,500, to a well of questionable value is a good illustration of the injurious effects of water. It might be remarked in passing that this is one of the greatest oil wells the world has ever seen, its total production to date being over 105 million barrels, valued at over 50 million dollars.

ESTIMATION OF OIL RESERVES

The principal economic use of oil geology is in the location and estimation of oil reserves. The estimates serve as a basis for the valuation of the deposit as distinguished from the surface value of the land and the improvements. Several methods of estimating oil reserves are in use. The two principal types are (1) those which involve a study of the extent, thickness, and character of the reservoir rock, its saturation, pressure under which the oil exists, and any other factors which may throw light on the probable contents of the reservoir, and (2) those which utilize the actual production of wells as a basis for calculating the future production of these or similarly located wells.

The first method is the only one applicable to undeveloped fields, and for such is most useful in arriving at rough approximations of the possibilities. Its use, however, necessitates, among other uncertain factors, the assumption of a ratio between the "contained" and "recoverable" oil that is at once arbitrary and unreliable.

The second method, which utilizes figures based on the actual performance of wells, immediately dispenses with many of the uncertainties, as it deals directly with recoverable oil, and if based on sufficient data can be counted as fairly reliable in most instances and remarkably so in some. The utilization of production statistics for the method involves the plotting of decline or cumulative production curves, the horizontal line usually denoting the time factor, while the vertical refers to the production. In the case of the decline curve, the production figures are plotted direct for each time unit, while in the cumulative curve, as its name implies, cumulative figures for each time unit are employed. The important feature in both cases and the one requiring care and experience is the projection of the curves to show the probable future production of the well or tract, as it is this estimate on which the valuation of the property depends. The method involving the use of production statistics is applicable only to producing fields, except indirectly; it is the method in commonest use for appraisal purposes and within the past two or three years has been the subject of much discussion and of ingenious modifications for practical use.

COST OF PRODUCTION

The cost of production is one of the items directly related to valuation that is intimately affected by both natural and artificial factors. The most important among the former are depth of the deposit from the surface and character of the overlying rocks. These two geologic factors, the one structural and the other lithologic, practically determine the cost of the well and the operations of recovery, and by so doing fix the cost of production and greatly influence the commercial value of the deposit. Other items, such as the character of the reservoir rock—whether it be a soft sand which flows into the well with the oil and has to be removed from time to time to permit of the recovery of the oil, or whether it be so hard and fine grained that it has to be "shot" to insure the migration of the oil into the well, and other factors—have a direct bearing on the quantity of production and its cost, and hence affect valuations.

CERTAINTY OF PREDICTING FACTORS

The certainty with which predictions as to the various natural factors enumerated above may be made has a profound influence on valuation, especially of prospective oil land. It is not at all unusual to be able to accurately predict the depth to the oil sand, its thickness and character, and even the quality of the oil. This makes it possible in such instances to accurately estimate the cost of wells and production. But to predict the quantity of oil to be produced is not so easy, though in many fields, such as those in California, where lithologic conditions and pressures are more or less uniform, even the quantity and rate of production may be fairly accurately forecast. In many localities, especially in the Mid-continent and Appalachian and intervening fields, where the structural conditions may be uniform over wide areas, the lithologic character of the reservoir rock is so variable that any but very generalized predictions are practically valueless. In the Mexican field, where the oil is associated with volcanic intrusives, the uncertainties are magnified. Here one may have a "gusher" at one point and 200 feet away, or even less, a practically dry hole. Other things being equal, values are in proportion to the certainty with which predictions can be made.

Utilization of Valuation Data

Utilization in Commercial Transactions

Having pointed out some of the relations existing between geologic factors and valuation of oil properties, let us see how this relationship is utilized in a practical way.

Attention was called in the introduction to the fact that only within the past few years has oil geology been developed and used in a business way. This has been due to two factors: (1) the prejudice against anything that savors of science or system on the part of the type of pioneer or "wild-catter" who makes a successful individual oil operator, and (2) the failure of properly trained men to study the geologic conditions surrounding the origin and accumulation of oil and the methods of its discovery and recovery. Once the attractiveness of the field of oil geology and technology, both from a pure scientific and commercial viewpoint, was appreciated and it was overrun by geologists and engineers desirous of getting into the game; and the word "game" is used advisedly, for, after all is said and done, the element of chance rules almost supreme in the initial stages of the industry.

At the present time the science of oil geology is utilized (1) in exploratory work, where the areas involved are vast and the appraisals of possibilities are along broad qualitative or very general quantitative lines; (2) in detailed surveys of prospective areas, where conclusions must be roughly quantitative as well as qualitative and where the experience of the geologist is put to the vital test of converting surface geologic evidence into definite location for wells, the success or failure of which will make or break his reputation; and (3) in the estimating of oil reserves and the rate at which they may be recovered and the ultimate translation of these estimates into dollars and cents—the crucial test of the engineer, the finished product of the appraiser.

UTILIZATION IN RELATION TO TAXATION

It seems a far cry from the paleontological laboratory or geologic expedition to the problems of the Treasury Department, but such is not the case under the exigencies of war. "Politics makes strange bed-fellows"; likewise war. Among the problems developing from the war-revenue measures were those of valuing the oil and mining properties of the country and the evolving of equitable methods of arriving at depletion and depreciation allowances in computing income and excess-profit taxes.

The writer has had the privilege, in collaboration with the most efficient oil geologists of the country, of undertaking the solution of these problems for the Internal Revenue Bureau. The country has been divided up into six districts, based on the geographic distribution of the producing territory of the United States, and each district is under the supervision of a competent geologist.

The work was undertaken with a great deal of trepidation, as the magnitude of the task was obvious. The question of valuation is being handled through the accumulation of data regarding *bona fide* transfers of prospective, proven, and producing oil properties, supplemented by the consideration of such other factors as enable the appraiser to reach a proper conclusion as to values. The solution of the depletion problem involves the preparation of decline and cumulative production curves with which the reserves of each tract of land in the country may be approximated and the proper depletion allowances computed. The depreciation question, hinging more or less on the rate of depletion, is being handled as a supplement of the depletion work. The problems, involving, as they do, millions of dollars in taxes, are practically all related directly to oil geology and are being worked out by the best oil geologists the country affords. This, which is probably the most striking illustration of the application of oil geology to valuation, now being carried out, is only one of the many problems in which this new science is being applied to one of our most important industries.

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