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OF

SCIENCE AND ARTS.

CONDUCTED BY

PROFESSOR SILLIMAN

AND

BENJAMIN SILLIMAN, JR.

VOL. XLIV.—APRIL, 1843.

NEW HAVEN:

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CONTENTS OF VOLUME XLIV.

NUMBER I.

NUMBER II.

ERRATA.

Page 24, line 7 fr. bot. for "Plate II" read "Plate VI."—P. 241, l. 18 fr. bot. for "in" read "by."—P. 383, l. 5 fr. top, for "56° 24′ 24″" read "49° 57′ 36″."

Vol. XLII, p. 366, l. 16 fr. top, for "trilobited" read "trilobate."

Ak73

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VOL. XLIV.—No. 1.—JANUARY, 1843.

FOR OCTOBER, NOVEMBER, AND DECEMBER, 1842.

WITH EIGHT PLATES.

NEW HAVEN:

Sold by B. & W. NOYES.—*Boston*, LITTLE & BROWN and W. H. S. JORDAN.—*New York*, WILEY & PUTNAM, C. S. FRANCIS & Co., and G. S. SILLIMAN.—*Philadelphia*, CAREY & HART, J. S. LITTELL and DREW & SCAMMELL.—*Baltimore, Md.*, N. HICKMAN.—*London*, WILEY & PUTNAM.—*Paris*, HECTOR BOSSANGE & Co.—*Hamburgh*, Messrs. NESTLER & MELLE.

PRINTED BY B. L. HAMLEN.

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TO CORRESPONDENTS.

Papers will appear in our next number, by Drs. Gardner, Plummer, Locke, Rev. J. H. Linsley, Prof. A. Litton, and Mr. C. W. Whittelsey.

Communications have been received from Mr. J. T. Hodge, Prof. Whitlock, and others.

The titles of communications and of their authors, must be fully given.

Authors should always specify at the head of their MSS. the number of extra copies of their communications which they may wish to have printed.

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Notice always to be given when communications sent to this Journal, have been, or are to be, published also in other Journals.

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JANUARY, 1843.

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BEFORE THE

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It is scarcely necessary to speak of the value of a work such as this, which must be of the greatest interest to the Practical Iron Master, as well as to every one concerned, either directly or indirectly, in this most important subject. The great French work of Le Blanc and Walter, being confessedly the most complete that has yet appeared, it will be reproduced in this, with additions—the Plates are all drawn to a scale, which will enable any one to construct, by their means, whatever may be required. The Essay on the Smelting of Iron with Anthracite Coal, by Mr. Roberts, will be found very interesting; it is the production of a gentleman of talents, and of sound judgment and unwearied research, who, highly favored by his position, has given the subject much attention. The numerous Analyses of the Coal and Iron Ores, both of our own country and Europe, will afford much interesting and useful information; while the statistical portion will be drawn from the most authentic sources. Richard C. Taylor, Esq., a gentleman well

known for his talents and accuracy in research, has promised his valuable assistance.

It is also intended to give abstracts of the most important recent patents relating to Iron, accompanied with critical remarks—the whole forming the most complete work on the subject that has yet been published in any country—and the only one of the kind in the English language.

CONDITIONS.

The work will be put to press as soon as there are 150 subscribers—and but a limited edition will be published.

It will be published in 8 parts, each part to contain 8 or more very large folio plates, with the accompanying text.

The price per part will be five dollars, payable on delivery.

As it is desirable to put the work to press as early as possible, gentlemen who may wish to subscribe, will confer a favor by transmitting their names to the publisher without delay.

GEOLOGICAL DRAWINGS AND ILLUSTRATIONS.

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The drawings are if desired fixed on rollers, adapted for lectures. Letters addressed to R. Bakewell, Instructor of Drawing and Perspective in Yale College, at Mr. Ebenezer Johnson's, Chapel street, New Haven, will be duly attended to.

Drawings and Plans of every description copied with dispatch.

New Haven, June, 1841.

Mr. Bakewell's drawings are excellent.—*Eds.*

American Journal of Science and Arts.

The following numbers of this Journal are wanted by the Editors, who will pay for them $1 each, or give in exchange current numbers as they appear.

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Number	1, 2.	1, 2.	1.	1.	1, 2.
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Vol.	XVII.	XXII.	XXVI.	XXVII.	XXIX.	XL.
Number	1.	1, 2.	2.	1, 2.	1, 2.	1.
Entire No.	35.	45, 46.	54.	55, 56.	59, 60.	81.

B. & B. Silliman.

New Haven, June 23, 1840.

FOSSIL FOOTMARKS.

The residence of the undersigned being in the vicinity of many localities of the New Red Sandstone of Connecticut River, in which exist the perfect Tracks of Birds and also the Fossil Impressions of Fishes, he is enabled to secure them as they are quarried, and will furnish them to Geologists whenever it is practicable, for a reasonable compensation. As it will only be possible to obtain a limited supply, he will furnish as a substitute, Plaster Models of all the varieties of Ornithichnites and Sauroidichnites, of which there are nearly forty, to all who may apply. These models will be cast in plates of uniform size, colored to conform to nature, and neatly packed in a box. They will be accompanied with a Descriptive Catalogue and sent according to direction for $10.

JAMES DEANE.

Greenfield, Mass., October, 1842.

I owe much to the kindness and zeal of Dr. Deane in procuring for me and my friends suits of these interesting fossil foot-marks, and I repose full confidence in Dr. Deane's discrimination and fidelity.

B. SILLIMAN.

Prof. SILLIMAN'S ADDRESS

BEFORE THE

ALUMNI OF YALE COLLEGE,

August 17th, 1842—pp. 44, 8vo.—price 25 cents, may be obtained of WILEY & PUTNAM, 161 Broadway, New York, and B. L. HAMLEN, New Haven, Conn.

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New Haven, June 23, 1841.

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ACKNOWLEDGMENTS TO CORRESPONDENTS, FRIENDS AND STRANGERS.

Remarks.—This method of acknowledgment has been adopted, because it is not always practicable to write letters, where they might be reasonably expected; and still more difficult is it to prepare and insert in this Journal, notices of all the books, pamphlets, &c., which are kindly presented, even in cases, where such notices, critical or commendatory, would be appropriate; for it is often equally impossible to command the time requisite to frame them, or even to read the works; still, judicious remarks, from other hands, would usually find both acceptance and insertion.

In public, it is rarely proper to advert to personal concerns; to excuse, for instance, any apparent neglect of courtesy, by pleading the unintermitting pressure of labor, and the numerous calls of our fellow-men for information, advice, or assistance, in lines of duty, with which they presume us to be acquainted.

The apology, implied in this remark, is drawn from us, that we may not seem inattentive to the civilities of many respectable persons, authors, editors, publishers, and others, both at home and abroad. It is still our endeavor to reply to all letters which appear to require an answer; although, as a substitute, many acknowledgments are made in these pages, which may sometimes be, in part, retrospective.—*Eds.*

SCIENCE.—FOREIGN.

Acadèmie Royale de Bruxelles. Bulletin de la Séance du 6 Novembre, 1841. Nos. 10, 11, 12—tome viii. Bruxelles, de l'Acadèmie. Received July 2d, 1842.

Instructions pour l'observation des Phénomènes périodiques. Extrait du tome ix, No. 1, des Bulletins de l'auteur, M. Quetelet. pp. 31.

Phénomènes périodiques du regne végétal. Academie Royale de Bruxelles, Extrait du tome ix, No. 2, des Bulletins.

Economie politique. Extrait de deux lettres de M. la professeur Karr de Heidleberg, au sujet de l'application des theories mathematiques à la solution de quelques problèms d'economie politique. Extrait du tom. viii, No. 9, des Bulletins.

Nouveau Catalogue des principales Apparitions d'Etoiles volantes, par A. Quetelet, Sec. perpetuel de l'Acadèmie Royale de Bruxelles. Extrait du tome xv, des Memoires de l'auteur. Received July 2d, 1842, via Boston. Pamphlet, quarto, pp. 60.

Annuaire de l'Academie Royale des Sciences et Belles Lettres de Bruxelles. Huitième année. 1842. The foregoing all through M. Quetelet.

An address delivered at the anniversary meeting of the Geological Society of London, on the 19th of February, 1841 ; and the announcement of the award of the Wollaston medal and donation fund for the same year. By the Rev. Prof. Buckland, D. D., F. R. S., President of the society. London, 1841. From the author.

Transactions of the Royal Society of Edinburgh, Vol. xv, Part II, containing papers read during the session, 1841–2. Edinburgh. Quarto, pp. 340. Received from the Society, through Mr. Wm. Vaughan.

An inquiry into the nature and pathology of granular disease of the kidney ; by Geo. Robinson. London, 1842. From the author.

Nouveau Memoire de l'Academie Royale de Bruxelles, tome xv, 1842. From the Society, by M. Quetelet.

Instructions pour l'observations de phénomènes periodiques de l'homme, par M. Schwann. Extrait du tome ix, No. 7, des Bulletins de l'Academie. From M. Quetelet.

Descriptions des cancellaires fossiles des terraines tertiares du Piémont, par Louis Bellardi. De l'auteur. Turin, Impriere Royale, 1841. Quarto pamphlet, pp. 50.

The Chemical Gazette, or Journal of Practical Chemistry in all its applications to Pharmacy, Arts, and Manufactures, conducted by William Francis and Henry Croft. No. 1. November, 1842. London.

Proceedings of the London Electrical Society. (Quarterly.) July and October, 1841. London. 8vo. From the society.

The Microscopic Journal and Structural Record, &c., edited by Daniel Cooper, Esq., surgeon to the forces, and George Bush, surgeon to the hospital ship Dreadnought. London. Nos. 19, 20, and 21. From the editors.

Griffin's Scientific Miscellany : an occasional publication. Part VIII, containing experimental researches in electro-chemistry, including the Bakerian lectures and memoirs read before the Royal Society in the years 1806, 1807, and 1808, on the chemical agencies of electricity, and on the metals of the alkalies and earths ; by Sir Humphry Davy, Bart., LL. D., F. R. S., &c. Glasgow, 1842. 8vo. From Mr. Griffin.

Voyages en Scandinavie, en Laponie, au Spitzberg et aux Feröe, Géologie, Mineralogie, Metallurgie et Chimie. Rapport fait à l'Institut de France, par MM. Brongniart et Elie de Beaumont, rapporteur. From M. Elie de Beaumont.

SCIENCE.—DOMESTIC.

Elements of Scientific and Practical Agriculture; by Alonzo Gray, M. D. Andover, 1842. 12mo. pp. 370. From the author.

A treatise on Algebra; by Geo. R. Perkins, A. M. Utica, N. Y. 1842. 8vo, pp. 360. From the author.

Fifth and sixth geological report, made to the twenty third and twenty fourth general assembly of the state of Tennessee, 1839, 1841; by G. Troost, state geologist. Nashville. From the author.

Quarterly summary of the college of physicians of Philadelphia. May, June, July, 1842.

The Cambridge Miscellany of Mathematics, Physics, and Astronomy. July, 1842. Edited by Profs. Pierce and Lovering. Nos. 1 and 2. Boston, 1842. From Prof. Lovering.

The Western Lancet, devoted to medical and surgical science; edited by L. M. Lawson, M. D. May, 1842. Cincinnati. From the editor. Do. No. 3, do. No. 4.

A monograph of the Limniades, or fresh-water univalve shells of North America; by S. Stehman Haldeman, Mem. Phil. Soc. of Nat. Sciences. No. 5. July, 1842. From the author.

Inquiry into the distinctive characteristics of the aboriginal race of America; read at the annual meeting of the Boston Society of Natural History, by S. G. Morton, M. D. Boston, 1842. From the author.

Dr. Paine's Essay on Vitality, and the remedial agents. New York, 1842. From the author.

Observations on the bituminous coal deposit in the valley of the Ohio and the accompanying rock strata, with notices of the fossil organic remains and the relics of vegetable and animal bodies, with a geological map; by S. P. Hildreth. Marietta, Ohio. Octavo pamphlet, pp. 153; from the American Journal, Vol. xxix, No. 1. From the author. Three copies.

Geological report upon the Fourche cave and its immediate vicinity; by W. Byrd Powell, M. D. Little Rock, Arkansas. From Rev. W. Stevenson.

Second report of the state geologist of New Hampshire, Dr. C. T. Jackson. Concord, June 3, 1842. 2 copies, from the author.

Popular Essay on Comets; by R. W. Haskins, A. M. Buffalo, 1842. From the author.

Report on the insects of Massachusetts injurious to vegetation. Cambridge, 1841. pamph. octavo, pp. 459.

W. C. Redfield, on whirlwind storms, with replies to the strictures and objections of Dr. Hare. From the author. pamph. pp. 65.

Freshwater and land shells of Vermont; by Prof. Charles B. Adams, A. M.

Quarterly summary of the transactions of the college of physicians of Philadelphia. August, September, October, 1842.

Animal Chemistry, in its application to physiology and pathology; by Justus Liebig, M. D., professor of chemistry in the University of Giessen, edited by William Gregory, M. D. New York, published by Wiley & Putnam, 1842. Small octavo, pp. 356. From the publishers.—The same, by Prof. J. W. Webster, of Harvard University. From the Editor.

Lectures on the applications of chemistry and geology to agriculture. Part II, on the inorganic elements of plants; by Jas. F. W. Johnston, M. A. F. R. S. University of Dublin. American edition, New York, 1843, Wiley & Putnam. 2 copies, from the publishers. 12mo. pp. 175.

Boston Journal of Natural History, containing papers and communications read before the Boston Soc. of Nat. Hist. Vol. iv, No. 2. Boston, C. Little & Jas. Brown, 1842. 8vo. pp. 128; with 4 plates. From the society.

The principles of Chemistry: prepared for the use of schools, academies, and colleges; by Daniel B. Smith. 2d edition. Philadelphia, Uriah Hunt, 1842. No. 2. From the author. 12mo. pp. 312.

The climate of the United States and its Endemic influences; by Samuel Forry, M. D. New York, Langley, 1842. 8vo. pp. 378. From the author.

Report on the Geology of Connecticut; by James G. Percival. Published under the direction of the commissioners appointed by the Legislature. New Haven, Osborn & Baldwin, printers. 1842. 8vo. pp. 495. From Hon. H. W. Edwards.

The North American Sylva; or a description of the forest trees of the United States, Canada, and Nova Scotia, not described in the work of F. Andrew Michaux, and containing all the forest trees discovered in the Rocky Mountains, the territory of Oregon, down to the shores of the Pacific and into the confines of California, as well as in various parts of the United States: illustrated by 122 fine plates; by Thomas Nuttall, F. L. S., &c. &c., in 3 vols. Vol. I, (first half,) Philad., J. Dobson, 1842. pp. 55, royal 8vo. 19 plates.

The American Almanac and Repository of Useful Knowledge, for the year 1843. Boston, D. H. Williams, 1842–3. 12mo. From D. H. Williams.

An attempt to unite the different theories concerning light, heat, electricity, galvanism, and magnetism; by C. Campbell Cooper. Part I.—Identity of caloric and electricity. Pamphlet. Philadelphia, 1842. From the author.

MISCELLANEOUS.—FOREIGN.

Twenty fifth annual report of the superintending committee of the London provident institution, November, 1841, with a classification of depositors.

Lumley's Bibliographical Advertiser. A dictionary of Greek and Roman antiquities; by Wm. Smith.

Episcopal Family Monitor, July 20th, 1842. From Messrs. Wiley & Putnam.

The Lady and Gentleman's Athenæum, July. Vol. I, No. 1. New York. From Messrs. Wiley & Putnam.

Circular of New Hampshire Medical Institution, Dartmouth College. Hanover, 1842. From Prof. Hubbard.

Report of the American Temperance Union. New York, 1842.

Third Annual Report of the Foreign Evangelical Society. May 10th, 1842. New York.

Tobacco. Letter from the secretary of the treasury, showing the exports and imports of tobacco, with the import into Great Britain. May 28th, 1842. From Hon. William W. Boardman.

Report of the committee of commerce on the light house at Flynn's Knoll, N. Y., and Brandywine Shoal, Delaware Bay. May 20th, 1842.

Speech of Mr. Huntington of Connecticut, in favor of electing representatives by districts through the United States. May 31st, 1842.

Speech of Mr. Huntington of Connecticut, on the bill to provide further remedial justice in the courts of the United States. May 11th, 1842. Washington. From the author.

Report of Mr. Pendleton of Ohio, on military posts—Council Bluffs to the Pacific Ocean. May 27th, 1842. From Mr. Trumbull, M. C.

Report on the commercial relations of the United States with foreign nations. From the Hon. Mr. Granger. Washington.

Report of Mr. J. P. Kennedy, on commerce and navigation. May 28th, 1842. From Hon. Mr. Trumbull.

Fifty fifth annual report of the regents of the University of the State of New York. March 1st, 1842. Albany.

Patronomatology, or an essay on the philosophy of surnames, read before the Connecticut State Lyceum, November, 1839. From Rev. Charles W. Bradley, A. M.

Report of the secretary of the navy in compliance with a resolution of the Senate, in relation to the invention of Thomas S. Easton, for preventing explosions of steam boilers. June 24th, 1842. From Hon. Wm. W. Boardman. Do. from W. R. Johnson. Do. from Hon. Mr. Huntington.

The eighteenth annual report of the American Sunday School Union. May 24th, 1842. Philadelphia.

Proceedings of the public meeting held in Boston, to aid the American Sunday School Union in their efforts to establish Sunday Schools throughout the valley of the Mississippi. 1841.

Annual announcement of Jefferson Medical College of Philadelphia. Session, 1842–1843. Philadelphia.

Forty second annual catalogue of the Westfield Academy, 1841-2. From Ariel Parish, A. M.

President Wheeler's discourse at the funeral of Prof. Marsh. July 6th, 1842. Burlington, Vt.

Catalogue of Wabash College, July 20th, 1842. From Prof. E. Hovey. Indianapolis.

History of the North Church in New Haven; by Rev. S. W. S. Dutton, pastor of the church. From the author. New Haven, 1842.

An address delivered before the Lafayette and Dialectrical Societies of La Grange College, Ala. Delivered and published at their request; by E. R. Wallace. Huntsville, 1842.

The charter of incorporation and bye-laws of the Connecticut Historical Society. Hartford, 1839.

The mineral springs of Western Virginia, with remarks on their use and the diseases to which they are applicable; by Wm. Burke. New York, 1842. Wiley & Putnam. From the publishers. Small duodecimo, pp. 290.

Bees, pigeons, rabbits, and the canary bird familiarly described; by Peter Boswell, of Greenlaw. pp. 164, duodecimo. New York. From the publishers, W. & P.

Compendium of the enumeration of the inhabitants and statistics of the United States, prepared at the department of state, and presented by the Hon. Mr. Huntington, M. C. Washington, 1841.

Catalogue of the officers and students of the Genesee Wesleyan Seminary, Lima, N. Y. Rochester, 1842. From Rev. George C. Whitlock.

Constitution and bye-laws of the Northern Academy of arts and sciences, and first annual report of the curators. Hanover, 1842. For the Conn. Academy. Do. to Prof. Silliman.

Twenty sixth annual report of the directors of the American Education Society, 1842. Boston.

Circular of the Georgia Female College, for the session of 1842-3. Macon, Ga.

Constitution and bye-laws of the New Haven Medical Association, adopted April 25th, 1842.

A discourse on the death of Mrs. Maria Ward Smith, wife of Rev. Eli Smith, who died at Beyroot, Syria, May 27th, 1842; by the Rev. Tryon Edwards. Rochester, N. Y.

Defence of Lieut. Charles Wilkes, as to the charges on which he has been tried. From Capt. Gerry.

Catalogue of fruit and ornamental trees cultivated at the Bloodgood nursery; by Wilcomb & King. Flushing.

Cattle show and fair of the New York State Agricultural Society, held in the city of Albany, on the 27th, 28th, 29th, and 30th days of September.

Catalogue of the Medical College of South Carolina, 1842.

Report of the joint special committee on the effects of lead pipes upon well water in the city of Lowell. 1842. 2 copies, one from Mr. D. Bixley, and another from the magistrates of Lowell.

Descriptive pamphlet of Mr. Dunlap's picture of Death on the Pale Horse.

A History of the republic of Rome; by Rev W. J. Bakewell. Pittsburgh, 1842. Octavo, bound, pp. 408. From the author.

Three Physico-Theological Discourses concerning the primitive chaos and the creation of the world, the general deluge, the dissolution of the world; by John Ray. London, 1713. From Mr. J. M. Wightman, 1842.

Catalogue of the Theological Seminary, Princeton, New Jersey, 1842–43. From David Trumbull, A. B.

Moral education. A lecture delivered at New Bedford, Aug. 16, 1842; by Geo. B. Emerson. From the author.

Catalogue of Dartmouth College, 1842–43. Hanover, N. H. From Prof. Hubbard.

Circular of the Medical College of Louisiana, from J. L. Riddell.

An address by Hon. James M. Garnett of Virginia, before the Agricultural Society of Newcastle Co., Delaware.

The Hierophant; conducted by Rev. George Bush. 1842. No. 3, August. From the conductor.

Speech of Mr. Brockway of Connecticut, upon the tariff bills. Washington.

The Dial, No. 10, October, 1842. From the Editors.

American Tract Magazine, extra. Public meeting of the Board of the American Tract Society at the Broadway Tabernacle.

The Farmer's Cabinet and American Herd Book. Philadelphia.

Southern Planter, July and August, 1842. From Miss. J. R. Shedden, containing an account of some fossil remains found near Natchez.

Wiley & Putnam's News Letter, Sept. 1842.—Literary Gazette, Philadelphia.—Catalogue and Circular of the College of Physicians, New York.—Medical Intelligencer, Philadelphia.

Annual report of the executive committee of the New Haven Young Men's Institute.

Fourth annual circular of the Rutgers Female Institute. From Mr. Chas. E. West, Principal.

Catalogus Universitatis Harvardianæ, 1842. From J. E. Worcester, Esq.

Commerce and navigation, document No. 273, House of Representatives, U. S.

"Dum Vivimus Vivamus!"—while we live let us live. A poem; by Wm. Oland Bourne. New York, 1842. Two copies, from the author.

Catalogus Senatus Academici, etc., in Collegio Washingtonensi, Hartfordiæ in republica Connecticutensi. Hartfordiæ, 1842. From Prof. J. Brocklesby.

Catalogue of Western Reserve College. 1842–3. From Prof. St. John.

Catalogue of the officers and students of Amherst College, for the academic year, 1842–43. From Prof. Hitchcock.

The Professions: an oration delivered before the literary societies of Marshall College, Mercersburg, Pa. By G. W. Burnap.

Proceedings of a convention of the friends of African colonization, held in Washington City, May 4th, 1842.

NEWSPAPERS.—DOMESTIC.

New York Journal of Commerce, July 2d, 1842.—The New World, July 23d, 1842.—New York Daily Tribune, Sept. 3d, 1842.—Berkshire Farmer, Sept. 1842, with an article on geology, by S. Reed.—Wilkesbarre Advocate, Oct. 25th.—Daily Journal, Montpelier, Oct. 1842.—New York Journal of Commerce, August 13th.—Morning Courier and Inquirer, Nov. 7th, 1842.—Farmington (Me.) Register, containing an account of an excursion to the summit of Saddleback Mountain.—Brooklyn Daily News, Oct. 5th, 1842.—State Telegraph, Jackson, Miss., October, containing a lecture on geology by Dr. Gist.—Ohio State Journal, Columbus, Oct. 1842.—Boston Daily News, July 25th.—Signal of Liberty, Ann Arbor, Michigan, with the prospectus of this Journal.—Troy Daily Whig, July 2d, 1842.—Farmer's and Mechanic's Advocate, Stamford, Aug. 17th, containing accounts of a severe thunder storm.—Dyott's Oracle of Health.—The Columbus Enquirer, Georgia, Aug. 31st, containing an account of a midsummer at the north.—Meteorological table for Cincinnati, July; from M. G. Williams, contained in the Cincinnati Chronicle.—Albany Gazette, Oct. 21st, 1842.—Evangelical Press, Aug. 1842.—Extra Evening Tatler.—Albany Gazette, Oct. 28th, with an advertisement of geological cabinets.—Troy Daily Whig, Oct. 2d.—Georgia Messenger, Macon, June 16th, an account by T. S. Bailey of singular phenomenon in the rivers on 7th of May.—Maryland Republican.—Washington Post, with account of the fair and cattle show. From A. Fitch, Jr.—Daily Evening Tatler.—New Monthly Messenger.—Bethel Flag.—Buffalo Republican.—Standard, Bridgeport, Conn., containing a catalogue of the mammalia of Connecticut; by J. H. Linsley.—A series of the Planter's Banner, Franklin, La.—Christian Intelligencer, 2 Nos., containing letters on geology, by and from Dr. W. C. Brownlee.

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THE

AMERICAN

JOURNAL OF SCIENCE, &c.

Art. I.—*Geological and Miscellaneous Notice of the Province of Tarapaca;* by John H. Blake—with a map.

Tarapaca, the southernmost province in Peru, is situated between latitude 19° and 21° 30′ S.; the Andes on the east, which separate it from Bolivia, and the Pacific Ocean on the west.

This province possesses many points of interest to the naturalist, and affords perhaps as interesting a field for research as any other portion of the continent of South America of the same extent. It forms a part of the great desert of Atacama; but though entirely destitute of vegetation, excepting in a few spots which are irrigated by water derived from the melting snow of the Cordilleras, it is much less desolate and lifeless, in its general aspect, than that portion to the south, which forms the western part of Bolivia and the northern part of Chili, where the oases are more widely separated, and throughout a large tract of country no living thing is to be found.

Two ranges of mountains and a plain, nearly level, extend north and south throughout the province; and between the Andes and the eastern chain of mountains lies an extensive plain inclined to the west. The surface of this latter plain is broken by numerous streams from the Cordilleras; it is formed of debris from the Andes, and is covered by huge angular masses of feldspar and trachyte, and numerous fragments of pumice and grains of sulphur. Among the neighboring mountains are seven or eight volcanoes, some of which occasionally emit a small volume of sulphurous vapor. Parts of this plain afford support to coarse grass and bushes; one kind of Cactus grows rankly, attaining the height of eighteen or twenty feet, and a foot or more in diameter. It is collected by the inhabitants, and being split, serves for doors and rafters to their houses.

The range of mountains bounding this inclined plain on the west, and separating it from the great pampa, or plain, of Tamarugal, is composed of sandstone, with beds of gypsum, and is intersected by deep and abrupt ravines, some of which extend to the sea, while others terminate on the eastern border of the plain. The range of mountains on the western side of the pampa of Tamarugal is composed of feldspar-porphyry, resting on granite. The base at the west, in many places, is washed by the Pacific Ocean; but in parts of the coast, as at Iquique, a plain of several square leagues intervenes, composed of shells adhering together and of the same species as now exist in great numbers on the shore. The greater portion of these shells are partially decomposed, and may be easily crumbled to powder, while many of them are perfect and bear no marks of abrasion. Inland toward the mountains they form a compact uniform bed, scarcely a trace of the original shells being discernible, and as we approach the shore the forms become gradually more distinct till we meet with the living shells on the coast. Near the mountains the plain is covered with fine siliceous sand, the position of which is constantly shifting by the wind, forming hills of considerable elevation, and presenting a great variety of beautiful plains and curves.

The pampa of Tamarugal, lying between the two ranges of mountains before mentioned, is from three thousand to three thousand five hundred feet above the level of the sea. At the north it is bounded by a similar plain of greater elevation, and at the south by a deep and broad *quebrada*, through which runs an inconsiderable river, called the Loa. The surface presents clay, sand, gypsum, and common salt, mixed with nitrate and sulphate of soda. The three former substances, separately, cover large tracts of country, as do also the latter, united in various proportions, and nearly free from earthy matter. In some parts of the pampa, particularly at the south and east, the beds of clay are many miles in extent, and present a surface uniformly smooth and level, and so hard that when riding over it the hoofs of the mules make no impression. On the eastern side, further north, the surface is sandy, and scattered over with numerous fragments of pumice, basalt, chalcedony, carnelian, and agate. Between Matilla and the mountain of Chalacollo, the soil is covered in several places with calcareous tufa for the space of an acre or more. Shrubs are here standing in the same position in which they grew, the smallest twigs remaining, presenting the singular spectacle of a once rank and luxuriant thicket converted into stone. Gypsum, more or less mixed with fragments of shells and marl, constitutes a large part of the surface of the pampa at the north. In the extreme northern part it presents a very remarkable appearance, being in flat rounded masses, slightly concave on the upper surface, from five to fifteen inches in diameter, and from one to two inches in thickness. They are compact and hard, and contain a few minute fragments of basalt. The same form also occurs in the beds of salt, which likewise constitute a large part of the northern section of the pampa, but in much larger and less regular masses; presenting on a grand scale the same appearance which is observed when evaporating saline solutions, where pellicles form and fall to the bottom of the vessel. These cakes of salt, many of them five or six feet in diameter and a foot thick, contain little insoluble matter; they lie piled one upon another to the depth of several feet, presenting a rough, white, and glistening surface, over which the traveller may ride all day without his horse's hoofs once touching the soil. Although more abundant at the north, these unmixed beds of salt are found in other parts of the province.

In the western part of the pampa, in latitude 19° 50′, at an elevation of about three thousand five hundred feet above the sea, and about two hundred feet above the adjoining plain, limestone, containing shells, rises from a bed consisting of pebbles and shells which are cemented together by salt, principally nitrate of soda. Part of the shells are decomposed, while others are perfect in form, and like those still found living on the rocks in the inlets of the sea. The same variety of limestone occurs on the opposite side of the mountains, near Molle, and is traversed, as is also the feldspar-porphyry of the neighboring mountains, by veins of the same salts which unite the shells and pebbles of the plain. Among the sandstone hills on the opposite side of the pampa, particularly in the vicinity of Pica, similar veins of anhydrous sulphate of soda occur. Many of them are a foot wide, and can be traced for several hundred yards; they are compact, hard, and dry, and at a little distance resemble veins of quartz. Barely a trace of insoluble matter was found in specimens taken from different parts of several veins. Some of the cavities afford small rhombic crystals.

In the northern and eastern parts of the province are numerous quebradas, or abrupt ravines, commencing at the base of the Cordilleras and extending in a westerly direction; some of them intersect the pampa and both ranges of mountains, others terminate at its eastern border, dividing the eastern range only. Numerous similar ravines intersect the country situated between the Andes and the sea, both north and south of the province we are describing. A remarkable feature disclosed by them is a difference of level on the sides, which has evidently been occasioned by the upheaving of the one or the subsidence of the other.

These quebradas vary in depth from a few hundred feet to three thousand feet below the level of the plain, and in width from a hundred yards to five or six times that distance. The bottoms are covered with sand and pebbles, bowlders of porphyry, feldspar, and granite, and huge angular fragments of trachyte, sandstone, and gypsum. In various parts of these ravines, where the rock is exposed, both on the bottoms and on the sides, are deep scratches or grooves, running in the direction of the ravine. The sides present bold precipices. On those of the quebradas which terminate at the border of the pampa, near the mountain of Chalocolo, water lines are plainly discernible; and the crevices in the rock, at an elevation of several hundred feet above the plain, are filled with the same kind of clay which covers for miles this part of the pampa.

The quebradas are generally barren; but in some parts of that of Pisagua, *alfalfa* is raised in considerable quantity. In the eastern parts of those of Camarones, Chisa, Pisagua, and Tiliviche, are small streams which take their rise in the Andes; they are absorbed or evaporated before they reach the sea.

Beneath the surface of a part of the pampa, lies an extensive forest of large trees, all of which are more or less inclined to the southwest. They are, for the most part, of the Algarobo species. The wood is dark brown, inclining to red, and very brittle; it burns freely and with little smoke, although it contains a large portion of resin. Parts of some of the trees have the appearance of having been charred. From latitude 20° I have traced this forest for nearly sixty miles in a southeast direction. About thirty miles further north, trees have also been discovered, and it appears not improbable, that the whole of this now barren plain was once a fertile and thickly wooded valley. In some places the branches of the trees are near the surface; and often, receding from these points in all directions, they are found more deeply buried, indicating an uneven surface of the valley in which they grew.

By sinking wells through the saline soil of the pampa, water has been found in some places at the depth of ten or twelve feet, while in other parts excavations have been made eight or ten times this depth without meeting with it. In general, after passing a few yards through marl, the wells terminate in a layer of coarse sand. On the western border of the pampa are several wells which have been sunk through trachyte, and brackish water obtained at a depth of from twenty to thirty five feet. In the neighborhood of Almonte, during my visit to that place, workmen were engaged in sinking a well, and had then attained the depth of one hundred and fifty feet without meeting with water. This well passed fifty feet through marl and clay, two feet through coarse sand, eighty feet through clay, ten feet through fine gravel, and terminated in a bed of coarse gravel and pebbles, mixed with large water-worn stones.

In the vicinity of Pica are two hot springs, one of which is 92° and the other 98° Fah. The water contains a small portion of carbonate of soda.

Among the hills which skirt the coast, and at their base on the western side of the pampa, are beds of *nitrate of soda*, which cover a tract of country not less than one hundred and fifty miles in extent. They are slightly elevated above the level of the plain, and covered by a light, dry, sandy marl, mixed with minute fragments of shells. This covering yields with a crackling noise to the pressure of the feet while walking over it, and thus affords an indication of the presence of nitrate of soda beneath, and is a common guide for those who are in search of it. Below this, and but a few inches from the surface, there is usually a layer of common salt, about a foot thick, possessing a coarse fibrous structure. Under this lies the nitrate of soda, resting on marl impregnated with saline matter and mixed with fragments of shells.

This salt, technically termed *caliche,* varies in the quantity of nitrate of soda which it affords, from twenty to seventy five per cent. With it there is generally more or less insoluble matter, consisting of red marl and fragments of shells, in some beds amounting to nine per cent., but averaging not more than three per cent. It possesses a granular structure, arising from irregular rhombic crystals, which vary considerably in size in different localities. Some of the beds are exceedingly compact, and when wrought, require to be blasted with gunpowder; while others are easily broken with the aid of a pick and shovel. Cavities are occasionally found partly filled with crystals, regular in form and nearly pure. The color varies in different beds, and in different parts of the same bed. Some specimens possess the whiteness of refined loaf sugar; others are reddish brown, lemon yellow, and gray. Every variety is found in the same bed, but the compact white and yellow is most abundant between the quebrada of Tiliviche and the point called Molina. The composition of average specimens from the beds which are worked, as determined by Mr. A. A. Hayes, is as follows:

Nitrate of soda, ...	64.98
Sulphate of soda, ...	3.00
Chloride of sodium, ...	28.69
Iodic salts, ...	0.63
Shells and marl, ...	2.60
	99.90

We are indebted to the same gentleman for our knowledge of the presence of *iodate of soda* and chloro-iodate of magnesia in combination with this salt.*

In various parts of the western coast of South America, between 18° and 23° of south latitude, nitrate of soda is found impregnating the soil in connection with other saline matter, and in some instances forming a thin crust on the surface; but no where in extensive beds as in the province of Tarapaca, between 19° 30′ and 20° 45′ south latitude, and 69° 50′ and 70° 5′ west

* The 'mother' water, at some of the refineries on the pampa, are very rich in iodic salts; their presence was first observed by noticing the deep blue color produced by some crumbs of bread which had accidentally fallen into the vats.

longitude, although it has been frequently mentioned by travellers as abundant in other parts of the coast. This error has probably arisen from the general use of the term *sallitre*, which is applied alike to saltpetre and other salts.

The nitrate of soda of Tarapaca affords employment for a large part of the inhabitants of the province. In 1837 one hundred and fifty thousand quintals were shipped from the port of Iquique; of this, about two thirds went to England, and nearly one third to France. Its recent introduction as a manure will probably greatly increase the demand for it in foreign countries.

The process of refining, through which the crude salt passes before it is transported to the ports for exportation, is rude and simple. The operation is conducted generally by Indians, under the direction of a Spanish major-domo. Each *officina* or working place, consists of a few rude huts, the walls of which are constructed of cakes of salt, cemented together with the mixed marl and salt obtained from the kettles in use for refining, the roofs being formed of mats, supported by rafters of Cactus.

All the work of refining is conducted in the open air. The apparatus consists of a few copper kettles, of the capacity of fifty gallons each, set within walls formed of cakes of salt, and shallow oblong square vats for crystallizing. The salt, as blasted from the bed, which is always near to the officina, is carried in bags on the backs of laborers near to the kettles, where women and children are employed in breaking it into fragments of the size of hens' eggs. About two thirds of each kettle being filled with the broken salt, and water added, a strong fire is maintained until the water becomes saturated, when it is dipped into tubs to settle, and from thence transferred while hot to the crystallizers. The undissolved portion which remains, consisting principally of chloride of sodium and earthy matter, is thrown aside as worthless, although frequently not more than one half of the nitrate has been separated, the same relative proportion of crude salt being at all times used, without regard to its quality.

Aside from the want of economy displayed in the refining process, the affairs of the officinas are well conducted. Each branch of the operation, from the breaking the salt from the bed up to the time when it is placed on board vessels for exportation, is conducted by a distinct class of laborers, who receive for their work a fixed sum on each quintal of the refined salt produced.

The cost to the refiner for labor, for each one hundred and two pounds, is about five reals, or 62½ cents; for fuel, from two and a half to three reals; for powder and tools, about one real; and for transportation to the port, from five to six reals; making in all $1 87½, which is probably something more than the average cost of nitrate of soda, exclusive of bags for packing, and the expense of constructing, keeping in repair, and superintending the establishment.

Ores of silver, antimony, and copper, are found in the porphyritic hills on the coast—the two former near Iquique, in extensive veins, the latter in inconsiderable quantity near Pisagua and in the vicinity of Tanna. Copper also occurs at the extreme southern and eastern part of the province, in veins traversing feldspar. The ores are sulphurets, carbonates, and muriate. In the same range of hills, a little further south, and without the limits of the province of Tarapaca, this latter ore has been found in such quantity as to give rise to extensive workings. It is procured by the Indians, and sold, under the name of *arenilla*, as sand for letter writing.

The silver mines of Guantajay and Santa Rosa, near Iquique, were formerly extensively worked, and have yielded a large amount of silver; but of late years, owing to the increased expenses of mining and the poor quality of the ore obtained, most of the workings of the former, and many of those of the latter, have been abandoned. The mines, at the time when I visited them, did not yield ore containing in the mass more than 0.31 per cent. of silver; but they formerly yielded an abundance of rich ore, and have afforded some of the largest and purest masses of native silver which have been found. In 1758 and 1789, two are said to have been discovered here, the one weighing eight hundred and the other two hundred pounds. Native and horn silver are still often extracted, but it is from the antimonial silver ores that the principal profit is derived. A mixture of chloride, sulphuret and native silver, mixed with galena and accompanied by quartz, is found in some small veins. The matrix is generally carbonate of lime, and the veins vary in width from a size barely perceptible to more than a foot.

The observed temperature of the air at the bottom of the workings, in the mines of Santa Rosa, was 98° Fah. That of the air at the surface, at the same time, was 84° Fah.

There being neither water nor fuel in the part of the country where these mines are situated, the ores are transported on the backs of mules to Tirana, for the extraction of the silver which they contain. The process here made use of for that purpose is simple, and, compared with that adopted in many parts of the country, economical. The ore, after having been assorted and broken into coarse fragments, is ground to fine powder by means of a semicircular stone, resting on a flat horizontal bed, which is rocked back and forth on the ore by men stationed on each side. It is then mixed with calcined shells, salt, and mercury, and boiled with water in a copper pan for six or eight hours. When the amalgam formed is sufficiently rich in silver, it is pressed in skins, through the pores of which a part of the quicksilver passes, leaving a ball sufficiently solid to be removed to a stone bench, where it is laid on a grating and covered by an iron cone. This cone is then made secure, by luting at the base, and a fire kindled around it to expel the remainder of the quicksilver, which is collected in a dish of water placed beneath. The silver is left light and porous, and in this state is known as *pláta penia.* Purchasers before buying generally melt it and run it into bars, or expose it for a long time to a red heat in a furnace, in order to ensure the removal of all the quicksilver.

In the vicinity of Tarapaca, and in the quebrada called Cauisa, to the southward of Matilla, and in the mountains of Chalacollo and Chalacolito, are small veins of sulphuret of antimony, which afford traces of silver. Near Cocina is a small vein of galena, and in Cauisa there are extensive veins of iron ore.

In the towns of Tarapaca, Camina, Pica, Matilla and Tirana, the soil is cultivated; but in the remaining towns of Iquique, Pisagua, Mexellones, on the sea-board, and the mining settlements of Guantajay and Santa Rosa, the soil is barren, and the inhabitants are dependent upon distant places for their supplies of provisions and fresh water. The latter is frequently brought from Chili, and commands in Iquique the high price of six cents per gallon.

In those places where there are means for irrigating the soil, though nearly destitute of organic matter, it is extremely fertile and yields the various fruits common to tropical climes. In the town of Pica grape vines are cultivated, and the fruit affords a peculiar wine of excellent quality. The coast furnishes an abundant supply of *guano*, which is the only manure made use of.

Formerly this article was procured from a small island opposite to Iquique; but this source has now become nearly exhausted. On the coast, a few leagues to the southward, it is found in large quantity, and it is from this place that a large part of the guano used in the country is derived. A number of small vessels are constantly employed in the trade, and it has been estimated that a hundred thousand quintals are yearly sold in Peru.

The value of this substance as a manure was known to the Peruvians before the time of the Spanish conquest; it had been transported hundreds of miles for fertilizing the soil of distant places. It is still carried on the backs of mules over rough mountain paths, many leagues inland, and at a great expense, for the use of the agricultural districts of Peru and Bolivia.

Most of the inhabitants of the ports of Tarapaca, of which Iquique, containing about twelve hundred, is the principal, are engaged in the saltpetre trade, while those of the inland towns are dependent upon agriculture, mining, and the reduction of silver ores for their support.

The climate is highly salubrious, and many of the inhabitants live to a great age. The sky, generally deep rich blue, is sometimes diversified with a few light flocculent clouds, but it never rains. The air is clear and dry, and the heat of the sun's rays intense, yet, owing to the extreme dryness of the air, and the consequent rapid evaporation, if protected from the direct rays of the sun, one suffers but little from the heat of the climate.

Upon wetting the bulb of a thermometer with water, in the shade, the mercury has been observed to fall 18° of Fahrenheit's scale. Dead bodies dry without putrefying, and in all parts of the province where there is much travel, the dead bodies of mules and horses are seen, often thrown up in piles as landmarks for the traveller. In the church-yard at Iquique two bodies were left on the ground by their relatives, who were unable or unwilling to pay the fees required for their interment in consecrated ground. Six weeks after I saw them in the same spot; they had become dry and shrivelled, without emitting any disagreeable odor.*

* As a further indication of the extreme dryness of the climate, I may mention that while travelling further south, among the Andes of Atacama, I met with a vein of common salt, pure and beautifully transparent. Some of this salt I afterwards saw in an Indian village, ingeniously wrought into frames for prints—the lustre undiminished, and the salt and paper perfectly dry.

In the southern part of the province, the phenomenon called *mirage* is often witnessed. Beautiful lakes, sometimes dotted with islands and bordered with bushes, are presented, and so perfect is the illusion that I have more than once followed for miles these deceptive appearances, in the hope that they might prove real. Sometimes objects appear of enormous dimensions; and, by such as are familiar to us, a singular sensation is produced, as when meeting a traveller on horseback, who is distinctly seen and almost within speaking distance, yet appearing of gigantic size.

A sea and land breeze daily occurs, and the air from the mountains sweeping over the pampa often produces whirlwinds, which carry up columns of sand from eighty to one hundred feet high. During the night thin strata of air, coming from the mountains, and much colder than the surrounding atmosphere, are often felt, producing a sensation, on the exposed face or hand, not unlike that produced upon coming in contact with a cold rod of iron. Contraction of the muscles, attended with severe pain, is sometimes the consequence resulting from exposure to them, and the Indians, who term them *mal-ayres*, are careful to avoid them by covering themselves with their *ponchos* when sleeping in the open air.

The province of Tarapaca is not rich in remains of the ancient inhabitants; there are vestiges however of interest in several parts of the plain. On the summit of a very regularly formed conical hill, near Tanna, are two large circles, one within the other, formed of large blocks of stone, which were evidently carried there from a distant part of the valley beneath, and if without the aid of machinery, at an immense expenditure of labor. Similar circles of stones, like those erected by the ancient Celtæ, are not uncommon in Peru and Bolivia.

At the base of the hill are the remains of a few stone habitations, the walls of which have fallen and are nearly buried. Removing the sand from one of them, the floor was found to be composed of cement, smooth and hard. A few earthen vessels, and several flat and hemispherical stones, were discovered; the latter had probably been used for grinding corn.

A mile or two from this place is an ancient cemetery, where a large number of bodies have been interred. Unlike those near Arica and many other parts of Peru, these bodies have for the most part crumbled into dust. They are buried in a sitting posture, with the arms crossing the breast; and are wrapped in cloths of woolen, some of which are fine and richly colored. As in the burial place near Arica, many of the skulls found here are elongated, full two thirds of the cerebral mass being behind the occipital foramen.

In the southern extremity of the pampa a single grave was discovered, distant from any remains of inhabitants, containing a body lying in a horizontal position, and dressed in skins of penguins neatly sewed together. At his side lay a bow and a quiver of arrows, the heads of which were formed of carnelian.

In various parts of the pampa are figures from twenty to thirty feet in size, formed in the sandy marl of the plain; the lines are from twelve to eighteen inches broad, and six or eight inches deep. The origin and meaning of these large hieroglyphics is unknown.

The most useful and extensive works of the ancient inhabitants which remain, are in the town of Pica, and consist of tunnels excavated through the sandstone of the inclined plain at the base of the mountains, for the purpose of obtaining water for irrigating the soil, and for which purpose they are still used by the Spanish inhabitants. These tunnels extend for a great distance, and when it is considered that they were formed without the aid of tools of iron, we must allow to the people who constructed them no small degree of skill, perseverance and energy.

Art. II.—*A New and Simple Method to find the Perpendicular Height of Mountains, Headlands, &c. above any given datum, from Barometrical and Thermometrical Observations;* by Oliver Byrne, Professor of Mathematics in the College for Civil Engineers, London.

[Communicated for this Journal.]

Rule.—Add the allowance found in Table I, for the difference of temperature taken by the attached thermometer, to the logarithm of that height of the barometer which corresponds to the least degree of the thermometer. Then to the logarithm of the difference of the logarithms of the heights of the barometer observed at the higher and lower stations, thus corrected, add the logarithm of the allowance found in Table II for the mean temperature of the detached thermometer, when increased by the constant number .92102; this sum will be the logarithm of the required height in fathoms.

Obs.—The first four figures of the logarithms of the heights of the barometer, together with the indices, are to be counted whole numbers, and the numbers taken from Tables I and II must always have five places of decimals, though they need not always be used.

Tables I and II may be dispensed with, as 456789 answers to a degree of the attached thermometer in Table I, and 0024680 to a degree of the detached in Table II.

Table I.

Of the allowance for the difference of the temperatures of the attached thermometer.

	Tenths.	Units.	Tens.			
1	0	4	567	8	9	
2	0	9	135	7	8	
3	1	3	703	6	7	
4	1	8	271	5	6	
5	2	2	839	4	5	
6	2	7	407	3	4	
7	3	1	975	2	3	
8	3	6	543	1	2	
9	4	1	111	0	1	

Table II.

Of the allowances for the mean temperatures of the detached thermometers.

	Tenths.	Units.	Tens.	Hundreds.			
1	0	0	0	24	6	8	0
2	0	0	0	49	3	6	0
3	0	0	0	74	0	4	0
4	0	0	0	98	7	2	0
5	0	0	1	23	4	0	0
6	0	0	1	48	0	8	0
7	0	0	1	72	7	6	0
8	0	0	1	97	4	4	0
9	0	0	2	22	1	2	0

Previous to M. De Luc commencing his experiments on the barometer, it was taught that a mean between the two temperatures shown by the thermometer attached and the height of the mercury in the barometer at two different stations, was sufficient to determine the perpendicular distance of those stations. But De Luc found by repeated experiments that an additional or detached thermometer was likewise necessary, which has since been confirmed by General Roy, Sir G. Shuckburgh, and others. However, before making further remarks, we shall illustrate the rule and tables just given by practical examples.

Examples.

I. The heights of the barometer at the bottom and top of a hill are 29.862 and 26.137 inches; the attached thermometer at the bottom and top indicates 68° and 63°; also, the detached thermometer at these stations gives 71° and 55° respectively. It is required to find the perpendicular height of the mountain.

Thermometer attached.		Thermometer detached.	
Lower station,	68°	Lower station,	71°
Higher "	63	Higher "	55
Difference,	5		2)126
		Mean,	63

Barometer at summit, where attach'd thermometer indicates least degree, } =26.137, log. 14172.557

From Table I, for 5 units, we have		2.28394
Log. corrected,		14174.84094
Barometer at the base =29.862, log.	14751.189	
Take	14174.84094	
Log.	576.34806	=2.7606848
Then, from Table II, for 6 tens,	14808	
For 3 units, (making in all 63°,)	00740	
Constant,	92102	
Log.	1.07650	=0.0320140

Height in fathoms =620.4385, corresponding to log. 2.7926988

II. Wishing to know the perpendicular height of Chraughaim mountain in the county of Wicklow, and having two barometers and detached thermometers which for months before precisely agreed with each other in different states of the air; leaving an assistant on a level with the sea near Arklow, with directions to make accurate observations every fifteen minutes from 3 to 4 o'clock, (our watches being previously regulated,) I proceeded to the top of the mountain, and at the appointed hour commenced observations. The mean result of the five were as follows: the barometer stood at the summit 28.635, and at the base 30.609 inches; attached thermometer 61° and 65.5°, and detached thermometer 54.5° and 70°, respectively. It is required from these data to find the height of the eminence.

Thermometer attached.		Thermometer detached.	
Lower station,	65.5°	Lower station,	70°
Upper "	61	Upper "	54.5
Difference,	4.5		2)124.5
		Mean,	62.25

Barometer at summit, where attached thermometer is least,	=28.635, log. 14568.972	
For 4 units, from Table I, we have		1.82715
For 5 tenths,		.22839
Log. corrected for temperature,		14571.02754

Barometer at the sea =30.609, log.	14858.491	
Subtract	14571.02754	
Log.	287.46346	=2.4585880

From Table II, we have,

For 6 tens,	14808	making up for 62.25°.
For 2 units,	00494	
For 2 tenths,	00049	
For 5 hundredths,	00012	
Constant,	92102	
Log.	1.07465	=0.0312671

Hence the height in fathoms = 308.926, log. 2.4898551

It may be observed, that this experiment was repeated at different times, and consequently in various atmospheres, yet the result never varied two feet. We may therefore conclude that the highest summit of the Wicklow mountains is very nearly 1853 feet above the level of the sea.

The above rule will be found to give results as accurate as either that of General Roy or of Sir G. Shuckburgh, and can be applied with greater ease. The investigation of this rule will be given in the author's course of mathematics; the present performance is too limited to enter on such an enquiry.

General Roy makes the height in fathoms

$$=[10000l\mp.468d]\times[1+(f-32^\circ).00245];$$

Sir G. Shuckburgh

$$=[10000l\mp.440d]\times[1+(f-32^\circ).00243] \text{ fathoms.}$$

Where l = the difference of the logarithms of the heights of the barometer at the two stations; d = the difference of the degrees shown by Fahrenheit's thermometer, attached to the barometer; f = the mean of the two temperatures shown by the detached thermometer, exposed for a few minutes to the open air in the shade, at the two stations. The sign *minus* takes place when the attached thermometer is highest at the lower station, and the sign *plus* when it is lowest at that station. 10000 (log. M – log.

III) was the expression formerly given to find the altitude in fathoms; M, III being the heights of the mercury at the base and summit of any eminence.

This formula is very easily applied, and not far from the truth, when an allowance is made for the increase of temperature above 31°, for this is the degree of temperature to which the above formula is calculated.

As air expands very nearly $\frac{1}{435}$th part of its bulk for every degree of heat, and suffers the same contraction for every degree of cold, the following rule was usually given.

Rule.—Observe the height of the mercury at the bottom of the object to be measured, and again at the top, as also the degree of the thermometer at both these situations, and half the sum of these two last may be accounted the mean temperature. Then multiply the difference of the logarithms of the two heights of the barometer by 10000, and correct the result by adding or subtracting so many times its 435th part as the degrees of the mean temperature are more or less than 31°: the last number will be the altitude in fathoms.

Ex. III. If the heights of the barometer at the bottom and top of a hill are 29.37 and 26.59 inches respectively, and the mean temperature 26°, what is the height?

$$\begin{aligned} \text{Log. } 29.37 &= 1.4679039 \\ \text{Log. } 26.59 &= 1.4247183 \\ \hline &\ 0.0431856 \end{aligned}$$

And $.0431856 \times 10000 = 431.856$.

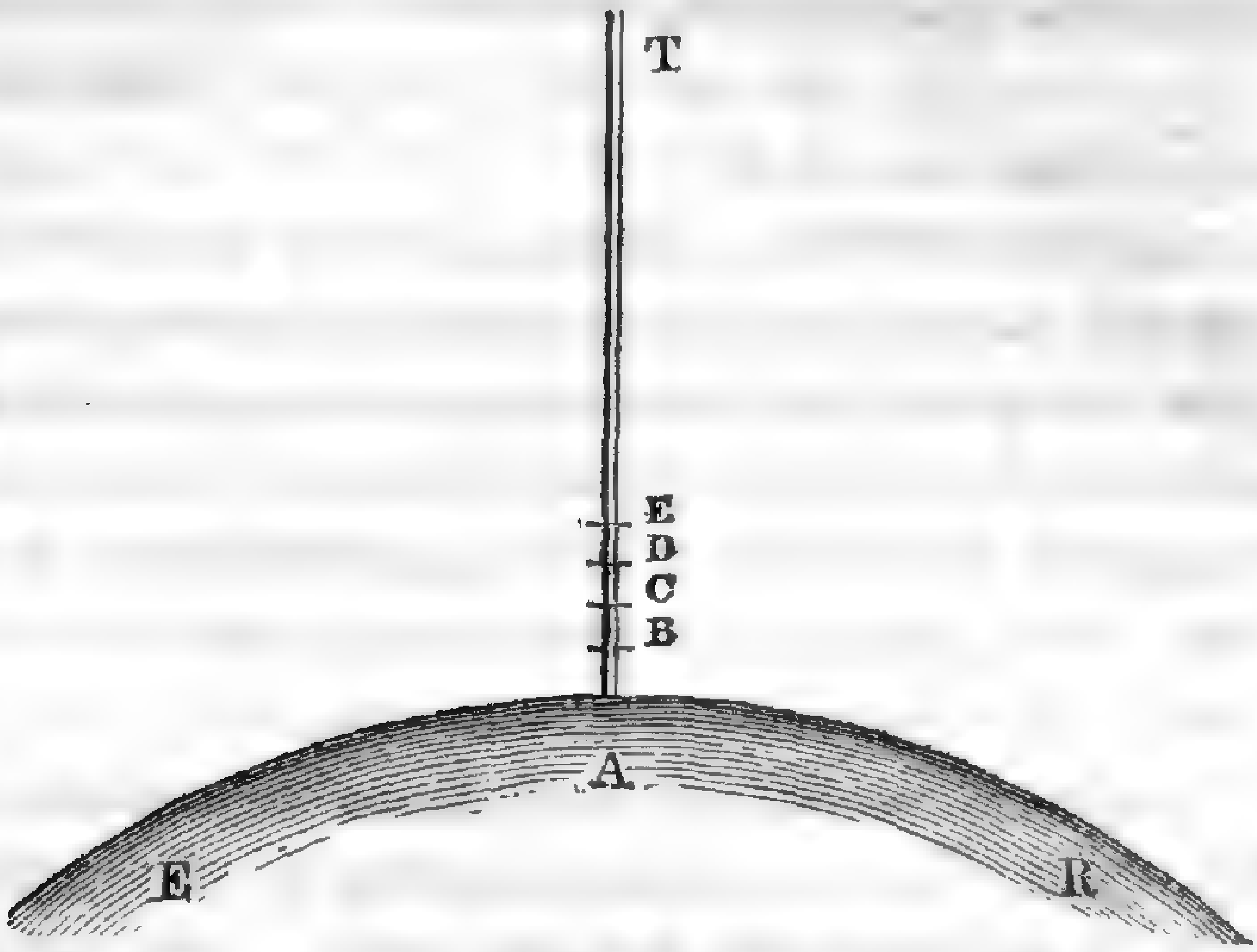
Then $31° - 26° = 5° =$ the mean temperature below 31°.

$\therefore 431.856 \times \frac{1}{435} \times 5° = 431.856 \times \frac{5}{435} = 4.964$, to be subtracted.

Height, $431.856 - 4.964 = 426.892$ fathoms.

We shall now investigate the last formula, to give an outline of the theory upon which this proposition is founded.

Let EAR represent part of the surface of the earth, and AT a column of the atmosphere. Conceive this column to be divided into a number of equal and infinitely small parts, as AB, BC, CD, &c., in each of which we may suppose the density to be uniform, because they are infinitely small. Now since the density of the air is always directly as the compressing force, therefore we have the density of the air in any of the portions AB, BC, &c. as the weight of the column of the atmosphere above that place; that is, if P represents generally the pressure, D the density of any



place, P′ the pressure at any other place, and D′ its corresponding density, we shall have P : D : : P′ : D′; that is, the pressure is to the density in a constant ratio, and may be represented by $n : 1$; $\therefore$ P : D : : P′ : D′ : : n : 1.

Consequently, $D=\frac{1}{n}P$,

$$D'=\frac{1}{n}P',$$

$$D''=\frac{1}{n}P'', \text{ \&c.}$$

That is, the density at any place is equal to, or may be measured by the $\frac{1}{n}$th of the pressure of the column of the atmosphere above that place, or by the $\frac{1}{n}$th of the compressing force.

Hence, if we make P stand for the pressure at the surface A, and let each of the parts AB, BC, CD, &c. be equal 1, then will $\frac{1}{n}P$ represent the weight or pressure of the part AB, and

$$\therefore P-\frac{1}{n}P=\frac{n-1}{n}P=\text{the pressure at B,}$$

$$\text{and } \frac{n-1}{n^2}P=\text{the density or weight of BC.}$$

In the same way, $\frac{(n-1)^2}{n^2}P=$ the pressure at C,

$$\frac{(n-1)^3}{n^3}P=\text{the pressure at D, \&c. \&c.}$$

So that the pressure, and consequently the density, will decrease in a geometrical progression, as the altitudes increase in an arithmetical progression.

Calling the density at the surface d^n, and the several altitudes 1, 2, 3, 4, &c., we shall have the following corresponding series.

Altitudes,	0,	1,	2,	3,	4,	5,	&c.
Corresponding densities,	d^n,	d^{n-1},	d^{n-2},	d^{n-3},	d^{n-4},	d^{n-5},	&c.

Dividing the latter series by d^n, we have

Altitudes,	0,	1,	2,	3,	4,	5, &c.
Corresponding densities,	1,	d^{-1},	d^{-2},	d^{-3},	d^{-4},	d^{-5}, &c.

This is strictly analogous to the property of logarithms. In fact, the several altitudes form a peculiar system of logarithms, of which the reciprocals of the corresponding densities are the natural numbers; from this circumstance they have been denominated atmospheric logarithms. From a similar circumstance, the Napierian are termed hyperbolic logarithms, because they express the areas contained between the asymptote and curve of an hyperbola. We shall write these atmospheric logarithms with large letters—thus, "Log."—to distinguish them from the Briggean or common logarithms, which are written "$\log._{10}$" or simply "log." and also from the hyperbolic, which are denoted by "n log."

Let a, A represent any two altitudes, and d, D their corresponding densities.

Then will $A = -\text{Log. } D$,

and $a = -\text{Log. } d$;

$$\therefore A - a = \text{Log. } d - \text{Log. } D = \text{Log. } \frac{d}{D}.$$

Now it is a well known property in logarithms, that by assuming different values for the base, there will be as many different systems of logarithms; and it is equally well known, that in all the various systems of logarithms, the logarithms of the same numbers can be converted from one system to another, by a constant multiplier or *modulus.*

The object of our present enquiry is to determine a constant multiplier that shall convert the common logarithm of a number into the atmospheric logarithm of the same number. To accomplish this, let

$$\text{Log. } \frac{d}{D} = x \log. \frac{d}{D},$$

$$\therefore A - a = x \log. \frac{d}{D}.$$

Then making $a=0$, or which is the same, if we suppose d to represent the density of the atmosphere at the surface of the earth, we shall have

$$A = x \log. \frac{d}{D}.$$

In order to find x, let us take the height of a homogeneous atmosphere, when the temperature shown by the thermometer is

31°, and the height of the barometer $29\frac{1}{2}$ inches, at 26057 feet; then the density at the surface, and one foot above it, will be

$$a=0;\ d=26057.$$
$$A=1;\ D=26056.$$

That is, the pressure at the surface will be equal to a column of air of uniform density of 26057 feet high; and consequently, one foot above the surface =26056 feet high, or a foot less.

Since the densities are as the pressures, we have

$$A-a=1=x\log.\frac{26057}{26056};$$

and making $26057=n$,

$$\text{we have } 1=x\log.\frac{n}{n-1}.$$

$$\text{But } \log.\frac{n}{n-1}=M\left(\frac{1}{n}+\frac{1}{2n^2}+\frac{1}{3n^3}+\frac{1}{4n^4}+\&c.\right)$$

And when, as in the present case, n is a large number, all the terms but the first may be neglected as unimportant; also since M=.43429448, the modulus of decimal or common logarithms,

$$\therefore 1=x\times\frac{.43429448}{26057};$$

$$\text{whence } x=\frac{26057}{.43429448}=60000, \text{ very nearly.}$$

The above formula is reduced to

$$A=60000\log.\frac{d}{D}\text{ feet};$$

or putting m and M the height of the mercury at the earth's surface and at the altitude A, then the fraction

$$\frac{d}{D}=\frac{m}{M}.$$

Also since six feet are equal to one fathom, the simple multiplier 60000 for feet becomes 10000 for fathoms, which is more convenient.

Hence instead of $A=60000\log.\frac{d}{D}$ feet,

$$\text{we have } A=10000\log.\frac{m}{M}\text{ fathoms},$$

which is the formula formerly used in measuring altitudes by the barometer.

With respect to the height taken for the homogeneous column of air different writers vary, but this difference does not affect the ultimate result. It is well established, that the height of an homogeneous atmosphere whose density would be equal to that of the air at the earth's surface, and weight the same as that of the real atmosphere when compared with a column of mercury or any other fluid of the same weight, the heights will be reciprocally as the specific gravities of the air and mercury or other fluid. So that, if we take the specific gravity of the air at the earth's surface at $1\frac{1}{5}$ when compared with distilled water at 1000, and that of mercury at 14000, also the column of mercury in the barometer $=29\frac{1}{2}$ inches, we have

$1\frac{1}{5} : 14000 :: 29\frac{1}{2} : 344166$ inches$=28680.5$ feet$=5.43$ miles.

But the specific gravity of fluids varies as their temperatures vary. It has been found by various experiments, that when the mercury in the barometer stands at 30 inches, and the thermometer at 55°, the specific gravity of air, water and mercury are nearly as $1\frac{1}{5}$, 1000 and 13600. Hence,

$1\frac{1}{5} : 13600 :: 30 : 340000$ inches$=28333\frac{1}{3}$ feet$=5.366$ miles,

the height of a homogeneous atmosphere. Again, taking the specific gravity of the air at the earth's surface at $1\frac{2}{9}$, which some affirm, and the barometer at $29\frac{1}{2}$ inches, it will be

$1\frac{2}{9} : 13600 :: 29\frac{1}{2} : 328255$ inches$=27358$ feet$=5.1814$ miles.

Hence, generally, we may assume if the air was of the same density at all altitudes as at the earth's surface, its height would be between five and six miles. But it matters not what degree of temperature we assume, for we can always accommodate the result to any other temperature, as before observed, by augmenting or diminishing the result by the $\frac{1}{435}$th part for every degree above or below 31°.

It may further be observed, that the common barometer is the best and most to be depended on; for many which are said to be improved, have only the recommendation of deviating from it in simplicity. It appears from accurate observations, that mercury stands higher in tubes of a larger than in those of a narrower bore; and therefore, when observations are made with different barometers, attention should be paid to the difference of their diameters. In order to prevent the effects of the attraction of cohesion, the bore of the tube should not be less than one fourth of an inch; but one third of an inch would be better.

We cannot conclude this problem better than by giving a remark of Prof. Dalby's, whose practice in trigonometrical surveying was extensive.

"In determining altitudes by the barometer, it is best to make the observations at the upper and lower stations at one and the same time as nearly as possible; but great care must be taken that the two barometers and also the thermometers are alike; that is, they should precisely agree when together in all states of the air. It is also necessary that the specific gravity of the mercury be well ascertained, because it is not equally pure in all barometers, which is the principal reason why different results are frequently obtained from observations made with different barometers at the same stations. Other circumstances however, not generally known, may contribute to such disagreement."

Thus Mr. Ramsden proved by experiment, that the quicksilver in barometer tubes made of different sorts of glass, will be sustained at different heights.*

Art. III.—*Description of Ancient Remains, Animal Mounds, and Embankments, principally in the counties of Grant, Iowa, and Richland, in Wisconsin Territory;* by S. Taylor—with four plates.

Few subjects have of late years more engaged the minds of scientific antiquaries, than the mounds in the valley of the Mississippi. It is in reality one fraught with much interest, and from which the veil of obscurity has never yet been drawn; and unless future investigations may illustrate facts which will aid the curious in tracing these antiquities to their origin, it is feared that the mystery will ever remain unsolved.

In the Holy Scriptures we find but a single passage wherein works in the form of mounds are spoken of, which passage instructed the wandering Israelites to establish mounds, in order to guide them on their return to the land of their nativity. The words referred to may be found in the twenty first verse of the thirty first chapter of Jeremiah, which are as follows: "Set thee up way-marks, make thee high heaps: set thy heart toward the high-way, *even* the way *which* thou wentest: turn again, O virgin of Israel, turn again to these thy cities." This passage, it would seem, strengthens the conjecture that the aborigines of

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* It is usual at present, in accurate meteorological records, to note the difference of height between barometers of flint and hard glass. See the tables of the Royal Society.

America are descendants from the Israelites, and probably from the lost ten tribes. Historians also inform us that tumuli, similar to those found in this region, were constructed by some nations of antiquity, among whom were the Scythians and Tartars. Could we be led to believe that North America was originally peopled by these wandering hordes, we might safely conjecture the origin of the myriads of various formed monuments of antiquity throughout the "great west." One matter, however, at the threshold of investigation, casts an almost insuperable difficulty in our way; and this is, the fact of the non-existence of tradition among the present generation of the Indian race, by which we can have the least hope of unraveling the mystery. This matter I have made a subject of inquiry whenever meeting with an intelligent, communicative Indian, and I have found that the various tribes which inhabit this section of country, express total ignorance on the subject of the origin of the mounds; some however are impressed with a belief, founded upon their superstitious notions, that those in the form of animals were constructed by the "great Manitou"—that they are indicative of plentiful supplies of game in the world of spirits; they are, therefore, looked upon with reverence, and are seldom molested by them. Tribes and even bands differ in their conjectures with regard to them. In conversing with an intelligent French gentleman, who, as a trader, resided near the Pembina (Selkirk) settlements on the Red River of the north, upon the subject of the antiquities of that region, he related a circumstance, which, it would seem, throws a glimmering light upon the origin of one class of these ancient works. After the termination of a battle between the Chippeways and Sioux Indians, (in which in self-defence it became necessary for him to participate,) the women and children of the former, who were the victorious party, in celebrating the achievement, created a mound, from the adjacent surface, about five feet in height, and in diameter eight or ten feet, upon the summit of which a pole ten or twelve feet in length was planted, and to this pole tufts of grass, indicating the number of scalps and other trophies achieved, were tied; around this mound, the warriors, with their usual ceremonies, indulged in mirth and exultations over the scalps of their ill-fated foes.

In this territory we find these works of antiquity in the dense forest, giving nurture to the largest trees, which measure in some instances three feet in diameter, and are frequently based upon the summit of some of these mounds, while upon others their branchless trunks lie prostrate and decaying, impressing upon the mind of the observer the vanity of the shadows which the people of an age long gone by had pursued. We also find them in the sparsely timbered regions, as well as upon the undulating prairie plains, principally in the vicinity of large water-courses, above the influence of high freshets or inundations. It is a remarkable fact, that they are seldom found upon hilly or upon sterile lands. They are also, so far as has come under my observation, confined to certain limits, seemingly in the form of the letter T, beginning at Prairie du Chien, on the Mississippi River, and extending eastwardly, embracing the remains along the Wisconsin River, Blue Mounds, and the Four Lakes, and continuing as far as Rock River, to where the top line of the T, extending from Green Bay, by the way of Fox and Rock Rivers, to the Grand de Tour, in Illinois, touches the upright part of the letter; these strips of country are in width in proportion to their length, covering the ancient works at Butte des Morts, Aztalan, Grand de Tour, and all intervening points. There are, however, many isolated groups in other parts of this region; for instance, those northwest of and in the vicinity of Galena, Illinois. Although their general position seems to be as curiously planned as the structures themselves individually are, it can be readily accounted for from the fact, as before stated, that they generally exist in the vicinity of large water-courses and lakes; and the main streams of the Fox River, of Green Bay, of Rock and Wisconsin Rivers, have their courses in the shape of the letter above mentioned.

May not those works which are found in the forests, have been constructed when the lands upon which they exist were yet prairie? That this region was once wholly submerged, as the sedimentary formations amply demonstrate, admits not of a doubt; subsequent to the subsidence of the waters then, and anterior to the occupancy of these lands by the primitive wanderers who constructed these works, some period must have elapsed; could the era when the waters subsided be traced by the fossils existing in the rock, the era of these ancient remains might be, possibly, conjectured.* We are all agreed, however, upon their re-

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* It cannot be supposed that our correspondent would unite two periods so remote as the geological eras of the fossiliferous rocks of Wisconsin and the sedimentary deposits which cover them, both of which must be considered as very long anterior to the appearance of man on our planet.—Eds.

mote antiquity, although they seem to us, at this time, not more ancient than they appeared to others in this region a century and a half ago.

Many who have written upon this subject, view these antiquities collectively as cemeteries; they suppose that when a location for sepulchral purposes was selected, the grounds in the vicinity were held sacred; that in constructing these tumuli, the material was brought from a distance, or collected in such a manner as to exhibit no indications of the adjacent soil having been removed; and as from the nature of the structure a constant disintegration took place, posterity were enjoined, as they wandered to and fro, to add earth to the heap;—and they believe that by such means these monuments gradually rose into existence. It is very evident that these works *were* heaped up, and by a race that has long since passed away; as to the material of which they are constructed having been brought from a distance, we have no other testimony than conjecture. From the excavations around and in the vicinity of many of them, more especially those in the form of the cross, I am persuaded that the material of which they are composed was obtained from the ground adjacent to them; while in the vicinity of those of other forms, the surface does not appear as though the earth of which they were constructed had been taken therefrom; so that the conjecture appears plausible, that some of these works were heaped up with accumulated material brought from a distance: it is upon dark and mysterious conjecture alone, however, that we ground our opinions. It is true that many of these works are now used by the Indians as burial places; that when they are selected for this purpose, the corpse, after the manner of some tribes, is inhumed in a sitting posture, enveloped in a blanket, accompanied with all its paraphernalia, and as a protection from the ravages of wolves or other rapacious animals, the grave is enclosed with pickets meeting over the centre; (for sketch of Indian tumulus, see Plate II, fig. 2.) Within the enclosure may be seen at times a supply of tobacco and some weapon of defence, and in some instances an ear of corn or other provisions are placed upon the grave! At the death of a distinguished chief or brave, as a token of the esteem in which he was held, at one end of the grave a post is planted, to which a white flag is appended; upon this post, stripped of its bark, the survivors

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inscribe, with red paint, the valor or worth of the deceased, in rude hieroglyphic characters.

Plate V.

Fig. 1 represents a group of earth-works, which I have presumed to designate as a *citadel;* it is situated upon section two, township eight, near the north bank of the Wisconsin River, one and a half miles west of the fourth principal meridian, in the county of Richland. The citadel is singularly planned; the walls comprise embankments of various forms, so arranged as to leave several openings or sally ports, guarded, however, by mounds in the interior; it occupies a prominent level space of about half an acre, the ground to the north, south, and westward, without the embankments for some yards, having a gradual descent, and to the eastward spreading into a beautiful plateau, which gives to the whole structure the imposing appearance of having been constructed as a place of refuge; upon the plateau, as well as to the southward, are numerous other embankments of various forms, the dimensions of some of which, they being disconnected, I did not take; to the westward, within four hundred yards, viewing them from the large or central mound in the citadel, may be seen at least a hundred similar to those forming the outlines of the citadel. The elevation of these embankments, generally, is no more than thirty inches, and of the lesser mounds twenty inches, while the altitude of the large mound, overlooking the whole group, is ten feet. The original sod, upon which the *walls* of the citadel rest, as well as the surface of the ground adjacent to many of them, does not appear as though it was used in constructing them. I made no excavations in these embankments, but from an examination of the central mound in the citadel, I have been led to the conclusion that they are similarly constructed. I am not borne out in this conclusion with regard to the construction of all the outer walls of the citadel; around those forming the east and northeast sides, excavations, from whence earth had been removed, are plainly indicated, so that the material, of which this portion of the work is constructed, was evidently obtained immediately adjacent. Notwithstanding the rank growth of vegetation upon all these works, and their having, in all probability, mouldered down from the original height to their present level, the angles and terminations are quite visible; near the northwest angle of the citadel, however, part of the embankment seems to have been destroyed, although it can be traced some distance toward the head of the animal, represented as enclosing the northern side of the citadel.

Fig. 2 is a section of the mound above referred to, showing the manner in which it was examined, as well as the connection of the superincumbent stratum of sod, with that of the original upon which the mound is based. In the examination of this mound, as well as in the measurement of some of the embankments bounding the north side of the citadel, I am indebted to the kindly assistance of James T. Hodge, Esq. of Plymouth, Mass., a gentleman favorably known as having been a member of the geological corps of the states of Maine and Pennsylvania. Mr. H. also visited with me many of the figures noticed in this article. In order effectually to examine the construction of this mound, a shaft, about midway between top and bottom, of sufficient dimensions to remove the earth conveniently, was sunk; in sinking to the depth of eight feet, we reached the original sod, which here assumes a different character from that coating the mound and the adjacent surface, being a hard and compact substance, denominated "hard-pan," caused, no doubt, from the pressure of the immense weight of earth upon it for centuries. The superincumbent mass, being a bed of ferruginous sand, having no appearance of stratification, and being free from admixture, the presumption is, that this mound was *not* constructed, as suggested, by small contributions, but was heaped up in the progress of construction to its completion without intermission. Continuing the shaft through the original sod, (which measures here six inches in thickness,) three feet farther, we found the substratum to be composed of alternating layers of ferruginous earth and sand. Having now sunk the shaft eleven feet from the surface, to remove the earth without a windlass became too laborious; we then, commencing above the hard-pan, penetrated the mound westwardly fourteen feet, being some distance beyond the centre, conveying the excavated earth through the shaft to the surface. Directly under the centre of the mound, in the drift, we sunk another shaft five feet in depth; the substratum here compares with that in the first shaft. Having now found much difficulty in removing the excavated earth, our drift being partially filled, we concluded, before we abandoned the examination, to oust a badger, which we had reason to believe had burrowed into the top of the mound, such places being favorable resorts for these animals. We found but little difficulty here in excavating the earth; it came down in masses, completely filling the last shaft, as well as the drift, leaving for us but a small aperture to make our egress. Having now thoroughly and satisfactorily examined the structure of the mound, which required the labor of two days, finding nothing of a curious nature in it, and feeling our position under this mass of arenaceous earth, completely hollowed out as it was, rather perilous, we made our way to the surface, which we reached in safety.

Fig. 3 I have designated the *horned bird* in my field-book, in order to distinguish it from others; its location is upon the east bank of Blue River, upon section sixteen, in township eight, of range one, west, in the county of Grant, where an extensive group of many hundreds of various forms may be seen. The numerical figures upon the drawing indicate its dimensions; the elevation of the figure at the breast is three feet, gradually diminishing toward the extremities of the wings, horns, and trunk, until they all, in a measure, become blended with the general surface. Bearings, east and west, with the head to the westward.

Fig. 4 approaches nearer to the form of the "turtle," so frequently spoken of by writers on the antiquities of the west, than any of the earth-works which have come under my notice in this region. Having searched in vain for a figure which, with a little stretch of the imagination, might be construed into the form of a tortoise, I have concluded that figures such as the one here represented, may have been by others styled the *turtle.* Figures of this description are by no means scarce in this region; the location of this one is within two hundred yards northwestwardly of the citadel represented in fig. 1; its length from the tip of the nose to its posterior extremity is seventy six feet, where in width it is eighteen feet, and over the projections representing claws, it is thirty seven feet; the greatest elevation near the junction of the neck is thirty inches, and at the narrow end fifteen inches; while the head, neck, and claws, are only nine inches. The whole figure, having a permanent coat of sward upon it, has retained its original shape in great perfection. Bearings, east and west, the head to the eastward.

Fig. 5 seems to have been intended as a representation of some fleet animal, perhaps the deer or the elk; the antlers, however, seem shortened; the head, neck, and tail, are erect, as though in the act of running. This figure is situated within a few feet of the river, and to the southward of the *citadel;* its elevation is about eighteen inches, the length of the body is one hundred feet, and averages in width twelve feet. Bearings, east and west; the head to the westward, and the legs projecting southward.

Fig. 6 is an embankment, situated upon the north bank of the Wisconsin River, and *east* of the fourth principal meridian; it resembles very closely the form of the frog, for which animal it was probably constructed; there are others adjacent to it, of a similar shape, as well as several in the form of a cross, mammillary mounds, and parallelograms. The dimensions of this huge figure, being in length one hundred and fifty feet, may be ascertained by reference to the scale upon which it is drawn; its elevation above the general surface is three feet. Bearings, east and west, the head to the westward.

Plate VI.

Fig. 1. Among the various animal-shaped works of antiquity, in this region, those in the form of the human species are numerous. This figure forms one of an extensive group of these works, of various shapes, situated upon section thirty five, in township nine of range one, west of the fourth meridian, and in the margin of the forest, extending into it, and having large trees growing upon it.* It is truly a giant, and measures, from the extremity of one arm over the breast to that of the other, as will be seen by the measurements noted upon the figure, two hundred and seventy nine feet and eight inches, and from the top of the head to the end of the trunk, one hundred and eleven feet and three inches; over the hips twenty eight feet; its legs in length are fifty four feet and ten inches; the shoulders, head, and breast, are elevated four feet above the adjacent surface; from thence to the extremities of the limbs the

* The arrow accompanying this, as well as all other figures in the drawings, indicates the cardinal points. In copying many of these figures upon the crowded plates, in order to place them in a more favorable position, I have deviated from the usual mode of drawing; this will account for what might perhaps be otherwise looked upon as an error.

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elevation gradually diminishes to one foot. Bearings, north and south, the head to the southward. In the centre of the breast of this figure, there is quite a depression. Indians frequently, after gathering their crops, dig pits in the earth for the purpose of securing their provisions and merchandise from the frosts of winter and depredations of the enemy; these pits, in the language of the French *voyageurs*, are technically called *caches*. The depression in this figure may have been occasioned by an excavation for this purpose. In a group of earth-works about a mile to the northwest of this one, another figure in the human shape, of like magnitude, may be seen; and of the same group, a very large mound forms a part. This mound, in circumference at its base, measures two hundred feet, and in height sixteen feet. The human figures, generally, do not seem to be as well proportioned as those in the form of quadrupeds; it is however strange that they should have been constructed upon such a gigantic scale.

Fig. 3 represents a complete circle, situated two or three hundred yards to the northeast of fig. 1. In wandering through the forest in quest of strange figures, I accidentally discovered this circle; I have since searched for it for the purpose of ascertaining by excavation, whether it may not have resulted from the accumulation of ashes proceeding from circular fires, which Indians on some occasions make, but I was unable again to find it. The embankment of this figure, which I measured on the spot, is only ten inches in height, and in width six feet; the diameter of the circle is one hundred and seven feet.

Fig. 4 was originally an embankment, approaching the form of a bear, but at this time it is partially destroyed by a road passing over it. I fortunately took its dimensions when it was yet perfect; its whole length from forehead to rump was fifty six feet, which was, in comparison to others, of small dimensions; the elevation was only about twenty inches; it may be seen within a hundred yards of my cabin, one mile and a half north of the river in Richland county. Bearings, north and south; head to the southward, and the legs projecting eastward.

Fig. 5 is one of a group of three, closely resembling each other, in the western part of the village of Muscoda, in the county of Grant; its length from the front part of the head to the end of the tail, is two hundred and sixty four feet; the numerical figures upon this one, as well as upon *all* others, indicate the dimensions of the structure, from actual survey and measurement. Its elevation is about thirty inches around the body, while the limbs at their extremities are diminished to a few inches. It lies within a few feet of the Wisconsin River, in an east and west direction, the head to the westward; and the legs of this one, as well as those of its companions, are projecting southward.

Fig. 6. The site of this singular shaped mound is upon an eminence, on section twenty seven, within a mile to the northwestward of the Eagle Mills, in the county of Richland. The northeastern part of the figure, especially from the neck eastward, approaches nearer the form of a buffalo, than any of these works which I have examined, having quite a protuberance, resembling the "hump" upon the back of that animal; the head, if I may so term it, is blended with what I conceive to be the trunk of the human figure, to which, projecting northward, are appended what were perhaps intended to represent horns. I must admit, that however much we may feel inclined, in viewing these antiquities, to let our imaginations lead us into erroneous ideas respecting them, I could not, at first sight, persuade myself that these appendages were really intended to represent horns; although after a second and third examination, I feel justified, from their intimate connection with the remainder of the structure, in designating them as such, notwithstanding they may, possibly, have been caused by the uprooting of trees. The elevation of the body of the part resembling the *buffalo*, near the hump at the widest point, is three feet; the legs, tail, neck, and horns, diminish as they recede to their extremities, to one foot. The southwestern part of the structure represents the trunk, head, and outstretched arms, of what may be termed the human figure, the arm extending northwestwardly being much the longest; the head, breast, and shoulders, are elevated three feet, while the end of the arms are only a few inches. This structure, differing so widely from all others in this region, is peculiarly strange, unless we can arrive at the conclusion that the animal-shaped mound has been blended in its structure with the one designated as resembling the human figure; and even this one may only represent the same object as is intended by those mounds described as the "citadel," Plate V. Immediately southwest, and within twenty feet of the head of this figure, commences a series

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to face Page 31.

of mounds, similar, with one exception, to those which are represented in the plates; for this reason I omit them. The altitude of this one is about eight feet, and its circumference is eighty feet.

Fig. 7 represents an embankment resembling in form the otter or the lizard; its location is upon section nine, near Blue River, and upon the level plain of the English Prairie. The outlines of this figure are very distinct; its elevation along the body is only fifteen inches; bearings, southwest and northwest; head to the southward, and the legs are projecting westward. The length of this figure, from one extremity to the other, is one hundred and thirty six feet and six inches. Figures of this class are frequently found in this region.

Fig. 8, (which may have been intended to represent a bow and arrow, or perhaps the rude sketch of a human figure, or bird with expanded wings,) lies north, and within a few rods of the Wisconsin River, about four miles west of the village of Muscoda. The elevation of this mound is only about one foot, while other figures in its vicinity are very prominent. In the adjacent forest, we find abundant evidence of industry, in the existence of a multiplicity of extensive groups of these monuments of antiquity; and, being in a beautiful and luxuriant district of country, they tend greatly to prove, that a dense population and a powerful people formerly dwelt upon this lovely site.

Plate VII.

Fig. 1 represents an interesting group of earth-works, in the village of Muscoda, (English Prairie,) in the county of Grant, the general direction of which is N. N. E. and S. S. W., beginning upon the bank of a bayou, near the river, and passing through several enclosures. The late cultivation of these grounds has in a measure obliterated and leveled these works, from what may be supposed was their original height; many of them are in the streets and upon the commons; the village in its future increase may cause a complete destruction of these works, so as to obliterate every trace of their shape. In this group are three figures in the form of the cross; in the centre of the largest of these, represented upon the figure by a circle, is quite a depression, occasioned, perhaps, in the same manner and for the same purpose as described in Plate VI, fig. 1; the outlines of these works are easily traced, although for the reasons above assigned, their elevation at this time does not exceed thirty inches. From the excavations around many of them, it is apparent that they must have been constructed with materials obtained adjacent to them. Some of these mounds, however, seem to have resisted the destructive action of time; those toward the southwest end of the group, are in height six feet; the distance from one extreme of the series to the other, is about four hundred and sixty yards; consequently, in order to include the whole cluster in one plate, it became necessary to reduce the scale, by doing which they appear diminutive, although they are constructed upon a scale equally grand as that of others. The site here is a beautiful level plain of arenaceous loam, being free from trees or shrubbery, (substratum, a fine white saccharoidal sand-rock,) so that a person from the eminence of the most prominent ones, may at a glance view the whole group. Human bones have been found in many of these.

Fig. 2 represents a species of ancient works, which, under various modifications, are very numerous, and comprise about one fifth of the embossed works in this region. This figure agreeing with the location, it being about "one mile from the English Prairie," is probably one of the groups referred to in an article on the subject of antiquities, published in Vol. XXXIV, of this Journal, by R. C. Taylor, Esq., a copy of which, from an esteemed friend, I was fortunate in obtaining, which document, in the absence of other works upon antiquities, has rendered me essential service in my observations upon this intricate subject. In regard to the "group of six," spoken of by Mr. T., I have frequently traversed the forest where they are said to exist, in search of figures having projecting beaks, as represented in Plate II of his article, but I have discovered none approaching that form nearer than that one under consideration.* Those figures near the Blue Mounds and the Four Lakes, which were personally examined by Mr. T., I am happy in saying, are faithfully represented, and many of them I have had the gratification of visiting.

* It must not from these remarks be inferred, that I have the remotest intention of accusing either Mr. T. or his informant of deceiving the public, by fictitious statements.

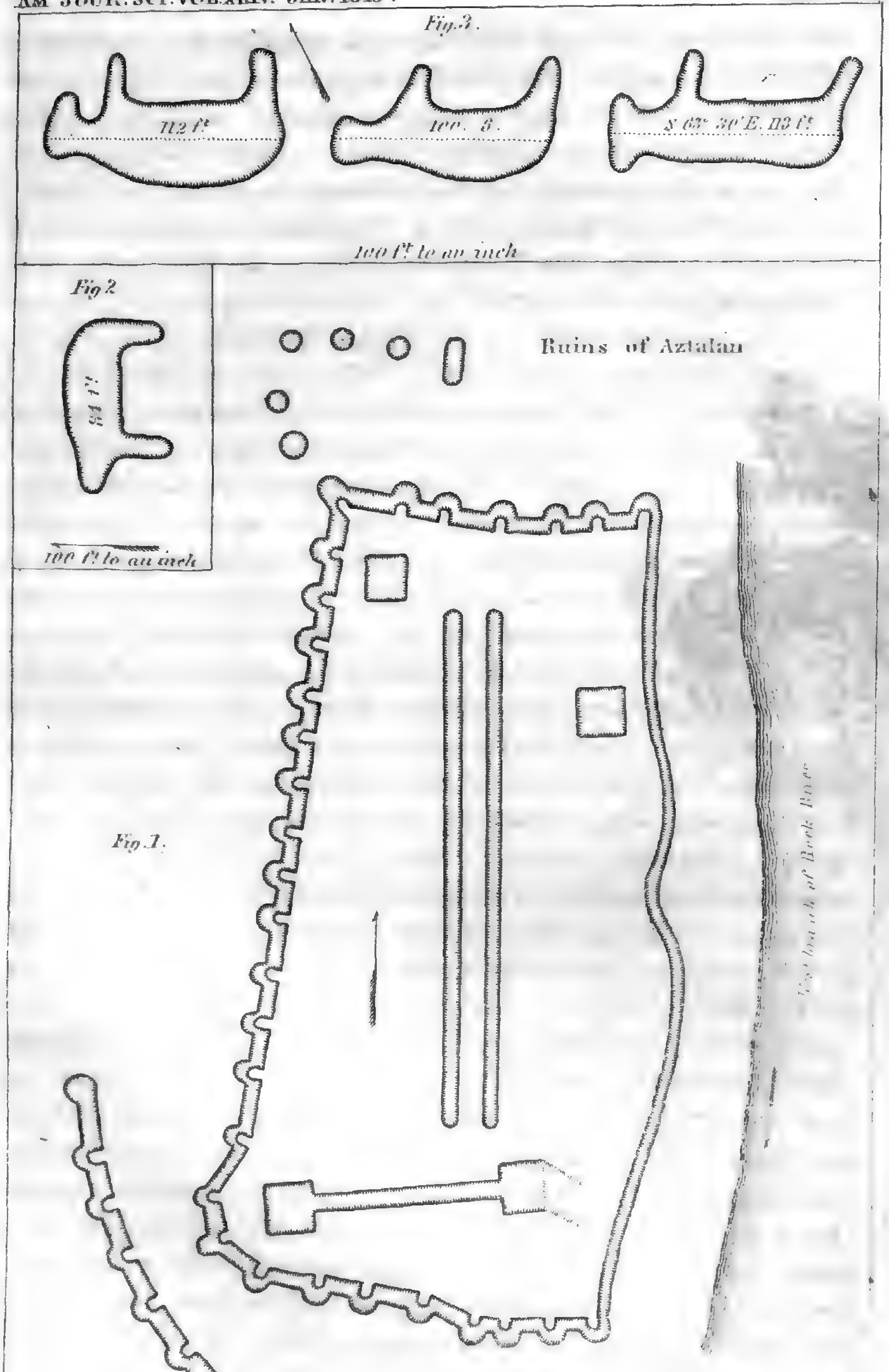
The elevation of this figure, as well as those of the group of which it forms a part, about the head, shoulders and breast, is four feet, diminishing toward the extremities, as in other cases, to one foot. Bearings of the trunk, N. 85° E., the head to the eastward. Between the base of the trunk and the southern wing, a mound measuring twenty one feet in diameter and five feet in height is erected. Notwithstanding the grand scale upon which this figure is constructed, there are no indications around or in its vicinity, which would in the least convey an impression that the adjacent earth had been removed, or that the material of which this work was constructed had been taken therefrom. The numerical figures along the dotted lines in the drawing, diverging from a common centre upon the breast, indicate the dimensions of the structure.

Fig. 3 is the centre one of a series of mounds, fifteen in number, extending the distance of about three hundred yards, and placed at intervals of twenty-five feet apart. It seems apparent that this figure must have been originally constructed as represented in the drawing by the dotted lines, having at these points an elevation of three feet, and that subsequently additional earth was heaped upon the head, breast and end of the trunk, elevating these parts three feet above the other points of the structure, now measuring six feet. Bearings, N. N. W. and S. S. E.; the head to the northward. The site of this figure, upon the northeast part of section thirty-five, north, and within a mile of the Wisconsin River, west of fourth meridian, is a commanding swell in the forest, and were it not for the lofty timber, would be an eligible position for an observatory, having an interesting view of the Wisconsin River for some distance above and below, and of the beautiful English Prairie to the southward, which is barricaded as it were by magnificent bluffs, extending along the river many miles.

Fig. 4 represents the terminating figure of the same series as that of fig. 3, above referred to. This figure, with the exception of the *brush*, resembles the reynard, with drooping tail; that it was really intended to represent that animal, I am not prepared to say. Earth-works of this form are frequently met with in this region, more especially in the forest in Richland County. For its dimensions, the reader is referred to the scale accompanying the drawing. Bearings, N. 45° W.; the head to the northwest, and the legs projecting southwestwardly; general elevation, eighteen inches.

Fig. 5 is a sketch of the outlines of a curious animal-formed embankment, which lies adjacent to the road, in the vicinity of fig. 8 of Plate VI. The form of this structure is very perfect and well proportioned; its elevation is about three feet, being highest about the middle and toward the back; its length from one extreme to the other is seventy-nine feet, and in width, over the middle, twenty-four feet. Bearings, east and west; the head to the westward, and legs projecting southward. Throughout this region embankments of this form are very numerous, some of which have two parallel projections from the back of the head, while in the present one they seem to be so blended as to represent but one. There is, I believe, in zoological history, no analogy to this figure.

Fig. 6, situated upon and near the east line of section thirty-five, to the eastward and within a mile of fig. 1, Plate VI, represents a human figure having *two heads!* which gracefully recline over the shoulders. This singular figure, so unlike the other works of similar form which have come under my notice, is the most perfect that I have seen; the arms, however, are disproportioned, being much too long; their full length in the drawing, for want of room, I have not delineated; all the whole parts are gracefully rounded; the stomach and breast are corpulent; and the entire structure seems to have retained, as I conceive, its original form through all the dilapidations of time. The perfection of this truly singular and interesting specimen of ancient earth-works, is convincing evidence that the ancient inhabitants of this region were *not* as ignorant of the arts as we have reason to believe the present race of Indians are; their works, however, prove that they possessed industrious habits, even if their labors had been bestowed upon objects of no apparent utility. The dimensions of this figure are as follows: widths, from one arm-pit, over the breast, to the other, twenty-five feet; over the arms, at shoulders, twelve, and tapering to four feet; over the hips, twenty feet; over the thighs, near the trunk, eight, and tapering to five feet; over the figure, above the shoulders, fifteen feet; over each neck eight, and over the heads ten feet: lengths of body, fifty



Ancient Remains, Animal Mounds &c.

feet; of legs, forty feet; of arms, one hundred and thirty feet; of necks and heads, from termination of dotted line, fifteen feet: elevations at breast, shoulders and abdomen, thirty-six inches; arms, at junction with shoulders, same height, diminishing toward their extremities, where they are but ten inches; the thighs, near the trunk, are twenty, while at the feet or extremities they are but ten inches. Bearings, north and south; the heads to the southward.

Plate VIII.

Fig. 1 represents the most singular group of ancient works yet discovered in western America. This monument of an ancient people is in the form of *mounds*, *embankments*, *buttresses* and *outworks*, which, connectedly, very much resembles a *fortification*, and was in all probability constructed for this purpose, by a warlike people! These antiquities, by common consent, are known as the "ruins" or "city of Aztalan," and they are situated in a flourishing region of country, destined, probably, to have in a few years as dense and powerful a population as we have reason to believe flourished there in former times. This is the same "ancient city," which a few years ago had its day, and in a measure shared the fate of the notorious "moon hoax," in the *press* of our country. The Cincinnati Whig, however, more lenient than many other papers, viewing it as a subject well worthy of scientific investigation, concluded a very interesting article, entitled "the ruins in Wisconsin," with the following paragraph:

"The whole subject is full of interest to all, and eminently recommends itself to the antiquary and the historian, and we hope that it will soon be adequately illustrated; but until we shall have more complete and authentic accounts of these ruins, their position to the points of compass, their extent, plan, material of construction, &c., it will be vain to enter into speculations concerning them. An accurate detail of facts is wanted for the basis of reasonable conjecture. All we can do in the mean while is to correct or expose the unauthorized guesses of ignorance and presumption."

I have never personally examined the "city of Aztalan," although I have been a resident of the mining region since the time it was still under the jurisdiction of Michigan. Having undertaken an interesting subject, and being desirous of embracing in it, with the antiquities of the western part of the territory, those of the ruins of Aztalan, I could conceive of no better mode of acquiring facts concerning them, than soliciting the aid of a friend, in whose probity and competency every reliance may be placed, to furnish me with a description of those ruins, as well as the character of the adjacent country. He has given me the following interesting account, which, depriving it in a measure of its epistolary form, I will here introduce.

"In answer to your enquiries respecting the ancient remains some years since discovered in the eastern portion of this territory, known as the 'ruins of Aztalan,' I regret that the limited opportunity afforded me of a minute examination of the same, will preclude the possibility of affording you such a description at this time as I could wish; although I have no doubt that a thorough examination would throw much light upon this mysterious subject. One thing I am convinced of, and that is, that I with many others who have looked upon the different accounts which have from time to time appeared, respecting the ancient works in this widely extended valley, as a deception and the ephemeral efforts of a penny-a-liner, have been mistaken, for it is no deception, but is well worthy of attention and investigation.

"Ancient embankments and tumuli of different form and construction are, as you are aware, abundant in many portions of our territory; I have examined many of these remains, but in my different researches have not as yet discovered the objects of the constructions which appeared to me at first view so distinct, or which forced upon my mind so strongly the conviction of the existence hereabouts of a former race of beings more skilled in the arts, than any of the present tribes of savages now known to us, as the appearance of the ancient mounds at and near to the town of Aztalan. The 'ruins' you particularly refer to, are to be found in the midst of a beautiful rolling country, conveniently interspersed with timber, and watered by Rock River and its tributaries; the principal remains, or what has been termed the 'walled city,' is situated in township seven, of range fourteen, east, in the Milwaukie land district. I know not that a better idea of the present appearance of these ruins can be conveyed than by the accompanying sketch, prepared some time since by Judge N. F. Hyer, a gentleman who has taken much interest in reference to this subject, and to whom I am indebted for much interesting matter respecting the same during a hasty ramble over the embankments and mounds, and through the immediately adjacent country. The drawing is faithfully executed; the 'citadel,' as there represented, consists of a brick wall, which at the base is from twenty to twenty five feet wide, at the present time, and, I should judge, about five feet in height; the projections of the wall indicated upon the plat, have certainly the appearance of buttresses, as constructed upon military works at this day; they are constructed also of brick, regularly built at intervals of from two to five rods, and extending beyond the wall about seventeen feet, of the same height as the main wall. The eastern wall, parallel with and immediately upon the bank of the river, is, at this time, but slightly visible, nor are there any appearances of buttresses, as upon the other portions of the wall. In proceeding upon the supposition that these are the ruins of an ancient fortification, we may conclude, that inasmuch as the eastern side was defended from egress by a deep and rapid stream, a wall and buttress similar to the one I have attempted to describe as bounding the western side, would have been unnecessary. The whole area, within the wall, comprises about twenty acres; within the enclosure are a number of square mounds or elevated plains, of the height of fifteen or twenty feet, as I should judge, and perhaps forty or fifty feet square upon the top, while others are of a more conical shape, and from their situation appear as what might now be termed block-houses, or places of *look-out;* that such were the objects of their construction, I am not prepared to say. There is also a distinct ridge running east and west, connecting two of these towers or mounds, as well as two parallel ridges running north and south, and extending nearly the whole length of the enclosure. There is also a cellar and stairway, I am informed, yet visible, descending within the mound of the northwest angle of the ruins; this, in my hurried examination, escaped my notice. I can therefore say nothing respecting it. The same remarks must also apply to the termination of a sewer, which is said yet to be perceived at a bend or angle, about midway in the eastern wall designated on the plat above referred to; this sewer is said to be about three feet below the surface, and arched with stone. Whether through this sewer water was supplied from the river or not, others can judge. Without the enclosure, and at those points where this work is not protected by the river, are numerous mounds varying from three to twenty-five feet in height, and from twenty to a hundred feet in circumference; and particularly at the southwest angle, there is an embankment forming the arc of a circle, with projections resembling the buttresses represented in the main wall, which requires but little stretch of the imagination to suppose was intended as an outwork for the defence of that particular point.

"In examining one of these mounds, I found the remains of a human skeleton, which had been previously exhumed, although by the action of fire the bones were so completely charred that they readily crumbled to pieces in the hand. I preserved a small piece of the skull, but have somewhere mislaid it or would now send it you.

"One word as to the 'brick wall;' let me not be understood to say that there is in the brick here found any regular appearance of *brick-laying*, as at present practiced. The walls which I examined, and from which at many different points, with a mattock, I broke off specimens, present now the appearance of a mass of *burned clay.** In what manner at first constructed, there is nothing to indicate, but that the walls and parapets consist of brick rudely burned and prepared with straw, after the ancient mode, the different specimens I gathered bear sufficient witness."

Fig. 2, approaching the form of a bear, is included in that extensive group of antiquities, before referred to, near Blue River, upon the English Prairie. This embankment, being much more elevated than many which I have examined, is about midway six feet in height; and in length, along the dotted line from east to west, eighty four feet; and its greatest width over the body is twenty feet. Bearings, due east and west; the head to the westward, and the legs projecting southward. The whole figure, and the grounds adjacent, are covered with *Corylus Americana* and *Lathyrus albidus*, which shrubs, in this region, are indicative of deep and rich soil.

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* A box, containing specimens of the "burned clay," as well as fragments of rudely platted matting and human remains, in a charred state, dug from these ruins, was some time ago forwarded through the curator, Dr. King, to the National Institution for the Promotion of Science, at Washington, D. C.

Fig. 3 is a group of three animal-formed mounds, which are also upon the English Prairie, about eleven miles to the eastward from fig. 2, and within the limits of Iowa County. The destination of these figures, if I may use the expression, seems to have been toward the northwest, their heads being in that direction; their legs, contrary to the direction of all others which I have examined, are projecting northeastwardly. For a more explicit description of these figures, the reader is referred to the scale upon which they are drawn. In the vicinity of these are many other figures, of various forms and dimensions. To the eastward, at a short distance, commences a series of mammillary mounds, varying from one to two and a half feet in height; these mounds are beautifully and with much regularity ranged at convenient intervals, and extend over a distance of about five hundred yards, terminating abruptly with a huge mound eighteen feet in height, and in circumference at base two hundred and twenty-five feet. To the northward and southward of the *figures*, and parallel with them, are numerous embankments with intervening spaces, representing gateways, a further description of which would only lead to unnecessary repetition.

I am well aware that the actual existence of earth-works as singularly shaped as those represented in the accompanying plates, may be, and even is by many who reside in the midst of them, looked upon with incredulity, which induces me to illustrate one of the many facts which proves that the minds of *all* are not directed alike to the same subject. *Quot homines tot sententiæ*—so, as various as are the opinions, so are the subjects, upon which the tide of their reflections flows. Col. William S. Hamilton, a gentleman who for many years has been a resident of the west, and has for a few years past been engaged in the business of smelting lead in the village of Muscoda, surrounded as it were by these monuments of antiquity, has almost daily passed over many of them without observing their structure, although the outlines of the numerous figures are very distinct; but after having their interesting features pointed out, Mr. H. now acknowledges that what he has heretofore viewed, in the various accounts which have appeared respecting them, as the result of strong imagination or of wild fancy, he had become thoroughly convinced of their singular beauty of construction and of their remote antiquity.

Having now briefly illustrated at least one figure of each species of artificial earth-works, embraced in a scope of country upon both sides of the Wisconsin River five miles in width, and in length fourteen miles, the river passing midway from one extreme to the other; and each figure having been the subject of careful observation, being part of the result of more than twelve months of incessant labor, in rambling in quest and taking admeasurement of about a hundred figures; an anxiety to embrace in this article a variety, has extended the subject and prolonged its completion much beyond what I at first had intended, but should the matter contained in these pages in the least degree aid antiquarians in their researches in Indian archeology, I will be amply rewarded.

In conclusion, fearing that I should have gone too far in hazarding conjectures why or how these antiquities *are as they are*, I ventured to give no conclusive opinion, inasmuch as there have already been too many wild speculations respecting many of them throughout this vast region. I have endeavored only to represent them as they really exist; in doing this it became necessary to take their dimensions in the field with a degree of accuracy, in order that the description may in future serve when all traces of the mounds are obliterated, (which in this fertile region will soon be the case,) as a record of what once existed. The subject is a dark one, and the word *mystery* seems stamped upon every foot-track of those who primitively flourished here. We know not whence they came—we know not where nor how they have departed. A people has passed—a nation has gone away—their history we know not, nor the history of their works.

Forest of Richland, Wisconsin, July 31, 1842.

Art. IV.—*Remarks and Observations on the Formation of Fogs;* by Prof. W. M. Carpenter, M. D.

Meteorologists, in examining into the causes which give rise to the formation of visible vapor, seem generally to have confined themselves almost exclusively to the theory of Hutton, considering it as incident upon the mixture of masses of air of different temperatures, and to have neglected in a very great measure the consideration of other causes. The intention of the following remarks will be to render it probable that other influences may, under favorable conditions, give rise to the same phenomenon; and also to show that the prevailing opinions respecting the conditions requisite to its production, are in some measure erroneous.

In the year 1819, a paper was published by Sir H. Davy, in the Philosophical Transactions, giving his views, and the result of his observations on the formation of fogs and mists. The following is a summary of the results at which he arrived. "After sunset, the earth commences to cool in consequence of radiation, but land and water are cooled by this operation at very different rates: the surface of the land cools much more rapidly than that of the water, and the air over the land becomes colder than that over the water, and when they both contain their due proportion of aqueous vapor, and the land is so situated as to permit the cold air from the land to mix with the warmer air over the water, the production of mist or fog will result. The density of such fog will moreover be greater as the land surrounding the water is higher, and the water deeper and warmer."

Here Davy follows Hutton, in attributing the formation of fog to the mixture of masses of air of different temperatures, and all the forms of visible vapor are now supposed to have their origin in some modification of the same influences.

He goes on to say, that in no case that came under his observation, "was fog formed on a river or lake, when the temperature of the water was lower than that of the air over it, even though the air was saturated with vapor." These remarks have been generalized and a law established, which I believe is generally received as true, that "*the formation of fog never takes place over water*, when its temperature is lower than that of the atmosphere, not even though the air should be saturated with vapor." Now we cannot hesitate about considering the observations of Davy as perfectly accurate, but it will be shown that the law is not of universal application, and that we should, consequently, protest against the too hasty generalization.

From a comparison of my own conclusions, drawn from observations on the fogs of our southern rivers, with those of Davy, drawn from observation on many of the great rivers of Europe, I should infer, either that the agencies governing their formation there must be different from those in which they originate here;

or that his observations, particularly on rivers running towards the south, must have been mainly limited to the last of summer, the autumn, or first part of the winter.* For, were the conditions always observed by him to accompany the formation of fogs absolutely requisite for their production, it would preclude the possibility of their occurrence on the Mississippi River during a considerable portion of the year, particularly during the latter part of the winter, the spring, and first part of summer. For, the large body of water which comes down that river at those seasons, from much colder regions, traverses the climates through which it passes so rapidly, that it does not acquire their temperature; and when it arrives here its temperature is always much below the mean of those seasons, and even below the ordinary monthly minima. Convinced that this must be the case, and aware that the water of the Mississippi River is pleasantly cold for drinking during all the spring and first summer months, which required, at those seasons, that it should be considerably colder than the air, I was satisfied that fogs, which are of common occurrence at those seasons, must be formed much more frequently over this body of water, than the temperature of the air was lower than that of the water. In the latter part of the year 1839, I commenced a series of observations with the view of testing the truth of these conclusions. The following table exhibits the results of observations, made during the formation or prevalence of fogs, when the temperature of the water was lower than that of the air.

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* Since the above was written, this inference has been confirmed by reference to Paris's Life of Davy. I give the names of rivers, on the fogs of which he made observations, with the place of observation, the general course of the rivers as to latitude, and the date of observation. 1st. The Rhine, between Cologne and Coblentz; course, north; date, 31st of May. 2d. The Danube, between Ratisbon and Vienna; general course of the waters from the sources, the tributaries being taken into account, northwardly; date, 9th, 10th and 11th of June. 3d. The Raab, near Kermond in Hungary; course, north; date, 11th July. 4th. The Save in Carniola; course, south; date, "end of August." 5th. The Ironzo, in the Friul; course, south; date, "middle of September." 6th. The Po, near Ferrara; course, south; date, "end of September." 7th. The Tiber; course, south; date, "beginning of October." So that only the Ironzo, the Po, and the Tiber, have southern courses to the points of observation, and on all these the observations were made during the autumn.

Date of observations.	Temperature of the water of the Mississippi River.	Temperature of the air at the surface of the water.	Temperature of the air at the height of six feet above the surface of the water.	Temperature of the air on shore.	Time of day at which the fog formed.
1842, Jan'ry 11,	46°	53.			10 A. M.
1841, Feb'y 15,	49.	59.			10 A. M.
" " 25,	50.	57.			11 A. M.
1840, March 27,	53.	61.25	61.75	62.25	3 P. M.
" " 29,	53.	60.	60.50	61.	11 A. M.
1841, April 1,	56.50	61.50			Night.
" " 2,	56.50	64.			3 P. M.
" " 5,	56.50	63.50	63.75	64.25	Night.
" " 19,	57.	71.			10 A. M.
1842, " 13,	54.	66.			11 A. M.
" " 27,	55.	59.50			9 A. M.
" " 28,	55.	63.			Night.
" May 3,	61.50	67.			Night.
" " 5,	61.50	70.			11 A. M.
" " 13,	61.75	67.50			Night.
" " 16,	61.75	74.			2 P. M.
1841, June 19,	69.	76.			10 A. M.

In one of the cases referred to in this table, the opportunities for making the observations were so favorable, the results so satisfactory, and the nature and variety of the phenomena so characteristic of the particular kind of fogs, that I will give a detailed account of the observations.

On the 27th of March, 1840, I was at the house of a friend, who lives on the bank of the Mississippi River. The weather was cloudy and rather cool for the season in this climate, the thermometer ranging from 55° to 65° Fahr., though it had ranged during some time previous at from 65° to 75°. The house was situated on the eastern bank, at the distance of about four hundred yards from the water's edge, the greater part of the distance intervening being a level plain, elevated at the time of observation only a few feet above the level of the water, then at a high stage; but near the house the land rises abruptly, forming a bluff about fifty or sixty feet in height, and on this bluff, near the declivity, the house is situated.

About noon there was a light shower of rain, and it remained cloudy during the remainder of the day. At 3 o'clock, P. M., being on the gallery, which commanded a view of the river, I perceived that fine wisps of vapor were forming in detached spots over the surface of the water. After examining the thermometer, which hung in a good exposure, I hastened to the water's edge, noted the temperature there, and again over the surface of the river, at the distance of about two hundred yards from the shore, where the temperature of the water was also observed. The results are given in the fourth line of the table, for March 27th, 1840, except that in addition, the temperature at the house on the bluff was 64°, being higher than on the bank.

The fog commenced to form in almost immediate contact with the surface of the water; indeed, when first observed, it had much the appearance of a very fine fleece floating in spots on the water. It thickened very fast, however, and before the observations over the water were concluded, which required perhaps ten minutes, the river was veiled in an impenetrable cloud, which stood over its surface to the depth of about fifteen feet. Although the upper part of the fog was elevated considerably above the level of the banks, it remained stationary over the bed of the river, and did not flow over the adjoining plain at all. The upper surface of the fog was level, and though defined with considerable accuracy, it terminated above in a thin haze; and although we could see nothing of a steamboat which passed near us, when we were on the bank, we had a fine view of all the upper parts of it after we had mounted upon a pile of wood, which raised us above the thickest part of the fog. The fog remained stationary over the surface of the river about an hour, and was then swept off by a brisk breeze which sprang up from the northwest.

The observations given above indicate the absence of those conditions which have generally been considered as requisite to the production of the phenomena under consideration. The temperature of the air was higher than that of the water, and that of the air over land, both on the immediate bank and on the higher lands back, was more elevated than that over the surface of the water. The fog could not have resulted from the mixture of the air from over land with that over the water, as the former was lighter than the latter, and could not flow down to the same level with it; neither could it have been produced by colder air from regions above the surface of the river settling down and mixing with that near the surface; for in this case we would expect to see the vapor commence to appear at some distance above the surface of the water, instead of in the air in immediate contact with that surface. We can hardly fail to attribute the production of fog in this instance to the refrigeration of the air, in consequence of communicating its heat to the water, and notwithstanding the low conducting power of atmospheric air, we must suppose the extension of refrigeration to depend mainly on the conducting power of the humid air. If the diminution of temperature was confined to the stratum of air in immediate contact with the aqueous surface, the deposition of water on that surface would be the only result; but as the chill becomes general through the air above the river, fog will be formed, and will increase in depth and density as the impression is augmented.

The general conclusions at which I have arrived in reference to the occurrence of fogs over the Mississippi River, are as follows:—

1. That fogs form over this river at all seasons.

2. That they occur frequently during the day-time as well as at night.

3. That those occurring during the day-time, though often very dense, are not so extensive as those occurring at night, and rarely extend over land at all.

4. Fogs originating in day-time are of much more frequent occurrence from February to May than at other seasons, though the nocturnal fogs of winter frequently do not disappear before 10 or 11 o'clock in the morning.

5. In a large majority of the observations made by me, during the prevalence of fogs on the Mississippi River, in autumn and winter, the water had a higher temperature than the air. These have been exclusively nocturnal fogs, which frequently extend over large regions of country.

6. In almost every instance in which I have observed the formation of nocturnal fogs over the Mississippi River, during the spring and earlier part of the summer, the river water has had a lower temperature than the air. These vary in extent, without any apparent connection with the relative temperature of the water and air; and the air over the higher lands is by no means invariably colder than that over and near the surface of the water.

7. In every instance in which I have observed fogs to form over the Mississippi River, in day-time, during the spring and

earlier summer months, the temperature of the water was lower than that of the air; and the air over the land warmer than that over the water. These rarely extend over land much, and are most frequently confined to the air over the bed of the river.

8. In all the diurnal and in most of the nocturnal fogs of the spring and first part of summer, observed by me, on the Mississippi River, the production of visible vapor commenced near the surface of the water, and increased in depth by additions above.

It appears therefore that the fogs of the Mississippi River may be considered under two heads:—1st. Those which originate when the water is warmer than the air. 2d. Those which originate when the water is colder than the air. The first kind prevails mainly during the fall and earlier part of the winter; the second kind are most common during the spring and early part of the summer. The first unquestionably results from the condensation of aqueous vapor, which rises constantly from the warmer water into the air, in quantities more than sufficient to saturate it at its temperature. The second, it appears to me, must result from the condensation of aqueous vapor already in the air, in consequence of the extension of refrigeration, from the cold surface of the water, through the warm and humid air above.

Among fogs which form over land, likewise, a large number have their origin independent of the causes assigned by the theory of Hutton. During almost every portion of the year, the high temperature of the air and earth during the day-time, increases the quantity of watery vapor in the air, and the dew-point temperature rises towards the hottest part of the day, so that the complement of the dew-point, except in winter, is rarely more than 10° or 15° Fahr., and most frequently not more than 5° or 10°. The temperature of the air gradually declines until about sunset, while the temperature of the dew-point remains almost fixed; thus diminishing the complement. After sunset, however, the earth cools rapidly by radiation, and the air participates, though not to the full extent, in the diminution of temperature.* It is not unusual to see the temperature of the air diminished by 15° or 20° between 3'clock P. M. and midnight, and

* "In every calm, still night, the air nearest the earth is colder than that which is more distant from it, to the height of at least 220 feet, this being the greatest height to which M. Six's experiments relate."—*Wells, on Dew, p.* 95.

a thermometer, with the bulb blackened, exposed, would indicate a still greater reduction for that of the earth; while the complement of the dew-point of the preceding day was not more than 5° or 10°. If the temperature of the earth alone suffered diminution, the effect would be limited to the deposition of dew, but at the same time that dew is being deposited upon the cooled surface, thus reducing the temperature of the dew-point, the temperature of the air is also diminishing, and it depends altogether upon the relative amounts of the two concurrent decrements, whether any of the atmospheric water will take the form of visible vapor in the air or not. My observations, though not sufficiently numerous to enable me to make a satisfactory application of mathematical formulæ to the results, will still enable us to come to some safe general conclusions on these points. When the diminution of atmospheric temperature and of the temperature of the dew-point depends on the influence of a cold surface, the former seems always to decline most rapidly. This may, I think, be accounted for if we observe upon what the rapidity of diminution in each case depends. The decrement of atmospheric temperature depends for its value—first, upon a certain amount or radiation to the cold surface; second, upon the conducting power of the air, that is, on its degree of humidity, or in other words, upon the temperature of the dew-point; third, upon the difference between the temperature of the air, and that of the refrigerating surface. The decrement of the dew-point temperature will depend—first, upon the deposition of dew, that is, it will be nearly as the amount of humidity; and second, upon the difference between the temperature of the dew-point and that of the cold surface.* If therefore we take c to represent the temperature of the surface, and φ and φ' to represent respectively the difference between the temperature of the surface and that of the air and dew-point, we shall have the temperature of the air $=c+\varphi$, and that of the dew-point $=c+\varphi'$; and if β be taken to represent the effect of radiation, we shall have the decrement of atmospheric temperature proportionate to $\varphi(c+\varphi')+\beta$; while the decrement of the dew-point temperature will be as $\varphi'(c+\varphi')$.

* These suppositions, though not strictly accurate, are sufficiently so for the present purpose, as the variations from the truth affect somewhat equally the two decrements.

Therefore the decrement of atmospheric temperature will be greater than that of the dew-point temperature, by $(c+\varphi')(\varphi-\varphi')+\beta$. If it be denied that radiation has any influence in this case, we still have the decrement of atmospheric temperature greater than that of the dew-point temperature, by $(c+\varphi')(\varphi-\varphi')$. Now this concurrent diminution, by unequal decrements, that of the atmospheric temperature being greatest, tends to reduce this temperature to equality with that of the dew-point; and after the two temperatures have in this way become equal, any farther diminution in the same ratio must result in the production of visible vapor in the form of fog. This explanation, if correct, will apply equally to fogs which form over cold waters, and many which form over land. Over land they occur most frequently towards morning, and are preceded, and perhaps at first accompanied, by the deposition of dew. They are most common in autumn and spring. When they form over large fields, or over our extensive prairies, they first begin to appear as a thin haze near the ground, and become deeper by additions above; but in dense forest regions, the condensation first commences about the tops of the trees, which taken together constitute in fact, in the present instance, the radiating and refrigerating surface.

When heavy and extensive fogs are dissipated in the morning by the heat of the sun, the visible is converted into invisible vapor, and becomes a part of the atmospheric air; but this change is not usually permanent, for the mixed atmosphere is heated, and rising in consequence of diminished specific gravity, it soon arrives at a region where the temperature is as low as its dew-point, when it again takes the form of visible vapor. Clouds thus produced, very often succeed to heavy fogs in this country during spring and autumn; they are generally formed at very short distances above the earth. Often, indeed, the vapor does not lose the visible form at all, but rises slowly until it arrives at a small elevation, when it commences to move as an ordinary cloud. This horizontal motion commences, generally, when the vapor has risen to the height of from one hundred to three hundred feet, but this region of clouds attains a greater elevation as the day advances, until it reaches the ordinary height. This kind of cloud is generally carried over this part of the country, towards the north or northwest, and sometimes continues to pass over during the earlier part of the day, until 10 or 11 o'clock, A. M.

They are not very dense, having rather a hazy appearance, but are very continuous; and it rarely rains from them while they retain this form.

These clouds, as well as the fogs from which they originate, exercise a highly beneficial influence on vegetation; the former protecting it from the direct heat of the sun, while yet moist from the dew and fog of the preceding night; and the latter, by putting a check upon nocturnal radiation, thus lessening the fluctuations of temperature.

Jackson, La., May 9, 1842.

Art. V.—*Abstract of a Meteorological Register, kept at Jackson, (Louisiana,) Lat.* 30° 51′ 25″ *N., Long.* 91° 9′ *W. of Greenwich, during three years ending Jan.* 1, 1842; by W. M. Carpenter, M. D.

YEARS. 1839. 1840. 1841. Months.	THERMOMETER. Mean Temperatures. Sunrise.	2 P. M.	Sunset.	Monthly mean of diurnal temperature.	Monthly mean of nocturnal temp.	Average mean per month.	Maximum.	Minimum.	Range.	Max. temp. sun's light. Bulb therm. blackened.	BAROMETER. Monthly mean.	Monthly average of diurnal maximum.	Monthly average of diurnal minimum.	Monthly maximum.	Monthly minimum.	Range.
January,	45.0	51.5	49.0	48.2	47.0	47.6	74	18	56		29.89	30.00	29.78	30.20	29.50	.70
February,	47.2	53.4	50.0	50.3	48.6	49.4	74	20	54		29.90	30.03	29.78	30.15	29.30	.85
March,	52.2	64.2	58.0	58.2	55.1	56.6	75	33	42		29.83	29.94	29.73	30.50	29.40	1.10
April,	62.3	71.5	65.5	66.9	63.9	65.4	82	54	28		29.92	29.95	29.90	30.12	29.70	.42
May,	67.6	75.7	72.4	71.6	70.0	70.8	85	54	31		29.92	29.94	29.89	30.10	29.70	.40
June,	74.5	84.5	81.3	79.5	77.9	78.7	90	64	26	157.5	29.90	30.00	29.80	30.00	29.80	.20
July,	77.5	88.0	84.0	82.7	80.7	81.7	94	73	21	164.3	30.00	30.02	29.98	30.10	29.78	.32
August,	75.0	86.5	83.3	80.7	79.1	79.9	91	66	25	156.	29.92	29.94	29.90	30.06	29.82	.24
September,	70.0	82.2	78.0	76.1	74.0	75.1	86	59	27	137.	29.91	29.93	29.89	30.05	29 76	.29
October,	63.4	73.0	70.0	68.2	66 7	67.4	85	29	56		29.91	29.95	29.87	30.10	29.69	.41
November,	48.0	54.0	50.0	51.0	49.0	50.0	77	25	42		29.87	29.89	29.85	30 03	29 70	.33
December,	46.0	52.3	49.5	49.1	47.7	48.4	67	18	49		30.00	30.05	29.96	30.20	29.90	.30
Annual mean,	60.7	69.7	65.9	65.2	63.3	64.2					29.91					

Mean temperature of five years, compared with the temperature of well and spring water of the same neighborhood.

Years.	Annual temperature.	Mean temperature of water of wells eighty feet deep.	Mean temperature of the water of the coldest springs.
1837,	64.18		
1838,	64.27		
1839,	64.14		
1840,	64.23		
1841,	64.28		
Mean,	64.24	64.25	64.25

Mo. Bot. Garden,

The water of our wells and springs is derived from extensive strata of sand, lying above, and sometimes between, beds of clay. These strata deviate but slightly from a horizontal position, and as the surface of the country is undulating, the same bed will be reached at different depths at places near together, so that wells are often seen twenty or thirty feet deep, or springs rising from hillsides, having water of the same temperature as wells in the same neighborhood having a depth of eighty feet. When the average depth of a stratum below the surface is seventy or eighty feet, the variation of temperature of the water of wells through the year is scarcely perceptible, and the annual mean is about 64.25°. But in wells bored to strata whose mean depth below the surface is not more than twenty five or thirty feet, the variation is often several degrees, and the mean afforded not lower than 67° or 68°.

Art. VI.—*Some Remarks on the methods in common use of obtaining the Mean Temperature of Places, and on the supposed difference between the Temperature of the Air and that of the Earth;* by Prof. W. M. Carpenter.

It is stated by Humboldt and others, that the mean temperature of the coldest springs in warm climates is often lower than that of the air of the same places. If we examine those agencies in which atmospheric temperature originates, and by which terrestrial temperature is modified, we shall perceive that such a condition could not exist, and consequently, that the observations on which such conclusions were based, were not accurate, or that some unsuspected agency must modify the relations which should otherwise be constant. In examining the meteorological records of our own country and of other parts, and comparing the observations made by different persons resident at the same places, we shall perceive that the results differ not more from the mean temperature of the place, than from each other. These discrepancies, or rather these departures from accurate results, are dependent on many circumstances. In the first place, thermometers of very inferior quality are in very general use, and they will often differ in the results afforded by four or five degrees. In the next place, sufficient importance is not attached to the position of the instrument; and instead of having it completely protected from reflection, and in almost complete obscurity, taking care at the same time to secure a free circulation of air about it, we find them often in such situation as to receive from walls or the earth a considerable portion of reflected heat; often in closed apartments or against walls, the temperature of which is influenced throughout the day by the full force of the sun. Galleries fronting the north are favorite places for suspending thermometers, under the impression that no heat is reflected from that side. In this way we often find, in this climate, that when a thermometer properly placed gives a temperature of 90° or 91°, all the others in the vicinity will stand at 95°, 100°, or even 110°; so that the annual mean derived would be very greatly above the true one. There is, however, it appears to me, a source of error much more general: I speak of the methods of calculating the mean after the observations have been made, and of the adoption into general use of methods which have been found to give correct results at particular places. The methods most in use are the following, viz. to take three observations, and from these calculate the mean directly; some fix upon sunrise, 2 P. M. and sunset; others, as in the Army Meteorological Register, and in those of most of the meteorological societies, upon 7 A. M., 2 P. M. and 9 P. M. I have ascertained, by taking the mean of hourly observations made here by myself, that any of these methods give means which, during every season of the year, are too high, and that the error in excess is greater, in proportion as the diurnal exceeds the nocturnal temperature. The last method, which is now in most general use, has probably been adopted in colder climates, as agreeing experimentally in its results with those derived from more frequently repeated observations during the same period; or possibly under the impression, that the 9 P. M. observation would give an approximation to the nocturnal mean, while the diurnal temperature would result as a mean from those at 7 A. M. and 2 P. M. In either case this method might be the best adapted for many climates, but as the relative value of the temperatures at the times fixed for observation are not constant, but will vary in passing from one climate to another, there is nothing in the method to suit it for universal use. The fact is, it appears to me almost impossible to fix upon any method, except that of hourly observations, which will in every place afford even an approximation to the truth, or obtain harmonious results from elements the relations of which are so variable as those of the temperatures of particular hours.

The method which I have adopted, and had in use about three years, I have found experimentally to give results, for this climate, almost in precise accordance with those derived from calculations based upon hourly observations. I have constantly kept daily observations at 6 and 7 A. M., 12 M., 2 P. M., sunset, and 9 P. M., so that I have been able to give my observation any of the common tabular forms, and have always obtained the general results according to each of the methods, and the results have verified my previous conclusions by varying from each other as follows: the mean annual temperature of this place, by my method, is 64.24° Fahr.; as derived from the sunrise, 2 P. M. and sunset observations, 66.30°; and during the same period, the mean obtained from the 7 A. M., 2 P. M. and 9 P. M. observations is 65.62°. In this way I have compared results by each method with each other, and with those obtained from hourly observations. I have found that the following method gives for this place results more accurate than any other I have been able to devise. The mean temperature of the diurnal portion of the twenty four hours is derived as the mean of the highest and lowest temperatures of that portion, that is, of the sunrise observation and that of 2 P. M. The nocturnal mean will in like manner result as the mean of the highest and lowest temperatures of that portion, that is, of the sunset and sunrise observations. The average of these two means will give the mean for the twenty four hours. By this method, the sunrise temperature being the lowest for the twenty four hours, will belong to both the diurnal and nocturnal portions, and will enter twice into the calculations, while the other two observations will represent the maxima of the portions of the twenty four hours to which they respectively belong.

If it is as I suspect then, that the results of observations and calculations are, particularly in hot countries, frequently too high, a little attention to the subjects above hinted at may show that there is really no difference between the mean temperature of climates and of water derived from such depths as not to be affected by the change of seasons. In my tables the most exact agreement is shown between these temperatures at this place. The same appears to be the case in the island of Cuba, notwithstanding that we find the following statement in the table in which Prof. Kupffer compares the annual temperatures of places with that of the earth: "Havana, temperature of the earth, 74.30°; of the air, 78.12°." Now Havana and Matanzas are in the same latitude, or nearly so, and there can be but little difference between the temperatures at the two places. Mr. A. Mallory, in this Journal, Vol. xxxi, p. 289, gives the annual temperature at Matanzas as 77.06° Fahr., which he derives as a mean of the following averages of observations: sunrise 72.17°, 2 P. M. 81.41°, and sunset 77.61° Fahr. Now a mean of these observations would unquestionably give a mean too high by more than a degree, which would reduce the mean down to about 76°. My friend Mr. W. H. Potter has been kind enough to examine the temperature of wells, &c. for me, during a residence of a year in that island, particularly near Cardanus, in the same latitude and near Matanzas, and the observations frequently repeated through the year; and the temperature, apparently invariable, was 76° Fahr., thus affording a probability at least, that there also the mean temperature of the air, and of the earth at a certain depth, is the same.

What inferences might not be drawn by travellers in this country, particularly if they belonged to the anti-Huttonian school of geologists? I will not go out of my own neighborhood. The temperature of water taken from strata whose average depth below the surface should be seventy or eighty feet, would be found to have at Baton Rouge a temperature of about 64.50° Fahr. But we are informed by the tables of the Army Meteorological Register, that the annual mean for that place is 68.07°, making a difference of 3.57°. Now we cannot mistrust the accuracy with which the observations were noted in this case, nor the correctness with which the calculations were made, but there can be no doubt that too high a temperature has been obtained for want of attention to things generally considered as of minor importance, and in consequence of the too general adoption of methods which have been found to give correct results in other climates.

Jackson, La., May 9, 1842.

Art. VII.—*Notices of some Trappean Minerals found in New Jersey and New York;* by Prof. Lewis C. Beck.

About three years since, I published in this Journal* a notice of some of the minerals, principally copper ores, found in New Jersey. I have continued these investigations during my leisure hours, and have been particularly interested in this work in consequence of the identity of some of these minerals with those of New York, in the study of which I have been engaged for six years past. The present communication will be confined chiefly to such as belong to the zeolite family, to which, however, the more general term *trappean* will better apply.

Short notices of some of these minerals, with reference to the New Jersey localities, will be found in Cleaveland and other mineralogical works; and some details are given concerning them in Prof. H. D. Rogers's Report on the Geology of New Jersey. They occur in greater or less number in many of the trap and greenstone ranges which traverse that state. Of these, Bergen Hill, near Jersey City, has hitherto afforded by far the greatest number of species, as well as the most characteristic and beautiful specimens. A particular account of this locality has been given by Mr. William O. Bourne in Vol. xl, p. 69, of this Journal, and he must have been very successful in his explorations, as his list of minerals sufficiently proves.

My attention was called to the Bergen Hill minerals in 1838, by receiving a collection of them from my friend, the Rev. John L. Janeway, who informed me that he had frequently obtained specimens previously to that time. I have since then made frequent visits to this locality, but I cannot boast of any thing like the collection described by Mr. Bourne. Some of the minerals which I have obtained, however, seem to be of sufficient interest to warrant more than a mere passing notice, and I therefore proceed to the immediate subject of this paper.

Stellite.—This is the name given by Dr. Thomson† to a mineral found in the rifts of a greenstone rock situated on the banks of the Forth and Clyde canal in Scotland. A mineral, which, for reasons to be shortly presented, I suppose to be identical with the stellite, has been found in considerable abundance

* Vol. xxxvi, p. 107.

† Outlines of Mineralogy, Geology, &c., I, 313.

in similar rifts in the greenstone at Bergen Hill. I have also found the same mineral, although less distinctly characterized, in the greenstone at Piermont, the *terminus* of the New York and Erie railroad. This mineral occurs in veins of various degrees of thickness, from an inch to that of a mere coating on the greenstone to which it is attached. In every case, however, the mass is made of fibres or crystals the form of which cannot be exactly determined, usually radiating from various centres, and interlacing each other in an irregular manner. Sometimes these fibres are parallel, when the mineral bears a resemblance to the nemalite of Nuttall. When the bundles of fibres are broken up, they separate into needle-form crystals, which are exceedingly sharp. These needles are tough, and when subjected to the pestle appear like amianthus. The same amianthoid appearance is exhibited by many of the specimens found at the locality, a change which may perhaps be referred to atmospheric agencies.

Color snow-white. Lustre silky, shining. Translucent. Hardness about 3. Specific gravity of a closely aggregated specimen, 2.836.

Alone before the blowpipe, our mineral fuses readily, even when in quite large fragments, and with slight bubbling, into a beautiful white enamel. With borax it melts into a transparent glass. It gelatinizes with muriatic acid. When heated to redness in a crucible, it forms a porcelainous mass from partial fusion. According to my analysis, the following is its composition, in one hundred grains, viz.

Silica, ...	54.60
Lime, ...	33.65
Magnesia, ...	6.80
Oxide of iron, with a little alumina, .	0.50
Water and carbonic acid, . . .	3.20*

Our mineral differs from the stellite of Dr. Thomson in its containing less alumina, oxide of iron, and water; but these may,

* The composition of the Scotch mineral, according to Dr. Thomson, is as follows, viz.

Silica, ...	48.465
Lime, ...	30.960
Magnesia, ...	5.580
Alumina, ...	5.301
Protoxide of iron, ...	3.534
Water, ...	6.108

after all, be accidental ingredients. The ratio of silica and lime, which are the essential constituents, is nearly the same. This mineral bears a close resemblance to Wollastonite, as described by Dr. Thomson; and the proportions of silica and lime are also nearly the same. But Wollastonite contains from nine to ten per cent. of soda. Moreover, in most of its properties, our mineral bears a greater resemblance to the zeolite family than any other; and if so, the only species with which it at all agrees in its composition is that above mentioned.

Mr. James D. Dana in his work on mineralogy remarks, that the stellite of Dr. Thomson is closely allied to natrolite. In answer to this, it is enough to say, that natrolite contains twenty four per cent. of alumina and sixteen of soda. Indeed, nearly all the minerals of the zeolite family contain from twenty to thirty per cent. of alumina; and none of them, I believe, except stellite and apophyllite, contain less than eight or ten per cent. of that earth.

Thomsonite, for which the Bergen Hill mineral has been quite generally mistaken, differs from it in its composition and in its behavior before the blowpipe. Thomsonite swells up like borax, becomes opaque and snow-white, but does not melt; on the other hand, all the specimens of supposed stellite from Bergen Hill melt quite easily into a white enamel. I will not say that Thomsonite has not been found, or does not occur, at Bergen Hill; but I have not met with it in any of the specimens which I have obtained from that locality.

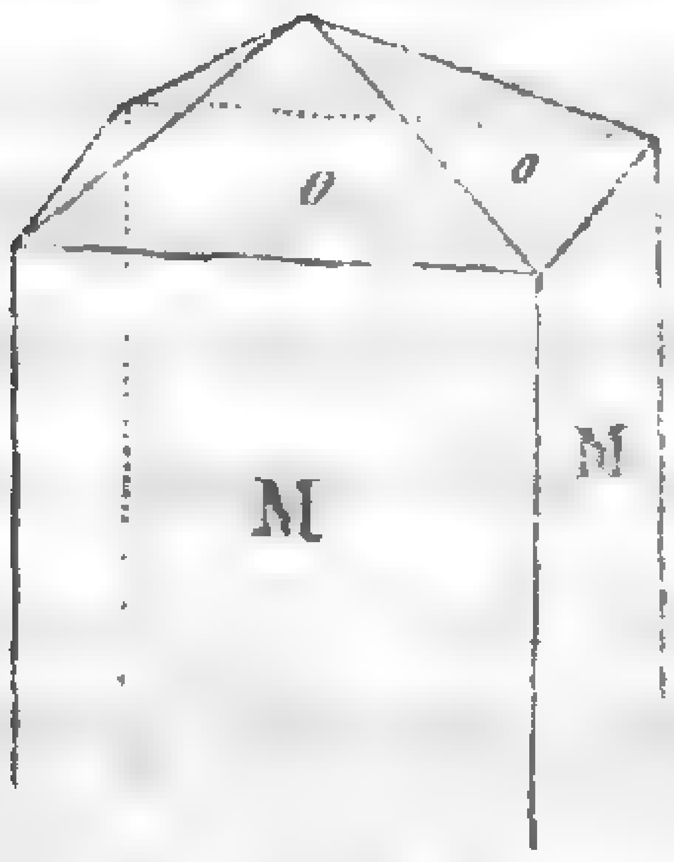
Apophyllite.—This mineral, which is very well described in standard works on mineralogy, occurs at Bergen Hill and Paterson, in greenstone and trap, and at Harlem, New York, in gneiss. The primary form, which is often found at the above localities, is a right square prism. It differs from the other minerals which it resembles, in cleaving easily in the direction of P, the cleavage presenting a high pearly lustre. The faces of the prism have a vitreous lustre, and are often striated longitudinally. The crystals are usually transparent, but occasionally they are entirely opaque and white; and in one of my specimens the change from transparency to opacity seems to be going on in the cabinet.

Before the blowpipe this mineral exfoliates and finally melts into a porous glass. The crystalline forms observed at the above localities are in all respects similar to those figured in elementary works. (Vide Shepard, Vol. II, p. 37.) Most of them in my specimens are quite small, although very distinct. My largest crystal is a little more than a quarter of an inch in diameter.

Mesotype.—I introduce this mineral here because it is apt to be mistaken for the preceding, with which it is often associated. As there is some confusion in regard to this species, it may be proper for me to state that this notice applies to the mineral called mesolite by Dr. Thomson and others, which I have observed only in the gneiss of Harlem, although according to Mr. Bourne, it has been found quite abundantly at Bergen.

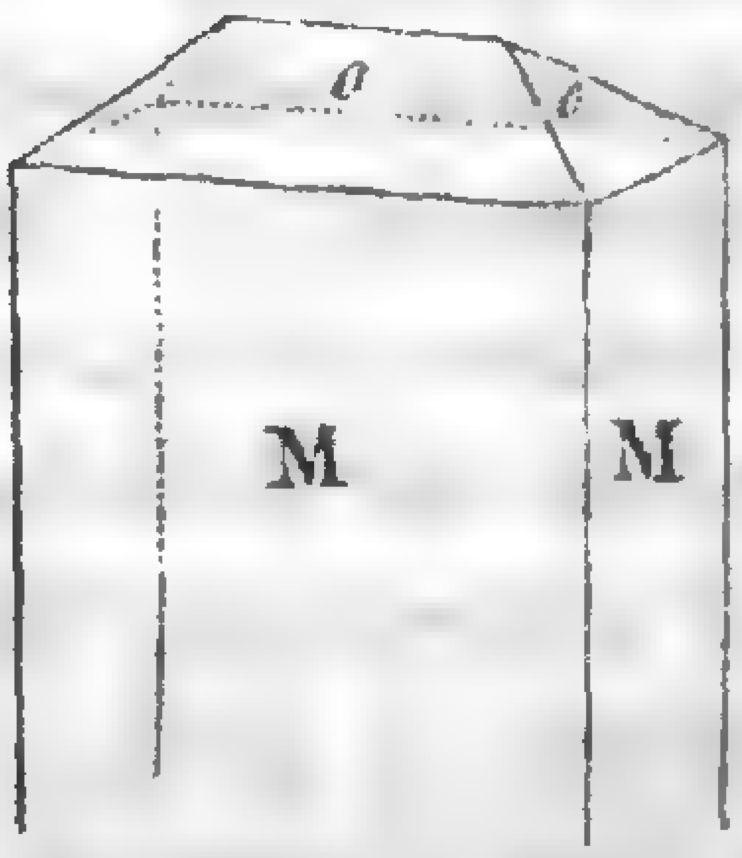
The mineral to which I refer, occurs in very minute but highly finished transparent crystals. The prisms appear to be rectangular, but on inspection they will be found to be slightly rhombic. The terminations, which are usually four-sided, are variously modified, as in the annexed figures. All these forms result from the different degrees of extension of the planes *o*.

Fig. 1.



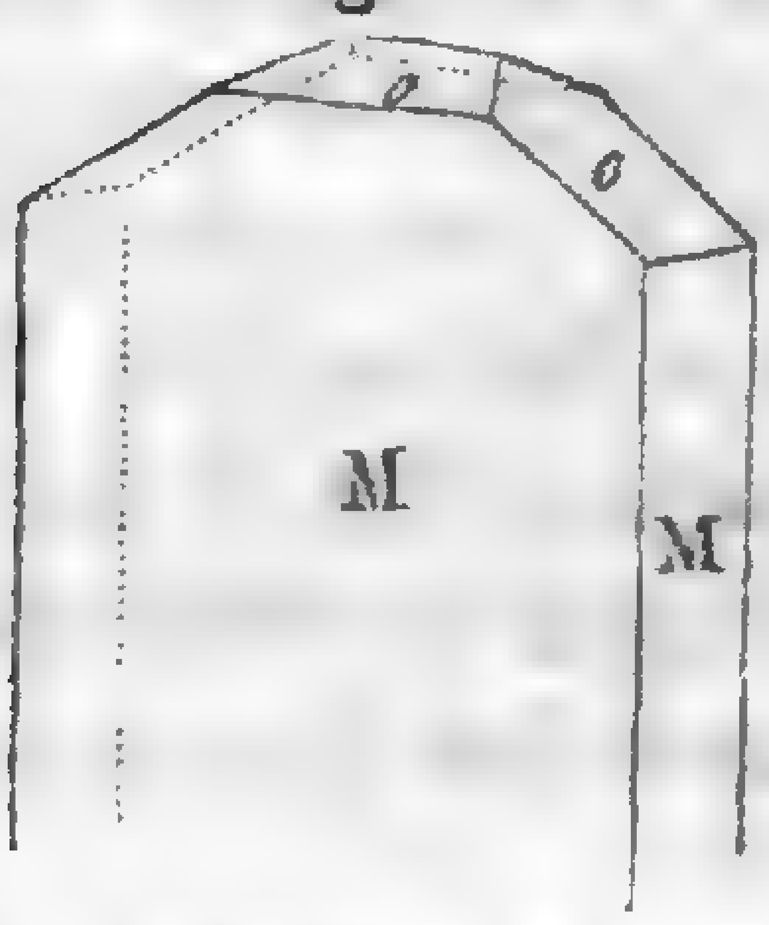
Harlem, N. Y.

Fig. 2.



Harlem, N. Y.

Fig. 3.



Harlem, N. Y.

M on M, 91° 20′
M on *o*, 116° 32′, *Haüy.*
116° 58′, *Phillips.*

Analcime.—This mineral occurs in small crystals of the leucite form at Bergen Hill, where it is associated with stellite, datholite, &c. It has long been credited to Paterson, N. J. I have also found crystals of the same form in the gneiss near Yonkers, in Westchester county, N. Y. The crystals are often quite opaque. I have not observed any other form than that usually figured.

Datholite.—This mineral has been found in abundance at Bergen Hill. It also occurs at Paterson, N. J., and at Piermont, Rockland county, N. Y. It may generally be distinguished by its high vitreous lustre. It is often associated with calcareous spar, having a highly modified form. The action of an acid and the blowpipe will of course readily decide the question.

In the flame of a lamp datholite swells up like borax and becomes opaque, in which state it may be crushed by the fingers. In hot nitric acid it is dissolved, leaving a siliceous jelly, which adheres to the flask. The crystals are usually transparent or translucent, but the forms are so highly modified as to baffle all my attempts to figure them.

Chabazite.—This is of rare occurrence at Bergen Hill, and the crystals, which are in all cases of the primary form, are very minute. In one of my specimens, the rhombohedrons are an eighth of an inch in diameter. There occur at this locality small obtuse rhombohedrons of calcareous spar, which might at first sight be mistaken for chabazite. But the lustre and cleavage, independently of the chemical characters, will sufficiently distinguish them.

The crystals are translucent or opaque, sometimes white, and at others of a brownish tint. The edges and angles are sometimes rounded, as if they had undergone partial fusion subsequently to their formation.

Prehnite.—It is abundant in various parts of New Jersey, especially at Paterson, Scotch Plains, and Bound Brook. About a year since, Dr. Houghton, the geologist of Michigan, and myself, discovered a vein of prehnite in the trap on Bound Brook, about eight miles northwest of New Brunswick, from which we obtained an abundance of the mineral; but the specimens do not possess much beauty. It has been found at Bergen Hill, but not abundantly. I have also identified it in the greenstone at Piermont, Rockland county, N. Y.

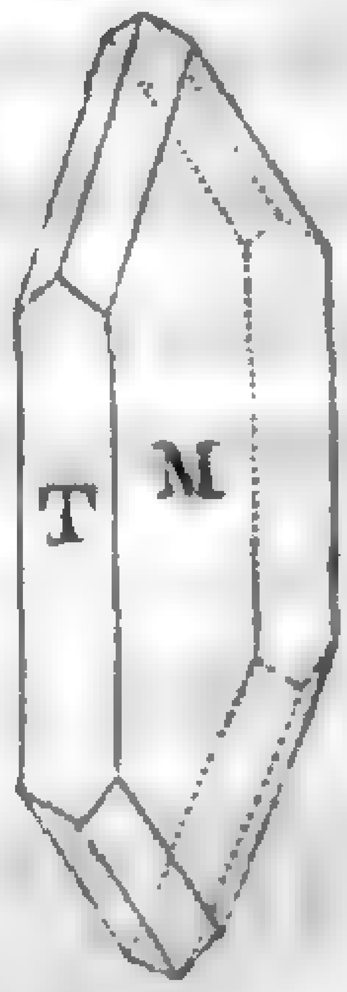
Most of the specimens from New Jersey which I have examined, are of a light green color, and are made up of globular or reniform masses, on the outer surface of which sections of crystals both of the primary and secondary forms may be observed. These crystals, however, are soon lost in radii which pass to the centre. This radiated arrangement may be observed in almost all the specimens.

Occasionally the masses of trap at Bound Brook have a thin coating of white or bluish white prehnite, in which the radiated arrangement has been exchanged for one that is indistinctly plumose. Sometimes, however, it is nothing more than a thin scale.

Prehnite froths before the blowpipe, and when the heat is continued, it melts into a compact colored globule. This globule is usually light green, but in a specimen from Piermont, it is nearly black, a fact which at first led me to doubt whether it was really prehnite. But I have no knowledge of any mineral with which it agrees so well as with this.

Stilbite.—This mineral occurs at Bergen Hill, Paterson, Bound Brook, and elsewhere in New Jersey. It is also found at Harlem, and in the counties of Westchester, Putnam, and Rockland, N. Y. The primary form has been met with in Putnam county, but the most common form in New Jersey, so far as my observation extends, is that represented by the accompanying figure.

Fig. 4.

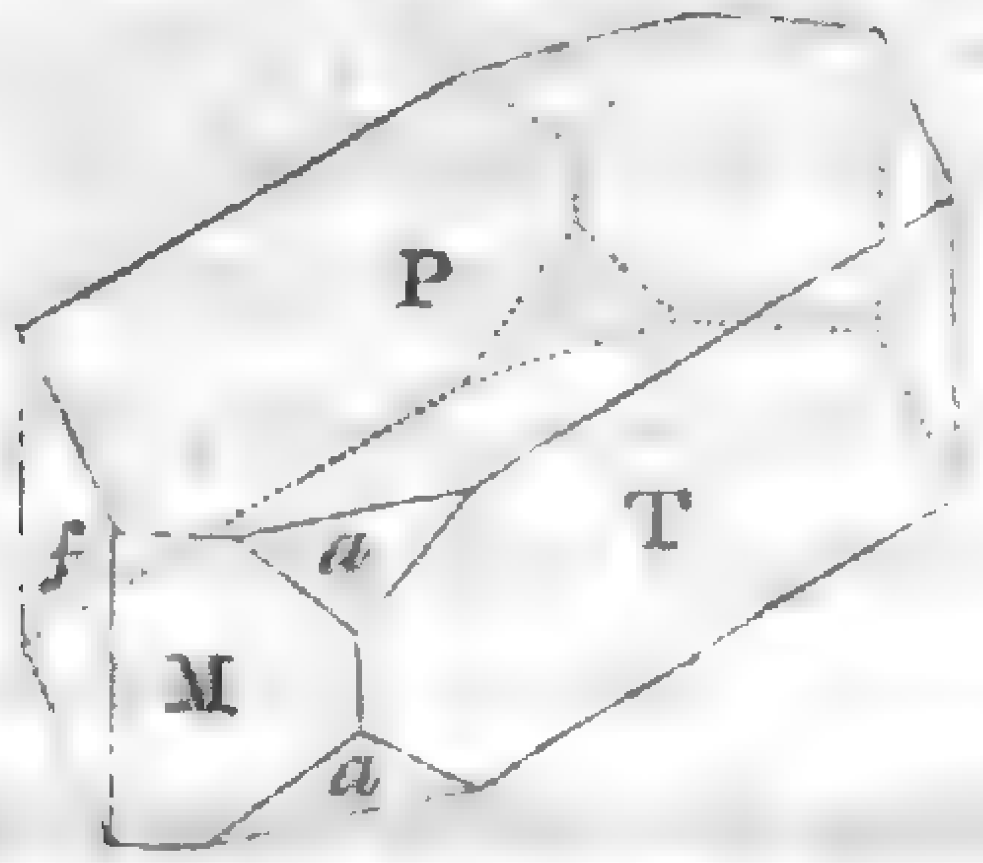


Bergen Hill, N. J.

The crystals are often aggregated in various ways, sometimes in the form of a sheaf. This is more common in those cases where they occur in cavities in the trap or greenstone, and it is then often difficult to determine the exact crystalline form. The mineral varies from transparent to opaque, and the color is white, yellowish, or red. The face M has a pearly lustre, but it is not so perfect as the face P in the next species.

Heulandite.—This often accompanies the preceding species, which it closely resembles in many cases. The splendent pearly lustre of the base of the prism will in general serve to distinguish it. This character is very marked in the small but quite perfect crystals found at Paterson and Bound Brook. At the latter locality I have noticed the primary form and the secondary, herewith figured.

Fig. 5.



Bound Brook, N. J.

M on T,	130° 00′
M on *a*,	146° 30′
T on *a*,	148° 00′
P on *a*,	111° 56′
M on *f*,	114° 20′

The crystals are often so much compressed as to resemble four or six-sided tables. Their color is usually white, and they are either translucent or opaque. The opaque crystals exhibit the pearly lustre in the highest perfection.

Besides the minerals already noticed, there have been found at Bergen Hill specimens of crystallized iron pyrites and galena, besides calcareous spar exhibiting various crystalline forms. Among these may be mentioned the *métastatique* and the *cuboide* of Haüy. The latter, as I have already observed, may easily be mistaken for the primary of chabazite. In other cases these crystals are modified so as to resemble some of the forms of datholite; and in a few instances they even have the high lustre of this mineral. I have indeed thought that they might be pseudomorphs. Several specimens of this kind, which I took for chabazite and datholite, were dissolved with effervescence in dilute muriatic acid. I will only add, that at Bound Brook and Paterson, agate balls occur in the trap. These are usually quite small and unimportant, but beautiful specimens, two or three inches in diameter, were several years since obtained from the latter locality.

Art. VIII.—*A short Notice of the Action of Potash upon Cholesterine;* by J. Lawrence Smith, M. D., of Charleston, S. C.

For some reasons we would be induced to place cholesterine among the fatty bodies, but from many of its characters, it would appear certainly not to belong to this class of bodies. The most important distinctions between these two bodies are—first, the want of action of a solution of potash upon cholesterine; and secondly, its high point of fusion, which is 298° Fah.*

Another difference which I am able to point out is, that cholesterine is heavier than water, whereas the fats are lighter. It will be found in works on chemistry, that cholesterine is lighter than water, and I attribute this to the fact, that the substance, as it crystallizes out of alcohol, was found to float on the surface of water; but this is owing to the air adhering to the crystals. To show that it is heavier, all that is necessary to be done, is to

* The melting point of most of the fats is below 140°.

throw a small piece of fused cholesterine into a vessel containing water, that must afterwards be made to boil, (this is done to drive away the air adhering to the surface of the body,) after which it will be found to sink, and remain at the bottom of the vessel even when the water is cold. I dwell thus much upon this, because I feel confident that there are other organic bodies that are said to be lighter than water, but which are actually heavier; for owing to the looseness of their structure, air insinuates itself between the molecules, and is afterwards held so firmly, that it is impossible to drive it away by the ordinary means. I now return to the first distinguishing character between cholesterine and the fats—the difference of the action of potash upon the two bodies.

Chevreul and others have shown, that if cholesterine be digested a great length of time in a boiling solution of potash, no change takes place; but here the cholesterine is not subjected to the action of the potash under the same circumstances as the fats; for in the case of the latter, the point of fusion being considerably below that of boiling water, the force of aggregation is in a great degree destroyed, and consequently does not oppose itself to the chemical action; whereas in the case of cholesterine, its point of fusion being much higher than that of boiling water, it remains solid, and therefore its force of aggregation opposes itself strongly to the action of potash, (supposing one to exist.) So then, the difference of the action of a solution of potash upon these substances, is not such a strong mark of distinction as it would at first sight appear to be, as it is impossible to subject them to this action under similar circumstances.

This fact is mentioned, not to show that cholesterine may be a species of fat—far from it; it is simply to attempt to exhibit, that there is no stronger reason for supposing that cholesterine is not a fat, because a boiling solution of an alkali does not act upon it, than there is for considering spermaceti a fat, because it is acted upon; as here the spermaceti is in a state of fusion, one that is favorable to this action, and the cholesterine solid, a state opposing this action.

In an article on spermaceti published in this Journal, Vol. XLIII, p. 301, I stated my reasons at large for not believing this body to be a fat, properly speaking, and at the same time explained how I supposed an alkali to react upon it; it was there ranked with athal and cholesterine. I then also stated, that although a boiling solution of an alkali might not react upon cholesterine, still I had no doubt that the alkali by itself, aided with a high temperature, would react upon it in a manner similar to that which it did upon spermaceti. From the kindness of M. Pelouze, who furnished me with a small quantity of cholesterine, I have been able to examine into the truth of this supposition.

The first circumstance necessary to be observed in the examination of this reaction, is to have the cholesterine intimately in contact with the potash, and this is done by rubbing together equal parts of the two substances in a mortar. The mixture was placed in a watch-glass, and spread out so as to expose a large surface to the air, the watch-glass was placed on a support in a copper vessel, (the air contained in this vessel could be brought to any required temperature;) the experiment being thus disposed, the vessel was heated, and by the time that the air in the interior arrived at 248° Fah. a change began to take place in the mixture, and at 266° Fah. it was of a dark brown color.

This was now treated with cold ether, which dissolved the unaltered cholesterine, and also a matter of a resinous character, which when dissolved in alcohol, and the alcohol allowed to evaporate spontaneously, is deposited in the form of little round concretions entirely devoid of crystalline structure; it is not soluble in any of the alkalies. What remains after the treatment by ether, is of a brown color, and completely soluble in water. If hydrochloric acid be added to this solution, it is decomposed, and a yellowish substance arises to the surface. This substance is soluble in ether, alcohol, potash, soda, and ammonia, as well as their carbonates; it does not crystallize, its alcoholic solution reacts slightly acid upon litmus paper. In fact it is an acid of a resinous character; its combinations with alkalies have the character of soaps. Its silver salt is of a yellow color, but soon becomes black by exposure to the light.

From the small quantity of cholesterine that was at my disposition, I have not been able to obtain sufficient of the acid to examine its composition, but I have no doubt that it is a new one.

If the mixture when heated be not well exposed to the air, very little of this acid is formed, even if we elevate the temperature as high as 300° Fah.; but on the contrary, a considerable quantity of the resin before mentioned (soluble in ether) is formed; this though is capable of being converted into the acid, by the action of potash, a high temperature, and free access of air. Thus then it will be seen, that the action of potash, instead of being a means of showing that spermaceti and cholesterine are two substances of entirely different natures, affords strong evidence of their being similar bodies. Further, the action of potash upon spermaceti, is to produce athalic acid and athal, the former capable of forming soaps with the alkalies, and the latter of being converted into the former by an alkali and a high temperature.

The action upon cholesterine is to form an acid (which it is impossible for me as yet to name) and a basic resin; the former forms soaps with alkalies, and the latter by the action of potash at a high temperature is converted into the former.

This article is meant as an appendix to the one on spermaceti, and as an additional proof of the analogy that exists between that body and cholesterine, they being two of a class of bodies which will no doubt be found to be tolerably numerous, and which class I propose to call *pseudo gras.* Among them may be mentioned spermaceti, cholesterine, athal, ambreine, and probably stearérine and elaïérine, two fatty substances found in linseed oil, and which M. Chevreul brought to the notice of the Academy of Sciences not long since. This class of bodies would appear to be a link between the fats and resins.

Paris, July 8, 1842.

Art. IX.—*A Review of the Researches upon the Dilatation of Gases, by M. V. Regnault;* by J. Lawrence Smith, M. D.

On the dilatation of atmospheric air.—"There does not exist in physics a numerical element that has been submitted to so many experimental determinations, as the coefficient of the dilatation of air; and nevertheless we cannot say that this coefficient has been determined with sufficient precision.

"The beautiful experiments of Gay Lussac upon the dilatation of gases, seemed to have completely decided this question. He showed by a great number of experiments that the coefficient of dilatation between 0° and 100° Cent. was the same for all gases and vapors, when they are not extremely near their point of condensation, and that its value was 0.375. This coefficient was adopted by all philosophers, and employed in their calculations, until M. Rudberg, a few years since, showed its inexactitude. By a series of experiments made with care, Rudberg sought to show that Gay Lussac's coefficient was too high, and that its true value was comprised between 0.364 and 0.365."

Regnault gives at length the experiments of Rudberg, and describes the instrument made use of; he then goes on to give a description of his own experiments, which were performed in four different methods, the first of which was similar to Rudberg's.

By the first method, the dilatation of air between 0° and 100° Cent. is shown to be—

1 . .	1.36556	8 . .	1.36634
2 . .	1.36626	9 . .	1.36689
3 . .	1.36659	10 . .	1.36610
4 . .	1.36579	11 . .	1.36671
5 . .	1.36625	12 . .	1.36591
6 . .	1.36549	13 . .	1.36641
7 . .	1.36673	14 . .	1.36673

"The mean furnished by these fourteen experiments is 1.36623. The difference between the two extreme numbers is 0.00140, that is to say, a little more than $\frac{1}{1000}$ of the quantity that was measured. The numbers furnished by these fourteen experiments are all higher than the mean, 1.3646, that Rudberg obtained in his experiments, made by a similar process. I believe that this difference arises from the fact, that in the experiments of Rudberg aspiration of the external air took place; and it appears to me difficult to operate after his manner without this cause of error presenting itself, and it is evident that it must have escaped his attention, for he does not speak of it.

"The errors produced by this aspiration become more and more sensible, as the volume of air operated on is smaller. It was some time before I succeeded in avoiding this aspiration, and I am persuaded that it exercised a sensible influence upon my first experiments, making the numbers too small; and what confirms me in this opinion is, that from the moment that the aspiration became impossible, I never obtained a number lower than 1.3658."

In these experiments, it was necessary in one step of the process, to immerse a glass tube having a bulb at one extremity into mercury, but the mercury not coming completely in contact with the tube, there would be a small portion of air adhering to it, and as the mercury would rise in the tube, the air would enter with it, and this is what Regnault here calls the *phenomenon of aspiration;* this he avoided by surrounding that part of the tube that was immersed in the mercury with tinfoil, and also by pouring a quantity of concentrated sulphuric acid upon the surface of the mercury.

Second method: the results obtained by this method are—

1	1.36629	10	1.36695
2	1.36645	11	1.36633
3	1.36593	12	1.36708
4	1.36610	13	1.36650
5	1.36585	14	1.36615
6	1.36590	15	1.36594
7	1.36615	16	1.36660
8	1.36591	17	1.36666
9	1.36708	18	1.36614

The mean of these experiments is 1.36633.

Third method: the results by this method are—

1	1.36688	7	1.36649
2	1.36688	8	1.36672
3	1.36612	9	1.36714
4	1.36643	10	1.36714
5	1.36651	11	1.36730
6	1.36626	12	1.36747

The mean of these experiments is 1.36679.

Fourth method.

1	1.36592	4	1.36682
2	1.36710	5	1.36674
3	1.36662	6	1.36580

The mean of these is 1.3665.

The following are the results of the four different methods:

First series,	1.36623
Second series,	1.36633
Third series,	1.36679
Fourth series,	1.36650
Mean,	1.3665

"Thus I adopt for the coefficient of the dilatation of dry air, for every degree of the Centigrade scale between the two fixed points of the thermometer, 0.003665."*

To this part there is a note, which is as follows: "M. Babinet has remarked to me, that in adopting for the coefficient of the dilatation of the air 0.366666 . . . this coefficient is represented by the fraction $\frac{11}{30}$, which is very convenient to employ in calculation."

"The former coefficient admitted for the dilatation of air being found inexact, it is evident that we cannot regard as demonstrated, that it is the same for all gases, and new experiments are necessary to decide whether this law be rigorously true or only approximative.

"I have made experiments upon nitrogen, hydrogen, oxide of carbon, carbonic acid, sulphurous acid, cyanogen, protoxide of nitrogen, hydrochloric acid and ammonia."

The following are the results of his experiments compared with air, dilatation between 0° and 100° Cent.

For air, . . .	mean of	50	experiments,	0.36650
" nitrogen, . .	"	3	" .	0.36682
" hydrogen, . .	"	4	" .	0.36678
" oxide of carbon,	"	2	" .	0.36667
" carbonic acid, .	"	4	" .	0.36896
" cyanogen, .	"	2	" .	0.36821
" protoxide of nitrogen,	"	3	" .	0.36763
" hydrochloric acid,	"	2	" .	0.36812
" sulphurous acid,	"	3	" .	0.36696

Regnault has just published a second series of experiments, and the coefficients for some of the gases have been changed; the reason for it will be seen in what he says.

"In all the experiments that have been as yet described, the dilatation of the gases have been determined in an indirect manner. The increase of the elastic force of a certain volume of the gas under an elevation of temperature was measured, and from this the dilatation deduced, supposing Marriotte's law to be correct. But, one can object by saying that this law is not demonstrated to be absolutely exact even for air."

"To leave no doubt as regards this mechanical theory of gas, I have made a new series of experiments, by means of a process

* This would be for every degree of Fahrenheit's scale 0.002036.

that would enable me to measure directly the increase of the volume of the gas, (it being apparently under the same pressure at 0° and 100° Cent.) This method is evidently the only one that we can employ for gases that do not follow the law of Marriotte under slight differences of pressure."

The instrument used was founded on the principle of M. Pouillet, *pyromètre à air*, and the results afforded by this series of experiments are—

For hydrogen, ...	0.36613
" atmospheric air, ...	0.36706
" oxide of carbon, ...	0.36688
" carbonic acid, ...	0.37099
" protoxide of nitrogen, . . .	0.37195
" cyanogen, ...	0.38767
" sulphurous acid, ...	0.39028

"The atmospheric air has afforded a number a little higher than the mean of the preceding experiments; but the difference is insensible, and besides it may be caused by the air not following rigorously the law of Marriotte.

"The oxide of carbon has given the same number as in the former experiments.

"The coefficients for carbonic acid and protoxide of nitrogen are much higher than those determined by the former experiments, and this is no doubt owing to the fact that these gases do not follow the law of Marriotte, and that their volumes at 100° Cent., under the greater pressure to which it finds itself subjected at this temperature in the first experiments, are smaller than they ought to be according to this law."

As for the difference in the coefficients of cyanogen and sulphurous acid, Regnault has ascertained that it was owing to an error in the first experiments, the gases not being perfectly dry.

The dilatation of gases under different pressures, calculated after the change of the elastic force.—"Philosophers admit generally, that the dilatation of gases is constant between the same limit of temperature, whatever be the pressure to which they are subjected; consequently, that it is entirely independent of the original density of the gas. But it is difficult to cite the experiments upon which this law is founded. Many observers having obtained the same value for the coefficient of the dilatation of air under different barometrical pressures, have concluded that it remains the same under all pressures. But the variations of the barometer in the same place are too small to permit us to come to so general a conclusion; it serves to prove only that very small variations of pressure have no sensible effect in changing the coefficient of the dilatation of air."

The results of the experiments of Regnault upon air under different pressures are—

Pressure expressed in decimal fractions, the ordinary pressure being considered 1.00000.	Dilatation.
0.1444	0.36482
0.2294	0.36513
0.3501	0.36542
0.4930	0.36587
0.4937	0.36572
1.0000	0.36650
2.2084	0.36760
2.2270	0.36800
2.8213	0.36897
4.8100	0.37091

"The preceding experiments demonstrate that the law admitted by philosophers, that is to say, that the air dilates the same fraction of its volume whatsoever its density, is not exact. The dilatation of air between the same limits of temperature increases as the density of the gas becomes greater, or in other terms, as its molecules approach one another."

The same is shown to be true for carbonic acid.

Pressure expressed in decimal fractions, the ordinary pressure being considered 1.0000.	Dilatation.
1.0000	0.36856
1.1879	0.36943
2.2976	0.37523
4.7318	0.38598

Conclusions.—"My experiments do not confirm the two fundamental laws of the theory of gases, admitted up to the present time by all philosophers, viz.

"1. All gases dilate equally between the same limits of temperature.

"2. The dilatation of the same gas between the same limits of temperature is independent of its primitive density.

"Ought these laws now to be abolished? I do not think so. I believe that these laws, as well as all those that belong to the gases, such as the law of volumes, &c., are true to *a certain* *limit;* that is to say, that they approach so much the nearer to results of observation, as the gas is examined in a greater state of dilatation."

"The laws apply to a perfect gaseous state, to which the gases approach more or less, according to their chemical nature; according to the temperature at which we consider them, which can be more or less distant from each of those points where there is a change of condition; finally and above all, according as they are more or less compressed."

Art. X.—*Description of the Bones of a New Fossil Animal of the Order Edentata;* by R. Harlan, M. D., F. L. S., &c.—with three plates.*

Order Edentata.

Genus *Orycterotherium.*

O. Missouriense, *nob.*

There is now exhibiting at the Masonic Hall, in Philadelphia, one of the most extensive and remarkable collections of fossil bones of extinct species of mammals which have hitherto been brought to light in this country, a gratification for which our scientific community will acknowledge themselves indebted to the perseverance of the enterprising proprietor, Mr. Albert Koch of St. Louis, Missouri. This collection consists mainly, of the largest skeleton of an aged mastodon hitherto disinterred in America, nearly complete. The proprietor not possessing the advantage of anatomical knowledge, has committed some errors in the articulation of the bones, which, no doubt, his ulterior researches will enable him to rectify; among these errors may be noticed here, ten or more supernumerary vertebræ in the spinal column, some supernumerary ribs, and the first rib occupying the position of the clavicle, &c.

The upper portions of the skull had mouldered away, but this could be artificially replaced with accuracy—the collection including perfect skulls of the same species, and another specimen

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* This memoir was originally read before the American Philosophical Society of Philadelphia, October 15th, 1841, and was reported in favor of publication in their Transactions. Uncontrollable circumstances having delayed the publication of its memoirs, leave was obtained for the withdrawal of the present paper.

having already been described by Professor Horner, in the Transactions of the American Philosophical Society for 1840. This skull was discovered in Crawford County, state of Ohio, and is in a prime state of preservation: the society has recently purchased it for the cabinet.

Mr. Koch's collection contains upwards of three hundred teeth of the mastodon, with numerous jaws, from the size of those of a calf, up to those of the immense skeleton.* There are also the remains of the ox, deer, elk, megatherium, and portions of the skeleton of *three individuals* of the *new genus* now under examination, judging from the relative size of the bones, some of which are duplicates. Of these individual skeletons, the following portions have been recognized: the os humeri of the largest, approaching that of the megatherium in size; the same bone of an individual about the size of the humerus of the *Megalonyx laqueatus;* and a third, younger and smaller individual, as is shown by portions of the clavicle, radius, &c. We have then, more or less perfectly preserved, two os humeri, two tibiæ, two portions of the radius, two portions of the clavicle; portions of several ribs; the cubitus, or ulnar bone, nearly perfect; sixteen loose specimens of teeth, and eight others in their sockets; two fragments of lower jaw, two fragments of upper jaw; five ungueal phalanges; the greater portion of a sternum, consisting of four pieces naturally articulated, and two other separate pieces; the major portion of the pelvis, &c. Numerous vertebræ were disinterred at the same time and place, and in the original notice of these remains were confounded with the skeleton of the new genus, but which subsequent observation referred to the skeleton of some phytivorous quadruped.

Not any of these bones are actually petrified, but having been imbedded in clay, are generally neatly preserved, and are very light and friable; the specimens having been coated with glue, it has given to them a blackish-brown color, but such as have lost this coating remain of a yellowish-clay or ochraceous color on the surface, and of a dead white color within. On subjecting portions of these bones to the action of muriatic acid solution, they were found to contain not the least vestige of animal matter.

* Among the most interesting specimens are two mastodon skulls, one adult, the other young, both of which have the tusks *in situ*, and which closely resemble the same parts in the elephant.

Geological position.—We copy Mr. K.'s description of the location of these bones, which he exhumed with much labor and expense, about two years since. The bones were found by the proprietor near the shores of the "Pomme de terre" or "Big-bone River," a tributary of the Osage River, in Benton County, state of Missouri, lat. 40°, long. 18°.

"There is every reason to suppose that the 'Pomme de terre' at some former period was a large magnificent river, from one half to three fourths of a mile in width, and that its waters then washed the high rocky bluffs on either side, where the marks of the waves are still perfectly plain: they present a similar appearance to that of the Missouri and Mississippi. Since the deposit of these bones, the bed of the 'Pomme de terre' has received several different strata, which occur as follows:

"Up to the time of the destruction of these animals, the original stratum forming the bed of the river consisted of quicksand. On the surface of this, and partly mixed with it, the bones were found. The next stratum is a brown alluvial soil, three or four feet in thickness; this contained and enveloped the bones. This stratum was mixed with a great quantity of vegetable matter, generally in a fine state of preservation; and what is still more surprising, all these vegetable remains are tropical, or of very low southern latitudes. They consist of large quantities of cypress burs, wood, and bark; a great deal of tropical cane, and tropical swamp moss; several stumps of trees, resembling logwood. Even the greater part of the flower of the Strelitzia class, which when buried was not full blown, was discovered imbedded in this layer; also several stems of palmetto leaves, one in which the fibres were nearly perfect. This stratum also contained iron ore.

"Next came a stratum of blue clay three feet in thickness; then succeeded one of gravel, from nine to eighteen inches in thickness, so densely compressed as to resemble pudding-stone. Then occurred a layer of light blue clay from three to four feet thick. On this, was another stratum of gravel, of similar thickness and appearance to the one first mentioned. This was succeeded by a layer of yellowish clay, from two to three feet thick; over this a third layer of gravel, of the same appearance and thickness; and lastly, the present earth's surface or soil, consisting of brownish clay mingled with a few pebbles, and covered with large oaks, maples and elms, as nearly as could be ascertained, from eighty to one hundred years old. In the centre of the above named deposit was a large spring, which appeared to rise from the bowels of the earth, as it was never affected by the severest rain or the severest drought."

Description.—Teeth: these consist of sixteen loose specimens, together with eight others still remaining in their sockets, there being two fragments of the inferior maxilla, one containing three teeth, the other two, *in situ;* also two fragments of the upper maxilla, one with two, the other with one tooth, *in situ.*

In form these organs bear a general, and in structure a close resemblance to the teeth of the *Megalonyx laqueatus*, (vide Medico-Physical Researches, p. 319,) a new species of fossil animal from the valley of the Mississippi, which I described in 1831.* In most of the specimens now under consideration, the teeth are marked on one or both sides by longitudinal deep grooves, dividing the crown into two irregular shaped surfaces slightly depressed in the centre; vid. Pl. I, figs. 1 and 3. Exceptions are found to this form of the teeth in some of the molars, which are merely flattened cylinders, Pl. I, fig. 7; also in the anterior or tusk-like molars, which resemble elongated, gently curved cylinders, slightly compressed on the inner surface of the shaft, Pl. I, fig. 5. These teeth, like those of the Bradypus, the Megalonyx, &c. consist of concentric cylinders of bone, ivory, and of pars petrosa. They are very irregular in relative size and position in the jaws, their crowns being transverse, oblique, or longitudinal, as regards their position in the alveoles; vid. Pl. III, figs. 1, 2 and 3. The jaws may have contained from six to seven teeth in each side of each jaw, as one fragment of the inferior maxilla, a portion of the anterior part four and a half inches long, contains three teeth *in situ*, and which, from the thickness of the bone, must have been more than double this length; Pl. III, fig. 1. These teeth are above three and a half inches long, their crowns being one inch two tenths, by eight tenths. They are generally more or less curved and project from the base of the lower jaw upwards and forwards, this direction being reversed in those of the upper jaw. The ultimate molar

* Professor Owen has since proposed a new generic name for this fossil quadruped, the *Mylodon Harlani.* See some further observations on this animal in Vol. XLIII, p. 141, of this Journal.


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in the lower jaw, like the same in the *Megalonyx laqueatus*, is about double the width of the others, and resembles somewhat two molars, united in the middle by a compressed bony ridge; Pl. I, fig. 1. It is about equal in length to the others, (the lower extremity is fractured in the present specimen,) and two inches in its long diameter, the crown varying from four to eight tenths in lateral diameter. The anterior or tusk-like molar is more than four inches in length, (vid. Pl. I, fig. 5.) There are two other specimens of tusks, belonging to a younger individual, rather shorter, more slender, and almost cylindrical in form.

Maxillæ.—We have observed in this collection only the four fragments above noticed. A portion of inferior jaw is three inches deep, and two inches thick; Plate III, figs. 1 and 4—a section of the anterior end. A fragment of upper jaw is much thinner; the crowns of the molars here scarcely project above the rim of the sockets. The alveoles are distinct, and the teeth are separated from each other from one and a half tenths to four tenths; Plate III, fig. 3. Plate III, fig. 2, a portion of lower jaw, and figs. 5 and 6, sections of anterior ends of figs. 2 and 3.

Os humeri.—There are two humeral bones in the collection, that now described being a portion of the skeleton of much greater dimensions than the individuals from which the other bones were obtained; in fact, the animal of which this humerus formed a part, must have approached in size the megatherium, judging from the massive structure of the bone, and from its length, being little more than six inches shorter than that of the megatherium.

This humerus belonged to the right side; its general form is that of the megatherium, and is yet more massive, presenting stronger marks for the attachment of its powerful muscles; it is destitute of the large foramen near the internal condyle, which distinguishes this bone in the megalonyx, orycteropus, and most other genera of this order. In the proportional width or transverse diameter of the condyloid crests, it resembles the same bone in the megatherium, which is likewise destitute of the foramen above noticed; in the last mentioned animal, this transverse diameter is one half the length of the humerus, this part being always greatly developed in such animals as possess great muscular power in their fore-arms and hands. Thus, in the orycteropus this diameter is three fifths of the length of the humerus;

in the rhinoceros it is one third, in the elephant one fourth; in the Ruminantia, on the contrary, these crests are but slightly developed. The deltoid protuberances of this humerus are much more strongly developed than those of the same bone in the megatherium; the internal protuberance is continued down the anterior surface of the shaft in a longitudinal crest, which is most elevated at its lower extremity, or sixty inches above the elbow joint. On the outer side of this elevation is a still bolder protuberance, whose free border curves outwards and backwards, and which, being opposed inferiorly and posteriorly by the crest of the external condyle, forms a groove or channel sufficiently capacious to receive the arm of a man; and running in a curve half around the bone, from the posterior to the anterior surface of the humerus, affords a lodgment for a large and powerful flexor muscle of the fore-arm. A shallow trace of this groove is observable in the humerus of the *Megalonyx laqueatus*, and is more forcibly developed in the chlamyphorus, (vid. Harlan's Medico-Physical Researches, p. 47.) The shaft of the humerus is slightly concave posteriorly, and somewhat depressed in its antero-posterior diameter.

The radio-humeral articulating surface is peculiar, consisting of two distinct facets, an exterior hemispherical head, projecting one and a half inches below the internal articular face, which is slightly concave in its transverse and convex in its antero-posterior diameter, exacting a corresponding peculiarity in the articulating surface of the ulna, rendering the structure and arrangement of this joint somewhat *sui generis*. There is a broad depression just above the condyles posteriorly, to accommodate the motions of the olecranon. The following are the dimensions of this humerus:

Total length of the bone, ...	20	inches.
Greatest diameter of the shaft, ...	5	"
Antero-posterior diameter of shaft, . . .	$3\frac{1}{5}$	"
Circumference of the shaft, ...	14	"
Diameter of the superior head, . . .	$7\frac{1}{2}$	"
Transverse diameter of the condyloid crests, .	11	"
Long diameter of the head, ...	$7\frac{1}{2}$	"
Transverse diameter of the radio-humeral articulating surfaces, ...	6	"

Vide Plate II, figs. 1 and 2—vertical view of its head.

The other humerus of the smaller individual measures eighteen inches in length, and is not so perfect at the condyles.

Cubitus.—This is a short, thick, and strong bone, the marks for the attachments of the muscles being equally pronounced as those on the humerus; the present bone however belonged to the opposite or left side, and to another and smaller individual. The most remarkable peculiarity of this cubitus consists in the size and form of the superior articulating surface, and of the olecranon process; the latter is broad and long, projecting upwards and nearly in a line with the axis of the bone, the superior anterior angle looking inwards, unlike the same parts in the megatherium and megalonyx. The radio-humeral articulating surface of this bone consists also of two distinct facets or surfaces; the internal is much the highest and largest, is concave in its longest or antero-posterior diameter, being nearly four and a half inches long by two and a half inches wide. The posterior portion of this surface is continued downwards and inwards, to form the internal, smaller, and more concave facet; this again is continued at its anterior border into an irregular triangular disk, horizontal to the shaft, for the accommodation of the proximal head of the radius. These irregular surfaces are neatly adapted to the corresponding surfaces of the humerus, and, together with the olecranon process, occupy about two thirds of the whole bone, as will be perceived by the measurements below. The upper line or rim of the shaft of this bone descends from the strongly pronounced coronoid process, and ascends from the posterior superior end of the olecranon, rendering the ulna somewhat triangular in form, diminishing in size towards the carpal joint; the shaft is strongly compressed and deeply scalloped its whole exterior side, with two deep depressions on its interior surface for the lodgment of its powerful muscles. The carpal articulating surface of this bone is nearly circular and somewhat convex; the disk for articulation with the superior head of the radius, is on the inner surface of the ulna; the larger oblong disk for the lower head of the radius is on the anterior portion of the ulna, so that the hand would remain in the state of pronation, still enjoying some liberty of motion. Vide Plate I, figs. 9 and 10—a view of the proximal extremity.

Dimensions of the cubitus:—total length, 16 inches; distance from the coronoid process to the inferior extremity of the bone,

$9\frac{8}{10}$ inches; from the coronoid process to the base of the cubit, $6\frac{7}{10}$ inches; smallest transverse diameter of the cubitus, $3\frac{7}{10}$ inches, and reduced in parts by muscular pressure from one and a half inches to half an inch in thickness. The carpal articulating surface is broken.

Radius.—There exists in this collection two specimens of this bone of different sizes, both with the distal extremity broken off and lost; the smaller of the two, which apparently was attached to the above described cubitus, is about 7 inches long and $2\frac{1}{2}$ inches thick, compressed in its antero-posterior direction, and strongly marked by grooves for muscular attachments; its proximal extremity is terminated by a glenoid cavity, which moves on the external condyle of the humerus, measuring $1\frac{1}{2}$ inches by $2\frac{1}{2}$ inches, and is in continuance with a protuberance on the inner border, with a convex surface, articulating with the disk on the outer and superior portion of the ulna. The larger radius is nearly three inches thick, and eight inches long from the fractured end. The inferior articulating surface being lost in both specimens, we can offer no views of its form.

Carpus.—Five bones of the wrist have been preserved; two only of the upper row have been brought to articulate with each other. In general character, these carpal bones resemble those of the megatherium, in which, as in the present species, both bones of the fore-arm are articulated with the bones of the carpus.

Claws.—Four of the ultimate or ungueal phalanges, forming a continuous series, have been preserved; these are too small to have belonged to the large individual, their size being better adapted to the smaller one. These are much smaller than those of the megalonyx, and in general form approach nearest to the ungueal phalanges of the orycteropus. The upper border of the largest is convex, but less so than a similar bone in the megalonyx or megatherium, and rather more curved than that of the orycteropus; but the two next in size, like those of the last named animal, have the upper borders nearly straight, but in the fourth one, on the contrary, this border is in both rather concave. They all possess the bony tubercle, together with the foramina on its posterior inferior surface, like those of the megatherium and megalonyx; but they differ from these last named animals, in the absence of the bony sheath surrounding the base of the claws. The articulating surface in each is divided into two facets by a vertical central ridge, continued above into a projection of the superior posterior border of the phalange, as in the megalonyx and orycteropus. Vide Plate I, figs. 11, 12, 13, 14.

The dimensions of the claws are as follows:

Length of the longest, measured along its base, (the superior posterior part being destroyed,) . . .	$4\frac{1}{2}$ inches.
Next in size, similar measurement, . . .	$3\frac{3}{10}$ "
The same along its superior border, . .	$3\frac{4}{5}$ "
Third, " " " . . .	3 "
Fourth, " " " . . .	$2\frac{1}{2}$ "

Varying in depth from one to two inches; in breadth from one inch to half an inch.

Tibia.—Two nearly perfect have been preserved, one belonging to the middle sized, the other to the smallest individual, and the latter to the left leg; we shall describe the latter, more particularly as it articulates aptly with the astragalus, the only bone of the tarsus obtained. The tibia is a short thick bone, depressed in its shaft, and thinnest on its exterior border, and much enlarged at each end; the superior articulating surface presents a simple, shallow, ovoid disk, or glenoid cavity, oblong transversely. From all appearances, the fibula of this animal must have been small and insignificant, there being no mark for its attachment observable on the upper portion of either specimen; there exists only a doubtful depression on the internal malleolus of the smaller one; it could, however, have had no connection with the tarsus. The articulation of the knee is as simple in the present case as it is most complex in the megalonyx; but a plenary compensation is found in the lower ankle joint of this animal. At the anterior and inferior extremity of the tibia, between the malleolus internus and externus, nearer to the internal, just where the same part in the megalonyx and megatherium projects furthest downwards, there commences a socket $2\frac{3}{5}$ inches long, $2\frac{2}{5}$ inches broad, and $1\frac{1}{5}$ inches deep, which receives a corresponding globular or ovoid projection of the anterior superior portion of the astragalus. This cavity descends downwards and backwards to two thirds the thickness of the tibia at this part, when it unites with a transverse semilunar articulating surface, which is slightly concave near the external malleolus only; this joins a corresponding articulating surface on the astragalus, posterior to the globular or ovoid projection above described, on the outer side of the base of which exists a foramen for the passage of vessels and nerves, large enough to admit the end of the little finger, into which, when at rest in an upright position, enters an angular projection formed at the outer and lower ridge of the socket. This extraordinary construction of the ankle joint is altogether proper to the animal under consideration, and which, whilst it affords the strongest possible movable articulation, would necessarily impede the hinge-like motion, in most animals very free at this part; and is confined principally to a semicircular and horizontal motion, as in turning the toes outwards and inwards. Plate I, fig. 15. The following are the dimensions of the tibia:

	Smallest specimen.	Largest specimen.
Total length, ...	10 inches,	$10\frac{1}{2}$ inches.
Transverse diameter of the superior head,	$5\frac{3}{10}$ "	$6\frac{7}{10}$ "
" " inferior head,	5 "	$5\frac{1}{2}$ "
Circumference of middle of the shaft,	$9\frac{1}{2}$ "	$10\frac{7}{10}$ "

Astragalus.—This is a large, irregular shaped bone, extensively marked with articulating surfaces, five in number; two for the tibia above, one below for the os calcis, and two anteriorly for the other tarsal bones. The articulating surface for the os calcis admits of a rocking or semi-ginglimus motion, in some degree compensating for the absence of this motion in the ankle joint, in which respect it differs from its congeners. Vide Pl. I, fig. 16. The dimensions of the astragalus are—

Extreme breadth, ...	$5\frac{3}{10}$ inches.
Height from the base of the bone to the top of the ovoid protuberance, ...	$4\frac{1}{10}$ "
To the base of the bone, ...	$2\frac{6}{10}$ "

Clavicle.—There are two or three pieces of bone resembling portions of clavicle. The two pieces now under consideration appear to have belonged to the smaller individual, and are the sternal and scapular extremities, each about five inches in length, two inches wide, and one fourth of an inch thick; the bones are of a strong structure, having but a slight development of cancelli within. Vid. Plate I, figs. 17 and 18—a transverse section.

Ribs.—Of the several portions preserved, one only is nearly perfect; a considerable portion of this has been fractured, and lost, from the sternal extremity. This piece is nearly two feet long, two inches wide, and one inch thick; the distance from the centre of one spinal articulation to the other is four inches;

the ribs vary in this respect from two to four and a half inches, according to their relative positions on the spine. Their structure is well developed.

Sternum.—Very little if any portion of this bone has as yet been found, either of the megatherium or megalonyx. Of the present animal we have a sternal piece, consisting of four bones united, measuring collectively thirteen and a half inches. There is one piece wanting from the top or anterior part of the column; another, or others, at the lower or posterior end. The second or top piece of this specimen measures five inches wide, three in depth, and five inches and five tenths in length. A longitudinal groove or channel runs along the inner surface. The facets for articulation with the cartilages of the ribs, measure from two to two and a half inches. Plate II, figs. 3, 4 and 5—transverse sections.

Pelvis.—A portion of the posterior, upper and lateral parts of the ilium, with four sacral vertebræ, are preserved, all ossified together; the anterior vertebra only of this piece being united with the ilium, though the adjoining vertebra above had also been articulated with the bones of the pelvis. The sacral bones proper had all lost their inner faces by decay. This portion of the ilium belonged to the left side; the whole is of a firm and massive structure. Three and a half inches below the sacro-iliac juncture, is another facet for union with the ischium, three by two inches in size. Vid. Plate III; fig. 7, the external, and fig. 8, internal view; fig. 9, articulating surface of the sacrum.

The present type is made up of points uniting it with the Megatherium, Megalonyx, Orycteropus, and Chlamyphorus, together with developments *sui generis* sufficiently characteristic.

P. S. Since the above memoir was presented to the American Philosophical Society for publication, I have observed in the American Journal of Science and Arts, Vol. XLI, p. 136, a very interesting notice of a new locality of similar fossil bones, communicated by Dr. H. C. Perkins, of Newburyport, Mass. These remains were found by Mr. E. Young, in December, 1839, on the Walhamet or Multnomah River, a tributary of the Columbia or Oregon, in latitude 41° north, about twelve feet under the earth. A tooth and os humeri of the Orycterotherium Missouriense, were found in company with the remains of Bos and Elephas. The crown of the tooth as represented in the Oregon specimen differs from the same in the present species, but may have been injured by friction or other accident. The animal to which these Oregon specimens belonged is not named by Mr. Perkins, but he very properly remarks their resemblance to those of the Megatheroid tribe.*

Art. XI.—*An Account of some new and rare Plants of North Carolina;* by M. A. Curtis.

Hypericum Buckleii: low, suffruticose, diffusely branched from the base, the branches slightly angled; leaves cuneate-oblong or obovate, subsessile, glabrous, very obtuse, minutely punctate; flowers terminal, solitary, peduncled; bracts subulate, small, a little below the flower; sepals foliaceous, unequal, oval or obovate, obtuse, much smaller than the petals, 3-nerved at the base; stamens numerous, shorter than the petals; capsule conical, acuminate with the united styles, 3-celled.

Stem 8–12 inches high. Leaves 6–7 lines long, 3–4 broad, pale beneath. Plant resembles *Ascyrum Crux-Andreæ.*—Discovered in 1839 on Whiteside Mountain, Macon County, N. C. Found the past season (1842) on many other mountains of North Carolina, south of the French Broad River, and in Georgia by Mr. S. B. Buckley.

Thermopsis Caroliniana: simple, erect, glabrous; leaves long petioled; leaflets oval oblong, cuneate at base, obtuse above, glaucous and with a fine appressed pubescence underneath; stipules large, about equalling the petiole, semicordate, clasping; flowers in a long terminal raceme, short pedicelled, alternate; bracts ovate, longer than the pedicels, deciduous; calyx silky villous, the teeth short and subequal, the upper one truncate, the lower triangular acute; legume oblong linear, straight, erect, appressed to the rachis, densely villous, 10–12-seeded.

Stem 3–5 feet high, glabrous except in the raceme. Raceme 6–12 inches long; leaflets 2½–3 inches long, about 15 lines wide,

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* Dr. Perkins proposed to name his specimen, if it proved to be a new animal, *Orycterotherium Oregonensis.* We have casts of the os humeri from both Dr. Perkins and Dr. Harlan, and can discriminate no difference; they must have belonged to the same animal.—Eds.

nearly equal; stipules 1½–2 inches long; legume about 2 inches, corolla (yellow) 8–10 lines long. Flowers May–July.—Discovered in 1839 near Pigeon River, Haywood County, and on the Hiwassee, Cherokee County, N. C. Found in many other localities the past summer by Mr. Buckley.

Thermopsis fraxinifolia: slender, branching, geniculate, nearly glabrous; leaves petioled; leaflets oblong, cuneate, smooth above, glaucous and with slightly pubescent veins beneath; upper stipules lanceolate, much shorter than the petiole, the lower larger and ovate; raceme terminal, peduncled; flowers (yellow) mostly alternate, on slender pedicels which are longer than the calyx and the linear lanceolate persistent bracts; teeth of the calyx short, the upper one emarginate, the lower triangular; legumes linear, falcate, compressed, spreading, pubescent, shortly stipitate, about 10-seeded.—*Baptisia mollis*, Nutt., *Gen.*, and Torr. & Gray, *N. A. Flora, in part.* *B. fraxinifolia*, Nutt., *Mss.*, ex Torr. & Gray.

Stem 2–2½ feet high; leaflets 2–2½ inches long; petioles ¾ of an inch; legumes 3 inches, on pedicels more than ½ an inch long, the stipe shorter than the calyx; flower 8–9 lines long.—Table Mountain; also on mountains in Henderson and Macon Counties, N. C.: Mr. Buckley. This plant does not turn black in drying, but the flowers become whitish. It appears to be confined to the mountains. My own specimens are derived from the same locality with Nuttall's *Baptisia mollis*, and are doubtless the same species. The plant collected by Schweinitz, (vid. Torr. & Gray, *N. A. Flora*, I. 695,) is probably the *Baptisia mollis*, Michx., to which a portion of the description in the N. A. Flora is alone applicable.

Baptisia mollis, Michx.: diffusely branching, and the whole plant rather softly pubescent; leaves petioled; leaflets lance-oblong, cuneate; stipules foliaceous, lance-oblong or ovate, about equalling the petioles; racemes terminal, peduncled, rather crowded; flowers (yellow) alternate or geminate, on pedicels shorter than the calyx; bracts lanceolate, longer than the pedicels; upper segment of the calyx obtuse or emarginate, the others lance-subulate; legume oblong, turgid, shortly stipitate.

Stem 2–2½ feet high; leaflets 2–2½ inches long, petioles about ½–¾ of an inch; pedicels 2–3 lines; legumes about ¾ of an inch, few seeded; stipe shorter than the calyx. Flowers bright yellow, which, as well as the rest of the plant, retain their color in drying. Flowers April–May.—This plant appears to be confined to the middle districts of country, and is doubtless the plant of Michaux. It has probably not been seen since Michaux's time, unless perhaps by Schweinitz, until it was detected last year (1841) by Dr. C. L. Hunter on the ridge of land which divides the forks of the Catawba in Lincoln County, and is but a few miles from Mecklenburg, where Michaux discovered it. I have the past season found it near my residence, (Hillsborough, N. C.) growing on rocky wooded hills.

This and the two preceding species are well worthy of cultivation.

Helianthus Dowellianus: nearly smooth, branched above; leaves triplinerved, rather thick, slightly dentate, on margined petioles, and with a short scattered pubescence; the lower ones opposite, large, broadly ovate, subcordate, obtuse, the upper alternate, oblong-ovate; peduncles long and slender; scales of the involucre lanceolate, acuminate, slightly ciliate, shorter than the disk, appressed; rays 12–15.

Stem 4–5 feet high; corymbose paniculate at the summit, the branches axillary, long and slender, with a scattered appressed pubescence, but like the leaves smooth to the feet. Limb of the lower leaves 7–8 inches long, 5–6 wide, strongly triplinerved; the petioles 3–4 inches long, connate at base; rays about an inch long; young achenia hairy at the summit and angles; pappus of two subulate awns; chaff of the receptacle smooth, entire.

Around Franklin, Macon County, N. C. Flowers Aug.–Sept.

Vaccinium ursinum: young branches ferruginously villous; leaves oblong lanceolate or lance-obovate, cuneate or obtuse at base, short petioled, ciliate, mucronate, sub-acuminate, pubescent, particularly when young, covered with resinous dots beneath; racemes loose, naked; flowers campanulate, on slender pedicels; teeth of the corolla short; stamens included; filaments dilated, hairy; anthers unawned; calyx with short triangular acute teeth, and with the ovary and raceme resinously dotted; fruit black, 10-seeded.—Fruit matures July and August.

Shrub 2–3 feet high, with spreading rather straight branches; leaves 2–3 inches long, 1–1½ wide; racemes from below the leaves at the summit of the previous year's wood; fruit globose, red and acid before maturity, slightly astringent and insipid when ripe. Found on most or all the mountains south of Buncombe County, but no where north of it. The flowers I had not seen until supplied this season with fine specimens by Mr. Buckley. In fruit it bears some resemblance to *V. resinosum.* It is well known, where it grows, under the names of *Bear-berry*, *Buck-berry*, and *Bear-whortleberry.* Dr. Gray informs me that it is noticed by Michaux, in his Mss. journal, under the name of "*Vaccinium d'Ours*," but I cannot discover that it is described in his Flora. He probably found it in fruit, and included it under *V. resinosum.* But this cannot belong to the section with urceolate corollas.

Lindernia saxicola: stems numerous, erect, branching, glabrous; leaves opposite, ovate or oblong, obtuse, entire, the lowest attenuate at base, the upper clasping; peduncles strong, much longer than the leaves, refracted in fruit; corol four times the length of the calyx, the tube gradually enlarging from the base; segments of the calyx linear, subequal; capsule acute when young, terminated by the somewhat persistent style.

Stem 3–5 inches high; leaves rather distant; corol about 4 lines long, blue with lighter veins, the lower lip whitish with large blotches of blue.—Flowers in August.

On rocks in the Hiwassee River; also on rocks at Tolula Falls, Georgia.—Mr. Buckley.

Juncus cylindricus: stem compressed, leafy; leaves plane, smooth; panicle cymose, decompound; spikes cylindrical, many flowered, compact, mostly pedicellate; sepals 6, the exterior lanceolate, very acute, carinate, narrowly margined, the interior a little longer, oblong, obtuse, with a broad membranaceous margin; stamens 3, shorter than the obtusely trigonous capsule; anthers tawny.

This may prove a variety of *J. aristulatus,* Mich.; but I am inclined to consider it distinct. The cymes are small and but little expanding; stem 2–3 feet high; spikes 30–40 flowered; florets 3-angled by the strongly keeled outer sepals; capsule retuse, a little shorter than the sepals; style hardly perceptible; stigmas 3.—Lincolnton, N. C.

Muhlenbergia filipes: cespitose, glabrous; leaves very long, involute; panicle large, diffuse, capillary, not wholly exsert; pedicels long; calyx much smaller than the corolla, unequal, both valves long awned; corolla with the lower valve three-awned, the lateral awns short, the intermediate very long, the interior valve short awned.

Stems 2–2½ feet high, forming large tussocks; leaves overtopping the culm, subulately attenuate; pedicels 1–1½ inch long; awns of the calyx unequal, the longest about two thirds the length of the awn of the corolla, which is an inch or more long; the lateral awns of the corolla are merely the disengaged sides of the summit of the valve. Habitat, sandy soil of the sea islands of North Carolina; also East Florida.—Flowers last of September–October.

This may be a variety of *M. Polypogon*, Trin.; if so, there are probably no characters by which either may be separated from *M. capillaris*, Trin.

Carex Mitchelliana: spikes in threes, peduncled, somewhat distant, oblong, subnutant; terminal spike staminate at the base and summit; the lowest peduncle scarcely sheathed; capsule ovate, acute, glabrous; scales oblong, the lowest with a long cusp much exceeding the fruit, the upper about equalling it. Habitat, wet places, Chatham County.—Flowers in May.

Stem 18 inches high, slender, acutely triquetrous, smooth, except at the summit; leaves shorter than the culm, smooth, except toward the tip; terminal spike staminate more than half its length, and a few staminate flowers at the summit as in the other spikes; spikes about an inch long, not very crowded; scales of the fruit at the base of the spike with a subulate point about a third longer than the capsule, but diminishing in length toward the summit, where they are about equal to and sometimes shorter than the capsule.

This is a distigmatic species and belongs to the same group with *C. crinita.*—Discovered in 1835.

Carex glaucescens, β. androgyna: spikes 4–5, erect, large, cylindrical, terminal one androgynous, (staminate at base,) the rest fertile.

Stem rigid, 3–4 feet high; spikes thick, 2½–3 inches long. This is an autumnal variety, flowering in October, and much the handsomest species I am acquainted with. Habitat, wet ground, Wilmington, N. C.

This species appears to be quite polymorphous. In its common form it has but one staminate spike; but it sometimes has two or more, and is then *C. verrucosa*, Ell.

Hillsborough, N. C., Nov. 1, 1842.

ART. XII.—*Selections from the Scientific Correspondence of* CADWALLADER COLDEN *with Gronovius, Linnæus, Collinson, and other Naturalists;* arranged by ASA GRAY, M. D.

DR. COLDEN, one of the earliest and most distinguished cultivators of science in North America, maintained, as is well known, an active correspondence with many of the most eminent men both in Europe and this country, on medical, philosophical, and scientific subjects, devoting to these pursuits the intervals of his public duties, as surveyor-general and member of the council, and, at a later period, as lieutenant and acting governor of the province of New York. Some of his letters and other papers on mathematical and philosophical subjects, in which he was particularly skilled, have been given to the public.* But, so far as I am aware, no part of his botanical correspondence has yet been published, excepting his two letters to Linnæus, which are included in the agreeable volumes edited by Sir James E. Smith.† Supposing that other botanical papers of equal interest might be brought to light, I availed myself of the permission kindly accorded me by David C. Colden, Esq. of New York, to examine the voluminous correspondence of his celebrated ancestor, and to select some portions for publication. I trust that these contributions to the early history of science in this country, will not be deemed inappropriate to the pages of the American Journal of Science.

Although Dr. Colden had acquired the rudiments of botany, as taught in the University of Edinburgh at the beginning of the last century, yet he paid little attention to the subject for twenty or thirty years after his arrival in this country. But having casually obtained some of the earlier writings of Linnæus shortly after their appearance, he zealously engaged in the examination of the plants around him, according to the new system, and soon established a correspondence with Gronovius of Leyden, Peter

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* Vide *American Medical and Philosophical Register*, Vol. I, (1810,) which contains a spirited biographical memoir of Gov. Colden, from the pen of Dr. Francis; and *Sparks, Works of Benjamin Franklin*, Vol. VI, (1838,) *passim.*

† *A Selection of the Correspondence of Linnæus and other Naturalists, from the original manuscripts*, Vol. II, p. 451–458.

Collinson of London, and, a few years later, with the great reformer of natural history himself.

I have not been able to find any draft of Dr. Colden's first letter to Gronovius, (acknowledged in the subjoined epistle from that botanist,) in which he transmitted a detailed description of the plants growing around his country residence at Coldenham, (near Newburgh,) New York. This communication was afterwards sent by Gronovius to Linnæus, who, in the year 1749, published the first part of it (to the end of class *Polyandria*) in the Transactions of the Royal Society of Science at Upsal, under the title of *Plantæ Coldenghamiæ, in provincia Noveboracensi Americes sponte crescentes, quas ad methodum* Cl. Linnæi *sexualem, anno* 1742, *&c. observavit et descripsit* Conwallader Colden. This early attempt at the systematic arrangement of our plants is really an extraordinary performance, considering the circumstances under which it was prepared, and fully merits the praise which Linnæus and Gronovius bestowed upon it. The plants are described with great accuracy and scientific skill; their medicinal uses indicated, and several new genera are proposed, although not named. The *magnitude* of this treatise is, however, greatly and strangely overstated by Colden's successor, the late occupant of the gubernatorial chair of New York; for instead of *filling two quarto volumes*, as Governor Seward gravely states, in his elaborate *Introduction* to the Natural History of New York, it actually occupies about twenty pages!

To Dr. Colden's first letter, Gronovius replies (in English) as follows:

Gronovius to Dr. Colden.*

Leyden, August 6th, 1743.

Sir—The 29th of July I was favored with your kind letter of the 28th of March, 1743, which came to my hand (by the care of Doctor du' Bois,) by the Rev. Mr. Dorsius; who told me, that in a short time he was resolved to go back to Pennsylvania; wherefore I shall answer to your letter as much as the time will permit.

Mr. Clifford, one of the richest merchants at Amsterdam, has printed his *Hortus* at his own expense, and doth not sell any copy, but is very liberal in making a present of it; so that if you

* The orthography in these letters has been corrected and modernized, but the phraseology is strictly adhered to.

can collect some seeds for him, I do not doubt he will present you with a copy of it. Sometimes it is to be met with in a public auction, where it commonly goes for about twenty five guilders.

The second part of the *Flora Virg.* is printed, of which I take the liberty to send to you a copy, besides a copy of the first part. But as you make no mention in your letter of some other books of Linnæus, besides his *Characters*, I take the liberty to present you with some other books, which you will find of great use to you, viz. with a new edition of his *Fundamenta Botanica*, in which you will find a great thesary of learned observations; but this book must be read over and over, and then you will easily perceive the laws which must be observed in making the characters, particularly about the *partes fructificationis*, where the *numerus, figura, proportio, et situs*, always must be observed.

I am infinitely obliged to you for the plants and characters you are so kind as to communicate to me. At present I am very much taken up with the public affairs, and the short staying of the Rev. Mr. Dorsius is the cause that I cannot examine your characters. What [can] I hope to do when our 20,000 men for assistance of the queen of Hungary, are marching, being about these affairs not only the seven provinces, but the towns themselves divided in their opinions! However there is great hope to a general agreement.

However, in reading now and then for a moment your characters, I am (without any flattery) surprised how you, in such a short time, could have such ideas of Linnæus's way in making up the *notas characteristicas*. Linnæus hath promised to give out one time or another the *Philosophia Botanica*, which should only consist of an explication upon the *aphorismi* of his *Fundamenta;* so that this book should be as a standard. A part of it is printed in the *Critica Botanica*, wherein certainly you shall find exceeding good remarks; wherefore I take the liberty to present to you my own copy of that book, which I have more than fifty times read over and over. I do not doubt you shall find by reading nicely the *Fundamenta*, the preface to the *Charters*, and this *Critica Botanica*, you may easily make yourself master of Linnæus's way. For myself, I assure you, I cannot give to you better directions. But if it is that you have still some scruples, I shall always be ready to answer them as much as I can.

Dr. Linnæus is not only content with his botany, but he extends his industry to all parts of natural history, and has brought me over to it, particularly persuaded by the power of my family, and I myself, too, in the government of this town, and by that way now and then departs to the states of Holland and other colleges. I get by our men of war an immense collection of natural things, of which my chiefest delight is in the *Lapides* and *Testacea*, that is, the *conchæ et cochleæ.*

I have tried *juxta Linnæi Fundamenta Botanica*, to give names to all these things, and printed a catalogue of it in the year 1740; since which time my collection increased once larger. You shall infinitely oblige me if you meet some of these things, to communicate them to me; particularly the *cochleæ* and *conchæ* of your country. I don't doubt there is in your country a good variety of snails, (as well at land as to the rivers,) whose *cochleæ* shells are different in shape and colors.

Now, sir, once more, I shall acknowledge myself very much obliged to you for your letter and characters, assuring that I shall always be glad to see your letters, to which I shall always answer upon [the] spot. Direct only to Mr. Sadelhof, merchant at Amsterdam, or to Mr. John Papin, junior, merchant at Rotterdam, with a direction to Dr. John Frederic Gronovius, Senateur de la Ville de Leyden, in Holland.

I am, dear sir, wishing you all health and prosperity, your most obedient servant, JOH. FRED. GRONOVIUS.

Leyden, October 3d, 1743.

SIR—The before-mentioned Mr. Dorsius hath told me that he was resolved in short time to go over to Pennsylvania, and promised me that he should call upon me and spend a night; whereupon I wrote these preceding lines, and made a packet of things mentioned there. To my great sorrow I did not hear any thing of that gentleman. In the mean time, I resolved to fix daily some hours to consider your characters. Once for all you must know that I [am] one who never will flatter any one; but with father Plinius *agnoscere per quem profecerim.* Indeed, I must confess, I hath [obtained] a great deal [of] light in your characters, about some plants mentioned in the *Flora Virginica.*

I take the liberty to send you my remarks, which have been ready a month ago, when I hath made this packet ready, that at the approach of Dorsius I could give it immediately in his hand, but alas! the fellow did not come; so that I was a whole month disappointed, having been very sorroy that I going to bed saw the packet lying in my room, and the next morning coming in my room, I saw it still lying there; till it happened that Mr. Otto, a gentleman from .... advised me of his going to England, who promised me to give it there in good hands.

I remain, dear sir, your most obedient servant,

JOH. FRED. GRONOVIUS.

GRONOVIUS TO DR. COLDEN.

Leyden, April 3d, 1744.

DEAR SIR—A month ago, I hath the pleasure that my particular friend, Mr. Canwan from St. Christopher, going to London, did me the favor, to take a small packet with him, directed to you, in order to look there for an occasion to send it to you. In the same packet you will find an answer to your characters, the *Fundamenta Botanica* of Linnæus, and his *Critica*, with the second part of the *Flora Virginica*, and my *Index Supellectilis.*

Since I have found that your characters, of which you send me a specimen under number 19, is responding to the *Diervilla*, but I believe a quite different species. In the mean time I had an occasion to write to my good friend Linnæus, and to get an answer from him, of which I communicate to you some particulars.

"Literas 17 Sept. datas accepi: ex iis percepi placuisse summo arbitro novum creare in America Botanicum; si ille tam multa præstet per te ac Claytonus, erint plantæ Americanæ certiores quam Europææ; et nulla rerum vicissitudo plantas et hos botanicos unquam obliterabit.

"Videtur certe cl. Coldenus vir acuminatus et occulatissimus: hoc tamen video, quod si ipsas plantas non communicat nobiscum, minus utilis erit. Hoc vero si fecerit, erit systema sexuale, et plantarum caracteres, et differentiæ tales, quales tota Germania, ne quidem Europa unquam proculeabit. Jucundissimæ mihi fuerunt observationes ejus, quas mecum communicas. Ego ad singula respondes." * * * * [Here follow several notes and remarks upon Colden's characters.]

These are the remarks which Dr. Linnæus hath made upon some heads of your characters, about which I have wrote to him. To satisfy him more, I left a copy for him of your characters. At the next occasion, you shall see the *Oratio Linnæi de Tellu-*

ris habitabilis incrementis, and *Celsii oratio de mutationibus generationibusque in superficie corporam cœlestium contingunt.* In both of which you shall find curious observations.

I hath kept this letter with me with intention, as soon [as] the winter is gone, to send it to you. It is now the 26th of February, when our public trek-boats went the first time to Amsterdam in this winter, having had a mighty pleasant frost, only two days very severe, but no snow at all.

In the mean time, I discovered that my friend who hath the care of the before-mentioned packet, hath not done right with some other packets. I suspect the same accident may [have] happened to the packet directed to you, wherefore I send to you another copy of Linnæus's *Characters*, of the *Flora Virginica*, of the *Fundamenta Botanica*, and my *Index*, besides another copy of my remarks upon your characters. There was in the before-mentioned packet a letter to you, but having no copy of it, I hope you shall excuse me to write another. The *summa summarum* was, that I always shall be glad with your letters, and sincerely answer to them; the second part of my letter was that I am thinking and meditating for a natural system of the *cochleæ* and *conchæ;* so that I beg you will be so kind to send to me the *testæ* of the land and sea-snails which are common in your country; and if there are to be met some shells and oysters, you shall oblige me with a couple of each species.

If you have any thing for me, pray direct it to Messrs. Dan. and Bar. Van Zadelhoff, merchants at Amsterdam, by whose care you get this paper. I shall be glad to hear from you as soon as you have received these things. If I can be more of any use to you, pray command freely, your most obedient servant,

Joh. Fred. Gronovius.

P. S. As you have seen that Dr. Linnæus hath desired a copy of all your characters, I have sent them to him. I hope by my next letter to you to have more of his considerations upon them, which I shall faithfully communicate to you.

The following letters exhibit Dr. Colden's just appreciation of the wide difference in character and object between an artificial and a natural classification. His proposition to arrange plants in natural groups or orders, in accordance with the *ensemble* of their affinities, employing some strictly artificial scheme for the single purpose of arriving most readily at the name of an unknown plant, as well as his remarks upon the gradations from one class and genus to another which the natural system when discovered may be expected to present, will appear really surprising, when it is remembered that he could have read nothing upon the natural classification more modern than the *Fragmenta Methodi Naturalis*, appended by Linnæus to his *Classes Plantarum*, (1738.) These letters are copied from the original drafts preserved among the Colden papers.

Dr. Colden to Gronovius.

[No date.]

Dear Sir—Your favor of the 3d of April, which I did not receive until the 15th of November, has so far exceeded the fondest of my hopes, that you have thereby laid me under the strongest obligations. I was, and still am, so conscious of my want of knowledge in botany, that I with good reason apprehended it was not in my power to be of any use to you or Dr. Linnæus, both of you consummate in that science. I cannot cease to admire the unwearied diligence and surprising accuracy of Dr. Linnæus, in forming his characters of such a vast number of plants. But it is to you more immediately that we in America are indebted; and it was merely in gratitude for the benefit we in America have received from your labors, that I offered any little assistance that is in my power, and which you have now laid me under the strongest obligations to perform. I must therefore previously excuse an imputation of negligence, which I am afraid I shall hardly avoid, in not complying with all that you may justly expect from me. For, as I am in public employments, I am frequently, during the summer season, obliged to attend to them in the city, where I have neither leisure nor opportunity to examine plants. So it has happened to me these two last summers; and it was accidental that, in the summer before them, I had so much leisure to examine the plants growing near my house in the country, and to make the observations which I sent to you. I shall next summer endeavor to collect for you all the specimens which you desire; and when I meet with any other plants which I think deserve your observation, I shall send you specimens of them, together with my own observations. I thank you likewise for your present of Linnæus's *Characters*, his *Fun-*

damenta Botanica, his *Oratio de Telluris habitabilis incremento*, *Celsi Oratio*, and your *Flora Virginica*, *Index Supellectilis*; all which I have received safe. But those by Mr. Canwan I have heard nothing of.

When you write to Dr. Linnæus, pray offer my humble service to him, and assure him that I shall be very proud of receiving his commands, if in any thing I can serve him in this part of the world. Last summer I sent the characters of the *Actæa* and *Christophoriana baccifera* to my good friend Mr. Collinson of London, which I believe he was to communicate to Dr. Linnæus. It was from Mr. Collinson this spring that I learned with pleasure that you had received my letter; for I began to suspect that it was either lost, or not worth your notice.

Since you have given me so much encouragement to propose my doubts on the Linnæan system, I shall take the liberty to make them as they occur, without further ceremony, trusting to the indulgence which you have already so fully shown me; though what I now presume to make goes even to the general distribution of his system into *Hermaphroditi*, *Monœcia*, *Diœcia*, and *Polygamia*. For if there be species of plants which are evidently of the same genus, and yet according to this system must be referred to different classes, you must certainly allow it to be a fault in the system. What has given occasion to me for this doubt, is what I wrote to you before, that I had observed some particular plants of the *Clematis* that carried only male flowers, while others of the same species bore all hermaphrodite flowers. There is no doubt of these plants being of the same species, because they agreed in every thing in the leaf, in the stem, &c., so that not the least distinction could be observed, except that the flowers in one were all male, in the other all hermaphrodite; neither did I observe any other plant of the same genus that had female flowers. The next is what I likewise observed to you before of the *Sagittaria*, that the species which I observed was evidently distinguished into male and female plants. You may assure yourself it is not an *Alisma*, according to Linnæus's character of this genus, nor no new genus; but that in every thing it agrees with the character of the *Sagittaria*, excepting that the flowers are male and female in different plants of the same species. Now I shall mention a third plant, a specimen of which I sent you, though without the flowers, no. 198, and which you tell me is the *Myrica foliis oblongis alternatim sinuatis*, Fl. Virg. p. 192.* According to Linnæus's character of the Myrica, it is of the *Diœcia* class; I can assure you that this species is of the *Monœcia*, and carries male and female catkins on the same plant. * * * I cannot think that I have accidentally fallen upon all the exceptions of this kind that are to be found. * * * In vegetables it is necessary that the males be in greater number than the females, in order to make sure of their impregnation; and therefore I do not think it against nature to have in the same species one plant with all male flowers, and another with hermaphrodite flowers.

One reason for Dr. Linnæus's establishing so many classes, (I suppose,) is to avoid as much as possible any of them from being too much crowded. This, I think, may be done, by dividing, as Mr. Ray and others have done, plants into trees and herbs. This is a distinction that all mankind make, and therefore I cannot doubt of its being a natural distinction; and certainly an obvious natural distinction is to be preferred to one more obscure. As to my part, if two plants, one a tree and the other an herb, should happen to agree in every part of the fructification, yet I could not persuade myself to think them of the same kind; there is something so very different in the whole formation and constitution between a tree and an herb. I know it is objected that there is no certain criterion to distinguish them, that any criterion hitherto given by botanists will agree to some herbs: but this objection I do not think sufficient, for it may be only a proof of our want of knowledge in giving the proper criterion, not that the distinction is not real. If this objection should hold, it may go further, even to destroy all distinction between vegetables and animals; because I know no criterion to distinguish animals from vegetables but what leaves room to doubt to which of them some species belong: witness the polypus, which has been the subject of late observation. Indeed, my opinion is, that the natural gradation from the lowest class to the highest is by such small and imperceptible steps, that it is very difficult to distinguish even the next step, either upwards or downwards; though at some distance the distinction be very remarkable. *For this reason, any system in botany would give me a strong prejudice in its favor, where there appears such a*

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* The *Comptonia asplenifolia* of Gærtner.—A. G.

gradation from one class to another, and from one genus to another, through the several species, that the step from one to the other becomes almost imperceptible. Whenever this system shall be discovered, I shall conclude it to be the natural system.

Give me leave to make another objection to the Doctor's system; for it is not with any view to depreciate so worthy and great a performance, but that I wish to have it as perfect as possible; and I hope he will live to make it such, as I know no man so capable of doing it. It is this, that the distinction of the *Syngenesia* class, according to the male, female, hermaphrodite, and neutral flowers is so very nice, and in many cases requires such a clear sight, and is apt to run the observers into confusion; which, by comparing the first and second editions of his characters, the Doctor himself has not been able to avoid in some instances. Add to this what I have before observed of the Clematis, Sagittaria, and Myrica, and we may have room to suspect that it does not truly and naturally distinguish the genera, but that the same species are subject to variations with respect to these. But it is time for me to stop. *Ne sutor ultra crepidam.* However, I must again mention what I before hinted to you of the *Gynandria Diandria*, that I still think that one distinguishing part of this natural class, among other parts of their character is, that the stamina or antheræ are affixed to the nectarium in some shape or other. This I have observed in all the species that I have had an opportunity to examine; which are indeed so very few that I can rely no more upon it than to recommend it to your examination; for I could not, after reading Linnæus's description of this kind of flower, discover any other stamina than what I take to be such. Since I wrote my former [letter], I examined the *Cypripedium*; there in the hollow of the nectarinm, this down or fine hairs appear, and if I be not mistaken, the antheræ are affixed not on their summit or top, as usual, but to the sides of the filaments below their summits. Two stamina seem not sufficient to me to impregnate the great quantity of seed contained in the capsule. Nature every where else seems careful to make sure work, even by profusion. You, who have the advantage of botanical gardens, may soon be satisfied whether there be any real ground for my conjecture.

I have long wished to see Dr. Linnæus's *Philosophia Botanica*, that is, ever since I saw the name of it mentioned, and was a little acquainted with his works. But when you wish in vain, it is needless for me to add my wishes. The reasons the Doctor gives you for suppressing that book, in my opinion, should not prevail with a good man in depriving mankind of a general benefit. * * *

The benefit of oil in the cure of the bite of the Rattlesnake has been confirmed in several instances in this country, and even that hog's lard is effectual; and of consequence it is probable that all oily things are. It has long been observed in this country, that hogs were never hurt by the bite of the Rattlesnake, or by any viper; though all our other cattle at some time or other have been. This I attribute to the natural defence they have by their fat, through which the teeth of the viper cannot penetrate, without giving the remedy at the same time. You know how dangerous it is to trust to experiments, unless they be performed with all requisite precaution; but I can assure you that I would rather trust to oil or hog's lard than to the famous *Polygala*, or Rattlesnake-root, or to any other medicine that I have heard of; because the beneficial use of this comes better confirmed to me than that of the Rattlesnake-root or any other. We chiefly trust to the warm external application.

As you seem to be pleased with my communicating the use of any plants discovered in this country, I shall tell you what I learned of the use of the *Hamamelis* from a minister of the Church of England who officiates among the Mohawk Indians. He saw an almost total blindness, occasioned by a blow, cured by receiving the warm steam of a decoction of the bark of this plant through a funnel upon the place. This was done by direction of a Mohawk Indian, after other means had for a considerable time proved ineffectual. * * * Dr. Linnæus is right in observing that I had mistaken the gemma of this shrub for the involucrum, in the description I gave of it.*

I have not yet been able to see the fruit of no. 131, but I cannot doubt of its belonging to the class of the *Tetradynamia*, for

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* Respecting Colden's description of this plant, Linnæus writes to Gronovius: "Quod vocat *Involucrum* est *Gemma*. * * Nectaria egregie describit, quæ videre nequivisti: fructus a te missus docet esse capsulam duram, nec nucem." Nevertheless, Colden's detailed character of the genus (*Plantæ Coldenghamiæ*, no. 18.) is not only perfectly correct, but is adopted by Linnæus in his *Genera Plantarum, &c.*, where the character commences with "*Involucrum triphyllum, triflorum*," etc.—A. G.

besides its agreeing in all its parts of the flower with the characters of that class, it agrees likewise in taste, having nearly the same with that of the *Nasturtium*. The Mohawk Indians told me that when they were quite faint with travel and fasting, if they can come at the roots of this plant to eat, they are refreshed, and their spirits restored wonderfully. Add to the character I gave what follows: *Radix longa, teres, repens, prominentiis plurimis angulosis.**

What I wrote to you of the species of the *Zea semine nudo*, I believe is entirely a mistake; for having some of these seeds, the plants which came from them produced seeds covered with a hard skin, as the other sorts are. I suspect some artifice was used to deceive me. Whatever Tournefort may say, I cannot doubt of these being distinct species of the Maize, notwithstanding that they cannot be distinguished either by the leaf or flower. But we that are well acquainted with the seed can distinguish the species, though it be very difficult to convey that distinction by words only to others. Sow the several species in the same soil and at the same time, they will come to ripeness at very different seasons in the year; and this property they never change, unless when sown together so that they bastardize.

This brings to my mind a thought which I have entertained, viz. that we have in America very few if any species of plants or animals entirely the same with those in Europe, except such as have been brought from thence: although some species are so nearly alike that it is difficult to describe the difference by words; though it be manifest to a curious observer.

If I, who understand so little of botany, were permitted to advise, I should propose the plants to be collected into their natural order or classes without regard to any system; after which I would make a system by which the same plants should be disposed according to the rules of that system, with the view only to assist learners or the ignorant to discover the proper name or place of each plant. And in this I would have no regard to the natural system, but even divide and separate the species of the same genus into different classes, if the rules of my system required it; for I would have this system to be looked on as nothing else but as an index to discover the plant one desires to find

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* The plant is doubtless the *Dentaria diphylla*.—A. G.

in its proper place; and therefore I should prefer a system that serves best to this purpose, though it should no way agree with the natural system. By this means many disputes would be avoided among botanists, and the system would be more beneficial to learners: for thereby they would have a double method of discovering any unknown plant; first from its natural conformity to some other known plant, and secondly, from some remarkable part of its character by which they are in the artificial system led to it.* I cannot forbear to wish that you would try this method in a new edition of your American plants, for my sake, and other unskillful philo-botanists in America.

I shall be obliged to you, if you will please to inform me of any new valuable works in medicine published with you. I have not the good fortune to have seen any thing in the materia medica that entirely pleases me.

You cannot expect much new in literature from this part of the world. I send with this a curious and new invention for warming a room with a small fire more effectually than can be done by a large fire in the common method, and is free of the inconveniencies which attend the Dutch and German stoves; because by this contrivance there is a continual supply of fresh warm air.† It may be particularly useful to you and Dr. Linnæus, by preserving your health while it keeps you warm at your studies. It is the invention of Mr. Benjamin Franklin, of Philadelphia, the printer of it, a very ingenious man. Experience confirms the benefit of it. * * *

I design to give our friend, Mr. Collinson of London, the trouble of conveying this to you; because we have not any ship at this time designed from this to Holland. He does me commonly the pleasure of writing to me twice in the year, at the seasons our ships commonly leave London, viz. in the end of February and beginning of September. When any ships from Amsterdam either go to New York or to Philadelphia, if your friends at Amsterdam please to direct your letters for me by the New York ships, to the care of Mr. Richard Nicholls, Postmaster in New

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* This proposal to employ a natural system of classification, with an artificial anaylsis to facilitate its application, appears to correspond entirely with the methods now in use.—A. G.

† The pamphlet here referred to (printed in 1774) is reproduced in *Sparks, Works of Franklin*, Vol. 6.—A. G.

York, or by the Philadelphia ships to the care of Mr. Benjamin Franklin, Postmaster in Philadelphia, they will come safe to my hands.

Dr. Colden to Gronovius.

Coldengham, in New York, Oct. 29th, 1745.

Dear Sir—I answered yours of the 3d of April, 1744, near twelve months gone, directed to the care of our common friend Mr. Collinson of London; which he tells me he has carefully transmitted to you. This is destined to be sent likewise to his care, since he is pleased with having this trouble put upon him. I am so little acquainted with the merchants in this place who trade directly to Holland, that the ships are commonly gone before I hear any thing of them. Besides, most of these merchants and masters of vessels are very careless of any thing of which they have no prospect of any profit.

With this, I send you the characters of some more plants, which I observed this year, and some corrections or additions to what I before observed, some dried specimens, and some seeds. I have presumed, with that freedom which is I think allowed in all philosophical inquiries, to mention some further difficulties which arise to me in the Linnæan system; and though you may perhaps easily solve them by showing my ignorance in botany as a science, yet, as probably the same difficulties may occur to others, it may be of some use, by giving you an opportunity of clearing up this matter, to others less versant in that science. * * *

My opinion that we have no species of plants in America precisely the same with those of Europe, requires much more knowledge of the plants than I can pretend to in support of it. But at the same time it will be difficult to demonstrate that it is false; for certainly some species differ, in which it is difficult to show the difference in words. For a carpenter can know a chip of one kind of timber from another, and can distinguish a piece of American oak from the English. We, who are used to the woods, can distinguish the trees and their several species by the bark alone; and yet I believe the most able botanist would be puzzled to describe either the grain of the several kinds of timber, or the differences of the superficies of the bark, so as to enable a stranger to distinguish them without further assistance. As to animals, I have never seen any precisely the same.

I must still insist upon it that there are several species of maize, though there be no difference in the shape and proportion of the several parts; but there is a great difference in the bulk and stature of the several species, and of their time of coming to maturity, though planted in the same soil and at the same time. Neither can I conceive that there can be any variations of the same genus where there are not different species: because I think that the variations arise from the farina of one species impregnating another species. * * * * We are well assured that all the Indian nations as far north as New England, planted maize when the English and Dutch first discovered them; and it is for this reason that it has obtained the name of Indian corn among the English. The Indians towards the sea on the New England coast, planted a small yellow sort, early ripe; those in the northern parts of New York, a small white sort, likewise; the Indians in the southern parts of New York, in Jersey and Pennsylvania, a larger sort, of variety of colors, and ripe a month later than the former sorts; and in Virginia and South Carolina they planted maize which rises to a great height, but is late in coming to maturity. In the several parts of the country where only one species is planted, no variations are observed. Now, sir, I think it not probable that all these far distant nations should so soon have received maize from the Spaniards, and to have adapted the several species of it to their several climates; and from the Spaniards only could they have obtained it, if it was not a native of their own country. But I even doubt if maize was common in Spain before the discovery in America. Besides, we observe that the Indians very slowly receive any of our customs; notwithstanding that the English and Dutch have lived so long with them, no Indian nation has hitherto sowed wheat or any other of our grains. For these reasons, I conclude that maize is a native of America, and that we have different species of it from those of Asia. My negroes tell me that they have kinds of maize in Africa very different from any in this country; and I am lately told that the Turkey maize is a different species from any we have.

I have, in considering Dr. Linnæus's characters of plants, endeavored to form some criterion for myself, whereby to know, when any new plant offers itself, whether it be of a distinct genus of its own, or a species of some other genus already known;

and to judge which of the botanists have pursued the method most conforming to the natural in their system; for I find that they all sometimes differ widely in this respect. For example, Linnæus ranks under the genus of *Convallaria* and under *Lonicera*, plants which all other botanists referred to several different genera: indeed, I think the parts of fructification in many of them may be sufficiently distinguished. On the other hand, it is very difficult for me to distinguish the genera of many of the class of *Hexandria* which he makes distinct. Since there is no manner of concord as to this grand point in botany among the ablest botanists, I may be allowed to suspect that no sufficient criterion has yet been discovered.*

I must therefore conclude, that however unsystematical and artless Mr. Ray's taking in the *tota facies* of plants into his distinction of the genera may be thought, we cannot entirely throw it aside, till some better criterion be found than has hitherto been given by botanists. For, so far as I can judge from the characters of plants given by others, and what little observation I have made, the parts of fructification alone, that is, the outward shape and form, number of cells, &c. is not sufficient in every case to distinguish the genera. Neither are the general shape and form of the leaves, stalks, and roots, or their proportion to each other, sufficient to distinguish the species, but that sometimes some other circumstances are to be considered, as particularly that of the time in which they acquire maturity.

But notwithstanding these objections I make to Dr. Linnæus's system, they no way lessen the esteem and value I have for his works; for I am more obliged to him than to any botanist I have seen. It is wonderful with what exactness he has observed such a vast variety of plants; and it is as wonderful to me that he has made so few mistakes in this great performance. What objections I make to his system will almost equally affect all systems. We have not as yet sufficient knowledge to adapt our systems

* Dr. Colden then proposes the faculty of hybridization as a test of this point, and considers those species which hybridize as belonging to the same genus, and those that cannot be made to hybridize to belong to different genera, and proceeds to defend this view by various illustrations drawn both from the vegetable and animal kingdoms. In his subsequent letter to Linnæus, (published in full by Smith in his *Correspondence, l. c.* Vol. II, p. 452,) he considers the same subject in a similar manner. It is hardly necessary to state, that he seems to confound proper varieties with hybrids.—A. G.

to nature; and therefore every systematic writer in every science would have forced nature to comply with his system, where he found that he could not bring his system to nature. From what I have now wrote, *you will see the reason I have to prefer the method of reducing the plants into certain orders (Ordines) rather than to a general system:* because I believe that by this method we will not so easily fall into mistakes of dividing or confounding what should be joined or separated, as we are tempted to do for the sake of a favorite system.

Dr. Colden to Gronovius.

Province of New York, Coldengham, May 30th, 1746.

Sir—The enclosed sheets are a copy of what I sent to you in the beginning of last winter, directed to the care of Mr. Collinson: but as we have heard that the ship was taken betwixt Portsmouth and the Dains, and carried into Dieppe, these papers must be lost. As such misfortune was to be expected, there was an outside direction on the packet in French, desiring the captors in such case to send them to the gentlemen of the Royal Garden at Paris; though I then thought, and still think, that such creatures as privateers commonly are, will very little mind any thing of the kind. But I mention this to you, in case the privateer be a man of some taste for learning, you may take some opportunity of inquiring after them. I sent along with the papers the specimens which you desired, and some others, together with the seeds of several plants; the loss of them I cannot at present repair. Pray God these wars may soon cease, for they are very destructive to learning. [He next announces the reception of a letter from Gronovius, dated July 9th, 1745, as well as the missing packet, despatched in 1743, and mentioned in Gronovius's letter of that year. This third letter of Gronovius, unfortunately, is not to be found among the Colden papers. At the close of this short epistle, Dr. Colden alludes as follows to the philosophical speculations which about this time occupied much of his attention.]

I design to order three copies of a small piece to be put up in this parcel, which I intend to submit to the examination of the learned, the printing of which I hope will be finished before this goes. It is on a subject which has puzzled philosophers in all ages; the solution of which I fancy that I have hit upon, and that it may be of use in the improvement of knowledge in every part of physics. I know not whether your taste be in this [department ?] of learning; but whether or not, I must beg the favor of you to desire some of your mathematicians, those chiefly versant in the Newtonian and Leibnitzian systems to peruse it, (of which no doubt you have some of distinguished character in your university,) and that you will favor me with your own and their opinion of it, as soon as your conveniency permits. I earnestly beg you to do it without compliment, and with the sincerity and freedom of a friend and philosopher, as you see I endeavor to write to you.

This country is now engaged in a most barbarous war with Indians, popish converts, set on by accursed priests to murder innocent people in their beds, or at their daily labor. Good God, what a religion must that be that incites men to such cruelties! And yet, from what we learn from the public news, your country seems not sufficiently apprehensive of being again subjects of such a bloody and cruel tyranny.

The *piece* referred to in the foregoing letter was doubless his "*Explication of the first causes of action in matter, and of the cause of Gravitation;*" which was first privately printed in New York, and afterwards (in 1746) reprinted in London without the author's knowledge. An enlarged edition, with the author's corrections, was published in London in 1751, with the title, "*The principles of action in matter, the Gravitation of bodies, and the motion of the planets explained from those principles.*" An extract from the preface of this treatise, exhibiting an outline of Mr. Colden's views, to which he attached high importance, is given in *Sparks, Works of Franklin*, Vol. 6, p. 96.

Dr. Colden to Gronovius.

New York, Oct. 1st, 1755.

To Dr. John Frederic Gronovius, Senateur de la ville de Leiden.

It is so long since I had the favor of a line from you, that I have frequently lamented the loss I sustain by being deprived of that correspondence with which you once honored me. Soon after my last to you, my time was so entirely taken up in the public affairs while the last war continued with France, that I could in no shape continue my botanical amusements. My advanced age, now in the sixty-eighth year of my life, made me think it high time to retire from business, and to indulge the remainder of life in more agreeable pursuits, which require less action than those I formerly engaged in, of which I am become incapable. I am now entirely wrapped up in philosophical amusements, of which perhaps you may see some fruit, if what I have done receive the approbation of those gentlemen to whose judgment it is submitted. * * * But you will perceive by what is inclosed, that botany is not entirely out of my thoughts.

I thought that botany is an amusement which may be made agreeable to the ladies, who are often at a loss to fill up their time. Their natural curiosity, and the pleasure they take in the beauty and variety of dress, seems to fit them for it. The chief reason that few or none of them have hitherto applied themselves to this study, I believe, is because all the books of any value are wrote in Latin, and so filled with technical words, that the obtaining the necessary previous knowledge is so tiresome and disagreeable, that they are discouraged at the first setting out, and give it over before they can receive any pleasure in the pursuit.

I have a daughter who has an inclination to reading, and a curiosity for natural philosophy or natural history, and a sufficient capacity for attaining a competent knowledge. I took the pains to explain Linnæus's system, and to put it in an English form for her use, by freeing it from the technical terms, which was easily done by using two or three words in place of one. She is now grown very fond of the study, and has made such a progress in it as I believe would please you, if you saw her performance. Though perhaps she could not have been persuaded to learn the terms at first, she now understands in some degree Linnæus's characters, notwithstanding that she does not understand Latin. She has already a pretty large volume in writing, of the description of plants. She was shown a method of taking the impression of the leaves on paper with printer's ink, by a simple kind of rolling press, which is of use in distinguishing the species. No description in words alone can give so clear an idea, as when assisted with a picture. She has the impression of three hundred plants in the manner you'll see by the samples. That you may have some conception of her performance and her manner of describing, I propose to inclose some samples in her own writing, some of which I think are new genus's. One is the *Panax foliis ternis ternatis,* in the *Flora Virg.* * * Two more I have not found described any where; and in the others you'll find some things particular, which I think are not taken notice of by any author I have seen. If you think, sir, that she can be of any use to you, she will be extremely pleased at being employed by you, either in sending descriptions, or any seeds you shall desire, or dried specimens of any particular plant you shall mention to me. She has time to apply herself to gratify your curiosity more than I ever had; and now when I have time, the infirmities of age disable me.*

Nothing could oblige me more than your having introduced me to some correspondence with Dr. Linnæus, from whom I have had the honor of some letters. With the last I received the first part of the *Plantæ Coldenghamiæ,* &c., which he has published in a manner very much to my advantage; but I have not seen the second part, which by a line at the end of the first is promised.† I am very unfortunate in not being able to continue any correspondence with him, by the want of every method of conveyance between us. I have attempted it unsuccessfully by way of London, I suspect by my friend ——'s neglect. However, sir, pray when you have an opportunity, make my compliments to Dr. Linnæus, and assure him that no man can have a higher esteem of his great merit than I have, or would more willingly, were it in my power, make a grateful return for the favors I have received. Please to let me know what new things he has done for the information of the world. No doubt he still continues to improve our knowledge, but I am entirely ignorant of every thing lately done by him.

I had the pleasure of conversing with Mr. Kalm, in his passing and repassing through this province; though I was at the time

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* Further information respecting Miss Jane Colden, the first botanist of her sex in this country, may be found in the correspondence of Collinson, Garden, and Ellis, with Linnæus, (noticed also in this Journal, Vol. XL., pp. 5–6.) In the second volume of the *Edinburgh Essays and Observations, physical and literary,* 1780, she has very correctly described under the name of *Gardenia,* the *Hypericum Virginicum* of Linnæus, (*Elodea,* Adans.) and skillfully indicated the characters which generically distinguish it from *Hypericum.* Miss Colden died unmarried.—A. G.

† The second part of the *Plantæ Coldenghamiæ,* we believe was never published.—A. G.

very much encumbered in business. As he had the advantage of being thoroughly conversant in the European plants, more than any other person who before him had been in America, I was full of hopes that the American part of botany would be brought to great perfection, and have longed to see his performance in print. However, as many plants must have escaped his researches, much must be left to the industry of those who follow him; whose labor must be much facilitated by what no doubt he has done.

It is so long since I received a line from you, that I know not how you are disposed to receive any thing from me, or that you are now desirous of having some seeds which you mentioned formerly, otherwise they would have been sent at this time. I likewise heard that you was deeply engaged in business.

The two letters which Dr. Colden received from Kalm were written from Philadelphia; the first shortly after his arrival in this country; the second just before his return to Sweden. They are written in tolerable English.

Kalm to Dr. Colden.

Philadelphia, September 29th, 1748.

Dear Sir—I have the honor to send to you the letters of Mr. Linnæus, which he did leave to me the last year, when I went from Sweden. It is about three weeks since I first came to this country. When I first was going from my own country, I thought that I should have the good luck to be here in the beginning of the spring; but great storms in the sea, besides the war, did hinder me from the same. Now, because I do not know if I can have the leisure and advantage to see you, sir, I could not forbear to send you the above mentioned Mr. Linnæus's letter. I can't enough express the kind love and great esteem he have for you. I should too think myself very happy if I could have the honor to see you and pay you my respects; but I can't tell any thing yet, if I am to take the road from hence to New England by water or by land. I am sent of the Royal Academy in Stockholm, to make several observations in the Natural History of the most Northern parts of America, in Botany, Zoology, Astronomy, etc. I have the mind to stay this next winter in Boston, or perhaps more to North; and the next summer, if God keep me safe, I think to see some part of Canada. In the latter end of the same summer I propose to return to England, and so to my own country. If I did know that you have no examplar of Mr. Linnæus' his *Fauna Suecica*, I could spare you one thereof. I have yet by me one examplar of his *Flora Zeylanica*; if I come to you, sir, you shall have the same too. I took some of his books with me to give to my friends and the lovers of Natural History in this country; which, perhaps, not always can have the same. I have self had the advantage to be by Mr. Linnæus about two years time, and was loved of him as [if] I was his own child. If it happens that I am to take the way through New York, I will pay you my respect. Ignoscas quæso, vir nobilissime, si minus bene lingua vestra . . . . utar, notitia enim omni elegantissima et utilissima hujus lingua penitus carebam, priusquam Anglicus salutas scire terras. I am, dear sir, your most humble servant,

PETER KALM.

P. S. I should be very much obliged to you, sir, if you could procure for me some seeds of the kind of *Acer* which they call *Sugar-Maple*, and whereof the Indians in some places make a sort of sugar.

KALM TO DR. COLDEN.

Philadelphia, January 4th, 1759.

DEAR SIR—Upon my last letter, which I had the honor to write to you, sir, I have not to this day received any answer; so that I am very uneasy, fearing you is not well. I had the honor then to acquaint you, that I had deferred my going home to the month of January, because I was not sure to find any ships going for Sweden at my arrival in London. Now as the ship upon which I intend to go from hence will not be ready to sail before in the middle of February next, I have taken the opportunity to write to you with another gentleman. I should be very glad, sir, if I could receive from you the letter to Mr. Linnæus, which you was so kind and promised to send to him: he will be exceedingly glad of that, as there are few persons he sets such a value upon as upon you.

But excuse me, dear sir, that I again am so bold to trouble you with the same that I before wrote to you.

1. Be pleased to give me a good history of your life; there is nothing we want so much as a *Biographia Botanicorum:* the old were very negligent in that. There are many of which we hardly know any thing but the name; nay, if we shall get to the history of their life, we are obliged to pick up here and there a word in the writings of their contemporaries. At our days we take a more trouble in that. I have already got the history of Mr. Bartram's life, and of Clayton's. I hope Dr. Gronovius will give out his *vitæ historiam.*

2. If it was not too much trouble to give me a catalogue of all the Quadrupeds you have any knowledge of to be here in North America, both wild and tame. My catalogue of them is this; of wild, Panther, Wild cat, Loup, ... Martin, Skunk, Mink, Fischer, Possum, Otter, Seal, Wolf, Red fox, Gray fox, Fox with a cross upon the back, Silver-colored fox, Black fox, White fox, Bear, White bear, Raccoon, Ground-hog, Porcupine, Talpa, Vespertilio, Rabbit, Hare; of Squirrel-kind, the Gray, Red, Black, Flying, Ground, White; Beaver, Musk-rat, Deer, Elg [Elk], Wild oxen of two sorts.

Is there more than one sort of Panther?

Is there more than one sort of Wild-cat? The French in Canada made a distinction between *Chat sauvage* and *Loup* . . .

Is there any *Mustela vulgaris* or Weasel, colore albo, vel alio colore?

Is there any Opossum so far to north as where you lives?

Are the Seals seen in Hudson's River?

Is there more than one sort of Wolfs?

Is there any white Bears different from that at Hudson's Bay?

Is it more than one sort of Porcupine?

Is it more than one sort of Bat, or Vespertilio?

How many sorts of Rat-kind?

Is there more than one sort of Deer? The French make distinction between *Cerf* and *Chevreuil.*

Have you heard any thing of the Moose-deer?

Pray, sir, give me a short catalogue of all the fishes you know to be in fresh water in your province. What is your opinion why people lose their teeths so soon in this country?

Will you not wonder to hear that I have found here in America, growing wild, the *Colocassia,* or *Faba Egyptia veterum;* which is that species of *Nymphæa* which you will find in *Linnæi Flora Zeylanica?*

My respect to Mistress Colden, the Misses, and young Master Colden: I have the honor to be, dear sir, your most humble and obedient servant, PETER KALM.

DR. COLDEN TO KALM.

SIR—I have the favor of yours of the 4th of last month; but that which you mention to have wrote preceding never came to my hands; so that, till I received your last, I did not know whether you had left America last fall, as you proposed, or not. This made me lately write to Mr. Franklin, to know the certainty of it. I heartily wish you a happy voyage home, and that at your return you may receive those rewards which your labors richly deserve. In answer to the questions you put to me, I shall inform you, as far as my knowledge allows me to go, and in such manner as I judge best suits the view of your queries.

As to what you desire to know of myself, though the account would come more properly from another, yet I shall briefly tell you the principal turns of my life. My father was a minister of the church of Scotland, and the oldest minister in it, before he died. He was much esteemed for his piety and strict morals, and had a considerable interest with many of the nobility. I was educated at the University of Edinburgh. My father's view in my education was for the church, as by his interest I could have no doubt of preferment in it. But after I had gone through the usual studies at the University, my inclinations were averse to entering into orders in the church, and I applied myself to the study of physic. I learned the rudiments of botany under Dr. Preston, whose name you'll find in *Ray's Methodus*. I went through a course of anatomy with Dr. Ariskine [Erskine?], and of chemistry with Mr. Wilson; both of them distinguished in their professions at London. The salaries of the ministers in the church of Scotland are very small; and the expense of my education had so far exhausted my father's pocket, that I found it was not in my power to make which it is necessary for a young physician to do in Great Britain, on his first appearing in the world. My mother had a sister in Philadelphia, a widow who had acquired some estate and had no children, and this induced me to try my fortune in America. I arrived at Philadelphia in the year 1710. Upon my arrival I became very inquisitive into the American plants; but they were then so little known, and I had so little assistance from my books, that I was soon discouraged. In the year 1715, I returned to Great Britain. I had conversation with Dr. Halley, and other men of learning at London in the mathematics, for my taste chiefly inclined me to that study. I went to Scotland, and married my present wife, and the year following returned to Philadelphia. I fixed [myself] there, with a view to practice physic; but in the year 1718, I had the curiosity to visit New York, without the least thought of changing my place of residence. I visited the then Governor of the place, General Hunter, as it is usual for strangers to do, though I had no manner of acquaintance with him. He received me more kindly than I expected, and though I staid but three days in the place, I was invited by him to particular conversations. General Hunter had served in the army from the time of the Revolution in Great Britain, under King William and the Duke of Marlborough, to the year 1709, when he was made Governor of New York. He had not only distinguished himself in the field, but likewise in the court, among the polite and men of learning. In about a fortnight's time after I had returned to Philadelphia, I very unexpectedly received a letter from Gov. Hunter, with an invitation to come to New York with my family, accompanied with the offer of an office of profit; which I accepted, and soon after removed to New York. Gov. Hunter continued in his government only two years after my removal; but I had the good fortune to be in favor with all his successors, one only excepted. In the year 1722, I was appointed one of the King's Council for the province of New York. The business of my office of Surveyor General of lands obliged me to be much in the country, and my intimacy with the governors occasioning a greater expense than suited my circumstances and tastes, accompanied with no small share of envy, I removed my family, about the year 1739, to the country. After which time I indulged my humor in philosophical amusements more than I could do while in town. It was some years after this, that I accidentally met with Dr. Linnæus's *Genera Plantarum.* I was so much taken with the accuracy of his characters, that I resolved to examine them with the plants that grow near my house; and this is the sole occasion of what you have seen from me in Botany, and which is so inconsiderable that I can have no pretensions to any merit in the science.*

As to your other queries I can give you but little satisfaction. You know a great deal more than I do of the quadrupeds in America. I never heard, nor did I imagine that we had so many species of Foxes in America as you mention. It is very unhappy that our climate is so fitted to the fox constitution. I know of neither Hare nor Rabbit in this country; what we have is a middle species between the two. I have heard of a white Squirrel. Panthers are so rare that we hear of one only in a dozen years. I have seen two species of the *Mustela;* one, *Mustela fulvo-nigricans inferiore parte capitis, gulæ, abdominis, et interiori femorum alba;* 2. *Mustela tota candidissima excepto çauda apice atro.* This last is the only beast of the ravenous kind that I have a value for; because one or two of them delivered my house and barn from rats, when I was like to be devoured by them. * * * It is a most beautiful white and soft fur, so that I do not doubt of its being the true ermine.

I never saw an Opossum, nor heard of any in this province. I never heard of more than one kind of Wolf, and I suppose that you know the Indian Dog is much shaped like a Wolf. I never saw any Porcupine but in the Mohawks country, nor have I ever heard of any in this part of the country. I know only one sort of Rat; none of the Rat-kind I believe are properly natives of America, but have been all originally imported. I have often heard of the Moose-deer. One, I think, since I came to the country was catched near Albany, but I can give you no description of it. I have heard that it is as large as an ox, and has a mane like a horse. Any country boy you meet with can inform you more of fishes than I can.

As to the reason of the children of the people from Europe (not the native Indians) losing their teeth so commonly, I attribute it entirely to the scurvy, of which scarce one family is free. * * * I have heard that the Indians eat the roots of one kind of *Nymphæa;* but I did not suspect it to be the *Colocassia,* because Linnæus ranks that with the *Arum.* Please to distinguish the species, and tell me the reason you think it the *Colocassia*

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* The reader will find another brief autobiography of Dr. Colden, in his letter to Peter Collinson, dated May, 1742.—A. G.

Ægyptiacum. It will give me the greatest pleasure to hear of your safe arrival at home, and that you have published the fruits of your labors in America. Mr. Franklin, at Philadelphia, will take care of any letters for me, or Mr. Collinson in London.

The Colden papers comprise three letters from Linnæus. The earliest is dated at Upsal, on the 6th of August, 1747, and was sent by a clergyman by the name of Sandin, who came to Pennsylvania. It contains a few remarks upon the manuscript *Plantæ Coldenhamiæ*, then in his possession, and a request that he would send dried plants and seeds. The second, without particular date, was written in the same year, and brought by Kalm, and contains many notes and queries respecting the plants of Colden's manuscript. To these, Dr. Colden replied at length in his letter of February 9th, 1748–9 (O. S.), and in another entrusted to Kalm a year after; which having both been published in full by Smith, in his *Correspondence of Linnæus*, need not be reproduced here. The first is chiefly occupied with Colden's views respecting the nature of genera, &c., which are substantially the same with those given in his letters to Gronovius. Linnæus briefly alludes to this subject in the following epistle.

Viro Illustri Convallad. Colden s. pl. d. Car. Linnæus.

Literas tuas vir illustris, 1748–9, Febr. 9 datas, accepi, et summa animi voluptate perlegi, utpote datas a Fautore longe remoto et curiosissimo. Sententiam quam fores de generatione plantarum ad instructionem generum, eadem est quam proposuit D. Mitchel in Actis Naturæ Curiosorum; statuis plantas ejusdem generis esse, quæ possunt genitura sese miscere; at ego has varietates dico, nec distincta genera. Sint exempli gratia Ranunculi species diversæ, quas nullus negabit genere convenire, attamen hæ nulla ratione possunt sese miscere aut una alteram fæcundare; sed Tulipæ [quædam] et Brassicæ, quæ tantum sunt varietates, miscentur facillime.

Dubia et obscura in re herbaria circa terminos et leges varias systematis explicavi in *Philosophia Botanica*, quæ etiamnum sudat, quam cum etiamnum e prelo non prodiit, doleo me hac vice ad te, vir illustris, mittere non posse. Habebis in eo libello omnia dubia enodata, quam primum prodeat.

Mitto *Acta Upsaliensia* pro anno 1743, ut videas primam partem *Descriptionum* Tuarum: altera pars imprimitur in anno 1744, quæ nondum a prelo exiit. Si habes plura mittas quæso, omnia candide actis inseram; utinam velles tum aliquot plantas siccas simul mittere et semina: occasio quotannis datur per theologos nostrates. Si quidquam sit quod in nostris terris desideras, parata tibi sunt omnia quæ a me expetas.

Multæ sunt inter tuas plantas rarissimæ, antea non descriptæ, nobis nec vivæ, nec siccæ visæ; utinam liceret has possidere in herbario nostro. Tu valeas et diu vivas. Has exorare debui ut testarem officia et observantiam meam Mecænatem in Floræ.

Dabam Upsaliæ, 1750, d. 10 Augusti.

Dr. Colden's correspondence with Peter Collinson commenced in the year 1740, and was continued without interruption during the life of that amiable and excellent man. Collinson's last letter is dated July 2d, 1768: he died on the 11th of August following, in the 75th year of his age. The selections I have ventured to make from this voluminous correspondence, form an appropriate supplement to Smith's very interesting collection of the letters of Collinson to Linnæus.

Mr. Collinson to Dr. Colden.

London, March 7th, 1741.

Dear Friend—You have much obliged me by yours of the 22d June, and I am glad to find my little offices were acceptable to you.

I communicated your letter and project* to Mr. Grayham, whose answer I enclose; he has also been so good as to get Mr. Sisson's proposal to make an instrument that will be suitable for your purpose.

I also lent Mr. Grayham your *History of the five Indian Nations:* he was mightily pleased with it, and hoped you would oblige the world with the second part; for that he had not read any that had gave him that satisfaction and information that yours did, because he was persuaded he could depend on your veracity. You really delight me in hopes of seeing the second part; but pray take your time and do it at your leisure.

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* Relative to an improvement in the quadrant, which Dr. Colden had suggested.—A. G.

Pray have you thought, or can you give a conjecture how America was peopled, or was it a separate creation? Most of your vegetables and many of your animals are different from ours, and yet you have some exactly like ours, of which I have specimens by me; for I have a large collection, considering my years and station, of natural varieties, and some artificial, from most parts of the world, which I am obliged to my distant curious friends for sending me. They afford me great entertainment at my leisure hours; and in the country, if I may boast, my garden can show more of your vegetables than perhaps any in this island, which I have been collecting some years from seeds, and growing plants sent me by my friends in your world; so that I am no stranger to America, being pretty well acquainted with most of its productions, whether animal, vegetable, mineral or fossil, perhaps beyond what you can imagine. The uses I make of them is to admire them for the sake of the great and all-wise Creator of them, to enlarge my ideas of his almighty power and goodness to mankind, in making so many things for his profit and his pleasure. I reason on their natures and properties, so far as I am or can be informed; I compare them with ours; in short, I esteem the regard I pay them as a piece of adoration due to their great Author.

Thus, my dear friend, you see I open all my mind to you, and tell you how I employ all my leisure hours, I may say minutes, from business. I hate to be idle, and think all time sadly lost that is not usefully employed; for which reason, clubs, taverns, and coffee-houses, scarcely know me. Home is the most delightful place to me, where I divide my hours in business, in innocent amusements, and in the dear society of a tender, kind, good woman, a boy and a girl. I may now say with Milton, I have now brought you to the state of earthly bliss, and sincerely wish all mankind as happy.

I had a letter from J. Bartram;* he much laments the disappointment of not seeing you. I am persuaded you would have

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* In a former letter, Mr. Collinson thus introduces the earliest native American botanist to Mr. Colden's notice.

"If an ingenious man, and a great searcher into nature, named John Bartram of Pennsylvania, should wait on you, please to give him what information you can in those things. He has been a considerable traveller in the world, and is employed by a set of noblemen and others to collect seeds and curiosities for been pleased with him; you would have found a wonderful natural genius, considering his education, and that he was never out of America, but is an husbandman, and lives on a little estate of his own about five or six miles from Philadelphia, on the river Schuylkill. He really surprised me with a beautiful draught on a sheet of paper of the falls of Mohawk River, which he took when he was there, with a particular account of it, and also a map of his own making of Hudson River, Delaware, and Schuylkill, and the bay, which takes in the provinces of New York, Jerseys, Pennsylvania, Maryland, and part of Virginia; for he has travelled all over these countries yet uninhabited beyond the mountains, as well as the inhabited parts along the bay and the sea-shore, from the Capes to your province. His observations and accounts of all natural productions that happen in his way, (and I believe few escape him,) are much esteemed here for their truth; and he wants not terms to express himself with some accuracy. I have procured him assistance from some curious persons here, to enable him to make further discoveries. Now, my dear friend, I rely on your candor to receive this rambling epistle, as it is intended, in friendly part. From a man much engaged in business correctness is not to be expected; for really I am obliged to write a paragraph now and then, subject to many interruptions.

My best wishes attend you: when leisure offers give a line to your sincere friend, P. COLLINSON.

MR. COLLINSON TO DR. COLDEN.

London, March 12th, 1742.

MY DEAR FRIEND—You have loaded me with many favors. How I shall make ample returns I know not: but if you will allow me time and have patience, I may in some measure testify my gratitude. If I may judge of your disposition, I persuade myself you will prove a merciful creditor, and then by little and little I may discharge my obligations. But at this season is our greatest hurry of business, [so] that I am afraid I shall only be

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them." The next year Mr. Collinson writes—"I have a letter from J. Bartram, who is full of gratitude for the kind reception at Coldengham. Your affable and generous treatment he will never forget; and indeed I have a due sense of the favors shown him, being partly on my recommendation."

able to acknowledge the favor of yours of May 5th, ... with your printing and botanic schemes inclosed.

You will expect I should give you some account of your curious manuscript. I cannot do this to my liking. Our people are so wretched mercenary, that they are unworthy of it. It is now in the hands of an honest, ingenious printer, (if I am not mistaken,) and in my next you will hear more from me. I am persuaded it will meet with approbation from the public; but the way to introduce it is through such wretched, narrow spirited creatures, who are wholly governed by interest, that it is really discouraging for an ingenious man to set pen to paper, if the common good did not counterbalance all other considerations.

I shall at my leisure consider your botanic essays. As to your printing scheme, a printer that is esteemed a knowing man and of great business, gives this answer to it, for it is out of my province. He says your scheme has been tried long ago by good hands, but was found to be expensive and inconvenient in many respects, and at no rate will do for any thing else but bibles, prayer-books, &c., and even in them it is very difficult; for alterations are not easily made, though the author of the scheme thinks otherwise. And as to authors, it would be a discouragement to them; for this method would at least cost them as much as printing a thousand copies in the common way. For the composing is the chief part of the expense, the press-work only coming to a trifle. It would be easy to convince any person of the impracticability of the thing, and the vast expense that would attend it, by a few minutes' conversation.

Now, my dear friend, I confide in your humane and candid disposition to excuse me from adding further, but that I am your obliged and affectionate friend, P. Collinson.

Extract from a draft of a letter from Dr. Colden to Mr. Collinson (without date) in reply to the above.

"It seems that I have the misfortune to trouble you with discoveries which I thought my own, which have been tried by others before. This is owing to my knowing so little of what passes in the world. I must own, however, that a few years since I read in one of the London newspapers that a new method of printing was discovered, which it was thought would be of use in the advancement of learning, and a particular benefit to authors: but of the manner not the least hint was given. This led me to think what possibly this method might be; on which occasion the scheme which I sent you occurred to my fancy. And as my own appeared to be feasible, and I heard nothing further of that mentioned in the newspaper, I concluded they might be different, and mine of use, though others should have failed. Mr. Graham's arguments fully convinced me that the improvements I proposed for a quadrant cannot answer my expectations; but I cannot say I am as much convinced that the method of printing cannot succeed, from the objection the printer has made to it. For the charge of composing and of the paper being the chief charge in printing, when once composing may serve for an age or longer, and for a hundred editions, it seems to me a strong argument in favor of the new method, rather than an objection to it. But perhaps the prejudice one has in favor of his own conceits, and a jealousy that printers may oppose it with a view to their own interest, may make me a less proper judge in this case. My situation in this country puts it out of my power to make any experiment of this method of printing; otherwise, could I have procured as many types of the fashion and metal which I propose, I should have put the matter out of dispute before I had sent my thoughts of it so far as England. But as the first author of it is in England, and has all the opportunities he can desire to recommend it, it is needless for me to think further of it. Only if you be acquainted with him, you may inform him that if the charge of lead plates be thought too great, that I think the impression from the types may be made on thin boards of lime-tree or poplar; both which are of a smooth grain, very soft while green, and hard when dry: and of any other thoughts I have had on the subject, I shall be well pleased that he make what use of them he thinks proper."

A particular account of Dr. Colden's invention, which resembles the early attempts at stereotype printing, is given in a letter to Dr. Franklin, dated October, 1743, which is published in the *American Medical and Philosophical Register*, (edited by Dr. Hosack and Dr. Francis,) and also in *Sparks, Works of Franklin*, Vol. 6, p. 18. The latter also gives the correspondence with Mr. Strahan (the printer to whom Mr. Collinson submitted the plan) upon this subject.

Dr. Colden to Mr. Collinson.

May, 1742.

Dear Sir—I never received any thing with more pleasure than yours of the 7th of March last; when I perceived by it that I had gained so great a share of your friendship, and that by such means as I had reason to fear might have deterred you from continuing any further correspondence by the trouble it has given you; and on a subject which proves fruitless, any otherwise than to show how careful a man should be not to be fond of any notions he conceives, or any subject which he does not perfectly understand, and how necessary and useful it is to consult those who are skillful. Mr. Graham in three lines, (masterlike in mechanics,) not only shows that my notions cannot be reduced to practice, which I only before suspected, but that they are likewise faulty in theory in that respect. Pray, sir, return my most humble thanks to him. I can have no hopes of making him any return; otherwise I should with a great deal of cheerfulness offer my service to him in this country. However, I still so far continue my opinion of the difficulties of making small instruments, especially if they be in any manner compounded, and likewise of the difficulty to observe with them to a sufficient accuracy, that I cannot as yet entertain a sufficient esteem of Mr. Sisson's instrument, because of the difficulty of discovering the errors and correcting them, when an entire circle is not used, otherwise than by a long series of observations made with the greatest accuracy.

Last winter I employed the greatest part of my leisure time in revising the first part of my *History of the Five Nations*, and in putting into some order the materials which I had collected about the year 1725 for the continuing of it; and I left so much of it as I could then get copied with Mr. Alexander, in March last, to be sent to you by a ship which proposed to sail for London soon after that time; and since that, I have got the remaining part of it copied, which I now send to him to be forwarded to you. I am truly ashamed that I could not have it copied in a better hand, but in the situation I am in I could not help it. My chief view in that work, I may truly say, is to do you a pleasure. However, if you think it may be useful to the public, you have my full consent to publish it in what manner you think fit; though I have no great fondness to appear as an author, while I am sensible how much more a man is likely to suffer from the malevolent tempers of many readers, than to gain any applause or benefit from those that are more candid and indulgent, where the design of writing appears to be useful, though it be weakly performed. Every man in my opinion owes so much to his country, that he should patiently submit to scoffs, and jests, and revilings, when he thinks he cannot avoid them by being useful; and I hope it will appear my design is, as it really was, in writing that history, to be in some degree useful to my country. If it be so, I shall truly gain my end, without any further view besides that of endeavoring to give some pleasure and amusement to you.

I once had entertained hopes of enlarging my knowledge of the Indian affairs and manners, by spending some time among them; but as I did not understand their language, and could have no interpreter but at a considerable expense, more than I could bear, I was forced to lay aside that design; and now I have little or no hopes of gaining more information than what I already have. I might have put in several more particulars, to show upon what grounds I have more than once blamed the mismanagement of the Indian affairs in this province; but I did not think it proper to be too particular, as it must throw severe reflections upon particular persons or families now in this province. I may venture to give you in private some particular facts which it is not proper by any means to be made public and general. * * * More such instances can be given. * * *

I look upon it, sir, as one of the happy incidents of my life, that I have had the good fortune to fall into a correspondence with you; because I take you to be one much of my own taste, and I have often wished to communicate some thoughts in natural philosophy, which have remained many years with me undigested; for we scarcely have a man in this country that takes any pleasure in such kind of speculations. Your communicating to me your private manner of life is the strongest instance of your friendship, and in some measure makes up the loss of a personal acquaintance, which I cannot hope to obtain. This encourages me to give you some account of myself, believing you may expect it, as I hope that you intend to continue your correspondence.

I was educated in Scotland, by my parents, with a view to be settled in the church there; and I had as great encouragement in that way by my father's interest, who was a minister of that church, as any young man could have; for my father was acquainted with, and had gained the esteem of many of the nobility and gentry, not only of those who thought as he did in respect to religious principle, but likewise of those who differed widely from him. But my taste and inclinations led my thoughts another way. I applied myself to the study of physic, and, as my father's fortune was not sufficient to enable me to push my fortune in England and Scotland, I went over to Pennsylvania, in the year 1710, where I had some relations. When I came first into America, I was very young,* and though I had some knowledge of books, I was absolutely a stranger to the world. The encouragement to a mere scholar is very small in any part of North America, and I had little sense of the value of money at that time, when it would not have been difficult for me by trade to have raised my fortune, as others did about the same time. I had sufficient for my present occasions, and I had not then learned to be concerned for the future.

In the year 1715 I married, and my family soon began to increase, which gave me some care that I had not before. Soon after this, going out of curiosity to see New York, I fell into Brigadier General Hunter's conversation, who was then governor of that place. He gave me an invitation to settle in New York, with an offer of his friendship, which I accepted. By his interest, I was made surveyor general of the king's lands in this province. Mr. Burnet soon succeeding him as the governor, I likewise gained his friendship, and he recommended me to be of the king's council for this province, in which two offices I have continued ever since. My family being considerably increased, I left the city at the time Mr. Burnet was removed from the government, and settled them in the country, where I now live, as being less expensive. I have been enabled to live above want, to keep free of debt, so as never to suffer a laboring man to go from my house without his wages, and I hope to be able to put my children in a way to provide for themselves by their own industry; which often proves more advantageous to them than leaving such estates as that they can hope to live without thought or care. My eldest son has for some years kept what we call a store in this part of the country. I suppose you know

* He was born, it appears, in the year 1688.—A. G.

of what kind of mercantile business it is, by your general knowledge of America. My eldest daughter is married, as to fortune beyond what I could expect in regard to my own, to one of the late Mr. De Lancy's sons. I doubt not you have heard of his father, he having been one of the most noted merchants in America. My younger children give me reasonable hopes of their doing well in the world, as they grow up, by their industry and virtue.

My removing to the country, I believe, has been of no disadvantage to my children, as it has freed them from many temptations to vice, to which youth is exposed in the city. My chief pleasure, like yours, is in my own family, with my wife and children, and I wish I could live so as never to be from them. I have always had a view to be useful to my country, (though I have had sometimes my designs that way grossly misinterpreted,) and I have taken most pleasure in speculation for that end. I cannot say how far I have succeeded; but none now deny the benefit of the trade at Oswego, in the framing of which scheme and reducing it to practice I had a considerable share. I have made a small spot of the world, which, when I first entered upon it, was the habitation only of wolves and bears and other wild animals, now no unfit habitation for a civilized family; so that I may without vanity take the comfort of not having been entirely useless in my generation. I once intended to have attempted the natural history of this province; and Mr. Burnet for my encouragement annexed a small salary to my office of surveyor general, to be paid out of his majesty's quit-rents. But Mr. H. Walpole at the same time having procured an additional salary as auditor of his majesty's revenue in America, mine was taken off, to make way for his, and I was left without any thing besides the perquisites of my office, which often are very precarious. This obliged me to lay aside all kind of study that was attended with expense of time and money. I hope, notwithstanding of this, to be able to entertain you from time to time with what may prove no disagreeable amusement, according to your own taste. I have at this time too far presumed on your patience; but it now begins to be difficult for me to leave off while I write to you; for I really am, dear sir, your most obliged and affectionately humble servant,

Cadwallader Colden.

MR. COLLINSON TO DR. COLDEN.

London, March 9th, 1743.

DEAR MR. COLDEN—You cannot be more surprised at the progress of botany in Doct. Linnæus, than I am to see what a proficient you are on his scheme. I could not have imagined it had reached in so short a time to the remote parts of North America, for I hear he has made several proselytes in different places on your continent. But your fame reached me long before your letter. My valuable friend, Dr. Gronovius, let me know what a fine present you have made him; the good man is in raptures. I doubt not but Doct. Linnæus has heard of it long before this. I shall soon send your curious observations to Doct. Linnæus; your criticisms are perfectly just; you have done me a pleasure in circulating it through my hands, because it sets me right who have not leisure for such nice observations, and to the Doctor, I know it will give him the greatest delight; any omission in him is not owing to his judgment, but want of growing subjects whose minute parts are more distinct, which are lost in dried specimens; so that at the same time that you are improving your own knowledge, you are greatly obliging your friend; and if all his pupils was equally as communicative as you are, his works would be more perfect and complete. It is no little disadvantage to him to be settled as in the fag end of the world. In his letters to me, he envies our happiness, who have a free and frequent intercourse with your world, and our gardens abound with its productions; and then we have annually seeds and specimens which produce something new, and proper to exercise the talents of so learned and curious a botanist. But a gentleman of your benevolent disposition may in some degree soften the severities of the north, and Flora may, in some little disguise, by your assistance, for once appear amidst ice and snow. A few specimens preserved and dried in paper, and a few seeds, sent him as opportunity offers, with your curious remarks, would be to him all that I have allegorically hinted; and I will take care they shall be safely conveyed to him. * * * I am, with much respect, your affectionate friend,

P. COLLINSON.

Dr. Colden to Mr. Collinson.

Coldengham, Nov. 13th, 1744.

Dear Sir—I have your very kind letter of the 3d of September. If I have had the good fortune to gain your esteem in any degree, and thereby a share in your friendship, I shall think myself well rewarded for any thing I have done; and when I consider the trouble you take, and the concern you have for the little reputation I can hope to obtain, I may flatter myself that I have gained no small share in both. This encourages me to go on in communicating to you what thoughts have occurred to me, which I think can be any way useful in the world or amusing to you. I cannot deprive myself so far of all self-esteem but to hope, that a life of fifty years, a greater part of it spent in some kind of speculation or other, may produce something worthy your inspection at leisure hours, and therefore I shall continue to communicate some thoughts which have at times occurred to me, on every opportunity I shall have of writing to you. * *

The observation you made in your former, that we have in America many different species of plants and animals from those found in Europe or other parts of the world, though under the same climate, is certainly true; and I think we may likewise add, that we have different species of men. This naturally enough leads to the question you put, whether they be the effects of a different creation. But, dear sir, I dare not pretend to give any answer in a matter so high and out of my reach. It is a subject fit to be treated only by first rate philosophers and divines. I should be glad to know your sentiments on it.

I had the pleasure of seeing Mr. Bartram at my house this summer. It is really surprising what knowledge that man has attained merely by the force of industry and his own genius. He has a lively fancy, and a surprising memory and indefatigable disposition. I warned him with some concern against his exposing himself so much to the inclemencies of our climate as he does, and though he thought his constitution proof against it, * * * [As to your] garden I must, dear sir, entreat you to give us some share of that pleasure, by publishing a description of the plants in it, for I cannot hope to partake with you in any other manner. The use you make of it in admiring the infinite variety and beauty of the works of the Creator, comprehends only one half of our religious duty, contained in Christ's first command to his disciples. It is properly the speculative part of religion which fits us for, and incites us to the practical part, or obedience to his second command, that of loving our neighbor as ourselves. The practice of the second command gives no less pleasure to a good man than the speculations of the first. You have a great deal, sir, in your power; that of being useful to almost one half of the world, to all America. We are very poor in knowledge, and very needy of assistance. Few in America have any taste of botany, and still fewer, if any of these, have ability to form and keep a botanical garden, without which it is impracticable to give complete characters of plants. In short, I *may* positively assert, that not *one* in America has both the power and the will for such a performance. Such a work is necessary; it will be a lasting benefit to mankind. It has all the motives to it which can incite a good man to any performance attended with trouble. I am sensible how much your time is taken up with business. But at the same time, I cannot doubt of your obtaining assistance from curious persons, perhaps much at leisure. I told Mr. Bartram of the design I have of entreating you: he was exceedingly pleased with it, and promises all the assistance in his power. * * * How much labor, and how many valuable collections in botany, more than in any other science, have been lost to the world by delay, and an endeavor of the author to complete his work before it appeared in public. I shall not presume to give my thoughts on any particular of the method to be observed in this work, because I have but a very superficial knowledge in botany; I shall only say, that I wish it to be in English, though I know that it is more difficult to do it in this language than in Latin. To encourage you in this, I enclose a description in English of two American plants, not as patterns, but to convince you what may be done, if I, who have so little skill in botany, have been able to make them tolerable. One of them I have for many years taken notice of as one of the signs of a fertile soil; but of late I cannot pass it without paying a particular regard to it. The reason of my choosing the other will appear in the description of it. But to return to the reasons I have for desiring your work in English:—1st. We have nothing in botany tolerably well done in English, so far as I have seen. 2d. It will thereby be more useful in America, where the learned languages are little understood. 3d. It may set many who do not understand Latin, the ladies especially, on amusing themselves with this study, and thereby procure more assistance in bringing this knowledge to perfection. The ladies are at least as well fitted for this study as the men, by their natural curiosity, and the accuracy and quickness of their sensations. It would give them means of employing many idle hours, both usefully and agreeably. As I cannot doubt that Mrs. Collinson has the same taste of pleasures with you, I am fond to believe that she will with pleasure, save you some trouble in such a work as I propose. No doubt your correspondents inform you of the uses of several plants. I wish something of that may be added; for as most of the plants are new to us, the uses of them must be so likewise. Indeed a plant may be long known, and the use but a late discovery.

This brings to my memory what I have read in Allen's London Dispensatory, under the word *Ipecacuanha*, of a root from Maryland, which in most of the shops had been substituted in place of the true Ipecacuanha, the use of which was forbid by the College of Physicians, on Sir Hans Sloane's information, that it was a kind of *Apocynum*. No doubt the college was in the right to forbid the substituting of one plant in place of another; but I am not well satisfied with the reasons given by Sir Hans, as delivered in that book; viz. that it is a poisonous plant, being a kind of *Apocynum*. Now to this I object, that it is doubtful whether any of the plants which are now known by the name of *Apocynum*, be really the *Apocynum* of Dioscorides, by whose authority alone our *Apocynums* are branded, so far as I know, with the character of poisons. Again: supposing Dioscorides' plant to be truly an *Apocynum*, it does not follow that all the species in America ranked under that genus are in like manner poisonous. Dioscorides says, that his *Apocynum* has a very offensive smell: I know an American species whose flowers smell very agreeably; and may not their virtues likewise differ as much? I think we have strong reasons to judge, that the kind of *Apocynum* substituted in place of Ipecacuanha cannot be poisonous, otherwise it could not so generally have taken its place. Sir Hans likewise affirms, that the roots of a kind of *Apocynum* are commonly vended in New Spain for Ipecacuanha; if so, I doubt the greatest quantity of Ipecacuanha in the shops is from thence. I have inquired of Mr. Bartram and others, to discover this Maryland Ipecacuanha; but can discover no roots under that name but two, both of them taken notice of by Mr. Clayton in *Gronov. Flora Virgin.* Neither of them can be the plant substituted for Ipecacuanha; because the one hardly works with double the dose of the true Ipecacuanha, and the other (an *Esula*) works violently with half the dose. You will oblige me by describing the species of *Apocynum*, substituted in place of Ipecacuanha, as Sir Hans affirms, or whatever other American plant it be.

I have presumed, I am afraid, too far upon your time and patience; but when I consider that I am grown old before I had the good fortune of any acquaintance with you, and that I can have but few opportunities of continuing it, and that only for a short time, I cannot forbear making the most I can of the opportunities granted me, and beg you will excuse, sir, * * *

Mr. Collinson to Dr. Colden.

London, April 26th, 1745.

My Dear Friend—I did not expect that I should have found any leisure to answer your kind letter of the 8th of December last; but I will find time to thank you for the perusal of your papers to Dr. Gronovius, which I read over and over, and was much pleased with your reasons and objections; [which] are of such weight as must affect the Linnæan system, and prevent its being universally received. Tournefort and Ray, in my judgment, are much preferable. Take this in general. I wish I could be more particular. I really wonder at your proficiency in so short a time. I transmitted those papers by a safe hand to Dr. Gronovius, who writes many curious remarks on the things sent by J. Bartram, in distinct pages. Could you see them, they would I am sure give you entertainment, and put you in a regular method for future enquiries in some matters. I really don't know such another knowing, indefatigable man, as Doct. Gronovius.

Your brother sent me a letter for you, complaining he had not heard from you, which I enclosed early in the spring to J. Bartram, by Capt. Bream, who I desired to forward it to you.

I am glad to hear of the Philadelphia society. I certainly think it cannot labor long when such wonders are all around them, ready brought forth to their hands, and to which we are great strangers; but because you see them every day, they are thought common and not worth notice.

Hitherto I have wrote only to blot paper; but now I tell you something new. Doct. Knight, a physician, has found the art of giving such a magnetic power to steel, that the poor old loadstone is put quite out of countenance; his steel magnets act on the needles, and transmit their power to knives, &c., as the loadstone. But he has also shown a secret on the loadstone not known before, by increasing its attractive power to a greater degree, and can at pleasure change the poles how he pleases. Take these examples:—A loadstone of a parallelopiped form he made the opposite end south poles, and the middle quite round all north poles. In another flat stone he made the opposite ends north poles, and the opposite sides south poles. In another loadstone of an irregular flat shape, he made half of each of the flat surfaces a north pole, and the other half a south pole, so as that the two half surfaces opposite each other, should be of a contrary denomination, with many other changes and varieties, showing he had the power to impress the faculty of either pole many parts of the loadstone, with as much ease as a loadstone will influence a needle. I am yours, P. COLLINSON.

Dr. Gronovius hopes you will continue your remarks, and send him seeds of any of your vegetable productions.

The subjoined postscript to a later letter of Collinson to Dr. Colden, will bring to mind the correspondence of this candid man with Linnæus on the same subject; (vide Smith's *Correspondence* of Linnæus; and this *Journal*, Vol. XL, p. 7.)

"Dr. Linnæus is now publishing his *Species Plantarum*, with many new-coined names, which will so puzzle the science of botany that it will be impracticable to comprehend it. The *Azaleas* he has turned into *Kalmias;* so that every book he prints will require a new edition of his *Nova Genera*, which is a grievous tax and imposition on the public."

MR. COLLINSON TO DR. COLDEN.

London, June 9th, 1755.

I cannot let this ship sail without asking you, how it fares with you this troublesome time? Your situation makes me anxious for you and your family. Crown Point I may call in your neighborhood: if we are so fortunate to take it, it will be well; but unless we can maintain it and support the country round it, it is probable the French Indians, as well as troops, may come to distress the country round it in revenge.

John Bartram's ingenious son William has sent a pretty map of the Drowned lands, including the mountains and a branch of the Delaware on one side, and North River and the Wallkill on the other; near which, between two rivers, you are pleasantly as well as securely settled, which may preserve you from sudden excursions. As inhabitants increase, the Drowned lands will by degrees be drained and become a most fertile spot.

J. B. has made many curious observations on all the country round, and the course of the rivers, &c. He says the limestone in the vale near the last run in the Wallkill, that is, between the Blue Mountains and Katskill Mountains, is composed of sea-shells, cockles, clams, &c.: but the most remarkable is below *Gosion*, [Goshen,] where the limestone has the most perfect cockle-shells that ever he saw. If any of these happen in thy way, I should like one or two specimens, as confirmations of the universality of the deluge; and seemingly not a great way from thy house are found the oddest kind of scollop-shells in stone that ever he saw: a sample of these will be acceptable. I have the pleasure to tell you that the *Saracenas* are now in flower, by planting them in moss, in artificial bogs. I had your cranberries fruited last year by the same method. * * * I am, my dear friend, truly yours, P. Collinson.

Mr. Collinson's explanation of the occurrence of marine shells in rocks and strata elevated far above the sea, absurd as it now appears to us, was the prevalent, if not the undisputed hypothesis of that day. Dr. Colden's views, as briefly expressed in the following letter, are far in advance of the age in which he lived. Indeed, I suspect that the records of science, down to a considerably later period, will not be found to furnish an explicit statement on this subject so perfectly in accordance with modern geology.

Dr. Colden to Mr. Collinson.

We have had a very extraordinary summer, the dryest in the latter part of it and hitherto, that ever was known, at least these forty years past since I came to America. I lost my fruit of all kinds by a frost the last day of May, and many likewise lost their rye, being in blossom at that time. There is something in cold and in frost which we do not understand. I suspect strongly that it rises from the ground, by [its] effects being so very different in different places at a small distance from each other, according to the difference of the soil and situation. The lowest situation and meadow grounds are generally most subject to frost. The difference of a few feet in height is often very sensible in the same farm. The most frequent frosts are in dry summers.

I have not met with any shells since I received your last; though I have frequently seen them near my house. Mr. Bartram some years since carried a considerable piece from my house: as soon as I can get any I shall send some to you. *These shells, and many other marine things found far within land and on the top of mountains, I think prove that those parts where these shells, &c. are found were once under water; but it does not prove that the face of the earth was at that time the same as it is now. I think the contrary, that it must be different now from what it was then, and that this difference probably has happened by great general earthquakes.*

In the bundle with the papers, I have mentioned there are some descriptions of plants by my daughter Jane, which I designed to have sent by a ship from hence to Holland, which sailed a few weeks since; but was prevented in sending them by the sickness of my youngest daughter. I am pleased that they go to you, that you may perceive how far she deserves encouragement, in giving such an example to others of her sex. Please to convey them with my letter to Dr. Gronovius, when you have a proper opportunity.

Mr. Collinson to Dr. Colden.

Ridge Way House, Feb. 25th, 1764.

I am here retired to my sweet and calm old mansion, and from its high elevation look forty or fifty miles round me, on the busy vain world below; envying no man, but truly thankful for the undeserved blessings good Providence hath pleased to confer on me. With a pious mind filled with admiration, I contemplate the glorious constellations above, and the wonders in the vegetable tribes below. I have an assemblage of rare plants from all quarters, the industrious collection of forty years. Some or other of them, all the year round and all the seasons through, are delighting my eyes; for in the depth of our winter, the plants from the Alps, Siberia, and the mountains of Asia, exhibit their pretty flowers and anticipate the spring; the Black Hellebore, with its large white flowers, the Aconite with its golden clusters—these show themselves before Christmas; for that reason the first is called the Christmas Rose. Primroses and Polyanthus, Wall-flowers, and some Violets, and single Anemones, flower all winter, unless a snow happens to fall, which is seldom. It seems a paradox, (considering our latitude,) to tell foreigners that vegetation never ceases in England. I am this instant come in from seeing your Skunk-weed; its early appearance and its singularly spotted flowers, attract the notice of every one. It hath been now a month in flower; by this you may guess the difference of seasons with you and us. But this winter (if it may be called so) is very different from all that has been remembered. We have had as mild and warm, but then, it hath been dry, sunny, and pleasant; whereas this hath been continued (I may say daily) rains: if a few frosty mornings, certainly rain at night, moist and warm, but attended with hurricane winds, and the air so beclouded it was rare to see the face of the sun. The consequences of such inclement weather hath been more shipwrecks and inundations than ever was known in one winter. It is very affecting to read the very deplorable accounts from time to time: the loss of sheep and cattle drowned, advances much the price of provisions; but, thank God, from the plenty of last year (though a bad harvest) our bread keeps under twelve pence a peck loaf, and we have been able to supply vast quantities to our indigent neighbors. I am assured some weeks eight or ten thousand quarters of wheat have been shipped off for France, Portugal, Spain, and Italy: this trade brings in great riches, being a surplus that we can spare without prejudice to ourselves. When I look back and consider the poor state of agriculture here in the last century, it affords a pleasure I cannot express, to see our extensive improvements made in this age. Then the city of London imported annually Polish wheat from Dantzic, (to the enriching foreigners,) to fill our granaries; for our culture of wheat was so sparing, that if a crop failed a famine was like to ensue. To prevent this terrible calamity, the city prudently sold it out one year under another. But the Dantzic corn trade hath been long left off, and instead of buying, we annually sell to our neighbors.

As often as I survey my garden and plantations, it reminds me of my absent friends by their living donations. See there, my honorable friend, Governor Colden: how thrifty they look. Sir, I see nothing but two fine trees, a Spruce and a Larch. That is true, but they are his representatives. But see, close by, how my Lord Northumberland aspires in that curious Fir from Mount Ida: look yonder at the late benevolent Duke of Richmond; his everlasting Cedars of Lebanon will endure when you and I and he are forgot: see with what vigor they tower away, how their stems enlarge, and their branches extend. But pray what are those Pines? Novelties rarely seen; that elegant one with five leaves is the Cembro Pine from Siberia: the other tall tree is the very long-leaved Pine of ten or twelve inches from South Carolina; they stand mementos of my generous friend, the late Duke of Argyle. That gentle tree, so like a Cypress, looks uncommon: that is the Lycian Cedar; the seed was given me by Sir Charles Wager, first Lord of the Admiralty, gathered in the Isle of Ivica, in his voyage to convey Don Carlos (the now king of Spain) to Naples. But those Balm Gilead firs grow at a surprising rate, it is pleasant to see: they renew a concern for my dear friend, Lord Petre; they came young from his nurseries, with all the species of Virginia Pines and Cedars. That Fir that grows near them is remarkable for its bluish-green; that was a present from my worthy friend, Sir Harry Trelawny: it is called the Black Spruce, he had it from Newfoundland; it grows delightfully. Regard the variety of trees and shrubs in this plantation, as Mountain Magnolia, Sarsifax, Rhododendrons, Kalmias, and Azaleas, &c. &c.: all are the bounty of my curious botanic friend, J. Bartram of Philadelphia. And for these pretty Fringe trees, Halesias, and Stuartia, all great beauties, I must thank my friend John Clayton, the great botanist of America. How fragrant that Allspice; how charming the Red-flowering Acacia, the great Laurel-leafed Magnolia, Umbrella Magnolia, and Loblolly Bay: these charming trees are the glory of my garden, and the trophies of that friendship that subsists between me and my very obliged friend, J. Lambol, Esq. of South Carolina.

Thus gratitude prompts me to celebrate the memory of my friends, amongst whom you have long claimed the respect and esteem of yours sincerely, P. Collinson.

Mr. Collinson to Dr. Colden.

London, Feb. 10th, 1768.

I had the pleasure of my dear friend's letter, with the packet for Lord Shelbourne, which was delivered. Now there is Lord Hillsborough appointed secretary for the colonies; for the future your application will be to him. We may now hope, as the colonies are his peculiar care, the public grievances will be redressed, yours in particular; so the sooner your application to him the better, and if it was conveyed by your friend, Sir Jeffrey Amherst, it may have more weight; for I have no acquaintance with him. * * *

I presume you have heard of the wonderful discoveries. Near the Ohio, about six hundred miles below Pittsburgh, and four from the river, is a great Licking-place: [here] George Crogan, Esq. found a great quantity of the great long elephant's teeth and bones. He sent over of these great long teeth or tusks six or eight, which I have seen and handled. Some about six feet long, and the same thickness as recent elephant's teeth of that size, and, what is remarkable, some are not in the least decayed: the ends cut off show as fine white ivory as recent ivory. Now is not this wonderful! A small tooth about two feet long, of a chestnut color, hath a fine natural polish, as if just taken out of the head of the young animal.

As there never were any elephants in America, neither could they subsist for the severe long winters where they are now found near the Ohio, what hypothesis can be formed to account for these being found there, under a bank on the sides of this great lick, where some portions of the bones and teeth lay exposed to view? (seven hundred miles from the sea.) Mr. Crogan believes, from the quantity of the bones and teeth, there could not be less than thirty animals. He sent the teeth to Lord Shelbourne and Benjamin Franklin. Portions of the like elephant's teeth found in Peru, were sent last year as a present to the Royal Society. Mr. Franklin talks of coming over by some of the summer ships: he is very well, and much caressed and admired here.

Pray give my kind respects to thy son David. I shall be greatly obliged to him for the information he intends me on the Rattlesnake. Any remarks on natural history will be very acceptable to your sincere friend, P. Collinson.

P. S. I thank good Providence I have lived to see a pair of your great Moose-Deer's horns sent to the Duke of Richmond: there is not a pair in the British Museum, which is a great loss to that grand collection, which is the wonder of the world.

It was always said the great Deer's horns found in the bogs in Ireland, some ten feet from tip to tip, was the same as the great Moose-Deer's of New England and Canada. But this pair shows there is no affinity; but your Moose horns are very like the Elk of Germany and Russia; so that the animal that produced the Irish horns is not now known to exist in all your discovered world, and it is not in our own parts; but possibly it may have being in Terra Australis, *or no where:* but that is not agreeable to the plan of Providence.

We have no room left for the letters of Dr. Garden of South Carolina; which, however, are for the most part so much occupied with private and colonial affairs, that they do not possess the same scientific interest as his published correspondence with Ellis and Linnæus. The following letter gives an account of his visit to the elder Bartram, at his well-known garden near Philadelphia.

Dr. Garden to Dr. Colden.

Philadelphia, Nov. 4, 1754.

Honored Sir—I cannot help, once before leaving Philadelphia, begging permission to intrude on a philosophic hour, in troubling you with the perusal of a few lines, according to my promise when I had the pleasure of seeing you at New York. Since my leaving that place I have met with very little new in the botanic way, unless your acquaintance Bartram, who is what he is, and whose acquaintance alone makes amends for other disappointments in that way. I first waited on him with Gov. Tinker and Dr. Bond, whom he received with so much ease, gaiety and happy alacrity, and invited to dine with so much rural vivacity, that every one were agreeably pleased and surprised. Unluckily Gov. Tinker had engaged some company to be with him that day, else we should have taken part of his botanic treat, which he seems fully designed to have some day this week.

One day he dragged me out of town, and entertained me so agreeably with some elevated botanical thoughts, on oaks, ferns, rocks, &c., that I forgot I was hungry till we landed in his house about four miles from town. There was no parting with him for two days, during which time I breakfasted, dined and supped, slept, and was regaled on botany and mineralogy, in which he has some excellent notions and grand thoughts. His garden is a perfect portraiture of himself; here you meet with a row of rare plants almost covered over with weeds, here with a beautiful shrub, even luxuriant amongst briars, and in another corner an elegant and lofty tree lost in common thicket. On our way from town to his house, he carried me to several rocks and dens, where he showed me some of his rare plants, which he had brought from the mountains, &c. In a word, he disdains to have a garden less than Pennsylvania, and every den is an arbor, every run of water a canal, and every small level spot a parterre, where he nurses up some of his idol flowers, and cultivates his darling productions. He had many plants whose names he did not know, most or all of which I had seen and knew them; on the other hand, he had several I had not seen, and some I never heard of. To-night I shall pay him a visit along with a Jamaica doctor; we set away after dinner, and design to remain all night with him.

I shall be glad to hear of Miss Colden's improvements, which no doubt increase every day, and may we again be surprised with more than a *Dacier*, even in America. Messrs. Wragg and Cleland left this place last Monday; we shall leave it in ten days. I shall expect the favor of a line from you soon, by the time I get to Charleston. I beg leave to offer my compliments to Mrs. Colden and your good family, and remain with great esteem, honored sir, your most obliged and very humble servant,

A. Alexander Garden.

Dr. Colden was appointed lieutenant governor of New York in 1761; and performed the duties of governor for a great part of the time, until 1775, when by the return of Governor Tryon, he was relieved from official cares. He then retired to a seat on Long Island, where he died on the 28th of September, in the 89th year of his age. "For the great variety and extent of his learning, his unwearied research, his talents, and the public sphere which he filled, Cadwallader Colden may be justly placed in a high rank among the distinguished men of his time."

ART. XIII.—*Notice of newly discovered Fish Beds and a Fossil Foot Mark in the Red Sandstone Formation of New Jersey;* by W. C. REDFIELD.

SINCE the discoveries of fossil fishes and foot marks in the red sandstone formation of Connecticut and Massachusetts, some of which fossils resemble in their characters the ichthyolites which are peculiar to the new red formations of England and Germany, it has been deemed important to ascertain if analogous memorials belong to the apparently kindred rocks in New Jersey, and which also extend southwesterly across several of the Atlantic states.

About four years since, the discovery of fossil fishes in the New Jersey rocks, near Boonton, was made known in this Journal. Soon after, I obtained several species from this bed, nearly all of which have been found to agree with the species which I had obtained from the sandstone formation in Connecticut.*

This agreement in fossils being established, it was still desirable to obtain further comparisons, and more especially, to determine whether the fossil footsteps, the *Ornithoid-ichnites* of Prof. Hitchcock, appear in the sandstone rocks of New Jersey.†

Early in the present autumn, specimens of ichthyolites from another locality in New Jersey, were presented to the New York Lyceum of Natural History, by A. R. Thomson, Esq. The specimens were obtained near the sandstone quarries of Peter M. Ryerson, Esq., in Pompton, about twenty five miles from New York, and perhaps ten miles northeasterly from the above mentioned fish-bed at Boonton. These fossils had been lately brought to light by excavations made in search of coal, in a thin bed of dark colored shale, the joints of fracture in which were found to contain indurated bitumen; which had doubtless exuded from the rock, in a softer state. This bed of shale separates the overlying "variegated calcareous conglomerate" of Professor Rogers, from the red sandstone rocks beneath; and is described by him

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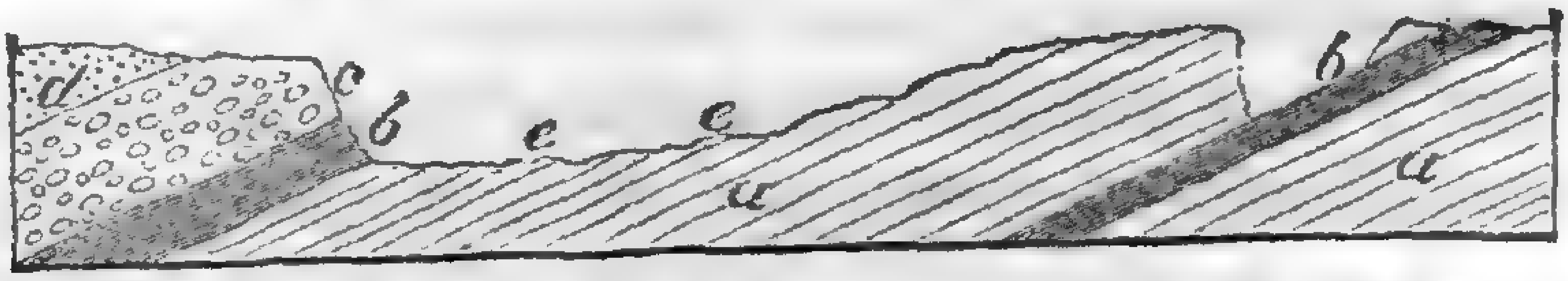
* This Journal, Vol. XXXV, for 1839, p. 192:—Vol. XXXVI, p. 186:—Vol. XLI, pp. 25–28.

† The term Ornithichnites, used by Professor Hitchcock in describing these foot marks, has been changed by him to Ornithoidichnites. See his late Report on the Geology of Massachusetts.

as "a thin bed of gray siliceous slate, very schistose," and, with the other rocks, dipping conformably "to the northwest."*

I have since visited this locality and obtained seven species of ichthyolites, all of the genus Palæoniscus. Three of these, viz. *P. fultus*, *P. latus*, and *P. Agassizii*, are common also to all the fish-beds of this formation in New England. Some of the remaining species seem nearly allied to ***Palæonisci*** of the magnesian limestone, in the New Red group of England; as appears from specimens sent me by Sir Philip Egerton, as well as from those which are figured in the ***Poissons Fossiles*** of Agassiz. The genus Catopterus, which is common at Boonton as well as in Connecticut, has not been found at Pompton. Of the several species which belong to the Connecticut localities no less than eight are found at Boonton; which comprise nearly the whole series, in both cases.

The ravine in which Mr. Ryerson's quarries are situated exhibits a good section of this part of the formation; the rocks being here exposed for several hundred feet in thickness, in the order of superposition. In passing up this ravine, we arrive at another thin bed of bituminous shale which dips perhaps two hundred feet below the fish stratum above described. A short examination of this inferior bed sufficed to show that it also contains ichthyolites; a specimen having been fortunately obtained, as I was on the point of leaving the ground. This is the first instance, perhaps, in which a plurality of fish beds, in the order of stratification, has been distinctly observed in the red sandstone formation of this country.



a a Red Sandstone. *b b* Fish Beds. *c* Calcareous Conglomerate. *d* Drift. *e e* Quarries.

While examining the sandstone quarries, which lie between the two beds of shale noticed above, I was fortunate enough to discover and obtain a well characterized foot print. This ichnolite belongs to a species which is found in the sandstone rocks about Middletown, in Connecticut. The specimen is in relief;

* See the final Report of Professor Rogers on the Geology of New Jersey, p. 125, *et seq.*

its character, tripartite, massive, or thick-toed, the two lateral toes very short compared with the middle one; length about six inches, breadth three and a half inches. The claws or nails of the lateral toes are well defined, and project from the bottom of the toe about four tenths of an inch. It appears to be a common variety of the *O. tuberosus* of Prof. Hitchcock, and is nearly represented in figure 21, Plate 37, of his final Report, in quarto, on the Geology of Massachusetts.

In the same quarries I observed numerous slabs and fragments which exhibit fossil impressions of rain drops, or hail; and also natural casts of the same in relief. There is great variety in these appearances; and some are of the most perfect character: while, in not a few cases, the oblique form of the impressions serves to show that the drops were driven by a strong wind. These impressions and their casts seem to indicate a nearly horizontal surface as pertaining to these beds, at the period when the impressions were made and overlaid by the superior deposits. The foot mark, also, exhibits no lateral inclination or obliquity in its form, that might correspond with the present high slope of these strata, which I estimate at about twenty degrees.

I obtained also a good specimen of ripple mark at these quarries. This appears to be not wind-ripple but water-ripple; and on the lower surface of this slab, within half an inch of the ripple, some marks of shrinkage cracks may be seen, such as occur in a drying surface of mud or silt. From this we may infer that the current of water which caused the ripple was produced by rain, or other transient cause.

The facts above noticed seem to render it probable that many foot marks, of various species, will be found in the red sandstone rocks of New Jersey; and that the time is fast approaching when the relations of this formation to those of the New Red system in European countries, can be profitably examined. Indeed, the considerations already adduced by Professor Hitchcock go far to establish the identity or equivalence of these formations.

New York, November 10th, 1842.

Art. XIV.—*Strictures on Prof. Dove's Essay "On the Law of Storms;"* by Robert Hare, M. D., Professor of Chemistry in the University of Pennsylvania.*

TO THE EDITORS OF THE AMERICAN JOURNAL OF SCIENCE.

Dear Sirs—Since forwarding my communication containing additional objections to Mr. Redfield's theory, I have perused Prof. Dove's Essay on the Law of Storms. I now send to you for the Journal, some strictures to which I conceive that essay liable.

But first allow me to take due notice of the note subjoined by you to the first page of my last communication, in which you allege your understanding of Mr. Redfield to be, that he represents "*the whirlwind as the cause of the violence, not the cause of the whirlwind.*" The language on which my understanding was founded, represents "a rotative movement of *unmeasured violence,*" as "the *only cause of violent and destructive winds or tempests.*" But admitting your impression to be correct, does it make the error less to say that a whirlwind is the only cause of its own violence? Besides, where is the difference between producing a whirlwind and producing the *violence* of a whirlwind? Is it contended by Mr. Redfield that there are two causes, one producing wind, the other the force or violence of wind? But is there not a great mistake made by Mr. Redfield and other advocates of the whirlwind theory, in treating gyratory motion as a *cause* of *violence?* Is it not evident, that whatever may be the cause or causes of aerial currents, gyration, instead of accelerating that velocity on which violence is dependent, must, by the expenditure of momentum resulting from collision with inert portions of the atmosphere consequent to centrifugal force, cause a great loss of velocity. (See my objections, par. 65.)

100. I have not been enabled to discover that Prof. Dove attempts to assign any cause for violent winds. Assuming that a wind, sufficiently violent, is blowing from south to north, he ingeniously makes a new application of the old doctrine of Halley, by which the westerly motion of the trade winds is ascribed to

* We had hoped to have published Prof. Dove's Essay, on which Dr. Hare comments, in connection with these 'strictures,' but are obliged from the crowded state of the present number to postpone it to our next.—Eds.

the diversity of the velocity of the earth's surface, at different distances from the equator, operating upon a wind blowing from one parallel of latitude to another. I am however unable to understand how any difference of momentum, thus arising, can act throughout all parts of a circle upon an elastic fluid, so as to sustain the equability of motion requisite to enduring gyration. It seems to me that the influence of the terrestrial motion can operate harmoniously neither upon each quadrant, nor each zone of a circle. The effect upon the south limb cannot, I think, coöperate with that upon the northern one.

101. Moreover, as the velocities of the aeriform particles in a whirlwind must be greater as they are farther from their axis, I do not see how a uniform force operating upon particles requiring such various velocities, can produce movements which can harmonize in causing a non-conflicting rotation of the whole mass.

102. How can this process avail to produce a revolution in the same direction in all the storms of this quarter of the globe, as alleged by Mr. Redfield and sanctioned by the author, when, agreeably to the most ample and satisfactory evidence adduced by Prof. Loomis, as well as general experience, some of the most violent storms of this continent travel from the northwest towards the southeast. In such cases, on account of its blowing obliquely towards or from the equator, the wind would change its position relatively thereto, only with a portion of the speed which is assumed in the calculation of Prof. Dove, and when the change of position should be in an opposite direction from that which he supposes, would it not cause the storm to whirl in the opposite way?

103. Prof. Dove in the second paragraph, page 211, employs the following language: "*As the West India hurricanes originate at the inner boundary of the trade winds, where, at the so called region of calms, the air ascends and flows over the trade in an opposite direction; it is probable that portions of this upper current, penetrating through the lower one, can give the first occasion of those storms. The high mountains of several of the islands, by offering a mechanical impediment, may be one cause of this effect, as the air flows with redoubled violence between two mountains.*"

104. Prof. Dove here alleges that the upper current *may* penetrate the lower, but does not say why it should do so. Where-

fore, it may be demanded, should the upper current penetrate through the lower current, and supposing it to do so, why should it be productive of a hurricane?

105. The professor goes on to say—"*It is evident, that if the above deduction of these phenomena be the true one, a similar whirlwind must be produced wherever, owing to any other mechanical cause, a current flowing towards a high northern latitude is more southerly on its eastern side than on its western.*"

106. It seems as if Prof. Dove, no less than Mr. Redfield, falls into the error of making the *cause of gyration* the only object of inquiry. It is, according to them, sufficient to show that the rotation of the earth, or the reaction of a mountain, may give a curvilinear direction to the wind. To account for the wind itself is not in the least necessary!

107. Can any thing be more inconceivable, than that a current of air, not previously moving with the force of a hurricane, should, by influence of the earth's motion, or a conflict with one or more mountains, be excited into a tempestuous fury? Whence comes the alleged peculiar violence of the whirling portion of the atmosphere noticed in such storms? Evidently *deflection* could not cause any *augmentation* of force. The velocity of the whirl would be less instead of greater than that of the generating gale, since the centrifugal force consequent to rotary motion would be productive of a collision with the surrounding atmosphere, tending to dissipate the momentum. This, as I have already observed, could receive no reinforcement, while the mass actuated by it would increase with the square of the distance from the axis. (See additional objections to Redfield's theory, par. 65.)

108. Prof. Dove has not considered the incompetency of a *local* cause of deflection, to beget permanency of rotation in a *travelling* storm; nor the impossibility of the endurance of a momentum sufficient to cause the violence of hurricanes without continuous exciting forces.

109. In a passage which I shall in the next place quote, the idea is advanced that the axis of a whirlwind may incline forward so as to cause the higher portion to precede the lower, and to make the lower stratum of the air forming the whirl exchange places with the upper stratum. This view of the phenomena I shall endeavor to prove erroneous.

110. Page 215, paragraph 3d, Prof. Dove has thus expressed himself: "*In considering the progressive advance of the whirlwind, we have not hitherto taken into account the resistance opposed to the motion of the air by the surface of the earth. This resistance, as Redfield justly remarks, causes the rotating cylinder to incline forward in the direction of its advance, so that at any station the whirlwind begins in the higher regions of the atmosphere before it is felt at the surface of the earth, where therefore the sinking of the barometer indicates its near approach. The inclined position of the axis causes a continual intermixture of the lower and warmer strata of the air with the upper and colder ones, thereby occasioning heavy falls of rain and proportionably violent electric explosions.*"

111. In order to appreciate the fallacy of the ideas above presented, it should be recollected that the "*rotating cylinder*" of air, which is represented as *inclining forward*, can receive this name only because the portion of the atmosphere of which it is imagined to be formed, is conceived to revolve within that cylindrical space which must of necessity be occupied by a whirlwind. To justify this appellation, the gyrating particles must all move in concentric circles about a common axis, and between places parallel to each other and at right angles to that axis. Any other rotative position of the parts must be inconsistent with enduring rotation, since it would bring different parts of the mass in collision with each other and with the air beyond the sphere of the gyration. It should be recollected, also, that agreeably to observation, hurricanes have been estimated to extend from one hundred to six hundred miles in breadth. Let us assume the diameter of a whirlwind storm of this kind to be three hundred and sixty miles. Of course the circumference being about three times as great would have three miles for every degree. It follows, that a vertical circle of the diameter of the storm, in a plane coinciding with the axis, would also have three miles for every degree. The altitude of storm-winds is well known not to be above two miles, so that the diameter would be at least one hundred and eighty times as great as the altitude of the axis. Can such a cylindrical mass of air be conceived to incline forward? One degree of inclination would lift the base in the rear three miles, and two degrees would lift it to the height of six miles, which is never attained by clouds. Besides, as the

density of the air in regions so elevated is only one half of that upon the earth's surface, is it conceivable that a whirlwind could consist of materials so disproportioned in weight?

112. Can the suggested process of circulation proceed when, in order for the lower stratum to exchange places with the upper, it would have to move nearly half of the circumference of the storm, or more than five hundred miles?

113. It has been shown that the *rotating cylinder* of air which constitutes a hurricane, agreeably to the language employed, and the theory espoused by Prof. Dove, may consistently with the observed dimensions of storms, have a diameter two hundred times as great as its altitude. The base, of this flat cylindrical aeriform mass, must be in contact with the terrestrial surface, and of course in collision with its ruggosities and inequalities, while all the rest of the rotating superficies, being contiguous to inert particles of the atmosphere, must incessantly share with them any received momentum. Is there any known cause of motion in nature, which can impart to a fluid and elastic mass so formed, composed and situated, the various velocities necessary to that simultaneous rotation of the whole which the creation of a whirlwind requires? In answering this question, it should be recollected, that the velocities must diminish from the zones of which the gyration is most rapid, towards the axis on one side, towards the circumference on the other.

114. Evidently no transient impulses can produce harmonious revolution throughout the mass, unless they act upon every particle so as to impart to each the peculiar velocity which its distance from the axis may require; and any enduring cause operating partially, could only affect the whole by a gradual process of participation which would cause it to be expanded beyond as well as within any "*rotating cylinder*" which might be created.

115. But admitting that in such a mass, under such circumstances, the gyratory violence of a hurricane could be induced, could this violence be sustained, after the cessation of the generating forces, merely by the rotatory momentum of an enormous aeriform disk, formed, proportioned, supported, and surrounded as the whirlwind above imagined must be, could any such exist?

116. One of the grounds taken by Prof. Dove, appears to me strikingly untenable. His language, Vol. III, p. 214, last paragraph, is as follows. "*The dead calm, suddenly interrupting*

the fiercest raging of the storm from opposite directions, which is shown in the register of observations at St. Thomas; that dreadful pause which fills the heart of the bravest sailor with awe and fearful expectation, receives a simple explanation on the rotary theory, but appears irreconcilable with the supposition of a centripetal inblowing, because two winds blowing towards each other from opposite directions must gradually neutralize each other, and thus their intensity must diminish more and more in approaching their place of meeting. This takes place on a great scale as respects the trade winds, and if the centripetal view of hurricanes were a just one, the same effect would be necessarily seen as the centre of the storm passed over the station of observation. But the phenomena shewn by observation are widely different. At St. Thomas, the violence of the tempest was constantly increasing up to 7h. 30m. A. M., when a dead calm succeeded, and 8h. 10m. the hurricane recommenced as suddenly as it had intermitted. How can this be reconciled with the meeting of two winds?"

117. I have made the preceding quotation from Prof. Dove's essay, conceiving it to contain evidence which must be fatal to the hypothesis which it is intended to prop. It establishes that in hurricanes the wind is liable *suddenly* to subside from its extreme violence to a calm, and then as suddenly to recommence blowing with as great violence as ever in an opposite direction. I am very much mistaken if I have not in my *additional objections to Redfield's theory* (67) demonstrated that, in *extensive* whirlwinds, the "*fiercest raging*" cannot be suddenly interrupted so as to leave a dead calm during the interval which takes place between two opposite winds; since in such storms, where they have a diameter not less than three hundred miles, for the same station to be exposed successively on opposite sides of the zone, where the wind is most violent, the storm must move at least one hundred miles, which would require from three to four hours.

118. Referring the reader to the essay above mentioned, I will urge, in reply to the query already quoted, that Prof. Dove's allegation that "*winds blowing* from opposite quarters *will neutralize each other*," arises from his forgetting that agreeably to the hypothesis which he is striving to confute, they are caused by a deficiency of pressure at the axis of the storm producing an up-

ward current for the supply of which they are required. It could not but be admitted by the learned professor, that when, under such circumstances, a fluid rushes from all quarters towards a focal area, the consequent motion must quicken as it approaches the ascending current. It must also be clear that when it moves from all parts of the circumference, the velocity must increase inversely as the square of the distance from the centre. Nevertheless, after the base of the ascending column is reached, evidently the horizontal afflux must be superseded by a vertical movement. Hence about the centre of the space around which the upward currents prevail there may be a calm.

119. It seems to be conceded that a tropical hurricane is a gigantic tornado. Of course it may be assumed that the features of these meteors are proportionable; and that the focal area of a hurricane, will be as many times greater than that of a tornado, as the whole area of the former is greater than that of the latter. In fact, if the focal areas be respectively bases of ascending columns moving with equal velocities, the quantity of air requisite to supply the upward currents thus constituted, will be as the squares of the diameters of the columns severally.

120. The diameters of the focal areas of tornadoes as observed in this country, seem in no instance to have exceeded five hundred feet. The focal area of the Providence tornado was estimated to be three hundred feet in diameter.* To supply an upward columnar current of ten thousand feet diameter, would require four hundred times as much wind as to supply an analogous current of a diameter of five hundred feet. It follows that a hurricane, equivalent to four hundred of the largest tornadoes, would not require a focal area greater than two miles in breadth. To cross this at the rate of progression attributed to great storms, by Prof. Dove, (thirty miles per hour,) only four minutes would be necessary.

121. Let us suppose the focal area of a hurricane for the base of a vertical current to be in diameter ten thousand feet, and that the space beyond the area be divided into zones by the circumferences of equidistant circles, concentric with each other and with the focal area. The circumferences being equidistant, the quantity of air over these zones will evidently be as the squares

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* See letter from Z. Allen, Esq. in this Journal, Vol. xxxviii, p. 76.

of their mean diameters. Of course if the zone nearest the area, having a mean diameter of ten thousand five hundred feet, move inwards with the velocity of one hundred miles per hour, the velocity at four times that distance, or forty two thousand feet, cannot be more than one sixteenth as great, or little more than six miles per hour. Thus at only four miles from the centre, the centrifugal velocity would scarcely be adequate to a breeze.

122. This calculation, founded on the idea of the confluence of the air equally from all points of the periphery, would seem too much to contract the theatre of great storms; but in point of fact, it probably never happens that there is a confluence of the wind from all quarters. In the storm of which the phenomena are so well recorded by Prof. Loomis, the wind blew principally from two opposite quadrants. But in either case the influence of the inward suction must diverge and diminish in force as the distance from the focal area increases, so that the greatest violence will be in the vicinity of its border, where the wind is most concentrated. For as soon as the confluent currents get within the border, they must be deflected upwards; and thus the central space must escape their influence; excepting the diminution of pressure consequent to the upward motion.

123. It is I hope thus rendered evident, that the facts adduced, in the quotation above made from Prof. Dove's essay, are quite consistent with the idea of winds rushing towards a focal area, while they are utterly irreconcilable with that in support of which they have been brought forward.

124. I would recommend Prof. Loomis's* observations to the candid attention of Prof. Dove, and would request him to show in what manner the earth's motion coöperated to produce it; or how the enormous length of the focal area, or area of minimum pressure, comparatively with its breadth, can be reconciled with the idea of its having formed the centre of an extensive whirlwind. There is another fact which would seem to be literally an unsurmountable obstacle to the rotation of a storm travelling from the valley of the Mississippi to the Atlantic coast. I allude to the interposition of the Alleghany mountains. Prof. Dove's imaginary aerial cylinder would be cut nearly in twain when bestriding

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* See American Philosophical Transactions for account of the storm of December, 1836.

that range. Obviously more than one half of the air in such a cylindrical mass would be below the average level of the summits of those mountains. Under such circumstances could it be conceived to rotate about a vertical axis?

125. I am aware that various writers have referred to the little *transient* whirls which are occasionally seen to take place in a windy time, carrying up dust, leaves, and other light bodies, as a support for the idea of whirlwind storms; and Mr. Redfield has alleged, "*that no valid reason can be given why larger masses of air may not acquire and develope similar rotative movements.*"

126. It appears to me that there are several valid reasons for not adopting the view of the subject which he has taken. The momentum by which any body is kept in motion, is as its weight multiplied by its velocity, while the expenditure of momentum is *cæteris paribus* as its surface. On this account, a globe of which the content in proportion to its superficies is preëminently great, will, in a resisting medium like the air, retain a rotary motion longer than an equal weight, of the matter forming it, in any other shape. The flat cylinder, in diameter about two hundred times its thickness, of which the existence would be necessary to an extensive whirlwind, is a form of which the surface would be very great in proportion to the quantity of matter which it contains. No observer ever noticed any whirl produced as above described, to have a diameter many times greater than its height, or to endure many minutes. Such pigmy whirls appear to be the consequence of eddies resulting from the conflict with each other, or with various impediments, of puffs or flaws of wind. No doubt in this way a deficit of local density is easily caused in a fluid so elastic as the air, and consequently by gravity as well as its elastic reaction, a centripetal motion is induced in the surrounding aerial particles. From the confluence and conflict of the air thus put into motion, a whirl may arise. The manner in which light bodies are gathered towards the axis of these whirls, shows that they are accompanied by a centripetal tendency. It is only when the wind blows briskly that such whirls are ever seen to take place, but tornadoes agreeably to universal observation occur when there is little or no wind externally. (See objections, par. 76.)

127. According to the evidence adduced by the advocates of the whirlwind theory, there is in this respect perfect similarity

in the phenomena of tornadoes and hurricanes. Beyond the sphere of the alleged gyration, there is but little if any atmospheric commotion, and certainly none competent to be the cause of a great whirlwind. It follows that pigmy whirlwinds and hurricanes can have no analogy. The former are never produced without a proportionable external activity in the wind, while comparative external quiescence seems to accompany the latter.

128. I will conclude by applying to Prof. Dove the stricture which I applied, on a former occasion, to Espy, and to Redfield. He has, I think, committed a great oversight in neglecting to take into consideration the agency of electricity in the generation of storms.

Art. XV.—*Striæ and Furrows of the Polished Rocks of Western New York;* by Prof. C. Dewey.

Two varieties of limestone underlie the grounds of Rochester and its vicinity. The principal one is the calciferous slate of Eaton, the limestone associated with the *Rochester shales* in the geological reports of the State. The other lies upon the preceding over a small extent, forming a stratum a part of a mile wide, and is the geodiferous limestone. Wherever the upper surface of these is found polished, viz. over a considerable portion of several square miles, the polish is marked by striæ and furrows of very variable depth and width and distinctness. These have attracted some attention, and it may perhaps subserve the interests of geology, to give some of their directions and appearances, and refer to the cause of them.

The striæ and furrows are intermingled without any regularity. Sometimes several occur in an inch; at others, few or none in such a space; sometimes a dozen or more large and small in passing over eighteen or twenty inches. They are now very fine, now coarse; now shallow, then deep. Their directions vary considerably; some of them greatly. I have laid the compass upon many in many different localities, and taken the *bearing* with considerable care.

1. On an uncovered portion of the polished surface below the falls of the Genesee in this city, the directions of some are very nearly S. W.; of others, S. 56° W., S. 64° W., S. 22° W., and

S. 34° W. The angle at which these may cross each other, is readily seen. In the distance of a few feet, many of these different directions are parallel to each other. The same is true of the following.

2. At a mile east of these falls, in the same rock, the bearings are generally N. 22° W. and N. 8° E. Of the first direction, there are many, and of the latter there are few.

3. In an excavation made on Main street, the directions are S. 33° W. and S. W., and S. 56° W. I made many observations on these striæ and furrows. These three localities are on the east side of the river, and the first and third are near the banks of the Genesee.

4. At the rapids of the Genesee and near the south line of the city, many striæ are nearly southwest. The rock is here so finely polished, as to reflect light like a mirror, and is often in deep and uneven surfaces.

5. About one hundred rods west of the rapids, and just without the city limits, are a multitude of furrows and striæ. Some are S. W., some S. 22° W., and some are N. 8° E., while one is nearly N. The last shows its beginning and direction, and is the only one I have seen in which the course in which the power acted is evident. The beginning is a broken surface, as if a heavy rock had fallen upon the surface and crushed it in part, and then been moved northwards. This furrow is half an inch wide.

In some other places the directions are from southwest to northeast, or the reverse. In all these cases no allowance was made for the variation of the needle, about 5°. The earth over these surfaces before their exposure was from three or four feet to ten, and even from twenty five to thirty feet. In some instances the directions or bearings of a line changed a degree or two in the course of a few feet.

To R. W. Haskins, Esq. of Buffalo, I am indebted for the bearing of the grooves in the rocks in and about that city. The observations were made by himself and Dr. Hayes at Black Rock,* and in other places he was assisted by Mr. P. Sargeant; all well known to be highly intelligent, careful, and accurate observers. In and about Buffalo they have noticed eight or ten localities of

* See Dr. Hayes's remarks on the striæ in the vicinity of Buffalo, in this Journal, Vol. xxxv, p. 191.

the grooves, some in limestone, and others in shale. The former rocks are more or less polished, and the latter smoothed, while both are grooved. In the corniferous rock of Eaton, at Black Rock, the nodules of flint, being much harder than the limestone, stand up in bold relief, but are regularly smoothed like the other, and the surface gradually slopes on all sides of the nodule to the general level.

1. At Upper Black Rock the course of the grooves "was found by us, allowance having been made for magnetic variation, to be from N. 15° 32′ E. to S. 15° 32′ W." Twelve feet of earth had been removed from this surface.

2. At another quarry one mile and a half north northeast of the last locality, the direction was from N. 28° 12′ E. to S. 28° 12′ W.

3. At five miles southeast of Buffalo, on the banks of Buffalo Creek, in smoothed shale, they found the grooves with the same general direction from the north towards the south. Mr. Haskins makes the same remark on all the localities about the city and in it.

Causes.—From the multitude of bowlders of granite, mica slate, quartz, &c. there can be no doubt that a mighty current has swept from the north, and that these have been worn and rounded considerably by the attrition of the materials in the waters. But it cannot be supposed that such a cause alone could have *polished* the surfaces. It might smooth the rocks, as the current and some attrition now do in beds of the streams; but no polish is produced in that manner. After the surface had been polished, such a current moving along rocks or bowlders, might form the striæ, furrows and grooves.

The *glacial* theory offers a very simple and plausible solution of both the polish and the grooves. The whole process could be accomplished at one and the same time, as Agassiz and others state it now to be done under and by the glaciers in Switzerland.

The *motion* of a glacier by the expansion of the ice formed from the water which passes into the cracks and crevices, is most obvious, and entirely philosophical. One consideration, not adverted to distinctly by the writers on the glacial theory, makes the subject even more palpable. It removes too the necessity of supposing the passage of water into those minute undulating air-cells or cavities in the ice. It is this: *Ice receives its greatest* *expansion at the freezing point of water.* The result of the crystalline arrangement of the particles then takes place. As the cold increases *the ice contracts*, like other bodies in the same condition. Hence it is that *ice cracks* on our rivers, ponds and lakes, as the cold becomes intense or is continued, and those cracks are from half an inch wide to a foot, and even four feet wide on the deep lakes. Hence the cracking of frozen ground when not covered with snow, with such tremendous explosions. Hence too the closing up of those cracks and rents as soon as the temperature rises near the point of congelation of water. The application of this principle is obvious. As the water trickles into the fissures in the ice of a glacier it must be frozen, and thus expanded; as the new ice cools more, contraction takes place, and new fissures or cracks must be the result. This process must be continually repeated in the warmer parts of the year, if the temperature of the ice below is depressed towards zero of Fahrenheit. Thus the heat of the atmosphere provides the trickling water, and congelation produces the motion by the expansion, and the cracks or fissures are renewed by the subsequent contraction. The expansion of the ice in different directions as it met with less obstacles would give striæ and grooves of various bearings. It is not certain the glaciers ever existed in this section of the country; it is certain that there was a rush of waters from the north.

The glacial hypothesis will not account for the position of the bowlders in many places. From the Alps a glacier may have transported the granite bowlders to a lower situation upon Mount Jura. But in our country we often find intervening hills higher than the source of the bowlders. The region south of Lake Ontario, which is covered with the sandstone bowlders from the shore of that lake, or a little south of the shore, is higher than the position of the sandstone. In Berkshire County, Mass., bowlders of graywacke, evidently removed from the hills of that rock in the adjoining part of the state of New York, are scattered through the valley of the Housatonic. But between the hills of graywacke and that valley, lies the Taconic range of mountains, along the boundary between the two states, and the latter are every where several hundred feet higher than the former. If the bowlders were once lodged on a glacier, the ice and bowlders must have been transported by a flood of waters over the

Taconic mountains. No glacier alone could have removed them to their present places, unless the Taconic range has since been elevated, or the graywacke hills have been depressed; and the evidence of either supposition is not to be seen in the dislocated appearance of the strata.

In Richmond, in Berkshire County, Dr. Reed has lately called my attention to a long line of bowlders of serpentine, stretching from the north part of Stockbridge across the valley of Richmond, and up a ravine on the Taconic range, and over that range into the valley beyond in the state of New York. Many of these are large bowlders, from twenty to fifty feet long, and twelve to twenty and forty feet wide, and eight to twelve feet thick. Of course, these bowlders show little appearance of having been rounded on their edges by attrition. Dr. Reed pointed me to one bowlder, fifty feet by forty, and twelve feet thick in some parts, and equivalent to forty feet square by ten feet deep, and containing near one thousand three hundred tons of two thousand pounds each. Several contain nearly one fourth as many tons. These bowlders occupy a space of only a few rods in width, while the graywacke bowlders are spread widely over the valley. The large bowlder is on the west side of the road leading from the church northwards, and some more than half a mile west of north from the church, on the eastern declivity of the Taconic range. These bowlders would seem to require, from their position and appearance, the combined machinery of large bodies of ice and of a mighty flow of waters from the west of north. It is only two or three miles north of the ravine just mentioned, that Prof. Hitchcock pointed out the furrows worn in the rocks by attrition and the mighty northern current, and at an elevation as high as that of the ravine.

Glaciers or icebergs and the strong current of waters, a union of the two powerful causes, probably offers the least objectionable solution of these wonderful changes, the polishing and furrowing of the rocks, and the transportation of bowlders. Indeed, the polishing might be done by great masses of floating ice in their mighty pressure upon the earth and gravel and stones upon the surface. If the motion of the glacier now polishes the rocks beneath, or even only smooths them, the motion of massive ice would effect the same, and also cut the striæ and grooves and furrows.

Rochester, N. Y., Sept. 17, 1842.

ART. XVI.—*Abstract of a Meteorological Register kept at Singapore, Lat.* 1° 20′ *North, Long.* 103° 52′ *East, from Nov.* 1, 1839, *to Feb.* 28, 1841, *with Notices of the Productions, &c. of the Island;* by JOSEPH S. TRAVELLI.

TO THE EDITORS.

Gentlemen—Herewith, in accordance with your request, I send an abstract of the meteorological register kept by me during the latter part of my residence in Singapore, together with some desultory remarks on the climate and on the state of agriculture on the island.

DATE.	THERMOMETER.					RAIN.					
	Highest of the month.	Lowest of the month.	Mean daily range.	Mean temp. from the highest and lowest of each day.	Mean from observations at 10 o'clock A. M. and 10 P. M.	Depth of rain, in inches.	Days on which it rained.	Hours of rain.	Evaporation.	Hours of sunshine.	Peals of thunder.
1839, Novem.,	87.30	73.00		78.72	78.76	11.17	21	62		123	67
" Decem.,	87.50	73.40	9.87	78.99	78.78	8.50	22	72	2.10	130	23
1840, January,	87.80	71.50	10.70	79.10	78.56	10.66	16	47	2.20	150	38
" Feb'ary,	87.20	71.50	11.59	78.99	78.84	6.45	23	45	2.60	178	180
" March,	88.60	73.00	10.60	79.84	80.60	6.64	15	25	2.35	207	103
" April,	89.20	73.70	9.87	80.33	80.31	10.37	18	61	2.30	198	171
" May,	90.00	73.00	9.77	80.76	81.01	5.16	14	36	3.20	221	74
" June,	88.00	72.00	7.96	81.16	81.34	7.97	9	43	3.45	211	18
" July,	88.70	72.00	9.79	80.86	81.40	5.56	10	20	4.75	228	42
" August,	88.00	71.80	8.34	79.59	80.54	8.66	13	46	3.85	174	72
" Septem.,	89.00	72.40	8.65	79.88	79.60	8.00	12	31	4.00	147	19
" October,	88.60	72.00	9.44	80.46	80.30	8.80	15	36	4.00	162	167
" Novem.,	86.00	73.00	7.75	81.12	78.54	7.27	16	47	3.40	79	3
" Decem.,	86.30	71.80	8.63	78.55	79.76	7.82	18	57	2.90	99	163
1841, January,	87.80	71.00	9.85	77.81	78.04	7.29	13				
" Feb'ary,	88.00	68.70	11.30	79.80	80.53	2.50	5	6		152	2

The observations were made at the mission school of the American Board of Foreign Missions, at Ryan's Hill, about half a mile from the town. The hill is about one hundred or one hundred and twenty feet above the level of the sea. The register from which the abstract is made, includes hourly observations, from half an hour or more before sunrise to ten o'clock, P. M., with occasional observations through the night, for almost every day of the sixteen months mentioned in the abstract. These details, however, would hardly suit your Journal, and I have therefore limited the abstract to the items of more general interest.

The statement as to the hours of rain, and the hours of sunshine, are of course mere approximations. In regard to the thunder, the difficulty is still greater. Some would understand "a peal of thunder" one way, and some another. My object was to record the number of times it thundered, without reference to the loudness of the report. In many instances I should, perhaps, have conveyed my impressions better by calling them a series of rumblings rather than distinct peals. We very seldom had what would be called in the United States a heavy thunder storm. In regard to the hours of rain and sunshine, and peals of thunder, the register pretends to nothing more than *guessing.* And yet to some, on these points, the mere *guess* of a person on the spot, recorded from day to day, for several months, may be interesting. The thermometer was placed in an open veranda, to which the external air had free access at all times. The cup for evaporation was suspended by the side of the thermometer.

The observations extend from Nov. 1, 1839, to Feb. 28, 1841. Taking the year 1840 by itself, the register gives the following results for the year.

Mean temperature, ...	80.09° Fah.
Highest "	90.00
Lowest "	71.50
Mean daily range, ...	9.34
Whole range, ...	18.50
Rain, (of which $\frac{5}{12}$ fell between 6 and 12 A. M.)	93.36 inches.
Evaporation, ...	39.00 "
Days on which it rained, ...	189
Hours of rain, ...	494
Hours of sunshine, ...	2054*
Rumblings or peals (?) of thunder, . .	1050

The climate of Singapore is an exceedingly pleasant one. The frequent rains throughout the whole course of the year, together with the constant sea breezes, free it in a great measure from the sultry heat of most tropical countries. The general distribution of rain throughout the year, has the happiest effect on vegetation, keeping it constantly fresh, and green. There is no rainy season

* Making an average of nearly six hours of sunshine for each day in the year.

in the ordinary sense of the word, although there is a manifest difference between the months corresponding to our winter and summer months, the winter months being more wet and the summer months more dry than at other times.

It is now about twenty three years since the island was settled by the British, and the population already amounts to about forty thousand. This population is composed chiefly of Malays and Chinese. The settlement is in every respect in a thriving condition. New roads are making around the island and into the interior. New streets are opening and new buildings going up in all parts of the town. In some directions as you ride along, you might imagine yourself in one of our new western settlements, from the vigor with which the agriculturalist is clearing and cultivating the land.

The investment of capital in agriculture, by Europeans, was retarded for a long time by the uncertainty which existed as to the terms on which the government would sell or let out the land. But this difficulty has been removed to a great extent within two or three years, by the adoption of definite regulations on the subject, putting the land at low rents on long leases. None of the land is sold by the government, in fee simple, but is let out on leases for a term of years.

Nutmegs, coffee and sugar are the principal products of the plantations of Europeans, while gambir, pepper, siri, betel-nut, and vegetables, are the products of the native farms. The clove was formerly cultivated with the nutmeg, but it is not much attended to now. It is regarded as more troublesome, less productive for the time being, and less durable than the nutmeg.

The *nutmeg* is usually planted on the hills or rising grounds in the vicinity of the town. Being a handsome tree, and usually set out with great regularity and carefully cultivated, it is continually adding to the beauty of the place. The trees, if well attended to, usually begin to bear fruit pretty well by the seventh or eighth year, and are said to go on increasing in value for many years. After the trees begin to bear well, say by the tenth year, they are said to yield a very large return on the capital invested. It is also said that should the price of nutmegs in commerce diminish one fourth, or even one third, that they would still afford a fair return. The trees are generally set out from twenty to thirty feet apart in the quincunx order, with the view of having one male tree in the centre of the largest number of female trees. It is not usual to set the trees out in this way, until they are from three to five or even six years old. Previous to this they remain in the nursery. The nuts are planted in the nursery from six to twelve inches apart. When the plants are about two years old, they are usually transplanted into another nursery, where they remain until they are set out as above mentioned. The young trees however, are said to be improved by transplanting, and some accordingly transplant them once a year before finally setting them out. When the nuts are first planted, they must be separated from the thick hull or rind which envelopes them, and they must be planted within twenty-four hours from the time they are taken from the tree. It is usually six weeks or two months before they come up. Some persons prefer keeping the trees in the nursery until they have blossomed, and thus shown their sex, previous to finally setting them out. When they are set out before they have shown their sex, it is always at the risk of a considerable amount of labor and expense for nothing, as the number of male and female trees is about equal, and as among the young trees there is no way of distinguishing the sex until they put forth their blossoms, which seldom appear before the fifth year.

The holes into which the trees are set at last, are usually prepared several months before the time of transplanting. Each hole is about five feet deep, and about five in diameter. This hole is filled with vegetable matter, top soil, and manure. As the mass settles down it is refilled in a similar manner. A sort of shed about seven feet high, open on two sides, is then built over each hole, and is covered on the top and on the two sides with a sort of thatch made of the leaves of the Nipah palm, (*Nipah fruticans*, Thunberg.) This shed lasts two or three years. By this time the tree has attained a sufficient size, and is left without any further protection. The tree grows slowly, but attains in time quite a large size. Its size and productiveness depend chiefly on the mode of cultivation. No grass or weeds are to be left about the roots, and suckers are to be removed. Every two or three years the tree is to be thoroughly manured. This is done by making a trench about a foot or eighteen inches deep, and as many in width, entirely around the tree, and corresponding in distance from the trunk with the outermost branches. This trench is filled with grass, top soil, manure, &c. as before. The object is to get the manure near to the ends of the roots, and this is effectually accomplished by this plan. One might think that the tree would be injured by wounding the roots, but constant experience shows that nothing tends so much to the health and vigor of the tree as this manuring. When digging the trench, in case they discover signs of disease in the root they do not hesitate to cut in to within a foot of the trunk even of a large tree, cutting off in so doing, roots two or three inches in diameter. This is frequently done also in getting out the roots of a species of grass which is the pest of the agriculturist in that part of the world. It is called by the Malays "sallang," (*Andropogon caricosum*, Lin.) It is a long grass, sometimes four feet or more high. It frequently covers whole fields, rendering them comparatively valueless from the difficulty and expense which it would cost to get out this grass. In the rich beds of the nutmeg it grows with great rapidity and must therefore be got out at all hazards. Its tenacity of life is so great that there is no security against its growing again, but by burning every piece of the root. Exposure of the root to the sun for months seems not to destroy its vitality entirely. And I have known it to come up through a layer of earth and gravel four feet deep, thrown out in digging a a cellar.

The foliage of the nutmeg being thick and of a dark rich green color, it is quite an ornamental tree. The brilliant colors of the ripe fruit, as the bright scarlet of the mace and the glossy black of the shell under the mace are seen through the bursting rind which evinces the maturity of the fruit, make it one of the most beautiful products of the vegetable world.

There is no particular season for gathering the fruit, though I have seen the contrary stated in some works on botany. The usual practice is to go around the plantation every day to examine each tree in quest of ripe nuts. Almost every good tree will exhibit more or less of ripe or unripe nuts, as well as buds and blossoms, nearly every day of the year. It is nevertheless true, that at certain times particular trees will bear more than at other times, and even particular plantations more than other plantations in the same neighborhood, but as far as I could learn from observation or by enquiring of those practically engaged in the business, there is no such thing as a nutmeg season.

Coffee. This has not been cultivated to any extent in Singapore until quite recently. As the nutmeg trees require so much room when they attain their full size, and as for some fifteen or eighteen years there is a large space left between the rows, they frequently plant coffee trees between the rows of nutmeg trees. As the coffee trees die out in the course of fifteen years, they do not interfere with the nutmegs, and yet occupy the ground to good advantage in the mean time. They are seldom over eight feet high. The plants are raised in the nursery, and are set out in holes, somewhat as described in reference to nutmegs, except that the holes and sheds are very small.

Sugar. The sugar cane has long been grown in the island in its natural state, for the use of the natives. But the cultivation of it on an extensive scale for the purpose of manufacturing sugar was introduced by our enterprising, polite and hospitable consul, J. Balestier, Esq. Mr. B. after having overcome a great variety of obstacles is now succeeding well. He obtained some time ago a gold medal from the Agricultural Society of India for the best sugar manufactured in the British East India possessions. His success is stimulating others to engage in the same business.

Of the products of native cultivation, pepper and gambir are the chief. The two are usually cultivated together, as the leaves of the gambir plant are supposed to form a very good manure for the pepper vines.

Pepper (*Piper nigrum*) is usually planted three or four feet apart, with stout rough rails, stuck into the ground, and eight or nine feet high, on which the vines climb. The pepper gardens resemble patches of hops, except that the foliage is thicker and of a darker green.

Gambir appears to be the produce of two different plants, one one of them a climbing plant, (*Nauclea gambir,*) and the other a shrubby plant. That from which this article is obtained in Singapore, is a shrubby plant from five to six feet high. The leaves and twigs are broken off and boiled in water in a large kettle or boiler until their strength is extracted, when the leaves are taken out and strewed as manure around the pepper and siri vines. The extract is then evaporated until it becomes like a thick stiff jelly. It is, probably, sometimes thickened also with earth and sago. While in this jelly state it is cut into cubes of about an inch, or a little more. It is sometimes called catechu, and also

Terra Japonica, in the books, and is also sometimes confounded with kino. Some of the books speak of kino and gambir as the product of the *Nauclea gambir*, which is doubtless one of the plants above alluded to. This article is used very extensively throughout India, and the Malay islands, as an ingredient in the masticatory or *quid*, in which the natives of all ranks indulge. This masticatory consists of gambir, betel-nut, tobacco, shell-lime and siri leaf, a green, pungent leaf in which the other ingredients are wrapped. Gambir has latterly become a considerable article of export to England, where it is used extensively for tanning purposes, on account of the large quantity of tannin which it contains. One pound of gambir is said to be equivalent, for tanning leather, to seven or eight pounds of oak bark. It is usually sold in Singapore at $2 50 to $2 75 per picul of one hundred and thirty three pounds.

Siri (*Piper betel*) is cultivated in gardens in the same way with pepper. It is a vine, belonging to the pepper family and resembling the *Piper nigrum*. It is cultivated altogether for the use of the natives. The leaf is the part principally used, although the fruit, somewhat resembling the spike or ear on which the pepper grows, is sometimes chewed by the natives, as a part of their *quid*.

Vegetables.—Of such as are found in the United States, the following are to be found in the Singapore market every day. Radishes, lettuce, pumpkins, squashes, cucumbers, egg-plants, (*Solanum melongena*, L.) sweet potatoes, onions, celery, string beans. Watermelons and musk-melons are also frequently to be had, but of a very inferior quality.

The chief native laborers by whom these articles are raised, are the Chinese. There are a large number of emigrants from China, arriving every year. The government encourage their going into the jungle as it is termed, and "squatting," and they will not allow them to be ousted without an equivalent for any improvements or clearings they may have made. As they pay no rent, and as there is plenty of excellent land to be found, and as they find ready sale for whatever thay can raise, they generally do well, especially as they are proverbially industrious.

Allegheny City, Pa., March 10, 1842.

Art. XVII.—*Abstract of the Proceedings of the Twelfth Meeting of the British Association for the Advancement of Science. Condensed from the Report in the London Athenæum.*

Continued from Vol. xliii, p. 376.

Sir J. Herschel stated, that the committee for *revising the nomenclature of the stars*, deferred reporting till the catalogue of stars, now in preparation under the auspices of the Astronomical Society, was ready for publication; and that the committee for the *reduction of meteorological observations*, in consequence of the illness of Mr. Birt, had been unable to make any considerable progress.

Sir D. Brewster gave a provisional report respecting the erection of one of *Mr. Osler's anemometers*, at Inverness. He also made a provisional report on the *hourly series of meteorological observations* made at Inverness from Nov. 1, 1840, to Nov. 1, 1841. The mean temperature of Inverness for the summer months was 52°.258; the mean temperature of the winter months, 40°.287; and the mean temperature of the whole year, 46°.272. This mean temperature occurred at 8h. 33m. A. M. and 7h. 42m., the critical interval being 11h. 9m., differing only a few minutes from the result obtained by similar hourly observations made at Leith. The observations made with the barometer, when reduced to the level of the sea, and to the temperature of 60°, indicate very distinctly the daily variation, with its two maxima and minima. The mean annual average of all the observations was 29.680 inches. The monthly mean indicated a *maximum* in December and in June, and a *minimum* in March and October.

Prof. Phillips called attention to the remarkable character of the curves of barometric oscillation which appeared derivable from these observations. As he understood the statement, the hours of maximum and minimum were found to be very different from, and even in contrast with those at which the oscillations occurred further south, as for example, about London. He pointed out the support which these observations lend to the formulæ of barometric oscillations in various latitudes and at different elevations, given in the Edinburgh Philosophical Transactions.

Mr. Scott Russell presented a report on the *abnormal tides of the Frith of Forth*, containing the results of his very elaborate observations. He also communicated a supplement to the former Report of the *Committee on Waves.* He divided waves into three great orders, obeying different laws. 1. Wave of the first order,—the wave of translation,—solitary, progressive, depending chiefly on the depth of the fluid: of two species, positive and negative. 2. Waves of the second order,—oscillatory waves,—gregarious: the time of oscillation depending on the amplitude of the wave. 3. Waves of the third order: capillary waves,—gregarious. The oscillations of the superficial film of a fluid, under the influence of the capillary forces, extending to a very minute depth: short in duration: of two species,—free and constrained. The various phenomena of these classes of waves had been thoroughly examined by the committee, and many interesting results developed.

*On a very curious fact connected with Photography, discovered by M. Möser, of Königsberg,** communicated by Prof. Bessel to Sir D. Brewster. The following are the general facts connected with the discovery. A black plate of horn or agate is placed below a polished surface of silver at the distance of one twentieth of an inch, and remains there for ten minutes. The surface of the silver receives an impression of the figure, writing or crest, which may be cut upon the agate or horn. The figures do not appear on the silver at the expiration of the ten minutes, but are rendered visible by exposing the silver plate to vapor, either of amber, water, mercury, or any other fluid. He (Sir D. Brewster) had heard Prof. Bessel say, that the vapors of different fluids were analogous to the different colored rays of the spectrum; that the different fluids had different effects, corresponding to those of the spectrum; and that they could, in consequence of such correspondence, produce a red, blue, or violet color. The image of the camera obscura might be projected on any surface —glass, silver, or the smooth leather cover of a book—without any previous preparation; and the effects would be the same as those produced on a silver plate covered with iodine.

This paper gave rise to an animated conversation, in the course of which M. Bessel said that he had seen some of the pictures taken by this process, which were nearly, but not quite, as good

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* See Dr. Draper's letter of reclamation of this discovery, at page 203 of this number.

as those obtained by Mr. Talbot's process. Sir D. Brewster said, this was the germ of one of the most extraordinary discoveries of modern days; by it there seemed to be some thermal effect which became fixed in the black substance; and not only so, but M. Bessel informed him, that different lights seemed to affect different vapors variously, so that there seemed to be something like a power of rendering light latent; a circumstance, which if it turned out so, would open up very new and curious conceptions of the physical nature of light: on the emission theory, it would be easy to account for this;—on the undulatory theory, he could not conceive how it could be possible.

Prof. Bessel, of Königsberg, made a communication on *the Astronomical Clock.* Having ever been of opinion, that this indispensable instrument to the astronomer, the transit clock, could only acquire perfection, if the pendulum, (separated from the works,) were made to vibrate in equal time, whatever the temperature and the arc might be: he would submit, whether the expeditious method of coincidences might not be employed for checking the pendulum in both respects. The pendulum, apart from the clock, being suspended from the wall; a clock, taken out of its case, might be placed before it, at a distance of six or eight feet; an object-glass of three or four feet focal length might be placed between both, so as to produce, exactly at the lower end of the pendulum of the clock, an image of the lower end of the other pendulum. Then the coincidences of both might be accurately observed by a telescope placed at a convenient distance. Similar contrivances had been described in an account of some pendulum experiments made at Königsberg: and the accuracy of the method was such, that the relative rate of both pendulums might be ascertained with sufficient accuracy in a very short time,—in from ten to twenty minutes. The rate of the pendulum was to be tried at different temperatures, being placed in a box, having an opening at the lower end, covered with glass, and so fastened to the wall, that the pendulum could swing within it. In the construction of the pendulum, attention should be paid to one thing, which seemed to have been much overlooked. It often happened that thermometers affixed to the top and bottom of a clock case did not agree, whence it was evident that the compensation, acting only below, would not compensate for the variation of the whole rod. He should prefer, on this account, the gridiron to the mercurial pendulum, especially if the rods began as low as possible below the point of suspension, and were carried on to the centre of gravity of the lens. He should prefer the several rods to be of equal diameter, and to be coated uniformly. Supposing the spring perfectly regulated, as well with respect to heat as the arc, only one cause would interfere with regular vibration-times. This was the effect of that part of the elasticity of the air which depends on the variation of the height of the barometer:—the other part, depending upon the variations of the thermometer, is comprised in the adjustment for the compensation for heat. There was a possibility of compensating the former, by fastening a barometer tube to the pendulum, and it would not be difficult to find the suitable diameter of the tube: but this complication of the pendulum would be rather inconvenient. At all events, the variations of the barometer were not very great, especially if the specific gravity of the pendulum be made as great as possible.

Application of the principle of the Vernier to the subdividing of Time, by Mr. F. Osler. A pendulum is to be used which should make ten swings in the time that the principal pendulum makes eleven, furnished with a small dial, and so placed that the coincidences or want of coincidence could be observed. The strokes of such a pendulum being counted, the time of every observed stroke of it, reckoned back from its coincidence with the principal, or seconds pendulum, would, it is obvious, be found in tenths of a second.

Mr. Dent reported on his *chronometrical experiment to determine the difference of meridians* between Greenwich and Devonport. He also reported respecting his *steel Balance Spring coated with pure gold* by the electro-metallurgic process; also of the performance of his clock, in which the impulse is given to the pendulum at or near the centre of percussion. By this contrivance he purposed to obviate the difficulty occasioned by the freezing of the oil at low temperatures. The stopping of clocks at very low temperatures had induced the Astronomer Royal to invent a *new* escapement which seemed to answer all the conditions required; an addition of twelve pounds could be added on to the weight of the clock, and yet a variation was produced in the arc of vibration amounting to only five minutes, while an addition of one pound to the weight of the ordinary Graham's escapement, made a difference of fifteen minutes. By Mr. Airy's plan, there was always (if the term may be used) an extra reservoir of force; keeping the train of wheels always up to their work, and capable of overcoming the resistance occasioned by the freezing of the oil. Mr. Dent then explained the principle of his patent Compensation-balance. Mr. Frodsham made some remarks on the compensation balance of chronometers, and explained a new balance of his invention. Sir Thomas Brisbane said, that praise was due to Mr. Dent as the first maker who had exerted himself to determine the difference of meridians by chronometers. He had shown that by chronometers the difference of longitude could be had with as much certainty as by any other method in use, and at an expense bearing no proportion to that of rockets, or any other means hitherto adopted. Dr. Robinson, of Armagh, was at present engaged in a series of rocket observations in Ireland. It had been the intention of Dr. R. to connect the Irish with the Scotch observatories, and for that purpose a large deposit of rockets had been obtained from government, and stood in Dumbarton Castle; but unfortunately the unfavorable weather in spring had prevented the execution of the design, and he had received a letter within a few days from Dr. Robinson, stating that the strong twilights of the present season would make it requisite to postpone the work until autumn: these facts would at once show the superior economy and saving of time to be attained by adopting Mr. Dent's suggestion of chronometrical observations. Mr. Holden inquired, why the method of moon-culminating stars, which was so simple and easy of application, was not preferred to any other in determining longitudes. Sir Thos. Brisbane replied, that not to say nothing of the heavier amount of labor required in such observations, he need only, in order to show the superiority of Mr. Dent's method, state the fact, that in a late attempt to connect the Royal Observatories of London and Paris, backed by all the instrumental accuracy and unrivalled skill of the observers at these two distinguished observatories, three hundred observations on moon culminating stars had given a mean deviating no less than thirty seconds from the truth. The President observed, that although the method of moon-culminating stars had in theory promised considerable accuracy in the determinations of longitudes, yet from some unexplained difficulties, it had in practice fallen far below the estimate that had been formed of it.

Sir D. Brewster made several communications. 1. On Luminous Lines in certain flames corresponding to the defective lines in the Sun's light. 2. On the structure of a part of the Solar Spectrum hitherto unexamined. 3. On the luminous bands in the Spectra of various flames. I. After noticing Fraunhofer's beautiful discovery as to the phenomena of the line D in the prismatic spectrum, Sir David said, he had received from the establishment of that eminent man, at Munich, a splendid prism, made for the British Association, and one of the largest perhaps ever made; and upon examining by it the spectrum of deflagrating nitre, he was surprised to find the red ray, discovered by Mr. Fox Talbot, accompanied by several other rays, and that this extreme red ray occupied the exact place of the line A in Fraunhofer's spectrum, and equally surprised to see a luminous line, corresponding with the line B of Fraunhofer. In fact all the black lines of Fraunhofer were depicted in the spectrum in brilliant red light. The lines A and B turned out in the spectrum of deflagrating nitre to be both double lines; and upon examining a solar spectrum under favorable circumstances, he found bands corresponding to these double lines. He had looked with great anxiety to see if there was any thing analogous in other flames, and it would appear that this was a property which belonged to almost every other flame. II. He had, by means of the prism from Munich, been enabled to extend the solar spectrum beyond the point where, according to Fraunhofer, it terminated immediately at the side of the line A, and he (Sir David) found one part to consist of about sixteen lines, placed so near to each other, that it was difficult to recognize the separation; but the lines as they approached to A were much nearer to each other than as they receded from it; consequently, that portion of the spectrum appeared concave, resembling so much the scooped-out lines of a moulding on wood, that it was scarcely possible to suppose that the beholder was not looking at such a moulding. He was led to observe an analogous structure near the line B; and upon carrying on this comparison of structure of one part of the spectrum with that of another, it seemed to him, that by and by, something important would result: for there was a repetition of a group of lines, and similar lines, through different parts of the spectrum, as if the same cause which produced them in one part, produced them in another. III. He had endeavored to procure all the minerals and artificial salts and other substances capable of combustion which could be had; and in order to have a suitable combination, he used an oxygen light analogous to the Bude light. Every one conducting these experiments was aware that it was necessary to pass the light through a narrow aperture; but this would reduce the intensity of the light so much, as to make it difficult to observe the rays at the extremity of the spectrum; but he found that he could obtain the effect of a small aperture by merely inclining the prism; so that with a good prism, the great lines in the solar spectrum might be seen by using an aperture three or four feet wide, the whole breadth of the window, by the mere inclination of the prism, which had the effect of producing a narrowing, facing the light. He had obtained two hundred or three hundred results which he had not had any leisure to group; but he would mention some of the general results. When nitrate of lead was thrown into combustion, remarkably fine lines were produced in the spectrum. The luminous line D of Fraunhofer, existed in almost every substance, especially in all into which soda entered, particularly in the flame of a common tallow candle: probably owing to the muriate of soda existing in the tallow. The hydrate of strontia and the iodide of mercury gave the lines very remarkably in yellow and green. Also in that remarkable substance, the lithoxanthemate of ammonia, first discovered and published by Mr. Fox Talbot, the fine lines were seen throughout the whole length of the spectrum; and there was a remarkable blue band, which he (Sir D.) had not distinctly recognized in any other flame. Indigo gave fine green and orange lines at equal distances from the D of Fraunhofer. Prussian blue did the same: calomel, nitrate of magnesia, litharge, also showed lines; the sulphocyanite of potash gave a violet and orange flame, with the lines extremely distinct. He hoped at the next year's meeting of the Association to be able to embody these various results in a regular report.

Sir D. Brewster made a communication on a *New Property of the rays of the Spectrum, with observations on the explanation of it given by the Astronomer Royal, on the principles of the Undulatory Theory.* If we cover half the pupil of the eye with a thin plate of any transparent body, and thus view a prismatic spectrum, so that the rays which pass by the plate interfere with those that pass through it, the spectrum is seen crossed with beautiful black and nearly equidistant bands, whose breadth, generally speaking, increased with the thinness of the plate. If the edge dividing the ray were directed to the red end of the spectrum, then fringes were seen: but no such fringes appeared when it was turned to the violet end of the spectrum. One peculiarity of these fringes, not before noticed, was that they had not the forms of bands, but rather the appearance of screws, or dotted black lines, or as if they were formed by the shadow of a plate of metal perforated by small openings. This, which appeared to be a new property of light, and to indicate a polarity in the simple rays of light, when separated from each other by refraction, he had commented on at the meetings of the Association at Liverpool and Bristol: and Mr. Airy, the Astronomer Royal, had given a paper and two publications on the subject, in which he endeavored to account for this upon the undulatory theory, arguing that the apppearance and magnitude of the fringe depended upon the diameter of the pupil or of the object-glass. Sir D. Brewster said, he had repeated all his experiments under every variety of form, varying the diameter of the pupil from its greatest expansion to its greatest contraction, and the diameter of the object-glass from four inches to a quarter of an inch, and the fringe remained utterly unaffected by these variations. He further found, that these fringes varied in magnitude with the distance of the eye from the refracting body, and not with the magnitude of the pupil. He stated several other results, all of which, he thought, could not be explained on the principles of the undulatory theory. This paper gave rise to much discussion; the prevailing opinion appearing to be that future discoveries would probably reconcile the phenomena in question with the wave-theory of light.

Sir D. Brewster made a communication on the *Dichroism of the Palladio-chlorides of Potassium and Ammonium.* Dr. Wollaston had found that a long crystal of either of these salts, when looked through transversely, had a green color, but when looked through from either end, had a red color; and he (Sir David) placed one of these long crystals transversely over another, in a cruciform shape, and then found that those portions of the centres of both, which were in contact, gave a red color, while all the ends of the crystals were red.

On the Geometric forms, and laws of illumination of the spaces which receive the solar rays, transmitted through quadrangular apertures; by Sir D. Brewster. His attention was called to this subject by an accidental discussion on the point whether or not Aristotle, in explaining the circularity of images formed by quadrilateral apertures, employed the appropriate idea when he said that those images are to a certain extent quadrilateral, but appear circular, because the eye is unable to recognize faint impressions of light. Prof. Whewell, in his History of the Inductive Sciences, had distinctly stated that Aristotle had not used the appropriate idea, and that the question was entirely a geometrical one, the appropriate idea being the rectilinear nature of light. Having been accidentally led to consider the subject, he (Sir David) had determined in a simple manner, the form of the aperture at all distances, had been led to take the same view of the subject with Aristotle, who seemed to have employed the appropriate idea.

Papers by Sir D. Brewster were also read on *Crystalline Reflection;* and on *the Existence of a New Neutral Point and two Secondary Neutral Points.*

On the improvement of the Telescope; by Mr. Fox Talbot. Mr. T. said that this subject occurred to him about two years ago, when the Earl of Ross (then Lord Oxmantoun) was making much larger specula for reflecting telescopes than had ever been obtained before: and he thought, if once we had a very large and perfect speculum, it might be possible to multiply copies of it by galvanic means. He had observed, that if an electrotype cast were taken from a perfectly polished surface, the cast was also perfectly polished; so that no defect of form from this cause could have an injurious effect on the speculum. The great and obvious defect was, that electrotypes were in copper, which reflects but little light. He mentioned these ideas to Professor Wheatstone, who said the same had occurred to him: and he showed him a paper which he had drawn up some few months before, and in which he suggested the taking galvano-plastic casts of specula in platina, palladium, silver or nickel, and for especial purposes gilding the copper; taking care that the two precipitations adhered well to each other. So that, said Mr. T., the idea had suggested itself independently to both of them; but on comparing notes, they found differences. Though it had occurred to Mr. T. to precipitate white metals, yet he did not think that platina would have a sufficiently beautiful white metallic polish. Prof. Wheatstone had, however, made choice of platina;

and varying the quantity till he found the required proportion, he obtained a mirror in platina, which appeared to have quite brilliant polish enough, and to be white enough to answer the purpose; and he considered, therefore, that Prof. W. had proved that at least in one form the specula of telescopes might be made by voltaic precipitation. His own idea was, that it might be possible to whiten the surface of the copper without injuring the form; and therefore, having obtained a speculum in very bright polish, he (Mr. T) whitened it and transformed it into sulphuret of copper: and after having retained it about a year, he did not perceive the smallest alteration in any respect. This therefore appeared to him a mode by which important results for astronomers could be obtained. For the last year, perhaps, nothing further had been done, either by Prof. Wheatstone or himself; but the other day, being at Munich, he (Mr. T.) visited Prof. Steinheil, who showed him his inventions, and told him he had discovered a method of making specula by the electrotype. It so happened, that Prof. S. and himself had published their respective methods about a month or six weeks before: the Professor having read a communication on the subject before the Academy of Sciences at Munich, and printed it, and he (Mr. T.) having published his in England. Their modes were, however, different, as Prof. S. precipitated gold upon the speculum of copper; and having precipitated a certain thickness of gold, he then precipitated copper on the back of the gold, to give it sufficient thickness. He (Mr. T.) should have thought beforehand that gold would not reflect light enough to be available; but Prof. S. informed him he had found, by careful experiment, that it reflected more light than polished steel. He allowed Mr. T. to look through a Gregorian reflecting telescope, of which the speculum was a common one but gilded, and he found, although a slight tinge of yellow was thrown over all the objects, the image was perfectly clear and well defined. Prof. S. said that in the course of the year, he should have a very large telescope, furnished not only with a speculum, but also with other apparatus, voltaically formed, so that telescopes might all be made from a good model, so as to insure greater accuracy of proportions: and in this way even very large telescopes might be constructed at a comparatively trifling expense. With reference to precipitating copper on the back of the gold, the Professor had a simple expedient for securing adhesion. He first precipitated gold from the cyanide of gold, and he mixed with it cyanide of copper, and kept gradually increasing the quantity of the latter sort; so that an alloy was precipitated, which was continually increasing the copper with respect to the gold, till he had a speculum whose surface was gold, and which then became an alloy, the quality decreasing, till at the bottom it became pure copper. This was important: because, without such experiments, one would not have known that such results would have followed; for some philosophers supposed, that if we attempt to precipitate the salts of two metals, only one was precipitated; but Prof. S. informed him that they precipitated in union. He thus obtained a speculum with a face of gold and a back of copper. But supposing the largest, cheapest, and best speculum were obtained, the framework of the telescope would be so gigantic, that few observers would be able to use the instrument. With a focal length of sixty to eighty feet, it would be quite unmanageable for any private individual. The idea occurred to him (Mr. T.) to have a tube fixed in an invariable position, and to have a perfectly true plane mirror, of a size somewhat larger than the concave speculum, placed in front of the tube, with an aperture in the centre. This plane reflector should be movable about its centre in any direction: so that luminous bodies, falling first upon the plane reflector, were then reflected against the concave reflector and passed through the aperture. The only motion requisite for the plane mirror would be one about its centre. The mechanical difficulties in the way of this plan would be far less than in the common method. Prof. S.'s idea on this point was somewhat different. He (Mr. T.) did not think important in what direction the tube of the telescope was placed. Prof. S.'s idea was, that it should be pointed directly to the pole of the heavens, and kept as steady as possible, and that the plane mirror should have a simple motion of revolution, indeed two motions, but about a rectangular centre.

Experimental inquiries on the strength of stones and other materials; by Mr. Eaton Hodgkinson. Numerous experiments on wood, sandstones, marbles, glass, slate, ivory, bone, &c. had been made by Mr. H. in order to ascertain the tensile, crushing, and transverse strength of each; also, as far as possible, the situation of the neutral line. The following is a summary of the results obtained on marbles and stones of various degrees of hardness.

Description of stone.	Crushing force per square inch called 1000.	Tensile force per square inch.	Transverse strength of bar 1 inch square and 1 foot long.
Black marble,	1000	143	10.1
Italian marble,	1000	84	10.6
Rochdale flagstone,	1000	104	9.9
High Moor stone,	1000	100	
Stone called Yorkshire flag,	1000		9.5
Stone from little Hulton,	1000	70	8.8
Mean rates,	1000	100	9.8

Or calling the mean crushing strength per square inch in the different articles experimented upon 1000, we have

	Crushing strength 1000.	Tensile strength.	Transverse strength.	Ratio of mean tensile to crushing strength.
In timber,	1000	1900	85.1	1 to 0.55
Cast iron,	1000	158	19.8	1 " 6.6
Glass, plate and crown,	1000	123	10.	1 " 7.8
Stone and marble,	1000	100	9.8	1 " 10.5

The ratio of the crushing force to the transverse force is nearly the same in glass, stone and marble, including the hardest and the softest kinds. Hence if we know the transverse strength, in any of these bodies, we may predict the other; and as glass and the hardest stones resist crushing with from seven to nine times the energy that they do being torn asunder, we may get an approximate value of the tensile force from the crushing force, or *vice versa*. These results render it probable that the hardest bodies, whether cast iron, glass, stone or marble, admit of certain atomic displacements, either in tearing asunder or crushing; these displacements being in a given ratio to each other or nearly so. In future calculations as to the strength of bodies, the crushing strength ought to be made the fundamental datum, for the reasons shown in this notice. The ratio of the transverse strength to the crushing strength is greater in cast iron than in glass, marble and sandstones, arising from the ductility of that metal. The necessity of enlarged inquiries in these matters will be seen, when it is reflected that calculations of the tensile strength of cast iron or marble or stones in general, made from the transverse strength by the modes used by Tredgold, Navier and others, give the tensile strength twice or three times as great as it ought to be.

Mr. Hodgkinson also presented a communication on the mode of conducting experiments on the resistance of air.

On a new general principle of Analytic Mechanics, by Prof. Jacobi of Königsberg. In the different problems relative to the motion of a system of material points which have been hitherto considered, one may make, said the Professor, an important and curious remark, "that whenever the forces are functions of the coördinates of the moving points only, and the problem is reduced to the integration of a differential equation of the first order of two variables, it may also be reduced to quadratures." The author has succeeded in proving the general truth of this remark, which appears to constitute a new principle of mechanics. This principle, as well as the other general principles of mechanics, makes known an integral, but with this difference, that whilst the latter give the first integrals of the dynamical differential equations, the new principle gives the last. It possesses a generality very superior to that of other known principles, inasmuch as the analytical expressions of the forces, as well as the equations by which we express the nature of the system, are composed of the coördinates of the movables in any manner whatever. The Professor proceeded to show the application of his principle, and its advantages in the following problems, viz. the orbit described by a planet in its motion round the sun; the motion of a point attracted to two centres of force, after Newton's law of gravitation; and the problem of the rotatory movement of solid bodies round a fixed point. He then enunciated the rule itself, by which the last integration to be effected in the problems of mechanics, is found to be reduced to quadratures, the forces being always functions of the coördinates alone; and observed, that when we have any system whatsoever of material points, the simplicity of the preceding theorem is in no respect altered, provided we give to the dynamical differential equations, that remarkable form under which they have been presented for the first time by the illustrious Astronomer Royal of Dublin, and under which they ought to be presented hereafter in all the general researches of analytical mechanics. It is true that the formulas of Sir W. Hamilton are referable only to the cases where the components of the forces are the partial differences of the same function of the coördinates; but it has not been found to be difficult to make the changes which are necessary, in order that these formulas may become applicable to the general case where the forces are any functions whatsoever of the coördinates. When the time enters explicitly into the analytical expressions for the forces, and into the equations of condition of the system, the principle of the final multiplier found by a general rule, is applicable also to this class of dynamical problems. There are also some particular problems into which enters the resistance of a medium, which give rise to similar theorems. It is the case of a planet revolving round the sun in a medium whose resistance is proportional to any power of the velocity of the planet. "The analysis," observed M. Jacobi, "which has conducted me to the new general principle of analytical mechanics, which I have the honor to communicate to the Association, may be applied to a great number of questions in the integral calculus. I have collected these different applications in a very extensive memoir, which I hope to publish on my return to Königsberg, and which I shall have the honor of presenting to the Association as soon as it shall be printed."

On certain cases of Elliptically Polarized Light, by Prof. Powell.—At the last meeting of the Association, Prof. Lloyd gave a *theoretical* investigation of certain results obtained by Sir D. Brewster relative to thin films from which polarized light is reflected. Besides completely explaining those results, Prof. L. infers that such films ought to give the portions of light reflected at their two surfaces differing in phase, and that the light should be consequently in general, *elliptically* polarized. The author of the present paper, before he was aware of the investigation of Prof. Lloyd, had made many observations on the elliptical polarization of light by reflection from metallic and other surfaces,—the method of observation being by the well known dislocation of the polarized rings. Some of these experiments went merely to prove the existence of elliptic polarization in cases where it had not previously been detected, as in certain minerals and other bodies in which it is seen, though of small amount. In other cases, the reflecting surface consisted of the thin films formed on polished metal by tarnish, by heat, or by the galvanic process of Nobili. In these instances, a verification of Prof. L.'s theory was afforded by *direct observation*. But, further,—these films give periodic colors, and in passing from one tint to another, the ellipticity, as disclosed by the form of the rings, underwent regular changes, passing from a dislocation in one direction to the opposite, through points of no dislocation or of plane polarization, the rings being alternately dark and bright centered. This afforded a further field for the application of theory, and Mr. Airy investigated a formula for the rings under these varying conditions, with which the phenomena are in perfect accordance.

Mr. Nasmyth presented a paper on the *application of the law of Definite Proportions to the Stratification of clouds,*—prefacing it with the remark, that he was first led to speculate on this subject, by observing the arrangement of clouds in fine weather; when, towards the horizon particularly, they may be seen extended in parallel bands or stripes. He conceived that the excess of vapor floating in the atmosphere beyond what the air could combine with, formed clouds; and that the air in each electrical state, was capable of sustaining a definite proportion of vapor, and consequently that the clouds of one class or description floated, (in what might be called a plane of equal electricity,) at a uniform distance from the earth.

Sir William Hamilton made a brief communication *on a mode of expressing fluctuating or arbitrary functions by mathematical formulæ.* The subject was illustrated by diagrams, and excited great attention among the eminent men present. Prof. Jacobi said that Lagrange stated it as his opinion that it was not possible to express these functions by any mathematical formulæ. It appeared however to him, that Sir W. Hamilton had shown that it was possible.

The following communications were also made to the section.

On recurring Decimal Fractions, and a new species of Logarithms; by Mr. Anthony Peacock.

Observations on Oceanic Waves; by Mr. W. Walker.

General Considerations on the Analytic Theory of Equilibrium and Movement; by Prof. Braschman, of Moscow.

Report of the Commissioners for the restoration of lost standards of Weights and Measures, and upon their proposal for the introduction of a Decimal System; by the Dean of Ely.

On the meteorology of the Northern Atlantic and on the Southwest Monsoon of India; by Mr. Hopkins.

On improved Permanent Magnets, and the modes of determining their powers, with certain undescribed Phenomena in permanent magnets; by Rev. Dr. Scoresby.

On the Meteorology of the Province of Coorg in the Western Gâts of India; by Col. Sykes.

On the Magnetic Action of instantaneous currents of Electricity; by Prof. Marianini.

On the causes of the dissimilarity in the Frictional and Voltaic Electricities, with remarks on the Decomposition of Water by the former, and on Magnetism; by Mr. Goodman.

(*To be continued.*)

Art. XVIII.—*On the Rock Salt and Salines of the Holston;* by C. B. Hayden.

The salines of the Holston River, Virginia, are in the shales belonging to the lower portion of the *carboniferous* series. These salines therefore occupy a medial geological position in relation to the other salines of the United States; those of Kenawha, and the west generally, being higher up in the carboniferous rocks, and those of New York, according to Messrs. Conrad and Vanuxem,* belong to the Wenlock limestone group of the Silurian System. With the exception of those near Durham, in England, which are in the carboniferous rocks, they are geologically lower than those of Europe. The Holston salines† are situated in a valley, the bounding hills of which here assume an amphitheatrical arrangement, rendering the topography highly favorable by its drainage to the formation of those subterraneous reservoirs of water, which by solution of the saline matter of the adjacent strata furnish the wells with brine, and to this local topography they are indebted for their copious, constant, and uniform supply. This amphitheatrical valley, to which the discovery of brine has been confined, is about one fourth of a mile in breadth, and a mile in length. The centre of this valley consists of an earthy alluvium with a few interspersed pebbles and small bowlders, forming a flat, through which the salt wells are sunk. This alluvium is generally from eighteen to twenty feet in thickness, and reposes immediately upon the gypseous rocks, which are here in an advanced state of decomposition, consisting generally of blue and red clay, derived respectively from the blue and red shales, with which the gypsum is associated. Undecomposed rocks are rarely met with *in situ* in the wells and borings. The existence of salt springs and licks induced a successful search for salt water very early in the history of the country, but it is understood that the wells were shallow, and the brine both weak and limited in amount, although then sufficient to meet the demand.

The wells of the present establishments are six in number, but two of which are now in operation. They vary in depth

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* Vide New York State Geological Report for 1840.

† Saltville, Washington County, Va.

from two hundred to three hundred and eighty six feet. All the wells at the present site are within an area of three hundred feet; to this area, and the original wells one mile northeast of the former, the discovery of salt water has been confined. The same general section is presented in all the wells and borings, the upper eighteen or twenty feet consisting of the before mentioned alluvium, to which succeed alternating strata of red and blue clay and gypsum, the latter generally predominating, and which continue to the depth at which water is found, usually about two hundred feet. Throughout the valley already described, gypsum is found, generally at the depth of from eighteen to twenty feet. Further east the gypsum crops out, being underlayed with red and blue shales; immediately overlying the gypsum is a fetid crystalline limestone of a dark color, quite cellular, the cells being filled with crystals of carbonate of lime. At times the gypsum is separated from this rock by a thin seam of black pyritous slate. The gypsum is laminated, the plane of lamination having a general parallelism with that of its dip, which corresponds with that of the associated rocks, which is here highly inclined, showing that the deposition of the gypsum was anterior to the present disturbed condition of the rocks. The exposed portion of the gypsum is from twelve to twenty feet in thickness. The gypsum, unlike that of New York, is a nearly pure sulphate of lime; sometimes crystalline, sometimes granular, usually white, except when exposed to infiltration from the shales, when it has a slight ferruginous tinge, which is usually the case in the upper portion, where it is also more distinctly crystalline. Occasionally, it contains thin seams of a dark smoky color, and sometimes radiating crystals of the same hue. Running through the mass are frequently seams of beautiful fibrous gypsum. Well defined plates of *selenite* are rare. Gypsum of this general character occurs over a region of country of about half a mile in breadth and fifteen miles in length; it is generally worked to the depth of from twelve to twenty five feet, without being passed through; at several localities it has been ascertained to extend to the depth of two hundred and three hundred feet, and in one instance over four hundred feet.

In 1840, in sinking a shaft at Saltville, after passing through the usual thickness of alluvium, and alternating strata of red and blue clay and gypsum, nearly forty feet of which was solid gypsum, *salt rock** was met with at the depth of two hundred and twenty feet, and continued to the bottom of the shaft, two hundred and seventy three feet, and was ascertained by boring to extend to the depth of three hundred and eighty six feet, without being passed through. In this well no water was met with. Nothing is known respecting the shape or extent of this deposit, except what is learned from the above mentioned shaft,—that it is there over one hundred and sixty six feet in thickness; it is however quite local in some directions, as it is not found in the neighboring shafts. If it is however, as seems most probable, the source of the brine, which has been worked for so many years undiminished in strength and quantity, it must be quite extensive. Much interest is imparted to this salt by its being the only fossil salt yet discovered in the United States. The salt is irregularly intermixed with red and blue clay and small fragments of shale, the salt usually predominating; occasionally the former are absent. These impurities, mechanically intermixed with the salt, were much more abundant in the upper portion of the bed, which was almost entirely free from them at the bottom of the shaft. The salt is compact, semi-crystalline, generally of a deep ferruginous color, though occasionally of a delicate flesh tint, and more rarely it is entirely free from coloring matter.

The planes of the shales intermixed with the salt have not that parallelism with each other which gravity would impart to them if deposited under quiet circumstances; and their irregular distribution through the salt, making with each other every conceivable angle, indicates agitation at the period of their deposition. Gypsum is occasionally interlaminated with the salt, and sometimes occurs in it, in fibrous crystals, more or less impregnated with salt. The salt is *anhydrous*, and nearly a pure chloride of sodium, as the following analysis will show.

Peroxide of iron, ...	0.470 gr.
Sulphate of lime, ...	0.446
Chloride of calcium, . . .	a trace.
" sodium, . . .	99.084

The local occurrence of a fault, the generally disturbed condition of the rocks, and the anhydrous nature of the salt, all argue the action of heat contemporaneously with, or subsequently to

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* See the notice of this fact, published in this Journal, 1841, Vol. XLI, p. 214.

the deposition of the salt. Not having a freshly quarried specimen, I have been unable to ascertain whether the gypsum overlying the salt, or that associated with it, is anhydrous. This brine is saturated, and like that of Droitwich and Cheshire, is undoubtedly derived from the solution of fossil salt. This origin of the brine will explain its general uniformity in strength and composition in the different wells, when unaffected by infiltrated fresh water, as well as its slight variation in strength by continued working; it being well known that where the salt exists in the rocks interstitially, the brine is soon weakened by the exhaustion of the saline matter, rendering it necessary to deepen the shafts from time to time. Does not this origin explain the singularly low temperature of the wells? which in one of them is the same as that of the springs of the vicinity, and in the others, but one or two degrees above them. The temperatures of the respective wells are as follows—

Anthony well, 226 feet in depth, . .	$54\frac{1}{2}°$
Kings " 206 " " . .	$53\frac{1}{2}$
New " 214 " " . .	$55\frac{1}{2}$
Mrs. Preston's spring, (average temperature of the springs of the county,)	$53\frac{1}{4}$
Spring at salt works, ...	$52\frac{1}{2}$

This slight elevation of the temperature of the wells is less than is due to their depth, and may perhaps be owing to the cooling influence of saline solution. The brine differs in composition from the rock salt only in the absence of the peroxide of iron, and in containing a larger proportion of sulphate of lime; the former existing in the rock salt in the state of an insoluble peroxide is not dissolved, and the latter is derived from the associated strata. The annexed analysis is of the brine of the *new Artesian well*, bored a few months since to the depth of two hundred and fourteen feet, water having been obtained at the depth of one hundred and ninety three feet. One thousand grains of the brine contain—

Chloride of sodium, ...	240.52
" calcium, . . .	.08
Sulphate of lime, ...	5.35
Water, ...	754.05
Alumina, ...	a trace.

This brine therefore contains about twenty five per cent. of olid matter, twenty four per cent. of which is pure chloride of sodium. The richest of the New York brines, according to Dr. Beck, contain but about fifteen per cent. of this ingredient, which is there associated with the magnesian salts in addition to the ingredients contained in the Holston brine. A wine pint of this brine yielded of saline matter 2432.25 grains, equal in a gallon to 19458 grains, or 2.77 avoirdupoise pounds; therefore, by the ordinary process of direct evaporation, hereafter to be described, eighteen gallons of the brine will produce one bushel of salt, weighing fifty pounds. As the object of the above experiment was to determine the *practical* value of the brine, the salt was not reduced to absolute dryness, but to the ordinary dryness of the salt of commerce.

The annexed table from Van Rensselaer, with Dr. Beck's additions, to which I have added the result of my experiment upon the Holston brine, will afford a comparison of the strength of the different brines of the United States.

At Nantucket, Mass.	350	gals. sea-water yield 1 bushel of salt.
Boon's Lick, Mo.	450	gals. brine yield 1 bushel of salt.
Conemaugh, Pa.	300	" " " " " "
Shawneetown, Ill.	280	" " " " " "
Jackson, Ohio,	213	" " " " " "
Lockhart's, Miss.	180	" " " " " "
Shawneetown, Ill. 2d saline,	123	" " " " " "
St. Catherine's, U. C.	120	" " " " " "
Zanesville, Ohio,	95	" " " " " "
Kenawha, Va.	75	" " " " " "
Grand River, Ark.	80	" " " " " "
Illinois River, Ark.	80	" " " " " "
Muskingum, Ohio,	50	" " " " " "
Onondaga, N. Y.	41 to 45	" " " " " "
Holston, Va.	18	" " " " " "

The entire absence from this brine of the magnesian salts, so prejudicial in other brines, and the extremely minute proportion of the other salts present, precludes the necessity of those preliminary operations practiced at other works for the separation of these impurities. The water is first pumped into large reservoirs, where the impurities held in mechanical suspension are deposited; it is thence conveyed to the kettles, and evaporated by rapid ebullition. As the salt is deposited, it is removed in wicker baskets, and suspended over the kettles until it is dry. The cast iron kettles are segments of spheres, of about the capacity of forty gallons; these are arranged in pairs, in parallel rows, over an oblong furnace heated by wood, the heat being applied directly to the bottom of the kettles. In the arrangement of the kettles and the construction of the furnace, little regard is paid to the economy of fuel, or heat. By direct evaporation, at a high temperature, salt of a superior character is expeditiously made; but the process is attended with the serious inconvenience of the formation of a hard saline incrustation on the bottom of the kettles, owing to the intense heat to which the salt, as it is deposited, is subjected. This incrustation, called by the workmen "*blocking*," is sometimes fused into a vitreous mass, and is generally crystalline, adhering with such tenacity to the sides of the kettle, as to require the pickaxe for its removal. A serious increase of expense attends the delay necessary for the removal of this incrustation, as well as from the more rapid corrosion of the iron kettles. That this incrustation is not, as is generally supposed, a deposit of the more insoluble impurities of the brine, will be seen from the following analysis. Two specimens, selected by the proprietors as representing the average character of the "blocking," proved nearly pure salt. They were of a snowy whiteness, compact, hard, laminated, highly crystalline and anhydrous.

	No. 1.	No. 2.
Sulphate of lime,	4.892 grs.	9.314 grs.
Chloride of calcium,	.068 "	.050 "
" of sodium,	95.040 "	90.510 "
Carbonate of lime,*	a trace.	.126 "

The annoyance occasioned by the "blocking," would be entirely obviated by evaporation at a lower temperature, which would also improve the character of the salt, as the slower the evaporation, the more perfect the crystallization. At Chesh-

* The occurrence of carbonate of lime in the "blocking," is probably attributable to its existence in the brine in a proportion so minute as to escape detection in the quantity operated upon; or possibly it may be held in mechanical suspension in the brine, that operated upon having been permitted to stand until all the mechanical impurities were deposited and the clean water decanted off.

ire, where the brine is similar to this, the water is evaporated at a temperature below ebullition, in shallow wrought iron pans of an area of from eight to twenty square feet, which last for several years. A pan of this description, brought over by the Hon. William C. Preston, was burnt out in a few months at Saltville. The requisite low and uniform temperature could be readily attained by the use either of steam or hot water, as a heating agent, applied in the manner so frequently done in the manufacture of sugar and similar departments of the arts. This modification in the manufacture, besides avoiding the "blocking" and improving the salt, would economize fuel, and the latter consideration should urge itself with serious force upon the proprietors, occupying as they do a region where forests must soon be exhausted, and where geology forbids the hope of the discovery of coal.

A large proportion of the salt manufactured at Saltville, is made by the process of direct evaporation, already described. A whiter and purer salt is made by first concentrating the brine in a wooden cistern, through which steam is conducted in a cast iron pipe, which sufficiently concentrates the brine to produce a deposition of most of the sulphate of lime; the concentrated brine is then conveyed to the kettles, and evaporated as in the other process. The salt thus manufactured is purer, more distinctly crystalline, and has a white satin lustre. The following analysis of the two varieties of salt, will show the difference in their composition. No. 1, is the salt made by the process last described; No. 2, by the first process, of rapid evaporation.

	No. 1.	No. 2.
Sulphate of lime,	1.444	1.820
Chloride of calcium,	.016	.034
" of sodium,	98.540	98.146

The two establishments at Saltville manufacture annually two hundred thousand bushels of salt.

Abingdon, Va., Nov. 30th, 1842.

Art. XIX.—*Description of a Carbon Voltaic Battery;* by B. Silliman, Jr., A. M., of the departments of Chemistry, Mineralogy, and Geology, in Yale College.

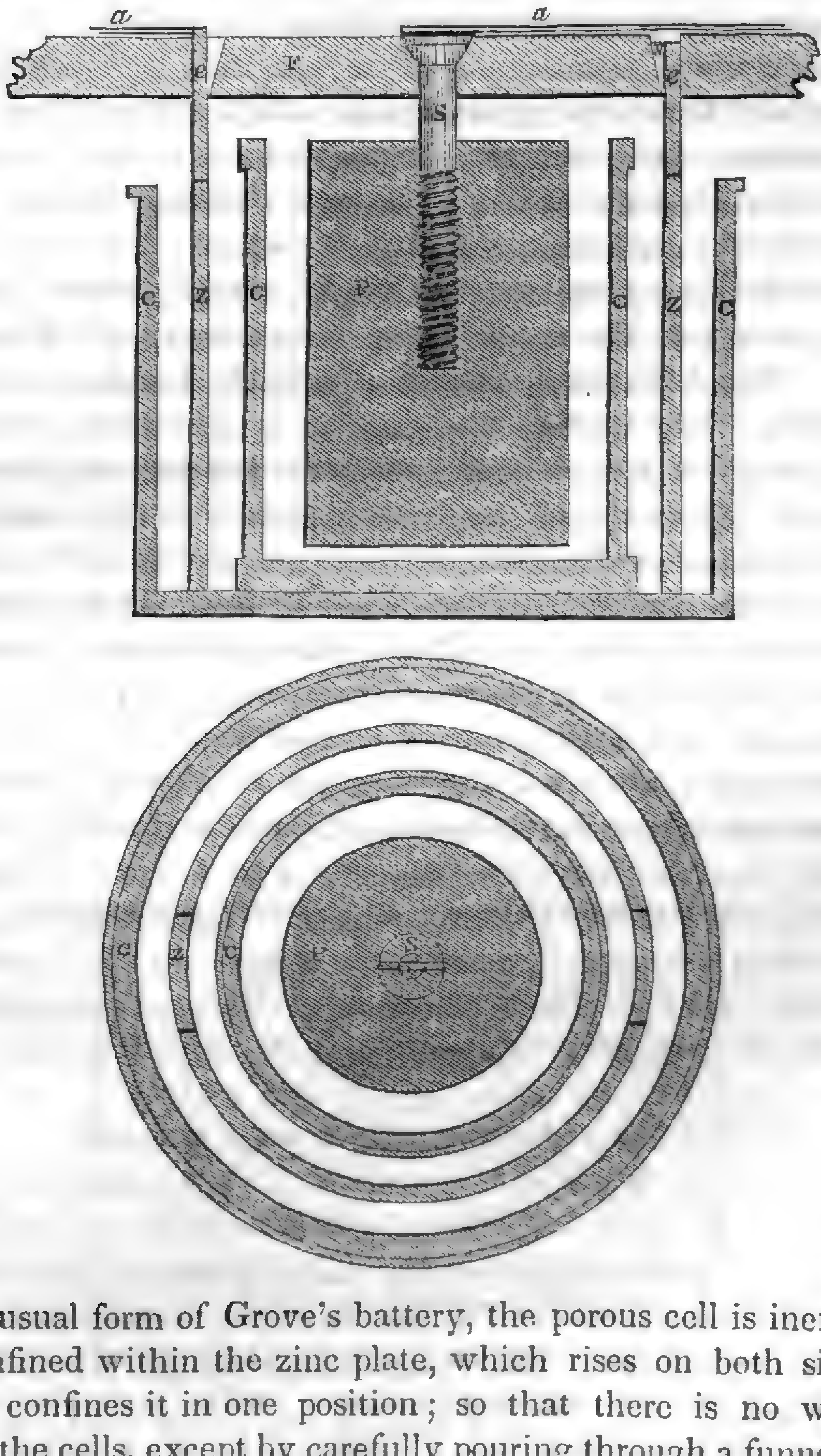
It was mentioned in the last number of this Journal (Vol. xliii, p. 393,) that a battery of considerable series was constructing by me on a plan, in many respects, similar to the nitric acid battery of Mr. Grove, but differing in the important particular of employing carbon as the negative element, in place of the platina of Mr. Grove's arrangement. I have since completed this battery, and made a series of experiments with it, and propose now briefly to describe it.

The substitution of plumbago for the platina, and the use of circular in place of flat cups, with certain details of less importance, are the principal points of difference between this and other nitric acid batteries. Fig. 1, gives a vertical and cross section of one member of the battery. The lettering is the same in both, and the drawings are on the scale of half the real size. C, the external cell, is of a peculiar glazed ware, which withstands the action of acids remarkably well.* This cell contains the dilute sulphuric acid, and is four inches in diameter, and three in height. Z is the zinc element, which is of cast zinc, and amalgamated with mercury. It is cast with two ears, (*e e*, in vertical section,) which pass up through the bar of the frame F, in which they are supported, being crowded by the small wedges *w*. The zinc rests on the bottom of the outer cell C. C, the inner cell, is of porous queen's ware, strengthened by a band of the same material at top and bottom, and resting within the zinc on the bottom of the outer cell; it is five eighths of an inch less in diameter than the zinc. In this cell is placed the nitric acid. P is the plumbago, which is in the form of a solid cylinder, three inches high, and two in diameter. It is supported by the brass screw S, which passes through the frame F. By this arrangement, one objectionable feature of Mr. Grove's battery is avoided, viz. the elements can be raised from the acids when not in use, each member dripping its acid into its own cup, and the whole being securely fixed

* This ware, as well as the porous cups, is manufactured at Jersey City pottery, where any similar articles can be obtained to order.

in the frame F F, figure 2, can rest on the edges of the outer cups until the experimenter is ready to continue his observations.

Fig. 1.



In the usual form of Grove's battery, the porous cell is inextricably confined within the zinc plate, which rises on both sides of it, and confines it in one position; so that there is no way to charge the cells, except by carefully pouring through a funnel, and they must be discharged when done with, by inclining the apparatus gradually on one side, until the nitric acid runs out. In the form here proposed, the cups are all filled very expeditiously, by pouring into each a measured quantity of acid; and during this operation, the inner cells may be removed from the outer, and allowed to stand in some convenient vessel until wanted for use, so that the intermixing of the acids, which always happens, more or less, in twenty four hours, is avoided.

The plumbago here employed, is the artificial mixture made at the crucible works, and crowded into moulds of the proper shape, and baked.* Only in a few instances have I found a cylinder fail from the action of the nitric acid. Having failed to procure readily a sufficient quantity of natural plumbago, I was induced to employ the present material, which, although inferior in the energy of the results to the native mineral, is still very good. The difficulty in procuring suitable cylinders of native plumbago, arises not from the want of an abundance of the material, but from the occurrence of numerous natural joints, even in that which seems externally quite sound, which cause them to fall to pieces under the saw.

The mode of connecting the plumbago with the zinc, suggested itself to me from the difficulty and expense of employing mercury, and I find it in every way better. The brass screw enters nearly two inches into the plumbago. Its neck passes snugly through the wood of the frame F, and the head presents a large surface to attach a connector from the next zinc.

Fig. 2.

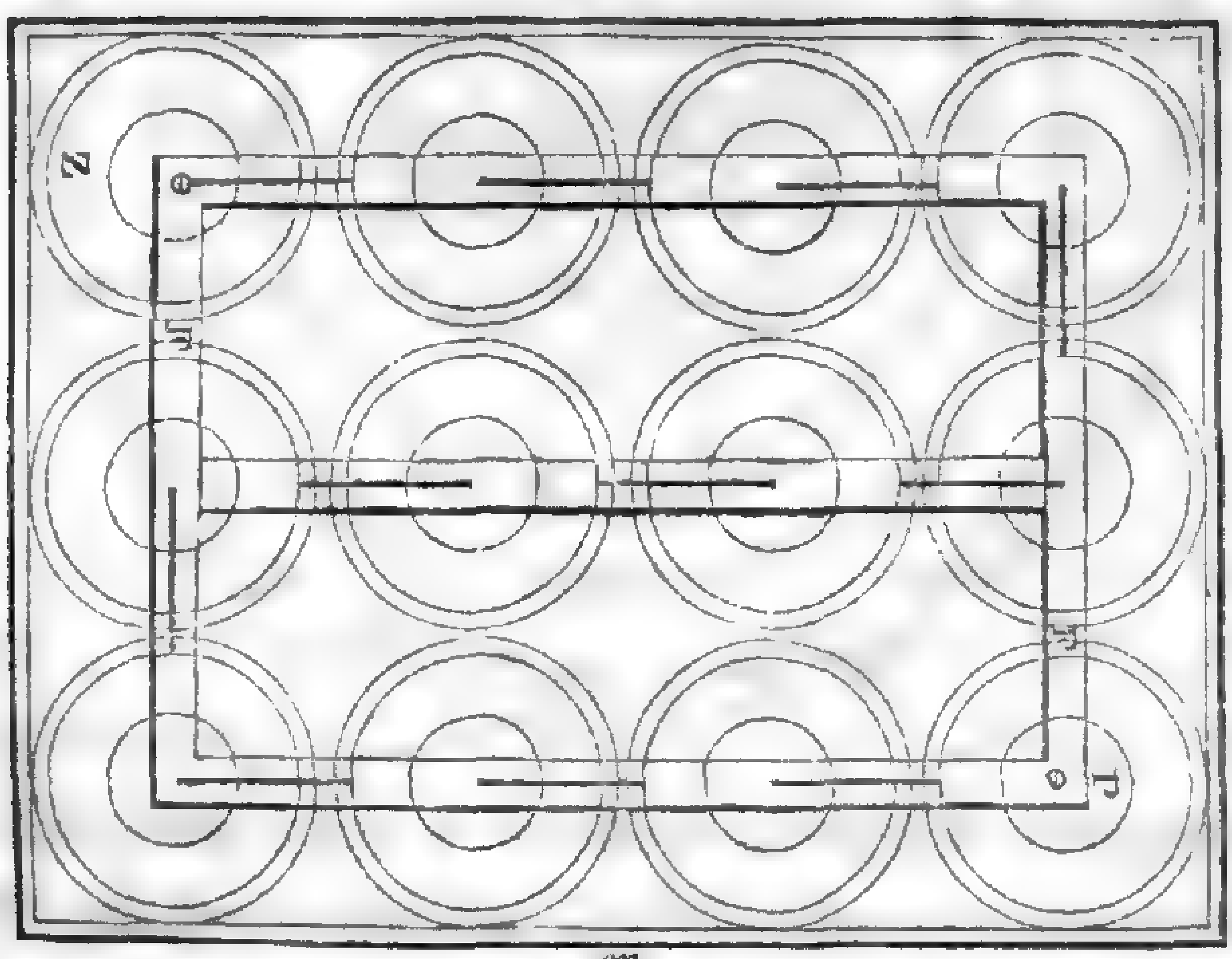


Fig. 2 shows the mode of arrangement adopted for each group of twelve members; the frame F F supports both the zincs and plumbagos, and by it they are removed from the cups; a connector of copper passes from the screw of each plumbago to the

* To be obtained of J. W. Ingall, Taunton, Mass.

zinc of the next pair, where it is soldered to the ear *e*, (fig. 1.) The other ear of the same zinc is cut off below the surface of the bar, and serves only to support the zinc firmly in its place. This arrangement, it will be seen by inspecting the figure, brings out the poles at the diagonally opposite corners, where a gallows screw is fixed for attaching the series to the next group of twelve. The twelve cups are confined in one position, by the mahogany tray *m*, in which they are easily moved from place to place.

In the present battery there are eight groups of twelve pairs each, or ninety-six pairs in all, exposing about one thousand two hundred square inches of plumbago to three thousand of zinc. The series may be connected with each other consecutively, and used with the intensity of ninety-six pairs, and the quantity of one member; or they may, by the ingenious suggestion of Dr. Hare in similar cases, be made to work with the intensity of twelve pairs, and the accumulated quantity of eight groups, each of twelve series. This beautiful mode of modifying the power and action of a battery of a given number of plates, has been described by Dr. Hare, in the Proceedings of the American Philosophical Society for 1840.*

The effects of this battery have given me much satisfaction. In some respects I feel a good degree of assurance that it is superior to any other form of voltaic battery with which I am acquainted. I regret that it is not in my power to compare it more accurately with other batteries, and especially with Mr. Grove's, as I have not now at my disposal a series on his construction.

It is not supposed that the plumbago is quite equal to platina for the negative element. Yet its surface is well calculated for an electrode, and its conducting power is remarkable, when we remember that it is not a metal, and indeed it seems to be but little behind platina in this respect, especially as, from the size of the mass employed, it may offer less resistance to the passage of an electric current, than may be the case with a thin strip of platina.

This is well worthy the name of a "constant battery," as its action is continued for many hours, with apparently little decline. The longest period in which I have employed one was eight

* The same account may be found in Vol. XL., p. 49, of this Journal.

hours, and at the end of that time I could perceive no sensible diminution of energy.

The ninety-six pairs, after having been in action some time, yielded from the decomposition of water, four cubic inches of mixed gases in twenty-seven seconds, or about nine cubic inches per minute. A series of nine hundred pairs of Wollaston's arrangement in this laboratory, (being as is supposed the largest battery on this construction in existence, in which the whole is plunged by one movement,) each zinc plate four by ten inches, gave at the same time, when excited in the usual way, four cubic inches in fifteen seconds. The time was therefore as fifteen to twenty-seven; but the surface of metals opposed to each other in the larger series, is one hundred and fifty-six thousand square inches, and that in the constant battery is four thousand two hundred. I infer from this, that *cæteris paribus*, the new battery must vastly exceed the old form, in all the classes of effects produced, but am unable from the above data to draw any definite ratio.

The brilliancy of deflagration produced from this arrangement is very great, particularly in the case of a continuous stream of mercury flowing from the narrowed point of a bent funnel, into which is thrust one pole of the battery, and falling on the surface of a pair of the same metal connected with the other pole. The inspection of this splendid combustion for several minutes with unguarded eyes, resulted in a severe inflammation, which confined me for a short time to a dark room.

In the note referred to, in the last volume of this Journal, page 393, I stated that a battery of six members of plumbago one inch in diameter, and two inches high, opposed by an amalgamated zinc coil two and a quarter inches in diameter, and two inches high, (equal to thirteen and a half square inches of zinc, and six inches of plumbago, or about one hundred and twenty square inches of opposing surfaces in the whole battery,) gave the remarkable result of four* cubic inches of mixed gases from water, in a little less than fifty seconds, and maintained fourteen inches of number thirty platina wire coiled into a spiral, at full incandescence, for nearly an hour! The following is taken from Mr. Grove's original article describing his battery, shortly after its invention. Speaking of his former account of his battery, (before the British Association at Birmingham,) he says, "I underrated

* Erroneously printed *in loc. cit.* 5.

very considerably the power of the battery, which with proper arrangements liberates six cubic inches of gas per minute, heats to a bright red seven inches of platinum wire one fortieth of an inch in diameter, burns with beautiful scintillations steel needles of a similar diameter, and affects proportionately the magnet. The battery consists of four pairs of zinc and platinum foil plates, each metal exposing a surface of fourteen square inches; the whole occupies less space than a cube four inches in the side."

I was agreeably surprised on reperusing this article, after the publication of my note above referred to, to find how favorable the comparison was between the plumbago and the platina. The platina battery of Mr. Grove exposed one hundred and twelve square inches of surface, the plumbago one hundred and twenty; the former gave one cubic inch of gas in ten seconds, the latter one cubic inch in twelve seconds.

The form of carbon most efficient in voltaic circuits, is that exceedingly hard and pure carbon which is deposited from coal gas on the heated inner surface of the retorts of the gas works; but it is so difficult to work it into shape, and to attach it to other substances, that it cannot be availed of. I have tried anthracite and bituminous coal, but not with success.

In the battery described in this article, as actually constructed, the two elements were, by mistake, placed at a greater distance than I had intended, and no doubt much power is by that means lost. In another apparatus which I am constructing, it is my intention to remedy this defect. Nearness of approach between the elements cannot be too much looked to in such arrangements.

In the 'Archives de l'Electricité,' (No. 1, p. 262,) M. De La Rive gives an account of some experiments made by him with a Grove's battery of forty pairs. He states the luminous arch produced between the poles, after contact and gradual separation, to be *one inch* in length. That produced by the subject of this paper, was about two inches, but was not accurately measured, nor was it taken in vacuo.

By way of illustration, I would remark, that the large Wollastonian arrangement before alluded to, when new, gave an arch in the open air of seven inches, and may now be usually depended on for an arch of from four to five inches in length.

M. De La Rive further observes that the transfer of particles of carbon from the positive to the negative pole is evident, and then goes on to describe the phenomenon. Dr. Kane also and Prof. Daniel, in their several works, refer to this fact as of recent observation. Now it is not a little remarkable, that at this late day this fact should be esteemed either rare or wonderful, since the same phenomena were more than twenty years ago described by Dr. Hare and Prof. Silliman, and have been witnessed ever since by all who have used or seen the powerful deflagrators of Dr. Hare.* Is not the carbon, when thus transferred, in the state of vapor?

It is not my wish in this article, to extol the merits of my battery, at the expense of any other, but merely to call attention to what I believe to be an important modification of Mr. Grove's admirable contrivance.

Yale College Laboratory, Dec. 14, 1842.

P. S. Since this article was written, I have, by simple accident, found (while looking for another matter) in the London, Edinburgh and Dublin Philosophical Magazine for January, 1840, page 35, a letter to the editors, from Mr. J. T. Cooper, giving notice of his having employed plumbago and other forms of carbon in voltaic combinations, with highly satisfactory results. This article I had not seen, and it proves again what has been so often proved before, that the same end will often suggest the same means; but undoubtedly the claim of priority in this case belongs to Mr. Cooper, to whom I cheerfully yield it. I have not, however, heard that any one has taken advantage of Mr. Cooper's suggestions to construct a large battery on this plan.

Dr. Hare also informs me that a similar plan was original with himself, and that a carbon battery was contrived in Germany more than a year since.

Prof. S. F. B. Morse, (to whom I cheerfully acknowledge my obligations,) has contrived a battery for moving his electro-magnetic telegraph, which surpasses in constancy any heretofore made. He assures me that he has had it in good working condition without interruption seven or eight days. Its peculiarity is in interposing between the zinc and platina of Mr. Grove's arrangement a second porous cell, containing strong sulphuric acid, which interrupts the mixing of the nitric with the dilute sulphuric acid, or *vice versa.*

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* See a correspondence on this subject in the 5th vol. of this Journal, (1822,) p. 108 *et seq.*, between Prof. Silliman and Dr. Hare.

Art. XX.—*Bibliographical Notices.*

1. *Report on the Geology of Connecticut;* by James G. Percival. Published under the direction of the Commissioners appointed by the Legislature. New Haven, Osborn & Baldwin, 1842. pp. 495, 8vo., with a map.—This long expected report has just come into our hands wet from the press, as we are closing our present number. From the cursory examination we have been able to give it, we are strongly impressed by the great amount of condensed information it contains, relative to the exact limits and extent of our several geological systems—the primary, the secondary, and the intrusive or plutonic rocks. When we remember the learning and talent of the author, and the laborious accuracy with which he investigates every subject which occupies his attention—that he began this exploration in 1835, and has pursued it with great diligence until the present year—that he has been personally and on foot in contact with every one of the four thousand six hundred square miles in the state; and has carefully collated and compared more than eight thousand specimens, beside much more numerous dips and bearings—it cannot be doubted that he has laid the foundation for all future investigations into the geology of Connecticut.

Dr. Percival tells us, that "this report is but a hasty outline;" by this we presume he intends that the full exposition of his theoretical opinions is not given; but we feel assured as to the fullness and accuracy of the facts, and that it will prove a satisfactory solution of a problem of the highest practical as well as scientific importance—the exact determination of the geological system of the state.*

That this report will meet the expectations of the unscientific but otherwise intelligent bulk of the reading population of the state, would be unreasonable to expect; it is not the object of the book to please or amuse, nor even to point out new sources of wealth, (though this duty has not been neglected whenever opportunity offered,) and we are not to suppose, that the greater portion of the legislative assemblies who have voted grants of money for its completion, looked mainly to these secondary ends. But we have the satisfaction of knowing that we have for all future time a faithful guide to our explorations, and a standard of comparison at once full and simple. The scientific geologist, who reads Dr. Percival's report, will only regret that he has not the advantage of the full exposition of the author's theoretical views on many points of interest to science at large—views which we know the accomplished au-

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* We are however not a little surprised, that he hesitates in expressing an opinion as to the age of our secondary. The facts published by others, render the determination almost beyond dispute, that our sandstones belong to the *new red* group.

thor possesses, and which, we trust, with the aid of the abundant unused notes and materials at his disposal, he will at some future time make public for the benefit of his fellow laborers.

It will ever be gratefully remembered, that we owe the present report on our geology, as well as that of Prof. Shepard on the *mineralogy* of the state, (vide this Journal, Vol. XXXIII, p. 151,) to the enlightened zeal of the Hon. HENRY W. EDWARDS, under whose advice, when in the gubernatorial chair in 1835, the measure was originally proposed, and who has since uniformly sustained it by his official and personal influence.

2. *Natural History of New York. By Authority. Part first—Zoology.* By JAMES E. DE KAY. New York, D. Appleton & Co., and Wiley & Putnam, 1842. 4to. introduction, pp. 188; text, (zoology, class mammalia,) 146; 33 plates.—The appearance of the first volume of the final report of the commission of scientific men appointed to survey the natural history of the important territory of New York, is hailed by all lovers of science in America as no ordinary occurrence. This is the first of ten volumes, which are to be divided as follows: General Introduction; Part I, Zoology, by James E. DeKay; Part II, Botany, by John Torrey; Part III, Mineralogy, by Lewis C. Beck; Parts IV and V, Geology and Palæontology, by William W. Mather, Ebenezer Emmons, Lardner Vanuxem, and James Hall.

It is not our present purpose to enlarge on this subject. The volume before us is a fair earnest of our expectations; mechanically the best done of any report yet published by our several states, it will doubtless prove equal to the demands of public expectation in its detail. The plates are better than those before published in any state report in this country; in general, they are very good, particularly the copper or steel plates, while the lithographs are not a fair example of the present state of that art.

Governor Seward's introduction is made up of matters and things in general, touching the history and progress of the state of which he has been for four years past the political head. It is interesting enough doubtless to the inhabitants of the "*Empire State*," and contains a succinct account of the origin and progress of the survey of which the ten volumes in question are the results; but it would be more appropriate for a gazetteer than as the introduction to the *Natural History* of a territory of forty six thousand two hundred square miles and lying in the temperate zone.

When the publication is completed, which we understand will be as soon as practicable, it is our intention to give in considerable detail, a *coup d'œil* view of the general scope of this great work, which, accomplished by so many laborers in seven years of research, and at a cost of one hundred and thirty thousand dollars, will ever redound to the liberality and enlightened zeal of the noble commonwealth whose work it is, and to the fame of the numerous gentlemen of science by whose united efforts it has been accomplished.

3. *Lectures on the application of Chemistry and Geology to Agriculture. Part II, on the Inorganic Elements of Plants.* By J. F. W. Johnston, M. A., F. R. S., &c. &c. New York, Wiley & Putnam, 1843. (American edition,) 12mo. pp. 175.—Just one year since, we gave (Vol. xlii, p. 187) our favorable opinion of the first part of Prof. Johnston's Lectures. It is therefore unnecessary to repeat those remarks here, which are only confirmed by the character of the present portion of the work.

According to the present state of our knowledge on this subject, the inorganic constitution of the soil and of plants, is a most essential and important branch of scientific agriculture. It is evident that no plant contains any inorganic element not contained in the soil in which it grows; it is certain that these inorganic constituents, although present in small proportion, in some way not very well understood, are essential to the growth and perfection of the plants containing them. An accurate knowledge, therefore, of the composition of the ashes of all agricultural plants is highly important, in order to determine if a given soil contains all that is essential to the growth of any particular species or family. Hence the importance of correct analyses of soils, in order to a judicious and effective application of manures and alteratives. Wheat cannot grow and perfect its fruit, without phosphate of lime; the double phosphate of lime and magnesia is essential to potatoes, and hence bran, which is peculiarly rich in this salt, is a very valuable manure for the potato. One of the most remarkable features of recent scientific progress, is the wonderful amount of talent and research which has within three or four years past been so successfully applied to the improvement of agriculture. A short time since, and no agricultural chemistry could be found, save the original treatises of Davy and Chaptal, and agricultural geology was an unknown term. Now, the shelves of the bookseller are crowded with new publications of this character, and every news-letter and advertiser contains some new announcement of agricultural works, while the strangest and most desirable part of the phenomenon is, that the demand is equal to the supply.

We are passing through a new era in this department of science, and when the present excitement has passed away, and we look for the actual progress made, the new principles established, and improved methods adopted, we venture to predict that in the changes effected, Prof. Johnston's works will be found to have acted an important part.

4. Elements of Chemistry, *including the actual state and prevalent doctrines of the science;* by the late Edward Turner, M. D., F. L. S. L. & E. Seventh edition. Edited by Justus Liebig, Ph. D., M. D., &c., and William Gregory, M. D., &c. London, Taylor & Walton, 1842. pp. 1274, 8vo.—At length after an interval of five years, is this long expected edition brought to a close by the publication (about a month since)

of the "second supplement," containing about 160 pages. The delay in its appearance, has resulted from a desire on the part of the editors to include in it all the recent discoveries of the science, and especially the new views of Dr. Liebig, contained in his Animal Chemistry, published last June. Those who purchased the first part of the sixth edition of this work, and have waited till now for its completion, will have the pleasure of repurchasing the whole, if they mean to be possessed of the entire seventh edition.

The chapters on salts have been remodelled in this edition, and the present views respecting salt radicals explained; while the entire organic chemistry has been rewritten, and many laborious series of researches undertaken at Giessen in reference to this end. A condensed summary is given of Prof. Liebig's views of the changes which occur during the life, growth, and nutrition of vegetables and animals; and a few pages added (as part fourth) on chemical analysis, taken chiefly from the recent work on that subject by Mr. Parnell.

5. *Elements of Chemical Analysis, Inorganic and Organic;* by Edward Andrew Parnell, Chemical Assistant in University College, London. London, Taylor & Walton, 1842. pp. 309, 8vo.—An original work on this subject in English and written in England, is indeed a rarity which cannot be allowed to pass unnoticed. It is not to be expected that in a work of 300 pages there should be any thing like the fullness of Rose's elaborate manual in two volumes. But this work was evidently written for the uninitiated in the analytic art, and in that view it is an excellent book. Indeed Mr. Parnell's book supplies a deficiency which has heretofore existed in the chemist's library, of a work intended for the non-proficient. The knowledge assumed to be familiar to the reader in the admirable treatise of Heinrich Rose is quite beyond the acquirements of most students, and hence his long, full descriptions of unknown processes serve to only darken counsel by words without knowledge.

A pretty full and well arranged set of tables, evincing much labor and care, follows the introduction, and contains in a convenient form for reference the most usual and important behavior of the various oxides and acids with reagents.

The various manipulations and necessary apparatus on which the success of analytical operations depends, are the subject of the first part of the book. Numerous and well drawn wood-cuts illustrate the text whenever such illustration is important. The quantitative analysis of various bodies is ably discussed, and the difficult analysis of natural silicates illustrated by well selected examples. This part of the work contains tables of great value and convenience to the student. The use of the blowpipe, and the treatment of substances in the inner and outer flame, is fully treated, which, with a description of the modes of detecting poisonous substances, (arsenic, lead, mercury, &c.) finishes the first part of the work.

Qualitative analysis forms the second part, and is mostly a condensation of the rules of M. Rose. A condensed view of the outline of organic analysis is then given, and the work concluded by a set of tables for calculation of the results of an analysis. These tables, so important to the working chemist, and which form so valuable a portion of Mr. Rose's work, were strangely omitted by Mr. Griffin in his English translation of that work.

6. *The Chemical Gazette, or Journal of Practical Chemistry, in all its applications to pharmacy, arts, and manufactures;* conducted by William Francis and Henry Croft, late students in the universities of Berlin and Giessen. London, Nov. 1, 1842. No. 1, (published on the 1st and 15th of every month,) pp. 32, 8vo.—Chemistry is certainly on the ascendant in England. The establishment and activity of the Chemical Society, the number and value of the recent chemical treatises—witness Daniel, Kane, Graham, Turner, (by Gregory and Liebig,) Parnell, Brande, &c., and now the issuing under such favorable auspices of this Journal—all show conclusively that both the demand and the supply is better than heretofore. In all this can be seen the influence of the British Association, and the benefit (as one part of the results of that institution) of a more familiar and personal knowledge of continental chemists and their doings.

Messrs. Francis and Croft have for some time past contributed valuable notices of what was doing on the continent to the Philosophical Magazine. The "Gazette," they state, is intended to be eminently practical, giving to the working chemist and pharmaceutist, the earliest possible information of all that occurs on the continent and in England, which is important to be known by them. The complexion of this their first number bears out their proposal. We copy the table of contents.

Introduction: Table of Elementary Bodies, with their Symbols and Atomic Weights. Scientific and Medicinal Chemistry.—On testing for Arsenic in cases of Poisoning: On the conversion of Ligneous Fibre into Starch, by Prof. Liebig: On Digestion: On Glucinum and its compounds: Fluid of the *Spina Bifida:* Solubility of the Sesquioxide of Iron in Carbonate of Ammonia: Analysis of the Gall Stone of a Sheep: Action of Sulphuretted Hydrogen on an acid solution of Zinc and Arsenic Acid. Analytical Chemistry.—Separation of Chloride of Magnesium from the Chlorides of Potassium and Sodium: Separation of Zinc from Nickel and Cobalt: Separation of Lead from Bismuth: Tests for Iodine and for Copper. Pharmacology.—Adulteration of Saffron: On the Barks of commerce. Chemical Preparations.—Preparation of finely divided Calomel, of Hyperchloric Acid, of pure Sulphuric Acid, of Chloride of Zinc, of pure Potassa and Soda: *Unguentum contra Tineam:* Employment of Sulphuret of Iron in cutaneous diseases. CHEMISTRY APPLIED TO ARTS AND MANUFACTURES.—Theory of Saponification, by Prof. Liebig: On the preparation of Ultramarine. REVIEWS.—Elements of Chemical Analysis, by E. A. Parnell, Esq. PATENTS FOR IMPROVEMENTS IN CHEMICAL PROCESSES.—Improvements in the manufacture of Sulphuric Acid; in the operation of Tanning.

7. *Elements of Chemistry, including the most recent discoveries and applications of the science to Medicine and Pharmacy, and to the Arts;* by ROBERT KANE, M. D., M. R. I. A., &c. &c. An American edition, by JOHN WILLIAM DRAPER, M. D., Prof. of Chem. in the University of New York. New York, Harper & Brothers, 1842. pp. 704, 8vo.—The appearance of an American reprint of Dr. Kane's excellent treatise was very acceptable to all devoted to the science in this country. It has been we understand extensively adopted as a text-book in many of our colleges; and from a careful perusal of it and use in our own laboratory for two years, feel assured that it is the best book now accessible to the American student. Dr. Draper has not added (nor was there occasion that he should add) much to the original text, but where he has done so it is generally from the results of his own researches, particularly in the physical part. We might mention as an instance occurring to us at this moment, his explanation of the theory of Daguerre's process. We regret that it was thought advisable in this edition to curtail the index. No book can have too full an index, and least of all a chemical book.

8. *An effort to refute the arguments advanced in favor of the existence of the Amphide Salts, or radicals, consisting, like cyanogen, of more than one element;* by ROBERT HARE, M. D., Professor of Chemistry in the University of Pennsylvania. 23 pages, pamphlet.—This is a very acute and able discussion of an obscure and difficult subject, to which we have neither space nor time to advert, in explanation of the title cited above. An attempt to subvert the present nomenclature of the oxy-salts, and to introduce a new arrangement of their elements, so as to make them correspond with the haloid salts, appears to us very unnecessary, and to be unsupported by any reasons, sufficiently important, to justify so annoying an innovation. Men of acute minds may arrange mentally the chemical atoms so as to produce results which harmonize, and leave no fractions to be disposed of. But bounds should be set to these intellectual recreations, especially when they produce a host of new names for principles whose existence cannot be proved, because they cannot be isolated. Many of the names, for example, recently introduced into the organic chemistry, are uncouth, complex, hypothetical, and at war with euphony. Dr. Hare's argument, as regards the new nomenclature of the oxy-salts, appears to us to be conclusive, and we trust that the beautiful language so long in use will not be set aside, nor the still more beautiful harmony of the saline elements disturbed.

9. Murchison's *Silurian System.*—It will be gratifying to geologists and naturalists generally, in this country, to learn that Mr. Murchison's celebrated work, on the Silurian System, can now be had at about half the price which it has heretofore been sold at in this country. We are authorized by Mr. Murchison to offer the work to *American* naturalists for £5 sterling. It may be had at this rate of Messrs. R. & J. E. Taylor, Red Lion Court, Fleet street, London, or more conveniently of Messrs. Wiley & Putnam, 161 Broadway, New York, and 35 Paternoster Row, London, for the same price, plus the duty and transit. This reduction is made expressly to promote the circulation of the work in this country, where the Silurian rocks hold so prominent a place, and is not extended to England, where the price is as originally, £8.

We may also state that Mr. Murchison will publish his researches in Russia next summer, in a separate and extended form.

10. *The Climate of the United States and its Endemic Influences, based chiefly on the records of the Medical Department and Adjutant General's office, United States Army;* by Samuel Forry, M. D. New York, J. & H. G. Langley, 1842. pp. 378, 8vo.—Dr. Forry has been long known by his meritorious labors in various departments of statistics, particularly by his "report on the sickness and mortality in the army of the United States," and the Army Meteorological Register. In the present work, advantage has been taken of the materials collected by the returns from the various military posts, and also from other sources, to draw out a general and comprehensive view of the climate of the United States, and of North America in general. We believe this task has never been before attempted, certainly not with any thing like the advantages possessed by Dr. Forry. The work seems well done, and the author deserves the commendation of all for his labors in this new and important field of investigation.

11. *Boston Journal of Natural History, containing papers and communications read before the Boston Society of Natural History.* Vol. IV, No. 2. Boston, C. C. Little & James Brown, 1842. pp. 128, 8vo.—The Journal of the Boston Society is now published with so much exactness as almost to acquire the character of a regular periodical work. In our 42d volume, p. 379, we gave the contents of the part of Vol. IV preceding the present part. The articles in this part are as follows:

Remarks upon Coral Formations in the Pacific, with suggestions as to the causes of their absence in the same parallels of latitude on the coast of South America, by Joseph P. Couthouy, continued from p. 105: Descriptions of some of the species of Naked Air-Breathing Mollusca inhabiting the United States, by Amos Binney: Additional descriptions of, and observations on, the Fishes of Massachusetts, by D. Humphreys Storer, M. D.: An inquiry into the distinctive characteristics of the Aboriginal Race of America, by Samuel George Morton, M. D.: Descriptions and figures of the Araneides of the United States, by Nicholas Marcellus Hentz: Descriptions of the Fishes of Lake Erie, the Ohio River, and their tributaries, by Jared P. Kirtland, M. D., continued from p. 26.: Description of a species of Helix, newly observed in the United States, by Amos Binney: Observations on the habits of the Python Natalensis, by Thomas S. Savage, M. D.: Observations on the characters and habits of the Ocellated Turkey, (Meleagris Ocellata, Cuv.,) by Samuel Cabot, M. D.: On the existence of (Siliceous?) Spiculæ in the exterior rays of Actinia, and memoranda concerning the Siliceous Animalcules of Boston, by J. W. Bailey: Enumeration of the Fishes of Brookhaven, Long Island, with remarks on the species observed, by Wm. O. Ayres.

The preponderance of zoological matter over the other branches of natural history, strikes the eye at once in glancing over this table of contents. This is of course in a great measure accidental, but it is nevertheless true, that the number of contributors in the zoological departments have been greater generally in this Journal than in botany, palæontology, and mineralogy. We observe in this number some typographical errors, an unusual thing in the Boston press.

12. *The North American Sylva; or a description of the Forest Trees of the United States, Canada, and Nova Scotia, not described in the work of F. Andrew Michaux, and containing all the Forest Trees discovered in the Rocky Mountains, the Territory of Oregón, down to the shores of the Pacific, and into the confines of California, as well as in various parts of the United States. Illustrated by* 122 *fine Plates.* By Thomas Nuttall, F. L. S., &c. &c. In 3 vols.—Vol. I, (first half.) Philadelphia, J. Dobson, 1842. pp. 55, royal 8vo.; 19 plates.

The first half volume of Mr. Nuttall's continuation of Michaux's Sylva made its appearance about three months since. We are happy to say that Mr. Nuttall's part of this justly esteemed work is issued in a mechanical style quite free from the faults of the reprint of the old portion, of which it forms the continuation. This part is on fine paper, well printed, and adorned with brilliantly colored plates. Mr. Nuttall's labors have probably nearly doubled the number of trees described by Michaux. This it seems is the farewell labor of Mr. Nuttall in the United States, as at the close of a beautiful preface to the present volume, in which he gives a very brief account of his wanderings, he says: "But the 'oft told tale' approaches to its close, and I must now bid a long adieu to the 'new world,' its sylvan scenes, its mountains, wilds, and plains, and henceforth, in the evening of my career, I return, almost an exile, to the land of my nativity!"*

* Mr. Nuttall left this country for England about one year since.

We append a list of the principal species mentioned in this half of the first volume, and when the remaining parts are before the public, will take more extended notice of it as a whole.

OAK.—Western oak, *Quercus garryana;* Holly-leaved oak, *Q. agrifolia;* Small-leaved oak, *Q. *dumosa;* Rocky Mountain oak, *Q. undulata;* Douglass oak, *Q. Douglassi;* Dense-flowered oak, *Q. densiflora;* Lea's oak, *Q. Leana.*

CHESTNUT.—Dwarf chestnut, *Castania alnifolia;* Golden-leaved chestnut, *C. chrysophylla.*

BIRCH.—Western birch, *Betula occidentalis;* Oval-leaved birch, *B. rhombifolia.*

ALDER.—Oregon alder, *Alnus *Oregona;* Thin-leaved alder, *A. tenuifolia;* Rhombic-leaved alder, *A. rhombifolia;* Sea-side alder, *A. maritima.*

ELM.—Opaque-leaved elm, *Ulmus *opaca;* Thomas's elm, *U. ramosa.*

HICKORY.—Small-fruited hickory, *Carya macrocarpa.*

MYRTLE.—Inodorous candle-tree, *Myrica inodora.*

PLANE-TREE.—California button-wood, *Platanus ramosa.*

POPLAR.—Narrow-leaved balsam poplar, *Populus angustifolia;* Cotton-wood, *P. lævigata.*

13. CHOISY, *de Convolvulaceis dissertatio tertia complectens Cuscutarum hucusque cognitarum methodicam enumerationem et descriptionem, etc.* (in *Mémoires de la Société de Physique et d'Histoire Naturelle de Genève,* Vol. 9, part 2, (1841–42,) pp. 261–288,) with 5 plates.—This full and finely illustrated monograph of a long neglected tribe of plants, is the more interesting to us, at the present moment, since Dr. Engelmann has been recently occupied in the study of the North American species, and published the results of his observations in the last (October) number of this Journal. Prof. Choisy describes thirty-eight species of Cuscuta from actual examination, twenty of which he has handsomely figured. Only four of this number are given as natives of extra-tropical North America, (although five others are derived from Mexico and New Spain,) viz. *C. Californica,* Chois. (California, *Douglas,*) and *C. glomerata, C. compacta,* and *C. Gronovii,* all new species from the United States. These species it is necessary to collate with those of Dr. Engelmann's monograph, especially as the former have the priority in publication; and we find, accordingly, that—

C. glomerata, *Chois. l. c. p.* 280, *t.* 4, *f.* 1 (St. Louis, Missouri, *Riehl, in herb. Reuter*) = Lepidanche Compositarum, *Engelm. monogr.*

C. compacta, *Chois. l. c. t.* 4, *f.* 2 (Alabama, S. Carolina, &c.) = Lepidanche adpressa, *Engelm. ined.;* a species which has only recently fallen under Dr. Engelmann's observation.

C. Gronovii, *Chois. l. c. t.* 4, *f.* 3 (Carolina to Boston) = C. vulgivaga, *Engelm. monogr.* (var. β.)

Prof. Choisy accords with Dr. Engelmann in retaining the name of *C. Americana* for the West Indian species. He also points out, and employs as an important distinctive character, the two states which the marcescent corolla assumes during the growth of the fructified ovary; either remaining persistent at its base, or elevated with it and covering its summit like a hood;—circumstances which have not escaped Dr. Engelmann's notice. According to the former, the entire capsule separates from the receptacle and falls to the ground unopened in the greater part of the genus; as Dr. Engelmann had remarked for the North American species. Prof. Choisy does not seem to have sufficiently regarded the characters upon which Dr. Engelmann founds his genus *Lepidanche;* but, as the sepals are nearly if not quite distinct in several species, and the flowers are often more or less bracteate, we suspect that this imbrication of bracts and sepals will not warrant the dismemberment of such a natural genus as Cuscuta. The other species described by Dr. Engelmann do not appear to have fallen under Prof. Choisy's observation. This author states that, so far as known, *Cuscutæ* are only parasitic on *Dicotyledons,* with the exception of a single instance, noticed by De Candolle, in which *C. minor* was found upon a grass: but Mr. Carey has detected the *C. Gronovii* on a species of *Leersia.* These parasites are most frequently found attached to *Compositæ* and *Leguminosæ,* perhaps because these are the largest and most widely diffused Exogenous families; but they avoid all plants which have acrid juices, such as *Ranunculaceæ* and *Umbelliferæ.*

14. Spring, *Monographie de la Famille des Lycopodiacées;* premiere partie, 1841, (in *Nouveaux Memoires de l'Académie Royale des Sciences et Belles-lettres de Bruxelles,* tom. xv, 1842,) pp. 110.—Prof. Spring, of the University of Liége, has been for several years occupied with the *Lycopodiaceæ,* and published in 1838, a *Beitrage zur Kentniss der Lycopodien* in the *Flora oder Botanische Zeitung* of Ratisbon, which was reproduced in the *Annales des Sciences Naturelles* for 1839. He has also recently elaborated the *Lycopodiaceæ* of Martius and Endlicher's *Flora Brasiliensis;* a work which we hope to notice at large in a future number of this Journal. This systematic monograph, so far as published, comprises the genus *Lycopodium,* as restricted by this author; who constitutes of *L. rupestre, L. apodum* and their allies, a distinct genus, with the name of *Selaginella,* and distinguishes the four genera of the order as follows:

1. Lycopodium. Antheridia unilocularia. Oophoridia nulla.
2. Selaginella. Antheridia unilocularia. Oophoridia 3–4-cocca.
3. Tmesipteris. Antheridia bilocularia. Oophoridia nulla.
4. Psilotum. Antheridia trilocularia. Oophoridia nulla.

Although he gives the name of *antheridia* and *oophoridia* to the two sorts of fructification in this family which are termed anthers and ovaries by some other botanists, he denies that they fulfill the office of these organs, or that the one in any way fecundates the other. This fecundation the author thinks impossible, for two reasons, (neither of which appear to us absolutely conclusive,) 1st, because there is no stigma or other conducting medium; and 2d, because the so-called anthers shed their contents in almost every instance (*Selaginella rupestris* and *S. spinulosa* being the only known exceptions) long before the so-called ovaries attain their full development. A stronger reason may be found in the fact, that the latter organs are not essential to propagation in this family, and are altogether wanting in three of the four genera which compose it. SPRING remarks, however, not only that these pollen-like grains burst like true pollen when thrown into water, with the escape of *fovillæ*, as Brown long ago remarked, but that he has also seen in *Selaginella*, (as Brown had remarked in *Psilotum*,) the production of *boyaux* from the grains, not unlike pollen-tubes. On the other hand, he has been able to confirm the fact of the germination of these grains in *Lycopodium;* but not in *Selaginella*, the only genus in which the two kinds of organs coëxist, and where a sexual antagonism may be supposed to take place: and in this case the globules or *spores* contained in the oophoridia have been observed to germinate and give rise to new plants. Germination, in both kinds of reproductive bodies, as in most other cryptogamic plants, takes place by the simple extension and growth of the whole spore, without the rupture of any integument.

Ninety-eight known, and three uncertain species of *Lycopodium* are here described; of which thirteen are natives of extra-tropical North America. Only two of this number are peculiar to this country, viz. *L. lucidulum* and *L. dendroideum.* It is remarkable that not only *L. complanatum* and *L. clavatum*, which are cosmopolites, but also *L. sabinæfolium*, which is otherwise restricted to British America, are found in Java. Our two principal southern species, *L. Carolinianum* and *L. alopecuroides*, both extend to Brazil; and the former is also a native of the Cape of Good Hope, Madagascar, &c.; and one of our Alpine species, *L. Selago*, is likewise antarctic. We append a conspectus of the North American species, with their geographical range.

Gen. LYCOPODIUM.

Sect. 1. Antheridiis sparsis, (Selago, *Dill.*)

§1. *Foliis undique conformibus.*

L. SELAGO, *Linn.*—Europe, Northern Asia, Boreal America, Azores, Peru, Falkland Islands, Van Diemen's Land.

§2. *Foliis fructigeris difformibus vel saltem minoribus.*

L. TAXIFOLIUM, *Swartz.*—Nootka Sound, (L. struthioloides, *Presl, rel, Hænk,*) Mountains of the East and West Indies, St. Helena.

L. lucidulum, *Michx.*—Pennsylvania to Newfoundland.

L. funiforme, *Cham.*—California? Guadeloupe.

Sect. 2. Antheridiis in amenta congestis, (Lycopodium, *Dill.*—Lepidotis, *Beauv.*)

§3. *Foliis caulinis conformibus, caulem circa circum obsidentibus.*

**Amentis simplicibus: ramis sterilibus et fertilibus difformibus.*

L. inundatum, *Linn.*—Europe and North America.

L. alopecuroides, *Linn.*—United States and Brazil.

***Amentis simplicibus: ramis conformibus.*

L. annotinum, *Linn.* (L. bryophyllum, *Presl.*)—Northern Europe, Asia, and North America.—A var. *β. pungens;* foliis erectis incurvis minus distincte serratis apice mucrone cartilagineo auctis, = L. reclinatum, *Michx.*, Newfoundland, Labrador, Greenland, Kamtschatka. [Also White Mountains of New Hampshire.—A. Gr.]

L. dendroideum, *Michx.* (L. obscurum, *Linn.*)—Newfoundland to the Mountains of Carolina and Oregon. [The author does not notice the two forms with which American botanists are familiar.]

L. sabinæfolium, *Wild.*—British America, Java!!

L. clavatum, *Linn.* (L. integrifolium, *Hook.* L. tristachyum, *Nutt.* L. inflexum, *Swartz.* L. serpens, *Presl*, etc.)—Europe, Northern Asia, India, Java, Japan, South Africa, North America from Newfoundland to North West Coast, Mexico to Brazil.

§4. *Foliis caulinis dimorphis, caule vel compresso vel dorso nudo.*

**Caule dorso aphyllo, ramis humo adpressis.*

L. Carolinianum, *Linn.* (L. repens, *Swartz.*)—New Jersey to Louisiana, Guadeloupe, Guiana, Brazil, Cape of Good Hope, Madagascar, Mauritius, etc. Ceylon?

***Ramis complanatis erectis.*

L. complanatum, *Linn.*—Europe, Northern Asia, India, Java, Newfoundland to Virginia, Jamaica, Mexico to Brazil.

L. alpinum, *Linn.*—Alpine Europe, Asia, and North America.

15. Endlicher, *Mantissa Botanica, sistens Generum Plantarum Supplementum secundum; auctore* Stephano Endlicher, (Vienna, 1842. pp. 114.)—This is the first of the occasional or annual supplements to his invaluable *Genera Plantarum,* which this author proposed to publish, as our readers are aware, (*Amer. Jour.* Vol. xli, p. 373;) and is particularly interesting on account of the synopsis of the anatomical characters of fossil plants which it contains, and which are elaborated from materials furnished by M. Unger of Grâtz. Among the *Acrobrya* or *Acrogens,* several fossil families are established which have no representatives in the vegetation of the present world; viz. the order *Calamiteæ,* placed between Equisetaceæ and Ferns, *Psaronieæ* and *Stigmarieæ,* between the latter and Lycopodiaceæ, and the *Sigillarieæ,* which with *Lepidodendreæ*

follow the Lycopodiaceæ. Besides the extinct Coniferous forms, eighteen fossil Exogenous genera are separately described in an appendix. The next supplement will exhibit the latest views of systematic botanists respecting the lower Cryptogamic, or Thallogenous plants.

16. *Hooker's British Flora.*—We understand that the fifth edition of this deservedly popular Flora of Great Britain has just been published; in which, it is gratifying to learn, the Natural System has at length taken the place of the artificial Linnæan method hitherto employed in that work.

17. *Phillips's Mineralogy, new American edition.*—We learn from the editor that D. Ticknor, of Boston, has in press a new edition of Mr. Phillips's popular treatise on mineralogy, edited by Mr. F. Alger, of Boston.

MISCELLANIES.

DOMESTIC AND FOREIGN.

1. *Entomological Society of Pennsylvania.*—On the 23d of August, 1842, a number of gentlemen devoted to entomological pursuits, met in York, Penn., and established a society under the title above named.

A constitution and by-laws were adopted, and the following gentlemen elected officers. Dr. F. E. Melsheimer, President, Dover, York Co., Pa.; S. S. Haldeman, Esq., Vice President, Marietta, Lancaster Co., Pa.; Rev. D. Ziegler, Rec. Secretary, York, Pa.; Rev. Dr. J. G. Morris, Cor. Secretary, Baltimore, Md.; Rev. S. Oswald, Treasurer, York, Pa.

Dr. Morris read a portion of a descriptive catalogue of the diurnal Lepidoptera of the United States.

The President reported verbally that he is acquainted with the *larvæ* of upwards of five hundred species of our native Lepidoptera; that he had discovered many new species, and is still pursuing his investigations.

The Recording Secretary exhibited his *larvæ* nursery, of which he had a considerable number feeding.

The Vice President called the attention of the Society to several rare Coleoptera found in Pennsylvania, of which he exhibited specimens. Among these were *Capes cinerea*, S., *Cistela marginata*, Zieg., *Hoplia virens*, Hald., *Dicælus violaceus*, Bon.

The Recording Secretary laid on the table a description of three new species of Coleoptera—*Spercheus Americanus*, *Cistela marginata*, and *Pedilus flavicollis*.

The Corresponding Secretary gave a verbal account of a luminous insect captured in Maryland, but was unable to determine whether it was an apterous female of some coleopterous species or a larva. It was more than two inches in length, and emitted, particularly from between the segments, a brilliant light. It died before undergoing any transformation.*

It was resolved that the Society prepare a catalogue (with specific characters of the new species) of the Coleoptera of the United States for publication.

At a meeting of the Society held in Baltimore, October 24th, considerable progress in the preparation of the catalogue was reported. Dr. T. W. Harris of Cambridge, Mass. was elected a corresponding member.

2. ***Foot-Marks and other Artificial Impressions on Rocks:*** in a letter from Prof. W. A. Adams to Prof. Silliman, dated Zanesville, Ohio, Aug. 6, 1842.—I was surprised to find in reading the 87th No. of the "American Journal of Science and Arts,"† that so many respectable authorities could be found on the affirmative side of the question, whether the human foot-prints found at St. Louis on the limestone rock are real impressions, and not works of art. The reasoning of Dr. Owen appears to me to be conclusive that they are artificial.

I have it in my power to communicate some facts tending still farther to illustrate this subject. In the spring of 1839, the high water in the Muskingum River caused a breach in the embankment of the canal at Zanesville. The embankment is constructed on the bank of the river, and is composed of earth, gravel, and fragments of sandstone, heaped upon similar rubbish placed there nearly forty years before, in erecting a mill-race around the natural falls in the river, the whole resting upon a sandstone rock, which constituted the bank of the stream. When this

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* I sent a description of this insect to a distinguished entomologist of Massachusetts, and he will excuse me for here inserting his reply without leave, as his observations on such subjects are justly considered valuable.—J. G. M.

"The beautiful luminous insect mentioned in your letter, has puzzled me much. The largest specimen that I have ever seen, measuring two inches and a half or more in length, was taken in Sutton, Mass., on the stump of a tree, by Dr. ——, who sent it to me in spirit, with an account of its splendid appearance when living. Since then, I have had several specimens alive, some found crawling on the grass in the evening, and others I have taken under stones. Abbot alludes to it in the preface to his "Insects of Georgia," and has sent me a figure of the insect with some remarks upon it. He supposes it to be mature; and from the form and structure of the mandibles, I thought it might be the female of *Phengodes plumosa*. All my specimens died without change, but perhaps if larvæ, they were too young and perished for want of their proper food. It cannot be the larva of an *Elater*, and it differs very materially from the known larvæ of *Lampyris* and *Telephorus*, two of which are mentioned in your letter. Were it not for the remarkable difference in the size of these luminous insects that have fallen under my own observation, I should think that they must be the wingless female of some species of *Lampyris* or *Phengodes*. I hope that you will be able to clear up the history of your insect."

† Vol. XLIII, p. 14—Dr. Owen on human foot-prints in the limestone rock at St. Louis, Missouri, q. v.

embankment gave way, a large body of water passed over this rock, sweeping its surface clean, and leaving the rock exposed, and as it appeared before the first settlers began their improvements. Upon the surface of this sandstone rock, elevated only a few feet above the level of the river, and immediately upon its margin, were found *engraved* the impressions of two human foot-prints, and a number of turkey tracks. That these were the work of art is beyond all question; the human tracks were of the natural size, and accurately drawn; the turkey tracks were of large dimensions. The outlines of the human feet were made by a dotted line, as if a pointed chisel and mallet had been used, and an intaglio attempted by the same instrument. The whole surface within the outline was dotted over, barely removing the original surface of the rock; the form of the turkey tracks was made by a series of dots, and the whole seemed to have been left unfinished. These feet pointed south and down the river.

The discovery excited some curiosity at the time, and the impressions were seen by hundreds of people, who well remember them. Before the embankment was repaired, the part of the rock containing the foot-prints was quarried and broken into fragments. This rock had been covered with earth and loose stone for a period beyond the recollection of any of the inhabitants of the place; there can be no doubt that these sculptures were the work of the Indians. In addition to this, I have been informed that there is the impression of a single foot-print, in a rock situated on the bottom of Licking Creek, about seven miles below Newark, in this state; I have not seen this impression, but am well convinced of its existence; it is described as being occasionally under water. I am not informed of the kind of stone in which this impression is made; if now extant, it is near the narrows of Licking Creek, and in the vicinity of the "*Black hand*"—another interesting monument, which was destroyed in constructing the tow-path of the Ohio Canal.

The north bank of the creek, about seven miles below Newark, is formed of precipitous sandstone rock, forming a perpendicular wall about forty feet in height; upon the surface of this wall, which at this spot leaned a few degrees over its base, and about twenty feet from the bottom, was engraved a gigantic human hand: *the drawing was exact*, and *the proportion accurately preserved.* The hand, including the wrist, was about eight feet long; the outline was cut or scratched on the stone, and the whole space within the outline was stained or painted black; the color remained until the hand was destroyed, and might have endured for ages, as it was protected from rain by the projection of the rock. This hand had been known for more than fifty years before it was destroyed, and the place is yet designated as the "*Black hand.*" I believe these several marks or sculptures are identical with the celebrated and more perfect specimens found at St. Louis. It may perhaps be difficult, at this period, to determine the purpose or meaning of these works of art. I have conversed with some old hunters and surveyors, who are well acquainted with the habits and character of the Indians; by them I am informed that it is the constant habit of the natives, when they quit an encampment, to leave some sign or hieroglyphic, designating the *course* they intend to pursue, and that the place is *abandoned.* For these purposes, what sign could be more appropriate than the prints of feet or *tracks?*

I am assured by an intelligent gentleman, who in early life was engaged in surveying the public lands in the west, that he has often seen such figures; sometimes a hand on a perpendicular surface, and print of feet where the surface is horizontal, as on the bark of a standing tree, or drawn with paint on a flat stone.

From these facts, is it reasonable to conjecture, that when a tribe have been compelled to abandon a permanent home, a more durable memorial of the event may be made by some labored sculpture on a permanent material, such as those found at St. Louis and here? The remarks of Dr. Owen are perfectly satisfactory in explaining the mode in which these works of art could be executed without the aid of iron.

We would add in further illustration of this subject, that Mr. Grey, in his journal of travels in Australia, Vol. I, p. 206,* gives a figure and the following description of a *head cut in sandstone rock.* "I was moving on when we observed the profile of a human face and head cut out in a sandstone rock which fronted the cave; this rock was so hard, that to have removed such a large portion of it with no better tool than a knife and hatchet made of stone, such as the Australian natives generally possess, would have been a work of very great labor. The head was two feet in length, and sixteen inches in breadth in the broadest part; the depth of the profile increased gradually from the edges where it was nothing, to the centre where it was an inch and a half; the ear was rather badly placed, but otherwise the whole of the work was good, and far beyond what a savage race could be supposed capable of executing. The only proof of antiquity that it bore about it was, that all the edges of the cutting were rounded and perfectly smooth, much more so than they could have been from any other cause than long exposure to atmospheric influences."—Eds.

3. *Dr. Draper's discovery of Latent Light, and of a curious class of Spectral Appearances connected with Photography.*—We find in the Nov. number of the London, Edinburgh and Dublin Philosophical Magazine, a letter from Dr. Draper to the editors, from which it appears that certain

* Journals of two Expeditions of Discovery in Northwestern and Western Australia, during the years 1837, '8, '9; by George Grey, Esq. 2 Vols. 8vo. London, 1841.

very curious observations on light, made by him from 1840 to the present time, have been credited to Dr. Möser of Königsberg, and an account given of them by Sir David Brewster, at the last meeting of the British Association.* Here is part of Dr. Draper's letter.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen—If there be a thing in which I have a disinclination to engage, it is controversy of a personal kind with scientific fellow laborers. But, as you well know, it ordinarily happens that there is no other gain to philosophers beyond the *mere credit* of their discoveries, they may be forgiven for reluctantly endeavoring to secure this their only reward.

I have recently returned from a long journey, undertaken for the purpose of making trials on the sunlight in lower latitudes, and am grieved to see in the reports that have reached this country of the Proceedings of the British Association, certain announcements, received from Professor Bessel, of phantoms which can be produced on surfaces by mercury vapor, by the breath, and other means,—*as though the thing were new.* Years ago, if you look into your own Journal, (Feb. 1840, p. 84; Sept. 1840, p. 218; Sept. 1841, pp. 198, 199;) you will find that I had published facts of the kind; spectral appearances, that could be revived on metals, glass, and other bodies, by the breath, by vapor of camphor, by mercury vapor, &c. The very purpose for which I described them, was the striking resemblance of some of them to Daguerreotope images. I I have repeatedly shown, that by placing a coin or any other object on iodized silver, *in the dark*, the vapor of mercury will bring out a representation of it. And in one of the papers just quoted, the condition under which camera images can be reproduced on a silver plate, even after the plate has been rubbed with rotten stone, is described.

I have further seen (Literary Gazette, July 23, 1842, Paris letter) that the fact that light becomes latent in bodies, after the manner of heat, was announced in France as a new and important discovery of Prof. Möser of Königsberg. In your own Journal, more than a year ago, you printed a long paper written by me on this very topic, (September, 1841, pp. 196, 204, 205, 206,) not merely announcing the fact, but giving rude estimates of the amounts: more exact numerical determinations I have *now* nearly ready for the press.† Yours truly, J. W. Draper.

University of New York, Sept. 26, 1842.

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* The report of Prof. Möser's alleged discovery, from the Athenæum, is inserted at page 159 of our present number.—Eds. Am. Jour.

† Since the above was in type, we have received the December number of the London, Edinburgh and Dublin Philosophical Magazine, and find much new, interesting and important matter relative to this subject, from Dr. Draper and Mr. Hunt, (pp. 453, 462;) also in the Athenæum for Nov. 19, an abstract of Mr. Hunt's paper, and further accounts of Prof. Möser's observations. We hope in our next to give the details of a discovery which promises to be of as much interest as the original observations of Daguerre.—Eds. Am. Jour.

Photography.—We copy the following from the same letter of Prof. Draper from which the foregoing is taken :—

The accompanying photographic impression of the solar spectrum, which I will thank you to give to Sir John Herschel, was obtained in the south of Virginia: probably you can make nothing like it in England, the sunlight here in New York wholly fails to give any such result. It proves, that under a brilliant sun, there is a class of rays commencing precisely at the termination of the blue, and extending beyond the extreme red, which totally and perfectly arrest the action of the light of the sky. This impression was obtained when the thermometer was 96° Fah. in the shade, and the negative rays seem almost as effective in protecting, as the blue rays are in decomposing iodide of silver.

The most remarkable part of the phenomenon is, that the same class of rays makes its appearance again beyond the extreme lavender ray. Sir J. Herschel has already stated, in the case of bromide of silver, that these negative rays exist low down in the spectrum. This specimen, however, proves that they exist at both ends, and do not at all depend on the refrangibility. It was obtained with yellow iodide of silver, Daguerre's preparation, the time of exposure to the sun fifteen minutes.

In this impression, six different kinds of action may be distinctly traced by the different effects produced on the mercurial amalgam. These, commencing with the most refrangible rays, may be enumerated as follows:—1st, protecting rays; 2d, rays that whiten ; 3d, rays that blacken; 4th, rays that whiten intensely; 5th, rays that whiten very feebly; 6th, protecting rays.

It is obvious we could obtain negative photographs by the Daguerreotype process by absorbing all the rays coming from natural objects, except the red, orange, yellow, and green, allowing at the same time diffused daylight to act on the plate.

This constitutes a great improvement in the art of photography, because it permits its application in a negative way to landscapes. In the original French plan the most luminous rays are those that have least effect, whilst the sombre blue and violet rays produce all the action. Pictures, produced in that way, never can imitate the order of light and shadow in a colored landscape.

If it should prove that the sunlight in tropical regions differs intrinsically from ours, it would be a very interesting physical fact. There are strong reasons to believe it is so. The Chevalier Fredrichstal, who travelled in Central America for the Prussian government, found very long exposures in the camera needful to procure impressions of the ruined monuments of the deserted cities existing there. This was not due to any defect in his lens; it was a French achromatic, and I tried it in this city with him before his departure. The proofs which he obtained, and which he did me the favor to show me on his return, had a very remarkable aspect. More recently, in the same country, other competent travellers have experienced like difficulties, and, as I am informed, failed to get any impressions whatever. Are these difficulties due to the antagonizing action of the negative rays upon the positive?

4. *On the Salt Steppe south of Orenburg, and on a remarkable Freezing Cavern;* by Roderick Impey Murchison, Esq., Pres. G. S.—This salt steppe is distinguished from many of those which are interposed between the Ouralsk and the Volga, or are situated on the Siberian side of the Ural Mountains, by consisting not of an uniform flat resembling the bed of a dried-up sea, but of wide undulations and distantly separated low ridges; nevertheless it is, Mr. Murchison states, a true steppe, being devoid of trees and little irrigated by streams. The surface consists of gypseous marls and sands, considered by the author to be of the age of the zechstein,* and it is pierced in the neighborhood of the imperial establishment of Illetzkaya Zatchita by small pyramids of rock salt. These protruding masses attracted the attention of the Kirghiss long before the country was colonized by the Russians, but it is only during a short period that the great subjacent bed has been extensively worked. The principal quarries, exposed to open day, are situated immediately south of the establishment, and have a length of three hundred paces, with a breadth of two hundred, and a depth of forty feet. The mass of salt thus exposed, is of great purity, the only extraneous ingredient being gypsum, distantly distributed in minute filaments. At first sight the salt seems to be horizontally stratified, but this apparent structure, Mr. Murchison states, is owing to the mineral being extracted in large parallelopipedal blocks twelve feet long, three feet deep and three wide. On the side where the quarry was first worked, the cuttings presented, in consequence of the action of the weather, a vertical face as smooth as glass, but at its base there was a black cavern formed by the water which accumulates at certain periods of the year, and from its roof hung saline stalactites. The entire range of this bed of salt is not known, but the mass has been ascertained to extend two versts in one direction, and Mr. Murchison is of opinion that it constitutes the subsoil of a very large area; its entire thickness also does not appear to have been determined, but it is stated to exceed one hundred feet. The upper surface of the deposit is very irregular, penetrating, in some places, as already mentioned, the overlying sands and marls.

* His extensive surveys of Russia have convinced Mr. Murchison that rock-salt and salt springs occur in all the lower sedimentary rocks of that empire, from great depths below the Devonian or old red sandstone system to the zechstein and the overlying marls and sandstones.

In consequence of the salt occurring at so small a depth, every pool supplied with springs from below is affected by it;* and one of them used by the inhabitants as a bath is so highly charged with saline contents that there is a difficulty in keeping the body submerged, and the skin on leaving the pool is encrusted with salt. This brine swarms with animalcules.

Mr. Murchison then describes the freezing cavern and the phenomena exhibited by it. The cave is situated at the southern base of a hillock of gypsum, at the eastern end of the village connected with the imperial establishment; and it is one of a series of apparently, for the greater part, natural hollows, used by the peasantry for cellars or stores. The cave in question is, however, the only one which possesses the singular property of being partially filled with ice in summer and of being destitute of it in winter. "Standing on the heated ground and under a broiling sun, I shall never forget," says the author, "my astonishment when the woman to whom the cavern belonged unlocked a frail door, and a volume of air so piercingly keen struck the legs and feet that we were glad to rush into a cold bath in front of us to equalize the effect." Three or four feet within the door and on a level with the village street, beer and quash were half frozen. A little further the narrow chasm opened into a vault fifteen feet high, ten paces long, and from seven to eight wide, which seemed to send off irregular fissures into the body of the hillock. The whole of the roof and sides were hung with solid undripping icicles, and the floor was covered with hard snow, ice, or frozen earth. During the winter all these phenomena disappear, and when the external air is very cold and all the country is frozen up, the temperature of the cave is such that the Russians state they could sleep in it without their sheep-skins.—*Lond. Edin. and Dub. Phil. Mag. for Nov.* 1842.

5. *Extracts from a letter addressed by Sir J. Herschel, Bart., F. G. S., to Mr. Murchison, explanatory of the Phenomena of the Freezing Cave of Illetzkaya Zatchita.*—"That the cold in ice-caves (several of which are alluded to in a part of this letter not published) does NOT *arise from evaporation*, is, I think, too obvious to need insisting on. It is equally impossible that it can arise from condensation of vapor, which produces heat, not cold. When the cold (by contrast with the external air, i. e. the difference of temperature) is greatest, the reverse process is going on. Caves in moderately free communication with the air are dry and (to the feelings) warm in winter, wet or damp and cold in summer. And from the general course of this law I do not consider even your Orenburg caves

* The abundance of these brine-springs in various parts of Russia must lead, the author says, to the abandonment of Pallas's hypothesis, that the saline pools and lakes are the residue of former Caspians; though he admits that some of the vast low steppes of the South formed the bottom of a former condition of the existing Caspian.

exempt, since however apparently *arid* the external air at 120° Fahr.! may be, the moisture in it may yet be in excess and tending to deposition, when the same air is cooled down to many degrees beneath the freezing point.

"The data wanting in the case of your Orenburg cave, are *the mean temperature of every month in the year of the air*, and of thermometers buried say a foot deep, on two or three points of the surface of the hill, which if I understand you right is of gypsum and of small elevation. I do not remember the winter temperature of Orenburg, but for Catherinenburg (only 5° north of Orenburg) the temperatures are given in Kuppfer's reports of the returns from the Russian magnetic observatories. If any thing similar obtains at Orenburg I see no difficulty in explaining your phenomenon. Rejecting diurnal fluctuations and confining ourselves to a single summer wave of heat propagated downwards alternately with a single winter wave of cold, every point at the interior of an insulated hill rising above the level plain will be invaded by these waves in succession, (converging towards the centre in the form of shells similar to the external surface,) at times which will deviate further from mid-winter and mid-summer the deeper the point is in the interior, so that at *certain depths* in the interior, the cold-wave will arrive at mid-summer and the heat-wave in mid-winter. A cave (if not very wide-mouthed and very *airy*) penetrating to such a point will have its temperature determined by that of the solid rock which forms its walls, and will of course be so alternately heated and cooled. As the south side of the hill is *sunned* and the north *not*, the summer wave will be more intense on that side and the winter less so; and thus, though the *form* of the wave will still generally correspond with that of the hill, their *intensity* will vary at different points of each wave-surface. The analogy of *waves* is not strictly that of the progress of heat in solids, but nearly enough so for my present purpose.

"The mean temperature for the three winter months, December, January, February, and the three summer months, June, July, August, for the years 1836, '7, '8, and the mean of the year, are for Catherinenburg as follows:—

Years.	Winter.	Summer.	Annual mean.
1836,	−10°.93 R.	+11°.90 R.	+1°.22 R.
1837,	−12°.90	+12°.93	+0°.30
1838,	−12°.37	+12°.37	+0°.60
Mean,	−12°.07 R.	+12°.40 R.	+0°.70 R.
	+ 4°.83 Fah.	+59°.9 Fah.	+33°.57 Fah.

"The means of the intermediate months are almost exactly that of the whole year, and the temperature during the three winter as well as the three summer months most remarkably uniform.

"This is precisely that distribution of temperature over time which ought under such circumstances to give rise to well defined and intense waves of heat and cold; and I have little doubt therefore that this is the true explanation of your phenomenon.

"I should observe, that in the recorded observations of the Catherinenburg observatory, the temperatures are observed two-hourly, from eight, A. M. to ten P. M., and not at night. The mean monthly temperatures are thence concluded by a formula which I am not very well satisfied with; but the error, if any, so introduced, must be far too trifling to affect this argument. The works whence the above data are obtained, are '*Observations Météorologiques et Magnétiques faites dans l'intérieur de l'Empire de Russie*,' and '*Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines de Russie*,' works which we owe to the munificence of the Russian government, and which it is satisfactory to find thus early affording proofs of utility to science in explaining what certainly might be regarded as a somewhat puzzling phenomenon, as it is one highly worthy of being further studied and being made the subject of exact thermometric researches on the spot, and wherever else any thing similar occurs."—*Lond. Ed. and Dub. Phil. Mag. for Nov.* 1842.

6. *Further observations on the Meteors of August* 10, 1842.—Such partial observations as the weather at this place permitted, were published in the last number of this Journal, (Vol. XLIII, p. 377,) from which it was confidently inferred that the meteoric display of the tenth of August recurred the present year with undiminished numbers. Various European observations which have reached us, confirm the justness of this conclusion.

At *Vienna*, according to a letter from Mr. C. J. Littrow, (published in the *Literary Age*, Vol. I, p. 22, Dec. 14, 1842,) there were observed by this astronomer and his assistants, on the night of the 10th of August last, *seven hundred and seventy-nine* shooting stars. The number of observers is not mentioned.

The following information has been obligingly communicated to me by M. Quetelet of Brussels.

At *Parma*, in Italy, observations were made by M. Colla. Aug. 9, from 9h. 3m. to 15h. 34m., 252 shooting stars were noted; on the 10th, from 8h. 36m. to 15h. 20m., 490 were seen. The number of observers is not stated.

The following table contains the results of the two nights taken together.

From	8h. 36m.	to 9,	. . . .	3 meteors.
"	9	" 10,	. . . .	37 "
"	10	" 11,	. . . .	96 "
"	11	" 12,	. . . .	84 "

From 12h.	to 13,	127	meteors.
" 13	" 14,	175	"
" 14	" 15,	170	"
" 15	" $15\frac{1}{2}$,	50	"

At *Berlin*, there were noted by M. Boguslawski, on the night of August 9th, from 9h. 14m. to 14h. 48m., 401 meteors; on the following night, from 9h. 7m. to 15h., 783. Number of observers not stated.

At *Brussels*, observations were made by M. Quetelet and an assistant, in concert with M. Boguslawski at Berlin, for the determination of differences of longitude. On the night of the 9th, from 9h. 30m. to 13h. 30m., 150 meteors were noted by two observers. On the night of the 10th, during 136 minutes, between 10h. and 14h. 26m., 167 meteors were noted. Part of this time there was only one observer. On the night of the 11th, during 120 minutes, between 10h. and 13h. 30m., 110 meteors were noted. There was on these occasions a decided radiating point, in the vicinity of the constellation Perseus, as in former years. The moon of course rendered invisible many meteors, which would have been seen in her absence; and many more must have been lost, for want of a sufficient number of observers. E. C. H.

7. *Meteoric Observations in November*, 1842.—Although it was not expected that a recurrence of the meteoric display of the thirteenth of November would be observed the present year, yet it was deemed desirable to keep watch at that period. The morning of the thirteenth proved to be so cloudy as to prevent observation. On the morning of the 14th, Mr. Joseph S. Hubbard and myself watched in the open air for one hour, commencing at three o'clock. During this time we saw 46 shooting stars, (25 in the W. and 21 in the E.,) the sky being partially obscured by clouds. At 4 o'clock the sky had become so cloudy that it seemed useless to watch longer. The meteors were not remarkable for brilliancy or for uniformity of motion; about two thirds of them obeying the usual radiant tolerably well. The Zodiacal Light was, as is common at this season of the year, very conspicuous, extending upward to α Leonis, and as seen by indirect glances, nearly as far as the nebula in Cancer.

The morning of the 15th was clear. Mr. Francis Bradley, watching alone in the northern sky for twenty minutes, observed *seven* meteors. On the mornings of the 16th and 17th the sky was overcast. On the evening of the 21st there was a considerable display of the *Aurora Borealis*. No certain indication of this phenomenon had been previously seen here since the 29th of September.

From the foregoing observations, it was concluded that there was this year at this place no very decided recurrence of the November meteoric display.

Rochester, N. Y.—A notice signed W. G., and published in the Western New Yorker, Rochester, Nov. 22, states that at that place, "the evenings of the 12th, 13th and 14th were covered with clouds, except at intervals, and during these a bright moon prevented observation. On the morning of the 15th, during the short interval that elapsed between the setting of the moon and daylight, a large number were observed—in one instance no less than six or seven at a time, and scattering ones continued to fall until the light of the morning hid them from view. They all seemed to start from about the same point in the heavens as in former observations, viz. in the N. E., at an elevation of about 60°; and their movement was to the S. E., though there were a few exceptions. At the rate they fell during the few minutes they were observed, the number would be from 150 to 200 in an hour."

It must be remembered, however, that it is not safe to infer the hourly number of meteors, from observations continued only a few minutes. It often happens that after a barren interval of many minutes, two, three or more meteors will appear in rapid succession, to be followed by another barren interval of considerable duration. E. C. H.

8. *Meteoric Observations in December*, 1842.—From the 4th to the 13th of this month, the weather here has, with few exceptions, been exceedingly unfavorable for celestial observation. Since the year 1838, the meteoric display of December 6–7 has apparently failed; but the weather has scarcely permitted us here to determine the question as regards the present year. The evenings of Dec. 4, 5, 6, 8, 11, 12, and 13, were overcast. On the 7th and 9th I watched alone soon after dark, for about half an hour, and saw each time only two or three meteors. The moon was shining, and the sky partly obscured. On the evening of the 10th, Mr. Bradley and myself watched an hour, ending 7h. 15m. P. M., and saw but three meteors. During this hour the moon was faintly shining, and the sky about two thirds obscured. The mornings of the 10th, 11th, and 12th were overcast.

In a letter published in *L'Institut*, No. 422, M. Colla states that at Parma, the number of shooting stars visible Dec. 6–7, 1841, was uncommonly small. On the nights of the 10th and 11th, however, he observed at that place, an unusually large number of these meteors. In half an hour during the night of the 11th, he counted in the northern quarter, 23 very brilliant meteors, nearly all of them having luminous trains. Their general direction was from S. to N. An assistant reported that the next morning meteors pursuing the same direction were uncommonly abundant. A similar phenomenon was, according to M. Colla, remarked at Parma in 1833 and 1836, on the same night, viz. Dec. 11–12.

In the same letter M. Colla mentions several other remarkable occurrences of shooting stars in 1842, viz. July 22; Aug. 9–12, 24 or 25; Sept. 10, 18, 19, 20; Oct. 10, 17, 25; Nov. 19. He does not state the number of meteors actually counted on these several occasions. With the exception of Aug. 9–12, perhaps none of these occurrences would be considered remarkable, if we assume *thirty per hour* to be the mean number of shooting stars visible at one place. E. C. H.

9. *Solar Eclipse of July* 8, 1842.—In Vol. XLII, (pp. 175—181,) we published a paper by R. T. Paine, Esq., of Boston, containing an elaborate computation of the phases of the total eclipse of the sun of July 8, 1842, for several places on the continent of Europe. The eclipse was carefully observed by numerous eminent astronomers. We are much gratified to learn by a letter from Mr. C. J. Littrow, Director of the Imperial Observatory at Vienna, to Mr. Paine, (*Literary Age*, Philad. Vol. I, p. 22,) that the computations of the latter corresponded remarkably well with the observations. Mr. L. remarks: "The observations accord better with your calculations than with those of any European astronomer. The comparison is as follows:

	h. m. s.		s.
Beginning of eclipse,	5 51 51.9	Diff. from your predic.	+33.9
" total darkness,	6 49 24.9	" "	+26.9
End "	6 51 21.9	" "	+26.9
Duration "	1 57.0	" "	0.0
End of eclipse,	7 53 55.9	" "	+19.9

"These differences are so small and so regular, as to leave nothing to be desired, and I should be glad to know the method which you have used. Accordingly, I take the liberty of offering you as a medium of publication of your calculations, the Annals of our Observatory."

10. *Discovery of a Comet.*—At 7 P. M., October 28, 1842, a telescopic comet was discovered in the constellation *Draco*, by M. Laugier, Astronomer at the Paris Observatory. It was extremely faint, and without any appearance of tail. At 10h. 10m. P. M., Paris mean time, its R. A. was 16h. 41m., N. dec. 68° 44′. In six hours the R. A. increased 3m. 34s. and the declination decreased 20′. M. Laugier observed it again Nov. 2, 4, 5, and computed its parabolic elements. Pingré's *Cometographie* mentions a comet seen in China, A. D. 1301, the elements of which, calculated from the observations of the Chinese, accord in a remarkable manner with the results of the new calculation. It is therefore possible that M. Laugier has had the good fortune to record the second passage of a comet whose period occupies more than five hundred years. On the 14th of November M. Laugier made to the Academy of Sciences of Paris a further communication on the comet, announcing that on the next day it would be at its nearest point to the earth, from which however it would then be distant more than seven millions of leagues.—*Lond. Athen. Nov.* 1842.

11. *Prof. Espy's Meteorological Enterprise.*—It is probably known to most cultivators of meteorology in America, that Prof. J. P. Espy, formerly of Philadelphia, has become connected with one of the departments of the War Office at Washington City. He has published blank forms for recording meteorological phenomena, and is ready to distribute them gratis to all who will take the trouble to keep a register and return it monthly to him, directed to the Adjutant General's Office, endorsed on one corner "Meteorology." His object is to collect the greatest possible amount of meteorological matter, and particularly records of violent storms of wind and rain. His position at the seat of government, with the powerful aid of the War Office, and the regular monthly returns of the meteorological registers kept at all our military posts, as well as those forwarded by colleges, academies, schools, and individuals, will enable him to command more extensive and valuable materials than have ever been in the possession of any single observer in this country.

We strongly recommend to all whose observations are not promised in another channel, to throw their efforts into the common cause. Copies of all registers kept for other purposes, are also most earnestly desired.

From such a mass of materials as can in this way be collected, great results must be expected, when we shall be able to solve the details by some generally accepted and unobjectionable mode. For by whatever *theory* they may be claimed, the *facts remain*, and in the end can sustain only the truth.

12. *Copal.**—This resin is found upon the coast of Africa. There are no trees in the vicinity, nor any thing to indicate that there ever were any. It is found a few inches below the surface of the ground, in beds resembling lava. The natives who gather it can give no account of its origin. When taken from the bed, the gum is covered with a black earthy substance, which nothing can remove but the strongest lye. As the only establishment known to exist for cleansing it, is at Salem, Mass., all the gum brought to this country is sent there. It is placed in vast vats, into which strong lye is poured, and after remaining there some days, it is removed, spread upon boards, and dried in the sun. The action of a stiff brush then removes the coating, and renders it fit for use. It is then assorted, the clear (which is the first quality) separated from the dark and the spotted; and it is then packed in boxes, and sent to all parts of the world.

By removing the native coating, the gum is left with a pale gold color. But by cutting with a knife through the second coating, a brilliant surface is presented, that nearly equals the brilliancy of precious stones. In many pieces of gum insects are found, large, perfect and beautiful; also fluids, some transparent, some colored.

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* This is an anonymous MS. communication to the Editors.

13. *Notice of the Discovery of an Electrical Fish on our coast:* in a letter from D. Humphreys Storer, M. D. to the Editors, dated Boston, Dec. 23, 1842.—A species of Ray possessing electrical powers has been known to the fishermen of Cape Cod and New York for many years, and called by them the *cramp-fish* or *numb-fish.* Mitchill in his paper on the "Fishes of New York," contained in the first volume of the Transactions of the Literary and Philosophical Society of New York, refers to this species; he had never seen it, but, from the facts he was enabled to collect respecting it, he supposed it to be identical with the European species, "*Raia torpedo*," and as such, introduces it into his memoir. In my Report on the Fishes of Massachusetts, I merely observed that a Torpedo was found on the coast of Cape Cod, but being unable to procure a specimen, I could not identify it. I have had the good fortune to procure a fine specimen within the last month, which was captured at Wellfleet; it was four feet and two inches in length, and proves to be the *Torpedo nobiliana,* Buonaparte. It agrees perfectly with Mr. Thompson's description, in the fifth volume of the Annals of Natural History, of a specimen taken on the Irish coast in 1838.

As some time may elapse before I can publish a contemplated paper on our fishes, I would avail myself of your valuable Journal to make the above mentioned fact known to ichthyologists.

14. *To detect minute quantities of Arsenic and Antimony.*—Dr. Brett, wishing to ascertain the minimum quantity of arsenic that can be detected by Marsh's process, has by experimenting on an alloy of zinc and arsenic, been able to detect one part in 5000; and by dissolving the arsenious acid in water, the minimum quantity of arsenic is =.00469 part. The sulphuric acid and zinc were previously purified.

With an alloy of zinc and antimony, 1 part antimony to 13000 zinc, would be detected by the metallic stain. In examining the sesquioxide of antimony under the form of tartar emetic in solution, the minimum quantity was =.00522 gr. When the quantity of the metals is very minute, the metallic stains are not apparent on white porcelain for some minutes, and the deposition, with a considerable volume of the gas, may even go on for fifteen or twenty minutes. The color of the arsenical stain is brown, and its intensity and that of the metallic stain increases with the quantity. The antimonial stains are of a dark, almost black color, except when approaching the minimum quantity of antimony, then the color resembles the brown of arsenical stains. When the hydrurets of arsenic and antimony are mixed, then (unless excessively minute) each exhibits its characteristic color, the brown arsenical stain appearing around the dark and almost black antimonial stain, owing probably to the greater volatility of the arsenic.—*Lond. Edin. and Dub. Jour. May,* 1842.

15. *Mineralogical systems.*—The following is the substance of the remarks by Prof. Whewell in his History of the Inductive Sciences, Vol. III, p. 526. In speaking of Mohs's system and of its failure, he says, the design of giving to all minerals names in accordance with his system was too bold to succeed. A new nomenclature truly was needed; but an improved classification ought to have furnished an improved nomenclature, as in botany by Linnæus. Mohs had not prepared his verbal novelties with the temperance and skill of the great botanical reformer. He called on mineralogists to change the name of almost every mineral with which they were acquainted, and the proposed appellations were mostly of a cumbrous form. Berzelius contrived a purely chemical system, on the *electro-positive* principle, both as to minerals and elements. Mitscherlich's *isomorphism* showed that minerals with very different electro-positive elements could not be distinguished. Berzelius then assumed the *electro-negative* character of the formative element as the basis of his system, viz. all sulphurets together, all the oxides, and all the sulphates. Gmelin, in 1825, assumed the same fundamental principle, and took account of the number of atoms or proportions; e. g. the silicates were simple, double —quintuple, as pitchstone—sextuple, as pearlstone, &c. Nordenskiöld assumed the same bases, but regarded also crystalline form. Beudant proceeded on the *electro-negative* principle, and on Ampère's circular arrangement of elementary substances.

Such schemes exhibit rather a play of mere logical faculty, exercising itself on assumed principles, than any attempt at the real interpretation of nature. Both these attempts by Mohs and Berzelius failed because there was no coincidence between them. A chemical arrangement and a natural history system should agree, and thus verify each other. The interior and the exterior systems should be *type* and *antitype, else they are not successful.*

He then proceeds to give other reasons. Berzelius's electro-*positive* principle was soon found false. Why should not the electro-*negative* prove so also? The former is isomorphous and the latter too; e. g. arsenic and phosphoric acids. How do we know that the external properties depend on electrical properties? The composition of all minerals, even of those most analyzed, is not yet certain; and this ancient notion of the "composition of a species," is unsettled by *isomorphism.* The constancy of the angles also is rendered doubtful by *plesiomorphism.* The *optical* properties, yet so imperfectly known, are somewhat arbitrary and capricious. The *chemical* and *optical* mineralogists have been obliged frequently of late to separate species which had been united, and to bring together those which had been divided. Every thing shows that classification *is still to begin.* A *fixity* of characters for the foundation of species is *not yet known.* The natural history classifiers assume that they can discover the *relative value* and *importance of those characters.* The *grouping* of species into a genus, or genera into an order, is not by *definite rules*, but by a *latent talent of appreciation*—a sort of *classifying instinct*. These are *purists*. Naumann in 1828 adopted a mixed system, the chemical and natural history. He made *haloides*, *salts of oxides*, *unmetallic* and *metallic*, and these *hydrous* and *anhydrous*, which divisions give good natural groups.

The *siliceous minerals* are the most difficult to arrange. He calls them *silicides*—metallic and unmetallic, amphoteric—and again hydrous and anhydrous. This is a good basis for future researches. Natural history began to make its appearance in systems of chemistry. The combination of the chemical, crystallographical, and optical properties, into some lofty generalizations, is probably a triumph reserved for future and distant years; for a complete geological survey of the whole earth is requisite as a foundation for sound theory.

16. *Atomic Weights of Elements.*—MM. Marchand and Erdmann are at present engaged in a series of researches, which seem to prove that Prout's idea, that all atomic weights are multiples of that of hydrogen, is correct. They have as yet examined only the following bodies:

Oxygen	= 100.	. . 8	Calcium	= 250	. . 20
Hydrogen	= 12.5	. . 1	Chlorine	= 450	. . 36
Carbon	= 75.	. . 6	Silver	= 1250	. . 100
Nitrogen	= 175.	. . 14	Lead	= 1300	. . 104

Phil. Mag. Nov. 1842.

17. *Sugar in the Stalks of Maize.*—Dr. Pallas, principal physician of the military hospital of St. Omers, addressed to the French Academy of Science during the past summer a memoir, in which he attempts to establish the following propositions—viz. 1st, that the stems of Indian corn contain little or no sugar before flowering: 2d, that just at the epoch of flowering only a mere trace of sugar can be extracted from them: 3d, that the same stems, at from twenty to twenty five days after flowering, while the grain is yet milky, contain one per cent. of crystallizable sugar: 4th, that still later, when the grain is perfectly ripe, the stems yield two per cent. of coarse sugar, besides four per cent. of rich molasses. The commission to whom this memoir was referred, (M. Biot, reporter,) however, very properly discredit these results, so contradictory not only to our theoretieal views, but also to all former observations; as, for example, those of Prof. Burger, who affirms that the sugar is most abundant in the juice of the stem of maize immediately after the expansion of the flowers, and that it diminishes as the grain is matured. The commission states that the experiments of Dr Pallas on this subject were so loosely conducted as to afford no results worthy of confidence.—*Comptes Rendus, August* 29, 1842.

18. *Correction of the statement concerning an alleged "Shower of red matter like blood and muscle."*—We published, at the request of Dr. Troost, (in this Journal, Vol. XLI, pp. 403, 404,) an account of what was believed by him and other competent judges to have been a shower or fall of red blood and muscle from the atmosphere. Our own belief in the facts was never full; not doubting that Dr. Troost had given a faithful account of the supposed phenomenon as related to him; but the event seemed so improbable, that we always feared some foul play on the part of the informers. Not long after our account was published, it was announced in the newspapers that the whole affair was a hoax devised by the negroes, who pretended to have seen the shower, for the sake of practising on the credulity of their masters. They had scattered the decaying flesh of a dead hog over the tobacco leaves. We are led at this late day to call attention again to what ought to be forgotten, because we are reminded by a notice in L'Institut for May 15, 1842, that we have never contradicted the statement in our pages, and that it therefore has been quoted by the editor of L'Institut as authority against a true statement of the facts, contained in a letter from M. de Castlenau, published in "Le National," a public journal. We regret that our neglect should have been quoted in support of error, and beg the editor of L'Institut to correct the statement.

19. *Obituary.*—It is our painful duty to record the death of Baron LOUIS LEDERER, Consul General for the Austrian States. His death took place on the 22d of December, 1842, at his residence in New York. Baron Lederer has been for many years a resident in this country, and has been long known as a zealous mineralogist. The death of this excellent man will be deeply felt by all who knew him, and particularly by his mineralogical friends in this country, who are under many obligations to him for his uniform kindness, and the important aid he was able to render them by means of his close connection with the imperial cabinet at Vienna. His collection of foreign minerals was purchased three or four years since by the University of Michigan, and his collection of American specimens in mineralogy, which is believed to be unique in its fullness, was offered by him for sale just before his death.

We had engaged to Baron Lederer, to publish in this number his own specification of the contents of his American cabinet, in relation to its sale in the United States; but not knowing what may be the views of his successors and representatives, we forbear until farther instructed.

APPENDIX

TO THE AMERICAN JOURNAL OF SCIENCE AND ARTS, VOL. XLIV, NO. 1.

VINDICATION

OF CLAIMS

TO CERTAIN INVENTIONS AND IMPROVEMENTS

IN THE

GRAPHIC ART.

BY ASA SPENCER.

Editorial Remark.—This communication came too late for insertion in the body of this number. By Mr. Spencer's desire, it is added in the present form. Although we are averse to controversy, we cannot object to the vindication of the just claims of individuals, especially when a discussion is conducted as in the present case, by both parties with perfect courtesy, and a disposition to do, as well as to claim justice.

Philadelphia, July 20, 1842.

Gentlemen—A few days since an acquaintance directed my attention to your very valuable work, entitled "A Manual of Coins and Bullion," published at the Assay Office of the Mint.

On looking over the sixth chapter, which contains a description of the plates, I was surprised to see in a work from such an enlightened quarter, so erroneous on account of the origin and progress of the medal-ruling machine, by which it is made to appear that the invention and improvement of the machine, belong exclusively to two gentlemen of the Mint, and I am spoken of as a mere copier from Mr. Gobrecht.

I will endeavor to give a plain and simple history of the whole affair. In the fall of 1816, I came to this city with Mr. Jacob

Perkins, whose object was, in conjunction with Messrs. Murray, Fairman & Co., to introduce into the art of bank-note engraving, a new and original style of work, the production of a machine invented by me, and called the Geometric Lathe. Mr. Gobrecht was, at that time, employed in the same establishment, which afforded us frequent opportunities of conversing on subjects connected with the business. He talked much concerning ruling machines—had a great desire to possess one, and told me he had attempted to make one, to be moved by a screw, but that it failed from the imperfection of the screw. He thought, however, that by the aid of such means and implements as were then in my possession, he could be furnished with a screw of sufficient accuracy for the purpose. I undertook to assist him; but had not proceeded far when I became convinced, that the plan we were upon would not answer. Having, however, embarked in the enterprise, I was unwilling to give it up, and began immediately to study some other plan; when a modification of the wheel and axle, like that of the plating mill, was fixed upon by me. I then made known to Mr. Gobrecht my entire want of faith in his old plan, and described to him my new one, in which I had the fullest confidence. He, however, was not pleased with it, and seemed loath to give up his own, but after consulting with Mr. Perkins, who gave a decided preference to my plan, and spoke in the highest terms of it, he became anxious to have the first machine on the new plan. I undertook it for him; my other engagements made it necessary, that I should get it up in the simplest form, and with the least possible labor. I had previously to this furnished Mr. Gobrecht, at his request, with a very simple plan for ruling waved lines, taken from a movement in the Rose Engine, and easily attached to any ruling machine, consisting of an arm or lever, with a point or touch, as it is called in the Rose Engine, resting on a waved surface and kept in contact with it by a suitable weight or spring. This touch being connected with the slide which carries the etching point, and made to move over the waved surface or model, a vibratory motion thus caused was, by joints adjusted for the purpose, communicated to the etching point.

Thus the machine was placed in Mr. Gobrecht's hands, with the exception, merely, of the waved surface or model, which was left for him to supply, to suit himself. This last appendage to the machine, (taken from the Rose Engine,) had not been in operation when he took it away. I have no recollection of having heard Mr. Gobrecht speak of the machine afterwards.

Soon after the delivery of it, Mr. Gobrecht exhibited his representation of the head of the Emperor Alexander. The novelty of its appearance attracted great attention, and was a subject of eager inquiry among the curious, as to how it was done, but Mr. Gobrecht maintained a rigid silence on the subject. It remained a profound and guarded secret, until I had prepared a machine of the same kind for my own use, and taken it to London, whither my business called me, and where it was finished and put in operation by me; and then and there, for the first time, I saw the appendage for ruling waved lines, in action, and was surprised to see with what truth and accuracy the model was represented on the plate under the etching point. The idea of changing the model immediately suggested itself. The waved model was removed, and a shilling put in its place, when a tolerable copy was at once produced. A distinguished machine-engraver tried the same experiment by my directions, at his place in Somerstown near London, with great success. When I asked him how he had succeeded, his answer was, "perfectly, it would not only copy the waved model, but any device that I put under the touch."

During my stay in London, nothing was done to bring the art of medal ruling into notice. On my return to Philadelphia, I was the first to unveil the mystery of medal ruling, but being deeply engaged in introducing improvements in bank-note engraving, I gave no attention to it until about 1826 or '7, when thinking it might afford an additional security against counterfeiting, I put my machine a little in order for experimenting, and executed a large plate, containing medals, a copy of which I herewith send you. The work was much admired. Impressions from the plate soon found their way to Europe, and excited to exertion the ingenious mechanicians of London and Paris. Mr. Saxton of this city also exercised his ingenuity on the subject. I do not perceive, however, in these specimens a greater degree of perfection than in my own. I find not the least difficulty in believing, that Mr. Saxton was enabled, by his own scientific skill, to remove the cause of the distortion. When this defect was pointed out to me in my specimen, which was not till some time after it was done, (it was not very glaring, and, in low relief, would not have been noticed,) I took an opportunity to search for the cause, and when found, it was, in my machine, remedied with great ease. I had only to lower the medal, allowing the touch to follow it down, until a line, drawn from the point of contact to the joint or fulcrum on which the touch turned, was at the proper angle of descent, and from the great number of productions which I have examined, there must be many besides Mr. Saxton and myself, who have got over the difficulty with the same ease.

I have been thus circumstantial, in order to shew how the result was arrived at. The circumstances which I have detailed will be recollected, in general, by all those who were in the establishment of Murray, Fairman & Co. at the time, but fully and particularly remembered by Mr. Gobrecht. All of this may have little to do, perhaps, with medal ruling in the abstract, the origin of which, in its truth and simplicity, may be made known in a few words. No one's ingenuity was taxed for this particular purpose. The movement taken from the Rose Engine, and by me applied to a ruling machine of my own invention, was designed for procuring waved lines and nothing more. When it was put in motion, it copied the wave with great truth and precision, and at the same time manifested, in a manner not to be mistaken, the fact, that it had also the power of copying medals with equal exactness and beauty.

In your account also it is said, that copies could not be taken *immediately* from the coins, because the picture would be reversed and the legends would read backwards, and that it was, therefore, necessary to obtain the impressions in metal hard enough to bear the tracer, and that without the seasonable invention of the electrotype by M. Jacobi, the work could not have gone on.

The modern art which you speak of, ingenious as it most certainly is and useful for many purposes, does not seem to me to be necessary, nor even called for in copying coins. The copies which I send you were taken sixteen years ago from the *original* medals. The devise may be reversed on the plate, simply by having the plate supported with its face *down* and the etching point pressed *up* to it, or by bringing the touch to act on the medal in the same way. Impressions of coins or medals, *if* necessary *at all*, are obtained with great facility in *shellac*, a material far preferable, in my opinion, to any metal whatever for such purpose, as it resists the tracer perfectly and causes no wear to its point.

I have thus addressed you on this subject, because I am interested in it, and because my name has been introduced into your work, in such a manner as to create the impression, that I was but a borrower from others of a machine, of which I was, in fact, the first and original inventor.

I am sure, gentlemen, that you will excuse the trouble which I give you in this communication. You are artists yourselves, and know, therefore, with what a jealous pride professional reputation ought to be guarded. I should be the last man to deprive another of any portion of his just desert, and I am equally unwilling that others should treat me with the like injustice.

Your obedient servant,

ASA SPENCER.

Philadelphia, July 26, 1842.

Sir—We have to acknowledge the receipt of your communication of the 21st inst., in which you object to the account we have given, in our "Manual of Coins and Bullion," of the origin and progress of the art of medal ruling, and complain that we "have introduced your name into our work, in such a manner as to create the impression that you were but the borrower from others of a machine, of which you were, in fact, the *first* and *original* inventor."

We can assure you that we feel great anxiety to do exact justice, as to the claims for an ingenious invention to which we owe so much, and that we would gladly take the earliest opportunity of correcting any error into which we might have fallen.

We cannot think, however, that you have ground for complaint, when we have before us the following original certificates; the first under your own signature, the second under that of a gentleman now an officer in one of the branch mints, and a man of unimpeached veracity. The first was published in the U. S. Gazette, and is as follows:

"I did not see the article in the U. S. Gazette of July 17th, signed Justice, concerning the invention of a ruling machine, before it appeared in print, consequently could not prevent the error contained in that article, which has since been pointed out to me. I take this opportunity to correct it, by saying, that it was never intended to deny, but always to admit, that Mr. Gobrecht was the first to discover the mode of ruling medals, as exhibited by his specimen, published in 1817.

(Signed,) ASA SPENCER."

Philadelphia, Aug. 8, 1830.

Second certificate.—"I do hereby certify, that in the year 1816, being then employed by Messrs. Murray, Draper, Fairman & Co. in their establishment, with Mr. Gobrecht and Mr. Spencer, I was in the knowledge of the circumstance of Mr. Gobrecht's employing Mr. Spencer to make for him a ruling machine, the plan of which was, that the plate was to be moved under the ruler by means of a screw, and a machine was actually commenced to operate on this principle. The screw, however, was abandoned, and the machine was constructed so as to graduate with a roller, and merely to divide and rule straight lines. While this machine was constructing, Mr. Gobrecht explained to me a plan he had invented, to copy, by a ruling machine, medals, and surfaces slightly or deeply undulated, which he wished me to keep a secret, as it might be of great importance, and before June, 1817, he shewed me his machine, with the part attached to copy medals and surfaces, which was constructed by himself, and in successful operation. At the same time he shewed a head of Alexander, executed by means of the machine.

(Signed,) D. H. MASON, *Machinist and Engraver.*"

July 26, 1839.

The above statements establish all the claims of Mr. Gobrecht. You certainly made the ruling machine for him, and doubtless, with your well known skill and ingenuity; but it was without the appendage necessary to adapt it for medal ruling. Your alteration of Mr. Gobrecht's plan of moving the platform, appears to have possessed no advantages; at any rate, Mr. Saxton has adopted the screw in the last instrument of his construction.

It surprises us that you make light of Mr. Saxton's device for removing the *distortion* produced by the original machine-engraving; and that you seem to suppose that the means of overcoming this defect, are so obvious as to have occurred "to many besides Mr. S. and yourself." Now we think, on the contrary, that the principle employed by Mr. Saxton, is exceedingly ingenious and by no means obvious. Many sagacious persons who have seen it in operation, have not been able to understand the principle upon which it acts; and it is certain that the art of medal ruling was long kept dormant and useless for the want of some plan for remedying the distortion. Mr. Saxton brought his improvement into successful action in 1829, and made no secret of it. The method you employ is essentially the same. Does it date as early?

In conclusion we freely declare, that the art of machine-engraving is under great obligation to you for bringing it before the public, both in Europe and America, and for applying it, with great skill and taste, to many important purposes. In any history of the invention, however brief, your name must necessarily have a place. It is introduced in our description; and although more might have been said of your agency, it would have imposed on us a necessity of dwelling upon the merits of many others, such as Terrel, Bate, Collas and Nolte, who have done much to bring this brilliant discovery into public notice and favor, which would have led us into a narrative diffuse and out of place.

Sensible of the polite and respectful manner of your letter, we have endeavored to reply in the same spirit, and now subscribe ourselves, Your faithful servants,

Jacob R. Eckfeldt,
Wm. E. Du Bois.

A. Spencer, Esq.

Gentlemen—I duly received your answer under date of the 26th July. I have been necessarily delayed in my reply.

I regret that I am compelled once more to call your attention to the subject of my former communication, which, however, I do with the less reluctance, as I am fully persuaded of the sincere desire which you express to do exact justice and correct any error into which you may have fallen.

What I complain of as unjust to myself is this, that in your work it is stated that you were indebted to Mr. Gobrecht for the art of medal ruling, and that the first specimen was executed by him, with a machine of his *own invention*, whereas, in fact, the specimen was by the machine which I invented and made for him, and by means of an appendage to it, suggested and furnished by myself.

I never pretended that my invention of *this* ruling machine, or the plan or principle on which it works, has more to do with ruling medals, than any *other* ruling machine on any other principle; but as a passage in your communication seems to intimate something of that sort, I beg leave to make a few remarks on that subject.

Early in 1817, when I undertook the making of a ruling machine, the only means tried for moving one, were two modifications of the inclined plane, viz. the wedge and the screw. Only the former was then in use. Two machines constructed on this plan were in this city, and were supposed to be the only two ever made in this country. Mr. Richard Fairman, of our establishment, owned one of them, and its possession was thought to give him great advantages over other artists. A ruling machine in those days was considered an important acquisition, but to get up one on the wedge principle was such a formidable undertaking that few had the resolution to encounter it. It was an awkward and unwieldy contrivance, and though the wedges or inclined planes were six or eight feet in length, its range of work was only three or four inches, and the working of it was laborious, slow and tedious. I had therefore only the screw to look to. It is very desirable that a ruling machine for the ordinary work of the engraving office, should be so constructed, that various parts of the plate may be brought under the etching point with facility and despatch. I could hit upon no way of doing this with the screw, without more labor than I had to spare. I therefore gave it up, and took in its place the principle of the wheel and axle, which I thought admirably adapted to the purpose. All the essential parts were of simple forms, and required no extraordinary skill or implement to produce them; its movement was light and easy, the carriage could be shifted from one point to another without trouble or delay; its dimensions were not more than one third of those of the wedge machine, and its range was six times as great. These advantages were obvious and decisive. Mr. Fairman gave up his wedge machine, and lost no time in procuring one on my plan, and when seen in London, it met with the decided preference of the most eminent engravers of that city.

You say that the machine which I had made for Mr. Gobrecht, was without the appendage necessary to adapt it tó medal ruling. I assert that Mr. G. had already been put in possession of this appendage by me, and at my suggestion, before the machine was commenced, and that it was constructed with special regard to the adaptation of this appendage for waved line ruling. The slide with joints of peculiar construction was wanted only for that purpose, and was not known or used in any other ruling machine.

In your reply, two certificates are brought forward to show that no ground of complaint exists on my part—the first under my own signature, and the second, that of Mr. Mason.

In my communication I admitted—I always admitted, that Mr. Gobrecht was the first to discover the mode of ruling medals by a machine; or in other words, that he was the first to discover, that the appendage which I suggested and prepared for his machine, for ruling *waved* lines, was equally capable of ruling *medals.* I do not see that my certificate admits more than this, and nothing more was intended by it. Nothing more was due. It was drawn from me much against my will, during a newspaper discussion, which arose without my knowledge, advice or suggestion. I felt extreme reluctance to be dragged into public controversy, for which I lacked time, inclination and ability, and it would, moreover, have been very disagreeable to those with whom I was connected in business. And I was willing thus to terminate the dispute. The first part of Mr. Mason's certificate agrees with my statement. The difference which appears in the other parts is unessential, or is reconcilable therewith, without an imputation in the least degree unfavorable to any one. Mr. Mason himself is well known to me, and is justly respected by all who know him for his integrity and professional skill. The only discrepancy which I can perceive, is in that portion of his certificate where it is said, that "when the machine was *constructing*, Mr. Gobrecht explained to me a plan he had *invented* to copy, by a ruling machine, *medals*, and surfaces slightly or deeply undulated." The plan here spoken of is undoubtedly the one suggested and furnished by me to Mr. Gobrecht. Although Mr. G. was in possession of this plan before the machine was commenced, it is not easy to suppose that Mr. G. more than myself, anticipated *medal* ruling from it. The supposition is not unreasonable, nor altogether improbable, that when Mr. G. made his confidential disclosure to Mr. Mason, it being new to him, he supposed, of course, that it was Mr. Gobrecht's invention, and if at this time the machine was finished and in Mr. G.'s possession, he knew that it would copy medals. These suppositions are rendered the more probable to me by the way in which Mr. Mason related the matter to me. He said Mr. Gobrecht invited him into his private room, showed him a machine of his invention for ruling medals, and exhibited as a specimen of its work a copy of the Alexander medal. Thus was conveyed to my mind the belief, that this was the first time that Mr. Mason had seen or heard of medal ruling.

It is remarkable that the Rose Engine, with one of its movements so identical with that of medal ruling, should have been so long in use without leading to that art. Perhaps a simple description of its action in the Rose Engine and in the ruling machine, may not be out of place here. The Rose Engine has two principal movements; one circular, the other lineal. The latter is adapted to ruling waved lines, which, in fact, it does in the Rose Engine. A waved surface of hard metal is fixed to the machine, a tracer is adjusted to the model, and a cutter to the plate that is to be engraved. The machine is put in motion, when a waved line is cut on the plate. The plate is then advanced a step, the motion is repeated and another line is cut. Thus a series of lines is continued until the work is done; and as the tracer passes always on the same line over the model, the lines on the plate are similar. To show more clearly the result of this movement in the Rose Engine and in the ruling machine, I have prepared two specimens, one from each.

I have taken for a model the word "Londini" in raised letters. The first series is after the manner of the Rose Engine, the plate alone being moved, the tracer passing always on the same line, which you may perceive is just where the right hand thin stroke of the N joins the thick one. The model is then fixed to the carriage, and made to move with the plate. The tracer is placed on the plane, a few lines above the letters; the ruling is commenced and carried on until the tracer comes on the same line on which it acted during the first series. The first series is made up of one minute section of the model, and, consequently, can give no indication of the device that may be on it. In the second series, where the model moves with the plate, a different section of the model is brought under the tracer at every line. A course of minute sequent sections is marked on the plate, and, these imbodied, show the device on the model.

Thus this movement, when it is attached to any ruling machine, and the model and plate move together, must *necessarily* show any one that it can rule medals.

I am truly sorry if any thing which I have said is construed to imply the least disrespect for the ingenious contrivance of Mr. Saxton, for removing the distortion. Certainly it was very remote from my intention to speak slightingly of him or his device. Such injustice would do little credit to my judgment or candor. No one acquainted with that gentleman's extraordinary skill, will be at all surprised at the ingenuity of his contrivance. All that I meant to say is, that I, being limited as to time and resources, was constrained to economy, and forced to use a different and a simpler method of remedying the difficulty. "Mr. Saxton brought his improvement into successful action in 1829, and made no secret of it." You say the method employed by me is essentially the same, and ask if it dates as early. I cannot say precisely at what time it was done, though I am quite certain it was sometime *after* that year, but long before I had any knowledge that Mr. Saxton had done any thing in that way. Although I had no idea *how* the thing was done, I had seen that it *was* done before I attempted it. I lay no claim to originality in removing distortion. Candor also compels me to confess how little I deserve your compliments for extending, improving and applying the art of medal ruling.

I have always said and always endeavored to maintain, that it *originated* with me, in consequence of my attaching the Rose Engine movement to the ruling machine, for the purpose of ruling *waved* lines; and that the result was, that the first three persons who put this appendage in action for ruling *waved* lines, obtained, *each*, something like a copy of a *medal* at the same time.

Here the affair rested. Nothing was done for the machine, to improve or enlarge its medal-ruling power, until I revived the art about the year 1827. The want of leisure limited the use of it to our own business of bank-note engraving. It was not long after this that my specimens made their appearance in London. Although my machine was known there in 1820, not enough of medal ruling had been seen to attract attention. But the appearance of these specimens soon excited the ingenuity of different artists, and about 1830, we find Bate of London, Collas of Paris, and Mr. Saxton, powerful competitors in this art, the importance of which was soon manifested in their various labors.

You will observe therefore, in conclusion, gentlemen, that I claim the invention as well as the construction of the machine which was furnished to Mr. Gobrecht in 1817, and also that I was the first to adapt the Rose Engine appendage to the ruling of waved lines by it, in which this art originated. I never claimed to be the first to use it or discover its use for copying medals. Indeed for two years after its construction, I was ignorant, in common with others, of the mode in which this was done, although I discovered it, as I have already stated in my first communication, immediately on using the machine myself.

I might then if I had not been too much immersed in my professional concerns, and too reluctant to engage in controversy, easily have established beyond contradiction, every fact which I have here asserted. I hope I have convinced you of the mistake into which you have fallen. It cannot seem strange that I should be mortified, if in such a standard work as yours, an error so unjust to me should remain uncorrected.

Your obedient servant,

Asa Spencer.

THE

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OF

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April, 1843.

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CONTENTS.

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THE

AMERICAN

JOURNAL OF SCIENCE, &c.

Art. I.—*Notice of the Life and Labors of* De Candolle: *extracted [and translated] from the Address delivered before the Royal Botanical Society of Ratisbon, at its meeting on the 28th of November*, 1841, *by the President*, Prof. von Martius.

Augustin Pyramus De Candolle, professor of botany at Geneva, died on the 9th of September, 1841. De Candolle exerted such an extensive and powerful influence upon the progress of botany, that he is identified with the history of the science in the present century.

The man who impressed the seal of his genius on the natural history, and especially on the botany of the last century, Linnæus, died at Upsal, on the 10th of January, 1778. On the 4th of February of that same year, twenty five days after the departure of Linnæus, and on the same day upon which the death of Conrad Celtis occurred, Aug. Pyramus De Candolle saw the light of day at Geneva. Thus did the spirit of the times, which guides the wisdom of man, transfer the *rôle* of the systematical classifier of plants from Sweden to the verdant shores of the Leman, and place it in the cradle of him, upon whose urn we now suspend the flower-garland of grateful reverence.

> "Scilicet a tumulis, et qui periere propinquis,
> Protinus ad vivos ora referre juvat."
>
> *Ovid, Fast. II.*

De Candolle was without doubt the Linnæus of our age. In the right understanding of what he has accomplished, lies the true measure both of his own greatness, and of the work done by his predecessor,—lies the sum of the progress which botany has made since the departure of Linnæus from the scene of his activity. The importance of systematic arrangement and classification was the leading idea in both their minds; and consequently both have been especially useful as *registrators* of the vegetable kingdom. Both, however, were influenced and guided by the ideas of vegetable physiology and morphology which each had formed. The systematic works of both, therefore, went hand in hand with their general views, received from them their impulse and signification, and reflected back the spirit which distinguishes their different epochs. They are accordingly as different in their manner of comprehending and of carrying out their ideas, as were the fundamental principles respecting the nature of plants which prevailed in the time of each. There is, however, this essential difference between them. The thoughts which Linnæus embodied in his system, were his own creation. De Candolle, on the other hand, adopted the ideas of the French school, founded on the natural method of A. L. de Jussieu, with the view to their full development in an universal descriptive system of the vegetable kingdom. We do not at present propose fully to trace the parallel between Linnæus and De Candolle, although some of its elements will be indicated in the brief sketch of the life and labors of our much lamented friend: but it remains for the historian of botany, to exhibit in detail the relations which these two men sustained to each other, and to the epochs in the progress of the science distinguished by their names.

Aug. Pyr. De Candolle sprung from a noble family of Provence, which, from religious considerations, removed to Geneva in the year 1558. The younger Catholic branch of the family, still existing in Provence, is now represented by the Marquis De Candolle, with whom the Genevan botanist always maintained the friendly relations of kindred. Augustin De Candolle, the father of our departed friend, was one of the first magistrates (premier syndic) of the republic of Geneva. In the early years of his life, the feeble health of the child gave much anxiety to his parents. In his seventh year he suffered from an attack of acute hydrocephalus; but fortunately conquered a disease so often fatal to childhood, or which in other cases so frequently leaves behind a feebleness of the mental powers. But the youth and man, with his well-organized head, fitted for the most difficult processes of thought, experienced no further ill effects from this distressing malady.

In the gymnasium, (*collège*,) he was not distinguished, except for his proficiency in Latin and French versification. By the time he reached the first class, in the year 1791, he had gained many prizes by his great facility in versification, and his uncommonly retentive memory. At this period, when his body and mind were proportionally and very rapidly developed, he entered into the "belles-lettres class," a division which answers to the German *lycealcursus*, or highest department of the gymnasium. The revolution about this time (1792) overflowing the limits of France, extended itself into Switzerland; the government of the canton of Geneva was overthrown; and the father of our De Candolle retired to an estate which he possessed in Champagne, a village near Grandson, between Yverdun and Neuchatel. The young man had until now devoted himself almost exclusively to classical studies. He had read the great Latin and Greek authors diligently, and with good effect on the development of his judgment; he had written many essays in French and Latin verse, and knew by heart a great number of classical passages from the literature of these languages. Even at the time of his leaving college, his memory retained so perfectly the first six books of the Æneid, that he could go on with the recitation of any portion of them taken at random, without hesitation. The study of history was peculiarly attractive to him, and for a long time he regarded himself as destined to the profession of an historian.

Somewhat later he attended to the lectures of Pierre Prevost on philosophy. Logic from the lips of this celebrated natural philosopher, the author of the valuable treatise on the equilibrium of caloric, had a powerful influence on his excitable mind. It gave him the habit of sharp and clear thinking, and was an excellent introduction to the different exact sciences, with the study of which he was employed in the years 1794 and 1795. Physics, the department of Marc. Aug. Pictet, had more attraction for him than mathematics.

Meanwhile his residence in the country, where he was accustomed to pass his vacations, had brought him nearer to nature. Without any book on botany, following the guidance simply of the objects themselves, he accustomed himself to the art of observation. At first this occupation had only the character of a pastime, or recreation. What afterwards suddenly induced him to devote himself wholly to the "amabilis scientia," was the excitement which he experienced in 1796 in the lecture room of the excellent Vaucher.*

The number of teachers at the Academy of Geneva was at that time very small. Mr. P. Vaucher, professor of theology, who soon after proved himself an accurate observer by his account of the Confervæ of fresh water, was giving in that year a free course on botany. De Candolle had only heard the first half of the course, when he returned to his parents at Champagne, determined to devote himself exclusively to this science. The attractive descriptions of Vaucher had revealed to him his own genius; and he chose at the age of eighteen the vocation to which he remained faithful during his whole life, with an enthusiasm which did not desert the man of sixty-three even on his death-bed. In these lectures he had become acquainted with the organs of plants. Returning to the country, he began at once to describe the plants which he found, indicating them by their common, not their scientific names, of which he was at that time ignorant. He considered himself fortunate a few months afterwards, when he received the first edition of Lamarck's *Flore Française*, and a few other botanical books, whose true value he immediately understood.

It was the custom at that time, in his native city, for the sons of rich parents to study law. De Candolle consequently began this study in the year 1796, but with the fixed intention of not allowing it to affect his future destination. One of his friends, who was closely connected with Dolomieu, induced him to pass the winters of 1796 and 1797 in Paris, under the eye of that celebrated observer of nature. He received his father's permission for this, and lived in the house of Dolomieu, by whom he was treated with paternal tenderness. He now attended the lectures of Vauquelin, Fourcroy, Charles, Portal, and Cuvier. In the

* [The teacher survived for about a year, his more celebrated pupil; vide Botanical Necrology in this Journal, Vol. XLIII, p. 215. An interesting biographical notice of M. Vaucher, from the pen of Alphonse De Candolle, has recently been published in the *Bibliothèque Universelle* at Geneva; an English translation of which appeared in the *Annals and Magazine of Natural History*, for November and December last.—A. G.]

Jardin des Plantes he had made the acquaintance of Lamarck, Deleuze, and Desfontaines. To the latter his heart was peculiarly drawn. The gentle repose of this learned and amiable man enchained him as to a second father;* and he preserved to his latest breath the most tender and grateful affection as well for him as for Vaucher. These winter sessions had opened to him a view into the depth and extent of natural science. He perceived the importance of the relations between physics, chemistry, and botany; he perceived that the latter science had reached a station where she required especially for her completion the aid of the others. He determined to labor in this field, and to help to bring botany out of her isolated position. This was besides the peculiar task of the period. The labors of our great M. von Humboldt, of Priestley, of Ingenhauss, &c. had extended the domain of botany in a similar direction. Accordingly he came out first with his treatise upon the nourishment of *Lichens*, which, in the summer of 1797, was laid before the *Société de Physique et d'Histoire Naturelle*, then recently established by Saussure at Geneva. His intercourse with Senebier and Vaucher confirmed him in this direction of his faculties. It is easy to perceive that, in the whole course of his literary labors, he sought to make the doctrines of physics and of chemistry available in their application to botany. We find the same spirit in his excellent treatise *Sur les Propriétés Medicales des Plantes*, (Paris, 1804, 4to,) of which Perleb has given a German version (1810) enriched with many valuable additions. He attempted in this work to represent more fully than had been before done, the parallel suggested by Linnæus, but opposed by other writers, between the outward forms of plants and their chemical constitution and adaptation to pharmacy; a labor in which he manifested a happy talent for tracing back various phenomena to their origin in general principles.

In the year 1798, Geneva was incorporated into the French republic. De Candolle, finding his future prospects much affected by this event, the property of his parents having been materially diminished by the catastrophes of the Revolution, determined

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* De Candolle honored the memory of his friend, who died on the 16th of November, 1833, by a "*Notice Historique sur la vie et les travaux de M. Desfontaines*," in the *Bibliothèque Univers.*, Feb. 1834.

to adopt the medical profession, and easily obtained the consent of his father, who hoped that he would be thus established in a lucrative mode of life. The son, meanwhile, whose enthusiasm for botany had increased from year to year, thought principally of the greater facilities he should thus enjoy for the pursuit of his favorite science. During this year he went the second time to Paris; and taking up his abode in the neighborhood of the *Jardin des Plantes*, he gave himself up with zeal to the study of its accumulated treasures. Lamarck encouraged him to labor with him in the botanical portion of the *Encyclopédie Méthodique;* in which he wrote the articles *Parthenium* and *Lepidium.* He also assisted Lamarck in the preparation of the article on *Panicum*, Poiret in that on *Paspalum*, described the species of *Senebiera*, and published his treatise on *Lichens*. At the request of Desfontaines, he undertook the preparation of the text for the *Plantes Grasses*, which Redouté had begun to represent in a splendid iconographical work. He received on this occasion the most friendly assistance from Desfontaines and L'Heritier, who gave him the free use of their rich collections and invaluable books. If neither this work, nor that on the *Liliaceæ*, which Redouté published somewhat later, (also with the assistance of De Candolle,) nor the *Astragalogia*, published in 1802, merit the praise of exact analytical descriptions of individuals, such as science now demands of *monography*, yet they already foreshow the facility and acuteness of systematic comprehension, which so fully characterize De Candolle's later efforts.

At this period he contracted a close friendship with the noble-minded Benjamin De Lessert, a man always open to every thing great and useful. The two friends glowed with the purest enthusiasm for the benefit of their fellow men. They founded the *Société Philanthropique*, whose first operation, during a time of public necessity, was the distribution in Paris of the Rumford soup. De Candolle was during ten years the secretary and an active member of that benevolent society. At this time he brought to maturity another institution of a similar tendency, which is still flourishing, viz. the *Société d'Encouragement pour l'Industrie Nationale;* he drew up the statutes for this society, and assisted until the year 1807 in preparing the bulletins issued by it. His activity in this field of philanthropy was maintained and enlarged by his intercourse with many distinguished men of similar views, such as the geometrician Lacroix, Biot, Cuvier, and the elder Brongniart. About this time he received the visit of two of the most distinguished citizens of the department of the Leman, who requested him to join them, in order to represent the interests of the department in a union of its *Notables*, which the First Consul had summoned. He accompanied them to the Tuilleries. Bonaparte inquired for the representative from Geneva, and turning to De Candolle endeavored to obtain from him the declaration that Geneva found herself happy in her union with the French republic. But courtesy could not bring the son of the Genevan magistrate, an upright friend to his country, to make an obsequious reply.

In the year 1802, De Candolle married Mademoiselle Torras, the daughter of a Genevan then resident in Paris. This marriage, founded on mutual affection, and made happy by love and harmony, gave him three children; of whom only one son survived the father. In the same year he was called to be *professor honorarius* in the Academy at Geneva, but did not yet engage in its duties. He remained in Paris instead, and gave at the *Collège de France*, in Cuvier's place, his first course on botany.

Benjamin De Lessert had purchased, in the year 1801, the rich and very interesting herbarium of the Burmann family. The duplicates he presented to his friend De Candolle; and the latter afterwards acquired the equally rich collection of plants made by L'Heritier, who had fallen a victim to assassination. These were the foundation of the immense herbarium which De Candolle increased, during his active life, to the number of from seventy to eighty thousand kinds, and which may be regarded not less for its copiousness, than on account of its exemplary order, and the rich variety of original specimens communicated by all the distinguished botanists of our times, as one of the greatest treasures in natural science of all Europe.

At the same time De Candolle began the preparation of his *Flore Française;* which, although announced as a second edition of the synonymous work by Lamarck, should be regarded as exclusively the production of De Candolle; since Lamarck gave to it only his name, and the use of his collections. Many years were employed in the collection of materials for this work in all the provinces of France. The author had opened a correspondence with all the botanists of the country, especially with Nestler, Broussonet, and Balbis, as well as with many foreign students of nature,—with Vahl, Pallas, Willdenow, Jacquin, the younger Hedwig and others, and made repeated journeys throughout France. This work, a truly great one, embracing a region rich in plants, was the first flora arranged according to the principles of the *Methode Naturelle.* The introduction to it, which exhibits a clear and orderly conception of nature, was De Candolle's first attempt to give a scientific representation of this theory. It met, as well as the annexed *Clavis Analytica,* with the greatest approbation. The work, the sale of which in the year 1804 had already reached to four thousand copies, is now quite out of print. It is the first book which has appeared in France in which we Germans find a satisfactory account of Cryptogamous plants resting on autopsy, a class of plants which had been before much neglected in France. The masterly manner in which an immense mass of materials has been treated,—the exactness with which the descriptions are given in a luminously technical style, whilst at the same time more is said of the geographical situations of plants than has been usually the case,—stamp this *Flore Française* as a work of great merit. With this alone, would De Candolle have fulfilled his obligations to the public, had he written absolutely nothing else. So thorough a production could not but meet with acknowledgment by the French government. Such men as Chaptal and Lacepede knew how great an influence on the national welfare a thorough knowledge of the vegetation of the country would exert. He received accordingly a commission in 1806, to travel through France and the kingdom of Italy, and to study these countries in a botanical and agronomical point of view. For six years he made a journey each summer, and gave an account of his observations to the Minister of the Interior. In these official reports he described the peculiarities of the district of country, noted the modes of culture in use, and presented plans for their improvement. He neglected no occasion to bring forward unobserved truths. His noble independence of character often led him to protest against faults in government, on which occasions he did not limit himself to his immediate commission. Some of these official reports have appeared from the press. He had at the same time formed a plan of preparing an extensive statistical work upon the condition of farming, and of every thing connected with it, which he would probably have completed, accustomed as he was to give to his plans the fullest development, if the political catastrophe of 1814 had not directed his activity into new channels. Only a few portions of that work were completed by him. One result of these journeys was the very valuable supplement, in a botanical point of view, to his *Flore Française.* Meanwhile he had been called, in the year 1807, to the professorship of the medical faculty at Montpelier. He repaired thither a few years later (1810) to take possession of the professorship of botany in the philosophical faculty, (*faculté des sciences,*) which was then created. He received the direction of the botanical garden, the collections of which he soon doubled. His active spirit animated the scholars, who flocked thither in great numbers. Since Magnol, the chair of botany at Montpelier had never exercised so favorable an influence on the academic youth. The clearness, fullness, and elegance of his style, the practical bearing which he gave to his teachings, with the genial serenity and freshness of his character, which united the glow of the Provençals with the serious diligence of the Swiss,—who could withstand such qualities? His ready talent for extemporaneous discourse, and the spirit and grace which he threw into his lectures, made his science charming even to women. Even if what passes by the name of botany among the fair sex in France and Switzerland be not precisely *his science,* yet it may be deemed a proof of his influence, that in those countries, a knowledge of plants is regarded as almost as essential an element in the education of women, as that of music with us sound-loving Germans.

One result of his academical labors at Montpelier, of great interest for the scientific public, was the publication of his *Théorie Elémentaire de Botanique;* the first edition of which appeared in 1813, the second in 1816. This book put into circulation a host of new and sound ideas in vegetable morphology and physiology. His talent for generalization is manifest throughout this work, often leading him, indeed, into bye-ways, which, however, like every excursion of the true enquirer, tend to bring him ultimately to a higher point of view. Two doctrines, here for the first time propounded in a scientific connection, that of the confluence or union of organs (*soudures*), and that of their unequal development or suppression (*avortemens*), have become, under certain points of view, canons in observation. It may be said in general of the theoretical views of De Candolle, that they differ in many respects from those of Linnæus, and often justly supersede them, because they are founded on broader and more physiological premises. I do not stop to point out these differences. It would be necessary to enter deeply into their respective modes of thinking, to do justice to either of these eminent enquirers into nature.

De Candolle's views approach more nearly, on the whole, to those of Gœthe; but it is not to be thence inferred that he was essentially aided by our great poet in the development of his ideas. Even in Germany, it was long before we understood Gœthe's object in his doctrine of metamorphosis. But when De Candolle was informed of the powerful impression which these views had made on our minds in Germany, he caused Goethe's book to be translated, and studied it diligently. In his later and larger work, (*Organographie Végétale,* 1832, translated into German and enriched with valuable notes by Meisner and Rœper,) may be found echos of Goethe's theory, and evidences of a further progress in that direction. It is not possible, however, definitely to assign to each individual his own property in truths which spread with rapidity and force among thinking men. They do not originate from one head, they belong to the *time*, which excites them in many minds, and enunciates them in various forms. In this view, nothing seems in more wretched taste than contention about the priority of a theoretical idea. The students of nature freely acknowledge that they derive their ideas from the objects of their examination, not from themselves; they announce them with so much the more confidence, in proportion as they recognize in them only the words of nature, which they have become worthy to hear.

The fall of Napoleon restored to our friend his political independence. He had returned to Geneva, in the year 1814, to visit his friends. The contemplation of the prosperity which the republic enjoyed on its separation from France, the associations of childhood, the patriotic pulsations of his heart; all drew him back again to his home. The political commotions in the south of France, at that period, were not adapted to render his residence there agreeable. Called during the Hundred days to be Rector of the University of Montpelier, he had to struggle with a host of difficulties, especially as the return of the Bourbons produced a dangerous reaction against those who had served under the Emperor, and especially against Protestant families. Although no partisan, yet De Candolle was obnoxious in both points of view. His own country presented (under less brilliant auspices to be sure than in Montpelier) the attractions of the father-land, the satisfaction of laboring for his compatriots, repose from political convulsions, and with all these sources of enjoyment, a society such as Geneva alone, situated as it is on the highway of the world, can collect together.

The State Council of Geneva created for him a professorship of natural history, and he returned on the 8th of November, 1816, into the service of his native country. The French government did not willingly part with him; his scholars at Montpelier made every possible effort to retain their beloved teacher, but in vain. In Geneva he had lectures to deliver in zoology as well as botany. In this field likewise he manifested his happy talent for instruction; and all his lectures were enthusiastically received by a crowd of hearers.

At the instigation of De Candolle, a botanical garden was instituted, of which he was the curator until his death. More than five hundred subscribers formed by degrees a fund of eighty nine thousand francs, appropriated to the support of this garden. This is not the only testimony of the sympathy of his fellow citizens in what De Candolle recommended as for the interest of science and of the town. There was accidentally entrusted to him for a short time, a large and valuable collection of drawings of Mexican plants, made by the Spanish botanists Leon Moçino and Cervantes, in Mexico. These being unexpectedly called for, all the artists and amateurs of the city assembled at his request, and in eight days' active labor made a complete copy of all these drawings. De Candolle told me with glistening eyes that this proof of the regard and affection of his fellow citizens was one of the most delightful experiences of his life. But who among his associates would not gladly have assisted in scientific efforts a man who was distinguished by so much gracefulness, by such transparent frankness, united with such fine tact in social intercourse. He was a keen observer, an accurate judge of the human heart. It was therefore easy for him to associate with all classes in society, and to influence all for the good of the commonwealth. This is manifest by his being chosen in the year 1816 into the

Council of the representatives of the canton, and being twice unanimously re-elected after the first time of service, in the years 1829 and 1839, by the voice of the people. As long as he lived in his paternal city, he was called by the confidence of his fellow citizens to situations of public responsibility. He examined with a penetrating glance the condition of municipal affairs at that time; a friend of order and of a peaceful progress, he procured many useful institutions, and applied himself to the carrying out of others which were projected by congenial patriots. He took an active part in the formation and enriching of the museum of the Academy; I have already mentioned that the botanical garden was created by him. As president of the Society of Arts, he animated every movement of his fellow citizens in the field of arts and manufactures. He considered attention to agriculture of peculiar importance in a small republic, which depends upon its neighborhood for the necessaries of life. On this account he founded in that society a peculiar class for agriculture, whose labors he promoted with the most lively interest. To impress the agriculturist with the importance of his calling, to awaken in him the spirit of emulation, of observation of nature, and of careful reflection, he regarded as one of his most pleasing duties, both as a citizen and as a man of learning.

His imagination was lively and excitable, if not creative; his feeling for beauty was pure and unprejudiced: he could not therefore be other than a warm friend of the fine arts, and he accomplished for their support in his canton, whatever lay in his power. Yet he did not carry his love for the fine arts to excess, but always regarded them merely as means for the embellishment of life: not so the attainment of objects of real utility; these lay nearer to his practical understanding, to his spirit of republican citizenship. On this account the Class of Industry in the *Société des Arts*, had reason to rejoice in his peculiar cöoperation. The report of two hundred pages, which De Candolle prepared in the year 1828, for the Industrial Association of Geneva, is a valuable testimony to his varied knowledge, and his devotion to the manufacturing interests of his country.

The institution of the council of the museum, the improvement of the schools through the extension of special instruction, the enlargement of the public library, the direction of schools for the people, the definitive organization of an institution for the deaf and dumb, his contributions for the erection of a small post for the use of the rural communities, and also for the founding of a better system for the instruction and examination of medical and surgical students—all these actions of an elevated patriotism, either originated with him, or received his earnest and effectual support. It must be particularly mentioned in this place, that he exercised the most beneficial influence on all the departments of public instruction by his counsel, by his powerful aid, and by the authority of his name.

It was his constant effort to increase the desire for knowledge, to extend the circle of science. He was inspired by that genuine aristocracy, which we find also in a Cuvier, a Fourcroy and a Laplace; he wished to raise science to the rank of a princess, that she might make herself the servant of mankind. In this sense also he was a great friend to publicity; he helped to introduce it into his country, he caused it to be prized at a period when it had not yet been regarded with favor, and in which it not seldom called out suspicion and alarm.

In his place as member of the representative council, subjects of great political importance were often referred to him. He discharged all such commissions with as much skill, as independent disinterestedness. More than thirty commissions of this kind were executed by him with as much assiduity, as if they concerned objects of his own favorite science. His friend, the first Syndicus Rigaud, who honored his memory by a discourse on occasion of the induction of the lately elected deputies, mentioned two such labors; one relating to a project for a committee for procuring provisions for the city (*comité des subsistances*) of the year 1820, and another two years later, on the project for reprisals against France in relation to their exports and imports; which had for its object the rejection of the project. Mr. Rigaud remarks on this subject, "The first report was an excellent work, which touched on the most important questions of national economy. It introduced also just ideas on the question of provision for the people by other means than by the government, at a period when the remembrance of a recent time of scarcity, had fixed many prejudices even in the minds of enlightened men. De Candolle exerted himself to present the doctrines of political economy in an intelligible manner, just as he tried to clothe every other species of knowledge in a popular dress. As early as the year 1817, he had published a treatise for the instruction of the public, on occasion of a disturbance among the people arising from the dearness of potatoes. In his report upon the project of introducing restrictions on the trade with France by way of retaliation, he developed the principles of true freedom of trade in his peculiarly lucid manner. His influence in the representative council was great. It was grounded on a high opinion of his character, as well as of his extraordinary talents, and on an eloquence which expressed his inward convictions with the fire of sudden inspiration. As a citizen and member of the council, De Candolle pursued steadily but one object; that of bringing opinions into harmony, of always drawing more closely the bonds of unity among the citizens of Geneva. He exerted himself to convince his numerous friends, often of different political parties, that extreme opinions could not find room in a small republic; and that reciprocal sacrifices were often required for the good of the country."

Gentlemen! It may perhaps appear at first sight irrelevant to the present occasion, to enter so much into detail respecting De Candolle's influence as a citizen and magistrate. Yet I find myself called upon to do so on many accounts. In the first place, we thus learn to appreciate the whole power of a mind, which could combine with an almost inconceivable productiveness in its own science so great a power for quite different affairs. We may also obtain additional points of comparison, which may place both aspects of De Candolle's character in a peculiar light. This warm devotion to his republican country, this self-sacrificing attachment to its interests, is a trait in which he resembles the sages and philosophers of classic antiquity. As Aristotle found time in the midst of his numerous works on physics, natural history, and philosophy, to write others on politics; as nearly all the Grecian philosophers, in addition to their widely different pursuits, were also practical or theoretical statesmen, so we find the citizen of the small Swiss canton, penetrated with ideas and feelings which belong to him only as a citizen of this inconsiderable spot of earth; he, the same man whose writings, composed in either the Latin or French language, are read from the Ganges to the Mississippi. We cannot escape the thought, that so active a devotion to the interests of the community could only exist in the mind of a learned man in whom the ancient associations of republicanism have not given place to the modern spirit,—the spirit of monarchical centralization. This old classical mode of thinking showed itself in many other great Swiss scholars, in Conrad Gessner, Alb. von Haller, Saussure, &c., as well as in De Candolle, though not in an equal degree. For however attached from inward conviction to the form of government of their country, not one of them had so earnest a desire to take an active part in the internal affairs of the republic. They were all rather theoretical students; while in De Candolle was reflected the spirit of our age, which passes onward from theory, from pure science, into realization in the form of useful ideas. The thought of the dignity and perfectibility of man, which the French revolution had so often in its mouth, only to degrade, shone out in the noble-minded, ardent citizen of Geneva, a son of the revolution in the highest sense of the word.

A comparison of Linnæus with De Candolle in this point of view, will result greatly in favor of the latter. We see Linnæus in Upsal, a remote and inconsiderable university-town of the north, active in the professor's chair, where he is surrounded by a crowd of young men eager for knowledge from almost every part of the earth; or we see him at the writing table of a small room, from which the dictator of natural history sends throughout the world his works, written in that terse, genial Latin in which his whole self is mirrored. There only lives Linnæus; or in *aula academica*, presiding over the discussions of his scholars; or in the small primitive botanical garden, where the registrator of the vegetable kingdom walks between formal rows of box and regular flower-beds in silent meditation. The northern natural historian withdrew himself from the world; he did not even deign to take part in the administration of the academic senate, which he regarded only as a burden. Restricting his society to a few friends, and to the infrequent visitors from other countries, Linnæus looked not upon the bustle of the world, except sometimes to deprecate it; only in the concrete study of nature does he find himself at ease. He is no cosmopolite, except that he studies nature in every zone; he recommends Swedish medicinal and esculent plants, instead of those which distant countries might offer. His mind becomes a denizen of every corner of the earth, but he belongs personally to Sweden alone. He allowed all political commotions to pass by him unheeded while absorbed in the contemplation of nature; chained to his little inkstand, from which he scattered through the world with luminous, aphoristic geniality, his thoughts, his anticipations of higher wisdom,—almost always expressed in the language of Scripture, and with an emphatic unction.

How entirely different was De Candolle. He is the man of the council, the man of the people. His power was felt as well in the Genevan republic, as in the republic of letters. No movement in the political world is to him a matter of indifference. He notices every change, and marks its relations to the progress of science. If he open his lecture room, it is not merely active young men who sit attentive at his feet. The *elite* of the fashionable world and of the higher walks are among his auditors; men and women of his own city, and numerous travellers from distant lands, who, between Paris and Rome, crowd the highway of European travel, passing through Geneva, all felicitate themselves upon having listened to his eloquent discourses. Whilst the northern student of nature meditates in solitude by the light of his study lamp, the pride of the learned world of Geneva, in his saloon, surrounded by the comforts of a half English, half French establishment, receives the visits of rich or celebrated friends, and of his fellow citizens, who talk of the movements of the political world, consult with him on the interests of their country, or listen to the voice of some enlightened citizen of the world, with lively interest in his far-reaching plans.

Thus are portrayed, in the persons of Linnæus and of De Candolle, not merely the state of the natural sciences, but also the more universal features of the spirit of their respective eras, as exhibited in the school and in life.

But in order to complete the portrait of our departed friend, I must now give a more particular account of those literary works which he commenced soon after his return to Geneva, when his mind had attained its full maturity; those works which especially authorize us to term him the Linnæus of our time; I mean his universal system of plants, an undertaking which was the result of the observations of many years, of repeated visits to the great collections of plants in Paris and London, and of a diligent correspondence with all the considerable botanists of the world, which he began to publish in the year 1818, and continued to labor upon with unexampled diligence until the end of his days. Since the death of Willdenow (in the year 1810) and the publication of the Enchiridion Botanicon of Persoon in 1809, botanical literature comprised no work which presented a universal view of all known plants according to their genera and species. The new edition of the *Systema Vegetabilium* of Römer and Schultes, made but little progress after the death of the former. The systematic knowledge of plants remained in a fluctuating state. Whilst numerous monographs appeared, and the materials were multiplied by discoveries in all the countries of the earth, there was no clue to guide in the labyrinth of countless forms. At the same time the necessity was constantly more and more felt of arranging plants, not in the dead frame-work of the Linnæan sexual system, but according to the so-called natural families in a comprehensive scientific whole. If we are not even yet able to conceive of these original types, as so many foci of the moving and forming spirit pervading the vegetable world, expressed in each individual case by more or less striking external characters; if we are obliged in the first instance to adhere to collective characters, that is, to the admission of a certain sum of distinctive marks; if it must further be acknowledged, that although we can perceive the principal characteristics, as they exhibit themselves in a few families, yet that we lose them entirely in their *organic*, that is, in their *universal* connection—in their evolution, as it were, out of each other; if especially we cannot deny that the natural method does not yet bring with it any philosophic satisfaction; that above all, the inward truth does not harmonize perfectly with *any* system,—it must however be acknowledged, that we can in no other way attain to an understanding of the kingdom of plants as a great whole, than by the path of a thoroughly concrete examination, led by the hand of analogy and induction. The German students of nature acknowledge that such an understanding cannot be obtained by speculation, nor by any constructive method; and they can only promise themselves favorable results by pursuing the path opened by Jussieu's *Methode Naturelle.* In other countries also—for example, in France and England, more recently in Italy likewise—Jussieu's doctrines had already struck powerful roots, and thus was the age expecting and prepared for a work which should extend the applications of the "natural system," carrying it on from the *genera* in which its founder had represented it, to the *species*, and giving by means of it a full and satisfactory description of the latter.

In order to have a due conception of the vastness of this undertaking, and its enormous difficulties, it is necessary that we should glance at the progress of descriptive botany. This part of the science, which so many regard as a lifeless register, others as the whole sum of botanical knowledge, dates no farther back, in a systematic form, than the sixteenth century. In 1584, Conrad Gessner published the first methodized work upon the vegetable kingdom. In 1623, Caspar Bauhin produced the first systematic register, (*Pinax*,) in which about seven thousand species of plants were indicated by names and some description, but without characteristics. Tournefort published the first work which can be properly called a systematic arrangement, in the years 1694 and 1700. His work contains nine thousand five hundred and sixteen articles, or about eight thousand species of plants; and this number was not materially increased in the next succeeding general work, the *Historia Plantarum* of Ray, in the years 1693 to 1704. In 1737, Linnæus gave his first systematic description of known plants. As Tournefort had introduced the conception of *genera* into science, that of *species* was now established, along with a method of description based on a well-founded and enlarged terminology. But Linnæus in throwing overboard a vast number of old and unintelligible accounts of plants as useless ballast, at once reduced the list of species to about seven thousand, a number which in the later editions of his *Systema* may have been increased to about twelve thousand. Since that time the increase of acknowledged species has been truly prodigious. In the last of the works of Linnæus in the year 1760, we find in the first five classes of his sexual system 1,835 species of plants; Vitman in 1790, has 3,491; Willdenow in 1797, 4,831; Persoon in 1806, 6,121; Römer and Schultes from 1817 to 1823, 13,519 species. In the first edition of Steudel's *Nomenclator Botanicus*, the first complete *Pinax* since Bauhin, the number of genera of phænogamous plants, or of the first twenty three classes of the Linnæan system, amounts to 3,376, and that of species to 39,684: the second edition of this celebrated work, on the other hand, which was finished in the current year 1841, reckons of phænogamous plants 6,722 genera, and 78,005 species.

De Candolle's task was therefore six times greater than that of Linnæus, if we only take simple numbers into consideration. But to this must be added the numerous difficulties which arise from the dispersion of materials throughout a literature in which the botanists of all civilized countries take part. Besides, in the time of Linnæus, science had much fewer foci than at present. Learned societies have now been formed in North and South America, in India and Java, for the promotion of the natural sciences, and separate portions of systematic botany are treated in periodical publications, monographies, and greater or smaller works, written not in Latin exclusively, as was formerly the case, but often in the language of the country. Hence the acquisition of the requisite literary apparatus merely, is now within the reach of only very considerable pecuniary means. De Candolle, with the most noble disinterestedness, sacrificed in this cause a great portion of his estate.

Equally formidable are the internal obstacles attendant upon the examination of vast collections of plants. The characteristics of the genera according to the natural method are made to rest upon organic peculiarities, which scarcely required a notice in the Linnæan system ; such for example, as the internal structure of the ovary, the ovule, and the seeds. The use of the microscope, neglected by Linnæus, is now become quite indispensable. The distinguishing marks of species are founded on numerous, and often very minute differences, which require a close examination of all the parts. To make out a diagnosis, the description must now be more circumstantial than formerly, when a few words were sufficient to discriminate between related species. Linnæus's *Systema Plantarum,* in the Reichardt edition of 1779, describes 7 species of the genus *Eugenia,* and only 13 of *Myrtus.* De Candolle, in the year 1828, has 194 of the former genus, and 145 of the latter, of which he forms two divisions. It is obvious to every one that this immense increase of the labor of the systematic describer must weigh heavily upon each separate species. To this must be added, finally, the necessity of regarding each plant no longer merely as a prepared, or, as it were, crystallized production of nature, as was done by Linnæus, but as a living and acting self-developing being : a view which has been elicited by the doctrines of morphology, and which cannot now be wholly excluded from merely descriptive treatises.

De Candolle began his great work in the year 1818, in an extended form, under the title of *Regni Vegetabilis Systema Naturale.* Two volumes had already appeared, when he perceived that so immense a field laughed to scorn the limits of human life; he therefore adopted a condensed form, and published seven volumes between the years 1824 and 1838. With an enthusiasm which has perhaps never inspired any other botanist, he devoted the greater part of the day to this gigantic task. Still he was not able to go through the whole extent of the vegetable kingdom in this manner. The work was interrupted by his death in the middle of the eighth volume; and a great portion of the so-called *Monopetalous plants*, as well as the classes of *Monocotyledones* and *Acotyledones*, are yet untouched.

De Candolle appears peculiarly great in the accurate comprehension of the characters both of genera and species. In the description of distinctive marks, he not unfrequently departs from the terminology of the Linnæan school. Whilst he at times describes a given object with admirable art, conveying the most lively image to the mind, his expressions occasionally fail of this distinctness. No one who can realize the greatness of the task, will be surprised that amidst such an overwhelming mass of materials, some objects should be described after a less thorough examination and scrutiny. But we never fail to recognize the intelligent, penetrating systematizer, furnished with the happiest talent for combination, even when not altogether fortunate or thorough in his observation of the particular subject. Well has the greatest English botanist said of him—*his head is still better than his eye.*

De Candolle has given a fuller development of his morphological and systematic views respecting particular families of plants and genera, in a series of treatises which have been regarded as models of monographical labor by all systematic botanists.* It should be particularly mentioned here that he enriched the geography of plants, elevated by Alex. von Humboldt to the dignity of a peculiar science, with many important facts, and exhibited also the practical side of this study. His general views on this subject are laid down in a valuable *Essai Elémentaire de Geographie*

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* *Memoires sur la famille des Legumineuses*, Par. 1825, 4to. *Collection de Memoires pour servir à l'Histoire du Regne Vegetal.* Par. 1828—1838. (10 Mem.)

Botanique, published in the 18th part of the *Dictionnaire des Sciences Naturelles.*

I pass over many of the minor scientific labors of this unweariedly active man, such as his systematic account of the species of Cabbage, his description of remarkable plants of the Genevan botanical garden, and numerous contributions to the memoirs of various scientific associations, who vied with each other in thus appropriating the activity of this admirable man. More than a hundred diplomas from learned societies in every part of the civilized world testify his scientific eminence, and the extension of his literary relations. Since 1808 he has belonged to the Royal Bavarian Academy of Sciences; since 1822, to the Royal Society of London. In the year 1826 he was chosen one of the eight *associés étrangers* of the Royal Academy of Sciences at Paris; and King Louis Philippe has testified his respect for the learned Genevan by bestowing upon him the cross of the Legion of Honor.

These various marks of respect could not dazzle a man who, in the most animated intercourse with science and with mankind, perceived the endlessness of the subjects of enquiry, and who exaggerated neither the measure of his own limited powers, nor the amount of his influence. Like all truly great men, De Candolle was modest; and the consciousness of his own worth is shown only in the lenity with which he judged others, and in the heartiness with which he applauded their services. His twofold enthusiasm to increase the knowledge and advance the welfare of the human race, reposed on a gentle but uncompromising character. From temperament he was impetuous, rapid in determination, firm and unfaltering in execution; he had the practical skill to carry his plans into effect in every variety of occupation. A practiced physiognomist would detect these characteristics at a glance. De Candolle was of a sanguine temperament, of middle stature, firm, broad-chested, with proportionably long and muscular arms, quick and elastic in his walk, light and brisk in all his movements. His oval face, shaded by thick black hair, and by its somewhat dark complexion reminding one of his Provençal origin, was not so much distinguished by the expression of a well marked and prominent profile, as by the high and finely arched brow, the mobility of the features, the fire of his brown, proportionably small eyes, which shone even through spectacles, and by the charm of his mouth. In speaking, the whole intellectual expression of the man was suddenly elevated. His ideas unfolded themselves easily and without effort in discourse, which, like his writings, inclined rather to rhetorical breadth than to exact conciseness.

The poetical element of his mind, which he manifested while yet a scholar in the college, remained active in him in later years. His fancy, both strong and rich, variously colored, blooming, and rapid in its movements, clothed his quick-rising conceptions in a light and graceful dress. He has left behind a great number of poems of a lyrical character, in which he represents the universal feelings of nature, or unfolds with grace and delicacy the emotions of the human heart. What we have seen of these reminds us of Lafontaine, Delille, and of our own Pfeffel. From 1821 to his death, he continued his autobiography with great particularity, in which are contained valuable materials for moral and literary history, often under the form of explanatory notes. His son will publish, with such omissions as circumstances require, this memorial of the untiring activity of this excellent man.

But while such variously directed labor found in itself the best intellectual reward, De Candolle was by degrees obliged to acknowledge the insufficiency of his physical powers for the task he had himself allotted to them. In the year 1825, he had the misfortune to lose his youngest son, a promising boy of thirteen years old. The philosopher sought to soften the sorrows of his heart by increased activity, and redoubled his zeal for the completion of his work; but from that time his health began to fail. He often suffered from attacks of gout, and from obstinate catahrral affections, and was obliged on that account to relinquish his professorship in 1834, which was transferred by the Senate to his son Alphonse. In the year 1835, he suffered from a severe illness. He was afflicted with an asthma, and a disease of the throat, [bronchocele?] for which excessive doses of iodine were prescribed. In consequence of this, he suffered from *œdema pedum*, and from nervous attacks, which increased until his death. He was never perfectly well after 1835, and his strength was so much exhausted that the progress of the dropsy, which from the month of June rapidly increased, could no longer be opposed with effect. He died at 6 o'clock in the evening of the 9th of September, [1841,] having lost his consciousness several hours earlier.

By his will of the 20th of February of the present year, [1841,] he left his library and his collection of plants to his son, with the condition that they should be open, as before, to the inspection of botanists, as if in a public establishment, and that students should have the use of them until the end of their term of study. The filial devotion of the son has made the fulfillment of these conditions a sacred duty. Many distinguished botanists have promised their aid for the completion of a work which transcends the powers of any individual.* De Candolle bequeathed to the Society of Natural History of Geneva the sum of two thousand four hundred francs, the interest of which is to be distributed in prizes for botanical monographs. The right of publishing new editions of his *Théorie Elémentaire*, and of his *Organographie*, he left to his friend and scholar Guillemin† in Paris; the same right with regard to the *Flore Française*, and the *Essai sur les Propriétés Medicales des Plantes*, he bequeathed to Prof. Dunal in Montpelier.

This is the image, in its essential features, of one of the most excellent men which the century has offered to receive the honors of science. In botany, that CANDOLLEA, the Australian shrub to which Labillardière has affixed his name, is not required to keep him fresh in the memory of his botanical associates: he has inscribed his own name on every page of the system of plants. Neither does posterity require the monument which his native city proposes to erect to his memory, nor the new "*Rue De Candolle*" next to the botanical garden in Rochelle, in order to say how great has been the influence of De Candolle in our time. *Exegit monumentum ære perennius.*

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* [Vide Amer. Jour. Sci., Vol. XLII, p. 376.]

† [This favorite pupil did not live even to commence the undertaking thus committed to his charge: he died early in the spring of 1842.—A. G.]

Art. II.—*On the application of M. Riench's test for the detection of Arsenic, to Medico-Legal Enquiries;* by D. P. Gardner, M. D., Lecturer on Nat. Hist. in Rutgers Inst., Corr. Mem. Lyceum Nat. Hist., New York, formerly Prof. Chem, &c. in Hampden Sidney College, Va.

The discovery of arsenious acid in complex fluids, has always been an important problem in the science of toxicology, by reason of its frequent use for criminal purposes. From the statistics of the *Viscount de Cormenin,* it appears that between the years 1830 and 1840, three hundred and thirty five cases of poisoning by arsenic, implicating four hundred and fourteen persons, were investigated by the Parisian tribunals alone; and two hundred cases more were ascertained, but not brought before the courts. The use of this substance, in minute doses frequently given, to produce a lingering death, has entailed upon the chemist the necessity of furnishing himself with the most delicate tests. For as the certainty of detection increases, the temerity of the criminal also becomes greater, so that the cause of morality is directly advanced by these researches.

Numerous tests are recognized in medicine, but all of them, with the exception perhaps of Mr. Marsh's, are of little value in collecting the poison out of its solutions. This part of the process is however the most important, for the profession have unanimously agreed that no series of precipitations is satisfactory, unless the metal be finally reduced. If sulphuretted hydrogen be used, as recommended by Dr. Christison, for the precipitation of the arsenic, we are embarrassed afterwards by the collection of the matter thrown down, and then the reduction. This, and many other processes, advised by writers, require much time, with the use of materials not belonging to the medicines of a country practitioner, and they are less delicate than the method of M. Riench for the discovery of arsenic as a commercial impurity in various fluids and reagents. The reader is referred to No. 126, December, 1841, of the London, Edinburgh and Dublin Philosophical Magazine, for an abstract of M. Riench's paper "*on the action of metallic copper on solutions of certain metals, particularly with reference to the detection of arsenic.*" That article led me to examine the utility of the test recommended in medico-legal questions, and from an extensive series of experiments, I became convinced of its great value, and have communicated this paper with a view of introducing to the notice of the profession, the simplest, and as I believe the most effectual method of *collecting* arsenic out of suspected fluids.

The principle on which the test is founded, is that *pure hydrochloric acid* exerts no action upon bright copper—but when certain metallic solutions are present, as an adulteration in the acid, it then attacks the copper, and there is thrown down the reduced metal of the solution. Many metals will thus precipitate themselves, but some do so when in minute quantities, whereas others fall down only when in large amount. Some salts of mercury are reduced without hydrochloric acid, whilst arsenic in no proportion can tarnish copper, unless the acid be present.

Before Riench's process could be adopted by physicians, it was necessary to ascertain how far organic matters, such as may exist in the contents of the stomach, &c. were calculated to embarrass the operator. The existence of medicines, in the suspected fluids, might also destroy the value of the test. These enquiries, and the hope of simplifying, and rendering the process more certain, caused me to enter upon a full examination of the matter.

There are three stages in the manipulation: 1. The collection of the arsenic in Riench's test; 2. Its sublimation; 3. Its separation from other substances.

1. The *collection* of the poison out of a solution, in which no other active metallic agent is present, is most easily effected by the following process. A piece of copper, in the form of wire or sheet, is to be filed bright, and introduced into the suspected fluid; to this is then added a small quantity of pure hydrochloric acid, and the whole boiled together in a glass flask. Upon examining the copper after a few minutes' ebullition, when arsenic is present its surface will either be found coated with a film of the color and lustre of rolled zinc, or a deep black with slight polish; or covered with scales of a black color. These various appearances depend upon the quantity of the poison present, and the extent of the surface of copper. If the boiling be continued after the scales are produced, they drop off, and are lost in the fluid, so that the collection becomes imperfect. All the arsenic present can be separated in this way, therefore the copper not only reduces it, but also affords us an accurate measure of the amount in solution.

In this part of the subject, the most important practical considerations are the determination of the amount of metallic copper to be used, and the method of procedure in dense fluids, such as broths, matter vomited, the blood, &c. In ascertaining the first, two things are to be remembered; 1st, that in the second step of the operation, the sublimation, the tube should be as fine as possible; and 2nd, the thickness of the deposit of arsenic is not at all important, excepting that less copper is required, but there is danger of its falling off in scales if too little is used. The only rule that is applicable to all cases, is to examine a known small proportion of the fluid first, and ascertain how much copper is necessary to deprive it of all its poison; the estimate thus made may be applied to the remaining portions of the solution. If sheet copper be used, it should be cut into strips, not exceeding one fourth of an inch wide and one inch long, so that they may be readily admitted into the subliming tube. The whole amount of poison present may be sublimed in several tubes. To be certain that all the arsenic has been separated from the fluid, successive strips should be introduced, and the boiling continued longer and longer, until no stain is produced in thirty minutes.

The color and consistency of the fluids examined, may be sources of much embarrassment, when the usual tests are employed; and filtration is a necessary part of the process when the fluid reagents are used. But neither of these qualities opposes serious impediments to the method under consideration. The coagulability of the solution is however a serious obstacle, because the arsenic is shut up in the coagula, and cannot be brought into contact with the copper. To remedy this evil, the coagulum should be cut up into minute pieces and warmed along with muriatic acid, the consequence of which is that all the poison will be dissolved by the acid, which is one of the best solvents of arsenic. The solid parts should be afterwards separated, by straining through a strong piece of cloth of close fabric. In this way, the drug can be collected out of blood. When the coats of the stomach are to be tested, they should be cut up, as is usually prescribed, and boiled with dilute acid. In operating upon fluids which are not yet coagulated, it is best to add dilute hydrochloric acid before heating, for in this way they do not form so dense a solid, but the acid exerts its solvent action throughout the mass, and may be afterwards pressed out by straining.

A certain number of copper strips will be thus obtained, coated with arsenic, and the next step is to remove from them any substances which may interfere with the second part of the process. Water, oleaginous matters, and solid particles from the solution adhering to the copper, are all to be removed. The first can be easily effected by bringing the metal into contact with bibulous paper, and then warming it gently. The other substances must be cleared off by introducing the strips into some warm water in a capsule, and moving the vessel so as to communicate a gentle motion to them; friction should be avoided. The cleaning may be continued in three changes of water, and the copper should be afterwards dried as already directed. It is of considerable importance that no empyreumatic vapor should be produced during the second stage of the manipulation.

2. The *sublimation* requires to be conducted with great nicety, when the quantity of poison is minute. The tube used should be about six inches long, and open at both ends, one of which is drawn out to a perforated point. The diameter must be diminished with the amount of metal collected; when that is very small it should not exceed one tenth of an inch, and never be larger than is necessary, and as small as possible to operate with. The strips used in delicate investigations will therefore be reduced in size so as to enter the subliming tube. The greatest attention must be bestowed to cleaning the tube thoroughly; a piece of rag, attached to a wire, should be introduced, and drawn up and down until all dust is removed; the outside should also be examined, so that it may be perfectly transparent. It must be dried by being warmed over the flame of a spirit lamp.

The copper strips, or as many as may be desired, are then to be introduced into the tube, so as to fill up the narrow end for about two inches. They must not be packed together, for it is necessary that air should pass freely amongst the pieces. The tube is next to be warmed up to the boiling point of water, so that any moisture that may still be present shall be driven off. The heat should be first applied at the occupied end, and the water as it condenses be heated so as to leave the tube entirely. If the condensing fluid be discolored by the presence of empyreumatic matters, this part of the process must be managed with great care, so that the copper be not subjected to too high a temperature. All moisture having been expelled, the tube is to be allowed to cool down, and is afterwards to be brought again into contact with heat, to sublime the arsenious acid.

The pointed end is first to be heated to approaching redness, care being taken that the aperture be not closed thereby, and the tube is then to be slowly moved through the flame until it warms each part in succession, as far as the enclosed metal extends. The temperature at which the sublimation takes place is 380° Fah., which should not be much exceeded. If the process has been well managed, and arsenic be present, it will be found in the form of arsenious acid, occupying a position around the tube about half an inch beyond the copper strips. The part of the glass surrounding the strips will be stained by a whitish opalescent film, which when the heat has been carried high becomes green; it is a salt of copper, as is proved by the action of the ferrocyanide of potassium. This stain does not in any way interfere with the test. The metallic copper is encrusted with dioxide.

The arsenious acid of the ring is formed by the action of the oxygen of common air, which as it passes over the heated metal combines with any arsenic present. It collects on the nearest cool place, in the form of minute octahedrons of a remarkable brilliancy. The crystals are much more characteristic of arsenic than the reduced metal, which may be counterfeited by many substances. The smallest amount of the acid that can be satisfactorily recognized is the $\frac{1}{500}$ of a grain. In examining it, the tube should be first cut immediately below the ring, and the deposit viewed with a magnifying glass at the open end near the crystals; in this way the influence of the refractive power of the tube can be avoided. The production of arsenious acid from the metal is also advantageous, inasmuch as the bulk is increased in the ratio of their equivalents, or as 75.34 to 99.34 (AsO_3). The field covered by the crystals is also larger than that which would be occupied by the reduced arsenic. There is no sublimate which can rise under the preceding circumstances, that will in any way embarrass the operator.

Arsenious acid thus obtained forms a perfect evidence, and none other will be desired by the chemist. But in medico-legal questions it is proper to collect it, and use the other tests, so as to remove all doubt from the minds of the jury. The best way to collect the acid, is to cut the tube with a file, immediately above and below the ring, and pound up the whole in a mortar of sufficient hardness. The particles of glass cannot interfere with any test to be used.* A portion of the powder thus obtained should be digested in pure water, and tested by ammonia-nitrate of silver, sulphuretted hydrogen, &c. Another part may be reduced with charcoal, and a third portion introduced into Marsh's apparatus.

In examining the delicacy of the foregoing process, it was found that one part of metallic arsenic in 200.000 parts of fluid can be detected, and $\frac{1}{500}$ gr. sublimed and satisfactorily recognized. Mr. Brett (Lond. Ed. and Dub. Phil. Mag., No. 132) did not succeed in detecting less than 00469, or nearly $\frac{1}{250}$ gr. of metal, by Marsh's apparatus. M. Riench considers the discoloration of the copper as a sufficiently delicate test for arsenic, but in my researches the color of the deposit did not appear so perfect a means of discrimination as the production of the sublimate of acid. Its adamantine lustre and triangular facets are much more distinctive; the ease with which it can be volatilized—its rising without previously fusing, are all characters rarely met amongst chemical substances, and are collectively, common to no other body. The length of time requisite to conduct an analysis up to the production of the sublimate, when the solution does not offer any cause of delay, does not exceed five minutes, for a small quantity. But the presence of other metals in the solution, as well as the impurities of commercial muriatic acid, are sources of embarrassment which it is necessary to examine before this test can be recommended to the profession. This inquiry forms the third division of our subject.

3. Arsenic exists in many specimens of hydrochloric acid met with in commerce. Such an adulteration is fatal to the delicacy of Riench's test; and the acid used in the foregoing process must be first carefully examined and purified. The method recommended by M. Riench for this purpose is the best—boiling the acid along with bright copper strips, as long as they continue to be stained with arsenic. The quantity of acid used must be in proportion to the amount of fluid; there is no danger of using too much. But the presence of remedial agents in the suspected mixtures is a source of much more difficulty. In the November

* It should always be remembered that glass not unfrequently contains arsenic, which is used in its manufacture as a deoxidizing and decoloring agent. Every sample of glass to be employed in toxicological experiments, should therefore be carefully examined to prove its purity.—Eds.

number for 1829 of the American Journal of the Medical Sciences, there is an exceedingly important paper by Samuel Jackson, late of Northumberland County, Pa., on a case of suspected poisoning, which shows how necessary it is to know what substances are present in the fluid submitted for examination, before forming an opinion. Four highly intelligent physicians, appointed a committee by the coroner for the examination of the contents of the stomach, &c. of a person recently deceased, drew up a report to the effect—that he had died from the effects of arsenic, when none of that drug existed in the matters analyzed. This serious error arose from the presence of tartar emetic and common salt, combined with the color of the fluids. The tests used were sulphuretted hydrogen, ammonia-sulphate of copper, ammonia-nitrate of silver, and the production of the arsenical alloy with copper. The action of all these reagents on the suspected mixtures, was examined by the side of their effects on a solution of arsenious acid, without detecting the error. But notwithstanding this, and a certain amount of moral evidence produced against the accused, it has been made clear by Dr. Jackson that none of the poison was present.

Of all the substances which may be met with in the matters examined, the salts of antimony give rise to the most serious difficulties. If sulphuretted hydrogen or Marsh's test be used, it is impossible to distinguish between arsenic and antimony, when both are present in certain proportions. It is therefore of the first importance, that this and other similar sources of error should be removed, so as to present to the jury a satisfactory report.

The metallic substances to be expected, whether in broths, medicines, or the contents of the stomach, &c. are the sulphates of copper and zinc, acetate of lead, nitrate of bismuth, corrosive sublimate, calomel, tartar emetic, and nitrate of silver. Of these the salts of silver and lead are precipitated by the addition of sufficient hydrochloric acid, and therefore do not interfere with the test. The sulphate, and other compounds of copper and zinc, do not exert any action in this case. But bismuth, mercury, and antimony, attack the copper strips as well as arsenic.

Bismuth.—Upon the addition of hydrochloric acid to the nitrate, a sub-nitrate is precipitated, but afterwards dissolved by the acid. From this solution is produced, almost immediately, a pinkish gray deposit of a crystalline texture, when clean copper is introduced. The reduced bismuth accumulates on the strips until it drops off. But the presence of this metal does not hinder the reduction of the arsenious acid, so that they are both thrown down together, the only inconvenience being that more copper is required, and care must be taken that the arsenic be not lost by the falling off of the deposit. If both agents be present, the character of the precipitate upon the copper cannot be considered as a test at all, for it will differ with the proportion of either. But by carrying on the process to the second step, the separation becomes perfect, for the arsenious acid sublimes away, and the bismuth remains at the lower end of the tube. In this way, $\frac{1}{500}$th grain of arsenic mixed with $\frac{1}{100}$th grain of nitrate of bismuth was clearly recognized after sublimation. This impurity is not however often to be expected in the fluids under consideration.

Antimony.—The entire separation of this substance from arsenic, by the method proposed, forms one of its chief advantages. If tartar emetic be present, it will not in any way affect the copper until hydrochloric acid is added. The first consequence of the introduction of the acid into the fluid, is the precipitation of the oxide of antimony, which it afterwards dissolves. From this solution the metal is rapidly deposited in the form of a gray crust, very much resembling that formed when arsenic alone is present in small quantity, but unlike the latter substance there is no darkening, or falling off of the antimonial crust. It is an exceedingly delicate test for antimony alone; but when arsenic is also present, the appearances depend upon the proportion of one to the other.

In subliming, arsenious acid is separated; this takes place at 380° Fah., long before the fusion of antimony at 800° Fah., which is nearly a red heat. In the case before us, more care than usual must be devoted to the heating of the tube, so that the melting point of antimony be not exceeded. This furnishes us with an additional argument in favor of carrying the process to the second stage, before an opinion can be formed upon sufficient grounds.

Mercury acts much more readily on copper than any of the preceding metals. If corrosive sublimate be present in the solution, it attacks the strips without assistance of any acid. The deposition of mercury takes place without heat, but boiling hastens the process. Under these circumstances, arsenic does not fall down, however great the quantity present. The mercurial deposit is at first gray, and afterwards, as the amount increases, presents all the physical characters of the metal, so that it cannot be mistaken for any other substance. But when the quantity in solution does not exceed one part in fifty thousand of the fluid, it will not fall until hydrochloric acid and heat are added—conditions under which arsenious acid also is reduced. So that mercury can be separated from a solution, except a small quantity, without disturbing the arsenic; but if the amount be very minute, both metals fall down, and the character of the stain is no longer a test. The presence of corrosive sublimate is however rarely to be expected, since it is the more active poison of the two; but the test under consideration may be used for the detection of mercury, as well as arsenic and antimony. Calomel may be looked for in many instances; and although it will not be found in fluids, yet in those cases where the coats of the stomach are macerated in dilute hydrochloric acid, for the solution of arsenious acid, it will also be dissolved and converted into corrosive sublimate. But this is not so great an inconvenience as might appear at first sight, for by sublimation the two substances are entirely separated from each other.

Introducing the copper strips coated with both metals into the subliming tube, and heating, the mercury will rise in the metallic form, and the arsenic as arsenious acid. The extremity of the tube surrounding the copper will not be stained green, as already stated, but be colored with the orange oxide of mercury. A microscopical examination of the sublimate will quickly decide the question whether any arsenious acid be present; for its octahedral form and transparency contrast strongly with the spheres of opaque mercury. In these cases it is best to dissolve the acid away from the metal, by boiling the whole sublimate in pure water and testing again, before an opinion is given.

The results of the examination made on this subject, may be condensed under the following heads.

1st. In consequence of the occasional failure of Marsh's test, as shown by Messrs. Danger and Flandin, and the length of time necessary to carry on the process, when minute quantities of arsenic are present, it has become a desideratum to possess some more certain means of collecting the poison out of solutions. The process recommended by M. Riench is the best yet discovered; but it is not a good test for the metal, because many other substances produce deposits which resemble that of arsenic to a great extent. *But by subliming always from the precipitate collected, the test is increased in value and certainty.*

2d. When solids or coagulable substances are submitted for examination, the addition of dilute hydrochloric acid is recommended as the most promising means of dissolving out the arsenious acid.

3d. Copper strips should be added until no stain is produced after thirty minutes' ebullition.

4th. Antimony and mercury are the only probable sources of embarrassment. The first is separated from the mixed precipitate by never raising the heat to 800° Fah. Large quantities of mercury are first removed by the action of copper alone; and when it is mixed with arsenious acid in the sublimate, the solution of the acid by pure water separates it from the metallic globules.

5th. The simplicity of the manipulation and the certainty of the result, are exceedingly strong recommendations. But the facility with which a large number of examinations can be made with only a small quantity of matter, is the most valuable feature of the process. If all the operations are conducted in small tubes, an ounce of the suspected fluid will be sufficient to yield ten or more portions of sublimate.

6th. All the difficulty and loss of collecting common precipitates are removed by using the test, and it is liable to fewer sources of failure than those already known.

In view of these advantages, Riench's test is recommended to the profession as a valuable contribution to toxicology.

Prince Edward Court House, Va., November 9, 1842.

Art. III.—*A Catalogue of the Birds of Connecticut, arranged according to their natural families;* prepared for the Yale Natural History Society, by Rev. James H. Linsley, A. M., Member of the Conn. Acad. of Arts and Sciences, of the Yale Nat. Hist. Soc., of the Boston Soc. of Nat. Hist., &c.

[Communicated to the Yale Natural History Society.]

In the "Catalogue of the Mammalia of Connecticut," published in Vol. xliii, of the American Journal of Science, I proposed to furnish next, for the Society, a *list of the birds*, which I now have the pleasure to transmit for the consideration of that body. I would beg leave to remark, that most of the birds herein named have been obtained in Stratford, Connecticut, and its vicinity, and are now in my cabinet. In comparatively few instances, where species have been found in the adjacent states of Massachusetts and New York, I have included them as belonging to this state, although I have not hitherto had the satisfaction to obtain them here. But I have studiously avoided admitting those, concerning which the evidence was unsatisfactory.

Those species of land birds marked as found in New Haven only, were obtained by J. D. Whelpley, M. D. of that city: to him and to several other gentlemen, who have kindly aided me by specimens and information, I improve this opportunity to tender my thanks.

An asterisk standing before the number of a species, refers to a *similar character and number* in the notes and observations.

Class II.

Sub-Class I. Insessores.

Order I. Accipitres.

Family *Vulturidæ.*

*1. Cathartes Aura, Illiger, Turkey Buzzard, Stratford.

Family *Falconidæ.*

*2. Falco peregrinus, Gmelin, Great-footed Duck Hawk, Stratford.

*3. F. sparverius, Linnæus, Sparrow Hawk, Stratford and New Haven.

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*1. The turkey buzzard is not very uncommon here. I have known it in Connecticut from a child, having at that period counted twenty in a flock in Northford in the month of August, though not knowing the name until I subsequently found them in similar flocks in the southern and middle states. Dr. Dekay also reports this species as found in New York. At the south, where they abound, it is seldom one attacks domestic poultry; but many years since I saw in Northford, in this state, a splendid male turkey buzzard pounce down upon a chicken about three quarters grown, and within about three rods of where I was standing with two other persons. As he turned his eye upon us, still standing upon the chicken, he appeared so much alarmed as to be unable to rise; we all ran upon him, and when within a few feet of him he rose, just clearing our heads, and dropping the chicken at our feet he hurried off. He however sailed about in the air for several hours, much to the terror of the occupants of poultry yards.

*2. This celebrated and powerful bird was wounded by a gun shot in Stratford, and after he was taken soon recovered, and was kept on poultry until he became too expensive to the owner, and he released him. Although I was disappointed in not obtaining him for my cabinet, being by the owner forgotten at the moment of his release, yet I had had previous opportunity to examine him, and was certain of its being the true *peregrinus* or duck hawk.

*3. I had a specimen of this bird in my cabinet that I shot in the south of Georgia, and it is believed to be often seen here. Dr. Whelpley mentions having seen it at New Haven.

4. Falco temerarius, Audubon, Little Corporal Hawk, Stratford and New Haven.

*5. F. columbarius, Wilson, Pigeon Hawk, Stratford and New Haven.

*6. F. chrysaetos? Linn. (*ossifragus* of Wilson,) Black Eagle, New Haven.

*7. F. Washingtoniensis, Audubon, Washington Eagle, Stratford.

*8. F. leucocephalus, Wilson, Bald Eagle, Stratford and New Haven.

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*5. I shot at an individual of this bird in my yard in November last, but failed to obtain him. He is a trifle larger than the preceding species of sparrow hawk.

*6. The black or sea eagle is not only a frequent visitor upon Long Island, but there are said to be two eyries of these birds near New Haven; that is, one is found at Mount Carmel, eight miles north of New Haven, and the other four miles east, on the high ridge of Saltonstall's Pond.

*7. This noble bird was shot in New Canaan in April, 1821, and was sent to me in Stratford by Mr. J. Silliman. He soon recovered from his wound, and became perfectly domesticated. I kept him a while confined, but soon found it unnecessary, because if he left my premises, he would return to the stand at night. I have known him to eat fourteen birds, (mostly *Muscicapa tyrannus*, king-bird,) and then he was satisfied for a week. He appeared to prefer this mode of living, and paid no attention to a daily supply. He however in the course of the summer became so mischievous among the young ducks of my neighbors, that I was compelled to kill him. A single anecdote of his conduct may not be uninteresting. While he had possession of my front yard, occupying the centre as his stand, (the walks making a semicircle to the door,) he would remain perfectly quiet if *gentlemen* or *ladies* entered; but if a person with tattered garments, or such persons as were not accustomed to come in at the front door entered the yard, it was actually dangerous for them, and they could only escape the tremendous grasp of his talons by running with their full strength and shutting the gate after them. Facts of this kind often occurred, and I was occasionally compelled to release from his grasp such individuals as he had taken captive. With one claw in the sward and grass, he would hold quietly any man with the other. My domestics, both male and female, often felt this power of his talons and grasp. He would not allow their passing in that yard, and long acquaintance did not change his temper towards them. If however such persons passed him in the adjoining yard, to the door in the rear of the house, he made no complaints. What renders this truly remarkable was, he had no training to this purpose while in my possession, and was wild when I received him.

The figures and descriptions of this bird which have fallen under my observation, agree well with my specimen—in dimensions about seven feet in alar extent, and the tail extended from one to two inches beyond the folded wings, according to the best of my recollection. In short, I am not aware of any fact that would tend to show him otherwise than the true Washington eagle.

*8. The bald eagle is quite common in this vicinity. One recently killed here on the Housatonic weighed 14¼ lbs., and measured 7½ feet in stretch of wings. An-

9. Falco fuscus, Gmelin, Hen Hawk, common.

*10. F. Sancti-Johannis, Bonaparte, Rough-legged Hawk, New Haven.

*11. F. buteoides, Nuttall, Short-winged Hawk, Stratford and New Haven.

*12. F. borealis, Audubon, Red-tailed Hawk, Stratford and New Haven.

*13. F. Cooperii, Bonaparte, Cooper's Hawk, Stratford ?

*14. F. Pennsylvanica, Aud., Broad-windged Hawk, Stratford and New Haven.

*15. F. hyemalis, Aud., Red-shouldered Hawk, Stratford and New Haven.

*16. F. cyaneus, Linn., Marsh Hawk, Stratford and New Haven.

*17. F. haliætus, Aud. and Wilson, Fish Hawk, common.

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other, killed here this spring, measured nine feet in alar extent, as I am informed by one who saw him. There has been, it is believed, for several years an eyrie of them near Black Rock in Fairfield. I seldom pass there at any season of the year without seeing at least one of these noble and magnanimous birds.

*10. The rough-legged hawk is said by Audubon to be the same as the *F. lagopus* and *F. niger*, or black hawk of Wilson.

*11. *F. buteoides* is what the farmers call hen hawk, and is quite destructive to poultry. It is supposed by some authors to be the young of *F. borealis.*

*12. The red-tailed hawk which I have in my cabinet, was killed here on the 24th of November last. Length nineteen inches, alar forty two inches: the young of one or two years with ten bands upon the tail. This species may be distinguished by its horrid and "unearthly scream."

*13. Cooper's hawk I imagine I have often seen here in autumn, but not having taken it am not sufficiently certain to insert its name without a query. But as it has been found both in Massachusetts and New York, it is highly probable that it will be occasionally found among this numerous family in Connecticut. The same may be said of No. 4, the little corporal hawk.

*14. Of the broad-winged hawk, a very rare bird, I obtained a female specimen in Stratford in the spring of 1841, and found it to compare well with a specimen in Peale's Museum, New York, which Mr. Audubon, who was with me at the time, assured me was correctly named.

*15. Of the red-shouldered hawk, I obtained a specimen in Stratford, January 6, 1843. Length nineteen and a half inches, and forty three inches in stretch of wings; five white bands upon the tail, with the end of the same also white. It is one of our finest looking hawks.

*16. The marsh hawk (*F. uliginosus* of Wilson) is the most common hawk found in our salt meadows and fields adjoining. My specimen is about twenty inches in length, and in plumage a male of the second year.

*17. The fish hawk is believed annually to breed in the large swamp in this town, from whence he daily repairs to the Housatonic for food. I recently obtained a specimen that measured five feet seven inches in alar, which had just taken a flounder (*Platina plana*, Storer) of twelve inches in length, which I also obtained

Family *Strigidæ.*

*18. Strix cinerea, Bonaparte, Great Grey Owl, Stratford and New Haven.

*19. S. nyctea, Wilson, Snowy Owl, Stratford and New Haven.

20. S. Assio, Audubon, Red Owl, common.

*21. S. Virginica, Wilson, Great Horned Owl, Stratford.

*22. S. otus, Wilson, Long-eared Owl, Stratford.

23. S. brachiotus, Wilson, Short-eared Owl, Stratford.

*24. S. nebulosa, Wilson, Barred Owl, Stratford and New Haven.

*25. S. Acadica, Gmelin, Little Owl, Stratford and New Haven.

26. S. flammea? Wilson, Barn Owl, Stratford.

*27. S. Scandiaca? Linn., Great horned White Owl, New Canaan.

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with him, and in the stomach of the latter I found four specimens of a *new shell.* I think I have seen it asserted that this hawk never makes two attempts at the same fish. But in this instance he caught the fish, and when rising out of the water it escaped his grasp, he immediately pounced again upon and succeeded in retaking it.

*18. A specimen of the great grey owl was killed in Stratford, and presented by me to the Yale Natural History Society on the 6th of January last. It proved to be the young of one or two years of this very rare bird.

*19. Of the snowy owl I have had five specimens. The female, which I sent to the cabinet of the Yale Natural History Society, weighed five pounds and ten ounces, and measured four feet ten inches in alar. The male weighed fourteen ounces less.

*21. The great horned owl is much less common than formerly. I have a specimen from New Milford, presented by Messrs. G. & E. Thompson of Bridgeport.

*22. The long-eared owl and the red or screech owl breed here.

*24. Of this rather rare bird I have obtained two specimens in Stratford this season.

*25. Of the *little Acadian owl*, I have obtained one specimen, which was found lying upon his back in a barn-yard in a cold morning in March, 1841, though still living. It is very rare here; I have seen but two individuals. His stomach contained only some half-digested angle worms, (*Lumbricus terrestris.*)

*27. About twenty years since, I had presented to me in New Canaan a most beautiful owl, shot in that place, the color of which was pure white with thick spots of dark brown, each of which was round and about the size of duck shot. It is believed the dimensions of the bird were somewhat less than the snowy owl. Soon after it was presented and left, and before I had measured and described it, the donor returned and took it "to send to New York," after which I heard nothing more of it. I have never been able to find any satisfactory description of it in the books; but the *Strix Scandiaca* answers the nearest to my own and my family's recollection of it. It is certain it was not the *S. nyctea*, which it most resembled.

Order II. Passeres.

Section 1. Ambulatores.

Family *Caprimulgidæ.*

*28. Caprimulgus vociferus, Wilson, Whip-poor-will, common.
29. C. Virginianus, Brisson, Night Hawk, common.

Family *Hirundinidæ.*

30. Cypselus Pelasgius, Temminck, Chimney Swallow, common.
31. Hirundo purpurea, Linn., Martin, common.
32. H. bicolor, Vieill., White-bellied Swallow, Stratford and New Haven.
*33. H. riparia, Linn., Bank Swallow, Stratford and New Haven.
34. H. rufa, Gmelin, Barn Swallow, common.
*35. H. fulva, Bonaparte, Cliff Swallow, ?

Family *Ampelidæ.*

36. Bombycilla Carolinensis, Brisson, Cedar Bird, Wax-wing, common.
*37. B. garrula, Vieill., Waxen Chatterer, ?

Family *Alcedinidæ.*

38. Alcedo Alcyon, Linn., King-Fisher, Stratford and New Haven.

Family *Trochilidæ.*

39. Trochilus colubris, Linn., Humming Bird, common.

Family *Certhidæ.*

40. Sitta Carolinensis, Wil., White-breasted Nuthatch, common.

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*28. The whip-poor-will has become somewhat rare in Fairfield County, but they abound in some parts of Branford, much to the annoyance of sleepers, as I have found by experience.

*33. The bank swallow breeds at Stratford Point, and at Birmingham, (Derby.)

*35. It is not many years since the *cliff swallow* was first found in New England. In 1836, I counted twenty two nests of them under the eaves of a barn near Machias, in Maine, all teeming with indescribable life and animation. Since then they have been found in most parts of New England, and if not already in Connecticut, there is little doubt that we shall soon have our share of their incessant and cheerful twitter.

*37. The *B. garrula* is found both in New York and Massachusetts, and doubtless occasionally visits Connecticut.

41. Sitta Canadensis, Linn., Red-bellied Nuthatch, Stratford and New Haven.

42. Certhia familiaris, Linn., Brown Creeper, Stratford and New Haven.

*43. C. varia, Aud., Black and White Creeper, Stratford and New Haven.

44. Troglodytes Ædon, Aud., House Wren, common.

45. T. hyemalis, Vieill., Winter Wren, Stratford and New Haven.

46. T. palustris, Aud., Marsh Wren, Stratford.

47. T. brevirostris, Nutt., Short-billed Marsh Wren, Stratford and New Haven.

Family *Turdidæ.*

48. Sialia Wilsonii, Swainson, Blue Bird, common.

*49. Turdus polyglottus, Linn., Mocking Bird, Stratford and New Haven.

50. T. migratorius, Linn., Robin, common.

51. T. felivox, Vieill., Cat Bird, common.

52. T. rufus, Wilson, Brown Thrush, common.

53. T. minor, Gmelin, Hermit Thrush, Stratford and New Haven.

54. T. Wilsonii, Bonaparte, Wilson's Thrush, Stratford and New Haven.

55. T. mustelinus, Aud., Wood Thrush, Stratford and New Haven.

56. T. aurocapillus, Wilson, Golden-crowned Thrush, common.

*57. T. Noveboracensis, Nuttall, New York Aquatic Thrush, Stratford. Rare at New Haven.

58. Parus atricapillus, Wilson, Black-capt Titmouse, common.

59. P. bicolor? Aud., Tufted Titmouse, New Haven.

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*43. *Certhia varia* I have often seen here in the spring in stormy weather, but never in autumn.

*49. The mocking bird was found to breed in Stratford twenty years since, but not later to my knowledge. Prof. O. P. Hubbard states that they were not unfrequently found at Pomfret, in Connecticut, ten years since.

*57. The New York aquatic thrush last season built a nest in my grape arbor, and laid three eggs, and then left on account of the pruning of the vines, which occurred as late as August 5th.

*60. Regulus calendulus, Bonaparte, Ruby-crowned Wren, Stratford and New Haven.

*61. R. cristatus, Vieill., Golden-crested Wren, Stratford and New Haven.

*62. R. tricolor, Wilson, Tricolored Wren, Stratford and New Haven.

63. Anthus aquaticus, Audubon, Brown or Red Lark, Stratford.

64. Sylvia æstiva, Audubon, Summer Yellow-bird, common.

65. S. coronata, Latham, Yellow Rump Warbler, Stratford and New Haven.

66. S. maculosa, Latham, Black and Yellow Warbler, Stratford and New Haven, not common.

67. S. petecchia, Latham, Yellow Red Poll Warbler, Stratford and New Haven.

68. S. virens, Latham, Black-throated Green Warbler, Stratford and New Haven.

*69. S. Blackburniæ, Latham, Blackburnian Warbler, Stratford and New Haven.

*70. S. icterocephala, Latham, Chestnut-sided Warbler, Stratford and New Haven.

*71. S. striata, Latham, Black Poll Warbler, Stratford and New Haven.

*72. S. pardalina, Bonaparte, Canada Warbler, Stratford and New Haven.

*73. S. castanea, Wilson, Bay-breasted Warbler, Stratford. Common at New Haven.

74. S. maritima, Wilson, Cape May Warbler, Stratford.

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*60–62. If these are all true species of Regulus, there ought to be another, which is more common than either, i. e. one which is not distinguished by any variety of color in the crown from that of the back. Audubon mentions the fact that the young in August are thus destitute of any peculiar color in the crown, but as late as October and November last, I saw and took numbers of this description in my garden. My own opinion is, that though all these peculiar marks of the crown are very distinct in different individuals, yet that they are only indications of different ages of the same species, and therefore not even varieties of the Regulus. The full feathered adult is probably *Regulus tricolor*, (vide also Dr. Kirtland's Report on the Zoology of Ohio, to the Ohio legislature, 1838, p. 183.)

*69, 70, 71, 73, and several others, are often extremely numerous at New Haven in the month of May.—J. D. W.

*72. *Muscicapa Canadensis* of Wilson. Where Stratford and New Haven are mentioned, the birds are very rare.

*75. Sylvia mitrata, Latham, Hooded Warbler, New Haven.

*76. S. Canadensis, Latham, Black-throated Blue Warbler, Stratford and New Haven.

77. S. Pinus, Wilson, Pine Warbler, common.

78. S. autumnalis, Audubon, Autumnal Warbler, Stratford and New Haven.

79. S. Parus, Wilson, Hemlock Warbler, Stratford and New Haven.

80. S. Americana, Audubon, Particolored Warbler, Stratford and New Haven.

81. S. trichas, Audubon, Maryland Yellow-throat, common.

*82. S. trochilus, Latham, Yellow Wren, Stratford and New Haven.

*83. S. rubricapilla, Wilson, Nashville Warbler, Stratford and New Haven.

*84. S. Roscoe, Audubon, Roscoe's Warbler, Stratford.

*85. S. sphagnosa, Bonaparte, Pine Swamp Warbler, New Haven.

86. S. auricollis, Latham, Orange-throated Warbler, New Haven.

*87. S. azurea, Audubon, Cerulean Warbler, Stratford.

88. S. vermivora, Wilson, (*Dueris vermivora* of Audubon,) Worm-eating Warbler, New Haven.

*89. S. discolor, Audubon, Prairie Warbler, New Haven, rare.

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*75. The Hooded Warbler was seen in New Haven by Dr. Whelpley, in the month of June, and of course probably breeds in that vicinity.

*76. I obtained two specimens of this rare bird in this place last autumn, which I now have.

*82. The yellow wren, in autumn, is one of the most common of the Sylvias in this section.

*83. Of the Nashville warbler I have seen but one specimen, which I took in my garden two years since.

*84. An esteemed writer in the Journal of Science, for January, 1842, page 133, says, that "Audubon's species of *S. Roscoe,* is the young of the Maryland yellow-throat."

*85. Dr. Whelpley, who has taken the *sphagnosa,* and as well as myself the *Canadensis,* cannot be satisfied that they are the same species as indicated by the article in the Journal to which reference has already been made.

*87. *S. azurea* I saw here in April, 1841, and recognized it from a specimen I had from Ohio.

*89. See also Journal of Science, Vol. XLII, page 136, for notice of *Prairie Warbler* in Connecticut.

*90. Sylvia Swainsonii, Audubon, Swainson's Warbler, New York and Massachusetts.

*91. S. agilis, Wilson, Connecticut Warbler, New York and Massachusetts.

*92. S. chrysaptera, Wilson, Golden-winged Warbler, New York and Massachusetts.

*93. S. pensilis, Audubon, Yellow-throated Grey Warbler, New York and Massachusetts.

*94. Sylvia flava ? West River.

Family *Muscicapidæ.*

95. Muscicapa ruticilla, Wilson, Red Start, Stratford and New Haven.

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*90, 91, 92, 93. These four warblers are said by Rev. Mr. Peabody and Dr. Dekay, to have been found in Massachusetts and New York. I have the *S. pensilis*, which I shot in Georgia, and the *chrysaptera,* from Ohio, through the kindness of Prof. J. P. Kirtland, and though I have no knowledge of their being found living in this state, yet there is good evidence of their occasional visits during migration seasons.

*94. The *Sylvia flava* of Linnæus, *yellow wagtail,* I have, with some hesitation, added to the above list of our Sylvias. In the autumn of 1840, I saw a large flock of wagtails near West River, (New Haven,) and had a good opportunity to watch them and their motions for some length of time, while they were apparently feeding on insects among the high grass. At every instance when one alighted, *it wagged the tail four times,* invariably; even if it changed position every moment, there were four distinct wags of the tail, and no more. They are thus described: "breast and abdomen yellow, the two outer tail feathers obliquely half white at the tips, length about six and a half inches, weight five drachms—are diffused throughout Europe, Siberia, and in Madeira, in moist situations." To the above description, as far as I could discover without taking in hand, these birds agreed. They appeared unusually tame and fearless, but I had no means of securing one; and therefore with much regret left them feeding where I found them. They are said to feed on the aurelia of Phryganea, and these abound in our fresh-water streams, as I have witnessed. [This species occurs in spring and fall at New Haven. It is a late visitant and rarely seen.—J. D. W.]

We have no other bird, or any description of authors that has fallen under my observation, which corresponds to these. The *Turdus aquaticus*, of Audubon, is about the same length, but these birds had longer tails and less bodies, and much more yellow on the breast, and certainly were not that species, (*T. aquaticus.*) I only hesitate to add this species to our list, because they have not been otherwise named as belonging to this country. I trust, however, they will yet be found not only to be American birds, but actual tenants of Connecticut, at the season of their migrations.

Sir Wm. Jardine, Nat. Library, division Ornithology, birds of Great Britain and Ireland, Vol. XI, page 200, says: "They are found at the extreme counties of Scotland, and southward it reaches Spain and Italy, and its extra-European range is India, Japan, Java, and Sumatra." It therefore can be nothing surprising, should it wander here in its southern and western flights.

96. Muscicapa fusca, Bonaparte, Phœbe, common.

97. M. rapax, Wilson, Wood Pewee, Stratford and New Haven.

98. M. Acadica, Gmelin, (*querula* of Wilson,) Small Pewee, Stratford. [Common in gardens in New Haven.—J. D. W.]

99. M. Traillii, Audubon, Trail's Fly-catcher, Stratford.

100. M. Tyrannus, Brisson, King-bird, common.

*101. M. crenata, Linn., Great-crested Fly-catcher, Stratford and New Haven.

102. M. Cooperii? Nuttall, Olive-sided Fly-catcher, Stratford.

103. M. cœrulea, Wilson, Blue Grey Fly-catcher, Stratford.

104. Icterea viridis, Bonaparte, Yellow-breasted Icterea, New Haven. [Haunts the vicinity of springs, and builds in watered hollows.—J. D. W.]

105. Vireo flavifrons, Bonaparte, Yellow-throated Fly-catcher, Stratford and New Haven.

*106. V. solitarius, Vieill., Solitary Vireo, Stratford and New Haven.

107. V. Noveboracensis, Bonaparte, White-eyed Vireo, common at New Haven.

*108. V. gilvus, Bonaparte, Warbling Vireo, Stratford and New Haven.

109. V. olivaceus, Bonaparte, Red-eyed Vireo, Stratford and New Haven.

Family *Lanidæ.*

*110. Lanius borealis, Richardson, Butcher Bird, Stratford and New Haven.

*111. L. Carolinensis, Audubon, Loggerhead Shrike, Stratford.

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*101. A specimen of the *great-crested fly-catcher,* was shot by me in the spring of 1838, in my front yard, the only living individual of this bird I have seen in this state.

*106. The solitary vireo I took here last fall, but it is extremely *rare.*

*108. Of the *Vireo gilvus* I have taken several specimens, differing very much in their plumage. The under parts of one were mostly bright yellow.

*110. The *Lanius borealis* in January, 1841, killed a robin here, by one blow of his little foot in the throat of the robin, and though I found on taking both immediately after, that the robin weighed about a quarter more than the butcher-bird, yet he carried the robin on the wing with perfect ease. Another, a few years since, made an attack upon a cage of Canary birds hanging in my sitting-room, myself and family present, and within a few feet of the cage. The former had the *bones* and *flesh* of a mouse in his stomach, which proved him guilty of conduct not before charged to him.

*111. I shot a specimen of the loggerhead shrike in Georgia, which I now have in my cabinet, and have long supposed it to be occasionally found here.

*112. Lanius excubitoroides, Swainson, Grey Shrike, Stratford.

Family *Corvidæ.*

113. Corvus Americanus, Audubon, Common Crow, common.

114. C. ossifragus, Wilson, Fish Crow, Stratford.

115. C. cristatus, Wilson, Blue Jay, common.

*116. Quiscalus versicolor, Audubon, Crow Black-bird, common.

117. Q. ferrugineus, Bonaparte, Rusty Black-bird, Stratford and New Haven.

*118. Q. baritus, Bonaparte, Thrush Black-bird, New Haven.

119. Sturnus ludovicianus, Linn., Meadow Lark, common.

*120. Icterus Baltimore, Wilson, Baltimore Oriole, common.

*121. I. spurius, Bonaparte, Orchard Oriole, Stratford and New Haven.

*122. I. phœnicius, Daudin, Red-wing Black-bird, common.

123. I. Pecoris, Audubon, Cow Black-bird, common.

124. I. agripennis, Audubon, Rice Bird, Bob-o-link, common.

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*112. The grey shrike is said by Nuttall, to be found in winter in the vicinity of Boston, and of course in the north of Connecticut.

*116. Two pairs of crow black-birds have regularly, for many years, nidificated and raised their young on trees about twenty feet distance from my house. No more or less than two pairs yearly returned to the same trees, until the present there are three pairs.

*118. Of the *thrush black-bird*, one specimen only has been observed by Dr. Whelpley at New Haven, and of course is rare in Connecticut.

*120. That bachelors abound among the *Baltimore orioles*, I may remark that a pair nidificated upon an elm near my garden, as they have done for twenty years past; after taking the male for my cabinet, I found the female immediately obtained another mate, and, as an experiment, I killed him and six more in succession, making eight in the whole; and yet she did not forsake the nest, but raised her young, and had still a husband to assist in rearing them, and returned in the following year as usual.

Also of the *house wren*, I once took the male after sunset, while the female was in incubation, and at 7 o'clock next morning she had a new mate singing to her, and he appeared, without exception, the most exquisitely happy of all birds I ever beheld. His apparent joy evinced almost a mental distraction—shaking his wings, singing incessantly, jumping and twirling like the merriest dancer; and he proved faithful in rearing the young not his own!

*121. The *orchard oriole* has for years raised a brood upon the same tree, within a few feet of my front door. At the close of the year in which he raised his first brood, he had not acquired the adult plumage; in the spring following he had.

*122. In Vol. xxxvii, page 195, of this Journal, may be found a notice of the *carnivorous* nature of the red-wing black-bird, as discovered here.

Family *Fringillidæ.*

125. Alauda cornuta, Wilson, Horned Lark, Stratford and New Haven.

*126. Emberiza nivalis, Wilson, Snow Bunting, Stratford.

127. E. Americana, Wilson, Black-throated Bunting, New Haven, very common.

128. Pyrhula enucleator, Temminck, Pine Grossbeak, New Haven. [Not uncommon in February.—J. D. W.]

129. Tanagra rubra, Wilson, Black-winged Summer Red-bird, Stratford and New Haven.

*130. T. æstiva, Wilson, Summer Red-bird, Stratford and New Haven.

131. Fringilla cyanea, Wilson, Indigo Bird, Stratford, very common.

132. F. leucophrys, Temminck, White-crowned Finch, Stratford.

133. F. albicollis, Wilson, White-throated Sparrow, Stratford and New Haven.

134. F. graminea, Audubon, Bay-winged Finch, Stratford and New Haven.

135. F. melodia, Wilson, Song Sparrow, common.

136. F. Savanna, Wilson, Savannah Sparrow, Stratford.

137. F. hyemalis, Audubon, Snow Bird, common.

*138. F. passerina, Wilson, Yellow-shouldered Sparrow, Stratford and New Haven.

139. F. arborea, Wilson, Tree Sparrow, Stratford and New Haven.

140. F. socialis, Wilson, Chirping Sparrow, common.

141. F. Juncorum, Nuttall, Rush Sparrow, Stratford and New Haven.

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*126. Large flocks of the snow bunting were repeatedly seen here in the winters of 1840, '41 and '42. Previous to that period, I had not seen one here in eighteen years, at which time I killed one and presented it to Mix's museum, New Haven.

*130. The summer red-bird is more rare than the scarlet tanager; though I have taken both here during the season of cherry blossoms; and last season I saw the latter come five times in about an hour, and carry away cherries in his bill, evidently to his young. That these birds eat cherries, I am not aware was before known.

*138. The *yellow-shouldered sparrow* I have not seen, but found a nest of the eggs, as decided by Dr. Brewer of Boston, to whom I sent some of them.

142. Fringilla maritima, Audubon, Sea-side Finch, Stratford and New Haven.

143. F. palustris, Audubon, Swamp Sparrow, Stratford and New Haven.

144. F. tristis, Linnæus, American Gold Finch, common.

*145. F. Pinus, Wilson, Pine Finch, Stratford and New Haven.

146. F. linaria, Linn., Lesser Red Poll, New Haven.

147. F. rufa, Wilson, Fox-colored Sparrow, Stratford and New Haven.

148. F. littoralis, Nuttall, Shore Finch, Stratford and New Haven.

149. F. erythrophthalmia, Linn., Ground Robin, common.

*150. F. Ludoviciana, Bonaparte, Rose-breasted Grossbeak, Middletown and New Haven.

*151. F. purpurea, Wilson, Purple Finch, New Haven.

*152. F. cardinalis? Bonaparte, Cardinal Grossbeak, New Haven.

*153. F. ambigua.

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*145. Of the *pine finch* I took one specimen from a large flock, which was here in my yard as late as Nov. 7th, 1840. But it is evidently a very rare bird in Connecticut. Nuttall says their summer dress and breeding habits are wholly unknown.

*150. The rose-breasted grossbeak was found in Upper Middletown, in the summer of 1841, as I was then informed by Dr. Warner of that town, the discoverer. The first and only instance of the kind, to my knowledge, in Connecticut; but I have since learned that they are common at Hartford, where they breed.

*151. Dr. Whelpley informs me that a flock of the *purple finch* was seen at New Haven in the month of February, 1837.

*152. The *cardinal grossbeak* is said by Rev. Mr. Peabody, to be "seen at irregular intervals, in the villages on the Connecticut River." But is it not probable they escaped from some cages, as multitudes of them are thus introduced from the south? I may add that the *Fringilla ciris* of Audubon, the beautiful *nonpareil*, might in this manner be said to be an inhabitant of Stratford for a season, as I brought on three from Savannah, and they escaped my cage by a blow from a cat's foot, which set them at liberty, and they refused to return.

*153. I have inserted the *Fringilla ambigua* of Nuttall, merely for the purpose of remarking, that a year or two since I noticed the wood pewee, (*M. rapax* of Wilson,) feeding a strange bird larger than herself; and watching them, I found her to carry him insects seven times in about a minute. I took them both at a shot, and instantly recognized Mr. Nuttall's "ambiguous sparrow," and at the same time was apprehensive it was the *young cow black-bird*, from its foster parent; which, by the way, is the first instance I can learn that the *wood pewee* was ever thus duped by that bird of imposition. Mr. Audubon assured me it was new to him, and also that the young bird is both the *ambigua* and *cow black-bird*, as I had supposed, and that he had Mr. Nuttall's specimen in his possession. Al-

*154. Curvirostra Americana, Wilson, Common Crossbill, Trumbull.

*155. C. leucoptera, Wilson, White-winged Crossbill, Trumbull.

Section 2. SCANSORES.

Family *Picadæ.*

156. Picus auratus, Audubon, Golden-winged Woodpecker, Stratford and New Haven.

*157. P. pileatus, Wilson, Log Cock, Stratford and New Haven.

158. P. erythrocephalus, Wilson, Red-headed Woodpecker, Stratford. Rare at New Haven.

*159. P. Carolinus, Wilson, Red-bellied Woodpecker, Stratford.

160. P. varius, Wils., Yellow-bellied Woodpecker, N. London.

161. P. villosus, Wilson, Hairy Woodpecker, Stratford and New Haven.

162. P. pubescens, Wilson, Downy Woodpecker, common.

*163. P. tridactylus, Bonaparte, Three-toed Woodpecker, ?

Family *Cuculidæ.*

164. Coccyzus Americanus, Bonaparte, Yellow-billed Cuckoo, Stratford and New Haven.

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low me here to suggest, that I imagine the manner of rearing by different foster parents, and of course different food, gives the young black-bird a somewhat different appearance. This specimen of mine had more resemblance to a sparrow, than many of that species I have seen, and even those raised by the song sparrow, which are the most common.

*154. The common crossbill has been repeatedly seen in Trumbull, in this county, by a Mr. Beers of this place, as he informs me. They were feeding very fearlessly among the thick hemlocks, and were easily shot.

*155. The white-winged crossbill is more common than the preceding species, in Massachusetts and New York, and Mr. Nuttall says they are found as far south as the middle states, and it is doubtless occasionally an inhabitant of Connecticut.

*157. The log-cock, once common here, is much more rare at present, and this is true of others of this family, except *P. pubescens,* which is still abundant, and believed to be greatly injurious in taking the sap from young fruit trees. The most healthy tree is the most liable to his attacks; and hence he is commonly called *sap-sucker.* The immense number of little circular holes in the bark of fruit trees, are all caused by this bird.

*159. The red-bellied species I saw in Stratford, ascending an apple tree, on the 16th of October last, the only specimen I ever saw.

*163. The three-toed woodpecker has been seen in Massachusetts, and Audubon saw it as far south as Pennsylvania; and probably it visits the northern parts of Connecticut.

165. Coccyzus erythrophthalmus, Bon., Black-billed Cuckoo, Stratford and New Haven.

Section 3. Gyrantes.

Family *Columbidæ.*

166. Columba migratoria, Linn., Passenger Pigeon, common.

167. C. Carolinensis, Linn., Mourning Dove, Stratford and New Haven.

*168. C. domestica, Linn., Domestic Pigeon, introduced.

Sub-Class II. Grallatores.

Order Gallinæ.

Family *Phasianidæ.*

*169. Meleagris Gallopavo, Linn., Wild Turkey, Northford.

170. Pavo cristatus, Linn., Pea Fowl—introduced.

171. Numida Meleagris, Linn., Guinea Fowl—introduced.

172. Gallus domesticus, Linn., Common Fowl—introduced.

Family *Tetraonidæ.*

173. Tetrao umbellus, Linn., Partridge, common.

*174. T. Cupido, Linn., Pinnated Grouse—exterminated.

175. Perdix Virginiana, Latham, Quail of New England, common.

*176. P. Californica, Latham, Californian Quail, Bridgeport.

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*168. All our domestic pigeons, including *tumblers, pouters, fantails, shakers, smiters, turbits, helmets, turners,* the *crested,* the *laced,* &c., are only varieties of one and the same species, and so also is the *carrier.* Of the last named variety, I have in my cabinet one of a pair, for which the gentleman, Mr. E. Thompson, who presented it, gave thirty dollars in New York, and has succeeded in rearing them.

*169. The last *wild turkey* that I have known in Connecticut, was taken by a relative of mine, about thirty years since, on Letoket Mountain, in Northford. It was overtaken in a deep snow, and thereby outrun. It weighed, when dressed, twenty-one pounds.

*174. Nuttall says, found in Connecticut, in shrubby barrens.

*176. A specimen of the Californian quail was killed near Bridgeport, in December, 1840; another had been shot the year previous. They had probably been introduced by sportsmen from Mexico. They differ from ours, in having a black throat and a beautiful crest of six feathers. A good figure may be seen in Vol. III, page 200, of Wright's Buffon.

Order Grallæ.

Family *Charadridæ.*

177. Charadrius semipalmatus, Bona., Ring Plover, Stratford.
*178. C. melodus, Bona., Piping Plover, Stratford.
179. C. pluvialis, Wilson, Golden Plover, Stratford.
180. C. Wilsonius, Ord, Wilson's Plover, Stratford.
181. C. vociferus, Linn., Kill Deer, Stratford.
182. C. Helveticus, Bona., Black-bellied Plover, Stratford.
*183. Hematopus polliatus, Aud., American Oyster Catcher, Stratford.
184. Strepsilus interpres, Illiger, Turn Stone, Stratford.

Family *Gruidæ.*

185. Ardea Herodias, Wilson, Great Heron, Stratford.
*186. A. Egretta, Gmelin, Great White Heron, Stratford.
*187. A. candidissima, Gmelin, Snowy Heron, Stratford.
188. A. cœrulea, Linn., Blue Heron, Stratford.
*189. A. Nycticorax, Wilson, Night Heron, Stratford.
*190. A. minor, Wilson, American Bittern, Stratford.
191. A. virescens, Linn., Green Heron, Stratford.
192. A. exilis, Gmelin, Least Bittern, Northford.

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*178. *C. melodus*, though a rare bird, breeds in this town near the shore of the Sound.

*183. The oyster catcher is now rare here, but fifteen years since they were not very uncommon in autumn.

*186. The *egretta* (*alba* of Bona.) is inserted upon the authority of Audubon, who saw it in Massachusetts, and as it is south and west of us, we are evidently entitled to an occasional call.

*187. I once saw a beautiful white heron in a salt marsh in this village, which I did not obtain, but supposed it to be the *candidissima* of Gmelin and Wilson.

*189. The night heron (or squalker as it is called here) breeds abundantly in our swamps, and a few years since it was quite an object to hunt for the nests, in order to obtain the eggs, with which lads would easily fill their hats, and thus afford a fine meal for a family.

*190. I obtained a fine specimen of the American bittern two years since, which had previously given great alarm to many of our inhabitants by its peculiarly doleful and mournful sounds at evening. One man who was laboring near the swamp, it is said, ran a mile in the greatest consternation, alleging that "the d—l was after him." It is also stated by several of our most respectable inhabitants, that forty seven years since, one hundred men united in a company on the Sabbath, to traverse this swamp, and succeeded in killing one of these same birds, and that their sounds have not been heard in the town since, until the former instance occurred which secured a specimen to me. Goldsmith has very happily expressed the *booming* of the bittern. "It is impossible (he says) for words to give any ad-

Family *Tantalidæ.*

*193. Ibis Falcinella, Bona., Glossy Ibis, Stratford.

Family *Scolopacidæ.*

*194. Numenius longirostris, Wils., Long-billed Curlew, Stratford.

195. N. Hudsonius, Latham, Esquimaux Curlew, Stratford.

196. N. borealis, Latham, Small Curlew, Stratford.

197. Tringa alpina, Wilson, Dunlin or Ox-Bird, Stratford.

*198. T. Schinzii, Bona., Schinz's Sandpiper, Stratford.

199. T. pectoralis, Bona., Pectoral Sandpiper, Stratford.

200. T. rufescens? Vieill., Buff-breasted Sandpiper, New York and Massachusetts.

201. T. subarquata? Aud., Curlew Sandpiper, New York and Massachusetts.

202. T. maritima? Brunrick, Brownish-purple Sandpiper, New York and Massachusetts.

203. T. Wilsonii, Nuttall, (*pusilla* of Wilson,) Wilson's Sandpiper, Stratford.

204. T. cinerea, Wilson, Knot or Ash-colored Sandpiper, Stratford.

205. T. semipalmata, Wilson, Semipalmated Sandpiper, Stratford.

*206. Totanus semipalmatus, Tem., Pill-will-willet, Stratford.

207. T. vociferus, Sabine, Great Yellow Shanks, Stratford.

208. T. flavipes, Bona., Lesser Yellow Shanks, Stratford.

*209. T. Glottis, Bechstein, Green Shanks, Stratford.

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equate idea of its solemnity. It is like the interrupted bellowing of a bull, but hollower and louder, and is heard at a mile's distance, as if issuing from some formidable being that resided at the bottom of the waters." Ours is equally appalling, and was said by many persons to be heard a mile at evening.

*193. Of the ibis I obtained five individuals about six years since; they were killed in this town by a sportsman to whom they were unknown. It is the only instance I have found or heard of this bird in Connecticut.

*194. I have a large specimen of the long-billed curlew, killed here August 3, 1841. Length twenty seven inches, three feet five inches in alar, bill six inches. But I have another from Florida, presented by Mr. Goodsell of Bridgeport, whose bill measures seven and a half inches, though I think it the same species.

*198. Of Schinz's sandpiper I took two specimens three years since, the only ones I have seen.

*206. The pill-willet, the green-rump tattler, the spotted-tattler, and Bartram's tattler, all breed here.

*209. Of the green shank, I have seen but one, which was killed here last autumn.

210. Totanus chlorapygeus, Bona., Green-rump Tattler, Stratford.

211. T. macularius, Bona., Spotted Tattler, Peet-weet, Stratford.

212. T. Bartramius, Wilson, Bartram's Tattler, Stratford.

*213. Caladris arenaria, Illiger, Sanderling, Stratford.

*214. Limosa fedoa, Vieill., Great Marbled Goodwit, Stratford.

215. L. Hudsonia, Swainson, Rose-breasted Goodwit, Stratford.

*216. L. Edwardsii? Richardson, White Goodwit, Stratford.

217. Scolopax grisea, Gmelin, Red-breasted Snipe, Stratford.

218. S. Wilsonii, Temminck, Wilson's Snipe, Stratford.

219. Rusticola minor, Nuttall, Woodcock, Stratford.

Family *Rallidæ.*

*220. Rallus elegans, Audubon, Rail, Stratford.

221. R. crepitans, Gmelin, Clapper Rail, Stratford.

*222. R. Virginianus, Linn., Virginia Rail, Stratford.

*223. R. Carolinus, Linn., Carolina Rail, Stratford.

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*213. The sanderling is also rare here. I have obtained but one specimen.

*214. The great marbled goodwit was here last August in large flocks, but very shy. I obtained one—length nineteen inches, alar thirty four, tarsus three and a half, and bill four and a half inches.

*216. The white goodwit (*L. Edwardsii*) I shot here fifteen years since, from a flock of other plovers, flying southwest, at a place called Fresh Pond, in this town. It is the only specimen I have ever seen or heard of south of Hudson's Bay. I then supposed it to be a large *white snipe*, though a sportsman with me at the time was sure it was a goodwit, the bill being turned upward and very long. Since I have seen the description of *Fedoa Canadensis*, (*L. Edwardsii* of Richardson,) I feel quite confident this was a specimen of that very rare bird. Not knowing its extreme rarity, and as my attention was then more particularly directed to land birds, I did not preserve it. But I have a good recollection of it, and no season has since passed in which I have not very desirously looked for it. It was larger than Wilson's, or the red-breasted snipe. It was mostly a *pure white*, except some portions of it were rather a yellowish white, and "*rostro sursum recurvo.*"

*220. A female specimen of *R. elegans* was taken alive in this town and confined in a cage, where it soon deposited an egg, both of which I obtained immediately. On opening the bird, I found many eggs of different sizes, sufficient to establish the fact that this species breeds here, though before unknown even to visit New England. She was chased into a hole in a bank near a salt marsh, and thus secured. The egg is larger and darker at the greater end than those of the clapper rail, which breeds abundantly here.

*222. The Virginia rail also breeds here, as I sent an egg to Dr. Brewer, which he decided to be of this species.

*223. The Carolina rail was so abundant here last autumn in the marshes of the Housatonic, that something like hundreds were killed in a few hours, and that too for several days together. They were esteemed a great delicacy.

224. Rallus Noveboracensis, Bon., Yellow-breasted Rail, Stratford.

*225. Gallinula Martinica? Gmelin, Purple Gallinule, Massachusetts and New York.

*226. G. galeata? Leichtinstein, Florida Gallinule, Massachusetts and New York.

*227. Fulica Americana, Gmelin, Cinerous Coot, Stratford.

Family *Phalaropidæ.*

228. Phalaropus fulicarius? Bona., Red Phalarope, New York and Massachusetts.

*229. P. Wilsonii, Sabine, Wilson's Phalarope, Bridgeport.

230. P. hyperboreas? Latham, Hyperborean Phalarope, New York and Massachusetts.

Family *Recurvirostridæ.*

231. Recurvirostra Americana? Linn., Avocet, New York and Massachusetts.

Order Anseres.

Family *Anatidæ.*

*232. Cygnus musicus, Bona., Whistling Swan, Fairfield.

*233. Anser hyperboreus, Pallas, Snow Goose, Stratford.

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*225 and 226. Although the gallinules are reported as found in Massachusetts and New York, it is doubtful whether they have ever visited the shores of Connecticut.

*227. Of the cinerous coot I have had five specimens killed here, although it is by no means common.

*229. Wilson's phalarope I have in my cabinet; it was killed in Bridgeport, and sent to me by a friend, and is probably one of the rarest birds in New England. It is not only beautiful, but the great quantity of plumage on a bird so small and delicate, together with his unique bill, seems to render it one of the most peculiar of this class of animals.

*232. The swan was shot on this shore in Fairfield and taken alive; it was afterwards purchased by Messrs. G. & E. Thompson of Bridgeport, with a view to domesticate; it however survived but two weeks, and was by them presented to my cabinet. Two other swans were seen in Stratford last fall, as I am informed by a neighbor who saw them. Another was killed here some years since, that weighed twenty seven pounds. I find the *vertebræ of the neck* to be twenty one, *ex*clusive of those in the anterior part of the back—*in*clusive, twenty seven. The carcase of my swan was thrown out into the field, with the supposition that some animal would devour it; although this occurred the 29th of December last, it lies there still untouched! though almost daily visited by dogs.

*233. Two individuals of the snow goose were killed here some years since, as I am informed by a sportsman who saw them. Five individuals were seen here Oct. 5, 1842, by Mr. B. Livingston of this place, as he assures me.

*234. Anser Hutchinsii ? Rich., Southern Goose, Stratford.
*235. A. Canadensis, Vieill., Wild Goose, Stratford.
236. A. leucopsis, Becht, Barnacle Goose, Stonington.
*237. A. Bernicula, Bona., Brant Goose, Stratford.
*238. Anas clypeata, Linn., Shovel-bill Duck, Stratford.
*239. A. strepera, Linn., Grey Duck, or Gadwell, Stratford.
240. A. acuta, Linn., Pheasant Duck, Stratford.
241. A. boschas, Linn., Mallard, Domestic Duck, Stratford.
242. A. Americana, Gmelin, Widgeon, Stratford.
*243. A. moschata, Willoughby, Musk Duck, introduced, Stratford.
*244. A. obscura, Gmelin, Black Duck, Stratford.
*245. A. sponsa, Linn., Wood Duck, Bride Duck, Stratford.
246. A. discors, Linn., Blue-winged Teal, Stratford.
247. A. crecca, Forster, Green-winged Teal, Stratford.
*248. Falligula mollissima, Bona., Eider Duck, Stratford.

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*234. *Anser Hutchinsii*, it is believed, is not unfrequently taken here in the spring, and is called southern goose, because it does not winter here.

*235. Hundreds of the common "wild geese" winter at the mouth of the Housatonic, and so near my own dwelling, that I often with my *telescope* present a distinct view of their *eyes* to my friends who call. Birds are said to be near enough to shoot, when their eyes are visible to the sportsman. Many are killed here merely for sale by gunners, who frequently send them to New York.

*237. The brant is common here in winter; and at Stonington it is said by sportsmen, that three kinds are found there, as I am informed by J. H. Trumbull, Esq., of that borough, viz. the *common* or *May brant*, the *blue brant*, and the *speckled brant*. These differences of plumage may possibly be occasioned by different ages of the same species, though I am not aware of such varieties, and therefore am induced to believe the *Anser leucopsis*, or barnacle goose, to be one of them. Nuttall says, the latter species are stragglers along the coasts of the United States.

*238. The shovel bill is a very rare bird here, though I have obtained two fine male specimens this season. It is one of the most beautiful of the genus.

*239. Flocks of the grey duck were here as early as August last season, and were among the best of ducks for the table.

*243. The musk duck, improperly called "Muscovy," were introduced from Brazil, where they are found wild. The oil sacks at the root of the tail, ought always to be removed before cooking, on account of the musky odor contained in them. As they lay in trees, those who rear them ought to place old barrels in the crotches of trees near home, for their use.

*244. The black duck (*obscura*,) breeds here occasionally, and might be easily domesticated.

*245. Four specimens of the summer duck were killed here this season, one of which I have.

*248. One or two eider ducks were killed here two or three years since, by Mr. Lucius Curtis.

*249. Falligula spectabilis, Bona., King Duck, Stratford.

250. F. perspicillata, Bona., Surf Duck, Box Coot, Stratford.

*251. F. nigra, Bona., Scoter Duck, Stratford.

252. F. fusca, Bona., White-winged Coot, Velvet Duck, Stratford.

253. F. Americana, Swainson, Amer. Scoter Duck, Stratford.

254. Fuligula rubida, Bonaparte, Ruddy Duck, Stratford.

255. F. Labradora, Bonaparte, Pied Duck, Stratford.

*256. F. Valisneria, Stephens, Canvass-back Duck, Stratford.

257. F. ferina, Stephens, Red-headed Duck, Stratford.

258. F. clangula, Bonaparte, Golden-eyed Duck, Stratford.

259. F. marilla, Stephens, Broadbill, Bluebill Duck, Stratford.

260. F. rufitorsque, Bonaparte, Ring-necked Duck, Stratford.

261. F. albeola, Bonaparte, Buffel-headed Duck, Stratford.

262. F. glacialis, Bonaparte, Long-tailed Duck, Old Wife, Stratford.

*263. F. histrionica, Bonaparte, Harlequin Duck, ?

Family *Mergidæ.*

*264. Mergus Merganser, Linn., Goosander, Stratford.

*265. M. cuculatus, Linn., Hooded Merganser, Stratford.

*266. M. serrator, Linn., Red-breasted Merganser, Stratford.

Family *Pelicanidæ.*

267. Phalacrocorax carbo, Dumont, Cormorant, Stonington.

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*249. I have obtained here this season two specimens of the king duck, said never to have been seen here before. They are among the best for the table.

*251. The ducks answering to Bonaparte's *F. nigra*, which I find here, I imagine will be found to be the young, or a mere *variety* of the *perspicillata*. Several of these species exhibit great variety of plumage at different ages. One passed the last winter here, said to be pure white, and he escaped the effects of shot enough to have killed a score.

*256. One or two individuals of the *canvass-back duck* were shot here by Mr. L. Curtis, two years since.

*263. The harlequin duck is said by Nuttall, to be an accidental visitor as far south as the middle states; but not found here, to my knowledge.

*264. A fine specimen of the *goosander* was taken here alive a few days since, which I have in my cabinet, and three others have been recently shot, one of which I have.

*265. I have also two specimens of the rare *hooded merganser*, of one or two years old, which were shot here in a fresh-water pond.

*266. The *red-breasted merganser* is not uncommon here. I have had three or four specimens. It is improperly called the *shell drake*, by gunners in this state.

*268. Phalacrocorax dilophus, Nuttall, Double-crested Cormorant, Stratford.

269. P. graculus, Dumont, Shag, Guilford.

*270. Sula bassana, Lacepede, Gannet, Solan Goose, Stratford.

271. S. fusca, Brisson, Booby, Guilford.

Family *Laridæ.*

*272. Larus argentatus, Brunrick, Herring Gull, common.

*273. L. atricilla, Linn., Black-headed Gull, Stonington.

*274. L. capistralus, Temminck, Small Black-headed Gull, Stonington.

275. L. tridactylus, Latham, Kittiworke Gull, Stonington.

276. L. zonorhynchus, Richardson, Ring-billed Gull, Stonington.

277. L. canus, Linn., common Gull, Stonington.

278. L. fuscus, Linn., Silvery Gull, Stonington.

*279. L. Bonapartii, Swainson, Bonaparte's Gull, Stratford.

280. L. marinus, Linn., Black-backed Gull, Stratford.

281. Sterna Hirundo, Linn., Marsh Tern, Stratford.

282. S. minuta, Wilson, Silvery Tern, Stratford.

*283. Rhynchops nigra, Wilson, Black Skimmer, Stonington.

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*268. I have a fine specimen of the *double-crested cormorant*, killed here March 15, 1841, by Mr. David Bennet. It is one of our rarest as well as most extraordinary birds; has a fine black crest back of each eye, weight 5 pounds 7 ounces.

*270. The true solan goose killed here, which I presented to Yale Natural History Society, had in its stomach a bird, and in the stomach of the latter was also a bird—destruction on destruction. Mr. B. Silliman, Jr. and Dr. Whelpley, who opened the stomach, observed this fact, as the former gentleman informed me. It was previously supposed this bird lived wholly on fishes.

*272. The herring gull is very abundant in this harbor during winter and spring.

*273, 274. The black-headed gull and the next species are occasionally killed in Stonington, as I am informed by Mr. J. H. Trumbull. The other species of gulls which follow, are said by various authors, to be found from New York to Maine, and though I have not been so happy as to find them, yet as they are birds of passage, they probably pass over, and occasionally stop in Connecticut.

*279. I obtained an individual of this beautiful species of gull, August 1, 1842, under circumstances which induce the belief it had been remaining several days with us. Its stomach contained the larva of several species of Phryganea, (cadew flies,) and several of those beautiful little fishes, *Gasterosteus Noveboracensis*, Cuv., (New York stukle back,) all of which are very common in our fresh waters.

*283. The *Rhynchops nigra*, or cut-water, I think I have seen in storms of wind, from Massachusetts Bay to Florida, and not far from the southeastern corner of our state. I have always found sailors to be their mortal enemies, believing their presence an indication of bad weather. The adaptation of their bill to their necessities, is a wonderful indication of wisdom in the Contriver.

*284. Lestris Richardsonii, Swainson, Jagar, Bridgeport.
*285. Puffinus cinereus, Cuvier, Sheer-water, Stonington.
*286. P. obscurus, Cuvier, Dusky Petrel, ?
*287. Thalassidroma Leachii, Bonaparte, Fork-tail Petrel, ?
*288. T. Wilsonii, Bona., Stormy Petrel, Long Island Sound.
*289. Procellaria glacialis, Linn., Fulmar Petrel, ?

Family *Colymbidæ.*

*290. Colymbus glacialis, Linn., Great Northern Diver, Stratford.
*291. C. septentrionalis, Linn., Red-throated Diver, Stratford.
292. Podiceps rubricollis, Latham, Red-necked Grebe, Stratford.
293. P. cristatus, Latham, Crested Grebe, Stratford.
294. P. cornutus, Latham, Horned Grebe, Stratford.
*295. P. minor, Latham, Little Grebe, Stratford.

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*284. Some intelligent sportsmen in Bridgeport, are confident of having seen this jagar in our water. It is said also to be found in Massachusetts and New York.

*285. The sheer-water is common throughout the whole Atlantic coast, though not often in the Sound; it is frequently seen near our southeast corner, and may be considered one of our birds, if we can lay claim to any thing apparently always on the wing.

*286. Dr. Dekay has inserted the dusky petrel in his report on the birds of New York, and it is so nearly allied to the preceding species, it has probably an equal claim to our attention.

*287. The fork-tailed petrel is said by Audubon, to be more common on the coast of Massachusetts, than the following species, which is often in our Sound.

*288. I have seen the stormy petrel, (*Mother Carey's chicken,*) not only in our Sound, but even west of Stratford, and sitting quietly upon the water. I sent a specimen to the Yale Natural History Society, that I caught at sea, by floating about two hundred feet of thread in the air, against which it flew, and thus became entangled and taken.

*289. The fulmar petrel is said by Audubon, to be found from Long Island to Newfoundland.

*290. The great northern diver is not uncommon here on our shores.

*291. Of the red-throated diver I obtained a specimen from Mr. G. Landon, of Bridgeport, in November last, the young of the year; twenty-seven inches in length, and forty-four inches in alar extent. Mr. L. also sent me the preceding species a few days since.

*295. The grebes above named, are all found here occasionally. The only specimen of the little grebe (*P. minor,*) that I have seen, and now have with other species in my cabinet, was killed by flying against the light-house on Stratford Point. This comfort and direction to the benighted mariner, proves a great source of destruction to migrating birds. The increase of light-houses on our coast, serves greatly to decrease the numbers of the feathered race, that fly within their deadening influence. I find the skulls of such not unfrequently broken, and

296. P. Carolinensis, Latham, Pied-billed Dab-chick, Stratford.

Family *Alcidæ.*

*297. Mormon arcticus, Illiger, Puffin, Long Island Sound.
298. Alca torda, Gmelin, Razor-billed Auk, L. Island Sound.
299. Uria grylle, Latham, Black Guillemot, L. Island Sound.
*300. U. alle, Temminck, Little Auk, or Sea Dove, Stratford.
*301. U. Brunnechii, Sabine, Large-billed Guillemot, ?
302. U. troille, Latham, Foolish Guillemot, ?

I would beg leave to add that I have not adopted the new generic names of Messrs. Swainson and Audubon, because I cannot perceive the advantage of giving half a dozen or more new genera to birds usually denominated Sylvia. A student, by a glance at the bill, will perceive what is a Sylvia, and he then has only to find the species. But if some are *vermivora*, and others *sylvicola*, and *culicivora*, &c. &c., it appears to me only affording him unnecessary trouble. Our synonyms are already so numerous and perplexing, it appears rather desirable to reduce than to increase new generic or specific names. I mention these facts, merely as a reason why I have not (as it may appear to some members of the Society) kept up with the age, or the new nomenclature.

Elm Wood Place, Stratford, Conn., November, 1842.

P. S. Since the publication of "the Catalogue of the Mammalia of Connecticut," I have obtained the *Arvicula hirsutus* of Dr. Dekay, which he had described as a new species. He informed me, on the examination of my specimen, that he believed it to be the same species. I took it when recently drowned in a

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always bloody. It has been reported that five hundred dead birds were found in one morning last autumn, at the foot of the light-house on Falkner's (Falconer's) Island.

*297. The puffin and the following species are said by Audubon, Nuttall, and others, to be found from New York to Newfoundland, and of course must occasionally, at least, be in our Sound.

*300. The little auk came on board a Stratford vessel, near Martha's Vineyard, and was taken by Mr. John Brooks of this place, and brought alive to me. I have therefore inserted the species here as one of our birds.

*301. The young of the large-billed guillemot and the next species, (*troille,*) are said by Nuttall, to be seen on the coast of the middle and northern states in winter, and sometimes the old birds are seen here, but not so frequently as the younger. However, I have not seen them, and have omitted the insertion of some other species, that perhaps have equal claim to our notice. Future opportunities may afford us better evidence of their occasional visits to our little State.

cistern of water at my house. I have also since obtained from the borders of Litchfield County, in this state, the true black squirrel, (*Sciurus niger*, Linn.,) very rare with us.

I would also add, that a *white fox* was last winter killed in Oxford. The fur was coarse, like hair, and very short, but in other respects it resembled, by description of those who saw it, the arctic fox, *Vulpex lagopus* of Richardson. And Richardson describes this species as "having short white hair in summer." It is more than probable, therefore, this was the true arctic fox, though far south of his usual running. I hope yet to see the skin. Fur purchasers who have seen it, describe it as of little or no value for their purpose.

It is believed that in the course of a few weeks, I shall be able to complete my catalogues of the third, fourth, and fifth classes, viz. Reptiles, Fishes, and Mollusca of Connecticut.

Art. IV.—*A double Sulphite of the Protoxide of Platinum and Soda, a new salt;* discovered and investigated by A. Litton, and Schnederman.*

This compound is formed, when a solution of the bichloride of platinum is saturated by sulphurous acid, and afterwards neutralizing the fluid with the carbonate of soda. By this process is produced a very voluminous and almost colorless precipitate, which, as investigation showed, is a double salt of the sulphite of the protoxide of platinum and the sulphite of soda.

This salt is, when dry, an amorphous, white powder; when moist however, it has a yellowish tinge, which is the deeper, the more the solution is concentrated out of which it is precipitated. In cold water it is very slightly soluble. The solution is colorless, neutral, and leaves by evaporation, a white, varnish-like mass. In warm water it is somewhat more soluble, since the warm saturated solution, upon cooling, becomes slightly troubled and untransparent. In alcohol it is insoluble. From its solution in water, it is precipitated by the addition of the chloride of sodium, as a white, fleecy precipitate. It acts in a similar manner with many other, though not all salts, and the compound, when thus thrown down, is perfectly white.

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* Communicated to this Journal, by Prof. Litton.

One of the most remarkable properties of this salt is, that when dissolved in water, the presence of the platinum is not indicated by the usual tests. The solution remains unchanged upon the addition of hydrosulphuric acid and hydrosulphuret of ammonia. Upon the addition of an acid however, the salt seems to undergo decomposition, and the solution, even at the common temperature, is slowly colored; whereas by heating, it immediately becomes brownish red, and at length the sulphuret of platinum falls down. This precipitate is entirely soluble in the hydrosulphurets of the alkalies. The salt does not however seem to be decomposed by alkalies, as it is by acids.

By acids, even when diluted, this salt is easily decomposed, and, with the escape of sulphurous acid, dissolved. The solution in hydrochloric acid gives, when evaporated, crystals of chloride of sodium; and upon the addition of ammonia, a green somewhat crystalline precipitate, the platino-protochloride of the hydrochlorate of ammonia. The solution in sulphuric acid gives, by evaporating, crystals of the sulphate of soda, and exhibits then the dark color of the sulphate of the protoxide of platinum. By a certain concentration of the acid, metallic platinum falls down; a property which was found to belong to the sulphate of the protoxide of platinum purposely prepared. The solution of the salt in nitric acid assumes, upon evaporation, a brownish red color; if sal-ammoniac is then added, no precipitate is formed; if however the solution is evaporated almost to dryness, and sal-ammoniac added, there is formed in abundance the platino-bichloride of hydrochlorate of ammonia. The brownish red color seems here to arise from the formation of the sulphate of the peroxide of platinum.

In a solution of the cyanuret of potassium, this salt is very soluble, and by evaporation, crystals of the platino-cyanuret of potassium are obtained.

If this salt is exposed to the temperature of nearly 200°, (Centigrade,) it loses completely its water of composition. Heated to 240°, it undergoes no farther change, but when exposed to a still greater heat it begins to suffer decomposition. This decomposition is completely effected by long exposure to a red heat, and a mixture of sulphate and sulphite of soda with metallic platinum remains behind.

In order to determine the quantity of the soda and the platinum, the salt was mixed with sal-ammoniac and then exposed to a red heat. The residue, which consisted of platinum and the chloride of sodium, was washed with water, and to this water sulphuric acid was added, in order to determine the soda by weighing the sulphate of soda thus formed. In order to determine the quantity of sulphurous acid, a weighed portion of the salt was put into water and through this chlorine conducted. From this, the sulphuric acid which was thus formed, was precipitated by the chloride of barium. Of the salt dried at 200°, (Centigrade,)

1. 1.850 grammes, gave 1.190 sulphate of soda, which is equivalent to 0.521 soda; and 0.543 platinum, equivalent to 0.587 protoxide of platinum.

2. 1.108 grammes, gave 0.328 platinum, =0.3546 protoxide of platinum.

3. 1.488 grammes, gave 0.954 sulphate of soda, =0.418 soda.

4. 0.867 grammes, gave 1.234 sulphate of baryta, =0.3395 sulphurous acid.

5. 0.874 grammes, gave 1.249 sulphate of baryta, =0.3436 sulphurous acid.

These numbers give for the composition of the salt the following formula: $3NaOSO_2+PtOSO_2$; according to which in 100 parts, are contained,

	Calculated.	1.	2.	3.	4.	5.
Soda,	28.53	28.18		28.09		
Protoxide of platinum,	32.44	31.73	32.00			
Sulphurous acid,	39.03				39.16	39.32

The salt when dried by 100°, (Centigrade,) lost, when heated up to the temperature 200°, according to three different experiments, 3.90, 4.28, 4.16 per cent. of water. This gives for the formula of the salt, containing water, $2(3NaOSO_2+PtOSO_3)+3HO$, according to which formula, the quantity of water reckoned amounts to 3.94 per cent. If protoxide of platinum is suspended in water, and sulphurous acid conducted through it, the former, although with difficulty, is gradually dissolved with a green brownish color, and out of this solution the above described salt can be precipitated by the carbonate of soda.

When this double sulphite is dissolved in only so much diluted sulphuric or hydrochloric acid as is requisite to its solution, and the fluid by a low temperature evaporated, a yellow powder falls down, in proportion as the sulphurous acid escapes, which is also a compound of the sulphite of the protoxide of platinum with the sulphite of soda, but with a smaller quantity of the latter, than the first described salt. Of this salt, after having been well washed and dried at 100°. (Cen.)

1. 0.884 grammes, gave 0.306 sulphate of soda, =0.134 soda, and 0.410 platinum, =0.443 protoxide of platinum.

2. 0.443 grammes, gave 0.487 sulphate of baryta, =0.134 sulphurous acid.

These numbers correspond to the formula, $(NaOSO_2+PtOSO_2)+HO$.

According to which the salt contains in 100 parts,

	Calculated.	1.	2.
Soda, - - -	14.81	15.17	
Protoxide of platinum,	50.53	50.13	
Sulphurous acid, -	30.40		30.22
Water, - - -	4.26		

The quantity of water was not directly determined, but estimated from the loss. To obtain this salt in great quantity, is difficult, because of its great solubility in water; and consequently, when well washed in order to free it from all impurities, but little remains upon the filter. Its solution in water has an acid reaction, and is not precipitated by chloride of sodium; but in other respects, exhibits most of the properties of the first described salt, when treated with acids and alkalies.

ART. V.—*On the occurrence of Fossil Human Bones of the præhistorical world;*—extract of a letter from Dr. LUND of Logoa Santa, South America, translated and communicated for this Journal, by Rev. E. E. SALISBURY, Professor of Oriental Languages in Yale College.

TO PROFESSOR SILLIMAN.

My dear Sir—As I was reading the "Cologne Gazette" for the 9th of this month, the other day, my eye fell upon an article entitled, "Fossil human bones of the præ-historical world." It is an extract from a letter of Dr. Lund, of Lagoa Santa, S. A., who, for the last six years has been engaged in examining the animal remains found in the chalk caves of the interior of Brazil, and is now publishing a work in the Danish language, which bears the title, Blik paa Brasiliens Dyreverdu, &c., or, "A glance at the animal creation which inhabited Brazil immediately before the present geological epoch, and the now existing order of things." The letter first appeared in the "Revista Trimensal" of Rio de Janeiro, 4th volume, 13th fasc., and I therefore thought it might not so soon come to your knowledge, which may account for my venturing to communicate it to you, as an item of new intelligence *from South America, by the way of Germany!*

The information it contains is, indeed, nothing decisive as to the existence of the human species cotemporaneously with those great extinct animals whose remains are found fossilized in the earth's strata. But, as relating to the first instance of the discovery of human bones in a fossil state, it is of some interest. After mentioning that, up to the date of his letter, he had discovered in two hundred chalk caves of Brazil, one hundred and fifteen species of mammalia, of which not more than eighty eight are now known to exist there, the writer proceeds:

"In the midst of these numerous proofs of an order of things quite different from the present, I yet have never found the slightest trace of the existence of man. I supposed therefore, that this question was decided, that human bones no where occur, when, unexpectedly, after six years' toil, I had the good fortune to find these bones; and, indeed, under circumstances which admit of speaking with some certainty in favor of their occurring again. These bones I fell upon in a cave, mingled with the bones of decidedly extinct animals, as for example, of the *Platyonyx Bucklandii*, *Chlamydotherium Humboldtii*, *C. majus*, *Dasypus sulcatus*, *Hydrochœrus sulcidens*, *&c.*, which directed my whole attention to these remarkable remains. Besides, they all bore the stamp of genuine fossil bones, inasmuch as they were partly converted to stone, and partly impregnated with small particles of oxide of iron, which not only gave them an extraordinary weight, but even to some of them a metallic glistening. As to the great age of these bones, no doubt can exist; but whether they date from the times of those animals, with the bones of which they were found lying together, in company, is a question which does not admit of being determined with equal certainty, since the cave is on the edge of a lake, of which the waters are yearly driven into it in the rainy season. Not only, therefore, might animal remains by degrees come there, but those brought there by the flowing of the water at later periods, might also mingle with the earlier. This supposition has in fact received confirmation, in that among the bones of extinct animals, there are also those of races still living. The condition of the latter, too, of which some appear to differ little from fresh bones, leads to this view, while others have reached the half metallic state spoken of, and between the two sorts a third and more numerous variety is distinguishable, which has reached a middle state in decomposition. A similar difference was observed also in the human bones, by which their varying gradations of age are clearly manifested. Yet all are so altered, as well in their constituent parts as in the joining, that one cannot deny them a high antiquity; and even should they not have come there cotemporaneously with the bones of extinct animal races, still they have a sufficient interest in this respect. From the investigations of European students of nature, it results, that no land animal, of which the bones appear in a truly fossil state, has lived within our historical period, and that they consequently mount up over three thousand years. If this conclusion is applied also to human bones existing in a like state, they too are of a like antiquity. Since, however, the process of fossilization is as yet little known, especially if the time necessary to this transformation comes into question, and if it is true that this time varies according to circumstances, we can attain only to a very indefinite approximation. Be it nevertheless as it may, in any case these bones must have a high antiquity, not only far outreaching the discovery of America, but even surpassing all historical documents of our race, since up to this time no fossilized human bones have been before met with. But hence it results, that Brazil was peopled at a very remote period, and probably before our historical era, and the inquiries which, therefore, urge themselves upon us, are these: who were these oldest inhabitants? from what race were they descended? and what was their manner of life, and their natural quality of mind? Happily, these questions may be easily solved. Being in possession of several more or less perfect skulls, I was able to define the position which they have occupied in the anthropological system. And in fact, the narrow head, the prominent cheek-bones, the angle of the face, the formation of the jaw, and of the cavities of the eyes, show that these skulls belong to the American race. The Mongolian tribes come, as is well known, the nearest to it, and the most striking difference between the two is the greater flattening of the head in the former species. In this point, the discovered skulls not only accord with those of the American tribes, but some of them are to such a degree pressed in, that the forehead almost entirely disappears. It is known that the human figures which were sculptured upon the ancient Mexican monuments, are of a wholly peculiar conformation, and that the cranium retreating backwards, was made to disappear immediately above the eyes. This anomaly, which in general is ascribed either to an artificial disfiguring of the head, or to the taste of the artists, thus receives a natural explanation, since it is proved, that a race of men lived in these regions which possessed this conformation of the head.* The skeletons found belonged to both sexes, and were of ordinary size; though two male skeletons showed a larger size. After these brief observations on the corporeal constitution of the primitive inhabitants of Brazil, we will also take into consideration their probable mental condition and the degree of their cultivation. Since now it is proved, that the development of the mental powers stands in direct relation to that of the brain, it follows from the formation of the skulls found, that the intellectual life of their possessors must have played a very inferior part, and that their progress in arts and crafts must have been, in the highest degree, meagre. This inference is confirmed by the discovery of an utensil of the most imperfect make, which lay buried alongside of the skeletons. It was a semi-spherical hornstone of ten inches in circumference, which was worn smooth on the flat side, and evidently must have served to bruise seeds and other hard substances. As it cannot be my object, on this occasion, to make a thorough exposition of the present subject, a labor which I give over to abler hands, I have merely to observe, farther, that I have found human fossil bones in two other caves also, which were almost without any gelatinous part and therefore easily friable, and showed a white fracture. Unfortunately, however, they did not occur with other animal bones, so that the question of chief importance, in respect to the longer existence of the human race on this earth, remains still undecided."

Bonn, on the Rhine, Sept. 14, 1842.

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* It is still probable, however, that this configuration of cranium was the result of artificial causes.—Eds. Am. Jour.

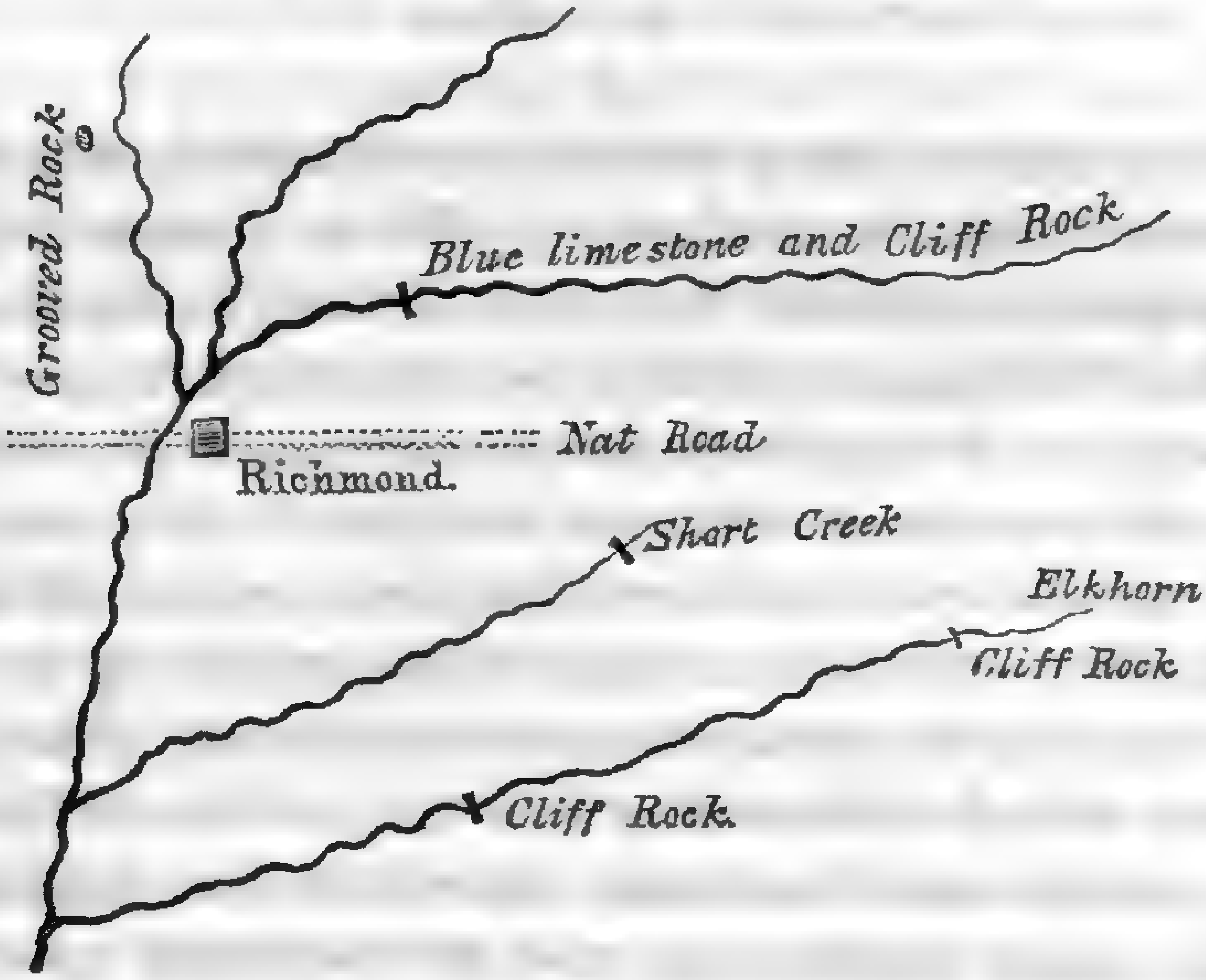
Art. VI.—*Suburban Geology, or Rocks, Soil, and Water, about Richmond, Wayne County, Indiana;* by Dr. John T. Plummer.

There is perhaps not a "nook of earth" which would not furnish some interesting geological facts to persevering inquiry. But it cannot be expected that those who act under the auspices of our legislatures, comprehending large territories in their examinations, whose researches are limited to a given period, should do more than to give us the great outlines of their subjects; the minuter delineations must be left, chiefly, to those whose permanent location gives them an opportunity of closely scrutinizing the features of their immediate neighborhoods. Many valuable specimens will find their way into the cabinet of the curious resident, which would not meet the eye of the transient examiner; and many useful facts may be developed in a series of years, but they will escape the glance of the passing inquirer.

The geology of Wayne County is so simple, that three paragraphs sufficed to tell its story to the state legislature, yet, to detail the discoveries made within less than a twentieth part of its area, pages will be required.

Richmond is situated about four miles from the eastern limits of the state, in latitude 39° 51′. The surrounding country, partaking of the general levelness of the western states, is without a hill of any note for many miles; the greatest elevation I have ascertained, is at the dividing line between this state and Ohio, near the route of the national road, where the land rises about one hundred and seventy six feet above the average level of the town. Three small streams, by their confluence half a mile north of the town, form the east fork of Whitewater River, which flows on the west side of Richmond, through a deep cut in the blue fossiliferous limestone. The national road passing through the main street of the town, intersects the river nearly at right angles, by an artificial cut through its banks. Two small streams called Short Creek and Elkhorn, run westwardly into the Whitewater, several miles south of the town. Clear Creek, another diminutive stream, also south of the town, supplies the river with water on the western side. The annexed sketch will serve to illustrate our topography, and to exhibit the localities of our different formations.

Fig. 1.

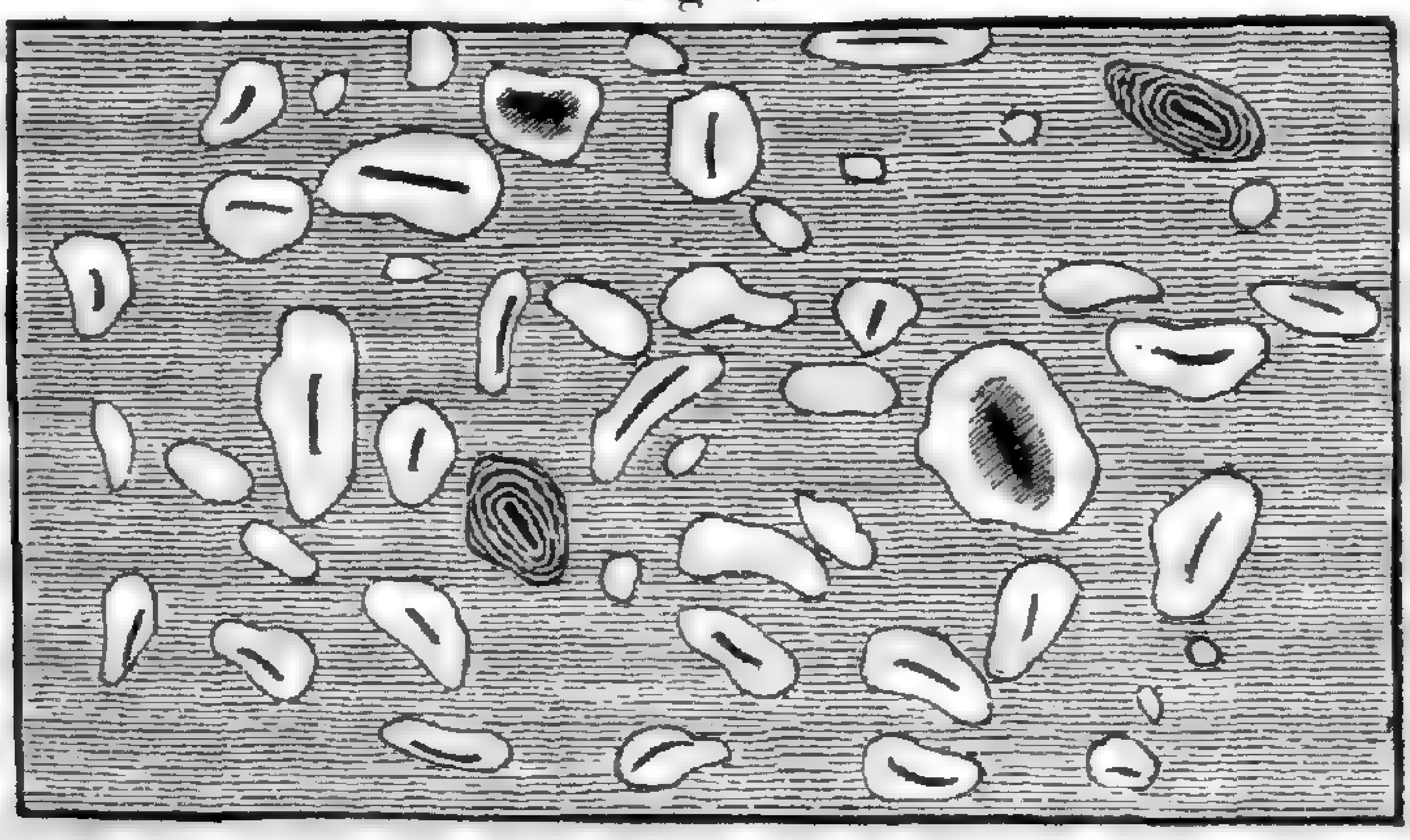


Geological Formations.—The bed of the river opposite Richmond, lies eighty five feet below the level of the plain on which the town stands; and is formed of the *blue fossiliferous limestone,* (the Trenton limestone of the New York geologists,) which rises in some places nearly to the surface of the soil, and constitutes the steep banks of the river. Numerous quarries have been opened for miles along these river escarpments; and they have furnished most of our building stone, the materials for macadamizing the national road for several miles east and west of town, the rocks for the masonry of several national bridges, and for much of our lime. From the dimensions given, it is obvious that we have an inexhaustible supply of limestone.

To the eye, the strata of this formation appear to lie perfectly horizontal; but from the occurrence of more recent formations to the east and the west of us, with other geological data, there appears to be a gentle dip in those directions, producing a declivity sufficiently great to admit of the superposition of the coal strata at the western side of Indiana and the eastern side of Ohio. Agreeably to this, Richmond is placed exactly on an anticlinal line running nearly north and south. The layers of the blue limestone vary from less than an inch to more than a foot in thickness, and some of them furnish fine flagstones. Between some of these strata lie seams of a fine blue uncalcareous clay, possessing an unctuous feel and a great degree of plasticity. By way of experiment, I had several little articles of pottery manufactured from it, and they proved to be of finer quality than our ordinary ware; and from their lighter color, it appears that this clay is not so ferruginous as that usually employed at this place. Most of the strata through which the national road passes are composed of calcareous matters of various degrees of hardness, and of bluish argillaceous masses and sandy particles, all so intermingled as to give a very broken structure to the rock. In consequence of the presence of clay, it is unfit for making good lime. Two or three specimens of the more friable portions yielded nearly one third of their weights of siliceous matter, insoluble in muriatic acid. This argillaceous member of the blue fossiliferous limestone, is known as "*marlite;*" its greatest depth is about twenty feet. Below this the rock is quite geodiferous, exhibiting numerous cavities lined with beautiful calcareous spar, sometimes associated with groups of minute crystals of satin spar; small guhrs of light brown compacted sand, in some instances feebly effervescent with acids, are also very common; spheroidal masses of sulphate of barytes often occur; and laminæ of sulphate of strontian, crossing the pockets of calcareous spar, are more rarely met with. A small specimen of galena was disclosed in one of the quarries several years ago.

In the upper strata of this formation, I have discovered a character of quite a novel kind. So far as I have learned, it has never been described, and perhaps not yet elsewhere been seen. It consists in the presence of *pisolitic balls*, imbedded in every portion of the solid rock. They average perhaps one third of an inch in diameter; and the nucleus in almost every instance appears to be a very small fragment of a Strophomena or some other thin shell. Generally no concentric layers are discoverable in them; but in other instances the succession of delicate incrustations is distinctly visible. These pisolitic concretions are always much lighter colored than the body of the rock, from which they are not separable, but are completely consolidated with it; its fracture sometimes displays a pretty marble-like surface. Fig. 2 presents nearly a fac-simile of a portion of this rock. These pisolitic strata vary from two to ten feet in depth; and are frequently found blended with the "marlite." The central portions of some of these concretions are a shade or two darker than the parts more remote from the nucleus. Whatever was the origin of these calcareous pellets, their formation was evidently extraneous to the rock which holds them, and they were deposited with the shells and other fossils that generally accompany them.

Fig. 2.



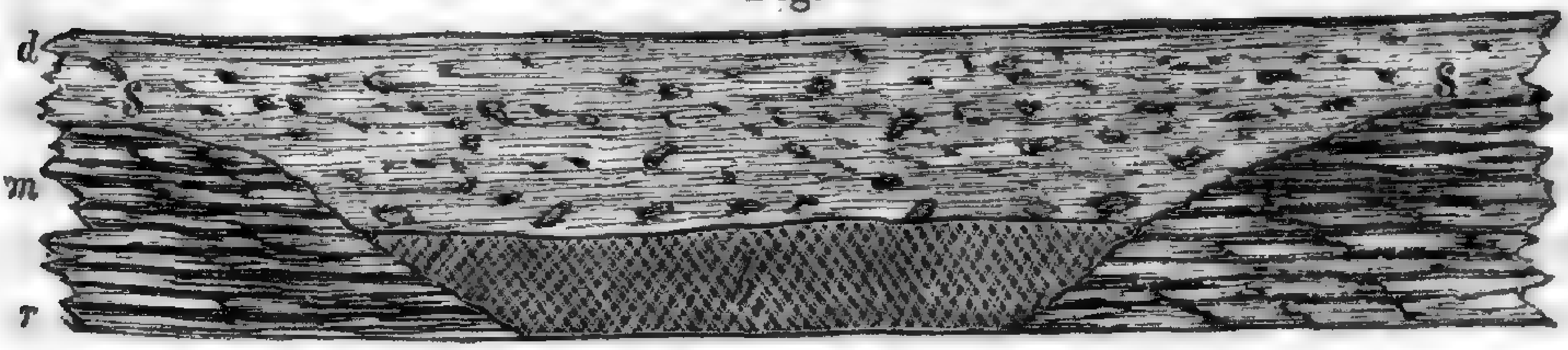
I have critically examined great numbers of them for the purpose of ascertaining their character. I conceive that waters impregnated with carbonic acid bubbling up through beds of fragmentary exuviæ, have encrusted the marine particles, until some greater outbreak swept them into distant regions, to be deposited with other sedimentary matter, and hardened into rock: it is in reference to this appearance, that I have called them pisolitic balls. Researches in other localities, if such exist, may finally determine their true character. Fig. 2 represents a portion of this pisolitic rock, which is entirely free from the usual fossils of the blue limestone; but in general, as already hinted, these pellets are mingled with them; and I have seen these globular masses resting in the cavities of some orthoceratites, as if they had been intruded into the empty shell before it was filled with the calcareous sediment.

Undulated Flagstones.—Many of our flagstones are well marked with parallel undulations, which vary in their distance from each other from half an inch to two or three inches, and rise an eighth of an inch to perhaps in some instances half an inch above the level of the stone. The stratum which supplies these flagstones has been traced a number of rods in one of the quarries; and throughout, the upper surface is not roughened, as other layers are, by imbedded fossils, but consists of a crust of sedimentary matter, which when soft appears to have been drifted into little ridges, by the ruffling of superincumbent water.

Bones of a Snake.—The larger fissures of this formation are sometimes thickly lined and even filled with stalagmite, a considerable mass of which was thrown out from a quarry seventy or eighty feet above the level of the river, containing numerous vertebræ and ribs of a snake. Most of these bones had lost their natural connection and were scattered through the stalagmite. They were not abraded, but all the processes, canals, and articulating surfaces were perfect. The bones were however so nearly decomposed that they could be easily crushed; and from the dryness of the quarry and the thickness of the stalagmite, as well as the friable condition of the vertebræ and ribs, they may claim considerable antiquity. A comparison of one of these vertebræ with the largest vertebra of a Coluber two feet long, would give to the animal a length of six or eight feet, provided the number of vertebræ was nearly equal in both. Previous to opening the quarry, the rock was completely covered by soil, and there was no apparent access to the fissure in which the stalagmite was found.

Channels of ancient streams filled up with diluvium.—The cut through this formation for the passage of the national road, exposes the beds of no less than three small streams, which appear to have run parallel with the present one, and are now covered by a thick deposit of diluvium. The section of the largest one, which is on the west side of the river, presents the following appearance on the north side of the road.

Fig. 3.



r, level of the road. *s*, smoothed surface of the rocks. *m*, marlitic rocks. *d*, diluvium. *b*, dark colored silt.

The bed of this ancient stream is about four hundred feet from the escarpment of the present river, whose waters flow nearly twelve fathoms below, in perpendicular measurement. The road ascends gently in the direction towards *r*. The silt is six feet deep, and consists of a dark bluish earth, strongly contrasting with the yellowish diluvial clay above it. This silt contains no bowlders, but pockets of coarse sand, and pebbles of various sizes are found in it. I collected some of the gravel, and it proved to be composed chiefly of fragments of granite, jasper, limestone slate, hornstone, and greenstone. The diluvium which fills up the remainder of the channel is about ten feet thick immediately over the silt, and is the same that forms the general surface of the country; bowlders however are much more numerous in the course of the obliterated stream than in any other part of the diluvium exposed to view by the cut for the road. These bowlders are from both primitive and fossiliferous rocks; several large flat pieces of blue limestone containing exuviæ were found among them, thoroughly smoothed on both sides, and rounded at the edges. Among the fossils I have not been able to discover more than two species which are not common to the neighboring rocks; yet from their being so greatly worn on every part, and accompanying the primitive bowlders, and indeed from some of them lying above the level of the adjoining strata, it is highly probable that they were transported from some distant locality of the blue fossiliferous limestone.

At *s*, on both sides of the channel, the upper surface of the stratified rock is evidently water-worn. The distance between these two points is one hundred and twenty six feet; this was probably the usual width of the stream, and agreeably to this supposition the depth of water was about six feet. Water constantly issues from between the silt and diluvium, and trickles down the sloping surface *b*, which it has whitened with a calcareous incrustation. Laborers in the vicinity have excavated little pouches at the junction of the diluvium and silt, and by this means they procure sufficient water to quench their thirst; the pedestrian also not unfrequently turns aside here for a refreshing draught. Whitewater River, near Richmond, at times becomes a formidable current; but in the driest seasons of the year, the quantity of water flowing in its channel has been estimated to be barely sufficient to supply a canal, so that it can hardly be questioned that the now buried channel once conveyed at least as much water as our present river. As the silt on the other side of the road is comparatively dry, the plausible supposition is, that the stream flowed to the south.

In the silt, removed for the passage of the national road, sticks and other vegetable matter were found; and in portions of this fluviatile deposit which I have examined, I have detected at various times, small soggy pieces of wood, such as we find at the bottom of existing waters; also fragments of the ribs of leaves and their nervous ramifications, and a well characterized piece of *pine.* I know of no pine forests within perhaps two hundred miles of this place. In addition to the evidences already given that this deposit occupies the bed of an ancient river, I would also notice the strong bilge-water odor of the recently dug silt: it is every where perceptible in the upper portions of the deposit; but at the bottom of the bed, where the silt is exceedingly compact, being almost consolidated into rock, this characteristic smell of decomposing vegetable matter in water, is very faint. Whether this stream favored the growth of molluscans, or whether a diluvial torrent scoured out the superficial sediment and its exuvial contents, I shall not pretend to say; but I have not been able to descry even the fragment of a shell in all this depth of mud.

Almost every section of the silt made with the spade, exposes a number of fine dark blue spots, as if so many nodules of earthy phosphate of iron had been divided. The exterior of some of the pebbles and of the sticks, was deeply stained with this coloring matter. I suspected it to be a natural ink, produced by the action of the tanno-gallic acid of the buried vegetable substances, upon the ferruginous matter lying in contact with them; and the use of appropriate reagents confirmed the supposition. By suspending some of this tanno-gallate of iron in water, by means of a little mucilage, I obtained a small bottle of *antediluvian ink,* formed thousands of years ago, when

> "History, not wanted yet,
> Leaned on her elbow, watching Time, whose course
> Eventful, might supply her with a theme."

*In these words I offer you a specimen of this entombed ink.**

Curious to know the composition of this deposit, I subjected some of it to mechanical and chemical separation. Upwards of two thousand grains taken from the highest portions of the silt, dried, and carefully reduced to powder as far as the mass was easily susceptible of it, were sifted; first, through a sieve whose interstices were the twelfth of an inch wide; secondly, through

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* It is of a deep black and fine lustre, contrasting strongly with the paler hue of the rest of the MS.—Eds.

another whose interstices were the twenty-fourth of an inch in width; and lastly, through gauze, by which a very fine dust was obtained. This process yielded the following proportions of pebbles, sand, and dust, in one thousand grains.

Very coarse sand and pebbles, -	80 grs.
Coarse sand, - - -	86 "
Fine sand, - - - -	125 "
Dust, - - - -	709 "

Sixteen hundred grains of this dust thoroughly dried, were heated to redness, to remove vegetable matter. The loss sustained was 36 grs., or exactly 2.25 per cent.; coinciding with the result of a previous experiment on one fourth the quantity of the powder, within .03 of a grain. Another quantity of nearly 800 grains lost but 1.02 per cent. Two hundred grains of it were then fused with carbonate of soda, and dissolved in hydrochloric acid. The solution was evaporated, and the silica separated by redissolving the residue in pure water. From the aqueous solution, the alumina and iron were precipitated by bicarbonate of soda and hydrochloric acid. A trace of manganese was discovered in the clear liquid, by means of chlorine. The lime was precipitated by oxalate of ammonia, and the magnesia by ammonia and phosphate of soda. The presence and quantity of potash, and of the sulphate and phosphate of lime, were determined by separate processes; the weight of the crystals of alum formed by boiling 100 grains of the ignited dust in sulphuric acid, enabled me to estimate the proportion of potash, including the almost inappreciable quantity derived from the incineration of the vegetable substances in the silt; the digestion in a large quantity of water, of several hundred grains of the fine dry dust, yielded the sulphate of lime; and the careful evaporation of a solution in hydrochloric acid, and the addition of pure water to the residue, left me an undissolved substance, which I considered phosphate of lime, slightly tinged with iron. These salts of lime I deducted from the oxalate of lime obtained by the first experiment; and having verified several parts of the analysis by other methods, I think the following results may be relied upon as correct. In 100 grs. of the thoroughly dried silt dust, there are of

Vegetable matter, - - -	2.25
Silica, - - - -	75.84
Alumina, - - - -	7.30
Oxide of iron, - - -	2.23
Manganese, - - - -	a trace.
Carb. of lime, - - -	8.46
" " magnesia, - - -	1.27
Sulphate of lime, - - -	.69
Phosphate of lime, - - -	1.40
Potash, - - - -	.56
	100.00

I suspect much of the carbonate of lime noted in the above table, is due to the infiltration of the calcareous water constantly trickling over the sediment in the channel of the obliterated stream.

The two ancient channels on the east side of the river, are indicated by the greater quantity of bowlders in them, and in the diluvium directly above them, together with the grooved and smoothly worn state of the rock. They contain no silt; and the water appears to have run northwardly over the naked rock, into a ravine not far off, for the smoothed edges of the strata at the point exposed denote the former existence of a waterfall of several feet, with a northern pitch. At the foot of the little cascade was found a granitic bowlder two or three feet in diameter, resting in a cauldron-like cavity, apparently formed by the sand and gravel being twirled around the bowlder by the falling water. The outlines of these smaller beds are fast becoming obscured by the mouldering "marlite," and the vegetation upon its exposed surface.

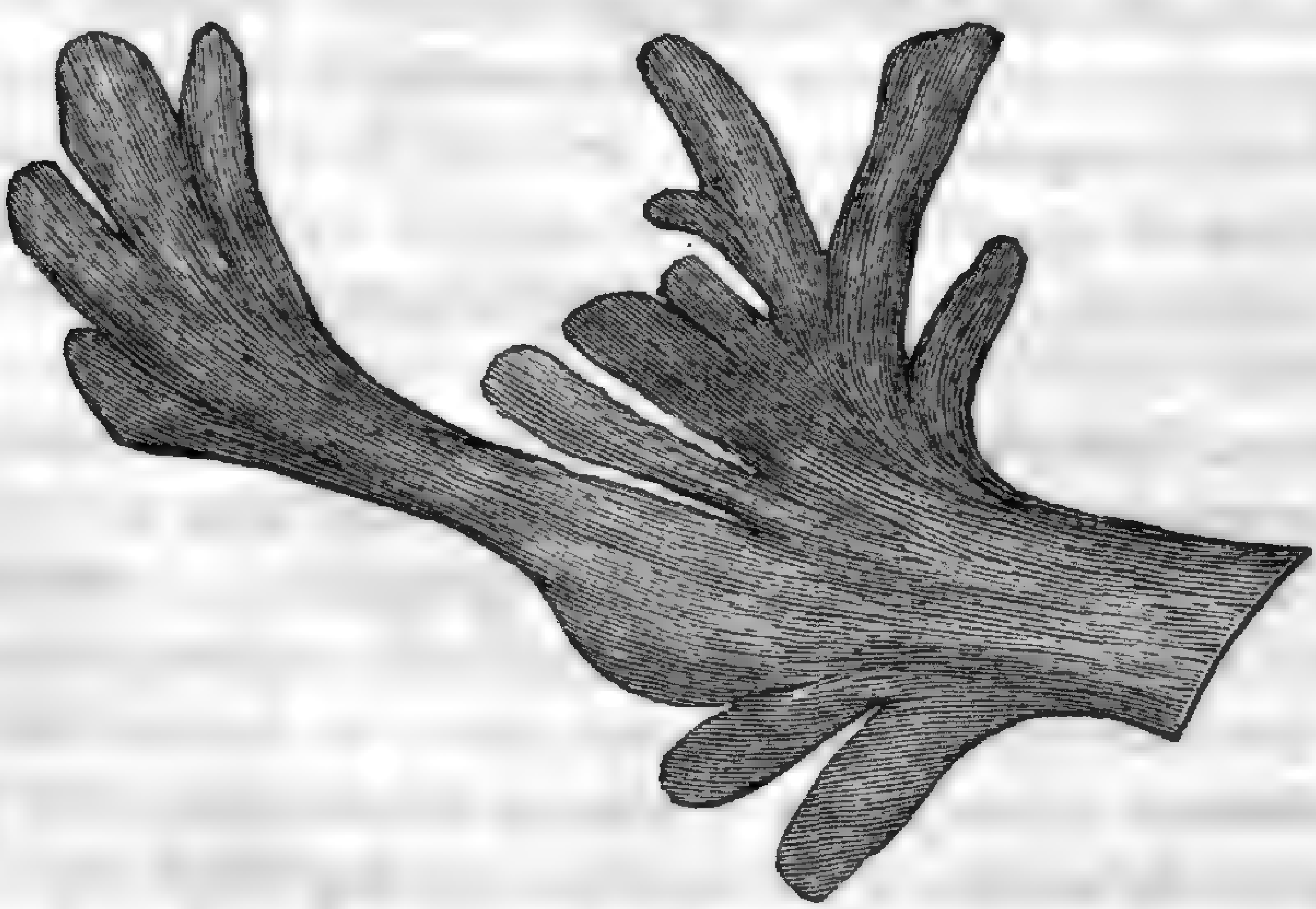
Fossils.—The profusion of exuviæ in our blue limestone, attracts the attention of even common observers, who see in their unusual forms, fantastic and fanciful resemblances. But under the eye of science, their honey comb, twigs and frogs, become various forms of coral; their snakes prove to be the univalve shell Orthocera; their parrots' heads are the bivalve called Atrypa; their buffalo horns are converted into Cyathophylla; their terrapin into a water-worn stone; and their buttons are transformed into encrinites.

Vast numbers of the shells of this formation are unbroken; and many retain all the sharpness of outline, and the delicate markings of a recent shell. The hinge and the interior of the shell are often found in a state of fine preservation; these parts being exposed by the separation of the valves, anterior to the process of petrifaction taking place in them. Most of our fossils are therefore easily characterized. Being constantly and very easily detached from the rock by the common agencies of time, bushels of them can be readily collected in the course of a day. So far as I have witnessed, all the fossils of this deposit are calcareous, in which they differ from those of the formation immediately above it.

Persevering research has enabled me to collect upwards of forty different species of organic remains from the rocks in this vicinity. I greatly regret that I cannot furnish a perfect catalogue of these species; my means for gaining paleontological information are very limited; but conceiving that at least a pittance of information may be gleaned from it, I offer you the following incomplete enumeration:—

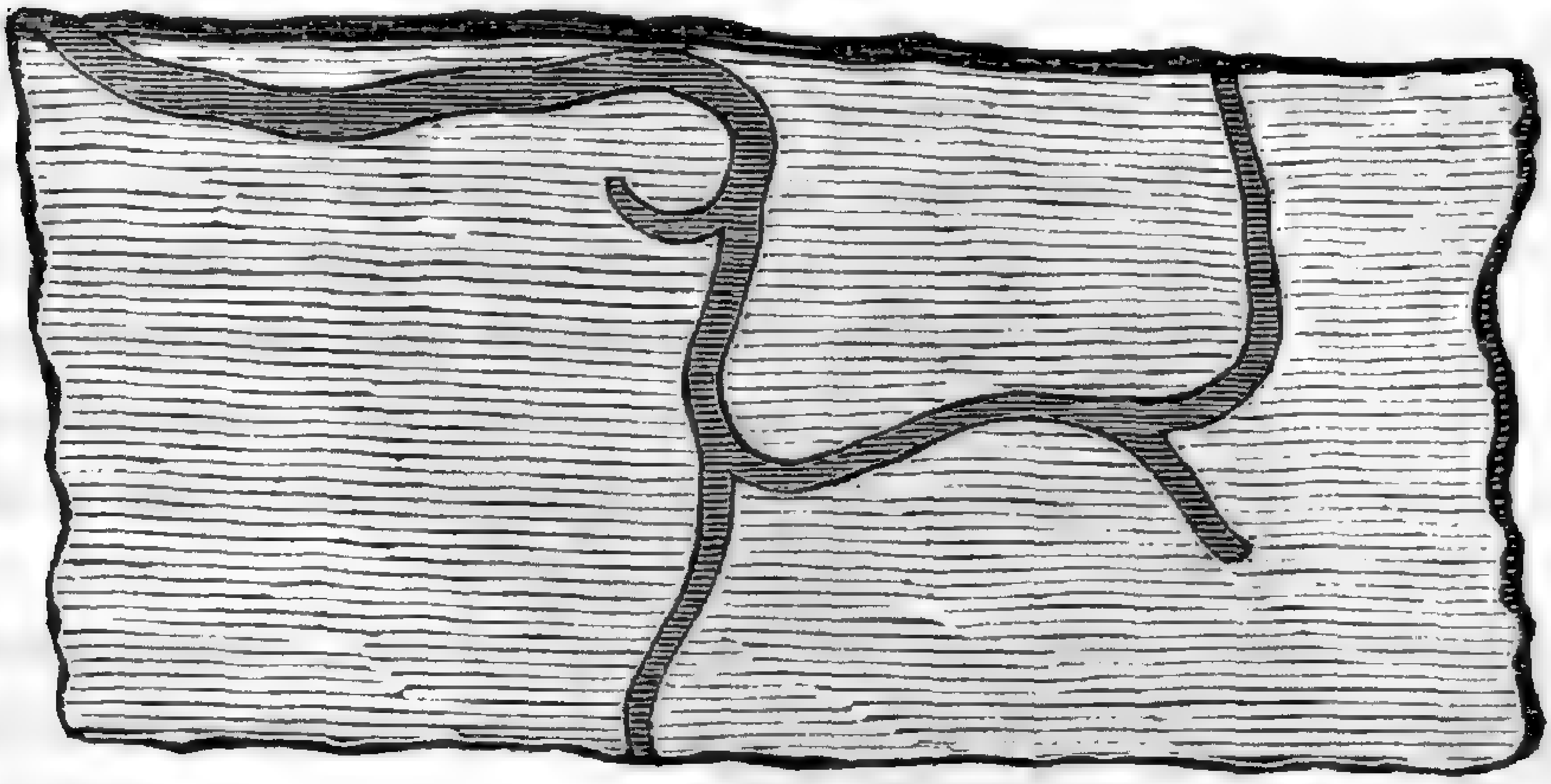
Algæ.—If paleontologists are correct in assigning the *fucoides* to the vegetable world, we have, I think, four species of this sea-weed; and it is a remarkable fact, that the surfaces of the layers presenting them are nearly smooth, more or less laminated,

Fig. 4.



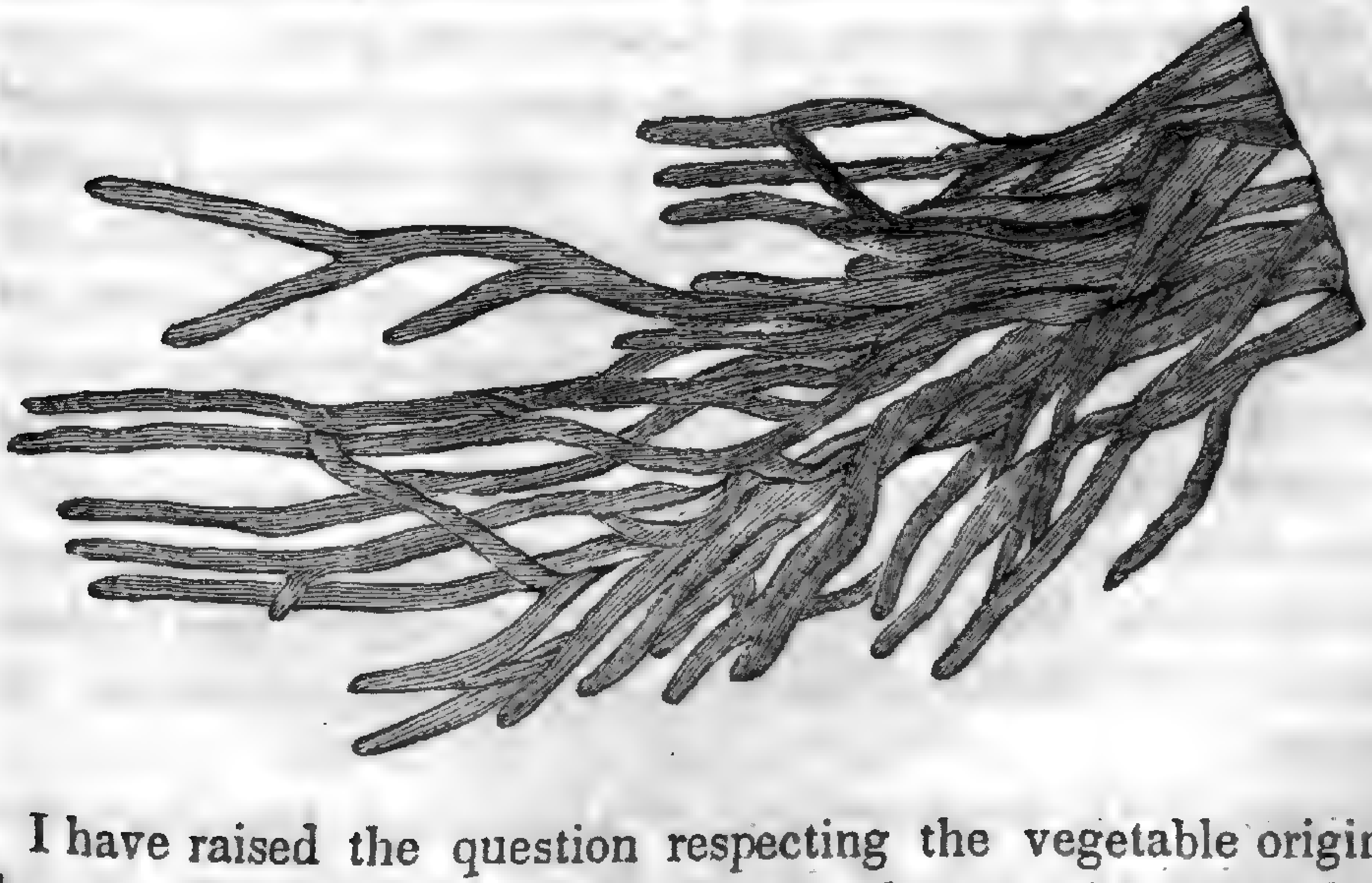
and entirely free from other fossils; while the surfaces of the other strata are generally very much roughened by the partially imbedded shells. These fucoidal surfaces are considerably lighter colored than the interior of the rock over which they are spread. Specimens of this fossil are not readily found, as they are comparatively rare. Fig. 4, represents one of the species,

Fig. 5.



and it is the only specimen I have met with. Fig. 5, is reduced from the original to one-fourth the size. A transverse section of this fossil would exhibit a quadrangular form. Fig. 6, is of the natural size.* The fucoid portrayed in fig. 7, I have always found in pale colored masses, seldom larger than that represented in the cut, and they have almost invariably granular fragments of other fossils adhering to them.

Fig. 6.



I have raised the question respecting the vegetable origin of these fossils, in consequence of having discovered in several specimens of cord-like ridges, like fig. 5, a structure resembling that

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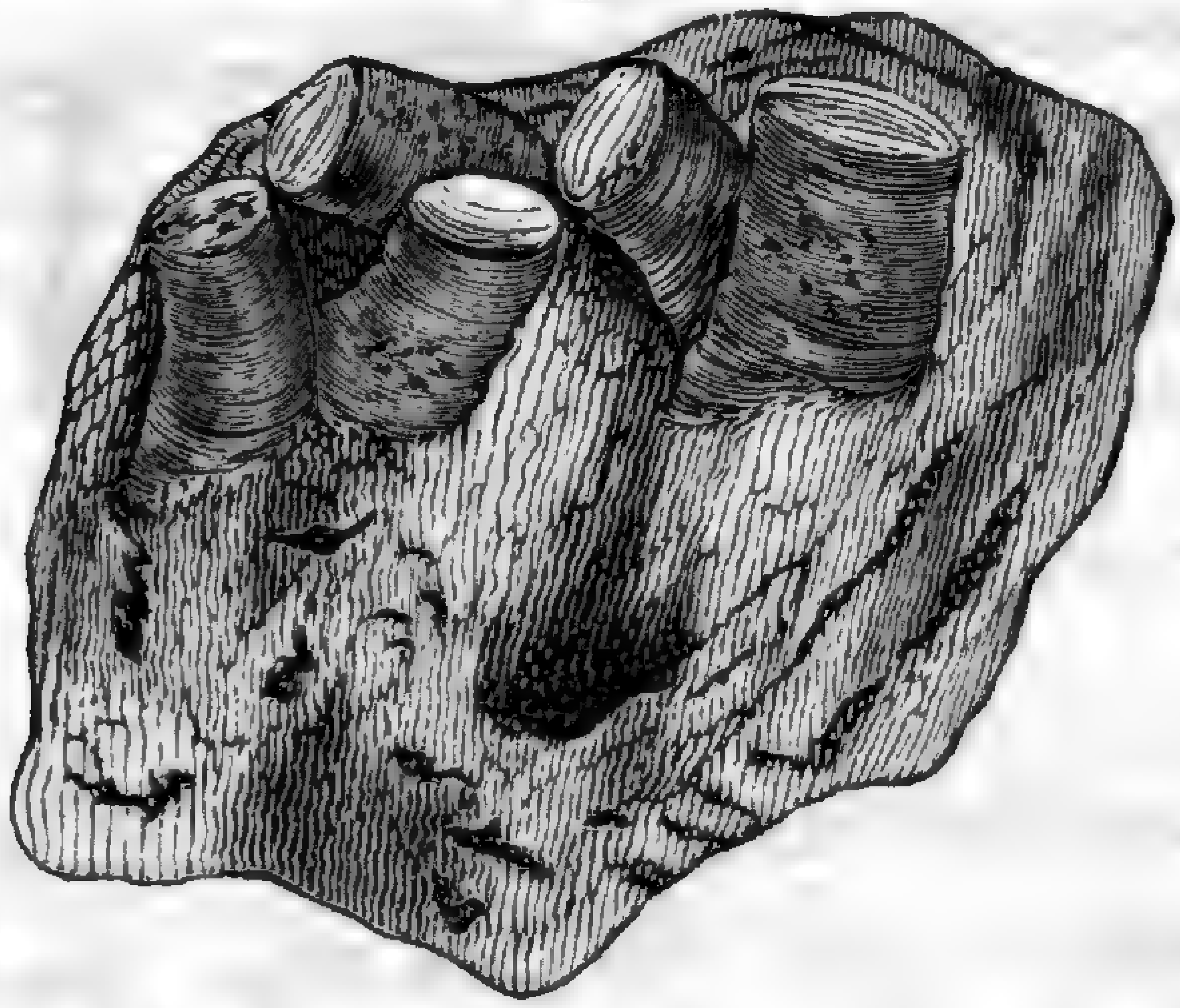
* Figs. 5 and 6 are probably the same fossil; fig. 6 being a group of simple members, each like fig. 5 having the same simple parallelisms and bifurcations.—Eds.

of some species of Gorgonia. To the naked eye they have the appearance of ordinary fucoides; but under a glass of considerable magnifying power, the structure mentioned is very obvious.

Fig. 7.



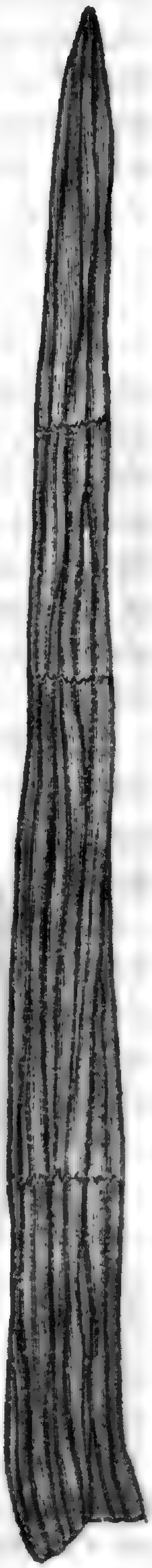
I know that in perhaps most instances no organized structure is visible; but it is possible for that to be obliterated, while the general form is preserved, and I have therefore made the above statement to excite others to further microscopic examinations of these fossils.

Of the *Radiaria*, we have, so far as I have seen, but a few examples; here and there fragments of small *encrinites* are found, and still more rarely, equally small *pentacrinal* joints.

Among the *zoophytes*, I find two species of *Cyathophylla*, of which the *C. ceratites* is much the most common; *Favosites Cliffordi*, of which I have discovered but a single specimen; frequently specimens of *Eschara*, and a few species of *Astrea*, of *Calamopora*, and of *Cosinopora*. Most of these fossils are exceedingly common, and when broken in the rock, easily arrest the attention by their white interior. A step-stone at my door, with a surface of six square feet, exposes sections of eighty one Cyathophyllæ. The *poras* mentioned, are strewed every where along our river banks. The Eschara is perhaps confined to the "marlite."

Bivalves.—Of these, we have two species of *Delthyris;* the *Strophomena alternata*, *S. semiovalis*, *S. deltoidea*, and I think three other species; the *Orthis testudinaria*, and two other species; a small *Pentamera;* the *Atrypa Wilsoni*, and one other species; several species of *Pterinea;* two species of *Avicula;* a very rare species, which, as far as the shell is exposed, has the similitude of a small Unio;* and another, by no means common, having inequilateral valves marked with delicate concentric lines. Except these anonymous ones, and the *Strophomena semiovalis*, *S. deltoidea*, and *Orthis testudinaria*, these shells are very common. The *Pterinea* is chiefly found in the "marlite;" and indeed, this bed is most prolific of the genera. On breaking open these bivalves we often find them filled with calcareous spar; even the thin Strophomenæ are generally compacted in the same manner.

Fig. 8.



Univalves.—One species of *Turbo*, (Monodonta?) and this is not common; one species of *Turritella*, which is still less frequently found; a single specimen of a large *Conularia quadrisulcata;* five species of *Orthoceratites;* the *Platyceras ventricosum* (Conrad;) a *Bellerophon*, and a fossil which I venture to place here, and call for present convenience, *Aulacera.* Of these, the orthoceratites is much the most common. The largest species I have always found in the "marlite;" one specimen, which as usual is not perfect, is upwards of two feet long, and another eighteen inches long; some have a diameter of but one fourth of an inch, while the larger ones, which instead of being cylindrical are elliptical, have a long diameter of five inches and a transverse diameter of three inches. The siphuncle of these large specimens is lateral, and varies from two to two and a half inches in diameter.

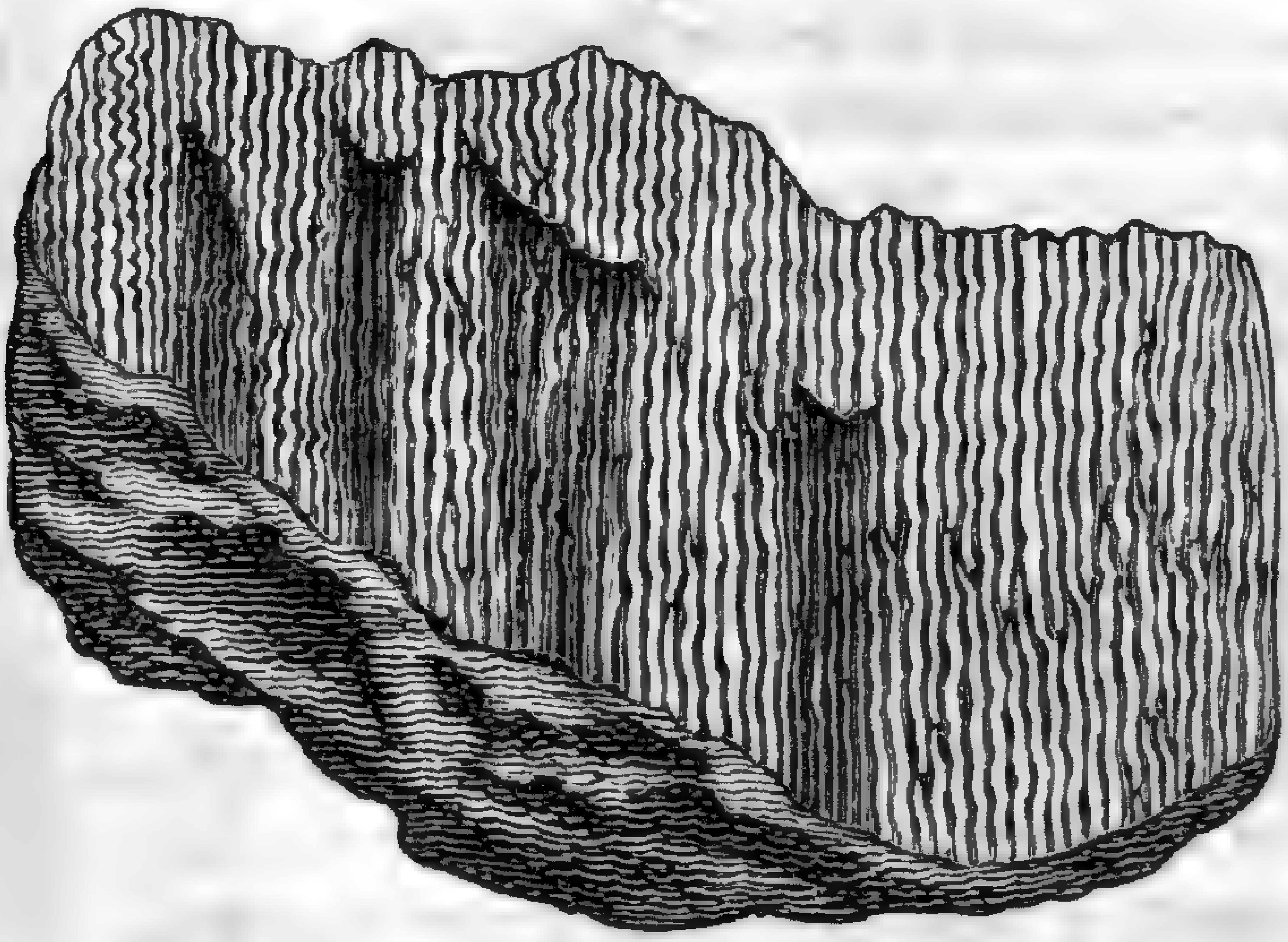
Insulated as I am with respect to the seats of science, and inaccessible to me as are the appropriate authorities, I do not know whether the fossil I am about to describe is already familiar to the paleontologist, or whether it is altogether unique; but as none of my scientific friends, who on visiting this place, have seen it, and none who have received casts of it appear to have been acquainted

* I say Unio by way of comparison; I do not suppose it to be a fresh-water shell.

with it, it is probably not common, and perhaps is new. The *Aulacera*, as I have provisionally named this fossil, is a long, somewhat cylindrical body, coarsely and unequally fluted throughout its whole length, and having a tubular cavity passing through it longitudinally. The specimen in my possession, though evidently imperfect, is about three feet in length, and tapers gently to one end, which is almost entire. The larger extremity presents a fractured surface of an oval form, two and three quarter inches by two inches in diameter. Being broken into several pieces, the fossil exhibits the large siphunculoid cavity, either filled or lined with calcareous spar, and a light colored interior, while the whole surface of the specimen is of a darkish brown and resembles an extremely thin cuticle. I have repeatedly, but in vain, sought for a multilocular structure in this probable nondescript; not the slightest indication can be detected of any kind of structure, besides what I have already mentioned, unless I may add two sharp lines which run along opposite sides of the fossil, as if they were the sutures of a long pod. The sketch on the preceding page will give you a tolerable idea of its appearance. It was found in the "marlite."

Crustaceans.—I have often found fragments of trilobites in this formation, and they have generally been portions of gigantic

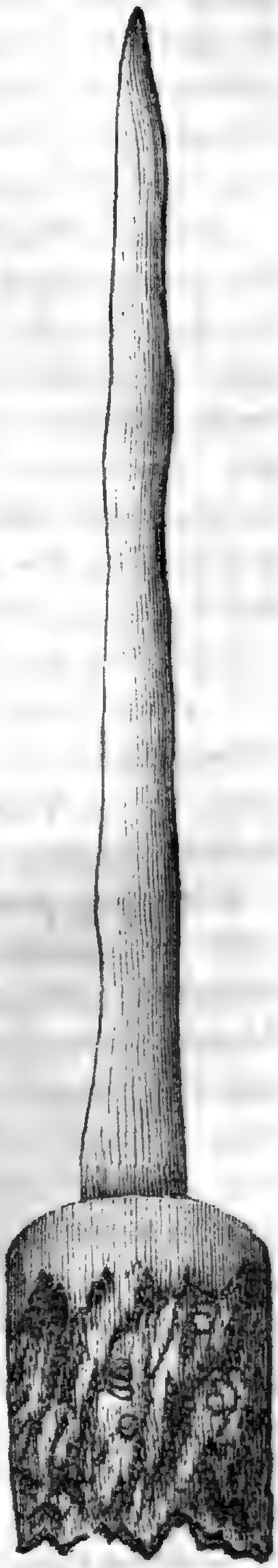
Fig. 9.



species. Among the largest pieces is one which belonged to an *Isotelus megistos*, nearly equal in size, it is believed, to Dr. Locke's mammoth specimen. (See this Journ., Vol. XLII, p. 366.)

I have never met with the "thorn-like appendages" which characterize this species, and yet these appear to be most common at Cincinnati. This species, with the *Isotelus gigas* and the *Asaphus caudatus*, constitutes all the crustaceans I have been able to recognize. Other fragments in my cabinet may possibly belong to different genera from these just mentioned; but they are probably too small to give satisfactory indications of their place in the family of trilobites.

Fig. 10.



I have several other fossils which I have not attempted to arrange in the foregoing catalogue, and two of these I will sketch in this place. Fig. 9, is a congeries of somewhat quadrangular fibres, slightly corrugated, as if they had been torn from a broad and flat muscle and had contracted, and were then laid upon the surface of the rock. The drawing is nearly as large as the specimen before me. No traces of organism can be detected in the fossil. Is it a fucoides?

The other fossil I have considered the siphuncle of a large orthoceratite, with the form of a few of the chambers retained in the rock; but as others who have seen it look upon it as a novel specimen, and as a noted eastern geologist, after much hesitation, supposed it might be a huge encrinite, I have thought best to exhibit a drawing of it.

This fossil is more than two feet long, and is about five inches wide at the larger end. The slender portion of it and part of the broader extremity are smooth; but the remainder is as rough and full of shells as any other piece of rock. A transverse section of the elongated part would present a lenticular figure.

That other fossils than those enumerated may yet be brought to light in this neighborhood, is probable; that the foregoing list does not contain all that are found in other localities of the blue limestone in the west, is certain. Perhaps we may safely look upon our strata as constituting the summit of this formation; this would lead us to anticipate some difference in its fossil contents from the lower strata, which appear at Cincinnati and other places to the south of us. Hence, as we go in that direction, we find an increase of crinoideans; other species, and indeed genera of trilobites, as *Cryptolithus tesselatus;* one or more species of *Apocrinites;* much larger bivalves of the same genera as ours, with other similar variations.

Diluvial Scratches.—A few months ago, while collecting specimens in natural history two miles north of the town, my attention was arrested by the flat, smooth, and whitened surface of some quarried rocks which had been thrown together in a heap. On close examination, I satisfied myself that art had not wrought this appearance, but that the rocks had been polished and marked with parallel lines by some great natural force. Deeply interested in the discovery, I looked around to find some of the rock in place; but I soon ascertained that the grooved stratum could not be seen without the aid of spade and mattock. The stream on which it occurs, and the locality, may be seen by reference to fig. 1. The quarry not having been used for some time, the oozy bank above it had slidden down so as to cover the stratum to a considerable depth.

As soon as possible after the discovery, I returned to the spot with the necessary implements and a person to assist me. By diligent digging and shoveling, we exposed the uppermost and desired surface in a number of places, to a sufficient extent to ascertain accurately the character of the grooves and their direction. The grated stratum was covered by an exceedingly dense clay, which required the strongest efforts with the mattock to penetrate and detach it from the rock. With care we separated several large flakes of the clay, and on examining the under surface, we found it beautifully impressed by the rectilinear engravings of the stone on which it had rested. Above this indurated clay, lie gravel, sand, clay, and soil, to the depth of fifteen feet. The stream runs by the quarry, perhaps thirty feet below the level of it.

On taking the direction of the lines with a compass, we found them to run S. 20° W. In several places water trickles over the upper stratum of rock, and *appears* to have erased the lithographic furrows, for the surface is undulating, and the depressions are as smooth as the elevations: whether these portions of the rock were ever scratched, I will not pretend to say; but they now have every appearance of water-worn rocks. The continuation of the same layer a few yards distant from these places, exhibits the veritable grooves, varying from a mere scratch to furrows an inch or more wide, and with one or two exceptions, running exactly parallel with each other. The average depth of these grooves is perhaps an eighth of an inch, and their breadth and shallowness give to the surface of the rock a *vittated* appearance. This surface in every instance is *whitened* by the powder of the scraped rock, and the powder being impalpable, renders the surface smooth to the touch, like that of soapstone. All these effects denote clearly the nature of the cause, which has left to us this hoary memorial of its action.

It is observable that the direction of these markings does not accord with the general course of such as are found elsewhere in the United States. The following references (all I can at present command) exhibit a very striking deviation from the course of the grooves, reported in the several instances cited.

1. Primitive rocks of Maine, (Jackson,) N. 15° to 20° W.
2. Greenstone, New Jersey, (Rogers,) N. 45° W.
3. Trenton limestone, N. Y., (Emmons,) N. 25° to 48° W.
4. Ripple marks, N. Y., indicate a force from N. 30° W.
5. Cliff rock, near Dayton, O., (Locke,) N. 26° W.
6. Blue limestone, Richmond, Ind., (Plummer,) N. 20° *East.*

In the above table, I have designated the direction of the markings in accordance with the supposition, that the impulse causing the motion was from the north. But however strong the evidence may be that *bowlders*, &c. have been swept southwardly, I do not consider it conclusive that the same force abraded the rocks. On the contrary, may I presume to say, indications appear to contradict the supposition. And I will risk my claims to sagacity still farther, by stating that neither the iceberg theory, the glacial theory, nor any other that I have studied, appears to be sufficient to account for the phenomenon in question; and that it has for a long time been an interesting question with me, *whether, in the upheavings and tiltings of the various formations, the strata have not, in the places of "diluvial" scratches,* *slidden upon each other so as to have left these striæ.* I anticipate a number of grave objections to this conjecture; but perhaps a re-examination of all the premises, will diminish the incredibility which may appear to be attached to it, and ultimately lead to the conviction, that the evidences in its favor outweigh those which are opposed to it. At present, I shall leave the suggestion without support, reserving such arguments as seem to me to favor it for a more suitable occasion, if such should be given.

So far as I have learned, the grooves found at this place are the first that have been descried in the blue limestone of the west. And I may here remark, that while removing the superincumbent mass from this rock, I exposed a large pebble which was flattened and scratched on one side, as if it had been drawn with great weight over some hard surface.

Leaving the blue fossiliferous limestone, which has so long detained us, we will pass to the superimposed formation, well known at the west, as

The cliff limestone.—By turning to fig. 1, it will be seen that this rock makes its appearance at four different places in this vicinity, the most distant locality marked being six miles from this town. In two of these places the cliff rock is seen resting upon the blue limestone, the line of demarkation being very obvious.

This formation consists in general of a much lighter colored rock than the subjacent deposit; being a pale gray, or yellowish or dull white. It is of very variable texture and hardness; sometimes it is much preferred for macadamizing to the blue limestone; in other localities it is wholly unfit for this purpose. I have had it pointed out to me in one or two places in Ohio as pure sandstone; sixteen miles from this town, as in other localities, it is found free from fossils and compact as marble; and again, we find it almost an aggregation of shells. I have seen it when broken often as resplendent as mica; and I have seen it granular and so darkly stained as to take away all lustre. Its stratification is no less various than its physical properties. Sometimes it is in thin and broad layers, and furnishes excellent flagstones; sometimes it occurs in coarse lenticular masses, then in large, thick blocks; and again, as our most valuable building stone. This last form of it is frequently slightly impaired in quality by the presence of small cubes of sulphuret of iron, which, decomposing, tarnish the surface of the stone. I have also several times discovered in this building stone, as it is emphatically called, little cavities filled with a black, waxy matter, which I suppose to be bitumen.

In this immediate neighborhood, this rock presents itself in thin, lenticular layers. It furnishes our best lime, and is preferred for "metaling"* the national road, but is rarely used in building. The well known fitness of this rock to form water-falls, is prettily exhibited here in several instances. These cascades have, from their resemblance in miniature to the Niagara Falls, been called by Dr. Locke *niagarets*. To my eye, the similarity is quite striking. At one of these water-falls, I have found portions of this limestone with dark brown stains of bitumen, the odor of that substance being very strong.

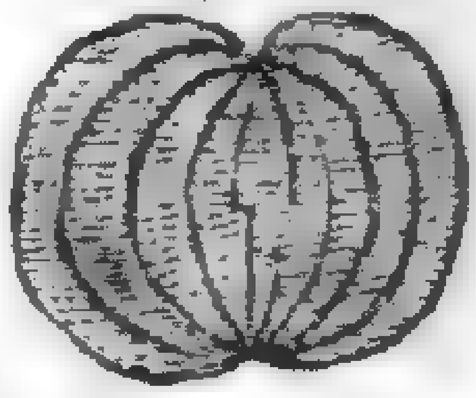
Fossils.—In this formation fossils are seldom obtained entire; they are mostly calcareous, but sometimes siliceous. Imperfect as they are, I have been able to make out the following genera: among the *zoophytes*, a species of *Gorgonia;* two species of *Retepora;* a small, invariably black *Cyathophyllum*, and a larger, always light-colored species; *Tubipora catenulata; Catenipora labyrinthica;* an *Eschara* different from that of the blue limestone; *Calamopora Gothlandica*, and another species.

Of *bivalves*, I have discovered the *Atrypa affinis*, *Strophomena rugosa*, and three other species, two of which resemble *S. deltoidea* and *S. alternata; Orthis testudinaria*, and apparently two other species; and in one locality the rock is almost exclusively composed of large and fine *Pentamera*, some of them being several inches in diameter.

Crinoideans are much more common in this formation than in the rocks below; and of the *crustaceans*, we have the *Calymene senaria.*

These are not all the fossils afforded by our cliff rock, but they are, I think, the principal kinds. Among the unnoticed species is a smooth, hemispherical and concave body, half an inch in diameter, with a folded margin. Another fossil, perhaps, deserves a figure: the appearance of this film on the rock is well calculated to give the observer an impression of its being a winged seed. I have al-

Fig. 11.

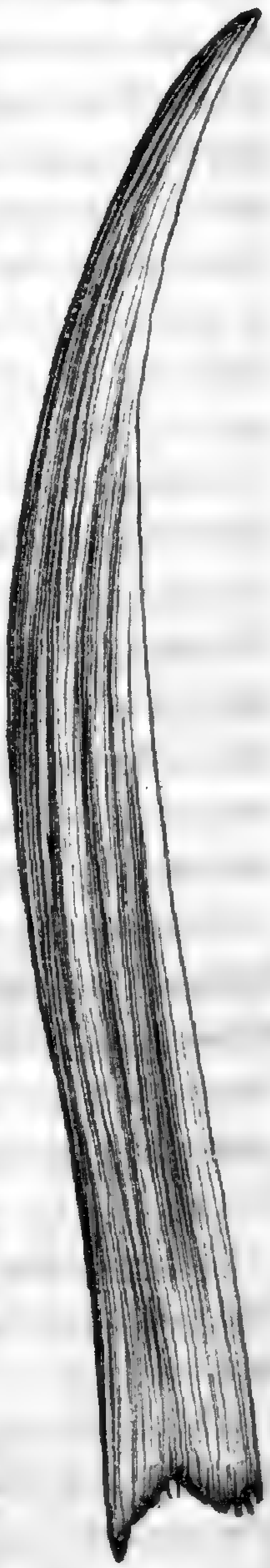


* The term "metal" is in general use among engineers to designate the fragments of stone used as the foundation or superstructure of roads.—Eds.

ways found this *samaroid* of the size represented in the preceding figure.*

The only siliceous fossil of this formation in my cabinet, is the *Tubipora catenulata.* The greatest thickness of the cliff limestone in this vicinity does not exceed thirty feet.

Fig. 12.



Diluvium.—Upon the last mentioned formation repose alternating layers of yellowish clay, sand, and gravel, covered by a rich soil, and underlaid very generally by a bluish marly clay of a sulphurous or some similar offensive smell. I have already stated in the early part of this article, that the diluvium varies greatly in depth; and I may add here that the thickness of the clay immediately below the soil, as far as numerous excavations for wells and cellars expose it, is about six feet, so that the soil has a substantial substratum.

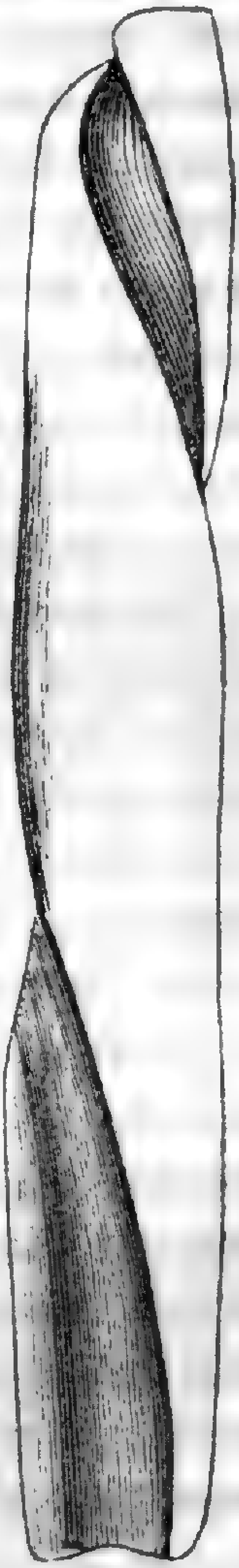
Remains in the diluvium.—While excavating a passage for a road through a hill, the workmen exhumed two *horns* from the gravel, a number of feet below the soil and clay. From the curvature of these horns, their similarity in size, and their being found together, a reasonable inference arises that they were a pair belonging to the same animal. They are so nearly decomposed that they scarcely bear the most delicate handling. They are ten inches long, an inch and a half in diameter at the base, solid, gently tapering towards the summit, and slightly furrowed longitudinally. See figure 12.

In a similar situation was discovered a piece of *ivory*, six inches long and three fourths of an inch in diameter. A hole passes through its entire length, and a spiral groove performs just one revolution round it, in running from one end of the ivory to the other, giv-

* *Samaroid.*—I suppose the rigid critic will be offended at this mongrel word, (*samera* and *eidos.*) But many authorized English words have been formed by a combination of the Latin and Greek.

ing to it a twisted appearance, as represented in the following figure.

Fig. 13.



The fistular passage is evidently the work of art; the spiral channel is as unquestionably the work of nature. The tube appears to have been formed by boring at the opposite ends of the ivory with a slender and sharp fragment of flint, fitted to a handle; for the interior shows that the operator was at first unsuccessful in bringing about a union of the opposite borings, and a piece of flint was found broken off at the bottom of the tube, where it had passed by the bore from the other extremity: this *cecum* was afterwards plugged with a piece of bone, and the calibre, differently directed, was made to inosculate with the contrary boring.

The surface of this ivory article has been smoothed, probably by artificial means, yet quite possibly by accidental attrition. Small light brown spots on several parts appear to be remains of the original surface; and in the volute, patches consisting of several exceedingly thin films laid upon each other, as if they had served as enamel, are very apparent; while myriads of striæ scarcely perceptible to the naked eye, wind spirally around, crossing the volute at right angles, dipping down into it and passing under the filmy crusts, and reappearing on the opposite side. The interior of this bone is of a pure white color, exhibiting in a favorable light concentric and rather elliptical curvatures, possessing a beautiful pearly iridescence. At the ends lines are seen running parallel with the transverse curvature of the volute, and crossed by radiating or parallel lines imperceptible to the unassisted eye; it is heavy and exceedingly hard, like the enamel of a tooth.

In this connection I shall take the liberty of going considerably beyond the limits which I have prescribed to myself in the foregoing sections, for the purpose of embracing two other osseous fossils of less dubious age—a *tusk* and a *tooth.* Both of these were noticed in this Journal, (Vol. XL, p. 149, 1841,) in a very brief manner, amounting to little more than a mere announcement of their discovery, and I do not know that much need be added to that information.

The tusk was exhumed in gravel, fifteen feet below the surface, while excavating the Whitewater canal, near Brookville, about thirty miles south of Richmond. It has a chalky whiteness, is very brittle, solid throughout its entire length, which, when the specimen was disinterred, was nearly six feet; and exhibits at the ends about a dozen concentric laminæ of variable thickness. The characteristic zigzag lines are finely displayed at the fractured extremities. The diameter of the tusk is four inches, and the curvature of it is nearly the segment of a circle, whose radius is two and a half feet.

The grinder was found near Jacksonsburgh, twenty miles west of Richmond; and though several considerable pieces of it are lost, it weighs four pounds. The interior is spotted here and there with a clear cerulean stain. This mastodon tooth is so well preserved, that before it came into my possession a dentist attempted to make artificial human teeth of the whitest portions of it. It has four pairs of prominences.*

A *club-shaped instrument*, formed apparently of cliff limestone, was also taken out of the gravel ten feet below the surface, near the spot where the tusk was found. It is nearly seventeen inches long, rounded at one end and tapering towards the other extremity. It would make a massive weapon, and indeed it has been colloquially called a war-club. It may have been an Indian hommony pestle; and if so, its interment to the depth of ten feet is somewhat mysterious, and constitutes the most interesting feature of the discovery.

Ancient Wood and Stumps of Trees.—I have long contemplated with interest that bluish, calcareous clay, noticed at the beginning of this section, (diluvium,) as constituting one of the lower members of our diluvium. By personal inspection I have ascertained that it extends over a large portion of the western country; I have met with it repeatedly in Ohio, in Indiana, and in Illinois, recognizing it by its physical composition, its color and smell. The interest attached to it arises from its being every where the repository of relics of vegetation. At Springboro',

* I will mention here, merely to designate another locality, that another fossil grinder in my cabinet is said to have been found in Trumbull County, Ohio. It has ten prominences.

forty four miles east of Richmond, wood has frequently been dug out of this stratum in excavating wells. In one instance, a stump, I think of pine, was found in an erect posture; and a piece of wood, now in my possession, obtained there at the depth of fourteen feet, is evidently *pine* or some closely allied species of Conifera; even the terebinthinate taste is still perceptible in it. This piece of fossil wood has greatly shrunk and split, separating at the junction of the annual layers and along the fibres into quadrangular strips. The surface is black, as if it had been covered with ink. For a long time after it was taken from the earth, it retained the characteristic sulphurous odor of this stratum. No pine grows near Springboro'.

About the same distance west of Richmond, in Madison County, twigs and other pieces of wood have also been found at the depth of twenty seven feet. The fragments I have obtained from that locality resemble *elm*, but I cannot be certain that that is the actual species of timber.

In excavating a well in Richmond, several sticks, and a *chip* having palpable marks of an edged tool upon it, were disinterred nearly thirty feet below the surface. The sticks I saw a number of times in the house in which they were preserved, but I do not know to what wood they belonged; and the species to which the chip belongs I have not yet (and it has been examined repeatedly in my cabinet for a number of years) been able to determine; I can only say, that on cutting it the texture is found to resemble that of sycamore (*Platanus Occidentalis*) more closely than of any other wood with which I have been induced to compare it.*

Bowlders.—I have taken much pains to examine the mineral character of these transported masses, as I have found them strewed over the neighboring country, or thrown out from various excavations. My original intention was to make a thorough examination in this vicinity, and report the result of my investigations in an article exclusively devoted to the subject; but as these bodies very properly claim attention in the present essay, I shall curtail my notes respecting them so as to admit a brief report in this place.

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* See a notice from Prof. Carpenter in this Journal, Vol. xxxvi, p. 118, giving an account of buried wood bearing axe-marks.—Eds.

It has appeared to me that these travelled specimens have attracted too little notice; a minute inquiry into their composition, instituted all over the country, would probably be repaid by several interesting developments of a nature the geologist can easily foresee. Even a careful collection of the reports already made on this subject would be useful.

The largest bowlder I have found in my limited district, would weigh by calculation about forty three tons. In general these rocks are much smaller, and altogether they are not very numerous. In some situations, hundreds of them of movable size are thrown together, while usually, they are not sufficiently large and common to excite attention. As an approximation to the number of bowlders may be of some service, in comparing this district with others, I will state that after a careful survey of an area of one hundred and forty four miles, forming a square around Richmond, I suppose that if the bowlders two feet and upwards in diameter were equally distributed over this surface, they would not vary much from three hundred and twenty to each square mile; which would give to the whole area specified, forty six thousand and eighty bowlders of the dimensions above stated.

The large bowlder mentioned, rests, with many smaller ones and another nearly its equal, in a peat-field in the northern suburbs of Richmond. All of them are granitic rocks, and they probably repose upon the wet gravel and clay which lie beneath the peat; the peat is about three feet deep.

I proceed to enumerate very briefly the chief varieties which have found their way into this section of country, embracing all kinds of transported fragments, regardless of their bulk. I have collected and labelled upwards of seventy easily distinguishable specimens, but in the present enumeration I shall be more general. Perhaps the most convenient division for my purpose, will be into siliceous and non-calcareous, and calcareous kinds, and I will therefore arrange the specimens under these two heads.

Siliceous and Non-Calcareous.

1. Almost every variety of granite, gneiss, sienite, and of greenstone. These constitute the larger masses, and the greater proportion of the drift; and they sometimes contain garnets, magnetic oxide of iron, and other minerals. Mica slate is seldom found.

2. Milky quartz.
3. Sandstone.
4. Hornstone.
5. Jaspery iron ore.
6. White siliceous pieces, with *encrini*, &c.
7. Colored siliceous pieces, with small *Pentamera*.
8. Amygdaloid. Base green; amygdalæ black.
9. Porphyry. White feldspar in green epidote, with groups of large hexahedral prisms of epidote on the surface.
10. Cellular quartz. Some specimens with veins of blue quartz, and splendid minute crystals of the same, lining fissures in the bowlders; other specimens have a well marked oolitic structure.
11. Striped jasper.
12. Siliceous masses of *Favosites*, *Tubulariæ*, and *madrepores*.
13. Dark brown sandstone, with branched *corals* and small *aviculæ*.

These cannot be considered common: some of them are rare.

14. Large blocks of pale red and other colored quartz are not of unusual occurrence.
15. Hornblende rocks.
16. Several varieties of puddingstone.
17. Masses of beautiful breccia, having a whitish base set with hornstone, and bright red and other colored jasper. Rare.
18. Siliceous slate. Not common.
19. Clay slate. Not common.
20. Common jasper. Frequently found.

Calcareous.

1. Fetid carbonate of lime. Some pieces with fine large crystals of white foliated *sulphate of strontian;* other blocks contain numerous small bivalves, closely resembling *Cytherinæ*. Of frequent occurrence.
2. Calcareous tufa, from deep excavations in the diluvium. Common.
3. Compact brown limestone with quartzose concretions, and a small *trilobite*, *encrinites*, *Favosites*, a *Delthyris? Cyathophylla*, and other madrepores. Rare.
5. A brown bowlder highly charged with *iron*, and divided in every direction by thin plates of *sulphate of barytes*. Rare.

6. Siliceous limestone. Not very uncommon.

7. Silicicalce, (Cleaveland.) Rare.

8. Saccharoid block, with sulphur yellow stains and *encrinites*, and large, somewhat *stellar impressions*.

Where are the localities that furnished all this variety of bowlders? Were the masses transported to this place by direct, deflected, or converging currents? How do the bowlders remotely east, west, and south of Richmond, differ from those in the above catalogue? I hope that increased or continued attention to these denizen rocks, will enable us ere long to answer these questions satisfactorily. A strict examination in numerous districts, would guide the inquirer into this subject with a great degree of precision. If the salient point of the rock containing Cytherinæ is ascertained, and fragments are found strewed in diminishing quantities and lessening sizes from that point to Wayne County, so that the last bowlder falls at Richmond, what forbids, with the aid of collateral evidence, a close approximation to the nature of the force producing the distribution, the direction, height and width of its action, its violence, and its velocity? In some districts, serpentine is found out of place; it will be perceived by the foregoing enumeration, that none has been found here: where shall the limits of its dispersion be drawn? The same question may be asked concerning every other bowlder; and when correct answers shall have been obtained, a highly interesting and comprehensive map may be constructed, from which the geologist may safely derive the elements of a theory.

Water.—Springs and oozing banks are common all over this section of country, and water may be obtained every where at depths varying from two to thirty feet; the average depth of our wells being twelve to sixteen feet. The water is cold and clear, and is acceptable to those who do not prefer soft water. Being highly charged with carbonate of lime, our tea-kettles are constantly encrusted with a calcareous deposit, which requires occasional removal to prevent it from entirely obstructing the spout. Similar incrustations take place in the open air on stones, sticks, and earth, and over whatever the water runs, whenever circumstances are favorable to the deposition of lime. The fissures in our blue limestone are sometimes lined with a tolerably pure alabaster; but the inlets to the water having been closed long since by the calcareous deposition, these crevices are always dry. In one of them I found part of the lateral surface covered with an exceedingly light and perfectly pure white substance, to the depth of about an eighth of an inch. Such was its levity and feeble attachment, that a light breath was sufficient to blow it off the rock; and these characters, united with its whiteness, led me to suppose it was magnesia mechanically separated from the water trickling into the fissure. I conceive that a constant and forcible current of air, throwing the water into spray, would be adequate to this effect. A process, not very different from this, appears to separate the lime from the magnesia in our tea-kettles, causing more of the latter earth to lodge on the higher parts of the vessels: it is in both cases a species of winnowing, the magnesia representing the chaff, and the lime the grain. The substance in question may have been carbonate of lime, (fossil farina,) with no greater proportion of magnesia than the water holds in solution; but I am inclined to the other opinion; and I regret that while the fragment of rock covered with the powder was in my possession, the only conclusive evidence of its composition, chemical tests, were not procured.

North of the town, near the grooved rocks described in the foregoing pages, many tons of *calcareous tufa* have been formed, involving leaves, stems, twigs, moss, and shells. Some of the impressions of the leaves are beautifully preserved, and are very easily recognized as perfect images of the foliage of beech and sugar-tree.

In the subterranean vault of a brewer, stalactites are constantly produced; and barrel-heads, staves, and other things, left for some time upon the floor, or standing in unemployed corners of the vault, become coated with carbonate of lime. The stalactites are generally tubular and about the size of a large quill: they are removed before they become solid.

I have suggested above, that the water of this district contains *magnesia*. By careful evaporation, I find that a gallon of water from my well contains thirty-two grains and forty-seven hundredths of earthy matter, which on analysis proved to be,

Carbonate of lime, - -	32	grs.
Carbonate of magnesia, - -	0.47	"

These are not quite the proportions in which these carbonates exist in our rocks, there being ten hundredths of a grain more magnesian carbonate than the same weight of limestone would furnish. I do not pretend to say, that the water subjected to experiment derives its earthy matters from our blue or our cliff limestone; I merely point out the similarity of composition.

For the purpose of showing the very remarkable difference in the quantity of magnesia obtained by complete evaporation of a given measure of water, from the quantity found in a certain portion of the deposit in a copper tea-kettle, I will compare the analyses of equal portions of the earth thus differently procured.

Having carefully collected, with the bowl of a silver spoon, a large quantity of the most recently deposited matter from the inside of a tea-kettle, as *high up* as the deposit was made, I dried it, and weighing 176 grs. well pulverized, I poured pure water upon it, and was surprised to meet with considerable difficulty in making the liquid wet the powder. On adding hydrochloric acid, effervescence followed, and when the solution was rendered slightly acid, it was left one or two days to depurate, for I discovered insoluble particles floating about in the liquid, and I suspected them to be vegetable matter. On separating this insoluble portion by filtration, and drying it, it weighed one grain and thirty-three hundredths. Subjecting it to the flame from a blowpipe, it resisted the greatest heat I could raise. From a light brown it became a pale red colored substance, perhaps from the accidental presence of iron. It was not soluble in the mineral acids, hot or cold; it was not gritty; with potash it formed a green globule full of air bubbles, but did not appear to be chemically combined with the alkali; the globule was not wholly soluble in water, and I recovered by elutriation nearly the whole of the original powder.

The clear liquid treated with oxalate of ammonia, and afterwards with ammonia and phosphate of soda, yielded by the usual processes, one hundred and sixty six grains and three tenths of carbonate of lime, and eight grains and five tenths of carbonate of magnesia. From these experiments I deduce the following tables of comparison:

Composition of deposit obtained by complete evaporation—100 grs.

Carbonate of lime, - - - -	98.56
" magnesia, - -	1.44
	100.00

Composition of the highest part of the deposit in a tea-kettle—100 grs.

Carbonate of lime, - - -	94.54
" magnesia, - -	4.82
Insoluble matter, - - -	0.75
	100.11
Deduct hygrometric absorption, -	11
	100.00

I suppose this to be very nearly the composition of the earthy matter of most of our well and spring water. But we have likewise numerous springs furnishing a tolerably strong chalybeate water; this I have not yet analyzed. It imparts a yellowish stain to the ground over which it flows, gives the characteristic result with tincture of galls, and has a very sensible chalybeate taste. These were for a long time regarded by the inhabitants as sulphurous waters, and were called "sulphur springs;" they are now sometimes resorted to by invalids from town, and their water drunk as a gentle tonic. In connection with ferruginous waters, I may observe that twelve or fifteen years ago I found bog iron ore of apparently good quality in the vicinity of Richmond, but I suspect the quantity is not very great.

Several years ago, I received a paper containing 186 grs. of a dirty powder, with a note, saying the powder was "obtained by boiling down about three gallons of well water in a brass kettle. The water drank freely, purges." From this powder I procured the following results. If three gallons of water produced the 186 grs., then one gallon of the water contains

Sulphate of magnesia, - -	48 grs.
Carbonate of lime, - - -	7 "
Siliceous crystals, - - -	2 "
Vegetable matter and loss, - -	5 "

The crystals appeared to be pure silica, and under the microscope were found to be acicular in shape and remarkably brilliant. The well which furnished this water, I have been informed, has been filled up by the owner. It lay considerably out of the circle I have chiefly confined myself to; but I thought it worth embracing in the present sketch for preservation.

Besides the three kinds of water already mentioned, we find a fourth variety in a few wells, which at certain seasons only, contain an exceedingly offensive fluid, wholly unfit for use. At these seasons the water is not clear, and it does not become limpid by remaining several days at rest. At length, however, it is partially purified by a portion of the matter which produces the turbidness, becoming black and falling to the bottom; specks, and sheets of mould also appear upon the surface. The old methods of ascertaining the presence of sulphuretted hydrogen, detect none of the gas in this nauseous water; but according to the result of repeated applications of Pasquier's iodic test, each gallon of the water contains 2.36 cubic inches of *hydrosulphuric acid.* Tincture of iodine is certainly a very delicate reagent in this case, but I fear it is not unequivocal in its indications. Excepting the properties mentioned, I do not know that this periodically loathsome water differs from our common well water, and I have ventured to entertain the conjecture, that in the few wells under consideration, the water is rendered offensive by the growth and decay of *fungi* in the well. My reasons for this opinion are: 1. The periodical character of this condition of the water, for fungi as well as other plants have their regular seasons of germination. 2. The odor resembles that of very putrid mushrooms. 3. The vegetable matter in the water becomes black, as many of the fungi do. 4. The turbidness of the water appears to the naked eye exactly like water in which some delicate agarics were macerated. 5. The absence of any other ostensible cause of the disagreeable quality of the water. 6. A piece of recently gathered *Boletus badius*, which I happened to have in my office, was cut into shreds and macerated half an hour in pure cold water. To the decanted liquid a few drops of clean starch water were added, and having been properly mixed with the boletic water, tincture of iodine was dropped in, the resulting blue color immediately disappeared, as in the well water similarly tested.

If this should prove to be the cause of the loathsomeness of the water, it may not be difficult to remove it.

Soil.—In Vol. XL, page 198, of this Journal, is given an extract from one of my letters, in which I say, that in soil producing plants [reported by chemists as] containing sulphate of lime, I have not been able to discover any indications of that salt. To this remark the Editors have appended a note, founded on the supposition that the soil examined had been digested with alkaline carbonates. Such however was not the case. And believing, that if the plants really contained a sulphate, it must be derived from the earth in all probability, but *not necessarily*, I have tested various portions of soil from different situations and taken from different depths; but I have not been able to procure the slightest cloudiness with the usual reagents, nor to discover any trace of sulphuric acid in any way.

The soil of this district is generally rich, and being based upon a clay stratum, bids fair to be durable. The analysis given in the volume just cited is as follows:

One hundred grains of soil from woodland—

Soluble geine, - - -	4.50 grs.
Insoluble " - - -	7.00 "
Carbonate of lime, - - -	1.75 "
Phosphate " - - -	.50 "
Aluminous matter, - - -	12.50 "
Siliceous " - - -	74.00 "
	100.25
Gain, - - - -	25
	100.

A specimen of soil taken from a cultivated field yielded a different result. After drying it several weeks in my office by the ordinary summer air, I heated 480 grs. of it for an hour, at a temperature of 400°; the loss was 47 grs. Another 480 grs., after careful trituration, separated as follows:

Sand and vegetable fragments, -	2 grs.
Fine sand, - - - -	12 "
Dust passed through gauze, - -	466 "
	480 "

Four hundred grains of the thoroughly dried earth, when calcined, lost eleven grains. A large proportion of potash was discovered by Mitscherlich's method, and the presence and quantity of the other ingredients were ascertained by the usual processes. After moistening some of the earth and reducing it to a thin paste, I added nitric acid to it; not a single bubble of carbonic acid appeared. Several trials resulted in the same way; the soil therefore contains no carbonate. Crenic and apocrenic acids were made apparent by digestion with carbonate of ammonia, and the use of the cupreous precipitant. And as near as I can arrive at the general result of my experiments, it is as stated below:

100 grs. contain 11.75 grs. of hygrometric water.
100 " " 2.75 " of vegetable matter.

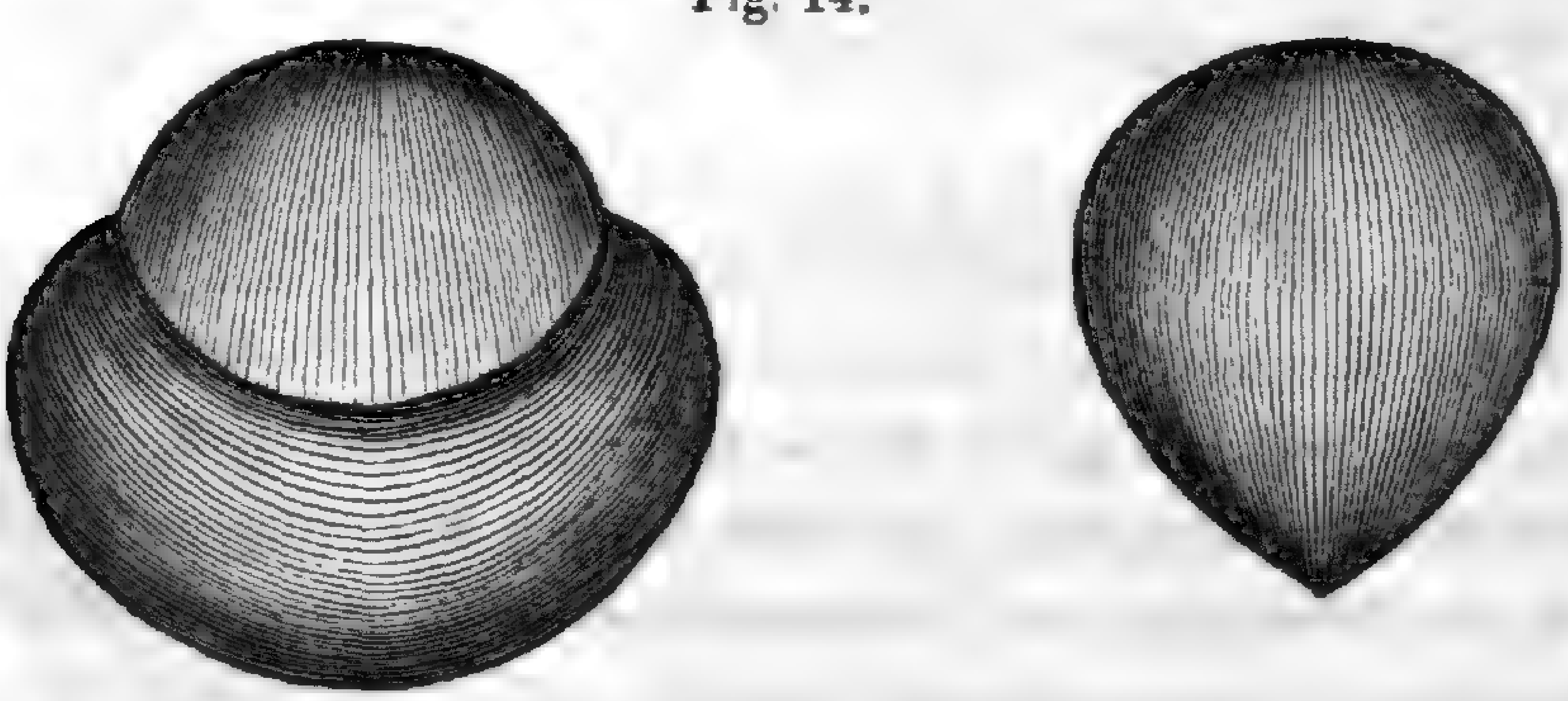
And in 100 grs. of the calcined earth there are of

Silica from solution, - -	86.5
Alumina, - - -	5.8
Peroxide of iron, - -	2.7
Potash, - - - -	4.8
Phosphate of lime, - -	0.6
Magnesia, - -	(slight cloudiness.)
	100.4 – gain, .4 = 100 grs.

The unexpected quantity of potash led me to suppose that ashes had been liberally applied to the field at a recent period, or that a log had been burned on the spot whence the soil was taken; but in addition to the fact, that, in common with lime and gypsum, ashes are scarcely ever spread upon our lands, I found that the field in question had long since been cleared, and having been considered disproportionably rich at the side whence the specimen for analysis was obtained, no fertilizing applications had been made to it.

Peat fields are found on various farms, generally two to four feet deep. All the specimens of the peat which I have tried, yielded red ashes, and this color is said to indicate the best quality of this species of fuel.

Fig. 14.



Nodules of sulphuret of iron (iron pyrites) are often met with on the surface. A few of them assume fantastic forms, while others mimic closely the figure of some organic body. I have in my cabinet a specimen found in a bog, that is well calculated to impress any one with the belief that it is the transubstantiation of an acorn and its cup into iron pyrites. Some time after I procured it, the cup split lengthwise and fell into several pieces, so as to separate the acorn-formed nucleus, and thus expose its whole form. It will be perceived by reference to the preceding figure, that compared with the position of an actual acorn in its cup, the ends appear to be reversed, the more pointed end being concealed in the cup.

Mounds.—It was not until I met with the suggestion in Bancroft's History of the United States, that I was aware that any geologist entertained the opinion, that these tumuli were fortuitous formations. And as the Association of American Geologists has set afloat the question of their natural or artificial origin, I have embraced these embossments of the earth's surface in the present article.

I find on turning to my note-book, that I have made a memorandum of seven mounds existing within the compass of a few miles around Richmond. Most of these tumuli would be termed very small, varying from five to twelve feet in height. As far as they have been excavated they are composed of clay; but I have never discovered any stratification in them. At the base of one which I opened in 1823, I found a human skeleton, with the head exactly in the centre of the mound, fragments of wood coal, the bones of some other animal, and a piece of ivory reduced to great thinness by artificial means. In another mound, deeply interred, was found a considerable quantity of charred Indian corn. The bones could not be taken out whole; they were so decomposed as to crumble to pieces in the hand. The ivory was much less decayed.

I cannot myself believe that these are natural cones, formed by deposition from gyratory waters, or by any accidental cause; but on the contrary, during the examination of numerous mounds in the west, large and small, in various situations, connected and unconnected with other embankments, I have been irresistibly led to acknowledge the correctness of the popular opinion, that they are the work of human hands.

Richmond, Indiana, Oct. 12, 1842.

Art. VII.—*Variation of the Magnetic Needle at Providence, R. I., from A. D.* 1717 *to* 1843; by M. B. Lockwood, Civil Engineer.

The following table, showing the magnetic variation, *nearly*, for the last one hundred and twenty six years, has been constructed from actual observation, from observations on record, and from the recorded bearings of a very great number of permanent objects at various periods.

For example, Richard Jackson, on a map of a part of Providence, states the variation in 1717 at 9° 36′ W. In 1769, at the transit of Venus, it was carefully ascertained by Dr. B. West to be 6° 30′ W. In 1815, Moses Brown, Benoni Lockwood, and George Sheldon, agreed by different methods in making it 6° 30′ W. In 1819, Friends' Boarding School edifice was erected, fronting the magnetic south; from which, the variation at that time is ascertained to have been 6° 37′ W. Since 1835, the variation has been obtained by careful observation.

It is not to be supposed that the accompanying table is correct to a minute, so imperfect were the instruments used during most of this period; yet from a great diversity of trials, where the bearings recorded are known to have been made by different individuals, the error can in no case be large.

Years.	Magnetic variation.	Years.	Magnetic variation.
1717	9° 36′ w.	1790	6° 10′ w.
1720	9 28	1795	6 10
1725	9 14	1800	6 15
1730	8 54	1805	6 19
1735	8 39	1810	6 24
1740	8 15	1815	6 30
1745	7 59	1819	6 37
1750	7 40	1825	6 51
1755	7 21	1830	7 10
1760	6 57	1835	7 34
1765	6 43	1840	8 25
1769	6 30	1841	8 31
1775	6 20	1842	8 39
1780	6 16	1843	8 46
1785	6 13		

Providence, January 1, 1843.

Art. VIII.—*On the Law of Storms;* by H. W. Dove.*

[From Poggendorff's *Annalen der Physik und Chemie*, 1841.]

That a considerable decrease of atmospheric pressure should be an effect of any unusual disturbance of the atmosphere is a supposition so natural, that it at once occurred to those who first remarked that the weight of air surrounding us is not always the same. For the purpose of measuring these changes, Otto von Guerike attached a scale to the water barometer which he had invented, and in the 21st chapter of the *Mirabilia Magdeburgica*, in Schott's *Technica Curiosa*, he records the following remarkable observation:—"In the year 1660 the air was once so uncommonly light, that the index pointed below the lowest mark on the glass tube; on seeing this, I said to the persons who were present, that doubtless there was a great storm somewhere; two hours afterwards the tempest was raging in our district, though with less violence than it had done over the ocean." To mention only one more recent example, I may recall the storm of the 17th January, 1818, of which the ravages are still visible in the forests of Prussian Lithuania, after the lapse of nearly a quarter of a century. This storm extended from the coasts of England to Memel, and was felt throughout a region of two hundred and forty German miles in length, and forty-one German miles in breadth. On the 18th of January the barometer fell at Königsberg eight lines in eight hours. The whole fall between the 3rd and the 17th of January was twenty-one lines. In Edinburgh the fall of the barometer and the violence of the storm were also both remarkable.

The experience of the last two centuries has so far confirmed the remark of Otto von Guerike, that the scales attached to our common barometers usually terminate with "very stormy." But its applicability is not confined to the temperate zone: in lat. 70° N., lon. 70° W., the warning afforded by a fall of $9'''.29$ in the marine barometer, enabled Scoresby to avoid the dangers of a tempest which lasted two days uninterruptedly, and he consequently strongly recommends the use of these instruments to whalers when in high latitudes. In the regions of the trade

* Taken from Part X. of Taylor's Scientific Memoirs, Vol. III, p. 197.

winds, and of the monsoons, the numerous examples of greatly diminished pressure ushering in the typhoons and West India hurricanes are well known.

[M. Dove then proceeds to recount several such instances. In 1837 the harbor master at Porto Rico warned the shipping in the port to prepare against a storm, as the barometer was falling in an unusual manner; at 8 P. M. the preceding evening it had stood at $333'''.28$, and had sunk to $315'''.27$. The precautions taken were unavailing; thirty-three vessels at anchor were all destroyed, and at St. Bartholomew alone two hundred and fifty buildings were overthrown. At the same time the barometer fell at St. Thomas from $337'''$ to $316'''$, and the ravages of the hurricane were even greater than at Porto Rico. He describes the violent effects of the wind on this and other occasions, in the destruction of vessels in the harbor, forts, houses, and larger buildings; in dragging large guns (twenty-four pounders) along the ground; driving boards through trees and walls several inches in thickness, &c. &c. He refers also to Gen. Baudrant's account of the destruction of Basse Terre in Guadaloupe by a hurricane in 1825; and to hurricanes in 1828 and 1836 in the island of Mauritius, where in 1828 the barometer fell to $316'''$, and in 1836, having stood at $337'''.00$ at 5 A. M. on the 6th, it had fallen to $317'''.85$ at 8 A. M. on the 8th. M. Dove says further in a note, "A striking instance of the great mechanical power, even of smaller hurricanes, occurred near Calcutta in April, 1833, when a revolving storm, not above half an English mile in breadth, passed between Calcutta and the great salt-water lake three miles to the east of that city, and in the space of four hours, on a track of sixteen miles in length, caused the death of two hundred and fifteen human beings, and injured two hundred and twenty-three. It overthrew one thousand two hundred and thirty-nine fishermen's huts; a bamboo was driven quite through a wall of five feet thick, piercing the covering of masonry on both sides, so that the Editor of the Indian Review says a six-pounder would scarcely have had the same effect."]

If two phenomena frequently occur together, we may surmise, with some degree of probability, that they have a casual connexion; but it may remain quite undecided which is the conditional, and which the contingent phenomenon; or both may be effects of a third phenomenon, which is itself their common cause.

Further, if one of the phenomena be really an immediate consequence of the other, we may not be able to infer with certainty that the same effect might not have been produced in some other way.

If barometric minima almost always occur when the atmosphere is agitated by tempests, on the other hand we frequently see a very low barometer, when mild vernal breezes interrupt the severe cold of winter and appear to introduce the temperature of a more genial season. As it seemed however difficult to believe that such gentle winds could cause any considerable disturbance in the equilibrium of the atmosphere, the great diminution of pressure on such occasions has been attributed to other causes. The idea that the convulsions of the surface in earthquakes could not be unconnected with the atmosphere, was one of such natural occurrence, that the barometer was always looked to in the expectation of its indicating these phenomena at great distances. This idea appeared to be confirmed, when, four days after the destruction of Messina in 1783, the barometer in Europe fell unusually low. Van Swinden accordingly inferred a connection between the two phenomena; but on a comparison of the meteorological observations made at the time, and recorded in the Manheim *Ephemerides*, Brande found that on the 9th of February the barometer fell below its average height by 14 lines in Lyndon, in Rutlandshire; 13½ in Amsterdam and Franeker; 12¾ in Dunkirk; 12½ in Middleburg; 12¼ in Paris; 11½ in Laon, Nantes, and Cambray; 10½ in Brussels, Chartres, Poictiers, and Rochelle; 10 in Troyes and Montmorenci; 9 at Göttingen, Mayence, Metz, Limoges, and Bordeaux; 8 at Copenhagen, Erfurt, Würzburg, Lyons, Mezier in Guyenne, and Oleron; 7 at Spydberga in Norway, Stockholm, Berlin, Vienna, Manheim, Geneva, and Vienne; 6 at Sagan, Prague, Regensburg, on the St. Gothard, and at Montpelier; 5 at Marseilles and Montlouis; 4 at Ofen and Padua; 3 at Petersburg, Mafra, Bologna, and Rome. Thus it appeared that the barometer was lowest in England and Holland, and that in approaching Italy it differed less and less from its mean height, so that the independence of the two phenomena became highly probable.

If, as in this instance, such simultaneous observations sometimes serve to show, that what had been regarded as evidence of essential connexion was merely an accidental coincidence between two independent phenomena, we have much reason to hope that a careful examination of such observations may lead also to the actual discovery of the true causes of the phenomena. On Christmas eve, 1821, after a long continuance of stormy weather, the barometer sank so low in Europe, that the attention of all meteorologists was strongly drawn to the circumstance. Brande requested, in the scientific journals, that all the observations made at that time might be sent to him, and published his conclusions from their intercomparison in his *Dissertatio physica de repentinis variationibus in pressione Atmosphæræ observatis*, 1826. The conclusion he arrived at was, that some unknown cause of diminished pressure was moving over the surface, and that the air flowed in on all sides towards that part; therefore, that the storm so produced was *centripetal*, (*vergere procellarum directionem ad idem illud centrum*,) arising from the tendency of the surrounding air to restore the equilibrium deranged at any particular part.

Brande had previously tried to support the same view by an examination of some analogous barometric minima in his 'History of the Weather in 1783,' published in 1820; but it is remarkable how little the observations adduced by him correspond to that view. In the storm which on the night of the 11th and 12th of March, according to Toaldo, advanced from Naples to Venice in three hours, or one hundred and forty feet in a second of time, the distance being two hundred and seventy-six Italian miles, it appears so little probable that this was a flowing in towards Switzerland, which was the centre of least pressure, that Brande himself is forced to suppose that the current of air flowing with extraordinary force towards Venice, had produced a kind of enormous whirlwind, causing the air to flow from Marseilles to Corsica, in order, adds he, "then to join the great current." When he says further on, "but these are only conjectures; it is certain, however, that as the wind was east at Copenhagen, and southeast at Ofen, there is a flowing in almost completely round the circumference," (for which, however, we have only the evidence of the North in Berlin,) we might with more reason regard the directions named as tangents to circles round that centre rather than as radii.

According to the view which I had taken, that the mean atmospheric variations are produced by the conflict of two currents above the place of observation, it necessarily follows that the absolute extremes of these variations must arise from the exclusive prevalence of one of the two currents over the other. Thus a barometric minimum would be a phenomenon of the *south* current; when viewed as occurring simultaneously at several places, the south current itself; when viewed locally, a stormy passage through the minimum of the wind circle; or comprehending both views, a whirl or whirlwind advancing in the direction of the south current, *i. e.* from S. W. to N. E. In confirmation of this view, I subjected the observations collected by Brande and others to a new examination, and in a treatise, entitled 'On Barometric Minima,' which appeared in Poggendorff's 'Annals' in 1828, Vol. XIII, p. 596, I pointed out, that a simple explanation of all the phenomena could be afforded, on the assumption of one or more great rotatory currents, or whirlwinds, advancing from S. W. to N. E.; and I remarked at the same time, that in all the hurricanes of the southern hemisphere which I had examined, the rotatory movement was in the opposite sense to that which took place in the northern hemisphere. As the example discussed in that paper contained a complete refutation of the idea of a flowing in towards a centre, I will here repeat the principal quantitative determinations.

On the 24th December, 1821, at 6 P. M., the barometer stood below its average height, as follows:—22 lines at Brest; 19 at Helston and Nantes; 17 at Gosport; 16½ at Dieppe; 15 in London, Haarlem and Paris; 11 at Strasburg, Geneva and Bremen; 10 at Zurich, Göttingen and Bergen; 9 at Joyeuse and Augsburg; 8½ at Wurzburg; 8 at Regensburg and Leipsic; 7 at Prague, Breslau and Christiania; 6½ at Cracow, Apenrade and Abo; 5 at Turin and Modena; 3½ at Florence; 3 at Tilsit and Petersburg; 1½ at Rome; 1 at Molfetta.

On the 25th December, at 3 A. M.—22 lines in London; 21½ at Dieppe; 20 at Gosport and Boston; 19 at Helston; 18½ at Paris; 18 at Haarlem; 18½ at Kinfauns Castle; 16½ at Strasburg; 15 at Heidelberg; 14 at Cologne, Regensburg and Göttingen; 13 at Geneva, Zurich, Augsburg, Berlin and Bergen; 12½ at Joyeuse; 12 at Regensburg, Gotha and Leipsic; 11 at Prague and Breslau; 9 at Turin; 8 at Milan and Cracow; 7½ at Christiania; 6 at Abo; 5 at Florence, Rome and Tilsit; 3 at Molfetta; 2½ at Petersburg.

On the 25th December, at 10 A. M.—23 lines at Middleburg; 21 at Gosport; 20½ at Haarlem; 18 at London; 17 at Helston; 16 at Dieppe, Göttingen and Bremen; 15 in Paris, Strasburg and Bergen; 14 at Heidelberg, Gotha and Leipsic; 13 at Zurich, Augsburg, Vienna, Prague and Breslau; 12¼ at Joyeuse and Inspruck; 11½ at Cracow and Dantzic; 11 at Padua; 9½ at Christiania; 8 at Florence; 7 at Tilsit; 6 at Rome, Molfetta and Abo; 3 at Petersburg.

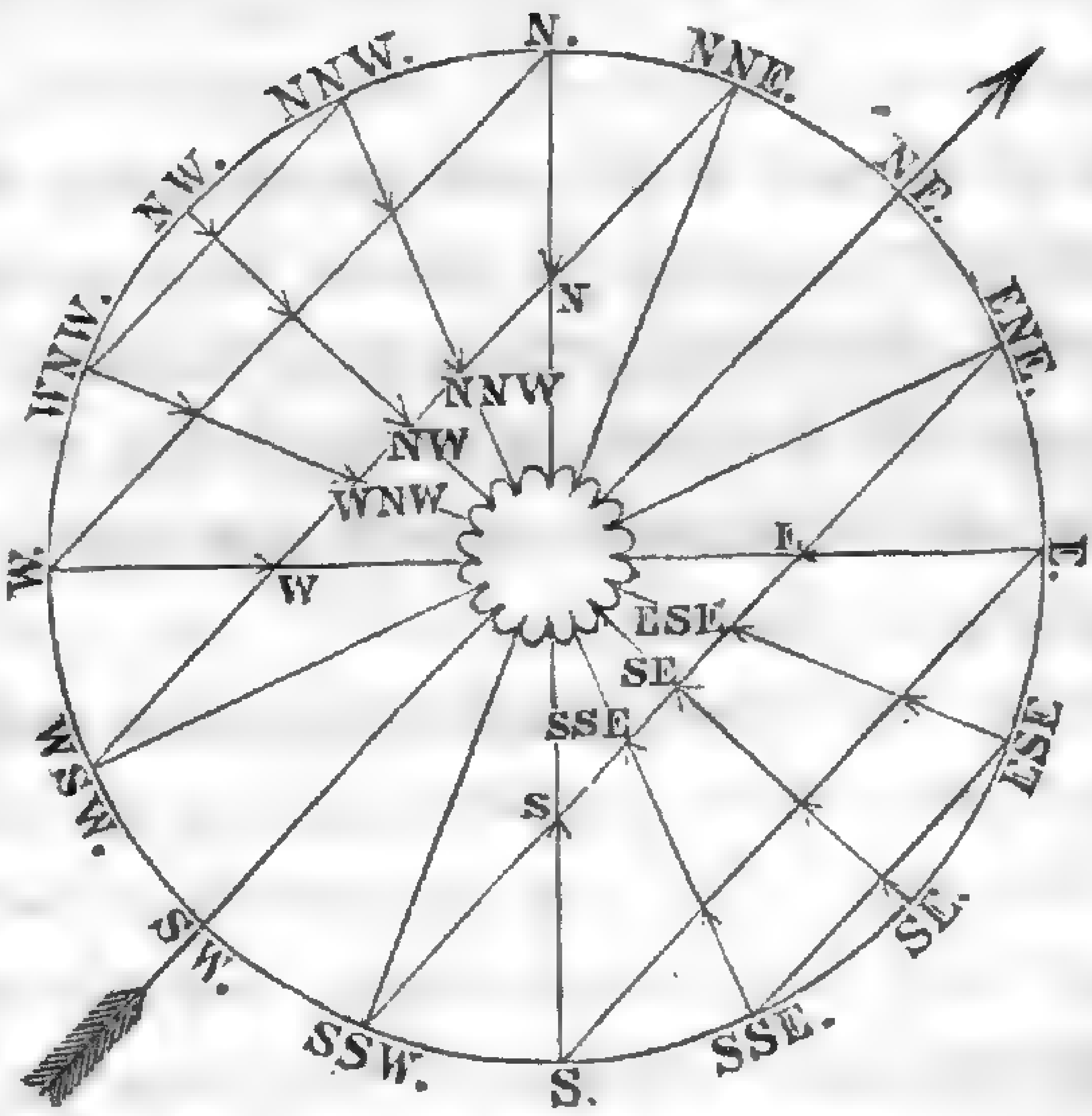
On the 25th December, at 8 P. M.—17 lines at London; 16½ at Helston and Apenrade; 16 at Haarlem and Bergen; 15 at Bremen; 14 at Dieppe, Göttingen and Dantzic; 13 at Paris, Gotha, Breslau and Christiania; 12 at Strasburg, Berlin and Cracow; 11 at Turin, Zurich and Augsburg; 10½ at Padua; 10 at Prague; 9 at Tilsit; 8 at Florence; 7 at Molfetta; 4 at Petersburg.

According to the one view,—in which it is considered that the atmospheric pressure at a given place being from some cause unusually diminished, an inflowing takes place from all sides,—there will be equilibrium between the several particles on a line in which the pressure is equally diminished, and the general direction of the wind will be perpendicular to that line. According to the other view,—in which the complex phenomenon is regarded as the consequence of a rotatory movement,—the general direction of the wind will be that of the above named line itself. Thus the two assumptions lead to two directions of the wind at right angles to each other. We have therefore next to inquire to which assumption the observations correspond.

From the above mentioned observations, it follows that the march of the minimum was from the French coast towards the southwest point of Norway, or nearly from Brest to Cape Lindenaes. What was the direction of the wind at the different stations in reference to this moving minimum? Was the direction *towards* the minimum? or was it tangential to circles having the place of minimum as their common but constantly moving centre? This may be directly tested in the most simple manner, by laying down on four maps the place of the minimum for the four epochs, 6 P. M. 24th December, and 3 A. M., 10 A. M., and 8 P. M. 25th December, and then marking on these maps the directions of the wind simultaneously observed at the several stations. If the arrows on the maps are found to be tangents to concentric circles, the actual existence of these circles may be assumed, and the directions of the wind which follow from such a supposition may be compared with the observations.

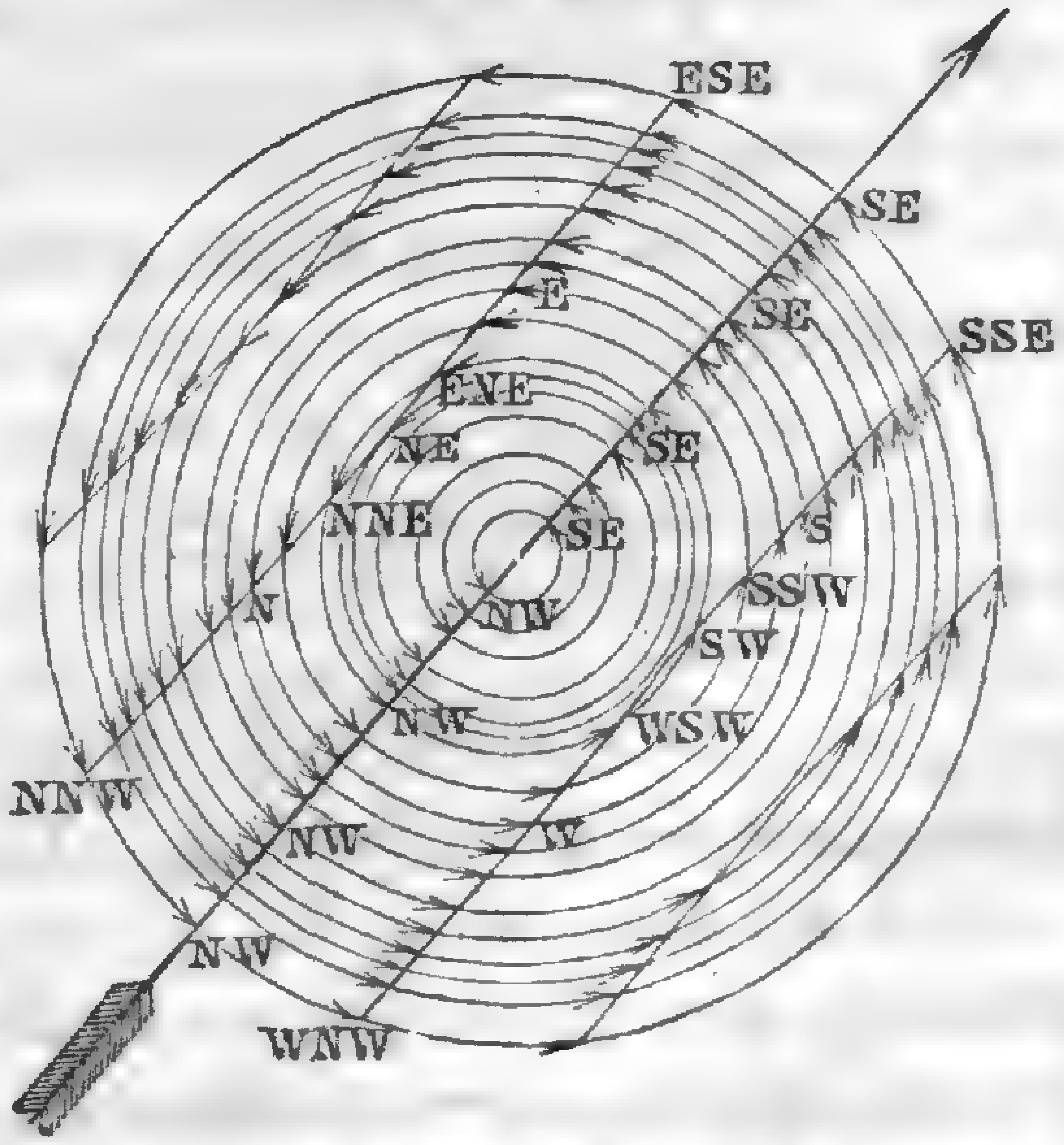
As the march of the minimum is from Brest to Cape Lindenaes, France, Italy, Germany, Denmark and Russia are on the southeastern side of the main path of the storm, Ireland, Scotland and Iceland are on the northwestern side, and England is nearly in the middle of its course.

Fig. 1.



On the supposition of a centripetal inflowing according to Brande's view, as the phenomenon passes over a station on the southeastern side, a wind vane at that station should be seen to pass successively from E. N. E. through E., E. S. E., S. E., S. S. E. and S., to S. S. W.; and at a station on the northwestern side the vane should pass from N. N. E. through N., N. N. W., N. W., W. N. W. and W., to W. S. W. (Fig. 1.) If, on the other hand, the storm is rotatory and turning in the opposite direction to that of the hands of a watch, the wind at a southeastern station will pass from S. S. E. through S., S. S. W., S. W., W. S. W., and W., to W. N. W.; and at a station on the northwestern side, from E. S. E. through E., E. N. E., N. E., N.N.E., N. and N. N. W. (Fig. 2.) At places situated in the middle of the phenomenon the wind should change suddenly, according to the first view, from N. E. to to S. W.; and, according to the second view, from S. E. to N. W.

Fig. 2.



Both suppositions agree in leading us to expect a rotation *with* the sun on the southeastern side of the storm, and *against* the sun on the northwestern side; and in the middle a calm, dividing winds blowing in opposite directions; but they disagree in making a difference of 90° in the two opposite positions of the wind vane at the commencement and at the termination of the rotation.

The observations in the memoir referred to are decidedly in favor of the second view, and adverse to the first; for the rotation of the vane never begins with N. N. E., N., or N. N. W., and ceases with W. S. W., W., or W. N. W., but always begins with E. S. E. and S. E., and ceases with S. W. and W. It was so in Germany, Italy, Denmark and Russia. In England the wind before the minimum was east, not north; in France it was principally S. W., and in Iceland it was first N. E. and afterwards N., just as the rotatory movement would require at a point so distant from the centre.

[In a map accompanying the original memoir, M. Dove has marked the successive circles of the rotatory storm as it advanced from the beginning to the end of the observations, in order to facilitate the comparison of the observations with the theoretical assumption. Where the advancing circles are stopped by the Spanish mountains and the Maritime Alps, those points are assumed as centres of new circles or whirlwinds.]

The observations give—

1. On the Northwest side of the Storm.

Naes in Iceland, N. E., N.

2. Nearly in the middle of the Storm.

Helston, E., min., W.
London, S. E., min., N. W.
Owen's Row at Islington, S. E., min., N. W.
Cambridge, S. E., min., W.
New Malton, southerly storm.

3. On the Southeast side of the Storm.

Boulogne-sur-Mer, S. S. E., S., E. N. E., min., W. N. W.
Paris, S., min., W. S. W.
Joyeuse, southerly storm, min.
Nismes, S., min., S. W., N. W.
Vivarias, S. E., min., S. E.
Strasburg, S. E., E., S., min.
Haarlem, S. E., E. S. E., S.S.E., min., S. S. W., S. W.
Schwelm, S., min., S. W.

Cologne, S. S. E., S. E., min., S., W. S. W., S. W.
Coblentz, S. W., min., S., S.W.
Salzuflen, S. E., min., S.
Wetzlar, S. S. E., min., S.S.W., S. W.
Minden, S. E., min., S.
Carlsruhe, S., min., S. W.
Göttingen, S. E., S. S. E., min., S. W.
Regensburg, E., S. E., min.
Augsburg, S. W., min., W.
Quedlinburg, E., min., S. W.
Zellerfeld, S., min., W.
Leipsic, S. W., min., S.

Zschoppau, min., at S. W.
Annaberg, S. E., min., S. W., W.
Prague, W., min., S. W., W.
Breslau, S. W., min., S.
(Disturbing influence of the mountains to the S.)

Leobschutz, S.
Dantzic, S., min., S.
Königsberg, S. E., min., W.
Tilsit, S. W., S. E., min., W.
Petersburg, S. E., E., S. E., S. S. E., min.
Geneva, S. E., min.
Zurich, E., min., S. E., W.
St. Gall, S. E., S. S. E., min., S. E.

4. Modified, on the South side of the Alps.

Milan, W., S. W., min., W., S. E.
Pavia, S. E., min., S. W.
Modena, S. E., min., S. W., W.
Padua, W., S., min., N.
Florence, S., S. S. W., min., S. W.
Rome, S. S. E., S., min., S.S.E., S., S. S. E.
Molfetta, S. E., S., min., S.S.W.

The two views which I have thus contrasted have recently formed the subject of a very animated discussion. On the one side Mr. Redfield, of New York, has been led, by a most careful examination of the phenomena accompanying the very frequent storms on the coasts of the United States, to the same conclusion as that which I had arrived at for Europe. On the other hand, the view enounced by Brande has also found an American supporter in Mr. Espy of Philadelphia. The tornado of the 19th of June, 1835, gave occasion to Mr. Espy to assume the hypothesis of centripetal storms. After the tornado* Mr. Bache and Mr. Espy visited the site of a wood over which it had passed, for the purpose of examining the direction in which the trees

* Notes and diagrams illustrative of the directions of the forces acting at and near the surface of the earth, in different parts of the Brunswick tornado of June 19th, 1835.

had been overthrown, and they found that the tops of all the trees pointed to a centre, the most western trunks lying with their heads towards the east, those to the north with their heads towards the south, the eastern ones towards the west, and the southern ones towards the north. An eye-witness of this storm, Prof. L. C. Beck, maintains, on the contrary, that it was a decided whirlwind, and asserts that no one who beheld it could think otherwise, unless they brought with them previously embraced theoretical views. Mr. Espy's account of the cause of the inflowing towards a centre is the following:—He considers that when aqueous vapor is condensed into the form of a cloud, it disengages heat, which heat causes the air which contained the vapor to expand six times the loss of volume from the condensation of the vapor. This air he supposes to ascend therefore with a velocity of 364 feet in a second, and at the height of hail-clouds to exert on a square foot of surface a pressure of 120 cwt., capable of carrying up a cubic block of ice of a foot and a half dimension, or even of lifting an elephant. These conclusions, which are termed by Mr. Espy himself "extraordinary and unexpected," are to be found in a memoir consisting of sixteen pages, and bearing the modest title of 'Theory of Rain, Hail and Snow, Waterspouts, Landspouts, Variable Winds and Barometric Fluctuations, and examination of Hutton's, Redfield's, and Olmsted's Theories.' We are indebted to the repeated attacks of this author for having given occasion to some excellent memoirs from Mr. Redfield.* The collection of observations,

* Remarks on the prevailing Storms of the Atlantic Coast. (Silliman's American Journal, xx, No. 1.)

Hurricane of August, 1831. (To the editor of the Journal of Commerce.)

Observations on the Hurricanes and Storms of the West Indies, and of the Coast of the United States. (Blunt's American Coast Pilot, 12th edit.)

On the Gales and Hurricanes of the Western Atlantic. (Sill. Amer. Journ. xxxi, No. 1.)

Meteorological Sketches, by an Observer. (Sill. Amer. Journ. xxxiii, No. 1.)

Remarks on Mr. Espy's Theory of Centripetal Storms, including a Refutation of his Positions relative to the storm of 3rd of September, 1821, with some notices of the fallacies which appear in his examinations of other Storms. (Journal of the Franklin Institute.)

On the Courses of Hurricanes, with Notices of the Typhoons of the China Sea and other Storms. (Sill. Amer. Journ. xxxv, No. 5.)

The Law of Storms. (New York Observer, 18th January, 1840.)

Whirlwinds excited by Fires, with further Notices of the Typhoons of the China Sea. (Sill. Amer. Journ. xxxvi, No. 1.)

to serve as materials, formed by Mr. Redfield with the greatest care, has further received a highly important augmentation, by the magnificent work which the present Governor of the Bermudas, Lieut. Colonel Reid, has published on the subject.* Col. Reid has arrived at precisely the same result as Mr. Redfield, and I know by written communications, that both these gentlemen have done so quite independently of my earlier researches. But Redfield and Reid, besides placing on a wider basis the rotatory movement which takes place in opposite senses in the two hemispheres, have added further some very material observations, whose empirical establishment is entirely their own; these I shall now attempt to connect theoretically with the rotation movement.

In my first researches on the subject of the winds, I had referred both the law of rotation and the rotatory movement of storms to the mutual action of two currents of air, each tending to press aside the other; but a more close investigation of the phenomena has taught me to regard the law of rotation as resting on more general conditions, and as being a simple and necessary consequence of the rotation of the earth. The principle of Hadley's theory of the trade winds thus generalized, explained fully all the rules which had been found for the non-periodic variations of the meteorological instruments in the northern hemisphere, and permitted the prediction of rules for the southern hemisphere; but it did not explain the rotatory movement of storms, and consequently when I published my *Meteorologische Untersuchungen*, Berlin, 1837, which were made to embrace all that I had previously written on the subject, I was obliged to retain the earlier theoretical representation, since that which had been thus empirically deduced had been fully confirmed, but without its connexion with the principle of the general theory being shown. The object of the present memoir is to supply this deficiency. From the researches of Redfield and Reid we have the following facts:—

1. Storms which originate within the tropics preserve the first direction of their path almost unaltered, until they enter either

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* An attempt to develop the law of storms by means of facts arranged according to place and time, and hence to point out a cause for the variable winds, with a view to practical use in navigation; illustrated by charts and wood cuts. London, 1838.

of the temperate zones, when their course becomes deflected into one almost at right angles to the former. Thus the storms of the northern hemisphere move from S. E. to N. W., until they have past to the north of the tropic of Cancer, when their course becomes from S. W. to N. E.; and, on the other hand, the storms of the southern hemisphere, whose progress within the tropics is from N. E. to S. W., take a new direction on entering the southern temperate zone, and then move from N. W. to S. E.

2. The breadth of the whirlwind, which increases very gradually within the tropics, becomes suddenly greatly augmented at the time when the path undergoes the above described flexure on passing those limits. The chart of the West India hurricane of the middle of August, 1837, in Colonel Reid's work, and that of the Mauritius storm of March, 1809, in Berghau's atlas, are examples of these phenomena in either hemisphere. The course of storms is further illustrated by a chart of Redfield's, in which the tracts of ten are laid down. The paths of two of these storms, which did not extend beyond the tropics, are rectilinear; that of the 23rd of June, 1831, passes from Trinidad by Tobago and Granada, through the middle of Yucatan to the neighborhood of Vera Cruz; that of the 12th of August, 1835, passing from Antigua by Nevis, St. Thomas, St. Croix, Porto Rico, Hayti, Matanzas, and Cuba, and thence to Texas.

The courses of the eight storms which passed the boundaries of the tropics were as follows:—

The storm which ravaged Barbadoes on the night of the 10th of August, 1831, reached Porto Rico on the 12th, the Keys, St. Jago de Cuba on the 13th, Matanzas on the 14th, the Tortugas on the 15th, the Gulf of Mexico on the 16th, and finally, Mobile, Pensacola and New Orleans on the 17th, so that it had passed over a space of 2,000 nautical miles in about 150 hours, or at the rate of $13\frac{1}{2}$ miles in one hour. Its direction before reaching the tropic was N. 64° W.

The storm which began on the 17th of August, 1827, in the neighborhood of Martinique, reached St. Martin and St. Thomas on the 18th, passed to the northeast of Hayti on the 19th, reached Turks Island on the 20th, the Bahamas on the 21st and 22nd, the coast of Florida and South Carolina on the 23rd and 24th, Cape Hatteras on the 25th, Delaware on the 26th, Nantucket on the 27th, Sable Island and Porpoise Bank on the 28th, having passed over 3,000 nautical miles in eleven days. Within the tropics, the direction of its path was N. 61° W.; in the latitude of 40° it was N. 58° E.

The storm which began in the neighborhood of Guadaloupe on the 3rd of September, 1804, reached the Virgin Islands and Porto Rico on the 4th, Turks Island on the 5th, the Bahamas and the Gulf of Florida on the 6th, the coasts of Georgia and of the Carolinas on the 7th, Chesapeake Bay, the mouth of the Delaware and the neighboring parts of Virginia, Maryland and New Jersey on the 8th, Massachusetts, New Hampshire and Maine on the 9th. Its curved path from Guadaloupe had extended over 2,200 nautical miles in six days, or at the rate of 15½ miles an hour.

The storm which prevailed at St. Thomas on the 12th of August, 1830, passed near Turks Island on the 13th, the Bahamas on the 14th, the Gulf and coast of Florida on the 15th, along the coasts of Georgia and the Carolinas on the 16th, those of Virginia, Maryland, New Jersey and New York on the 17th, George's Bank and Cape Sable on the 18th, and the banks of Newfoundland on the 19th; it advanced therefore eighteen miles an hour. Now if we take the actual velocity of the wind in its rotatory direction as five times greater than the progressive movement of the storm, we have the air moving through 18,000 miles in seven days.

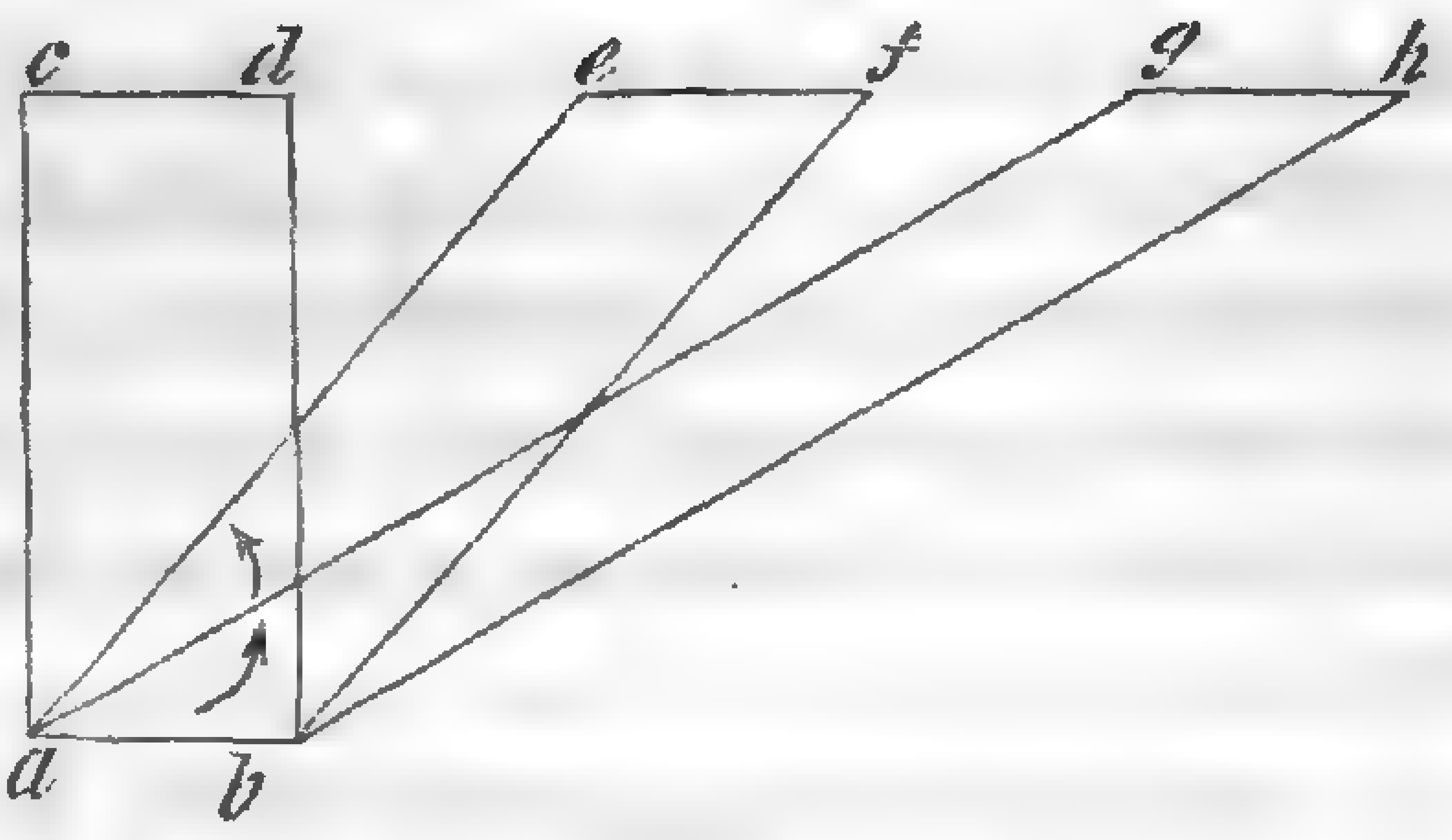
The most eastern storm was that of the 29th of September, 1830. Beginning to the north of Barbadoes, in the 20th degree of latitude, in long. 68°, lat. 30° its course became northerly, and subsequently, after passing to the west of the Bermudas, northeasterly, until on the 2nd of October it reached the east end of the banks of Newfoundland.

A very violent storm, but of much less diameter than the preceding, prevailed at Turks Island on the 1st of September, 1821. On the following day it was felt north of the Bahamas, early on the 3rd it reached the coast of the Carolinas, later in the day New York and Long Island, and in the following night it passed over the states of Connecticut, Massachusetts, New Hampshire and Maine, a course of 1,800 miles in sixty hours; its mean velocity was therefore thirty miles an hour.

The course of the storm of the 28th of September, 1838, was similar to the one last described. The storm of the 22nd of August, 1830, was much slower in its progress. It began in lat. 20°, to the north of Porto Rico, and preserving about the same distance from the coast of North America, reached the banks of Newfoundland on the 27th.*

The explanation of these phenomena appears to be the following:—

Fig. 3.



Let *a b* (Fig. 3.) be considered to represent a series of material points parallel to the equator; supposing these points to receive from any cause whatever an impulse in the direction *a c* towards the north, then, inasmuch as they would be transferred from greater to lesser parallels of latitude, they would move towards *g h* if the space *d b h* were a vacuum; but if there be in this space air not in movement, then the particles in *b*, as they move towards *d*, will be continually coming in contact, in the space *d b h*, with particles of air of inferior rotatory velocity, and thus their own eastward velocity will be diminished. Thus the point *b* will move towards *f* instead of towards *h;* but the particles at *a* have near them, on the side of *b*, particles of originally equal rotatory velocity with their own; they move therefore as they would do *in vacuo*, *i. e.* towards *g*. Therefore if *a b* be a mass of air impelled from the south towards the north, then the direction in which the storm is blowing will be much more southerly on its eastern side than on its western side, where it will be more westerly, and thus there will arise a tendency to rotate in the sense S. E. N. W. This

* As the direction in which the storm is advancing is quite a different thing from the direction in which the rotating current may be blowing at a given place, it is easy to see what incorrect conclusions might be arrived at by an exclusive consideration of observations merely local. Thus Raynal in his *Histoire Philosophique et Politique des deux Indes*, Vol. V, p. 72, says that recent observers had noticed that the storms which had ravaged the Antilles from time to time had always come from the northwest, from which he concludes that they came from the mountains of Santa Martha, although all that is really indicated is that the islands are situated on the southern side of a rotating storm, in which the movement is in the opposite sense to that of the hands of a watch, or from east to west, which agrees perfectly with the observations referred to above. The frontispiece of the sixth volume of Raynal's work is a vivid picture of a West India hurricane.

rotatory tendency would not be produced at all, if there were no resisting mass in the space *d b h;* it will therefore be proportionate to the resistance thus opposed to the westerly deflection of the storm, and the force of the rotatory movement of the storm will be the greater the more the original direction of its path is preserved. In the zone of the northern trade winds, the space *d b h* is filled with air which is flowing from N. E. to S. W.; the resistance will here be the greatest, so that the air in *b* may be so checked in its westward tendency, that it may preserve its direction towards *d* almost unaltered, whilst *a* tends towards *g;* the rotatory motion of the storm will therefore be the most violent, whilst at the same time its course will be rectilinear, and its breadth unaltered. But when it reaches the temperate zone, there will be in the space *d b h* air which is already moving from S. W. to N. E.; the resistance hitherto encountered at *b* will therefore be suddenly greatly diminished, or altogether removed; and hence the direction *b d* is suddenly changed into the direction *b h*, so that the storm is suddenly deflected almost at right angles, and at the same time its breadth increases rapidly from the cessation of the difference between the movements of the points in *a* and the points in *b*. The phenomena in the southern hemisphere may be derived in the same manner; the rotation is in the opposite sense, and the change of direction on passing the boundary of the tropics is analogous.*

As the West India hurricanes originate at the inner boundary of the trade winds, where at the limit of the so-called region of calms the air ascends and flows over the trade in an opposite direction, it is probable that portions of this upper current penetrating through the lower one, give the first occasion of those storms. The high mountains of several of the islands, by offering a mechanical impediment, may be one cause of this effect,

* The above derivation of the rotatory movement only applies when great masses of air, of a considerable extension in breadth, are set in motion; lesser whirls of wind or water, as water-spouts, &c., are produced by other causes, and therefore probably do not show either regular direction or definite opposition in reference to the two hemispheres. Mr. Redfield observed a small tornado rotating like the greater storms; but Colonel Reid saw from the government-house, Bermuda, a water-spout rotating in the opposite sense. The observations of Akin at Greenbush near Albany, of Dwight at Stockbridge in Massachusetts, and of Dr. Cowles at Amherst, of whirlwinds of great force taking place in forest fires in calm weather, show that a strong ascending current may also produce a rotatory motion.

as the air flows with redoubled velocity between two mountains. The reason of the course of the storm being in the first instance from S. E. to N. W., may be explained by the circumstance, that according to the theoretical deduction which has been given, this direction is the one most favorable to the origination of a rotatory movement. If, as may happen, the first impulse should be from S. W. to N. E., the northeast trade blowing in the opposite direction would equally check all the points of the advancing line, so that no tendency to rotate would be produced.

It is evident that if the above deduction of these phenomena be the true one, a similar whirlwind must be produced whenever, owing to any other mechanical cause, a current flowing towards higher northern latitudes is more southerly on its eastern side than on its western, where the direction is more towards the east. Observations collected by Piddington,* show this to have been the case in the storm in the bay of Bengal on the 3rd, 4th and 5th of June, 1838. It was one of those storms which usually accompany the change of the northeast to the southwest monsoon, which change takes place in the bay of Bengal between the 15th of May and the 15th of June.† Throughout the greater part of its course, "it was a gale or strong wind blowing with tolerable steadiness from one quarter of the compass," and it was only at a particular part that it was a hurricane or violent wind blowing in a circle or vortex of greater or less diameter.‡ It blew as a violent southwest monsoon in the space between the east coast of Ceylon and Masulipatam, and across the bay of Ben-

* Researches on the Gale and Hurricane in the Bay of Bengal on the 3rd, 4th and 5th of June, 1838, being a first Memoir with reference to the Theory of Storms in India. Journal of the Asiatic Society of Bengal, No. 91, p. 550; Second Part, No. 52, p. 631.

† According to the observations of Brown, it commenced at Anjarakandy on the Malabar coast in 1820–1833, on May 20, 31, 27, June 15, May 21, June 18, May 26, June 5, May 9, 26, June 16, 2, 6; at Canton it set in, according to the Canton Register, from the 20th to 28th of April in 1830, from the 7th to 17th of April in 1831, from the 4th to 7th of April in 1832, from the 9th to 14th of April in 1833, from the 3rd of April to 8th of May in 1834, and from the 8th to the 21st of April in 1835.

‡ This seems to be also the case with the storms which accompany the change from the southwest to the northeast monsoon. These storms, which the Spaniards in Manilla call "los temporales," are not accompanied by rain, but the air is every where darkened by the salt spray from the sea. On the coast of Coromandel these storms are termed "the breaking out of the monsoon." On the Malabar coast the Portuguese call those which are peculiarly violent, "Elephanta."

gal towards the mountain range of Arracan, where it turned completely at right angles, advancing into the interior, up the Ganges, and blowing as a southeastern current over Calcutta and Benares to Cawnpoor, Lucknow and Agra. At the place of flexure near Arracan—in the focus, as Piddington expresses it, of the parabolic course of the storm—a whirlwind originated and advanced parallel to the coast, and passing off the mouths of the Ganges, moved in a direction from between E. N. E. and E. towards W. S. W. and W., from Shapooree Island towards Vizagapatam, Gangam, Juggernaut, and the mouths of the Mahanuddy and Bramnee, rotating like the West India hurricanes in the sense S.E. N.W.

Here we have a whirlwind rotating in a precisely similar manner to those before described, arising (under circumstances originally quite different) when the direction of the storm on its eastern side was more towards the north than on its western side; and possibly the typhoons of the Chinese sea may owe their origin to similar causes. In the complex phenomenon of the southwest monsoon the conditions are analogous. The wind, from being southwest in the Indian sea and the bay of Bengal, becomes more nearly south in the Chinese sea; more extended observations are required to show whether this deflection is caused by the chain of the Philippines, or whether it is an immediate consequence of the bordering of the monsoon and the trade. Horsburgh says expressly,* that on the south coast of China the typhoons from July to September make the wind vanes when near the coast point successively N. W., N., N. E., E., S. E. and S., and that further off the coast they point instead to N., N. W., W., S. W., S. In other words, the typhoons are storms rotating S. E. N. W., passing along the coast from east to west, so that the northern half of the whirlwind impinges on the coast, and the southern half covers remoter portions of the sea. The Raleigh typhoon of the 5th of August, 1835, which passed from Bashee Island, between Luconia and Formosa towards Macao, in the direction from E. S. E. to W. N. W., is a recent example of these storms, and corresponds perfectly to the description which has just been given.

But if these rotatory storms arise from the southwest monsoon being more southerly on its eastern than on its western side, and

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* India Directory, Vol. II, p. 233.

if for this very reason they move from east to west, they will prevail by preference in the eastern part of the Indian Ocean; and in fact it has been remarked by Dampier that on the coast of Coromandel storms are looked for in April and September, which are the months of the change of the monsoons, whilst on the Malabar coast they are frequent during the whole westerly monsoon.

Having thus found in the typhoons a confirmation of the principles which were applied to the West India hurricanes, we may proceed to consider in greater detail the phenomena which accompany these great disturbances of the atmosphere.

When in the storms of the regions of the trades the rotating cylinder from the lower trade impinges on the upper current, it is evident that inasmuch as a southwesterly direction of the wind prevails above, the reasoning which has been made use of for the lower part of the same cylinder when it passed beyond the external limit of the trades, becomes applicable to its upper part, which will immediately spread, and will advance in a different direction from that of the lower part of the whirlwind. Thus will arise the secondary phenomenon of suction in the middle of the whirlwind, producing a diminution of pressure on the surface of the earth, and this for two reasons, viz. inasmuch as the rotation causes the air to fly from the centre, and as moreover the whirlwind widens conically in ascending, and consequently the upper strata are more distant from the axis of the cylinder than the lower, which have therefore a tendency to ascend in order to compensate the diminished density above.

But that the storm itself does not originate from this kind of suction, will be evident on a closer consideration of the observations. I will take as an example the hurricane of the 2nd of August, 1837, for which we have simultaneous observations at St. Thomas and at Porto Rico, which are shown in comparison in the following table :—

	ST. THOMAS.			PORTO RICO.		
Mean time.	Barometer.	Wind.			Barometer.	Wind.
h. m.	'''			h.	'''	
Aug. 1. 18 0	337					
2. 2 10	335	N. W.	Increasing tempest.			
3 20	334	N.				
3 45	334	N.				
4 45	332	N.				
5 40	331.5	N. E.				
5 45	330	N. E.	Hurricane.			
6 30	328	N. W.				
6 35	325.5	N. W.				
6 45	324	N. W.				
7 0	324	N. W.				
7 10	322	N. W.				
7 22	318.5	N. W.				
7 30	317	N. W.				
7 35	316.5	Dead calm.				
7 52	316					
8 10	316			8	333.28	N. N. E.
8 20	316					
8 23	320	S. S. E.	Hurricane.			
8 33	321	S. E.				
8 38	322	S. E.				
8 45	323	S. E.				
8 50	324	S. E.				
9 0	326	S. E.			332.16	
9 10	328	S. E.				
9 25	329	S. E.				
9 35	330	S. E.				
9 50	331	S. E.				
10 10	332	S. E.		10	331.03	
10 35	333	S. E.				
11 10	333.25	S. E.		11	329.90	E.
11 30	333.5	S. E.		12	315.27	Hurricane.
14 45	335	S. E.		15½	328.43	S.
20 0	336.5	S. W.		16	332.16	
21 0	336.75	E.				

The dead calm suddenly interrupting the fiercest raging of the storm from opposite directions, which is shown in the register of observations at St. Thomas,—that dreadful pause which fills the heart of the bravest sailor with awe and fearful expectation,—receives a simple explanation on the rotatory theory, which requires that at the centre of the whirlwind the air should be in repose; but appears irreconcilable with the supposition of a centripetal inflowing, because two winds blowing towards each other from opposite directions must *gradually* neutralize each other, and thus their intensity must diminish more and more in approaching their place of meeting. This takes place on the great scale in the trade winds; and if the centripetal view of hurricanes were the just one, the same effect would necessarily be seen as the centre of the storm passed over the station of observation. But the phenomena shown by observation are widely different.

At St. Thomas the violence of the tempest was constantly increasing up to *7h 30m* A. M., when a dead calm succeeded, and at *8h 10m* A. M. the hurricane recommenced as suddenly as it had intermitted. How can this be reconciled with the meeting of two winds? Besides, the air at Porto Rico should have been flowing towards St. Thomas at that time, and therefore should have been west, whereas it was N. N. E., just as is required by a whirlwind of which St. Thomas was then the centre.

A remark of the St. Thomas Observer, Hoskiaer, to the effect, that at each gust the mercury in the barometer sunk two lines and then immediately rose again to the same height as before, shows the diminution of atmospheric pressure to be not the cause, but rather a consequence attendant on the violent movement of the air.

In considering the progressive advance of the whirlwind, we have not hitherto taken into account the resistance opposed to the motion of the air by the surface of the earth. This resistance, as Redfield justly remarks, causes the rotating cylinder to incline forwards in the direction of its advance, so that at any station the whirlwind begins in the higher regions of the atmosphere before it is felt on the surface of the earth, where therefore the sinking of the barometer indicates its near approach. The inclined position of the axis causes a continual intermixture of the lower and warmer strata of air with the upper and colder ones, thereby occasioning heavy falls of rain, and proportionably violent electric explosions. The cold air appears to precipitate itself from the cloud, and the storm to assume the form called by the Greeks ἐκνεφίας. This may also explain the phenomenon known to the navigators of the torrid zone under the name of bulls' eyes, *i. e.* a small black cloud appearing suddenly in the sky in violent motion, which becoming apparently self-developed, soon covers the face of the heavens, and is followed by an uproar of the elements, rendered doubly striking by the previous untroubled serenity of the sky.

[M. Dove then quotes from Colonel Reid's work, a very vivid description given by eye-witnesses of the Barbadoes hurricane of the 10th of August, 1831, in which violent electric explosions (both lightning and meteors), the heavy rain, the tremendous force of the wind, its changes of direction, and its interruption by lulls, all form part of the picture.]

Let us now consider these storms on their entrance into the temperate zone. Their change of direction from S. E. to S. W., on passing the outer boundary of the trade winds, has been explained on the assumption of the storm meeting with S. W. winds, instead of the N. E. wind which had till then opposed its advance. It must however be remembered, that the direction of the wind in the temperate zone is not constant but varying. Such phenomena as those which have been described require for their occurrence, that the S. W. winds do actually predominate previously in the temperate zone: barometric minima accompanied by storms are therefore only observed when those conditions are fulfilled. They were so in a high degree previous to the time of the minimum on the 24th of December, 1821; for in November and December the mean direction of the wind had been southwest in Penzance, London, Bushey, Cambridge, Lancaster, Manchester, Paris, Brest, Dantzic, Königsberg, &c.; and it appears from the *Bibliothèque Universelle*, that a more or less stormy southwest wind prevailed throughout the middle region of western Europe.

We have before assigned reasons for the sudden increase of breadth and diminution of intensity which accompany the change of direction of the course of the storm. It will be seen by the converse of the same reasoning, that the intensity increases again, when smaller whirlwinds are, from any cause, developed from the larger one. Such was the case in the Mediterranean at the time of the minimum of the 21st of December, already noticed, when the advancing masses of air, arrested in their progress by the Spanish mountains and by the Maritime Alps, were set into violent rotatory motion around these points as fresh centres; and we find accordingly that the force of the storm was particularly great there no less than at the primary centre. In regard to the latter, the Brest accounts say, on the 26th of December, "We have been living for fourteen days in the midst of storms, which have not ceased to rage with unparalleled fury." In London there was the highest flood which had been seen since 1809. At Portsmouth one gust from the S. S. E. is spoken of as almost unprecedented, and the sea rose to an enormous height. In regard to the secondary centres, the ravages of the storm in and around the Mediterranean were very great. From Leghorn to Barcelona it was terribly destructive. On the southern declivity of the Alps enormous masses of rain fell, and Venice, Genoa and Nice were overflowed. In Appenzell the tempest was such as the oldest inhabitants had never witnessed; it raged with peculiar force in the valleys; the mountains presented such an obstacle to the pressure of the stream of air, that the barometer stood much higher on their southern than on their northern declivity.

We see that those barometric minima of the temperate zone, which are caused by the entrance of tropical whirlwinds, are distinguished from the same phenomena in the torrid zone by their greater extension as well as by the different direction of their course. In the case of the minimum of the 2nd of August, 1837, the difference of barometric pressure at St. Thomas and Porto Rico, places scarcely twenty miles apart, was 15 lines. On the 24th December, 1821, the difference of pressure at Brest and Bergen was only 12 lines, the amount of the absolute minimum being the same in both instances. On the 21st of May, 1823, on the Hidgelee coast, the barometer fell on board the Duke of York, between 8 A. M. and 11 A. M., from 325‴ to below 298‴, or 27 lines in three hours, as shown both by the barometer and sympiesometer, (the fluid in both instruments having sunk for the space of half an hour below the visible part of the tubes, which began at 298‴,) the simultaneous fall at Calcutta having been only 8 lines. We see thus, that the fall of the barometer previous to the minimum, and its subsequent rising, take place much more rapidly within the tropics than in the temperate zone; but if we consider the total diminution of pressure, we shall find that it is much greater in temperate than in tropical regions. In the former it may be compared to an extensive valley with gentle declivities, in the latter to a deep ravine with precipitous sides. Besides the causes of diminished atmospheric pressure in the tropical regions, an additional cause comes into play in the temperate zone, viz. the high temperature brought from lower latitudes by the rapid movement of the air from that direction. On the 24th of December this was very considerable. In Tolmezzo the thermometer rose to 25° Reaumur in the shade; at Geneva it rose suddenly 5° in the night of the 24th and 25th; at 1 A. M. on the 25th it reached 12°.5, which was its highest point. In Boulogne, Paris, and Hamburg the temperature was unusually high. It seems evident that when so warm a current of air was flowing towards the pole over Europe, the cold air displaced by it must flow southward in some other quarter, and according to the rotation of the whirlwind, this might be expected to be in America. In effect, the thermometer at Salem in Massachusetts, in the latitude of Rome, stood — 10°.2 R. on the 24th of December, and a few days later at — 14°.2 R., and all accounts from America speak of an unusual degree of cold.

But these phenomena are not peculiar to the winter months. The storm which ravaged St. Thomas and Porto Rico on the 2nd of August, was followed in the middle and on the 21st of the same month by two very violent storms, which are described in detail in Col. Reid's work; at the same time unusual heat, accompanied by most violent storms of wind and heavy rain, prevailed in Europe. From the 10th to the 20th of August, the thermometer stood at +30° R. in Messina, and between +28° and +30° at Naples; on the 12th it stood at +30° at Rome, whilst at Rothen and in the Emmethal the torrents, swollen by the violent rains, swept along rocks of 60 cwt. In Silesia the heat was oppressive. In Gallicia and Prussia this unusual heat was followed near the end of the month by a remarkable cold. This had prevailed in America during the great heats in Europe, for at Rochester, in the state of New York, on the 4th of August, the extraordinary phenomenon of a night frost had been witnessed.

If in these meteorological phenomena of the temperate zone we recognize the manifest influence of the quickly succeeding disturbances of the atmosphere within the tropics, we shall at once see the reason why deviations from the order of change in the direction of the wind, which results from the law of rotation, namely, S. W. N. E., are a sure sign of very unsettled weather; a remark which has been made by almost all observers who have carefully examined the connexion of the direction of the wind with the accompanying phenomena of the weather. On the northwestern side of a rotatory storm the wind vane turns N. W., W., S. W.; the usual order, according to the law of rotation, being exactly opposite, *i. e.* S. W., W., N. W.

We thus see that the rotation of the earth on its axis causes three different phenomena:—1. The constant direction of the trade winds, and the regular alternation of the monsoons. 2. The regular order in the change of direction of the wind, which in both hemispheres is with the sun. 3. The rotatory movement of storms in a determinate order.

The course of the phenomena which we have been considering becomes very much complicated when the advancing storm meets another wind, or when it has successively to press aside currents of air from different directions. In treating of the minimum of February 2nd and 3rd, 1823, (Pogg. *Annal.*, Vol. XIII,) I have considered in detail a case of this kind, in which a north wind blowing directly against the southwest current, the meeting of the two produced a calm, which appeared to bear no sort of relation to the alteration of the atmospheric pressure. The minimum would seem to be divided into two portions by the current flowing towards its centre, so that there are two places of least pressure. The elucidation of these phenomena requires the comparison of observations made over a very extended surface; but as the view which I then took has been since confirmed by more complete data, I refer to it here. This case leads us on to the consideration of the phenomena which follow these great agitations of the atmosphere, when the equilibrium which had been violently disturbed re-establishes itself after the disturbing cause has ceased to act; but these secondary phenomena must not be confounded with the primary ones. North of the minimum there may often be found an unusually high barometer, accompanied by severe cold and by heavy falls of snow at the limit of contact between the warm and the cold air. The falls of snow do not enter far within the precincts of the cold, but rather form a border along its limits. As long as the minimum repels the cold air, and causes it to accumulate, the falls of snow, succeeded by thaw, recede likewise towards the north; but when the polar current forces its way underneath, the falls of snow are immediately followed by fresh cold advancing from N. E. to S. W.

When navigators are overtaken by a rotatory storm, the following are practical rules for escaping from its influence as soon as possible:—

1. In the northern temperate zone:—If the gale begin from the S. E. and veer by S. to W., the ship should steer to the S. E., for she is on the southeastern part of the storm. If, on the contrary, it begin from the N. E. and change through N. to N. W., the vessel should steer northwestward, for she is in the northwestern half of the storm.

2. In the northern part of the torrid zone:—If the storm set in from the N. E. and the wind change through E. to S. E., the

ship should steer N. E., for she is in the northeastern part of the storm; if it begin from the N. W. and change by W. to S. W., steer towards the S. W., for the vessel is on the southwestern side of the storm.

3. In the southern part of the torrid zone:—If the wind set in from the S. E. and alter by S. to S. W., the ship must steer N. W., for she is on the northwestern side of the storm; but if the gale begin from E. and pass through N. to N. W., steer S. E., for the vessel is on the southeastern side of the storm.

4. In the southern temperate zone:—If the gale set in from the N. E. and veer by N. to N. W., steer towards the N. E.; but if it begin from the S. E. and change through S. towards S. W., steer to the S. W., for in the first case the ship is on the northeastern, and in the second on the southwestern side of the storm.

If our examination of storms of the temperate zone has pointed to the hurricanes of the tropics as their source, we do not therefore conclude that causes originating these phenomena may not also exist in the middle latitudes. The violent tempests of the Black Sea and the Levant, which usually mark the beginning of the rainy season in those regions, and which, on that account, are called "Temporales," appear to owe their intensity to local conditions. But we possess no detailed account of the direction in which these storms move, and of the order of change in the indications of the wind vane during their prevalence. The absence of such information in regard to seas so much frequented is a remarkable circumstance, and is much to be regretted.

[M. Dove concludes his memoir with a very interesting description of the effects of the storm of the 10th of October, 1780, which it seems unnecessary to repeat to the English reader who has access to Colonel Reid's work, from which it is taken.]

Art. IX.—*Notice of the discovery of a New Locality of the "Infusorial Stratum;"* by M. Tuomey.

Since the brilliant discovery by Prof. Rogers, of the "infusorial stratum" at Richmond and on the Rappahannock, the attention of students of geology must have been directed with new interest to the beds of clay which form so conspicuous a feature in our tertiary deposits. And although in the present state of our knowledge, it may be impossible to determine the precise value of the characters to be derived from deposits of microscopic fossils, yet whatever that value may be, it must increase with every discovery tending to show the extension and cotemporaneous character of such deposits.

The existence of the "infusorial stratum" at Petersburg, taken in connexion with the discovery mentioned above, proves that it is no partial deposit, but one that may be looked for over our widely extended tertiary. Within the precincts of Petersburg, on Poplar Lawn, the western edge of the eocene is cut through and exposed in a small stream which crosses Walnut Street. For about the distance of one quarter of a mile southeast from this point, the eocene is again hid by what are called the "Heights," being immense diluvial beds which cap the high grounds around Petersburg. These beds are again cut through by a stream which forms the boundary of the corporation; and on the west side of the valley of this stream, the infusorial stratum occurs. During the summer I frequently visited this interesting locality, but was deterred from making a closer examination, by looking in vain for the very low specific gravity so characteristic of specimens from the Richmond stratum, which is found only in specimens taken from the middle of the stratum; and in a similar manner, doubtless, others have been misled. A recent visit to the Richmond deposit, satisfied me, as far as relative position and other external characters could, that the two deposits were identical. I was now encouraged to attempt, for the first time, a microscopic examination of the Petersburg stratum, and had the satisfaction to detect a few of the forms figured in this Journal, Vol. XLII, No. 1. To remove every doubt, a few grains were enclosed in a letter to Prof. Bailey, West Point, who politely writes, that he "found without difficulty, a number of the most characteristic microscopic fossils of the Richmond bed."

Prof. Rogers, I believe, has not referred the Richmond deposit to either division of the tertiary. I think it will prove to be a lower stratum of the miocene, at least such is the fact with regard to the Petersburg deposit, as may be seen by the casts of Pectens and other miocene fossils, which separate it from the eocene, whilst at Richmond similar casts are found between the top of the stratum and the overlying diluvium. The Petersburg stratum is not so conspicuous an object as that of Richmond, owing to the washing down of the diluvium; still it can be seen to advantage at various points of the locality described. The principal exposure presents a section of about fifty feet in height, which in the ascending order will stand thus,—eight feet dark green sand; twelve feet mottled sand and clay, both eocene, upper surface very irregular and covered by the usual thin stratum of dark colored pebbles; one foot white compact sand and clay containing casts of Pectens and other fossils, and passing into the infusorial stratum, which is thirty feet.

A more particular description will appear in a notice which I am preparing, of some of the localities around Petersburg, which may serve as a guide to geologists visiting this interesting portion of the tertiary formation of Virginia.

Petersburg, Va., Dec. 21st, 1842.

Art. X.—*Letter from Richard Owen, Esq., F. R. S., F. G. S., &c. &c., on Dr. Harlan's Notice of New Fossil Mammalia*—published in this Journal, Vol. xliii, p. 141.

[To the Editors of the American Journal of Science.]

London, Dec. 10th, 1842.*

Gentlemen—Permit me to state, in reference to some observations in an interesting notice of some new fossil mammalia, inserted in your valuable Journal, (Vol. xliii, p. 141,) by my esteemed friend, Dr. R. Harlan, that my genus *Mylodon* was not founded on the Doctor's description of the *Megalonyx laqueatus*, in his Medical and Physical Researches, (p. 319—332,) nor does it include that species, which is a true *Megalonyx.*

Dr. Harlan appears to have been led into the error by an article in the Penny Cyclopædia, which he attributes to me. In justice to the accomplished naturalists who contribute the zoological articles in that work, I cannot permit myself to retain the credit of writings which are not mine, but which any zoologist might be proud to acknowledge. The names of the contributors have been published by the society, to whom we owe the diffusion of the useful knowledge contained in the Penny Cyclopædia. I have never contributed any article to that work.

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* Received January 27th, 1843.

The genus *Mylodon* is characterized in the "Fossil Mammalia, of the Zoology of the Voyage of the Beagle," 4to. No. 3, 1839, pp. 68—72. In the preceding pages, Dr. Harlan's observations on his *Megalonyx laqueatus* (*Aulaxodon* or *Pleurodon*) are quoted in illustration of the genus *Megalonyx*, and I conclude by stating, "with reference to the Pleurodon of Dr. Harlan, after a detailed comparison of the cast of the tooth on which that genus is mainly founded, with the descriptions and figures of the tooth of the *Megalonyx Jeffersonii*, Desm., in the 'Ossemens Fossiles,' they seem to differ in so slight a degree as to warrant only a specific distinction."—p. 66.

The fossil described by Dr. Harlan, which belongs to the genus *Mylodon*, is the subject of the article, p. 334, succeeding that on the *Pleurodon* or *Aulaxodon*, in the "Medical and Physical Researches." It is the portion of the lower jaw in the possession of Mr. Graves, of New York. This jaw does not, in my opinion, belong to the *Megalonyx laqueatus*, but to a different genus of Megatherioid animals founded by me on fossils discovered by Mr. Darwin in South America.

If the *Megalonyx laqueatus* should hereafter prove—as Dr. Harlan thinks the structure of the tooth and knee-joint renders not improbable—to have a claim to generic distinction, the terms *Aulaxodon* or *Pleurodon* may not be inappropriate. To my genus Mylodon, they are decidedly inappropriate, since six out of the eighteen teeth, or one third of the entire dental series, are neither grooved nor fluted.

The sense which I have attached to the term *Mylodon*, is a mammal with molar teeth only. It may be objected, that canine teeth and incisors are absent likewise in other genera of Megatherioid animals. But the accepted term *Megalonyx* is open to the same objection, for the animal so called had not relatively larger claws than the *Megatherium*, *Mylodon*, or even the existing *Bradypus* and *Cholœpus*. Other Megatherioids besides *Scelidotherium*, are remarkable for the colossal proportions of their hind-legs. There are also quadrupeds as large as the *Megatherium*, and even larger than it. If Dr. Harlan's conjecture as to the generic distinction of his *Megalonyx laqueatus* prove correct, the same objection might be raised to his proposed names, *Aulaxodon* or *Pleurodon*, which would equally apply to many more mammalian genera, both recent and fossil, than would my term

Mylodon. Every experienced naturalist has felt the difficulty of obtaining an exclusively descriptive name for a new genus, and has acknowledged virtually the necessity of being content with a partially descriptive one, if it be euphonious and not sesquipedalian. I must however here remark, that the practice of proposing a name for an undiscovered future probable or possible genus is questionable, and certainly hazardous. It is true, that the danger of the inapplicability of such name will be diminished in the ratio of the extent of choice submitted by the anticipater to the future discoverer. Dr. Harlan has left but two names for the choice of the discoverer of the generic characters of his *Megalonyx laqueatus*, and, as only a single tooth of that species has been described, the names *Aulaxodon* and *Pleurodon* may prove as inapplicable to the entire dentition, as they would have been if either of them had been transferred from their position as synonyms of *Megalonyx laqueatus*, to the distinct genus *Mylodon.* I may however state, that the additional experience which I have had of the fossil remains of Megatherioid Edentata, has confirmed my belief, that the *Megalonyx laqueatus* of Dr. Harlan is a true *Megalonyx*, nor can I perceive any reasonable ground for its specific distinction from the *Megalonyx Jeffersonii.* The broken tooth (Plate XIII, figs. 7, 8, 9, Medical and Physical Researches) affords none, since its relative length, as entire, to its transverse or antero-posterior diameters cannot be known. The *Megalonyx* agrees with the *Megatherium* and differs from the *Mylodon*, in the greater relative length of the humerus, in the greater degree of convexity of the head of the bone, in the greater convexity of the articular surface for the ulna; the *Megalonyx* differs from both *Megatherium* and *Mylodon*, in the perforation of the inner condyle of its humerus. The Megalonyx agrees with Megatherium and differs from *Mylodon*, in the convexity of the proximal articulation of the tibia presented to the external condyle: it differs from both Megatherium and Megalonyx, in having both the condyloid articular surfaces of the femur distinct from the rotular surface.

So well marked is the distinction of *Mylodon* from *Megalonyx*, whether the bones of the latter genus be described under the names *Jeffersonii* or *laqueatus*, that the generic characters, first detected by me in the lower jaw of the *Mylodon*, are repeated in most of the bones of the skeleton. They have consequently been recognized by Dr. Harlan himself, when presented to his notice in the Edentate fossil, in the Missourian collection of Mr. Koch; but the Dr. having referred the lower jaw in the possession of Mr. Graves of New York, to the *Megalonyx laqueatus*, failed to detect the specific identity of the broken lower jaws of the Edentate animal in Mr. Koch's collection, with the New York specimen; and not having been aware that I had separated Mr. Graves' fossil generically from the *Megalonyx laqueatus*, Dr. Harlan, perceiving that the Missourian Edentate fossils were not referable to *Megalonyx*, conceives them to belong to an undescribed genus.

I have carefully examined these Edentate remains, brought by Mr. Koch from Benton County, Missouri. They belong to the same species of *Mylodon* as the lower jaw in the possession of Mr. Graves, in New York; and the name *Orycterotherium Missouriense*, must therefore sink into a synonym of *Mylodon Harlani*.

The humerus and tooth from the Oregon Territory, figured and described by Dr. H. C. Perkins, in Vol. XLII, of your valuable Journal, belong to the *Mylodon Harlani*.*

In the best preserved portion of the lower jaw of this species, in Mr. Koch's collection, there was evidence of four teeth, and no more, on each side. The first tooth resembled that in the *Mylodon Darwinii*, having a simple elliptical transverse section, and being neither ribbed nor fluted. The empty socket in the lower jaw at New York, erroneously referred by Dr. Harlan to his *Megalonyx laqueatus*, indicates the tooth to have had a similar simple form; the remaining three teeth in that jaw are identical in configuration and size, with those of the so called *Orycterotherium*. They resemble in structure the teeth of the Sloth and Megatherium, and widely differ from those of the Orycteropus. The humerus of the Missourian *Mylodon Harlani*, like that of the South American species of *Mylodon*, resembles the humerus of the *Megatherium* and *Bradypus tridactylus* in being imperforate, whilst that of the *Orycteropus* is perforated at the inner condyle.

The ungual phalanges or claw-bones of the Missouri *Mylodon*, retained more or less perfect traces of the osseous sheath con-

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* Mr. Owen of course had not seen Dr. Harlan's description and figures of the Missouri fossil bones, (*Orycterotherium Missouriense*,) contained in this volume, p. 69. The number containing that article was not published when Mr. Owen's letter was written.—Eds.

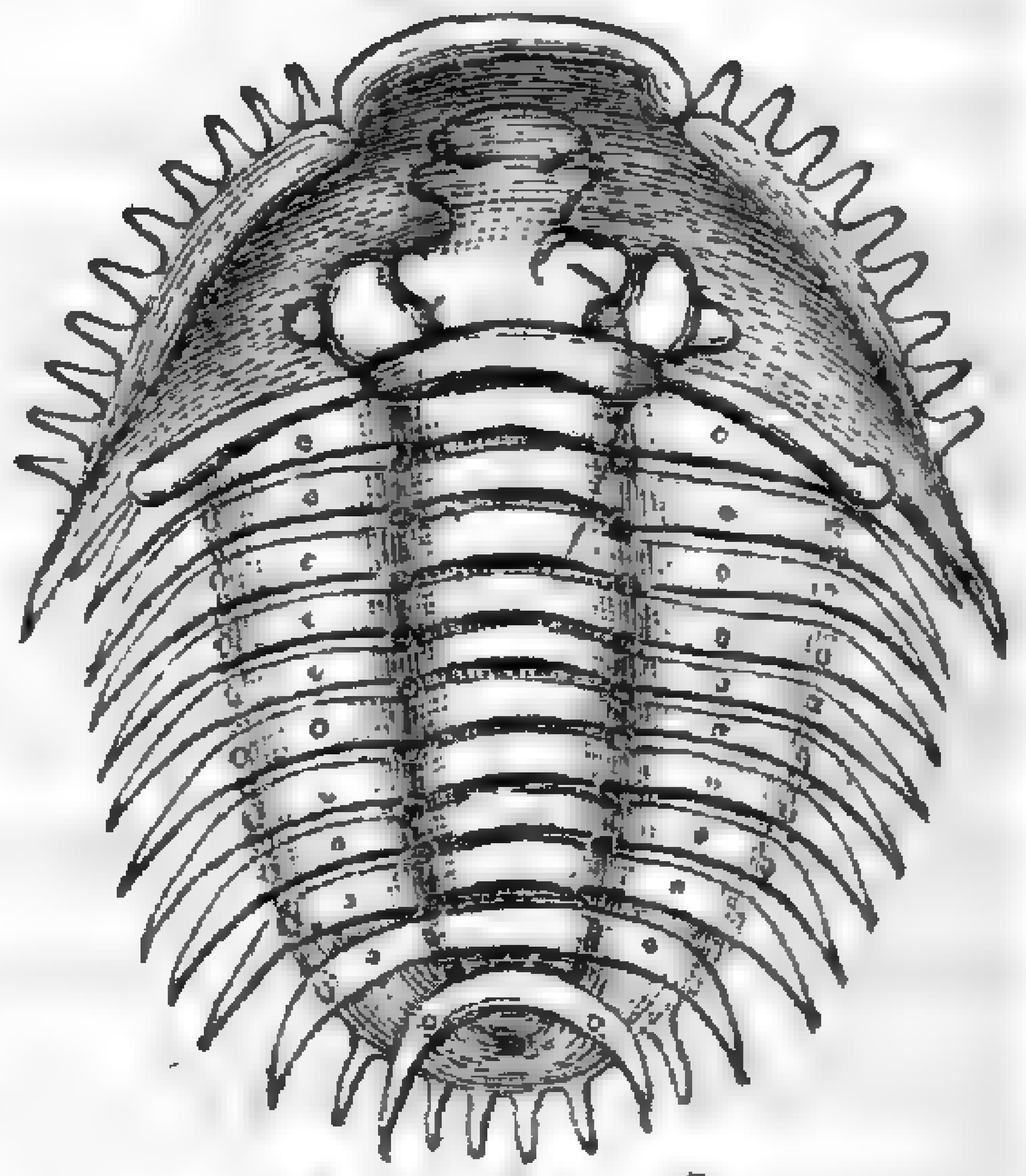
tinued from the upper and lateral parts of the base of the phalanx, as in the *Megatherium* and *Megalonyx*, but were less compressed, and presented the proportions of the claw-bone of the *Scelidotherium*. (Fossil Mammalia of the Beagle, p. 97, pl. 27.) The ungual phalanges of the *Orycteropus* have no such osseous sheath.

The tibia of the Missouri Mylodon corresponds with that of the South American species of Mylodon, and with that of the Megatherium, in the deep ovoid depression at the anterior and internal part of the lower articular end, which therefore is not peculiar to the Missouri *Mylodon*, although it forms a well marked distinction between it and the *Orycteropus*. The astragalus of the *Scelidotherium*, which has the same convex protuberance from the inner and fore part of its upper surface as that of the *Megatherium* and *Mylodon*, must, therefore, have governed the same excavation of the distal end of the tibia. The Megatherioid family thus appears to have been as strikingly distinguished by this structure of the ankle-joint, as the Sloth's are by the pivoted articulation of the astragalus with the fibula.

The opportunity afforded me by Mr. Koch, of examining the remains of the *Mylodon Harlani*, discovered by him in Benton County, and which species I had before known only by Dr. Harlan's figures, Pl. XV, figs. 2, 3, 4, in the valuable work already quoted, and by a drawing of the cast of the original specimen transmitted to me by Mr. Laurillard from Paris, fully confirmed the propriety of dissociating it from the *Megalonyx laqueatus* or *Pleurodon* of Dr. Harlan, and established, by the dental characters and those of the ankle-joint, the essential affinities of the genus *Mylodon* to the Megatherian family. In my report on the Missourium, printed in the Proceedings of the Geological Society, Dr. Harlan will find that I have duly acknowledged the originator of the opinion, that 'the Tetracaulodon was nothing but the young of the gigantic Mastodon.' To the excellent man and naturalist, Mr. Wm. Cooper of New York, the honor of this insight belongs. I have had the same pleasure in acknowledging the ingenious observations on the mastodontal fossils in Mr. Koch's collection, published in the Proceedings of the American Philosophical Society, for October, 1841.

I have the honor to remain, gentlemen, with much esteem, your obedient servant, RICHARD OWEN.

Art. XI.—*Notice of a new Trilobite, Ceraurus crosotus;* by John Locke, M. D., Professor of Chemistry and Pharmacy in the Medical College of Ohio.



Messrs. Silliman—I enclose to you a drawing of a new species of trilobite, evidently of the genus Ceraurus of Green. It is one of the smallest, and at the same time one of the most elegant of this family of extinct crustaceans; this drawing being magnified three times in linear dimensions. Fragments of this species have been repeatedly found in the rocks of this vicinity, especially the fringed margin of the shield; but it was not until last summer that I procured a specimen so nearly entire as to determine its generic relations. When Dr. Green established the genus of Ceraurus, it consisted of only one species, the *Pleurexanthemus.* But now that other species very closely allied to that are found, the justness of his discrimination is very apparent. I have named this new species *crosotus*, from the Greek word, signifying fringed. Dr. Green's description of his species—"Clypeo, postice arcuato, angulo externo in mucronem valde producto, occulis minimis remotis, post abdomine in spinam arcuatam utrinque extenso,"—applies quite well to the *crosotus;* but this last differs from the former in having the shield pectinate or fringed anteriorly. The spines of the shield and of the several ribs are more nearly straight. Besides the spines terminating the ribs, there are six slender teeth, similar to those of the anterior fringe, attached, not to ribs, but to the terminal margin of the tail, four of them between the two last costal spines, at *a*, and the other two outside or anterior to the same, at *b*. Each of the costal arches is marked by *two* tubercles or "pimples," (one in the other species,) one on its middle, and the other at the commencement of the free spine in which each costal arch terminates. These tubercles form four rows or lines down the body, two on each lateral lobe, the inner one being in the direction of the distant eyes.

Cincinnati, Ohio, Dec. 9th, 1842.

ART. XII.—*Abstract of a Meteorological Journal, for the year 1842, kept at Marietta, Ohio,* Lat. 39° 25′ N., Lon. 4° 28′ W. of Washington City; by S. P. HILDRETH, M. D.

Months.	Thermometer.			Fair days.	Cloudy days.	Rain and melted snow.		Prevailing winds.	Barometer.		
	Mean temperature.	Maximum.	Minimum.			Inches.	100ths.		Maximum.	Minimum.	Range.
January,	36.33	66	10	21	10	2	42	S. W., N.	29.90	28.98	.92
February,	37.33	71	6	13	15	3	12	S. W., S., S. E.	29.75	28.75	1.00
March,	52.00	85	36	14	17	2	66	S. W., W.	29.80	28.85	.95
April,	59.00	90	25	18	12	3	04	S. W., N., S. E.	29.73	29.00	.73
May,	60.33	80	33	19	12	4	21	N. N. W., S. W.	29.62	28.80	.82
June,	66.52	84	43	18	12	7	30	S. W., N., S. E.	29.63	29.20	.43
July,	69.90	90	50	23	8	2	37	N., S. W.	29.65	29.25	.40
August,	65.93	85	43	17	14	6	06	S. W., S. E.	29.70	29.25	.45
September,	64.24	89	35	22	8	3	21	S. S. W., N., S. E.	29.68	29.18	.50
October,	52.00	78	26	24	7	1	58	S. W., E., S. E.	29.65	29.05	.60
November,	37.00	72	11	15	15	3	04	S. W., E., S. E.	29.68	28.80	.88
December,	33.33	59	5	11	20	3	04	W., S. S. W.	29.82	28.90	.92
Mean,	52.83			215	150	42.07					

Remarks on the year 1842.—The year which has just closed has differed in some respects, in the distribution of heat and moisture, from many which have preceded it; although nothing very extraordinary has transpired, yet there is sufficient to merit our notice. It was rather remarkable for the mildness of the winter, the warmth of the spring, and the coolness of summer.

The mean temperature for the winter months is 36°.66, which is more than four degrees warmer than that of 1841. The quantity of snow which fell was very small, amounting to only a few inches. The ice in the Ohio River barely obstructed the steamboats for a few days, so that the navigation of that stream may be said to have been free during the whole winter. The cold was so slight that no ice was formed of sufficient thickness to put up for summer use in ice-houses; and the little which was stored away was taken up afloat in the Ohio in February, having come down from the Allegheny River. At Cincinnati the main supply of ice was brought from the Upper Mississippi, where it formed of a convenient thickness for this purpose. The want of this refreshing luxury was sensibly felt during the summer months by the towns bordering the Ohio, and by those who travelled in steamboats. It is quite a number of years since any failure of this kind has taken place in Ohio. The mercury at no time sunk lower than to 6° above zero, on the 17th of February; and on no other day during the winter was it below 10°. There were but a few days when it was below the freezing point.

The mean temperature for the spring months was 57°.11, which is nearly eight degrees above that of 1841. The temperature in March was 52°, and in April 59°. The latter month is usually considered to be of the same temperature as the mean for the year, which is between 52° and 53°, but this year it rose to seven above. March was 10° warmer than in 1841, and April was 12°.44—a very uncommon degree of heat for this climate. For several days in March the mercury rose to 85°, and in April it reached to 90°. The effect of this uncommon heat so early in the season was very apparent on vegetation, and the blossoming of fruit trees was accelerated by nearly a month over common years. Pastures, meadows, and gardens, put forth such a rapid growth, as to have by the 20th of February the freshness and vivid green of April. On the 11th of March the Hepatica triloba was in full blossom, although located in a northern exposure; on the 18th, crown imperial and early hyacinth; 19th, Aronia botryapium or June berry—peach tree also in blossom; 21st, imperial gage and pear tree; 27th, Sanguinaria Canadensis; 30th, Anona glabra or papaw. This fruit-bearing shrub does not usually blossom until more than a month after this period, and oftener as late as the fore part of May than before that time. It is very cautious in putting forth, and does not generally until all danger from frosts is past. This I have noticed to be the case with a large portion of our native fruit-bearing trees, and it must be an uncommon late frost which destroys their fruit. Some of them are remarkably shy in this respect, especially the chestnut, which does not blossom till late in June or early in July. The Acacia robinia is also another tree which is very cautious in putting out either leaves or blossoms till all hazard from spring frosts is past. Very different in this respect is the habit of our exotic fruit-bearing trees; in this climate they almost invariably put out their blossoms so early as to place the fruit annually in danger of destruction from frosts. This is more especially the fact on the banks of the Ohio River, and along the southerly slope of the valley of the Ohio; while the southerly portion of the valley of Lake Erie, within this state, sloping as it does to the north, is nearly exempt from frosts after the blooming of fruit trees, so that the border along this lake is the most certain fruit-producing district in the west. The trees here are not only later in blooming, but the northerly and westerly breezes, which bring our late frosts, being elevated in temperature by their passage across this great body of water, no doubt conduce largely to this exemption. No portion of the United States is better suited to the growth of fruit than the southerly shore of Lake Erie, to the distance of fifteen or twenty miles inland.

On the first and second mornings of April there was a smart frost, which destroyed the larger portion of the pears. The blossoms of this fruit stand out on such a long foot-stalk, that they are more liable to be killed by frosts than almost any other; the peach and apple, sitting nearer to the parent stem, are much less exposed, and often escape when the pear is destroyed. April 2d, apple in bloom; 3d, Circis Canadensis or red bud; 5th, Cornus florida or dog-wood—the 16th of this month is the earliest I ever saw it in bloom before, and that was many years ago; 6th, beech tree, Fagus ferruginea; 10th, apple shedding its blossoms. In common years we are favored with the sight and smell of this delicious blossom for two or three weeks, but the excessive heat of this year rapidly accelerated their decay as well as that of many other flowers. 17th, tulip in full bloom; 18th, Pæonia moutan or tree peony; 19th, Juglans cinerea or butternut; 25th, yellow rose and hedgehog rose; 29th, a hard frost. May 4th, pecan tree in blossom; 6th, Robinia pseudacacia. This tree usually puts out near the last of this month, but being of a slow, backward nature, was not hurried so much prematurely as many other trees by the early warmth of spring: last year this tree bloomed the 28th of May. 8th, Prunus Virginianus and Rubus villosus; 24th, peas fit for the table; 28th, strawberries ripe. June 15th, red Antwerp raspberry; 18th, cucumbers, grown in the open ground, large enough to eat; 22d, dahlia in blossom. July 4th, early Chandler apple ripe. With all this precocious putting forth of blossoms, the ripening of fruits was not any earlier than in common years.

The mean temperature of the summer months was 67°.45, which is five degrees below that of 1841, and considerably below the mean average of former years. The excessive heat of the early spring months seems to have exhausted the vigor of the following summer, by spending in youth the means which are provided for middle age. The late spring and all the summer months were remarkable for frosts, there being not one, even August, in which this event did not take place. It is true they were generally light, doing but little damage, yet showing an uncommon deficiency of free caloric in the air and earth. August, which is usually one of the driest months in the year, was this season very wet, there falling more than six inches of rain, and during the three summer months nearly sixteen inches—an amount quite unusual for this climate. This excessive moisture no doubt had a decided influence on the temperature of the summer. The winds were also from the northerly and easterly quarters, rather more than usual. The cool state of the atmosphere had a perceptible effect on the growth and ripening of fruits, especially those which require a high temperature for their well being. Grapes were very slow in maturing, and ripened poorly. The Cucurbitacea and melon crop was almost a complete failure, and all of them were deficient in that rich saccharine juice in which they abound in hot summers. On this account, peaches were not sweet, and lacked flavor. Apples and pears, requiring less heat, ripened finely, and furnished good crops where they escaped the frost; apples especially, from the rains in August, acquired an uncommon growth, and were remarkable for beauty of cuticle as well as size. Some of the *pomme-roi*, or autumn pippin, in a neighboring garden, weighed from twenty four to twenty eight ounces. The crops of grain, grass and potatoes, were very fine, especially the latter, the cool summer being well suited to their habit of growth. Wheat, which may be called the staple of Ohio, was never better, or more abundant.

The mean of the autumn months is 51°.08, which is about the medium for the climate. September, it will be seen, was nearly as warm as August. The last of November was colder than any weather we have had since. On the 17th and 18th of that month a severe storm of wind, rain and snow swept across the western country, doing great damage to the shipping on the lakes. These storms are almost annually periodical, especially on Lake Erie, sometime in November. Twenty or thirty years ago, before the business became so extensive, the navigation of that lake was closed by the first of November, and the vessels laid up in some safe harbor for the winter, no navigator deeming it prudent to venture abroad after that period.

Marietta, Jan. 14, 1843.

ART. XIII.—*Abstract of the Proceedings of the Twelfth Meeting of the British Association for the Advancement of Science. Condensed from the Report in the London Athenæum.*

(Concluded from p. 172.)

Sect. B. *Chemistry and Mineralogy.*

Dr. Playfair read an abstract of Prof. Liebig's Report on *Organic Chemistry applied to Physiology and Pathology.* We omit the summary of this communication, as Prof. L.'s highly interesting and important work has been widely circulated in this country.

Mr. Mercer communicated a paper on *some peculiar instances of the (so called) Catalytic Action.* Mr. M. had long considered that instances of catalysis were merely examples of chemical affinity, exercised under peculiar circumstances. A body never entirely yields up its chemical characters on uniting with other bodies. The iron in protoxide of iron, has still an affinity for more oxygen, and has not lost that affinity by its first union with that element. The intensity of affinity by which the simple elements are joined in the complex molecule, must be the measure of the stability of the compound. Mr. M. argued, that when the elements of a body are in mere static equilibrium by virtue of a feeble attraction, and when it is acted upon by another body possessing an affinity for one of its constituents, which constituent on the other hand, from peculiar circumstances, is not prone to combine with it, that in such a case, so called catalysis must ensue. Thus on mixing oxalic acid and nitric acid with a little water, and raising the temperature to 130°, no action ensues. But if a small portion of any protosalt of manganese be now added, the decomposition immediately commences, and all the nitric is converted into nitrous acid, whilst the oxalic acid passes into carbonic acid. He thus accounts for this singular action :—The carbonic oxide of the oxalic acid has a disposition to unite with oxygen; to gratify this disposition, it endeavors to withdraw it from nitric acid, but it is not sufficiently powerful to do so; still it places the atoms of the nitric acid in a state of tension. Another body (protoxide of manganese) now being introduced, which also has an affinity for oxygen, exerts this affinity, and the combined forces thus acting upon the nitric acid, occasion its decomposition. The moment the oxygen is withdrawn from its state of combination, it has two affinities to choose between, and the attraction of the oxalic acid being greater, it passes over to it, converting it into carbonic acid. The protoxide of manganese still remaining, will act on fresh portions *ad infinitum.* Most of the vegetable acids may be decomposed in a similar manner.

Mr. R. Hunt communicated a paper entitled "*Researches on the Influence of Light on the Germination of Seeds and the growth of Plants.* The subject had been intrusted to Mr. Hunt for experiment. He had provided six boxes, so constructed, that no light could enter except through glass, of different colors,—the first being deep red, the last deep green. In these boxes he had raised ranunculi, tulips, and other plants. The tulips he found germinated, the first under the orange-glasses, and last under the blue and green. Under the blue glass, the plants, although slower in germination, were more healthy, and promised to come to maturity, and be perfect flowers; while under the orange they were more forward but sickly. A curious result was noticed with respect to the red glass. Under all other circumstances, plants bent towards the light, but those under the red glass bent away from the light. In nearly all cases germination had been prevented by the absorptive power of the yellow rays.

On the agricultural importance of ascertaining the minute portions of matter derived from organic sources that may be preserved in the surface soil, and on the chemical means by which its presence may be detected; by Dr. Daubeny.—The researches of Sprengel and Liebig, by showing the manner in which minute quantities of certain ingredients may impart to the soil into which they enter as constituents, entirely new properties with reference to the purposes of agriculture, have given additional interest to the methods of analysis, which aim at determining the chemical composition of the surface, and of the substratum from which the former principally derives its chief ingredients. The rude mechanical method adopted, even by such chemists as Sir H. Davy, is no longer considered sufficient. The nature, as well as the amount of the organic matter present, and the existence of phosphates, &c. in the proportion of one thousandth or even one ten thousandth part of the entire mass, are points deserving investigation, and afford a clue to the description of manures most likely to be useful, and to the general treatment which the land may require. It is also obvious, that the same importance attaches to a knowledge of the constitution of the subsoil, since the advantages of exposing to atmospheric influences, and thus disintegrating the portions underneath by deep ploughing, and other methods of bringing the subsoil to the surface, will in a great degree depend upon its containing ingredients which the crop requires for its subsistence, and of which the superficial soil has been already in a great degree exhausted. Thus, for example, it will often become a question with the farmer, whether it will be more economical to mix with the soil a given quantity of phosphate of lime, or to incur the labor of so breaking up a portion of the subjacent rock, as to unlock, as it were, for the use of the crop, that quantity which it contains in close union with its other constituents. This inquiry, however, presupposes a knowledge on his part of the existence of phosphate of lime in the soil, and of the relative proportion it bears to the other ingredients,—data, which can be obtained only through the assistance of refined chemical analysis. A few simple and easy calculations may show how very small a proportion of this ingredient might suffice during a long period of time, for the demands even of those crops which require the largest amount of it for their nutrition. Suppose the subsoil of a single acre of ground, turned up to the depth of a foot, to weigh one thousand tons: now if this rock should be found to contain only a thousandth part of phosphate of lime, it will follow that no less than a ton of this substance might be extracted from the uppermost foot of the subjacent rock, by the action of the elements, or by chemical means. Now one ton of phosphate of lime would be adequate to supply one hundred and twenty five tons of wheat, or six hundred and eighty tons of turnips. And if we reckon the average crop obtained from an acre of land to be, of wheat one ton, and of turnips, fifteen, it is evident that we have at hand as much phosphate of lime as would be necessary for one hundred and twenty five crops of the former, or for forty five crops of the latter. Dr. Daubeny said he had great reason to believe, that many of our secondary rocks, those especially which contain organic remains, and which appear in a great measure to be made up of shells, would be found, if examined, to contain as large a quantity of phosphate of lime as that mentioned. Though the soil of Great Britain be found deficient in the phosphates, there is reason to believe the subsoil might in many cases be made, by proper management, to impart to it what was wanting. It is now some years since the discovery, by Mr. Buckland, in the lias and other secondary rocks, of the solid fæces of certain extinct animals, consisting of phosphate of lime, induced Dr. D. to test a variety of specimens of limestone, with a view of ascertaining whether traces of the same ingredient might be found in them. The result was that phosphate of lime in minute quantities was much too commonly distributed to be attributed to coprolitic matter, or to afford any independent evidence of its presence. When indeed we recollect that the shells of invertebral animals contain from three to six per cent. of phosphate of lime, and that according to Mr. Connel, the scales of extinct fish, taken from rocks as old as the coal formation, possess no less than fifty per cent. of the same ingredient, it would be wonderful, indeed, if all traces of this substance had disappeared from rocks, which appear often to be made up in a great degree of the debris of shells and other marine exuviæ. Dr. Daubeny was therefore not surprised at being informed by M. Schweitzer, who is entrusted with the management of the German Spa at Brighton, that he had detected in the chalk of Brighton Downs, as much as a thousandth part of phosphate of lime. From experiments since made by Dr. D. in the same rock, taken from various localities, he was inclined to believe that minute portions of this substance are present not uncommonly in that formation. The frequent occurrence of phosphate of lime in calcareous rocks, and the probability of its being derived from the shells, or bony matter of the living beings contained in the calcareous rock, led Dr. Daubeny to suspect that traces also of the organic matter which contributed to make up the animal structure, might likewise be found accompanying it. To determine this, the Dr. had applied a test to about fifty different specimens of limestone selected from his cabinet, and found, that whilst the solutions of the pure marble, such as that of Carrara, continue unaffected, the equally pure and white limestones taken from the chalk and tertiary limestones in general become distinctly darkened by the addition of nitrate of silver.—Dr. D. read a letter from M. Schweitzer, who had been precluded from employing the secondary limestones in obtaining carbonic acid wherewith to impregnate his mineral waters, owing to an empyreumatical odor which the gas carried up, and which he attributed to an organic cause. To obtain a perfectly pure carbonic acid, for his imitation of the Spas of the continent, he was compelled to resort to the pure kinds of marbles. With regard to the presence of organic matter in the subsoil, its detection may be matter of some agricultural interest, when we remember that the small quantities of nitrogen which are required for the growth of those vegetables that first start up in a new country could not have taken place from an accumulation of mould, by the decay of antecedent plants, but must have been derived in a great measure from the animal matter which is contained in the rock upon which they grew, and which proceeds from the exuviæ of races of beings belonging to a former period of creation. In a more advanced period of vegetation, this same material may be of some value to the crops that occupy the soil. Dr. D. suggested whether the more compact texture of certain calcareous rocks than of others, might not be connected with the existence in them of organic matter, which by its interposition may prevent a crystalline arrangement of its particles from taking place. It may be that the attraction between the particles of matter, which if uncontrolled, would prove too powerful for the agents of decomposition to overcome, may be weakened by the presence of organic matter, and thus be enabled to supply the vegetables that take root in it with the solid matter which their structure requires. To the geologist, too, it cannot but be of interest to trace the several steps by which the organic matter which primarily must have constituted so large a portion of the bulk of the various extinct animals and vegetables have disappeared from the strata which enveloped them.

Mr. Mallet read a paper on the *action of Air and Water on Iron.* This is the third report for which the Association is indebted to Mr. Mallet. The object of former tabulated results was to determine the actual loss by corrosion in a given time, and the comparative durability of rust of the principal *makes* of cast iron of Great Britain, and to discover on what durability depended. The tables of experiments now presented show, that the rate of corrosion is a decreasing one in most cases; and that the rapidity of the corrosion in cast iron is not so much dependent upon the chemical constitution of the metal, as upon its state of crystalline arrangement, and the condition of its constituent carbon. The present Report, too, extends the inquiry to wrought iron and steel, of which between thirty and forty varieties have been submitted to experiment. The results show that the rate of corrosion of wrought iron is in general much more rapid than that of cast iron or of steel. The finer the wrought iron is, and the more perfectly uniform in texture, the slower and more uniform is its corrosion. Steel corrodes in general more slowly, and much more uniformly, than wrought or cast iron. The results of the action of air and water in the several classes of iron have been examined and chemically determined. The substance spoken of as plumbago was next described. It is produced by the action of air and water on cast steel, especially that in the raw ingot, in the same way as it is in the case of cast iron. A quantity of plumbago, found in the wreck of the *Royal George*, absorbed oxygen on exposure to the air with such rapidity, that it became nearly red hot. Mr. Mallet next described a method of protecting iron by a modification of the zinc process. It was found impossible to cover the surface of iron with zinc, to which it had no affinity. The first process was to clean the surface of the iron, taking off the coat of oxide, and then immersing it in double chloride of zinc and ammonium, which covered it with a thin film of hydrogen, by which its affinity for the zinc is much increased. The iron was then covered with a triple alloy of zinc, sodium, and mercury. Mr. Mallet produced several specimens of his alloy, one of a bolt to be driven into a ship's side, and another a cannon shot covered with his preparation, and exposed to the weather on the roof of a building, and which was perfectly preserved. Cannon balls were so much oxidized by exposure to atmospheric influences, that in five or six years they became useless. The French Institute had been engaged in experiments to protect these, and had tried zinc, but had been compelled to abandon it. Mr. Mallet also brought under the notice of the Section a method of preventing the fouling which takes place on the bottoms of iron ships, especially in tropical climates, by means of which invention he had ascertained that plants and animals were prevented from adhering to the ship's bottom. According to Mr. Nasmyth's theory, corrosion on railways is checked by the trains passing over the rails always in one direction, and takes place when they pass in both directions. Mr. M. had made some experiments in order to determine this point, which are not yet complete; but he was inclined to think the difference between the two cases to be apparent, only, and not real. He is continuing his investigations, and hopes to report further on a future occasion.

Dr. Schunk read a paper on *Hæmatoxylin, the Coloring Principle of Logwood,* by Prof. O. L. Erdman, of Leipsic. The hæmatoxylin used by the author in his experiments, was prepared by the process of charcoal. In a state of purity, hæmatoxylin is not red; it is in itself no coloring matter, being merely a substance capable of producing coloring matters in a manner similar to lecanorin, orcein or phloridzin. The colors which it produces are formed by the simultaneous action of bases, (particularly strong alkalies,) and of the oxygen of the atmosphere. By the action of these it undergoes a process of eremacausis, which after forming coloring matters, ends in the production of a brown substance resembling mould. The color of hæmatoxylin varies from a pale reddish-yellow to a pale honey color. The crystals are transparent, have a strong lustre, and may be obtained a few lines in length. Their form is a rectangular four-sided prism, sometimes with a pyramidal summit. The taste of hæmatoxylin is similar to that of liquorice. With excess of ammonia, it forms what the author calls hæmatein, analogous to orcein, &c.

On an economical *Voltaic Combination of extraordinary power,* by F. W. De Moleyns, Esq.—The author stated that while the discoveries in electro-magnetism gave promise of its ultimate application as a motive power far surpassing steam, it was matter of much importance to discover a mode of charging or giving attractive power to *soft iron,* at a cost which should render it as a mechanical agent generally available. The voltaic arrangement now produced to the Section, the author believed would be found to possess in a very great degree those advantages so much desired for the proper development of electro-magnetic energy. The combination consisted of an acidulated solution of nitrate of ammonia, in contact with platina,—solution of muriate of ammonia and zinc; the nitrate solution being separated from the muriate by a diaphragm of wood, biscuit ware, or other porous substance not acted upon by the liquids. The acidulated solution was thus prepared: six ounces of nitrate of ammonia are dissolved in two fluid ounces of soft water, and this solution is then combined with an equal quantity, by measure, of the pure sulphuric acid of commerce, adding the acid gradually, the vessel containing the mixture being kept in a frigorific preparation, so as to prevent the heat evolved from exceeding 100°. The muriate of ammonia is dissolved in soft water to saturation. The zinc is *not* amalgamated, and the use of *cast* zinc is to be avoided. The platina is the thinnest foil that can be procured, but the author found that box-wood, cut to the thickness of veneer, and charred on each side *superficially*, might be substituted, and used with equal advantage. The author stated that with a voltaic combination consisting of half a fluid ounce of the acidulated nitrate solution, one ounce of the saturated solution of muriate of ammonia, a strip of platina foil three inches by two, surrounded by a piece of sheet zinc of equal surface, he had succeeded in supporting a weight of two thousand pounds with an electro-magnet of the horse-shoe form, measuring sixteen inches from pole to pole, and three fourths of an inch in diameter, and that the attractive force before contact was in proportion.

The following papers were also communicated to the Section.

On the Electrolyzing Power of a simple Voltaic Circle, by Prof. Schönbein.

On the manufacture of Sulphuric Acid, by Wm. Blyth.

Account of the Mineralogical and Geological Museum of the Imperial Mining Department of Vienna, by Prof. Haidinger.

On some new oxides of certain of the metals of the Magnesian family, by Dr. Lyon Playfair.

Account of a simplified Apparatus, for applying Circular Polarization to Chemical Inquiries, by Prof. Powell.

On the electric origin of the Heat of Combustion, by J. P. Joule.

On the causes of the irregularities of surface which are observable in certain parts of the Magnesian Limestone formations of this country, by Dr. Daubeny.

On the composition of the blood and bones of domestic animals, by Prof. Nasse of Marburg.

On the manufacture and purification of Coal Gas, by John Davies of Manchester.

On the formation of Cyanuret of Potassium in a Blast Furnace, by Dr. C. Bromeis of Cassel.

On a new product obtained from Coal Naphtha, by Mr. Leigh.

On Kakodylic Acid, and the Sulphurets of Kakodyl, by Prof. Bunsen of Marburg.

On the compounds of Carbon and Iron, by Dr. C. Bromeis.

Contributions to the history of the Magnesian Limestones, by Mr. Richardson.

On a peculiar condition of Iron, by Prof. Schönbein.

On a new method of analyzing Cast Iron and other metallic carburets, by Dr. Ure.

On the advantages and disadvantages of Hot Air, in effecting the combustion of Coal, by Mr. Williams.

On certain Thermometrical Researches, by Prof. Graham.

On the composition and characters of Caryophyllin, by Dr. Lyon Playfair.

Sect. C. *Geology and Physical Geography.*

On the Physical Structure of the Appalachian Chain, as exemplifying the laws which have regulated the elevation of great mountain chains generally; by Professors H. D. Rogers and W. B. Rogers.—The Appalachian chain of North America is described by the authors as consisting of a series of very numerous parallel ridges or anticlinal lines, forming a mountain belt generally one hundred miles in breadth, and nearly one thousand two hundred miles in length, stretching from the southeast angle of Lower Canada to Northern Alabama. 1. The strata which compose this chain are the American representatives of the Silurian, Devonian, and Carboniferous systems of Europe, united into one group of conformable deposits. The general direction of the chain being northeast and southwest, there is a remarkable predominance of southeast dips throughout its entire length, especially in the southeast or most disturbed side of the belt. Proceeding northwest, or away from the quarter of greatest disturbance, northwest dips begin to appear; at first few and very steep, afterwards frequent and gradually less inclined. 2. The authors consider the frequency of dips to the southeast or *towards* the region of intrusive rocks, accounted for by the nature of the flexures, which are not symmetric, the strata being more inclined on the northwest than on the southeast of each anticlinal, amounting at length to a complete folding under and inversion, especially on the southeast side of the chain, where the contortions are so closely packed as to present a uniform dip to the southeast. These folds gradually open out, the northwest side or inverted portion of each flexure becomes vertical, or dips abruptly to the northwest; proceeding further in this direction, the dips gradually lessen, the anticlinals and troughs becoming rounder and flatter, and the intervals between the axes constantly increasing till they entirely subside at about one hundred and fifty miles from the region of gneiss and intrusive rocks. The authors express their belief that a similar obliquity of the anticlinal axes will be found to obtain in *all* great mountain chains, their planes always dipping *towards* the region of chief disturbance. The inverted flexures are regarded by the authors as exhibiting simply a higher development of the same general conditions. The passage of inverted flexure into faults is stated to occur frequently, and invariably along the northwest side of the anticlinal or southeast of the synclinal axes; these dislocations, like the axes, maintain a remarkable parallelism. 3. The axes of the Appalachian chain are distributed in natural groups, the members of each group agreeing approximately in length, curvature, amount of flexure, and distance apart. Nine principal groups are described, in five of which the axes are straight, whilst the four which *alternate* with them are curved: in two of the curved divisions the line of strike is convex to the northwest, in the other two it is convex to the southeast. In every part of the chain the axes, whether curved or straight, maintain an approximate parallelism to those of their own division, and in the minor groups within the large divisions the parallelism is still more exact. The axes vary in length from insignificant flexures to lines frequently one hundred and sometimes one hundred and fifty miles in length, and they deviate very little from a rectilinear course, or, as the case may be, from a uniform rate of curvature. Some of the longer curved axes exhibit a difference of strike at their extremities of fifty miles in a distance of ninety miles, and the rectilinear axes of different divisions vary in their line of direction as much as 60°. As all the flexures were undoubtedly formed at one period, the authors consider these facts at variance with M. Beaumont's hypothesis, that dislocations of the same geological age are parallel to one and the same meridian. 4. The general declension in level of the Appalachian strata towards the northwest or away from the quarter of greatest local disturbance, is considered, by the authors, important in its bearing upon the subject of the elevation of broad continental tracts. The authors next proceed to notice memoirs, describing what they consider similar phenomena in Europe.

Theory of Flexure and Elevation of Strata.—From the consideration of the preceding general facts, the authors have arrived at a theory which they conceive applicable to the bending and elevation of strata generally. They state that the *oblique* form of all normal anticlinal and synclinal flexures "indicates that the force producing the dips was compounded of a wave-like oscillation and a tangential pressure;"—a purely vertical force exerted simultaneously or successively along parallel lines could only produce a series of symmetrical flexures, whilst tangential pressure, unaccompanied by a vertical force, would result in irregular contortions dependent on local irregularities in the amount of resistance. The alternate upward and downward movements necessary to enable the tangential force to bend the strata into a series of flexures, are such "as would arise from a succession of *actual waves* rolling in a given direction beneath the earth's crust." The authors observe that it would be difficult to account for the formation of grand yet simple flexures, by a repetition of feeble tangential movements, or by "a merely upward pressure, unaccompanied with pulsations on the surface of a fluid; and if this force be feeble and oft repeated, it is difficult to understand how it could return always to *the same* lines until they became conspicuous flexures." The authors suppose the strata of the region in question to have been subjected to excessive upward tension, arising from the expansion of molten matter and gaseous vapors; the tension would at length be relieved by many parallel fissures formed in succession, through which much elastic vapor would escape, and by thus removing the pressure adjacent to the lines of fracture, produce violent pulsations on the surface of the fluid below. This oscillatory movement would communicate a series of temporary flexures to the overlying crust, which would be rendered permanent by the intrusion of molten matter into the fractured strata originating the tangential force by which the flexures received their peculiar character before described. The authors do not deem it essential to this explanation, that in the production of axes of elevation, the strata should be permanently fractured to the surface. Fissures sufficient for the escape of vast bodies of elastic vapor, might open and close again superficially; and the strata may often be supported in their new position by subterranean injections not visible on the surface.

Identity of the undulations which produced the axes, with the wave-like motion of the Earth in Earthquakes.—The authors suppose all earthquakes to consist in oscillations of the earth's crust propagated with extreme rapidity; and they ascribe this movement to a sudden change of vertical pressure on the surface of an interior fluid mass, throwing it into wave-like undulations, such as would produce permanent flexures in the strata if more energetic, accompanied by the formation of dykes. The successive earthquakes of any region usually proceed from the same quarter, and this must also have been the case with the movements which gave rise to the parallelism of contiguous anticlinal lines. In illustration of the power of producing permanent lines of elevation which earthquakes have exhibited in modern times, the authors instance the Ullah Bund, an elevated mound extending fifty miles across the eastern arm of the Indus, which was the result of the great earthquake of Cutch in 1819; and another case recorded in "Darwin's Journal of Travels in South America," which a traveller described as a line of elevation of the strata, crossing a small rivulet, and shown in the fact that he found himself going down hill while ascending the dry deserted channel.

Date of the Appalachian Axes.—The authors describe the elevation of this chain as simultaneous with the termination of the carboniferous deposits of the United States, and as the cause which probably arrested the further progress of the coal formation. With one local exception on the Hudson, the whole series seems to have been deposited conformably, without any emergence of the land. That the elevation did not take place later, is shown by the undisturbed condition of the overlying beds, approximately of the age of the European New Red Sandstone. The elevation of the chief part of the great belt of metamorphic rocks on the southeast side of the chain is referred to the same great movement. In conclusion the authors remark, that an incomparably greater change in the physical geography of North America, and perhaps of the globe, seems to have occurred at the close of the Carboniferous epoch than at any previous or subsequent period; and they consider these changes, and the effect produced by them on the organic world, as affording some of the highest subjects of geological investigation.

Mr. Murchison confirmed the views given by the authors of this paper, of the great break in the series of geological deposits which occurs between the Palæozoic rocks and later deposits: the coincidence in the direction of some great chains in Europe and America, belonging to the same geological period, was very striking. He was not prepared to give any opinion upon Prof. Rogers's undulatory theory. Sir H. T. De La Beche described the general character of the anticlinal and synclinal lines, and stated, that whilst contortions of the strata sometimes assumed the character of mountain chains, at other times they occupied large tracts of low ground, as in the comparatively flat country of South Wales. He then made some observations on the space occupied by masses of rock over certain areas; the older rocks of England, if *flat-*

tened, would occupy a much greater space than at present; and the area of the Alps and Jura would be greatly extended if all their contortions were spread out. The phenomena described in the Appalachian chain, so far as small differences in the direction of the anticlinals were concerned, did not at all affect the brilliant theory proposed by M. Elie de Beaumont; the object of the geologist was to trace the correspondence in the direction of the *great lines of elevation*, and in this broad view the northeast and southwest direction of great part of the European rocks agreed remarkably with the direction of the Appalachian chain. He did not consider the pulsation of molten matter, as described by the authors of the paper, necessary to account for the flexures so very numerous in the strata of mountainous districts, but not confined to them, and in many instances unaccompanied by the intrusion of igneous rocks. The only force necessary for the production of such flexures and contortions was, the tangential or lateral pressure, in order to compress the strata into a smaller space. Contortions were formerly accounted for by a supposed secular diminution in the volume of the earth; the crust was compelled to accommodate itself to the diminished surface arising from the contraction of the mass. But it was to be remembered, that these contortions were not common to all the world; in Russia the strata presented one even bend over a wide area. Our knowledge of America, and much of the rest of the world, was imperfect, and until we were much better acquainted with the distribution and character of contorted strata all over the globe, we should not be able to account very rationally for the figures they assumed. Mr. Sedgwick pointed out those circumstances in the structure of the Appalachian chain which accorded with previous observations in Europe; the persistency of the strike of the strata, the parallelism of the anticlinal and synclinal lines, and the diminution in the amount of disturbance as the strata recede from the district where the greatest force was applied. He did not allow that the circumstance of curvilinear elevations was opposed to the theory of M. Beaumont, who had himself described curved elevations quite as striking. Most of the instances adduced by Prof. Rogers, in illustration of his view of the average inclination of the strata being greater on the side of each flexure *farthest* from the centre of the disturbing forces, did not in his opinion confirm the view the authors had taken of the origin of those contortions.

Again, Mr. Sedgwick stated, the position of the successive strata in the British chains, was not generally such as that which characterized the chain so carefully described by the authors of the paper. The effects of disturbing forces, such as the intrusion of igneous rocks, was chiefly dependent on the nature of the rocks affected. In Cumberland, the porphyritic rocks, which were evidently molten when introduced, had become hard by cooling, and had been fractured and dislocated along with the rocks among which they were intruded; but from the very nature of those rocks, they could not be thrown into many undulations. In North Wales, where the conditions differed, and the igneous rocks were less abundant, the alternating beds of solid porphyry and softer rocks were thrown into a series of anticlinal and synclinal lines; whilst in the Liege country the beds, when in a very soft and plastic state, had evidently been subjected to great lateral pressure, forcing them to assume enormous contortions, but never elevating them into mountains. The authors had, he thought, rather undervalued the power of tangential forces. These were well illustrated in the effects produced upon the soft slates of North Devon, by the intrusion of masses of granite many miles across, like that forming the forest of Dartmoor, between which and other granite masses, the strata were crumpled and thrown into innumerable undulations. He believed there was very little analogy between the phenomena produced by earthquakes and those attributed to continental elevation; the oscillations of the earth's surface produced by earthquakes were like those of a cord struck when subjected to tension: from the very nature of these vibrations, they might be propagated rapidly over a great part of the globe. The impulses of elevation, as far as any thing was known of them, were slow, acting over wide areas, and disrupting and contorting mountain masses. Nothing was more certain than that continental masses had risen and were rising in our time: Norway, for example, with curvations so slight as to be invisible. In the Southern and Pacific Ocean, Mr. Darwin had pointed out large areas rising and subsiding, some of them three thousand or four thousand miles in diameter. He stated that he was not prepared to grapple with a theory which was so imperfectly explained, and without diagrams; he only wished phenomena not to be pressed into its service, which either bore not upon it at all, or were perhaps opposed to it—namely, the phenomena of the British chains. He lastly endeavored to show how, in many cases, a reversed dip might be produced after the first protrusion of a central granitic axis. Prof. Sedgwick concluded with a merited compliment to the American nation for the elaborate surveys they had published, of which the present memoir was an example; the facts of which must, in the end, serve along with similar phenomena to form the base of a legitimate theory.

Dr. Dale Owen communicated a memoir *on the Western States of North America:* it was illustrated by maps, sections and diagrams of fossils. The grand feature of the country is the Illinois coal-field, equal in extent to all England, separated from another coal-field, that of the Ohio, by an axis of much older rocks. The object of the memoir was to identify those lower rocks with the systems which supported the carboniferous series of England.*—Mr. Phillips compared the extreme simplicity in the succession of strata and distribution of organic remains observable in these districts of North America under consideration, with the great breadth occupied in Ireland by calcareous beds of the carboniferous era, where a similar deficiency existed in the middle and superior members of the series. This particular American series was deficient in tertiary rocks; its cretaceous system was deficient in white chalk; the Neokomian beds and the oolites were all absent; the lias and new red sandstone were also deficient; and then came the coal, succeeded by limestones, sandstones and shales, and these by altered strata and granite. The analogy between the American cretaceous deposits and those of Europe was very striking; though specific differences did exist between the fossils of the two countries, yet these differences were very slight, merely marking the effect of local influence: regarded as a group, the two deposits were identical, and there could be no question of their contemporaneous deposition. Passing from the cretaceous deposits, we did not meet with the series which in Europe succeed to them, but we passed suddenly to the coal formation, without a trace of the fossils of the intervening beds; whilst the plants of the American coal measures, although they might differ specifically from those of Europe, belong to the same leading groups,—Stigmaria, Pecopteris, Neuropteris, Sigillaria, &c. In

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* This memoir was subsequently read before the Geological Society of London.

the limestone (E) under the coal, was shown the state of the sea of that period. There were the Productas, (the *P. antiquata* group,) the Lithostrations, and the Syphonophyllia, resembling those of the English and Irish carboniferous limestone, and the Pentremites sufficiently characteristic to afford a good temporary designation for the deposit. One fossil would seem to have strayed from its proper place, (the Calceola,) but as no single shell could be regarded as marking the boundary of a formation, this was not an *exception* to the law; the evidence given by a large number of forms proved these beds to be the equivalents of our carboniferous limestone. Beneath this limestone occurred a fine sandstone (D); the identity of its fossils was doubtful; it were unsafe to call them Silurian; the Spirifer represented was evidently one of that peculiar Producta-like group with transverse bars, belonging to the carboniferous limestone, and the other fossils seemed to indicate an intermediate term of life. In the lower limestone (B) a Pleurorhynchus was found,—a genus which occurred in Devonian, but not in the Silurian rocks; Pentremites were also figured as belonging to this deposit. The lowest deposits (A) forming the basis of the series, contained the usual Silurian forms, Orthidæ and Spirifera. These rocks appeared as one series of calcareous deposits, formed under circumstances less subject to fluctuation than their equivalents in Europe; and the continuity of specific forms and types of organization seemed to have been much greater than in countries where physical changes produced well-defined lines of separation in the deposits. The successive periods of deposition of strata were much better determined by organic forms than by the mineral constitution of the rocks, but in applying this principle it is impossible to be too cautious, the evidence being not of *time* but of *circumstance;* the character of the organic remains being determined by the physical conditions of the period. The order of physical changes, and the series of organic life, must be inquired into separately, and their results combined before we could be safe: he had no intention of interfering with or undervaluing investigations based on other grounds, but he believed he had presented a view which did not clash in its result with that given by investigations of another kind.

Mr. Sedgwick contended, that the expression *law* was not used in a correct sense, if by using it, we excluded the idea of laws of a higher order: it merely implied that, having grouped together a set of phenomena, we used the term to represent the state of our knowledge at any moment of time. Such were the laws of the distribution of organic remains; it was impossible to ascertain all the conditions which involved the appearance of any particular form of life; and we have never risen, nor can rise, to such laws. With respect to the identification of strata in distant countries by organic remains, in the absence of direct evidence, he considered this evidence was as strong as we could expect to obtain; having proved its correctness in this country, we applied it to more distant tracts. Assuming, in the first instance, a coincidence between the conditions and organic types of our own country, and that which we examine, if in this investigation we meet with nothing contradictory, we extend the value of our inductive process. Amongst the lower rocks of that part of America described, there was a carboniferous and pentremite limestone, an intermediate group, and a Silurian group, all bearing a remarkable analogy to those of England. The series as a whole was more calcareous, and therefore we might not expect the same tranchant differences which the alternation of masses of shale and sandstone had produced with us. An illustration of these local differences occurred in the interpolation of the calcareous beds, of which the *crumbling* colleges of Oxford were built, between the Oxford and Kimmeridge clays of the south of England. At Cambridge, these clays formed one uninterrupted deposit of mud, two thousand feet thick. In England, all the work was done; the long tiresome narrative, like an old chronicle full of enormous detail—like a book, too, some of the leaves were torn out, and others so defaced that no mortal man could read them. To supply this, we looked to other countries; and believing that nature has no starts, or blanks, seek to supply the deficiencies in our own series, by an examination of those of other countries. In reference to the economical importance of the district which formed the subject of the memoir, Mr. Sedgwick remarked, that this country possessed inexhaustible mineral treasures, and the finest inland navigation in the world, and pictured the influence it might be expected to exert, in the coming period of time, its effect on the fortunes of the civilized world, when all the intellect of the most active and energetic men on that part of the earth should be brought to bear on these treasures, and he rejoiced that men with English feeling and English blood, should be bringing them into operation.

Sir H. T. De La Beche remarked, that the principal groups of strata were separable all over the globe, and the physical conditions which produced those deposits and governed the changes of organic life must have been the same over large portions of the globe also. He assumed a similarity of condition, not perfect identity: those deposits were formed of the detritus of pre-existing rocks, and as there were not equal conditions for producing and carrying that detritus, there would be more striking deviations from the general rule in one place than another; the deposits might have no representatives at all *in time;* therefore to say that one deposit was perfectly represented by another, would be drawing conclusions without the necessary evidence. Both sections and fossils were necessary; but after all, we could only give names to represent the state of our knowledge; facts much more numerous and much stronger than those on which our divisions were founded, might compel us to alter all our names. The division of the older rocks into Carboniferous, Devonian, and Silurian, should be retained as long as possible; but the moment we attained a sufficient body of evidence we must modify our views. The different value of names in different parts of the world, rendered it useless to attempt to make American deposits square exactly with our own; our definition of carboniferous limestone would not apply to Ireland; *à fortiori,* we could not expect it to coincide strictly with America; and lastly, we should endeavor to make our nomenclature as effective as possible for distinguishing grouping formations in all parts of the world, so as to make the terms comparable.

On the Action of the North American Lakes; by Mr. H. Schoolcraft. Mr. S.'s observations on these lakes were made during a residence of nearly twenty years in that district, chiefly in the immediate vicinity of Lake Superior, and he was thus enabled to devote particular attention to the action of the lakes on their boundaries, under fluctuations of level by which they have been either considerably enlarged, or otherwise modified. In this respect Lake Superior, perhaps, affords more scope for observation than any other; its large area and great computed depth, seem more fully to develope the action of its waves upon the sandstone rocks which surround its southern margin. This is no where better shown than along the twelve miles of mural coast locally known as the *pictured rock;* the force of the waves impelled by the equinoctial gales has fretted and riddled these rocks into the most singular architectural forms; colossal caverns, into which large boats can enter, are formed under the impending rock. Along this coast of winding bays and headlands, extending altogether four hundred and fifty miles, the action of heavy currents has broken and comminuted the sandstone and graywacke, piling up the sand thus formed into elevated ridges, or spreading it out over wide plains. The most extensive field of action occurs between the eastward termination of the primary rocks, near Granite Point, and their reappearance in the elevated mountainous range of Gros Cape, at the head of St. Mary's Straits. Vast hills, or *dunes* of sand three hundred feet high, are formed along this line, and present a very remarkable appearance, from their perfect aridity, their elevation above the lake, and the generally uniform level of their summits. They appear to rest upon more compact beds of clay and gravel, and to have evidently been washed up by the waves and driven landward by the wind. Tempests of sand are thus formed, which spread inland, burying the tallest trees and carrying desolation in their track. The same wind and wave action is described by the author as taking place on some parts of the coasts of Lakes Huron and Michigan; *dunes* are first formed, and then spread inland, bearing sterility over thousands of acres formerly fertile and well wooded. Another effect produced by this drifted sand, is to occasion the formation of pools and morasses along its shifting boundary line, thus injuring other large tracts of country. The recent date of this formation, is often shown by buried trees and fresh-water shells found at great depths in excavating, or exposed by irruptions of the waves. Mr. S. describes other arenaceous deposits forming broad sandy belts, bordering the lakes, and supporting a light growth of pine, poplar, and birch; these he considers due to a similar action at an earlier period, when the water of the lakes stood at a higher level and occupied a wider area, a condition which is further indicated by the occurrence of wide lacustrine deposits in the same neighborhood. On the shores of the lakes there sometimes occurs a deposit of iron sand, often a foot in thickness, formed from the magnetic oxide of iron, which exists abundantly in the sandstones, and is set free by the action of the waves in comminuting the rocks.

Notice of Fossil Footsteps in the New Red Sandstone at Lymm, in Cheshire, by Mr. Hawkshaw.—The rock underlying the strata is a thick bedded sandstone, deeply impressed with oxide of iron, and very indistinctly stratified. Fossil footsteps had been found in nearly all the sandstone beds; those of the upper part were small pointed impressions, resembling the tracks of Crustacea; others were like the feet of birds; footsteps of the Cheirotherium also occurred in the upper beds, but they were of small dimensions, and appeared to increase in magnitude as the beds descended.

The following papers were also communicated, but our limits permit us to give only the titles.

Report of Committee for registering Earthquake shocks in Great Britain.

On the Structure and Mode of Formation of Glaciers, by James Stark, M. D.

Report on British Belemnites, by Mr. Phillips.

Report on the Fossil Fishes of the Devonian System, or Old Red Sandstone, by Prof. Agassiz.

On the Fossils of the Carboniferous Limestone of Ireland, by R. Griffith, Esq.

On the Microscopic Structure of Coal, by John Phillips, Esq.

On the Origin of Coal, by Mr. Williamson.

On the Great Lancashire Coal Field, by Mr. E. W. Binney.

On the Remains of Insects in the Lias of Gloucestershire, by Rev. P. B. Brodie.

On the occurrence of Bowlders in the Valley of the Calder, by Mr. J. T. Clay.

On the occurrence of Vegetable Remains in the New Red Sandstone of Staffordshire, by Mr. J. Dawes.

On the North Coast of America, by Mr. R. King.

On the stratified and unstratified Volcanic Products of the West of England, by Rev. David Williams.

Notice on the distinction between the striated surface of rocks and parallel undulations, dependent on original structure, by R. I. Murchison, Esq.

Summary of a Report on Chemical Geology, Part II, by Prof. Johnston.

Report on Fossil Mammalia, by Prof. Owen.

On recent and fossil semicircular cavities caused by air-bubbles on the surface of soft clay, and resembling impressions of rain-drops, by Dr. Buckland.

On some peculiar inorganic Formations and Fossils of the Magnesian Limestone, by Edwin Lankester, M. D.

On Magnesian Limestone, by Dr. Daubeny.

Sect. D. *Zoology and Botany.*

The following papers were communicated to this Section.

Report on the present state of the Ichthyology of New Zealand, by John Richardson, M. D.

Reports on dredging the sea at great depths, by Mr. Patterson.

Report of the Committee on the preservation of Animal and Vegetable Substances.

Description of a fish constituting a new genus, (*Macharium subducens*,) by Dr. Richardson.

On the Palpi of Spiders, by Mr. Blackwall.

On the Nidus and Growth of the *Purpureus lapillus*, and also on the *Patella pellucida* and *P. lævis*, by C. W. Peach.

Notices of the dates of first flowering of Plants, the migration of Birds, &c., drawn up by Mr. J. Couch of Cornwall.

List of periodical Summer Birds observed near Llanrwst, Denbighshire, North Wales, in the spring of 1842, by Mr. Blackwall.

Account of a species of Ichneumon, whose larva is parasitic on Spiders, by J. Blackwall, F. L. S.

Report of the Committee appointed to make experiments on the growth and vitality of seeds.

On the different species of Cotton Plants and the Culture of Cotton in India, by Prof. Royle.

On the promotion of Vegetable Growth, by Mr. Webb Hall.

On Liebig's Theory of Fallow Crops, by Rev. J. B. Reade.

Description of three new species of Mollusca of the genus Eolis, by Mr. Allder.

On the Varieties of the Human Race, by Dr. Hodgkin.

Report of the Committee appointed to draw up a plan for rendering the nomenclature of Zoology uniform and permanent.

Sect. E. *Medical Science.*

The following papers were communicated.

On the Construction and Application of Instruments used in Auscultation, by Prof. Williams.

On the influence of the Coronary Circulation on the Heart's action, by Mr. J. E. Erichsen.

On some peculiarities of Circulation of Blood in the Liver, by Mr. A. Shaw.

On the therapeutic application of air-tight Fabrics, by Prof. Williams.

On the relation of the season of birth to the mortality of children under two years of age, and on the probable duration of life as it is affected by the month of birth solely, and by the months of birth and death conjointly, by Mr. Catlow.

On a general law of Vital Periodicity, by Dr. Laycock.

On Lithotomy and Lithotripsy, by Mr. Wilson.

On the uses of the Muscular Fibres of the Bronchial Tubes, by Dr. James Carson, Jr.

On a case of Asphyxia, which occurred in the operations for clearing the wreck of the Royal George, by Dr. Richardson.

Sect. F. *Statistics.*

The following papers were communicated.

On the results of Spade Husbandry, Small Allotments, and Agricultural Schools, by Mrs. Davies Gilbert.

On the influence of the Factory System in the development of Pulmonary Consumption, by Mr. Noble.

Return of the Capital Punishments inflicted in Lancashire, from 1782 to 1841 inclusive.

Abstract of the Registers of the Collegiate Church in Manchester, by Rev. R. Parkinson.

Continuation of the Reports on Loan Funds in Ireland, by Mr. H. J. Porter.

On the Vital Statistics of the Spinners and Piecers employed in the fine spinning mills of Manchester, by Mr. Shuttleworth.

Report on the Cases before the Police of Manchester on Saturdays and Sundays, from Jan. 22 to June 15, 1842, by Sir Charles Shaw.

On the comparative Statistics of the Universities of Oxford and Cambridge, in the 16th, 17th, and 19th centuries, by Mr. James Heywood.

Contributions to Academical Statistics, continued from 1839, by Prof. Baden Powell.

On the *Monts de Piété*, established in Ireland, by Mr. H. J. Porter.

On the increase of the value of Property in South Lancashire, and particularly in the Hundred of Salford, since the Revolution, by Mr. Henry Ashworth.

On the Commercial Statistics of France in 1840, by Rev. Mr. Jones.

On Vital Statistics, especially with relation to the influence of the atmosphere on Mortality, by Dr. Ashton.

On the Statistics of Plymouth, by Mr. Woolcombe.

Report on the Vital Statistics of five large towns in Scotland, prepared by Mr. A. Watt, under the direction of a Committee.

On the Destitution and Mortality of some of the large towns in Scotland, by Dr. Alison.

Report of the Manchester Statistical Society on the Vital Statistics of Manchester, prepared chiefly by its Vice President, Mr. Roberton.

Sect. G. *Mechanical Science.*

The following papers were communicated.

Report of the Committee on Railway Sections.

On a new self-acting Weir and Scouring Sluice, by Mr. Bateman.

On Ventilation, by a method proposed by Mr. Fleming of Glasgow, by Mr. Liddell.

On Straight axles for Locomotives, by Prof. Vignoles.

On Combustion of Coal with a view to obtaining the greatest effect and preventing the generation of Smoke, by Mr. Fairbairn.

On the pressure of Earth against Walls, by G. W. Bucke.

On Iron as a material for Ship-building, by Mr. Grantham.

Report of the Committee on the Forms of Ships, by Mr. Scott Russell.

On the Constant Indicator, by the inventor, Prof. Moseley.

Report of Experiments on the transverse strength of hot and cold blast Iron, by Mr. Fairbairn.

On the best form of Rails and the Upper Works of Railways generally, by Prof. Vignoles.

On an Indicator of the Speed of Steam Vessels, by its inventor, Mr. J. S. Russell.

On the use of *Béton* and *Concrete* in constructing Breakwaters, by Prof. Vignoles.

On a new Steam Engine worked with three kinds of pressure, viz. action of high pressure steam, the expansion of steam, and the atmospheric pressure caused by its condensation, by Mr. Shaw.

On the efficacy of the several plans for abating the nuisances from Smoke by effecting a more perfect combustion, by Mr. C. W. Williams.

Art. XIV.—*Astronomy and Photography at Rome.**

Remark by the Editors.—The following observations appeared first in the Liverpool Times of August 2d and September 5th, 1841, and we have no hesitation in departing from our usual custom of not republishing what has once appeared in the daily journals, because we feel confident that few if any of our American readers have seen either the memoir of the Roman astronomers, or the interesting review of it, from the pen of our esteemed friend and correspondent, Mr. J. Taylor of Liverpool.

"*A Memorial of sundry Observations made at the Observatory of the Gregorian University, in the Collegio Romano, by the Director*, P. Francisco De Vico, and the other Astronomers of the Company of Jesus, in the years 1840 and 1841. Rome, 1842. Marini & Co., printers."

This publication, of which a copy has been with the greatest courtesy transmitted to this town, will command the attention of the scientific world, not only by the important information contained in it, but also as being the precursor of a series of annual memoirs, intended to contain reports of future astronomical operations at Rome, in the observatory of the Collegio Romano, which will henceforth take its place in the first rank of that class of the European scientific establishments.

The observatory at the Collegio Romano may, in fact, be considered as the oldest in Europe, having been the station from which Clavius made his observations on the new star of the constellation of Cassiopeia, in the year 1572. From that time it had in succession for its superintendents the Jesuit Scheiner and the illustrious Cassini, followed by Bianchini and Boscovich, who die din 1787.

The wars of the French revolution interrupted for thirty years the peaceful pursuit of astronomy at Rome, but on the restoration of peace in the year 1816, Pope Pius VII. constructed the present observatory, which Leo XII. in 1824 restored, along with the rest of the Collegio Romano, to the order of the Jesuits. Since that time the observatory has enjoyed the particular patronage of the Generals of the Order, the set of instruments hav-

* Communicated to this Journal by the author.

ing been augmented by a famous refracting telescope of Cauchoix, by an astronomical theodolite of Gambey, by an excellent chronometer of Breguet, and a capital meridian circle of Ertel. Whether it be through the superior excellence of this particular telescope, or the greater clearness of the Roman sky, the services obtained from it have been most efficient, and such as may give a new starting point for the science of astronomy.

By the reports given in the memorial just published, the advantages are fully shown which may be derived from observations of the periodical falling stars, in corroboration of lunar and planetary observations, for the accurate determination of the relative position of places, not otherwise attainable by geodesical measurement. The success of the operations used in the instances of Rome, Naples, and Palermo, as respects each other, fully establishes the fact. The corrections obtained by these means have been adopted by the French astronomers in the Connoissance des Tems, although our Nautical Almanac is still in error. Taking the difference in longitude of Greenwich and of Paris at 9 minutes 21.5 seconds, the correct position of the Observatory at Naples will be 0*h.* 57*m.* 1*s.*5, longitude east of Greenwich, and 40° 51′ 46″.6 north latitude; lessening by 7*s.*8, the longitude hitherto given for Naples in the Nautical Almanac.

The longitude of the Collegio Romano, at Rome, will then be 0*h.* 49*m.* 55*s.*27 east of Greenwich, and its latitude 41° 53′ 52″ north, increasing by 0*s.*57, the longitude hitherto given in the Nautical Almanac. It is to be observed that there is a difference of half a second of time in the longitude of Paris and Greenwich, between the reckoning of the Connoissance des Tems and that of the Nautical Almanac, which might as well be reconciled, seeing that they are both such great authorities, and Paris and Greenwich such noted astronomical stations.

The mode of determining the longitude by observations of falling stars, was first suggested by Dr. Maskelyne, in the year 1783,* and was made use of in 1802, in Germany, but it was reserved for the Neapolitan astronomer Nobili to perfect the method, and to point out the right way to be followed in the practice of those observations.

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* See a notice on the determination of longitude by shooting stars, (this Journal, Vol. XLII, p. 399,) where it will be seen that Dr. Halley and Mr. Lynn have the credit of suggesting this mode of observation as early as 1719 and 1727.

PHOTOGRAPHICAL DELINEATION OF NEBULÆ.

One of the first uses made by Galileo of the telescope, on its invention, was the examination of some of the most remarkable nebulæ, and the delineation of their then state, as if he had anticipated future changes in their constitution. In his Siderius Nuncius, published in the year 1610, he gave drawings upon a large scale of the Pleiades, of the Belt and Sword of Orion, of the nebula in the head of Orion, and of the cluster of stars known as the Præsepe or Bee Hive, in the constellation Cancer. In these the ground is black, and the stars white. The positions of the stars are given with considerable precision, but there is no trace of the remarkable extent of lucid nebulous matter, nor of the deep black indenture and distinct outline which gives it something of the appearance of a bat's wing. The idea of perpetuating the appearance of this particular nebula of the Sword of Orion in his time, was taken up by Huygens, in the year 1656, and he has left what he vouches for a correct representation of it, as seen by him, but unaccountably passes over in silence the drawing left by Galileo. In Huygens's drawing and description, the shape of the nebula differs considerably from that which it now has, and the engraving in Sir John F. W. Herschel's Astronomy for the year 1833, is still more at variance with the present reality. In these circumstances, a doubt arises how far the apparent discrepancies are owing to actual physical changes in the nebula itself, or if they be owing to the imperfection of the instruments used, or of the vision or powers of accurate delineation possessed by the observers. That it is owing, in some degree, to physical changes is rendered probable, from alterations which have been seen to take place in the last three years. Fortunately, the Roman astronomers have hit on a means effectually to prevent future mistakes of vision or delineation. They have brought the Daguerreotype to bear on the object, and throwing the photographic image of the nebula and its stars on a lithographic stone, have, by an ingenious invention of the Signor Rondoni, which is still kept secret, fixed it there. From that stone they have been able to take impressions on paper, unlimited in number, of singular beauty, and of perfect precision, each star, each filmy nebulous streak faithfully depicting its own position. The scale is large, proportionate to the magnifying and light-collecting powers of the specula employed; the effect is wonderful, and is heightened by being thrown on a beautiful deep azure ground. A globe must have upwards of fifty yards diameter, equal to the width of our Exchange area, to have room for so large a representation of the nebula in question. The same process has been applied, and with equal success, to the nebula in the Girdle of Andromeda. Altogether, it is a discovery of the highest importance to astronomical research.

The account of the labors of the Roman and Neapolitan astronomers upon these different objects is highly interesting, as an example of successful care and diligence. Besides a mass of lunar and planetary observations made with micrometrical accuracy, and those on the falling stars, for the purpose of ascertaining the difference of the longitude of their two observatories, the Signor De Vico has drawn up a table, by micrometrical measurement, of the apparent right ascension and declination of twenty six stars encircling the double star Theta, in the nebula of the Sword of Orion, and contained in a space hardly exceeding that of half the apparent disc of the sun. This table, combined with the corresponding photographic portrait, will detect any changes that time may effect in that which has been justly styled the "transcendently beautiful Queen of the Nebulæ." By using a magnifying power of 824 on some nights of extreme purity of atmosphere, Signor De Vico has also succeeded in resolving the nucleus of the nebula of Andromeda into a number of luminous points equal in splendor, and very close to one another. He promises to give hereafter the positions of some of the principal of the great number of exceedingly minute stars scattered over this nebula, which since the year 1612, when it was first observed by Simon Marius, has engaged so much of the attention of astronomers. Neither Marius, nor Messier, nor Le Gentil could discern any star in it; and even Sir John F. W. Herschel could not recognize "the slightest appearance to give ground for a suspicion of its consisting of stars." But a happily constructed telescope, with a purer sky, has led the Roman astronomers to a different result. Here again the Daguerreotype comes with powerful aid to assist their investigations, and numerous minute stars are seen distinctly sprinkled over the beautiful photographic portrait.

So far may be considered as the first part of this most important memoir. The sequel contains observations on the ring and satellites of Saturn, and those by which the time of the rotation of the planet Venus on its axis, has at length been determined, and the spots of its disc correctly delineated. Ninety one designs, on a small scale, of the appearance of the spots on the disc of Venus, taken at various times, are annexed to the present memoir, and a regular map, on a large scale, is announced for the next publication.

THE PLANET VENUS, ITS DISC, AND DIURNAL ROTATION.

> Alma Venus! cœli subter labentia signa
> Quæ mare navigerum, quæ terras frugiferenteis
> Concelebras;—quoniam—suaves tibi dædala tellus
> Summittit flores, tibi rident æquora ponti,
> Placatumque nitet diffuso lumine cœlum,
> *Adsis.* *Lucretius, lib.* 1.

From the time when astronomy became a regular science, the construction of a correct table of the planetary movements, diurnal and annual, rotatory and orbitual, has been a main object of research, and, in fact, the index of the progress made. The knowledge obtained by the ancients, through a long course of careful observation, received little or no increase, after the time of Ptolemy, until Kepler arose; for the Copernican system was only a revival of the Pythagorean, the origin of which is lost in the mist of time. But the discovery by Kepler of the law of orbitual motion, accompanied as it was by the almost simultaneous invention of the telescope, gave a new impulse, and bestowed precision and certainty on that which before was vague conjecture. The times of the diurnal rotation of the planets Mars, Jupiter, and Saturn; the position of their poles in space; the inclination of the planes of their equators to the planes of their several orbits; the inclination of the planes of those orbits on the plane of the ecliptic, and the longitude of their nodes, were soon determined with considerable accuracy. All this was done as far as regarded those more distant, or what are called the superior planets; but when the same points were sought for in respect to the two inferior and nearer planets Venus and Mercury, new difficulties occurred to baffle the best directed efforts. The abundance of the light, illuminating Venus and Mercury, was found to be a cause of greater obscurity than the scantiness of it at the distance of Jupiter and Saturn. Even all the recent improvements in achromatic telescopes have proved insufficient, in the cold and thick atmosphere of the northern division of Europe, to overcome the intense radiance of the solar beams, forcibly reflected from objects placed so near the Earth and the Sun as Venus and Mercury are.

The superior distinctness of telescopic observations in southern latitudes was early experienced, although there appears to have been an unwillingness to recognize the fact amongst the astronomers north of Florence. Francisco Fontana, the Neapolitan astronomer, was the first to discover the libration of the moon in latitude, which had only been by analogy and in anticipation asserted by Galileo, who only detected the libration in longitude. It was Fontana also, who first observed the spots on the discs of Venus and Mars. By these he endeavored to ascertain the rotation of those planets, and on November 11th and 15th, and December 25th, 1645, and on January 22d, 1646, delineated from observation the phases presented by Venus. Like many others, in advance of the age in which they live, Fontana did not obtain the credence nor the credit which were his due. Riccioli and Grimaldi both seem to have viewed him and his labors with the eyes of rivals, and in their notice of his valuable discoveries, for such they were, hesitated mistrust. The lapse of two hundred years has at length brought a singular confirmation of the truth of what Fontana asserted that he saw. Riccioli says, (lib. vii, sect. 1, cap. 4.)—"In the observations of Francisco Fontana, I read that Venus was seen in the evening through the telescope, oblong, and about the same apparent size as the Moon seen without a telescope, *with a rough edge in the concave part*, and sending forth rays, especially when the figure was parabolic, and (that which never hitherto has been heard of,) *with one or two dark colored round spots, at one time beyond, at another within the body of the planet, so as to deform the disc*, as may be seen in the subjoined contracted sketches. If these things be true, (for far be it from us to call in question the good faith of those who affirm them,) it seems that we must say, that it was either some meteor, perhaps a patch, pledget, or belt, or some small cloud between the observer and Venus, or surrounding it; or that there are spots, like the solar spots, blown up, and as it were bubbles from the body of the planet Venus, *or like the caverns and mountains of the Moon, more or less illuminated accor-*

ding to their varying position with respect to the Sun, or perhaps owing to the rotation of Venus on its axis, or to its libration; for neither dare I say that Venus has satellites, until the day come that may teach us something more certain concerning this affair. It has never certainly been permitted for me, nor for Father Grimaldi, nor for Gassendi, as appears by the 3d book of his Institutions of Astronomy, to see in Venus, nor near Venus, those small globes (or spots) by means of any telescope."

So said the Father Riccioli two hundred years ago; remarks certainly not very encouraging to the communicators of new discoveries. But time, the vindicator, has at length done his work; and long after Galileo, Fontana, Riccioli, Gassendi, and Grimaldi have ceased from their watchings and their labors, and enjoy their rest, "unmindful of the call of the morning," the controverted points have come to be resolved:

> "*Omne,* quod optanti divom promittere nemo
> Auderet, volvenda dies, en attulit ultro."

The four delineations given by Fontana, correspond closely with the account given by the Roman astronomers, in the memoir now under discussion. In Fontana's delineation of the appearance of Venus on Nov. 11, 1645, the disc is rather more than the quarter, and has an oblong dark spot in the middle of the illuminated part; in that of Nov. 15, 1645, the disc is nearly the half section, with the ragged edge on the concave side, and dark detached spots at each horn of the crescent; in that of Dec. 25, 1645, the disc is gibbous, and has only one dark detached spot on the lower horn; in that of Jan. 22, 1646, the disc is a crescent nearly filled, and with a dark oblong detached spot right in the centre of the concave boundary.

The Roman astronomers inform us that Venus was observed by them on April 12, 1841, at six of the evening; that "the phase presented by the planet was rather small, and that near the point of the northern horn, and properly in the middle of the illuminated part, there was seen very plainly a dark oblong spot. It appeared as if a short and fine thread of black silk were placed so as to lie on that part. Whilst the usual observations were making of the diameter of Venus, the Signor Clemente Palomba, the assistant observer, to whom was entrusted the care of the instrument and of the micrometrical observations, gave us notice that to a certainty the entire disc of the planet was visible, and not merely the small portion illuminated by the sun. The magnifying power used was 120. The sun had set not long before. The thing is true, and was seen also by the others present. We have since happened to learn, that the same phenomenon was manifest to Meyer, at Griefswald, Oct. 20, 1759; to Harding, in 1806, on Jan. 24, Feb. 24, and March 28, in the morning; and lastly to Schröter, Feb. 14, 1806."

"1841, April 19.—The same appearances."

"1841, April 21, at 6 of the evening.—The spot of the northern horn appeared nearer to the limit of the shadow, and perhaps was already in part immersed in the penumbra. Magnifying power 241. At 6*h.* 33*m.*, it was seen surrounded with very bright light. With a magnifying power of 824, it was seen to be exactly like a lunar crater, when its highest banks are illuminated from the vertex two-thirds downwards. The Signor Palomba measured the apparent diameter of Venus, first from the illuminated limbs, and afterwards from the one illuminated, and from the other dark. This last always came out less than the former."

"1841, April 22 and 23.—The same appearances as the day before. The spot seemed enlarged in size."

"1841, April 30, 7*h.* 30*m.* in the evening.—The spot was very black and very clear to be seen. It was surrounded by a lucid ellipse, as the sides appear of a lunar crater seen obliquely. The limit of the dark part of the disc, although it had encroached a good deal into the interior of the spot, nevertheless did not yet cover the above mentioned bank, which, with its luminous half, appeared pretty well within the dark portion of the disc. Magnifying power, 824. Towards the extremity of Venus at the opposite horn, there began to be seen, first by Signor D. V. Mobili, and afterwards likewise by the rest, something similar; but the evening being too far advanced, the observations were dropped."

"1841, May 2.—Drawn by the curiosity of seeing the *crater* of Venus, besides the accustomed observers, there came divers others, who had the pleasure to be satisfied. At 43 minutes past 6 of the evening, the entire globe of Venus was distinctly seen; and however the dark part appeared advanced, yet it did not cover any portion of the illuminated edge which surrounded the spot. Magnifying powers, 241 and 824."

"1841, May 3, 6*h*. 40*m*. of the evening.—The summit of the lucid border of the spot towards the dark part of Venus was no longer seen with the magnifying power of 240. With the other of 824 it was hardly seen. With less magnifying powers the dark part of the disc appeared dentellated at that point.

"At 6*h*. and 50*m*. the lucid edge of the planet was not plainly terminated by a circular curve; towards the southern horn it appeared deficient; from thence by little and little it turned to be visibly terminated in a circular curve, but between it and the limit of the dark part there was visible a very subtile but well decided and long black spot, like a slender thread stretched from one horn towards the other."

"1841, May 4, at the same hour.—The darkness had covered entirely the half of the elliptic edge of the crater; and the northern peak of it appeared terminated in a triple point. Two of those points were the effect produced by the edge of the darkness upon the brink of the crater, which was seen to penetrate with two lucid arms into the dark part. Magnifying power, 824, and afterwards 1128."

The excellence of the telescope used by Fontana is particularly noticed by Gassendi. It was this, with his own adroitness and acute vision, which enabled Fontana to be the lucky discoverer of the spots on the discs of Venus and Mars, and of the spots and belts on that of Jupiter; but it was reserved for that truly great man, Dominic Cassini, to pursue the inquiry with success, about twenty years afterwards; to assign with precision the situation of the spots, and, by calculation, to determine the periods of the rotation on their axis, for each of those planets, with wonderful accuracy. His observations and calculations for Mars and Jupiter, were at once generally admitted; but, although he was, in reality, more successful in the instance of the rotation of Venus than in that of either of the other two, for he estimated it at 23 hours, 21 minutes, which was only 22 seconds less than the truth, yet, by a strange perversity, this was especially doubted, and his accuracy, and almost his veracity, called in question. He had seen the spots of Venus at Rome, but in vain tried to discover them with the telescope of the observatory at Paris. He persisted in his assertions and in his calculations; and the northern astronomers persisted in their doubts, to his great discomfort, and that of his worthy son, J. J. Cassini, who like Æneas, with becoming piety, stoutly sustained his father's good fame. But all would not do: the northern astronomers would not believe that any spots could be any where seen in Venus, for they were invisible at Paris and London. Cassini found himself in the predicament of Fontana, and the story of the man of Rhodes must have haunted his recollection.

Yet still there were intimations from time to time given by astronomers at Rome, that they did see spots as Cassini had seen them; and at last Francisco Bianchini, in 1726, gave a map of the planet and his estimate of its rotation, which he made to be nearly 24 days, 8 hours. Now the time of the true rotation, as ascertained by the authors of the memoir now produced, is 23 hours, 21 minutes, 22 seconds; and 25 of those rotations would take up in time 24 days, 7 hours, 54 minutes, 8 seconds, so that the theory propounded by Bianchini may be regarded as a compromise offered to those opposed to Cassini. The matter remained thus, in the slumber of doubt, for more than a hundred years, for the opinions of Schröter were merely an adoption of those of Cassini, although Lalande, Delambre, and Laplace repeatedly urged the necessity of constant observation in the southern observatories, for the solution of so important a problem; and it is only lately that, younger men having succeeded to the direction of the Roman observatory at the Collegio Romano, and having their lives before them, the matter has been taken in hand in earnest.

The methods used have been those particularized by Delambre, combining the known orbitual movements of the earth and of the planet, with the diurnal changes of position observed in the spots. For three years the observations made have been incessant; between January 1, 1840, and April 30, 1840, they amounted to one thousand six hundred and fifty in number. Designs of the apparent discs of the planet were taken several times a day. Of these ninety one are given with the memoir; the general map is to follow. The result of the whole is completely confirmatory of the statements and calculations of Cassini as far as regards the time of the rotation, and of the accuracy, in a great degree, of the map published by Bianchini. The error of Bianchini in his estimate of the rotation appears to be owing to his having mistaken the return of certain spots for those of others, and to his having founded his theory on too limited a number of observations.

The following are the farther results of the observations made on Venus, and of the calculations now founded on them for the year 1840:—

	Hours.	Min.	Seconds.
Rotation of Venus on its axis, . . .	23	21	21.9345
Inclination of the plane of the Equator of Venus to the plane of the Orbit of Venus, ...	56°	24′	24″
Inclination of the Orbit of Venus to the Ecliptic, ...	3	23	33.23
Longitude of the ascending node of the Orbit of Venus on the Ecliptic,	74	40	31
Longitude of the Vernal Equinox of Venus,	56	31	0
Latitude of ditto, South, ...	1	3	30
The Vernal Equinox of Venus therefore took place in space, measured on its Orbit, ...	18	11	18
Or in time before the planet in its course crossed the ecliptic, ...	11 days, 8 hrs. 30 min.		

As Venus crossed the ecliptic in her ascending node in the year 1840, on June 25, 15*h.* 10*m.*, the vernal equinox of Venus must have taken place on June 14, 6*h.* 40*m.* of that year.

SATURN AND HIS RING.

The observations on Saturn, its satellites and ring, being still in course of prosecution, in conjunction with those of Professor Schwabe, and as they will be given at large in the memoir for next year, it may suffice to state "that Saturn does not always keep the centre of the ring, but makes a small periodical movement from the centre." "That in 1840, for several nights in sequence, there was seen on the eastern point of the ring, a very small lucid point adhering immovably to the edge, so that it might be said to be one of the small satellites of the planet, that had attached itself to the limb."

"That there was on the superficies of the inner ring from time to time seen a small obscure trace like the belts of Jupiter and Saturn, but although different from that which has been considered as a subdivision of the ring, they both seem still to be of the same nature. The new division of the ring, seen by Encke, has also been visible at sundry times in Rome."

Liverpool, September 5, 1842.

ART. XV.—*Notice of Dr. Hare's "Strictures on Prof. Dove's Essay on the Law of Storms;"** by W. C. REDFIELD.

THE "strictures," under the title above quoted, in the last number of this Journal, appear to be, mainly, a continuation of Dr. Hare's "objections and strictures" on my views and statements relating to whirlwind storms. It seems proper, therefore, that the article should receive a passing notice.

Dr. Hare commences with a commentary on a note by which the Editors of this Journal had obviated the perversion of my views of the "cause" of violence in destructive tempests: and in further quoting, he improperly italicises the phrase "unmeasured violence," instead of "*rotative movement*," which last I had alleged as the cause of violence in storms. Having already explained the passage in question, I am unable to account for the tenacity with which an improper construction of my language is maintained.

Though seemingly conscious of the proper "difference" between a general "cause" of wind and the cause of its *violence* in peculiar cases, Dr. H. appears to think that "gyration, instead of accelerating that velocity on which violence is dependent, must, by the expenditure of momentum resulting from collision with inert portions of the atmosphere, consequent to centrifugal force, cause a great loss of velocity."

Now, "that velocity on which violence is dependent," can hardly need "accelerating." I may also remark, that the results which have been established by careful observations, are opposed to his speculative objection. Dr. H. here takes for granted, that which is necessary to be proved, and which I wholly deny, viz. the alleged "COLLISION with inert portions of the atmosphere consequent to centrifugal force" in a whirlwind. Observation shows us that the centrifugal force, in any particle or series of particles is held in check, and continuously *resolved* by the joint influences of the surrounding momentum and pressure, aided by the interior rarefaction and spiral discharge in the direction of least resistance which are "consequent" to these resolved and inwardly tending forces, in all cases of clearly marked vorti-

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* For Professor Dove's Essay, see page 315.

cal action. Indeed, my cursory explanation of the "violence" of whirlwind storms, had been founded mainly on the obvious fact that in the mere turning movement of a body of air, there could be no "collision" or obstruction to rapidity of motion, such as would obstruct or wholly restrain the *rectilinear* movement of the body, through the surrounding atmosphere.

Dr. Hare says, (par. 100,) "I have not been enabled to discover that Prof. Dove attempts to assign any cause for violent winds." I am far from viewing this as a blemish or defect. Had more strict attention been given to the actual movements of winds, with fewer attempts to "make" or "invent" a theory by which to account for all atmospheric phenomena, it is probable that greater progress would have been made in our knowledge of the causes by which they are controlled. I deem it profitable to adhere to that philosophy which patiently investigates *effects*, in order to "ascend," on a firm basis, "from these to causes."

Dr. H. again alleges, that "the velocities of the aeriform particles in a whirlwind, must be greater as they are farther from their axis," as is seen in the revolution of a solid. (par. 101.) This allegation is so directly opposed to observation, in whirlwind storms and in minor vortices, as to require no further examination. Nor are the rotative forces "uniform," at all distances from the rotative axis, as Dr. H. seems to assume. (par. 101.)

We next find the sweeping allegations, that "agreeably to the most ample and satisfactory evidence adduced by Prof. Loomis, as well as by general experience, some of the most violent storms of this continent travel from the northwest towards the southeast." (par. 102.) What is here meant by "general experience," I cannot divine; and the evidence adduced by Prof. Loomis, whatever may be its bearing, relates only to the storm of Dec. 21, 1836. But I trust it has been sufficiently shown, that the evidence thus adduced, and particularly the northeasterly range of "the area of minimum pressure," as set forth by Dr. Hare himself in his previous article, disproves the alleged *southeasterly* course of this gale, and shows its progress to have been *northeasterly* on this continent. The objection which it is attempted to support by these allegations must therefore fail. (See October number of this Journal, Vol. XLIII, p. 259, *et seq.*)

In par. 106 it is complained of Prof. Dove and myself that we attempt to explain only the "curvilinear direction" of the wind, and not its causes. It is hoped that my remarks above, on par. 100, are a sufficient answer.

It would afford me pleasure to relieve Dr. Hare from any perplexity in relation to the causes of violence in whirlwind storms; (par. 107,) but it appears to be necessary that he should first abandon his idea of their "collision with the surrounding atmosphere," together with his favorite objection in par. 65, that "in a fluid" the rotary motion near the axis "can be no quicker than in the case of a solid."

In par. 108, Dr. Hare does not show the absence of "continuous exciting forces" in a hurricane. Such forces are found in the "continuous" excess of atmospheric pressure which surrounds the hurricane or aerial vortex, and in causes which may be included in Prof. Dove's explanations.

Dr. Hare next endeavors to show that my own and Prof. Dove's view of the forward inclination of the axis of rotation in storms is erroneous. His objections seem founded partly on the term *rotating cylinder*, which has been used by Prof. Dove to designate the mass of air which constitutes the body of a storm, partly on the erroneous allegation that any other course of rotation than "in concentric circles" at "right angles to the axis," must be "inconsistent with enduring rotation," and partly on the limited height of the whirling action as compared with its superficial diameter and extent.* (par. 109–112.)

I do not perceive that either of these objections can have any weight, as proving that the axis has not a forward inclination. Indeed, such inclination seems far less difficult and more probable in a storm of 400 miles diameter, than of one-fourth of a mile. It is clearly evident that the lowest portion of a progressive storm of any kind, must be retarded by its impingement upon the earth's surface, thus causing its higher portions to be more advanced; and the fact is abundantly supported by observation. At the time of writing these lines, the rain-scuds in a storm from E. S. E. may be seen, on a low angle of vision, drifting at an elevation of a few hundred feet, with a velocity visibly exceeding that of the nearer aqueous drift which is moving at a lower level. The advanced appearances of a storm are often noticed in the region of clouds many hours before any change

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* For Prof. Dove's statement, see page 334.

takes place in the movements of the air below. A like advance of the storm in the higher region is indicated by the fall of the barometer, which usually commences before any other indication of its approach can be perceived. Often, too, the clouded axis of a whirling tornado or water-spout, has been seen in an inclined or curved position, while moving onwards. And in so dense a fluid as water, I have myself seen the axis of a vortex which was curved through sixty or seventy degrees from the vertical, while its vorticular gyration was so rapid as to sink and carry off a continued series of large bubbles of air, 800 times lighter than the fluid vortex.*

The additional objection, that such forward inclination of the axis of the storm "would lift the base in the rear," as if the rotary mass were a solid, seems hardly to require an answer. In a fluid stratum of any considerable thickness, we have just seen it cannot be necessary that the general plane of its rotation should be at right angles with the line of the axis. As aerial particles move freely over each other, the storm may be viewed as a series of plane or spiral strata, of any conceivable thinness, superimposed one above another, but each in advance of the next beneath.

In order that the "lower and warmer strata" in a storm may gradually rise by their spiral movements, to a higher and colder region, thus producing rain, it cannot be necessary "for the lower stratum to exchange places with the upper;" for the former will be continuously followed and replaced by the next contiguous portions of the lower strata of air which surround the storm; thus affording a continued supply of aqueous vapor for condensation in a higher region. But were it otherwise, a movement of "five hundred miles" in a given particle, or plano-series of particles, is but a diminutive item in the sum of the aerial movements, in a great storm or hurricane.

That the dimensions of a great storm may have "a diameter two hundred times as great as its altitude," is a fact neither new nor doubtful,—and the retardation of its lowest portion by its contact with the "terrestrial surface" is equally undeniable; but it does not follow, that, because the whirling body is likewise "contiguous to inert particles of the atmosphere," it must "incessantly

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* But in this case, as elsewhere, we might have looked in vain for greater or even *equal* velocities at increased distances from the axis of the vortex. See *ante* on § 101 and § 107.

share with them any received momentum." The vast disparity of the lateral to the vertical dimensions of a storm, seems to have once attracted the notice of Mr. Espy; but how this fact can be reconciled with the theory which teaches that a storm is essentially an ascending column of air, I cannot conceive. (par. 113.)

Those are "evidently no transient impulses" which produce a revolving action in storms; and the effects produced in the atmosphere by these impulses constitute the proper subject of inquiry. (§ 114.) No one pretends that the "violence of a hurricane" can be "sustained" by the mere "rotary momentum," after the "cessation" of the impelling forces. (par. 115.)

The ground "taken by Prof. Dove" relating to the calm or cessation of violence which is found on the earth's surface at the axis of a hurricane, quoted in par. 116, and which is deemed "strikingly untenable" by Dr. Hare, is mainly in accordance with direct observations; as I had formerly shown.* Dr. Franklin clearly recognized the absence of any force at the axis of a whirlwind: and the fact having been fully proved by observation, in gales and hurricanes, cannot now be set aside by "strictures" on Prof. Dove's language.

But Dr. Hare thinks he has "demonstrated" in his former objections, in par. 67, that "in extensive whirlwinds 'the fiercest raging' cannot be suddenly interrupted so as to leave a dead calm during the interval which takes place between two opposite winds." (par. 117.) I will now quote the *demonstration* referred to.

"67. Mr. Redfield alleges, [has shown,] that the storm of August 17th, 1830, whirling to the left, travelled from southwest to northeast at the rate nearly of twenty-seven miles [*eighteen miles*†] per hour; that its greatest diameter was from five hundred to six hundred miles; that of its severe part was from one hundred and fifty to two hundred and fifty miles. Thus it may be assumed, that in order for an observer to be exposed successively within the severe portion on the southeastern and northwestern limbs, the storm would have had to move at least one hundred miles, requiring nearly four hours. Hence if the storm in question were a whirlwind, instead of the change having been sudden, several hours would have been required for its gradual accomplishment."

This singular demonstration appears to consist in the assumption, "that in order for an observer to be exposed successively

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* See Prof. Dove's statement at page 333.

† See this Journal, Vol. xx, p. 37.

within the severe portion on the southeastern and northwestern limbs, the storm would have had to move at least one hundred miles." Now it is perfectly clear, that "the southeastern and northwestern limbs" of the severe portion of the storm thus moving "from southwest to northeast," *must separately have advanced on two parallel lines, and therefore never could both have been presented, "successively" or otherwise, to "an observer."*

Besides, the calm or cessation of violence, with the change ensuing from one violent wind to another blowing in an "opposite direction," has been known to occur *only at or near the center or axis "of the severe portion" of the storm.* This fact, likewise, renders the supposed *demonstration* wholly nugatory, even as a "stricture" or criticism. From these and similar comments of Dr. Hare, it would seem to be apparent, that he has acquired no correct apprehensions of the facts and statements thus commented upon. (par. 117.)

To obviate the just objection against the centripetal theory, that "winds blowing from opposite quarters *will neutralize each other*," Dr. Hare re-alleges the hypothesis that "they are caused by a deficiency of pressure at the axis of the storm producing an upward current for the supply of which they are required;" thus impliedly resorting, once more, to the erroneous hypothesis of "suction" to draw upward, against the force of gravity, the fast accumulating air, in order to make way for the supposed confluent winds. But let me ask, how can the weight and momentum of all these in-rushing winds be neutralized, *without whirling motion*, "by a deficiency of pressure at the axis?" This inward velocity he represents as increasing as the square of the distance passed over, while the successive areas *diminish* in a like ratio; but if the velocity be only *equable*, the aggregate weight and momentum will thus *accumulate* rapidly from all sides as the areas diminish, and can be turned upward *only by equal or greater forces.* Let us suppose "a deficiency of pressure" at the center of the storm equal to the fall of one inch and a half in the barometer, with a centripetal wind of eighty miles an hour; how many seconds would be required to increase the central pressure to an extent never before known? (par. 118.)

It is stated here that "after the base of the ascending column is reached, evidently the horizontal afflux must be superseded by a vertical movement," and "hence about the centre of the space around which the upward currents prevail, there may be a calm." We have just seen the incompatibility of such inward and upward movements with *diminished pressure.* And how is the fast accumulating resistance to the concentrating mass, *before it reaches* "the base of the ascending column," to be overcome? And, in such rapid concentration towards the "base" of the "column," how is the conservation of areas or spaces to be maintained?

Dr. Hare next treats of "focal areas" of hurricanes and tornadoes, in a manner implying that these areas may "be respectively bases of ascending columns moving with equal velocities," and says, "the focal area of the Providence tornado was estimated [by Mr. Allen] to be three hundred feet in diameter." But it was *the whole* visible form "of the tornado on the river" to which Mr. Allen ascribed this diameter. Without knowing definitely what is meant by "focal area," it appears sufficient to say, that so long as gravitation exists, no "upward columnar current of ten thousand feet diameter" can long need a concentric lateral supply at its base, moving "inwards with the velocity of one hundred miles per hour;" whether itself be moving "upwards" at the same or any other rate.

Surely, some experimental philosopher should contrive to mark the "upward" progress of these vast anti-gravitating columns, by means of air-bags, parachutes, or other visible objects. And here I mean not the slightest disrespect to my opponent, nor to his calculations "founded on the idea of the confluence of the air equally from all points of the periphery." But I have long had an irrepressible desire to find tangible *evidence* of the alleged "up-moving" masses or columns in the atmosphere; of which even the movements of the clouds afford us no satisfactory indications. (par. 119–121, *et seq.*)

I need not further expose the fallacy of the hypothesis of "the inward suction," in the alleged centripetal storm; which force is represented (par. 122) as having the "greatest violence" near the border of the "focal area," where "the confluent currents," we are told, "MUST BE DEFLECTED UPWARDS," perhaps by the "*deficiency*" of pressure, "and thus the central space MUST ESCAPE THEIR INFLUENCE;"—from which we MUST infer, that the entire upward deflection of the storm, at the zone of its "greatest violence," and notwithstanding its gravity and horizontal momentum, *is accomplished without the "influence" of any opposing force!* (par. 122.)

Dr. Hare seems to think the interposition of the Alleghany Mountains an insurmountable obstacle to the rotation of a storm travelling from the Mississippi to the Atlantic coast,—that the aerial mass would be cut nearly in twain when bestriding that range,—and that more than half of the air in such mass would be below the summits of those mountains. Waiving any errors here, it is allowed on all hands that such mountain elevations, together with the "rugosities and inequalities" of the continental surface, have much influence on the phenomena which are observed in the great whirlwind storms that cross this part of our continent. But it is remarkable, that none of the above considerations have prevented Dr. Hare from urging, against Prof. Dove and myself, his own exposition of the reported phenomena of such an overland storm, for the purpose of disproving rotation in tornadoes and hurricanes!—while he has passed unnoticed, reported observations of many storms of the open ocean and its neighboring flat coasts; observations which have fully proved the rotative character of such storms. (par. 124.)

I have already pointed out Dr. Hare's mistaken exposition of the phenomena recorded by Prof. Loomis of this overland storm; and the allusion now made by Dr. Hare to "the enormous length of the area of minimum pressure comparatively with its breadth," I conceive to be founded mainly in the error of mistaking the *path* of "the minimum pressure" for its *area* at a given moment.

The observed analogy of the action of small travelling whirlwinds to that of tornadoes and whirlwind storms, to which I had alluded, Dr. Hare labors to set aside, by once more assuming, erroneously, that "momentum" is the force by which the whirling body is kept in motion, and then alleging that the great superficies of a storm having a diameter two hundred times its thickness, must occasion the loss of its momentum. But he seems to forget that the same forces which produce rotation and momentum in a storm must be adequate to their maintainance. Prof. Dove shows that some of these forces are as enduring as the rotation of our planet and the movements and courses of its general winds; which coincides with my own views. Moreover, these constant rotative tendencies in the atmosphere can also be shown by an experiment, from which it will appear that the great extent of aerial surface in contact with the earth and with the surrounding atmosphere, in a storm, is peculiarly *favorable* to the development of these rotative influences.

In the case of the smaller whirlwinds, Dr. Hare now finds it necessary to admit the inward tendency of their rotation; which inward tendency, as I have formerly shown, is also exhibited in tornadoes, near the earth's surface; a fact which he had labored to disprove. A like tendency, I apprehend, is usually exhibited, though in quite a subordinate degree, in hurricanes and violent storms. And in cases where the successive changes in the direction and force of the wind in a storm have been observed and recorded, in different portions of its path, they have often been found analogous in character to those in smaller travelling whirlwinds.

As further relates to these small whirlwinds, Dr. Hare is greatly in error when he states, that "it is only when the wind blows briskly that such whirls are ever seen to take place;"—and he is likewise wrong in saying that "tornadoes agreeably to universal observation occur when there is little or no wind externally." I know of several cases which disagree with these allegations: and has Dr. Hare himself forgotten the state of the winds at the time of the New Brunswick tornado? A destructive tornado in Charleston, S. C., many years since, occurred during the prevalence of a storm. (par. 125–127.)

To me it appears, that the main course of discussion pursued by Dr. Hare in one hundred and twenty eight elaborate paragraphs, is essentially misapplied and erroneous. If the supporters of a rotative or whirlwind action in tornadoes and hurricanes had chosen to maintain their cause in a speculative manner, the case might have been different. But when their facts and results were offered on the basis of direct observations, which had been set forth, in many cases, with particularity and precision, it seems like a waste of words to assail these observed phenomena and results with *strictures* and *objections* of this character; volumes of which can never equal in value the direct observations which may be made of the phenomena of a single storm.

New York, February 7, 1843.

Art. XVI.—*United States Exploring Expedition.*

England and France have long been honorable rivals on the ocean, as well in exploring as in warlike expeditions. The voyages of Cook, Vancouver, Flinders, Parry, Beechey, King, Fitzroy, and Ross, are conspicuous in the annals of English navigation; while France is no less honored by her explorations under Bougainville, La Perouse, Labillardière, Duperrey, Freycinet, and D'Urville. Both countries have looked beyond the mere discovery of new lands, new commercial resources, and territorial aggrandizement. Their efforts have been directed towards an increase of knowledge in every branch of science, and there are few regions from the equator to the poles, which have not been tracked by their vessels. Whatever could illustrate the condition or resources of the regions visited; the customs, languages, or history of their unknown tribes; or the motion of the winds, the waters, the world, or the stars, has been thought worthy of observation. Cook was dispatched to the Pacific Ocean expressly to observe the transit of Venus, and Sir Joseph Banks and Forster accompanied him at different times in his voyages around the world. In the late voyage of Fitzroy, Mr. Darwin was associated with the expedition, and made large contributions to science. France has outstripped England in the liberality with which her expeditions have been fitted out, and in the magnificence of her publications. The many folio volumes of plates, published as the result of the voyages of Freycinet, Duperrey, and D'Urville, and those of Napoleon's expedition into Egypt, are among the most splendid productions of the age. They are a noble gift from France to the world.

America has at last taken her part in the labors of exploration. An Exploring Expedition has been sent out, and has returned. It was organized on a plan honorable to a nation that is second to none in enterprise and general education; and its results, when published, will, it is believed, equal in amount and interest, those of any expedition that has preceded it. The expedition sailed under the command of Lieut. Charles Wilkes, who was aided by intelligent officers, well fitted for the duties to which they were called; and the large number of charts that have been made in the course of the cruise, evince alike the energy of the commander, and the industry and skill of all engaged in the surveys. The duties have been extremely laborious, beyond the conception of the comfortable house-dweller at home. The loss of one schooner with all hands, including two officers; the total wreck of another vessel—the sloop of war Peacock—stripping the crew of every thing but their lives; the massacre of two officers by the savages of the Feejee Islands, and of a sailor by the treacherous Kingsmill Islanders, are the only fatal disasters: but they are a few only of its perils. Indeed there were dangers every where, by land as well as by sea. The personal adventures in the course of the cruise, told as simple tales, without exaggeration, would make a volume full of startling incidents, and replete with interest.

It is gratifying to learn that the country will soon be put in possession of the facts collected. Thus far those engaged in it have alone been benefited. They have collected information that will be invaluable to them as men of intelligence and members of society. It remains for them to give this information to the country, that the people who have borne the expense, may also partake of the profits. The affairs of the expedition are in the hands of the Library Committee of Congress, and under their direction, Captain Wilkes has been put in charge of the history of the voyage, the charts and philosophical observations, and the other departments of science are placed in the hands of those that had charge of them during the voyage. Each will prepare his own reports, reap his own honors, and be held responsible for his own facts. The extent of the work cannot be definitely stated: the plates will form several folio volumes in the style of the voyage of the Astrolabe.

As the country is much interested to know what has been done by the expedition, it is proposed to give, in as brief a manner as possible, some idea of the material on hand for publication, and the general character and extent of the collections. Our acquaintance with the gentlemen of the expedition, enables us to state many particulars which have not yet appeared in print, the accuracy of which may be relied on.

We prelude our remarks, by giving the track of the vessels as laid down in Capt. Wilkes's synopsis of the cruise.

On August 19, 1838, the vessels left the Capes of the Chesapeake and sailed for Rio Janeiro, making short calls at Madeira and the Cape Verds. From Rio, on the 6th of January following, they proceeded to Rio Negro, on the northern confines of Patagonia, and thence to Nassau Bay in Tierra del Fuego, just west of Cape Horn. From this place, the Peacock, Porpoise, and the two schooners, made cruises in different directions towards the pole; but the season was too far advanced for much success, as it was already February 24th before they sailed. The schooner Flying Fish, notwithstanding, reached latitude 70° 14′ S., nearly the highest attained by Cook, and not far from the same longitude. The ship Relief was ordered to enter a southern channel opening into the straits of Magellan, but met with constant gales, and barely escaped being wrecked, after a loss of four anchors, at an anchorage she had made under Noir Island, to escape the rocks of a lee coast. The Vincennes remained at Nassau Bay to carry on surveys and magnetic observations. In May of 1839, the vessels were again together at Valparaiso, with the exception of one schooner, the Sea Gull, which was lost in a gale shortly after leaving Nassau Bay. The vessels sailed on the 6th of June for Callao, Peru, and from here, the Relief, having proved ill-adapted for such a voyage, was dispatched home. On the 12th of July, the squadron left the South American coast and sailed west, visiting and surveying fourteen or fifteen of the Paumotu Islands, two of the Society Islands, and all the Navigator group, and on the 28th of November reached Sydney, New South Wales.

The vessels next proceeded on their second Antarctic cruise. Land was first discovered in longitude 160° E., and latitude 66° 30′ S. The Vincennes and Porpoise pursued the barrier of ice to the westward as far as 97° E. longitude, seeing the land at intervals for one thousand five hundred miles. When the barrier of ice permitted, the Vincennes sailed along "within from three fourths of a mile to ten miles of the land." In a place they called Piner's bay, soundings were obtained in thirty fathoms, and they had hopes of soon landing on the rocks; but a storm came up suddenly which lasted for thirty-six hours, and drove the vessel far to leeward; they consequently pushed on with their explorations to the westward, hoping for some more accessible place, but were disappointed.* Large masses of rock were collected

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* See the synopsis of the cruise by Capt. Wilkes.

from the icy barrier in close proximity to the land, which are now deposited in the National Gallery at the Patent Office. Two of the masses, one of basalt and the other of compact red sandstone, weigh each about eighty pounds. Besides these, there are many smaller specimens of gray and flesh-colored granite, gneiss, white and red sandstone, basalt, and reddish clay or earth. The Peacock was enclosed in the ice soon after reaching it, when penetrating towards an appearance of land ahead, and for twenty hours they were barely hoping for life. They had obtained soundings in 320 fathoms.* On the 24th of February, 1840, the Vincennes left the ice, and by the 24th of April, all the vessels were together at Tongatabu. During the Antarctic cruise, the scientific gentlemen were occupied making observations and collections in New Holland and New Zealand; they joined the squadron at the latter place.

After delaying a day or two at Tongatabu, the squadron proceeded to the Feejees, where nearly four months were industriously occupied in surveys and various scientific observations. Thence they sailed for the Sandwich Islands, passing on the way and surveying several small coral islands. The Vincennes spent the winter at this group, and in the course of it, the pendulum and other philosophical instruments were carried to the very summit of Mauna Loa, an elevation of fourteen thousand feet. Occasionally, at sunset, they observed the sublime spectacle of the shadow of this mountain dome projected upon the eastern skies.

During the same time the Peacock and schooner Flying Fish were cruising in the equatorial regions of the Pacific, visiting and surveying numerous scattered coral islands, besides the Navigator's and the Kingsmill group, and others of the Caroline Archipelago. The Porpoise made charts of several of the Paumotu Islands not before surveyed, and touched again at Tahiti.

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* There has been much incredulity in the country with regard to the discovery of this land, owing probably to mistaking the dispute with the French with regard to priority of discovery, for a dispute with regard to discovery itself. The facts here stated set the subject at rest. Within a few weeks, acknowledgments have reached this country from the French expedition, yielding the priority to the American expedition, and it will be so stated in their forthcoming publications. The part of the line of land which Ross is said to have sailed over, was a discovery claimed by Bellamy, and which Capt. Wilkes added to the chart he sent Capt. Ross, with Bellamy's name accidentally omitted in copying.

In the spring of 1841, the Vincennes and Porpoise were early on the coast of Oregon. The Peacock and Flying Fish arrived there in July, and while attempting to enter the Columbia, the Peacock met with her disaster. There were several land expeditions into the interior of Oregon, of from five hundred to one thousand miles each, and one of about eight hundred miles, from the Columbia River, to San Francisco in California.

The vessels left California in November of 1841, touched for supplies at the Sandwich Islands, and proceeded to Manilla in the Philippines; thence to Mindanao, and through the Sooloo Archipelago, and the straits of Balabac, to Singapore, which place they reached in February of 1842. They proceeded thence by the straits of Sunda to the Cape of Good Hope, and passing by St. Helena, the squadron arrived at New York in June of 1842, having been absent from the country about three years and ten months, and having sailed between eighty and ninety thousand miles.

The number of islands surveyed during the cruise of the exploring expedition, is about two hundred and eighty, besides eight hundred miles on the streams and coast of Oregon, and one thousand and five hundred miles laid down along the land and icy barrier of the Antarctic continent. Numerous islands of doubtful existence have been looked for, shoals have been examined, reefs discovered and laid down, harbors surveyed and many for the first time made known, and the latitudes and longitudes of the points visited have been determined with all possible precision. Very many of the doubtful points in the geography of the Pacific have been cleared up, and the expedition is prepared to supply our navigators with the most complete map of the ocean ever published.

Next to Oregon, the Feejee group may be considered the most important of the unexplored regions visited by the squadron. This group is a perfect labyrinth of lofty islands and coral reefs, and many disastrous wrecks have already occurred to our trading vessels in those seas. The islands are visited for biche-da-mar,* tortoise shell, and sandal-wood; and there is no part of the year in which there are not some Yankee cruisers threading their dan-

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* The biche-da-mar is a kind of sea-slug—a sluggish, cucumber-shaped animal, that lives about the reefs. It is boiled and dried over a smoking fire, and carried in ship-loads to the Chinese market, where it is esteemed a great delicacy.

gerous way among its thousand reefs. The whole number of islands in the group, is about one hundred and fifty; one of these contains about four thousand square miles, and another is but little smaller. They are rich and fertile, and will one day rank first in the Pacific for resources, as they are now first in extent and number. The harbors are numerous and convenient.

Much might be said of Samoa or the Navigator Islands, which, though less extensive, are more beautiful than the Feejees, and contain at least five times as much fertile land, in proportion to their extent, as the Sandwich Islands. But our remarks would lengthen out beyond allowed limits, should we speak even cursorily of the various regions that have been examined.

A few unknown islands were fallen in with, and one was discovered at midnight, just in time to avoid its reefs. But many such discoveries are not to be expected at this late day. At the island referred to, the natives were so completely ignorant of white men, as to believe them inhabitants of the sun; for they thought that the great ship, or "floating island," as they called it, might sail off from the sun when it comes to the surface of the sea at night, or leaves it in the morning. All their little property was brought out by the terrified people, as a peace-offering to their imagined deities; and when the boats shoved off from the shore, they pointed to the sun and asked in their language, "you going back again?"

Observations with the magnetic needle, thermometer, and barometer, have been constantly made throughout the cruise. The deep-sea lead with a self-registering thermometer attached, has been sent down in the various seas passed over, and many interesting facts have been observed, that throw light upon the upper and under currents of the ocean. Observations were also made on shooting stars, the zodiacal light, the aurora australis, tides, the course and rotary character of gales, &c. &c.

The manners and customs, mode of life, superstitions and religious observances, traditions, &c. of the people met with in the course of the cruise, received constant attention, and complete collections were made of their implements, manufactures, articles of dress, &c. These collections are now nearly arranged in the Hall or National Gallery at the Patent Office. Separate cases or parts of cases are allotted to the different islands or groups of islands, and when labelled throughout—which is now in progress—the condition of the various tribes or races, and the degree of civilization among them, will be at once apparent to the eye. By a walk through the National Gallery, we travel with more than railroad speed over the Pacific, and examine into their various productions and the relative intelligence of the savages. The degradation of the New Hollander stands out in bold relief in contrast with the more advanced, though no less barbarous Feejee. With the former, a war-club, and one or two other implements of war, including a small elliptical shield, is their all—there are no dresses, no household utensils, for they use neither, and live without houses. Two cases* are filled with articles of Feejee manufacture, and among them are war-clubs of various kinds, spears, bows and arrows, native cloth of numerous patterns, dresses of the men and women, with bracelets and necklaces of shells and human teeth, wigs of Feejee hair, showing the mode of dressing the head, native combs, paint for painting the face, their pillows, (a stick like a broom-handle supported on short legs at each end,) musical instruments, models of canoes—indeed all the arts and manufactures of the island are well represented; and were the chief Veindovi living, a visit to the hall with Veindovi at hand, would be little less interesting than visiting the islands themselves. One advantage at least—no danger would be apprehended from a ferocious race of cannibals, that are ready to attack all intruders into those seas. Several Feejee skulls are to be found in a separate case containing the skulls collected by the expedition. Among them, one bears the marks of the fire in a large burnt spot on the top of the head. Early one morning, soon after the Peacock came to anchor off a small Feejee town, she was boarded by a large number of natives, who came off with their half eaten bones in their hands—the remains of the past night's cannibal feast. They continued eating the human flesh on deck, as unconsciously as we would eat an apple. One had the skull just referred to in his hand, and as he consented to part with it for some trifle, he gouged out the remaining eye and went on eating off its muscles. This fact, so revolting, is here stated on account of the prevalent unwillingness to admit that cannibalism actually exists among savages. This was seen both by men and officers, and from the facts col-

* The glass cases in the hall measure twelve feet by four, and are eight feet high.

lected there can be no doubt of their entertaining an actual relish for human flesh. The pottery of the Feejees is among the most remarkable of their manufactures, as this art is not known to the Polynesian races. Collections equally curious were obtained at other places, but we must pass them by without remark.

The portfolios of the artists are rich in scenes of every kind illustrating the islands or regions visited, and their inhabitants. The scenery of the islands, their mountains and forests, their villages with interior and exterior views of huts and public houses—their spirit houses or temples—fortifications—household utensils—canoes—the natives in council—dressed and painted for war—the domestic scenes of the village—costumes—tattooing—modes of cooking, eating, drinking cava, taking and curing fish, swimming, gambling and other amusements,—their war-dances—club-dances—jugglery—and numerous other particulars illustrating their manners and customs have been sketched with fidelity. The portraits too are numerous, and so faithful that the natives who had not seen them taken, on beholding them would cry out with surprise the name of the individual represented.

The number of sketches of scenes and scenery amounts to more than five hundred, besides five hundred others of headlands; the number of portraits is about two hundred. They have been taken at all the places visited, from Madeira where the vessels first stopped, throughout the cruise, to St. Helena. It is unnecessary to enumerate the particular regions.

The principal importance of the observations and sketches illustrating the different races, consists in their bearing upon the history of these races, their migrations, and their physical and moral characteristics. These subjects, in connection with the study of languages, which together constitute the science of ethnography, received special attention during the cruise. The opportunities for observation have been unusually good, and the information collected will prove, it is believed, highly interesting. Only a few of the results can be here alluded to.

It has been long known that the inhabitants of the principal groups, scattered over the Pacific to the east of the Feejee islands —those usually included under the general name of Polynesia— belong to one race, and in fact are one people, speaking dialects of one general language closely allied to the Malay. Materials have been obtained for a comparative grammar and dictionary of the most important dialects, (including those of the Sandwich, Society, Friendly, Navigator, and Hervey islands and New Zealand,) and from this comparison and the traditions of several of these islands, it is believed that the original seat of the population—viz. in the Navigator Islands—has been satisfactorily determined, and the course of the migrations has been traced out by which the different groups were peopled.

The vast island or continent of New Holland has heretofore been generally supposed to be inhabited by numerous tribes speaking languages entirely distinct. An opportunity however was found of obtaining a grammatical analysis of the languages of the inhabitants of two tribes living more than two hundred miles apart, and ignorant of each other's existence; which has resulted in showing a clear and intimate resemblance, not merely in the great mass of words, but in the inflections and minute peculiarities of the two languages. By the aid of several vocabularies, the comparison has been extended across the entire continent, and has afforded fair grounds for believing that the inhabitants of New Holland, like those of Polynesia, are one people, speaking languages derived from a common origin. Much information was obtained from the missionaries and others, concerning the character, usages, and religious belief of this singular race.

The inhabitants of the extensive and populous Feejee group have been viewed with peculiar interest, from their position between the yellow Polynesian tribes on the east, and the Oceanic negroes on the west. The result of inquiries, pursued with care during a stay of nearly four months, has been to throw new and unexpected light on the origin of this people, and their connection with the neighboring races. A mass of minute information in regard to the customs, traditions and languages of these islanders, including a grammar and a dictionary of about three thousand words, will be given to the public.

The Kingsmill Islands are another interesting group, first accurately surveyed by the vessels of the expedition. They lie in the western part of the Pacific, directly under the equator. They are sixteen in number, all of coral formation, the highest land on any of them rising not more than twenty feet above the level of the sea, and their united superficies not exceeding a hundred and fifty square miles. They afford no stone but coral, no quadrupeds but rats, and not more than thirty species of plants. Yet on this confined space, thus scantily endowed by nature, was found a dense population of more than sixty thousand souls, in a state not inferior, as regards civilization, to any of the other islands of the Pacific. It is obvious that the character and customs of this people, as modified by their peculiar condition, must have presented much that was novel and striking. By the aid of two sailors who were fortunately found living on these islands—one of whom had been detained there five years without an opportunity of escaping—these points were minutely examined, the relations of the language determined, and the probable origin of the natives ascertained.

In the territory of Oregon, vocabularies have been obtained of twenty six languages belonging to thirteen distinct families—a surprising and unexampled number to be found in so small a space. In general, where a multitude of unrelated idioms have been believed to exist, more careful researches, by discovering resemblances and affinities before unperceived, have greatly reduced the number. On the northwest coast of America, however, this rule does not hold good, and careful investigation, instead of diminishing, has actually increased the number of languages between which no connection can be proved. On the other hand, traces of affinity have been discovered where none were supposed to exist; and it is worthy of note, that one family of languages has been found extending from the vicinity of Bheering's Straits to some distance south of the Columbia River.

At Singapore, the expedition procured from an American missionary there resident, a collection made by him with great pains and at considerable expense, of valuable Malay and Bugis manuscripts, relating to the history, mythology, laws, and customs of the East India islands. Since the loss of the splendid collection of Sir Stamford Raffles, which was burned along with the vessel in which it had been shipped for England, this is believed to be the best in existence. It is likely to be of great service hereafter, not less to the historian, than the philologist.

The birds of the expedition already make a fine display in the National Gallery, although but two thirds are yet arranged. In all there are about a thousand species collected, and double that number of specimens. Contrary to expectation, many of the birds of Oceania were found to have a very limited range. Some of the groups have species peculiar to themselves, and several insessorial species were found to be confined to a single island. About fifty new species were obtained.

The field for mammalia afforded by the voyage has been very limited. None of the Pacific islands, including New Zealand, contain any native mammalia, except bats. Much interesting information was however obtained relative to species met with on the continents visited, and a few new species were collected.

The following is a list of the number of species in the other departments of zoology, as nearly as can now be determined:—

Fishes,	829	Shells,	2000
Reptiles,	140	Zoophytes, exclusive of corals,	300
Crustacea,	900	Corals,	450
Insects,	1500		

Of these the number of new species is nearly as follows:—

Fishes, about	250	Shells,	250
Reptiles,	40	Zoophytes, exclusive of corals,	200
Crustacea,	600	Corals,	100
Insects,	500		

The following catalogue contains the number of species of reptiles and fishes collected at the islands and countries visited:

	Fishes.	Reptiles.
Madeira and Cape Verds,	12	6
Rio Janeiro,	104	25
Patagonia and Tierra del Fuego,	14	5
Valparaiso,	32	11
Peru,	56	10
Paumotu Islands and Tahiti,	87	7
Samoa (or Navigators),	64	8
Australia,	30	18
New Zealand,	25	6
Tongatabu and Feejees,	131	15
Sandwich Islands, about	100	4
Oregon, about	60	15
California, "	20	2
Sooloo Sea,	18	8
Manilla,	32	1
Singapore,	21	9
Cape of Good Hope,	4	
At sea,	9	

Of the six hundred new species of crustacea, about two hundred are oceanic species, of many of which, even the genera or families are unknown. The ocean swarms with minute crustacea, and it is seldom that a hand-net is thrown in good weather without bringing up some novelty. In some seas they are so numerous as to color the ocean red, over many square miles of surface, as was observed off the South American coast near Valparaiso. These are the red or bloody waters that have been described. When thus numerous, these animals are often called whale's feed, and it is believed that they are actually the food of the "right whale." Each animal is not over a twelfth of an inch long, yet they swarm in such numbers as to afford subsistence to these monsters of the deep. The fibrous net-work of whalebone, in the roof of the whale's mouth, is fitted to strain out these animals from the water which passes through and is ejected by the spout-holes. Many minute dissections have been made of these and other crustacea, and some interesting physiological facts brought to light. As the species are often transparent, nearly all the processes of life, even to the motion of every muscle and every particle that floats in the blood, are open to view.

The Anatifa (a species of barnacle) has been traced through its metamorphoses, from the young state when it resembles a Cypris and swims at large with distinct compound eyes, to the adult animal; and its connection with crustacea is placed beyond doubt.

The collection of corals at the National Gallery is one of its principal attractions. The great beauty and variety of these productions is not conceived of, even by those best acquainted with other collections in our country. These are the material that constitutes the immense reefs of the Pacific and East Indies—some of which exceed a thousand square miles in extent. More than three fourths of all the islands of this great ocean have been built up through the labors of the coral animal. The formation of these islands, and the growth of the coral animal, the filling up and opening of harbors, and the rising of reefs—all interesting subjects of discussion, received particular attention; and the number of coral islands visited, and reefs examined, have afforded unusual opportunities for these investigations. Colored drawings have been made of a large number of coral animals, which will convey some idea of their singular beauty and richness of colors. Many of these animals are wholly unknown to science, as this is a branch of zoology to which comparatively little attention has heretofore been paid, on account of the inaccessible regions in which they occur.

The following is the number of zoological drawings made during the cruise, in the departments of science here enumerated:

Reptiles, ...	75	species.
Fish, ...	260	"
Mollusca, (shells with the animals,)	500	"
Zoophytes, (exclusive of corals,)	350	"
Corals, ...	140	"
Crustacea, ...	500	"

The variety and beauty of marine animals in the coral seas of the Pacific are beyond description. Like birds in our forests, fish of brilliant colors sport among the coral groves, and various mollusca cover the bottom with living flowers. A new world of beings is here opened to an inhabitant of our cold climate; and many of these productions are so unlike the ordinary forms of life, that it is difficult without seeing them, to believe in their existence. Those that have looked over the beautiful colored drawings by the artists of the expedition, are aware that this description falls short of the truth.

A large number of new species yet remain to be drawn. While there were so many things requiring immediate attention, it was impossible to sketch all, and those were selected for sketching on the spot, whose forms and colors were most liable to change.

Ten thousand species of plants, and upwards of fifty thousand specimens, constitute the herbarium of the expedition. The following catalogue gives the number of species collected at the several places visited:—

Madeira, . .	300	Feejee Islands, .	786
Cape Verds, . .	60	Coral Islands, .	29
Brazil, . . .	980	Sandwich Islands, .	883
Rio Negro (Patagonia),	150	Oregon, . .	1218
Tierra del Fuego, .	220	California, . .	519
Chili, . . .	442	Manilla, . .	381
Peru, . . .	820	Singapore, . .	80
Tahiti, . . .	288	Mindanao, . .	102
Samoa (Navigator Ids.),	457	Sooloo Islands, .	58
New South Wales,	787	Mangsi Islands, .	80
New Zealand, .	398	Cape of Good Hope,	300
Auckland Islands, .	50	St. Helena, . .	20
Tongatabu, . .	236		9646

Including the mosses, lichens, and sea-weeds, the number will exceed ten thousand. Besides dried specimens, two hundred and four living plants were brought home, and are now in the green-house in the yard of the Patent Office, along with many others raised from seeds. The kinds of seeds obtained, amount to eleven hundred and fifty six. Many of the expedition plants are now growing in the various green-houses of the country, and also in England and Europe. Specimens of different woods have been preserved, the most interesting of which are those of large arborescent species of Oxalis, Viola, Ripogonum, Piper, Geranium, Argyroxiphium, Dracophyllum, Rubus, Bromelia, Lobelia and Compositæ of various kinds, besides sections of the Tree Ferns and Palms of the tropics. There are colored drawings of one hundred and eighty species of plants, beautifully executed.

Besides the observations at which we have glanced, in the departments of zoology and botany, particular attention was paid to the geographical distribution of plants and animals, and many important facts have been ascertained. The reports on this subject, with the accompanying illustrative maps, will be found to be among the most interesting of the results of the expedition. This subject bears upon the distribution of fossil animals, and the early history of our globe, and is exciting much attention among those interested in geological investigations.

The regions examined by the expedition have been highly interesting in a geological point of view. The islands of the Pacific east of New Caledonia are either basaltic or coralline. A large number of the latter (as already stated) have been examined, and much that is important has been brought to light. The facts strongly confirm Darwin's theory with regard to the formation of these islands, but lead to very different conclusions respecting the areas of subsidence and elevation in the Pacific. Numerous facts bearing upon this subject were collected. The basaltic islands are of various ages, from the most recent volcanic to a very remote period—probably as far back as the middle of the secondary era. The older islands are remarkable for their singular topographical features. There is scarcely any part of the world where such profound gorges, and sharp and lofty peaks and ridges, are thrown together in a manner so remarkable. On one of the high ridges of Tahiti, (Society group,) about six thousand feet above the sea, the summit edge is so sharp, and the sides of the mountain so nearly vertical, that the adventurous traveller may sit astride of it, and look down a precipice of a thousand feet on either side. In no other way except by thus balancing and pushing himself along is it possible, for about thirty feet, to advance towards the summit before him—yet a thousand feet higher—for the bushes which are growing on the crest elsewhere and serve as a balustrade, are here wanting. The famous coral bed on the mountains of Tahiti, was looked for without success.

The Sandwich Islands contain basaltic rocks of all ages, from the most recent volcanic to the most ancient in the Pacific, besides coral rocks and elevated reefs; and they are full of interest, both as regards the structure and formation of igneous and limestone rocks, and geological dynamics. The lofty precipices and examples of shattered mountains before the eye, are astounding to those who see only the little steeps, of a few hundred feet at most, in the surface of our own country. There is evidence that the island of Oahu is the shattered remnant of two lofty volcanic mountains. A precipice on this island, upwards of twenty miles long and from one to three thousand feet high, is apparently a section of one of these volcanic mountains or domes, along which it was rent in two, when the greater part was tumbled off and submerged in the ocean.

Oahu is fringed in part with a coral reef, twenty five feet out of water; and similar proofs of still greater elevation are met with on the other islands.

New Holland afforded the expedition a collection of coal plants from the coal region; the coal is bituminous and the beds are extensive. Large collections were also obtained of fossil shells and corals, (about one hundred and eighty species in all,) from the sandstone next below the coal. The geology of the coal region, and of the overlying sandstone, and the fossiliferous sandstone below, together with the trap dykes and beds, will prove highly interesting. These are the only rocks observed.

About one hundred species of fossils, including vertebræ of cetacea, and remains of four species of fish, crabs, echini and shells, were collected from a clayey sandstone, near Astoria, on the Columbia. Various explorations were made in the interior of Oregon, and on a jaunt overland to California.

The Andes were ascended both in Chili and Peru, and in the latter, an ammonite was obtained at a height of sixteen thousand feet.

The collections at the National Gallery contain suites of specimens from all the regions visited, including gems, and gold and iron ores from Brazil, the copper and some of the silver ores of Peru and Chili, besides others illustrating the general geological structure of these countries.

But our remarks have already extended to an unexpected length. The facts enumerated, although but here and there one from the mass which have been collected, are sufficient to evince that the nation which has done honor to itself in sending out an exploring expedition so liberally organized, will have no reason to be disappointed in the results. European nations already appreciate it, and speak higher praise than has yet been heard on this side of the waters. The advantages accruing to commerce alone, from the large number of surveys made, reefs discovered and laid down, unknown harbors examined, resources of islands and countries investigated—and from the permanent footing on which intercourse with the Pacific islands has been placed by the settlement of long standing difficulties and the ratification of treaties, and the impression produced by an armed force, more than repay for expenditures. The expedition has performed the duties of an ordinary squadron in the Pacific, and has accomplished in this way many fold more in that ocean, than any squadron that ever left our country; and if the expenses of keeping the vessels in commission are cancelled on this score, the sum which remains for the extraordinary duties performed will be but small.

But while we render to those whose labors have obtained the results of the expedition their full due of credit, we cannot forget that there are others, and one in particular, whose zeal and untiring exertions in planning, and urging forward to its completion this enterprise, deserve more than a passing acknowledgment. Mr. J. N. Reynolds was left behind, yet, though unrewarded for his efforts by the pleasure of accompanying the expedition, and adding to its laurels, his distinguished merits will not be forgotten or disregarded by his countrymen.

Art. XVII.—*Analysis of the Scott Spring, Scott County, Virginia*; by C. B. Hayden, of Abingdon, Va.

The uniform temperature of this spring, (68½°,) fifteen or sixteen degrees higher than the average temperature of the springs of the vicinity, classes it among the *thermal* waters, and renders it a natural medicated warm bath, subserving all the purposes of health and luxury, without being sufficiently high to impart to it the usual disagreeable flavor of warm waters.

One wine gallon contains 41.14 grs. of saline matter, consisting of

Chloride of sodium, Chloride of aluminium,	1.51 grs.
Sulphate of soda,	a trace.
Sulphate of magnesia,	12.75 grs.
Phosphate and sulphate of alumina,	a trace.
Carbonate of lime,	6.42 grs.
Sulphate of lime,	20.46 "

Art. XVIII.—*Notice of the Discovery of a nearly complete Skeleton of the Zygodon of Owen (Basilosaurus of Harlan) in Alabama*; by S. B. Buckley, A. M.

Some years ago a few imperfect vertebræ of this animal were sent to Philadelphia, which were found near the Wachita River in Louisiana. These were described by Dr. Harlan in 1834, and referred to a lost genus of the Saurian order. From the great size of the bones, he called it the Basilosaurus. Subsequently Harlan obtained other bones of his Basilosaurus, which were found on the plantation of Judge Creagh, of Clark County, Alabama, and forwarded to Philadelphia by that gentleman. These were one or two fragments of the jaws with teeth, of which the upper portion was broken off and lost, also pieces of ribs, with some other long bones belonging to its paddles, and several vertebræ with the processes broken off. These Harlan also described in the "Transactions of the American Philosophical Society." Part of these bones were taken by Harlan to London, where they were pronounced by Owen, from a microscopical examination of the teeth, to belong to a genus of mammalia between the Saurians and Cetacea. He named it the Zygodon, in allusion to the curious form of the molar teeth.

Our skeleton was discovered on the plantation of Judge Creagh, the same gentleman who forwarded the bones already noticed to Harlan, and from the same neighborhood in which those were obtained. The entire vertebral column is nearly perfect, except two or three of the cervical, which are much broken, and it is possible that others from the same part of the skeleton are lost, since the vertebræ near the head were disjointed and scattered over a surface of several feet, but the remaining portion of the vertebral column was in an almost unbroken series to the extreme tail. The entire length of the skeleton, including the head, is nearly seventy feet! Some of the ribs must have been upwards of six feet in length, but of these we only have fragments, including their extremities and central parts. We have also other long bones belonging to its paddles, as the animal was probably an inhabitant of the water. These are small in proportion to the size of the other bones. The principal organ of locomotion of the animal seems to have been its tail, which is short and thick. Many of the dorsal vertebræ are sixteen or eighteen inches long, and upwards of twelve inches in diameter. The transverse processes are from three to six inches long. The spinal and also the lateral processes are of about the same length. These last three are united at the base, where they form an arch through which the spinal marrow ran. This arch with the lateral and spinal processes is easily detached from the main body of the vertebræ. The head is much broken, yet we have portions of both jaws with the teeth inserted in nearly a perfect state. The molar teeth are inserted into separate cavities of the jaw by two long roots. The upper portion of these teeth is somewhat hastate, with large and rather blunt serratures on the lower part of the anterior and posterior margins, as in those of the Iguanodon. The average longer diameter of a section of the molar teeth is about four and a half inches. The anterior teeth have a single root, are sharp-pointed, conical, slightly curved, and laterally compressed; the transverse section parallel to the base forming an ellipse. The length of the anterior teeth, including the root, is five or six inches, and the longest diameter nearly two inches. The form of the molar teeth is so peculiar that it is impossible to give a correct idea of them without the aid of plates.

This Zygodon or Basilosaurus was imbedded in a marly limestone soil. The upper portion, to the depth of one or two feet, is a rich black vegetable mould. Beneath this is a yellowish white marl, yielding easily to the mattock, and containing few organic remains. Most of the bones were in this marl from one to six feet beneath the surface. At the depth of about six feet is a green sand or marl resembling the green sand of New Jersey, and containing few organic remains. The vertebral column, as has been before remarked, lay in an almost unbroken series from the head to the extreme tail, and appeared to occupy the place upon which the animal died. The bones are more or less fossilized, having lost nearly all the animal matter, and been penetrated by carbonate of lime. Yet a large portion of their surface retains the smooth and ordinary appearance of bone. The enamel of the teeth is also retained. Numerous sharks' teeth and shells are scattered over the surface, or imbedded in the soil. The most common of the shells are several species of the genera Ostrea, Exogyra, Pecten, Echinus, Conus, and Scutella. The rocks of the immediate vicinity are limestone, which is sometimes as white and nearly as soft as chalk, but destitute of flints or organic remains. This variety is often sawed into blocks and used for building chimneys. The rock partly surrounding the field in which our bones were discovered, is a white limestone filled with nummulites. The gray limestone, with more or less organic remains, is the prevailing rock of the immediate neighborhood. These limestones often present an almost perpendicular escarpment of rock, sometimes in the form of little islands, against which the waves of the olden time appear to have dashed. The spot seems once to have been an estuary or arm of the sea, interspersed with small islands, and here the Zygodon appears to have lived. Bordering the limestone within a mile of this place, is a red sandstone apparently destitute of organic remains. This forms the most elevated part of the country, and extends over a large portion of Clark County, affording a poor soil, of which the prevailing timber is the long-leafed pine (*Pinus palustris*) associated with dwarf oaks. This sandstone often affords hollow cylinders several inches in diameter and from one to three feet long, the cavities of which frequently contain a red ochre, (oxide of iron,) sometimes used by children as a pigment.

Bones of the Zygodon have been seen in Washington County, Mississippi, and from thence they have been found in several places as far east as Claiborne, on the Alabama River. Judge Creagh relates, that when he first moved to Clark County, about twenty years ago, these bones, consisting mostly of large vertebræ, were so numerous as seriously to interfere with the tillage of some of his fields, and hence they burned large quantities of them in the fires of their log heaps. At this time scattered vertebræ, generally much broken and wanting processes, are lying on the surface of the ground in almost every field of Judge Creagh's and the neighboring plantations. Among these no head or part of one is known to have ever been seen, except those parts which Dr. Harlan described, and these in our possession. The reason of this is, that the jaws were hollow and composed of a thin plate or plates of bone filled with animal matter; and when this matter contained in the cavities was destroyed, the exterior plates were easily broken. It may be well to mention that Clark County is situated between the Alabama and Tombigbee Rivers, about one hundred miles north of Mobile.

For a knowledge of this huge being of a former and remote age, the public are greatly indebted to Judge Creagh, who kindly assisted and furnished hands to assist in digging out the bones, and provided materials to make the boxes for containing them. He also had them conveyed to the Tombigbee River, (twelve miles,) after which he refused to take any compensation from one who went to his house a stranger, without even a letter of introduction. The skeleton was sent via Mobile to New York, where it is at present in fourteen large boxes, some few of which have been opened to gratify the curiosity of several scientific gentlemen, who are ready to testify that we have a unique and veritable skeleton of the Zygodon.

ART. XIX.—*Notice of the Great Comet of* 1843.

ON Tuesday, the 28th of February, 1843, a brilliant body resembling a comet, situated near the Sun, was seen in broad daylight, by numerous observers in various parts of New England. It was discovered on the same day at Waterbury, Salem, and Wolcott, Conn.; New Bedford, Braintree and Haverhill, Mass.; Woodstock and Rutland, Vt.; Plymouth and Concord, N. H.; Gray and Portland, Me., and doubtless also in many other places from which no announcement has been received.

From Drs. G. L. Platt and M. C. Leavenworth, and Messrs. S. W. Hall, Alfred Blackman, and N. J. Buel, of Waterbury, who in common with a large part of the adult population of that town, observed this remarkable phenomenon with great interest, the following particulars have been obtained. The comet was there first noticed as early as half past seven o'clock in the morning, the sky at the time being quite clear. It was seen as late as 3 P. M.; after which time the sky was considerably obscured by clouds and haziness. The appearance was that of a luminous globular body with a short train;—the whole taken together being estimated at two or three degrees in length; its position, east of and below the Sun. A comparison of the various diagrams made by observers at Waterbury, gives the place of the nucleus of the comet at 10 A. M., Feb. 28, in R. A. $345\frac{1}{2}^{\circ}$, S. decl. 9°; but as the diagrams are not entirely consistent, this determination cannot be relied upon within about one degree in each element. It is greatly to be regretted that the observations were not more precise, but it is to be hoped that accurate determinations were made by navigators in various parts of the world, by some of whom the comet was undoubtedly detected on that day.

The nucleus of the comet, as observed by the naked eye, was distinctly orbicular in form, its light equal to that of the moon in midnight in a clear sky; and its apparent size about one eighth the area of the full moon. Some of the observers compared it to a small cloud strongly illuminated by the Sun. The train was of a paler light, gradually diverging from the nucleus and melting away into the brilliant sky. To one observer, the train seemed to diverge for nearly half its length, and then to converge, forming a double convex figure.

An observer at Woodstock, Vt. states that the comet was there seen at noonday on the 28th, at an estimated distance of 5° or 6° east from the Sun. Its length was nearly 3°, and appeared like a small white cloud. "On viewing it through a common three feet telescope of moderate power, it presented a distinct and most beautiful appearance,—exhibiting a very white and bright nucleus, and a tail dividing near the nucleus into two separate branches, with the outer sides of each branch convex, and of nearly equal length, apparently 8° or 10°, and a space between their extremities of 5° or 6°."

The observers at New Bedford, estimated the brilliancy of the comet to be equal to that of Venus, (intending doubtless her brilliancy at night,) and the length of the tail about 3°.

At Portland, Me., Capt. Clark measured the distance of the nucleus from the nearest limb of the Sun, and found it to be 4° 6′ 15″. The hour of observation is not stated, nor the direction of the body from the Sun.

It is not certain that the comet was seen by daylight after the 28th of February. There is however some reason to suppose that it was seen the next day by observers at Waterbury. On the 3d, 6th, and 7th of March several unsuccessful attempts were made here to discover the comet while the Sun was above the horizon. Many persons in various places imagined that they saw the comet in the day time during the first week in March, with the naked eye, but they were probably deceived. It might undoubtedly have been seen in suitable telescopes for several days following the 28th, and it is to be hoped, that the very important observations which could then have been made, were secured at some of the European observatories.

In the evening sky the train was detected, as a narrow luminous streak in the twilight, by a few observers in this country, about the 1st of March, but it appears to have received from them no special attention. On the evening of the 2d of March it was seen in much brilliancy at St. Thomas, and in lower latitudes may have been seen in greater splendor a day or two earlier. On the evening of the 3d it was observed at Key West, and excited much attention. The next evening it was noticed at Waterbury, and on the evening of the 5th also by several persons in this city. On the evening of the 6th it was more generally observed. About seven o'clock it presented a long, narrow, and brilliant beam, slightly convex upwards, the lower end being apparently below the horizon. The presence of the moon six days old impaired, of course, the brilliancy of the light, but notwithstanding this, the train shone with the distinctness and splendor of a bright auroral streamer. Rising at an angle of about 29° with the horizon, and cutting it at a point about S. 62° W., the train extended for 30° along the southern part of the constellation Cetus, grazing on its southern margin *τ Ceti*, and terminating apparently near τ^2 *Eridani*. The breadth of the train was about 2° at its upper extremity, and less than 1° where it was lost in the vapors on the horizon.

The next evening (7th) was clear, and notwithstanding the moonlight and twilight, as early as seven o'clock, the comet presented a magnificent spectacle. It extended through an arc of about 43°, its lower end nearly touching the horizon, while the upper reached nearly to the star 19 *Eridani.* Its breadth near the horizon was less than one degree; and gradually increased towards the upper extremity, where it may have been equal to two degrees and a half. The curvature of the train upwards, although very noticeable, scarcely exceeded two degrees. The light was nearly uniform, and similar to that of illuminated fog. This evening the nucleus appears to have been for the first time discovered after sunset, by Mr. S. J. Parker, at New York, about 7 o'clock, with the aid of a small telescope. He describes it as then "quite large, and so rare that it gave but little light." This may have been owing to the twilight and the vapors about the horizon. A few observers in this city supposed they saw the nucleus with the unaided eye the same evening, at 7 o'clock, or a few minutes earlier; describing it as a hazy star enveloped in a coma, between which and the bright part of the train, the connection could be traced only by indirect glances. With the telescope, the nucleus was seen and its place determined by Mr. W. C. Bond, at Cambridge, Mass., on the 9th; by Mr. Wm. Mitchell, at Nantucket, Mass., on the 10th; and by observers here and in various other places on the 11th. The apparent place of the nucleus, (without any correction,) as estimated here at 7h. 5m. on the 11th, was in R. A. 1h. 43m., S. dec. 11° 35′. Its appearance in the telescope, with a magnifying power of 55, was that of an indefinite globular body, somewhat elongated behind, with a concentration of light near or a little in advance of the centre, which at times seemed to consist of three faint stellar points. The apparent diameter of the nucleus with its nebulosity, was 3′ or 4′, but as the outline faded away imperceptibly, no precise measurement could be made. It appeared a very inconsiderable mass, in comparison with the immense train by which it was attended. The train immediately proceeding from the nucleus had apparently only about one half its breadth, but this appearance is probably due to the loss of light in the telescope.

Since the 11th, numerous measurements of the place of the nucleus have been taken with the micrometer attached to the Clark telescope belonging to Yale College; but these observations have not hitherto been available, for want of full catalogues of the stars.

On Friday evening, the 17th March, the moon being fairly out of the way, the comet shone with great brilliancy and beauty. The curvature of the train was less than on former occasions. Its length was now only about 34°, the extremity terminating a little beyond the star 60 *Eridani.* After this date the comet gradually faded, the nucleus advancing in R. A. and diminishing in southern declination, while the extremity of the train scarcely advanced in R. A., although it moved slowly towards the equator. It was last seen here by the naked eye on the evening of April 3, when, in consequence of the advancing light of the moon, it was barely discernible. The nucleus will probably be visible in the telescope for some time to come.

Numerous observations of the position of the nucleus, were made on and after the 11th, by Mr. S. C. Walker and Prof. E. O. Kendall, with the aid of the excellent instruments at the High School Observatory in Philadelphia. From measurements taken on the 19th and 26th of March, and 2d of April, these gentlemen computed a third set of elements as follows:

Perihelion passage,	Feb. 27.240348 m. t. Phil.
Longitude of Ascending Node,	1° 16′ 21″.4 ap't. Eqx. Mch. 26.
Inclination, ...	35° 40′ 51″.8
Longitude of the Perihelion,	276° 59′ 32″.5
Perihelion distance, . . .	0.00818296
Motion, retrograde.	

In the communication announcing his second set of elements published in the United States Gazette, of Philadelphia, March 27, Mr. Walker remarks:—"These elements do not agree with those of any comet on record; it must therefore be new. They account for the comet's being seen in the day time on the 28th February, and 1st of March. It had just passed its perihelion; and on the 28th was far enough east of the sun to be seen in the position quoted by the observers at [Waterbury, Ct.] Woodstock, Vt., Portland, Braintree, New Bedford, &c.

"The great comet of February, 1843, is one of the most remarkable that has ever appeared in the history of the world for its physical peculiarities. * * It is not less remarkable in its geometrical relations. Of all the comets on record whose elements have been computed, (about one hundred and forty five in number,) this of February 1843 approaches nearest the sun except the great comet of 1680, whose perihelion distance according to the accurate computations of Encke was about six hundred thousand miles from the sun's centre. That of the present comet is about eight hundred thousand. When we consider that the sun's surface is four hundred and forty thousand miles from its centre, we find that both comets approached much nearer the sun's surface than that surface to the centre.

"The *period* of the comet of 1680 is somewhat remarkable. Encke found that one of *fourteen thousand* years would suit the observations rather better than the supposition of its moving away in a parabola never to return. This affords some ground for conjecture concerning the period of the present comet.

"Astronomers have dwelt with astonishment on the rapidity with which the comet of 1680 whirled round the sun at the instant of its perihelion passage. This was such that if continued, it would have carried it ten times round the sun in one day. The present comet would have gone five times round the sun in the same time. In fact, it went half round in four hours from two hours before to two hours after its perihelion passage. The elements of the present comet require nice observations for their determination. Though only twenty seven days past its perihelion, this comet has 169° of anomaly in its parabolic orbit. This anomaly is far greater than that at which all comets except that of 1680, have disappeared from view. Indeed, so unexpected is the circumstance of a comet's being seen at this anomaly, that Burckhardt extended his table of anomalies of comets, only to 164°, in consequence of which Prof. Kendall and myself had to compute a new table for our own use for the occasion. Some idea of this remarkable peculiarity may be formed by considering that a comet having an average perihelion distance, (the mean distance of the earth for instance,) would be a whole century in arriving at that point of its orbit to which the comet of 1843 has passed in less than a month. This circumstance accounts for the bad success of the first attempt to determine the elements from only approximate estimates of the comet's place,—a method which with ordinary comets, usually affords satisfactory information of the general character of the orbit."

Of all the comets hitherto recorded, six only are known to have been visible in the presence of the Sun—viz. those of 43 B. C., A. D. March 1402, June 1402, 1532, 1577, 1744.

A letter from Prof. Loomis of Hudson, Ohio, dated March 27, states that on account of clouds he had been able to secure only three observations on the nucleus, viz. on the 11th, 21st, and 25th. Before this last observation, Prof. L. had made a computation of the elements founded upon the first two observations, and an estimate of the position of the nucleus as reported on the 28th of February. With the excellent instruments of the Hudson observatory, Prof. L. has no doubt ere this secured a series of accurate observations; but on account of his distance from us, we cannot expect to receive his elements in season for the present number.

From observations taken by Mr. Wm. C. Bond, at Cambridge, Mass., the following elements have been computed by Prof. Benjamin Peirce.

Perihelion Passage,	Feb. 27.01 m. t. Cambridge.
Longitude of Ascending Node, . . .	348° 33′
Inclination, ...	39 16
Longitude of the Perihelion, . . .	280 31
Perihelion distance, ...	0.00872
Motion, retrograde.	

In order to verify the elements published by Mr. Walker, computations were made here several days since, by Messrs. James Nooney and James Hadley, from observations taken at Philadelphia previous to March 27,—and with the following result:

Perihelion Passage,	Feb. 27.203 m. t. Greenwich.
Longitude of Ascending Node, . .	356° 31′
Inclination, ...	36 37
Longitude of the Perihelion, . . .	272 19
Perihelion distance, ...	0.0147
Motion, retrograde.	

The following Ephemeris of the Comet was computed from the elements last stated, for one hour after midnight, Greenwich mean time.

(*Geoc. Long. and Lat.*)

Days.	Longitude.	Latitude south.
April 7th, . . .	64° 10½′ .	26° 33½′
" 10th, . . .	66° 44¾′ .	26° 28⅚′
" 13th, . . .	69° 03′ .	26° 23′
" 15th, . . .	70° 32⅙′ .	26° 19′
" 18th, . . .	72° 34¾′ .	26° 12½′
" 21st, . . .	74° 29′ .	26° 06′

P. S. In a communication published in the *Philadelphia Gazette*, April 6, 1843, Messrs. Walker and Kendall remark, that the present comet may perhaps be that of December, 1689, with the elements of which, excepting the inclination, this agrees tolerably well.

New Haven, April 7, 1843.

MISCELLANIES.

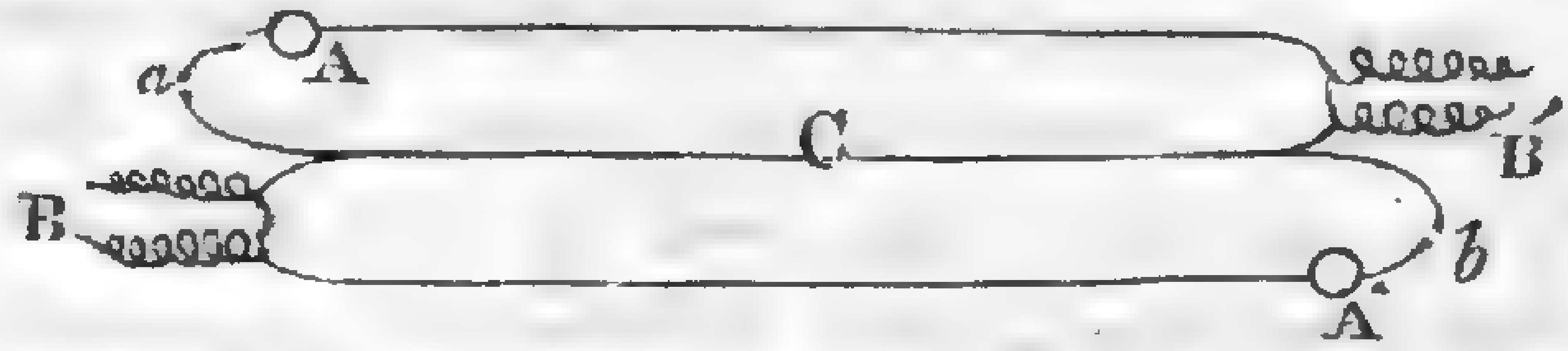
DOMESTIC AND FOREIGN.

1. *Fossil Birds.*—Letters from Dr. G. A. Mantell, London, and Prof. Chas. Daubeny, Oxford, Eng., to the senior Editor, mention the arrival in England of the bones of a gigantic Struthoid bird, from New Zealand. The impress made by its feet would fully equal the largest tracks observed by Dr. James Deane and Prof. Hitchcock, in the val-

ley of the Connecticut. The arrival of these bones, and of a fine collection of the American foot-prints in the rock forwarded by Prof. Silliman to Dr. Mantell, on behalf of Dr. Deane, have produced a decided conviction on the part of Dr. Mantell and of Mr. Murchison, the president of the Geological Society, as well as of the society itself, that the impressions in the valley of the Connecticut, and also certain others found in the red sandstone of New Jersey, and forwarded by Mr. Redfield, are genuine, and that birds both small and gigantic, once walked over the surface of the rocks in question, when they were soft.

The bones of birds from New Zealand were found in an alluvial formation, and a tradition still exists among the natives, that only within a century past have the birds themselves become extinct. The bones have been referred to Mr. Owen, who will report on them at an early session of the society. Bones of the Mastodon have also been received in England from New Holland.

2. *Passage of two or more Electrical Currents over the same Conductor without interference;*—extract from a letter of Prof. S. F. B. Morse, dated Jan. 17, 1843.—Prof. Fisher and myself made an important discovery just before we left New York, namely, that several currents of electricity will pass upon the same wire without interference, either in the same direction, or in opposite directions. You will understand me from the following diagram.



A A′, batteries; B B′, electro-magnets; C, wire for both circuits; *a*, broken part of circuit of A battery; *b*, broken part of circuit of A′ battery.

If *a* be closed, a current passes of course through C, and B′ becomes a magnet; now if *b* be also closed, B becomes a magnet, and a current is detected in C also; showing that two currents independent of each other occupy the same wire without interference. This discovery I have at once reduced to practice. The wire for the two circuits which I use for my two instruments in the Capitol, is composed of three instead of four threads.

3. *Account of an exhibition of Shooting Stars, seen in the day time.* (Communicated to Professor Olmsted.)

Winchester, Virginia, August 23, 1842.

Sir—I take the liberty of sending you a short account of a *shower of shooting stars*, seen in this neighborhood on the 22d of August. Mr. Nathan Lapton, an intelligent gentleman of the society of Friends, and of undoubted veracity, informs me that between one and two o'clock P. M., the attention of himself and family was called by some children in the garden, to "something uncommon in the sky." As they did not perceive at first what attracted their attention, the children cried out, "Look up there and see those shining things." Looking upward to a point near the zenith, he says they immediately noticed a continual shower of meteors. Being dazzled by the rays of the sun, they smoked a piece of glass, and observed this singular sight for an hour or more. They appeared to proceed from a point near the sun, and to shoot in every direction, many apparently falling towards the earth. From Mr. Lapton's description of this meteoric display, it must have very much resembled that of November, 1833. Your obedient servant,

CHARLES CHASE.

4. *Earthquakes in the United States.*—Since the commencement of the year, several earthquakes have occurred in the United States, but no serious damage has been occasioned by them: the following is a list.

Jan. 4, throughout the western, southern, and middle states. Feb. 8, throughout all the Atlantic states, about 10 A. M. Feb. 14 and 15, at New Orleans. Feb. 16, at St. Louis, Louisville, &c. March 14, at various towns in the state of Vermont.

5. *Great Earthquake in the West Indies, Feb.* 8.—A most frightful earthquake was experienced in the windward or easternmost cluster of the West India islands, on the 8th of February, 1843. Guadaloupe, and the neighboring isle of Montserat, seem to have been the focus of the disruption. The latter isle is said to have been enveloped in smoke or dust, as if thrown up from a volcano. The town of Point Petre, Guadaloupe, was almost wholly destroyed, every building being thrown down, and more than five thousand persons killed among the ruins. The shock was felt distinctly in various parts of the United States.

6. *On the Leafing of Plants:* Remarks by the Archbishop of Dublin before the Royal Irish Academy.—It is well known that there is a diversity in the times of leafing and shedding in individual trees of the same species—e. g. hawthorn, sycamore, horse-chestnut, beech, &c.—sometimes as much as a fortnight; and the earliest in leaf are also the earliest shed, the same individuals keeping their time every year. Hence the question whether this diversity arises from the "separable accidents" of soil, situation, &c. or from "inseparable accidents," which constitute what physiologists call *varieties.* An experiment was tried by grafting an early hawthorn on a late, and *vice versa.* The scions kept their times, (about a fortnight's difference,) as if on their own stocks, thus proving that it was a case of "*seedling variety.*"

Many other such varieties are known, not only of apples, peaches, &c. but of wild trees also, differing in shape of leaf, form of growth, color and size of fruit, &c. and also *time of ripening*. It was therefore to be expected that these should be like in respect of times of leafing. This may throw some light on the question respecting "*acclimating*." It may be that species may be brought to bear climates originally ill suited; not by any especial virtue in the seeds, *ripened in any particular climate*, but by multiplying seedlings, a few of which, out of multitudes, may have qualities suited to this or that country; e. g. some to cold, some to drought, some to wet, &c. In some cases, a plant's beginning to vegetate later may secure it from spring frosts, which would destroy a precocious variety; in others, earlier flowering may enable a tree to ripen fruit in a climate in which a later would be useless. Further, the experiment shows that the common opinion respecting the commencement of spring vegetation—the rise of the sap from the roots through the trunk and branches to the twigs, is groundless.

7. *New York State Reports.*—Since our last, seven of these important volumes have made their appearance, and in such quick succession that we have hardly had time to glance at their contents. It is our purpose, as these reports will probably be seen by few of our readers, to give in future numbers of this Journal a somewhat extended notice of each of the great divisions of the work; and in carrying out this design, we shall have the aid of gentlemen eminent in the several departments, and whose opinions will be received as authority.

8. *Flora of North America*, by Drs. Torrey and Gray.—The third part of Vol. II, of this work, was published in February last. It is smaller than any of the preceding parts, comprising only 112 pages, and including merely the remainder of the vast order *Compositæ*. The species of Compositæ described in this work, amount to one thousand and fifty, (doubtless one tenth of the phenogamous flora of our country,) which are comprised in one hundred and ninety-nine genera. The ensuing portion of the work will close the second volume, and contain the remaining monopetalous exogenous families.

9. *Thracia inequalis.*—We are requested by Prof Adams to state, that this shell, which was described in our last July number, is the *Periploma trapezoides*, Desh. Consequently the name of *Thracia inequalis* must take the rank of a synonym. The species has recently been found in Mobile Bay.

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APRIL, 1843.

ADVERTISING SHEET

OF THE

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Boston, March 11, 1842.

* One of pump No. 4 is in use by Dr. Webster, Harvard University, one by Prof. Lovering, Harvard University, one by Prof. Olmsted, Yale College, one by Prof. Young, Dartmouth College, one by Prof. Hopkins, Williams College, one by Prof. Whipple, Oberlin Collegiate Institute, several of No. 3, and some thirty are now in use in academies in the United States.

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The residence of the undersigned being in the vicinity of many localities of the New Red Sandstone of Connecticut River, in which exist the perfect Tracks of Birds and also the Fossil Impressions of Fishes, he is enabled to secure them as they are quarried, and will furnish them to Geologists whenever it is practicable, for a reasonable compensation. As it will only be possible to obtain a limited supply, he will furnish as a substitute, Plaster Models of all the varieties of Ornithichnites and Sauroidichnites, of which there are nearly forty, to all who may apply. These models will be cast in plates of uniform size, colored to conform to nature, and neatly packed in a box. They will be accompanied with a Descriptive Catalogue and sent according to direction for $10.

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B. Silliman.

Prof. SILLIMAN'S ADDRESS

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OF

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