THE

AMERICAN JOURNAL

OF

SCIENCE AND ARTS.

CONDUCTED BY

PROFESSORS B. SILLIMAN, B. SILLIMAN, JR.,

AND

JAMES D. DANA.

SECOND SERIES.

VOL. X.—NOVEMBER, 1850.

WITH AN INDEX TO VOLS. I—X.

NEW HAVEN:
PUBLISHED BY THE EDITORS.

Printed by B. L. HAMLEN—Printer to Yale College.

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NUMBER XXVIII.

SCIENTIFIC INTELLIGENCE.

* Incorrectly spelt GARRIC.

APPENDIX.

NUMBER XXIX.

SCIENTIFIC INTELLIGENCE.

NUMBER XXX.

APPENDIX.

ERRATA.

Page 39, line 13 from bottom, for "JOHN GARRIC, M.D.," read "JOHN GORRIE, M.D."

Vol. ix, p. 277, 2nd line from bottom, for "carbonates," read "carburets."
" " " 281, 5th " " " for "zinc," read "lime."
" " " 239, 3d " " " for 67, read 87, and at top of next page, for 3:2, read 2:1, and for "one-half," read "the same."

Vol. ix, p. 422, 5th line from top, for "volume," read "weight."
" " " 409, 10th line from top, transpose 72·47 and 27·53.
" " " 429, 19th " " " for "pyroxene," read "borax."

Vol. x, p. 138, 2nd paragraph, for *Souyet*, read *Louyet*. See further, p. 414.

VOL. X. JULY, 1850. No. 28.

Published the first day of every second month, price $5 per year.

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Printed by B. L. HAMLEN—Printer to Yale College.

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April, 1849.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. I.—*Review of the Geological Report on the Chippewa Land District of Wisconsin and part of Iowa, made in the year* 1847, *under the direction of D. D. Owen, M.D.*, U. S. Geologist for Wisconsin and Iowa.

In our last volume we gave a brief notice of a geological survey of the Chippewa Land District of Wisconsin and a part of Iowa; made under instructions from the Secretary of the Treasury of the United States and conducted by Dr. D. D. Owen as principal Geologist aided by Dr. J. G. Norwood as Assistant Geologist, and by the following gentlemen who conducted the operations of the sub-corps: Dr. A. Litton, Dr. B. F. Shumard, Mr. B. C. Macy, Mr. John Evans and Mr. A. Randall.

We propose in the present number to give a further brief review of that report setting forth the general and most important results.

The report now before us contains only an account of the field operations of the U. S. Geological corps engaged in the above district during the summer and autumn of 1847, and is therefore only the commencement of the work continued in 1848 and 1849.

The situation and extent of the country examined is given on page 8 of the report as follows:

"The seat of operations of the party appointed to explore the mineral lands of Wisconsin and a portion of Iowa was, during the summer and autumn of 1847, in a portion of country lying chiefly east of the Upper Mississippi above Lake Pepin, and extending north to Lake Superior. There was, however, also included a portion of Iowa, stretching north from the northern boundary of the geological survey of 1839, as far as the St. Peter's river; and also a tract of country north of the Wisconsin river. The principal streams which water it are Black river, the Chippewa, and the St. Croix, flowing southerly into the Mississippi; Mauvaise Rivière, (Bad river,) and Bois Brulé, (Burnt Wood river,) falling into Lake Superior; and Turkey river, Upper Iowa, Hokah, Miniska, Wazi Oju, Cannon, Vermillion, and a portion of St. Peter's flowing eastwardly into the Mississippi. It comprehends so much of the Chippewa land district as lies within the boundaries proposed for the State of Wisconsin, the southern part of the country bordering on the Kickapoo, together with the chief part of the Winnebago reserve, the half breed tract and a strip of the Sioux country west of, and adjacent to, the Mississippi, and reaching north to the St. Peter's.

"It lies between 43° and 47° north latitude, and between 89° and 94° of longitude west of Greenwich, and embraces about forty-six thousand square miles of surface."

In addition to the general reconnoissance made of the above region of country, a detailed survey was made of about thirty townships, west of the 4th P. M. on Black river in latitude 44° 30′, and about sixty townships on the St. Croix river in latitude 45°. Nineteen sections have been constructed by Dr. Owen and the other gentlemen of the corps at the most important points on the Mississippi, and six on the St. Croix arranged in such a manner as to form a diagram of comparative heights, not only of the outline of the country, but also of the principal members of the prevailing geological formations. One continuous section extends from the mouth of the Wisconsin, where the survey of 1839 terminated, to the Falls of St. Anthony; one from the Mississippi to the Falls of the St. Croix; one from the mouth of the St. Croix to the Falls of St. Anthony on a more extended scale than that appended to the general Mississippi section; one from Lake Superior to Portage Lake; one from the head waters of Wisconsin river to the Dalles of that river; one from Lake Superior to the Falls of the St. Louis river; one from Lake Superior along the 4th P. M. to Black river, and thence to the Mississippi; one along the correction line from the 4th P. M. to the Mississippi; one from the Kinikinick in a southwest direction across the St. Croix. Also a particular section at the Falls of St. Anthony. Diagrams of summit levels show the relative elevation of ground between the Mississippi and Lake Superior in three different directions.

Several of the above sections are so contrived with landscape back ground, as to present not only the natural exposure of rock, but also the general outline and appearance of the country through which the sections run.

The report is also illustrated by a *provisional* geological chart showing the approximate boundaries of formations; and by twenty-three sketches exhibiting the geological features of the landscape.

The geology of the district is described under the following heads:

1. Formations of the Upper Mississippi.
2. Formations of Red Cedar river and the Winnebago reserve.
3. Formations of the interior of the Chippewa Land District.
4. Formations of Lake Superior.
5. Drift.

In our notice of these topics we must necessarily confine ourselves to leading results.

It appears that the Upper Mississippi country north of the Wisconsin river to within a short distance of the mouth of the St. Peter's, is based on the magnesian limestones and sandstones older than the lowest of the formations of the valley of the Ohio; a portion of them being cotemporaneous with the calciferous group and Potsdam sandstone of the New York survey.

The portion of the report relating to these formations, which will be read with peculiar interest, is that which relates to the fossiliferous beds of these formations, since hitherto they have been regarded as almost barren of organic relics.

The results of this survey seem to show that it is to this portion of the United States that we must look to throw most light on the earliest organic forms.

On this subject we find on page 14 the following paragraph:

"If we except the white sandstone the terminating mass of F. 1 e,* the one upon which the lower† magnesian limestone (F. 2) rests, nothing definite was known up to the period of the present survey, of the nature or character of the underlying beds just described. Neither had any well defined organic remains been described anywhere beneath the grey and blue fossiliferous beds which form the upper portion of the sections at Prairie du Chien; so that there was an entire absence of all palæontological evidence as to the exact place which these strata occupied in the geological series. It is, therefore, with no small degree of satisfaction that I find myself able, from the observations of last summer, to disclose a new feature in the palæontology of Western America, and thus to furnish, not only to the geologist, a key to the stratigraphical position of the rocks north of the Wisconsin river, but, at the same time, to the miner his surest and safest guide by which to direct operations in the search after mineral wealth.

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* The lowest bed at the mouth of the Wisconsin river.

† In contradistinction to the upper magnesian limestone, the lead bearing rock of the Mineral Point District. Se report of 1839.

"More than one half of the above series, supposed at first to be quite barren of all organic forms, has already been proved fossiliferous, and subsequent search will no doubt develop others.

"It is, moreover, worthy of remark that the most fossiliferous portion of F. 1 is not its upper part, but beds which lie quite low down, even seven to eight hundred feet beneath what was, up to this time, considered the limit of the fossiliferous strata of the Mississippi valley. Yet these strata prove to be as densely crowded with organic relics as any of the most fossiliferous strata of the blue limestone of Ohio, Indiana, and Kentucky. The proportion of genera and species, it is true, is not great, but the number of individuals is immense; some slabs are so covered with shells, that it would be difficult to place the finger on a spot without touching some of them. The prevalent genera are *Lingulas* and *Orbiculas;* yet associated with these are some remarkable forms of crustaceans. The specimen figured on Pl. 7, fig. 1, is the pygidium of a peculiar trilobite, armed with spinous processes projecting from its posterior margin. I obtained it in thin bedded silico-calcareous layers near the level of low water mark below Mountain Island, in connection with schistose gritstones. The latter are charged with very perfect specimens of a small fossil shell having a nacreous lustre closely allied to *Obolus Apollinis* of Eichwald, which are found by thousands in the inferior sandstones of the protozoic strata of Russia. Associated with these shells are also found remarkable compressed sub-conical bodies, the nature of which I have not fully determined; but they are, perhaps, spines of much larger trilobites than the one represented on Pl. 7, F. 1. Yet these beds are identical with layers near low water at Mountain Island, entirely beneath a Lingula sandstone which, in all probability, is the western equivalent of the Lingula beds of the New York Potsdam sandstone, considered by most American geologists the oldest fossiliferous rock in the United States. The imbedded species of each seem the same, as far as one can judge from mere casts. At a little higher geological level, between Prairie a la Crosse and Bad Axe, I obtained numerous casts of Orthis in an impure, dark, flesh-colored, calcareous rock, associated with bucklers of a species of trilobite, which appears to be new. In a similar rock, near the foot of La Grange mountain, I also observed casts of Delthyris beneath green sandstone. (F. 1 c.)

"About the same geological horizon, in an argillo-calcareous rock, near the level of the head of Lake St. Croix, is a very large species of *Asaphus*. the buckler and post-abdomen of which is figured on Pl. 7, Fig. 2 and 3. Finally, at Marine Mills, ten miles above Lake St. Croix, only eighteen or twenty feet higher, viz.: in F. 1 e, in a soft gritstone, are abundance of bucklers and post-abdomens of small trilobites, possessing some analogies in form to the genera *Agnostus*, *Trinucleus*, and *Triarthrus*, (see Pl. 7, Fig. 4,) and probably belonging to more than one species; one of which seems to have been provided with spines, at least two inches in length, curved at the extremity into the form of a barbed fish hook. Fig. 5, Pl. 7, represents one of these, found by Mr. B. C. Macy near the mouth of Miniskah."

Speaking of the distinction between the *lower* and *upper* magnesian limestone formations of the Upper Mississippi, Dr. Owen remarks, page 19:—

"The traveller who has visited the Upper Mississippi cannot fail to have remarked the peculiar outline of hill that bounds the prospect on either side of this picturesque portion of that majestic river. He must especially have noticed the conspicuous perpendicular walls of rock, that rise from out the grassy slope, or green copse wood, in massive cliffs, and terrace the heights as with interrupted natural battlements from the Maquoketa river to Lake Pepin. It is not, however, until the geology of the country has been closely inspected, that he is able to discover that the hills which present themselves to view below Turkey river do not belong to the same geological era as those which appear above the mouth of that stream. Nay, so uniform are they in their general aspect, that the miner himself, who has spent the best part of his days in excavating and exploring their recesses, is wont to regard them as identical. So they are, looking only to their chemical composition. Both are limestones, highly magnesian,* in heavy beds, of great compactness and durability; but they are separated from each other by from one hundred and fifty to two hundred feet of other strata, the upper hundred feet of which teem with peculiar races of fossil forms, constituting a distinct geological epoch, and marking a long lapse of time that has intervened between the period of deposition of these limestones. In my former report I have designated them the "lower" and "upper" magnesian limestones of Wisconsin and Iowa. This distinction, as will appear more fully hereafter, is of the first importance in drawing conclusions regarding the mineral value of the country I have been instructed to explore.

"All the conspicuous escarpments of magnesian limestone, south of Turkey river, are composed of the upper of these formations, whilst all those north of Wisconsin river, as far as Lake Pepin, are of the lower.

"An inspection of hand specimens is in general not sufficient to enable even the geologist to determine from which of these magnesian limestone formations it has been taken, so like are the two in general aspect. Viewed on a large scale, however, some dis-

* The proportion of carbonate of magnesia varies from 10 to 42 per cent.

tinctive peculiarities can be observed; the lower magnesian limestone has both in its upper and lower portion often oolitic beds interstratified; it has occasionally green particles disseminated through it, and on the whole is rather more compact and darker colored. The only certain methods, however, of determining to which of these formations any given rock belongs, is to note the order of superposition; or, still better, to determine the nature of the imbedded organic remains, which differ materially in the two."*

One of the most important results of the survey is the establishment of the fact that the *lower* magnesian limestone as well as the upper, is metalliferous, yielding both galena and copper ore similar to that found south in the Mineral Point District in the upper magnesian limestone.† It appears, however, as yet a matter of doubt whether these ores will be found in as rich profusion in the northern part of Wisconsin as in the Mineral Point and Du Buque Districts.

The lower sandstones of the Upper Mississippi were found to attain their greatest elevation and development a few miles below Mountain Island, or, as it is known to the French Canadian, "*Montagne qui trempe a l'eau.*"‡ From thence with some few irregularities, as indicated by the diagram of comparative heights,

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* The paragraphs describing in a graphic manner the physical features of this magnesian limestone, are cited in our last volume on page 307.

† In confirmation of the correctness of the statistics collected in 1839 and inserted in the geological report for that year, regarding the produce of the lead mines situated in the upper magnesian limestone of the Mineral Point and Du Buque Districts of Northern Illinois, Dr. Owen has appended to the present report a table of the actual recorded shipments of lead from Galena from February to December, in the years 1841, 2, 3, 4, 5, 6 and 7, as furnished to him by Mr. James Carter, banker at Galena, Illinois. It is as follows:

Months.	Pigs lead, 1841.	Pigs lead, 1842.	Pigs lead, 1843.	Pigs lead, 1844.	Pigs lead, 1845.	Pigs lead, 1846.	Pigs lead, 1847.
February.......	.....	.....	.....	.....	5,287	.....	.....
March	4,080	80,125	.....	78,636	97,746	28,841	15,669
April..........	91,296	65,080	73,449	82,737	104,558	126,073	82,231
May...........	91,233	46,515	122,224	89,982	93,623	142,489	119,391
June	57,110	37,959	74,475	80,784	87,058	113,209	185,084
July...........	58,820	54,436	77,333	66,699	68,153	83,559	110,383
August	37,257	43,250	67,233	55,200	107,957	50,257	61,462
September	16,092	39,081	45,400	54,203	63,424	58,827	67,761
October........	46,286	54,941	67,473	63,072	78,887	71,668	63,825
November......	50,640	26,472	33,734	53,288	71,767	54,291	65,873
December	.....	.....	.....	.....	.....	1,500	.....
Total........	452,814	447,859	561,321	624,601	778,460	730,714	771,679
First arrival of steamboats ...	March 22	March 9	April 10	March 5	Feb. 26	March 10	March 29
Last departure of steamboats ...	Nov'r. 22	Nov'r. 16	Nov'r. 26	Nov'r. 21	Nov'r. 23	Dec. 2	....

As a pig of lead will weigh, on an average, 70 pounds, it appears from the above table that the annual produce has varied, in the last seven years, from nearly thirty-two millions, to upwards of fifty-four millions of pounds.

‡ Literally, the Mountain which soaks in the water.

the strata gradually decline; so that at Red Rock the fossiliferous shell limestone and underlying incoherent white siliceous sandstone, which crowned the hills bordering on the lower Wisconsin river, and which soon ran out after passing Prairie du Chien, were again in place, forming the table land. Gradually increasing in thickness towards St. Paul's and Carver's Cave, these beds form there the entire mural ledges above the waters of the Mississippi and constitute the escarpment of Fort Snelling, as well as at the Falls of St. Anthony. Five or six miles above this, these and indeed all the protozoic strata, are seen for the last time in ascending the Mississippi, being lost under the drift deposits which cover all the rocky strata from view to the vicinity of the Sank Rapids where the granite protrudes.

In regard to the physical features of the celebrated country between the mouth of the St. Peter's and the Falls of St. Anthony, we extract the following:

"The Falls of St. Anthony are at present seven miles from the mouth of St. Peter's river. It is, however, more than probable that they once occupied a position at or near Fort Snelling. Of course, little evidence can be gathered of the rate of wearing, from actual observation of the inhabitants recently settled there, but, judging from the condition of the strata themselves, there must have been a rapid retrocession. The cement, which holds together the particles of the St. Peter's sandstone, is so slight that it is with difficulty a solid specimen can be obtained. Yet this is the rock, with a covering only of fifteen or twenty feet of schistose limestone, to protect it from the swift current of the Mississippi, which forms the base of the falls.

"The confused heaps of disjointed masses of limestone, piled together below the falls, indicate the undermining action in progress. The inclined position, too, of the ledges of limestone there, for several hundred yards above the chute, contrary to the local dip, has mostly been produced by the water which sweeps over them, entering the extensive rents which run across the strata at this place, and gradually washing out the particles of sand upon which these ledges repose, thus allowing them gradually to sink, and causing huge blocks to become, from time to time, detached and precipitated into the rapids beneath. In this way the fall will, probably, after a lapse of time, be converted into a rapid. For, in proportion as the fall shall recede, the sandstone, by reason of its dip, will diminish in thickness, and at length disappear beneath the river bed. From observations of the dip at the falls, this latter contingency will occur when the fall has been worn back some six or seven miles from its present position.

"There can be little doubt that the rate of erosion at the falls of St. Anthony must be more rapid than at the falls of Niagara, since the soft sandstone of the former locality is more easily washed away than the Niagara shale.

"On the brink of the gorge, near Fort Snelling, no fluviatile remains have been yet found at a height where the waters may be supposed to have flowed in former times; but Dr. Shumard, who was instructed to collect evidence of any ancient river deposits at a higher level, observed over the limestone at the falls a bed of drift of about eleven feet in thickness, and resting on that, a bed of sand containing species of *Cyclas*, *Limnæa*, *Physa* and *Planorbis*, and this deposit he traced on to the same level for nearly half a mile below the present position of the falls.

"The same gentleman also observed, half a mile below the falls, and about a quarter of a mile east of the gorge, on rising ground over which runs the trail to St. Paul's, a white marl charged with the same genera of shells, but of different species.

"The former of these deposits is doubtless of fluviatile origin, and affords evidence of the river having flowed, for a short distance at least above the gorge; the latter seems to be a lacustrine deposit, the bottom of some drained lake, of which there are numerous instances in the Chippewa land district.

"If we except these beds and the underlying drift, no formations of more recent date than the shell limestones of St. Peter's were observed along the Mississippi from the Wisconsin river to the Falls of St. Anthony. This statement will apply also to the country east of the Mississippi, as far as the water-shed between that stream and Lake Superior, except along the valley of the St. Croix above the falls."—pp. 31, 32.

The 2d Chapter contains a description of the geological formations of the Winnebago Reserve, a tract of country in Iowa lying between lat. 43° and 43° 30′, extending on the west side of the Mississippi as far as long. 93° 30′. Until the autumn of 1848 the Winnebago Indians occupied this neutral ground between the white settlements and the Sauk and Foxes; when the former tribe were removed to the Two River country above the Sauk Rapids.

The principal streams which water the Winnebago Reserve are: Turkey, Upper Iowa, and Red Cedar. It was along these streams that the examinations were chiefly directed.

We learn from the report that along that part of Turkey river which meanders near the southern line of this Reserve tract, fossiliferous limestones, of the same age as those above the second terrace at Prairie du Chien, form low ledges crested with cedars and surmounted by a green slope from which the upper magnesian limestone rises in fantastic shaped cliffs.

Ascending the same river to the Indian Agency and Fort Atkinson, the strata gradually rise. At the former locality, the elevated ground usually presents a mural cliff of from fifty to sixty feet high, resting on table land, the elevation of which above Turkey river is about one hundred and forty feet. Here and there are also isolated, mound-like hills, nearly flat on the top and so symmetrical in form that, were it not for their height and extent and their being composed in their interior of solid ledges of rock, one might be tempted to attribute to them an artificial origin.

The lower strata are more schistose, being chiefly marlites with alternations of calcareous layers. The most conspicuous fossils are: ***Leptæna Madisoniensis***, ***L. sericea***, ***L. alternata***, ***Orthis formosa***, ***Pleurotomaria bilex***, and ***Isotelus megistos***. Others less abundant, are: *Atrypa capax*, *A. exigua*, *Spirifer lynx*, *Trochus bilex*. Several trilobites probably of undescribed species allied to *Phecops;* one near to *P. calicephalus*, but with compound eyes. There was also obtained in this vicinity a specimen of *Bumastis barriensis* which was probably washed from higher ground.

The Upper Iowa for the last fifty miles of its course, flows between cliffs of lower magnesian limestone based on the lower sandstones, which latter, with an interlamination of magnesian limestone towards its upper part, constitutes the greater part of the rock exposure near the confluence of that stream with the Mississippi.

There are many fine examples on this river of those remarkable castellated forms which the lower as well as upper magnesian limestone often assumes. Several sketches illustrating the features of the country, accompany the report.

On the Upper Iowa, about longitude 91° 50′, the following fossils were found in the lower 150 feet of rock: *Leptæna sericea*, *Leptæna rugosa*, *L. Madisoniensis*, *L. deltoidea*, *L. alternata*, *Pleurotomaria lenticularis*, *Cyathophyllum ceratites*, *Coscinopora sulcata*, *Bellerophon bilobatus*, *Isotelus megistos*, *Orthis testudinaria?* *O. formosa*, *Illænus crassicauda*, *Murchisonia bellicincta*, *M. subfusiformis*, *Atrypa capax*, *Orthis subæquata*, *Atrypa hemiplicata*, *Orbitulites? reticulatus?*

In this part of the Reserve the formation is evidently of the same age as that near the Agency on Turkey river, which lies about twenty miles to the south. Both appear to represent the Trenton limestone and Hudson river group of New York, and to be cotemporaneous with the Lower Silurian system of England.

Regarding the formation of the western portion of the Reserve along the Red Cedar river we extract the following:

"On Red Cedar there is a change in the geological formation of the country. Either this is the western limit of the formations of Upper Iowa and Turkey rivers, or else the southwest dip carries the formations before reaching the Red Cedar beneath the water courses; for on crossing the Red Cedar, the first ledges that came under my observation, only a few hundred yards to the west of it, were found to be charged with the large variety of *Atrypa prisca* and a *Spirifer*, very abundant in the shell beds of the falls of the Ohio, and allied to the *S. ostiolata* of the Devonian system of the *Eifel*, if not identical with it. These strata appear to be the equivalent of the rocks described in my report of 1839, as occurring lower down on the same stream in the Dubuque district, so that the line of bearing between these two formations, Silurian and Devonian, seems to run nearly parallel with that stream.

"The Red Cedar limestone extends up its west branch as far as I penetrated, i. e., to near the north line of the Winnebago Reserve; it also stretches away to the west as far as the limits of my observations, viz.: to Willow river. On Shell Rock, the east branch of Otter, I found in it, besides *Atrypa prisca*, casts of *Lucina proavia*, and an undetermined species of *Leptæna*. But the fossil which is most abundant, and most universally distributed through the rocks of this western portion of the Winnebago Reserve, is a very fine structured coral, composed of concentric layers, like the genus *Stromatopora*, but so close together that the layers can only be distinguished by close inspection with a magnifier. On Lime river, the west branch of the Otter, the strata are so full of this close grained coral that it might with propriety be called a coralloid limestone.

"Some of the beds of this formation consist of very close textured and smooth calcareous beds, like the lithographic limestones, splitting with a flat conchoidal fracture. In it I did not observe any fossils; but I had not many opportunities of examining exposures either of it, or, indeed, of any of the other members of the formation in question, along the line of my route. The geologist who undertakes to investigate the vast prairie countries of the Mississippi valley must be provided with no common share of patience and perseverance. He must be content to travel for half a day together without seeing aught but a rich black soil, covered, as far as the eye can reach, even down to the very edge of the small streams, with a thick and high growth of prairie grass, with, perhaps, a faint outline of timber cutting the distant horizon. He must be prepared to wade swamps, to ford streams waist deep, or, in times of freshets, to plunge in and breast the current. He must not shrink beneath a broiling sun, without even a bush to cast a faint shadow over an occasional resting place. He must think himself fortunate, if he can reach, at night, a few scattered oaks to plenish his fire, and boil his camp-kettle; and he may consider it a special instance of good luck, if, in return, he can catch a glimpse of a rock exposure once or twice a day. He may travel for days together without lighting on any object more interesting than the hillock of the prairie dog, or the broad lair of the bison."—pp. 36, 37.

The western portion of the Reserve is said to present no indication of being a mineral country. Some portions of the Turkey river and Upper Iowa country afford a little lead ore.

The third chapter of the report is devoted to a description of the formations of the interior of the Chippewa Land District. In our notice of it we must be brief.

The protozoic strata which have been described as forming the sections on the Mississippi, extend on the east side of the Mississippi for an average distance in a direct line into the interior of fifty to seventy-five miles; that is, to the falls of the principal eastern tributaries of that river. Here the crystalline rocks first appear above the water courses forming low falls and rapids. These igneous ranges do not rise abruptly in this part of the United States into elevated mountains; on the contrary they are seldom seen except in the immediate cuts of the streams, being covered for the most part with drift. The character of the country generally towards the summit levels leading to Lake Superior, is a succession of terraces of moderate elevation, chiefly composed of drift, often having a nucleus no doubt of granite, syenite or hornblende rocks; but these protrude only occasionally. At intervals the streams are ruffled into rapids, being filled with boulders which obstruct materially their navigation. A portion of these boulders may have been transported from great distances; the greater part however appear to be not far removed from the parent rock.

It is matter of surprise that so large an area of the interior of this district, and indeed of the sources of the Mississippi generally, should be level tamarack and cedar swamps, since in approaching a great water shed that gives rise to one of the largest rivers in the world, one is led to anticipate a country with physical features of quite a different character.

Interposed between the crystalline and igneous rocks of the interior of the district and the lowest sandstones, some green and red schistose beds have been observed at different localities. These appear to have been derived from the decomposition and detritus of the more easily decomposing felspathic granites.

The lower beds of sandstone adjacent to the igneous outburst, are not unfrequently changed to a hard quartzite. The red pipestone so highly prized by the northern tribes of Indians, is found also near the junctions of these formations, associated with quartzite.

We learn from the 4th chapter of the report that, except in the valley of the St. Croix, the red sandstones, marlites, slates, conglomerates and associate trap ranges which prevail in the district along the south shore of Lake Superior, extend only as far as the elevated ridges, whence rise the tributaries of the Mississippi on the one hand and the streams emptying into Lake Superior on the other. Over a large portion of this area the solid rocky beds are covered by comparatively recent deposits of drift sand, red marls and red clay; the latter derived apparently from the disintegration of the strata on which they repose; and have been produced by oscillations of the surface during the more recent periods of uplifts.

At various points along the range of outburst of the igneous rocks, mineral veins have been observed. The examinations had not been sufficiently minute to pronounce on their productiveness. The hypogene rocks observed by the Wisconsin corps through this region of country are: granites, syenite, hornblendic rocks, greenstone, and various kinds of trap.

The highest ridges of the south shore of Lake Superior, situated in the Chippewa Land District, are represented as formed of hornblende rocks, metamorphic slates, syenite and trap. These are estimated to be over a thousand feet above the lake, and are situated from ten to sixteen miles from its shore.

No organic remains had so far been found in the formations of Lake Superior by which to establish their age. Neither had any localities been discovered, where beds of known geological position might solve the question by indicating the relative order of superposition.

The principal mass of sandstone constituting the south shore and west end of Lake Superior in Wisconsin, appears to have a different lithological character from the sandstones beneath the lower magnesian limestone on the Mississippi, described in the 1st chapter of the report. So far as this evidence can be depended on, it militates against the supposition of their geological parallelism.

On the west side of the Mississippi, north of the Winnebago Reserve, as far as the St. Peter's river, the lower magnesian limestone and underlying sandstones prevail as far as the examinations had extended, i. e., for about half a degree of longitude. The former of these formations occupies the greatest area.

ART. II.—*On Rutilated Quartz Crystals from Vermont, and Phenomena connected with them;* by FRANCIS ALGER, A.M., Member of the American Academy, and of the Society of Natural History, Boston.

(From the Proceedings of the American Association for the Advancement of Science, held in Cambridge, in August, 1849.)

Mr. ALGER presented a paper on the quartz crystals from Waterbury, Vermont, containing acicular or capillary Rutile, and exhibited illustrative specimens of great perfection and beauty. He compared them with other specimens from the Alps and Brazil, and pointed out some important phenomena in which they differed from those, and all other rock crystals he had seen.

Erratic masses of rutilated quartz had, from time to time, been found in Waterbury, and several of the neighboring towns, and they had even been picked up in New Hampshire; but their geological association, or the character of the rock from which they originated, had not been well understood until recently. Mr. Alger had lately visited a remarkable locality of this mineral, where a true vein, two feet or more in width, had been brought to light in making a deep cut through a hill in Waterbury, on the line of the Vermont Central Railroad. The rock is a very tenacious talcose slate, sometimes passing into mica slate, and prevails to a great extent in this part of Vermont. Metalliferous veins are rarely contained in it, but veins of quartz are common. The vein here referred to, consisted principally of common amorphous quartz, presenting internal cavities or druses, lined or studded with projecting prismatic crystals, sometimes colorless and transparent, but more frequently of a smoky color, or brownish yellow tint, (Cairngorm.) The pure glassy white crystals, are but rarely penetrated by the acicular rutile, while the colored varieties abound with it, and seem in fact to owe the intensity of their color to the very prevalence of it through their substance.

The rutile is sometimes grouped in tufts of radiating crystals, proceeding from a common point, and shooting through the quartz; this being also the ordinary manner of its occurrence in the Brazilian specimens. The direction of many of these diffused crystals in the position they now occupy, would seem to show that they had been subjected to some electrical or polarizing influence, by which they had been arranged very nearly in a line parallel with that of the apex or perpendicular axis of the crystals of quartz in which they are imbedded. It would seem that they were once floating, as it were, in the transparent and liquid medium of the silicious mass; or else, what is more probable, in the simultaneous crystallization of both quartz and rutile, slowly or otherwise, there was superadded a polarizing influence which caused them to converge towards one point. Or again, it may be that these peculiarities are confined to those quartz crystals which projected downwards in the cavity of the vein at the time of their formation; and thus the rutile, from its greater specific gravity, would have a tendency to crystallize and extend itself downwards, rather than in any other direction. Mr. Alger could not state from actual observation at the locality, whether such was the fact.* The appearance referred to is most marked in those crystals in

* That mineral veins may owe their origin in many cases to electrical or electro-chemical agencies, has been shown by the experiments of Fox, Becquerel and others, who by ingenious contrivances imitating the conditions which were supposed to exist in nature, have produced precisely similar results, even the formation of various crystallized bodies; but the subject has not been pursued with that direct application to the origin and structure of crystals, in their natural repositories, which it deserves. "Future investigations, (as has been said by Dana,) with regard to the position of crystals in rock strata, especially in granite and allied rocks, may prove that the electric currents in constant circulation around the earth have been active agents in determining the direction in which the axes of crystals lie, and the course of cleavage planes." Dana's Mineralogy, second edition, p. 88.

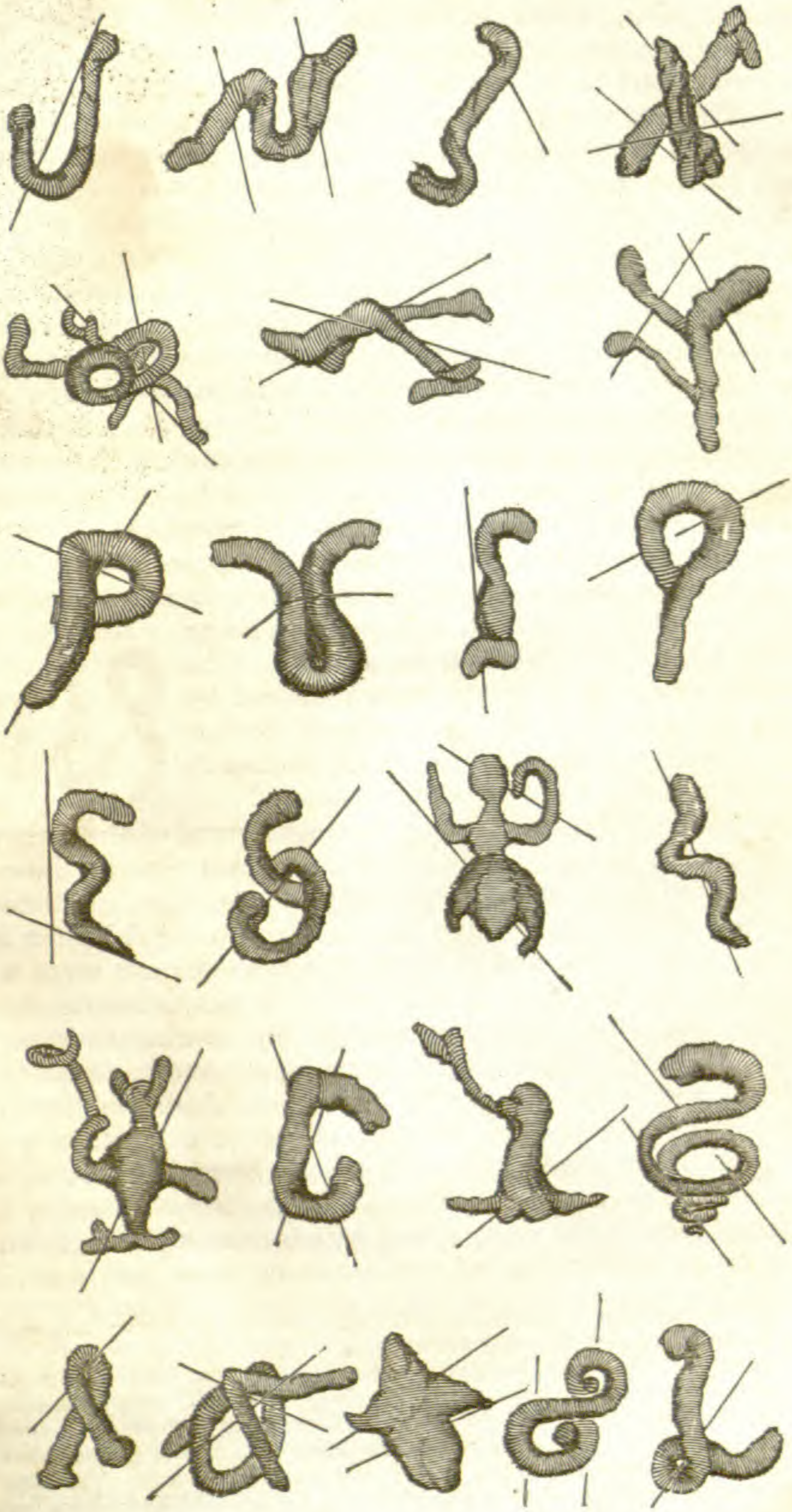
which the rutile exists in the most delicate hair-like and needle-shaped forms, (*Venus hairstone;*) and, in some instances, these delicate prisms are bent towards the ends most remote from the apex of the quartz crystals. They are sometimes four inches in length.* By transmitted light, their color is reddish brown; lustre like that of polished copper. Some few of the needles are entirely black, and closely resemble schorl. It was the opinion of Mr. Kennedy, a scientific engineer, and a very close observer, who was present at the opening of the vein, that the crystals of quartz enclosing rutile, were confined to one side only of the vein, thus indicating two periods in its formation, in one of which no rutile was present to intercrystallize with the mass. All the recently obtained crystals are very much discolored by iron rust, and the vein appears to be "run out." But its loss will undoubtedly be soon supplied by other sources.

Prof. Hubbard, of Dartmouth College, in whose possession is the finest specimen of this mineral found in the United States, first noticed a most interesting fact in regard to these crystals, namely, that the needles of rutile in some cases, had shot completely through the quartz crystals, and stood out in relief upon their surfaces, as if protruded by the sudden effort of their crystallization. The same appearances were presented to a small extent, by one of Mr. Alger's specimens. If produced in the manner supposed, the quartz must have been in a liquid state; if not produced in that manner, the crystallization of the rutile must have continued after that of the quartz had ceased. The latter seems the most reasonable supposition, and is favored by analogous phenomena in other crystallized minerals.

Imitative forms of mica contained in the Quartz.—The surfaces of two of the large crystals exhibited by Mr. Alger, as well as several smaller fragments of crystals, were covered by minute but very brilliant scales of gold-colored mica; and these sometimes penetrated the quartz in company with the rutile, and, in the same manner, seemed confined mostly to the darkest colored varieties of the quartz. But the appearance presented by this mica, is curious and altogether unique, for in the substance of the crystals, it has assumed the most fantastic forms, appearing in tortuous and vermicular ramifications, some of them bearing such a striking resemblance to organized bodies, as to give the first impression that they are actually the remains of insects or worms. The figures on the next page present a correct representation in a magnified form, of some of the most curious of these appearances exhibited by Mr. Alger's specimens. He had dissected out several of them, and found them to be composed entirely of small plates of mica more or less closely united parallel with the cleavage

* The polished specimens in which these prisms are exhibited, (known in French jewelry as *Flèches d'amour,*) are rarely surpassed in beauty by the finest to be met with in foreign collections. The reticulated forms are thus shown in great perfection.

1.



planes of the mineral. In fact, they are elongated hexagonal crystals of mica, twisted or distorted into every imaginable shape. Their laminated micaceous structure is shown perfectly by the microscope, and is represented by the transverse lines in the figures. The resemblance of the third figure on the fourth row, to some species of Araneides, is not too remote to suggest them instantly to the mind; and the general resemblance of several of the figures to the blood leech and common worms is still more striking—these being produced by the successively diminishing diameters of the little plates of mica until they terminate nearly in a point. But the origin of these resemblances was evidently fortuitous, and could not have been in any way connected with organic matter. They are interesting principally as furnishing a new fact in the department of imitative mineralogy, and they appropriately suggest the term vermiform mica as most characteristic of their general appearance. Vermiform should therefore be included among the imitative shapes assumed by minerals. The striking resemblance between several of these figures and the worm-like projections thrown out by the separating folia of *vermiculite* when exposed to a red heat, will occur to every one who has experimented on the mineral. The following examples of them were obtained by heating a fragment of the pure mineral broken from a specimen lately analyzed in Dr. Jackson's Laboratory. They are of natural size.

2.

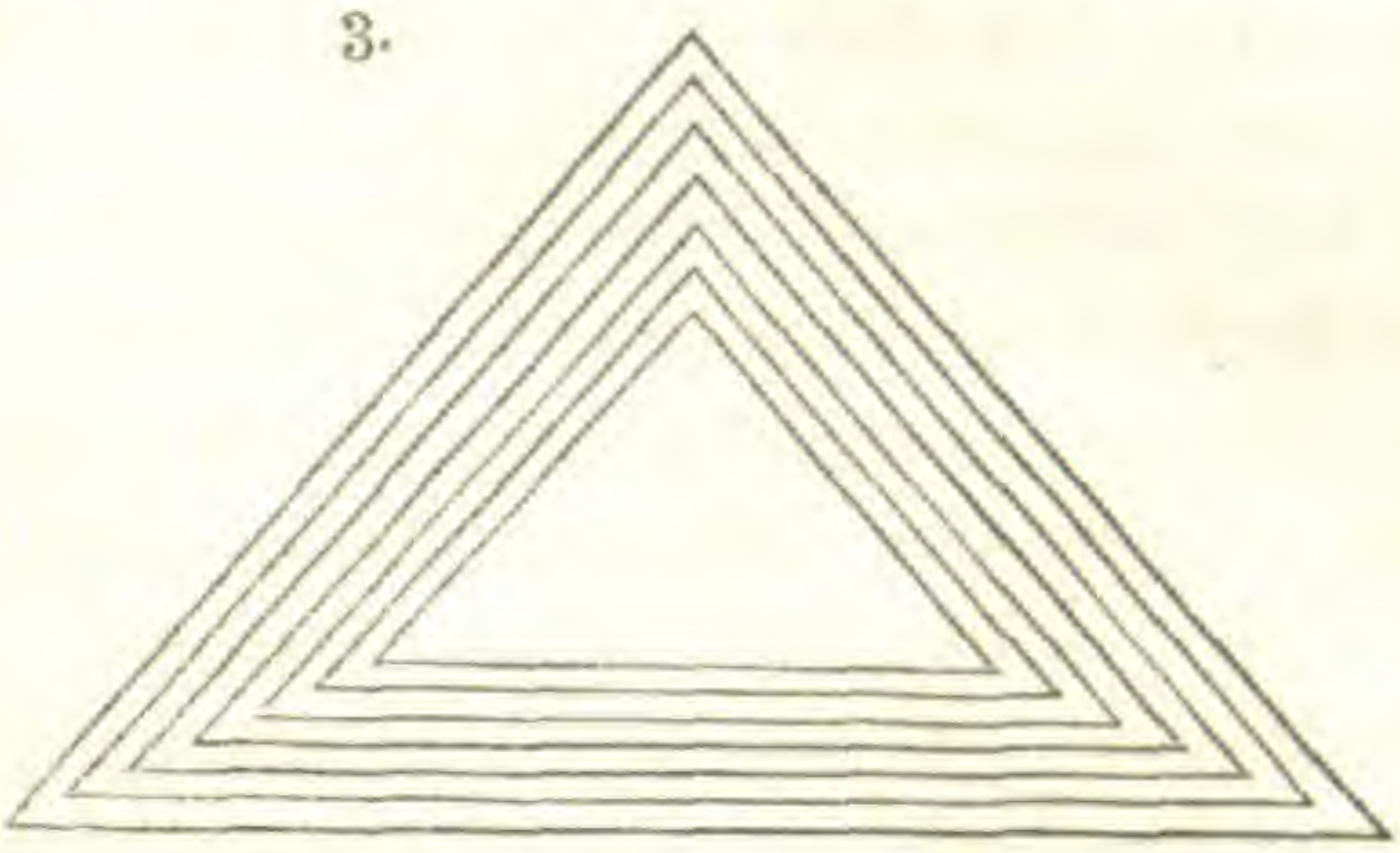
The straight lines seen passing through several of these figures, are intended to show the needles of rutile that actually intersect these concretions of mica in the body of the stone. In some of them the rutile passes through the circular space left by the folding over of the mica, and its crystallization does not seem to be interrupted by the mica in any case. A characteristic feature of rutile, but never shown in any of the specimens from this locality, (i. e., the geniculated forms,) seems to be imitated by the mica, and is best shown by the second figure in the lower row.* The color of this mica by transmitted light, is a pale green, and the mineral seems to agree in external characters with the substance from other localities. Considerable quantity of it was found loose in the vein, mixed with broken crystals of rutile.† The only appearances at all analogous to those just described,

* Prof. Hubbard's specimen presented the appearance in so marked a manner, as to lead to the impression that they were rutile.

† A portion of this carefully separated, was found to lose nearly 15 per cent. of water when heated to the melting point of glass. A peculiar empyreumatic odor was at the same time given out, but there was no reaction of fluorine. Exposed in a platinum crucible to a white heat for twenty minutes, it became grayish black and partially fused into a mass. In this state it was slightly magnetic. The large proportion of water seems to ally it with pennine, or perhaps with hydro-mica from the Alps—*Wasserglimmer* of M. Morin;—while its crystalline form, an oblique rhombic prism changed into a six-sided figure by the truncation of its acute lateral edges, refers it at once to common mica.

which had come to the knowledge of Mr. Alger, were those mentioned and figured by Dr. McCulloch, and described in vol. ii, of the Geological Transactions of London. But in this case, the substance was chalcedony, and the imbedded masses composed of chlorite, had nothing of a crystalline structure, and in fact were rather imitative of vegetable, arborescent forms.

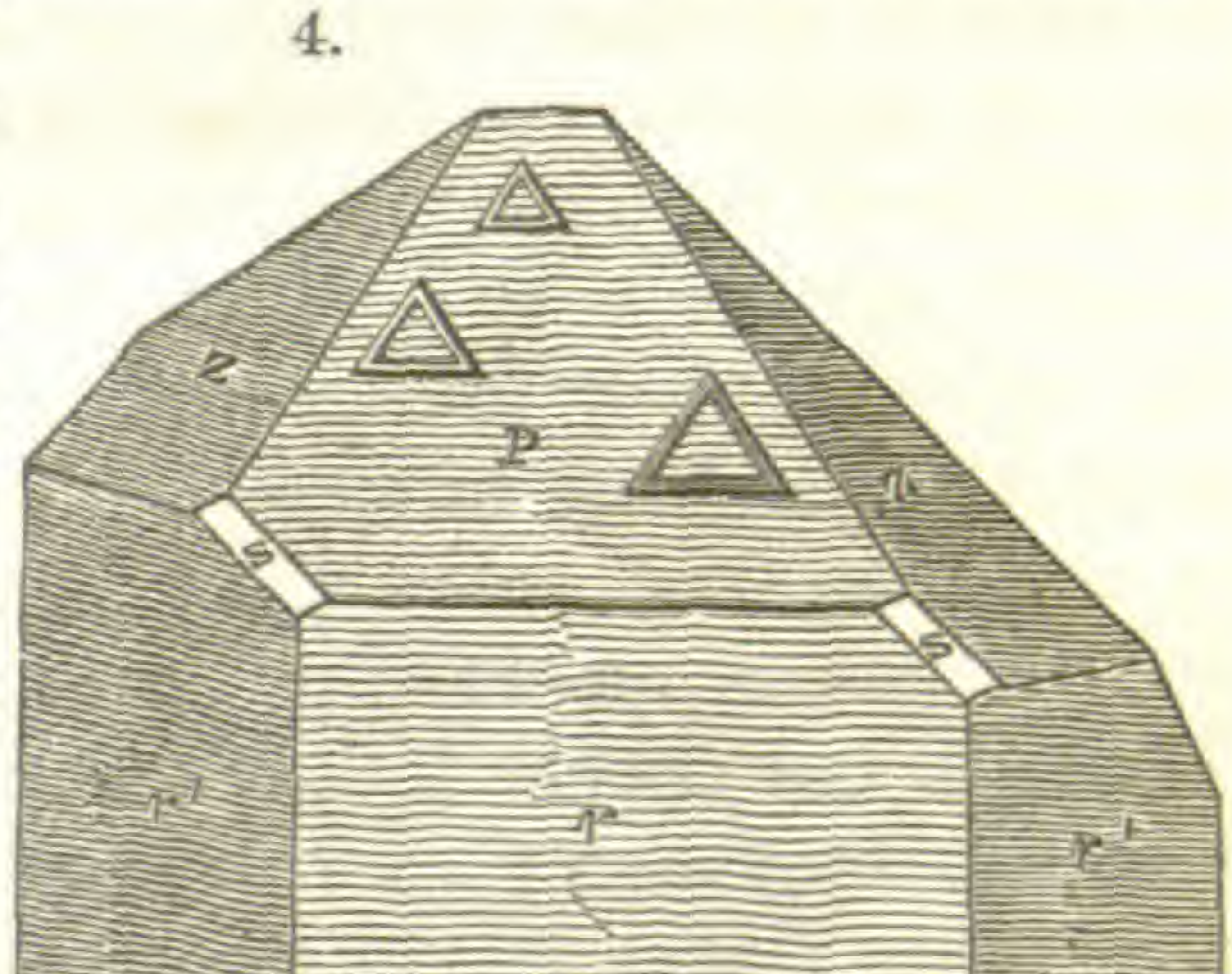
3.



STRIATED QUARTZ CRYSTALS.

The annexed figure represents some of the striæ which appear on the acuminating planes of the crystals, and are parallel with their edges of combination with the adjoining planes, as shown in the figure below. They are usually mere superficial triangular lines so slightly impressed as to be visible only when held in a particular position in regard to the light; but in a few cases, these configurations, commencing at a small point before the crystal had attained its full size, continue to widen with every fresh layer of particles deposited upon the faces of the crystal, until they produce cavities of considerable depth. That they were formed in this manner, is indicated by the step-like appearance of the sides of these cavities—an appearance which is more strikingly presented in a few cavities of a different shape, of one of which the annexed figure furnishes an example. In this, it will be noticed, the cavity is rhomboidal, and might at first be mistaken for the impression left by some foreign substance which had disappeared. No substance having such form, has been found attached to any of the crystals from this place, and although the angles at which the sides meet each other, (about 72° and 108°,)* are nearly those of calc spar, or carbonate of iron, it is evident from the enlargement of the cavity towards the surface of the crystal, or the hopper-like appearance assumed by it, that neither

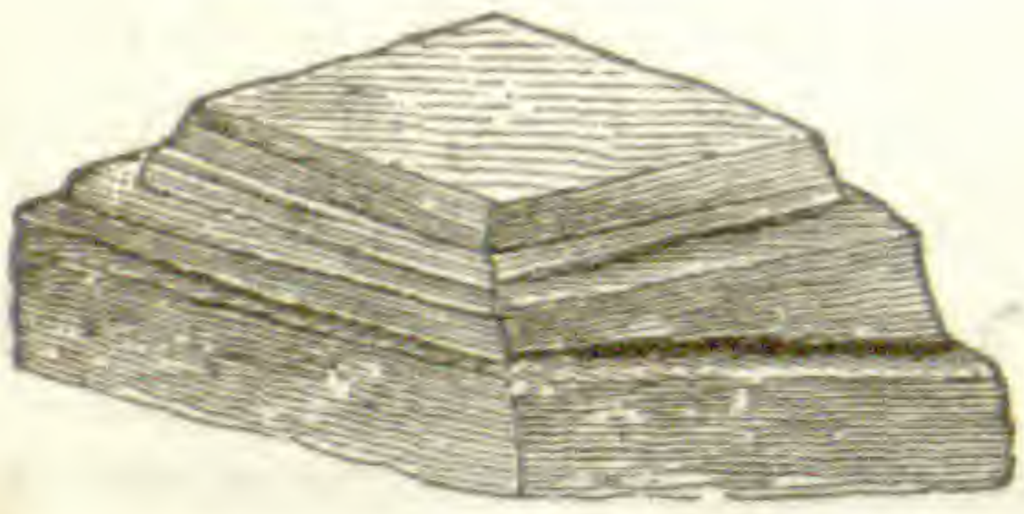
4.



* These angles were incorrectly given in the published Proceedings of the American Association. They have now been obtained by measuring the casts taken from its cavity, instead of the cavity itself.

of these substances could have produced it. The quartz, while depositing itself around either of them, would have taken the exact form of either, precisely as we see such impress of their forms in other crystals of subsequent formation, as for example, in the quartz crystals from Herkimer, New York. So it is evident, that if any substance ever occupied the cavity, it must have received its form from the cavity, without communicating any to it; and thus a pseudomorphous crystal may have been produced in a manner somewhat different from usual: viz., by filling up a cavity which had never been occupied by any crystallized substance whatever. Should such pseudo-crystals of infiltration or deposition, be met with in similar cavities of the quartz from this locality, it will become an interesting enquiry to determine whether they have the same composition, or are various depending upon accidental circumstances. If the view here taken of them be correct, they cannot come under the designation of any of the pseudomorphs hitherto described, (as they do not assume the form of any other crystal which has disappeared,) and we must accept such explanation of them as is afforded by the quartz itself. It is not easy to trace in these cavities, as it is in those before spoken of, any certain relation between them and the crystalline structure of the quartz whose surfaces they impress; their sides are not parallel with any of the striæ as seen upon the faces of the crystals; they indicate an interruption in the process of crystallization, and we have only to suppose a successive retrocession or withdrawal of particles in such parallel directions, thus enlarging the cavity outwardly, as the crystal itself increased in size. They do not appear to have been produced by the irregular combination of two or more crystals or by the union of different crystalline planes leaving spaces between them, because the striations which mark the faces of the crystal, and which have been intercepted by the cavity, appear opposite to each other on both sides of the cavity. This is shown by the figure. It therefore appears to belong to one crystal. The cavity intended to be represented by the large figure measures one inch on a side, and half an inch in depth; the striations at bottom are parallel with those upon the face of the crystal. It is more accurately shown by the following drawings taken from a cast of it in wax, which presents it in opposite positions.* The crystal itself, has a rhom-

5.



6.

* The deep bevelment on the broadest surface of these casts, (not shown in the cuts,) gives the true angle at which the plane *r* of the crystal (see fig. 4) meets plane *z*—the cavity extending partly into one of the planes of the pyramid.

boidal plane on each of the adjacent lateral angles of the prism, and not as is usual, on the alternate angles. It is permeated in every part by acicular rutile. This rutile, in one or two instances, has shot through similar cavities in other crystals, its delicate hair-like needles remaining unbroken while their opposite extremities are deeply imbedded in the quartz. In one of these cavities the depth is more than twice that of the transverse diameter, but the same step-like succession of layers is observed as in the large one, diminishing in their descending order, until they nearly terminate in a point towards the centre of the crystal. Compared with the large cavity, there is greater evenness of outline in these little ridges, and a nearer approach to parallelism between them.

Art. III.—*Examination of Kirkwood's Analogy;* by Sears C. Walker.

(From the Proceedings of the American Association, 2nd meeting, held at Cambridge, 1849, p. 212.)

In order that Kirkwood's Analogy should apply to all the planets of the Solar System, including the interpolated planet between Mars and Jupiter, four fundamental conditions must prevail for each planet considered as the middle of a consecutive series of five; namely:

I. . . . . . . $a = \frac{(a_{,}+b_{,})c_{,}+(a'-b')c'}{c_{,}+c'}$

II. . . . . . . $m = [(a-a_{,}-b_{,})c_{,}]^2$

III. . . . . . . $m = [(a'-a-b')c']^2$

IV. . . . . . . $\theta = \frac{2\pi}{\varkappa} \cdot a^{\frac{3}{2}}$

Where

$a_{,,,}, a_{,,}, a, a', a''$ = the five mean distances of the planets.

m = the mass in parts of the sun's mass similarly accented.

T = the sidereal year, in earth's mean solar days.

θ = the sidereal rotation in the same.

k = Gauss's *revolution constant* from Kepler's third Law.

$\varkappa$ = a similar *rotation constant* from Kirkwood's Analogy.

$r_{,}$ = the inner radius of the sphere of attraction for the third planet.

r' = the outer radius of the sphere of attraction for the third planet.

$r_{,,}$ = the outer radius of the sphere of attraction for the second planet.

r'' = the inner radius of the sphere of attraction for the fourth planet.

$D = r_, + r' =$ Kirkwood's diameter of the third planet's sphere of attraction.

$$\alpha = \frac{a}{D}$$

From these definitions the value of D is thus derived:

$$\frac{m_,}{r_{,,}^2} = \frac{m}{r_,^2}$$

$$\frac{m}{r'^2} = \frac{m'}{r''^2}$$

$$r_{,,} = a - a_, - r_,$$

$$r'' = a' - a - r'$$

$$\frac{m}{r_,^2} = \frac{m_,}{(a - a_, - r_,)}$$

$$\frac{m}{r'^2} = \frac{m'}{(a' - a - r')}$$

$$r_, \sqrt{m_,} = (a - a_, - r_,)\sqrt{m}$$

$$r' \sqrt{m'} = (a' - a - r')\sqrt{m}$$

$$r_, = \sqrt{m}\left(\frac{a - a_,}{\sqrt{m} + \sqrt{m_,}}\right)$$

$$r' = \sqrt{m}\left(\frac{a' - a}{\sqrt{m'} + \sqrt{m}}\right)$$

$$D = (r_, + r')$$

$$= \sqrt{m}\left(\frac{a - a_,}{\sqrt{m} + \sqrt{m_,}} + \frac{a' - a}{\sqrt{m'} + \sqrt{m}}\right)$$

$$= \left(\frac{2\pi}{\varkappa}\right)^{\frac{2}{3}} \cdot \left(\frac{1}{\theta}\right)^{\frac{2}{3}} \cdot a$$

$$b_, = \left(\frac{2\pi}{\varkappa}\right)^{\frac{2}{3}} \cdot \left(\frac{1}{\theta_,}\right)^{\frac{2}{3}} \cdot a_, - \sqrt{m_,}\left(\frac{a_, - a_{,,}}{\sqrt{m_,} + \sqrt{m_{,,}}}\right)$$

$$b' = \left(\frac{2\pi}{\varkappa}\right)^{\frac{2}{3}} \cdot \left(\frac{1}{\theta'}\right)^{\frac{2}{3}} \cdot a' - \sqrt{m'}\left(\frac{a'' - a'}{\sqrt{m''} + \sqrt{m'}}\right)$$

$$c_, = \frac{\sqrt{m_,}}{b_,}$$

$$c' = \frac{\sqrt{m'}}{b'}$$

$$b_, = \sqrt{m_,}\left(\frac{a - a_,}{\sqrt{m} + \sqrt{m_,}}\right)$$

$$b' = \sqrt{m'}\left(\frac{a' - a}{\sqrt{m'} + \sqrt{m}}\right)$$

$$\sqrt{m}+\sqrt{m_{,}}=(a-a_{,})c_{,}$$
$$\sqrt{m'}+\sqrt{m}=(a'-a)c'$$
$$m=[(a-a_{,}-b_{,})c_{,}]^2$$
$$m=[(a'-a-b')c']^2$$
$$a=\frac{(a_{,}+b_{,})c_{,}+(a'-b')c'}{c_{,}+c'}$$

Computation of the Value of the Constant called $\varkappa$.

If we use Leverrier's* mean distances and masses in his theory of Mercury, with the exception of Adams's† value $\frac{1}{21000}$ for the mass of Uranus, and Hansen's‡ periods of rotation, we find by condition (IV.) the following values of

$$\varkappa=\frac{2\pi}{\theta}\cdot\alpha^{\frac{3}{2}}$$

Namely:

$\varkappa=15.179$ by Venus.
$=14.811$ by Earth.
$=15.593$ by Saturn.

Whence, with double weight for Saturn:

$\varkappa=15.300=$ mean value adopted.

If we form another constant $k'=\left(\frac{\varkappa}{2\pi}\right)^{\frac{2}{5}}$ we find

By Venus, $k'=1.9377$
Earth, $=1.9054$
Saturn, $=1.9772$

From which it appears that an approximate value for the rotation times might be obtained from

$$\theta=\left(\frac{a}{2D}\right)^{\frac{3}{2}}$$

But the other formula is preferable; and we have

$$\theta=\frac{2\pi}{\varkappa}\cdot\alpha^{\frac{3}{2}}=\frac{2\pi}{15.3}\cdot\alpha^{\frac{3}{2}}=\frac{\pi}{7.65}\cdot\alpha^{\frac{3}{2}}$$

With this value of $\varkappa$, using the data above mentioned, interpolating the a, m, and θ of the fifth or hypothetical planet, called Kirkwood, and three masses, viz.: of Mercury, Mars, and Uranus, the following normal elements of the primary system are obtained, in which all of the above four fundamental conditions are fulfilled for each middle planet of five. For Neptune, Mr. W. had used his own value of the mean distance, and Prof. Peirce's mass from Bond's measures of the elongation of the satellite. The interpolated values are enclosed in parentheses.

* Additions à la Connoissance des Temps, 1848, 17–26.
† Proceedings R. A. Soc., vol. ix, pp. 159, 160.
‡ Schumacher's Jahrbuch, 1837.

Table of Planetary Elements, conforming rigorously with Kirkwood's Analogy.

Planet.	Mean distance.	Mass in parts of the sun's mass.	Sidereal revolution in earth's mean solar days.	Diameter of the sphere of attraction.
	a	m	θ	D
Mercury*	0.387.099	$\left(\frac{\dagger 1}{2802311}\right)$	1.003.473	(0.198.122)
Venus	0.723.333	$\frac{1}{401847}$	0.972.917	0.377.908
Earth	1.000.000	$\frac{1}{\ddagger 354936}$	0.997.270	0.513.934
Mars	1.523.691	$\left(\frac{1}{2107404}\right)$	1.025.936	0.768.429
Kirkwood	(2.908.511)	$\left(\frac{1}{1353240}\right)$	(2.406.104)	(0.830.951)
Jupiter	5.202.800	$\frac{1}{1050}$	0.385.907	5.035.373
Saturn	9.538.852	$\frac{1}{\S 3512}$	0.437.003	8.497.477
Uranus	19.182.730	$\left(\frac{1}{23733}\right)$	(1.396.779)	(7.875.342)
Neptune	30.039.500	$\frac{1}{20000}$		

* An interpolated planet with a mean distance $a=0.20$ and a mass of $\frac{1}{4739670}$, would harmonize with the above system.

† This interpolated mass of Mercury compares with observation thus:—

Leverrier's first mass, ...	$\frac{1}{1.909.700}$
Walker's interpolated mass, . .	$\frac{1}{2.802.311}$
Leverrier's second mass, . . .	$\frac{1}{3.000.000}$
Encke's value, ...	$\frac{1}{4.865.771}$

‡ And for Mars:

Burckhardt's mass, ...	$=\frac{1}{2.680.637}$
Walker's interpolated do, . .	$=\frac{1}{2.107.404}$

§ And for Uranus:

Adams's mass, ...	$\frac{1}{21000}$
Walker's interpolated mass, . .	$\frac{1}{23733}$
Lamont's mass, ...	$\frac{1}{24605}$
Struve's mass, ...	$\frac{1}{26860}$

Conclusions from the above table.

From this comparison of authorities, it appears that with a constant value of $\varkappa = 15.300$, and with assumed masses of Mercury, Mars, and Uranus, equally plausible with those heretofore employed, the a, m, and θ, of the fifth or hypothetical planet may be thus interpolated:

$$* \; a = 2.908.511$$
$$m = \frac{1}{1.353.240}$$
$$\theta = 2.239.035$$

And then the system of nine values of a, m, and θ, will be normal with reference to Kirkwood's analogy, and each of the four fundamental conditions (I.) (II.) (III.) and (IV.) will be rigorously fulfilled for every middle planet of five.

Mr. Kirkwood had remarked in his letter,† that his analogy required the assumption of a fifth planet between Mars and Jupiter. If the Geological Section was allowed the privilege of restoring fishes, lizards, and elephants, there was no reason why the Physical Section should not be permitted to restore a planet.

Remarks on the Degree of Constancy of $\varkappa$.

The limits within which it is possible to vary the value of $\varkappa$, without making some of the interpolated elements inadmissible, are about one-twentieth of the adopted mean value of 15.300. We may therefore conclude that, WHETHER KIRKWOOD'S ANALOGY IS OR IS NOT THE EXPRESSION OF A PHYSICAL LAW, IT IS AT LEAST THAT OF A PHYSICAL FACT IN THE MECHANISM OF THE UNIVERSE. The quantity D, on which the analogy is based, has such immediate dependence upon the nebular hypothesis, that it lends strength to the latter, and gives new plausibility to the presumption that this, also, is a fact in the past history of the solar system.

Such, then, is the present state of the question. Thirty-six elements of nine planets, (four being hypothetical,) appear to harmonize with Kirkwood's analogy in all the four fundamental equations of condition for each planet.

To suppose that so many independent variable quantities should harmonize together by accident, is a more strained construction of the premises than the frank admission that they follow a law of nature.

If, in the course of time, the hypotheses of La Place and Kirkwood shall be found to be laws of nature, they will throw new light on the internal organization of the planets, in their present, and in any more primitive state, through which they may have passed.

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* The mean distance is greater than that of the asteroids, except Hygeia, which has $a = 3.18$.

† This Journal [2], ix, 395.

For instance, we may compute the distance p, from the centre, at which any planet must have received its projectile force, in order to produce at the same time its double movement of translation and rotation. Now let $v=$ the planet's present angular rotatory velocity. Then, $K=pmv$, will be a constant quantity denoting its momentum of rotation.

If the planet, in a more primitive state, existed in the form of a ring revolving round the sun, having its present orbit for that of the centre of gravity of the ring, the momentum K of rotation must, by virtue of the principle of conservation of movement, have existed in some form in the ring. It is easy to perceive that this momentum K is precisely the amount which must be distributed among the particles of the ring, in order to preserve to all the condition of dynamical equilibrium, while those of each generating surface of the ring were wheeling round with the same angular velocity. It is also clear that this mean angular velocity must be that of the primary planet in its orbit, and accenting the quantities p and v for the case of the ring, we have the equations,

$$K=pmv=p'\,m\,v'$$

$$\frac{p'}{p}=\frac{v}{v'}=\frac{T}{\theta}\,\frac{\varkappa}{k}\,D^{\frac{3}{2}}$$

$$p'=\frac{\varkappa}{k}\,.\,D^{\frac{3}{2}}.\,p$$

But on the hypothesis that the law of decrease of density from centre to surface in the primitive shape was the same as at present,

Let $r=$ the present radius of the planet;

$R=$ that of the generating figure of the primitive ring;

Then, $\frac{p'}{p}=\frac{R}{r}$

And, $R=\frac{\varkappa}{k}\,.\,D^{\frac{3}{2}}\,.\,r$

The value of R from this formula, comes out a very small fraction of D for the small planets, and nearly equal to D in the case of Jupiter, Uranus, and Saturn. If any inference can be drawn from this result, it is unfavorable to the hypothesis that the primitive law of decrease of density was the same as the present.

If the planets have really passed from the shape of a revolving ring to their present state, the prevalence of Kirkwood's analogy shows a nice adaptation of parts in every stage of the transition.

If the primitive quantity of caloric (free and latent) had undergone a very great change beyond that now indicated in the cooling of their crusts; if the primitive quantity of movement of rotation had been different from its actual value for any planet; if the law of elasticity of particles for a given temperature and distance from each other varied from one planet to another in the primitive or present states; in either of these cases, the analogy of Kirkwood might have failed. As it is, no such failure is noticed; we are authorized, therefore, to conclude, that the primitive quantity of caloric,—the law of elasticity,—the quantity of movement of rotation,—the past and present radii of percussion,—the primitive diameter of the generating surface of the rings, and the present dimensions and density of the planets, have been regulated by a general law, which has fulfilled for all of them the four fundamental conditions of Kirkwood's hypothesis.

After Mr. Walker had concluded, Mr. George P. Bond inquired of Mr. W. as to the applicability of his remarks on primary rings to the case of the secondary ring in the system of Saturn.

Mr. W. replied, that in the case of the breaking up of a primary ring, the day of the new planet would be equal to the year of the ring, provided the new diameter was the same as that of the generating figure, and the same law of decrease of density from centre to surface was preserved. In this case we should have

$$K = rmv = r'mv'$$

$$v = v'$$

and therefore $$p = p'$$

Such, however, is not the case in fact with the primary planets. The new diameter is contracted by the more immediate action of the central mass, more than it is expanded by the increase of free caloric. The new diameter is, therefore, so much smaller than the primitive D, that p' is changed into p, and v', or the yearly mean angular velocity, is changed into v for the daily value.

We may extend the nebular hypothesis, and Kirkwood's analogy to the secondary systems. If they are laws of nature, they must apply to both. In the secondary systems the day and month are the same. This fact has remained hitherto unexplained. Lagrange showed that if these values were once nearly equal, a libration sets in round a state of perfect equality; but he offered no conjecture as to the cause of the primitive equality. On the nebular and Kirkwood's hypothesis, it would only be necessary that upon the breaking up of the ring, the primitive diameter of the generating figure and law of relative density of layers, should be preserved, in order to maintain a constant value of $p = p'$, and consequently of $v = v'$.

Prof. Henry has shown that the moon, and probably the other satellites, by excess of radiation above absorption, have reached their constant minimum amount of free and latent caloric.

Perhaps this is the very condition required to maintain $p=p'$, and consequently $v=v'$. In this case we may conclude that p had exceeded p' immediately after the breaking of the ring, and only arrived at a state of equality by the loss of caloric from radiation.

Prof. Peirce remarked that Kirkwood's analogy was the only discovery of the kind since Kepler's time, that approached near to the character of his three physical laws. Bode's law, so called, was at best only an imperfect analogy. Kirkwood's analogy was more comprehensive and more in harmony with the known elements of the system. The diameter of the sphere of attraction, a fundamental element in this analogy, now for the first time gave an appearance of reality to Laplace's nebular hypothesis, which it never had before. The positive testimony in its favor would now outweigh the former negative evidence in the case, however strong it may have been. It follows at least from Kirkwood's analogy, that the planets were dependent upon each other, and therefore connected together in their origin, whatever may have been the form of the connection, whether that of the nebular hypothesis, or some other not yet imagined.

ART. IV.—*On Kirkwood's Analogy;* by Dr. B. A. GOULD, Jr.

(From the Proceedings of the American Association, 2nd meeting, held at Cambridge, 1849, p. 363.)

THE subject which Mr. Walker brought to the notice of this Section on Saturday, is one of far more than ordinary interest. Besides the elegant simplicity of Mr. Kirkwood's formula, his theory must, if it be confirmed, materially influence our views of cosmogony and of the theory of the Universe. I have devoted all of the time which my duties have allowed since Mr. Walker made his communication, to the numerical examination of the Analogy to which he referred and which prompted his beautiful investigations. I will state the results, though not in the fullness which they deserve, and with which I at first hoped to be able to give them; for although the subject is large, and one which we cannot expect to exhaust for many years, yet the time of the Section is so precious at this late hour, that I shall limit myself to as brief a statement as possible.

Mr. Kirkwood's theory, as regards the rotation of the planets will, if found to be true,—and the presumption seems to-day strongly in favor of its truth—furnish a remarkable and unexpected argument in support of the nebular hypothesis. The minds of many have been wavering of late with regard to this hypothesis; their doubts have been strengthened by the unqualified assertions that all nebulas are resolvable; but this analogy of

Kirkwood tends most strikingly to confirm it—so much, indeed, that if this latter be true, I do not know how any one can resist the argument which it furnishes in favor of the former, in so far as it applies to our solar system. It is then no longer a hypothesis, but becomes a probable theory.

I will give a very short sketch of the quantities I have used in repeating Mr. Walker's computation.

[Dr. Gould then gave upon the blackboard the masses of the planets, and the periods of rotation which he had used, differing from those used by Mr. Walker.]

These are the masses which I have used; and these are the times of rotation as given in the books. I do not know how accurate the latter may be considered; perhaps to minutes, perhaps even less so. I believe they have all been determined by the observation of spots. If so, what proof have we that the spots do not move, no matter what the number of rotations used in determining the period? I have marked the period of Uranus as doubtful, because I do not know upon what authority it rests, having only found it in a table of a popular work by Sir J. Herschel, with a mark of doubt prefixed. It does not agree at all with this theory.

In considering a question of this kind, we must remember the nature of our investigations. The subject is to a certain extent, necessarily general, and the appearance of precise harmony could not be expected even were our data exact, which they are not. The nature of the problem requires a general, not a special agreement between observation and theory. When we are considering the evolution of order from chaos, we cannot pretend to a knowledge of all the physical forces which exerted an influence. We go back to a supposed time when the planetary spaces were filled with nebular matter; we assume the existence of certain nuclei or centres of attraction; and, from our knowledge of the solar system, as it now is, infer the relative force which these several centres of attraction must have exerted, and assign to each its proportionate realm. If now we find that the spheres of influence belonging to the several nuclei are harmoniously connected, by a simple formula, with the periods of rotation as observed to-day,—an element before omitted in our investigations—we discover a remarkable corroboration of the probability of our hypothesis. This is what Kirkwood's formula professes to be—a simple relation between the time of rotation and the diameter of the sphere of attraction.

The subject being then a strictly general one, we are not warranted in demanding that exactness of numerical agreement, requisite for the verification of theories of a more special nature. If, as circumstances appear to indicate, more careful investigation should lead to the general adoption of the theory of Kirkwood, I should not desire that this should be denominated a LAW. Nature's laws must be precise and complete. The relation which we are considering claims only to be approximate—an analogy. And in speaking of it, I shall call it by this name, *Kirkwood's Analogy.* And where we have by hypothesis a right to expect analogy and not perfect accordance, the want of perfect accordance must not be considered to cast doubt upon the theory. Besides, if Kirkwood's Analogy were the result of a general law, would not the action of the law be modified in all probability by circumstances which would prevent us from perceiving any strict mathematical precision? The considerations which would be strong arguments against hypotheses of other kinds, do not appear to me weighty when applied to any thing so rude as the motion of chaotic matter.

There is a formula known as "Bode's law"—an empirical formula—expressing a supposed analogy, for which no reason was ever assigned and which, even before it was broken by the new planet Neptune, was found utterly devoid of that universality and precision which must characterize all laws of nature. Though it was considered a remarkable coincidence, and perhaps as capable of suggesting some law of nature, no true mathematician could ever have regarded it as a real LAW. Moreover, Gauss had shown long since that it did not hold for Mercury.

This "Law of Bode" was analogous to the theory of Kepler, that as there were but five regular solids, there could be but five planetary intervals, and therefore no planet between Jupiter and Mars.

Kepler's theory was totally overthrown by the discovery of Uranus, as the other has been by the discovery of Neptune. Bode's law, to which, by the way, Bode's name has been improperly given, would make the distance of Neptune beyond the orbit of Uranus nineteen times the distance of the Earth from the Sun, while it is in fact less than eleven times this distance beyond it, so that the fallacy of this formula must now be so evident as to require no demonstration.

Discordances such as those which exist in the application of this law to the planetary system, would afford sufficient reason for rejecting the analogy of Kirkwood; but, with even these discordances, the fact, that a single formula would approximately represent the truth to so great an extent, would justify us in bestowing much time upon its consideration.

[Dr. Gould then gave a brief sketch of the points of connection between the nebular hypothesis and the new analogy,—showing how the one would lead to the other.]

It will be remarked that in the phrase "sphere of attraction," the word sphere is not used in its geometrical sense. Nor is a planet necessarily in the centre of its sphere of attraction, for upon the one side, as is the case with the earth, may be a planet comparatively near, and upon the other a smaller planet at a greater distance; whence it is evident that the extent of the sphere of attraction will be much less upon the former side than upon the latter.

In Mr. Walker's theory he assumes $\varkappa$, in the equation $\theta = \frac{2\pi}{\varkappa} \cdot \alpha^{\frac{3}{2}}$, to be a constant and equal to $\frac{1}{(k)^{\frac{3}{2}}} \cdot \frac{1}{365,2563744}$, the term k being nearly 2. In the following formulas I shall denote the quantities which refer to the Earth by a single accent, those referring to Mars by two, to Jupiter by four, and to Saturn by five, reserving three accents for the hypothetical planet between Mars and Jupiter. I make use of Mr. Walker's formula for the sphere of attraction as follows:—D being the diameter of the sphere, a being the mean distance of the planet, and m being its mass.

For Jupiter we have

$$D^{\text{iv}} = \sqrt{m^{\text{iv}}}\left(\frac{a^{\text{iv}} - a'''}{\sqrt{m^{\text{iv}}} + \sqrt{m'''}} + \frac{a^{\text{v}} - a^{\text{iv}}}{\sqrt{m^{\text{iv}}} + \sqrt{m^{\text{v}}}}\right)$$

So for Mars;

$$D'' = \sqrt{m''}\left(\frac{a'' - a'}{\sqrt{m''} - \sqrt{m'}} + \frac{a''' - a''}{\sqrt{m''} + \sqrt{m'''}}\right)$$

In these formulas you will perceive that every thing is known, except the mass and distance of the new planet; or the *old* planet, if you please. The only assumption is the truth of the nebular hypothesis. If we knew the values of D^{iv} and D'', the spheres of attraction of Jupiter and Mars, we should have two equations and but two unknown quantities, a''' and m''', and should thus have this planet restored by the nebular hypothesis alone.

I have assumed in the computation for the value of k, not 2,—the constant which Mr. Walker supposes it to be, not only from his calculations, but from *a priori* reasoning,—but the mean of the values obtained from each planet, using the masses as given in the books, though affected of course, with some inaccuracy.

Now recurring to the above phenomena, let us take, for the sake of convenience,

$$D^{\text{iv}} - \left(\frac{a^{\text{v}} - a^{\text{iv}}}{\sqrt{m^{\text{iv}}} + \sqrt{m^{\text{v}}}}\right)\sqrt{m^{\text{iv}}} = A,$$

$$D'' - \left(\frac{a'' - a'}{\sqrt{m''} + \sqrt{m'}}\right)\sqrt{m''} = B,$$

and then we have,

$$\frac{m'''}{m^{\text{iv}}} A = a^{\text{iv}} - a''' - A,$$

and hence $$\sqrt{m'''} = \frac{a^{\text{iv}} - a'' - (A+B)}{\frac{A}{\sqrt{m^{\text{iv}}}} + \frac{B}{\sqrt{m''}}}$$

Here all the quantities on one side are known, and we use the mass of the new planet thus obtained for the solution of the problem. This being substituted in either of the previous equations, will give us the mean distance of the planet.

Or, if for convenience we make $\frac{A}{B}\sqrt{\frac{m'''}{m^{\text{iv}}}}=C$

we shall have— $$a''=\frac{a^{\text{iv}}-A+(a''+B)\,C}{1+C}$$

Now the only question is as to the value to be adopted for Mr. Walker's constant, which it seems to me should be deduced from observation only.

In computing the value of k, I obtain for

Venus, ...	1.9374
Earth, ...	1.9030
Saturn, ...	1.9763
The mean of this, ...	1.939

is the quantity assumed for k, in obtaining the results of which I shall speak; these three planets being the only ones to which the formula can be applied. It will be seen that the values obtained are most confirmatory of Mr. Walker's results. Unless we suppose the nebular matter to have been equally distributed through the solar system, we could not expect to find k absolutely *constant*, even if it were an approximation to the number 2.

Prof. Walker here remarked that the constant used by Dr. Gould answered better than the constant 2.

Dr. Gould continued,—We do not know the extent of Mercury's influence inside its orbit, and hence cannot know the diameter of its sphere of attraction. Nor can we apply the formula to Mars or to Jupiter, for we do not know what planet may have been between them. We cannot apply it to Uranus, for we do not know its period of rotation. There remain but the three planets mentioned above.

Calculating, from the equations thus developed, the mass and distance which would belong to a planet between Mars and Jupiter, and thence, by Kirkwood's analogy, the corresponding time of rotation, we do not find it so great, that, by mere centrifugal force, the planet could have been exploded, and its mass scattered in the form of asteroids.

From a very rough computation of the place and size of the hypothetical planet, I obtain a mean distance 3.12,—and a mass $\frac{1}{361,360}$. This mass is very much smaller than the mass of our earth, and would agree with the supposition of a small planet, smaller even than Venus, but would be at least equal in size to twelve or fifteen Asteroids.

This gives rise to a great many speculations, most interesting and important in their bearing upon the theory of the universe. I wish to dwell upon the fact that we neither know accurately the period of rotation, or the mass of most of the planets. The only element which is really well known, is the distance of the primary planets from the sun. Then there is the difficulty to which I also alluded, in ascertaining the magnitude of spheres of attraction, that we cannot assume the nebulous matter to be equally dense; so that it cannot be demanded that the analogy should be very accurately expressed by any given data.

It is now extremely important that observations should be made upon the periods of the rotation of several planetary bodies, and it is much to be desired, as bearing upon this problem, that those who occupy themselves with what may be called the natural history of astronomy should determine the times of rotation anew, and thus enable us to decide upon the truth of a law, the discovery of which may be important in the history of astronomy.

Prof. Walker made a remark on Saturday, with reference to the position to which Mr. Kirkwood will be entitled, should his theory be found true. The Section seemed surprised at this remark. I do not wish to express myself strongly, but certainly when we look back upon the labors of Kepler, who strove so many years with results so unpromising, until he discovered the laws which underlie the whole fabric of our solar system, and then turn to Mr. Kirkwood, a teacher in the interior of Pennsylvania—who without the sympathies of kindred minds, or the use of any library of magnitude—without calling even upon the aid of strict mathematical analysis—has fixed his attention upon this one problem, and investigated it in all its bearings, until after ten years of patient thought and labor, he has arrived at such a result as this—we cannot but be struck with the similarity of the two cases; nor can we consider it as very derogatory to the former to speak hereafter of Kepler and Kirkwood together as the discoverers of great planetary harmonies.

Art. V.—*On the Natural Terraces and Ridges of the country bordering Lake Erie;* by Charles Whittlesey, of Cleveland, Ohio.

Through the assistance of the engineers, engaged at various times, in surveys for railroads and canals in Northern Ohio, I have been enabled to determine the elevation of our "Lake ridges" at numerous points, between the Pennsylvania line and Sandusky Bay, a distance of 130 miles. I am more particularly indebted for these levels to J. H. Sergeant, Esq., who has run several lines west of Cleveland, and to Messrs. Harback and Smith, engineers for the Cleveland, Painesville and Ashtabula Railroad Company.

When these surveys do not cross the ridges and terraces, they have still been the basis upon which by short cross levels taken with a pocket instrument, I have obtained the elevations; and the results I think, cannot be wide of the truth.

There may be an extreme discrepancy of three feet among them, however, arising from changes in the surface of the Lake, which is the common plane of reference.

My opinion has been for many years, that the "*ridges*" are not "ancient beaches" of the Lake, although some of the *terraces* may be. It is indispensable to a beach, that it should at its foot or water line be *perfectly horizontal.* The Lake ridges are not so; and this fact, taken with the external form which they assume, clearly gives them the character of *submarine* deposits.

There are points on this coast where there are four ridges rising in succession from the Lake, as in the township of Ridgeville, Lorain County. In other places there are *three*, as from Geneva to Ashtabula; from Euclid through Painesville to Geneva, two; and from Cleveland to Euclid, one. There are places where it is difficult to trace any; and in others as at the city of Cleveland, where there are two or three branches or divisions of one ridge for short distances, all about the same level and liable to terminate suddenly. The ridges are sometimes upon the crest of a terrace, and sometimes lie, like a highway of water-washed sand, on the gently inclined surface of a plain, that descends towards the Lake. From a regular and beautiful elevated roadway, the ridge occasionally breaks into sand knolls, as at Avon Centre, Lorain County; at Ohio City near Cleveland, and at Painesville, Lake County.

Where nothing to the contrary is stated, the height given is that of the summit of the ridge, terrace or knoll. The first ridge, or that nearest the Lake, is known in the county as the "North Ridge." The others have different names at different places; as the "Middle Ridge," "Chesnut Ridge," "Butternut Ridge," and "South Ridge."

Elevation of the North Ridge, beginning at the Eastern part of the Western Reserve.

Conneaut, Ashtabula Co., above Lake Erie, . .	120 feet.
One mile west, ...	145 "
Four miles east of Ashtabula village, . . .	132 "
(Base of same for several miles, 85 to 95.)	
County line between Lake and Ashtabula Co., northern slope of North Ridge, .	107 "
Eight miles west in Lake County, . . .	125 "
Centreville, 1 mile north of village, . . .	105 "
Painesville, ...	120 "
Mentor—well defined for 2 miles level, . . .	109 "

Willoughby, ...	85 feet.
Seven miles east of Cleveland, . . .	112 to 118 "
Three " " " "	113 " 118 "
Two " " " " at crossing of Cleveland and Pittsburg Railroad, .	128 "
Cleveland city, ...	96 to 108 "
Ohio City, ...	114 "
Rockport, Rocky River, ...	90 "
One mile west, ...	105, 107 and 126 "
Avon, Lorain Co., east of Centre one mile, . .	85 "
" " " Centre sand knolls, . . .	105 "
Russelton, Lyme, Huron Co., ...	120 "

This table embraces a distance of one hundred and twenty (120) miles, where it appears the lowest summit is 85 feet, and the highest 145, showing a difference in longitudinal direction of 60 feet. I have not visited all the positions here given, but the greater part of them, and for the rest am informed by the engineers that there is no higher ground between the ridge and the Lake. In all cases there is a smooth uninterrupted plain, on the Lake side, over which the water of the Lake is everywhere visible, when the forest timber, which is heavy, is cleared away. It is variously composed of blue marly clay, of coarse drift called "blue" and "yellow hardpan," and of coarse sandy and gravelly drift; but the soil is for the most part clayey, and wet between and below the ridges. The streams, little and great, cut deep and steep gullies through the superficial deposits, and also into the rocks below. From the cliff limestone at Sandusky, eastward and to the state line, the superficial matter rests on slates, sandstones and shales, corresponding to the Hamilton, Chemung and Portage groups, of the New York Reports.

Elevation of the second Ridge, called the "South" and "Middle" Ridge.

Near Kingsville, Ashtabula County,	(south ridge,)	152 feet.
Centreville, ...	" " .	122 "
Two miles east of Cleveland,	" " .	135 "
Two miles southwest of Ohio City,	middle " .	149 "
Dover Centre, 12 miles W. of Cleveland,	" " .	163 "
Rockport, 7 miles " "	" " .	130 "
Ridgeville, Lorain County,	" " .	168 "

This ridge is more broken and less continuous than the first, or "north ridge," and is in general heavier. In Rockport, Dover and Ridgeville, on the northern or Lake slope, it is from 16 to 20 feet above the base at its foot, and on the rear 5 to 10 feet. Behind it, as with all the ridges, is flat, swampy land, and small rivulets that drain the low ground, running parallel with the swell, to some creek, or occasionally breaking through towards the Lake. These lands are very rich, and with a moderate expense are drained by ditches cut through the ridge. The slope of the flat lands between and before the ridges is sufficient to carry off all the water in ditches that have a free current. Most of this land is coming under the plough in this manner, although it is equally well calculated for grass. No country can possess more rural beauty than that along these "ridge roads." The land in a longitudinal view, is apparently level, as far as the eye can reach; and the buildings congregated along the line of the road appear to be arrayed in curved lines, gently waving to the right and left as you proceed. Looking from one of the interior ridges, which are generally perceptibly higher than the next one towards the Lake, if the timber is not standing, another, and rudely parallel row of farm houses, barns, orchards, &c., is seen at the distance of one, two or three miles; the intermediate space perfectly smooth and cultivated, and beyond lies the blue water, and the horizon.

The composition of one ridge does not materially differ from another. It is formed of coarse, water-washed, yellowish sand, or of fine gravel, principally the comminuted portions of the adjacent rocks. The rocky fragments are not generally worn perfectly round, or oblong, as beach shingle is, but are more flat, with worn edges. There are mingled with the sandstones and shales that compose this gravel, scattered pieces of quartz, flint, also granite, and trappean rocks, limestone and ironstone.

The basis of the ridge corresponding with the impervious clayey soil between, gives rise to a great many springs on the Lake or lower side; and this water frequently deposits bog iron ore, that has been used extensively in furnaces along the lake shore.

From near Dover Centre, west to Elyria, and even to Vermillion River, the second or "middle ridge," rests on a coarse grained sandstone or "grindstone grit," which farther east in Cuyahoga County rises above the level of the Lake ridges. Between the Black and Vermillion rivers, I have not succeeded in procuring the elevations. Here they are well developed, and show more branches or collateral lines, extending from one ridge to another, than is observed farther east.

By digging shallow wells, the inhabitants find water in abundance, and generally good. In these wells from 12 to 18 feet deep, there are thrown out as a common occurrence, sticks, timber and leaves, in a decaying state. I have in my cabinet some pieces of this wood, furnished by Dr. Moore of Dover, who took it from a bed of carbonaceous matter in a well of his, twelve feet below the surface. The well is situated on the middle ridge, 163 feet above the Lake. Pieces of timber six inches through have been found, represented as being water-worn like drift-wood. Those in my possession are solid, with a very fine grain resembling the willow.

Dr. Moore, an intelligent physician of my acquaintance, says he has seen shells thrown from the bottom of wells, resembling "periwinkles," a common name for Lymnea. Similar shells in fragments are said to have been thrown from a pit two miles west of Cleveland, on the north ridge. In the "blue marly clay" beneath this ridge, I have found a Helicina, and a Planorbis, shells characteristic of the loess of the Rhine, and of St. Louis, and the Wabash in Indiana. The palæontological evidence is therefore, as far as it goes, in favor of the idea of very recent and fresh water deposits.

It will be seen from the second table of heights, that the greatest difference is there forty-six feet; and that the summit of the ridge rises, from Rockport to Dover in seven miles, thirty-three feet, but from Dover to Ridgeville, six miles, it is nearly level.

Through these two distances, making thirteen miles, the height of the ridge above its base is about the same, from sixteen to twenty feet; and consequently the base has an equal rise in a longitudinal direction. Two miles west of Ridgeville Centre, the top of the middle ridge has descended from 168 to 149 feet. The foot of the north, or first ridge, and of the terrace on which it is frequently situated, approaches nearer to a horizontal line than the ridge itself, but still differs from a perfect level. It is at

Conneaut Creek, ...	75 feet.
Four miles east of Ashtabula, ...	85 "
Several miles west of "	95 "
Painesville, ...	85 "
East of Willoughby, several miles, . .	60 to 65 "
Three miles west of Willoughby, . . .	60 "
Euclid Creek, 12 miles east of Cleveland, .	75 to 85 "
Seven miles east of " . . .	105 "
Two " " " . . .	102 "
Ohio City, ...	75 "
Rockport, ...	70 "
Avon, ...	70 "

It is not easy to determine with precision where the base or foot of a ridge graduates into the plain; and consequently there is not that accuracy in the elevations for the base, just given, that we attain when measuring the summit or crest. But they are a close approximation, and although remarkably uniform, are by no means equal, as they should be if the base of the ridge represented an ancient coast-line; the greatest difference being forty-five feet, or about the same as the variation along the top of the second ridge.

There are but few measurements in my reach of the third and fourth ridges. In Huron County, south of ***Russelton***, there are two low swells of land parallel with the shore, apparently about on a level with each other, and not much above the main ridge at Russelton, which is reported at 120 feet.

The third ridge, in Ridgeville, Cuyahoga County, is one mile southerly from the second or "middle ridge," and is not very prominent, rising six to ten feet above the low ground. At this place it is 186 feet above the lake, or eighteen feet above the middle ridge, and eighty-one above the highest part of the north ridge in Avon, five miles north.

The fourth or last and highest well defined Ridge.

2½ miles southwest of Ohio City, . . .	173 feet.
1¼ miles southeast of Ridgeville Centre, . .	203 "
West bank of west fork of Black River, Elyria,	195 "

Distance embraced, twenty-five miles.

The materials of the most southerly or interior ridge, are in general coarser than in the others, showing a more violent or less lasting aqueous action. This is observed everywhere at the west. The more *elevated the drift*, the more does it exhibit the effects of *strong currents* in the transportation of large pieces of rock, in the shape of coarse gravel. The lower portions, especially those that lie near the surface of the great Lakes, not only on Lake Erie, but on Michigan and Superior, are fine, argillaceous or marly, laminated, and with few pebbles.

The terraces have not been as much noticed as the ridges, and consequently their height is not as well known. From Rockport to Avon, the north ridge is upon the edge of a terrace, the foot from seventy feet above the lake, down to sixty feet; its crest from one hundred and five down to eighty-five. Directly opposite this, about five miles more inland, a considerable portion of the fourth or south ridge, (known as the "Butternut,") is also on a terrace of about twenty feet, on its northern face; in fact all the ridges partake of the nature of terraces, in places; the northern slope being generally the longest. But the geological composition of the terrace on which the ridge rests is different, and either a rock or a drift of more compact and resisting kind.

Between Newburg and Euclid, nine miles, the northern face of the terrace is very bold, its base from 120 to 150 feet, and its crest 200 to 225 feet. It is here composed of fine grained sandstone (Waverly), and blue and red shales. East of Euclid, the terrace sometimes divides into two, the lower one supporting the north ridge. It is the same for several miles east of Willoughby, the crest of the first or lowest terrace being about one hundred feet and its base seventy to eighty feet, and formed of blue hardpan resting on shales. In Erie County, on the line of the Mansfield railroad, it is composed of cliff limestone supporting black slate; its base about 130, and its summit 180 feet.

It will be interesting now to compare the elevations above given, for ridges in Western Ohio, with those of the great lakes in other states. In Michigan, at the east line of Washington county east of Ann Arbor, is a well-defined ridge running nearly north and south, whose summit is 140 feet above Detroit River, at Detroit. Around Monroe, in Wayne county, Michigan, are some irregular sand ridges, not more than thirty feet. They are also visible on the north shore of Lake Erie, in the flat country between Erie and Huron; their elevation is not known, but they are apparently as high as 200 feet.

Mr. Roy, a Canadian engineer, has made a section across the ridges back of Toronto to Lake Simcoe, as reported by Mr. Lyell, and has given their respective elevations as follows:

No. 1.—one mile north of Toronto, 20 to 30 feet high— base above Lake Ontario, ...	108 feet.
No. 2.—2½ miles from Toronto, 50 to 70 feet high— base above Lake Ontario, ...	208 "
No. 3.—5 miles from lake—10 feet high—summit,	288 "

Five other ridges *or terraces* are given by Mr. Hall, in the geology of the 4th district of New York; also on the authority of Mr. Roy, referring apparently to the elevation of their base.

No. 4.—above Lake Ontario, ...	308 feet.
No. 5. " " "	344 "
No. 6. " " "	420 "
No. 7. " " "	680 "
No. 8. " " "	762 "

Mr. Lyell observed *eleven* ridges between the Lake and the summit for Lake Simcoe, the elevation of the eleventh, or last and highest, corresponding with No. 7, of the New York Report. The elevation of Lake Erie above Ontario is generally stated at 332 feet, so that the three first ridges or terraces, in rear of Toronto, are below the surface of Lake Erie.

Mr. Barrett, a New York engineer, furnished Mr. Hall with the height of some points on the Lockport ridge, south of Lake Ontario and opposite Toronto. They are as follows:

At Lockport, ...	158 feet.
Middleport, 10 miles east, ...	185 "
Albion, Orleans county, ...	188 "
Brockport, Monroe county, ...	188 "

None of these correspond in height with those on the north shore, as they should do if they were the result of littoral action at a beach, for the surface of the water would be level. If we suppose them to have been formed in that manner, when the water stood at the base of a ridge, the rivers must have discharged at the same level. Here should, on that hypothesis, be found deltas, and evidence of bays or lagoons. The waters having settled away, at the present period the streams discharge at a lower level, their channels being worn deeper and larger, cutting through the ridges and terraces that lie between the present and the ancient level. If the ancient mouth was at a point different from that where the present channel cuts a ridge, it should be visible in the present form of the ridge. If it was at the same point, there should be marks of such action as always accompanies the meeting of running currents with dead water. But our streams appear to cut the ridges as though they were barriers preëxisting, and broken through by the current.

Terraces composed of the rocks or other general deposits of a country, appear to be much stronger proof of ancient shores than limited sand ridges. When we rise above 240 feet from Lake Erie, the well defined terraces disappear; and from that line to 600 to 650 feet, the general elevation of the table land in Northeastern Ohio, the surface presents a confused arrangement of heavy drift, covering the rocks at various depths, in long massive knolls, without ranges or parallelism. Towards the west, the summits of the lake streams are lower, and the present surface of Northwestern Ohio and Northern Indiana, of Illinois, Michigan, New York and Canada West, with much of Wisconsin and Iowa, would be submerged by a sea rising 250 feet above Lake Erie, or 815 above the ocean.

The Wabash and Maumee summit, at Fort Wayne, Indiana, is 246 feet above Lake Erie. The summit between the waters of Saginaw Bay, and of Lake Michigan, . 108 ft. above L. Erie.

Summit between Pishtaka and Rock River in Illinois, ...	218	"	"
Lake Winnebago, ...	177	"	"
Summit between Lake Ontario and Lake Simcoe, Canada, ...	197	"	"
Mouth of St. Peter's River, Fort Snelling, Minesota, ...	179	"	"
Missouri River at Fort Leavenworth, west line of state of Missouri, . . .	181	"	"

If, therefore, the relative level of the land was the same as now, when the diluvial sea existed at high levels, its extent must have been very great, at the supposed stage of 250 feet above Lake Erie. At this or any other supposable stage, if it remained stationary long enough to form cliffs and banks at one place, it would produce the same effects, in kind if not in degree, at another; and we should be able to trace beaches or shores over all the vast west, for such cliffs are due to the action of winds and waves, always in operation on bodies of water. But under-surface currents are not universal, or in such general operation as to form everywhere, bars and longitudinal banks or spits. If the Atlantic Ocean should suddenly settle one hundred feet, or any other distance, would there not remain a distinct shore, well defined and traceable its entire length? At the mouth of some rivers and bays or inlets, would be seen limited sand ridges, their bases upon an exact level. On the ancient bed of the sea opposite sandy coasts, like North Carolina and New Jersey, would appear long, narrow and rudely parallel ridges, of such materials as are easily moved by currents, that would *not* be level longitudinally.

The evidence of the existence of ancient currents acting upon the drift, regularly and irregularly, is abundant. They have acted at all elevations, as well on the highest lands in Ohio at 1350 feet above the tide, as at the sources of the Mississippi, 1680 feet. The evidence is that they were powerful, and in general erratic or irregular and fitful. Such currents would not leave ridges, but rounded elevations. For this discussion it is immaterial whether the relative change in the level of land and water was due to subsidence or upheaval. The change has taken place, and before that period there were at great depths currents of water, both gentle and strong, giving form to the present exposed surface of the earth.

It is to this wide-spread power that we must resort, to explain most of the diluvial phenomena which are observed. What can be reasonably assigned to the wearing action of waves along a coast line, is limited and not pervading; not an universal, but a local, tardy and inefficient geological agent.

Art. VI.—*On the Quantity of Heat evolved from Atmospheric Air by Mechanical Compression;* by John Garric, M.D.

Physical science teaches us that condensation of matter is, with a few apparent exceptions, an invariable source of heat. Atmospheric air, subjected to mechanical compression, evolves it in large quantities, and in proportion to the degree of force applied. But though this general fact is well known, and has been examined by numerous and able experimenters, the numerical quantities of heat furnished by given volumes, under given pressures, have never been accurately ascertained. It may, indeed, be said that there is scarcely a subject in the whole range of natural philosophy, which has elicited more discordant results.

Every phenomenon of nature possesses in itself an interest well calculated to excite the mind to an investigation of its cause.

In acquiring a right understanding of the part it plays in the general system and economy of nature there is a gratification independent of all pecuniary or selfish considerations. But in the solution of the question, "what is the quantity of heat given out by the condensation of air?" there is a more immediate interest arising from the practical use to which the fact may be applied. It is favorable to the interests of science that the prospective value of the knowledge has justified experiments on this subject, as objects of commercial speculation; for otherwise its full elucidation would require an outlay of money too heavy to be expended by the mere lover of nature.

The atmosphere, from any thing we can observe taking place in it, affords but a remote and insignificant idea of the extensive relation existing between it and heat. Our sensations may indicate suffering from diminution, or inconvenience from exaltation of temperature, and the thermometer may mark the extent of its changes; but neither furnishes us with any evidence of the absolute or even the comparative quantity of caloric in air. Other considerations, however, present us with reasons for believing that it contains more of the principle of heat, in proportion to its absolute quantity of matter, than any other body found in a natural state.* Though not experimentally demonstrable, it cannot be doubted that the aerial form of the atmosphere, like that of vapors, is caused by the presence of a definite amount of heat.

Air may be condensed and dilated any number of times, and there will be with each change, a simultaneous and proportional diffusion and absorption of heat. The determination of the quantities of caloric thus alternately set free and rendered latent, is, apparently, a simple problem, which it is generally thought can be easily solved. But to furnish precise results not only is science and experimental skill demanded, but quantities of air must be used which imply a magnitude of apparatus, with facilities for manipulation, such as private philosophers can rarely command. It is true that if "instruments be exceedingly exact, and an experimenter be fully equal to the task of using them properly," the error upon moderate, may be quite as small in proportion as upon large quantities of most substances. But in this assumption, qualifications are implied to which few experimenters can lay claim; nor when possessed, would they be adequate to insure perfect accuracy in operating upon so attenuated a fluid as air which larger apparatus might attain.

In the attempts to determine the quantities of heat set free or absorbed by changes in the volume of air, these principles are well

* According to what are considered the most accurate experiments and deductions on this subject, the absolute heat of atmospheric air, estimating it from the standard of the absolute heat of water, viz. 1,000—is 18,000. See Mudie on the air, page 115.

illustrated; they have been rendered very difficult and replete with errors, from the delicacy of manipulation employed, and the consequent minuteness of result to be ascertained. Limited by the convenience and necessity of economy, philosophers, in their experiments on this subject, have been in the practice of using very small quantities of air. Thus narrowed, the endeavors to ascertain the quantity of heat evolved by the condensation of such a fluid, surrounded as it must be, by a comparatively great weight of matter, and extent of conducting surface, have produced an apparent effect so insignificant, or the means of measuring it have been so imperfect that it cannot be said truth has been approached.

In an investigation for the improvement of science, and still more when it is proposed, as in the present instance, to apply it to practical purposes, it is highly important to fix and determine essential data. So indispensable are they to any useful result, that too much care and nicety cannot be spent on them; because when once done with the accuracy of truth, they not only lay a foundation, upon which we may without rashness hazard fame and fortune, but they save much labor by forming a constant for any number of calculations, or any modification of experiments. In regard to air, the change in its sensible temperature, under an increase of density, is known to be great from the fact that by its sudden compression we may inflame tinder, and many other substances immersed in it. This trivial experiment, although in itself too indeterminate to found a valuable conclusion upon, has formed the basis of many chemical researches, having for their object the solution of the question in view.

To aid in determining the precise temperature to which compression would elevate gases, M. Thenard, many years ago, made a number of ingenious experiments. In trials with gases other than air, chlorine and oxygen, he used fulminating powders which exploded at different temperatures; and thus found that substances which decompose at 205° Centigrade, or 401° Fahrenheit, exploded in hydrogen, azote and carbonic acid, when compressed suddenly and strongly by hand in a glass tube. These experiments indicate that a very considerable accession of temperature may be obtained from the condensation of gases which are not supporters of combustion; but, admitting that atmospheric air conforms to the same law, the force employed is too vaguely expressed to show a precise relation between the heat evolved, and the compressing power. They are, however, interesting as proving that considerable intensity of heat may be obtained by trivial means, under unfavorable circumstances.

About the same time that M. Thenard made his experiments, M. Colladon instituted others better calculated to determine an exact limitation of the heat set free from air by compression. He states that to ignite sulphur, it is necessary to reduce atmospheric air to one-eighteenth of its volume, and to set fire to starch, to one-thirtieth.*

Philosophers, in their experiments on air, have been in the practice of dividing it into certain proportions—as the half, the fourth, the eighth of the volume experimented upon—and of considering each division as affording an equal measure of heat as the result of condensation. Now, the number of such divisions in reducing air to one-eighteenth of its volume, is a little more than four, and to one-thirtieth, nearly five; while the temperatures at which the above named substances burn, may be stated respectively, at nearly 500° F. and 600° F. Here we have data, which if correct, would at once solve the question involved. By dividing these numbers by each other we shall have as the result of the compression of air, about 120° F. for every reduction of it to half its previous volume. If these data could be relied upon, the subject would need no further investigation; but so different have been the conclusions from analogous experiments in the hands of the other philosophers, that new and more satisfactory ones are demanded. For while, as we have seen, M. Colladon says it requires a condensation of atmospheric air equal to one-thirtieth of its volume to ignite tinder, M. Gay Lussac, who made similar experiments on a larger scale, estimates that a reduction to one-fifth is sufficient.†

Nor have other experiments and processes of reasoning, for deducing a precise solution of this apparently simple question, though conducted by the most eminent chemists and mathematicians of the past and present ages, been more uniform or more satisfactory. The methods of proceeding have been different from each other, and from those already referred to, but they show a similar variance, of from one to five and upwards, in their deductions.

Dalton found, in experimenting with the common air-pump of the laboratory, that if the density of air be suddenly doubled by compression, its temperature rises 50° F. This experiment is among the earliest we find on the subject, and comes, if we can regard the 50° F. as computed at the standard of the specific heat of water, nearer the truth, for the first reduction of air from its ordinary temperature and pressure, to half its volume, than any subsequently recorded.

According to some experiments of Leslie, conducted upon a somewhat different plan, it would appear that atmospheric air

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* American Journal of Science [I], vol. xx, p. 180.

† It will be shown in the course of this communication, how this very great difference in apparent result, may arise from comparatively small errors of the experiments.

rarefied until its density was three-fifths of its natural density, when suddenly restored acquired about 48° F. of temperature.

Professor Espy states as the result of experiment, that if air have its density doubled or be reduced to half its volume, its temperature will be increased about 90° F., and that air expanded to half its density, or double its volume, generates a cold of about 90° F.

That accurate and profound mathematician, the late Mr. Ivary, has attempted to deduce from calculation, upon data to which I have been unable to get access, the precise quantity of caloric given out by the condensation of atmospheric air. According to his estimate one degree of heat is evolved from it, when under a condensation equal to $\frac{1}{180}$ of its volume; and consequently, if a volume of air be reduced to half its bulk the heat given out will be equal to 180° F. Both Laplace and Leslie, in attempts to reconcile the calculated velocity of sound, with that which is deduced from experiment, made similar calculations; but the theorems by which they are expressed are both abstruse and inaccurate, for though they regard the quantity of heat evolved from air by condensation as "profuse and powerful," the amount the latter assigns is less than that mentioned by Mr. Ivary; and his estimate, it will be seen, does not equal the truth.

The researches of M. Gay Lussac, to whom science is more indebted for an extensive and profound investigation of this subject than to any other philosopher, led him to consider a condensation of air into one-fifth of its volume as sufficient to ignite tinder—a degree of heat which he states is almost 580° F. This estimate, applied to volume, according to what has been mentioned as the custom of experimenters, would give about 280° F. for every time the density of air was doubled.

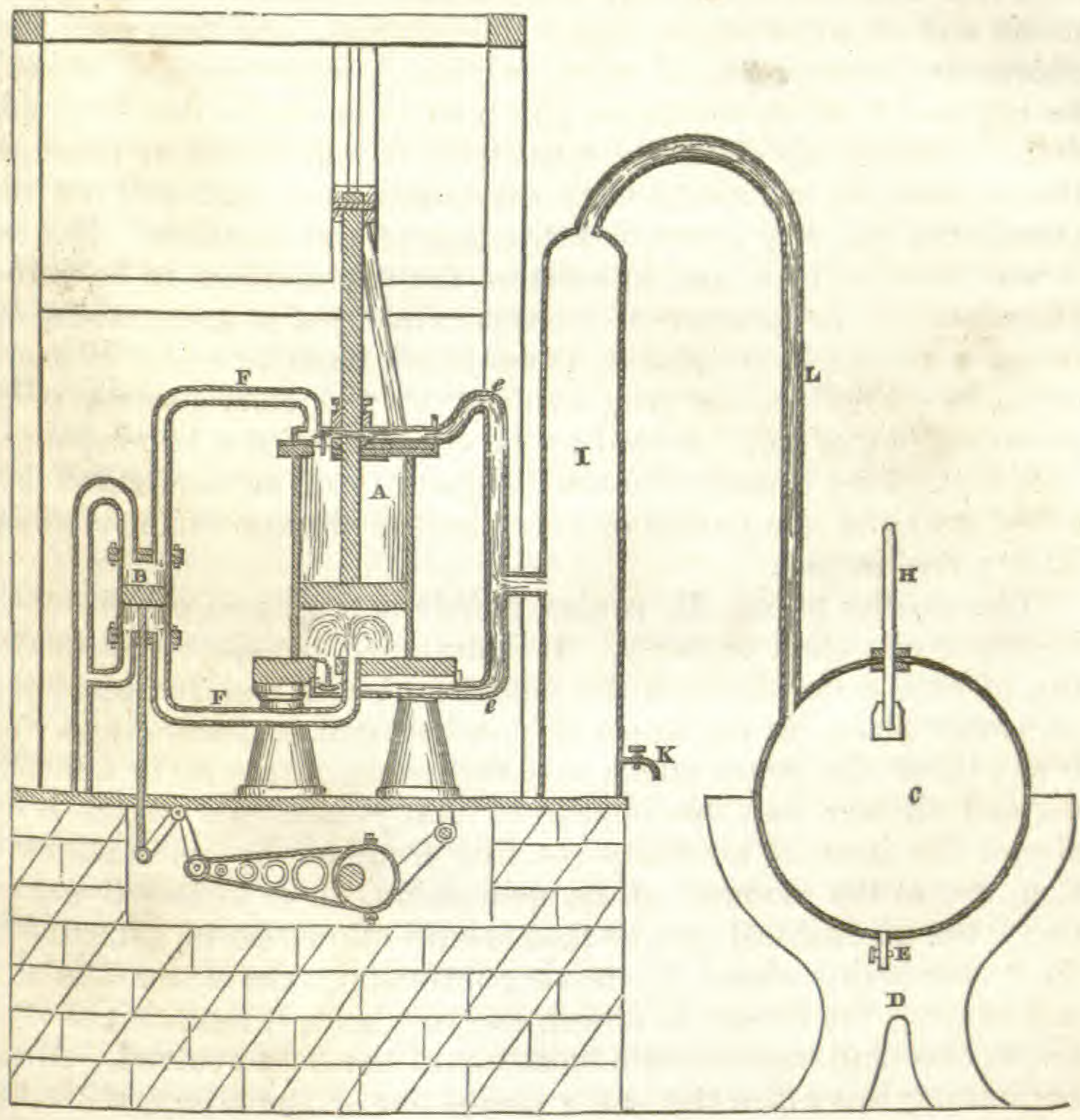
Upon a subject on which so many scientific men have directed their attention and tasked their ingenuity to discover methods of accurate observation, it would seem presumptuous to attempt to devise processes more exact than have been already employed. Yet the diversity in the results furnished by such able experimenters renders questionable the truth of any of them, and shows that their modes were defective, or, at least, that fresh investigation is necessary to enable us to confirm or reject their data with certainty. That in the general economy of nature the relation of caloric to air is of high importance, and that the investigation of the laws of their action is worth the labor they may require, and is indeed one of the most interesting enquiries, in both a scientific and practical point of view, to which the attention of mankind can be turned, are apparent from considerations already alluded to.

To improve our knowledge of these relations, so as to deduce the laws by which they are governed, we must study the subject on a larger scale than it has been heretofore examined; and with an object in view sufficiently valuable to justify the expenditure of money for adequate machinery, and of time for a long series of careful and precise observations. Experiments on a small scale, upon a material so attenuated as air, can scarcely be expected to speak absolute truth, or to answer the question as to what is the actual result of large operations. And besides being on a scale of suitable magnitude, they cannot be made accurately by rude and unskillful hands; they must be performed by persons qualified by knowledge and practice to conduct them through all the circumstances which a great range of temperature, and a variety of degrees of condensation may present. By conjoining expectations of pecuniary profit with philosophical views, it has been placed in my power to fulfill these conditions, and by a long course of expensive experiments, amidst much embarrassment of many kinds, to elicit a near approach to truth. A portion of the money expended in conducting these experiments, was furnished by some commercial gentlemen of the city of New Orleans.

As the object of these gentlemen, in advancing their money, was pecuniary gain, and this depended upon the quantity of heat absorbed, or in other words, the quantity of cold produced, and its applicability to the manufacture of ice, from the expansion of air after its condensation, I was constrained to pay as much, perhaps more attention to the evolution of this effect than to the quantity of heat disengaged by the condensation. Two series of experiments were thus carried on at the same time, and each was made to act as a check upon the accuracy of the other; while no labor or care was spared to render both accurate. To make these experiments, a large and powerful machine has been constructed, planned for measuring alike the heat developed by condensation, and the heat absorbed by rarefaction of any volume of air—from two cubic feet to two thousand cubic yards or more, and under any degree of condensation from two to eight atmospheres. By operating with such masses of air, the errors into which previous experimenters had fallen, have been, in a great measure avoided.

In the present article I propose to give an account of the quantity of heat evolved by the condensation of air under a compressing force of from two up to eight atmospheres consecutively; and in a succeeding number of this Journal to show the quantity absorbed by the expansion of the same air from a tension of two, four, and eight atmospheres, to the ordinary atmospheric pressure.

The machine, already mentioned as constructed, was intended for measuring alike the heat evolved by the condensation of air, and the heat absorbed by its subsequent expansion. The portion employed for the former purpose is represented in the figure below, which is an end view of the whole machine, and consists essentially of a large double acting force pump, A; a smaller force pump, B; and a reservoir or air magazine, C.



The larger pump, A, is constructed on the common principles, well known to mechanics, of the double acting force pump for air or water; and its object is the condensation of atmospheric air. It receives the air through valves, which may be constructed like the induction valves of the low-pressure steam engine, but which, in the present instance, are placed in the lids, opposite to each other, and open inwards. After compressing the air to the required degree, this pump forces it through eduction valves into the pipes *e, e,* which lead to the reservoir. The pump is thirteen inches in diameter with a twenty-four inch stroke of piston; and consequently, has a capacity, exclusive of the space occupied by the piston rod, of 3144 cubic inches.

Besides this large pump there was a smaller condensing pump, (not seen in the figure,) four inches in diameter, seventeen inches stroke, and, after deducting the average space occupied by the piston rod, of 206 cubic inches capacity. This pump was fitted with valves, and communicated with the reservoir, in the same manner as the larger pump. Its object was to charge the reservoir with air to any required tension before setting the larger pump and its antagonistic engine in operation, and thus save the enormous consumption of power which, it was supposed, would be required to work the larger pump so as to obtain the same effect. Though theoretically necessary, it was found in practice that it could be advantageously dispensed with, and will not be introduced into any future construction of the machine. But as it was attached to it, and in most of the experiments to be hereafter detailed, its measure of air was condensed at every stroke of its own and the larger piston, its operation must be taken into account in estimating the heat evolved by the condensation of a given volume of air. Added to the capacity of the larger pump, $(3144+206=)$ it made the whole quantity of air condensed by every stroke of the pumps or every half revolution of the engine, 3350 cubic inches.

The smaller pump, B, is also double acting, and of 56 cubic inches or one quart capacity. It is designed to inject that quantity of cold water through the pipes F, F, and a sieve-like plate, or "rose" placed in the upper lid, and several perforations in the lower lid of the larger pump, and thence distribute it, in a finely divided shower into the interior of that pump. Its object is to absorb the heat of elasticity set free from air by its condensation, and at the moment of its generation, so as to lessen materially the mechanical power that would otherwise be consumed. As it effects this object by mixing intimately the water and air, and causing the former to absorb the free heat, it renders the water an excellent approximate measurer of the heat evolved. After performing this office the water passes out of the pump, with the condensed air, into the reservoir.

The reservoir, or air magazine, C, employed in these experiments, is cylindrical in form, eleven feet in length, thirty-two inches in diameter, and has a capacity of about sixty-two cubic feet. It is made of sheet iron, with hemispherical heads, in the manner of a steam boiler. It is furnished with a safety valve about three inches in diameter, and loaded with a weight, through the intervention of a lever. At one end it has a man-hole affording admittance into its interior; and it is provided with a stopcock, E, inserted into its most dependant part, through which the water of injection, received from the pumps, and precipitated from the air by its greater specific gravity, may be discharged into the open atmosphere, or returned again to the pump, B, for a renewal of its duty. It is supported by three cast iron frames, one of which is represented at D.

When the air attains a certain pressure in this reservoir, it is allowed to discharge itself, by means of a communicating pipe, into an engine which works expansively through a valve so constructed, as to permit of being arranged to cut off the communication with the reservoir at any portion of a stroke. The air in this way, and independent of the safety valve, is prevented from attaining more than a certain degree of pressure.

There were several appendages to the machinery employed in these experiments which were not absolutely necessary, and would not be used in a practical application of the principle. Among them is the vessel, I. This was a reservoir of a former experiment, and was used, in this instance, to separate the water of injection from the condensed air, the former of which passed through the cock, K, while the latter proceeded through the pipe, L, into the reservoir, C. It is mentioned, and represented in the drawing, in order that all the circumstances attending the experiments may be understood.

As a means of measuring with precision the pressure of air in the reservoir, a gauge consisting of a glass tube, closed at the upper end, having a length of twelve inches, and an internal diameter of a quarter of an inch, was used. After filling this tube with dry air, and inserting it through the upper surface of the reservoir, into a cup of mercury, in communication with the air within the reservoir, it was found to act as a convenient manometer capable of giving the same indications, by the contraction of the contained air, as would have been given in similar circumstances by a column of mercury, placed as in the barometer, and of a height due to the density of the air. The accurate graduation of this instrument presented some difficulties, and was approximately attained by filling it with successive equal-weighed portions of mercury, and marking on the glass tube, and a metal scale surrounding it, the volume thus indicated. These marks were intended to express the scale of heights of the mercury, above its original level in a cup, in atmospheres of thirty inches in height. The instrument requires for perfect accuracy, corrections for changes of atmospheric temperature, as well as for the increased temperature of the air enclosed in the reservoir. But disregarding these sources of error, and probably some in charging the tube with mercury, it was found to be a valuable instrument; its indications were not only more easily attained, but were more accurate than those of the steam indicator, or common safety valve.

The standard which I have relied upon, in conducting the experiments of this research, for determining a measure of heat, is the assumed quantity required for the conversion of a pound of ice into water. In the absence of any experiments of my own upon the subject, I have taken the quantity at the generally received one of 140° F., though some observations incline me to consider it as several degrees less.

With the foregoing apparatus numerous experiments have been made; but as a small number, conducted on proper scientific principles—more particularly if they agree with each other, and have been, as in these instances, checked by the observations of two persons—are as satisfactory as any greater number, I have detailed, and given the calculations in full of only a few. These will serve as examples for any one who is disposed to compute from the observed data, the resulting quantity of heat obtained, and thus prove the truth, or error of the reported result. Finding that observations were made with more accuracy in proportion as experience familiarized me with them, I considered that the later ones were more to be trusted than the earlier, and I have, therefore, with one exception, selected them for this communication. The exception referred to, is contained in the first of the following tables,* and was the earliest experiment made by me with the machine I have just described. It is introduced for the reasons that it cannot be materially inaccurate, and that the temperature of the atmosphere at the time, was very different and much lower than at any of the later experiments.

It is proper to remark that in conducting these delicate experiments many causes of error have been either unavoidable, uncorrected, or overlooked. Owing to defects of mechanical contrivance and unskillful workmanship, incidental perhaps to every new device, and a noviciate intercourse with practical mechanics, the machine was not capable of performing all its duties with the accuracy the natural laws involved called for. And notwithstanding a great desire to avoid and correct errors from the leakage or irregular working of the machine, by compensating for them, I cannot in all cases rely upon the correctness of the allowances made therefor.

The thermometers used in these experiments were manufactured in New Orleans, and their indications differed so much from each other that they could not be fully relied upon. The nature of the investigation required instruments that should mark, if possible, the tenth part of a degree of Fahrenheit; but none that I could obtain was of sufficient sensibility to be read accurately within half a degree; and, therefore, all indications set down as more minute, must be regarded as conjectures, or interpolated calculations. As I found that no two thermometers agreed with each other, I was aware that it was desirable one or more should be verified by a comparison with the in-

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* See continuation, in next number of this Journal.

dications of an unquestionable standard, but this was not attainable. One common source of error, however, in the observations of the thermometers was avoided. Apprehensive from the smallness of their scales of graduation that difference of vision might lead to error, I was careful to obtain frequent observations from two or more persons, and in no instance, where there was a difference of opinion, was the observation recorded. Still, it is possible that the conclusions arrived at may in some instances be inaccurate; for I found that when observations were made by two persons on the same thermometer, they often differed from each other as much as a degree, and there is reason for supposing that even when alike they might be erroneous. I have dwelt upon the possibility of error from the imperfection of both thermometers and observers, because from the mode of experimenting adopted, a slight difference between the actual heat evolved and that observed, would present a very considerable difference in the proportion in the air itself.

The want of a barometer prevented any modifications in the calculation of results, on account of variations in the density of the air.

(*To be continued.*)

ART. VII.—*On the Computation of the Sun's Daily Intensity at the exterior surface of the Earth, and Secular Changes of Heat;* by L. W. MEECH, A.B., Preston, Ct.

THERE are reasons for believing that the phenomena of solar heat may be as completely interpreted by analysis as are the tides of the ocean. The subjoined contribution is offered in pursuance of this object.

I. Before proceeding to the general investigation, let it be proposed to determine the diminution of heat arising from the secular decrease of the eccentricity of the earth's orbit: in consequence of which, the orbit approaches to the form of a circle, and the earth is constantly removing to a greater distance from the sun, between the apses; the transverse axis remaining invariable. Let then e represent the eccentricity, regarded as constant for one year; ϱ, the radius-vector; φ, the mean anomaly; and θ, the true anomaly, for any given time. Also it is known that $\pi\sqrt{1-e^2}$ expresses the whole area of the ellipse; and $\int\frac{1}{2}\varrho^2 d\theta$, that of the elliptic sector corresponding to θ. Whence by Kepler's law of equal areas,

$$\pi\sqrt{1-e^2} : \int\tfrac{1}{2}\varrho^2 d\theta :: 2\pi : \varphi, \text{ or}$$

$$\frac{1}{\varrho^2} = \frac{d\theta}{d\varphi\sqrt{1-e^2}}.$$

Since heat varies "inversely as the square of the distance" ϱ, this equation evidently measures its intensity. To obtain the sum of the intensities in a year, the equation must be multiplied through by the uniform $d\varphi$, and integrated between the limits o and 2π: then rejecting the constant factor 2π, there remains the relative

$$\text{Annual amount of heat} = \frac{1}{\sqrt{1-e^2}}. \qquad (1.)$$

Again, let the slight decrement of e in one or more centuries be denoted by h; writing $e-h$ in place of e and developing for the first power of h by Taylor's Theorem; there results the proportional

$$\text{Secular diminution} = \frac{-eh}{1-e^2} = -eh - e^3h - \ldots \qquad (2.)$$

On January 1, 1801, the value of e was 0·01678357, with a centurial decrement of $-0{\cdot}00004163$, the centurial value of h. And at this rate the orbit would become a circle in 40,300 years, though it is improbable that it will reach this limit. The diminution in a century is readily ascertained by substituting the values of e, h, in the last formula, which gives $-0{\cdot}000\,000\,69$; and it will be shown hereafter in the computation for Mendon, that such numbers are nearly or quite proportional to the corresponding degrees of Fahrenheit's scale. Hence the secular decrease of the annual quantity of heat, arising from secular change of the sun's distance alone, is too small to be sensible to the thermometer in a hundred years, in a thousand, or in ten thousand years, and scarcely so, at the utmost limit, when the orbit becomes a circle. For even then, the mean temperature on the equator, which is now 82°, would not fall more than 0·025 of a degree of Fahrenheit.

It is thus demonstrated that the mean annual heat received by the earth as a whole, is virtually constant during a sidereal year, so far as secular change of the sun's distance alone is concerned; and by reason of the nearly constant excess, the same may be concluded of the tropical or civil year.

II. Again, let it be proposed to ascertain *the sun's relative intensity at any given instant during the day.* For this purpose,

Let L = the 'apparent' Latitude of the place,
D = the sun's meridian Declination,
Δ = the sun's semi-diameter,
A = the sun's Altitude, and
H = the Hour-angle from noon.

The horizontal projection of the sun's disc on a plane at the exterior surface of the earth is well known to be an ellipse; and if 1 denote the sun's radius, 1 will likewise denote the semi-conjugate axis of this projected ellipse; while the horizontal projection of 1, which is $\frac{1}{\sin A}$, will be the semi-transverse axis. The area of the elliptic projection is therefore $1\times\frac{1}{\sin A}\times\pi$. But the intensity of the same quantity of heat being inversely as the space it covers; the reciprocal of this area $\frac{\sin A}{\pi}$, or rejecting the constant divisor π, *sin* A *will measure the sun's intensity at the altitude* A, supposing the distance to be constant.

But the sun's intensity further varies inversely as the square of the distance, that is, directly as the square of the apparent diameter or semi-diameter of the disc. Hence

$\varDelta^2 \sin A$ measures the sun's intensity at any given instant during the day.

To assign the value of sin A, by spherical trigonometry, the sun's distance from the pole or co-declination, and from the pole to the zenith or co-latitude, and the included hour-angle from noon H, are given to find the third side, or co-altitude. The well-known formula for this case becomes, by writing sines instead of the cosines of their complements,

$$\sin A = \sin D \sin L + \cos D \cos L \cos H, \text{ and}$$

$$\varDelta^2 \sin A = \varDelta^2 \sin D \sin L + \varDelta^2 \cos D \cos L \cos H. \quad (3.)$$

At the time of the equinoxes, D becomes 0, and the expression of the sun's intensity reduces to $\varDelta^2 \cos L \cos H$. That is, the degree of heat at different places, then, decreases from the equator toward each pole, proportional to the cosines of the respective latitudes. At other times of the year, however, a different law of distribution prevails, as indicated above.

The sun's intensity at a fixed distance being as the sine of the altitude, it follows that the sun shining for sixteen hours at an altitude of 30°, would raise the temperature of a plain as high, as if it shone for eight hours from an altitude of 90°, or from the zenith, since sin 30° is ·5, and sin 90° is 1.

III. Proceeding now to the general problem, it is required to determine the quantity of heat radiated upon a given place at the exterior of the earth on any given day. The quantity radiated at any instant, has already been made known, as $\varDelta^2 \sin A$: hence multiplying the last equation above by the uniform dH, and integrating,

$$\int \varDelta^2 \sin A\, dH = \varDelta^2 \sin D \sin L \,.\, H + \varDelta^2 \cos D \cos L \sin H. \quad (4.)$$

Regarding H as a semi-diurnal arc, the second member will express the daily quantity of heat for a half day, and so for the whole day. Also by integrating between the limits of any two hour-angles H′, H″, the quantity of heat will be found for any assigned part of the day. The sun's declination has here been taken as constant, and it might easily be shown that what the sun radiates by rising earlier than the implied time, is compensated by a uniform change of declination producing an earlier setting sun.

For testing this formula, the intensities were computed for the 15th day of each month, on the latitude of Mendon, Mass.; and the results were found to agree very nearly with those observed at that place about *one month later*,* as follows: the observed values are taken from the American Almanac for 1849, and are derived from fifteen years' observations.

Computed values.			Observed values.	Difference.
January 15, ...	5040	23°.3	24°.3, February 15, ..	+1°.0
February 15, ...	7142	33°.1	33°.5, March 15, ...	+ .4
March 15, ...	9764	45°.2	45°.8, April 15, ...	+ .6
April 15, ...	12574	58°.3	55°.0, May 15, ...	−3°.3
May 15, ...	14482	67°.1	64°.5, June 15, ...	−2°.6
June 15, ...	15346	71°.1	71°.8, July 15, ...	+ .7
July 15, ...	15085	69°.9	68°.9, August 15, ...	−1°.0
August 15, ...	13437	62°.3	61°.0, September 15,.	−1°.3
September 15, ...	10860	50°.3	48°.5, October 15, ...	−1°.8
October 15, ...	8080	37°.5	38°.9, November 15, .	+1°.4
November 15, ...	5638	26°.1	27°.7, December 15, .	+1°.6
December 15, ...	4510	20°.9	26°.0, January 15, ...	+5°.1

It may be proper to observe that the preceding formula was divided by sin L, a constant factor; and the numbers in the second column were then successively computed: their sum divided by twelve, gave 10163 as the mean, to be compared with 47°.1, the observed mean at Mendon. Then as 10163 : 47°.1 : : 5040 : 23°.3, Jan. 15, etc. Let it also be observed, that the Mendon values are the monthly means, which do not always fall on the 15th day, but nearly so.

Before applying the formula further, let it be simplified by means of the astronomic equation, $-\cos H = \tan L \tan D$.

Dividing and multiplying the above intensity by $\sin D \sin L$, and at the same time substituting $-\cos H$ for its equal; and $\tan H$,

$$\text{Daily intensity} = \varDelta^2 \sin L \sin D\,(H - \tan H). \qquad (5.)$$

This expression is much simpler than the one before employed, and by thus computing the temperatures for different places, and comparing the results with those actually observed, the extent of local causes will be disclosed. The ease with which daily and monthly temperatures may now be computed, it is believed will render this formula valuable in meteorological researches. It is

* Since writing the above, I find it elsewhere stated that the observed epochs of maximum and minimum temperature in the North Temperate Zone fall about a month later than their expected times at the solstices. The reason assigned for this circumstance is, that for about a month after the summer solstice, the earth continues to receive during the day more heat than it loses at night; and conversely after the winter solstice, it loses more heat during the night than it receives during the day.

to be mentioned here, that H is an arc of a circle whose radius is 1, which will readily be found by reducing the degrees and minutes, to minutes, and multiplying them by sin 1′, or by multiplying the minutes in time by sin 15′; also using the natural tangent of H, with its trigonometric sign. When the computation is for one place only, sin L being a constant factor may be rejected, leaving,

$$\text{Daily intensity} = \Delta^2 \sin D\,(H - \tan H.) \qquad (6.)$$

For example, the results are subjoined for Calcutta, in latitude 22° 35′ N.; the observed temperatures follow the computed, in order:—

January,	59°.2; 65°.1	July,	92°.3; 82°.6
February,	69°.2; 70°.0	August,	88°.9; 82°.9
March,	79°.5; 78°.1	September,	82°.5; 82°.4
April,	88°.0; 83°.3	October,	72°.6; 81°.0
May,	92°.0; 85°.5	November,	61°.8; 73°.4
June,	93°.1; 84°.7	December,	56°.2; 66°.6

From this it appears, that at midsummer about one-tenth of the heat received is carried away by those upward currents which produce the trade-winds, by sea-breezes, and the conducting power of the soil; also a less quantity of heat is restored by the ground, and by winds in mid-winter.

IV. Is the temperature of summer and of winter in the southern hemisphere, the same as with us, at equal latitudes? On this point, the formula $\cos H = -\tan L \tan D$, clearly shows that the length of the day there is the same as in the northern hemisphere six mouths later. The difference of temperature is therefore limited to the values of Δ^2. Now the earth being nearest the sun at the northern mid-winter or on January 1st, Δ^2 is at that time measured by $\frac{1}{(1-e)^2}$; while at mid-winter in the southern hemisphere Δ^2 is as $\frac{1}{(1+e)^2}$. The difference is $\frac{4e}{(1-e^2)^2}$; or the proportional difference, taking our mid-winter's temperature as 1, is

$$\frac{4e}{(1+e)^2} = 4e - 8e^2 + \ldots = 0{\cdot}06 \text{ nearly.}$$

The southern mid-winter is therefore from 1° to 2° colder; and the mid-summer from 4° to 5° hotter than at an equal latitude in the northern temperate zone, as will appear by multiplying the Mendon values by 0.06. This advantage of the northern hemisphere is due to the advance of the perigee in longitude of 11″.8 per year, which in time will return to the southern hemisphere the advantage we now possess. The summer season of the southern temperate zone being hotter, is also shorter by about eight days than in the northern hemisphere; which difference is ascribed to the more rapid motion of the earth about the perigee.

V. Are the winters now, as cold as at the first settlement of New England? The impression generally prevails that the winters are growing milder, and the spring is later than formerly. Certainly the snows have not been so deep as in the days of Cotton Mather; neither has Boston bay nor the Chesapeake of late years, been frozen over, as far as the eye could reach. So far as this change is astronomic, and not the result of local changes, it may be determined by means of the formula

$$\text{Daily intensity} = \Delta^2 \sin D\,(H - \tan H).$$

In the first place, let it be proposed to ascertain the change of each factor, for the last two centuries, which will refer the formula to the epoch of A. D. 1650, the period of our colonial history. The first factor Δ^2, depends for its value on the anomaly θ, which has an annual change of $11''.8$, and on the eccentricity e, which has a secular variation of 0.000041. Accordingly, introducing these increments into the analytic expression for the inverse square of the radius-vector, which is the value of Δ^2, and developing by Taylor's Theorem, it is found that both sources of variation cannot affect the present values of Δ within the fourth significant figure, in two centuries: hence these sources of change may be safely neglected in the present calculation.

The second factor, $\sin D = \sin\omega\,\sin T$; where ω denotes the obliquity of the ecliptic, and T the sun's longitude. Since the tropical or civil year is the interval between two successive returns to the same longitude, $\sin T$ ought in the present case to be regarded as constant. But in referring back, ω has a secular increase of $45''.7$, or $91''.4$ in two centuries. Let this increment be denoted by ω'', and the contemporary increment of D by D''. Then developing for the first power of each in the above equation, by Taylor's Theorem, $\cos D\,.\,D'' = \cos\omega\,\sin T\,.\,\omega''$. Here substituting the value of $\sin T$ from the preceding equation, and dividing by $\cos D$,

$$D'' = \cot\omega\,\tan D\,.\,\omega''.$$

For the third factor involving the semi-diurnal arc H, astronomy gives $-\cos H = \tan L\,\tan D$; and developing as before, by Taylor's Theorem, $\sin H\,.\,H'' = \tan L\,.\,D'' \div \cos^2 D$. Substituting for D'' its value found above, also dividing by the preceding equation, $-\cos H = \tan L\,\tan D$,

$$-\tan H\,.\,H'' = \frac{\cot\omega\,.\,\omega''}{\cos^2 D}.$$

Recurring now to the formula of daily intensity, let it be denoted by S, at the same time developing for the first power of the increments of the two variables, D, H; the resulting increment is

$$\frac{S}{\sin D}\cos D\,.\,D'' + \frac{S}{H - \tan H}\left(1 - \frac{1}{\cos^2 H}\right)H'', \text{ or}$$

$$S \cot D \,.\, D'' - S \tan^2 H \,.\, H'' \div (H - \tan H).$$

And substituting the values of D″, and H″, it becomes

$$S \cot \omega \,.\, \omega'' + S \tan H \cot \omega \,.\, \omega'' \div (H - \tan H) \cos^2 D,$$

which may further be reduced, and dividing by S, the *proportional* secular

$$\textit{Increment of heat} = \cot \omega \,.\, \omega'' \,(H + \tan H \tan^2 D) \div (H - \tan H). \quad (7.)$$

Lastly, restoring the value of S, there results the *absolute* secular

$$\textit{Increment of heat} = \Delta^2 \sin D \cot \omega \,.\, \omega''(H + \tan H \tan^2 D).$$

At the time of the equinoxes, D, and with it this increment becomes zero: the greatest values may hence be inferred to prevail near the solstices when D equals ω. For example, computing by formula (7) the proportional increment for December 15, on the latitude of Mendon, and multiplying into the observed number of degrees, it appears that two centuries ago, the mid-winter was colder by 0°.035 F., and the mid-summer hotter by 0°.025 F.: that is, the temperature of the year is growing more equable by slight gradations. It has already been shown in IV, that the year is remarkably so, compared with that in the southern hemisphere.

That the spring is now later, may be accounted for, by the fact that less snow falls, and the ground being uncovered, has, during the winter, radiated more heat into space, and so at the opening of spring is colder than when covered through the winter by a deep stratum of non-conducting snow. And in general, any variation from the mathematical laws here demonstrated, must evidently be looked for, as the effect of cultivation or other terrestrial causes. Besides, as the sun is the great source of the change of seasons, winds, evaporation, etc., these laws, especially the formulas in III, may yet serve as a basis for computing *a priori* the dew-point, monthly humidity, and range of the barometer.

Lastly, among other secondary causes which tend with striking uniformity to counteract extremes of heat and cold, is the vast size of the sun. Were the same amount of light and heat radiated from a body smaller than the earth, the effects would be restricted to a smaller portion of the earth's surface. But as it is, the sun constantly illuminates half the earth's surface and a belt around the earth sixteen miles wide besides, which may be termed 'the zone of differential radiation;' and this is yet very much widened by atmospheric refraction. The effect of this arrangement is evidently to maintain a warmer temperature, besides rendering the transitions more mild and gradual. In conclusion, the evidences of stability of temperature demonstrated in the present article, it is proper to observe, strikingly accord with the authoritative declaration, that, "while the earth remaineth, seed-time and harvest, and cold and heat, and summer and winter, shall not cease."

ART. VIII.—*Notice of Fossil Bones from the neighborhood of Memphis, Tennessee;* by JEFFRIES WYMAN, M.D.

THE specimens of fossils here noticed were kindly sent me for the purposes of description, by Prof. B. Silliman, Jr. They were obtained from the region of Memphis, by Mr. Lawrence of Louisville, and, as is supposed, from the diluvium of the Mississippi. They are representatives of the following genera of mammals, viz., Mastodon, Megalonyx, Castor, Castoroides. They all present points of interest, especially the last three, which belong to genera whose remains are either quite rare or but very imperfectly known.

As yet, our knowledge of the osteology of Megalonyx is far behind that of the allied genera Megatherium and Mylodon; its dentition likewise is but very imperfectly understood. Many fragments of bones and teeth of Megalonyx doubtless exist in public and private collections in this country, which are yet undescribed. Any notices of individual specimens, especially of the teeth, which may be published, will be of great value to the paleontologist in enabling him to form more complete ideas of the organization of this most interesting race of extinct animals as well as of its geographical distribution.

The remains of Castoroides are exceedingly rare and are known only from a lower jaw and some of the bones of the extremities, discovered in Ohio, and described for the first time by Mr. J. W. Foster from fragments of bones discovered by Dr. Dickerson in the neighborhood of Natchez, Miss., and a cranium nearly entire found in the town of Clyde in New York. Although the Castoroides has so wide a geographical distribution, the above constitute the only localities excepting the one noticed in this communication, references to which have fallen under my observation.

The remains of beavers in a fossil condition would seem from the absence of published reports, to be quite as rare as those of the preceding genus. Doubtless many of them exist in cabinets, but have not yet found their way into scientific journals or treatises. Notices of their remains would likewise be of great interest, if for no other purpose than to establish their identity with, or dissimilarity to, the existing species, or to determine the former geographical distribution of these animals on the North American continent. The remains of Castor Europæus, (which by some has been regarded, though on insufficient grounds, as identical with the American species,) have been found in England, generally in peat bogs,* in the marls of Perthshire and Berwickshire in Scotland,† and on the continent in the valley of the Somme

* Owen. British Fossil Mam. and Birds, p. 190.

† Lyell. Principles of Geology, vol. iii, p. 326.

in Picardy,* in the peat of Flanders, and on the shores of Lake Rostoff.

In this country Prof. Spencer F. Baird, in an interesting communication to the American Association for the Advancement of Science, states that in one of the caves in Pennsylvania, he has found the remains of recent beavers associated with bones of recent species of wolves, foxes, bears, rabbits, muskrats, otters, lynxes, panthers, &c., as well as with some bones, about five per cent. of the whole, of extinct species.† I have not been able to find any notice of remains of beavers found in the United States in a fossil condition. At present, beavers are confined to the more northern districts, extending as far as the Mackenzie river, lat. 67° or 68°, though we have occasional notices of their existing as far south as the mountains of North Carolina, (S. B. Buckley in Am. Journ. Sc., vol. iii, [2], p. 434,) and in several places in Alabama, (Prof. R. T. Brumley in Am. Jour. Sc., vol. iv, [2], p. 285.) They have been found likewise near Peter's mountain in Virginia, near Ashville, N. C., near Milledgeville, Ga., in Dallas county, Ala., and Flanders county, Ky. Dr. Bachman thinks they formerly existed over the whole continent as far south as the tropic of Cancer.‡

No. 1. *Mastodon giganteum.*—This was one of the earlier teeth of a young Mastodon; the longest diameter of the crown was 1·8 inch; its transverse diameter at the posterior edge was 1·6 inch, and at the anterior 1·2 inch, allowance being made for the enamel which is in part deficient. The points of the crown have been entirely worn off, the grinding surface is quite oblique, highest on its outer edge, somewhat excavated, and very beautifully polished. At the posterior inner angle the crown is worn below the level of the enamel, but on its outer face remains of the bases of three ridges are still visible, invested with their coating of enamel.

The remains of two roots or fangs still exist, one of which had been almost wholly absorbed, and the other had the length of an inch, although absorption of its extremity had obviously taken place. The small size of this tooth, together with the existence of but three ridges on the crown, indicate that it was the third tooth in the series.

No. 2. This is likewise a tooth of a young Mastodon, but larger than the preceding. The crown was more worn, its surface less oblique, and more deeply excavated. On one of its margins, vestiges of three ridges remain, and in addition a vestige of a rudimentary fourth ridge. The longest diameter of the grinding surface was 2·7 inches; its larger transverse diameter 2·1, and its

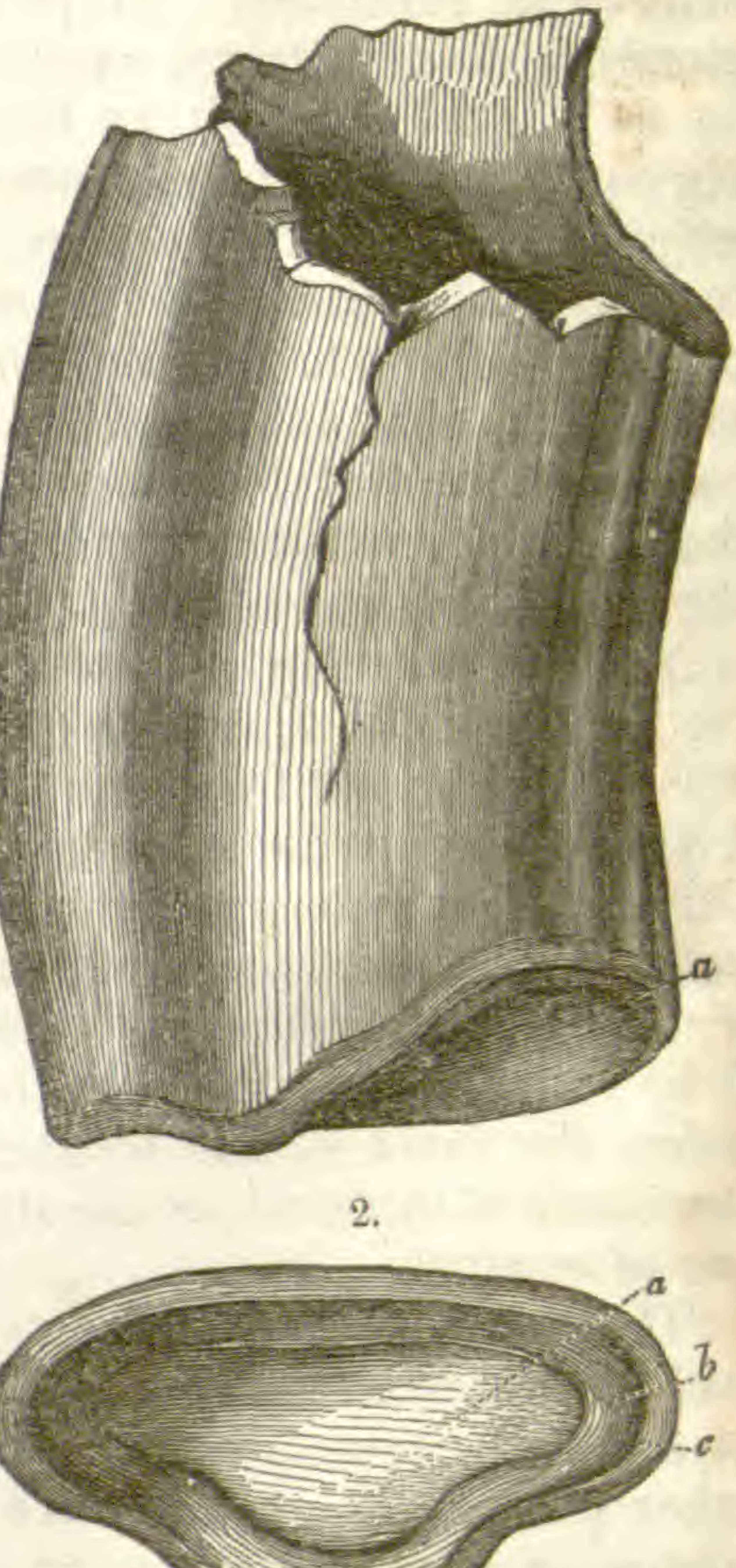
* Cuvier. Oss. Foss., tome viii, p. 108, 4th edit., 1836.
† Proceedings of Am. Assoc. Adv. of Science at Cambridge, 1849, p. 352.
‡ Audubon and Bachman. Vivip. Quad. of North America, vol. i, p. 356.

smaller 1·8 inch. This, from its greater size, as well as from the existence of a rudimentary fourth ridge, was probably the fourth tooth in the series.*

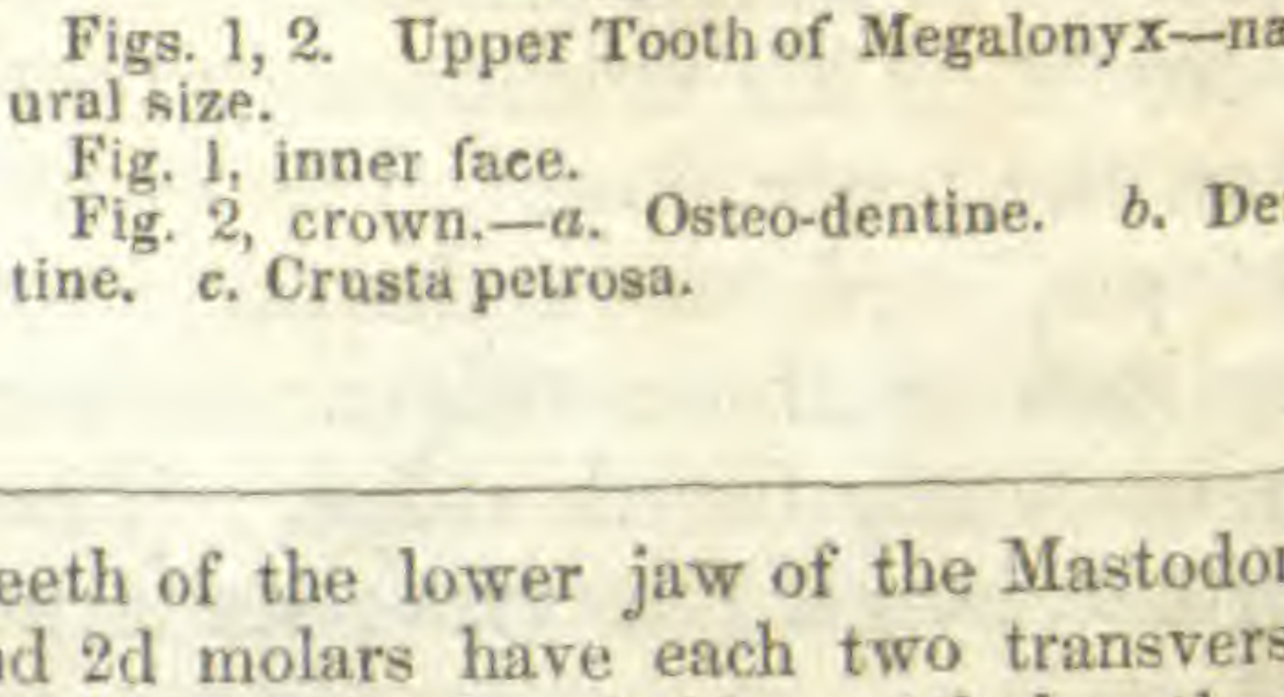
The absorption of the fangs, as well as the grinding down of the crown have been carried so far, that it seems quite probable that it actually had been, or was just upon the point of being shed.

No. 3. *Tooth of Megalonyx laqueatus Harlan.*—This tooth, which is represented of its natural size in the adjoining figures, (figs. 1 and 2,) has the grinding surface perfect, but the opposite extremity is broken off. The form of the grinding portion is elongated oval, has an excavation in the middle, which is continuous with a deep emargination on one of the sides of the tooth (fig. 1 *a*); its longest diameter is 1·6 inch, and its transverse 0·8 inch. On the inner face of the tooth there exists a strongly projecting ridge, which extends from the crown to the base. On the ground surface, three distinct portions may be discovered, viz., 1st, a central one (fig. 2 *a*), consisting of ossified pulp, *osteo-dentine*, 2d, a middle portion (*b*) of *dentine*, and 3d, an outer incrustation of *crusta petrosa*, (*c*). The outer edge of the dentinal portion forms a sharp and prominent ridge around the circumference of the crown; the osteo-dentine having been worn away within it and the crusta petrosa on the outside. At the broken end, the pulp cavity is quite large, having walls of only about 0·2 inch in thickness, and is filled with a compact earthy deposit.

1.



2.



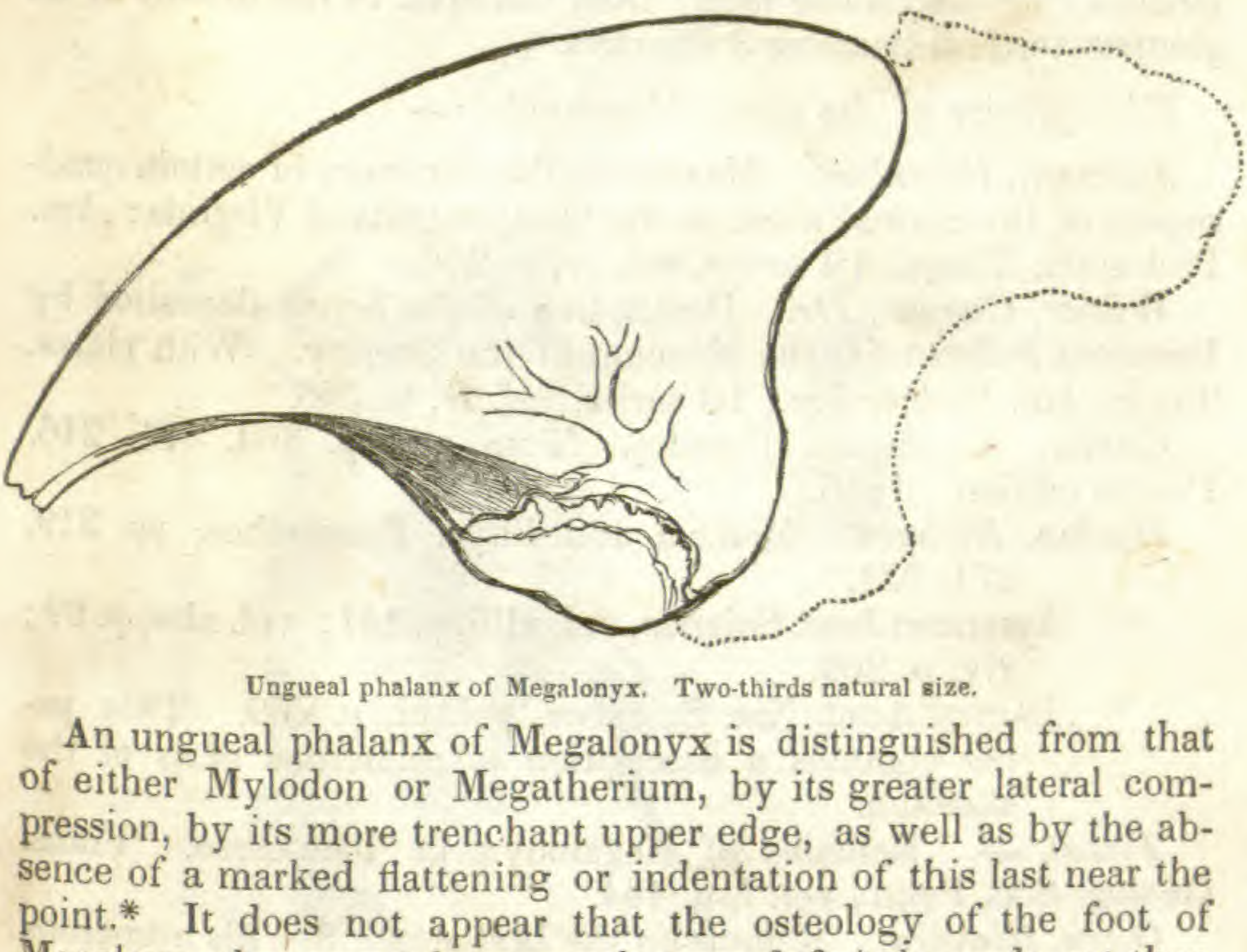
Figs. 1, 2. Upper Tooth of Megalonyx—natural size.
Fig. 1, inner face.
Fig. 2, crown.—*a*. Osteo-dentine. *b*. Dentine. *c*. Crusta petrosa.

* With regard to the succession of the teeth of the lower jaw of the Mastodon, the following rule holds good: the 1st and 2d molars have each two transverse ridges; 3d, three ridges with an anterior and posterior basal ridge; 4th, has three ridges with a large posterior tuberculate talon; 5th, resembles the preceding but larger; 6th, supports four ridges and a small talon; 7th, five ridges and a talon. See Owen's Odontography, p. 620; also J. B. S. Jackson, M.D., Proceedings of Bost.

This tooth resembles that of M. Jeffersonii, figured by Cuvier in the Ossemens Fossiles, in the existence of the lateral ridge, but in its proportions does not materially differ from that of M. laqueatus, Harlan, described and figured in his Med. and Phys. Researches.

No. 4. *An ungueal phalanx of Megalonyx.*—This is imperfect and was from a young animal, as is indicated by the fact the epiphysis forming the articulating surface was not cöossified with the body of the bone, and has been detached. (Fig. 3.) Its general resemblance to the corresponding part of one of the Carnivora is sufficiently strong to render excusable the error of President Jefferson, who referred some remains of Megalonyx to this group of animals.

3.



Ungueal phalanx of Megalonyx. Two-thirds natural size.

An ungueal phalanx of Megalonyx is distinguished from that of either Mylodon or Megatherium, by its greater lateral compression, by its more trenchant upper edge, as well as by the absence of a marked flattening or indentation of this last near the point.* It does not appear that the osteology of the foot of Megalonyx has as yet been much more definitely made out than is its dentition.

The specimen here described differs in its proportions from those of which I have been able to find any figures, it being much higher in proportion to its length, allowance being made for the absence of the articulating epiphysis. Its upper edge is quite regularly curved, rounded near the base, but the half near the apex is trenchant. The point of the bone is depressed, and is separated from the tuberosity by an obtuse angular notch. The under edge of the bone is rounded near the apex, but becomes

Soc. Nat. Hist., vol. ii, pp. 60 and 140. Dr. Jackson has shown by actual examination that the molar tooth described by Prof. Owen as replacing the first and second milk teeth, and developed in the jaw beneath them in Mastodon angustidens, does not exist in the Mastodon giganteum.

* Owen's Memoir on the Mylodon, p. 106.

flattened and is divided by a prominent ridge, which forming the base of the tuberosity in front, becomes gradually thicker and stronger as it merges into the latter. Laterally near the point and at the base are numerous vascular channels and openings; one orifice of much greater size than the rest, exists on each side just above the tuberosity, and from it several deeply impressed channels extend towards the extremity of the bone. The tuberosity projects from each side of the bone, the upper surface of this projection being excavated into a deep and narrow channel.

In consequence of the absence of a part of this bone, the proportional measurements of the whole cannot be given. Its greatest height from the tuberosity vertically upwards is 3 inches, its greatest thickness is one inch; from the apex to the middle of its greatest vertical diameter 3·5 inches.

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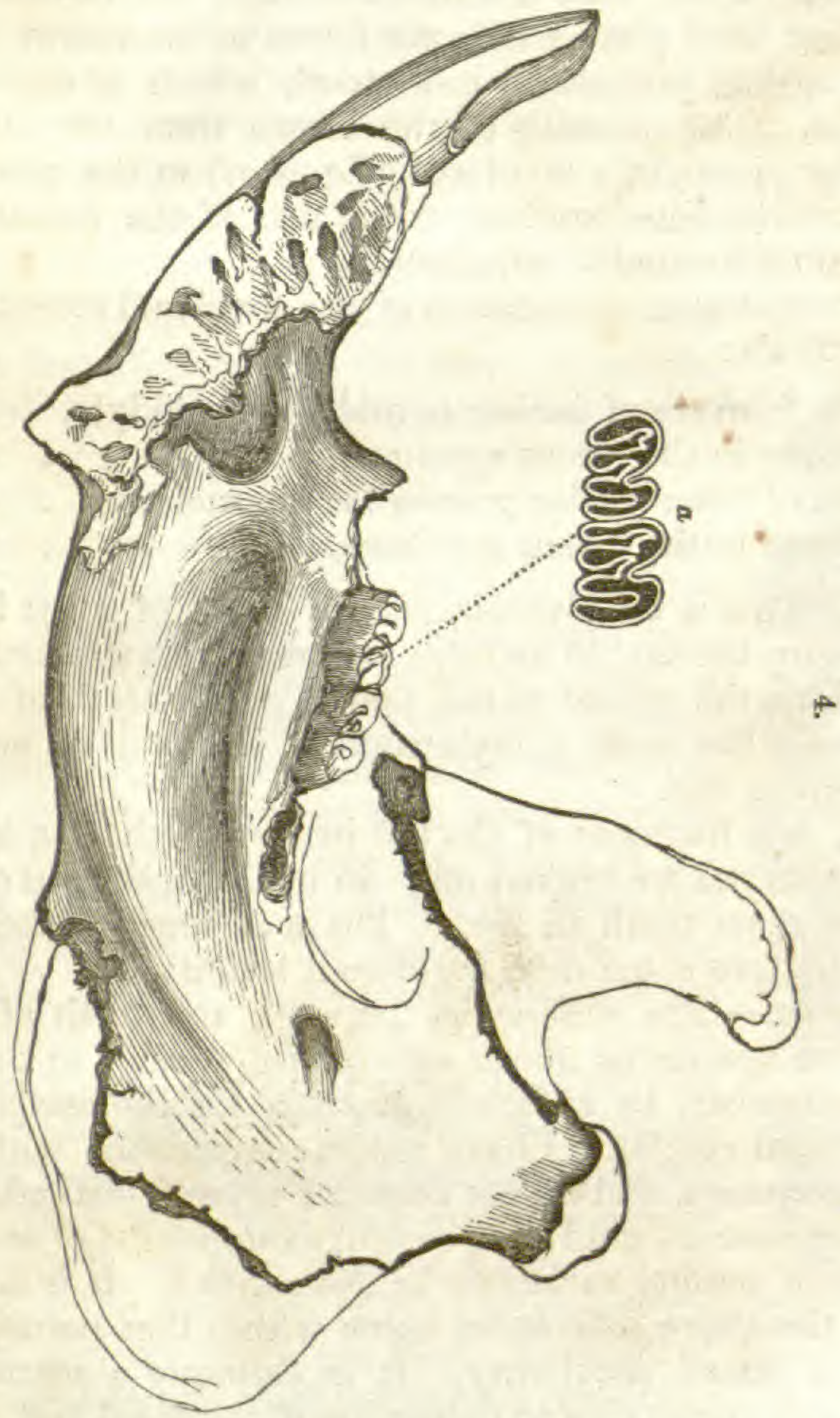

Right Lower Jaw of C. fiber, americanus. Natural size.

No. 5. *Lower Jaw of a Beaver.*—This is the jaw of the right side, (fig. 4,) is deficient in a portion of the coronoid process which is broken obliquely across from the condyle to near its base in front; a large portion of the "angle" of the bone is likewise destroyed, and the first and fourth molar teeth are lost. In its general conformation it corresponds very nearly with that of a specimen of a common beaver (Castor fiber, americanus), from the neighborhood of Moose-head Lake, except that the impressions for the attachments of the temporal and masseter muscles are more strongly marked in the fossil than in the recent specimen. The form of the condyle is somewhat different in the two; that of the fossil specimen being longer in its antero-posterior diameter and resting on a much broader neck; the whole jaw when viewed from above is also more curved, having the form of an italic *ʃ* more strongly marked.

The second and third molars alone exist, and on close comparison present some peculiarities not found in the recent jaw. The anterior fold of enamel in each tooth which is directed across nearly the whole breadth of the crown from the inner to the outer edge, is slightly involuted (fig. 4 *a*,) at the point where it approaches the outer surface, while that of the recent specimen forms a simple rounded termination.

The comparative dimensions of the fossil and recent specimens are as follows:

Length from tip of incisor to middle of condyle,	3·9 inches.
The same in the recent specimen, ...	3·5 "
Length of the alveolar process for the molars, .	1·5 "
The same in the recent specimen, ...	1·3 "

No. 6. This is an alveolar portion only, of a left lower jaw, and contains the 2d, 3d and 4th molars, which gradually become smaller from the second to the fourth. The teeth of this specimen present the same complication of the folds of enamel as in the preceding one.

No. 7. Is a fragment of the left incisor tooth of a beaver, but both extremities are broken off. In its dimensions it corresponds with the same tooth in No. 5, but is of much darker color and appears to have come from a different locality.

Remarks.—The differences between the teeth of the fossil and recent specimens above referred to, would, in the minds of many naturalists, be sufficient grounds for the establishment of an additional species. I have made comparisons with only two recent specimens, and do not consider myself justifiable in forming a new species, until by a careful examination of several jaws, the limit of natural variations be determined. It is not improbable that the above differences come within that natural limit, or may be a sexual peculiarity. It is certainly a matter of some interest and importance to determine if the fossil and recent species are identical.

No. 8. *Castoroides Ohioensis, Foster.*—The most interesting specimen from the Memphis collection is a large fragment of the right half of the lower jaw of this recently discovered species, a species not only interesting for some of its osteological peculiarities, but for the fact of its being the most gigantic member of the order of Rodents hitherto discovered, whether recent or fossil.*

The present fragment is 7·3 inches in length, though the condyle, coronoid process, "angle" and the whole of the under portion of the jaw have been broken off—a portion of the notch (sigmoid) between the condyle and coronoid process remains. The four molar teeth with their alveolar dependencies are entire, and a large fragment of an incisor tooth is still preserved lodged in its partly destroyed alveolus. The jaw is larger and somewhat more massive that the one described by Mr. Foster, but in other respects does not appear to differ materially from it. An approximation to the proportional dimensions of the three jaws which have been noticed may be deduced from the following measurements.

Length of the grinding surface of the molars in

The specimen from New York,	2·7½	inches.
" " " Ohio,	2·8	"
" " " Memphis,	3·1	"

In some other measurements the Memphis specimen exceeds the preceding ones in the same proportion.

5.

Molar teeth of Castoroides Ohioensis, right side of lower jaw,—natural size.

The grinding surface of the molar teeth taken together is somewhat concave in the direction of its length. The anterior tooth rises nearly three-fourths of an inch above the alveolus, the second and third sink nearly to its level, but the fourth is a little more elevated. Each tooth consists of a series of elongated elliptical plates or laminæ of enamel which include the dentinal portion; they are directed obliquely across the crown, and cemented together by a layer of crusta petrosa. The front tooth (fig. 5 *a*) which is the largest, has four laminæ, the first quite small and the third the longest and the most oblique. On the outer face there is a deep groove between the third and fourth plates, and on the inside one between the first and second, another between the second and third, and a very indistinct one between the third and fourth. The second and third grinders have but three laminæ, and their grinding surface has the outline of an hour glass or a figure 8, in consequence of the existence of but a single groove on the outer and on the inner face. In both of these teeth the first and third laminæ are parallel to each other, but the second makes an angle with them, so as to touch the first on the outside, and the third on the inside of the

* This species was first recognized and described by Mr. J. W. Foster in Am. Jour. Sci., vol. xxxi, p. 80, also in the second Report of the Geological Survey of Ohio, p. 81. A cranium nearly entire was subsequently discovered in Clyde, N. Y., and was described and figured in the Boston Journal Nat. History, vol. v, p. 385.

tooth. The outer groove is situated between the second and third plates and the inner between the second and first, but in consequence of the obliquity of the third plate, are nearly opposite. The fourth tooth is the smallest, the laminæ are all nearly parallel to each other and the lateral grooves are much less deep than in the preceding teeth.

It does not appear from any examination which I have made of the fractured teeth, that the different laminæ are united together at their bases by a continuous layer of enamel passing from one plate to the other, as is the case in the teeth of the elephant. Each tooth appears to be made up of a series of flattened dentinal columns invested with enamel and simply cemented together by crusta petrosa.

The incisor tooth in a transverse section is of a triangular form, with rounded angles; its inner face smooth and concave, its anterior convex and fluted with well marked parallel grooves, and its posterior convex and smooth. The double curvature of this tooth is quite remarkable; when viewed laterally its curve is that of the segment of a circle; when viewed from below it has a curvature in a plane at right angles to the preceding, which curve as is shown very distinctly in the cast of Mr. Foster's specimen is in the form of an *ſ*, as is the case in the beaver and many other Rodents. If both of these curves were continued they would produce a spiral. It is well known that the incisor teeth of Rodents are constantly wearing away at the apex, and are as constantly replaced at the base, so that if we would suppose the growth continued through life, and that no abrasion took place, there would be eventually produced a spiral of several revolutions in the form of a cork-screw. We have an approximation to this in Rodents which have lost one of their incisors, and in whom the opposite one no longer worn off, has continued to increase in length sometimes forming more than one revolution.*

The remains of the Castoroides though they have seldom been found, have nevertheless as already stated, a wide geographical distribution, having been discovered in New York, Ohio, Tennessee and Louisiana, and in all these localities except New York, have been associated with the remains of the Mastodon; and Mr. Hall refers the deposit in which the New York specimen was found to the same period as the deposits in which the Mastodon has been discovered in the neighboring portions of that state.†

It is also interesting to notice the existence of the remains of Castor and Castoroides in the same localities, one of which continues to the present period, and the other, the gigantic representative of the Rodents, has disappeared with the corresponding one of the Pachyderms.

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* See Catalogue of Cabinet of Bost. Soc. Med. Improvement, specimens 537 and 538.

† Notice of the Geological position, and an anatomical description of the cranium of Castoroides Ohioensis.—By James Hall, Esq., and Dr. J. Wyman. Boston Journal of Nat. Hist., vol. v, p. 385.

ART. IX.—*On the Geological Structure of Keweenaw Point;* by Dr. C. T. JACKSON, U. S. Geologist.

(From the Proceedings of the American Association, 2nd meeting, held at Cambridge, 1849, p. 288.)

THIS remarkable promontory extends from the south side of Lake Superior nearly into the middle of the Lake, from 46° 40′ to 47° 29′ north latitude, and is comprised between 87° 55′ and 89° 30′ west longitude. Its general direction is to the E.N.E., in the line of the trend of the great dikes or masses of Trap rocks which form its central ridges.

The surface of the country is broken and rolling, and some of the hills attain an elevation of nearly 900 feet above the lake level. Although but few species of rocks exist on this promontory, they present phenomena of remarkable scientific and practical interest. On the immediate coast, excepting at a few points, the first rocks that meet the eye of the geologist are a coarse conglomerate, made up of large rounded and smooth pebbles of red porphyry quartz, altered slate and sandstone, masses of epidote rock, syenite and hard greenstone trap, mostly of the porphyritic variety, and regular strata of fine grained red and grey or mottled sandstone, devoid of any fossil contents.

The direction of the strata of sandstone and conglomerate is parallel to the line of uplift of the trap rocks, or E.N.E., W.S.W. Its dip is toward the W.N.W., at various angles, being greatest near the trap rocks, which come between their strata and divide the great masses of sandstone throughout the whole length of the promontory. The strata remote from the trap on Keweenaw Bay, and the opposite side of the point, at the portage, are horizontal or but slightly waving, while near the trap rocks the dip of the strata is generally as high as 30°, and sometimes more. The conglomerate rock is limited to the borders of the trap range, and is of the same geological age as the finer grained sandstones, and alternates with them.

At the line of junction of the trap rocks and sandstones, the sandstone and trap are interfused, producing that singular and very important metamorphic rock amygdaloid, a rock closely resembling the vesicular lava of volcanoes, but having its cavities filled with a great variety of curious and interesting minerals.

In the memoir published by Mr. Alger and myself on the mineralogy and geology of Nova Scotia, in the American Journal of Science, vols. xiv, xv, 1828, will be found an account of the origin of amygdaloidal rocks, like those of Keweenaw Point, and it may not be uninteresting to compare the trappean ranges on Lake Superior with those of Nova Scotia. On inspection of the map it will be seen that the great trappean band on Keweenaw

Point is parallel with that on the borders of the Bay of Fundy in Nova Scotia, and it will be further noticed on examination of the geology of these distant regions, the conditions of the rocks are similar, if not identical. The trap of Nova Scotia, like that on Lake Superior, protrudes from below the red sandstone, supposed to be the new red, and passes between the strata in the line of least resistance. Amygdaloid, with species of minerals similar to those of Lake Superior, excepting Prehnite, which is rarely found in Nova Scotia, exist also at the line of junction of the trap rocks and sandstones of Nova Scotia. Native copper occurs in the amygdaloid of both places, but is more commonly found in the trap tuff of Nova Scotia, while it occurs more abundantly in the amygdaloid of Lake Superior. Heulandite is rare in the Lake Superior trap rocks, while it is extremely abundant in Nova Scotia, but the other minerals are of the same species in both places.

It will be observed on examination of the geological maps, that the same gentle crescentic curving of the trap bands towards the northwest was noticed in both countries, a fact also recorded by Dr. Percival and Prof. Rogers, in their reports on the geology of Connecticut and New Jersey.

General geological laws seem to have prevailed in all the regions where trap rocks have burst through sandstone, the effects of heat being recognizable, and proportional to the relative masses of intruded rocks.

It cannot fail to strike every geologist familiar with rocks of igneous origin, and their effects on sedimentary strata, that the history of the origin of trap rocks is indelibly recorded, and that they are really lavas that have risen from the interior of the globe through fractures in its crust, taking the line of least resistance by passing between the strata.

By the influence of heat the sedimentary strata were interfused with the igneous rocks, and it is a singular fact that amygdaloid is most abundantly produced by the action of trap rocks on sandstone, and that copper is the most usual metal found in the fissures, amygdules, and pockets of the resulting amygdaloid.

True workable veins of native copper in this class of rocks had not been described, so far as I know, anterior to my researches on Lake Superior, and it was regarded as contrary to all experience that this metal should thus occur in quantities sufficient for profitable mining. The only locality where native copper has been mined to any extent, is in Siberia, but the metal is not in trap.

Having satisfied myself of the fact that adequate quantities of the metal did exist in veins in the amygdaloid trap of Lake Superior, I ventured to recommend the opening of mines on Keweenaw Point, on and near Eagle River, and the result has proved that native copper veins can be profitably wrought. I mention this fact now in order to recall to your minds the objections that were made to my views on this subject at our meeting in New Haven, in 1845. The predictions I then made are now fully verified.

Nature of the Veins in the Trap rocks.—There are two classes of veins known to miners on Lake Superior, viz.: 1st. Those running with the "country," or parallel to the course of stratified rocks through which the trap rocks pass—veins that are sometimes called beds, or interstratified masses. And 2nd. Those which cross the "country," or cut transversely at various angles the line of direction of the strata.

These last are called true veins, and are the only ones on which miners have thus far placed reliance as to their continuing rich to any considerable depth. I do not regard the question as fully settled by experience in this district, that mining should be confined to the transverse veins, for there is reason to believe that both classes of veins are of the same origin, and no facts have yet been adduced to prove that veins running with the "country" cannot be advantageously wrought. On the contrary, it is known that large quantities of native copper are raised from this class of veins on the Ontonagon River, and it is probable that some on Isle Royale will ultimately prove valuable. A few good practical experiments in mining will settle this mooted point in practical geology. It is obvious, since the trap rocks are not really stratified, that this class of veins cannot be correctly denominated interstratified, though they may be imbedded.

The first class of veins run, as will be understood by what I have previously said, nearly E.N.E., W.S.W., varying with the flexures of the line of junction of the trap and sandstone, and are included between the two rocks in amygdaloid or in epidote, this mineral being the most usual gangue or matrix of the copper. Regular walls of solid copper of some inches in thickness, have been observed in one of the new mines opened in the Ontonagon River, and sheets of considerable size have been found in the east and west veins on Isle Royale.

Mining operations are now in progress to test the permanency of these veins; we shall know in a year or two the results.

The second class or tranverse veins run generally in a course N. 26° to 30° W., S. 26° to 30° E., and consequently cut across the line of direction of the trap rocks and adjacent strata. They are especially rich in the amygdaloidal trap, and thus far have not been profitably worked beyond its limits. In the hard trap rock they are pinched or become narrow, thin plates of metallic copper filling these seams in the trap. The "veinstone" contains the following species of minerals: prehnite, calc spar, laumonite, leonhardite, quartz, datholite, chabasite, mesotype, apophyllite, feldspar, analcime, and wollastonite.

The most common veinstone is prehnite, which occurs in regular symmetrical veins, the prehnite encrusting the sides of the fissures and closing in the middle of it by crystallized botryoidal surfaces. At the surface these veins are rarely more than six inches in width and containing only minute scales of metallic copper, the presence of which in decomposed veins is most readily detected by spots of green carbonate of copper, derived from exposure of the metal to the air and water. These narrow and poor veins of prehnite enrich in copper as they descend into the rock, until at last the prehnite gives way to copper and its space is entirely occupied by it, a thick vein of solid copper filling the fissure, while the prehnite was either absorbed by the rock, or the condition of the rock was such that it could not be formed.

At the Cliff Mine of the Boston and Pittsburg Mining Company, the vein at the top of the cliff consisted of prehnite, containing only minute scales of copper, and was only six inches wide, but it was found on descending that this vein widened, about two hundred feet lower down, to eighteen inches, and lower still it had widened to two feet, and was charged with from five to thirty per cent. of metallic copper, and some particles of silver. The average yield of a large sample of the vein at the surface was found to be $5\frac{6}{10}$th per cent. of copper, and it was estimated that the ore could be practically "bucked" or picked up to 15 per cent.

The width of the vein was estimated to be three feet at the base of the hill, where it was still concealed from view by the soil. On driving a level into the cliff and one at the base of the hill, the vein was proved to be much richer than at the surface, and on sinking a shaft to the depth of 226 feet below the base of the hill, it exposed sheets of copper varying in thickness from a few inches to three feet. These masses of copper filled the vein, and the prehnite and other zeolitic minerals disappeared. By carrying forward levels at the proper points, sixty feet below each other, and by stoping out the backs of the levels, large flattened ellipsoidal masses of copper were exposed, and removed by heavy blasts of gunpowder. These masses were then cut up by mortising out channels through them by means of steel chisels, driven by a heavy sledge hammer.

Some idea may be formed of the rapid increase in richness of this lode by comparing the poor prehnite vein at the top of the hill with the ponderous masses of pure copper that are now cut up in the mine below. One mass of pure copper extracted while I was on the survey, weighed eighty tons, and other masses probably of equal magnitude were in process of being uncovered.

Taking into account the height of the cliff in which the vein is seen, and the depth of the shafts at its base, we have the vein proved 526 feet deep, and thus far it has been steadily enriching, and has surpassed the most sanguine expectations of all the miners and geologists who had examined it.

Already this mine sends to market nearly a thousand tons of copper ore per annum, the ore being estimated to contain sixty per cent. of pure copper after it is cleansed of the adhering rock. This mine, it is understood, has paid for itself and made a dividend of ten dollars per share to its owners.

It is highly probable that other mines on Keweenaw Point, if wrought with the same energy and skill, would prove equally valuable, but thus far no mining equivalent to that of the Boston and Pittsburg Mining Company has been attempted, and it is difficult to find a miner so competent to the task as Capt. Jennings, the Cornish miner, who has had charge of this remarkable mine. I exhibit to you a profile and plan of the mine, in which all the excavations are fully delineated.

Among other promising mines are the North American, the Copper Falls, the Northwest, and the Phenix, all of which have been sufficiently proved to warrant the belief that they can be advantageously wrought, but still it must be remembered that even in the best known mineral districts, mines frequently fail to prove profitable from causes that are not at once foreseen. The North American Company's mines are situated very near the Cliff mine, on the west branch of Eagle River, and are now wrought with energy, and give promise of success nearly equal to that of the Cliff mine before described. The veins are similar in their nature and in their contents, so that I need not describe them.

The Copper Falls mines have been opened to a considerable extent, and from one of the veins a single mass of copper was taken that weighed eight tons. It was sawed into pieces and sent to market. I exhibit to the section a specimen sawed from this mass. It is perfectly pure copper, and as dense as the purest hammered copper of commerce, showing its perfect fineness. There is a considerable proportion of native silver mixed with the copper of this mine, and in the green veinstone, a specimen of which I lay before you. Silver is found also in most of the copper mines of the lake, and frequently in sufficient quantities to be of commercial value. It is most curiously united with the copper, and in some of the pieces I lay before you, the metallic copper is actually porphyritic with masses of silver, and yet the silver is absolutely pure, and the copper is also pure, there being no alloying or chemical union, but a mere metallic cementation at the line of contact. This phenomenon is seen in all the localities on the lake where native copper and silver occur together, and this state of the metals must have arisen from a common cause acting in every one of the veins. It is not capable of being explained in the present state of chemical and geological knowledge, and is a subject for experimental research. The solution of this question will lead to an explanation of the origin of the native copper and silver veins, the rationale of which we have not yet reached.

All the experiments I have devised and executed, to discover the cause of the separation of the copper and silver, as seen in these specimens, have given negative results. The experiments were made on fused alloys of silver and copper. The metals did not separate by galvanic agency. Little has yet been done towards extensive working of the Northwest Company's copper mine. It is situated a few miles from Eagle Harbor, and several rich veins of native copper, with some native silver, have been discovered and wrought to a small depth. The want of confidence in this new kind of mining prevents the investment of sufficient capital and the employment of a sufficient mining force to work the mines. I am confident, that, with capital and skill, this company's mine might soon be rendered profitable.

The old Lake Superior Company, the first organized for mining on Lake Superior, was unsuccessful in its first operations from several causes, among which the want of miners capable of carrying on the work in a proper manner was the chief; and it should be remembered that mining for native copper was, at that time, a new business to both English and American miners. Numerous changes were made and many new shafts were sunk, but no regular preconceived plan was ever carried out, and hence the mines are in a neglected state. Much allowance should be made for the newness of the enterprise, the wilderness state of the country, and want of skill in the miners hastily collected from districts wholly unlike the one they were sent to explore. There was also an erroneous opinion prevalent among many of the original stockholders that mining could be made profitable from the outset,—a most fallacious idea. The company soon closed up its mining operations, and I have heard that a new organization has been since adopted, and it is hoped, if mining operations are again begun, a regular system will be pursued, modelled after the plan of the Boston and Pittsburg Company's workings. The Lake Superior or Phenix Company's veins are rich in native copper and silver, and although the leader or prehnite vein is but a few inches wide, it will doubtless lead to a solid copper vein, like those heretofore described. Masses of pure copper, of large size, weighing some thousands of pounds, were obtained from an ancient ravine or excavation that had been worn out by the river running over the vein, and large pieces of silver were also found. These show the contents of the lode in the true vein. Most of the work heretofore executed at this mine has been done in the western wall of the vein and not in the vein itself. To the company owning the Lake Superior mine is due the credit of the earliest mining enterprise on the Lake, and those who have followed after them should remember that they opened the way and introduced the business of mining into the then unbroken wilderness of Lake Superior.

It is still a question among geologists and miners whether veins were filled by igneous injection or sublimation, or by aqueous and galvanic deposition. This question is one of very great scientific and practical interest, and is exceedingly difficult to answer so far as relates to the native copper and native silver of Lake Superior.

The objections to the igneous origin of native copper are, 1st, that the metal bears the imprint of crystals of prehnite, as seen in the specimen I lay before the section, and we cannot account for the fact that this zeolite was not rendered anhydrous by the molten copper. 2dly, that if the copper was melted, since its fusing point is much higher than that of silver, that the silver is not alloyed with the copper, but is separate from even a trace of it in chemical combination, though small particles and large lumps of silver are mixed and united with the metallic copper.

These objections are equally strong against the theory of sublimation of the copper, and since silver is not volatile at the highest temperature of our furnaces, we could not account for the presence of that metal by a simultaneous sublimation of the metals.

Against the theory of its aqueous deposition, or its origin from any solution of copper, it may be urged that if the metal was in chemical solution, no material capable of causing its decomposition with the deposition of the copper in a metallic state exists in the vein, and no salt, if any supposed acid solvent, which would result from the decomposition of its combination, exists in the vein. Again, it would be impossible for the chasm to contain a sufficiency of any copper solution, however concentrated, to produce the solid metallic copper filling the fissure, for, as before observed, the masses of copper are from a foot to three feet in thickness, and occupy the whole space of the sundered rock.

Galvanic segregation it has been supposed would explain the origin of these copper veins. But we may ask, from what was the copper segregated? It is impossible for galvanism to create the metal from the ingredients of trap rocks, or sandstone; and we can hardly imagine any arrangement of the rocks that would produce a galvanic battery with its poles so arranged as to effect the deposition of a vein of solid copper two or three feet in thickness.

It is well known that the trap rocks are magnetic, and that they possess polarity at the surfaces of disjunction. This has been fully substantiated by the researches of Dr. Locke and others on the Lake Superior mineral lands, but this magnetism is obviously the effect of the earth's inductive magnetism exerted on the very large proportion of magnetic iron ore entering into the composition of the trap rocks, a quantity so large that I have seen pig iron made directly from those rocks by fusion in a blast furnace, about twelve per cent. of iron being reduced from it; and we know, from the experiments of Dr. Locke, that even fragments of the trap rock are both magnetic and polar. It remains to be proved that there are any *electric* currents in the native copper veins, for such currents are by no means proved by deviations of the magnetic needle, which are doubtless produced by the magnetic polarity of the trap rock itself.

The occurrence of bright scales and perfect crystals of native copper in perfect crystals of prehnite, datholite, calc spar, and quartz, would seem to indicate a simultaneous deposition of the copper and those crystallized minerals including it, or that they were impregnated with native copper by sublimation immediately before the injection of the principal copper vein took place. If we could admit the igneous formation of zeolites, and of calc spar, there would be less difficulty in accounting for the veins by sublimation or injection, or by both methods, but this chemists will not readily admit, for the zeolitic minerals are generally hydrous.

It is also a question whether the native copper in the amygdaloid was derived from the interfused sandstone, or was mechanically brought up with the trap rock. It has been imagined, that since the sandstone is made up of the detritus of more ancient rocks which might have contained copper ores, that the copper ore being deposited with the sand was reduced by the action of the trap. This idea would be plausible, if it could be shown that the sandstone in the vicinity of the trap contained copper in a sufficiency to account for that in the amygdaloid; but such is not the case. It has been said that local deposits of the ore might have taken place in portions of the sandstone strata, and that the trap rocks came up and reduced it. This would be imputing a most remarkable degree of intelligence to the trap rocks, that they should know exactly where the copper ore was deposited, and come up at those places expressly to smelt it! I must confess that I cannot attribute the origin of the copper to any other causes than those which produced the trap rocks themselves, and that the copper came from the molten interior of the earth seems, at least from what we know of igneous agencies, to be most probable. There are veins in the conglomerate rocks which are filled with calcareous spar, containing crystals of copper, some of which will weigh half a pound, and are generally in the rhombic dodecahedral form. One of the calc spar veins at Agate Harbor has yielded masses of copper weighing several hundred pounds.

At Copper Harbor, a large vein of solid black oxyd of copper was found in the conglomerate rock. This ore is not known to exist in any considerable quantity elsewhere. The ore in the vein was fourteen inches wide, and for a short time the mine furnished a good supply of copper ore, yielding about sixty or seventy per cent. of metallic copper. It was soon exhausted, a bed of fine grained sandstone cutting off the copper vein, the calc spar only continuing in the sandstone below. Among the masses of black oxyd of copper brought from the mine at Copper Harbor, Mr. J. E. Teschemacher discovered regular cubic crystals of the ore, crystals which show that the ore is not a mere mechanical mixture of copper smut with earthy matters for a cement, as some have supposed. A pure specimen analyzed in my laboratory yielded 79·86 per cent. of copper.

There are also found at the Copper Harbor mine, chrysocolla, or hydrous green silicate of copper, and the black silicate, which contains a less proportion of water. These ores, we can easily conceive, might be produced by the decomposition of a solution of copper by the action of a hot solution of lime. The black oxyd may have been derived either from a solution, or from igneous sublimation. We know that black oxyd of copper is sublimed from the crater of Vesuvius, and is deposited in fine splendent scales like specular iron ore in the lavas.

Chlorid of copper is very volatile, and is sublimed in the crater of Vesuvius. It is also known to be volatilized in the blast furnace. The experiments of Mr. Frederick W. Davis, at the Point Shirley copper works, have fully demonstrated the fact that a considerable proportion of copper is lost by sublimation from copper ore, containing the chlorid of that metal.

These facts may at some future day serve to explain some of the phenomena relating to the formation of metallic veins. At present there is no part of geological science so little understood as the theory of veins, and on this account I am desirous of calling the attention of the Section to this subject.

With respect to the age of the red sandstone of Lake Superior, I would remark that there have been and still are differences of opinion. No distinct fossils having been found in it, the usual index for fixing the geological age of stratified rocks is wanting.

From the mineralogical character and the geological associations of the rocks, their parallelism to those of Nova Scotia, and their mineral contents, I was led in 1844, to suggest the identity of the two formations, as contemporaneous, and regarded the Lake Superior sandstone as the new red, or at least as of the same age with that of Nova Scotia, New Jersey and Connecticut. This idea I still favor.

During the last year the linear surveyors, who were engaged in subdividing townships on the southern portion of Keweenaw Point, discovered a large protruding mass of Silurian limestone, around which the sandstone strata are horizontal. My assistant was sent to examine this limestone, and states that its strata lines dip about 30°. A fragment of a fossil, probably a Pentamerus, was also found in the limestone. These facts would seem to prove that the sandstone is above the Silurian limestone, and consequently that it is either the old or the new red. The absence of fossil shells in the sandstone would lead us to conclude that it does not belong to the old red, and consequently we are led back to my original opinion, as published in the American Journal of Science, in 1845, that the Lake Superior sandstone is of a later date, and is probably the new red.

This opinion was also expressed by Monsieur De Verneuil, during his visit to the Lake in 1846, but I do not know from what data his opinion was formed. It has been asserted that the Lake Superior sandstones pass beneath the Silurian rocks, but I do not think the fact has ever been observed.

Isle Royale.—This island is situated on the north side of Lake Superior, in latitude 48° North, longitude 89° West. It is about forty miles in length, and five or six miles wide. It presents a broken and rugged outline on its coast and is deeply indented by long and narrow inlets and bays, all of which are parallel to the ranges of the trap rocks which constitute the ridges traversing the island throughout its length. Several small lakes are also seen lying between the trappean hills and coinciding with their line of bearing.

The general direction of this island is parallel to that of Keweenaw Point, and the trap rocks are of the same geological age and have uplifted the sandstones of the same epoch.

On the southwestern end of the island the fine red sandstone strata are seen near Card's Point, and they extend along the coast of Siskawit Bay to Epidote Cove, forming gently sloping sheets extending out into the lake to a considerable distance. Conglomerate rocks border the coast nearly to Rock Harbor and lie next to the trap. The inland boundary of the sandstone was ascertained by my sub-agents to be parallel to the coast line where it is exposed. About one-fourth the area of the island is sandstone and conglomerate rock. All the rest of it consists of trap, which forms ridges attaining an elevation of from three hundred to five hundred feet above the lake, and extending in a broken line throughout the whole extent of the island. In some places isolated masses of trap rocks form tall towers standing like high chimneys on the hill sides; in others, picturesque islands covered with dark spruce trees are seen jutting out on the coast, or standing like watch towers at the entrance of the harbors.

Isle Royale was better known to the Indians as a good place for catching Siskawit than as a mining region; and it is probable that the name *Menung*, signifying a good place, refers to the fisheries, but it is certain from the "Relacions" of the Jesuit fathers, that they were aware of the existence of an abundance of copper boulders upon its shores.

Numerous explorers had visited Isle Royale anterior to my survey, but mining operations had not been entered upon to any extent on account of the difficulties arising from some official misunderstanding as to permits for leases. Dr. Locke had selected some veins and beds of copper for the Ohio and Isle Royale Company, and explorations were going on to determine the probable value of several veins.

There are two kinds of veins on this island, as before mentioned. The widest are those near Rock Harbor. They are thick beds of solid epidote rock filled with small spiculæ of copper, there being from eight to ten per cent. of the metal in the gangue. These beds dip but slightly from the horizon, rarely more than 15 or 20°, and crop out on the south side of the island a few feet above the surface of the Lake. Beneath the copper-bearing bed of epidote, which is a foot in thickness, is a large bed of barren epidote rock, six feet thick, and very hard. Trap rocks overlie the whole, forming bold precipitous shores. No mining operations have yet proved the extent of these cupriferous epidote rocks, but they are exposed to a sufficient extent to render it probable that they will prove of value.

Another set of true veins occur, cutting the trap rocks nearly at right angles, and traversing the country. These veins are generally narrow, and are filled with datholite, prehnite, and native copper. The datholite is very abundant, and may prove of economical importance either as a flux for copper ores, or as a material suitable for the manufacture of borax. One of the locations of the Ohio and Isle Royale Company was named, by Mr. J. H. Blake, Datholite, on account of the abundance of that mineral in the veins of copper.

At Todd's Harbor, mines have been opened, and a considerable quantity of native copper has been obtained by Mr. McCulloch. Other veins have been opened at Scovill's Point, but as yet none of the veins on the island have been sufficiently proved to authorize the erection of permanent works for mining and smelting. One vein of each kind, opened to a considerable depth, would give much valuable information concerning the permanency of the veins, and determine whether they widen and enrich or not.

It is extremely difficult for any one to decide absolutely on the value of a metalliferous vein, and it is only possible to form an approximate estimate where all the conditions of the problem are capable of being determined, and it is rarely the case that we have any thing more than a superficial view of the contents of a vein. It has been proved by a writer in the French Annales des Mines, that in Germany and France only one-twentieth of the mines surveyed and recommended by the Royal Engineers of mines have paid a profit to the stockholders, hence we should remind persons about to engage in mining adventures that their chances of success are only about five per cent.

When a mine is well proved it generally holds good veins, rarely running out in depth unless the rock changes, and then the vein also generally alters.

How far the native copper of the Lake Superior mines continues in depth is yet unknown, but the veins if they traverse sandstone strata will certainly change in that rock, and experience has thus far shown that the copper diminishes in that portion of the vein which traverses the sandstone. This has been fully proved at the Copper Falls mines, where a bed of sandstone, seventy-two feet in thickness, has changed the character of the lode where the vein passed through it, calc spar filling the chasm and the copper nearly disappearing in the veinstone.

It was hoped that the vein would enrich after it had passed through the sandstone into the nether bed of trap rock, but it was found to be diffused into string veins of little practical importance. Owing to the limited extent of the amygdaloidal trap, the true or transverse veins are not of great length, two thousand feet being perhaps an approximation to their linear extent, though it is possible that some may be longer. The idea of tracing a vein by its course over an extensive tract of country has proved fallacious on Lake Superior, and only the geological character of the country can be relied upon as a tolerably correct guide. The river beds, depressions in the soil, corresponding to the usual direction of the veins, afford the best facilities for finding veins, and by means of the solar compass and magnetic needle, lines of contact of the sandstone and trap may be readily found, and the amygdaloid is formed at those junctions. It was observed in my first visit to the Lake, that the productive copper veins occur where there are the greatest number of alternations of sandstone and trap rocks, as shown by the diagram exhibited to the Section. No less than six alternations of these rocks were observed near Copper Falls and Lake Superior mine in my surveys during the summers of 1844 and 1845. Subsequent researches have confirmed this observation.

From these data we should expect copper veins at the line of contact of the sandstone and trap on Isle Royale, but thus far in only a few places have the rocks been uncovered in the vicinity of the junction—some loose masses of native copper found on the shore of Siskawit Lake and the veins at Datholite Cove being the only facts obtained in confirmation of this opinion.

In the hard columnar and compact trap there is little hope of finding valuable veins, for only narrow and tightly pinched seams of copper have thus far been found in these rocks.

On the Ontonagon river there are several veins of copper that run with the "country." They are now in course of trial, and it will soon be ascertained whether they can be profitably worked or not. The opinion of a practical miner, on whom I place reliance, is favorable to some of these mines. It was my intention to have examined them myself, this summer, before drawing up my report, but it has been ordered otherwise.

Rich ores of iron have been found in inexhaustible quantities in the district of country extending from the Menomonee River to Dead River. I have not had an opportunity of examining the localities myself, but I had obtained rich specimens of the ore from the Menomonee river, in 1844, through the agency of M. Barbeau, who obtained them from the Indians, and in 1845, the Indian chief who furnished those specimens, guided Mr. Pray to the Iron Mountain near the Menomonee River. During the past summer this locality has been also examined by one of my assistants.

ART. X.—*Analysis of Algerite;* by RICHARD CROSSLEY.

Read before the Bost. Soc. Natural History, by C. T. JACKSON, April 17, 1850.

THIS mineral having been already described in this Journal for July, 1849, vol. viii, No. 22, renders it unnecessary to say much more on this head. I have in addition, however, to remark that many of the crystals are encrusted with idocrase, and in some instances are penetrated, so much so that it required great caution to secure such portions as were free from that mineral. The crystals being cleared of decomposed parts were broken to coarse fragments, and the honey-yellow pieces reserved for examination.

Before the blowpipe, alone, it readily fuses with intumescence to a white blebby glass: with soda it gives a dirty-white slag: with borax and phosphorus salt it gives a clear bead faintly tinged by iron and leaves a siliceous skeleton. Heated in a closed tube it gives off water which reacts feebly alkaline, and the powder, at first of a light buff color, darkens and assumes a brownish tinge. About 1 gramme of the coarse fragments gave a spec. grav. of 2·78. It is a little harder than calc spar, from 3 to 3·5.

Nearly 2 grammes of the mineral were very finely powdered, intimately mixed and divided into two portions. One was appropriated to the determination of the water and the other for the estimation of the carbonic acid. This latter portion was then *attacked* by a mixture of sulphuric and hydrochloric acids. The usual mode of analysis was afterwards pursued, and there resulted :—

Silica,	51·27
Alumina,	23·10
Peroxyd of iron,	1·48
Magnesia,	5·18
Carbonate of lime,	4·21
Potash,	9·97

To ascertain the purity of the silica it was fused with carbonate of soda and was found to contain 1·31 per cent of alumina. A second *attack* by acids would doubtless effectually decompose this mineral. Correcting the silica and alumina and adding the water determined on the first portion, the composition of the selection made will be thus:—

Silica, ...	49·96
Alumina, ...	24·41
Peroxyd of iron, . . .	1·48
Magnesia, ...	5·18
Carbonate of lime, . . .	4·21
Potash, ...	9·97
Water, ...	5·06
	100·27

The amount of carbonic acid directly estimated agreeing so nearly with that in the carbonate of lime obtained, evidently shews that the lime is not a constituent of the mineral. Deducting, therefore, the carbonate of lime and reducing the remaining members to per-centage proportions, they will stand thus:—

		Oxygen.	Ratio.
Silica, . .	52·00	27·01	7
Alumina, . .	25·42	11·88	3
Peroxyd of iron,	1·54	·47	
Magnesia, . .	5·39	2·08	1
Potash, . .	10·38	1·75	
Water, . .	5·27	4·68	1 or 1⅓
	100·00		

The above composition is very well represented by the formula,

$$(\dot{Mg}, \dot{K})^3 \ddot{Si} + 3\ddot{\overline{Al}} \ddot{Si}^2 + 3\dot{H}.$$

Boston, April 18, 1850.

ART. XI.—*On the Telluric Bismuth of Virginia;* by Dr. C. T. JACKSON.

I DISCOVERED this ore in May, 1848, among some specimens of native gold given me by Mr. Knowles Taylor. At that time I had not a sufficient quantity of the mineral to enable me to make a complete analysis of it, but having made a blowpipe assay and satisfied myself that it was an ore of Tellurium, I communicated my results to the American Journal of Science and Arts, and my note was published in vol. vi, No. 17, 1848, p. 188.

My only object then was to announce the discovery of Tellurium, and I intended at the earliest moment in my power to procure a larger supply of the ore and to complete my analysis. Subsequently, Mr. Fisher* of Philadelphia made an analysis of

* This Journal, [2] vii, 282.

a specimen which he obtained from the mint, and discovered that the metal which I had supposed to be lead in my cupellation process was bismuth. This fact I am enabled to confirm by my own analysis. Mr. Fisher states that selenium takes the place of sulphur. This is not the case in my specimens, all of which yield sulphur when treated by nitric acid or by the blowpipe, and selenium exists only in minute traces.

Last spring I made a hasty visit to the gold mines of Spottsylvania County, Va., and obtained at Whitehall a few pieces of the tellurium ore. These specimens were in mica slate in nodules, and were incrusted with yellow oxyd of bismuth. The gold found in these specimens is not chemically united with the tellurium ore, but exists in small scales between the folia in its metallic state and varies in proportions from 2 to 5 per cent. The tellurium ore is found incrusting masses of native gold, and the edges of the laminæ of tellurium ore impress the metallic gold with well marked striæ and indentations indicating that the tellurium ore was deposited first in the cavities or veins, and the gold was then deposited upon it. This is the order of deposition in many specimens I have seen.

The tellurium ore is found both in the quartz beds or veins and in the mica slate, and is always mixed with native gold.

Form, thin scales frequently intersecting each other; no regular crystals observed. Occurs in lamellar masses readily cleavable, the laminæ splitting like sulphuret of molybdena, which it much resembles in appearance. It is flexible and not elastic. Sectile and not brittle. Color and lustre like flexible foliated graphite. Its hardness =1. Lustre splendent metallic. Color of streak like that of lead.

Analysis on one gramme of the picked scales :—

Bismuth, ...	58·80
Tellurium, ...	35·05
Gold, ox. iron and earthy matter, . .	2·70
Sulphur, ...	3·65
	100·20

This gives the formula of Tetradymite, $2\text{Bi Te}^3 + \text{Bi S}^3$ = Bismuth 59·6, sulphur 4·5, tellurium 35·9.

The tellurium after it was reduced by sulphurous acid, was attacked by means of carbonate and nitrate of potash by fusion, and the contents of the crucible being dissolved and acidulated by means of nitric acid and treated with nitrate of baryta, did not give a weighable quantity of seleniate of baryta. The selenium therefore exists only in minute proportions and is a mere *trace*.

In addition to the above, I would observe that the yellow oxyd of bismuth occurring investing the nodules of tellurium ore, is not carbonate of bismuth, for it does not effervesce with acids. Dr. Chilton had observed *carbonate of bismuth* with the gold rock of South Carolina, some time before I found this, and sent me a specimen of it. This ore, (bismutite,) from the gold district of Chesterfield, S. C., has been analyzed by Rammelsberg, (Pogg. Ann., lxxvi, 569, 1849,) who obtained $\dddot{\mathrm{Bi}}$ 90·00, $\ddot{\mathrm{C}}$ 6·56, $\dot{\mathrm{H}}$ 3·44, whence he has deduced the formula $3(\dddot{\mathrm{Bi}}\,\ddot{\mathrm{C}}+\dot{\mathrm{H}})+\dddot{\mathrm{Bi}}\,\dot{\mathrm{H}}$, equivalent to $4\dddot{\mathrm{Bi}}$, $3\ddot{\mathrm{C}}$, $4\dot{\mathrm{H}}$.

Art. XII.—*On a supposed New Mineral Species;* by Henry Wurtz, of New York.

This mineral was found near Cambridge, Mass., at "Milk Row Quarry," the well-known locality of prehnite. It occurs as an incrustation upon the surface of the syenitic rock, coating the sides of fissures, and presenting the appearance of brilliant plates overlying one another like the scales of a fish.

Its color is black; lustre, resinous; streak, dark olive green; structure, distinctly fibrous, somewhat like that of tremolite; feel, soapy. Hardness, about 2; slightly translucent in thin laminæ. The fibres are brittle. No cleavage was found. Under the lens it exhibits white crystalline specks, which the analysis proved to be calcareous spar.

It is easily decomposed with effervescence by hydrochloric acid, which is thereby colored deep green, silica being left behind as a white powder. Its fusibility is about that of natrolite, or 2 on Kobell's scale. It fuses to a dull black opaque globule which is slightly magnetic.

Two determinations of the specific gravity, made upon two different specimens, gave the same number, 2·69, which being about the specific gravity of calcareous spar, no correction due to the 12 to 23 per cent. of this mineral which the analyses indicate, need be made.

Qualitative analysis indicated the presence of carbonic acid, water, lime, soda, silica, oxyd of iron and alumina.

For the quantitative analysis, a portion of the mineral was finely elutriated, and then dried at 212° in a steam-bath until it lost no more weight. Attempts to separate the calc spar by means of very dilute acetic acid were unsuccessful. The acid became immediately colored red by dissolving some of the iron.

When the dark-green powder of this mineral is heated to redness in the air, it assumes upon cooling a red color, indicating peroxydation of the iron. The water determinations were therefore made in small bent tubes, which were sealed up immediately after the expulsion of the water, and the mass thus cooled out of contact with the air. The residue, after the expulsion of the HO, had, in this case, a black color.

The carbonic acid was determined by the well known method of the weighed flask and Ca Cl tube.

The other constituents were determined in the usual manner, by dissolving the mineral in HCl, etc. In making the two following analyses, the most extreme precautions were used to ensure accuracy. Every product of the analysis was reanalyzed after being weighed. Thus, the silica, after being weighed, was fused with carbonate of soda and the $\dddot{\overline{Al}}$ and $\dddot{\overline{Fe}}$ which had been left in it by the HCl separated, weighed, and added to the HCl solution, their weight being deducted from that of the silica. In one case, this weight amounted to half a per cent. of the substance used. Difficulty being found in separating the $\dddot{\overline{Al}}$ and $\dddot{\overline{Fe}}$, by means of KO, in one experiment the $\dddot{\overline{Fe}}$ and $\dddot{\overline{Al}}$ were weighed together, and the $\dddot{\overline{Al}}$ calculated by deducting the amount of $\dddot{\overline{Fe}}$ afterwards found in the mass.

	I.	II.	Mean.	Oxygen.
$\dddot{Si}$	27·70	27·32	27·51	14·29
$\dddot{\overline{Al}}$	10·30	10·17	10·23	4·78
$\dot{Fe}$	27·99	27·55	27·77	6·17
$\dot{Na}$	1·23	. .	1·23	·32
$\dot{H}$	8·69	8·73	8·71	7·74
$\dot{Ca}$	12·25	12·98	12·61	
$\ddot{C}$	10·32	10·09	10·29	
			98·35	

The first thing to be observed here is that the quantity of $\ddot{C}$ found is just sufficient to form $\dot{Ca}\ddot{C}$ with the $\dot{Ca}$ found, 12·61 $\dot{Ca}$ requiring 9·91 $\ddot{C}$, so that, in all probability, the lime present is all in the form of calc spar.

So far, I had proceeded upon the hypothesis that this mineral contained no $\dddot{\overline{Fe}}$, judging from its deep color and the color of its solution in HCl; knowing also that the best analysts have generally supposed that all the iron contained in minerals which give deep green powders is in the form of Fe O. This error of which so many examples may be found in mineralogical works, has its origin in the great difficulties always met with in the separation of the two oxyds of iron from one another, and also in the small difference between the equivalents of the two oxyds,—9 per cent. of protoxyd making only 10 per cent. of peroxyd.

The suggestion of a scientific friend that the mineral might still contain both oxyds, led to a renewal of my research.

The stock of elutriated material, originally prepared, being exhausted, it was necessary to repeat the analysis from the beginning on another specimen, which was accordingly done, in the same manner as previously, with the exception that the $\dddot{\overline{Fe}}$ was determined according to the method of Fuchs, that is, by weighing a piece of bright copper before and after its immersion in the HCl solution of a weighed quantity of the mineral, contained in a stoppered bottle, which was kept, during the operation, in boiling water. The ratio of the loss sustained by the copper to the $\ddot{\overline{\mathrm{Fe}}}$ present, is that of their equivalents.

	I.	II.	Oxygen.	
$\dddot{\mathrm{Si}}$	30·86	30·93	16·05	16·05
$\ddot{\overline{\mathrm{Al}}}$	3·92	. .	1·83	7·89
$\ddot{\overline{\mathrm{Fe}}}$	20·25	20·17	6·06	
$\dot{\mathrm{Fe}}$	21·97	. .	4·88	5·30
$\dot{\mathrm{Na}}$	1·62	. .	·42	
$\dot{\mathrm{Ca}}\,\ddot{\mathrm{C}}$	12·77	. .		
$\dot{\mathrm{H}}$	8·94	. .	7·95	7·95
	100·33			

The ratio for the oxygen of the protoxyds, peroxyds, silica and water, 5·30 : 7·89 : 16·05 : 7·95 = 1 : 1·49 : 3·03 : 1·50 or quite closely 1 : 1½ : 3 : 1½. This gives the general formula

$$\dot{\mathrm{R}}^2\,\dddot{\mathrm{Si}}+\ddot{\overline{\mathrm{R}}}\,\dddot{\mathrm{Si}}+3\dot{\mathrm{H}}$$

or the special formula,

$$(1.)\qquad (\dot{\mathrm{Fe}}\tfrac{581}{631}+\dot{\mathrm{Na}}\tfrac{50}{631})^2\,\dddot{\mathrm{Si}}+(\ddot{\overline{\mathrm{Al}}}\tfrac{10}{43}+\ddot{\overline{\mathrm{Fe}}}\tfrac{33}{43})\dddot{\mathrm{Si}}+3\dot{\mathrm{H}}.$$

An attempt to bring the first two analyses under this formula gave the following result. If we suppose that a portion of the iron, whose oxygen, when in the form of $\ddot{\overline{\mathrm{Fe}}}$, is equal to one-half of the oxygen of the $\ddot{\overline{\mathrm{Al}}}$ present, was actually present in the form of $\ddot{\overline{\mathrm{Fe}}}$, the following numbers present themselves:—

	Mean.	Oxygen.	
$\dddot{\mathrm{Si}}$	27·51	14·29	14·29
$\ddot{\overline{\mathrm{Al}}}$	10·23	4·78	7·17
$\ddot{\overline{\mathrm{Fe}}}$	7·97	2·39	
$\dot{\mathrm{Fe}}$	20·60	4·58	4·90
$\dot{\mathrm{Na}}$	1·23	·32	
$\dot{\mathrm{H}}$	8·71	7·74	7·74
$\dot{\mathrm{Ca}}\,\ddot{\mathrm{C}}$	22·90		
	99·15		

The ratio 4·90 : 7·17 : 14·29 : 7·74 = 1 : 1·46 : 2·92 : 1·58 or nearly 1 : 1½ : 3 : 1½, as before.

Thus, upon this hypothesis, the general formula $\dot{\mathrm{R}}^2\,\dddot{\mathrm{Si}}+\ddot{\overline{\mathrm{R}}}\,\dddot{\mathrm{Si}}+3\dot{\mathrm{H}}$ represents these analyses also very well. The special formula for this variety would be

$$(2.)\qquad (\dot{\mathrm{Fe}}\tfrac{14}{15}+\dot{\mathrm{Na}}\tfrac{1}{15})^2\,\dddot{\mathrm{Si}}+(\ddot{\overline{\mathrm{Al}}}\tfrac{2}{3}+\ddot{\overline{\mathrm{Fe}}}\tfrac{1}{3})\dddot{\mathrm{Si}}+3\dot{\mathrm{H}}\ (\text{or } 3\tfrac{1}{4}\dot{\mathrm{H}}).$$

The composition is near that of groppite, for which Rammelsberg writes the formula $\dot{\mathrm{R}}^2\,\dddot{\mathrm{Si}}+\ddot{\overline{\mathrm{R}}}\,\dddot{\mathrm{Si}}+2\dot{\mathrm{H}}$. But groppite is a rose-red species, containing little iron and lime and much magnesia with potash, and moreover Svanberg's formula for it is very different. There is sufficient reason therefore, for believing the species here described as distinct, and I therefore propose for it the name *Melanolite.*

As the recent investigations of Mr. J. D. Dana, published in the American Journal of Science for March, 1850, have given increased interest to the subject of atomic volume among minerals, I have here calculated the atomic volume of these varieties.

(1.)			(2.)		
$\dddot{\mathrm{Si}}^{2}$	=	1154·62	$\dddot{\mathrm{Si}}^{2}$	=	1154·62
$\ddot{\mathrm{Al}}\frac{10}{43}$	=	149·26	$\ddot{\mathrm{Al}}\frac{2}{3}$	=	427·87
$\ddot{\mathrm{Fe}}\frac{33}{43}$	=	767·45	$\ddot{\mathrm{Fe}}\frac{1}{3}$	=	333·33
$\dot{\mathrm{Fe}}1\frac{531}{631}$	=	828·68	$\dot{\mathrm{Fe}}1\frac{65}{75}$	=	840·00
$\dot{\mathrm{Na}}\frac{100}{631}$	=	61·36	$\dot{\mathrm{Na}}\frac{10}{75}$	=	51·63
$\dot{\mathrm{H}}^{3}$	=	337·50	$\dot{\mathrm{H}}^{3\frac{1}{4}}$	=	365·62
		3298·87			3173·07

These two numbers, which are the atomic weights of the two varieties, divided by the specific gravity 2·69, give for (1), the atomic volume 1226·35, and for (2), 1180·, the difference between which is about $\frac{1}{36}$th. If we go farther, and divide these numbers by the number of atoms of the elements in each variety, we shall obtain the C atomic volume, as this term is used by Mr. Dana. In one case we get 53·32, and in the other 50·21. If reference is now made to the tables of atomic volumes given by Mr. Dana in the American Journal for March, 1850, p. 242, it will be seen that these numbers are nearly identical with those given by him for three varieties of talc, a mineral which the one now under consideration resembles in many important characters. On the contrary, chlorite, a mineral to which this one is so intimately connected as to have been mistaken for it by an eminent mineralogist, gives altogether a different number. I have calculated the C atomic volume of chlorite from one of Kobell's analyses which gives the formula

$$\dddot{\mathrm{Si}}^{2}+\ddot{\mathrm{Al}}+\dot{\mathrm{Mg}}^{4\frac{2}{3}}+\dot{\mathrm{Fe}}^{\frac{1}{3}}+\dot{\mathrm{H}}^{4}$$

and obtained 43·6. The hisingerite from Ridderhyttan also resembles this mineral very closely. Its formula, according to Hisinger and Kobell, is $\dot{\mathrm{Fe}}\,\dddot{\mathrm{Si}}+\ddot{\mathrm{Fe}}\,\dddot{\mathrm{Si}}+6\dot{\mathrm{H}}$; and specific gravity 3·045. Its ordinary atomic volume is 1077·05, and its C atomic volume 39·9, thus approaching that of chlorite, instead of that of this mineral.

Laboratory of the Coll. of Phys., Crosby st., New York, April 1, 1850.

Art. XIII.—*The Erratic Phenomena about Lake Superior;* by Prof. Agassiz.

[We cannot better notice the very valuable and interesting work of Prof. Agassiz and his associates upon the Lake Superior Region,* than by citing one of its chapters. The following observations on the Erratic Phenomena of the region constitute Chapter X, of the work, pages 395–416.]

* The title of the volume and its contents are mentioned in our last number, p. 455.

So much has been said and written within the last fifteen years, upon the dispersion of erratic boulders and drift, both in Europe and America, that I should not venture to introduce this subject again, if I were not conscious of having essential additions to present to those interested in the investigation of these subjects.

It will be remarked by all who have followed the discussions respecting the transportation of loose materials over great distances from the spot where they occurred primitively, that the most minute and the most careful investigations have been made by those geologists who have attempted to establish a new theory of their transportation by the agency of ice.

The part of those who claim currents as the cause of this transportation has been more generally negative, inasmuch as, satisfied with their views, they have generally been contented simply to deny the new theory and its consequences, rather than investigate anew the field upon which they had founded their opinions. Without being taxed with partiality, I may, at the outset, insist upon this difference in the part taken by the two contending parties. For since the publication of Sefstroem's paper upon the drift of Sweden, in which very valuable information is given respecting the phenomena observed in that peninsula, and the additional data furnished by de Verneuil and Murchison upon the same country and the plains of Russia, the classical ground for erratic phenomena has been left almost untouched by all except the advocates of the glacial theory. I need only refer to the investigations of M. de Charpentier, Escher, Von Derlinth and Studer, and more particularly to those extensive and most minute researches of Prof. Guyot in Switzerland, without speaking of my own and some contributions from visitors, as the Martins, James Forbes and others, to justify my assertion that no important fact respecting the loose materials spread all over Switzerland has been added by the advocates of currents since the days of Saussure, de Lüc, Escher and Von Buch; whilst Prof. Guyot has most conclusively shown that the different erratic basins in Switzerland are not only distinct from each other, as was already known before, but that in each the loose materials are arranged in well-determined regular order, showing precise relations to the centres of distribution, from which these materials originated; an arrangement which agrees in every particular with the arrangement of loose fragments upon the surface of any glacier, but which no cause acting convulsively could have produced.*

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* A comparison of the maps showing the arrangement of the moraines upon the glacier of the Aar in my *Système Glaciaire*, with the map which Prof. Guyot is about to publish of the distribution of the erratic boulders in Switzerland, will show more fully the identity of the two phenomena.

The results of these investigations are plainly that the boulders found at a distance from the central Alps, originated from their higher summits and valleys, and were carried down at different successive periods in a regular manner, forming uninterrupted walls and ridges, which can be traced from their starting point to their extreme peripheric distribution.

I have myself shown that there are such centres of distribution in Scotland and England and Ireland. And these facts have been since traced in detail in various parts of the British Islands by Dr. Buckland, Sir Ch. Lyell, Mr. Darwin, Mr. McLachlan and Professor James D. Forbes, pointing clearly to the main mountain groups as to so many distinct centres of dispersion of these loose materials.

Similar phenomena have been shown in the Pyrenees, in the Black Forest, and in the Vosges, showing beyond question, that whatever might have been the cause of the dispersion of erratic boulders, there are several separate centres of their distribution to be distinguished in Europe. But there is another question connected with this local distribution of boulders which requires particular investigation, the confusion of which with the former has no doubt greatly contributed to retard our real progress in understanding the general question of the distribution of erratics.

It is well known that Northern Europe is strewed with boulders, extending over European Russia, Poland, Northern Germany, Holland and Belgium. The origin of these boulders is far north in Norway, Sweden, Lapland and Liefland, but they are now diffused over the extensive plains west of the Ural Mountains. Their arrangement, however, is such that they cannot be referred to one single point of origin, but only in a general way to the northern tracts of land which rise above the level of the sea in the Arctic regions. Whether these boulders were transported by the same agency as those arising from distinct centres, on the main continent of Europe, has been the chief point of discussion. For my own part, I have indeed no doubt that the extreme consequences to which we are naturally carried by admitting that ice was also the agent in transporting the northern erratics to their present positions, has been the chief objection to the view that the Alpine boulders have been distributed by glaciers.

It seemed easier to account for the distribution of the northern erratics by currents, and this view appearing satisfactory to those who supported it, they at once went further, and opposed the glacial theory even in those districts where the glaciers seemed to give a more natural and more satisfactory explanation of the phenomena. To embrace the whole question it should be ascertained,

First, Whether the northern erratics were transported at the same time as the local Alpine boulders, and if not, which of the phenomena preceded the other; and again, if the same cause acted in both cases, or if one of the causes can be applied to one series of these phenomena, and the other cause to the other series. An investigation of the erratic phenomena in North America seems to me likely to settle this question, as the northern erratics occur here in an undisturbed continuation over tracts of land far more extensive than those in which they have been observed in Europe. For my own part, I have already traced them from the eastern shores of Nova Scotia through New England and the North Western States of North America and the Canadas as far as the western extremity of Lake Superior, a region embracing about thirty degrees of longitude. Here, as in Northern Europe, the boulders evidently originated farther north than their present location, and have been moved universally in a main direction from north to south.

From data which are, however, rather incomplete, it can be further admitted that similar phenomena occur further west across the whole continent, everywhere presenting the same relations. That is to say, everywhere pointing to the north as to the region of the boulders, which generally disappear about latitude 38°.

Without entering at present into a full discussion of any theoretical views of the subject, it is plain that any theory, to be satisfactory, should embrace both the extensive northern phenomena in Europe and North America, and settle the relation of these phenomena to the well-authenticated local phenomena of Central Europe.

Whether America itself has its special local circumscribed centres of distribution or not, remains to be seen. It seems, however, from a few facts observed in the White Mountains, that this chain, as well as the mountains of northeastern New York, has not been exclusively—and for the whole duration of the transportation of these materials—under the influence of the cause which has distributed the erratics through such wide space over the continent of North America. But whether this be the case or not, (and I trust local investigations will soon settle the question,) I maintain that the cause which has transported these boulders in the American continent must have acted simultaneously over the whole ground which these boulders cover, as they present throughout the continent an uninterrupted sheet of loose materials, of the same general nature, connected in the same general manner, and evidently dispersed at the same time.

Moreover, there is no ground, at present, to doubt the simultaneous dispersion of the erratics over Northern Europe and Northern America. So that the cause which transported them, whatever it may be, must have acted simultaneously over the whole tract of land west of the Ural Mountains, and east of the Rocky Mountains, without assuming any thing respecting Northern Asia, which has not yet been studied in this respect; that is to say, at the same time, over a space embracing two hundred degrees of longitude.

Again, the action of this cause must have been such, and I insist strongly upon this as a fundamental point, the momentum with which it acted must have been such, that after being set in motion in the north, with a power sufficient to carry the large boulders which are found everywhere over this vast extent of land, it vanished or was stopped after reaching the thirty-fifth degree of northern latitude.

Now it is my deliberate opinion that natural philosophy and mathematics may settle the question, whether a body of water of sufficient extent to produce such phenomena can be set in motion with sufficient velocity to move all these boulders, and nevertheless stop before having swept over the whole surface of the globe. Hydrographers are familiar with the action of currents, with their speed, and the power with which they can act. They know also how they are distributed over our globe. And, if we institute a comparison, it will be seen that there is nowhere a current running from the poles towards the lower latitudes, either in the northern or southern hemisphere, covering a space equal to one-tenth of the currents which should have existed to carry the erratics into their present position. The widest current is west of the Pacific, which runs parallel to the equator, across the whole extent of that sea from east to west, and the greatest width of which is scarcely fifty degrees. This current, as a matter of course, establishes a regular rotation between the waters flowing from the polar regions towards lower latitudes.

The Gulf Stream on the contrary runs from west to east, and dies out towards Europe and Africa, and is compensated by the currents from Baffin's Bay and Spitzbergen emptying into the Atlantic, while the current of the Pacific, moving towards Asia and carrying floods of water in that direction, is maintained chiefly by antarctic currents, and those which follow the western shore of America from Behring's Straits. Wherever they are limited by continents, we see that the waters of these currents, even when they extend over hundreds of degrees of latitude, as the Gulf Stream does in its whole course, are deflected where they cannot follow a straight course.

Now without appealing with more detail to the mechanical conditions involved in this inquiry, I ask every unprejudiced mind acquainted with the distribution of the northern boulders, whether there was any geographical limitation to the supposed northern current to cause it to leave the northern erratics of Europe in such regular order, with a constant bearing from north to south, and to form, on its southern termination, a wide, regular zone from Asia to the western shores of Europe, north of the fiftieth degree of latitude, before it had reached the great barrier of the Alps? I ask whether there was such a barrier in the unlimited plains which stretch from the Arctic seas uninterrupted over the whole northern continent of America as far down as the Gulf of Mexico?

I ask, again, why the erratics are circumscribed within the northern limits of the temperate zone, if their transportation is owing to the action of water currents? Does not, on the contrary, this most surprising limit within the arctic and northern temperate zones, and in the same manner within the antarctic and southern temperate zones, distinctly show that the cause of transportation is connected with the temperature or climate of the countries over which the phenomena were produced. If it were otherwise, why are there no systems of erratics with an east and west bearing, or in the main direction of the most extensive currents flowing at present over the surface of our globe?

It is a matter of fact, of undeniable fact, for which the theory has to account, that in the two hemispheres the erratics have direct reference to the polar regions, and are circumscribed within the arctics and the colder part of the temperate zone. This fact is as plain as the other fact, that the local distribution of boulders has reference to high mountain ranges, to groups of land raised above the level of the sea into heights, the temperature of which is lower than the surrounding plains. And what is still more astonishing, the extent of the local boulders, from their centre of distribution reaches levels, the mean annual temperature of which corresponds in a surprising manner with the mean annual temperature of the southern limit of the northern erratics.

We have, therefore, in this agreement a strong evidence in favor of the view that both the phenomena of local mountain erratics in Europe and of northern erratics in Europe and America have probably been produced by the same cause.

The chief difficulty is in conceiving the possibility of the formation of a sheet of ice sufficiently large to carry the northern erratics into their present limits of distribution; but this difficulty is greatly removed when we can trace, as in the Alps, the progress of the boulders under the same aspect from the glaciers now existing, down into regions where they no longer exist, but where the boulders and other phenomena attending their transportation show distinctly that they once existed.

Without extending further this argumentation, I would call the attention of the unprejudiced observer to the fact, that those who advocate currents as the cause of the transportation of erratics, have, up to this day, failed to show, in a single instance, that currents can produce all the different phenomena connected with the transportation of the boulders which are observed everywhere in the Alps, and which are still daily produced there by the small glaciers yet in existence. Never do we find that water leaves the boulders which it carries along in regular walls of mixed materials; nor do currents any where produce upon the hard rocks *in situ* the peculiar grooves and scratches which we see everywhere under the glacier and within the limits of their ordinary oscillations.

Water may polish the rocks, but it nowhere leaves straight scratches upon their surface; it may furrow them, but these furrows are sinuous, acting more powerfully upon the soft parts of the rocks or fissures already existing; whilst glaciers smooth and level uniformly, the hardest parts equally with the softest, and, like a hard file, rub to uniform continuous surfaces the rocks upon which they move.

But now let us return to our special subject, the erratics of North America.

The phenomena of drift are more complicated about Lake Superior than I have seen them any where else; for, besides the general phenomena which occur everywhere, there are some peculiarities noticed which are to be ascribed to the lake as such, and which we do not find in places where no large sheet of water has been brought into contact with the erratic phenomena. In the first place, we notice about Lake Superior an extensive tract of polished, grooved and scratched rocks, which present here the same uniform character which they have everywhere. As there is so little disposition, among so many otherwise intelligent geologists, to perceive the facts as they are, whenever they bear upon the question of drift, I cannot but repeat, what I have already mentioned more than once, but what I have observed again here over a tract of some fifteen hundred miles, that the rocks are everywhere smoothed, rounded, grooved and furrowed in a uniform direction. The heterogeneous materials of which the rocks consist are cut to one continuous uniform level, showing plainly that no difference in the polish and abrasion can be attributed to the greater or less resistance on the part of the rocks, but that a continuous rasp cut down every thing, adapting itself, however, to the general undulations of the country, but nevertheless showing, in this close adaptation, a most remarkable continuity in its action.

That the power which produced these phenomena moved in the main from north to south, is distinctly shown by the form of the hills, which present abrupt slopes, rough and sharp corners towards the south, while they are all smoothed off towards the north.

Indeed, here, as in Norway and Sweden, there is on all the hills a lee-side and a strike-side. As has been observed in Norway and Sweden, the polishing is very perfect in many places, sometimes strictly as brilliant as a polished metallic surface, and everywhere these surfaces are more or less scratched and furrowed, and both scratches and furrows are rectilinear, crossing each other under various angles: however, never varying many points of the compass on the same spot, but in general showing that where there are deviations from the most prominent direction, they are influenced by the undulations of the soil. It has been said, that the main direction of these striæ was from northwest to southeast, but I have found it as often strictly from north to south, or even from northeast to southwest; and if we are to express a general result, we should say that the direction, assigned by all our observations to the various scratches, tends to show that they have been formed under the influence of a movement from north to south, varying more or less to the east and west, according to local influences in the undulations of the soil. It is, indeed, a very important fact, that scratches which seem to have been produced at no great intervals from each other, are not absolutely parallel, but may diverge for ten, fifteen, or more degrees.

There is one feature in these phenomena, however, in which we never observe any variation. The continuity of these lines is absolutely the same everywhere. They are rectilinear and continuous, and cannot be better compared than with the effects of stones or other hard materials dragged in the same direction upon flat or rolling surfaces; they form simple scratches extending for yards in straight lines, or breaking off for a short space to continue again in a straight line in the same direction, just as if interrupted by a jerk. There are also deeper scratches of the same kind, presenting the same phenomena, only, perhaps, traceable for a greater distance than the finer ones. These scratches, instead of appearing like the tracing of diamonds upon glass, as the former do, would rather assume the appearance of a deeper groove, made by the point of a graver, or perhaps still more closely resemble the scratches which a cart-wheel would produce upon polished marble, if the wheel were chained, and coarse sand spread over the floor, the wheel continuing to move onward but without revolving. The appearance of the surface, crushed by the moving mass, is especially distinct in limestone rocks, where grooves are seldom nicely cut, but present the appearance of a violent pressure combined with the grooving power, thus giving to the groove a character which is quite peculiar, and which at once strikes an observer who has been familiar with its characteristic aspect. Now, I do not know upon what the assertions of some geologists rest, that gravel moved by water under strong heavy currents will produce similar effects. Wherever I have gone since studying these phenomena, I have looked for such cases, and have never yet found modern gravel currents produce any thing more than a smooth surface with undulating furrows following the cracks in the rocks, or hollowing their softer parts; but continuous straight lines, especially such crushed lines and straight furrows, I have never seen.

When we know how extensive the action of water carrying mud and gravel is on every shore and in every water current,—when we can trace this action almost everywhere, and no where find it similar to the phenomena just described, I cannot imagine upon what ground these phenomena are still attributed to the agency of currents. This is the less rational as we have at present, in all high mountain chains of the temperate zone, other agents, the glaciers, producing these very same phenomena, with precisely the same characters, to which, therefore, a sound philosophy should ascribe, at least conditionally, the northern and Alpine polished surfaces, and scratched and grooved rocks, or at least acknowledge that the effect produced by the action of glaciers more nearly resembles these erratic phenomena than does that which results from the action of currents. But such is the prejudice of many geologists, that those keen faculties of distinction and generalization, that power of superior perception and discrimination which have led them to make such brilliant discoveries in geology in general, seem to abandon them at once as soon as they look at the erratics. The objection made by a venerable geologist, that the cold required to form and preserve such glaciers, for any length of time, would freeze him to death, is as childish as the apprehension that the heavy ocean currents, the action of which he sees everywhere, might have swept him away.*

Now that these phenomena have been observed extensively, we may derive also some instruction from the limits of their geographical extent. Let us see, therefore, where these polished, scratched and furrowed rocks have been observed.

In the first place they occur everywhere in the north within certain limits of the arctics, and through the colder parts of the temperate zone. They occur also in the southern hemisphere, within parallel limits, but in the plains of the tropics, and even in the warmer parts of the temperate zone we find no trace of these phenomena, and nevertheless the action of currents could not be less there, and could not at any time have been less than in the colder climates. It is true, similar phenomena occur in Central Europe and have been noticed in Central Asia, and even in the Andes of South America, but these always in higher regions, at definite levels above the surface of the sea, everywhere indicating a connection between their extent and the colder temperature of the places over which they are traced.

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* Berlin Academy, 1846.

More recently, a step towards the views I entertain of this subject, has been made by those geologists who would ascribe them to the agency of icebergs. Here, as in my glacial theory, ice is made the agent; floating ice is supposed to have ground and polished the surfaces of rocks, while I consider them to have been acted upon by terrestrial glaciers. To settle this difference we have a test which is as irresistible as the other arguments already introduced.

Let us investigate the mode of action, the mode of transportation of icebergs, and let us examine whether this cause is adequate to produce phenomena for which it is made to account. As mentioned above, the polished surfaces are continuous over hills, and in depressions of the soil, and the scratches which run over such undulating surfaces are nevertheless continuous in straight lines. If we imagine icebergs moving upon shoals, no doubt they would scratch and polish the rocks in a way similar to moving glaciers. But upon such localities they would sooner or later be stranded, and if they remained loose enough to move, they would, in their gyratory movements, produce curved lines, and mark the spots where they had been stranded with particular indications of their prolonged action. But nowhere upon arctic ground do we find such indications. Everywhere the polished and scratched surfaces are continuous in straight juxtaposition.

Phenomena analogous to those produced by icebergs would only be seen along the sea-shores; and if the theory of drifted icebergs were correct, we should have, all over those continents where erratic phenomena occur, indications of retreating shores as far as the erratic phenomena are found. But there is no such thing to be observed over the whole extent of the North American continent, nor over Northern Europe and Asia, as far as the northern erratics extend. From the arctics to the southernmost limit of the erratic distribution, we find nowhere the indications of the action of the sea as directly connected with the production of the erratic phenomena. And wherever the marine deposits rest upon the polished surfaces of ground and scratched rocks, they can be shown to be deposits formed since the grooving and polishing of the rocks, in consequence of the subsidence of those tracts of land upon which such deposits occur.

Again, if we take for a moment into consideration the immense extent of land covered by erratic phenomena, and view them as produced by drifted icebergs, we must acknowledge that the icebergs of the *present period* at least, are insufficient to account for them, as they are limited to a narrower zone. And to bring icebergs in any way within the extent which would answer for the extent of the distribution of erratics, we must assume that the northern ice fields, from which these icebergs could be detached and float southwards, were much larger at the time they produced such extensive phenomena than they are now. That is to say, we must assume an ice period; and if we look into the circumstances we shall find that this ice period, to answer to the phenomena, should be nothing less than an extensive cap of ice upon both poles. This is the very theory which I advocate; and unless the advocates of an iceberg theory go to that length in their premises, I venture to say, without fear of contradiction, that they will find the source of their icebergs fall short of the requisite conditions which they must assume, upon due consideration, to account for the whole phenomena as they have really been observed.

But without discussing any farther the theoretical views of the question, let me describe more minutely the facts as observed on the northern shores of Lake Superior. The polished surfaces, as such, are even, undulating, and terminate always above the rough lee-side turned to the south, unless upon gentle declivities, where the polished surfaces extend in unbroken continuity upon the southern surfaces of the hills, as well as upon their northern slopes. On their eastern and western flanks, shallow valleys running east and west are as uniformly polished as those which run north and south; and this fact is more and more evident, wherever scratches and furrows are also well preserved and distinctly seen, and by their bearings we can ascertain most minutely, the direction of the onward movement which produced the whole phenomena. Nothing is more striking in this respect than the valleys or depressions of the soil running east and west, where we see the scratches crossing such undulations at right angles, descending along the southern gentle slope of a hill, traversing the flat bottom below, and rising again up the next hill south, in unbroken continuity. Examples of the kind can be seen everywhere in those narrow inlets, with shallow waters intersecting the innumerable highlands along the northern shores of Lake Superior, where the scratches and furrows can be traced under water from one shore to the other, and where they at times ascend steep hills, which they cross at right angles along their northern slope, even when the southern slope, not steeper in itself, faces the south with rough escarpments.

The scratches and furrows, though generally running north and south, and deviating slightly to the east and west, present in various places remarkable anomalies, even in their general course along the eastern shore of the lake. Between Michipicotin and Sault St. Marie we more frequently see a deflection to the west than a due north and south course, which is rather normal along the northern shore proper, between Michipicotin and other islands, and from the Pic to Fort William; the deep depression of the lake being no doubt the cause of such a deviation, as large masses of ice could accumulate in this extensive hollow cavity before spreading again more uniformly beyond its limits. To the oscillations of the whole mass in its southerly movement, according to the inequalities of the surfaces, we must ascribe the crossing of the straight lines at acute angles, as we observe also at the present day under the glaciers, as they swell and subside, and hence meet with higher and lower obstacles in their irregular course between the Alpine valleys.

In deep, narrow chasms, however, we find now and then greater deviations from the normal direction of the striæ, where considerable masses of ice could accumulate, and move between steep walls under a lateral pressure of the masses moving onwards from the north. Such a chasm is seen between Spar Island and the main land opposite Prince's Location, south of Fort William, where the furrows and scratches run nearly east and west. But here also, there is no tumultuous disturbance in the continuation of the phenomena, such as would occur if icebergs were floated and stranded against the southern barrier. The same continuity of even, polished surfaces, with their scratches and furrows, prevails here as elsewhere. The angles which these scratches form with each other are very acute, generally not exceeding 10°; but at times they diverge more, forming angles of 15°, 20° and 25°. In a few instances, I have even found localities where they crossed each other at angles of no less than 30°; but these are rare exceptions. It may sometimes be noticed that the lines running in one direction form a system by themselves, varying very little from strict parallelism with each other, but crossing another system, more or less strongly marked, of other lines equally parallel with each other. At other times, a system of lines, strongly marked and diverging very slightly, seem to pass over another system, in which the lines form various angles with each other. Again, there are places,—and this is the most common case,—where the lines diverge slightly, following, however, generally one main direction, which is crossed by fewer lines, forming more open angles. These differences, no doubt, indicate various oscillations in the movement of the mass which produced the lines, and show probably its successive action, with more or less intensity, upon the same point at successive periods, in accordance with the direction of the moving force at each interval. The same variations within precisely the same limits may be noticed in our day on the margin of the glaciers produced by the increase or diminution of the bulk of their mass, and the changes in the rate of their movement.

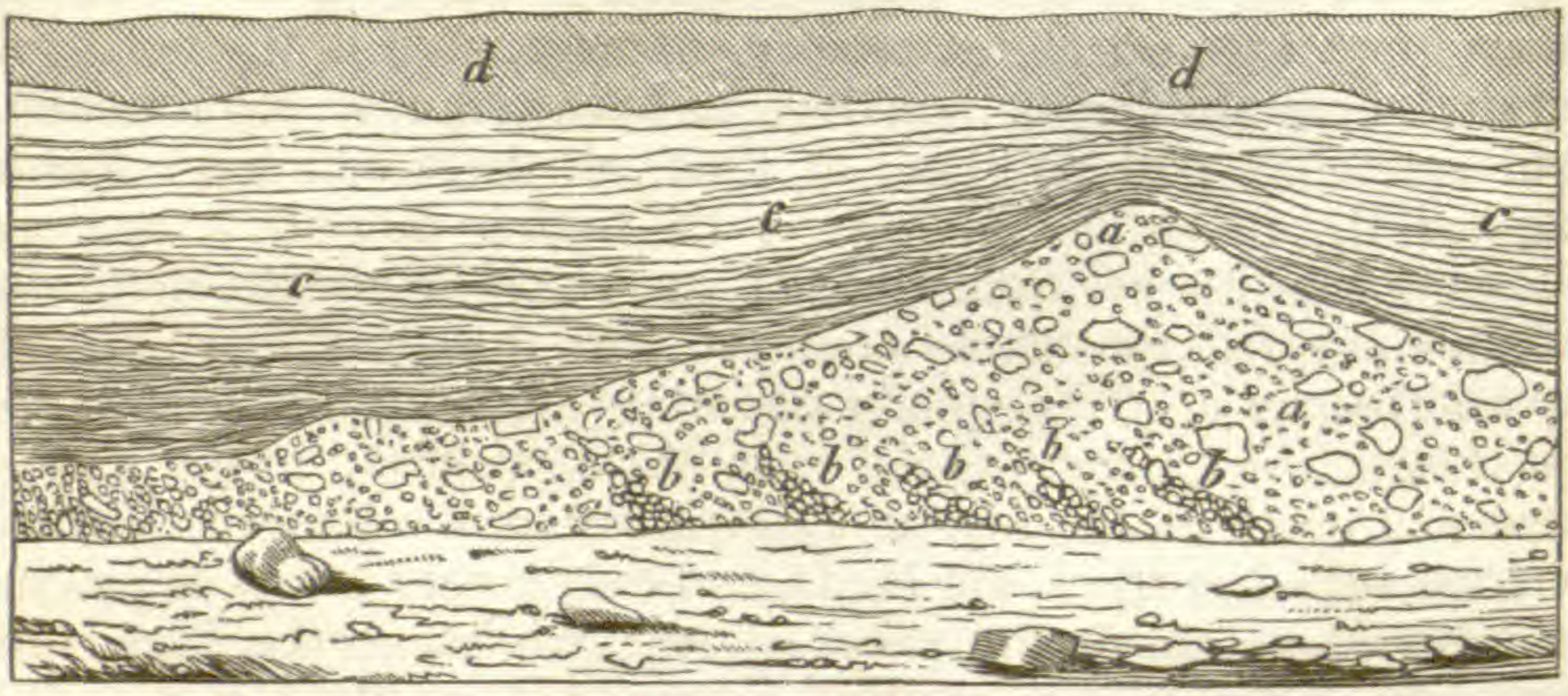
The loose materials which produced, in their onward movement under the pressure force, such polishing and grooving, consisted of various sized boulders, pebbles and gravels, down to the most minute sand and loamy powder. Accumulations of such materials are found everywhere upon these smooth surfaces, and in their arrangement they present everywhere the most striking contrast when compared with deposits accumulated under the agency of water. Indeed, we nowhere find this glacial drift regularly stratified, being everywhere irregular accumulations of loose materials, scattered at random without selection, the coarsest and most minute particles being piled irregularly in larger or smaller heaps, the greatest boulders standing sometimes uppermost, or in the centre, or in any position among smaller pebbles and impalpable powder.

And these materials themselves are scratched, polished and furrowed, and the scratches and furrows are rectilinear as upon the rocks *in situ* underneath, not bruised simply, as the loose materials carried onward by currents or driven against the shores by the tides, but regularly scratched, as fragments of hard materials would be if they had been fastened during their friction against each other, just as we observe them upon the lower surface of glaciers where all the loose materials set in ice, as stones in their setting, are pressed and rubbed against underlying rocks. But the setting here being simply ice, these loose materials, fast at one time and movable another, and fixed and loosened again, have rubbed against the rock below in all possible positions; and hence not only their rounded form, but also their rectilinear grooving. How such grooves could be produced under the action of currents, I leave to the advocates of such a theory to show, as soon as they shall be prepared for it.

I should not omit here to mention a fact which, in my opinion, has a great theoretical importance, namely, that in the northern erratics, even the largest boulders, as far as I know, are rounded, and scratched and polished, at least, all those which are found beyond the immediate vicinity of the higher mountain ranges; showing that the accumulations of ice which moved the northern erratics covered the whole country; and this view is sustained by another set of facts equally important, namely, that the highest ridges, the highest rugged mountains, at least, in this continent and north of the Alps in Europe, are as completely polished and smoothed as the lower lands, and only a very few peaks seem to have risen above the sheet of ice; whilst, in the Alps, the summits of the mountains stand generally above these accumulations of ice, and have supplied the surface of the glaciers with large numbers of angular boulders, which have been carried upon the back of glaciers to the lower valleys and adjacent plains without losing their angular forms.

With respect to the irregular accumulation of drift-materials in the north, I may add that there is not only no indication of stratification among them, such unquestionably as water would have left, but that the very nature of these materials shows plainly that they are of terrestrial origin; for the mud which sticks between them adheres to all the little roughnesses of the pebbles, fills them out, and has the peculiar adhesive character of the mud ground under the glaciers, and differing entirely in that respect from the gravels and pebbles and sands washed by water currents, which leave each pebble clean, and never form adhering masses, unless penetrated by an infiltration of limestone.

Another important fact respecting this glacial drift consists in the universal absence of marine as well as freshwater fossils in its interior, a fact which strengthens the view that they have been accumulated by the agency of strictly terrestrial glaciers; such is, at least, the case everywhere far from the sea-shore. But we may conclude that these ancient glaciers reached, upon various points, the sea-shore at the time of their greatest extension, just as they do at present in Spitzbergen and other arctic shores; and that therefore, in such proximity, phenomena of contact should be observed, indicating the onward movement of glacial material into the ocean, such as the accumulation within these materials of marine fossil remains, and also the influence of the tidal movements upon them. And now such is really the case. Nearer the sea-shores we observe distinctly, in some accumulations of the drift, faint indications of the action of the tide reaching the lower surface of glaciers, and the remodeling, to some extent, of the materials which there poured into the sea. A beautiful example of the kind may be observed near Cambridge, along Charles River, not far from Mount Auburn, where the unstratified glacial



drift (*a*) presents in its upper masses strictly the characters of true terrestrial glacial accumulation, but shows underneath faint indications (*b*) of the action of tides. Above, regular tidal strata (*c*) are observed, formed probably after the masses below had subsided. The surface of this accumulation is covered with soil (*d*).

The period at which these phenomena took place cannot be fully determined, nor is it easy to ascertain whether all glacial drift is contemporaneous. It would seem, however, as if the extensive accumulation of drift all around the northern pole in Europe, Asia and America was of the same age as the erratics of the Alps. The climatic circumstances capable of accumulating such large masses of ice around the north pole, having, no doubt, extended their influence over the temperate zone, and probably produced, in high mountain chains, as the Alps, the Pyrenees, the Black Forest, and the Vosges, such accumulations of snow and ice, as may have produced the erratic phenomena of those districts. But extensive changes must have taken place in the appearance of the continents over which we trace erratic phenomena, since we observe in the Old World, as well as in North America, extensive stratified deposits containing fossils which rest upon the erratics; and as we have all possible good reasons and satisfactory evidence for admitting that the erratics were transported by the agency of terrestrial glaciers, and that therefore the tracts of land over which they occur, stood at that time above the level of the sea, we are led to the conclusion that these continents have subsided since that period below the level of the sea, and that over their inundated portions animal life has spread, remains of organized beings have been accumulated, which are now found in a fossil state in the deposits formed under those sheets of water.

Such deposits occur at various levels in different parts of North America. They have been noticed about Montreal, on the shores of Lake Champlain, in Maine and also in Sweden and Russia; and, what is most important, they are not everywhere at the same absolute level above the surface of the ocean, showing that both the subsidence, and the subsequent upheaval which has again brought them above the level of the sea, have been unequal; and that we should therefore be very cautious in our inferences respecting both the continental circumstances under which the ancient glaciers were formed, and also the extent of the sea afterward, as compared with its present limits.

The contrast between the unstratified drift and the subsequently stratified deposits is so great, that they rest everywhere unconformably upon each other, showing distinctly the difference of the agency under which they were accumulated. This unconformable superposition of marine drift upon glacial drift is also beautifully shown at the above mentioned locality near Cambridge. (See Diagram.) In this case the action of tides in the accumulation of the stratified materials is plainly seen.

The various heights at which these stratified deposits occur, above the level of the sea, show plainly, that since their accumulation, the main land has been lifted above the ocean at different rates in different parts of the country; and it would be a most important investigation to have their absolute level, in order more fully to ascertain the last changes which our continents have undergone.

From the above mentioned facts, it must be at once obvious that the various kinds of loose materials, all over the northern hemisphere, have been accumulated, not only under different circumstances, but during long-continued subsequent distinct periods, and that great changes have taken place since their deposition, before the present state of things was fully established.

To the first period,—the ice period, as I have called it,—belong all the phenomena connected with the transportation of erratic boulders, the polishing, scratching and furrowing of the rocks and the accumulation of unstratified, scratched, and loamy drift. During that period, the main land seems to have been, to some extent at least, higher above the level of the sea than now; as we observe, on the shores of Great Britain, Norway and Sweden, as well as the eastern shores of North America, the polished surfaces dipping under the level of the ocean, which encroaches everywhere upon the erratics proper, effaces the polished surfaces and remodels the glacial drift. During these periods, large terrestrial animals lived upon both continents, the fossil remains of which are found in the drift of Siberia, as well as of this continent. A fossil elephant recently discovered in Vermont* adds to the resemblance, already pointed out, between the northern drift of Europe and that of North America; for fossils of that genus are now known to occur upon the northernmost point of the western extremity of North America, in New England, in Northern Europe, as well as all over Siberia.

To the second period we would refer the stratified deposits resting upon drift, which indicate that during their deposition the northern continent had again extensively subsided under the surface of the ocean.

During this period, animals, identical with those which occur in the northern seas, spread widely over parts of the globe which are now again above the level of the ocean. But, as this last elevation seems to have been gradual, and is even still going on in our day, there is no possibility of tracing more precisely, at least for the present, the limit between that epoch and the present state of things. Their continuity seems almost demonstrated by the identity of fossil shells found in these stratified deposits, with those now living along the present shores of the same continent, and by the fact that changes in the relative level between sea and main land are still going on in our day.

Indications of such relative changes between the level of the waters and the land are also observed about Lake Superior. And here they assume a very peculiar character, as the level of the lake itself, in its relation to its shores, is extensively changed.

All around Lake Superior we observe terraces at different levels; and these terraces vary in height, from a few feet above the present level of the lake, to several hundred feet above its surface,

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* See this Journal, vol. ix, p. 256.

presenting everywhere undoubted evidence, that they were formed by the waters of the lake itself.

As everywhere the lake shores are strewed with sand and pebbles stranded within certain limits by the waves, the lowest accumulations of loose materials remain within the action of heavy storms, and within such limit they are entirely deprived of vegetation.

Next, another set of beaches is observed, consisting generally of coarser materials, forming shelves above the reach of even the severest storms, as shown by the scanty cryptogamus vegetation, and a few small herbaceous plants which have grown upon them.

Next, other beaches, retreating more and more from the shores, are observed, upon which an older vegetation is traced, consisting of shrubs, small trees, and a larger number of different plants, among which extensive carpets of wonderful lichens sometimes spread over large surfaces of greater extent. And the gentle slope of some of the terraces shows that the lake must have stood at this level for a longer time, as higher banks rise precipitously above them, consisting also of loose materials, which must have been worn out and washed away, for a considerable time, by the action of the waves from the lake. In such a manner, terrace above terrace may be observed, in retreating sheltered bays or along protected shores, over extensive tracts; sometimes two or three in close proximity, perhaps within twenty to fifty feet of each other; and again, extensive flat shores, spreading above to another abrupt bank, making the former shore, above which other and other terraces are seen; six, ten, even fifteen such terraces may be distinguished on one spot, forming, as it were, the steps of a gigantic amphitheatre. The most remarkable of all the amphitheatres has been sketched by Mr. Cabot, and forms the frontispiece to this volume. Its height has been determined by Mr. Logan, in his Geographical Report of Canada, page 10, where it is minutely described. I therefore refer to this account for further details. I would only mention here, that the first shelf, within the reach of the lake, consists of minute sand, and forms a narrow strip of sterile ground along the water-edge; next, we have a slope of about 10°, followed by a flat terrace, extending for nearly fifty paces to a second very steep slope, about 26° and 30° inclination; then a sloping terrace with an inclination of near 16°, stretching for eighty to a hundred paces, above which rises another steep slope of 20°, beyond which an extensive flat, slightly sloping, extends for several hundred paces, crowned by some irregular ridges at its summit, and along the rocky ledges which form the bay at the bottom of which this high gravel bank rises.

In connection with these lake terraces, we must consider also the river terraces which present similar phenomena along their banks all around the lake, with the difference that they slope gradually along the water courses, otherwise resembling in their composition the lake terraces, which are altogether composed of remodeled glacial drift, which, from the influence of the water and their having been rolled on the shores, have lost, more or less, their scratches and polished appearance, and have assumed the dead smoothness of water pebbles. Such terraces occur frequently between the islands, or cover low necks connecting promontories with the main land, thus showing, on a small scale, how by the accumulation of loose materials, isolated islands may be combined to form larger ones, and how, in the course of time, by the same process, islands may be connected with the main land.

The lake shores present another series of interesting phenomena, especially near the mouth of larger rivers emptying into the lake over flats, where parallel walls of loose materials, driven by the action of the lake against the mouth of the river, have successively stopped its course and caused it to wind its way between the repeated accumulations of such obstacles.

The lower course of Michipicotin River is for several miles dammed up in that way by concentric walls, across which the river has cut its bed, and winding between them, has repeatedly changed its direction, breaking through the successive walls in different places. The largest and lowest of these walls, a kind of river terrace near the margin of the lake, shuts at present the factory from the immediate lake shore, and the river, which has cut its way between the rocks to the right and the walls, has left a bold bank in this dam on its left shore.

An important question now arises, after considering these facts, how these successive changes in the relative level of the lake and its shores have been introduced. Has the water been gradually subsiding, or has the shore been repeatedly lifted up? Merely from the general inferences of the more extensive phenomena described above, respecting the relative changes between land and sea, I should be inclined to admit that the land has risen, rather than to suppose that the waters have gradually flowed out.

But there are about the lake itself sufficient proofs, which leave in my mind not the slightest doubt that it is the land which has changed its level, and not the lake which has subsided.

In the first place, to suppose that the lake had once stood as high as the highest terraces, it would be necessary to admit that its banks were, all round its shores, sufficiently high to keep the water at that highest level, or, at least, that there were, at the lower outlets, bars to that height, which have been gradually removed since. But neither is the main land sufficiently high, at the western extremity and along the southern shores, to admit of such a supposition, nor is there about the outlet of the lake, between Gros Cap and Cap Iroquois, an indication of a barrier which has been gradually removed. There, as everywhere along the lake shores, the loose movable materials consist of the same drift, the accumulation of which, at various levels, we are aiming to account for. If, therefore, we consider this same drift as the barrier under whose protection the lake modeled other parts of its mass, we should be compelled to admit another cause to remove the barrier, a supposition for which there is not the slightest indication in the geological structure of the country. But if, on the contrary, we suppose the lake to have removed the barrier, there is no cause left for its accumulation, and the changes in the comparative level of the main land and the terraces remain equally unaccounted for.

Indeed, the terraces are so unequal in their absolute level when compared to each other, that a gradual subsidence of the lake removing a barrier of loose materials at its outlet could never explain their irregularity. But if we suppose that the innumerable dykes which cross, in all directions, the rocks which form the shores of the lake, have at various intervals lifted up these shores, we have at the same time a cause for the change of the relative level between the terraces and the lake, and also for the change of its absolute level, as it removed larger and larger portions of materials accumulated at its eastern extremity.

That these dikes have produced such changes will not be doubted by any one who may study the phenomena described in the following chapter respecting the origin of the present outlines of the lakes, as produced by the intersection of all the dikes traversing the metamorphic and plutonic rocks of the northern shores.

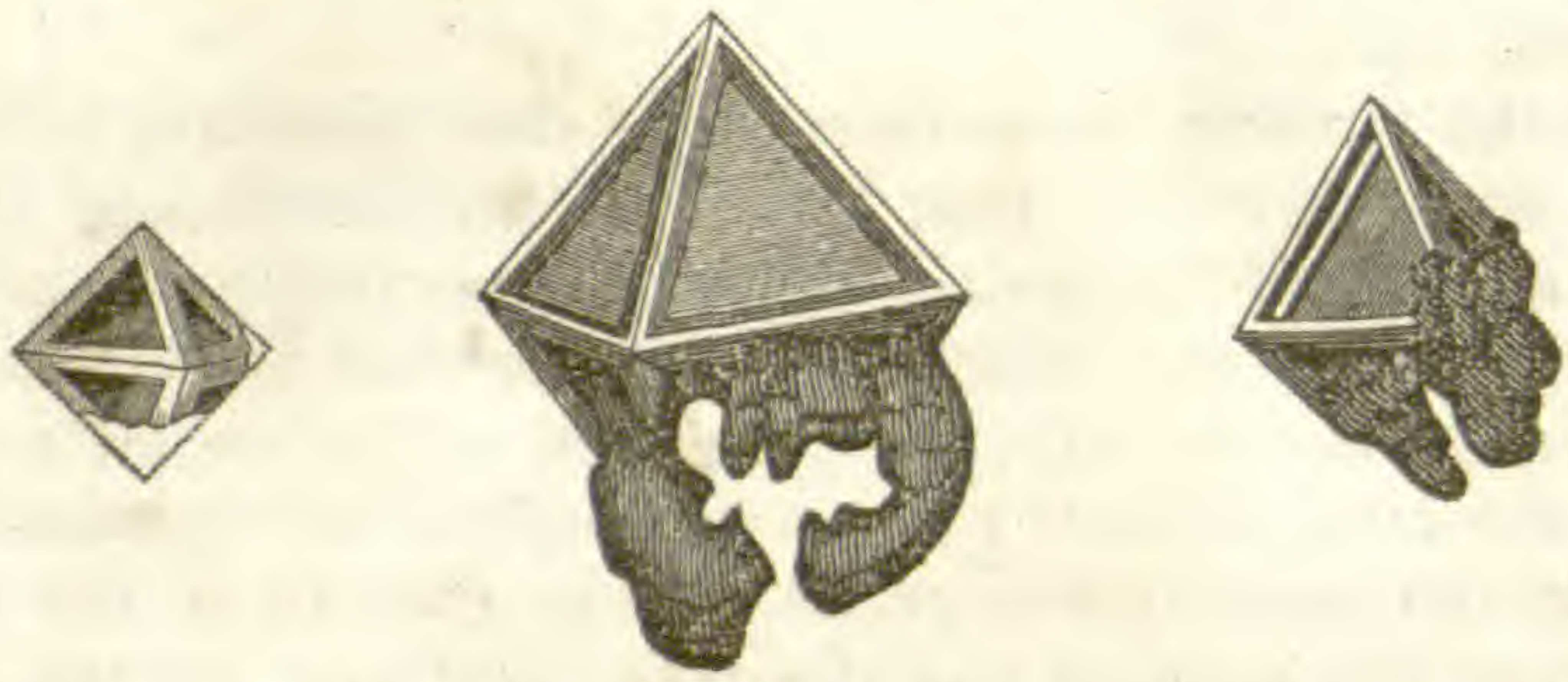
We should therefore conclude that, as there has been a general gradual change between the relative level of the main land and sea, so there has also been a gradual local change in the relative level of the lake and its shores; and hence the local phenomena would only corroborate the induction derived from more general geological facts.

Art. XIV.—*Crystallized Gold from California;* by Francis Alger, Boston.*

I have lately had an opportunity of examining some parcels of California gold, which have afforded specimens worthy of especial notice. Those which I purpose to describe in this paper, I obtained from the collections brought home by Mr. George E. Tyler, of this city, and Mr. H. B. Platt, of New York. They consist of well characterized octahedral crystals, simple and modified, the surfaces of which have been but slightly disfigured by

* Read, in part, before the Boston Society of Natural History, April, 1850.

attrition, or the effects of transported action usually observed in other specimens. I cannot say that I have ever before seen what was unquestionably a genuine crystal from this new land of gold;—an irregular crystalline plane could only occasionally be traced out in former specimens; but here we have examples of crystallization, as perfect among the small ones especially, as are to be seen in magnetic iron ore or in spinelle. The most striking examples on a large scale, are three octahedrons of the dimensions of the accompanying figures. They are isolated crystals, and the smallest one,



which is the most perfect, is so entirely free from any adhering portion of the matrix to which it must have been attached, as to lead me to believe that this matrix was a much softer material than the quartz in connection with which the gold is usually found. Although its exact locality is not known, it is probable, as indicated by its slightly worn appearance, that it has been but recently dislodged from its original resting place. This crystal presents four pretty regular faces, and has three of its solid angles perfectly formed to a point. It exhibits no modifications; but two of its faces are depressed—one of them by a very deep cavity which extends not quite to the edges of the plane, but so near to them as to leave a narrow ridge, or border, all around the cavity and parallel with the edges, thus giving the same triangular outline to each. It appears as if the crystal had been in a liquid state, and that soon after the outside had congealed, the inner portion, or a part of it, had run out, leaving the surrounding consolidated edge just referred to. I have seen something similar to this formed among artificial crystals, as for instance, metallic lead (which takes the form of an octahedron) and lead ore partially desulphurized, when the metal was allowed to flow off slowly, just as the outer crust had formed over the surface of the crystals.

The large crystal presents only one half of the octahedron, its base blending with the massive gold, or only indicating the incipient planes of the lower pyramid. Three of its planes are perfectly smooth surfaces, excepting along their edges, which are prominently marked by the same projecting border or ridge already described on the smaller crystal. This border may have been produced in the same manner by the shrinking away of the metal, or it may be the result of that kind of crystallization which is dependent on a greater intensity of molecnlar attraction in one direction, or axis, than another. It would seem in this case, as if the molecules arrived at the points of contact along the edges of the crystal, faster than they could be appropriated, and thus they have accumulated in these little ridges. This peculiarity is not confined to the large crystals, for it is observed even among the smallest; but it is confined to the unmodified octahedrons. In a few crystals, there is a double series of these ridges, the inner one representing, apparently, the commencement of another crystalline face within the cavity of the larger one. This is beautifully shown in the third figure on the last page, also answering to the natural size of the crystal.*

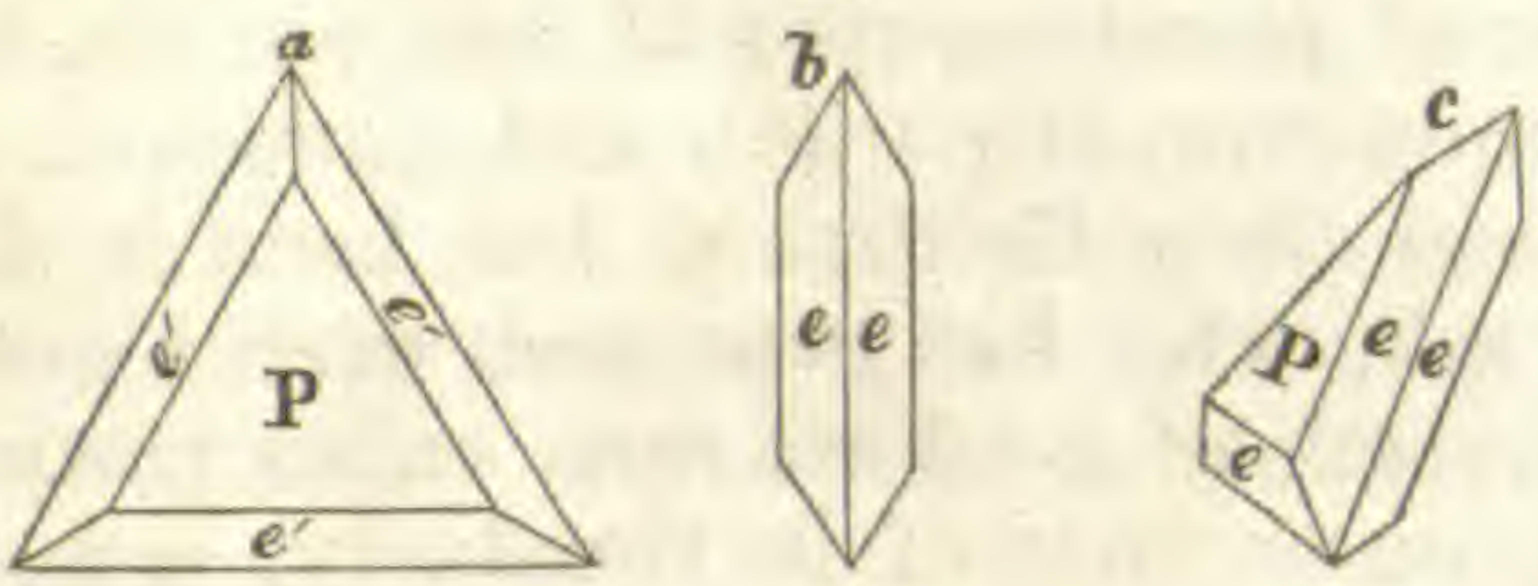
The great size of these crystals, and the fact that some of the cavities contained portions of oxyd of iron probably produced by the decomposition of pyrites, have led some to regard them as pseudomorphs of sulphuret of iron. I am not disposed to ascribe any such forced and unnatural origin to these beautiful productions. I believe them to have been formed under the ordinary circumstances of crystallization, either in an open space, or while surrounded by a matrix so soft and accommodating, as to allow them full freedom to take the form it was intended they should take. Were the crystals cubes, there might be some reason for regarding them in the light of pseudomorphs of iron pyrites, because this is the most common form of pyrites, and, moreover, all the pyrites that I have seen from California, has been in that form. But, we may well ask, who has ever seen even a cubic pseudomorph of gold? Crystals of gold are rare, cubes particularly so, and yet this form, on account of its simplicity, is made the primary form; whereas it would seem as reasonable in cases of the regular system, to select that form as the primary which is most commonly and perfectly presented by the mineral, provided there is no cleavage to guide us in the determination; and there does not appear to be any, well made out, among most of the native metals. By assuming those which most commonly occur in nature, we seem to recognize a sort of inherent disposition, a preference, as it were, which is shown by the mineral itself; and we avoid what seems to be a palpable inconsistency,

* The two large crystals above described were obtained from the very choice and beautiful collection of specimens, made with great care, and at no small expense, by Mr. Platt. This gentleman, during a most prosperous residence of two years in San Francisco, and while occupying a situation which brought him into daily and almost hourly contact with persons returning from the mines, has evinced his good taste by purchasing the most interesting specimens obtained by them. He has consequently been rewarded by the finest amateur collection hitherto brought from California. It comprises a great variety of ramified, arborescent, dendritic and other imitative forms, here and there showing crystalline faces, all of them being sometimes most fantastically joined together in the same specimen. He informs me that in obtaining this collection, he had examined gold to the amount of more than four millions of dollars.

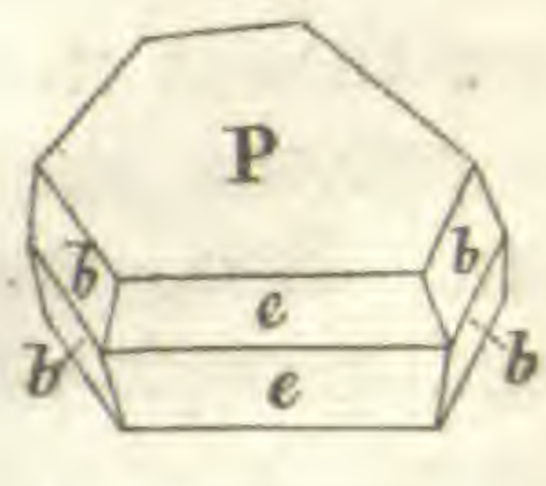
viz., the establishing of a cube as the primary form of minerals, which have never been known to occur under such form, and which even present a distinct octahedral cleavage. This is the case with two at least. If we take the simplest form, the cube should be made the primary of native iron, copper, lead, silver, and mercury; and so of some others, which occur in octahedrons and are not determined by any certain cleavage. In the case of copper, some authors have made the cube its primary.* Haüy (Traité, 1808) even expressed his doubts as to the existence of cubic gold, while he cites examples of the octahedron; and Beudant, (Min., 1832,) says they are very rare.† Mohs implies the contrary, for he says (Min. Ed. by Haidinger) they are often hollow, while the octahedrons are smooth. Cleaveland describes the crystals in general as small and imperfect, and Nicol, in his late work, in the like manner, observes, "they are small and very small." I hope we may yet say of our California gold crystals, they are large and very large, as much for the benefit of mineralogists, as a reward to the industry and hard toil of the diggers.

Crystals of rare modifications.—Among the specimens collected by Mr. Tyler, I have found several rare modifications of this metal, such as come to us in their most perfect forms from Brazil. I here give figures of two of them. One, fig. 1, (*a*, *b*, *c*,)

1.



2.



represents a compound form produced by the union of two opposite sections, or segments, of an emarginated octahedron; a form not unfrequently presented by octahedral spinelle. The other, fig. 2, has the apparent form of irregular six-sided tables, with truncated edges, and is a modification of the same form. They are tolerably well represented by the figures referred to, but the planes of the dodecahedron (*e*) are less conspicuously defined on the real crystals, owing to their extreme thinness, and their edges being rounded off, or otherwise disfigured. For this reason, we can distinguish but two of the primary octahedral planes on any of these crystals. Crystals somewhat resembling

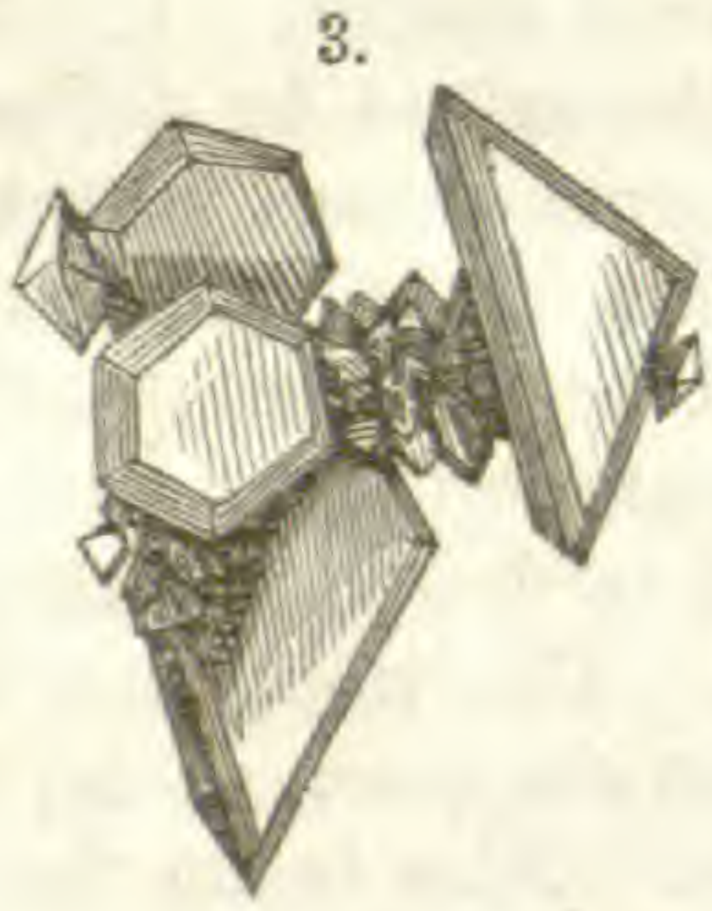
* They differ in regard to silver and iron, some adopting the cube, and others the octahedron, as the primary.

† Cronstedt, in his Mineralogy, says, "I have procured in Transylvania a specimen of cubic native gold, but I have never seen it any where else." In Levy's enumeration of the splendid Turner collection formed by Henry Heuland, eight examples are given of the regular octahedron, and only two of the cube, one of these being from the very locality Cronstedt speaks of.

fig. 2, have been brought from Brazil and Siberia; they had the same flattened form of six-sided tables, but they are the result of a different kind of modification, and are not macles. Dufrénoy and Levy,* have each given a figure of a very perfect example of the modification here referred to (*triforme* of Haüy) derived from the cube, octahedron and dodecahedron ; and I was at first, inclined to regard my crystals as the same, only more deeply truncated on the solid angles *a*, as lettered by Dufrénoy. Further examination however, proved them to be the same macled combination as fig. 1, with the additional replacements indicated by the letters *b*, which have changed the triangular faces P, and the whole crystal, into a hexagonal figure as above represented. These new planes are sometimes unequally extended, and roughened by inequalities, while the primary faces P, are perfectly smooth and brilliant.†

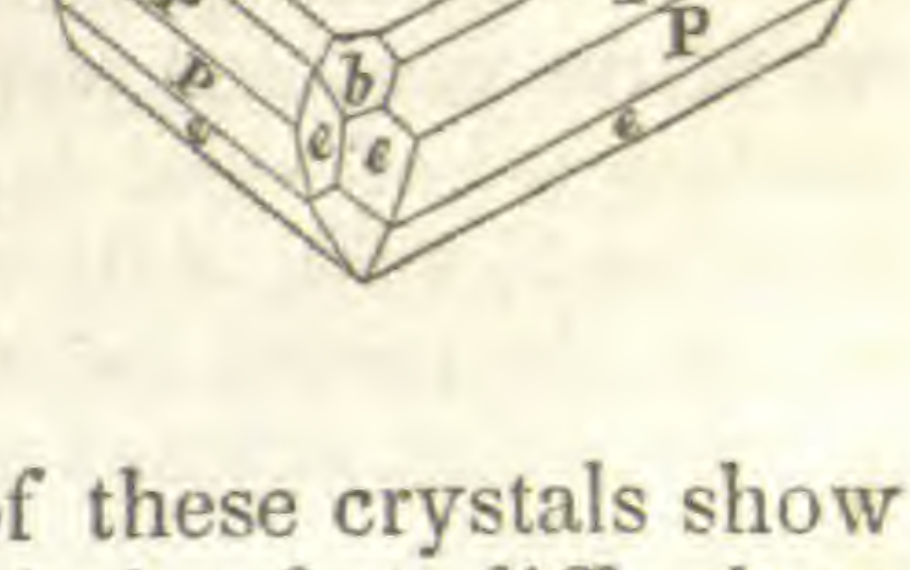
Some of the unmodified macles, as shown in different positions by fig. 1, *a*, *b*, *c*, are very distinctly formed, the edges between *e e*, uniting the two segments of the octahedron, being well defined. This is the most strikingly shown on the largest of these crystals, the lowest one of the group represented by fig. 3.‡ Owing to its position, it is only partially visible in the group as here drawn, and fig. 4 is intended to show it as it appears on the opposite side of the specimen in juxtaposition with two other crystals, one a very beautiful and smooth planed octahedron with emarginated edges. Dufrénoy has described a macle of gold quite similar to fig. 1, and it may be seen figured in the volume of elegant and copious crystallographic illustrations which accompany his treatise.§ It came from Matto-Grosso, in Brazil, and is in the collection of the School of Mines, Paris. It differs from the example here described, in exhibiting more of the planes of the octahedron and dodecahedron. Fig. 5 is a copy of his figure, but regarding the octahedron as the primary form of gold, I have, besides conforming his lettering to the notation of Phillips, made primary planes of those which he gives only as secondaries of the cube.

3.



4.

5.



It should be observed that as nearly all of these crystals show the effects of more or less abraded action, it is often difficult to

* Fig. 576, plate 144, of Dufrénoy's, and fig. 3, plate 47, of Levy's Atlas of Plates.

† In the Turner collection there is a single macle crystal answering almost exactly to fig. 2, and Levy has figured it in his Atlas, plate 47, fig. 4.

‡ This group represents the figures as magnified to about twice their natural size.

§ Atlas, plate 145, fig. 581.

distinguish planes confined within such narrow limits, and which are too small or too rough to admit of accurate measurement. We can hardly expect to see many perfect crystals from California, until the rocks themselves are systematically explored, from whence have proceeded the millions of fragments that are now scattered over the plains and valleys. We may then, from the indications already afforded, look for crystals of gigantic dimensions, and possessing all their native unaltered beauty.

Boston, May 17, 1850.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Diffusion of Liquids;* by Professor Graham, F.R.S., (Proc. Roy. Soc., Phil. Mag., Feb., 1850, vol. xxxvi, p. 139.)—The apparatus used in studying the diffusion of salts and other substances into water was very simple. It consisted of an open phial to contain the solution of the salt to be diffused, which was entirely immersed in a large jar of pure water, so that the solution in the phial communicated freely with the latter. Phials cast in a mould of the capacity of four ounces of water, or more nearly 2000 grains, were generally employed, which were ground down to a uniform height of 3·8 inches. The neck was 0·5 inch in depth, and the aperture or mouth of the phial 1·25 inch in diameter. The phial was filled up with the solution to be diffused till it reached the point of a pin dipping exactly 0·5 inch into the mouth of the bottle. This being the solution cell or bottle, and the external jar the "water-jar," the pair together form a "diffusion cell." The diffusion was stopped, generally after seven or eight days, by closing the mouth of the phial with a plate of glass, and then raising it out of the water-jar. The quantity of salt which had found its way into the water-jar—the diffusion product as it was called—was then determined by evaporating to dryness.

The characters of liquid diffusion were first examined in detail with reference to common salt.

It was found, first, that with solutions containing 1, 2, 3 and 4 per cent. of salt, the quantities which diffused out of the phials into the water of the jars, and were obtained by evaporating the latter, in a constant period of eight days, were as nearly in proportion to these numbers, as 1, 1·99, 3·01 and 4·00; and that in repetitions of the experiments, the results did not vary more than $\frac{1}{40}$th part. The proportion of salt which diffused out in such experiments amounted to about $\frac{1}{8}$th of the whole.

Secondly, that the proportion of salt diffused increases with the temperature; an elevation of 80° F. doubling the quantity of chlorid of sodium diffused in the same time.

The diffusibility of a variety of substances was next compared, a solution of 20 parts of the substance in 100 water being always used.

Some of the results are as follows, the quantities diffused being expressed in grains: chlorid of sodium 58·68, sulphate of magnesia 27·42, sulphate of water 69·32, crystallized cane-sugar 26·74, starch-sugar 26·94, gum-arabic 13·24, albumen 3·03. The low diffusibility of albumen is very remarkable, and the value of this property in retaining the serous fluids within the blood-vessels at once suggests itself. It was further observed, that common salt, sugar and urea, added to the albumen under diffusion, diffused away from the latter as readily as from their aqueous solutions. Urea itself is as highly diffusible as chlorid of sodium.

In comparing the diffusion of salts dissolved in 10 times their weight of water, it was found that isomorphous compounds generally had an equal diffusibility, chlorid of potassium corresponding with chlorid of ammonium, nitrate of potash with nitrate of ammonia, and sulphate of magnesia with sulphate of zinc. The most remarkable circumstance is that these pairs are "equi-diffusive," not for chemically equivalent quantities, but for equal weights simply. The acids differed greatly in diffusibility, nitric acid being nearly four times more diffusive than phosphoric acid; but these substances also fell into groups, nitric and hydrochloric acids appearing to be equally diffusive; so also acetic and sulphuric acids. Soluble sub-salts and the ammoniated salts of the metals present a surprisingly low diffusibility; the quantities diffused in similar circumstances of the three salts, sulphate of ammonia, sulphate of copper, and the blue ammonio-sulphate of copper being very nearly as 8, 4 and 1.

When two salts are mixed in the solution-cell, they diffuse out into the water atmosphere separately and independently of each other, according to their individual diffusibilities. This is quite analogous to what happens when mixed gases are diffused into air. An important consequence is, that in liquid diffusion we have a new method of separation or analysis for many soluble bodies, quite analogous in principle to the separation of unequally volatile substances in the process of distillation. Thus, it was shown that chlorids diffuse out from sulphates and carbonates, and salts of potash from salts of soda; and that from sea-water the salts of soda diffuse out into pure water faster than the salts of magnesia. The latter circumstance was applied to explain the discordant results which have been obtained by different chemists in the analyses of the water of the Dead Sea, taken near the surface; the different salts diffusing up into the sheet of fresh water, with which the lake is periodically covered, with unequal velocity.

It was further shown that chemical decompositions may be produced by liquid diffusion; the constituents of a double salt of so much stability as common alum being separated, and the sulphate of potash diffusing in the largest proportion. In fact the diffusive force is one of great energy, and quite as capable of breaking up compounds as the unequal volatility of their constituents. Many empirical operations in the chemical arts, it was said, have their foundation in such decompositions.

Again, one salt, such as nitrate of potash, will diffuse into a solution of another salt, such as nitrate of ammonia, as rapidly as into pure water; the salts appearing mutually diffusible, as gases are known to be.

Lastly, the diffusibilities of the salts into water, like those of the gases into air, appear to be connected by simple numerical relations.

These relations are best observed when dilute solutions of the salts are diffused from the solution-cell, such as 4, 2 or even 1 per cent. of salt. The quantities diffused in the same time from 4 per cent. solutions of the three salts, carbonate of potash, sulphate of potash, and sulphate of ammonia, were 10·25, 10·57, and 10·51 grains respectively; and a similar approach to equality was observed in the 1, 2, and $6\frac{2}{3}$ per cent. solutions of the same salts. It also held at different temperatures. The acetate of potash appeared to coincide in diffusibility with the same group, and so did the ferrocyanid of potassium. The nitrate of potash, chlorate of potash, nitrate of ammonia, chlorid of potassium and chlorid of ammonium formed another equi-diffusive group. The *times* in which an equal amount of diffusion took place in these two groups appear to be as 1 for the second to 1·4142 for the first, or as 1 to the square root of 2. Now in gases, *the squares of the times* of equal diffusion are *the densities of the gases.* The relation between the sulphate of potash and nitrate of potash groups would therefore fall, to be referred to the diffusion molecule or diffusion vapor of the first group having a density represented by 2, while that of the second group is represented by 1.

The corresponding salts of soda appeared to fall into a nitrate and sulphate group also, which have the same relation to each other as the potash salts.

The relation of the salts of potash to those of soda, in times of equal diffusibility, appeared to be as the square root of 2 to the square root of 3; which gives the relation in density of their diffusion molecules, as 2 to 3. Hydrate of potash and sulphate of magnesia were less fully examined, but the first presented sensibly double the diffusibility of sulphate of potash, and four times the diffusibility of the sulphate of magnesia. If these times are all squared, the following remarkable ratios are obtained for the densities of the diffusion molecules of these different salts, each of which is the type of a class of salts, hydrate of potash 1, nitrate of potash 2, sulphate of potash 4, sulphate of magnesia 16, with nitrate of soda 3, and sulphate of soda 6.

In conclusion, it was observed, that it is these diffusion molecules of the salts which are concerned in solubility, and not the Daltonian atoms or equivalents of chemical combination; and the application was indicated of the knowledge of the diffusibilities of different substances to a proper study of endosmose.

2. *On the Occurrence of Formic Acid in Stinging Nettles;* by Dr. Gorup-Besanez, (Journ. für Prakt. Chem., xlviii, p. 191; Chem. Gaz., Jan. 1, 1850, p. 8.—Some time ago, F. Will showed, by microchemical and microscopical experiments, that the fluid in the hairs of the so-called procession-caterpillar (*Bombyx processionaria*), which causes an inflammation of the skin, as well as the liquid in the poisonous organs of some insects, is nothing else than formic acid. It became highly probable therefore that formic acid would also occur in the vegetable kingdom already formed; and the first class of plants which was thought of was that which, by means of stinging hairs of similar organs, produces an analogous effect to the sting of certain insects.

About a pound of the collected plants *Urtica urens* and *dioica*, was cut small and pressed, and submitted to distillation with about four times the quantity of water and a few drops of concentrated sulphuric acid.

The distillate was opalescent; a few oil-drops floated on its surface; it had a very offensive odor and a scarcely perceptible acid reaction. Mixed with carbonate of soda and evaporated to dryness in the water-bath, it furnished a brownish mass, a very small portion of which was deliquescent, the greater part consisting of the excess of carbonate of soda.

This mass was now very cautiously decomposed in a glass retort by the gradual addition of dilute sulphuric acid, when a distinctly acid distillate was obtained in the well-cooled receiver, which, neutralized with ammonia, gave all the reactions characteristic of formic acid. This experiment however did not appear to me to furnish a satisfactory proof of the presence of ready-formed formic acid in the plant, as formic acid can be *produced* from the most different organic substances by concentrated sulphuric acid; and it was possible that in the present case the formic acid might have been produced by decomposition towards the end of the operation.

Five pounds of stinging nettles were therefore distilled with a corresponding quantity of water without any addition of sulphuric acid, and a product obtained perfectly similar to the one above mentioned. It was neutralized with carbonate of soda, evaporated to dryness, the residue decomposed with dilute sulphuric acid, and the acid distillate digested with carbonate of lime, in order to avoid the excess of carbonic acid and excess of alkali, which rendered the reactions very indistinct, and filtered. The yellowish solution, concentrated in the water-bath, proved to be formiate of lime. It reduced salts of silver and mercury, gave with sulphuric acid the characteristic odor of formic acid, and with sulphuric acid and alcohol the still more characteristic smell of formic ether; oxalate of ammonia showed the presence of lime.

The amount of formic acid present in stinging nettles is certainly small; but this will not appear surprising, if we suppose that this acid is contained only in the stinging hairs, an assumption which is confirmed by the microscopic observations of Will and Lucas. When, for instance, solution of silver is added to the plant under the microscope, and a gentle heat applied, reduction always first occurs at the extremity of the stinging hair.

3. *Method of Preparing Theine;* by H. Heinsius, (Scheidk. Onderzoek., part v, p. 318; Chem. Gaz., March 15, 1850, p. 119.)—Stenhouse has recommended the preparation of theine from the extract of tea according to Mohr's method. The following is still more simple:—Old spoiled tea is placed in an iron pot, which is covered with filtering-paper, and the whole then covered with a cylindrical paper cap. The temperature is now cautiously and gradually raised, when a sufficient quantity of pure theine will be found to have collected on the paper.

4. *Test of the Presence of Sugar;* (L'Institut, No. 846.)—M. Maumené has mentioned a new reagent for ascertaining the presence of sugar in certain liquids. It is the bichlorid of tin. He announces his having observed, contrary to the statement of Liebig, that the chlorid acts on sugar even in the dry state. A temperature of only 100° C., is necessary for determining the reaction; and even in the cold it is produced after some time. In either case it forms a brown matter partly

soluble in water—caramel—which is brilliant black when dried. If the solution of sugar and bichlorid of tin are left to spontaneous evaporation at the ordinary temperature, the color soon becomes brown, and gradually deepens; and at the end of a year or eighteen months, it is changed to a uniform jelly of a lustrous black color. The same result takes place more rapidly on evaporating the solution by heat.

A test cloth of some woollen fabric, such as white merino, is to be applied for three or four minutes in a solution of one pint bichlorid of tin and two pints of water, the liquid allowed to drain off and the merino dried in a water-bath. For testing urine, a drop of the urine is to be put on a strip of the stuff, and held over a red hot charcoal or a candle, the presence of sugar is detected by a black stain.

Such is the delicacy of this test that ten drops of diabetic urine in about six cubic inches of water are capable of staining the merino a brownish black. Paper, linen, etc., being darkened by the chlorid cannot be used in place of wool. Ordinary urine and its constituents are not darkened by this test.

Bichlorid of mercury, chlorid of antimony, and other chlorids may be used in place of bichlorid of tin. They act by depriving the sugar of water and forming a caramel containing more carbon than ordinary caramel. Different sugars act in the same manner, and other analogous compounds, as ligneous fibre, cotton, paper, starch.

The author also suggests that the reaction of sugar and oxymuriate of tin may produce a brown paint that will become very important in the arts.

5. *On Iodine in fresh water Plants;* by Ad. Chatin, (L'Institut, 847.)—It is known that iodine has been detected by Müller in the cress, "*Nasturtium officinale*," a fresh water plant. But it does not appear to be understood that it occurs also in other plants growing in fresh waters. M. Chatin has examined numerous species, growing apart from any saline waters or sources, and has ascertained that it is of frequent occurrence. He examined with success the cress about Paris, and on extending his researches, found it in the following species.

	Iodine reaction.
Potamogeton crispum, stagnant waters of Gentilly,	traces.
" *pectinatum*, the Seine, near St. Cloud,	strong.
Arundo phragmites, marshes near Meudon, . .	traces.
" " marshes of St Quentin, . . .	strong.
Scirpus lacustris, Meudon, ...	traces.
" " St. Quentin, ...	strong.
Typha latifolia, St. Quentin, ...	"
Littorella lacustris, St Quentin, ...	"
Ranunculus aquatilis var. *heterophyllus*, pools of water of Satory, ...	traces.
Sagittaria sagittifolia, the Seine, near Neuilly, . .	strong.
Chara fœtida, turfy waters, Gentilly, ...	traces.
Conferva crispata, Seine, near St. Cloud, . . .	strong.
Pontinalis antipyretica, pond near Charenton, .	"
Nasturtium amphibium, Meudon, ...	traces.
Gratiola officinalis of commerce, ...	"
Menyanthes trifoliata, Meudon, ...	"

Olisma plantago, Meudon, ...	traces.
Stratiotes aloïdes, pond, Marly, ...	"
Acorus calamus of commerce, ...	"
Veronica beccabunga of commerce, ...	"
Phellandrium aquaticum of commerce, the fruit, .	"
Inula helenium of commerce, ...	"
Symphytum officinale of commerce, . . .	"
Rumex nemorosus, swamp on the Seine, near Neuilly, .	"
Potentilla anserina, " " "	"
Potentilla supina, marsh, St. Quentin, ...	"
Polygonum hydropiper of commerce, . . .	"

Mr. Chatin concludes :—

(1.) That those plants growing in running waters, or on the borders of large bodies of water which may be strongly agitated by the winds, contain more iodine than those of stagnant waters.

(2.) That the proportion is very small in species that are imperfectly submerged or only at intervals.

(3.) That the proportion of iodine appears to be independent of the nature of the plant or its place in the natural system.

The anti-scrofulous effects of the cress, Veronica, Phellandrium, &c., are explained by the presence of iodine.

6. *Analysis of certain gold-colored Bronze Antiquities found at Dowris, near Parsonstown, in the King's County, Ireland;* by Thos. L. Cooke, (Royal Irish Acad.; Chem. Gaz., No. 181, p. 176.)—The articles were part of a celt and a portion of a horn. The golden hue of these ancient bronzes suggested to some the idea of an admixture of zinc, an ingredient never yet observed in ancient bronzes. Such was not the fact, as may be seen from the analysis. The specific gravity of the celt was 8·767. The author details his process for separating copper, tin and lead, which it is unnecessary to repeat at present.

	The celt consisted of	The horn consisted of
Copper, .	85·232 . .	79·345
Tin, . .	13·112 . .	10·873
Lead, .	1·142 . .	9·115
Sulphur and carbon,	0·150 . .	——
Loss .	0·364=100. .	0·667=100.

The author notices the loss of copper in the vapor of nitrous acid, which he endeavored to avoid by passing the gas evolved in the action of the nitric acid on the alloy (contained in a small tubulated retort) through ammonia solution in small Woulf's bottles. He also notices the very fine qualities of toughness and of hardening acquired by this alloy when hammered, enabling it to cut not only flesh but even bone. It also takes a fine polish.

7. *Mannite, its atomic Weight and Compounds;* by Dr. W. Knop, (Pharm-Cent. Blatt., Nov., 1849, and Jan., 1850; Chem. Gaz., Mar. 1st, and April 15th, 1850.)—The author shows that the lead compounds of Favre are mere mixtures of basic lead salts with uncombined mannite, and therefore give no aid in determining the atomic weight. No combinations with acids seem to give a favorable result—but at last a compound of formic acid was obtained, curious for the manner of its formation. Crystallized *oxalic* acid and mannite fused together at 230°,

and then allowed to remain for several hours at about 212°, gave off large quantities of formic and carbonic acids. The nearly colorless syrup cooled to a transparent mass, soluble in alcohol of 0·83, in which mannite is nearly insoluble. The solution soon decomposes into formic acid and mannite. No true salts would be formed, but the compound was found to be one equiv. of formic acid and one of mannite estimated at $C_6\ H_7\ O_6$.

The author in his first paper points out the nitric mannite as most suitable for determining the atomic weight. In his second paper he discusses the analysis of Strecker, and agrees with him that it is sex-nitrated, its formula will then be $C_{12}\ H_8\ O_{12}+6NO_4$. The reduction by metals and by sulphuret of ammonium is so unlike that of ordinary nitric compounds, that Dr. Knop brings it as a proof, that nitric acid is contained as such, and not as hyponitric acid. It must be remembered however, that chemists do not generally consider that hyponitric acid as such, is contained in these compounds.

For the preparation of explosive mannite the process of Stenhouse is said to be the best. Mannite is to be dissolved in very cold fuming nitric acid in the proportion of half an ounce to 2 ounces of acid—cold sulphuric acid is then added, until white grains cease to deposite. The mixture to be poured into a large quantity of water—the crude product dissolved in boiling alcohol and again poured into a quantity of cold water—after some time the mannite cakes and may be filtered and washed. G. C. SCHAEFFER.

8. *On Dulcose, a Homologue of Grape Sugar;* by A. LAURENT, (Comptes Rendus, Jan., 1850.)—This is a new sugar from Madagascar, of uncertain origin. After fusion its formula is $C_{28}\ H_{28}\ O_{24}$, differing from grape sugar by $C_4\ H_4$. When dissolved, it takes up 3 equivalents of water. A crystallized compound has been obtained with 4 equiv. of water.

By the action of nitric acid on dulcose, mucic acid is formed. This sugar, which has but a slightly sweetish taste, is said to be without action on polarized light and to be incapable of alcoholic fermentation.

According to Gerhardt, the erythro-mannite obtained by Dr. Stenhouse, from certain lichen, may, on the supposition of a slight error in the analysis, be considered as a homologue of mannite, which it resembles in its properties. This view however, would make the compounds with nitric acid, sex-nitric instead of quinqui-nitric, as stated by Dr. Stenhouse. Since the discovery of Strecker that nitric mannite is in fact, sex-nitric, this supposition becomes highly probable.

The discovery of a homologue of sugar opens a new field of enquiry, while it gives additional confirmation, if any were needed, to the views of MM. Gerhardt and Laurent on this subject. It is possible that some of the known varieties of sugar, may, on more careful examination, prove additional examples of homologues. G. C. S.

Among other recent investigations in connection with the subject of sugar we may mention the following:—

9. *Sugar in the Liver.*—BEQUARD and BARRESWILL have demonstrated the presence of sugar in the liver of animals, even where saccharine or amylaceous food forms no part of their diet. It is found in no other organ. G. C. S.

10. *Sugars of Honey.*—M. Soubeiran has demonstrated the presence of three sugars in honey. 1st, ordinary glucose. 2nd, a sugar with right-handed rotation, and alterable by acids. 3d, a sugar with left handed rotation to an amount nearly double that of the last. This is called liquid sugar of honey; it is uncrystallizable, may be converted into transparent solid sugar which melts with great ease; it is easily acted upon and destroyed by alkalies. This sugar has been kept for years without change or crystallization. G. C. S.

11. *Sugar in the flowers of Rhododendron ponticum.*—Dr. B. Slhamer has proved that this is pure cane sugar. The honey collected by bees from these flowers is said by Tournefort to possess narcotic properties. No such substance however could be obtained by the author, and not a trace of nitrogen could be detected. G. C. S.

12. *Sugar of milk in the Cotyledons of seeds.*—H. Braconnot states that he has determined the presence of sugar in the cotyledons of the acorn and other seeds, and also of the other constituents of milk. These substances being found in the egg, we have a remarkable analogy in the instrument provided for the infant animal and plant. G. C. S.

13. *Production of Sugar in the urine by wounding the brain.*—M. Bernard has found that by wounding a certain portion of the floor of the fourth ventricle, the urine in an hour and a half or two hours, resembles that of diabetes, becoming clear and abundant with a very large quantity of sugar in solution. The blood also contains a large quantity of sugar. The portion of the fourth ventricle in which a wound produces this effect, is confined to a small space a little above the origin of the 8th pair of nerves.

The results of this curious discovery promise to throw new light upon the process of digestion. G. C. S.

II. Mineralogy and Geology.

1. *Notice of Trilobites in the Cabinet of Dr. Julius S. Taylor*, (in a letter addressed by Dr. Taylor to the Editors.)—Believing that any information upon the subject of trilobites is at all times acceptable to the scientific world, I venture to tell you of a remarkable portion of one found a few days past by me. It is an "Isotelus megistos," and I think presents the most remarkable evidence of their gigantic size, of any specimen now extant. It was found in our blue limestone strata, and presents the tail or "post-abdomen," and seven of the segments across the back, nearly entire. Its width is 9¼ inches, and its length a little exceeds this. Thus you perceive, that if we had the other segment and the head, we should have one entire that would measure at least 18½ inches in length and 9¼ in breadth!

I see that Mr. Barrande of Prague is of the opinion that trilobites change greatly according to age. Of the correctness of that opinion I should have some doubts, as I have a variety of the Isotelus megistos from half an inch, up to the gigantic one above mentioned, and I find no difference in them either in proportions or segments, each having eight, and each portion being equal in length. I have also numerous specimens of the Calymene senaria, from the size of the smallest pea up to the size of one inch in width, and in them I find no difference. And of several other varieties, I have many portions of different ages, all of which have exact resemblance. Of the Calymene Blumenbachii, I have them from one inch to three and a half in length, more or less perfect, and in them I find no change in appearance. Thus it would appear that in our varieties, at least, we have no metamorphosis of the earliest of the moving animals. However, I have not seen his work, and the notice of it may be too short to give a correct idea of what he means.

Carrolton, Montgomery Co., Ohio, April 18, 1850.

2. *Observations on the Mica Family;* by J. D. Dana.—The Mica family includes many distinct compounds formerly confounded, and the old names have on this account become inapplicable. The species *Oblique* or *Potash mica* is not the only oblique or potash mica, and some other appellation for this species has become necessary; and the writer has therefore adopted in his system of Mineralogy the name *Muscovite* from its common appellation, Muscovy glass. The other species have similar distinctive appellations, as *Biotite*, for hexagonal mica, *Phlogopite* for a trimetric mica, *Margarodite* for a second potash or oblique mica, etc.; and it is probable that several other species besides those now known, are yet to be distinguished.

The following observations on the different kinds of mica are mainly from the volume just referred to.

Rammelsberg in his 4th Supplement (p. 76) has calculated anew the several analyses of muscovite, and lays down the formulas of the varieties. But by some oversight, he makes the ratio for the oxygen of the protoxyds, peroxyds and silica, 1 : 12 : 15 equal to $\frac{3}{4}$: 9 : 12, as if 4 : 5 equals 3 : 4; and the formula deduced is that based on the former ratio. As the point is of some importance, we give here the ratios from his calculations, making the oxygen of the alumina 12, for better comparison with that of the silica as well as the protoxyds. A slight variation in the protoxyds, whether from impurity or not, varies largely the ratio to the other ingredients: while the ratio between the peroxyds and silica, since it is much nearer equality, is not liable to such fluctuations. The results are as follows:

	$\dot{R}$	$\dddot{R}$	$\dddot{Si}$				
Utö, Rose,	1·63 :	18·62[a]:	24·68	= 1·05 :	12 :	15·91.
Broddbo, Rose,	1·42 :	17·76 :	23·95	= 0·96 :	12 :	16·18.
Fahlun, Rose,	1·39 :	18·56 :	24·01	= 0·90 :	12 :	15·52.
Kimito, Rose,	1·56 :	18·54 :	24·09	= 1·01 :	12 :	15·59.
Ochotsk, Rose,	1·44 :	17·89 :	24·52	= 0·97 :	12 :	16·45.
Zsidovácz, Kussin,	1·71 :	17·93 :	24·97	= 1·14 :	12 :	16·71.
Mean, excluding the last,				0·98 :	12 :	15·93
Mean, including the last,				1·005 :	12 :	16·06

a Corrected.

The mean therefore of the five analyses from which Rammelsberg deduces his ratio is very closely 1 : 12 : 16, and if Kussin's analysis be added, it gives still more nearly this ratio; moreover, the separate analyses correspond more nearly with 1 : 12 : 16 than 1 : 12 : 15. Rose's formula $\dot{K}\dddot{Si}+4\dddot{Al}\dddot{Si}$, adopted by this author, is not therefore sustained by the analyses.

Neither does Kussin's analysis correspond to the ratio $1\frac{1}{3} : 12 : 15$, whence Rammelsberg derives incorrectly (but correctly from $1 : 9 : 12$) the formula $\dot{K}\dddot{Si} + 3\ddot{\bar{Al}}\dddot{Si}$, but more nearly to $1\frac{1}{8} : 12 : 16\frac{1}{2}$, though probably essentially $1 : 12 : 16$, like the preceding.

A mica from Unionville, Pa., occurring in scaly granular masses, afforded J. D. Darrack, in the Laboratory of J. C. Booth, (Min., **1850**, p. **357**,)

$\dddot{Si}$ 46·75, $\ddot{\bar{Al}}$ 39·20, $\ddot{\bar{Fe}}$ *trace*, $\dot{Mg}$ 1·02, $\dot{K}$ 6·56, $\dot{Ca}$ 0·39, $\dot{H}$ 4·90 = 98·82.

This gives for the ratio 1·61 : 18·32 : 24·29 = 1·05 : 12 : 15·91 or nearly 1 : 12 : 16.

The Abborforss mica (Svanberg) and Fuchsite (Schafhäutl) afford,

Abborforss, 3·27 : 15·06 : 20·61 = 2·60 : 12 : 16·42 = $2\frac{2}{3}$: 12 : 16.
Fuchsite, 2·33 : 17·88 : 24·91 = 1·53 : 12 : 16·32 = $1\frac{1}{2}$: 12 : 16.

The first corresponds to the ratio 2 : 9 : 12, and formula $\dot{R}^2\dddot{Si} + 3\ddot{\bar{Al}}\dddot{Si}$. Taking the Fuchsite at $1\frac{1}{3}$: 12 : 16, it gives the formula $\dot{R}\dddot{Si} + 3\ddot{\bar{Al}}\dddot{Si}$.

Margarodite, *Schafhäutl.*—This pearly white biaxial mica and others allied have afforded the following analytical results.

Analyses : 1, Schafhäutl ; 2, Delesse, (Ann. d. Mines, [4], xvi, 202, 1849) ; 3, 4, Brewer, of the Yale Laboratory, (Min., 1850, 359) ; 5, Rammelsberg, (4th Supp., 75) :

	$\dddot{Si}$	$\ddot{\bar{Al}}$	$\ddot{\bar{Fe}}$	$\ddot{\bar{Mn}}$	$\dot{Mg}$	$\dot{Na}$	$\dot{K}$	$\dot{H}$
1. Zillerthal,	47·05	34·90	1·50	—	1·95	4·07	7·96	1·45=98·88, Schafhäutl.
2. St. Etienne,	46·23	33·08	3·48	*trace*	2·10	1·45	8·87	4·12, F *trace*=99·33, D.
3. Monroe, Ct.,	49·96	32·85	*trace*	—	1·08	2·89	7·91	4·46[a], Cl 0·14=99·29, B.
4. " "	49·97	32·60	—	—	1·41	*undetermined*		4·46[a], Brewer.
5. Loc. doubtful,	47·84	32·36	3·06	—	1·28	1·55	10·25	2·43, $\dot{Ca}$ 0·29=99·06, R.

[a] A mean of 4 determinations.

The ratios are as follows,

1,	3·17 : 16·35 : 24·45 = 2·33 : 12 : 17·95 = 1·16 : 6 : 8·97
2,	2·65 : 16·46 : 24·02 = 1·98 : 12 : 17·51 = 0·99 : 6 : 8·76
3, 4,	2·63 : 15·35 : 25·96 = 2·05 : 12 : 20·29 = 1·02 : 6 : 10·14
5,	2·71 : 16·03 : 24·86 = 2·03 : 12 : 18·61 = 1·01 : 6 : 9·30

Rammelsberg and Delesse agree nearly in giving the ratio 1 : 6 : 9. Schafhäutl's has the protoxyds a little in excess, and Brewer obtained an excess of silica. But it is probable that all come under the same formula, $\dot{R}\dddot{Si} + 2\ddot{\bar{R}}\dddot{Si}$. The mica of Monroe, Ct., occurs with fluor spar and topaz in large scales, aggregated into wedge shapes. G=2·79—2·81. The St. Etienne mica has G=2·817 ; Rammelsberg's 2·831 ; Schafhäutl's 2·872. In this species the oxygen ratio of the peroxyds and silica is 2 : 3, while it is 3 : 4 in muscovite. The *Gilbertite* of Thomson appears to be altered muscovite, or margarodite.*

* The mineral *Gilbertite* has the aspect of a pearly white mica, occurring in scales often radiately aggregated. The following are the analyses given ; 1, Lehunt, of true Gilbertite from Cornwall, (Thom. Min., i, 235) ; 2, Thomson, Cornwall, different locality.

	$\dddot{Si}$	$\ddot{\bar{Al}}$	$\dot{Mg}$	$\dot{Ca}$	$\dot{Fe}$	$\dot{Na}$	$\dot{H}$
1.	45·15	40·11	1·90	4·17	2·43	—	4·25=98·01, Lehunt.
2.	47·80	32·62	1·60	—	5·18	9·23	4·00=100·43, Thomson.

The first analysis gives for the oxygen of the protoxyds, peroxyds and silica, 2·48 : 18·75 : 23·46, whence the ratio 1·59 : 12 : 15·01. Dufrenoy deduces 2 : 12 : 16. Like

Phlogopite, (or Rhombic Mica) according to Meitzendorff's analyses has the ratio $1 : \frac{2}{3} : 1\frac{2}{3}$ and formula $3\dot{R}^3\,\dddot{Si}+2\bar{R}\,\dddot{Si}$, which is the formula of idocrase. Rammelsberg places here a mica from Sala analyzed by Svanberg. The species includes brown or yellowish coppery mica from Natural Bridge, Pope's Mills, Gouverneur, and Vrooman's Lake in northern New York; a black mica from Moriah, Essex Co.; a silvery mica from Edwards, N. Y.; a transparent mica from Pope's Mills, a variety of the brown. The optical character of these micas has been observed by the writer in connection with B. Silliman, Jr.

Biotite (Hexagonal or Magnesia Mica) has the ratio 1 : 1 : 2, and formula $\dot{R}^3\dddot{Si}+(\ddot{\bar{A}l}, \ddot{\bar{F}e})\,\dddot{Si}$, which is essentially the formula of garnet. A chrome magnesia mica gives the ratio $\frac{1}{2} : 1 : 2$, but whether uniaxial or not is not certain. A Pargas biotite gives, $\frac{2}{3} : 1 : 2$, and the formula $\dot{R}^2\,\dddot{Si}+\bar{R}\,\dddot{Si}$ (Rammelsberg); and a Rosendal, $1 : 1 : 2\frac{1}{2}$, or the formula $3\dot{R}^2\,\dddot{Si}+2\bar{R}\,\dddot{Si}$.

Lepidolite or Lithia mica.—The Lithia micas have been the subject of study by Rammelsberg, and he concludes that in them as well as topaz, the fluorine replaces oxygen, or is in the same state of composition as the oxygen. There are several distinct chemical compounds included, and it is not definitely known that they are all biaxial. Brewster observes that in one lithia mica part of the specimen was biaxial and part uniaxial. The ratios given by Rammelsberg are,

a. 1 : 3 : $6=\dot{R}\,\dddot{Si}+\bar{R}\,\dddot{Si}$—mica of Ural, Chursdorf, Utö, Rozena, Altenberg (Stein).
b. 2 : 9 : $15=2\dot{R}\,\dddot{Si}+3\bar{R}\,\dddot{Si}$—mica of Zinnwald.
c. 1 : 2 : $5=3\dot{R}\,\dddot{Si}+2\bar{R}\,\dddot{Si}$—mica of Juschakowa.
d. 1 : 6 : $7=\dot{R}^3\,\dddot{Si}+6\bar{R}\,\dddot{Si}$—mica of Altenberg according to Turner.

In the Ural, Chursdorf, Utö, and Rozena micas, the fluorine is to the oxygen as 1 : 20; in the Altenberg, (Stein), 1 : 60; in the Zinnwald, 1 : 14, or 1 : 11; in the Juschakowa, 1 : 8; in the Altenberg, (Turner), 1 : 25.

The Ural mica (Turner) affords then the formula,—writing for $\dot{R}\,\dddot{Si}+\bar{R}\,\dddot{Si}$, $(R\,O+Si\,O^3)+(R^2\,O^3+Si\,O^3)$,

$$20[(R\,O+Si\,O^3)+(R^2\,O^3+Si\,O^3)]+[(R\,F+Si\,F^3)+(R^2\,F^3+Si\,F^3)];$$

and in the same way the other formulas may be written out. Making O and F isomorphous, this formula becomes,

$$[R\,(O, F)+Si\,(O, F)^3]+[R^2(O, F)^3+Si(O, F)^3].$$

Emerylite and Euphyllite.—These species have undergone recent re-examinations, and the species now stand as follows.

Emerylite, *J. Lawrence Smith.* Corundellite and Clingmanite, *Silliman*, Jr.—Foliated like mica; folia easily separable. Either in coarse plates or in masses consisting of aggregated spangles.

H.=3·5—4·5. G.—2·995, Silliman, Jr. Lustre pearly. Color white. Translucent, to nearly opaque. Folia brittle or slightly flexible.

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muscovite the protoxyds bear a small proportion to the peroxyds, and the ratio is very near that derived from some analyses of this species. The specimens look like a slightly altered mica. Including the water, the oxygen ratio is 1·59 : 12 : 15·01 : 2·41. The oxygen ratio for the second analysis is 4·16 : 15·24 : 24·84=1·64 : 6 : 9·78, approaching margarodite, excepting in the larger amount of protoxyds.

The relation of the species to the micas is evident; yet other analyses are desirable.

Composition.—$\dot{R}^3\dddot{Si}+3\ddot{\overline{Al}}^2\dddot{Si}[+3\dot{H}]$=Silica 30·58, alumina 50·99, lime 13·96, water 4·47. Analyses: 1, J. Lawrence Smith, (Am. J. Sci. [2], vii, 285); 2, 3, 4, Wm. J. Craw, in the Yale Laboratory, (Silliman, Jr., Am. J. Sci. [2], viii, 379); 5, 6, ibid, (private communication); 7, C. Hartshorne, in Laboratory of J. C. Booth, (private communication from Prof. J. C. Booth); 8, Silliman, Jr., (private communication):

	$\dddot{Si}$	$\ddot{\overline{Al}}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{Na}$	$\dot{K}$	$\dot{H}$	
1. Asia Minor,	30·	50·	13·	—	—	—	—	$\ddot{\overline{Fe}}$, $\dot{Mn}$, $\dot{K}$ 3, $\ddot{\overline{Zr}}$ 4=100, S.
2. Village Green,	32·31	49·24	10·66	0·30	2·21	(loss)	5·27	=100, Craw.
3. "	31·06	51·50	9·24	0·28	2·97	"	5·27	=100, Craw.
4. "	31·26	51·60	10·15	0·50	1·22	"	4·27	=100, Craw.
5. "	30·18	51·40	10·87	0·92	2·23	0·54	4·52	=100, Craw.
6. Unionville,	29·99	50·57	11·31	0·62	1·62	0·85	5·14	=100, Craw.
7. "	32·15	54·28	11·36	0·05	*undetermined*		0·50	,$\ddot{\overline{Fe}}$ *trace*, Hartshorne.
8. North Carolina,	29·17	48·40	9·87	1·24	6·15	—	3·99	,HF 2·03=100·80, S. Jr.

Analyses 6, 7 are of the *Corundellite*, and 8, of the *Clingmanite*. The following are the oxygen ratios for the protoxyds, peroxyds, and silica:

	$\dot{R}$		$\ddot{\overline{R}}$		$\dddot{Si}$						
1.	4·20	:	24·42	:	15·59	=	1·08	:	6·26	:	4
2.	3·87	:	23·01	:	16·79	=	0·99	:	5·48	:	4
3.	3·43	:	23·93	:	16·14	=	0·85	:	5·93	:	4
4.	3·40	:	24·12	:	16·24	=	0·85	:	5·94	:	4
5.	4·03	:	24·02	:	15·68	=	1·03	:	6·13	:	4
6.	4·01	:	23·64	:	15·53	=	1·17	:	6·08	:	4
7.	3·65[a]	:	25·37	:	16·71	=	0·92	:	6·08	:	4
8.	3·99	:	23·92	:	15·84	=	1·08	:	6·04	:	4
					Mean		=1·00	:	5·99	:	4

[a] Supposing the alkalies equivalent to the loss.

The mean result is quite closely 1 : 6 : 4, and the same is afforded by several of the particular results. As Dr. Smith found no water, and Hartshorne only 0·5 per cent., the species is primarily anhydrous. Including the water found by Craw in his specimens, the ratio is 1 : 6 : 4 : 1.

Commonly yields water in a matrass, with sometimes a trace of fluorine. B.B. exfoliates and emits a bright light and finally fuses on the edges.

Detected by Dr. J. Lawrence Smith, with the corundum of Magnesia, Asia Minor, and also at Naxia and Niconia. Also found with corundum at Village Green, Delaware Co., Pa.; at Unionville, Chester Co., Pa., (Corundellite); at the Corundum locality in Buncombe Co., North Carolina, (Clingmanite).

Euphyllite, *Silliman*, Jr.—Structure as in common mica, but laminæ not as easily separable. Biaxial; angle between the optical axes 71½°, Silliman, Jr.

H.=3·5—4·5. G.=2·963—3·008. Lustre of cleavage surface bright pearly, inclining to adamantine. Color white to colorless; sides faint grayish sea-green or whitish. Transparent to translucent; at times opaque or nearly so. Laminæ rather brittle.

Composition.—Analyses: 1, 2, H. Erni, in the Yale Laboratory, (private communication); 3, 4, T. Garrett, in J. C. Booth's Laboratory, Philadelphia, (private communication):

	$\dddot{Si}$	$\dddot{\overline{Al}}$	$\dddot{\overline{Fe}}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{Na}$	$\dot{K}$	$\dot{H}$	
1. Unionville,	43·69	44·69	——	3·98	0·75	0·98	0·82	5·60	=100·41, Erni.
2. "	43·45	44·98	——	4·34	0·69	*undetermined*		4·97	, Erni.
3. "	45·93	48·23	0·60	3·53	2·44	——	*trace*	——	=100·73, Garrett.
4. " (dif. loc.)	45·33	46·47		2·36	*trace*	——	*undet.*	——	, Garrett.

The following are the oxygen ratios for the analyses:

1.	1·80	20·88	22·65	=	0·95	11·06	12
2.	1·88	21·02	22·57	=	1·00	11·18	12
3.	1·96	22·82	23·87	=	0·98	11·47	12
4.	——	21·72	23·55	=	——	11·07	12
			Mean,		0·98	11·19	12

The ratio **1 : 11 : 12** appears therefore to be that authorized by the analyses. As no water was found in the 3d and 4th analyses, the species is essentially anhydrous. Including the water found by Erni, the ratio becomes **1 : 11 : 12 : 3**, and the formula $\dot{R}^3\dddot{Si}+11\dddot{\overline{Al}}\dddot{Si}+9\dot{H}$.

It is still possible that **1 : 12 : 12** will prove to be the true ratio; and in that case the ratios for margarite, emerylite, euphyllite, and muscovite, have an obvious simple relation, they being respectively **1 : 12 : 8 : 2 : 12 : 8 ; 1 : 12 : 12 ; 1 : 12 : 16.**

In a matrass, often yields water. B.B. exfoliates, emits a strong light, and in the forceps fuses on the edges. Gives traces of fluorine, but none of lithia.

Occurs associated with tourmaline and corundum at Unionville, Delaware Co., Pennsylvania. The impression of the crystals of tourmaline on the lateral surface of the mica, leaves a very smooth, hard looking surface. Also in the same vicinity in aggregated laminæ, or scales; this variety afforded the analysis No. 4 above.

Recapitulation.—The following are the oxygen ratios observed among the micas, and the formulas corresponding:—

(1.) Micas having atomically as much alumina as silica, or a larger amount. These micas are harder than those of the following division, and brittle or but slightly elastic.

1. *Margarite,*	1 : 12 : 8	=	1	12	8	$\dot{R}^3\dddot{Si}^2+6\dddot{\overline{Al}}^2\dddot{Si}$
2. *Emerylite,*	2 : 12 : 8	=	1	6	4	$\dot{R}^3\dddot{Si}+3\dddot{\overline{Al}}^2\dddot{Si}$.
3. *Euphyllite,*	1 : 12 (?11) : 12					? $\dot{R}^3\dddot{Si}+11\dddot{\overline{R}}\dddot{Si}$.

(2.) Micas having atomically less alumina than silica

1. *Muscovite, a.*		1 : 12 : 16	=	1	12	16	$3\dot{R}, 12\dddot{\overline{Al}}, 16\dddot{Si}$.
	b. Fuchsite,	$1\frac{1}{2}$: 12 : 16	=	1	8	$10\frac{2}{3}$	
	c. Abborforss,	$2\frac{2}{3}$: 12 : 16	=	1	$4\frac{1}{2}$	6	$\dot{R}^2\dddot{Si}+3\dddot{\overline{Al}}\dddot{Si}$.
2. *Margarodite,*		2 : 12 : 18	=	1	6	9	$\dot{R}\dddot{Si}+2\dddot{\overline{Al}}\dddot{Si}$.
3. *Lepidolite, a.*		4 : 12 : 24	=	1	3	6	$\dot{R}\dddot{Si}+\dddot{\overline{R}}\dddot{Si}$.
	b.	$2\frac{2}{3}$: 12 : 20	=	1	$4\frac{1}{2}$	$7\frac{1}{2}$	$2\dot{R}\dddot{Si}+3\dddot{\overline{R}}\dddot{Si}$.
	c.	6 : 12 : 30	=	1	2	5	$3\dot{R}\dddot{Si}+2\dddot{\overline{R}}\dddot{Si}$.
	d.	2 : 12 : 14	=	1	6	7	$\dot{R}^3\dddot{Si}+6\dddot{\overline{R}}\dddot{Si}$.
4. *Phlogopite,*		18 : 12 : 30	=	1	$\frac{2}{3}$	$1\frac{2}{3}$	$3\dot{R}^3\dddot{Si}+2\dddot{\overline{R}}\dddot{Si}$.
5. *Biotite, a,*		12 : 12 : 24	=	1	1	2	$\dot{R}^3\dddot{Si}+\dddot{\overline{R}}\dddot{Si}$.
	b, Pargas,	8 : 12 : 24	=	1	$1\frac{1}{2}$	3	$\dot{R}^2\dddot{Si}+\dddot{\overline{R}}\dddot{Si}$.
	c, Chrome,	6 : 12 : 24	=	1	2	4	$\dot{R}^3\dddot{Si}^2+2\dddot{\overline{R}}\dddot{Si}$.
	d. Rosendal,	(?) 12 : 12 : 30	=	1	1	$2\frac{1}{2}$	$3\dot{R}^2\dddot{Si}+2\dddot{\overline{R}}\dddot{Si}$.
6. Chamouni, Delesse,		(?) $4\frac{4}{5}$: 12 : $19\frac{1}{5}$	=	1	$2\frac{1}{2}$	4	$3\dot{R}^2\dddot{Si}+5\dddot{\overline{R}}\dddot{Si}$.
7. *Lepidomelane,*		4 : 12 : 16	=	1	3	4	$\dot{R}^3\dddot{Si}+3\dddot{\overline{R}}\dddot{Si}$.

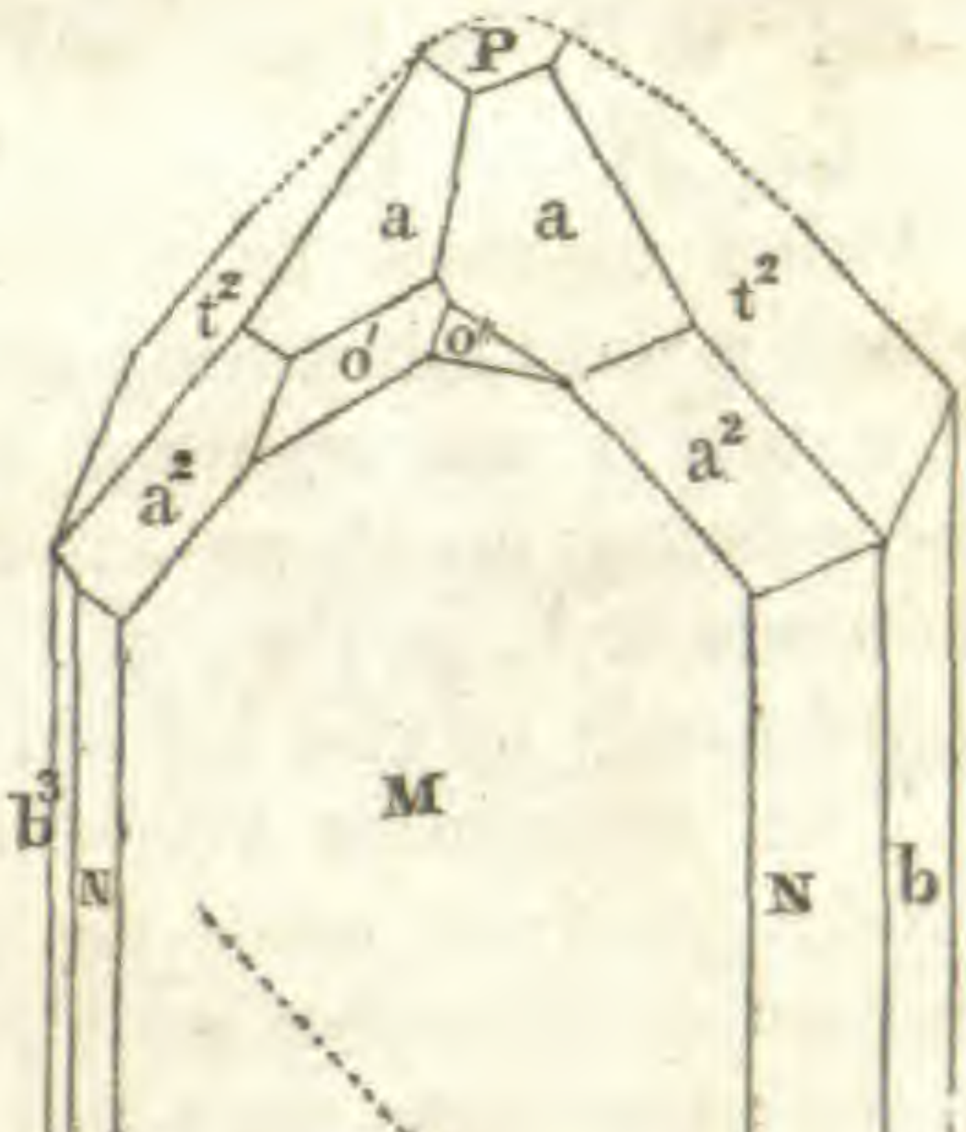
The mineral *Diphanite*, resembles margarite and euphyllite, according to Breithaupt. It has the ratio $1 : 4\frac{1}{2} : 3\frac{3}{4}$ or $2\frac{2}{3} : 12 : 10$.

We have not written a formula for the muscovite. It is evident from a survey of the compounds, that there must be some more satisfactory mode of exhibiting the relations of substances, than by the formulas often made out. They frequently make seeming diversity instead of showing relationships, and tend to mislead the eye not thoroughly acquainted with the wide variation of formula consequent on a slight change of ratio.

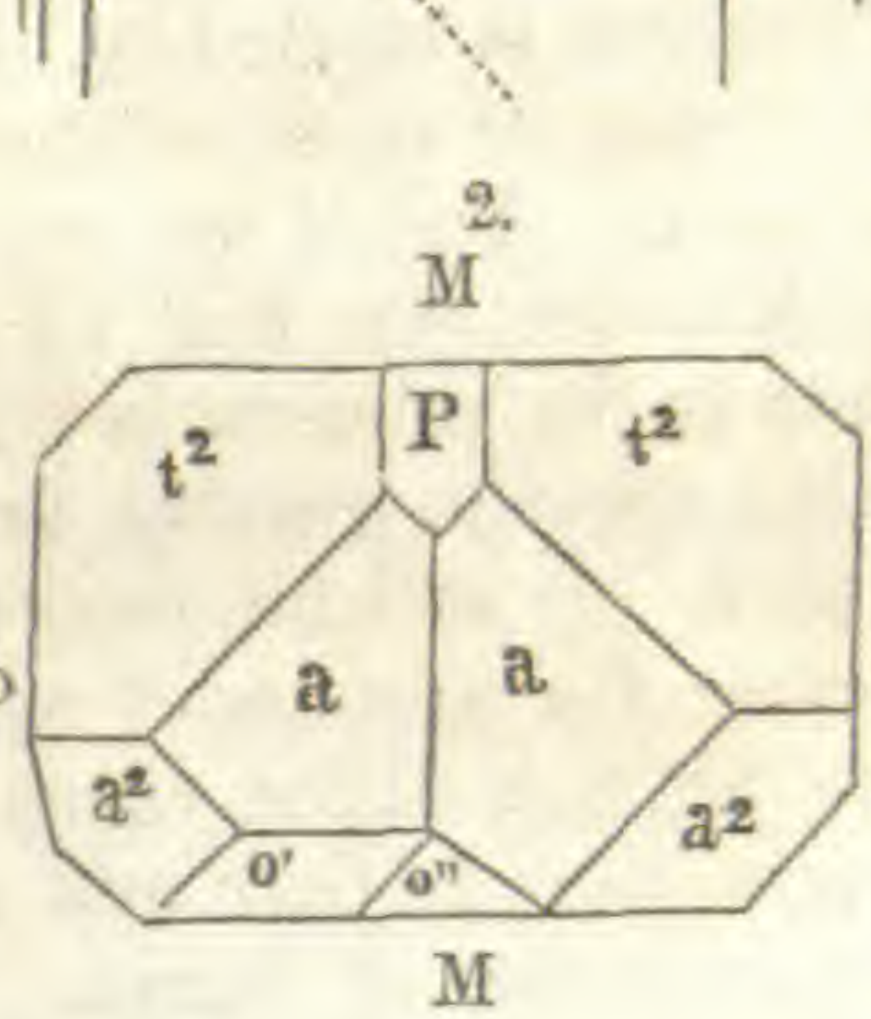
The amount of water which analyses afford varies from 0 to 5 per cent.; and as it exists variously or not at all, in micas in which the oxygen ratio is the same, there is reason for believing it the result of a partial alteration of the mica, like that which takes place in iolite. The hydrous varieties are commonly more or less wanting in transparency.

3. *Spodumene*, (Dana's Min., 1850, p. 693.)—The annexed figure of a crystal of Spodumene, from Norwich, Mass., has been made by the author from a specimen belonging to Mr. Charles Hartwell. Mr. Hartwell had measured the angles, and presented a notice of the crystal to the meeting of the American Scientific Association at Cambridge, in 1849. On examination, the author finds that the crystal is monoclinic, and further that Spodumene is actually isomorphous with Pyroxene. The annexed figure represents the crystal as it is, with a vertical plane b^3 on the right and the corresponding one on the left wanting, and also with the two planes o'' and o' unmated; moreover the back planes of the summit are absent, as the crystal is not entire or is broken on that part. The edge of intersection of o' and o'' is uncertain, as the surface has been abraded. But the rest of the faces figured are smooth, though not bright. The direction of the intersections as seen in a vertical view are shown in figure 2; the edge between o' and a^2 has a parallel direction with that between a and t^2, but that between o' and o'' is doubtful. The crystal is $2\frac{1}{4}$ inches long, $1\frac{5}{8}$ inches wide, and 1 inch thick. The color is grayish, with a tinge of green. Besides the usual cleavages—the orthodiagonal perfect, and the prismatic scarcely less so—there are distinct traces of cleavage parallel to each orthodiagonal edge of the pyramid a—a direction pointed out on the figure by the dotted line on plane M. The surface of plane M moreover is very finely crossed by lines corresponding to this cleavage direction. The crystal is bisected along its orthodiagonal section, and on holding one of the halves up to the light it is seen to be translucent, and marked throughout with the same lines as on the surface.

1.



2.



The following are the angles, as observed with the common goniometer by Mr. Hartwell and by the author.

	Hartwell.	Author.
N : N	87°	87°
N : N (lateral angle)	93°	93°
N : M	133°	133° 30′
N : b	137°	136° 30′
N : b^3	153°	154°
M : b^3		107°
M : t^2		106°—107°
M : P		69° 40′
M : a	100° 30′	100° 30′
M : a^2	116°	116°
M : o′		127°
M : o″		140°
t^2 : t^2 (over P)		79° 30′
t^2 : b	140°	139° 45′
a^2 : t^2		140° 30′
a : a (front)	117°	117°

The crystallographic expressions for the planes are indicated in the lettering, except for the planes o′, o″, which are not determinable with certainty. In Naumann's system of notation they are :—

0P	∞P	∞P∞	∞P′∞	∞P′3	P	2P	2P′∞	3P3?	mPn
P	N	M	b	b^3	a	a^2	t^2	o′	o″

Other crystals have since been obtained by Mr. Hartwell, one of which is six inches long and nearly half this in breadth. The planes P, o′ and o″ are wanting, and the back edge of the plane t^2 is straight, there being no plane —m P. The complete description of the crystal is hence ∞P, ∞P∞, ∞P′∞, ∞P′3, P, 2P, 2P′∞. The plane ∞P′3 is seen only on the left side of plane M, as in the figure.

The specimens afforded B. Silliman, Jr., a strong lithia reaction.* These facts set aside an idea adopted by Dufrénoy and Pelouze, and becoming somewhat prevalent, that spodumene is a lithia feldspar; and at the same time they fail to sustain the suggestion of the writer, that spodumene is related to andalusite. The formula of spodumene best expressing its composition as ascertained by analysis, is $\dot{R}^3 \ddot{Si}^2 + 4\dddot{Al}\ddot{Si}^2$ (Kobell). And if we consider the protoxyds and peroxyds as replacing one another, we find 15 of oxygen in the oxyds to 30 in the silica, which is the relation in pyroxene. With the above formula, and 3·17 for the specific gravity, the A atomic volume is 2852; the B, 168; the C, 43·2. It will be observed that 43·2 is only as much smaller than the number for pyroxene† as it is larger than that for andalusite. Moreover 2852 is about *three times* the corresponding number for hornblende, *four* to *four and a half* times that of pyroxene and its varieties, and *twice* that of borax.

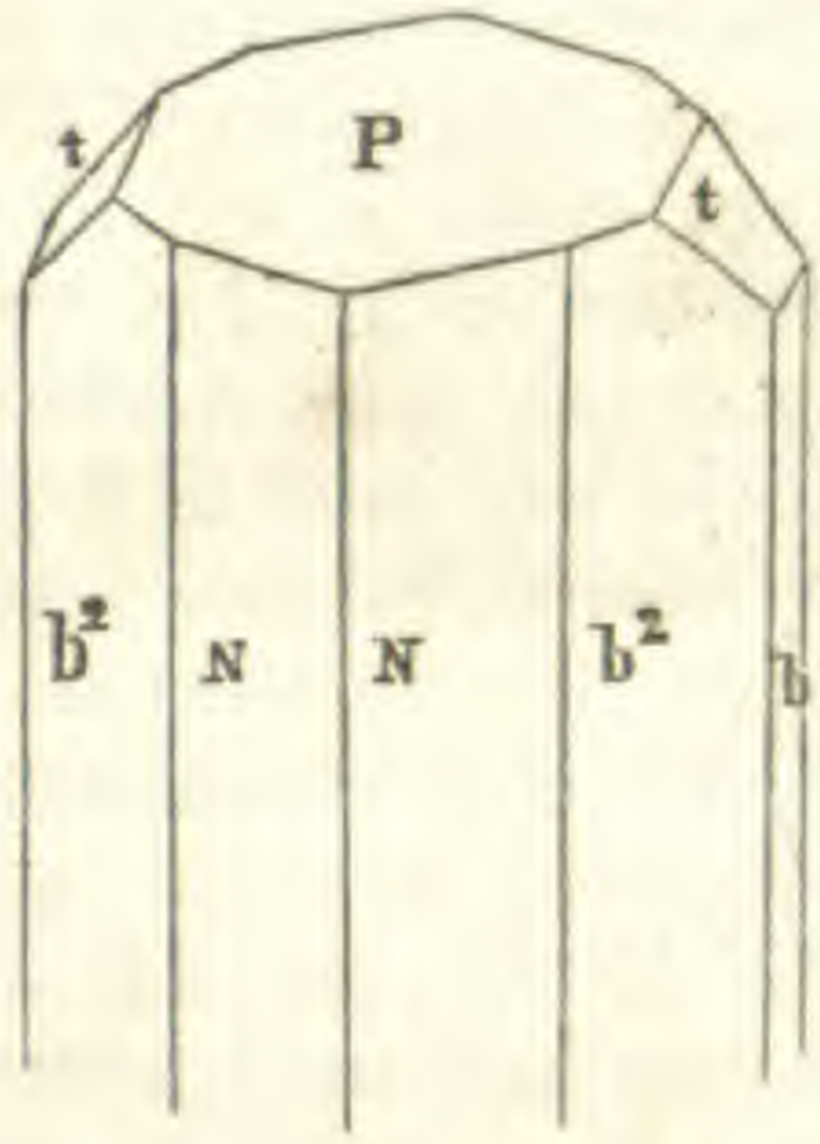
---

* The spodumene of Conway, Mass., in the same range, was tested for lithia successfully by G. T. Bowen, in 1824, and analyzed so far as to obtain $\ddot{Si}$ 65·3, $\dddot{Al}$ 24·5, which agrees with the usual analyses of the species.—*This Journal*, 1824, *viii*, 120, 121.—The specific gravity of the Norwich spodumene according to a recent determination by G. J. Brush, is 3·18.

† See this Journal, last volume, pp. 241, and 429.

4. *Anhydrous Prehnite.*—The Anhydrous Prehnite of J. D. Whitney, (Bost. Jour. Nat. Hist., v, 487,) has been examined by Dr. C. T. Jackson, and found to contain the same per-centage of water as prehnite. In his trials he found 4·7 and 4·15 per cent. of water, and subsequently, 4·18, 4·45, 4·50, 4·41 per cent. Other specimens examined by Mr. G. J. Brush in the Yale Laboratory, afforded 4·913, 4·817, 4·864 per cent. This mineral appears therefore to be identical with ordinary prehnite. The existence of water in this variety was first ascertained in a blowpipe experiment by Mr. J. E. Teschemacher of Boston.

5. *On a new crystalline form of Staurotide, and Isomorphism of Staurotide with Andalusite and Topaz*; by J. D. Dana.—The accompanying figure represents crystals of staurotide from the spodumene locality, Norwich, Mass., which were obtained by Mr. C. Hartwell. The faces are smooth excepting sometimes t and P. The angles obtained with the common goniometer are as follows:—



$N : N = 129° 20'$
$b^2 : b^2$ (over N) $= 93°$
$N : b^2 = 162°$
$P : t = 131°$ (129° – 132°)

The crystals are black and semi-metallic in lustre. The largest are 1½ inches in breadth.

The isomorphism of this species and andalusite is sufficiently close to merit attention.

Calculating from $N : N = 129° 20'$, the prism $b^2 = 93° 8'$. In Andalusite, the prism corresponding to the latter is 91° 20', and this gives for the former 127° 57'. P : t in Staurotide = 131°, in andalusite 144° 50'; and the planes have therefore the axial relation of 3 : 2.

For the relation of topaz and andalusite, see last volume of this Journal, p. 407.

The atomic volume of the St. Gothard staurotide, calculated from one of Jacobson's analyses corresponding to $(\frac{4}{5}\dddot{A}l+\frac{1}{5}\dddot{F}e)^2\dddot{S}i$, and specific gravity 3·74, gives for the A atomic volume 536, and for the C atomic volume 38·3: for a St. Airolo staurotide, with the formula $(\frac{4}{5}\dddot{A}l+\frac{1}{5}\dddot{F}e)^3\dddot{S}i^2$, and specific gravity 3·7, we obtain the A atomic volume 890·5, the C value 38·7. Andalusite afforded (last volume of this Journal, p. 235) A = 962·5, C = 41·85; and topaz (loc. cit., p. 240) A = 2344; B = 42·6.

6. *Platinum of California.*—In a letter from J. E. Teschemacher, we are informed that he has detected a proportion of platinum among the grain gold of California, that will render it an important object of search in that region. In an ounce of the fine grains, he found about 50 granules which proved to be this metal. He observes that the proportion obtained is about as large as from the South American mines.

7. *Fowlerite.*—This name, applied to a variety of manganese spar or rhodonite, was not originally proposed by Prof. Shepard, as stated on p. 410 of last volume of this Journal.

III. Zoology.

1. *Remark on the Genus Nocticula of J. V. Thompson;* by J. D. Dana. —The genus Nocticula established by Thompson in his Zoological Researches, No. 2, p. 52 and plate 5, (Cork, 1829,) is closely related to the Euphausidæ.* The general form of the animal, its thoracic and abdominal appendages, and the antennæ, eyes, and short beak, are as in Thysanopoda and Euphausia; and the last abdominal segment has the acuminate character with the naked barb either side near apex, which occurs in this family.† The legs are not however represented as bifid, the outer branch and branchial appendages having been overlooked, (and in the suggestion of this error we make the "*due allowance* for drawings made at sea of such minute objects" which Thompson asks of his readers.) The number of thoracic legs is stated at sixteen, but this includes, as the drawing shows, a pair of maxillipeds. Excluding these, there will then be 7 pairs, which is the number in Thysanopoda; and it seems probable that Nocticula and Thysanopoda are identical, and if so, the former name has the precedence.

The species described and figured by Thompson, was taken in the Atlantic, between latitudes 5° 25′ S., and 29° 30′ N., and longitude 17° 18′ W., and 32° 55′ W., on the 6th, 12th, and 25th of September. It was brilliantly phosphorescent. It is called the *Nocticula Banksii*, as it is supposed by Thompson, and with apparent good reason, to be identical with the *Cancer fulgens* of Sir Joseph Banks, a species observed between Madeira and Brazil, and published with a drawing by Macartney in the Philosophical Transactions for 1810. This drawing, Thompson has copied, and in it 7 pairs of thoracic legs are represented.

2. *Observations on some Crustacea, in the collections of the Academy of Natural Sciences at Philadelphia;* by Prof. L. R. Gibbes of Charleston, S. C., (Proc. Acad. Nat. Sci., Philad., March, 1850, v, 25.)—The *Chorinus armatus* of Randall (Memoir in Jour. Ac. Nat. Sci. Philad., viii, 106,) is a Pericera, and agrees with *Pericera cornuta* of Edwards, (Crust., i, 335.)

The *Cancer mercenaria* of Say, is a *Pseudocarcinus.* It is referred by Edwards with some doubt to the genus Xantho of Leach (M. Edw., Crust., i, 399); but his description of *Pseud. ocellatus* applies in every particular to the *mercenaria.*

Grapsus longipes, *Randall*, is *Grapsus cruentatus*, according to Gibbes, and the statement that Surinam is its locality, is considered a mistake.

Grapsus hirtus, *Randall*, is *G. rudis*, M. Edw., the latter name having the priority.

Guaia ornata, *Randall*, is an *Ilia*, and a distinct species.

To the above, the Committee of the Society add—that *Macrophthalmus compressipes*, Randall, (published in 1840,) is *Gelasimus telescopicus*, Owen, Voyage of Blossom (1839), and *M. podophthalmus*, Voyage of Bonite, (1841.)

Pachygrapsus parallelus, *Randall*, is *Grapsus Thukujar*, Owen, loc. cit.; and Pagurus decorus, Randall, is *P. pictus*, Owen.

* See this Journal, [2], ix, p. 130.

† Milne Edwards alludes to this character in the name of his Thysanopoda, *T. tricuspidata.* Ann. des Sci. Nat., xix, 1830, 454, note.—The genus Nocticula is not referred to by Edwards, either in his Memoir, or in his Hist. Nat. des Crustacés.

In most of these cases in which Dr. Randall has been forestalled, it has been by publications that were not through the press when his memoir was in course of preparation.

3. *Conspectus Crustaceorum quæ in Orbis Terrarum circumnavigatione*, Carolo Wilkes *e Classe Reipublicæ Fæderatæ Duce, lexit et descripsit* J. D. Dana. AMPHIPODA, No. I. (Proc. Amer. Acad. Arts and Sciences, ii, 201).—The following are the names of the sixty-two new species described in this paper.

Familia 1. *Orchestidæ :—*

Genus I. Talitrus : *species*, novi-zelandiæ, gracilis, ornatus.

Genus II. Talitronus, (*Dana*) : *species*, insculptus.

Genus III. Orchestia : *species*, sylvicola, tenuis, rectimanus, spinipalma, scutigerula, nitida, dispar, quadrimanus, serrulata.

Genus IV. Allorchestes, (*Dana*) : *species*, compressa, verticillata, hirtipalma, gracilis, peruviana, humilis, australis, brevicornis, novi-zelandiæ, intrepida, orientalis, graminea.

Familia 2, *Gammaridæ*, Sub-familia 1, *Lyssianassinæ :—*

Genus I. Lysianassa : *species*, brasiliensis

Genus II. Uristes, (*Dana*) : *species*, gigas.

Genus III. Stenia, (*Dana*) : *species*, magellanica.

Familia 2. *Gammaridæ*, Sub-familia 2, *Gammarinæ :—*

Genus I. Gammarus : *species*, asper, suluensis, albidus, hirsuticornis, emissitius, tenuis, furcicornis, tenellus, orientalis, quadrimanus, validus, setipes, pilosus.

Genus II. Amphitoë : *species*, peculans, fissicauda, pubescens, inæquistylis, peruviana, tenuicornis, indica, rubella, fucorum, tongensis, peregrina, brevipes, simplex, nodosa.

Genus III. Œdicerus : *species*, novi-zelandiæ.

Genus IV. Erichthonius : *species*, macrodactylus, pugnax.

Familia 3. *Corophidæ :—*

Genus I. Corophium : *species*, quadriceps.

Genus II. Clydonia, (*Dana*) : *species*, gracilis, longipes.

Familia 4, *Icilidæ :—*

Genus Icilius, (*Dana*) : *species*, ovalis.

4. *Oithona plumifera of W. Baird.*—The genus Scribella described in this Journal, 2nd ser., i, 227, 1846, and also viii, 279, is identical with *Oithona* of Baird, published in 1843 in the Zoologist, and the *Scribella scriba* appears to be the same species with the *Oithona plumifera.* J. D. D.

5. *On the circulation and digestion in the lower Animals ;* Prof. Agassiz, (Proc. Bost. Soc. Nat. Hist, 1850, p. 206.)—Prof. Agassiz read a paper on the circulation and digestion in the lower animals, showing that the circulation in the Invertebrata cannot be compared to that of the Vertebrata.

Instead of the three conditions of chyme, chyle, and blood, which the circulating fluid of the Vertebrata undergoes, the blood of that class of the Invertebrata, which he had particularly studied, the Annelida, is, according to Wagner, simple chyle, colored chyle; the receptacles of chyle in different parts of the body are true lymphatic hearts like those found in the Vertebrata ; this kind of circulation is found in the Articulata and

Mollusks with few exceptions, some Echinoderms, &c. In the Medusæ and Polyps, instead of chyle, chyme mixed with water is circulated: this circulation is found in some Mollusks and intestinal worms; it may be seen plainly in Beroë. Prof. Agassiz thinks that the embryological development of the higher animals shows a similar succession in the circulating function. He also examined the connection of respiration with the circulation: in Vertebrata, the gills are found between branches of the blood system; in Invertebrata, the chyliferous system is acted on by the respiration; the gills of fishes, then, cannot be compared to the gills of Crustacea, Articulata and Mollusks. No gills are connected with the chymiferous circulation; animals having this circulation have no true respiration; they have only tubes to distribute freely aërated water to the different parts of the body.

6. *Low State of Development of Mammals and Birds in Australia and New Zealand*, (Gould's Birds of Australia; Jameson's Jour., xlviii, 362.)—Geological researches into the structure of the globe, shew that a succession of physical changes have modified its surface from the earliest period up to the present time, and that these changes have been accompanied with variations not only in the phases of animal and vegetable life, but often in the development also of organization: and as these changes cannot be supposed to have been operating uniformly over the entire surface of the globe in the same periods of time, we should naturally be prepared for finding the now existing fauna of some regions exhibiting a higher state of development than that of others; accordingly, if we contrast the fauna of the old continents of geographers with the zoology of Australia and New Zealand, we find a wide difference in the degree of organization which creation has reached in these respective regions. In New Zealand, with the exception of a *Vespertilio* and a *Mus* which latter is said to exist there, but which has not yet been sent to this country, the most highly organized animal hitherto discovered, either fossil or recent, is a bird; in Australia, if compared with New Zealand, creation appears to have considerably advanced, but even here the order *Rodentia* is the highest in the scale of its indigenous animal productions; the great majority of its quadrupeds being the Marsupiata (kangaroos, &c.) and the *Monotremata*, (*Echidna*, and *Ornithorhynchus*), which are the very lowest of the Mammalia; and its ornithology being characterized by the presence of certain peculiar genera, *Talegalla*, *Leipoa*, and *Megapodius;* birds which do not incubate their own eggs, and which are perhaps the lowest representations of their class,* while the low organization of its botany is indicated by the remarkable absence of fruit bearing trees, the *Cerealia*, &c.

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* The genera, *Talegalla*, *Leipoa*, and *Megapodius*, form part of a great family of birds inhabiting Australia, New Guinea, Celebes, and the Phillippine Islands, whose habits and economy are most singular, and differ from those of every other group of birds which now exist upon the surface of the earth. In their structure they are most nearly allied to the *Gallinaceæ*, while in some of their actions, and in their mode of flight, they much resemble the *Rallidæ;* the small size of their brain, coupled with the extraordinary means employed for the incubation of their eggs, indicates an extremely low degree of organization.

The three species of the family inhabiting Australia, although referable to three distinct genera, have many habits in common, particularly in their mode of nidifica-

7. *New Species from Lake Superior described by M. Agassiz.*—The interesting and valuable volume lately issued by Prof. Agassiz and his associates, contains descriptions of numerous new forms of animals. We now enumerate the following species of fish described for the first time by Prof. Agassiz in this volume.

Acipenser lævis, Agass., p. 267.

" *carbonarius*, Agass., p. 271, (Plate 5, fig. 1.)

" *rhynchæus*, Agass., p. 276.

Pimelodus felis, Agass., p. 281.

PERCOPSIS, Agass., new genus. We quote the remarks of Prof. Agassiz in full respecting the very remarkable new genus of fish to which he has assigned the name *Percopsis*.

"In order fully to understand and perfectly to appreciate the characters of this genus, and the interest involved in its discovery, it is necessary to remember various relations of the different types of the whole class, which however do not constitute generic distinctions, although they bear upon the peculiarities of this new type.

"In the first place, it is a matter of no little importance that, among the fishes of former ages, we find everywhere types which differ widely from the forms of our time, and that those forms are the more different, as they belong to older geological deposits. The differences are even so great, that out of the four orders of this class, there are only two which constitute the fauna of fishes in the older formations; two orders, which in our day are comparatively reduced, I mean the Placoids and Ganoids. Moreover, the types are peculiar in all epochs. For instance, the sharks of former days, especially those of older epochs, resemble solely that curious genus of Port Jackson, New Holland, the Cestracion, which is so remarkable among the living fishes as to form a group by itself. The Ganoids, of which there are so remarkably few in the present creation, such as the gar-pike (Lepidosteus) of this continent, are not less peculiar, and in connection with those ancient Placoids, constitute the only representatives of the class of fishes throughout the earliest geological ages down to the deposits of the chalk, when new families of other orders, the Ctenoids and Cycloids, begin to make their appearance, preparatory as it were to the present development of that class, and are successively diversified with the modified adaptations of the whole class. Now the genus Percopsis is as important to the understanding of the modern types of fishes as Lepidosteus and Cestracion are to the understanding of the ancient ones, as it combines characters which in our day are never found together in the same family of fishes, but which in more recent geological ages constituted a striking peculiarity of the whole class. My Percopsis is really such an old-fashioned fish, as it shows peculiarities which occur simultaneously in the fossil fishes of the chalk epoch, which however soon diverge into distinct families in the tertiary period, never to be combined again.

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tion, each and all depositing their eggs in mounds of earth and leaves, which, becoming heated either by the fermentation of the vegetable matter, or of the sun's rays, form a kind of natural hatching apparatus, from which the young at length emerge fully feathered, and capable of sustaining life by their own unaided efforts.

"This ancient character of some of the American fishes agrees most remarkably with the peculiarity of the vegetation of this continent, which, as I have shown on former occasions, resembles also the fossil plants of prior ages.

"The geographical range of these peculiar, old-fashioned beings is also very remarkable, they living in temperate, or rather cold climates, when their earlier representatives lived in warmer epochs.

"The most striking features of the fishes of the tertiary period and those of our time consist in their belonging to two groups of the class only; one, the Ctenoids, with rough, combed scales, in which the respective representatives have also prominent serratures on prominent spines upon the head, in the operculum in particular, and in the fins; the other, the Cycloids, smooth, with simple scales with an entire margin, in which some few types however have also spinous fins.

"Now my new genus, Percopsis, is just intermediate between Ctenoids and Cycloids; it is, what an ichthyologist, at present, would scarcely think possible, a true intermediate type between Percoids and Salmonidæ.

"The general form of this genus reminds us of the common perches, but it is easily distinguished from them, by the fact that its head and the opercular apparatus are smooth and unprovided with denticulations, as also by the presence of a small adipose fin, as in the salmons. The anterior dorsal is also a small fin, composed of soft branched articulated rays, as in the salmons. The ventral fins are placed at the middle of the abdominal cavity, as in the Abdominales in general. The scales, however, are truly serrated as in the Percoids, a structure which, as far as I know, does not occur in any of the Abdominales. The conformation of the mouth is also as in the Perches, that is to say, the intermaxillaries form alone the upper margin of the mouth, and the maxillaries stand behind as a second arch, but the vomer and palate are entirely destitute of teeth.

"This fish, of which I shall publish a full anatomy, should be considered as the type of a distinct family, under the name of *Percopsides*."

Species *Percopsis guttatus*.

The other new species described are:—

Grystes fasciatus, p. 295.
Cottus Richardsoni, p. 300.
Cottus Franklini, p. 303.
Boleosoma maculatum, p. 305, plate iv, fig. 3.
Pileoma zebra, p. 308.
Gasterosteus nebulosus, p. 310, pl. iv, fig. 4.
Gasterosteus pigmæus, p. 314, pl. iv, fig. 1.
Esox Boreus, p. 317.
Lota maculosa, p. 325.
Salmo Siscowet, p. 333, pl. i, fig. 3.
Coregonus sapidissimus, p. 344.
Coregonus latior, p. 348.

Rhinichthys, Agass., new genus. This new genus contains small Catostomi whose essential character is as the name indicates, to have a conical prolongation of the rostrum.

ERRATA.

In last No. p. 399, bottom line, for "Journal," *read* "Proceedings."
" " p. 409, 10th line from top, transpose 72·47 and 27·53.
" " p. 429, 19th " " " for "pyroxene," *read* "borax."

The next No. of this Journal will be published on the first of Sept.

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SCIENTIFIC INTELLIGENCE.

(For remainder of Contents, see third page of Cover.)

Vol. X. SEPTEMBER, 1850. No. 29.

Published the first day of every second month, price $5 per year.

THE

AMERICAN JOURNAL

OF

SCIENCE AND ARTS.

CONDUCTED BY

Professors B. SILLIMAN, B. SILLIMAN, Jr.,

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42-6

SECOND SERIES.

No. 29.—SEPTEMBER, 1850.

NEW HAVEN:

PUBLISHED BY THE EDITORS.

FOR AGENTS ADDRESSES, SEE SECOND PAGE.

Printed by B. L. Hamlen—Printer to Yale College.

The postage on this Journal to any distance is 9½ cts.

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April, 1849.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

Art. XV.—*Observations on the Contrast in the Physical Features and Resources between the Old World and the New World;* by Prof. Arnold Guyot.*

The appearance of Professor Guyot's work on Physical Geography has been briefly announced in this Journal. The pleasure and profit it is calculated to afford, the importance and novelty of its views, the high intellectual and moral tone that pervades it, impel us to give it a fuller notice. The subject is geography;—not the dry *anatomy* of the earth's surface, of which our common school-books treat;—but geography as taught by Humboldt and Karl Ritter, and interpreted anew with fresh vigor and much originality by our author. The work treats of the physical features of the globe with reference to the history of man. The bearing of its peculiarities of outline, of surface, of climate, and of productions, on the distribution of man, and the progress of civilization, is the grand subject in hand; and it is ably discussed with a command of a wide range of facts, and great beauty and fertility of thought. Geography is raised to a level with the higher intellectual sciences, and the Christian spirit infused into it gives it a still more exalted character. The various facts and deductions brought forward one after another, are finally made to concentrate upon the one great idea of man's moral destiny.

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* From a work entitled, *The Earth and Man: Lectures on Comparative Physical Geography, in its relation to the History of Mankind,* by Arnold Guyot, Prof. Phys. Geog. and Hist. at Neuchâtel, Switzerland; translated from the French by C. C. Felton, Prof. in Harvard University. Boston, 1849. xviii and 310 pp. 12mo.

Prof. Guyot commences by explaining Physical Geography as a description of the earth and of its relation to the life upon it. He says, in the course of his illustrations of the subject, "If the Rocky Mountains bordered the eastern coast of North America, and closed against the nations of the East and of Europe the entrance to the rich valley of the Mississippi; or if this immense chain extended from east to west across the northern part of this continent and barred the passage of the polar winds which now rush unobstructed over these vast plains; let us say even less—if, preserving all the great present features of this continent, we suppose only that the interior plains were more slightly inclined towards the north, and that the Mississippi emptied into the Frozen Ocean;—who does not see that in these various cases, the relations of warmth and moisture, the climate in a word, and with it, the vegetation and the animal world would undergo the most important modifications, and that these changes of form and of relative position would have an influence greater still upon the destinies of human societies, both in the present and in the future?" This paragraph exhibits the aim of the work, which the author afterwards more explicitly states as follows—after mentioning the great problems which physical geography offers for investigation: He says, "We shall endeavor to solve these problems by studying, first, the characteristic forms of the continents, the influence of these forms on the physical life of the globe; then the historical development of humanity. We shall have succeeded, if we may have shown to you—

"1. That the forms, the arrangement, and the distribution of the terrestrial masses on the surface of the globe, accidental in appearance, yet reveal a plan which we are enabled to understand by the evolutions of history.

2. That the continents are made for human societies, as the body is made for the soul.

3. That each of the northern, or historical continents, is peculiarly adapted by its nature, to perform a special part which corresponds to the wants of humanity in one of the great phases of its history.

Thus nature and history, the earth and man, stand in the closest relations to each other, and form only one grand harmony."

In carrying out this plan, the author is borne up by his subject, the works of creation, to the Supreme Intelligence whose beneficent designs are so strongly marked around and within us.

The facts of science are not yet sufficient to sustain beyond a doubt all the conclusions arrived at; still the tendency is apparently in the direction in which Prof. Guyot would lead us; and however the reader may differ in some minor points, he will rise from the work with enlarged views of the earth and its fitness for man's mental and moral development. There are many portions of the volume which we should be glad to transfer to our pages. The chapter on the Relief of the Continents contains views of high importance, which show that the author has studied the world, and not merely a ledge of rocks or an isolated territory. He does not, with the prevalent geological school, regard the elevation of mountain chains as the great events in the physical history of our globe, and the rising of the vast plateaus and plains of the continents as a mere accompaniment; but considers, in view of the amount of evidence in physical geography, the change of level in entire continents as the essential fact, and mountains but the accidents attending the vast changes that have been in progress.

Passing by this and other chapters (or lectures) we cite a portion of Professor Guyot's observations on *the Contrast in Physical Features and Resources of the Old World and the New World*,* referring to the work for the continuation of the subject and the many conclusions deduced.

"The most prominent feature of the arrangement of the continents is the grouping of the two Americas in one hemisphere, and that of the four others in another hemisphere. This division of the continents into two worlds is so evident from the first glance, and is at the same time so convenient in practice, that it has passed into common speech as one of those observations admitting no contradiction.

But to bring out prominently the contrast of these two worlds, they must be studied more in detail than we have thus far done; we must compare them, in order to deduce, by the comparison itself, the special character of each. This is what we are going to attempt. We have already seen that they differ in the forms of their relief and in their climate; we shall further see that these fundamental differences produce analogous effects in the organized beings, and in the entire physical life of each of the two worlds. Finally, we shall speak of the manner in which they act upon each other and seem, by their very nature, destined not to live isolated, but to form together a single organism, a grand harmony.

During the whole of this study, please to remember, gentlemen, that we are in the realm of nature, and not in that of history. The America we are seeking to understand is that which Columbus and his successors discovered, still entirely a virgin

* These Lectures were delivered by Prof. Guyot in French at the Lowell Institute, in January and February, 1849. The vigorous and eloquent thoughts of the author are well expressed in the elegant translation of Prof. Felton.

world, centuries ago; and not the New World of history, of which we shall have to speak later, that has come to plant itself on that soil.

A general comparison of the two groups of continents, will call to mind some of the leading features we have already become acquainted with, and add still others.

The Old World and the New World differ in the groupings, and in the number and extent of the continents composing them; in their astronomical situation, with respect to the climatic zones; in the general direction of their lands; in their interior structure. This assemblage of opposite characters secures to each of them a climate, a vegetation, and an animal kingdom, peculiarly their own.

I say first, in their groupings:—

The Old World is composed of four continents. Setting aside Australia, which is only an island in the midst of the oceanic hemisphere, it numbers three, all very near each other, aggregated and forming an oval compact mass, whose extent far surpasses that of every other terrestrial space. It presents a solid extent of land, the most vast, the most unbroken, the least accessible in its centre to the influences of the ocean. The Old World is preëminently the *continental* world.

The New World has only two continents, North America and South America, America and Columbia, as I should like to call them—to render justice where right belongs—if it were not forbidden to change names consecrated by long usage. These two continents are not grouped in one mass, nor placed side by side, but separated from each other, not touching upon their long sides, but by their exterior angles, standing in line, rather than grouped. They are situated in two opposite hemispheres, and thus more distant from each other, apparently, and less neighboring.

The result of this remarkable disposition is that narrow, lengthened, slender form we see in the New World. No portion of the interior is very remote from the sea-coasts; everywhere it gives access to the influences of the ocean, in the midst of which it is placed, like a long island. This form already contrasted with that of the Old World, gives to it its character. The New World is essentially *oceanic*.

The astronomical position, relatively to the climatic zone, is also not the same in the two worlds.

The Old World is, as it were, crowded back upon the north of the equator; it belongs for the most part, to the northern hemisphere and to the temperate zone. Of the three principal continents composing it, the two whose importance is by far the greatest, Europe and Asia, are temperate. Asia penetrates the torrid regions only by the southern peninsulas; Europe at no point; Australia is sub-tropical; Africa only is truly tropical. Even if we take in the last two continents, more than two-thirds of the lands are situated in the temperate regions, one-third only in the equatorial regions. The Old World is then essentially *temperate.*

In the New World the lands are distributed in a manner nearly equal in the two zones and in the two hemispheres. We find that of the countries it includes, those which are the most richly endowed, are situated under the sun of the tropics. Compared with the Old World, the New World is thus essentially *tropical.*

The general direction of the lands, or the direction in which their length extends, is the inverse in the two worlds. The Old World has its greatest prolongation from east to west, in the line of the parallels; the New World from north to south, in the direction of the meridians. Both have a length of about 7,500 miles, but the breadth of the Old World is nearly double that of the New. This disposition is of the greater consequence for the distribution of the climates in each of them, since this configuration coincides, as we have seen, with the interior structure, with the direction of the principal mountain chains and of the table lands. From one end to the other of the Old World, over a space of several thousand miles, the migratory tribes are able to pursue their adventurous roaming course, by following, according to their custom, the great features of relief of the soil, without witnessing any change in the vegetation or the animals that surround them. They change place, but not climate, nor ways of life. This similarity of climates over long spaces is, then, a property of the Old World, and must have singularly favored the dispersion of the primitive tribes.

In the New World, on the contrary, the zones of similar climates are short and numerous; and if we travel over the whole length of the two Americas, we pass twice in succession through all the temperatures, from the frozen climate of the pole to that of the equator, and from the burning climate of the equator to that of the poles. This diversity of climate gives their character to the Americas.

Meantime, the interior structure modifies in the two worlds these climatic relations, in such a manner as to correct the uniformity of climate in the Old by more marked contrasts, and the too great diversity of the New, by gentler and better graduated transitions. We shall see this as we proceed to a closer examination of the structure of the two Americas, which will particularly occupy us this evening.

What characterizes the interior structure of the New World is its simplicity. In place of the variety of the Old World, where, in spite of a few general features common to both, each continent is, as it were, cast in a separate mould, the two Americas seem absolutely formed upon the same plan. This plan may be sketched out in a few lines. Two triangles, their vertices turned to the south, one situated northwest of the other; the long cord of the Rocky Mountains and of the Andes, running the length of the extreme western coast, and binding the two continents together; great plains on the east, forming the larger part of their surface; a slight elevated chain along the Atlantic coast of both, the Alleghanies in North America, the Serra do Espinhaço and the Serra do Mar of Brazil, in South America; finally, in the centre, three short, transverse chains, that of Parime, in the Guyanas, that of Venezuela, and that of the great Antilles, broken into a number of islands;—these, in a few words, are all the essential features of this vast division of the world.

That which constitutes the richness of organization in the continents, is the number and abundance of internal contrasts calling out at once the activity of nature and that of man. The Old World is full of them; America has only a small number, all tending to disappear by reason of the structure itself.

Thus in Asia and Europe, the line of the highest lands, the continental axis, extending from the Himalaya to the Alps and the Pyrenees, divides these two continents into two unequal parts, one north and one south, opposite in climate, in vegetation, and even in races. Scarcely anywhere is the transition from one to the other gradual; almost everywhere it is abrupt and sudden. The table lands of Tubet and frigid Mongolia touch the tropical plains of China and of the Indies; the traveller who passes the Alps, abandons the severe landscapes and the firs of the North, to descend, by a single day's journey, into the ever verdant gardens and the orange groves of fair Italy; he exchanges the cold mists of the North for the sun of the South, and often leaves on one side, the snows and frosts of winter, to find on the other, the warm breath of spring, its verdure and its flowers.

This striking contrast between the North and the South reflected in the character and history of all the nations of Asia and Europe, is doubtless found in America; nowhere is it better known than in this country. But in nature it is almost effaced, it is softened down. It does not form a barrier; it nowhere presents an abrupt change; nowhere breaks the unity. On account of the continued plains of the continent, we see the natural character of the North gradually melt into that of the South. Between the shores of the Frozen Ocean and the Gulf of Mexico there lies the whole distance from the icy regions to the tropics. But it is only slowly, and over long spaces, that we pass through the transition. From the treeless polar plains, where flows the Mackenzie River, whose only covering is the mosses and the lichens, we pass only by degrees to the coniferous forests of Lake Superior; then to the oaklands of Wisconsin: the walnuts, hickories, and the chestnuts of Ohio and Kentucky next appear; farther south, the magnolia and the palmetto already herald the air of the tropics and the neighborhood of the Gulf of Mexico. Two thousand four hundred miles separate the extremes of this scale of vegetation which almost touch each other in the Himalaya.

It is, moreover, to these vast plains, which offer no obstacles to the dissemination of the species, and to the absence of great chains from east to west, that we undoubtedly owe the appearance at the North, of plants and animals that seem to belong only to the tropical regions. It is not without surprise that the European landing on these shores sees the humming bird, that diamond of the tropics, glancing in the sun in a country which winter clothes, during long months, in a thick mantle of snow and ice.

It is the same towards the South, where we see the palm trees and the parrots of the tropics, here and there, as far as the Pampas of Buenos Ayres, much beyond their natural limits.

Thus the contrast between the North and the South is softened, reduced; but it is not annihilated; it exists on a great scale from one of the continents to the other: for North America is temperate, and South America is tropical.

America is cut by the Andes into two parts, east and west, as Asia and Europe are cut into two parts, north and south. But this contrast also is almost neutralized, as we shall soon understand.

The inequality is here carried to the extreme, to such a reduction of one of the parts, that it loses its importance, and, so to speak, its power of reaction. The western coast, dry and barren, has not extent and influence enough to enter upon an effective rivalry with the vast countries of the East. Moreover, the difficulty of communication renders the mutual action and the intercourse between the countries situated at the foot of the two inclinations, still more rare. Finally, the two sides of the Andes, being under the same latitudes, have the same, or nearly the same climate, and only differ in the degree of moisture or dryness falling to their portion. The West of the two Americas is only a narrow strip, not to be compared with the great plains of the East occupying nearly the entire continent, and giving it its character.

America is then less rich in internal contrasts than the Old World, but it has more of unity, because it is more simple. Undoubtedly in this uniformity of structure, in this absence of obstacles to a free circulation from end to end of this world, we are to look for one of the principal causes of that common character, of that American physiognomy which strikes us in all the organized beings of this continent, and which we find again in man himself, in the Indian, all the tribes of whom, from the banks of the Mackenzie River to Patagonia, have the same coppery tint and a family likeness in features, impossible to mistake.

The climate of the New World, compared with that of the Old, is distinguished by the abundance of pluvial waters, in general, by a greater humidity. We have seen in what manner this phenomenon is the consequence of its narrow and lengthened form; of the opening of the great plains—that is, of the two continents almost entire—to the winds of the sea; of the absence of high mountains in the East; in a word, of the configuration and general exposure of this part of the globe. While the Old World, with its compact figure, its vast plateaus, its high lands in the East, receives only an average of 77 inches of water by the year under the tropics, America receives 115 inches. The temperate regions of Europe have 34 inches; North America, 39 inches.

Add to this abundance of water, the extent of plains which permit the development of vast systems of water courses, and you will understand the existence of that innumerable multitude of rivers and lakes, which are one of the most characteristic features of the two Americas. Notwithstanding a much smaller extent than that of the Old World, the New possesses the largest rivers on the earth; the richest in waters, those whose basins occupy the vastest spaces. Where can we find, on the surface of the globe, a river equal to the mighty Marañon, that giant among the rivers of the earth, gathering its waters from a surface of a million and a half square miles, and bearing them to the ocean, after a course of 3,000 miles? This mighty monarch receives in his progress the homage of tributaries, each of which, by its greatness and the abundance of its waters, would suffice for the wants of a whole vast country. Such are the Ucayale, the Rio Purus, the Rio Negro; above all, the Madera, rivalling in importance the river to which it yields the honor of giving a name to their united waters. The farther it advances in its majestic course, the more its proportions increase; and before arriving at the ocean, its broad sheet, from the middle of which the eye cannot reach the banks, seems rather to be a fresh-water sea, flowing sluggishly towards the ocean basin, than a river of the continent. Far from its mouth, the fresh and muddy waters of the Amazon are still distinguished at a glance of the eye from the saline and limpid waters of the ocean; and their slime, borne along by their currents, goes to form, farther north, a new soil on the shores of the continent.

In the other continent, the Father of Waters, the mighty Mississippi, the second of the rivers of the earth, equals in length the Marañon itself; for its winding course is 3,000 miles. But its basin covers only from 8 to 900,000 square miles. Who does not know the importance of tributaries like the Missouri, which wrongly gives up its name for that of its less powerful brother; like the Ohio, the Beautiful River, the stream with transparent waters; like the Arkansas, and so many others composing that vast system of arteries that vivify the whole West, and that are destined to assume daily a greater and greater importance? And these immense rivers are not isolated. At the side of the Marañon, the La Plata has a course of not less than 1,900 miles, and more than a million of square miles send it their waters. At the side of the Mississippi, the St. Lawrence has a course of 1,800 miles and a basin of nearly a million of square miles.

The Old World offers nothing similar. The greatest of its rivers, the Yan-tse-Kiang in China, has a course of only 2,500 miles. The Ganges and the Nile are far from equalling it. The Volga, the greatest of the rivers of Europe, exhibits a course of only 1,700 miles: and if it were necessary to enumerate in America rivers like the Rhine, so celebrated, it would be almost by hundreds that we should have to cite them.

And what shall we say of the abundance of its lakes? The group of the great lakes of Canada, so characteristic of North America, finds nowhere a parallel. It contains at once the largest lakes in the world, and the greatest mass of fresh water united on the surface of the continents. These vast seas of fresh water, together with the St. Lawrence, cover a surface of nearly 100,000 square miles, and it has been calculated that they contain almost one-half of all the fresh waters on the surface of our planet. They, too, are not alone, and a glance of the eye at the map, enables us to perceive in the North a multitude of lakes but little inferior in extent: the lake Athapescow, Winnipeg, Slave Lake, the Great Bear, are worthy to figure side by side with the lakes of Canada and of the St. Lawrence.

The rivers and the lakes are the wealth, and justly form the boast of America. No continent possesses so great a number, or those of so large extent, so well provided with water, so navigable. Not only do they fertilize the rich countries they traverse, but they are now, and will become still more so, the great highways of commerce between all the parts of this vast world; we already see enough to hope everything of the future.

Thus, gentlemen, the watery element reigns in the New World; add to this, that although half of its lands are exposed to the rays of the tropical sun, yet, all the conditions being equal, America is, on the whole, a little less warm than the Old World, and we shall have the essential features of its climate,—the oceanic climate. This is what America owes to the fundamental forms and the relative disposition of its lands; while the Old World is indebted to the preponderance of land for its dry and continental climate.

Let us now follow out the consequence of these physical circumstances upon the development of organic beings, and the character of the New World will come out in all its clearness.

The warm and the moist—these are the most favorable conditions for the production of an exuberant vegetation. Now, the vegetable covering is nowhere so general, the vegetation so predominant, as in the two Americas. Behold, under the same parallel where Africa presents only parched table lands, those boundless virgin forests of the basin of the Amazon, those selvas, almost unbroken over a length of more than 1,500 miles, forming the most gigantic wilderness of this kind that exists in any continent. And what vigor, what luxuriance of vegetation! The palm trees, with their slender forms, calling to mind that of America itself, boldly uplift their heads 150 or 200 feet above the ground, and domineer over all the other trees of these wilds, by their height, by their number, and by the majesty of their foliage. Innumerable shrubs and trees of smaller height fill up the space that separates their trunks; climbing plants, woody-stemmed climbing lianos, infinitely varied, surround them both with their flexible branches, display their own flowers upon the foliage, and combine them in a solid mass of vegetation, impenetrable to man, which the axe alone can break through with success. On the bosom of their peaceful waters swims the Victoria, the elegant rival of the Rafflesia, that odorous and gigantic water lily, whose white and rosy corolla, fifteen inches in diameter, rises with dazzling brilliancy from the midst of a train of immense leaves, softly spread upon the waves, a single one covering a space of six feet in width. The rivers rolling their tranquil waters under verdurous domes, in the bosom of these vast wilds, are the only paths that nature has opened to the scattered inhabitants of these rich solitudes. Elsewhere, in Mexico and Yucatan, an invading vegetation permits not even the works of man to exist; and the monuments of a civilization comparatively ancient, which the antiquary goes to investigate with care, are soon changed into a mountain of verdure, or demolished, stone after stone, by the plants piercing into their chinks, pushing aside with vigor, and breaking with irresistible force, all the obstacles that oppose their rapid growth.

South America, and particularly the basin of the Amazon, is the true kingdom of the palm trees; nowhere does this noble form of vegetation show itself under a greater number of species. This is a sign of the preponderating development of leaves over every other part of the vegetable growth; of that expansion of foliage, of that *leafiness*, which is peculiar to warm and moist climates. America has none of those plants with slender, shrunken leaves, like Africa and New Holland. The Ericas, or heather, so common, so varied, so characteristic of the flora of the Cape of Good Hope, is a form unknown to the New World. There is nothing which resembles those Metrosideri of Africa, those dry Myrtles (Eucalyptus) of New Holland, and willowleaved acacias, the flowers of which shine with the liveliest colors, but whose narrow foliage, turned edgewise to the vertical sun, casts no shadow. Everywhere, long and abundant leaves, an intense verdure, a strong and well-nurtured vegetation, these are what we find in tropical America.

North America, in spite of its more continental climate, shares no less in this character of the New World. The beauty and extent of the vast forests that cover its soil, the variety of the arborescent species composing them, the strong and lofty size of the trees which grow there, all these are too well known for me to stop to describe them. It is because this continent adds to a more abundant irrigation, a soil slightly mountainous, almost everywhere fertile, securing to it always an equal moisture, a more abundant harvest of all the vegetables useful to man.

Not only is the vegetation abundant in the New World, but it is universal, and this is a farther characteristic distinguishing it from the Old. We do not see there those vast deserts, so common in the other continents, and occupying a considerable portion of their surface. The Deserts of California and that of Atacama are exceptions, and, compared with those of Africa and Asia, scarcely seem made for anything except to serve as specimens. The llanos of Orinoco, which their geological nature dooms apparently to the fate of Sahara, are copiously watered during the rainy season, and are covered then with an admirable vegetation. Life, which seemed almost to slumber, almost extinguished, springs up again more beautiful and more vigorous. To the powdered sand swept along by the winds, succeed rich pastures, where range a multitude of indigenous animals, mingled with herds of horses, and wild asses, coming from Europe; and thousands of reptiles buried in the watery slime during the dry season, reappear, and fill again with life the temporary rivers and lakes with which these valleys are then overflowed. The pampas themselves are not without vegetation, and support at all times numerous herds. And who is ignorant that the vast prairies of the Mississippi and the Missouri produce every year an abundant vegetation, on which feed the bisons and the other wild tenants of the country.

But what becomes of the animal in the peculiar kingdom of vegetation? Blessings are shared; all treasures do not belong to one country alone. This luxuriant vegetation, it might be said, seems to stifle the higher life, in the animal world. Animal life is, as it were, overruled, enfeebled; it does not occupy here the first rank, which is its due; for that which favors it is dry heat, the continental element.

From one end to the other of the animal scale, the families that seem to give to these countries their character, by the number of their species and by their relative abundance, are those which connect themselves, by their mode of life, with the aqueous, or with the vegetative element.

Hence, nothing is more splendid, more sparkling, than the insect world in South America. The inexhaustible variety of their species, the brilliancy of their colors, the size of their bodies, make them one of the most beautiful ornaments of these regions. Here live the Hercules beetle, the largest of the Coleoptera; and those brilliant butterflies with broad wings, the Menelaus, the Adonis, the Achilles, whose varying colors glitter in the sun like diamonds. But why be astonished? The existence of this little animal world is almost altogether dependent on the vegetation; the wealth of the one must create the wealth of the other.

Among the vertebrates, no family is so largely represented as that of the reptiles, for moisture is their element, and the rivers and temporary lagoons of the rainy season are peopled with Caimans, the crocodiles of the New World; the Iguanas, the most gigantic of the lizards; the Basilisks and other species, which multiply in the warm and still waters. The forests harbor in great numbers serpents of every form and figure, even to those monstrous boas, that are the terror of the natives themselves. They seem to be at home in this country.

But among the superior animals, development seems to be arrested; it is incomplete. The prevailing types are at the same time the inferior types. Among the birds, the stilt-plovers, inhabitants of the marshes and the shores, of which the number of species surpasses far, in America, that of any other continent. In the mammifera, the order of Edentata, the Armadillos, the Pangolins, the Ant-eaters, the Sloths, which characterize, more than any other family, the fauna of South America, not only in the present epoch, but also in the geological ages. And if we seek representatives of the higher orders, we find them less numerous in species, smaller in size; in a word, far inferior to the corresponding animals of the Old World. In the order of the Pachyderms, instead of the elephant, the rhinoceros, the hippopotamus, those giants of the Old World, the feeble and harmless tapir and the pecari. In the Ruminants, instead of the camel and the dromedary, the lama of the Andes, which reaches only half their size. Instead of the lordly lion of Africa, and the ferocious tiger of the islands of the Ganges, the Ounce and the Jaguar of the forests of Brazil, which are scarcely more than large cats. In the monkeys, finally, those with a prehensile tail, which are peculiar to America, are reckoned among the least perfect, the lowest of their order in the scale of organization.

Not only are the superior animals ill represented on this continent, but they have not the strength, nor the indomitable courage, not the ferocity, nor the intelligence of the similar creatures of the Old World. In all tropical America especially, as we see, the whole animal kingdom remains in an inferior condition. It is subjected to the watery element, and to the vegetable world; for in those regions where vegetable life is the superior, animal life stands but in the second degree.

North America, however, in consequence of her more continental character, possesses some superior types, which recall to mind, and perhaps equal, those of the Old World. The majestic bison, the deer, the elk, and the bear, give evidence of that same vigorous northern nature which predominates in the temperate continents, and of which, as we shall see, North America possesses her share.

Man himself, the indigenous man I mean, bears in his whole character the ineffaceable stamp of this peculiarly vegetative nature. Living continually in the shadow of those virgin forests which overspread the earth that he inhabits, his whole nature has been modified thereby. The very copper hue of his complexion indicates that he lives not, like the negro, beneath the scorching sunbeams. His lymphatic temperament betrays the preponderance in his nature of the vegetative element. The Indian is of a melancholy, cold and insensible race. "Foreign to our hopes, our joys, our griefs," says a traveller, "it is rarely that a tear moistens his eyes, or that a smile lights up his features." The most barbarous tortures cannot extort from him a single complaint, and his stoical indifference is disturbed only by vengeance or jealousy. If he sometimes exhibits a display of prodigious muscular force, he is yet without endurance. Who knows not that when the first invaders of the New World endeavored to compel the inoffensive Indians, who had received them as gods, to the rude labors of the mines and the cultivation of the soil, these men of the woods, incapable of enduring fatigue, perished in agony by thousands? And it was thereupon that the Europeans substituted for the Indian the robust and vigorous native of the Old World, the negro, who, still to this day, used as the instruments of the white man's labor, endures, I had almost said, daily, a degree of toil equal to that which destroyed the native of the country.

The social condition of the Indian tribes is tinctured, in an equal degree, by the powerful influence of his vegetative character. The Indian has continued the man of the forest. He has seldom elevated himself above the condition of the hunter, the lowest grade in the scale of civilization. The exuberance of the soil has never been of value to him; for he asks not of the earth his nourishment. He has never even ascended to the rank of the pastoral man. With him no domestic animals are maintained to feed him with their milk, or clothe him with their fleeces, as they are by the nomadic races of the Old World. From one extremity of America to the other, we find the same lamentable spectacle; the people of the elevated table lands of Mexico and Peru, are the only exception to this picture, and this exception goes far to establish the influence of the vegetative and humid nature of the lower plains of America. For if these nations do not exhibit the same character of inferiority, if they have raised themselves a little higher in the sphere of humanity, by the aid, perchance, of elements foreign to their own continent, it cannot be for other cause than that, living in those heights, those aërial islands, above the influence of the hot and humid atmosphere, they have been removed from the potency of its action.

Such, gentlemen, is the order, the admirable connection of the phenomena of nature with each other. The conformation and position of the New World give to it a hot and watery climate; this impresses its own character on all the organized creation. Man himself, the one being preëminently free, is liable to its influence, in proportion as he neglects the exertion of those superior faculties with which he is endowed for the conquest and subjugation of that nature which was intended, not to govern, but to serve him.

We may rest, then, in this conclusion, that, as compared with the Old World, the New World is the humid side of our planet, the *oceanic*, *vegetative* world, the passive element that awaits the excitement of a livelier impulse from without. Such is the America of Nature, such was it before the arrival of the man of the Old World. We know already, and we shall see better yet hereafter, all that his superior intelligence has been enabled to effect in the way of improving upon nature."

ART. XVI.—*On the relation of the Laws of Mechanics to Perpetual Motion*;* by JEREMIAH DAY, late President of Yale College.

I CAN not engage to furnish any infallible remedy, for the mortifying disappointments which abound in this fascinating field of investigation; yet it may not be altogether a waste of time, to pay some little attention to a subject which has levied such heavy contributions, on the inventive faculties of modern mechanicians and philosophers. The hints which I propose to suggest will be principally on the *nature* of perpetual motion, the *benefits* to be expected from it, the *difficulties* to be encountered

* *Messrs. Editors*,—If it be consistent with the purpose of your invaluable Journal, to admit upon its pages a paper which offers to the attention of the readers nothing original, no newly discovered principles of science, no improved combinations of machinery; the following plain thoughts on the application of the established laws of mechanics to the subject of *perpetual motion* are at your disposal.

Very respectfully, yours, &c.,

New Haven, July, 1850. J. DAY.

by the inventors, and the methods by which we may form an opinion whether it has been actually attained.

Perpetual motion is a motion continued without ceasing, and without any renewed application of force. A machine may be said to possess a principle of perpetual motion, if it continues to move, as long as it is kept in repair; and if it requires no new force to be applied to it from without. It is no part of the requisition concerning perpetual motion, that the machinery should never be out of repair. If it goes when it is in order, that is all that is demanded. What is looked for, is not perfection in the construction of the apparatus, but an unfailing moving force. On the other hand, it is required, that this force belong to the machine itself; that it be not an application from without. A stream of water may run continually. It may be applied to turn the wheel of a mill. It may in this way, become a never failing moving force. If the mill could be kept always in repair, the stream might be sufficient to turn it, as long as the world stands. But this is not what is meant by the perpetual motion of a machine, as the expression is commonly used. The moving force is here no part of the mill; it is applied from without. What is wanted is a principle of motion within the machine itself; or at least, so connected with it, as to accompany it wherever it may be removed.

It is farther expected that the moving force should require no new supply of *materials* to keep it in action. In the steam engine, the moving force is within the machine; and its agency may be continued without interruption. But a constant supply of fuel is necessary, to keep it in operation; and there is a continual demand for labor, to furnish the fuel. To save this expense of materials and labor, is the very purpose for which perpetual motion is wanted.

It is this which renders the proposed invention so highly important. Extravagant as the expectations of many ardent inventors may appear, there is reason to believe, that in this case, they would be more than realized, if the long sought for principle could once be laid hold of, and brought into general operation. It would be of no great use perhaps, to produce an instrument which would *merely go*, a wheel, for instance, which would revolve continually, without having force enough to result in any valuable product. A sawmill which would run of itself would be an object of curiosity; but would be of very little practical use, if the saw merely played up and down, without cutting the timber. The force which is necessary to put a machine in motion is generally much less, than that which is requisite to accomplish the object for which the machine is constructed. But there is reason to believe that if a perpetual moving force could once be discovered, it might be increased, to any desirable extent:

that if a power could be found which would continually overbalance the resistance of the air, and the friction of the parts of the apparatus, it might be so multiplied, as to produce any of the effects for which force is required in the arts. If for instance the moving principle were a weight, and ten pounds could be so applied as to preserve an uninterrupted motion of the machine, a hundred pounds would accomplish much more. As the weight might be easily enlarged, to almost any extent whatever, there would scarcely be any bounds to the effects to be produced. The vast expense now incurred, in providing reservoirs of water, in producing steam, in procuring labor, &c., as moving forces, might be wholly dispensed with. In the various manufacturing establishments, in this country, and in Europe, millions of dollars might be saved, in a single year. The invention might be considered a mine of wealth, even to a nation. It would not, perhaps, necessarily follow, that a power which would put in operation a *standing* machine, as a mill or manufacturing establishment, would be adapted to the *progressive* motion of a plow or a coach. But it would seem less difficult to make such an application of a moving force, than to originate the force itself. If this additional improvement could be effected, our oxen and our draft horses might be relieved from their labors; our carriages would be seen rolling through the streets of themselves; and we should be brought near to the state of perfectibility predicted by Godwin, when a plow need only to be let loose into a field, to accomplish the work of tillage.

Mankind have not been insensible to the immense advantages to be derived from perpetual motion. There is perhaps, no one subject on which the inventive faculties have more frequently been called into exercise. It is doubtful whether a single year, or even a single week has passed, for centuries, in which it has not occupied the earnest attention of some one. Many are probably employed in the search, who never disclose their attempts to the public. They have a double motive for keeping the secret; on the one hand, to secure to themselves the rewards of their success; on the other, to conceal their disappointment, in case of a failure. Persons of very various descriptions and occupations have been engaged in the pursuit. The man of science and the illiterate mechanic, have both eagerly contended for the prize, and generally with equal success.

It is natural to inquire, what can be the cause which has produced such universal failure, where the zeal for attaining the object has been so ardent, and so long continued. What are the difficulties which, with so provoking a pertinacity, unceasingly throw themselves in the way of the sanguine inventor? It would be in vain to attempt to enumerate *all* the obstacles which a subject so fruitful in disappointments presents. They will vary according to the means which are used to effect the object, and according to the characters of the different projectors. The man of science will be liable to embarrassments of one kind; the mere mechanician, to those of another. The chief impediments in the way of the man of science are certain *general principles*, denominated laws of nature. His ingenuity is exercised in endeavoring to evade these; but they obstinately meet him at every turn; and in spite of all his windings, he finds himself brought back to the same ground from which he started.

Nothing but what is capable of producing *motion*, can be the cause of *perpetual* motion. The moving forces which are commonly applied to machines are weights, springs, running water, steam, wind, and the strength of animals. Of these, water, wind, and animal strength can not produce what is commonly meant by perpetual motion, because they are not constituent parts of the machine. They are forces applied from without. But steam, springs, and weights may belong to the machine itself. Of these, the weight of a heavy body is that which is generally made use of, to produce perpetual motion. A weight is a very simple moving force, and may be made very powerful. The only difficulty with respect to it is, that it produces its effect only by *descending;* and that when it reaches the ground, its operation ceases. If it could be made to descend and then ascend, with the same force, it might keep a machine in perpetual action.

The difficulty is not, as some seem to have supposed, that a weight can move a body in one direction only. It can produce motion in *any* direction; not indeed by mere pressure, but by the aid of some very simple apparatus, for instance, a cord passing over a pulley. Nothing is easier than to change the direction in which a given power is to act upon the object to be moved. A weight may make a body either ascend, or descend, or move horizontally. But the real difficulty is, that the power will not move at all, after it has reached the ground. Its operation then ceases entirely. The great thing wanted, is not a change in the direction, but a *continuance* of the motion.

Neither does the obstacle to perpetual motion consist in the law, that matter is incapable of originating motion in itself, or of effecting any *change* in its own motions. The difficulty lies not in *beginning* the motion, but in rendering it perpetual. The labor and expense of merely putting a machine in operation would be of no account, if the movement would only continue. Now the first law of motion, instead of obstructing the continuance of the action, is the very principle on which that continuance depends, unless there is a renewed application of external force. There is one known instance of actual perpetual motion, the revolution of the heavenly bodies. This is owing to the fact, that they are incapable of putting a stop to their own motion, and that there is nothing else to stop them. But if matter were endowed, like animals, with a principle of voluntary motion, it would be as easy for it to cease to move, at pleasure, as to begin to move. It would then be as difficult to produce perpetual motion in a machine, as in a horse or an elephant.

Some of the principles which really stand in the way of the inventors of perpetual motion are the following.

1. In every machine, there is some *loss* of motion, from friction and the resistance of the air; and commonly a much greater loss, from the expenditure of force, in producing the effect for which the machine is designed. When a certain momentum is given to a body, it will continue the same, till there is some cause to vary it. A wheel suspended freely on an axis would revolve with a uniform velocity, if friction and the resistance of the air could be entirely removed. Perpetual motion would then be a thing of course. But these obstructions it is impossible to avoid; and they necessarily occasion a continual loss of motion.

In addition to this, a machine, to answer any practical purpose, must not only *go*, but it must also be capable of a constant expenditure of force upon the effect to be produced. A slitting mill, for instance, must not only be kept in motion, but must furnish a continual supply of force, to separate the iron. The loss of motion on this account is far greater than that occasioned by friction and resistance of the air. Both must be compensated by a renewal of force from some quarter or other. It is the aim of the inventor of perpetual motion, so to arrange his apparatus, that this new supply shall be furnished from the machine itself; that it shall not be dependent on any application from without. But in attempting to accomplish this object, he finds himself under the necessity of encountering a second unaccommodating principle.

2. Every body which communicates motion to another, loses an equal portion of its own motion. In other words, action and reaction are equal. It follows from this, that no portion of matter can communicate to another portion a greater momentum than it possesses itself. If that part of a machine in which the moving force begins to act could produce, in another part, a motion greater than its own, we might obtain a *multiplication* of force; and in this way, we might secure a surplus, to supply the deficiency occasioned by friction, &c. But after all the trials which have been made upon bodies in every conceivable variety of condition, they obstinately refuse to communicate what they do not themselves possess. On this fact is founded a third important principle.

3. *No combination of machinery* produces any real increase of force. This is the fact with respect to each of the instruments called mechanical powers taken singly; and it is equally true, in whatever way they may be combined with each other. The use of all apparatus of this kind is not to create force, nor to increase it, but merely to apply it. It is true, indeed, that a mechanical power may be so contrived, that a small weight may raise a very great one. But it will raise it a very short distance only. If one is a thousand times as large as the other, the latter must descend a thousand feet to raise the former one foot. So that the *momentum* of the large body is no greater than that of the smaller one. There is, therefore, no increase of force obtained.

One or two cautions are necessary, however, in estimating the velocities of the power and the weight. In the first place, the velocity is to be reckoned *in that direction* in which the moving force of the body acts. Thus if a body moves down an *inclined plane*, the moving force is gravitation, which acts towards the center of the earth. The velocity, when we are calculating the equilibrium, must be estimated in this direction. In the second place, there must be brought into the account, *that part* only of the power which is concerned in producing the effect. If a weight act *obliquely* on the arm of a lever, a part of its force will be lost. This must be thrown out of the estimate.

With these qualifications, we shall find, that however complicated may be the apparatus, the power will be to the weight in equilibrio, as the velocity of the weight to the velocity of the power. As their momenta are equal, there is no increase of force produced by the machinery. This is a proposition, however, which one who is in pursuit of perpetual motion can not be brought fully to believe, till he has learned it by mortifying experience. He expects, by some peculiar arrangement of his levers, and wheels, and inclined planes, to make it appear that this is only a *theoretical* maxim, intended to answer the purposes of speculative philosophy. He seeks after modes of combination which may vary, as much as possible, from those already in use, that he may fall upon the grand secret, in some mysterious disposition of parts, which has hitherto escaped observation. But the effect of all this is commonly to *diminish* the power which he wishes to increase. For although, according to the received laws of mechanics, no combination of machinery will create force; yet it is very easy, by an unskillful arrangement, to destroy motion.

After all, the projector finds an unfailing source of motion in the *weight* of a heavy body. Though he may acknowledge, perhaps, that machinery will not of itself produce force, yet he considers that *gravitation* is a power which is perpetually operating, and that, if he can only arrange his apparatus so as to catch this force, and apply it to his use, he has all that he wants. He here finds a very important exception to the general law, that a body is incapable of putting itself in motion. Matter has a continual and powerful tendency to move towards the earth. A body needs only to be left to itself, to descend with a force proportioned to its weight. But he is driven even from this refuge, by a fourth general principle.

4. A body, *by its weight*, communicates motion in no other way than by *descending;* and when it has once reached the earth, its operation ceases, till it is raised up again, with a force equal to that with which it descended. This does not mean that a body can, *in no case*, communicate motion except by descending itself. A heavy body moving in *any* direction may impel another in the same direction. A cannon ball may drive before it the object which it strikes. This is not done, however, by the weight of the ball, but by the momentum which it has received from a foreign force. What we are now considering is the motion produced by the *gravity* of the body, not that which is occasioned by the application of mechanical force from without. Neither does the principle just stated imply, that a body may not, *by its weight*, cause *another* body to move in any direction except towards the earth. The weight in one scale of a balance may cause those in the other side to rise. But to do this it must itself descend. Nor in the third place, is it to be understood, that a body can not by its weight, have an effect of *any* kind without descending. It may, even in a state of rest, have great influence in *preventing* motion. It may be a powerful *resisting* force, but it is not then a moving force. In preserving the equilibrium of bodies *at rest*, very great advantage may be derived from the application of the instruments called mechanical powers. By means of a lever or an inclined plane, one pound may be made to balance a thousand. Archimedes might have held a world in equilibrio. But the moment the lever is put in motion, the comparative importance of the smaller body is lost. What is gained in weight is lost in velocity.

With these explanations, we may consider the principle as universal, that no body produces motion by its weight, except by descending. If, for instance, a heavy body is laid upon an inclined plane, this plane may move horizontally. But unless the *weight* descends, the motion of the plane is owing to some other cause than the pressure of this weight.

Now the great difficulty in making the gravity of a body a source of perpetual motion is: that the body must soon reach the ground, that in descending it acquires only a certain degree of momentum, that it can communicate no greater momentum to the machine, and that, in order to repeat the operation, there must be a force at least equal to this, to bring it back to the height from which it has descended. There is, therefore, no force at all derived from the weight, except during the period of a single descent, and that only equal to the power which had been applied to raise it up to the point of starting. It will be said, perhaps, that the weight may be made to restore itself, that it may acquire a velocity in falling, sufficient to raise it to the same height again. So it may. The ball of a pendulum may raise itself to the same point from which it started, or nearly so. But it can do nothing more. The force acquired in the descent will be all expended in the ascent. There will be nothing left to be applied to any machinery.

There is no avoiding of this result, unless some way can be contrived to make the body either acquire a greater force by falling, or expend less in rising. No method has yet been devised, to bring a body to the ground by its weight, with a greater force than that which it acquires by falling *perpendicularly.* It may be made to roll down an inclined plane, to descend on the arc of a circle, on the arm of a lever, or along a series of lines differently inclined to the horizon. But in every such case, though it is easy to *diminish* the force of the descent; yet there is no way of *increasing* it, but by the application of a foreign impulse. On the other hand, a body can, by no device, be raised up to its original height, but by a power equivalent to that which it acquires in descending. There is, according to the received laws of mechanics, no way in which it may be brought back again, with a less expenditure of force than that which is necessary to raise it up *perpendicularly.* It may be carried round in such a manner as to employ a *greater* power; but nothing *less* than this will be sufficient to restore it.

But may not advantage be taken of some of the *mechanical powers,* to effect the object with more economy of force. Suppose the weight be made to *descend* on the longer arm of a lever, and to *ascend* on the shorter arm. If one be twice as long as the other, may not one pound raise nearly two pounds? It may. But it will raise them only half as far as it moves itself, so that nothing is gained by *this* expedient. Such is the uniform result of the projector's devices to bring up his weight with a less force than that which it acquires in falling perpendicularly. Like the stone of Sisyphus, in spite of all his efforts, it is forever rolling back upon him.

We shall be brought to the same conclusion, by another view of the subject. No body of machinery has any tendency to move by its weight, any longer than this motion will cause the *center of gravity* to descend. This is a principle of very convenient application, because it brings us at once to the result. We are under no necessity of inquiring into the peculiar *structure* of the machine. Whatever be the arrangement of the parts, as soon as the center of gravity of the whole has reached the lowest point, the motion will cease, except that it may continue a while, from the momentum already acquired.

We may, upon this principle, easily perceive the defect of those numerous perpendicular *wheels*, which have been formed with cavities in the radii or other parts, so as to contain fluids or balls rolling alternately to and from the center. The object here is to have the weights near the *axis*, while they are ascending, and near the *periphery* when descending, so as to act upon the principle of the wheel and axis. The fallacy is the same here, as in the longer and shorter arms of the lever. But to detect this, there is no necessity of examining the particular structure. Whatever be the disposition of the parts, when the center of gravity of the wheel and its contents has reached the lowest point, the tendency to move, from the weight, is at an end. The defect of the contrivance is also evident from the consideration, that as the wheel revolves, each of the balls must rise to a *certain height*, and it is immaterial by what *route* it arrives there, whether by going round on the periphery, or taking a shorter course near the axis.

Similar difficulties will be presented, if instead of the weight of a *solid* body, we substitute the pressure of a *fluid*. According to what is called the hydrostatic paradox, a pound of water may balance and set in motion a quantity ever so great. But the motion will not, perhaps, be more than a thousandth part of an inch, before the effect will cease.

After dwelling so long upon the weight, as a moving force, it will not be necessary to enter into a particular consideration of the action of *springs*. The difficulty, in the two cases, is nearly the same. A spring which is coiled up, as in a watch, produces its effect by expanding. When it is unbent, its action ceases till it is wound up again; and to wind it up requires a force equal to that with which it expands. There is, therefore, no balance left, for the purposes of machinery.

In the case of *steam*, the continuance of the motion is to be sought for, not upon mechanical, but upon chemical principles. Here perhaps there is more reason to hope, because the ground has not been so long and so thoroughly explored. But it is not improbable, that the balance of *affinities* in chemistry, will prove to be as untractable, as the balance of momenta in mechanics. A similar remark may be applicable to the imponderable agents, electricity, galvanism, and magnetism.

Some of the difficulties which lie in the way of the *man of science*, in his pursuit after perpetual motion, have now been stated. But perhaps this is not the class of persons which are most likely to succeed, in this field of investigation. The invention, if it should ever be made, may proceed from some one who has little or no knowledge of the laws of mechanics. The philosopher commences the inquiry with ardor; but soon stumbles upon one of his unyielding general principles, and then abandons the pursuit as hopeless. The uninformed mechanic is not so easily disheartened. If he fails in one attempt, he sees no reason why he should not succeed in the next. Perseverance supplies the want of skill. His very *blunders* may turn to his advantage, by leading to combinations of machinery which a person of more scientific views would have rejected without trial. *He* however is not exempt from embarrassments. They are mostly of a nature not to be particularly described. But they all terminate in one; his machine *won't go.*

It must be acknowledged, however, that although this is the general result, it is not universal. There is, here and there, a solitary exception. A man, after years of thought, and toil, and disappointment, finds at length the object of all his wishes attained. His machine goes. His labors are at an end. His fortune and his fame are secured. He has shown himself superior to all the pretenders to science and mechanical skill. His name is to go down to posterity, in the same rank with Bacon and Newton. But in the intoxication of success, one thing seems to have escaped him. He has forgotten that all *motion* is not *perpetual* motion. His machine stops. His dream is ended; and he awakes to the realities of the life of a sober mechanic.

Must we then be driven to the conclusion, that perpetual motion is absolutely and forever impossible? Shall we obstinately close our eyes and ears against all proof which may be offered of its actual existence? Shall we reject the testimony of our senses, when we see the machine really in motion? When a man professes to have made the great discovery, and calls upon us to accede to his pretensions, we have a right to suspend our opinion, till he has made good his claim, either by shewing the *principle* on which it depends, or by furnishing the proof by *actual trial.* If a new principle is advanced, in opposition to those hitherto received, we may safely admit it, when it has stood the test of as thorough an investigation as they have. The established laws of mechanics have been the subject of strict examination for centuries; they have been turned in almost every conceivable point of view, for the very purpose of eliciting from them perpetual motion; they have been scrutinized by the profoundest mathematicians, and the most skillful experimenters; they have been put to the trial, in a thousand different forms of machinery. Almost every mechanical instrument now in use exhibits experimental proof of their soundness. When any *new* principle can plead as much in its favor, it may fairly be put in competition with the old ones.

But the inventor, though he may not boast of *theory* on his side, has that which is of far greater importance, the support of *fact*. His machine *goes*. So does a time piece go. It may be made to go, for a whole year together, without being wound up. But this is not *perpetual* motion. In a clock, a force is wanted to supply the loss from friction. If the instrument is nicely constructed, this loss may be very small. A large weight may furnish the requisite supply, for a long time, without coming to the ground. But when it has descended as far as it can, its power is exhausted, and the motion ceases. To determine from trial whether any particular machine has an unfailing principle of action, we must wait till the force first applied has had time to spend itself. If we are impatient to come to a more speedy decision, there is a way in which the point may be soon settled. The apparatus may be so well adjusted, that it may take weeks or months, perhaps, to exhaust the moving force, in merely overcoming the friction. But let it be required to do something more. Instead of merely going, let it be applied to some practical purpose. Let it be employed, for instance, in the grinding of grain, or the sawing of timber. In proportion as the resistance is increased, the time of its action will be shortened, if, like other machines, it is dependent on a renewed application of force from without. But if it possesses a perpetuating principle, this may be made to supply that part of the loss of momentum which proceeds from the resistance of the grain or the timber; as well as that which is owing to friction and the air. In this manner, the instrument may be brought to the test of experiment, in a very short time.

With respect to any supposed invention, in years past, of an instrument for perpetual motion; we have only to inquire, whether it is now in operation, in our manufacturing establishments of every description; whether our mills, and forges, our steamboats, and our railroad cars are actually moved by it. An improvement which is to produce so complete a revolution in practical mechanics, could not long be concealed, or confined to the inventor. It would be coveted and circulated, as eagerly as a bank of guineas. A man professes to have discovered an inexhaustible treasure. He has unfolded a secret, which thousands have sought for in vain. He has opened a mine of gold, which is accessible to every one. Its value is beyond all computation. And yet, strange to tell, it lies neglected, and forgotten, neither wrought by himself, nor by any one else.

ART. XVII.—*Contributions to the Mycology of North America;* by Rev. M. J. BERKELEY, of England, and Rev. M. A. CURTIS, of South Carolina.

101. POLYPORUS RHIPIDIUS, Berk.—Ad truncos putridos. Sept. —Ohio (Lea); Santee Canal, S. C. (Ravenel.)

102. P. VALENZUELIANUS, Mont.—Ad truncos emortuos. Santee Canal;—Ravenel.

103. P. EPILEUCUS, Fr.—Ad truncos Pini. Autumno. Society Hill and Santee Canal.

104. P. RIGIDUS, Berk.—Ad lignum cariosum. Hieme. Mr. Ravenel.

105. P. (Anodermei) FISSILES, Berk. and Curt.;—albus; pileo dimidiato vertice elongato rugoso hic illic aculeato-setoso; intus fibroso-fissili insigniter zonato; poris mediis subrotundis acie obtusiuscula.—Autumno. On old stumps. Society Hill, S. Car.

Cap 6 in. broad, 2 long, or, with the vertex, 3½, undulated, with numerous raised lines which are sparingly aculeato-setose; substance splitting into fibres, zoned throughout; pores about $\frac{1}{50}$ in. broad.—A fine species belonging to the same section as P. *spumeus*, remarkable for its strongly zoned highly fibrous substance; allied to *P. borealis*, but with larger pores, firmer dissepiments, and a more fibrous texture.

106. P. SENEX, Mont.—Ad truncum. Autumno. Santee Canal, Mr. Ravenel.

107. P. VELLEREUS, Berk.—Oregon. U. S. Exploring Expedition.

108. P. LINDBLADII, Berk.—Ad truncos dejectos. Autumno. Ravenel.

109. P. LACERATUS, Berk.—Ad truncos emortuos, præsertim Quercuum. Hieme. Vere. N. and S. Car.

110. P. FLORIDANUS, Berk.—Ad truncos emortuos Myricæ, &c. Hieme. Ravenel.

111. P. ARCTICUS, Klotsch.—Ad truncos emortuos. Autumno. Hieme. N. and S. Car. et N. York. Dr. Sartwell.

112. P. (Inodermei) CHARTACEUS, Berk. and Curt.;—rigido-membranaceous ambiens e resupinato utrinque reflexus; pileo leviter zonato albido sericeo; poris sistotrematoideis pallidis. Subter ram. et trunc. dejectis Liriodendri. Vere. Hillsborough, N. Car.

Effused for several feet and completely surrounding the smaller branches, broadly reflexed; margin membranaceous but rigid; pores about $\frac{1}{50}$ in. broad, breaking into obtuse lamellar processes, extending to the margin where they are shallow and more distant.—Allied to *P. pinsitus*, of which we at first thought it was a variety, but differing in its nearly smooth pileus and the singularly decomposed pores.

113. P. NIGER, Berk.—Ad trunc. Querc. deject. cariosum. Hieme. S. Car. (Ravenel.) Ohio, (Lea.)

114. P. CREMOR, Berk. and Curt.; resupinatus albus; margine obsoleto, poris parvis subrotundis, dissepimentis crassis, acie obtusissima.—Ad ram. querc. dejectos. Vere. Society Hill.

Allied to *P. vulgaris*, but differing in its thick dissepiments and the obtuse edge of the pores.

115. P. SALMONICOLOR, Berk. and Curt.;—resupinatus crassus, mycelio mucedineo albo, poris rotundis rubellis demum elongatis purpureo-fuscis.—Ad lignum pineum adustum. Aestate. Santee Canal. Ravenel.

Effused, several inches broad, margin thin, centre thick, of a rich salmon color, at length brown. Pores small, at length torn and angular. When fresh this appears to be very tender and easily injured, in which state it becomes dark purple brown, with a resinous aspect. Not closely allied to any described species. Its nearest affinities are with *P. purpureus*, Fr. and *P. rhodellus*, Fr., or better with *P. carneofuscus*, P.

116. DAEDALEA CINEREA, Fr.—Ad trunc. dejectum. Autumno. R. Island; Mr. Bennett.

117. D. SÆPIUM, Berk.—Ad lignum Quercûs, Taxodii, &c. Autumno. Hieme. Ohio (Lea), R. Island (Mr. Bennett), N. and S. Carolina.

118. LENZITES CRATÆGI, Berk.—Ad Liquidambar. Society Hill, S. C.

119. FAVOLUS CURTIPES, Berk. and Curt.;—pileo tenui reniformi carnoso-rigido glabro; stipite brevissimo disciformi; poris mediis sinuatis plicatis.—Ad trunc. mort. Autumno. Ravenel.

Cap 2 in. broad, $1\frac{1}{2}$ long, very smooth, rigid, and tawny when dry. Pores $\frac{1}{36}$ in. broad, pale, undulated and crisped; edge white. An extremely pretty species resembling *F. cucullatus*, Mont.; but the pores are less rigid and smaller, the substance more fleshy.

120. GLŒOPORUS CONCHOIDES, Mont.—Ad trunc. prostratum. Society Hill, S. Car.

121. MERULIUS CORIUM, Fr.—Ad cort. Quercûs, Castaneæ, &c. Autumno, Vere. S. Car.

122. M. INCRASSATUS, Berk. and Curt.;—effusus resupinatus, crassus margine breviter reflexo; plicis poriformibus.—Ad lign. pineum cariosum. Society Hill, S. Car.

Effused for several inches, resupinate, with the margin shortly reflexed, dirty white and slightly silky. Substance thick, fleshy. Folds forming small brownish pores. Allied to *M. tremellosus*.

123. Porothelium Friesii, Mont.—Ad corticem Juniperi. Hieme. Santee Canal; Ravenel.

ARRHYTIDIA, nov. gen.—Hymenophorum a mycelio mucedineo contexto formatum, marginatum, tectum hymenio ceraceo molli lævi sine plicis. Sporæ oblongæ.

124. Arrhytidia flava, Berk. and Curt.—Ad ramos dejectos Pini. Society Hill, S. Car.

Forming little scattered, sometimes confluent patches, ⅛ in. broad, consisting of a white mycelium which forms a distinct border to the smooth orange yellow hymenium; sometimes the border is double. Spores oblong, fixed obliquely at the base.

This has just the habit of *Psilopezia*, but is without *Asci*. It is a distinctly bordered, mostly pezizæform *Merulius*, destitute of folds.

125. Hydnum velutinum, Fr.—Ad terram in sylvis. Hillsborough, N. Car.

126. H. graveolens, Delastre.—Ad basin caudicum in sylvis. Aestate. Society Hill, S. Car.

127. H. pulcherrimum, Berk. and Curt.;—dimidiatum crassum lobatum spongiosum carnoso-fibrosum hirsùtum intus zonatum, margine tenui; aculeis breviusculis subulatis. Ad truncos prostratos in paludosis. Aestate, Autumno. Santee Canal, (Mr. Ravenel,) and Society Hill, S. Car.

Pilei very white becoming pale tawny, subimbricated, 3–6 in. broad, 2–3 long, 1–2 thick, of a soft spongy consistence, clothed with copious stiff down which is more or less matted with age, sometimes as if gummed together. Substance slightly zoned. Teeth 1–2 lines long, tawny.—This splendid species is closely allied to *H. septentrionale*, but is smaller, far less imbricated, and clothed with much stiffer hairs. The general texture is precisely that of *Pol. hispidus*.

128. H. pithyophilum, Berk. and Curt.;—resupinatum effusum, subiculo tenuissimo farinaceo-byssoideo; aculeis ochraceis compressis apice dentato-laceris.—Ad ram. dejectos (lign. et cort.) pineos, Autumno, Hieme. Society Hill, S. C.

Forming confluent patches several inches in length. Subiculum extremely thin, sometimes farinaceous, sometimes finely byssoid, white. Teeth pale ochraceous 1 line long, more or less toothed or jagged at the apex.—Resembling resupinate states of *H. ochraceum*, but differing both in the subiculum and teeth.

129. H. ciliolatum, Berk. and Curt.;—resupinatum orbiculare tenue vix a matrice solubile; aculeis brevibus compressis floccoso-ciliolatis.—Ad ramos dejectos (lign. et cort.) Quercus et Liriodendri (?). Autumno. Society Hill.

Forming small orbicular patches, an inch or two in diameter, nearly white with a pale ochraceous tint, thin, margin minutely byssoid, sometimes slightly raised. Teeth short, minutely ciliated with fine flocci, extending to the very margin where they are merely little downy fascicles.—Resembling resupinate forms of *H. ochraceum*, but differing widely in the nature of the teeth.

130. H. PLUMOSUM, Duby.—In rimis cort. crassæ Pini. Hieme. Society Hill.

ART. XVIII.—*Experimental Researches in Electricity.—Twenty-third Series;* by MICHAEL FARADAY, Esq., D.C.L., F.R.S., etc.*

§ 29. *On the polar or other condition of diamagnetic bodies.*

2640. FOUR years ago I suggested that all the phenomena presented by diamagnetic bodies, when subjected to the forces in the magnetic field, might be accounted for by assuming that they then possessed a polarity the same in kind as, but the reverse in direction of, that acquired by iron, nickel and ordinary magnetic bodies under the same circumstances (2429. 2430.). This view was received so favorably by Plücker, Reich and others, and above all by W. Weber,† that I had great hopes it would be confirmed; and though certain experiments of my own (2497.) did not increase that hope, still my desire and expectation were in that direction.

2641. Whether bismuth, copper, phosphorus, &c., when in the magnetic field, are polar or not, is however an exceedingly important question; and very essential and great differences in the mode of action of these bodies under the one view or the other, must be conceived to exist. I found that in every endeavor to proceed by induction of experiment from that which is known in this department of science to the unknown, so much uncertainty, hesitation and discomfort arose from the unsettled state of my mind on this point, that I determined, if possible, to arrive at some experimental proof either one way or the other. This was the more needful, because of the conclusion in the affirmative to which Weber had come in his very philosophical paper; and so important do I think it for the progress of science, that in those imperfectly developed regions of knowledge, which form its boundaries, our conclusions and deductions should not go far beyond, or at all events not aside from the results of experiment (except as suppositions), that I do not hesitate to lay my present labors, though they arrive at a negative result, before the Royal Society.

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* From the Trans. Roy. Soc., London, Part I, for 1850.—Read March 7 and 14, 1850.
† Poggendorff's Annalen, January 7, 1848, or Taylor's Scientific Memoirs, v, p. 477.

2642. It appeared to me that many of the results which had been supposed to indicate a polar condition, were only consequences of the law that diamagnetic bodies tend to go from stronger to weaker places of action (2418.); others again appeared to have their origin in induced currents (26. 2338.); and further consideration seemed to indicate that the differences between these modes of action and that of a real polarity, whether magnetic or diamagnetic, might serve as a foundation on which to base a mode of investigation, and also to construct an apparatus that might give useful conclusions and results in respect of this inquiry. For, if the polarity exists it must be in the particles and for the time permanent, and therefore distinguishable from the momentary polarity of the mass due to induced temporary currents; and it must also be distinguishable from ordinary magnetic polarity by its contrary direction.

2643. A straight wooden lever, 2 feet in length, was fixed by an axis at one end, and by means of a crank and wheel made to vibrate in a horizontal plane, so that its free extremity passed to and fro through about 2 inches. Cylinders or cores of metal or other substances, 5½ inches long and three-quarters of an inch diameter, were fixed in succession to the end of a brass rod 2 feet long, which itself was attached at the other end to the moving extremity of the lever, so that the cylinders could be moved to and fro in the direction of their length through the space of 2 inches. A large cylinder electro-magnet was also prepared (2191.), the iron core of which was 21 inches long and 1·7 inch in diameter; but one end of this core was made smaller for the length of 1 inch, being in that part only 1 inch in diameter.

2644. On to this reduced part was fixed a hollow helix consisting of 516 feet of fine covered copper wire: it was 3 inches long, 2 inches external diameter, and 1 inch internal diameter; when in its place, 1 inch of the central space was occupied by the reduced end of the electro-magnet core which carried it; and the magnet and helix were both placed concentric with the metal cylinder above mentioned, and at such a distance that the latter, in its motion, would move within the helix in the direction of its axis, approaching to and receding from the electro-magnet in rapid or slow succession. The least and greatest distances of the moving cylinder from the magnet during the journey were one-eighth of an inch and 2·2 inches. The object of course was to observe any influence upon the experimental helix of fine wire which the metal cylinders might exert, either whilst moving to or from the magnet, or at different distances from it.*

* It is very probable that if the metals were made into cylinders, shorter but of larger diameter than those described above, and used with a corresponding wider helix, better results than those I have obtained would be acquired.

2645. The extremities of the experimental helix wire were connected with a very delicate galvanometer, placed 18 or 20 feet from the machine, so as to be unaffected directly by the electro-magnet; but a commutator was interposed between them. This commutator was moved by the wooden lever (2643.), and as the electric currents which would arrive at it from the experimental helix, in a complete cycle of motion or to and fro action of the metal cylinder (2643.), would consist of two contrary portions, so the office of this commutator was, sometimes to take up these portions in succession and send them on in one consistent current to the galvanometer, and at other times to oppose them and to neutralize their result; and therefore it was made adjustible, so as to change at any period of the time or part of the motion.

2646. With such an arrangement as this, it is known that, however powerful the magnet, and however delicate the other parts of the apparatus, no effect will be produced at the galvanometer as long as the magnet does not change in force, or in its action upon neighboring bodies, or in its distance from, or relation to, the experimental helix; but the introduction of a piece of iron into the helix, or anything else that can influence or be influenced by the magnet, can, or ought to, show a corresponding influence upon the helix and galvanometer. My apparatus I should imagine, indeed, to be almost the same in principle and practice as that of M. Weber (2640.), except that it gives me contrary results.

2647. But to obtain correct conclusions, it is most essential that extreme precaution should be taken in relation to many points which at first may seem unimportant. All parts of the apparatus should have perfect steadiness, and be fixed almost with the care due to an astronomical instrument; for any motion of any portion of it, is, from the construction, sure to synchronize with the motion of the commutator; and portions of effect, inconceivably small, are then gathered up and made manifest as a whole at the galvanometer; and thus, without care, errors might be taken for real and correct results. Therefore, in my arrangements, the machine (2643, &c.), the magnet and helix, and the galvanometer stood upon separate tables, and these again upon a stone floor laid upon the earth; and the table carrying the machine was carefully strutted to neighboring stone-work.

2648. Again, the apparatus should itself be perfectly firm and without shake in its motion, and yet easy and free. No iron should be employed in any of the moving parts. I have springs to receive and convert a portion of the momentum of the whole at the end of the to and fro journey; but it is essential that these should be of hammered brass or copper.

2649. It is absolutely necessary that the cylinder or core in its motion should not in the least degree disturb or shake the experimental helix and the magnet. Such a shake may easily take place, and yet (without much experience) not be perceived. It is important to have the cores of such bodies as bismuth, phosphorus, copper, &c., as large as may be, but I have not found it safe to have less than one-eighth of an inch of space between them and the interior of the experimental helix. In order to float, as it were, the core in the air, it is convenient to suspend it in the bight or turn of a fine copper wire passing once round it, the ends of which rise up, and are made fast to two fixed points at equal heights but wide apart, so that the wire has a V form. This suspension keeps the core parallel to itself in every part of its motion.

2650. The magnet, when excited, is urged by an electric current from five pairs of Grove's plates, and is then very powerful. When the battery is not connected with it, it still remains a magnet of feeble power, and when thus employed may be referred to as in the *residual state.* If employed in the residual state, its power may for the time be considered constant, and the experimental helix may at any moment be connected with the galvanometer without any current appearing there. But if the magnet be employed in the excited state, certain important precautions are necessary; for upon connecting the magnet with the battery and then connecting the experimental helix with the galvanometer, a current will appear at the latter, which will, in certain cases, continue for a minute or more, and which has the appearance of being derived at once from that of the battery. It is not so produced, however, but is due to the *time* occupied by the iron core in attaining its maximum magnetic condition (2170. 2332.), during the whole of which it continues to act upon the experimental helix, producing a current in it. This time varies with several circumstances, and in the same electro-magnet varies especially with the period during which the magnet has been out of use. When first employed, after two or three days' rest, it will amount to eighty or ninety seconds, or more. On breaking battery contact and immediately renewing it, the effect will be repeated, but occupy only twenty or thirty seconds. On a third intermission and renewal of the current, it will appear for a still shorter period; and when the magnet has been used at short intervals for some time, it seems capable of receiving its maximum power almost at once. In every experiment it is necessary to wait until the effect is shown by the galvanometer to be over; otherwise the last remains of such an effect might be mistaken for a result of polarity, or some peculiar action of the bismuth or other body under investigation.

2651. The galvanometer employed was made by Ruhmkorff, and was very sensible. The needles were strengthened in their action and rendered so nearly equal, that a single vibration to the right or the left occupied from sixteen to twenty seconds. When experimenting with such bodies as bismuth or phosphorus, the place of the needle was observed through a lens. The perfect communication in all parts of the circuit was continually ascertained by a feeble thermo-electric pair, warmed by the fingers. This was done also for every position of the commutator, where the film of oxyd formed on any part by two or three days' rest was quite sufficient to intercept a feeble current.

2652. In order to bring the phenomena afforded by magnetic and diamagnetic bodies into direct relation, I have not so much noted the currents produced in the experimental helix, as the effects obtained at the galvanometer. It is to be understood, that the standard of deviation, as to direction, has always been that produced by an iron wire moving in the same direction at the experimental helix, and with the same condition of the commutator and connecting wires, as the piece of bismuth or other body whose effects were to be observed and compared.

2653. A thin glass tube, of the given size (2643.), 5½ by ¾ inches, was filled with a saturated solution of protosulphate of iron, and employed as the experimental core: the velocity given to the machine at this and all average times of experiment was such as to cause five or six approaches and withdrawals of the core in one second; yet the solution produced no sensible indication at the galvanometer. A piece of magnetic glass tube (2354.), and a core of foolscap paper, magnetic between the poles of the electro-magnet, were equally inefficient. A tube filled with small crystals of protosulphate of iron caused the needle to move about 2°, and cores formed out of single large crystals, or symmetric groups of crystals of sulphate of iron, produced the same effect. Red oxyd of iron (colcothar) produced the least possible effect. Iron scales and metallic iron (the latter as a thin wire) produced large effects.

2654. Whenever the needle moved, it was consistent in its direction with the effect of a magnetic body; but in many cases, with known magnetic bodies, the motion was little or none. This proves that such an arrangement is by no means so good a test of magnetic polarity as the use of a simple or an astatic needle. This deficiency of power in that respect does not interfere with its ability to search into the nature of the phenomena that appear in the experiments of Weber, Reich and others.

2655. Other metals than iron were now employed and with perfect success. If they were magnetic, as nickel and cobalt, the deflection was in the same direction as for iron. When the metals were diamagnetic, the deflection was in the contrary direction; and for some of the metals, as copper, silver and gold, it amounted to 60° or 70°, which was permanently sustained as long as the machine continued to work. But the deflection was not the greatest for the most diamagnetic substances, as bismuth, or antimony, or phosphorus; on the contrary, I have not been able to assure myself, up to this time, that these three bodies can produce any effect. Thus far the effect has been proportionate to the *conducting power* of the substance for electricity. Gold, silver and copper have produced large deflections, lead and tin less. Platina very little. Bismuth and antimony none.

2656. Hence there was every reason to believe that the effects were produced by the currents induced in the mass of the moving metals, and not by any polarity of their particles. I proceeded therefore to test this idea by different conditions of the cores and the apparatus.

2657. In the first place, if produced by induced currents, the great proportion of these would exist in the part of the core near to the dominant magnet, and but little in the more distant parts; whereas in a substance like iron, the polarity which the whole assumes makes length a more important element. I therefore shortened the core of copper from 5½ inches (2643.) to 2 inches, and found the effect not sensibly diminished; even when 1 inch long it was little less than before. On the contrary, when a fine iron wire, 5½ inches in length, was used as core, its effects were strong; when the length was reduced to 2 inches, they were greatly diminished; and again, with a length of 1 inch, still further greatly reduced. It is not difficult to construct a core of copper, with a fine iron wire in its axis, so that when above a certain length it should produce the effects of iron, and beneath that length the effects of copper.

2658. In the next place, if the effect were produced by induced currents in the mass (2642.), division of the mass would stop these currents and so alter the effect; whereas if produced by a true diamagnetic polarity, division of the mass would not affect the polarity seriously, or in its essential nature (2430.). Some copper filings were therefore digested for a few days in dilute sulphuric acid to remove any adhering iron, then well washed and dried, and afterwards warmed and stirred in the air, until it was seen by the orange color that a very thin film of oxyd had formed upon them: they were finally introduced into a glass tube (2653.) and employed as a core. It produced no effect whatever, but was now as inactive as bismuth.

2659. The copper may however be divided so as either to interfere with the assumed currents or not, at pleasure. Fine copper wire was cut up into lengths of 5½ inches, and as many of these associated together as would form a compact cylinder three-quarters of an inch in diameter (2643.); it produced no effect at the galvanometer. Another copper core was prepared by associating together many discs of thin copper plate, three-quarters of an inch in diameter, and this affected the galvanometer, holding its needle 25° or 30° from zero.

2660. I made a solid helix cylinder, three-quarters of an inch in diameter and 2 inches long, of covered copper wire, one-sixteenth of an inch thick, and employed this as the experimental core. When the two ends of its wire were unconnected, there was no effect upon the experimental helix, and consequently none at the galvanometer; but when the ends were soldered together, the needle was well affected. In the first condition, the currents, which tended to be formed in the mass of moving metal, could not exist because the metal circuit was interrupted; in the second they could, because the circuit was not interrupted; and such division as remained did not interfere to prevent the currents.

2661. The same results were obtained with other metals. A core cylinder of gold, made of half-sovereigns, was very powerful in its effect on the galvanometer. A cylinder of silver, made of sixpenny pieces, was very effectual; but a cylinder made of precipitated silver, pressed into a glass tube as closely as possible, gave no indications of action whatever. The same results were obtained with disc cylinders of tin and lead, the effects being proportionate to the condition of tin and lead as bad conductors. (2655.)

2662. When iron was divided, the effects were exactly the reverse in kind. It was necessary to use a much coarser galvanometer and apparatus for the purpose; but that being done, the employment of a solid iron core, and of another of the same size or weight formed of lengths of fine iron wire (2659.), showed that the division had occasioned no inferiority in the latter. The excellent experimental researches of Dove* on the electricity of induction, will show that this ought to be the case.

2663. Hence the result of division in the diamagnetic metals is altogether of a nature to confirm the conclusion, that the effects produced by them are due to induced currents moving through their masses, and not to any polarity correspondent in its general nature (though opposed in its direction) to that of iron.

2664. In the third place (2656.), another and very important distinction in the actions of a diamagnetic metal may be experimentally established according as they may be due either to a true polarity, or merely to the presence of temporary induced currents; and as for the consideration of this point diamagnetic and magnetic polarity are the same, the point may best be considered, at present, in relation to iron.

2665. If a core of any kind be advanced towards the dominant magnet and withdrawn from it by a motion of uniform velocity, then a complete journey or *to* and *from* action might be

* Taylor's Scientific Memoirs, v, p. 129. I do not see a date to the paper.

divided into four parts; the *to*, the *stop* after it; the *from*, and the *stop* succeeding that. If a core of iron make this journey, its end towards the dominant magnet becomes a pole, rising in force until at the nearest distance, and falling in force until at the greatest distance. Both this effect and its *progression* inwards and outwards, cause currents to be induced in the surrounding helix, and these currents are in one direction as the core advances, and in the contrary direction as it recedes. In reality, however, the iron does not travel with a constant velocity; for, because of the communication of motion from a revolving crank at the machine (2643.), it, in the *to* part of the journey, gradually rises from a state of rest to a maximum velocity, which is half-way, and then as gradually sinks to rest again near the magnet:—and the *from* part of the journey undergoes the same variations. Now as the maximum effect upon the surrounding experimental helix depends upon the velocity conjointly with the intensity of the magnetic force in the end of the core, it is evident that it will not occur with the maximum velocity, which is in the middle of the *to* or *from* motion; nor at the *stop* nearest to the dominant magnet, where the core end has greatest magnetic force, but somewhere between the two. Nevertheless, during the *whole* of the advance, the core will cause a current in the experimental helix in one direction, and during the whole of the recession it will cause a current in the other direction.

2666. If diamagnetic bodies, under the influence of the dominant magnet, assume also a polar state, the difference between them and iron being only that the poles of like names or forces are changed in place (2429. 2430.), then the same kind of action as that described for iron would occur with them; the only difference being, that the two currents produced would be in the reverse direction to those produced by iron.

2667. If a commutator, therefore, were to be arranged to gather up these currents, either in the one case or the other, and send them on to the galvanometer in one consistent current, it should change at the moments of the two *stops* (2665.), and then would perform such duty perfectly. If, on the other hand, the commutator should change at the times of maximum velocity or maximum intensity, or at two other times equidistant either from the one *stop* or from the other, then the parts of the opposite currents intercepted between the changes would exactly neutralize each other, and no final current would be sent on to the galvanometer.

2668. Now the action of the iron is, by experiment, of this nature. If an iron wire be simply introduced or taken out of the experimental helix with different conditions of the commutator, the results are exactly those which have been stated. If the machine be worked with an iron wire core, the commutator changing at the stops (2665.), then the current gathered up and sent on to the galvanometer is a maximum; if the commutator change at the moments of maximum velocity, or at any other pair of moments equidistant from the one stop or the other, then the current at the commutator is a minimum, or 0.

2669. There are two or three precautions which are necessary to the production of a pure result of this kind. In the first place, the iron ought to be soft and not previously in a magnetic state. In the next, an effect of the following kind has to be guarded against. If the iron core be away from the dominant magnet at the beginning of an experiment, then, on working the machine, the galvanometer will be seen to move in one direction for a few moments, and afterwards, notwithstanding the continued action of the machine, will return and gradually take up its place at 0°. If the iron core be at its shortest distance from the dominant magnet at the beginning of the experiment, then the galvanometer needle will move in the contrary direction to that which it took before, but will again settle at 0°. These effects are due to the circumstance, that, when the iron is away from the dominant magnet, it is not in so strong a magnetic state, and when at the nearest to it is in a stronger state, than the *mean* or *average state*, which it acquires during the continuance of an experiment; and that in rising or falling to this average state, it produces two currents in contrary directions, which are made manifest in the experiments described. These existing only for the first moments, do, in their effects at the galvanometer, then appear, producing a vibration which gradually passes away.

2670. One other precaution I ought to specify. Unless the commutator changes accurately at the given points of the journey, a little effect is gathered up at each change, and may give a permanent deflection of the needle in one direction or the other. The tongues of my commutator, being at right angles to the direction of motion and somewhat flexible, dragged a little in the *to* and *from* parts of the journey: in doing this they approximated, though only in a small degree, to that which is the best condition of the commutator for gathering up (and not opposing) the currents; and a deflection to the right or left appeared (2677.). Upon discovering the cause and stiffening the tongues so as to prevent their flexure, the effect disappeared, and the iron was perfectly inactive.

2671. Such therefore are the results with an iron core, and such would be the effects with a copper or bismuth core if they acted by a diamagnetic polarity. Let us now consider what the consequences would be if a copper or bismuth core were to act by currents, induced for the time, in its moving mass, and of the nature of those suspected (2642.). If the copper cylinder moved with uniform velocity (2665.), then currents would exist in it, parallel to its circumference, during the whole time of its motion;

and these would be at their maximum force just before and just after the *to* or inner stop, for then the copper would be in the most intense parts of the magnetic field. The rising current of the copper core for the *in* portion of the journey would produce a current in one direction in the experimental helix, the stopping of the copper and consequent falling of its current would produce in the experimental helix a current contrary to the former; the first instant of motion *outwards* in the core would produce a maximum current in it contrary to its former current, and producing in the experimental helix its inductive result, being a current the same as the last there produced; and then, as the core retreated, its current would fall, and in so doing and by its final stop, would produce a fourth current in the experimental helix, in the same direction as the first.

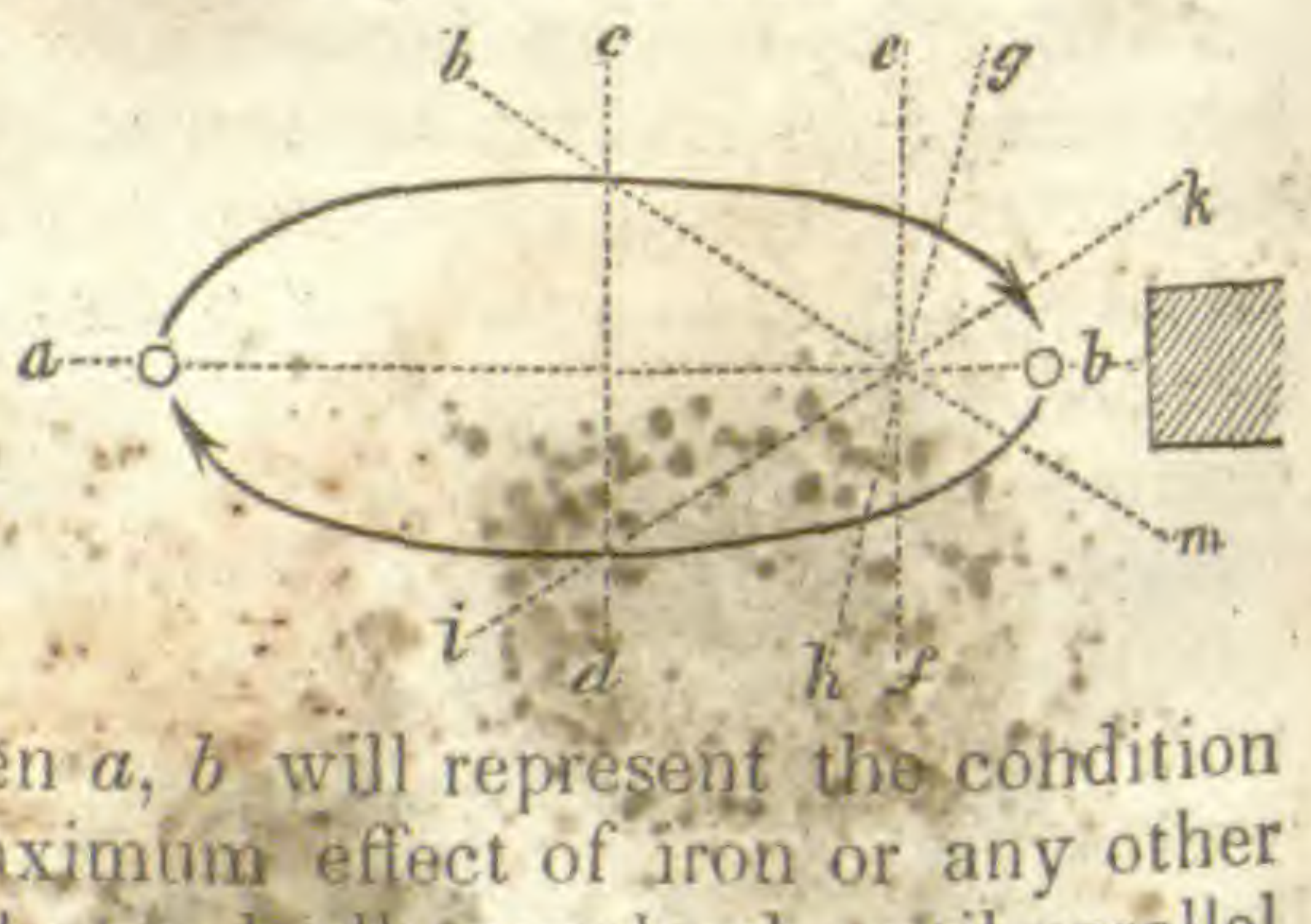
2672. The four currents produced in the experimental helix alternate by twos, *i. e.* those produced by the falling of the first current in the core and the rising of the second and contrary current, are in one direction. They occur at the instant before and after the stop at the magnet, *i. e.* from the moment of maximum current (in the core) before, to the moment of maximum current after, the stop; and if that stop is momentary, they exist only for that moment, and should during that brief time be gathered up by the commutator. Those produced in the experimental helix during the falling of the second current in the core and the rising of a third current (identical with the first) in the return of the core to the magnet, are also the same in direction, and continue from the beginning of the retreat to the end of the advance (or from maximum to maximum) of the core currents, *i. e.* for almost the whole of the core journey; and these, by its change at the maximum moments, the commutator should take up and send on to the galvanometer.

2673. The motion however of the core is not uniform in velocity, and so, sudden in its change of direction, but, as before said (2665.), is at a maximum as respects velocity in the middle of its approach to and retreat from the dominant magnet; and hence a very important advantage. For its stop may be said to commence immediately after the occurrence of the maximum velocity; and if the lines of magnetic force were equal in position and power there to what they are nearer to the magnet, the contrary currents in the experimental helix would commence at those points of the journey; but, as the core is entering into a more intense part of the field, the current in it still rises though the velocity diminishes, and the consequence is, that the maximum current in it neither occurs at the place of greatest velocity, nor of greatest force, but at a point between the two. This is true, both as regards the approach and the recession of the core, the two maxima of the currents occurring at points equidistant from the place of rest near the dominant magnet.

2674. It is therefore at these two points that the commutator should change, if adjusted to produce the greatest effect at the galvanometer by the currents excited in the experimental helix, through the influence of, or in connection with, currents of induction produced in the core; and experiment fully justifies this conclusion. If the length of the journey from the stop out to the stop in, which is 2 inches (2643. 2644.), be divided in 100 parts, and the dominant magnet be supposed to be on the right hand, then such an expression as the following, 50|50, may represent the place where the commutator changes, which in this illustration would be midway in the to and from motion, or at the places of greatest velocity.

2675. Upon trial of various adjustments of the commutator, I have found that from 77|23 to 88|12, gave the best result with a copper core. On the whole, and after many experiments, I conclude that with the given strength of electro-magnet, distance of the experimental core when at the nearest from the magnet, length of the whole journey, and average velocity of the machine, 86|14 may represent the points where the induced currents in the core are at a maximum and where the commutator ought to change.

2676. From what has been said before (2667.), it will be seen that both in theory and experiment these are the points in which the effect of any polarity, magnetic or diamagnetic, would be absolutely nothing. Hence the power of submitting by this machine metals and other bodies to experiment, and of eliminating the effects of magnetic polarity, of diamagnetic polarity, and of inductive action, the one from the others: for either by the commutator or by the direction of the polarity, they can be separated; and further, they can also be combined in various ways for the purpose of elucidating their joint and separate action.



2677. For let the arrows in the diagram represent the to and from journey, and the intersections of the lines, *a*, *b*, or *c*, *d*, &c., the periods in the journey when the commutator changes (in which case *c*, *d* will correspond to 50|50, and *e*, *f* to 86|14), then *a*, *b* will represent the condition of the commutator for the maximum effect of iron or any other polar body. If the line *a*, *b*, be gradually revolved until parallel to *c*, *d*, it will in every position indicate points of commutator change, which will give the iron effect at the galvanometer by a deflection of the needle always in the same direction; it is only when the ends *a* and *b* have passed the points *c* and *d*, either above or below, that the direction of the deflection will change for iron. But the line *a*, *b* indicates those points for the commutator with which no effect will be produced on the galvanometer by the induction of *currents* in the mass of the core. If the line be inclined in one direction, as *i*, *k*, then these currents will produce a deflection at the galvanometer on one side; if it be inclined in the other direction, as *l*, *m*, then the deflection will be on the other side. Therefore the effects of these induced currents may be either combined with, or opposed to, the effects of a polarity, whether it be magnetic or diamagnetic.

2678. All the metals before mentioned (2655.), namely, gold, silver, copper, tin, lead, platina, antimony and bismuth, were submitted to the power of the electro-magnet under the best adjustment (2675.) of the commutator. The effects were stronger than before, being now at a maximum, but in the same order; as regarded antimony and bismuth, they were very small, amounting to not more than half a degree, and may very probably have been due to a remainder of irregular action in some part of the apparatus. All the experiments with the divided cores (2658, &c.) were repeated with the same results as before. Phosphorus, sulphur and gutta percha did not, either in this or in the former state of the commutator, give any indication of effect at the galvanometer.

2679. As an illustration of the manner in which this position of the commutator caused a separation of the effects of copper and iron, I had prepared a copper cylinder core 2 inches in length having an iron wire in its axis, and this being employed in the apparatus gave the pure effect of the copper with its induced currents. Yet this core, as a whole, was highly magnetic to an ordinary test-needle; and when the two changes of the commutator were not equidistant from the one stop or the other (2670. 2677.), the iron effect came out powerfully, overruling the former, and producing very strong contrary deflections at the needle. The platinum core which I have used is an imperfect cylinder, 2 inches long and 0·62 of an inch thick: it points magnetically between the poles of a horseshoe electro-magnet (2381.), making a vibration in less than a second, but with the above condition of the commutator (2675.) gives 4° of deflection due to the induced currents the magnetic effect being annulled or thrown out.

2680. Some of the combined effects produced by oblique position of the commutator points were worked out in confirmation of the former conclusions (2677.). When the commutator was so adjusted as to combine any polar power which the bismuth, as a diamagnetic body, might possess, with any conducting power which would permit the formation of currents by induction in its mass (2676.), still the effects were so minute and uncertain as to oblige me to say that, experimentally, it is without either polar or inductive action.

2681. There is another distinction which may usefully be established between the effects of a true sustainable polarity, either magnetic or diamagnetic, and those of the transient induced currents dependent upon *time*. If we consider the resistance in the circuit, which includes the experimental helix and the galvanometer coil, as nothing, then a magnetic pole of constant strength passed a certain distance into the helix, would produce the same amount of current electricity in it, whether the pole were moved into its place by a quick or slow motion. Or if the iron core be used (2668.) the same result is produced, provided, in any alternating action, the core is left long enough at the extremities of its journey to acquire, either in its quick or slow alternation, the same state. This I found to be the fact when no commutator nor dominant magnet was used; a single insertion of a weak magnetic pole gave the same deflection, whether introduced quickly or slowly; and when the residual dominant magnet, an iron wire core, and the commutator in its position, *a*, *b* were (2677.) used, four journeys to and from produced the *same* effect at the galvanometer when the velocities were as 1 : 5 or even as 1 : 10.

2682. When a copper, silver, or gold core is employed in place of the iron, the effect is very different. There is no reason to doubt, that as regards the core itself the same amount of electricity is thrown into the form of induced circulating currents within it, by a journey to or from, whether that journey is performed quickly or slowly: the above experiment (2681.) in fact confirms such a conclusion. But the effect which is produced upon the experimental helix is not proportionate to the whole amount of these currents, but to the maximum intensities to which they rise. When the core moves slowly, this intensity is small; when it moves rapidly, it is great, and necessarily so, for the same current of electricity has to travel in the two differing periods of time occupied by the journeys. Hence the quickly moving core should produce a far higher effect on the experimental helix than the slowly moving core: and this also I found to be the fact.

2683. The short copper core was adjusted to the apparatus, and the machine worked with its average velocity until forty journeys to and from had been completed; the galvanometer needle passed 39° west. Then the machine was worked with a greater rapidity, also for forty journeys, and the needle passed through 80° or more west; finally, being worked at a slow rate for the same number of journeys, the needle went through only 21° west. The extreme velocities in this experiment were probably as 1 : 6; the time in the longest case was considerably less than that of one vibration of the needle (2651.), so that I believe all the force in the slowest case was collected. The needle is very little influenced by the swing or momentum of its parts, because of the deadening effect of the copper plate beneath it, and, except to return to zero, moves very little after the motion of the apparatus ceases. A silver core produced the same results.

2684. These effects of induced currents have a relation to the phenomena of revulsion which I formerly described (2310. 2315. 2338.), being the same in their exciting cause and principles of action, and so the two sets of phenomena confirm and illustrate each other. That the revulsive phenomena are produced by induced currents, has been shown before (2327. 2329. 2336. 2339.); the only difference is, that with them the induced currents were produced by exalting the force of a magnet placed at a fixed distance from the affected metal; whilst in the present phenomena, the force of the magnet does not change, but its distance from the piece of metal does.

2685. So also the same circumstances which affect the phenomena here affect the revulsive phenomena. A plate of metal will, as a whole, be well-revulsed; but if it be divided across the course of the induced currents it is not then affected (2529.). A ring helix of copper wire, if the extremities be unconnected, will not exhibit the phenomena, but if they be connected then it presents them (2660.).

2686. On the whole, the revulsive phenomena are a far better test and indication of these currents than the present effects; especially if advantage be taken of the division of the mass into plates, so as to be analogous, or rather superior, in their action to the disc cylinder cores (2659. 2661.). Platinum, palladium and lead in leaf or foil, if cut or folded into squares half an inch in the side, and then packed regularly together, will show the phenomena of revulsion very well; and that according to the direction of the leaves, and not of the external form. Gold, silver, tin and copper have the revulsive effects thus greatly exalted. Antimony, as I have already shown, exhibits the effect well (2514. 2519.). Both it and bismuth can be made to give evidence of the induced currents produced in them when they are used in thin plates, either single or associated, although, to avoid the influence of the diamagnetic force, a little attention is required to the moments of making and breaking contact between the voltaic battery and the electro-magnet.

2687. Copper, when thus divided into plates, had its revulsive phenomena raised to a degree that I had not before observed. A piece of copper foil was annealed and tarnished by heat, and then folded up into a small square block, half an inch in the side and a quarter of an inch thick, containing seventy-two folds of the metal. This block was suspended by a silk film as before (2248.), and whilst at an angle of 30° or thereabouts with the equatorial line (2252.), the electro-magnet was excited; it immediately advanced or turned until the angle was about 45° or 50°, and then stood still. Upon the interruption of the electric current at the magnet the revulsion came on very strongly, and the block turned back again, passed the equatorial line, and proceeded on until it formed an angle of 50° or 60° on the other side; but instead of continuing to revolve in that direction as before (2315.), it then returned on its course, again passed the equatorial line, and almost reached the axial position before it stood still. In fact, as a mass, it vibrated to and fro about the equatorial line.

2688. This however is a simple result of the principles of action formerly developed (2329. 2336.). The revulsion is due to the production of induced currents in the suspended mass during the falling of the magnetism of the electro-magnet; and the effect of the action is to bring the axis of these induced currents parallel to the axis of force in the magnetic field. Consequently, if the time of the fall of magnetic force, and therefore of the currents dependent thereon, be greater than the time occupied by the revulsion of the copper block as far as the equatorial line, any further motion of it by momentum will be counteracted by a contrary force; and if this force be strong enough the block will return. The conducting power of the copper and its division into laminæ, tend to set up these currents very readily and with extra power; and the very power which they possess tends to make the time of a vibration so short, that two or even three vibrations can occur before the force of the electro-magnet has ceased to fall any further. The effect of *time*, both in the rising and falling of power, has been referred to on many former occasions (2170. 2650.), and is very beautifully seen here.

2689. Returning to the subject of the assumed polarity of bismuth, I may and ought to refer to an experiment made by Reich, and described by Weber,* which, if I understand the instruction aright, is as follows: a strong horseshoe magnet is laid upon a table in such a position that the line joining its two poles is perpendicular to the magnetic meridian and to be considered as prolonged on one side; in that line, and near the magnet, is to be placed a small powerful magnetic needle, suspended by cocoon silk, and on the other side of it, the pole of a bar magnet, in such a position and so near as exactly to counteract the effect of the horseshoe magnet, and leave the needle to point exactly as if both magnets were away. Then a mass of bismuth being placed between the poles of the horseshoe magnet is said to react upon the small magnet needle, causing its deflection in a particular direction, and this is supposed to indicate the polarity of the bismuth under the circumstances, as it has no such action when the magnets are away. A piece of iron in place of the bismuth produces the contrary deflection of the needle.

* Taylor's Scientific Memoirs, v, p. 480.

2690. I have repeated this experiment most anxiously and carefully, but have never obtained the slightest trace of action with the bismuth. I have obtained action with the iron; but in those cases the action was far less than if the iron were applied outside between the horseshoe magnet and the needle, or to the needle alone, the magnets being entirely away. On using a garnet, or a weak magnetic substance of any kind, I cannot find that the arrangement is at all comparable for readiness of indication or delicacy, with the use of a common or an astatic needle, and therefore I do not understand how it could become a test of the polarity of bismuth when these fail to show it. Still I may have made some mistake; but neither by close reference to the description, nor to the principles of polar action, can I discover where.

2691. There is an experiment which Plücker described to me, and which at first seems to indicate strongly the polarity of bismuth. If a bar of bismuth (or phosphorus) be suspended horizontally between the poles of the electro-magnet, it will go to the equatorial position with a certain force, passing, as I have said, from stronger to weaker places of action (2267.). If a bar of iron of the same size be fixed in the equatorial position a little below the plane in which the diamagnetic bar is moving, the latter will proceed to the equatorial position with much greater force than before, and this is considered as due to the circumstance, that, on the side where the iron has N polarity, the diamagnetic body has S polarity, and that on the other side the S polarity of the iron and the N polarity of the bismuth also coincide.

2692. It is however very evident that the lines of magnetic force have been altered sufficiently in their intensity of direction, by the presence of the iron, to account fully for the increased effect. For, consider the bar as just leaving the axial position and going to the equatorial position; at the moment of starting its extremities are in places of stronger magnetic force than before, for it cannot be doubted for a moment that the iron bar determines more force from pole to pole of the electro-magnet than if it were away. On the other hand, when it has attained the equatorial position, the extremities are under a much weaker magnetic force than they were subject to in the *same places* before; for the iron bar determines downwards upon itself much of that force, which, when it is not there, exists in the plane occupied by the bismuth. Hence, in passing through 90°, the diamagnetic is urged by a much greater difference of intensity of force when the iron is present than when it is away; and hence, probably, the whole additional result. The effect is like many others which I have referred to in magnecrystallic action (2487–2497.), and does not, I think, add anything to the experimental proof of diamagnetic polarity.

2693. Finally, I am obliged to say that I can find no experimental evidence to support the hypothetical view of diamagnetic polarity (2640.), either in my own experiments, or in the repetition of those of Weber, Reich, or others. I do not say that such a polarity does not exist; and I should think it possible that Weber, by far more delicate apparatus than mine, had obtained a trace of it, were it not that then also he would have certainly met with the far more powerful effects produced by copper, gold, silver, and the better conducting diamagnetics. If bismuth should be found to give any effect, it must be checked and distinguished by reference to the position of the commutator, division of the mass by pulverization, influence of time, &c. It appears to me also, that, as the magnetic polarity conferred by iron or nickel in very small quantity, and in unfavorable states, is far more readily indicated by its effect on an astatic needle, or by pointing between the poles of a strong horseshoe magnet, than by any such arrangement as mine or Weber's or Reich's, so diamagnetic polarity would be much more easily distinguished in the same way, and that no indication of that polarity has as yet reached to the force and value of those already given by Brugmann and myself.

2694. So, at present, the actions represented or typified by iron, by copper and by bismuth, remain distinct; and their relations are only in part made known to us. It cannot be doubted that a larger and simpler law of action than any we are yet acquainted with, will hereafter be discovered, which shall include all these actions at once; and the beauty of Weber's suggestion in this respect was the chief inducement to me to endeavor to establish it.

2695. Though from the considerations above expressed (2693.) I had little hopes of any useful results, yet I thought it right to submit certain magnecrystallic cores to the action of the apparatus. One core was a large group of symmetrically disposed crystals of bismuth (2457.); another a very large crystal of red ferroprussiate of potassa; a third a crystal of calcareous spar; and a fourth and fifth large crystals of protosulphate of iron. These were formed into cylinders of which the first and fourth had the magnecrystallic axes (2479.) parallel to the axis of the cylinder, and the second, third and fifth, had the equatorial direction of force (2594. 2595. 2546.) parallel to the axis of the cylinder. None of them gave any effect at the galvanometer, except the fourth and fifth, and these were alike in their results, and were dependent for them on their ordinary magnetic property.

2696. Some of the expressions I have used may seem to imply, that, when employing the copper and other cores, I imagine that currents are first induced in them by the dominant magnet, and that these induce the currents which are observed in the experimental helix. Whether the cores act directly on the experimental helix or indirectly through their influence on the dominant magnet, is a very interesting question, and I have found it difficult to select expressions, though I wished to do so, which should not in some degree prejudge that question. It seems to me probable, that the cores act indirectly on the helix, and that their immediate action is altogether directed towards the dominant magnet, which, whether they consist of magnetic or diamagnetic metals, raises them into power either permanently or transiently, and has their power for that time directed towards it. Before the core moves to approach the magnet, the magnet and experimental helix are in close relation; and the latter is situated in the intense field of magnetic force which belongs to the pole of the former. If the core be iron, as it approaches the magnet it causes a strong convergence and concentration of the lines of magnetic force upon itself; and these, as they so converge, passing through the helix and across its convolutions, are competent to produce the currents in it which are obtained (2653. 2668.). As the iron retreats these lines of force diverge, and again crossing the line of the wire in the helix in a contrary direction to their former course, produce a contrary current. It does not seem necessary, in viewing the action of the iron core, to suppose any direct action of it on the helix, or any other action than this which it exerts upon the lines of force of the magnet. In such a case its action upon the helix would be indirect.

2697. Then, by all parity of reasoning, when a copper core enters the helix its action upon it should be indirect also. For the currents which are produced in it are caused by the direct influence of the magnet, and must react equivalently upon it. This they do, and because of their direction and known action, they will cause the lines of force of the magnet to diverge. As the core diminishes in its velocity of motion, or comes to rest, the currents in it will cease, and then the lines of force will converge; and this divergence and convergence, or passage in two directions across the wire of the experimental helix, is sufficient to produce the two currents which are obtained in the advance of the core towards the dominant magnet (2671. 2673.). A corresponding effect in the contrary direction is produced by the retreat of the core.

2698. On the idea that the actions of the core were not of this kind, but more directly upon the helix, I interposed substances between the core and the helix during the times of the experiment. A thick copper cylinder 2·2 inches long, 0·7 of an inch external diameter, and 0·1 of an inch internal diameter, and consequently 0·3 of an inch thick in the sides, was placed in the experimental helix, and an iron wire core (2668.) used in the apparatus. Still, whatever the form of the experiment, the kind and amount of effect produced were the same as if the copper were away, and either glass or air in its place. When the dominant magnet was removed and the wire core made a magnet, the same results were produced.

2699. Another copper lining, being a cylinder 2·5 inches long, 1 inch in external diameter, and one-eighth of an inch in thickness, was placed in the experimental helix, and cores of silver and copper five-eighths of an inch in thickness, employed as before, with the best condition of the commutator (2675.): the effects, with and without the copper, or with and without the glass, were absolutely the same (2698.).

2700. There can be no doubt that the copper linings, when in place, were full of currents at the time of action, and that when away no such currents would exist in the air or glass replacing them. There is also full reason to admit, that the divergence and convergence of the magnetic lines of force supposed above (2697.) would satisfactorily account for such currents in them, supposing the indirect action of the cores were assumed. If that supposition be rejected, then it seems to me that the whole of the bodies present, the magnet, the helix, the core, the copper lining, or the air or glass which replaces it, must all be in a state of tension, each part acting on every other part, being in what I have occasionally elsewhere imagined as the electro-tonic state (1729.).

2701. The advance of the copper makes the lines of magnetic force diverge, or, so to say, drives them before it (2697.). No doubt there is a reaction upon the advancing copper, and the production of currents in it in such a direction as makes them competent, if continued, to continue the divergence. But it does not seem logical to say, that the currents which the lines of force cause in the copper, are the cause of the divergence of the lines of force. It seems to me, rather, that the lines of force are, so to say, diverged, or bent outward by the advancing copper (or by a connected wire moving across lines of force in any other form of the experiments), and that the reaction of the lines of force upon the forces in the particles of the copper causes them to be resolved into a current, by which the resistance is discharged and removed, and the line of force returns to its place. I attach no other meaning to the words *line of force* than that which I have given on a former occasion (2149.).

Royal Institution, Dec. 14, 1849.

ART. XIX.—*Notices regarding the plants yielding the fibre from which the Grass-cloth of China is manufactured.* Communicated by DR. D. J. MACGOWAN,* in a letter to Sir JAMES HUME, Hon. Sec. of the Agric. Society of India.

[IN consequence of communications received last year from Dr. Campbell, Superintendent of Darjeeling, and Mr. T. F. Henly, (Journal, vol. vi, part 1, p. 30,) respecting the superior character of the fibre of the *Kunchoora* of Rungpore, (*Urtica tenacissima*, Roxb.,) in which some observations were made regarding its probable identity with the plant yielding the material for the well known and valuable "grass-cloth" of China,—the Society was induced to refer the matter to Dr. Macgowan, at present stationed at Ningpo, to whose obliging kindness it is indebted for the following interesting particulars, which have been obtained partly by correspondence with scientific friends in other parts of the empire, partly by consulting botanical works and enquiring among the natives, and also from personal observations. It is worthy of remark, that the Society's museum contains several specimens of the fibre of *Urtica tenacissima*, in various stages of manufacture, received from Assam and Cachar, where it is known by the name "*Rheea*;" from the Shan country, where it is called "*Pan*;" and from Ava and the Tenasserim provinces. The plant is commonly met with in those countries; and it is also found in the Straits' Settlements, where it is called "*Ramee*." (See Low's work on Penang and Province Wellesley.) All the correspondents of the Society, from whom specimens have been received at various intervals during the last ten years—Major Jenkins, Mr. Landers, Colonel Burney and Major Macfarquhar, speak in the same terms respecting the great strength, durability and fineness of this fibre. The Shans are stated to use it for every kind of cordage, but the Assamese and natives of Rungpore and Dinagepore, employ it merely for manufacturing into towing lines and fishing nets. The success which has attended the experiments made in Leeds and other towns in Great Britain to manufacture cloth from a mixture, in about equal proportions, of Chinese grass and sheep's wool, having led to a great demand for the former article in a raw state, it becomes a useful subject of enquiry whether the *Kunchoora* of Rungpore, and *Rheea* of Assam, could not be made a profitable article of export in competition with the China material.—*Eds. of Jour. of Agric. and Hor. Society.*]

Sir,—The inquiries you have done me the honor to propose in behalf of the Agricultural Society of Bengal, respecting the plant

* From the Journal of the Agricultural and Horticultural Society of India, for 1848.

from which "grass-cloth" is manufactured, embrace more than can be satisfactorily answered at the present time. The subjoined account, though meagre, and in several particulars incomplete, will be found to contain much of the information you seek. It is cultivated in this vicinity, but as it is of an inferior quality, and does not flower until autumn, my description is less complete than if written in a more favored locality, or at a more advanced season of the year.

Description and History.—Grass-cloth is manufactured from the fibres of a plant, called by the Chinese *Má;* it is a generic term, under which several varieties, if not species, are included, amongst these the *Tung Má*, *Pi Má*, *Sing Má*, *Tien Má*, and others are used only as therapeutic agents. Cloth is manufactured from the *Chú Má*, *Tá Má*, *Kin Má*, *Luh Má*, *&c.* There is also a species of grass-cloth made from the *Kóh:* all these have likewise a place in the pharmacopœia of China. In imitation of the native botanists from whose works this account has been mainly derived, I shall principally limit my remarks to a description of the *Chú Má*, which belongs to the natural order of *Urticeæ*—it is a *Cannabis* or hemp, but differing from *Cannabis sativa*, sufficiently to warrant another designation. Perhaps until it becomes better known, it may be called *Cannabis sinensis.* It has an irregular cellular root, of a yellowish white color, which sends up annually ten to fifteen, or more stems, to the height of from 7 to 10 feet. The stems are upright, slightly fluted, pilous, and herbaceous: its leaves are on long petioles, alternate, ovate, roundish, serrate, simple; the upper surface pilous and dark green, the lower of a silvery-grey. The flowers are described as minute, numerous, of a light green color, on a catkin-like receptacle or spike. It is found at the base of hills and on dry soils, from Cochin China to the Yellow river, and from Chusan to the farthest west that researches can for the present extend, and abounds chiefly in Kiangsú, Sy, Chuen, Kongnain, Chikiang, Fuchkien, and Canton provinces. Native writers do not include the latter province as its region. It is certainly remarkable, that there is no notice of the *Má* in the work to which you refer, [Fortune's Wanderings in China,] as it is cultivated extensively in many places visited by the author, and grows even on the walls of Ningpo. The plant is mentioned in the Chinese classics, and was undoubtedly cultivated and employed by them a thousand years prior to our era. It is mentioned in the *Shú King* as an article of tribute from the central part of China in the time of Yu, B. C. 2205: doubtless it came into use in far more remote antiquity. The Chinese *Herbal* says, "its origin is unknown."

Medical properties.—The root is described by writers on materia medica as innocuous, sweet to the taste, of a cold nature, and possessed of cathartic properties. The root, seeds, and leaves are all officinal. A long list of diseases are enumerated in which the plant is efficacious, but these throw no other light on its properties than to suggest it is comparatively inert. It is partly because of its not possessing the narcotic properties of the *Cannabis sativa*, that a difference is presumed to exist between them. In this connection I may remark, that grass-cloth is superior to linen for garments in hot climates, the latter being a rapid conductor of caloric is often unsafe, the former is not so good a conductor, and therefore more suitable. This may be owing either to the fact of the former being hot-pressed in a calender, (by which it is rendered compact and smooth, whilst the process to which the other is subjected for the same purpose, but partially affects it,) or to original differences in the fibres of European and Chinese linen.

Planting the seeds.—This takes place in May. Great care is first taken in the selection of seeds, and in the preparation of the soil. The seed should be gathered on the appearance of frost, those produced from a recent root are the best. After being dried they are stowed away in a basket or jar mixed with sand, or dry earth, others say moist earth. The jar is then covered with straw to protect the seeds from the cold, as if exposed to its influence they yield an imperfect plant. Before planting, the seeds are tested by immersion in water, those which float are to be rejected, those at the bottom planted. A loose dry soil is to be selected, if near a canal or rivulet it is preferable. The ground is to be well ploughed, and broken finely, manured, and then divided into beds about eight yards long, and one wide; the beds are to be raked, and afterwards made compact with a hoe. After this it is watered and left for a night: on the following day raking up and pressing down is repeated. The beds being smooth, two or three table spoonsful of seed are mixed with a bowl of earth, and sown broad-cast over half a dozen beds, then they are swept with a broom to cover the seeds. In some places the seeds are first made to sprout, and then planted in drills, which are carefully filled up. Just before the blades appear, a framework is to be constructed over the beds, on which mats should be spread to protect them from the heat of June and July. The matting must be kept moist by day and removed at night, that the blades may receive the dew of heaven. The beds are to be constantly weeded. When the plant is about two inches high, the framework and matting may be removed. When three inches high, it should be transplanted, having been well watered the night before; the blades should be taken up separately with a portion of earth and planted in a field far removed from mulberry trees, about four inches apart. It may form a border to the ceralia and vegetables, protecting them from the depredations of domestic animals, all of which avoid the *Má*. In dry weather, the field is to be watered every three or four days, until the second decade, when it may be watered every tenth day. In November and December manure it with horse or buffaloe dung, earth, straw or any rubbish, a foot or more thick, to protect it from cold. In March rake it away and expose the plant, watering it in dry weather, and using rubbish of any kind for manure. A caution is given never to use swines' dung, as it is "saltish" and hurtful to the *Má*. In the third or fourth year, some say in the second, the plant may be cut and used.

Planting the roots.—The roots are to be cut into pieces of three or four fingers' length, and are to be planted in May, half a yard apart, and watered every three or four days. On the appearance of the blades use the hoe and water them; they will be mature for cutting in the second year. In the course of ten years the roots become unfruitful, the shoots may then be cut off, and if enveloped in earth, and covered with matting, can be transplanted in places 30 or 40 inches distant. The ground should be first well prepared with manure, and freely manured afterwards: the manure being half water. Here, as before, the plants should be hoed from time to time. In many cases fresh earth, pulverized bricks, ashes, &c., are used for manure. Some years the husbandman has his crop injured by worms, he needs therefore to seek for and destroy them as they appear by picking them off. It not unfrequently happens, that the crop is in some places remarkably small, and sometimes the produce is very great without assignable cause.

Cutting the Má.—It yields three crops every year. The first cutting takes place in June. Care is to be taken not to cut the young shoots, keep therefore an inch from the ground. In a month or two, the shoots are seven or eight feet high, when the second cutting takes place: do not cut the original stem. During the latter part of September, or in October, the last cutting is performed, from which the finest cloth is made: the first being inferior, coarse, and hard. After each cutting, the plant is to be covered with manure, and watered; but not day by day unless it be cloudy. At Canton the plant is pulled up by the roots every year, from which it is evident that it differs widely from the *Má* just described. Perhaps that which is produced at Canton is *Cannabis sativa.*

Peeling the Má.—On being cut, the leaves are carefully taken off with a bamboo knife, by women and children, generally on the spot. It is then taken to the house, and soaked in water for an hour, unless it is already wet by recent showers. In cold weather the water should be tepid. After this the plant is broken in the middle, by which the fibrous portion is loosened, and raised from the stalk; into the interstice thus made, the operator, generally a woman or a child, thrusts the finger nails, and separates the fibre from the centre to one extremity, and then to the other.

The striping process is very easy. It appears to be difficult to remove the fibres from the Canton *Má*, as it is soaked in water for more than 48 hours before peeling, which is done by men. They first cut off the roots, and then separating the fibre from the stalk, strip it off by drawing it over a pin, fixed in a plank. In either process half of the fibre is taken off at one stroke. The next process is scraping the hemp, to facilitate which the fibre is first soaked in water. The knife or scraper is about two inches long; its back inserted in a handle of twice the length. This rude implement is held in the left hand, its edge which is dull, is raised a line above the index finger. Strips of hemp are then drawn over the blade from within outwards, and being pressed upon by the thumb, the pilous portions of one surface, and the mucilaginous part of the other are thus taken off. The hemp then "rolls up like boiled tendon:" after being wiped dry, it is exposed to the sun for a day, and then assorted, the whitest being selected for fine cloth.

Bleaching and dividing.—A partial bleaching is effected on the fibres, before they undergo further division, sometimes by boiling, and at others by pounding on a plank with a mallet. These operations are in some places repeated. After being dried in the sun, an important operation then succeeds by women and children, to whom is entrusted the tedious process of splitting the fibres, which they do with their finger nails. Expert hands are able to carry this division very far. When this process has been preceded by hatcheling, the shreds are finer and softer. The threads are formed into balls, and subjected to frequent soaking and washings. The ashes of the mulberry leaf are recommended to be put in the water with the hemp, others use lime, for a whole night. Some simply expose it to dew and sun. In rainy and cloudy weather, it should be exposed to a current of air in the house: moisture darkens it. The threads are now ready for splicing, the work of women and children, the labors of the agriculturist being concluded when the threads are rolled into balls, after being sized or stiffened with rice-water. Before the thread is ready for the weaver, the balls are steamed over the vapor of boiling water in a closed oven. They are then spread out to dry. The subsequent stages, until the cloth is removed from the loom, include nothing which interests, or at least instructs, artizans in the West.

Varieties of the Má.—The *Chú Má* is found wild, but in this state attains only three or four feet, and is seldom used except for twine. *King Má* grows nearly as high as the *Chú Má*, the fibre is separated only by rotting in water. The stalks are dipped in sulphur and employed for matches. Its flowers are yellow. Very coarse cloth and sandals are made from its fibre. The thread of this *Má* made in *Kongsi*, is said to be as fine as silk. *Tá Má* or *Hán* (dynasty) *Má*, and also called fine hemp, is likewise employed for making cloth and for ropes; its fibre is used as a support to the pith employed as candle wicks. *Luh Má* produces the hemp of which rice bags are manufactured, and also ropes. The *Tung Má* and the *Pi Má* are used for making pigments, one serving for cakes, the other for paper. The only other *Má* that need be named in this list is the *Chí Má*, *Sesamum indicum.* It was brought from India in the reign of Kingti, B. C. 156. It is now found in all parts of the empire. Its seeds are used in cakes, and like almost every kind of *Má*, it yields oil. *Flax.*—It would seem from various English and Chinese Dictionaries, that flax is found in China, but of the existence of *Linum usitatissimum*, I cannot discover the slightest evidence. It seems to have been confounded with the last named plant. The above are all the facts respecting the *Má* which I have been able to glean from native authors; the deficiency can only be supplied by personal observation. The *Chú Má* of this place can be inspected and described when in flower. This imperfect account of the plants producing the fibre from which the grass-cloth of commerce is manufactured (evidently a misnomer), would be yet more defective were the *Kóh* plant to be wholly omitted. It is described as a creeper, which every year springs from an immense root, and grows from ten to thirty feet in length, clinging to trees when within reach. The root is purple on the outside, and white within; it is made into flour like arrow-root. I have found it an useful substitute for the *Maranta arundinacea.* Its leaves have three points, they are long, green on the face, and bright below. In August it has blended purple flowers. The fibre is strongly adherent to the stalk, and is only loosened by boiling it in water. It is then taken off with the finger nails, exposed for a long time to running water, and beaten with mallets. It undergoes the same processes as the *Má*, but seems to require more beating and boiling: wooden utensils make it dark. The *Kóh*, like the *Má*, has been manufactured from high antiquity, and is found throughout the same extensive region in which the latter abounds. The cloth made from it is *yellow*, and as fine as ordinary grass-cloth; it cannot be bleached white; in summer it is much worn by respectable Chinese. The best brought to Ningpo, is called *Háinán Kóh* cloth, and is perhaps manufactured on the island of Háinán. If by the *Kunkhura* you mean the *Urtica tenacissima* of Roxburgh, it is probable that it is a different plant from either variety of the *Má.* As the hindrance to the manufacture of the former is owing to the difficulty attending the peeling of the fibre, would it not be well to treat it, as the Chinese do the *Kóh*, viz. by boiling? If the *Kunkhura* should prove useless, may not the catalogue of Dr. Wallich, which contains forty-seven species of *Urticeæ*, include the *Má*, the *Kóh*, or an available substitute? The observations of Dr. Roxburgh, on the various specimens of fibrous vegetables, the produce of India, may perhaps be consulted with advantage in connection with the Chinese account of the *Má.* Besides the enclosed, I shall only be able to send you some seeds of the *Chú Má.* There can be no doubt that if the seeds of the proper kind be procured from China, the plant may be introduced into India if it be not indigenous to her soil. To secure success it would perhaps be well to procure them from each of the open ports of China. From Canton is exported the finest cloth manufactured in the empire. This superiority the Chinese attribute to the greater skill of the Canton workmen. Foreigners have referred that fibre to two plants, *Cannabis sativa*, and *Sida tilæfolia* (Dr. Abel).* From Shanghai is exported the fine strong fibre you have referred to, the *Urtica nivea*. It was through the efforts of the H. E. I. Company of Canton, that seeds were procured and forwarded to England. By referring to the 47th page of the 72nd Vol. of the Philosophical Transactions, there will be found a paper on Chinese Hemp by Mr. Furgusson, and a notice of the experiments tried with the seeds in England, (vid. Encyc. Britannica.) Had they possessed the hints here extracted from Chinese writers on this plant, the experiments of the gentlemen named in the above paper would probably have been more successful. Should the Society wish to pursue the subject further, some useful information may be obtained from a series of sketches, 120 in number, illustrating the culture and manufacture of grass-cloth, by Tinqua, 12 New China st., Canton, a translation of the text accompanying the drawings may be found in the Chinese Repository for May, 1847. It was the special province of Monsieur Hedde, of the late embassy from France to China, to collect facts and specimens illustrative of the agriculture and manufactures of China. The result of his observations have, I believe, been published in Paris, containing a notice of one variety of the *Má.* Osbeck, a pupil and countryman of Linnæus, and Dr. Abel, Naturalist of Lord Amherst's embassy, have noticed the *Má.* The Agricultural Society of Bengal, aiming to develope the industrial resources of a great empire, and thereby to improve its inhabitants, has a claim upon the services of all who can contribute in any manner to its objects. Allow me to assure the Society, that I shall take great pleasure in responding to any further calls you may have occasion to make.

Ningpo, June 1st, 1848.

Note by Dr. Falconer.—As there are no specimens accompanying Dr. Macgowan's paper on the grass-cloth of China, it is im-

* It has also been referred to *Corchorus capsularis.* Vide Bennett's Wanderings in N. S. Wales and China.—*Eds. of Jour. of Agric. and Hort. Society.*

possible to decide as to the accuracy of his statement that the fibre is produced by a species of *Cannabis*, which he provisionally calls *Cannabis sinensis*. This point cannot be determined without an examination of the flowers;* and Dr. Macgowan does not appear to have seen them. But the description given by him is entirely that of the species of *Bœhmeria* (formerly *Urtica*), called *B. nivea* or *tenacissima* by botanists, or of a nearly allied species; and I am not aware of any evidence to support the idea that the China grass-cloth (*Chú Má*) is derived from a species of *Cannabis*. One of the other kinds mentioned by Dr. Macgowan, *King Má*, was forwarded to Mr. Roxburgh, from Canton, by Mr. Kerr, in 1812.

Art. XX.—*On the Quantity of Heat evolved from Atmospheric Air by Mechanical Compression;* by John Gorrie, M.D.

(Concluded from p. 49.)

The arrangement of the following tables is deemed sufficiently clear to render their object intelligible; the first column represents the date of the experiments, and the second the hours at which observations were made and the duration of the experiment. The next three columns represent a series of observed temperatures: the first gives the temperature of the atmosphere; the second shows the temperature of the water used for absorbing the heat of the condensed air, before injection; the third shows the temperature of the same water after the process of injection has enabled it to absorb all the heat it can take up from the condensed air; it also represents the sensible temperature of the air after admixture with the water of injection. Following these, are two columns; the first showing the apparent tension of the air in the reservoir, in atmospheres, as indicated by the gauge; the second the number of revolutions of the engine per minute. The three next columns represent certain quantities of air: the first gives the quantity in cubic inches condensed at each stroke of the pump; the second the quantity in cubic feet condensed per minute; the third, the quantity in pounds avoirdupois, condensed per minute. In calculating this last column I have taken 525 grains as the weight of a cubic foot of air, at a temperature of 60° F. and mean barometric pressure; and applied Dalton and Gay Lussac's law, that it dilates and contracts $\frac{1}{480}$ of its volume for every increase and diminution of its temperature of 1° F. from the fixed point of 32° F. Of the two remaining columns the first shows the

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* A second reference has been made to Dr. Macgowan on this point, and specimens will no doubt be shortly received.—*Eds. of Jour. of Agric. and Hor. Society.*

Dr. Macgowan writes that he has forwarded seeds to the Patent Office, Washington, and to the American Institute.

quantity of water in pounds, injected into the condensing pumps, at each stroke of the piston; and the last the quantity of the same water injected per minute.

TABLE I.

Experiment made at Cincinnati.

Date.	Hour.	Temperature of the			Apparent tension of air in atmospheres.	Revolutions of engine.	Quantities of the				
		Atmosphere.	Water before injection.	Water after injection.			Air condensed at each stroke of the pump.	Air condensed pr. minute in cubic feet.	Air condensed per minute in pounds.	Water injected per stroke in pounds.	Water injected per minute in pounds.
1848.	A. M.										
Nov. 21,	11.	42	45								
	11.30	"	"	51	5½	13	3144	47·305	3·627	2	52
	12.	44	"	53 –	6	15	"	54·583	4·210	"	60
	P. M.										
	12.30	"	"	53	6½	"	"	"	4·210	"	60
	1.	46	45+	54	6½	"	"	"	"	"	"
	1.30	45	45	53	6	15½	"	56·400	4·350	"	62
	2.	44	"	56	"	15	"	54·583	4·210	"	60
	2.30	"	"	53–	"	"	"	"	"	"	"
	3.	"	"	"	5½	"	"	"	"	"	"
	3.30	"	"	"	5½	"	"	"	"	"	"
	4.	"	"	" –	6	"	"	"	"	"	"
	4.30	43	"	"	5¾	"	"	"	"	"	"
	5.	43	"	"	6	"	"	"	"	"	"
Average and difference,				8–	6+	15	3144	54·583	4·210	2	60

Remarks.—In this experiment only the larger condensing pump was used.

The pump being near the steam engine furnace, the air entering it was considered 4° above the open air, (or at 60 F.)

The difference at 2 P. M., of 11° between the water before and after injection, was supposed to be owing to the imperfect working of the jet pump.

In the course of the experiment there were several stops of about 15 minutes each.

From the data presented by the table, it would seem to be an easy matter to form a quantitative determination of the heat evolved by the condensation of atmospheric air. The difference of temperature in the water of injection, before and after injection, the quantity of air condensed, and the quantity of water heated per minute, are all the elements necessary for the calculation at any given tension. By reference to columns 4 and 5, this difference of temperature, in the preceding experiment, is found to be 8° F., the quantity of air condensed per minute, is, according to column 10, 4·210 pounds, and column 12 shows that an average of 60 pounds of injection water is heated per minute. A simple multiplication of the first and last numbers, would, if they were correct, show the actual quantity of heat evolved per minute, by the beforementioned quantity of air subjected to a condensation of six atmospheres: Thus, $8^\circ \times 60 = 480^\circ$, is the number of degrees one pound of water is heated in a minute. If we divide this amount by 140° F., for the latent heat of water, and the standard measure of heat I have adopted, it will give (480÷140=) 3·429, for the number of pounds of ice that the heat would melt per minute. This product being again multiplied by 360′, for the duration of the experiment in minutes, gives (3·429×360=) 1234, or, multiplied by 1440, for the number of minutes in a day, gives (3·429×1440=) 4938, for the number of pounds of ice which the heat evolved in the preceding experiment, would melt in a day.

But there are several circumstances which interfere with this simple calculation, and which require to be considered, and their distinct effects computed before we can arrive at a just conclusion as to the value of the experiment.

1. From the construction of the perforated plates, or "roses," through which the water of injection was delivered to the air in the act of condensation, as well as from the arrangement of the pipes which transmitted the mixture of air and water to the reservoir, I can have no doubt that the latter performed its duty of absorbing as much of the heat set free by the condensation, as possible; but still it could only be a mixture, and the air had, at least, the same temperature as the water. The air was thus possessed of 8° F. more of sensible temperature than when it entered the pump. This additional heat forms an uncompensated equation of temperature of appreciable magnitude requiring an indispensable allowance for its effects. That these were very considerable was rendered evident in the subsequent expansion of the air; for, while besides a diminution of the calculated cooling effects, various parts of the engine, in which the expansion took place, equally exposed to the atmosphere and less favorably influenced for the convection of *cold*, were covered with ice, produced from the condensation of atmospheric vapor, the air-pipe, communicating between the reservoir and the engine, was free from frost or moisture during the continuation of the experiment, though, upon its ceasing, it became speedily covered with frost. In forming an estimate of the proportion which the operation of this free heat bears to the whole of the heat evolved, it seems to be necessary to take into consideration the different specific heats of air under the ordinary atmospheric pressure and the various states of condensation at which experiments may be made.

This is a difficult subject of investigation, and one on which my labors have not led me to satisfactory results, or to an arrangement of them sufficiently clear and accurate to justify my presenting them to the public. But as it is not my present object to solve all the delicate questions which this subject presents, but simply to attain such approximations to facts, as may enable practical men to render them subservient to their purposes, I shall not enter into the minute examination of this subject. A few experiments are recorded in scientific works which show that the specific heat of air, at different densities, diminishes at a much slower rate than its specific gravity: thus, according to the observations of Clement and Desormes, the specific heat for air for equal volumes, at temperatures from 32° to 140°, and barometric pressure from 3·74 to 39·6, increased in a ratio as follows, viz.

Barometric pressure.		Specific heat.
at	39·6	1·215
"	29·84	1·000
"	14·92	·693
"	7·44	·540
"	3·74	·368

From this table it is apparent that the specific heat of atmospheric air, considered in the ratio of its weight or mass, diminishes as the density increases; and it may be calculated, that when air in the same volume is condensed to double its density, its specific heat is increased from 1·000 to 1·467; when its density is quadrupled it becomes 1·883; and when its density is eight times increased its specific heat is 2·717. The densities increasing in the geometrical progression 1, 2, 4, 8, correspond nearly to the specific heats in the arithmetical series, 5, 4, 3, 2.

If the view I have taken of this law be correct, and it be applicable to an explanation of the heating operation of air in the condition referred to, we are prepared to examine the extent of its influence. Before, however, this can be determined, we must form an estimate of the whole amount of heat evolved by and at the specific heat of air, under a given degree of condensation, in order to assign the proportion that belongs to that in the reservoir. By referring to the 10th column of the preceding table, we find that an average of 4·210 pounds of air condensed with a force of six atmospheres, heats 60 pounds of water 8° F., or, that one pound of air heats 14·250 pounds of water 8° F., or, that one pound of air heats one pound of water 114° F. As the specific heat of air at ordinary temperature and pressure is 2669, one pound of it, in evolving heat enough to warm an equal quantity of water, 114° F., must set free what would heat itself, 427° F., besides the 8° F., which, in the experiment, it retained above the atmospheric temperature. Under a condensation of six atmospheres the specific heat of air, we have seen, is about 2·500; and this amount must be multiplied by 8° F., for the extra temperature of the air in the reservoir, making 20° F. to enable it to represent its proportional heating effect. Now, the proportion of 20° F. (the equivalent to the free heat in the reservoir) to 427, (the remainder of the heat evolved by condensation) is, nearly, as 1 to 21; and, therefore, the relation of the heat retained by the air, after admixture with the water of injection, is equal to about one twenty-first part of the 8° F., which the water of injection absorbs, or is about the $\frac{38}{100}$ of a degree of Fahrenheit at the specific heat of water.

2. Another correction of the table is necessary on account of the heat of condensation, which the water of injection is unable to absorb, imparting an increased elasticity to the air in the reservoir. This air being confined within fixed bounds, the effect of the heat is to increase its pressure on the surface by which it is confined, instead of the natural one of enlarging its dimensions. The consequence is that the gauge of the reservoir marks a higher tension than it would have if all the free heat of the air were absorbed. The actual changes of elasticity which are produced by mechanical condensation, without allowing the compressed air to lose any heat after compression, and for all proportions in which that heat is retained, have been mathematically investigated. They are expressed, however, by theorems too abstruse, in comparison with the importance of the object to be at present attained, to find a place in this communication. Possessing a more important bearing upon the quantity of cooling effect produced by the expansion of condensed air, and still more in a relation with the quantity of mechanical power consumed in the condensation of air, they will be examined more minutely when I come to consider these portions of my subject. At present I deem it sufficient to say that, from the partial calculation I have made, I feel authorized to consider that 8° F. of unabsorbed heat add to the elasticity of air, under a tension of six atmospheres, about one-third of an atmosphere of pressure.

3. Radiation is a cause of diminution of the apparent heating effect from the condensation of air, requiring a correction for perfect accuracy. The loss of heat from this cause is dependent somewhat upon the nature of the material and extent of surface, but more upon the difference between the temperature of the hot body, and that of the surrounding medium. As this difference, in the case before us, is but 8° F., the rate at which heat is lost must be very small, and scarcely appreciable by instruments.

4. A correction of an opposite character must be made for the heat generated by the friction of the piston and piston rod of the pump. Attempts were made, in the course of this and other experiments, to ascertain the precise quantity of heat generated by this means, but they were not successful. Upon propping open the induction valves of the pump, so as to allow free ingress and egress to the air, while the piston worked and the water of injection flowed as usual, I could not perceive by the thermometer, any change in the temperature of the water. Yet all experience

proves that heat is generated by friction; and Rumford has shown that the quantity is very considerable. According to an experiment directed towards attaining the maximum heating effect from this cause, he found that a mechanical force of one horse power evolved heat enough to raise the temperature of 26½ pounds of water 90° F. in 2½ hours; which, it may be easily calculated, is sufficient to melt 6⅝ pounds of ice per hour. I consider that upon the principle on which the friction of the steam engine and similar machines is usually calculated, the condensing pump, used in this experiment, consumed nearly one-half of a horse power. If this assumption be correct, and Rumford's experiment be considered a standard formula for such calculations, the heat generated by the piston and piston rod must have been equivalent to the fusion of about eighty pounds of ice per day; or was equal to an increase of temperature of $(80 \div 5000 = 8° \div 625)$ ·125 of a degree.

Applied to the data of the preceding table, these corrections will make the whole amount of heat evolved by air, compressed with a force of six atmospheres, as follows:

Heat as shown by the water of injection, ...	8°
" added, for correction 1, ...	·375
" " " " 2 } estimated at . . .	·125
" " " " 3 }	
	8°·500
" deducted, for correction 4, ...	·125
	8°·375

Now, as 60 pounds of water were heated 8°.375 per minute, heat enough must have been disengaged, by the machine, to melt $(60 \times 8°·375 \div 240 =)$ 3·575 pounds of ice per minute or $(3·575 \times 144 =)$ 5660 pounds per day.

Having explained in a general way the principles concerned in correcting the data, furnished by experiment on our mode of determining the heat evolved by the condensation of air, we are prepared both to exhibit and correct any number of experiments. The principles are seen to be general conclusions, affording approximations only to the position in which truth lies, and requiring modifications before they can be considered representations of actual facts. They are therefore of value, chiefly, as guides in future experiments, by which they may be confirmed, or, if erroneous, rendered accurate. Such as they are, I have applied them to the correction of the following tables.

TABLE II.

Experiments made in New Orleans.

Date.	Hour.	Temperatures of the			Apparent tension of air in atmospheres.	Revolutions of engine.	Quantities of the				
		Atmosphere.	Water before injection.	Water after injection.			Air condensed at each stroke of the pump.	Air condensed per minute in cubic feet.	Air condensed per minute in pounds.	Water injected per stroke in pounds.	Water injected per minute in pounds.
1849. June 23	A. M. 10	82	79	84	2	22	3144 } 206 }	85·300	6·093	2	88
" 29	P. M. 1.	82	79	82	"	18		69·791	4·985	2	72
" "	1.30	"	79	84	"	"	"	"	"	"	"
" "	2.	83	79	84	"	"	"	"	"	"	"
July 25	A. M. 7.30	79	76	80	"	17	"	65·914	4·708	2	68
" "	8.	82	79	84	"	"	"	"	"	"	"
" "	9.	"	79	84	"	"	"	"	"	"	"
" 26	8.	84	77	82	"	"	"	"	"	"	"
Average and difference, 5° –					2	18	3350	69·791	4·985	2	72

Remarks.—In these experiments both pumps were used. The observation on the experiment of June 23, was made while the reservoir was being rapidly charged to 8 atmospheres. In the other experiments the tension in the reservoir was steadily maintained at 2 atmospheres.

This table is drawn up from experiments made on four different days, and under some variations of circumstances, in regard to the temperatures of the atmosphere and water of injection, as well as the manner in which they were performed. The resulting difference between the temperatures of the water before and after injection, after the pump and its appendages were raised to a temperature common with the latter, was so nearly uniform, for all the experiments, that it may be considered identical. It shows, according to the thermometers used, that two pounds of water were heated 5° F. at every stroke of the pump; or, that 3350 cubic inches of air, compressed till its density was doubled, heated two pounds of water 5° F. This is a little above the average, and it may be more correct to consider the amount of heat 4°·75.

To this must be added for correction, 1, on account of heat retained by condensed air, ...	2·50
" " " " 2, " " of over-estimate of pressure of air, }	·175
" " " " 3, " of radiation, }	
	5·175
From this must be deducted for correction 4, on account of heat from friction, ...	42
	5·133

The causes interfering with the accuracy of operations of the present character are so delicate and intricate in their nature that it cannot be expected a few experiments should be deemed sufficient to settle them with mathematical precision; yet, we cannot err on the side of too great a quantity in assuming that fully $5\frac{1}{8}°$ is the action of the aforementioned quantity of air, condensed to double its density, from its atmospheric condition, upon the specified quantity of water.

TABLE III.

Experiments made in New Orleans.

Date.	Hour.	Temperatures of the Atmosphere.	Temperatures of the Water before injection.	Temperatures of the Water after injection.	Apparent tension of air in atmospheres.	Revolutions of engine.	Quantities of Air condensed at each stroke of the pumps.	Quantities of Air condensed per minute in cubic feet.	Quantities of Air condensed per minute in pounds.	Quantities of Water injected per stroke in pounds.	Quantities of Water injected per minute in pounds.
1849. June 23	A. M. 10.10	82	79	85	4	18	3144 206 3350	69·791	4·985	2	72
" 25	P. M. 12.30	84	78	84	"	15	"	58·015	4·144	"	60
" 29	4.	80	80–	86	"	17	"	65·914	4·708	"	68
" "	4.15	79	79	86	"	"	"	"	"	"	"
July 25	A. M. 11.30	84	79	85+	"	15	"	58·015	4·144	"	60
" "	12.	"	79	86	"	18	"	69·791	4·985	"	72
" "	P. M. 12.45	"	79	86	"	16	"	62·037	4·431	"	64
" 26	A. M. 8.	82	77	84	"	15	"	58·015	4·144	"	60
" 27	8.	"	77	83	"	17	"	65·914	4·708	"	68
" "	9.30	86	78	84+	"	16	"	62·037	4·431	"	64
" "	10.30	84	78	84+	"	15	"	58·015	4·144	"	60
" "	M. 12.	86	78	85	"	16	"	62·037	4·431	"	64
Average and difference,				7°	4	16	3350	62·037	4·431	2	64

Remarks.—The tension in the reservoir, as marked by the gauge, was seldom at exactly 4 atmospheres; it was sometimes a little above and sometimes a little below the mark.

The difference of temperature between the water before and after injection I consider as full 7° F.

From this table it may be deduced that the condensation of 3350 cubic inches of air, at the ordinary pressure, and a mean temperature of 84°, to one-fourth of its volume, disengages heat enough to raise the temperature of two pounds of water 7° F. Applying the principles of correction heretofore established, we must add for correction on account of heat retained by the condensed air, .333

7·333

Add for correction number 2 } estimated at, ·110
" " " " 3 }

7 ·443

Deduct for " " 4 heat from friction, ·83

7°·360

TABLE IV.

Experiments made in New Orleans.

Date.	Hour.	Temperatures of the Atmosphere.	Water before injection.	Water after injection.	Apparent tension of air in atmospheres.	Revolutions of engine.	Quantities of Air condensed at each stroke of the pumps.	Air condensed per minute in cubic feet.	Air condensed per minute in pounds.	Water injected per stroke in pounds.	Water injected per minute in pounds.
1849.	P. M.						3144				
June 18	1.15	80	79	87	8	20	206	77·542	5·538	2	80
							3350				
" "	2.	"	79	88	"	"	"	"	"	"	"
July 26	1.	74	77	86	"	15	"	58·015	4·154	"	60
" "	1.30	78	77	88	"	"	"	"	"	"	"
" "	2.30	82	77	86	"	16	"	62·037	4·431	"	64
" "	4.30	"	"	85	"	15	"	58·015	4·144	"	60
" "	5.30	"	"	85	"	"	"	"	"	"	"
" 27	12.30	"	78	86+	"	16	"	62.037	4·431	"	64
" "	1.30	84	81	89	"	15	"	58·015	4·144	"	60
" 28	1.40	82	78	86+	"	"	"	"	"	"	"
" "	3.15	"	"	86	"	16½	"	63·975	4·569	"	66
" "	5.30	86	79	87+	"	16	"	62·037	4·431	"	64
" "	7.45	82	78	86+	"	"	"	"	"	"	"
Difference and average,				8¼° F.	8	16	"	62·037	4·431	2	64

Remarks.—On July 26, at 1°.30′ it will be observed the difference of temperature of the water of injection, before and after injection, was 11° F. This unusual quantity of heat, as in the first experiment, was supposed to be owing to imperfect working of the jet pump, thereby causing a less than the regular qnantity of water to be injected.

As in the former tables, the data for table IV. are taken from experiments made on different days. It appears from a comparison of the fourth and fifth columns, that the difference in temperature of the water, before and after injection, several times indicates 9° F., but as it was oftener simply a full 8° F., I have not thought advisable to take 8°·5 for the mean of observation, though I feel assured there can be no exaggeration in considering it . . . . 8°·250

Add for correction number 1	·400
" " " " 2 } 3 }	·135
	8 ·785
Deduct for " 4	·125
	8 ·660

In the deductions I have made from the preceding tables, I have supposed that the allowances for the real over the apparent pressure of the air in the reservoir, and for radiation, are about equal to the opposing effect from friction. According to the mode of proceeding we shall have 8°·660 as the quantity of heat evolved from 3350 cubic inches of air as subjected to a compressing power of eight atmospheres at a temperature of about 84° F.

I have deemed it necessary, for a full elucidation of the subject before us, to append a table of deductions, drawn up from some of the data furnished by the preceding tables. In explanation of this table it may be stated that the first column is intended to represent the mean temperature of the atmosphere during the time of the experiment. Column 2 gives the mean difference between the water of injection before and after injection; and the 3d is designed to show the difference, in the same water, under the various corrections which must be made upon the simple thermometrical observations. The 4th column represents the tension of the air in the reservoir, according to observation; and the 5th the same tension as corrected for excess of temperature.

To enable us to compute the effects without great and undue labor, it is necessary to reduce both the air and the water, used in the experiments, to some common measure or weight. The pound weight as the most convenient standard, has been adopted. By reducing columns ten and twelve, of the preceding tables, to the common rate, we have obtained column 6 of the following table.

The 7th column shows the quantity of heat set free by the condensation of one pound of air at the specific heat of air. The figures in the column are ascertained by assuming that the specific heat of air—assigned by Delaroche and Berard, viz., ·2669 water being 1·0000—is correct, and then multiplying the quantity by the number of degrees, F., one pound of air heats one pound of water, under a given degree of compression, as shown in column six. It is in truth a mere representation of the intensity of heat evolved by the simple condensation of air.

Columns 8 and 9 show the quantity of heat—expressed in the number of pounds of ice it would melt—which the engine is capable of producing in twenty-four hours, at fifteen and twenty revolutions per minute; and are introduced as suggestive of pecuniary considerations.

The 10th and 11th columns show the difference in the degrees of heat, at the specific heats of water and air, which the air in certain states of condensation disengages.

As my object, in conducting the foregoing experiments, has been to furnish practical rather than scientific data, I have not thought it appropriate to introduce the calculations of the following table in full, nor have I attempted their perfect accuracy. The results set down for the tension of 3, 5 and 7 atmospheres must be considered as rather the deductions of careful conjecture than the precise answers to either observation or calculation. It is no doubt practicable to make observations which shall express a very close approximation to the actual temperature evolved from air, for all tensions; but it would require thermometers of much greater delicacy and accuracy than I could obtain, and some modifications of the mode of experimenting pursued. It is, therefore, desirable on both scientific and practical grounds, that a more extended series of experiments on air, similar to those above detailed, should be made, with such improved and superior apparatus, as experience has demonstrated is necessary, and with the care and skill which none but those practised in such matters are competent to exercise. In the mean time, the following table may be regarded as sufficiently near to truth to present a plain view of the objects for which it was drawn up; and any one who is so disposed may test its general correctness by particular examples.

TABLE V.

Deductions from Tables 1, 2, 3, 4.

Mean Temperature of					Quantity of heat set free by the condensation of						
The atmosphere.	The difference between the water before and after injection.	Corrected difference between the water before and after injection.	Apparent tension of air in atmospheres.	Real tension of air in atmospheres.	One pound of air at specific heat of water.	One pound of air at specific heat of air.	Air expressed in pounds of ice per day, at 15 revolutions of engine.	Air expressed in pounds of ice per day, at 20 revolutions of engine.	Ratio of difference of heat set free at specific heat of water.	Ratio of difference of heat set free at specific heat of air.	Remarks.
83	5°.–	5.125	2		74.	277	3112	4220	74.	277	By observation.
"	6 .5		3								" calculation.
84	7.–	7.333	4		105.	395	4474	5966	31.	118	" observation.
"	7 .5+		5								" calculation.
46	8.–	8.333	6	5.666			5150	6866			" observation.
83	8.+	8.500	7								" calculation.
"	8°.25	8.666	8	7.500	125.	472	5343	7125	20.	80	" observation.

Before adverting to the particular object for which the foregoing table was drawn up, I may mention that the question of the quantity of heat evolved by air, under a tension of eight atmospheres, was subjected to another and very simple test, which confirmed the accuracy of the deductions comprised in the last line. During an experiment of an hour's duration, the water of injection, instead of being supplied as usual by the city hydrant, was taken from and returned to a butt of about 130 gallons capacity, containing about 1100 pounds of water. At the commencement of the experiment, the temperature of the water was 77° F. (the atmosphere being 79° F.), at the end it was 112° F., the engine working twenty revolutions a minute. The quantity of ice, which the heat thus disengaged would melt, is equal to $(1100 \times 35 \div 140 =)$ 275 pounds, or for twenty-four hours 6600 pounds. There was but a very slight diminution in the rate at which the temperature of the water increased.

In corroboration of the general accuracy of the experiments it may be further mentioned, that the converse of the result here sought, viz., the quantity of heat which the condensed air in its expansion is capable of absorbing, or, in other words, the quantity of ice it is capable of producing, was, after making due allowances, sufficiently nearly equal to the quantities as set down in columns eight and nine, to prove that there has been no material error of observation or calculation.

The chief object of Table V. is to present in a form obvious at a glance, the changes of temperature which correspond to proportional changes in the density of air. Philosophers have so often found the phenomena of nature conforming, in all their circumstances, to uniform laws, that they seem to have supposed the evolution of heat, from the mechanical condensation of air, must follow some course of regular progression. The convenience to experimenters which has resulted from the establishment of that law by which aerial bodies adjust their volume and pressure exactly to each other, has induced them to hope that one equally simple would be discovered for measuring the proportion of heat set free from air by condensation. And, under the impression that such a law must exist, equal proportions of heat have been considered to be set free, under regular diminutions of volume, or increase in density of air. Thus, Dalton, finding, on experimenting with a small quantity of air, that when it was doubled in density it evolved 50° F. assumed this as the measure for every subsequent doubling of its density. Leslie, observing a similar result, from restoring dilated air to its natural density, assigned a similar quantity of heat to a similar reduction of volume; but as a law founded on such a result was incompatible with the phenomena presented by a greater increase of density, he deduced the following rule for determining the change of temperature produced by any degree of rarefaction or condensation of air, viz.: "Multiply 25 by the difference between the density of air and its reciprocal, the product will be the difference of temperature on the Centigrade scale." Thus, if the density be doubled or the rarefaction reduced to one half, $25\times(2-\frac{1}{2})=37\frac{1}{2}°$ Centigrade ($=67\frac{1}{2}$ F.) indicates the change of temperature by doubling the density or rarity of air. By this formula the quantity of heat evolved by the higher degrees of condensation is enormously increased; for if the air were condensed 30 times we should have $25\times(30-\frac{1}{30})=749°$ Centigrade (1412° F.) for the elevation of temperature. It may be remarked of this reasoning that it represents an hypothesis more nearly the reverse than in conformity with any natural law, of an increase or reduction of temperature, deducible from experiment. Mr. Ivory, acting upon a similar view of natural progression, expresses a rule for the changes of temperature, corresponding to given changes of density, by assigning 1° F. as the quantity of heat evolved from air, under a reduction of $\frac{1}{180}$ of a volume, no matter of what density. The preceding tables of experiments, however inexact they may be, are sufficiently extended and precise to prove that all the above formulæ are erroneous.

But so probable, at the outset of my experiments, did it appear that the relation between the condensation of air and the disengagement of heat, followed some law of arithmetical or geometrical proportion, and so firmly was the probability impressed on my mind, that observations of a different tendency produced only doubts of their accuracy. The quantity of heat obtained by reducing air from its atmospheric state to half its volume, I assumed as the basis of a calculation for every subsequent similar reduction, until frequent repetition of the experiment taught me the error and the necessity of a different conclusion.

An inspection of columns 6, 7, 10, 11, of Table V, will prove that all previous estimates of the heat set free by a change in the density of air, as well as all supposed natural laws of regular progression, adopted in connection with them, must be inaccurate. This apparent deviation from the general practice of nature admits of an explanation. Variations of pressure, and variations of heat evolved in consequence, follow each other as cause and effect; yet, there is an interference, particularly in respect to äerial bodies, arising from their property of elasticity, which prevents these variations from being equal in different cases. The cause of volume and the cause of elasticity are the same, and this is undoubtedly caloric; and the difference between them is dependent upon a modified affinity for this principle, which appears to increase, in proportion as the pressure the elastic body is subjected to is greater. In consequence of this growing affinity, the tension of air must increase in a much greater ratio than the quantity of heat set free during the reduction of volume.

In the experiments, as recorded, all preconceived notions of a uniform law of progression of any kind were set aside, and nothing were regarded but the quantity and tension of the materials employed, and the indications of the thermometer. The experiments, indeed, show that if there is any precise law of progression, or fixed relation between an increase of tension and the amount of latent heat set free in air by it, it must be in an inverse and rapidly diminishing ratio. Thus, the quantity of heat generated by compressing air to half its volume, is, as shown in columns 6 and 7, sufficient to elevate the temperature of an equal weight of water 74° F. and of its own body 277 degrees F. When it was reduced to one-fourth of its volume the increase of heat became for water 105° F. and for air 395° F.; and when condensed to one-eighth of its original volume, the heat was, for water, 125°, and for air 472° F.

According to these observations and deductions, while the densities of air increased in the geometrical progression 2, 4, 8, the heats evolved corresponded, nearly, to the arithmetical series 3, 4, 5. But the ratio in the differences of temperature between the assigned densities followed a very different rate of progression from either; thus for the densities 2, 4, 8 atmospheric pressures, the corresponding differences of heat evolved were in the decreasing numbers, nearly, (277, 118, 80) 3·5, 1·5, 1.

I deem it highly probable that the foregoing deductions are very near to true expositions of the relations between the condensation of atmospheric air, and the evolution of its latent heat. The discrepancies between the actual observations and the numbers which should belong to a law of progression, are more reasonably to be referred to error in the former than in the latter. A slight examination of the tables will shew how very important it is in such experiments, that the instruments for making observations should be both delicate and accurate. It has been mentioned that the thermometers used could not be relied upon within half a degree of their indications; while an error to this extent would produce a difference in the deductions of the first line of Table V, of one-tenth part and, of course, materially affect the value of the experiment. Besides the necessity of proper philosophical instruments, the series of observations on both densities and temperature ought to have been more extended to justify the attempt to establish a natural law from their deductions. So well aware was I of the importance of an extended succession of observations, that in drawing up the design of the machine, I endeavored to adjust its proportions so that it should be capable of working to a tension of sixteen atmospheres; but on trial, many portions of it were found too weak to sustain such a pressure. I will, however, repeat that such experiments as the machine admitted of being made, have been made under a full conviction of their importance, and with a determination that nothing should be noted which my own senses did not perceive, nor set down but as the result of repeated and confirmed observation; and, notwithstanding the imperfection of instruments and machinery, I am of opinion that they cannot be in error to any considerable extent, nor can I hesitate to recommend them to public confidence.

Apalachicola, March 7, 1850.

ART. XXI.—*Notice of Remains of Vertebrated Animals found at Richmond, Virginia;* by JEFFRIES WYMAN, M. D.

THE remains which are the subject of this notice were discovered in the tertiary beds which form the foundation of the city of Richmond, and which extend beyond its limits over a vast area. Mr. Conrad regarded these beds as identical with the English crag under the name of "medial pliocene," and Professors W. B. and H. D. Rogers describe them under the name of Miocene.* The opinion of these last geologists is confirmed by the observations of Sir Charles Lyell.† Prof. W. B. Rogers informs me in addition that in the bottom of some of the ravines, a stratum exists containing Eocene fossils.

A large proportion of the bones and teeth here described were discovered and exhumed by Dr. Martin Burton of Richmond, in the various ravines which intersect this city and its vicinity, but more especially those at its eastern and western extremity. The largest ravine, the one through which Shockoe creek makes its way into the James River, is about one hundred feet in depth and many hundreds in breadth. From this there branch off on either side many smaller ravines with nearly vertical walls. The most remarkable of these are on the western side. On the eastern slope there are none so deeply cut, but on the bottom of them may be seen a layer of silicious animalcular deposit which was noticed by Profs. Rogers. Prof. J. W. Bailey of West Point, has shown that it contains many species of infusorial forms in vast numbers.‡ This stratum, according to Prof. W. B. Rogers, is near the lower limit of the Miocene in that vicinity.

In the Miocene deposits are found the remains of Vertebrata in great numbers, especially of Sharks and of a small species of Myliobatis. The infusorial stratum and the strata immediately above it contain stones of very small size only, and give no evidence of rapid currents of water or of heavy waves; every thing seems sedimentary in its character. More superficially is a deposit of clay containing casts of Pecten in great abundance, and above this, rounded stones become quite numerous, and are used for the purpose of paving. On the recent removal of a portion of Mayos hill, a stratum of dark blue marine mud was exposed in which were discovered pieces of drift wood, still preserving their general characters, and even their microscopic structure, though so soft and decayed as to yield to very slight pressure from the fingers; its tenacity was so slight that it became difficult to remove the pieces without breaking. In some instances they were

* Trans. Am. Philos. Soc., Philad., 1836.
† Proceedings Geolog. Soc., Lond., Feb. 26, 1845.
‡ See Prof. Bailey's several articles in Am. Jour. of Science.

perforated with canals similar to those made by the Teredo. If the wood thus discovered were allowed to dry in the open air, as its moisture evaporated, it became more and more solid, and soon assumed the constitution of lignite, having the density, brittle fracture and shining surface of coal. The progress of this change I had an opportunity of noticing in a piece of wood which lay on my table from the time it was exhumed until it was completely converted into lignite. Fruits well preserved were occasionally found, and in one instance a nut which was recognized by Prof. Agassiz as belonging to the genus Carya or hickory. I have as yet seen but very few unequivocal remains of reptiles. These consist of a few teeth which will be noticed in a subsequent portion of this paper. Coprolites have also been found in great numbers some of them of unusual large size.

Remains of Mammals.—*Seals.* Among the most interesting of the relics discovered by Dr. Burton were parts of the cranium of an animal belonging to the natural family of Phocidæ, a family of which but few remains had been previously detected, and in so far as I have been able to find any record, only in one other locality in the United States. The bones were fragile, and had evidently been crushed previous to exhumation. The pieces in my possession consist of two temporal bones nearly entire, a fragment including a portion of the parietal and occipital bones, and in addition a part of the base of the skull. The reëntering angle of the occiput, the well marked depressions corresponding with the cerebral convolutions on the parietal bones, the form of the cranial cavity, the deep fossa above the internal auditory foramen, the vascular canals opening on the occiput, and the inflated tympanic bones, all indicated an affinity to the Phocidæ. The size varied but little from that of the common Harp seal, (P. Groenlandica.) The presence of an interparietal crest indicating a large development of the temporal muscles, offers a diagnostic sign by which it may be distinguished from P. barbata, P. Groenlandica, P. hispida, P. mitrata, and P. vitulina. From those species of seals which are provided with a crest the fossil presents a well marked difference in having the mastoid process much larger, more rounded, and prominent, nearly equalling the tympanic bone in size. The entrance to the carotid canal is in full view when the base of the skull is turned upwards. The imperfectly divided canal which lodges the Eustachian tube and the tensor tympani muscle is of remarkable dimensions, especially when compared with that of P. Groenlandica. The interparietal crest extending from the occiput to the anterior edge of the frontals, is most narrow posteriorly where it is but slightly elevated above the surrounding bones.

The fragments of cranium above described were found in the Shockoe creek ravine near the base of Church Hill. In the ravine at the eastern extremity of the city and in the neighborhood of the penitentiary, Dr. Burton obtained several other portions of the skeleton of another seal. These consisted of an imperfect cervical vertebra, a lumbar vertebra nearly entire, a fragment of the sacrum, coccygeal vertebra, fragments of ribs and the lower extremity of a fibula. Their generic characters have been satisfactorily made out by comparisons with recent bones.*

In figure 1, page 232, I have represented the coccygeal vertebra which corresponds in its general characters very accurately with recent bones of P. Groenlandica from the same region of the vertebral column. The small size of the vertebral canal and the imperfect transverse process, the wide spread articulating processes and the blunted spinous process indicate its affinity to the seals.

The fragment of a left fibula (figs. 2 and 3) presents at its lower extremity (fig. 3), an oblique regularly concave articulating surface, on its inner face, and on its outer (figs. 2 and 3,) an elevated ridge or crest on either side of which is a groove for the passage of a tendon.

Phocodon, Agassiz.—Of this interesting genus, (which is so nearly allied with *Squalodon* of Grateloup, *Basilosaurus* of Harlan, *Zeuglodon* of Owen and J. Müller, *Dorudon* of Gibbes, and the remains from which the empirical *Hydrarchos* of Koch was made, I have received from Dr. Burton only a single tooth, which was identified by Prof. Agassiz.†

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* The only other locality for fossil bones of seals which I have seen noticed as occurring in the U. States, is the one mentioned by Dr. C. T. Jackson (in his final Report on the Geology of New Hampshire) at South Berwick, Maine, in marine mud, about thirty feet below the surface, and which were brought to light on digging a well. These were sent to me for description and proved to be an ulna and a radius.

Cuvier in the last edition of the Ossemens Fossiles (t. viii, p. 454) after having set aside many pretended discoveries, says: "en effet rien n'est plus rare que des os de Phoques, et de Lamantins parmis les fossiles." "Je n'ai pu obtenir d'ossemens fossiles de phoques bien' constatés que des seuls environs d'Angers."

The fragments described by Cuvier as belonging to two individuals, were subsequently asserted by Blainville to belong to one and the same bone, and were shown to be those of a Manatee instead of a Seal, a discovery on which M. de. B. dwells with peculiar satisfaction. (Osteographie, Genus Phoca, p. 40.)

Among the veritable discoveries of the remains of fossil seals, Blainville cites the following, (Osteograph., Gen. Phoca, p. 41.)

1. Teeth from Maestricht, accompanied with those of sharks. Boué, Journ. de Geol., t. iii, p. 31.

2 Teeth, a vertebra and a cranium, found in Westphalia. Hermann von Meyer, Paleologica, p. 131.

3. Also from the same authority, teeth very similar to those of seals, and of sharks, from Laxberg near Aix la Chapelle.

4. A fragment of an ilium is referred to, described by Eugene Robert, found in the shell tufa of Iceland with Cyprina Islandica and other recent shells. This is probably from an existing species.

5. In the Museum of Perth, Hungary, exists a hind foot, found near Vienna in the valley of the Danube.

† Prof. A. will soon publish a complete memoir, on this genus found in the United States, in which will be given the result of his personal observations drawn from large collections of remains.

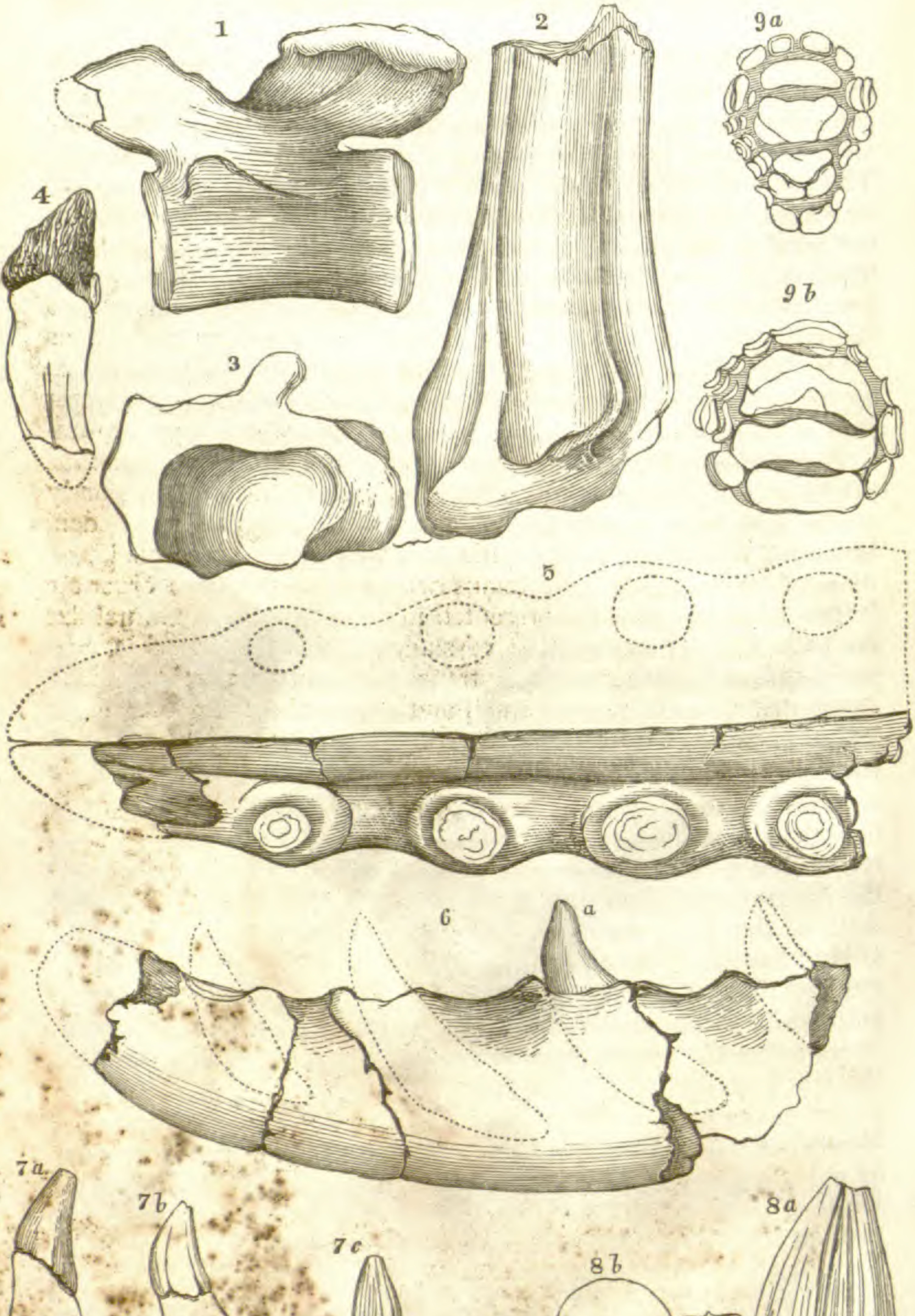
This tooth (fig. 4,) is $1\frac{2}{10}$ inch in length, having a small portion broken from the end of its root. The crown is conical, compressed on its inner face, with slightly trenchant edges, the posterior of which is provided with a slightly projecting tubercle. The enamel over its whole surface is roughened by small irregular ridges, the general direction of which is from the base towards the apex of the crown, those at the apex being the most minute. This tooth must have been placed at the anterior part of the jaw; all those of the posterior lateral portions being deeply indented.

Dr. Burton has also found a petrous bone with its convoluted appendage, which has all the Cetacean characteristics and which Prof. Agassiz likewise refers to the genus Phocodon.

Delphinus.—From the same locality and in the immediate neighborhood of the spot in which the vertebræ and other bones of the seal were found, Dr. Burton obtained four vertebræ all belonging apparently to one individual and nearly of the size of those of the common porpoise, (D. delphis.)

The fragment of a lower jaw represented in figures 5 and 6, if the indications of the teeth are followed, is Cetacean, though the jaw itself is somewhat unusual. The crowns project but slightly above their alveoli, and are small when compared with the roots which are long, bulging near the middle and deeply imbedded in their sockets. The extent to which they penetrate into the jaw is made obvious by three fractures, which intersect the alveoli near their termination. A reptilian feature in the jaw may be recognized in the depressions or grooves which exist on its sides in the intervals between the teeth, as if for the reception of the teeth of the upper maxillary bone, a condition which Dr. R. W. Gibbes has shown to exist in the Basilosaurus. Each tooth is surrounded by a distinct circular ridge. The teeth are solid, the pulp cavity being wholly filled up. By reference to figure 5 it will be seen that the two halves of the lower jaw are united through the whole extent of this fragment (the symphysis) by a flat surface as is the case in the sperm whales and in the Gavials among Crocodiles. It differs however from the latter in the absence of an enlargement of the jaw at its extremity, (gradually diminishing to a point as in Cetaceans,) as well as in the deficiency of teeth of larger dimensions which in Gavials correspond to canines. Were it not for the prolonged symphysis the jaw might be regarded as belonging to the genus Phocodon. In its symphysis it closely resembles that of some species of Delphinus.

The teeth represented in figure 7, are regarded as belonging to an animal of the same species as the jaw above described, presenting the same form of the crown and root represented at *a* figure 6, the only one which was preserved entire. These teeth are either solid or provided with only a minute pulp cavity. This jaw resembles that of Delphinus more closely than that of any other Cetacean.


1
2
9a
4
9b
3
5
6
a
7a
7b
7c
8b
8a

This fragment I found in one of the small ravines on the western side of the Shockoe Creek Ravine.

In one of the species of Zeuglodon figured by Müller, there exists a lower jaw with a prolonged symphysis as in the above specimen, though the teeth of the first are less numerous. He also figures a portion of an upper jaw which is turned up, presenting a convexity corresponding with the concavity of the outline of the lower fragment here described. Fig. 6.

In addition to the above, there have been found from time to time for many years, fragments of bones of the larger Cetaceans of which I have numerous specimens, but have seen none of the dimensions of those of the largest existing whales. Their specific characters have not been recognized. I have a single imperfect tooth nearly five inches in length, the crown of which is quite small compared with the root, and its surface is irregularly striated or reticulated as in the figures of the teeth of Zeuglodon, given by Johannes Müller in his memoir on this last animal.

Reptiles.—The only remains which represent this order are teeth, one of which is represented in figure 8 *a* of its natural size and which is obviously that of some species of Crocodile. Its form is blunt conical, with a transverse section nearly circular, and having on each side a small and well defined ridge largest at its upper extremity and terminating below in a delicate point. The surface of the tooth is marked with minute longitudinal striæ. The lateral ridges divide the tooth into two unequal portions as shown in the section, figure 8 *b*.

Some of the teeth of one other species of reptile allied to the Crocodiles was found in company with the preceding, but which differ from the described forms in having the whole surface covered with well defined longitudinal flutings extending from the base to the apex—similar to those of Mosasaurus; but the tooth is without an enlarged base.

The precise locality from which these Reptilian teeth were obtained, I am unable to state; they were from the tertiary deposits.

Fishes.—Of all the remains found in this locality, those of fishes are by far the most numerous, and of these the largest portion belong to the Plagiostome group, and will therefore be almost necessarily represented by the teeth alone. As nearly all the genera and species have been previously described, a catalogue of them will be all that is necessary.

Lamna compressa, Agass.

L. acuminata, Agass.

L. crassidens, Agass.

L. elegans, Agass.

All of these have been found in the Eocene beds of S. Carolina by Dr. R. W. Gibbes. The specimens above enumerated were found in digging wells in Richmond, but whether from Eocene or Miocene beds I am unable to state.

Otodus appendiculatus, Agass.
O. lineatus.
O. obliquus, Agass.
O. lanceolatus.
Oxyrhina hastalis, Agass.
Ox. xiphodon, Agass.
Hemipristis serra, Agass.
Carcharodon angustidens, Agass.
Galeocerdo contortus, Gibbes.
G. Egertonii, Agass.
Sphyrna lata, Agass.
Glyphis subulata, Gibbes.

Notidanus primigenius. This species is quite rare. The specimen here referred to is the only one which I have seen mentioned as coming from the U. States. This was described by Dr. R. W. Gibbes in his memoir on the fossil Squalidæ.*

Genus Myliobatis.—After the genus Lamna, Myliobatis may be mentioned as the one which furnishes the largest number of remains.

Genus Phyllodus.—The palatine or pharyngeal plates, fig. 9 *a* and *b*, were found by Dr. Burton, and as I believe as far as any record appears to the contrary for the first time in the U. States. Of this remarkable genus first described by Agassiz, nothing has yet been discovered but the plates similar to those represented by the figures. Each of the central plates consists of a series of superimposed layers. Agassiz† and Owen‡ have given the details of their microscopic structure. All the species enumerated by Agassiz, six in number, were found in the London clay of Sheppey. Fig. 9 *a* resembles P. toliapicus. Fig. 9 *b* does not resemble any of the species figured in the Poissons Fossiles, but is imperfect. Owen regards Phyllodus as allied to Scarus, but Agassiz does not admit the correctness of this opinion.

Genus Pogonias.—The peculiar pharyngeal teeth of these fishes have been several times discovered, and I have recently received from Dr. Burton broken specimens of large size.

Vertebræ of fishes.—These exist in great numbers, but almost invariably disjointed. I have no means of determining them. Some few have the elongated bodies of the Scomberoids.

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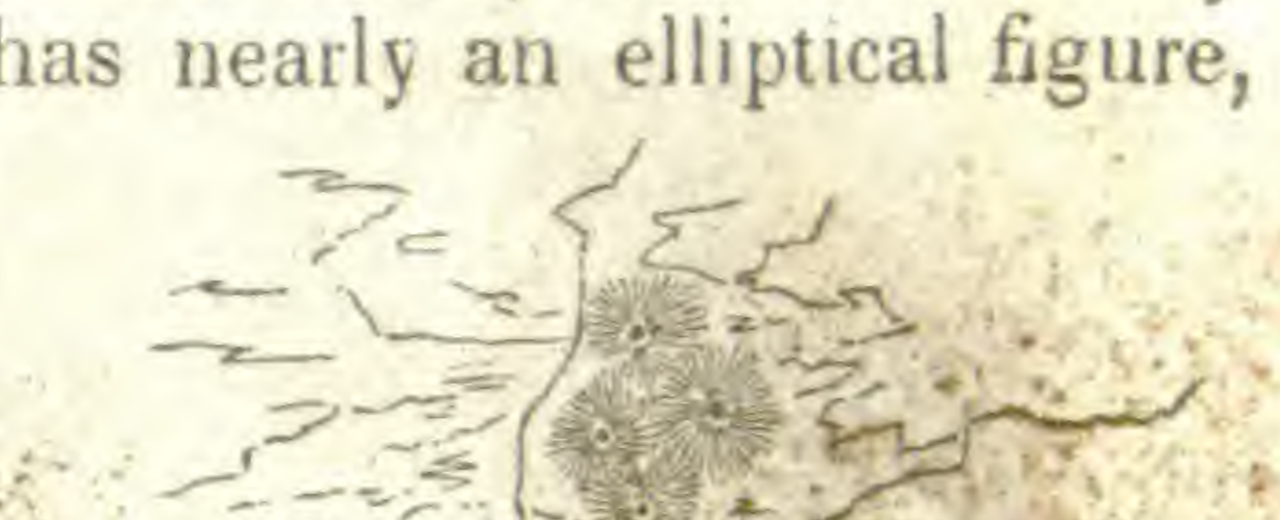
* Nearly all the names given above were furnished me by Dr. Gibbes. For a full description of the genera and species here enumerated, his valuable memoir can be referred to as containing the most complete description as yet given of the fossil Squalidæ the U. States. See his Memoir, Jour. Acad. Nat. Sciences, Philadelphia, 1849.

† Poissons Fossiles, t. ii, p. 238. ‡ Odontography.

Coprolites.—Of numerous specimens of these remains, there is one of unusual dimensions. It is six and a half inches in length and three inches in diameter. It presents indications of having been moulded upon a spiral membrane; its fractured surface exhibits fragments of bone and something resembling the scales of fishes. The color of the fractured surface is of a reddish brown.

ART. XXII.—*On the Volcanic Eruptions of Hawaii;* by JAMES D. DANA—Continued from vol. ix, p. 364.

IN a former article, a historical account was given of the eruptions of Kilauea, the great pit-crater on the flanks of Mount Loa, situated at a height of about 4000 feet above the sea. The following pages contain what information we have collected respecting the summit eruptions of Mount Loa.* The crater is a deep pit in the very summit of the mountain dome, and is called by the natives Mokua-weo-weo.† It has nearly an elliptical figure, as shown in the annexed cut, with its diameters 13,000 and 8000 feet respectively, the longer lying in a north-by-west and south-by-east direction. But the deep part of the crater is nearly circular, and has the breadth of the smaller diameter, the northern and southern portions being shallow. The walls, through a considerable portion of their circuit, are abrupt or even vertical, and are stratified in structure like the sides of Kilauea; on the west side the height was found by Henry Eld, Jr., to be seven hundred and eighty-four feet, and on the east four hundred and seventy feet.



The bottom of the pit when examined by Captain Wilkes and the officers of the Vincennes, consisted of solidified lava, through which there were several fissures and fumaroles emitting steam and sulphur vapors in large volumes. Some parts were rough

* The summit crater was examined and thoroughly explored by Captain Wilkes and the officers of the Vincennes, and a detailed account of it is given in the fourth volume of the Narrative of the Expedition. The above sketch is a reduced copy of that in the Narrative, p. 111. The author's explorations did not reach above Kilauea, his time having been limited by orders to other groups of Islands.

† The name of the crater is pronounced *Mokooah-wayo-wayo.*

with clinkers, while in others, smoother tracts of solid lava constituted the surface. The fissures had in general a north-northwest and south-southeast direction, and one near the west bank had ejected lavas at no distant period. Two cinder cones at the bottom, consisting of light scoria, were remarkably perfect in form, and one was two hundred feet high. About many of the fumaroles there were the same salts that occur at the sulphur banks of Kilauea.

Besides the large pit there were two others, one on the north, and another (called Pohakuo-hanalei) on the south, both of which may be looked upon as subordinate to the central crater, as they are enclosed within the same general rim or outline. There is also another small pit, distinct from these, a short distance to the south.

Into Pohakuo-hanalei, a stream of lava had run from Mokua-weo-weo, and Capt. Wilkes remarks that it looked like a cascade of iron which had become solid before reaching the bottom. There were several deep fissures in the vicinity of this pit, and every appearance of recent eruptions. "The lava at the mouth of some of the chasms appeared as though it had been thrown up and plastered on the edges in clots, which seemed of the consistency of tar or melted sealing-wax of various colors, the most predominant a dark brown."

There were several small cones about the summit, both to the north and south of Mokua-weo-weo.

The rocks of the summit, where there was evidence in their appearance of recent origin, resembled those of modern ejections below. But the walls of the crater are described as consisting of a compact grayish rock without cellules and often breaking in plates. The specimens obtained were a grayish clinkstone speckled with a white feldspar, with no trace of a cellule, and no resemblance to ordinary lavas. They consisted mostly of the feldspar.

But little is known with regard to the eruptions of the summit crater. Yet there is abundant evidence that, even at the present time, its fires are not entirely inactive.

An eruption is stated to have taken place on the 20th of June, 1832, and the mountain continued burning for two or three weeks; the lavas broke out, in different places, and were discharged from so many vents, that the fires were seen on every side of the dome, and were visible as far as Lahaina, upwards of one hundred miles.*

The first ascent to the summit of Mount Loa by a foreigner was made by Dr. Douglass. This author describes it as far sur-

* See this Journal, volume xxv, 201, in a communication from Rev. J. Goodrich, dated Nov. 17, 1832.

passing Kilauea in sublimity and violent activity. Mr. Douglass's observations are, however, received with incredulity by the residents. The crater, if thus active, would, like Kilauea, have shown evidence of it in an illuminated cloud at night. But neither this nor any other proofs of its action were noticed at the time by the Hawaiians or by the whites residing among them.*

An eruption took place in January, 1843, which is described by Messrs. Andrews and Coan.† It broke out at the summit, on the 10th of January, and continued down the slopes of Mount Loa in two streams; one flowed to the westward towards Kona; the other flowed northward to the foot of Mount Kea, and then dividing, one part continued on towards Waimea northeastward, and the other towards Hilo, eastward.

We cite here the account given by the Rev. Mr. Coan of the American Mission at Hilo on the eastern shore, who has spent much time in his many explorations of Mount Loa.

"On the morning of January 10th, before day, we discovered a small beacon fire near the summit of Mauna Loa, directly in the rear of our station, about thirty miles distant. This was soon found to be a new volcano, bursting out on the northeastern slope of the mountain, at an elevation of near 13,000 feet. From this time, the eruption increased in magnitude and intensity, from day to day, till it presented a scene of sublime splendor, disgorging vast columns of fiery fluid which rolled in a broad, burning river down the side of the mountain. Subsequently the lava appeared to burst out at several different points lower down the mountain, from whence it flowed off in the direction of Mauna Kea, filling the great valley between the mountains with a sea of fire, and throwing a broad sheen of light upon the heavens. Some of our nights have presented scenes sublimely grand and fearfully magnificent. The position of the eruption is such that it can be distinctly seen from the window of our

* A comparison of the statements in the following paragraph by Mr. Douglass, with the observations by the officers of the Vincennes, will show that this incredulity is probably not misplaced. "The mountain (Mount Loa), with an elevation of 13,517 feet, is one of the most interesting in the world. The journey to the top took me seventeen days. On the summit is a volcano, nearly twenty-four (?) miles in circumference, and at present in terrific activity. You must not confound this with the one situated on the flanks of Mauna Roa, and spoken of by the missionaries and Lord Byron, and which I visited also. It is difficult to attempt describing such an immense place. The spectator is lost in terror and admiration at beholding an enormous sunken pit, (for it differs from all our notions of volcanoes as possessing cone-shaped summits with terminal openings,) five miles square of which is a lake of liquid fire, in a state of ebullition, sometimes tranquil, at other times rolling its blazing waves with furious agitation, and casting them upwards in columns from thirty to one hundred and seventy feet. This volcano is 1272 feet deep; I mean down to the surface of the fire; its chasms and caverns can never be measured." Extracts from the Journal of Mr. Douglass, Magazine of Zoology and Botany, 1837, i, 582.

† Missionary Herald, xxxix, 381, 463; and xl, 44. The course of the stream, and its origin were particularly examined by the Rev. T. Coan.

dormitory, so that we can lie on our couch and watch the fantastic and ever varying action of the fires from evening to morning. The rapid disgorgement of the gory flood, the lofty rising of brilliant pillars—like burning brass—the irregular shooting of coruscations, and the fearful flow of the molten sea—all tend to excite a wakeful spirit, and incline us sometimes to keep vigils for most of the night. For about four weeks, this scene continued without much abatement. At the present time, after six weeks, the action of the fire is greatly diminished, though it is still somewhat vehement at one or two points along the line of eruption. The flow of the lava has probably extended twenty miles. As to its breadth and depth, we can form no opinion at this distance. We apprehend that the fires have nearly spent their force, though in this we may be mistaken."

[On Monday the 6th of March, Mr. Coan, in company with Mr. Paris and seven natives, left Hilo on their tour of exploration.]

"We did not take the usual route—that pursued by Captain Wilkes and others, via Kilauea—but directed our course at once for the stream of lava, as it was seen flowing on the high plains between Mauna Loa and Mauna Kea. Our general course was west-south-west, through a vast forest, so interwoven with jungle as to render it in most places impenetrable. As the season was peculiarly dry, we chose for our path the rocky bed of a river, called the River of Destruction, from the quantity and rush of its waters during the rainy seasons. The stream was now so low that we could pass up its bed and under its banks by leaping from rock to rock, and frequently crossing from side to side, now and then also ascending its banks and beating our way for a short distance through the brushwood, to avoid deep water, perpendicular precipices, or the accumulated masses of drift-wood. These drift wood accumulations consisted often of majestic trees which had been torn violently from their places, and, with roots, trunk, and branches, carried down the stream to some narrow pass, where their progress was arrested by the approaching banks, by vast rocks, or by a sudden bend in the stream, so as to form an impregnable chevaux-de-frise against the traveller.

[The night was spent by Mr. Coan and his party in the outskirts of the forest already mentioned.]

On the second day, we again entered the bed of the stream, and pursued our romantic course along the serpentine and rocky channel, and between its precipitous and often overhanging banks, which sometimes presented frowning battlements of dark naked lava, and sometimes retreated in graceful slopes of luxuriant soil, adorned with trees, shrubs, vines, and parasitical plants, or spread with a splendid carpet of soft velvet moss. In this lofty and deep forest, and amid these everlasting solitudes—unbroken except by the gurgling of the wasted stream, the dashing of the cascade, or the mighty rush and the deep thunder tones of the mountain torrent, and, I should add, by the enchanting strains of the ten thousand songsters whose notes seemed to fill every leaf and shrub and tree with animated joy—we pursued our quiet way till the outstretching shades of evening admonished us to prepare for repose.

[The night was passed in a booth of boughs and ferns, erected for the emergency on the bank of the river.]

Early the next morning, we pursued our way up the stream, and at noon found ourselves fairly out of the forest with the lofty summit of Mauna Kea rising in hoary grandeur before us. We were now at its base, and in the high, open country occupied by herds of wild cattle. We bent our course south-south-west, over a beautiful rolling country, sprinkled here and there with clumps of low, spreading trees, which looked like orchards in the distance. Our way was along the upper skirts of the forest, having Mauna Kea with its numerous peaks and lateral craters on our right. At evening we came in full view of Mauna Loa, bearing south by west from us. We pitched our tent under an ancient crater, four hundred feet high, now covered with trees and grass.

Here we had a splendid view of the great terminal crater on the summit of the mountain, about twenty-five miles distant, and also of the vast flood of lava which had flowed down the northern side of the mountain to the plains below, some part of which lay burning at our feet, at the distance of four or five miles. We were now seven or eight thousand feet above the level of the sea; and we could see the dark clouds gather, and the lightnings blaze below us, while the deep toned thunder rolled at our feet. At the same time, a storm of hail spread along the shore and fell upon the station at Hilo. This was the first hail seen at our station since our arrival at the Islands. At twilight a smart shock of an earthquake, which lasted thirty seconds, added to the sublimity of the scene; while a blazing comet hung over us in the vaulted sky. As darkness gathered around us, the lurid fires of the volcano began to glow with fervid heat, and to gleam upon us from the foot of Mauna Kea, over all the plain between the two mountains, and up the side of Mauna Loa to its snow-crowned summit, exhibiting the appearance of vast and innumerable furnaces, burning with intense vehemence, and throwing out a terrible radiance in all directions. During the night we had thunder and lightning; and in the morning both mountains were beautifully mantled in snow.

It was now Thursday, and we left our encampment and proceeded three or four miles towards the new stream of lava, and again pitched our tent on the side of an old crater, two hundred feet high and one mile in circumference, and covered with trees and shrubbery to its summit. It was surrounded at its base, however, by a vast field of naked scoria of the most jagged character, the deposit of some former eruption which had flowed around the little fertile hill, and left it like an island in the ocean, or like an oasis in the desert. Leaving our natives to prepare our encampment and collect fuel, water, etc., we set off for the nearest stream of active lava, distant about two miles. Our road was over sharp jagged lava, thrown up in tumultuous confusion; but we soon made our way to the molten stream, and, thrusting our staffs into the viscid mass, took out and cooled specimens which we carried home with us. You will understand that we were now on the great plain between Mauna Loa and Mauna Kea, about 7,000 feet above the level of the sea, not having as yet commenced the direct ascent of the mountain. On this plain, between the bases of the two mountains, we spent the day in traversing and surveying the immense streams of fresh scoria and slag, which lay smouldering in wild confusion farther than the eye could reach,—some cooled, some half-cooled, and some still in a state of igneous fusion. The scoriform masses which formed the larger portion of the flowings, lay piled in mounds and extended in high ridges of from thirty to sixty feet elevation above the substratum on which it rested, and forming a barrier so indescribably jagged and rough as to be nearly impassable. It seemed as if this vast sea of earthy and rocky fusion had been suddenly solidified, while in a state of the most tumultuous action. Besides these high and broad ridges of scoria, there were parallel streams of slag, solidified on the top, like ice on a river. This was smooth, of lustrous black, and in a vitrescent state, forming the superincumbent crust of a deep molten river which rolled beneath, and which betrayed its burning course at innumerable cracks and seams and blow-holes, in which the fiery fluid was seen, or through which it was expelled in gory jets.

We spent the whole day in exploring this vast sea of lava, and were astonished at its immense area. In rolling down the side of the mountain, one broad stream had shot off in a westerly direction, towards Kona. Another mighty river had flowed northward till it was intercepted by the base of Mauna Kea, when it divided into two branches, one flowing in a north-west direction towards the plains of Waimea, and the other arm stretching north-east and flowing towards Hilo. These three main branches, if united, would form probably a river five or six miles broad; and the longest of them cannot I think, have advanced less than twenty-five or thirty miles. They are still flowing, but their progress at present is slow, as they are on a vast plain, and their celerity is also retarded by fissures and caverns, and by fields of old scoria which covers those high regions. Should the eruption continue, and should the quantity of fusion be sufficient to overcome the obstacles and reach the regions where the face of the country declines rapidly towards the sea, the descent will then be quick and easy to the coast, both on the eastern and western shores. This may take place though I am rather of the opinion that the fires will have spent their force before they reach the sea.

Besides the three great branches described, there are numerous smaller ones, shooting out laterally and irregularly from the main streams, both on the side and at the base of the mountain. These form together an indescribable labyrinth.

After travelling hard all day, without being able to reach the extreme ends of the two great western branches of the eruption, we returned at evening to our tent, weary, but gratified nearly to oppression by the vastness and the terribleness of the scenes we had witnessed.

[During the night, a dense, dark cloud invested the eminence on which the travellers had encamped; this was charged with electric fluid, which soon began to blaze around them with terrific splendor, accompanied, at the same time, with startling peals of thunder. They felt that they were "in a sea of electricity;" and realized the sublimity of the expression, "The God of Glory thundereth." At length the storm passed away, and the volcanic fires which had been concealed by the tempest, "resumed their merry dance, spouting forth their gory masses in fantastic and ever varying forms, at different points, from mountain to mountain, along the whole line of eruption."]

Ascent to the Crater.—The next morning we rose early, and made our preparations for visiting the summit of the mountain, distant about twenty miles. As we did not suppose it possible to reach the summit and return to our camp the same day, we provided ourselves with caps, flannels, mittens, cloaks, comfortables, etc., for sleeping upon the lava on the side of the mountain; and taking a little food and a calabash of water, we committed our luggage to two strong natives, leaving the rest of our company where we had encamped during the night. Thus prepared we set off, expecting to spend two days upon the mountain. Our way at first lay over a field of scoria of an indescribably sharp and jagged character; and we had not proceeded more than half a mile before we found that the two natives who carried our clothing fell in the rear, and followed us at the rate of less than a mile an hour,—the road being so inconceivably rugged that they could not quicken their pace without being in danger of stumbling at every step, and breaking their calabashes, and tearing their flesh. We halted suddenly, held a short consultation, set down our calabashes on the lava, took a little biscuit in our pockets, laid our cloaks and umbrellas on our guides, and leaving everything else behind, set out again for the summit of the mountain. After passing this tract of scoria, we came to a field of more compact and smooth lava, lying along the borders of the new stream. Here we moved on rapidly, at the rate of three and a half or four miles an hour. As we had left most of our clothing and food, with all our water, behind, and as we clung to the hope of reaching the original point of eruption—a vast, active crater, within a few hundred feet of the highest part of the mountain—we felt it necessary to press hard and improve every moment, as we must return the same day, or probably perish with cold amid those high regions of snow and tempests.

To describe our road would be tedious, if not impossible. Sometimes we were on ancient deposits and sometimes on the new; sometimes on broad fields of smooth, shining lava, and sometimes crossing extended tracts of the sharp, spurry kind before mentioned; now we were climbing a high ridge of loose scoria and slag, and then feeling our way down a ravine amidst poised and pendant masses that seemed to say, "Touch us not lest we bury you from the light of day."

At ten o'clock we were fairly at the foot of the mountain proper, and began a more regular and rapid ascent, though we had been gradually rising for an hour or two before. The new streams of lava, spread to the breadth of several miles over the side of the mountain, for the most part were nearly cooled; in many places, however, they were burning hot, and emitting smoke, steam, and pungent gases.

At noon we lost sight of our native attendants, who were unable to keep up with us in our rapid and forced march, and we saw them no more during the day. We were now on the new eruption, and our ascent became more and more steep; while the rarity of the atmosphere affected our respiration, so that it was difficult to proceed many rods without stopping to pant and recover breath. The lava on which we were treading gave indubitable evidence of powerful igneous action below, as it was hot and full of seams, from which smoke and gas were escaping. But we soon had ocular demonstration of what was the state beneath us; for in passing along we came to an opening in the superincumbent stratum, of twenty yards long and ten wide, through which we looked, and at the depth of fifty feet, we saw a vast tunnel or subterranean canal, lined with smooth vitrified matter, and forming the channel of a river of fire. which swept down the steep side of the mountain with amazing velocity. The sight of this covered aqueduct—or if I may be allowed to coin a word, this *pyroduct*—filled with mineral fusion, and flowing under our feet at the rate of twenty miles an hour, was truly startling. One glance at the fearful spectacle was worth a journey of a thousand miles. We gazed upon the scene with a kind of ecstasy, knowing that we had been travelling for hours over this river of fire, and crossing and recrossing it at numerous points. As we passed up the mountain, we found several similar openings into this canal, through which we cast large stones; these instead of sinking into the viscid mass, were borne instantly out of our sight upon its burning bosom. Mounds, ridges, and cones were also thrown up along the line of the lava stream, from the latter of which, steam, gases, and hot stones, were ejected into the air with terrible hissings and belchings.

We had proposed to commence our return at one o'clock in the afternoon; but the hour came and we were still far from the summit. We then added half an hour to the ascent. This passed, but we had not reached the end of our journey. We went on adding half hour to half hour till three o'clock, at which time we reached the verge of the great crater where the eruption first took place, near the highest point of the mountain. This was in the region of perpetual snow; and to reach it we had passed through snow for the last three miles. Here we found two immense craters close to each other, of vast depth and in terrific action; but we had not a moment left to stay and survey them minutely. Kneeling, therefore, among these awful scenes to bless the Hand which had led us thus far, and to ask protection on our return, we turned our faces down the mountain.

Though weary and way-worn, almost to the last degree, we felt that we must regain our tent, long lost in the distance, or run the hazard of perishing upon the mountain. We ran, walked, clambered, descended, stumbled, feeling unable at every step to drag one foot after the other, and yet necessity impelled us to proceed. At length night came on; we were still in a trackless waste of frowning lava, not less than eight or ten miles from our camp. But by the cheering aid of a moon in her first quarter, we could still trace the distant outlines of the green hill on which our cottage of branches stood. We plodded on, alternately walking and resting, at rapid intervals, until a fog came on us shutting out at once the hill, the heavens, and even the volcanic fire from our sight. We could not now keep our course, as we could not see our compass. We wandered some, but not far from the track. Still there was little hope that we should reach our camp.

In about an hour, however, the fog dispersed, the moon and stars looked benignantly upon us, and the volcanic fires began again to play on our left; and after persevering toil, with indescribable weariness, we reached our tent a few minutes before eleven at night. I need not say that our thirsty, exhausted, and lacerated frames welcomed rest and refreshment; nor that our thankful spirits felt untold satisfaction in view of the wondrous scenes we had witnessed during this laborious and eventful day."

[On the following morning, Messrs. Coan and Paris, with the natives who accompanied them, set out upon their return to Hilo.]

In a letter subsequently received by the author from Mr. Coan he states:—"The angle of descent down which the lavas flowed from the summit to the northern base of Mauna Loa is 6°; but there are many places on the side of the mountain where the inclination is 10°, 15°, or 25°, and even down these local declivities of half a mile to two miles in extent, the lava flowed in a continuous stream. This was the fact not only during the flow of several weeks upon the surface, but also in that wonderful flow in the subterranean duct, described in the Missionary Herald. There was no insurmountable barrier in the way of the flow from the summit of Mauna Loa to the base of Mauna Kea, a distance of twenty-five or thirty miles. The stream sometimes struck mounds or hillocks, which changed its course for a little space, or around which it flowed in two channels, reuniting on the lower side of the obstacle, and thus surrounding and leaving it an island in the fiery stream. Ravines, caves, valleys, and depressions were filled up by the lava as it passed down the slope of the mountain, and between the two mountains. In conclusion, I remark, that the stream was continuous for more than twenty-five miles, with an average breadth of one and a half miles, and flowed down a declivity varying from 25° to 1°."

The quiet character of the eruptions of Kilauea was remarked upon in a former article. It is still more surprising that an outbreak of such magnitude as that just described should have taken place, without warning to the islanders.—From a height of 14,000 feet, lavas break out, in a copious flow, and for 25 miles there is a rending of the mountain, and continued ejections; and yet there are none of the terrible convulsions and heavings and darkening showers of ashes or cinders that accompany the eruptions of many smaller volcanoes: there is not even a grumbling beneath to suggest alarm; the first indication was the discovery of what appeared to be a small "beacon fire" near the summit of the dome. During all this time, Kilauea, on the flanks of the mountain at a level 10,000 feet below, was in its usual active condition, and as an observer says, it showed not the least signs of sympathy. The crater is an opening 3½ miles in length; and within it, an area 1500 and 1000 feet in its diameters, was at the time in constant undisturbed ebullition.

Since 1843, there has been another eruption from the summit, and we look for an early account from the island to give our readers. It took place the past year, and was as remarkable as that described for its quietness of progress, and its extent.

Art. XXIII.—*Mineralogical Notices.**

I. New Species.

1. Tritomite, (S. H. Weibye and N. J. Berlin, Pogg., lxxix, 299, 1850.)—Crystals tetrahedral; surfaces smooth and having a reddish crust; cleavage indistinct. Lustre submetallic vitreous. Color dull brown; streak dirty yellowish-gray. Translucent only on the edges. H. between feldspar and apatite. G.=4·16—4·66. B.B. becomes white, intumesces somewhat, and sometimes decrepitates. In a matrass yields water and gives a weak fluorine reaction. With borax dissolves to a reddish yellow glass, which is colorless on cooling. With muriatic acid, when pulverized, yields chlorine and gelatinizes. *Composition* according to N. J. Berlin, (specific gravity of specimen, 4·24.)

$\dddot{Si}$	$\ddot{\bar{Al}}$	$\ddot{\bar{Ce}}$	$\ddot{\bar{La}}$	$\dot{Y}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{Fe}$	$\dot{Na}$	Mn,	Cu, Sn, W,
20·13	2·24	40·36	15·11	0·46	5·15	0·22	1·83	1·46		4·62

loss by ignition 7·86=99·44.

The production of chlorine on heating with muriatic acid shows that the mineral contains the cerium either wholly or in part, as peroxyd. The mineral appears therefore to be a hydrous silicate of the peroxyds of cerium and lanthanum and of lime.

This mineral is from the island of Lamö near Brevig in Norway, and occurs with leucophane and mosandrite in a coarse syenite.

2. Catapleiite, (Weibye and Sjögren, ibid.)—Probably monoclinic; found only in imperfect prismatic crystals of 120° nearly, showing sometimes traces of other vertical faces. Cleavage basal, perfect. Surfaces smooth, with little lustre; on fracture, weak vitreous. Color light yellowish brown. Streak isabella-yellow. Opaque. H. near that of feldspar; G.=2·8. B.B. in the platinum forceps fuses easily to a white enamel; with borax dissolves with difficulty to a clear colorless glass; cobalt solution colored blue. Dissolves easily in muriatic acid, when pulverized, without gelatinizing. Composition according to Sjögren:—

	$\dddot{Si}$	$\ddot{\bar{Zr}}$	$\ddot{\bar{Al}}$	$\dot{Na}$	$\dot{Ca}$	$\dot{Fe}$	$\dot{H}$
1	46·83	29·81	0·45	10·83	3·61	0·63	8·86
2	46·52	29·33	1·40	10·06	4·66	0·49	9·05
Oxygen,	24·15	7·72	0·65	2·58	1·33	0·11	8·04

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* In the writer's treatise on Mineralogy (third edition), which left the press in May last, the science of Minerals is posted up, as far as could be done in this country, to the time of its publication. The author proposes to continue his appendix to that work in this Journal, giving in each volume an abstract of all the mineral information which has been received through the different foreign and American publications. J. D. D.

Whence for the protoxyds, peroxyds, silica and water, 4·02 : 8·37 : 24·15 : 8·04 = 1 : 2 : 6 : 2, giving the formula

$$3(\dot{N}a, \dot{C}a)\dddot{S}i + \dddot{Z}r^2\,\dddot{S}i^3 + 6\dot{H}.$$

The earth called zirconia was proved to be identical with the zirconia of the Fredericksvärn zircon. Whether it may not be the allied earth Noria, it is at present difficult to determine.

This mineral is associated with the Tritomite.

3. Atheriastite, (Weibye and Berlin, ibid.)—Dimetric. In square or eight sided prisms terminating in a pyramid having the terminal angle 135°. Cleavage lateral, perfect, surfaces even and smooth, but not shining. Color verdigris-green, commonly somewhat dirty; streak greenish-gray. Opaque. Fracture uneven and splintery. B.B. in the forceps intumesces, and fuses easily to a dull brown glass. In fine powder slightly decomposed in muriatic acid. Composition according to Berlin:—

	$\dddot{S}i$	$\dddot{A}l$	$\dot{C}a$	$\dot{M}g$	$\dot{F}e$	$\dot{M}n$	$\dot{H}$
	38·00	24·10	22·64	2·80	4·82	0·78	6·95
Oxygen,	19·73	11·25	6·47	1·10	1·07	0·17	6·22

This gives for the oxygen of the protoxyds, peroxyds, silica and water. 8·81 : 11·25 : 19·73 : 6·22, whence the author deduces the formula $4\dot{R}^3\,\dddot{S}i + 5\dddot{A}l\,\dddot{S}i + 9\dot{H}$, or, as more probable, $2\dot{R}^3\,\dddot{S}i + 3\dddot{A}l\,\dddot{S}i + 4\dot{H}$. From an iron mine near Arendal, in granite with black garnet and Keilhauite. It had been taken for scapolite. [Is it not still possible that the mineral may be an *altered* scapolite? The angle of the pyramid of scapolite is 136°, which is very near that given for the Atheriastite.]

4. Eudnophite, (Weibye, von Borck, and Berlin, ibid.)—Trimetric. Crystals rhombic prisms of about 130°; $o : d = 130°$ *. Cleavage, basal perfect; diagonals less perfect. Cleavage face somewhat pearly. Also granular massive, and sometimes having a feathery arrangement. Color white, to gray or brown. Subtranslucent to translucent. H. = 5·5. G. = 2·27. B.B. fuses to a clear colorless glass. Pulverized forms a jelly with muriatic acid. Composition according to von Borck and Berlin:—

	$\dddot{S}i$	$\dddot{A}l$	$\dot{N}a$	$\dot{H}$
1	54·93	25·59	14·06	8·29 = 100·87
2	55·06	23·12	14·06	8·16 = 100·41

This gives the formula of analcime, $\dot{N}a^3\,\dddot{S}i^2 + 3\dddot{A}l\,\dddot{S}i^2 + 6\dot{H}$, and the compound is therefore dimorphous. Found with leucophane on the island of Lamö, Norway, in syenite.

* The angle between *d* and *s* is given at 120°; and if this be right the vertical prism (*d* : *d*) must also have an angle of 120°.—D.

5. Dillnite, *associated with Diaspore*, (A. Hutzelmann, cited in Pogg., lxxviii, 575, from the Bulletin of the Freunden der Naturwiss. in Vienna.)—There are three substances forming together the gangue of the diaspore of Schemnitz:—One (*A*) is gray, slightly greenish, with a weak greasy lustre and faint translucence; H.=2·5–3·0; G.=2·735. A second (*B*) is white, opaque, with an even and flat conchoidal fracture, and firm; H.=3·5; G.=2·835. Adheres a little to the tongue. A third (*C*) is white, opaque, earthy; H.=1·8—2·0; G.=2·574. Adheres strongly to the tongue. The first and third have been analyzed by Karafiat, the second by Hutzelmann, as follows:

	$\dddot{Si}$	$\dddot{\bar{Al}}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{Fe}$	$\dot{Mn}$	$\dot{K}$ & $\dot{Na}$	$\dot{H}$
A.	49·50	27·45	5·56	0·72	1·03	*trace*	10·20	5·10= 99·56
B.	22·40	56·40	*trace*	0·44	*trace*	*trace*	*trace*	21·13=100·37
C.	23·53	53·00	0·88	1·76	——	——	——	20·05= 99·22

Excluding the magnesia and reducing to a per-centage, B and C become

	$\dddot{Si}$	$\dddot{\bar{Al}}$	$\dot{H}$
B.	22·41	56·45	21·14
C.	24·36	54·88	20·76

The first (*A*) is near agalmatolite, whose formula is

$$\dot{K}\,\dddot{Si}+2\dddot{\bar{Al}}^2\,\dddot{Si}^3+3\dot{H}.$$

The other two are probably identical, and to these the name *Dillnite* is applied. Haidinger deduces the formula $\dddot{\bar{Al}}^2\,\dddot{Si}+4\dot{H}$= Silica 24·97, alumina 55·56, water 19·47; and this he observes may be resolved into $\dddot{\bar{Al}}\,\dddot{Si}+2\dot{H}$, the formula of nacrite and much kaolin, and $\dddot{\bar{Al}}\,\dot{H}^2$, a hydrated alumina. [The formula $\dddot{\bar{Al}}^2\,\dddot{Si}+4\frac{1}{2}\dot{H}$ is still nearer C, giving silica 24·39, alumina 54·23, water 21·38.]

The diaspore of this locality has the specific gravity 3·340.

6. Brongniardite, (M. A. Damour, Ann. des Mines, [4], xvi, 227.)—Brongniardite is an ore of antimony, lead and silver, from the mines of Mexico, whence it was brought by M. de Castelnau. The specimen was a compact mass weighing about 15½ pounds, and having one surface sprinkled with pyrites. The following are its characters:—Massive, without cleavage. Lustre metallic, resembling that of polybasite or bournonite. Streak-powder grayish-black. Hardness above that of calcite, but scratched by a point of iron. Specific gravity at 18° C., 5·950.

B.B. on charcoal, decrepitates, fuses easily at a temperature below red heat, giving off an odor of sulphur and white vapors. After roasting, it yields a globule of silver, surrounded with a yellow areola of oxyd of lead. In a closed tube, decrepitates, fuses and affords a feeble sublimate of a reddish-orange color, surmounted by a white sublimate. In an open tube decrepitates, fuses, and affords an odor of sulphur, and a white sublimate of peroxyd of antimony covers the walls of the tube. Rapidly attacked by concentric nitric acid, disengaging nitrous vapors and depositing sulphur, oxyd of antimony and sulphate of lead. With dilute acid there is a slow action yielding vapors of sulphuretted hydrogen; the silver and lead are partially dissolved, and there is a gray deposit in small needles of sulphuret and oxyd of antimony retaining a notable proportion of lead and silver. Attacked also by chlorohydric acid, and when pulverized by a boiling lye of caustic potash. Composition:—

	S	Sb	Ag	Pb	Cu	Fe	Zn
1.	19·38	29·95	25·03	24·74	0·54	0·30	0·40 = 100·34
2.	19·21	29·60	24·46	25·05	0·61	0·26	0·32 = 99·51
3.	19·14	29·75	24·81	24·94	0·70	0·22	0·37 = 99·93

The result gives the formula $Pb\,S + Ag\,S + Sb\,S^3$ [equivalent to $2(Pb, Ag)S + Sb\,S^3$,] = Sulphur 19·08, antimony 30·66, silver 25·65, lead 24·61 = 100.

M. Damour observes that the composition approaches most nearly that of the Schilfglaserz (Freislebenite), the mean of two analyses of which by Wöhler, is, S 18·74, Sb 27·38, Ag 22·93, Pb 30·27 = 99·32.

[The formula is identical in general character with that of feather ore (heteromorphite of Rammelsberg), it differing only in having half the lead replaced by silver. Wöhler's formula for the Freislebenite corresponds to $7(Pb, Ag)\,S + 3Sb\,S^3$ or $2\frac{1}{3}(Pb, Ag)S + Sb\,S^3$, the silver and lead being in the proportions of 3 to 4.*]

II. Described Species.

Dolomite from the Muschelkalk near Saarbrücken, (R. Wildenstein, J. für prakt. Chem., xlix, 154, 1850.)—This dolomite is crystalline granular, and yellowish gray in color. G. = 2·753. Composition according to Wildenstein:—

$\ddot{C}$ 46·30 $\dot{C}a$ 30·50 $\dot{M}g$ 20·13 $\dot{F}e$ 1·17 $\dot{K}$ trace, Clay and sand 1·88 = 99·98

or $\dot{C}a\,\ddot{C}$ 54·47, $\dot{M}g\,\ddot{C}$ 41·62, $\dot{F}e\,\ddot{C}$ 1·88, $\dot{K}$ trace, Clay and sand 1·88 = 99·85

Dolomite from the zinc mines of Altenberg near Aix la Chapelle, (K. Monheim, cited by J. f. pr. Chem., xlix, 318, from Verhandl. des nat. Vereines der preuss. Rheinlande, v., 41.)—Composition:—

$\dot{C}a\,\ddot{C}$ 54·31, $\dot{M}g\,\ddot{C}$ 43·26, $\dot{Z}n\,\ddot{C}$ 1·38, $\dot{F}e\,\ddot{C}$ 0·99, $\dot{M}n\,\ddot{C}$ 0·56, $\dddot{S}i$ 0·48 = 100·98,

corresponding to equal equivalents of carbonate of lime and carbonate of the other bases.

Green Spathic Iron from near the zinc mines of Altenberg, (K. Monheim, ibid, xlix, 318, from the same.)—The crystals have the specific gravity 3·60. Composition according to Monheim:—

$\dot{F}e\,\ddot{C}$ 64·04, $\dot{M}n\,\ddot{C}$ 16·56 $\dot{C}a\,\ddot{C}$ 20·22 $\dddot{S}i$ 1·10 = 101·92,

* $9(Pb, Ag)S + 4Sb\,S^3$ or $2\frac{1}{4}(Pb, Ag)S + Sb\,S^3$ is even nearer the analysis of freislebenite, it giving,
Sulphur 18·65, antimony 28·65, silver 23·13, lead 29·57 = 100.—D.

This corresponds to $8\dot{\text{Fe}}\,\ddot{\text{C}}+2\dot{\text{Mn}}\,\ddot{\text{C}}+3\dot{\text{Ca}}\,\ddot{\text{C}}$. The mineral resembles the kapnite, a calamine containing over 15 per cent. of carbonate of iron (zinkeisenspath, of the Germans).

Electric Calamine from Altenberg, and from Retzbanya, Hungary.—Analyses by K. Monheim, (ibid, xlix, 319, from the same.)

	$\ddot{\text{Si}}$	$\dot{\text{Zn}}$	$\dddot{\overline{\text{Fe}}}$	$\ddot{\text{C}}$	$\dot{\text{H}}$
1. Altenberg, milky cryst.	24·31	65·74	0·43	0·31	7·51= 98·30
2. " limpid "	25·40	67·05	——	0·31	7·47=100·23
3. Retzbanya,	25·34	67·02	0·68	0·35	7·58=100·97

The results sustain the formula deduced by Berzelius, $2\dot{\text{Z}}\text{n}^3\,\ddot{\text{Si}}+3\dot{\text{H}}$. Specific gravity of the Altenberg mineral, 3·43, 3·45, 3·47, 3·49.

Manganesian Calamine.—Analyses by K. Monheim, (ibid, xlix, 382, from the same); 1, from Herrenberge near Riom, in pale green rhombohedrons; G. = 4·03. 2, ibid, dull green, G. = 2·98. 3, from Altenberg, G. = 4·20.

	$\dot{\text{Zn}}\,\ddot{\text{C}}$	$\dot{\text{Mn}}\,\ddot{\text{C}}$	$\dot{\text{Fe}}\,\ddot{\text{C}}$	$\dot{\text{Mg}}\,\ddot{\text{C}}$	$\dot{\text{Ca}}\,\ddot{\text{C}}$		
1.	85·78	7·62	2·24	4·44	0·98,	$\ddot{\text{Si}}$ 0·09,	$\dot{\text{H}}$ *trace*=101·15
2.	74·42	14·98	3·20	3·38	1·68,	$\ddot{\text{Si}}$ 0·20,	$\dot{\text{H}}$ 0·56 = 98·12
3.	84·92	6·80	1·58	2·84	1·58,	Electric calamine,	1·85= 99·57

Nontronite from Andreasberg.—Composition according to Mehner, (J. f. pr. Ch., xlix, 382, from Trommsdorff's J., xii, 27, 1826):

$\ddot{\text{Si}}$	$\dddot{\overline{\text{Fe}}}$	$\dddot{\overline{\text{Al}}}$	$\dot{\text{Ca}}$	$\dot{\text{Fe}}$	$\dot{\text{H}}$
40·495	33·705	1·095	1·112	2·259	21·816=100·482. G.=2·337

Pyrophyllite from Westanå, (N. J. Berlin, Pogg., lxxviii, 414.) —This pyrophyllite occurs in a quartzose gangue along with micaceous iron ore. G. = 2·78—2·79. Analyses by Berlin:—

	$\ddot{\text{Si}}$	$\dddot{\overline{\text{Al}}}$	$\dddot{\overline{\text{Fe}}}$	$\dot{\text{Ca}}$	$\dot{\text{Mg}}$	$\dot{\text{Mn}}$	$\dot{\text{H}}$
1.	67·77	25·17	0·82	0·66	0·26	0·50	5·82=101·00
2.	65·61	26·09	0·70	0·69	0·09	0·09	7·08=100·35
Oxygen,	34·09	12·42					6·29

In three other trials, the water came out 5·62, 5·77, 7·29. Berlin deduces the formula $\dddot{\overline{\text{Al}}}{}^2\,\ddot{\text{Si}}{}^5+2\dot{\text{H}}$ = Silica 65·66, alumina 29·22, water 5·12. The result is very near Rammelsberg's, who suggested the same formula, and also as equally probable, $\dddot{\overline{\text{Al}}}\,\ddot{\text{Si}}{}^3+\dot{\text{H}}$ = Silica 69 65, alumina 25·73, water 4 62. The composition, as stated by Berlin, is the same with that of an agalmatolite analyzed by Walmstedt; and Rammelsberg remarks upon its approximation to cimolite.

Stilbite of Gustafsberg, Sweden, and Barbro Mine, Norway, (ibid, 415). Analyses by Sjögren:

$\ddot{\text{Si}}$	$\dddot{\overline{\text{Al}}}$	$\dddot{\overline{\text{Fe}}}$	$\dot{\text{Ca}}$	$\dot{\text{H}}$
57·41	16·14	0·25	8·75	16·60 = 99·15
58·41	16·56	——	7·89	16·53, $\dot{\text{Mg}}$, $\dot{\text{Mn}}$, alkalies, 0·54 = 99·93

The mineral was dried in powder at 100 C. over sulphuric acid; and in this way, he found the water to correspond with the usual formula, instead of 18 to 19 p. c., as in many other analyses.

Sodalite from the Island Lamö near Brevig in Norway, (Pogg. Ann., lxxviii, 413.) This lavender blue mineral has been mistaken for glaucolite. It occurs in nodular masses in elæolite, and sometimes has a crust of the latter mineral. Composition according to von Borc :—

$\dddot{\text{Si}}$	$\bar{\dddot{\text{Al}}}$	$\dot{\text{Na}}$	$\dot{\text{K}}$	$\dot{\text{Ca}}$	$\dot{\text{Mg}}$
38·86	30·82	22·03	0·51	1·21	0·44, with *trace* of Sn, Mn, W, Mo, and chlorine undetermined =93·87.

A Red Zeolite from near Upsala, Sweden, (N. J. Berlin, Pogg., lxxviii, 415.)—The red zeolite of Upsala resembles that of Ædelfors. The following are analyses; 1, of the Upsala mineral by Sjögren; 2, of that of Ædelfors by Retzius; 3, the same by Hisinger; 4, a similar mineral from Fahlun by Hisinger; and 5, another from Märtenberg by the same.

	$\dddot{\text{Si}}$	$\bar{\dddot{\text{Al}}}$	$\bar{\dddot{\text{Fe}}}$	$\dot{\text{Ca}}$	$\dot{\text{H}}$
1.	51·61	19·06	2·96	12·53	14·02=100·18
2.	60·28	15·42	4·16	8·18	11·07= 99·11
3.	53·76	18·47	4·02	10·90	11·23= 98·38
4.	60·00	15·6	1·8	8·0	11·6 = 97·0
5.	53·37	16·50	2·36	13·00	10·67= 95·90

Berlin deduces for the Upsala mineral, the formula

$$\dot{\text{Ca}}^3\dddot{\text{Si}}^2+3\bar{\dddot{\text{Al}}}\dddot{\text{Si}}^2+12\dot{\text{H}}$$

=Silica 51·53, alumina 21·49, lime 11·92, water 15·06=100. This is Gerhardt's formula for laumonite.

Berlin suggests that the other minerals may be the same species and that the difference is due to an excess of silica from mixed quartz.

Hyposclerite, (C. Rammelsberg, Pogg., lxxix, 305.)—This mineral from Arendal was named and described by Breithaupt and analyzed by Hermann; the latter obtained the oxygen ratio 1 : 2 : 6 and formula $\dot{\text{R}}^3\dddot{\text{Si}}^2+2\bar{\dddot{\text{Al}}}\dddot{\text{Si}}^2$. Rammelsberg has arrived at a very different result, and for comparison Hermann's analysis is also inserted in this place. G.=2·61, Breithaupt; 2·66, Hermann; 2·63—2·66, Rammelsberg.

	$\dddot{\text{Si}}$	$\bar{\dddot{\text{Al}}}$	$\bar{\dddot{\text{Fe}}}$	$\dot{\text{Ca}}$	$\dot{\text{Mg}}$	$\dot{\text{Mn}}$	$\dot{\text{Na}}$	$\dot{\text{K}}$	
1. Herm.,	56·43	21·70	0·75	4·83	3·39	0·39	5·79	2·65,	$\dot{\text{Ce}}$ & $\dot{\text{La}}$ 2, ign.* 1·87=99·80
2. Ramm.,	67·62	16·59	2·30	0·85	1·46	——	10·24	0·51,	ign. 0·69=100·26.

Rammelsberg thus shows that the species is nothing but albite, giving the ratio 1 : 3 : 12, or more exactly 1·25 : 3 : 12·5. Allowing for a small admixture with pyroxene the slight discrepancy that exists is removed.

Castor and Petalite identical, (G. Rose, Pogg., lxxix, 162.)—G. Rose in this memoir shows that castor and petalite are identical in cleavage, hardness and physical characters, corresponding to their close approximation in chemical composition. From a

* The expression *ign.* in this and other places, stands for "loss by ignition."

specimen in the Berlin Royal Museum he obtained the angle 129°, near that given by Breithaupt; but one of the planes (P) was *not* a cleavage plane. The other plane (M) was transversely striated and a second cleavage was found parallel to the striations, which gave with M the angle 141° 32′–141° 35′. This angle is the same observed in petalite, and this was confirmed from specimens by Prof. Rose, who obtained 141° 35 to 142°. Petalite affords in the same zone on the other side of M still a third cleavage (imperfect, however,) which according to Breithaupt forms with M the angle 117°; but this was not detected with certainty in the mineral castor; the largest specimen in hand was but five lines long. The following results show the similarity in composition.

	$\dddot{\overline{Si}}$	$\dddot{\bar{Al}}$	$\dot{Li}$	$\dot{Na}$	
1. *Petalite,*	77·067	18·000	2·660	2·273=100.	Hagen.
2. *Castor,*	78·012	18·856	$\dot{Li}$ and trace of $\dot{K}$,$\dot{Na}$ 2·760, $\dddot{\bar{Fe}}$ 0·613=100·241,		Plattner.

The characters with the blowpipe and acids are also similar.

Manganesian Idocrase, (M. Websky, Pogg., lxxix, 166.)—This idocrase is associated with a mangan-epidote, heteroclin and a fusible mica in quartz, at St. Marcel in Piedmont. The crystals are like those of ordinary idocrase. Color sulphur to honey yellow; streak white. Glass of borax with a small portion of the mineral becomes readily in the oxydation flame of an amethystine color; and in the reduction flame with tin there is a weak iron reaction.

Thulite from the Iron Mine Klodeberg near Arendal, Norway, (N. J. Berlin, Pogg., lxxviii, 414.)—The thulite of this locality resembles rhodonite; it occurs massive, with a splintery fracture and fine rose-red color. G.=3·34. Composition according to Berlin:—

$\dddot{\overline{Si}}$	$\dddot{\bar{Al}}$	$\dddot{\bar{Fe}}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{Mn}$	$\dddot{V}$	ign.	
40·28	31·84	1·54	21·42	0·66	0·95	0·22	1·32	=98·23;

the alkali was undetermined. The constitution is that of epidote.

Allanite from Krux near Schmiedefeld in Thüringer-Wald, (H. Credner, of Gotha, Pogg., lxxix, 144.)—The allanite occurs in a coarse granite, and also along with crystals of specular iron, overlaid by calcite and fluor spar, in a bed of magnetic iron ore. It is either massive or crystallized. H.=5½ to 6. G.=3·790, mean of four determinations. Not magnetic. Color jet black, with a faint greenish tinge. Streak and powder greenish-gray, inclining somewhat to brownish yellow. B.B. on charcoal fuses with little intumescence to a black shining globule; with borax, and more distinctly still with salt of phosphorus, gives in either flame a brownish yellow glass which is colorless when cold, and with the latter reagent leaves a silica skeleton. In a matrass yields some water, without changing color. Not decomposed by muriatic acid. Composition according to Credner:—

	$\ddot{Si}$	$\bar{\bar{Al}}$	$\dot{La}$	$\dot{Ce}$	$\dot{Y}$	$\dot{Fe}$	$\dot{Mn}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{H}$
1.	36·82	16·94	13·32			17·11	0·56	14·84	0·86	0·28=100·73
2.	37·55	15·99	9·30	3·19	0·56	16·83	0·23	13·60	0·22	1·80= 99·27

The proportion of silica is larger than in other allanites, and that of oxyds of cerium and lanthanum less. Credner observes that Scheerer obtained the oxygen ratio for protoxyds, peroxyds and silica 2 : 3 : 5, but that his result agrees more nearly with 2 : 3 : 6. [Scheerer's formula for his allanite was $3\dot{R}^3\ddot{Si}+2\bar{\bar{R}}\ddot{Si}$, corresponding to the ratio 3 : 2 : 5. Rammelsberg has since examined the orthite of Hitteroe and obtained the ratio 1 : 1 : 2, by special investigation with respect to the state of oxydation of the bases. If so much of the iron in the first of the preceding analyses be considered peroxyd as will give the ratio 1 : 2 between the oxygen of the alumina and silica, the other bases in the analysis being taken as protoxyd, make very nearly the amount necessary to afford the ratio 1 : 1 : 2; calculation giving for the oxygen 9·30 : 9·56 : 19·13.]

On the Mineralogical and Chemical composition of some Rocks; by M. Delesse, (Ann. des Mines, [4] xvi, 233, 323.)—1. *Quartziferous porphyry;* a, *a variety from Montreuillon, district of Château-Chinon,* containing quartz in small dodecahedral crystals, and lamellar macles of white orthoclase, a little red andesite and a green mica, disseminated through a whitish or greenish white feldspathic paste.—b, *a variety from near Saulieu (Cote-d'Or,)* containing grains of hyaline quartz, some orthoclase in reddish lamellar crystals, in a brownish red feldspathic base.

	$\ddot{Si}$	$\bar{\bar{Al}}$	$\bar{\bar{Fe}}$	$\bar{\bar{Mn}}$	$\dot{Ca}$	$\dot{K}, \dot{Na}, \dot{Mg}$,	ign.	
a.	71·7	15·0	2·9	——	0·4	(diff.) 8·8	1·2	=100
b.	77·5	12·9	2·5	*traces*	0·4	" 6·7	—	=100

2. *Feldspar of a Euphotide;* a, *from Mount Genèvre;* b, *from Odern in the Vosges.*—The feldspar in each case is in lamellar macled crystals of the triclinic system, and has a white or greenish color.

	$\ddot{Si}$	$\bar{\bar{Al}}$	$\bar{\bar{Mn}}$	$\dot{Fe}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{Na}$	$\dot{K}$	ign.	
a. (mean)	49·73	29·65	*trace*	0·85	11·18	0·56	4·04	0·24	3·75	=100
b. "	55·23	24·24		$\bar{\bar{Fe}}$ 1·11	6·86	1·48	4·83	3·03	3·05	= 99·83

The specimens were not wholly free from talc and minute veins of serpentine matters. The latter had a neater cleavage than is usual with the feldspar of euphotide, yet was impure also with some carbonate, probably a carbonate of iron, lime and magnesia. Delesse observes that a large proportion of silica and alkalies with less of alumina and lime explains the neater cleavage. He deduces for the $\dot{R}$, $\bar{\bar{R}}$ and $\ddot{Si}$ of No. *b*, the oxygen ratio 1 : 3 : 7. It is evident that the feldspar of euphotide differs in species, and is not always labradorite as Boulanger supposed; it sometimes approaches labradorite, sometimes anorthite, and sometimes other varieties of feldspar.

3. *Diallage of the Euphotide of Odern.*—The diallage is in olive green crystalline lamellæ. It fuses before the blowpipe with very great difficulty, much greater than the feldspar associated with it. It afforded

$\dddot{Si}$	$\bar{\dddot{Al}}$	$\bar{\dddot{Cr}}$	$\dot{Fe}$	$\dot{Mn}$	$\dot{Ca}$	$\dot{Mg}$	ign.
49·30	5·50	0·30	9·43	0·51	15·43	17·61	0·85=98·93

Delesse observes that it approaches hornblende, especially that from Nordmark analyzed by Bonsdorff. The chromic acid he believes to be chemically combined in the mineral. He deduces the formula $\dot{R}^3 \dddot{Si}^2$, which is the formula of the species pyroxene. The oxygen for the protoxyds, peroxyds and silica equals 13·51 : 2·66 : 25·61; and supposing the alumina to replace the silica, the ratio 1 : 2, characteristic of pyroxene, is given. This ratio is more exact when silica and alumina replace one another in the ratio of $3\bar{\dddot{Al}} : 2\dddot{Si}$.

4. *Talc of the Odern Euphotide.*—This talc is in imperfectly radiated translucent lamellæ, often more than a centimetre in length, and constitutes compact masses which cut like a soapstone. Composition:—

$\dddot{Si}$	$\bar{\dddot{Al}}$	$\dot{Fe}$	$\dot{Mn}$	$\dot{Ca}$	$\dot{Mg}$ (diff.)	ign.
59·61	0·81	3·95	0·56	2·88	28·41	3·78=100

This result shows, Delesse observes, that it is near that from Little St. Bernard, analyzed by Berthier, and may be represented by the pyroxene formula $\dot{R}^3 \dddot{Si}^2$.

[The oxygen of the protoxyds and silica equals 12·99 : 30·97 (=1 : 2·38) with 0·38 for that of the alumina. The ratio corresponding is 3 : 7, (the alumina being excluded,) which is equivalent to $\dot{Mg}^9 \dddot{Si}^7$. The ratio 4 : 9 (= 1 : 2·25) which differs but little from the result, affords the formula $\dot{Mg}^4 \dddot{Si}^3$, which has been found for other varieties of talc.]

5. *Serpentine of the Euphotide of Odern.*—The serpentine is sometimes in veins as if subsequently filled into fissures, and sometimes in nodules within feldspar, or so graduates into feldspar that it seems to have been formed at the expense of this mineral or of the euphotide as a whole, by a species of metamorphism.

6. *Hornblende of the Diorite of Pont Jean*, valley of the Moselle, near St. Maurice in the Vosges.—This hornblende is fibrous and of a fine green color and forms lamellar masses. G.=3·059. Composition:—

$\dddot{Si}$	$\bar{\dddot{Al}}$	$\bar{\dddot{Cr}}$	$\dot{Fe}$	$\dot{Mn}$	$\dot{Ca}$	$\dot{Mg}$ (diff.)	$\dot{Na}$	$\dot{K}$	ign.
50·04	8·95	0·24	9·59	0·20	11·48	18·02	0·81	0·08	0·59=100

The soda is attributed to feldspar which was detected in the mineral with a lens.

7. *Feldspar of the Diorite of Pont Jean.*—The hornblende is associated with a greenish feldspathic paste containing striated crystalline tables of a triclinic feldspar, which sometimes have a radiated arrangement. B.B. more fusible than the hornblende, as

usual with the feldspar of diorites; attacked by sulphuric acid. Composition:—

$\ddot{Si}$	$\dddot{Al}$	$\dot{Fe}$	$\dot{Mn}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{Na}$	$\dot{K}$	ign.
53·05	28·66	0·90	*trace*	6·37	1·51	4·12	2·80	2·40=99·81

It is a variety of labradorite. Delesse concludes that the radiated structure is found more especially in feldspars containing the smaller proportions of silica.

8. *Aphanite of Saint-Bresson (Haute-Saône.)*—The color is deep green, verging towards grayish black. It is very tough and compact. It fuses easily in a glass furnace and affords an obsidian about as hard as that of feldspar. G.=2·968. Composition:—

$\ddot{Si}$	$\dddot{Al}$, $\dddot{Fe}$	$\dot{Mn}$	$\dot{Ca}$	$\dot{Mg}$ (diff.)	$\dot{Na}$	$\dot{K}$	ign.
46·83	30·33	*trace*	9·55	6·86	3·57	0·87	1·99=100

The small proportion of silica is worthy of note. The main ingredient is a feldspathic paste, which it is difficult to assign to any species.

9. *Andesite of the Diorite of Faymont.*—Lustre a little greasy and with the other characters of andesite. Composition:—

$\ddot{Si}$	$\dddot{Al}$	$\dddot{Fe}$	$\dot{Ca}$	$\dot{Na}$, $\dot{K}$, (diff.)	ign.
59·38	25·57	*trace*	6·50	7·30	1·25=100

The crystals are often penetrated or enclosed by quartz, showing that they were formed amid an excess of silica.

10. *Hornblende of the Diorite of Faymont.*—Occurs in black lustrous laminæ and in crystals. Composition:—

$\ddot{Si}$	$\dddot{Al}$	$\dot{Fe}$	$\dot{Ca}$	$\dot{Mg}$ (diff.)	Alkalies	ign.
41·99	11·86	22·22	9·55	12·59	1·32	0·47=100

Associated with the hornblende, there is sometimes black mica, pyrites, a little magnetic iron, and sphene.

11. *Feldspar of the porphyry of Schirmeck.*—This feldspar is in tables, oblong and striated, and pertains to the triclinic system. It is slightly greenish and a little pearly in lustre. G.=2·686. Composition:—

$\ddot{Si}$	$\dddot{Al}$, $\dddot{Fe}$	$\dot{Ca}$	$\dot{Na}$, $\dot{K}$, $\dot{Mg}$ (diff.)	ign.
65·74	18·49	4·17	10·60	1·00=100

Delesse considers it an oligoclase, judging from the proportion of silica and amount of lime.*

Wolfram.—Analyses by Robert Schneider of Halle, (J. f. pr. Chem., xlix, 322.)

	$\dddot{W}$	$\dot{Fe}$	$\dot{Mn}$	$\dot{Ca}$	$\dot{Mg}$	
1. Zinnwald,	76·01	9·81	13·90	1·19	——	=100·91=$2\dot{Fe}\dddot{W}+3\dot{Mn}\dddot{W}$.
2. Hartz, Mine Glasebach,	76·04	19·61	4·98	0·28	*trace*	=100·92=$4\dot{Fe}\dddot{W}+\dot{Mn}\dddot{W}$.
3. Hartz, Mine Pfaffenberg,	76·21	18·54	5·23	0·40	0·36	=100·74=$4\dot{Fe}\dddot{W}+\dot{Mn}\dddot{W}$.
4. Hartz, Mine Meiseberg,	76·25	20·27	3·96	0·28	0·15	=100·91=$5\dot{Fe}\dddot{W}+\dot{Mn}\dddot{W}$.

* M. Delesse states in a note that geologists wishing collections of the rocks of the Vosges described in his memoirs, can obtain them by addressing M. Mareine, conducteur des ponts-et-chaussées, à Remiremont (Vosges.) He will give full labels with the specimens, and charges the moderate sum of 30 francs for 100 specimens.

This last result is the mean of three analyses; the per-centage relation for the formula is, $\dddot{W}$ 76·36, $\dot{Fe}$ 19·74, $\dot{Mn}$ 3·90 = 100.

Schneider concludes from his investigations that the true view of the composition of Wolfram is expressed by the genera formula $\dot{R}\,\dddot{W}$, making wolframic or tungstic acid to contain three atoms of oxygen.

Native Copper containing Silver from Chili, (M. F. Field, Quart. J. Chem. Soc., No. ix, iii, 29.)—A native copper from a mine about twenty leagues east of Coquimbo and six from the Cordillera of the Andes, afforded Mr. Field for one specimen, Copper 98·91, silver 1·09 = 100; for a second, of a whitish color, Copper 92·4, silver 7·6 = 100.

Native Gold.—Analyses by A. Levol, (Ann. Ch. Phys., [3] xxvii, 310.)

	Gold.	Silver.	Copper.	Ratio of Au to Ag.	
1. Senegal, grains,	84·50	15·30	0·20	6 : 1=	Au 84·6
2. " large scales,	86·80	11·30	0·90	8 : 1=	88·0
3. N. America, grains,	91·00	8·70	0·30	12 : 1=	91·6
4. California, a piece,	92·70	6·90	0·40	14 : 1=	92·7
5. Senegal, irregular grains,	94·00	5·85	Plat. 0·15	18 : 1=	94·2
6. A piece, loc. not given.	98·30	1·70		58 : 1=	98·1

On the Meteoric Iron of Zacatecas; by Dr. C. Bergemann of Bonn., (Pogg. Ann., lxxviii, 406.)—The Zacatecas meteorite, which has been described by Sonneschmidt, Humboldt, and others, has been recently analyzed by Dr. Bergemann. The mass weighs over 20 cwt. It has a light steel gray color within, a hackley fracture, and the crystalline figures are small and irregular. Specific gravity according to Bergemann 7·4891, at 9° C., which agrees with the determinations by Burkart and Rumler. Analysis gave for its composition:

Fe	Ni	Co	Cu	Mg	Mn	C	S	C & a little Fe	Fe, Ni, P	$\dot{Fe}\ddot{Cr}$
85·094	9·895	0·668	0·630	0·187	*trace*	0·164	0·845	0·334	1·649	1·482

Dr. Bergemann thence deduces for its actual constitution—

Nickeliferous iron, ...	93·77
Magnetic pyrites, ...	2·27
Chromic iron, ...	1·48
Phosphuret of nickel and iron, . .	1·65
Carbon, ...	0·49
	99·66

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Preparation of Metacetonic Acid;* by Dr. F. Keller, (Chem. Gazette, May 1st, from Liebig's Annalen, Feb., 1850.)—A quantity of wheat paste is mixed with ten parts of water at 122°–140° F.; one fourth its weight of scraps of leather is then added with a quantity of chalk and the whole left in a warm place to ferment. At the end of a few days in summer, the process is complete and a light spongy mass subsides. The lime salt is converted into a soda salt, and the acid separated by sulphuric acid, is found to be a mixture of metacetonic with acetic acid. These are separated by saturating a portion with carbonate of soda, adding the remainder and distilling off the metacetonic acid, which by this process is readily obtained in large quantities. T. S. Hunt.

2. *On the Amid Compounds of Tungsten;* by Prof. Wöhler, (Chem. Gazette, May 1st, from Göttingen Nachrichten, 1850.)—When dry ammonia is passed over protochlorid of tungsten, a reaction takes place at first spontaneous which is completed by the aid of a gentle heat, sal-ammoniac and hydrogen gas being evolved. The product is a black partially fused mass, which evolves ammonia when heated in the open air and finally burns, yielding yellow tungstic acid. Heated to whiteness in a charcoal lined crucible, it leaves metallic tungsten, and a similar result is obtained in a current of hydrogen gas at a low red heat, ammonia being disengaged. Fused with hydriodate of potash, a tungstate is formed with the evolution of hydrogen and ammonia. From the results of analysis, M. Wöhler concludes, that there are two very similar compounds, a nitruret and amidid of tungsten, the second being a product of the decomposition of the first by the action of hydrogen, and that the two are generally obtained in a state of admixture. He represents the one as $2W N+WNH_2$ and the other as W_2N+WNH_2.

The action of ammonia upon tungstic acid at a low red heat yields a substance resembling the latter in its properties, of which the composition is represented by $3WN+W_2NH_2+2WO_2$. Identical with or very similar to this is the product obtained by igniting out of contact of air a mixture of tungstate of potash with chlorid of ammonium, which was a long time since described by the author as the black oxyd of tungsten. It however evolves hydrogen and ammonia by the action of hydrate of potash, and even when heated alone. When heated to whiteness in a close vessel, it leaves metallic tungsten.

Wöhler finds, that contrary to the statement of Berzelius, tungstic acid is not reduced to the metallic state by the action of hydrogen; at a temperature above the melting point of silver an oxyd is obtained which is not farther changed. It is of a violet brown color, and under a magnifier appears crystalline with a metallic lustre.

[These different combinations of tungsten, nitrogen and hydrogen, with the addition in some cases of oxygen, will probably be found to correspond to different ammoniacal salts of the polymeric forms of tungstic acid as yet imperfectly known. The different amids of phosphoric acid already described by M. Gerhardt are analogous compounds.] T. S. H.

3. *On the Decomposition of Iodid of Potassium;* by Prof. SCHÖNBEIN, (Chem. Gazette, May 1st, 1850, from Pogg. Annal., lxxviii, p. 513.)—The anhydrous metallic acids, arsenic, molybdic, tungstic, antimonic and chromic, when triturated with iodid of potassium, separate iodine even in the cold, and abundantly by the aid of heat; artificial stannic acid as well as the native tin-stone effect a similar decomposition by heat. With bichromate of potash the whole of the iodine may be expelled, while neutral chromate of potash and oxyd of chromium remain behind; three parts of the bichromate suffice to decompose two of the iodid, the reaction being

$$5(KO, 2CrO_3)+3KI=8(KO\,CrO_3)+Cr_2O_3+3I.$$

Anhydrous perchlorid of iron liberates iodine from the iodid of potassium at the ordinary temperature, and a highly concentrated solution of the perchlorid of iron, mixed with an equally concentrated solution of the iodid, precipitates a large portion of the iodine in a crystalline form; $Fe_2Cl_3+KI=2Fe\,Cl+K\,Cl+I$.

The persulphate and all the persalts of iron act in a similar manner, so that the peroxyd of iron may be used instead of the peroxyd of manganese to separate iodine from alkaline iodids.

The dry persalts of copper, even the carbonate, liberate iodine from the iodid on the application of heat.

The bromid of potassium in the dry way is affected by most of these reägents, but with less ease than the iodid. Bichromate of potash and persalts of iron are most efficient; the latter may also be employed in the moist way to obtain bromine by distillation.

The chlorids of K and Na are not decomposed by any of the above agents, even at a strong heat; but a partial decomposition is effected especially with bichromate of potash and the chlorids of Ba, Sn, Ca and Mg, attended with the evolution of chlorine and the formation of neutral chromates and oxyd of chromium. T. S. H.

4. *On Furfurol;* by A. CAHOURS, (Ann. de Chim. et de Phys., xxvi, 277).—The author has reëxamined this substance which he prepared by the action of sulphuric acid upon bran as recommended by Dr. Fownes;* by employing a little less sulphuric acid he obtained a much larger portion of the oil, 100 parts of bran yielding 2·75 parts of furfurol. The distillation of bran with water alone, does not yield any, nor can it be obtained from lignine, starch or gluten, even by the aid of sulphuric acid.

The analyses of M. Cahours confirm the exactness of the composition given by Fownes to furfurol. The specific gravity of its vapor is 3·342–3·346, corresponding for two volumes to the formula $C_5H_4O_2$, the equivalent which had already been adopted by Laurent and Gerhardt.

Chlorine and bromine give with furfurol only resinoid products; nitric acid, whether concentrated or diluted, acts energetically upon it, and

* This Journal, new series, vol. ii, p. 407.

gives as the final product oxalic acid. M. Cahours has also confirmed the results of Fownes as to furfuramid and furfurine. The action of sulphuretted hydrogen upon an alcoholic solution of furfuramid or of hydrosulphuret of ammonia on furfurol produces a sulphuretted compound *thiofurfurol*, $C_5 H_4$ (OS). Seleniuretted hydrogen gives an analogous compound. By dry distillation thiofurfurol is transformed into a substance crystallizing in colorless needles, very soluble in alcohol and slightly soluble in boiling water. It contains $C_9H_8O_2$ and is formed from two equivalents of furfurol; $2C_5H_4(OS)=C_9H_8O_2+CS_2$.
T. S. H.

5. *On the action of Chlorine upon Lactic Acid;* by G. Staedeler, (Ann. der Chem. und Pharm., lxix, 333.)—The author has recognized the production of *aldehyde* and *chloral* by the distillation of lactic acid with a mixture of sea-salt, peroxyd of manganese, and sulphuric acid. When the quantity of chlorine is small the aldehyde predominates.
T. S. H.

6. *Preparation of Oxychlorid of Carbon;* by A. W. Hofmann, (Ann. der Chem. und Pharm., lxx, 129.)—A convenient and rapid mode of preparing this compound of oxyd of carbon and chlorine is to pass a stream of carbonic oxyd gas through perchlorid of antimony in ebullition; the salt is reduced to a protochlorid. This reaction may serve to detect oxyd of carbon in a gaseous mixture, for the odor of the oxychlorid is very characteristic.
T. S. H.

7. *On the Preparation of pure oxyd of Cobalt;* by P. Louyet, (Comptes Rendes des Trav. de Chim., Oct., 1849.)—The ordinary process for purifying oxyd of cobalt from the oxyds of nickel and iron with which it is almost always associated, is that of M. Liebig, which consists in converting them into sulphates and exposing the dried salts to a red heat. By this means the sulphates of iron and nickel are decomposed with the escape of their acid, while the sulphate of cobalt is unchanged. The author has however found that when mixed with a large amount of sulphate of cobalt, a portion of the iron salt escapes decomposition even after a long continued red heat. He recommends for the purification of the product thus obtained the following process.

The calcined sulphate is dissolved in water, and a portion of gelatinous hydrated oxyd of cobalt is added, or what is the same thing, a quantity of carbonate of soda sufficient to precipitate in the form of carbonate a portion of the cobalt. The mixture is digested for some time at a boiling heat, until the violet color of the oxyd of cobalt is replaced by the dirty yellow of oxyd of iron, from the decomposition of the iron salt by the cobaltic oxyd. If the cobalt precipitate be slightly in excess, the salt of cobalt remaining in solution contains not the slightest trace of iron; the nickel of the impure oxyd is entirely removed by the previous process of calcination.
T. S. H.

8. *On the action of heat upon the Caprylate of Lime;* by G. Guckelberger, (Ann. der Chem. und Pharm., lxix, 201.)—The dry distillation of this salt mixed with an excess of hydrate of lime yields an oily matter, which after a little time becomes solid, and is purified by crystallizing in boiling alcohol. It is *caprylone*, an acetonid like that afforded by the lime salts of the homologous acids, and forms a

white crystalline mass resembling china wax. It is lighter than water, in which it is insoluble, tasteless and nearly inodorous; it fuses at 40° C., and boils at 178° C., distilling without alteration. Analysis gives for its formula $C_{15}H_{30}O$, and its formation is by the polymorphosis of two equivalents of caprylate; $2C_8H_{15}BaO_2 - CO_3Ba_2 = C_{15}H_{30}O$.

It is not affected by potash, nor by nitric acid in the cold; by heat the action of the latter is violent, yielding a yellow compound which forms detonating salts and is evidently a nitrized acid. T. S. H.

9. *On the determination of Nitrogen;* by NOELLNER, (Ann. der Chem. und Pharm., lxvi, 314.)—In the determination of nitrogen by igniting the substance with soda-lime after the process of Will and Varrentrapp, the author proposes to pass the ammoniacal vapors through a solution of tartaric acid in absolute alcohol. The bitartrate of ammonia which is thus formed is complely insoluble, and is deposited in a crystalline form. T. S. H.

10. *Examination of Castoreum;* by F. WOEHLER, (Ann. der Chem. und Pharm., lxvii, 360.)—The author had already suggested the existence of *phenol* in this substance, and has been able to verify it by distilling the castoreum with water, when a small portion of an oily liquid having all the reactions of phenol was obtained. The residue of this distillation yielded crystals of *benzoic acid* and *salicine*, and the mother liquid from the crystals of the benzoic acid gave with ferric salts the reactions of *salicylic acid.* T. S. H.

11. *On the Composition and Metamorphoses of Conine;* by J. BLYTH, (Compt. Rend. des Trav. de Chem., Oct., 1849, from Ann. der Chem. und Pharm., lxx, 73.)—The recent analytical results of this chemist do not accord with those already obtained by M. Ortigosa. The difficulty of obtaining the alkaloid in a pure state is such that it is not easy to deduce a reliable formula except from the analyses of its saline combinations. From a careful comparison of the results, M. Gerhardt is led to retain the formula already adopted by him in his Précis (vol. ii, p. 66), $C_8H_{15}N$, or in the ordinary notation, $C_{16}H_{15}N$; that of Mr. Blyth is $C_{17}H_{17}N$, and that of Ortigosa $C_{16}H_{16}N$, neither of which gives a number of equivalents of nitrogen and hydrogen divisible by 4. The results obtained by M. Ortigosa approach very closely to those calculated from M. Gerhardt's formula.

According to Mr. Blyth, the boiling point of conine is 168°–171° C.; but it is altered by heat so that the temperature rises during the distillation. Its density is ·878. It is volatile at ordinary temperatures, giving off a pungent odor which affects the eyes and produces white fumes with nitric, hydrochloric and acetic acids. In a dry state it does not affect test papers, but on the addition of a drop of water its reaction is strongly alkaline. Conine readily coagulates albumen, and precipitates the salts of Cu, Pb, Zn, Mn, Al and Fe, it precipitates also nitrate of silver but an excess of conine dissolves the precipitate; it dissolves the chlorid of silver as readily as ammonia. Most of the salts of conine are decomposed by evaporation, leaving gummy residues; many of these are crystallizable as the hydrochlorate, but very deliquescent.

Conine is a very alterable substance and resinifies by the action of the air; the ordinary product of its oxydation is *butyric acid*, which is obtained from it in various ways,—by boiling a solution of the chloro-platinate, by acting upon it with an excess of bromine and evaporating in the product *in vacuo*, by chromic and nitric acids, etc. According to M. Gerhardt the reaction will be as follows $C_8H_{15}N+2H_2O+O_2=2C_4H_8O_2+NH_3$.

Mr. Blyth supposes a simultaneous formation of carbonic acid which his formula demands. T. S. H.

12. *On the Composition and Metamorphoses of Piperine;* by TH. WERTHEIM, (Comptes Rend. des Trav. de Chim., Oct., 1849, from Annal. der Chem. und Pharm., lxx, 58.)—The *chloro-platinate of piperine* is obtained by mixing concentrated alcoholic solutions of piperine and bichlorid of platinum, and having added an excess of concentrated hydrochloric acid, abandoning the mixture to spontaneous evaporation. It is obtained in very large fine crystals of an orange red color, which are very little soluble in water, but are apparently decomposed by a large portion, and are to be washed with strong alcohol. The composition of this salt deduced by the author from his analyses is ($C_{70}H_{37}N_2O_{10}$, ClH, PtCl_2), and he concludes that crystallized piperine contains 2 eq of Aq, and is $C_{70}H_{37}N_2O_{10}+2HO$.

When piperine is mixed with three or four parts of soda-lime and exposed to a temperature of 150°–160° C., there passes over a large amount of a colorless oily alkaloid, without a trace of ammonia, which is found to be identical with the *picoline* of Mr. Anderson. The residue is brown and contains a yellowish azotized resin having acid properties. If the mixture of piperine and soda-lime is heated to 200° C., ammonia is disengaged with the picoline, and the residue contains an uncrystallizable acid which is not nitrogenous.

The formula above proposed does not correspond with the law of divisibility, and M. Gerhardt proposes in its place the following formula deduced from the analyses of M. Laurent and his own, and closely according with the results obtained for the chloro-platinate by the author. $C_{35}H_{36}N_2O_5$+Aq, or in the German notation, $C_{70}H_{36}N_2O_{10}+2HO$.

He remarks that the decomposition evolving picoline and ammonia, shows piperine to be a diamid, analogous to the *carbanilamid* of M. Chancel,* in which the two equivalents of alkaloid will be represented by picoline and ammonia coupled with a non-azotized (bibasic) acid; the azotized acid resinous body described, will be the monobasic amid-acid of the former. These two acid bodies have been analyzed by M. Wertheim, but not being crystallizable we have not yet data for determining satisfactorily their composition. T. S. H.

13. *On a New Gunpowder;* by M. AUGENDRE, (Comptes Rendus, in Chemical Gaz., May, 1850.)—The proportions giving least residue with maximum effect are—

Powdered crystallized yellow prussiate of potash, . .	1 pt.
" white sugar, ...	1 "
" chlorate of potash, ...	2 "

The ingredients separately powdered are carefully mixed, small quan-

* This Journal, March, 1850, p. 275.

tities may be rubbed together in a mortar; for large quantities 2 or 3 per cent. of water must be added and the mixture made in a bronze mortar with a wooden pestle, and it may then be granulated and dried.

This compound differs from ordinary gunpowder in being explosive even in the form of fine powder, no granulation is therefore needed. Other advantages claimed for it are its easy formation, the ingredients previously powdered may be mixed only when wanted—the want of action of air on the separate materials, while charcoal for gunpowder is injured by exposure; and lastly, the greater force.

On the other hand are its erosive effect on iron barrels, for which bronze must be substituted, and its dangerous inflammability.

[This compound having already obtained a newspaper notoriety has probably been very generally tried. The cost of materials and the difficulty of keeping it will be a bar to very extensive use. In our own trials, it was found to become somewhat moist, and the original pulverization was found difficult in the case of the sugar and prussiate of potash.

It is dangerous to introduce gunpowder, charcoal or sulphur; the smallest quantity caused, in the hands of the discoverer, a tremendous explosion.] G. C. SCHAEFFER.

14. *Purification and properties of Chloroform;* by Prof. WM. GREGORY, M.D., (Med. Jour. Sciences, in Chem. Gaz., May, 1850.)—Prof. G. in general confirms the results of Soubeiran and Mialhé.* Still even from the hands of the best manufacturers, chloroform has until recently been found contaminated by chlorinated oils mentioned in our former notice. Prof. G. considers the disagreeable effects of chloroform as entirely due to these oils. It is therefore a matter of some consequence to have delicate tests for the purity of this important substance.

Pure and colorless sulphuric acid of 1·84 at least, on agitation is colored yellow or brown, as the oil is more or less impure. Perfectly pure chloroform does not color the acid.

Pure chloroform when poured upon the hand or a handkerchief, rapidly evaporates, while the less volatile oils remain and are recognized by their color, which is quite persistent. Dr. Simpson mentioned to the author that while using a chloroform, which had so constantly produced unpleasant effects that he threw it away, the handkerchiefs became quite offensive from the smell left upon them which remained after washing. Another test is the specific gravity which for the perfectly pure article is 1·500.

Mr. Kemp, the author's assistant, also noticed another remarkable test of purity. As soon as the acid was no longer colored by the chloroform, the latter exhibits a strong convexity downwards toward the acid.

The process for purification proposed by Mr. Kemp is to agitate with strong pure sulphuric acid, allowing the liquid to remain in contact, with occasional agitation. Half its volume of acid will be enough, and if but little color is given, a second use of the acid is not needed, but

* This Journal, Jan., 1850, p. 115.

this should be tried on a small portion in a test tube for greater certainty—the purification is finished with peroxyd of manganese, with which it is to be agitated and left in contact until the odor of sulphurous acid is removed.

Redistillation is not required, in fact, is not necessary to the manufacturer, who has only to wash well the first product with water and purify as above.

As an instance of what the author considers the gross ignorance of persons pretending to manufacture such articles, he refers to a sample examined by him of sp. gr. about 1·000, and which seemed to have the following origin. The maker obtained two fluids from distillation, not knowing that the heavy one was chloroform he threw it away and put up the lighter, a mixture of pyroxylic spirit, its original impurities, the chlorinated oils and a mere trace of chloroform, and labelled it *pure chloroform.* Almost pure *from* chloroform, Dr. G. well says.

It is useless to call such conduct the result of gross ignorance, for in such cases a gross ignorance is gross rascality. G. C. S.

15. *On the preparation of Chlorate of Potash.*—Prof. F. C. CALVERT, in a paper read before the Chemical Society of London, describes the following process as producing a maximum quantity of chlorate. The current of chlorine is passed into a solution of 100 fl. grs. containing 102·33 grs. anhydrous potash—its density 1·1. ·358 grs. quick-lime are to be added and the whole slightly heated. The result was 220 grs. of the chlorate beside about 20 grs. left in the mother water. The precise density of the liquid is important. G. C. S.

16. *Aspartic Acid formed from Bimalate of Ammonia ;* by M. DESSAIGNES, (Comptes Rendus, March, 1850.)—Piria has proved that asparagine and aspartic acid are the amids of malic, as oxamid and oxamic acid are the amids of oxalic acid.* Succinic acid produced in the fermentation of asparagine has also been obtained from the fermentation of the malates in such quantities as to render the malates the best source of this hitherto rare acid.† Still all attempts at forming asparagine or aspartic acid from malic acid have failed, as chemists have not as yet succeeded in forming malic ether.

The author has however reached the desired end by the dry distillation of bimalate of ammonia. The residue with a heat of about 370° is a sparingly soluble amorphous reddish mass. Well washed with hot water this mass is changed into a very stable acid which however is not the aspartic. Heated for some time with hydrochloric acid this substance is no longer precipitated by water and yields on evaporation crystals containing hydrochloric acid which are easily purified. It is only necessary to dissolve these in boiling water, divide the solution, neutralize one-half in the ammonia and add the other half, to obtain on cooling a quantity of prismatic crystals of aspartic acid.

The form of aspartic acid thus obtained is not identical with that of the same acid from asparagine, but the salts are identical in form with the aspartates hitherto known. Analysis renders it certain that this is in reality aspartic acid.

* This Journal, Nov., 1848, p. 420. † Ibid., Jan., 1850, p. 117.

[Possibly the intermediate compounds above described may be an amid of one of the acids into which the malic is so readily transformed by heat—but as the anhydrid of malic acid is known, this acid would be the anhydrid of aspartic acid.] G. C. S.

17. *Stibethyle;* by C. Löwig and E. Schweitzer.—The authors decomposed "iodid of ethyl" by the alloy of antimony and potassium, formed according to Serullas' process by igniting antimony and cream of tartar. The decomposition is violent, and the product, *stibethyle*, is spontaneously inflammable—hence numerous precautions are required in manipulation. The formula given is $C_{12}H_{15}Sb$, which the authors regard "equivalent with antimoniuretted hydrogen in which three atoms of hydrogen are replaced by three atoms of ethyle," and hence conclude, that "*its composition presents nothing remarkable.*" They also regard it as a "radical" analogous to kakodyle.

Kakodyle has not held its ground as a radical, and the new substance must soon lose all claim to that title.

Stibethyle seems to form a fine crystalline salt with nitric acid, but the property of forming a salt with an acid has never been claimed as belonging to radicals, but to their oxyds.

The following view of the composition of this substance seems to be probable, and with due respect to its able and distinguished discoverers, we consider it as highly remarkable.

Antimony like phosphorus belongs to the nitrogen class and forms corresponding compounds. Antimoniuretted hydrogen is hence a sort of ammonia, we can hardly say ammonia with its nitrogen replaced by antimony, although this expression is used. SbH_3, or stib-ammonia, can take the place of NH_3. In the compound ammonias of Wurtz, that of the caproic series would be $C_{12}H_{15}N$. Stibethyle is then caproamine, in which antimony takes the place of nitrogen. The spontaneous inflammability of the new substance is owing to the presence of antimony, which in its relations to oxygen differs from nitrogen.

Wertheim has shown that by the action of hydrate of potash, narcotine yields C_6H_9N, or the compound ammonia of the metacetic series. We add a tabular view of the known compound ammonias, named according to the series to which they belong.

		Discovered by
Hydric, . .	H_3N	———
Formic, . .	C_2H_5N	Wurtz.
Acetic, . .	C_4H_7N	"
Metacetic, . .	C_6H_9N	Wertheim.
Butyric, . .	$C_8H_{11}N$	Anderson.
Valeric, . .	$C_{10}H_{13}N$	Wurtz.
Caproic, . .	$C_{12}H_{15}Sb$	Löwig and Schweitzer.

G. C. S.

18. *Action of Nitric Acid on Rhubarb.*—M. Garot (Journ. de Pharm. et de Chem., Jan., 1850) describes the result of the action under the name of *erythrosin*. From 10 to 20 per cent. may be obtained from the rhubarb. No analysis has been made—but there seems to be a mixture of rhubarbine of former authors and a nitric compound;

probably similar to acids derived from aloes by the action of nitric acid. Like these acids, erythrosin possesses high tinctorial power, and is recommended for its properties as a suitable dyeing material.

G. C. S.

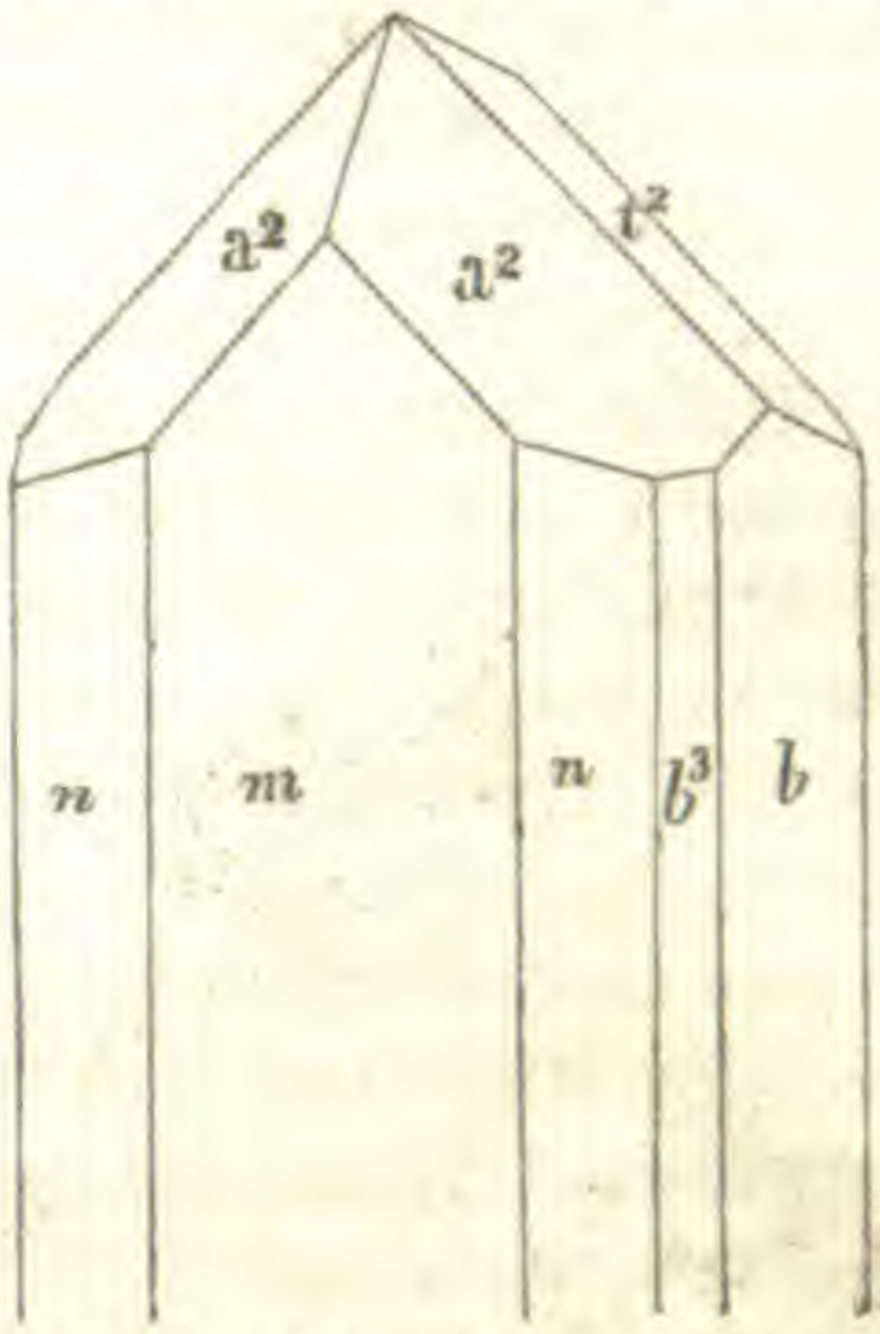
19. *On the Atomic Weight of Molybdenum;* by N. J. BERLIN, (J. f. pr. Chem., xlix, 444, 1850.)—From the analyses of three molybdates of ammonia, Berlin deduces for the atomic number of molybdic acid, ($\dddot{Mo}$) as follows, 875·75, 876·57, 873·42, 873·25, affording the mean 874·75. He hence concludes that the number obtained for molybdenum by Svanberg and Struve, 575·83, based on 200 as the value of sulphur, must be right or very near the true one; and not 588·966, based on 200·75 as the value of sulphur. Hence also 200 is the true atomic number for sulphur.

II. MINERALOGY AND GEOLOGY.

1. *On a new Spodumene Locality at Norwich, Mass.;* by C. HARTWELL, Member of the Theological Institute of East Windsor, Ct., and E. HITCHCOCK, Jr., of Amherst, Mass.—This locality, in the north part of Norwich, was discovered by Mr. Eben Weeks of that place, and opened by the writers. The spodumene occurs in large veins of quartz, surrounded and partly intermingled with albite and feldspar, running north and south through a hill of mica slate. It has been seen for the most part in one large vein, which appears at the surface only in two places, about thirty rods apart. Associated with the spodumene, are crystals of staurotide of a new form, mica, imperfect specimens of beryl, and enormous black and blue tourmalines not perfect in figure. One vein, mostly albite, exhibits the spodumene in a decomposed state nearly as soft as gypsum. The spodumene is doubtless abundant, though a large quantity is not attainable without great expense. Crystals have been obtained with the breadth ranging from one-half inch to four inches, and some much larger are visible in the rock. It is usually massive in the albite, extended into the quartz in crystals, terminated at one end, and sometimes more than a foot in length. The longest individual that has been obtained measures eleven and a half inches. Much of the mineral has a beautiful pinkish hue. The crystals are usually opaque, but some are nearly translucent in parts. Good specimens are acquired with difficulty. One reason lies in the perfection of the cleavages, especially that parallel with the orthodiagonal, which renders them easily broken. Another lies in the nature of the gangue, as the quantity is full of rifts, rendering blasting difficult, and allowing access to the mineral for the weather, which has given it all a rusty appearance. The crystals can be cleaned by acid with no injury except perhaps a little damage to the lustre. The forms of the terminations are various. The crystal having the greatest number of planes, has been described in Dana's System of Mineralogy, 3d ed., p. 693, and now belongs to Prof. B. Silliman, Jr. Other crystals are similar in general form, but with a less number of planes. One of the specimens has only *P* wanting, and *o′*, *o″* are well developed. In others the plane *a*, *o′* and *o″* are absent as well as *P*, as in the following figure; but the plane *a* is usually present.

The angles obtained with the common goniometer are as follows:—

m (back plane)	: t^2	$=102° 30'–106°$
n	: t^2	$=134°$
b	: t^2	$=138° 20'$
a	: t^2	$=148°$
t^2	: t^2 (over the summit)	$=80° 30'$
m	: a^2	$=116°$
b	: a^2	$=134°$
n	: a^2	$=144°$
b^3	: a^2	$=142°$
b	: a^2	$=135°$



The angle m : t^2 is generally about 102° 30′, although one crystal gave us 106°.*

The surface m in all the specimens, is finely covered with lines, as mentioned by Dana.

The new form of the staurotide occurring at the same locality,† gave us the following angles. (See figure, p. 121.)

$$P : t = 131°$$

$$b^2 : b^2 \text{ (over b)} = 86°$$

This form is not usually found entire, but a few specimens have been obtained with only a slight imperfection. The plane P is frequently inclined from the true position, and often but one is found. In some individuals the plane b is nearly or quite obliterated by the planes N, in others it deeply truncates the crystal. Many of the crystals appear joined to the spodumene, in connection with which it usually occurs. The mineral is very brittle, and hence arises the difficulty of obtaining good forms.

2. *Age of the Nummulitic formation of the Alps;* (from the Address of Sir C. Lyell before the Geol. Soc., Quart. Jour. Geol. Soc., No. 22, 1850.)—In the chronological classification of the materials composing the crust of the earth, it has been often asked, whether we ought to ascribe to the older tertiary epoch, or to the cretaceous system, the great nummulitic formation of the Alps, and other parts of Europe. This much-controverted question,—one, as I shall presently point out, of the highest theoretical interest, in reference to the hypothesis of the unabated intensity of the existing agents of change,—was declared by M. Boué, some years ago, to be the great problem of the day, and Sir R. Murchison has therefore devoted to its consideration a large portion of his memoir. M. Boué indeed announced in 1847 his own conviction that the nummulitic rocks belonged to the eocene or lower tertiary period, and remarked, in a paper read to the French Geological So-

* New measurements of several crystals recently received confirm this result. In one crystal, 109° were obtained; but in those of the best planes, using a cleavage surface for *m*, the angle obtained was 103°.— J. D. D.

† See last number of this Journal, p. 121.

ciety in that year, how much delight Alexander Brongniart would have experienced, had he lived to see one of his boldest and most startling generalizations thus crowned with success.* Al. Brongniart had in fact declared many years before, that the shells of the summit of the Diablerets, one of the loftiest of the Swiss Alps, which rises more than 10,000 feet above the sea, were referable to species characteristic of the eocene strata of the neighborhood of Paris. He only felt considerable hesitation, he said, in assigning to them so modern a date, because the overlying limestones were so compact and homogeneous as to agree in lithological character with much older secondary rocks.

Several of the most animated discussions which have taken place in this room since 1825, have turned, as you will recollect, on this subject, especially when the fossil shells brought by Mr. Pratt from Biaritz in the Pyrenees were laid upon our table. A decided opinion was then expressed by many of us that the nummulitic series of that southern chain must be referred to the lower part of the eocene group, as it was made clear that the proportion of fossil species common to the Biaritz beds and the chalk was extremely small—much too small to imply a cretaceous age for the strata in question, or even a zoological passage from the cretaceous to the tertiary formations. They who have read with care the successive numbers of the 'Bulletin' of the Geological Society of France, are aware how much that body has been occupied with the same problem, and how steadily the evidence in favor of the same important conclusion has been gaining strength. M. d'Archiac, writing in 1847 on the fine collection of Biaritz shells submitted to his inspection by Mr. Pratt, observed that forty-eight, or one-fourth of the whole series, were identical with fossils of the lower eocene of the Paris basin, while the rest were all tertiary forms except four, which belonged to species of the chalk.† In a paper by M. Deshayes, read to the Geological Society of France in June, 1844,‡ that able conchologist declared, after examining the Biaritz fossils, "that the whole of the nummulitic system must be classed as tertiary; an opinion confirmatory," he said, "of the results previously arrived at by M. Leymerie in the Corbières, and of M. Bertrand Geslin in the Alps." Lastly, I may observe, that you will find similar opinions recorded in the 'Bulletin,' either in the memoirs or verbal comments of MM. Deshayes, Charles Desmoulins, Raulin, Leymerie, Tallavigne, Delbos, Desor, Boué, Archiac, and Alcide D'Orbigny, all published in the course of the last six years. Whether a real transition from the cretaceous to the tertiary strata can be made out, is a point which has also been fully discussed, and how far the Maestricht beds are represented in the Pyrenees. It appears from the researches of MM. Desmoulins and Raulin, that some few of the characteristic fossils of Maestricht have really been found in that chain; but you will, I think, agree with M. Deshayes, that they are not enough to establish the existence of any true equivalent of the Maestricht group—that distinct and uppermost division of the chalk to which the Faxoe coralline limestone in Seeland, as well as the pisolitic strata of Sezanne near Paris, are referable.

* Bulletin, vol. v, 2nd series, pp. 69, 71.
† Bulletin, vol. iv, 2nd Series, p. 1006.
‡ Translated in Quart. Journ. Geol. Soc., 1845, p. 111.

When we consider that the age of the nummulitic formation of the Pyrenees, however clearly it may now be determined to be tertiary, has been regarded by so many able authorities as a subject of perplexity and debate up to so late a period, we cannot feel surprised that MM. De Beaumont and Dufrénoy, in constructing their geological map of France many years before, should have referred these strata in the Alps, and in the regions bordering the Mediterranean, to an age anterior to the calcaire grossier of Paris, especially when we learn that even now M. Agassiz affirms, that out of 139 species of echinoderms described by him from the nummulitic beds of the Mediterranean, one species only is common to them and the calcaire grossier. The same geologist maintains that all the fish of Glarus and Monte Bolca, which according to the latest opinions must be classed as eocene, differ entirely from those of Sheppy.* Yet I am by no means disposed to question, on the ground of this want of agreement in the ichthyolites, that the Glarus slates are in truth tertiary, still less to doubt that the limestone of Monte Bolca belongs to the same period: I have always regarded the latter as eocene from the time when I visited that locality in company with Sir Roderick Murchison in 1828. You have seen also, in the classification of the three successive eocene formations established by Mr. Prestwich for the older tertiary deposits of Great Britain, that while each division is characterized by its peculiar assemblage of shells, a part only of the species pass from one division to another, and that the specific difference of the mammalia belonging to each division, and still more of the first, as determined by Agassiz, is extremely marked.

The researches, above alluded to, of Sir Roderick Murchison in the Alps in 1847, and the palæontological evidence of various eminent writers brought together by him in illustration of his views, have, I think, shown unequivocally, that, together with the nummulitic limestone, an enormous thickness of overlying strata of dark-colored slates, marls, and fucoidal sandstones, provincially called Flysch, are separable from the cretaceous system of Northern Europe, and must also be regarded as lower eocene. His attempt however to make out a passage from the tertiary to the secondary series by means of an intervening group of marls, green sandstone and impure limestone, appears to me to be far less successful, since a true representative of the Maestricht beds is wanting in the Alps, or is very ill-defined, and no other equivalent assemblage of organic remains is enumerated sufficiently rich in forms, or intermediate in character, to fill up the wide gap between the eocene strata and the chalk.

I have dwelt thus at length on the age of the nummulitic series, because its recognition as a tertiary deposit draws with it consequences of the utmost theoretical importance, and is singularly confirmatory of a remark made by M. Desnoyers many years ago in his address to the French Geological Society, namely, "that the more the Alps are studied the younger they grow." This saying was elicited by the admission by competent observers, that certain schistose rocks of great thickness, containing dark writing slates, originally classed as "transition

* Bulletin, vol. v, pp. 414, 415.

formations" by some of the followers of Werner, and regarded as of palæozoic age, were really secondary. Now we are called upon to go much further; for these same strata belong to the flysch, and therefore constitute what is by no means the base of the eocene system. To the English geologist who is old enough to remember when all the soft clays and loose sands overlying the chalk, some of them containing shells of species identical with those now living, were looked upon as very modern, and as the creations of yesterday, in comparison with the rocks of the higher Alps, it may well appear a startling proposition to learn that the clay of London was in the course of accumulation as marine mud at a time when the ocean still rolled its waves over the space now occupied by some of the loftiest Alpine summits. It will follow, moreover, as a corollary from the same data, as before hinted, that not only the upheaval of the Alps, but all the principal internal movements, dislocations, inversions and contortions of the strata, are subsequent to the origin of the nummulitic deposits, and had not therefore even commenced till great numbers of the eocene vertebrate and invertebrate animals had lived and died in succession.

3. *On the Geographical Limits of the Chalk Formation;* by Leopold von Buch, (from the Monatsberichte der Akademie der Wissenchaften zu Berlin, für 1849, p. 117. Compare also, Betrachtungen über die Verbreitung und die Grenzen der Kreide-Bildungen, Bonn, 1849. Aus den Verhand. des naturhist. Ver. der. Pr. Rheinlande; cited from the Quart. J. Geol. Soc., No. 21.)—The small extent towards the poles which the chalk formation attains, compared with the Jurassic strata, and still more with the palæozoic deposits, has been regarded by Dr. Boué, not without some probability, as the most ancient known effect of the influence of climate on the fauna of former worlds. In reality the most northerly point on the whole earth in which chalk has as yet been found is, according to Prof. Forchhammer's determination, in the vicinity of Thistedt in Jutland, not quite in 57 degrees of latitude, or in that of Aberdeen in Scotland, of Calmar, Mitau, Twer and Casan. In the British islands the chalk does not reach so far north; the last appears on the south coast of the island of Rathlin near the Giant's Causeway, in the latitude of Apenrade, of Bornholm and of Tilsit. Flamborough Head in 54° is its last appearance in England. In Russia this limit always sinks deeper towards the south. From Grodno, where the chalk still appears in 54°, it runs, as laid down in Murchison's masterly geological map, through Mohilew and Orel, a degree and a half of latitude south of Moscow, and from Simbirsk downwards along the Wolga, even to the Caucasus. MM. Murchison, Verneuil and Keyserling have very unexpectedly discovered this chalk on the banks of the Ural river, ninety English miles below Orenburg, in 51½°. The Muchodjar mountain determines its limits towards the east. The immense extent of Siberia from the Ural to Ochotzk, and from the Altai to the Icy Sea, has now been so minutely and carefully examined by so many mining engineers, naturalists and gold-seekers, that we may well doubt the occurrence of cretaceous beds in this whole region.

Everywhere along this border, only the upper chalk appears, the strata so peculiarly characterized by *Gryphæa vesicularis*, *Belemnites mucronatus* and *mammillaris*, by *Inoceramus Cuvieri* and *Cripsii*, by

Ostrea Diluvii, *Terebratula carnea* and *semiglobosa*, by *Ananchytes ovata*, *Galerites vulgaris* and *albogalera*, and similar fossils. Older cretaceous strata first appear only in proportion as we descend towards the south, and in the Caucasus, in Daghestan, these older (*neocomien*) beds, according to the excellent observations of Abich, attain a thickness of nearly 5000 feet. It resembles a mighty wave, sweeping far down from the highest summits of the Caucasus and gradually dying away on the margin of the older formations in the plain [on the north].

Beyond the ocean the cretaceous formations terminate in the Atlantic regions of the United States before they have reached the city of New York, so that their limit scarcely touches the 40th degree of latitude, or sixteen degrees lower than in Europe. In Kentucky and Tennessee it remains below 37°. But it is very different far up on the Missouri; this great river flows uninterruptedly from the foot of the Rocky Mountains for 1400 English miles, through strata of chalk, at least as far as the mouth of the Sioux river. This is the result of the accounts and collections of the Prince of Neuwied and of the reports of the celebrated astronomer Nicollet. In these western parts of America therefore, the chalk formation rises to 50° of latitude, or full ten degrees higher than in the eastern portion. Here also it shows a continuous extension greater than that of any other formation known on the surface of the globe. Captain Fremont saw chalk strata, fields covered with *Inoceramus Cripsii*, on the river Platte, Lieutenant Abert on the Arkansas, and as far as Santa Fe in New Mexico, and Dr. Wislizenus found them also beyond the Rio del Norte near Monterey and Laredo, according to the reports published in 1848 by the Congress at Washington. The Rocky Mountains and their continuation to the east [west ?] of Santa Fe in New Mexico, have entirely cut off this cretaceous sea. No trace of chalk was discovered either by Captain Fremont on the Columbia river, or on the Humboldt river in the wonderful "Great Basin" down to the Pacific, or yet by the observant Captains Cooke and Johnston in Sonora and California along the Rio Gila.

Nevertheless the whole of this so vastly extended chalk formation consists only of the upper beds. After very careful and accurate investigation, Sir C. Lyell decided, that in the whole of North America chalk strata from the Maestricht beds down to the gault alone occurred; and Mr. Ferdinand Römer, as the result of his highly valuable and accurate researches in Texas, goes the length of considering all the strata in that region, already so far removed from the Atlantic coast, as entirely of the upper division, and not even once touching on the gault.

This peculiarity is, however, singularly enough limited to North America alone. Even in Mexico deeper beds already appear to occur.

M. Galeotti has brought Trigoniæ from Tehuacan, on the borders of the province of Oaxaca, which he has described as *Trigonia plicato-costata*.* This *Trigonia* belongs to the division of the *Trigoniæ Scabræ* of Agassiz, and differs but slightly from *Trigonia aliformis*, Sow.

* Bulletin de Bruxelles, iii, No. 10.

The latter is however characteristic of the middle chalk, *craie chloritée*, as also of the gault. In the middle of the chief Cordillera of Anahuac, twelve French miles northwest from Tehuacan, this shell is so universally abundant and large that it may be regarded as the distinctive mark of the entire formation. One is astonished, he says, to find such immense accumulations of fossil shells, so many fragments of ammonites several feet in diameter, or of gigantic coral stems in this place, so much so that there is perhaps no other place on the surface of the globe where such an enormous mass of organic remains lies scattered over many square miles. Now this *Trigonia* appears again in South America in the mountains of Santa Fe de Bogota, from which M. von Humboldt first brought it to us. The organic remains enclosed in the strata of these mountains of Santa Fe de Bogota prove most decidedly the occurrence of the middle chalk, as I have endeavored to show in the description of Humboldt's collection of American petrifactions (Berlin, 1839), and as Alcide d'Orbigny has still more fully proved in his no less instructive than masterly work on M. Boussingault's collections. But as the cretaceous formations in New Grenada attain a thickness of more than 5000 feet, it is not surprising that the organic remains of the lower cretaceous strata, or the *neocomien*, should also be found in this place. D'Orbigny has described an *Exogyra* from Socorra, which is not distinct from the *Exogyra Couloni* of the *neocomien*. Many specimens of this same *Exogyra* were collected by the late Dr. Meyen on the declivities of the volcano of Maypo in Chili at the height of 13,000 feet, and (badly) figured.* Darwin also† found it on the Portillo Pass in the Peukenes chain, not far from Maypo, but also sixty English miles further north on the Uspalata Pass. The *Exogyra Couloni* or *aquila* is, however, a true and very decided characteristic shell for the *neocomien*.

The fossils collected by Darwin in the mountains above Copiapo, and Coquimbo in northern Chili, and those which Domeyko, Professor of Mineralogy in Coquimbo, has sent to Paris, again belong to newer cretaceous strata, and are found in part also at a distance from it beyond the great knot of transition beds which, along with older rocks, have intruded into the chain of the Andes.‡ The most remarkable of these forms is the beautiful univalve, which Humboldt first brought from San Felipe in the south of Quito, near the Amazon river, and which was figured and described by me as PLEUROTOMARIA HUMBOLDTII in the 'Petrifactions,' v. f. 26, first published in 1839. D'Orbigny, followed by Darwin, names it "*Turritella Andii*," but whether correctly is still very doubtful. It appears peculiarly characteristic of the whole southern districts of America. Darwin has found it in abundance in the strata of Coquimbo, on the Rio Claro and near Arqueros, and in like manner above Guasco and near Las Amolanos in the principal valley of Copiapo. This *Pleurotomaria* is always conjoined with

* Acta der Leopold. Academie, vol. xvii, pt. ii, p. 649, t. 27, f. 5.

† Geol. Observations on South America, 1846.

‡ "Beyond and north of the great shut in mountain-basin of Titicaca, composed of older rocks, the trias and carboniferous limestone." (Bet. über die Ver. der Kreide-Bild., p. 27.)

the *Pecten*, occurring even in the northern regions between Montan and Guancavelica in such incredible numbers, that it forms fields, nay, mountains of petrifactions, long and very generally known to the natives under the name of "*Choropampas*." (*Pecten alatus*, Dufresnoyi, d'Orb.) It was this shell also that, in 1761, excited so great astonishment in Ulloa at the great elevation above the level of the sea, at which mountains composed of shells were seen, and this astonishment was repeated in all text-books, till it was discovered that the shells had not necessarily lived at this elevation, but might have been raised up from the depths of the sea. Since *Hippurites organisans* (D'Orb., p. 107, t. 22) occurs with the pecten-strata, it is evident that all these beds in Peru, as at Coquimbo and Copiapo, must be conjoined at least with the gault; a result which is most strikingly confirmed by an *Exogyra* which M. Domeyko has sent to Paris. This is indeed perfectly identical with the *Gryphæa* (*Exogyra*) *Pitscheri* from Texas, already described and figured by Morton, and the position of which above the gault at Friederichsberg has been very accurately ascertained by Ferdinand Römer.

Lower cretaceous strata, similar to those of Aconcagua, are nevertheless not altogether unknown in the Andes mountains near Lima. The celebrated zoologist von Tschudi has found, on the eastern declivity of the mountains, between Oroja and Yauti, near Tarma, along with many others, some perfectly characteristic neocomien shells:—*Pterocera Emerici*, (D'Orb., p. 216,) *conoïdea*, Goldfs.; *Holaster dilatatus* and *Holaster complanatus* or *Spatangus retusus*, both identified by Agassiz; *Diadema Bourgeti*, also determined as such in Neufchatel; *Pecten cretosus*, Brgnt. and *Pecten quinquecostatus*.

According to this, the cretaceous formation in South America appears to be developed in an entirely different manner, in much greater thickness and variety than to the north of the Gulf of Mexico, and the agreement with the European cretaceous strata is also much more complete in the Andes. It is, however, highly remarkable, that in North America the cretaceous strata are spread out quite horizontally over immense spaces, and that they consist chiefly of clay and sand, and other slightly coherent masses. In South America we only see black limestones or compact sandstones, of such consistence, that one often believes them to be pure quartz, as between the Maranon and Lima; along with this, the strata are never horizontal, but always more or less inclined; a disturbed position which they evidently can only owe to powerful disturbing forces. There can be less doubt in regard to this, when it is seen, as Meyen informs us, that the precipitous cone of the Volcano of Maypo consists for two-thirds of its height of chalk rich in petrifactions, and that throughout the whole of Chili masses of gypsum, many thousand feet thick, surround the volcanos, and the cretaceous strata first appear quite above them. But when we leave this desolation, the chalk also has vanished. It never reaches the plain of the Pampas on the east; a chain of Devonian strata at the eastern foot of the Andes does not permit it even once to touch on the outskirts of the vast plains of the Pampas. As little can it extend westwards towards the Pacific Ocean. A considerable elevation on the mountains must be attained before it is met with. What then can induce the chalk to run along, only in the direction of the high mountain ridge of the volcanos, and only in a narrow band, and never to descend into the plains!! In the whole of Brazil, in the wide regions of La Plata, Paraguay, Bolivia, the chalk is never again seen, and indeed does not exist. Is it not like a band of chalk which has been formed along the volcanic fissure of the Andes before the mountains were elevated, perhaps because the slightly concealed fissure had produced the conditions of life and existence for the cretaceous mollusca on a more extensive and easily attainable scale?

Darwin has followed the cretaceous strata to the extreme point of the continent. He saw cretaceous shells in abundance on the top of Mount Tarn 2000 feet high, near Port Famine in the Straits of Magellan, and in 53 degrees of south latitude, and consequently three degrees of latitude higher than on the Missouri. *Ancycloceras simplex*, d'Orb., and *Hamites elatior*, Sow., leave no doubt of the chalk. The Hamite is even, says Prof. Edward Forbes, one of the largest ever seen, fully 2½ inches in its largest diameter. Darwin's discovery probably determines the most southern limit of the cretaceous formations; and hence polar influences may have here also opposed its further extension towards the Pole. J. N.

III. Zoology.

1. *Contributions to the Natural History of the Acalephæ of North America;* by L. Agassiz: Part I, on the Naked-eyed Medusæ of the shores of Massachusetts in their perfect state of development, (Transactions of the American Academy of Arts and Sciences, 2nd Series, Vol. III, Part I, Article IX, pp. 221 to 316, 4to, with 8 plates.)—Professor Agassiz, in this paper, gives his readers Part I. of his researches on the Medusæ of the American Coast. His investigations have brought to light much that is new and of deep interest to science: and they are illustrated by beautiful as well as faithful engravings. The species described are *Sarsia marabilis*, *Hippocrene superciliaris*, *Tiaropsis diademata*, and *Staurophora laciniata*.

Prof. Agassiz, in the course of his memoir, makes some observations on the classification of these animals, and gives reasons for including all the naked-eyed Medusæ in one family. He points out the little importance to be attached (except for generic and specific distinctions) to the number of tentacles, and the position of the ovaries in these Medusæ. The grand characters of the group or family are as follows:——consisting of a gelatinous disk with the margin re-entering so as to form a cavity beneath;—a central digestive cavity, from which tubes, carrying chyme and forming a chymiferous system, radiate towards the margin and connect with or pass into a circular tube situated near the margin;—tentacles and eye-specks along this margin; mouth central but varying in size and form;—reproductive organs following the chymiferous system;—a nervous ring adjoining the submarginal chymiferous tube;—generation alternate, one form Polypoid, and the other Medusoid.

Among the species described, we select for particular notice the *Hippocrene superciliaris*, the observations on which embrace the principal points determined by the author. This beautiful Medusæ is a globular bell-shape animal with 4 bunches of tentacles on the lower margin. In the inner cavity of the bell, at centre, there is a dark four-sided mass, containing the mouth and digestive cavity, about as broad as long. In some species, as the genus *Sarsia*, this mass is elongated into a movable proboscis, in others it is scarcely projecting. From its angles, in the Hippocrene, proceed internally four tubes which carry from the digestive cavity the chyme or fluids after digestion, diluted with more or less of the external waters; these tubes pass to the border and here connect with a circular tube which follows the margin around, and in which the chyme continues its course. This, as Prof. Agassiz explains with many details, is the circulating and digestive system combined, of this and other species of the family. The tubes may be closed at their connection with the stomach, "showing that the food is not admitted before it has undergone a certain degree of elaboration; but no sooner has it been reduced to the requisite degree of fluidity, in which the particles of the nourishing materials appear like little globules, than they open, the nutritive fluid passes into the radiating tubes, circulates regularly through these tubes along the inner walls of the disk, and through them passes into the circular tube around the lower margin." Prof. Agassiz observes that while the circulation is properly a *chyme* circulation in these animals, it is a *chyle* circulation in Articulata and Mollusca, and in Vertebrata alone, *true blood*. The particles in the circulating fluid of these Medusæ are very irregular in size and color and are evidently only the imperfectly digested food.

The *nervous cord*, as distinctly made out by Prof. Agassiz, follows the circular submarginal tube on its inner side, and like that, is a complete ring. At the base of the cluster of marginal tentacles there is an enlargement or bulb-like prominence. Each tentacle contains within its base a black point which subserves the purpose of vision. The bulb is not hollow in the Hippocrene, but contains an enlargement of the nervous cord, which, as our author shows, may be considered a ganglion, although not purely nervous matter in its constitution, and this position of the ganglion has a direct relation to the organs of sense clustered near it. The nervous cord, under a high magnifier, appeared as a string of several rows of nucleated ovate cells, ranging in irregular lines, the cells not strictly in juxtaposition by their ends, but alternate more or less, so as to form a cord-like mass; and it was distinctly observed passing into the angles of the sensitive bulbs. The dark spot of the bulb consists of pigment cells, which point to the centre of minute black eye-specks, showing a close connection between these dark dots and the centre of the bulb where the nervous ganglion is seated; "and though this is not an arrangement known in the organs of vision of any other animals, we are at least reminded by these peculiarities of the structure of the compound eye of insects, in which the pigment pillars intervening between the nervous mass at the base of the eyes present a structure not very different from that of the radiating cones in the bulb of the Hippocrene."

There is a similar ganglion in each of the four bulbs. A branch of the nervous thread, or what appeared to be a continuation of it, was detected along each chymiferous tube leading upward towards the digestive cavity; and above, near where these tubes bend towards this cavity, the cord passes around so as to form a circle, about the upper part of the chymiferous system or near the centre of the disk. There is thus a marginal, and (if there be no mistake as to its nervous character,) a central circle to the nervous system; from the latter at a point half way between two chymiferous tubes a branch passes off and descends to the buccal or digestive mass below, and other branches go to the inner muscles. Professor Agassiz observes that there is a difference between the cord of the lower margin and the other threads, and he was not fully satisfied of the real nature of the latter. Instead of consisting of distinct cells, they are thin threads in which the cellular appearance is almost gone, excepting where they combine to form a plexus in which some of the threads have the form of long caudate cells: they differ from muscular fibres as much as they do from the main nervous cord. No contraction was observed in them; this fact, and also their connection with the chymiferous tubes and the sensitive bulbs below,—the nature and position of the plexuses as well as their branching to the digestive organ and to the muscles—favor the view taken. The close juxtaposition of the chymiferous system—the source of nutriment—with the nervous system and the sensitive bulbs and eye-specks, is remarked upon as a fact of much interest.

The muscular system is minutely developed in this memoir and well brought out in the figures. The muscles consist of contractile cells, rather than of fibres. Over the surface of the bell-shaped part of the disk, the epithelium consists of irregular polygonal cells, of very faint outline, hardly distinguishable except by a kind of mosaic arrangement seen by means of their granular contents. On the sides and above the disk as well as on the inner surface of the main cavity, these cells are more regular and distinct. The network of muscles, or rather lines of contractile cells, beneath the epithelium extend in two main directions; the main bundles being vertical, four in number, and alternating with the chymiferous tubes; and the others transverse circular, in four narrow ranges, but with others smaller and less regular. The fibres are chiefly superficial though penetrating somewhat into the gelatinous disk. In the inner cavity, there are four vertical muscular bundles, ranging between the chymiferous tubes, which terminate midway between the sensitive bulbs; and also eight others much smaller, alternating by twos with these four. This same cavity has a transverse layer of contractile cells lining the whole interior, as was distinctly observed on close examination. It properly consists of four parts, extending from one chymiferous tube to another. There is still another muscular system extending between the four sensitive bulbs, within the lower margin, which is made up of a thick layer of muscular fibres, with which alternate some few radiating fibres, crossing the former at right angles and most numerous about the eye-specks.

It is now well known that these Medusæ in one stage of their existence have a polyp form, and these Hydroid polyps are therefore only imperfect Medusæ. The Sarsia in one of its conditions is a *Coryna*, and the species of Coryna pertaining to the *Sarsia mirabilis* (although not figured in this memoir, which treats only of these animals in their perfect state of development) is well known in the Boston Harbor and has been often collected by Prof. Agassiz. The polyp of the Hippocrene is still unknown: but we have here the suggestion, with some good reasons for admitting its correctness, that it is a *Tubularia*, a fine species of which is well known in the same harbor.

Many other points are detailed in this memoir which we have to pass by at this time.

Besides the species mentioned as described in this paper, the author briefly mentions two new species of *Thaumantias* (T. diaphana and T. pilosella); and also a new genus near Hippocrene which he calls *Nemopsis*, in allusion to the fact that two of the eye-specks of each cluster have a slender pedicel instead of being sessile like the others near the bases of the tentacles. The species was taken in Nantucket Harbor, June, 1849, and is named *N. Bachei.*

2. *On the Structure of Nummulina;* by W. B. CARPENTER, M.D., F.R.S., (Quart. J. Geol. Soc., No. 21, Feb. 1, 1850.)—In this elaborate and well illustrated memoir, Mr. Carpenter sustains the opinion that the Nummulites are true Foraminifera. The succession of cells in a spiral order was pointed out by D'Orbigny as favoring this view. Carpenter remarks also that the cells or chambers are not even and regular as in the chambered shells of the cephalopodous mollusks, but irregular and of varying size, and hence infers that the cells must have corresponded to separate individuals, like the polyps of a compound coral zoophyte, and that they were the result of successive gemmation. The septa are found under a high lens to be double, each cell having its own proper wall. A distinct perforation passes through the septa, as observed by D'Orbigny, and still more minute apertures, varying in number and position, exist on the surface of each septum penetrating, however, only a single wall. The successive whorls of the Nummulite are well known to envelop and invest completely each the preceding, so that in a vertical section each chamber of the medial plane is seen to be covered with as many layers of shell above and below its own roof and floor, as there are chambers intervening between it and the nearest margin of the sections. A series of perforations were detected by Carpenter, extending from the exterior directly downwards until they reach the roof and floor of the central plane which they do not penetrate. They terminate over the septa and pass actually into the *interseptal spaces*, which have numerous communications with the chambers themselves. Thus there is a direct communication of a tubular character between the interseptal spaces of the central plane and the external surface, so that all these spaces communicate with the medium, which the animal may inhabit. "In the *Nummulina complanata* and other species in which each investing whorl is in contact with the one it encloses, except at its edge, the perforations have the form of fissures, that correspond with the subjacent septa, towards which they directly pass." These fissures are usually found to be filled with opake matter; and the dark bands thus formed in a transparent section are seen to be crossed by delicate white lines, which seem to indicate a division of the fissure into a number of tubes of irregular form, probably for the passage of pseudopodia. The animal segments of the several cells though distinct have a connection by fibres or tubes, and it is probable that the inner whorls retained their vitality, although so deeply inclosed,

and absorb their nourishment probably by means of filamentous pseudopodia projecting through the system of passages leading from the medial plane to the inner surface.

The memoir contains also the results of a microscopic investigation of the Orbitolites, showing their relation to the Nummulites, and also observations upon the Orbitoides, indicating that they also are Foraminifera.

IV. Miscellaneous Intelligence.

1. *On the Negro Races of Oriental Africa;* by M. Serres, (L'Institut, No. 857, June 5, 1850, from the Proceedings of the Academy of Sciences of Paris, June 3, 1850.)—M. Serres read a report on the negro races of Oriental Africa, south of the equator, observed by M. de Froberville. The conclusions of the report are as follows:—That the gradation or degradation of the physical characters of the Ostro-Negros justifies in all points the divisions which the author has laid down;—that among these characters, the prognatism of the maxillaries, the thickness and prominence of the lips, the woolly or frizzled tendency in the hair, the shades of color in the skin, are particularly and properly brought out;—that with regard to the prognatism of the maxillaries and the peculiarity of the lips "en forme de boudin," there is a regular gradation from the Congo-Guinea negro to the Eastern Oceanic negro, and from these to the Caffro-Bechuans, and these last graduate into the Semitic mongrels in those physical conditions where they are observed in some branches of the Caucasian race;—that this gradation of characters, of so much interest in the study of the races of men, is well sustained by a comparison of the busts executed by M. de Froberville, and better still by the portraits obtained by means of the daguerreotype by Dr. Jacquart;—that the existence of a type uniformly disseminated among the negro tribes distributed south of the equator to the gulf of Mozambique having close relations to the Semitic type is a fact of great interest in anthropology;—that this interest is independent of the Phenician origin which M. de Froberville attributes to it, and which at present is only an hypothesis;—that the repetition of an oceano-negro type among one of the groups of Oriental Africa is also a most curious result bearing upon the affiliation of races, in spite of the disagreement which exists in this subject between ethnology and anthropology;—that, finally, from the analysis and comparison of the physical characters of the Ostro-negros, the conclusion of the author is obvious and decisive, that "the more thoroughly and comprehensively the Congo-guinean, Caffro-bechuan and Ostro-negro races are studied, the more distinctly does the unity of the human race come out as an established principle in science.

2. *Fabrication of Zinc compounds not injurious to health.*—At a meeting of the Academy of Sciences at Paris in June last, M. Sorel, replying to some authors who at preceding sessions of the Academy had made observations tending to show that zinc was not innocuous, stated that for fifteen years he had employed in his establishments for the galvanization of iron several hundred workmen, a large number of whom were occupied with pulverizing and sifting the gray or suboxyd of zinc, for galvanic painting, and in no instance had any of the work-

men of the establishment, although in the midst of an atmosphere containing much of the oxyd, suffered at all from it. The white oxyd of zinc had also been fabricated for some months, without any ill effects, although the men breathe considerable quantities of the oxyd.

3. *Anchor-Ice;* by Prof. C. Dewey.—Water sometimes freezes upon stones below the unfrozen surface of the water. Fastened in this way, it passes under the common name of *anchor-ice.* As soon as the ice is detached, it rises to the surface and floats upon it. In explaining this phenomenon, it has been said, that in thus freezing, the water parts with its latent caloric to the stone and crystallizes upon it, and that its crystals are then *heavier* than water. The first part of this solution is only a statement of the fact in chemical language, and the last part is opposed by the fact that the detached ice uniformly and directly rises to the surface.

The observed facts are the following. Anchor-ice forms in still or running water, and often where the current is rapid; on stones of quartz, granite, mica-slate, or limestone, and on wood fresh or old under water; in masses from less than an inch to two or three inches thick, over stones several inches in diameter as well as on smaller ones; of a spongy or fibrous appearance under the water, but composed of flat irregular crystalline plates or tables, with regular angles and terminations, cohering or strung along upon each other, exceedingly white and beautiful; when the surface of the water is free from ice, and the *temperature of the whole is at the freezing point* as shown by the thermometer, and the atmosphere is below the freezing point several degrees; when none is on the stones at sunset, they are covered in the morning after a clear and cool night. This ice begins to leave the stones in the morning, and before noon has all risen and disappeared, often covering the whole surface of the water for some time. Repeated instances of these various particulars were observed by me in the latter part of March last, in Queechy River, at Woodstock, Vt., a stream there three to six rods in width and of variable depth. The stones lay loose on the bottom, often on sand, and had no connection with rocks which sometimes extended from the bottom to air or to the frozen earth in the banks out of the water. This ice was formed all along the bottom, probably for miles where the same circumstances occurred. I saw it also on wood under water, and in one instance on a piece fresh sawed from a log of hemlock. The depth to the ice (below the surface of the water) was from a few inches to three feet, and the water sometimes nearly still and at other places in rapid motion.

The principle of this phenomenon is probably the same as the following.

Saturated solutions of some salts, as sulphate of soda, at a high temperature, may be gradually cooled at rest without depositing the salt. The agitation of the cooled solution, on putting a rod of glass, wood or metal into it, commonly, but not *always*, causes the deposition of the substance. A crystal dropped into the solution directly begins the crystallization of the salt. It is well known that water may be cooled in a still vessel below 32° without freezing, but a little agitation produces congelation of a part of the water in long fibrous or laminated crystals.

The stream of water is cooled to the freezing point, and by the cold of the night its temperature is still more reduced. In this state the caloric is more than saturated, and the stones and wood under the water perform the part of the crystals or rod in the solution of salt, and the ice forms upon them.

The quantity of anchor-ice formed would be small for an obvious reason. Every pound of water frozen would evolve 142° of caloric, which would raise 142 pounds of water one degree, or 71 pounds two degrees. Allowing the temperature of the stream to be reduced to 31° or 30°, the congelation of a relatively small quantity would thus prevent the further congelation, while the crystals of ice would be formed under water where the solid bodies, as wood or stone, are.

Considering how poor a conductor of caloric wood is, it can hardly be supposed, that anchor-ice is formed on solid bodies merely because they are better conductors of caloric than water is, and the more so, because they must have the same temperature as the water.

Rochester, July, 1850.

P. S. Rocks sometimes pass from a frozen bank, by connection with which their temperature may be as low as 20°, into and under water, which congeals on them because they are so cold, and thus forms *solid* ice. This is not a case of anchor-ice, nor has it the form of that ice. When a rock, projecting from water, congeals the water into solid ice around it, this is not *anchor-ice*. To the first of these cases is doubtless to be referred that of James River, noticed in vol. xxxvi, p. 186 of this Journal. The explanation of it in vol. xli, p. 407, will hardly be considered applicable to real anchor-ice. It is probable that some of the ice in James River may be of the kind above described, though I have never seen it in *solid* masses, but of a spongy appearance. This form the anchor-ice retains after it has risen to the surface of the water.

4. *Discovery of the Great Lake "Ngami," of South Africa;* (Let- from the Rev. David Livingston, addressed to the Rev. Authur Tidman, Foreign Secretary, London Missionary Society; dated, Banks of the River Zonga, 3rd September, 1849; cited from Jameson's Journal, July, 1850, vol. xlix, p. 156.)—*Dear Sir*,—I left my station, Kolobeng (situated 25° South lat., 26 East long.), on the 1st of June last, in order to carry into effect the intention, of which I had previously informed you, viz., to open a new field in the north, by penetrating the great obstacle to our progress, called the Desert, which, stretching away on our west, northwest, and north, has hitherto presented an insurmountable barrier to Europeans.

A large party of Griquas, in about thirty waggons, made many and persevering efforts at two different points last year; but, though inured to the climate, and stimulated by the prospect of much gain from the ivory they expected to procure, want of water compelled them to retreat.

Two gentlemen, to whom I had communicated my intention of proceeding to the oft-reported lake beyond the desert, came from England for the express purpose of being present at the discovery, and to their liberal and zealous co-operation we are especially indebted for the success with which that and other objects have been accomplished. While waiting for their arrival, seven men came to me from the Batavana, a tribe living on the banks of the lake, with an earnest request from their chief for a visit. But the path by which they had come to Kolobeng was impracticable for waggons; so, declining their guidance I selected the more circuitous route, by which the Bermangueato usually pass, and, having Bakwains for guides, their self-interest in our success was secured by my promising to carry any ivory they might procure for their chiefs in my waggon; and right faithfully they performed their task.

When Sekhomi, the Bermangueato chief, became aware of our intentions to pass into the regions beyond him, with true native inhumanity he sent men before us to drive away all bushmen and Bakalihari from our route, in order that, being deprived of their assistance in the search for water, we might, like the Griquas above mentioned, be compelled to return This measure deprived me of the opportunity of holding the intercourse with these poor outcasts I might otherwise have enjoyed. But through the good providence of God, after travelling about 300 miles from Kolobeng, we struck on a magnificent river on the 4th of July, and without farther difficulty, in so far as water was concerned, by winding along its banks nearly 300 miles more, we reached the Batavana, on the lake Ngami, by the beginning of August.

Previous to leaving this beautiful river on my return home, and commencing our route across the desert, I feel anxious to furnish you with the impressions produced on my mind by it and its inhabitants, the Bakoba or Bayeiye. They are a totally distinct race from the Bechuanas. They call themselves Bayeiye (or men), while the term Bakoba (the name has somewhat of the meaning of "slaves,") is applied to them by the Bechuanas. Their complexion is darker than that of the Bechuanas; and of 300 words I collected of their language, only 21 bear any resemblance to Sitchuana. They paddle along the rivers and lake in canoes hollowed out of the trunks of single trees; take fish in nets made of a weed which abounds on the banks; and kill hippopotami with harpoons attached to ropes. We greatly admired the frank, manly bearing of these inland sailors. Many of them spoke Sitchuana fluently, and, while the waggon went along the bank, I greatly enjoyed following the windings of the river in one of their primitive craft, and visiting their little villages among the reed. The banks are beautiful beyond anything we had ever seen, except perhaps some parts of the Clyde. They are covered, in general, with gigantic trees, some of them bearing fruit, and quite new. Two of the Boabab variety measured 70 to 76 feet in circumference. The higher we ascended the river, the broader it became, until we often saw more than 100 yards of clear deep water between the broad belt of reed which grows in the shallower parts. The water was clear as crystal, and as we approached the point of junction with other large rivers *reported to exist* in the north, it was quite soft and cold. The fact that the Zonga is connected with large rivers coming from the north awakens emotions in my mind, which make the discovery of the lake dwindle out of sight. It opens the prospect of a highway, capable of being quickly traversed by boats, to a large section of well-peopled territory.

One remarkable feature in this river is its periodical rise and fall. It has risen nearly three feet in height since our arrival, and this is the dry season. That the rise is not caused by rains is evident from the water being so pure. Its purity and softness increased as we ascended towards its junction with the Tamunakle, from which, although connected with the lake, it derives the present increased supply. The sharpness of the air caused an amazing keenness of appetite, at an elevation of little more than 2000 feet above the level of the sea (water boiled at 207½° thermometer), and the reports of the Bayeiye, that the waters came from a mountainous region, suggested the conclusion that the increase of the water, at the beginning and middle of the dry season, must be derived from melting snow.

All the rivers reported, to the north of this, have Bayeiye upon them, and there are other tribes on their banks. To one of these, after visiting the Batavana, and taking a peep at the broad part of the lake, we directed our course; but the Batavana chief managed to obstruct us, by keeping all the Bayeiye near the ford on the opposite bank of the Zonga. African chiefs invariably dislike to see strangers passing *them to tribes beyond.* Sebitoane,—the chief who in former years saved the life of Sechele *our* chief,—lives about ten days northeast of the Batavana. The latter sent a present as a token of gratitude. This would have been a good introduction; the knowledge of the language, however, is the *best* we can have. I endeavored to construct a raft, at a part which was only fifty or sixty yards wide, but the wood, though sun-dried, was so heavy it sunk immediately; another kind would not bear my weight, although a considerable portion of my person was under water. I could easily have swam across, and fain would have done it; but, landing without clothes, and then demanding of the Bakoba the loan of a boat, would scarcely be the thing for a messenger of peace, even though no alligator met me in the passage. These and other thoughts were revolving in my mind as I stood in the water,—for most sorely do I dislike to be beaten,—when my kind and generous friend Mr. Oswell, with whom *alone* the visit to Sebitoane was to be made, offered to bring up a boat at his own expense from the Cape, which, after visiting the chief, and coming round the north end of the lake, will become missionary property. To him and our other companion, Mr. Murray, I feel greatly indebted,—*for the chief expense has been borne by them. They* could not have reached this point without my assistance; but, for the aid they have rendered in opening up this field, I feel greatly indebted; and, should any public notice be taken of this journey, I shall feel obliged to the directors if they express my thankfulness.

The Bayeiye or Bakoba listened to the statements made from the Divine Word with great attention, and if I am not mistaken, seemed to understand the message of mercy delivered better than any people to whom I have preached for the *first* time. They have invariably a great many charms in the villages; stated the name of God in their language (without the least hesitation) to be "Oreeja;" mentioned the name of the first man and woman, and some traditionary statements respecting the flood. I shall not, however, take these for certain, till I have more knowledge of their language. They are found dwelling among the reed all around the lake, and on the banks of all the rivers to the north.

With the periodical flow of the rivers great shoals of fish descend. The people could give no reason for the rise of the water, further than that a chief, who lives in a part of the country in the north, called Mazzekiva, kills a man annually and throws his body into the stream, after which the water begins to flow.

The sketch which I enclose is intended to convey an idea of the river Zonga and the lake Ngami. The name of the latter is pronounced as if written with the Spanish ñ, the *g* being inserted to show that the ringing sound is required. The meaning is "Great Water." The latitude, taken by a Sextant on which I can fully depend, was 20° 20′ south, at the northeast extremity, where it is joined by the Zonga; longitude about 24° east. *We do not, however, know it with certainty.* We left our waggon near the Batavana town, and rode on horseback about six miles beyond it to the broad part. It gradually widens out into a Firth about 15 miles across, as you go south from the town, and in the south-southwest presents a large horizon of water. *It is reported* to be about 70 miles in length, bends round to the northwest, and there receives another river similar to the Zonga. The Zonga runs to the northeast. The thorns were so thickly planted near the upper part of this river, that we left all our waggons standing about 180 miles from the lake, except that of Mr. Oswell, in which we traveled the remaining distance; but for this precaution our oxen would have been unable to return. I am now standing at a tribe of Bakurutse, and shall in a day or two re-enter the desert.

The breadth marked is intended to show the difference between the size of the Zonga, after its junction with the Tamunakle and before it. The farther it runs east, the narrower it becomes. The course is shown by the arrow-heads. *The rivers not seen, but reported by the natives,* are put down in dotted lines. The dotted lines running north of the river and lake, show the probable course of the Tamunakle, and another river which falls into the lake at its northwest extremity. The arrow-heads show also the direction of *its* flow. At the part marked by the name of the Chief Mosing it is not more than 50 or 60 yards in breadth, while at 20° 7′ it is more than 100, and very deep.

The principal disease reported to prevail at certain seasons appears, from the account of the symptoms the natives give, to be pneumonia and not fever. When the wind rises to an ordinary breeze, such immense clouds of dust arise from the numerous dried-out lakes called salt-pans, that the whole atmosphere becomes quite yellow, and one cannot distinguish objects more than two miles off. It causes irritation in the eyes, and, as wind prevails almost constantly at certain seasons, this impalpable powder may act as it does among the grinders in Sheffield. We observed cough among them, a complaint almost unknown at Kolobeng. Musquitoes swarm in summer, and the Banyan and Palmyra give in some parts an Indian cast to the scenery.

(Signed) David Livingston.

5. *Comparison of Fahrenheit and Centigrade Thermometers;* (L'Institut, No. 854, Acad. Sci., Paris.)—M. A. d'Abbadie observes that the usual formula for comparing Fahrenheit and Centigrade thermometers, ($C=\frac{(F-32°)\times 100}{180}$, when C and F represent respectively the degrees of the two thermometers,) supposes that 180° Fahrenheit just equals

100° of Centigrade; but in France the height of the barometer for graduation is 760 millimeters, while in England it is 30 inches, equivalent to 761·9862 millimeters. The Centigrade scale corresponding to 212° Fahrenheit is therefore 100·0727 degrees.

To this correction there is still another, (for instruments made at London and Paris,) amounting to $\frac{1}{10}$th of the preceding, which depends on the difference in the intensity of gravity at London and at Paris. Representing by G the force of gravity at Paris, and by g that at London, and deducing G and g from the observed length of the pendulum, we have, Log. $\frac{G}{g} = -1·998797$. We hence obtain 759·785 millimeters for the height of the barometer at zero in London. The difference is equal to 0·215 millimeters or 0·0079 degrees.

These two corrections being applied, the Parisian scale should stand at 100·08066 degrees, when the London scale marks 212 degrees. The formula then becomes $C = \frac{(F - 32°) \times 100·08066}{180}$.

The correction is small; but in exact observations, the thermometer is read to 0·08 degrees; and it is desirable that even a slight error should not be added to errors of observation.

6. *Discovery of an Infusorial Stratum in Florida;* by Prof. J. W. Bailey.—While on a visit to Tampa (Fort Brooke), Florida, I noticed on the shores of Hillsborough Bay, between the mouth of Hillsborough River and Ballast Point, a white crumbling rock, which by its lightness, friability and other characters, somewhat resembled the Infusorial Marls of Virginia. An examination made upon the spot with a common Coddington lens enabled me to see small circular discs upon the freshly fractured surfaces, which subsequent examinations proved to belong to the genus Coscinodiscus. Numerous other marine species of Navicula, Gallionella, &c., together with numerous spicules of Sponges were recognized, although the highly indurated or lapidified condition of the strata rendered the determination of the species more than usually difficult. Of the precise geological position of this deposit I cannot be certain, but it is associated with strata containing fossils which appear to belong to the epoch of the eocene tertiary. This discovery of an extensive infusorial bed in a region so remote from those previously noticed in Virginia and Maryland, and apparently in an older geological formation will, it is hoped, lead to the examination of other localities, as it is not improbable that the deposit near Tampa may be a portion of a deposit as remarkable as those discovered by Prof. W. B. Rogers in Virginia and Maryland.

7. *On the Application of Magnetism as a Motive Power;* by R. Hunt, (Proc. Society of Arts, Athenæum, No. 1179, p. 588.)—In this paper the author called attention, in the first place, to the numerous attempts which have been made to apply electro-magnetism as a power for moving machines, and particularly described the apparatus employed by Jacobi, Dal Negro, M'Gauley, Wheatstone, and others, noticing incidentally the machines recently constructed by Mr. Hjorth. Since, notwithstanding the talent which has been devoted to this interesting subject, and the large amount of money which has been spent in the

construction of machines, the public are not in possession of any electro-magnetic machine which is capable of exerting power economically; and finding that, notwithstanding the aid given to Jacobi by the Russian Government, that able experimentalist has abandoned his experimental trials,—the author has been induced to devote much attention to the examination of the first principles by which the power is regulated, with the hope of being enabled to set the entire question on a satisfactory basis. The phenomenon of electro-magnetic induction was explained, and illustrations given of the magnetization of soft iron by means of a voltaic current made to circle around it. The power of electro-magnets was given, and the author stated his belief that this power could be increased without limitation. A voltaic current produced by the chemical disturbance of the elements of any battery, no matter what its form may be, is capable of producing by induction a magnetic force, *this magnetic force being always in an exact ratio to the amount of matter (zinc, iron, or otherwise) consumed in the battery.* Several forms of the voltaic battery were explained, particularly those of Daniell, Grove, Bunsen, and Reinsch, the latter being constructed without metals, depending entirely on the action between two dissimilar fluids, slowly combining. The author had, however, proved, by an extensive series of experiments, that the greatest amount of magnetic power is produced when the chemical action is most rapid. Hence, in all magnetic machines, it is more economical to employ a battery in intense action, than one in which the chemical action is slow. It has been proved by Mr. Joule, and most satisfactorily confirmed by the author, that one-horse power is obtainable in an electro-magnetic engine, the most favorably constructed to prevent loss of power, at the cost of 45 lb. of zinc, in a Grove's battery, in twenty-four hours; while 75 lb. are consumed in the same time to produce the same power in a battery of Daniell's construction. The cause of this was referred to the necessity of producing a high degree of excitement, to overcome the resistance which the molecular forces offer to the electrical perturbations, on which the magnetic force depends. It was contended, that although we have not perhaps arrived at the best form of voltaic battery, yet that we had learnt sufficient of the law of electro-magnetic forces to declare that, under any conditions, the amount of magnetic power would depend on the change of state—consumption of an element—in the battery, and that the question resolved itself into this:—

What amount of magnetic power can be obtained from an equivalent of any material consumed? The following were regarded as the most satisfactory results yet obtained:—1. The force of voltaic current being equal to 678, the number of grains of zinc destroyed per hour was 151, which raised 9,000 lb. one foot high in that time. 2. The force of current being, relatively, 1,300, the zinc destroyed in an hour was 291 grains, which raised 10,030 lb. through the space of one foot. 3. The force being 1,000, the zinc consumed was 223 grains; the weight lifted one foot 12,672 lb. The estimations made by Messrs. Scoresby and Joule, and the results obtained by Oersted, and more recently by Mr. Hunt, very nearly agree; and it was stated that one grain of coal consumed in the furnace of a Cornish engine lifted 143 lbs. one foot high, whereas one grain of zinc consumed in the battery lifted only 80 lbs. The cost of 1 cwt. of coal is under 9*d.*; the cost of 1 cwt. of zinc is 216*d.* Therefore, under the most perfect conditions, magnetic power must be nearly 25 times more expensive than steam power. But the author proceeded to show that it was almost proved to be an impossibility ever to reach even this, owing, in the first place, to the rate with which the force diminishes through space. As the mean of a great many experiments on a large variety of magnets, of different forms and modes of construction, the following result was given:—

Magnet and armature in contact, lifting force . .	220 lb.
" distant $\frac{1}{250}$ of an inch . .	90·6
" " $\frac{1}{125}$ " . . .	50·7
" " $\frac{1}{63}$ " . . .	50·1
" " $\frac{1}{50}$ " . . .	40·5

Thus at one-fiftieth of an inch distance four-fifths of the power are lost. This great reduction of power takes place when the magnets are stationary. The author then proceeded to show that the moment they were set in motion a great reduction of the original power immediately took place; that, indeed, any disturbance produced near the poles of a magnet diminished, during the continuance of the motion, its attractive force. The attractive force of a magnet being 150 lb. when free of disturbance, fell to one-half, by occasioning an armature to revolve near its poles. Therefore, when a system of magnets which had been constructed to produce a given power is set in revolution, every magnet at once suffers an immense loss of power, and consequently their combined action falls in practice very far short of their estimated power. This fact has not been before distinctly stated, although the author is informed that Jacobi observed it. And not merely does each magnet thus sustain an actual loss of power, but the power thus lost is converted into a new form of force, or rather becomes a current of electricity, acting in opposition to the primary current by which the magnetism is induced. From an examination of all these results, Mr. Hunt is disposed to regard electro-magnetic power as impracticable, on account of its cost, which must necessarily be, he conceives, under the best conditions, fifty times more expensive than steam power, and is at present at least 150 times as expensive.*

8. *Improvements in the Air-pump;* by Mr. Varley, Jr., (Athenæum.)—In place of the two barrels and vibrating intermittent motion of the ordinary pump, Mr. Varley has a continuous circular motion in the handle, and one double-acting barrel. The piston-rod is attached to a crank on the motion shaft, and the cylinder oscillates from its bottom, a packed joint being done away with by having the tube between the barrel and the receiver coiled spirally, which, by its spring, gives play enough for oscillation of the barrel. Mr. Varley explained his larger pump, in which there are some contrivances in addition to those already mentioned. Instead of a valve opening inwards into the barrel by the pressure of the air, as in the old pumps, the valve is worked by an eccentric, and is so arranged as to open a communication between

* A new invention by Prof. Page, described at the recent meeting of the American Association, sets aside entirely this calculation; the whole expenditure for the zinc consumed in producing with his machine 1 horse power for 24 hours, is 20 cents.

the top and the bottom of the barrel at each stroke, by which the rarefaction of the air is doubled. He has obtained, with this pump, a vacuum of $\frac{1}{10}$ of an inch of mercury.

9. *On Photography on Glass;* by T. A. MALONE, (Athenæum, No. 1179, p. 589.)—In repeating the experiment of M. Niepce de Saint-Victor on photography on albumen (published in the *Technologiste* for 1848,) I was led to devise a plan of my own for making "glass negatives." I proceeded as follows:—To the white of an egg its own bulk of water was added; the mixture beaten into a froth was then put into a strainer made of letter-paper so twisted as to form a cone, having a small aperture at its apex; pinned near the base to hold the paper to its shape. The clear diluted albumen soon passed through into a wide-mouthed bottle, which answered the double purpose of a receptacle for the fluid and a support to the cone. A piece of plate-glass, thick or thin, as you please, was then rubbed with a solution of caustic alkali, washed in water, and dried with a cloth: just before applying the albumen, the glass was breathed upon and rubbed with new blotting-paper; then, to remove dust and fibres, cotton wool was used. Unless this latter and every other precaution is taken to prevent dust, the picture will be full of spots produced by a greater absorption of iodine (in a subsequent process) in those than in the surrounding parts.

Now pour the albumen on the glass, inclining the plate from side to side until it is covered; allow the excess to run off at one of the corners, keeping the plate nearly vertical. As soon as the albumen ceases to drop rapidly, breathe on, or warm the lower half of the plate; the warmth and moisture of the breath will soon cause it to part with more of its albumen; wiping the edges constantly hastens the operation.

Until this plan was adopted, the coatings were seldom uniform; the upper half of the plate retained less albumen than the lower,—of course care must be taken to warm only the lower half. When no more albumen runs down, dry the plate. I use for this purpose a double-ring gas-burner of some eighty jets. A common fire answers as well, save now and then it imparts a little dust.

The film, when dry, is quite free from cracks, and is so thin and transparent that the brilliancy of the glass is unimpaired. It is almost necessary to mark it to know which side has been coated.

The next operation is to iodize the plate. Dilute pure iodine with dry white sand in a mortar, using about equal parts of each. Put this mixture into a square glass trough, and over it place the albumined plate; as soon as the latter has become yellow in color, resembling beautiful stained glass, remove it into a room lighted only by a candle, or through any yellow translucent substance—yellow calico, for instance. Here plunge it vertically and rapidly into a deep narrow vessel containing a solution of "aceto-nitrate" of silver, made by adding three ounces of nitrate of silver to two ounces of glacial acetic acid, diluted with sixty ounces of distilled water. Allow it to remain until the transparent yellow tint disappears, to be succeeded by a milky-looking film of iodid of silver. Washing with distilled water completes this operation. The plate is now ready for the camera. After it has been submitted to the action of the light pour over its surface a saturated solution of gallic acid. A negative Talbotype image on albumen is the result. Washing with water before and after immersion, in a solution of one part of hyposulphite of soda in 16 parts of water, until the yellow tint is removed from the shadows, completes the process.

But where is the novelty? Let us go back a step. While the gallic acid is developing its reddish-brown image, pour upon the surface a strong solution of nitrate of silver:—the brown image deepens in intensity until it becomes black. Another change commences: the image begins to grow lighter, and, by perfectly natural magic, finishes by converting the black into white, presenting the curious phenomenon of the conversion of a Talbotype *negative* into, apparently, a Daguerreotype *positive*, but by very opposite agency, no mercury being present;—metallic silver (probably) here producing the lights, while in the Daguerreotype it produces the shades of the picture. I have said probably, because it may be unwise to speculate chemically upon appearances which may depend solely on molecular arrangement:—an intricate subject, to which I hope this communication may prove a slight contribution.

Prof. Wheatstone has suggested to me the desirableness of substituting blackened wood or blackened ivory for glass plates; we should probably then have the novelty of a Daguerreotype on wood free from some of the disadvantages attendant on polished metal. Mr. Cundall suggests the application of it to wood blocks for wood engravers for certain purposes, making the drawings by light instead of by hand.

10. *Lead Statuary;* (Athenæum, No. 1176, p. 511.)—I doubt not every true lover of art will feel grateful to your Edinburgh correspondent for his excellent letter on the subject of the fitness of lead as an economical and most perfect substitute for bronze or other costly material for statuary and other sculptural works of art.

I trust the subject which he has brought before your readers will receive all due attention from those who practice, as well as from those who desire to encourage, this noble department of art.

The object which I have in view in intruding on your attention on this occasion is to confirm, as a practical man, the perfect fitness of lead as a substitute for all such works of art as have hitherto been executed in bronze or marble; and to add that, owing to the comparatively low temperature at which lead melts, and the ease and perfection with which it can be cast into the most intricate and delicate forms, our artists may resume that admirable system of casting groups of statuary and other complex sculptural designs which was in use during the finest periods of Greek and Italian art,—namely, by the employment of wax as the material for the original work, which yields such perfect facility for the execution—and when completed coating or enveloping the wax original in plaster of Paris, and then melting out the wax, and so leaving a most perfect mould, be the intricacy or complexity of the original ever so great. By this mode our artists may revel in the most difficult "undercutting," and be certain to bring forth a metal casting as sharp and perfect in all the integrity of its parts and minute details as was the original.

The addition of about five per cent. of *antimony* to the lead will give it not only great hardness, but enhance its capability to run into the most delicate details of the work. As to the durability of the lead for such works of art, any one who has observed the next to no waste which has taken place in lead exposed on the roofs of ancient buildings, will have in this way most abundant and satisfactory proof that it is in every sense of use as durable a material as bronze when subject simply to atmospheric action.

It would give me pleasure to enumerate several practical details in respect to the employment of lead for the purposes in question,—as also to detail the process of moulding hollow statues, &c.,—should you or any of your readers think such information worthy of your attention. I am, &c. JAMES NASMYTH.

11. *British Association.*—The British Association commenced its twentieth Meeting at Edinburgh on Wednesday, July 31. By Wednesday night 900 names had been recorded, and the receipts amounted to £814. At the first meeting, Wednesday night, Sir David Brewster, the President for the session, addressed the Association.

12. *Sun and Moon;* (L'Institut, No. 857.)—M. NIEPCE DE ST. VICTOR has obtained images of the sun and moon on beds of albumen rendered sensible by an accelerative process peculiar to it. These photographic experiments confirm the opinion before stated by MM. Fizeau and Foucault, that the centre of the sun gives out rays of a greater photogenic power than those of the sides.

13. *On a cloud of dust which obscured the sun for two days in Russia, on the 29th and 30th of April,* 1840, *during a clear sky and quiet weather;* by EHRENBERG, (Monatsb. Berlin Acad., Jan., 1850.)—This powder was furnished Ehrenberg by M. Eichwald. Microscopic examination brought to light 49 animal forms, soft portions of plants, a few crystals, a morpholite and some sand. This powder is distinguished from that of the trade winds by some prominent forms. Ehrenberg believes that there is reason for concluding that this meteoric powder is neither a terrestrial powder nor simple volcanic cinders.

14. *American Zoological Journal.*—We take pleasure in announcing the speedy appearance of a Zoological Journal at Cambridge (Massachusetts), under the direction and editorship of Prof. Agassiz. Zoologists will hail it with great pleasure, and all interested in science will be rejoiced that it is in hands so able. Its memoirs will bear upon whatever pertains to animal life, its development, anatomical structure, physiological relations, &c., and hence those who may find profit in its pages are numerous throughout this as well as other countries. It will mark the progress of this department of science over the world, and each number therefore will be laden with new truths from the most recent studies of animal life, at home and abroad. The editor is so well known for his own profound researches and original discoveries, that a word from us on this point would be superfluous.

15. *Discovery of the Antarctic Continent.*—This discovery, alluded to on page 137, was made in the winter of 1839–1840, in January, 1840, and not 1839. The French made their first discovery on the afternoon of the 19th of January, *three days* after the observation mentioned on the page referred to.

16. *Meeting of the American Association for the Advancement of Science at New Haven.*—This session commenced on the 19th of Au-

gust and was continued through the week with great zeal and interest. Prof. A. D. Bache, Superintendent of the U. S. Coast Survey, was president of the meeting. The number in attendance was larger than ever before, and the papers offered covered a wide range of subjects. The meeting was subdivided into three Sections—"General Physics and Mathematics"—"Geology and Natural History," and "Chemistry and Mineralogy." A general meeting was held for an hour morning and afternoon, and in the evening a session of a more popular character, at which time addresses and discussions were heard. Prof. Henry, Secretary of the Smithsonian Institute, and President of the Cambridge Meeting, delivered his address on Thursday evening. As this noble discourse will be published in full, it is unnecessary to say more than that it contained a most admirable review of ethics for science and was peculiarly fitted for the present period of scientific progress. The next *annual* meeting of the Association will be held at Albany on the 3d Monday of August, 1851, and the semi-annual meeting on the 1st Monday of May next, at Cincinnati. Prof. L. Agassiz is the President and Prof. W. B. Rogers, General Secretary, of the year. Prof. Spencer F. Baird, of the Smithsonian Institute, was appointed permanent Secretary, and charged with the publication of the proceedings and scientific correspondence. Professor Bache gave, as President of the Charleston Meeting, an interesting sketch of that session held in March last.

NATURAL HISTORY AND GEOLOGY.

On the position and character of the Reptilian Footprints in the Carboniferous Red Shale formation of Eastern Pennsylvania. By Prof. H. D. ROGERS, Boston.

On the coal formation of the United States, and especially in Pennsylvania. By Prof. HENRY D. ROGERS.

On the connection of the deposits of common salt with climate. By Prof. HENRY D. ROGERS.

On the decomposition of Rocks and Minerals by water impregnated with carbonic acid. By Profs. W. B. ROGERS, and H. D. ROGERS.

Tertiary Fossils of Marshfield. By Dr. C. T. JACKSON.

On ancient Pot-holes in Rocks. By Dr. C. T. JACKSON.

The genus Amia, a true living representative of the old family of Cœlacanthi. By L. AGASSIZ, Harvard.

On the age of the metamorphic rocks of Eastern Massachusetts. By Prof. L. AGASSIZ.

Comparison between the young caterpillars of Lepidoptera and the adult Larvæ of Musquitoes, and on the mode of formation of Stigmata. By Prof. L. AGASSIZ.

Remarks upon the care which certain fishes take of their young. By Prof. L. AGASSIZ.

On the development of compound organs from single cells. By Prof. L. AGASSIZ.

Singular development of the liver, air-bladder and kidneys in Siluridæ. By Prof. L. AGASSIZ.

On the development of compound eyes in Articulata. By Prof. L. AGASSIZ.

On some points in the structure of Scleroderms and Gymodonts. By Prof. L. AGASSIZ.

On the structure of the mouth in Crustacea. By Prof. L. AGASSIZ.

On the differences of Structure of Cells in Animals and corresponding differences in their functions. By Prof. L. AGASSIZ.

Comparison of the face of fishes with that of other Vertebrata and Man. By Professor L. AGASSIZ.

On the relation between coloration and structure in the higher animals. By Prof. L. AGASSIZ.

On the growth of the Egg, prior to the development of the embryo. By Prof. L. AGASSIZ.

On a new type of scales in Fishes. By Prof. L. AGASSIZ.

On a fossil species of Walrus found by Prof. Fraser on the shores of New Jersey. By Prof. L. AGASSIZ.

On the probable age of the Moa Bone Beds of New Zealand. By REGINALD N. MANTELL, C. E.

Notice of the discovery of a portion of the upper jaw of the Iguanodon with teeth in their natural position. By REGINALD N. MANTELL.

Relations of terrestrial Mollusca in Jamaica—gradation of species into each other. By Prof. C. B. ADAMS, Amherst College.

On the nature and origin of the species of terrestrial Mollusca, in the Island of Jamaica. By Prof. C. B. ADAMS.

Suggestion on changes of level in North America, during the drift period. By Prof. C. B. ADAMS.

On the value of Shells of Mollusca as furnishing distinctive characters. By Prof. C. B. ADAMS.

Curious growth of a Potato. By S. WEBBER.

Essay on the classification of Nemertes and Planariæ. By CHARLES GIRARD, Cambridge.

On a new Generic Type in the class of worms. By CHAS. GIRARD.

On a new American Saurian Reptile. By CHAS. GIRARD.

On the early uses of the metals as a medium of exchange. By J. H. GIBBON.

On the Volcanoes of central America, with observations on the Geographical and Topographical features of Nicaragua. By E. GEO. SQUIER.

Some observations on the gold formation of Maryland, Virginia and N. Carolina. By Prof. W. R. JOHNSON, Washington.

On the coal formation of Central North Carolina. By Prof. W. R. JOHNSON.

Researches on the origin, development, and nature of the spermatic particles throughout the Vertebrata. By Dr. W. J. BURNETT, Boston.

On the Vibriona of Ehrenberg, not Animals, but Plants. By Dr. W. J. BURNETT.

On Utricles as the primordial forms of all animal tissues. By Dr. W. J. BURNETT.

On the relation of the distribution of Lice to the different Faunæ. By Dr. W. J. BURNETT.

Notice of the habits of Ploiaria brevipennis. By Rev. THOMAS HILL.

Notice of observations on drift Striæ in New Brunswick. By Prof. JAMES ROBB, Fredericton, N. B.

On the Taconic system. By T. S. HUNT, Montreal.

On some localities of Magnesite with remarks on its connection with the origin of Serpentine. By T. S. HUNT.

On the Cylindrical Structure observed in Potsdam Sandstone. By FRANKLIN B. HOUGH, Somerville, N. Y. Read by Prof. C. U. SHEPARD.

Remarks on the Geology of Mackinac, Drummond of St. Joseph's Islands and the northern shores of Lake Michigan. By Prof. JAMES HALL.

Remarks on the seventeen year Locust. By Miss MORRIS.

On the theories of the Deluge in reference to the Ethnographic distribution of the human race. By J. P. LESLEY.

On the optical characters of American Micas. By Prof. B. SILLIMAN, Jr.

On the origin of a curious spheroidal structure in certain sedimentary rocks. By Professor B. SILLIMAN, Jr.

On the analogy between the mode of reproduction in plants and the alternating generations of some Radiata. By Prof. JAMES D. DANA, Yale.

Description of the new genera of Plants found by Col. Fremont in California. By Dr. TORREY.

Fossil Coniferous Wood from the Devonian strata of Lebanon, Marion Co., Ky. By Prof. GEORGE C. SCHAEFFER. Read by Dr. W. J. BURNETT.

PHYSICS AND MATHEMATICS.

On the origin and classification of Mechanical Powers. By Prof. JOSEPH HENRY, Secretary of the Smithsonian Institution.

Analysis of the Dynamic Phenomena of the Leyden Jar. By Prof. HENRY.

On the fundamental principles of Dynamics. By Prof. BENJAMIN PIERCE, Harvard.

On the probable period of the fundamental star *a* Virginis. By Prof. B. PEIRCE.

On galvanic wave time. By R. CULMANN, Bavaria.

Description of an instrument for exhibiting the mode of vibration in a molecule of unpolarized light. By Prof. E. S. SNELL, Amherst.

On Lunar distances. By Prof. W. M. CHAUVENET, of the U. S. Naval Academy.

Description of the Tidal observations at Cat Island in the Gulf of Mexico. By Prof. A. D. BACHE, Superintendent U. S. Coast Survey.

On the modes adopted in the Coast Survey, for Charts of Currents. By Prof. A. D. BACHE.

On the use in the Coast Survey, of the Zenith Telescope, by Talcott's method, and the discussion of the observations. By Prof. A. D. BACHE.

On the late periodical visitation of the Aurora Borealis. By Prof. D. OLMSTED.

Cause of the sudden disappearance of the ice of Lake Champlain. By Prof. D. OLMSTED.

On certain points of electrical theory. By Prof. D. OLMSTED.

On some peculiar properties of a compound of Lard and Rosin. By Prof. D. OLMSTED.

Notice of a powerful Magnet. By Prof. B. SILLIMAN, Jr.

On the optical moving of figures. By Prof ELIAS LOOMIS, New York University.

On electrical phenomena observed in certain houses. By Prof. LOOMIS.

On the continuance of the Magnetic and Meteorological Observations at the Toronto Observatory. By Prof. LOOMIS.

Some remarks on the theory of the solar spots. By Prof. H. D. ROGERS, Boston.

On the theory of Storms. By Dr. HARE.

On the use of the zenith telescope in the determination of Latitude. By Prof. LEWIS R. GIBBES, Charleston, S. C.

Description of a new instrument for measuring the angle contained between the optic axes of crystals, and for goniometrical purposes. Accompanied by the angles contained between the optic axes of some American Micas. By W. P. BLAKE, Yale Laboratory.

On the extension of Bode's Law. By Prof. STEPHEN ALEXANDER, Princeton.

On the law of the induction of an electrical current upon itself. By Mr. J. H. LANE, Patent Office.

On a Whirlwind produced by the burning of a Cane-brake in Alabama. By ALEXANDER FISHER OLMSTED, New Haven.

On Monsoons on the shores of the North Atlantic. By Prof. J. H. COFFIN.

On the apparent necessity for revising the received systems of Dynamical Meteorology. By W. C. REDFIELD, New York.

Communications on the Solar Eclipse of July, 1851. By Lieut. C. H. DAVIS, Supt. of the Nautical Almanac.

On the numerical computation of the co-efficients of the perturbatory function of Planetary motion. By SEARS C. WALKER.

On Barometrical measurements and the distance to which corresponding observations may be used for that purpose. By Prof. ARNOLD GUYOT, Cambridge.

On a system of Meterological Observations established in the State of New York by order of the Regents of the University, in connection with, and according to the direction of, the Smithsonian Institute. By Prof. ARNOLD GUYOT.

On the velocity of the galvanic Current in the Telegraph wires. By Dr. B. A. GOULD, Jr.

On the Astronomical Journal. By Dr. B. A. GOULD, Jr.

Method of ascertaining the velocity of the Galvanic current. By ORANGE JUDD, Yale Analytical Laboratory.

Scientific interest of the proposed Industrial Exhibition at London in 1851. By W. R. JOHNSON.

On the influence of temperature on the absorption of light. By Prof. WOLCOTT GIBBS, New York.

On a mode of effecting the Achromatism of the Telescope by Lenses of the same dispersive power. By G. P. BOND, Harvard.

Notes on the progress of the determinations of the difference of Longitude between Greenwich and Cambridge, for the Coast Survey, by W. C. Bond, Director of Cambridge Observatory. By G. P. BOND, Cambridge.

On the variation in the proper motion of the fundamental star *a* Virginis. By Mr. E. Schubert of Cambridge.

On a new method of observing and recording astronomical R. A. and N. P. distances. By Prof. O. M. Mitchel, Cincinnati.

On the Laws of Perfect Musical Intonation, and their application to the Church Organ. By H. W. Poole.

A plan for stereotyping Catalogues of Libraries by separate titles, and for forming a general stereotype Catalogue of public Libraries of the United States. By C. C. Jewett, Assistant Sec. and Librarian of the Smithsonian Institute.

On the Nautical Almanac. By Charles H. Davis, Superintendent of the Nautical Almanac.

Elliptical Tables of the Planet Neptune. By Prof. George W. Coakley, St. James College, Maryland.

CHEMISTRY AND MINERALOGY.

Account of six new Mineral species. By Prof. Charles U. Shepard.

Notice of Foreign Meteorites and of a large stone lately found, of the Linn Co. fall, Missouri, Feb. 7, 1847. By Prof. C. U. Shepard.

Some notices of American Minerals. By Prof. Charles U. Shepard.

On the absorption of Carbonic Acid by acids and saline solutions. By Profs. W. B. Rogers and R. E. Rogers.

On a new method of decomposing silicates in the process of analysis, with an analysis of the Pink Scapolite of Bolton. By F   ry Wurtz, New York.

On the availability of the Green Sand of New Jersey, as a source of potash and its compounds. By Henry Wurtz.

On the Troostite of New Jersey. By Henry Wurtz.

On Canadian localities of Ilmenite and Chromic Iron, with remarks upon the association of these minerals with the Gold of Canada and California. By T. S. Hunt, Canada Geol. Commission.

On the analysis of Soils and the ashes of Peat. By T. S. Hunt.

On the determination of Phosphoric Acid. By T. S. Hunt.

On a locality of Asphaltum and its origin. By T. S. Hunt.

On artificial Crystals of Sesquioxyd of Chromium ($\ddot{\overline{C}}r$,) with some observations on their formation. By W. P. Blake, Yale Laboratory.

On a new test for the nitrates. By G. C. Schaeffer, Prof. of Chemistry in Center College, Ky. Read, with comments, by T. S. Hunt.

On the assumed existence of Ammonia in the general atmosphere. By Aug. A. Hayes, M.D., Asssayer to the State of Massachusetts. Presented by Dr. C. T. Jackson.

Allanite of Franklin, N. J. By Dr. C. T. Jackson.

Telluret of Bismuth and Gold. By Dr. C. T. Jackson.

On the manufacture of Zinc and Zinc white. By Dr. C. T. Jackson.

Analysis of red marl of Springfield, Mass. By Dr. C. T. Jackson.

Zircon, Sodalite, Cancrinite, &c., Litchfield, Me. By Dr. C. T. Jackson.

Analyses relative to the economical value of Anthracite Coal Ashes. By Jonathan B. Bunce, Yale Laboratory.

On the relation of the chemical constitution of bodies to taste. By Prof. E. N. HORSFORD, Harvard.

On the connection between the chemical constitution of bodies and color. By Prof. E. N. HORSFORD.

On the Spheroidal State. By Prof. HORSFORD.

On the adulteration of Vermilion. By HENRY BROWN, Cambridge Laboratory.

Ammonia in Atmospheric Air. By Prof. E. N. HORSFORD.

Analysis of Phlogopite from St. Lawrence Co., New York. By WM. J. CRAW, Yale Laboratory.

Determinations of Nitrogen in two varieties of Indian Corn. By WM. H. BREWER, Yale Laboratory.

Analysis of the ash of Sweet Corn. By WM. H. BREWER.

An account of some experiments upon the cause of fermentation. By Dr. HENRY ERNI.

On American Spodumene. By GEORGE J. BRUSH.

On some peculiar properties of a compound of Lard and Rosin. By Prof. D. OLMSTED, Yale College.

Notice of two American Meteoric Irons. By Prof. B. SILLIMAN, Jr.

Proper height of Lightning Rods. By Prof. ELIAS LOOMIS, New York University.

On Rutile in Quartz and other Minerals. By Prof. O. P. HUBBARD, Dartmouth College.

1. *Proceedings of the American Association for the Advancement of Science;* THIRD MEETING, held at Charleston, S. C., March, 1850. 216 pp. 8vo.—This volume made its appearance in time to greet the members of the Association at their recent meeting in August; and, considering the short interval since the adjournment at Charleston, the vote of thanks from the Association to the Secretary, Prof. Lewis R. Gibbes, for his superintendence of the work, was doubly due him. At this time we can mention only a list of the papers brought before that meeting.*

Catalogue of Plants inhabiting the vicinity of the Santee Canal, S. C. By H. W. RAVENEL, Esq.

On the influence arising from the discovery of the Gulf Stream upon the Commerce of Charleston. By Lieut. M. F. MAURY.

On the exudation of ice from the stems of Vegetables. By Prof. JOHN LECONTE.

Characteristics of the Hindoo Skull. By S. KNEELAND, Jr.

On Meat Biscuit. By G. BORDEN, Jr.

Report of Committee on the communication of Lieut. MAURY, upon Winds and Currents.

The Manatus not a Cetacean but a Pachyderm. By S. KNEELAND, Jr.

A simple demonstration of the theorem, that the attraction of a sphere upon an exterior particle is the same as if the sphere were concentrated at its centre. By Prof. JAMES H. COFFIN.

On some of the applications of Natural Science to the moral laws of ancient nations. By Dr. J. H. GIBBON.

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* The asterisk where used below, signifies that an abstract of the memoir was not handed to the Secretary for publication in the Proceedings.

Results of observations on the direction and force of Wind at the Coast Survey Stations. By Prof. A. D. BACHE.

On the application of the Electro-Chronograph in determining the figure and density of the earth. By Lieut. M. F. MAURY.

Remarks on preceding paper by Prof. L. R. GIBBES; fitness of Stone Mountain, Ga., for such observations; smallest interval appreciable by the ear between the beats of two chronometers.

On the existence in some individuals of two Insensible spots on the Retina. By Prof. LEWIS R. GIBBES.

Researches on the generation and development of the Opossum. By Dr. MYDDELTON MICHEL.

Remarks of Prof. L. AGASSIZ on preceding paper; necessity of further details.

Remarks of Rev. Dr. BACHMAN on the same paper; vigor and power of suction of young opossum, just taken from the uterus.

On the Palæozoic Rocks of Alabama. By Prof. M. TUOMEY.*

On the peculiar sensations produced by a damp atmosphere. By Dr. W. L. JONES.

On the Fossil Equus. By Dr. ROBERT W. GIBBES.

Remarks on the preceding paper. By F. S. HOLMES. No fossil mammalian remains except cetacean, in the Eocene marl of So. Ca.

On the Northern Elephas, and on Mastodon angustidens. By Dr. R. W. GIBBES.

On Fossils common to several Formations. By Dr. R. W. GIBBES.

Remarks on the preceding paper by Prof. AGASSIZ and Prof. TUOMEY; the species common to two or more strata are very few.

On the air bladder of the Drum-fish, Pogonias fasciatus, and the mechanism by which the sound is produced. By Dr. J. E. HOLBROOK.*

Remarks on the preceding paper by Prof. AGASSIZ; development of air bladders.

Remarks on the paper of Prof. Tuomey, of yesterday, by Dr. E. RAVENEL and Lieut. MAURY; importance of the coal fields of Alabama to the navigation and commerce of the Pacific.

Remarks on the paper of Dr. R. W. Gibbes, of yesterday, by Prof. Agassiz; the species common to different formations are very few, mistakes in this respect are attributable to geological error.

Exhibition of a fossil reptile belonging to the genus Leiodon. By Prof. TUOMEY.

On the Currents of the Atlantic Ocean. By Lieut. M. F. MAURY.

Remarks on the preceding paper by Prof. AGASSIZ; or the fancied importance of the name of the first describer of a species.

On the Marine Flora of the Atlantic. By Prof. W. H. HARVEY.

On the comparative reflecting power of the Planets, Mars, Jupiter and Saturn. By Prof. LEWIS R. GIBBES.

Distribution of the Foraminifera on the Coast of New Jersey. By F. DE POURTALES.

Remarks on this paper, by Prof. AGASSIZ; the aid rendered to Naturalists, by the Coast Survey.

On the order of Succession of parts in Foraminifera. By F. DE POURTALES.

On the Principles of Classification in Zoology. By Prof. AGASSIZ.

On the American species of the genus Putorius. By the Rev. Dr. BACHMAN.*

On the alleged subsidence of the Coast of South Carolina. By Prof. TUOMEY.*

Examination of the Physical History of the Jews, in its bearings on the question of the Unity of the Races. By Dr. J. C. NOTT.

Remarks of Prof. AGASSIZ, after the reading of this paper; zoological evidence for the diversity of the Races.

Microscopic examination of the Pile of the Head of Albinos. By P. A. BROWNE, Esq.

On an easy mode of illustrating the difference in the Velocity of Sound in Gases. By Prof. LEWIS R. GIBBES.

On the Morphology of the Medusæ. By Prof. AGASSIZ.

Recent Progress of the Telegraph Operations of the United States Coast Survey. By Prof. A. D. BACHE.

On the General Circulation of the Atmosphere. By Lieut. M. F. MAURY.

Measurement of the Base Line on Edisto Island, S. C. By Prof. A. D. BACHE.

Account of three new American Meteorites, and geographical distribution of such bodies generally. By Prof. C. U. SHEPARD.

On the Structure of the Bones of Siren lacertina. By Dr. ST. JULIEN RAVENEL.*

On a new species of Menobranchus, from South Carolina. By Prof. LEWIS R. GIBBES.

On the recent Squalidæ of the Coast of South Carolina, and Catalogue of the Recent and Fossil Echinoderms of South Carolina. By Dr. EDMUND RAVENEL.

On the Cretaceous Formation of Alabama, and the Artesian Wells in that State. By Prof. TUOMEY.*

On the Resistance of Timber. By H. HAUPT.

On the Carcinological Collections of the United States, and descriptions of new species. By Prof. LEWIS R. GIBBES.

On the Morphological Differences of Organs. By Prof. L. AGASSIZ.*

Meteorological and Mortuary Chart of New Orleans, for 1849. By Dr. E. H. BARTON.

Observations on the Geology of Ashley River, S. Carolina. By F. S. HOLMES.

Remarks on the preceding paper, by Prof. AGASSIZ; the large number of Mammalia in the Fossil Beds of South Carolina.

Proximate Composition of parts of Flowers of Plants, and of the Plants themselves. By Dr. J. H. SALISBURY.

On the Structure of the Halcyonoid Polypi. By Prof. AGASSIZ.

18. *Editorial note with reference to the article on the Electro-Chronograph, in Volume III.*—The article on the "Electro-Chronograph," which was published in this Journal for September of last year, contained allusions and imputations of an improper personal nature, reflecting discreditably upon the motives and veracity of some of the most eminent and unimpeachable among American laborers in science. We regret that these justly offensive portions escaped editorial notice at the time the article in question passed through the press. Our ob-

ject in alluding to the subject at this late day, (late because our attention was only recently called to it,) is, at once, to disavow on our own behalf all the expressions and passages of an offensive and personal nature which that article contains—to whomsoever they may apply—and at the same time to deprecate, in the most emphatic manner, all personalities in science, every instance of which we most earnestly desire to exclude from our pages. This statement we esteem in entire accordance with the limitation of editorial responsibility upon which we have uniformly acted, and which is fully expressed on page 151 of this volume.

OBITUARY.

19. M. de Blainville.—At the funeral of M. de Blainville, which took place on Tuesday, the 7th of May last, a discourse was pronounced by M. Constant Prévost, another by M. Chevreul, and a third by M. Milne Edwards.—The following are some citations from the remarks of M. Prévost.

"But eight days since, on Tuesday last, at the same hour that now assembles us around this bier, I happened to be present at the last lecture of M. de Blainville. He was animated, full of his subject, and he entertained us for nearly an hour, exhibiting a freshness of ideas and facility of expression which bore no marks either of fatigue or apprehension. Some threatening symptoms had been experienced during the year past, but, with a force of character peculiar to him, he had sought to conceal them from all, and even from himself. Wednesday evening he proposed to visit his niece who was sick at Dieppe, intending to return in time for his lecture the following Saturday; but when about to start, he was found dead in the carriage where he had taken his seat but a few seconds before. * * * *

Marie-Henri Ducrotay de Blainville was born at Arques, September 12, 1778. A student of the military school of Beaumont near Touques, and destined, as a cadet of a noble family, to follow the career of arms, for some reason unknown to me he abruptly left the school in 1792. At the risk of his life he sought refuge on board a vessel cruising in the British channel and took part in several serious combats. Returning to France, M. de Blainville for some years of his youth, pursued with inconstant enthusiasm, an ardent imagination and an impetuous character, the study of different branches of literature and the arts. * * * *

At the age of twenty-seven (I believe) he was still undecided with regard to his future pursuits, when one day, as if by chance, or better, by the guidance of Providence, his vocation was irrevocably determined. He entered the college of France and heard a lecture by Cuvier. Struck at once with the interest of the subject and the eloquent words of the celebrated Professor, he left the building with the resolution to give himself to natural science and become a Professor. From this time his desultory habits were changed; in three years he went through courses of human anatomy and two years afterward (in 1810) he became a doctor of medicine. In 1812, having for some time assisted Cuvier at the College of France and at the Museum, he was appointed to the chair of Zoology, Anatomy and Physiology of the Faculty of Sciences; and in 1832, on the death of Cuvier, all eyes were upon M. de Blainville as the only person fitted to be his successor in the department of Comparative Anatomy. Thus 28 years from the period when his resolve was made had sufficed to place him, through his own efforts alone, in this supreme position in Science." * * * *

20. Christian VIII, King of Denmark, (from the Anniv. Geol. Address of Sir C. Lyell, Quart. Jour. Geol. Assoc., No. 22, 1850.)—Christian VIII, King of Denmark was enrolled a Fellow of this Society in 1822. Two years before that time, when travelling in Italy, he had witnessed an eruption of Vesuvius, and had read a description of it to the Academy of Sciences at Naples; a communication published in their Transactions, and afterwards reprinted in Leonhard's Journal for 1822.

From an early age he had taken a lively interest in the progress of natural history, and when Crown Prince, formed at Copenhagen, at his own expense, a magnificent collection of shells, the number of species being estimated at not less than 12,000, exclusive of fossils. When I visited the Danish capital in 1835, he placed this museum and his library at my disposal, and I had then an opportunity of knowing that he kept up an acquaintance with the new species added from year to year to his cabinet, then in charge of an able conchologist, Dr. Beck, and that he was very desirous of making his museum useful to all zoologists. Nor was he inattentive to the points of controversy then agitated respecting the geology of Denmark. He questioned me closely as to my opinion, whether the strata of Faxoe, containing certain species of *Cyprea*, *Oliva*, *Mitra*, and other genera usually regarded as characteristic of the tertiary period, really belonged to that epoch, or to the cretaceous rocks. That the latter conclusion was correct I had satisfied myself, after exploring the cliffs of Moën and Seeland, as I have explained in your Transactions; and you are aware that the Faxoe beds, together with those of Maestricht and Sezanne near Paris, have been recently classed as an upper member of the great cretaceous system.*

When Christian VIII. succeeded to the throne, the cares and duties of an absolute monarch did not make him forgetful of his former love for natural history. He was always accessible to scientific foreigners and natives, and set on foot several publications, among which I may mention the 'Gæa Danica' of Professors Steenstrup and Forchhammer. He also gave his patronage to a splendid botanical work on the palms of Mexico, by Professor Liebmann, and promoted liberally the geological expedition of Baron von Waltershausen and Professor Bunsen to Iceland. He also took care that a good naturalist should accompany the voyage of the Galathea around the world; and when that expedition returned, he directed that the valuable collections, made by the officers in various countries, should be divided equally between the Universities of Copenhagen and Kiel. As Crown Prince, he had been elected President of the Academy of Sciences at Copenhagen, and when he attended their meetings, after his accession to the throne, he always declined to be received as king, taking his place simply as a member, or as any other President. After a reign of nine years, he died in January, 1848.

* Geol. Trans., 2nd series, vol. v, p. 249.

21. Darius Lapham.—Mr. Lapham, at the time of his death (August of the present year), was Canal Collector at Cincinnati, and member of the State Board of Agriculture. He had charge of the preparation of the grounds for the State Fair, to be held in September.

In the death of Mr. Lapham the State Board of Agriculture has lost one of its most valuable members, and the State one of its best citizens. His death will be deplored by a very large circle of friends, to whom he was warmly attached for his many virtues, and modest, retiring deportment. It is but a few days ago that we received a letter from him, giving quite an encouraging account of the progress he was making in the extensive preparations for the State Agricultural Fair. To-day he is numbered with the dead.—*Ohio Statesman.*

22. Dr. Troost.—Gerard Troost, M.D., Professor of Chemistry, Geology and Mineralogy in the University of Nashville, after a protracted illness, departed this life on the 14th of August, at 1 o'clock, P. M.

Born and liberally educated in Holland, he early manifested a zealous devotion to Natural History and Chemistry, and more especially to the then infant sciences of Geology and Mineralogy. With a view to the more successful prosecution of his favorite studies, he visited Paris, and, for several years, was a pupil of the celebrated Haüy. He removed to the United States about forty years ago; and, in due time, became an American citizen. His entire life was consecrated to Geology and the kindred sciences. With what ability and success, his published writings and his well-earned reputation at home and abroad, may eloquently testify.

As a Professor in this University, during the last twenty-two years, and as the State Geologist of Tennessee for most part of that period, he won the confidence and respect of the community, by invaluable services in both capacities, as well as by the unaffected modesty, kindness, and uniform courtesy of his deportment towards all men. In the various stations and relations of life, public and private, he was without reproach and above suspicion. Beloved, trusted, honored, venerated, by all those most intimately connected or associated with him, he could not make an enemy—he had none.—(From the Proceedings of the Board of Trustees of the University of Nashville.)

V. Bibliography.

1. *Tabulæ Atomicæ: Chemical Tables for the Calculation of Quantitative Analyses of H. Rose*, recalculated for the more recent determinations of atomic weights, and with other alterations and additions, by William P. Dexter. 70 pp. 8vo. Boston: Little & Brown. 1850.—The Tables of Atomic Weights by Rose are well known to chemists and fully appreciated. The author of this American edition has not simply given us a reprint of Rose. As the atomic weights of several of the elements have been changed through the recent investigations of chemists, a large amount of recalculations became necessary both in the numbers for the compounds of the elements whose atomic numbers have been redetermined, and for very many other elements whose equivalents were based on those that have been altered. To insure perfect accuracy, the calculations, as the author states, were performed by himself both directly and by logarithms;

and the columns of multiples were computed separately by himself and another. From the care thus taken, and the thorough knowledge which the author possesses, we may feel fully assured of the correctness of the work. Those atomic weights have been taken that were deemed most trustworthy, the determinations of Berzelius, being allowed, as by Rose, the greatest weight; some recent determinations have been rejected for want of confirmation. A column of logarithms has been added to the tables by Mr. Dexter. A large part of the compounds given in Rose's work under chlorine and sulphur have been omitted, as they were "of comparatively little practical use." The volume commences with an introduction of a dozen pages, consisting of observations on the numbers adopted, and their authorities. The author has verified the calculations from the chemical data furnished by different experimenters, and observes that in some cases he has detected a small error of computation. The number determined for titanium by Berzelius from Rose's analyses is 301·55, while it should be 301·304. In a similar manner the number for osmium was found to be 1243·624, instead of 1242·624; that of tungsten should be 1183·36, instead of 1188·36; that of phosphorus as computed by Berzelius is 392·041, while it should be 391·72. The tables are printed in a fair open type and are easy of reference.

2. *First Biennial Report on the Geology of Alabama;* by M. Tuomey, Geologist of the State; Professor of Geology, etc., in the University of Alabama, 176 pp. 8vo. Tuscaloosa, 1850. M. D. J. Slade.—Prof. Tuomey presents in this Report the results of a general reconnoissance of the State, mentioning its great geological features and its resources, in order to show what is required for future explorations and the importance which attaches to the survey. The results obtained exhibit the state as rich in various mineral products as well as in facts of geological interest, and we shall look for much profit and instruction from its full survey by one so capable and so exact in observation. The following facts are from this first biennial Report.

Metamorphic Rocks enter Alabama from the upper corner of Carroll county; their northwest boundary passes through the southeast corner of Benton county, thence southwesterly three or four miles east of the town of Talladega, and crossing the Coosa above Fort Williams, it runs through the lower corner of Antauga county. The part of the state east of this line is wholly covered by these rocks. In the bed of the Coosa they are laid bare as low as Wetumpka, which, as observed by McClure, is the most southern exposure of this important group of rocks in the United States. The gold-bearing rocks extend from Randolph to Antauga. A white and gray marble is quarried near Talladega creek and worked for ornamental purposes. Some of the pure white is of fine quality, little inferior to the Italian, and blocks thirty inches in thickness may be procured.

A Silurian limestone is seen at Centreville on the Catawba, which forms the southern extremity of the Silurian System of Alabama, and probably of the United States. To the east, Silurian rocks spread out towards the Coosa in Antauga and Shelby, until they cover the metamorphic rocks in Talladega, Benton and Cherokee counties. Along the Coosa they make their appearance on the east, coming out from under the Carboniferous rocks of the Cahawba Valley, and higher up in the same manner at the Lookout Mountain, whence they extend to Rome in Georgia. Sixteen miles southeast of Tuscaloosa they are visible, and thence they stretch north to the head of Murphree's Valley, separating the Warrior coal field on the west from that of the Cahawba on the east; and in this direction they are confined to a series of continuous valleys. They constitute a range of hills known as the "Red Mountains," so called from the presence of a bed of red oxyd of iron. Other small patches are mentioned, and further explorations will probbly enlarge this extent of Silurian beds. The red iron ore consists of grains mostly flattened, like the lenticular argillaceous ore of New York. In some places it passes into a conglomerate containing siliceous pebbles. A bed of brown hematite occurs on Shultz Creek, also near the headwaters of Hurricane and Rockcastle Creeks twenty-four miles from Tuscaloosa, and at Bucksville.

Heavy spar occurs in a vein a foot or two thick, near Pratt's Ferry on the Cahawba, and in another similar above Elyton. It is ground up for white paint.

The carboniferous rocks cover all that part of the state above the lower falls of the rivers, not already described as belonging to the Red Mountain group. The greatest development of the calcareous beds enters Alabama as a prolongation of the Cumberland Mountains. Caves abound in these rocks, and the earth frequently contains nitrates, from which nitrate of potash is obtained. The coal measures every where rest on millstone grit. The mountains of Madison and Jackson counties and the hills of Morgan and Marshall counties are often capped with sandstones and shales containing beds of coal. The Raccoon and Lookout Mountains have coal beds of considerable extent. These connect with the measures of the Locust Fork of Warrior creek, and the Cahawba, while the isolated patches of the hills in Morgan and Lawrence run into those of the Sipsey and Mulberry Forks, in such a manner as to leave no doubt of their being parts of one continuous and vast coal field. The St. Clair coal field on the Coosa, connected with the Cahawba on the southwest contains about 150 square miles. The Cahawba coal field is bounded on the south and east by the Little Cahawba and its tributary, Shoal Creek, on the east by Shades Creek as low as Bucksville, and reaches within two miles of Montevallo and within three miles of the river at Lacy's Ferry. It contains 180 to 200 square miles. The Warrior coal field occupies the region drained by the Warrior river and its tributaries, covering for the most part the counties of Walker, Blount, Jefferson and Tuscaloosa, and having an area of about 5000 square miles. The carboniferous rocks of DeKalb, Morgan and Lawrence counties have not yet been explored. The Tuscaloosa coal has been long in use in the state.

The Report continues with observations on the cretaceous and tertiary rocks.

3. *A Treatise of Plane and Spherical Trigonometry;* by WILLIAM CHAUVENET, A.M., Professor of Mathematics in the United States Naval Academy. pp. 256, 8vo. Philadelphia: Henry Perkins. 1850.—In this treatise, the author as he states in the preface, has undertaken "to arrange a course of trigonometrical study sufficiently extensive to enable the student to comprehend readily any applications of trigonometry he may meet with in the works of the best modern mathema-

ticians." For this purpose it was necessary to extend the treatise much beyond the ordinary limits of the elementary text-books used in academies and colleges. But the author has sought to adapt his work for the use of College classes, by putting the elementary matter in larger type, so that it may easily be read independently of the other portion of the book. A complete and well arranged treatise of trigonometry has been greatly needed for the use of mathematical teachers, and of practical astronomers, surveyors, engineers, &c. No work of this kind has before been published in this country; and we know of no English work in which the subject of spherical trigonometry especially, is presented in a satisfactory manner. Many useful formulæ now commonly employed in astronomical investigations are not to be found in the larger and more recent treatises of trigonometry published in England. These deficiencies are well supplied in the work of Mr. Chauvenet: and from the examination we have been able to make we are satisfied that it will prove a more convenient and useful manual for the mathematical teacher and the practical mathematician than any similar English or American work. It would perhaps have been somewhat better for purposes of reference, if the author had added tables in the customary way, giving a synoptical view of the more useful trigonometric formulæ.

4. *A descriptive account of the Freshwater Sponges of the Island of Bombay with observations on their structure and development;* by H. J. Carter, Esq., Assistant Surgeon, Bombay Establishment, 22 pages, 8vo, with 3 plates, (from the Jour. Bombay Branch of the Roy. Asiat. Soc., No. xii, 1849.)—The author in this paper describes 5 species of Spongilla—the S. cinerea, alba, Meyeni, plumosa, new, and the S. friabilis? of Lamarck. With regard to them he observes that in each species excepting the *cinerea* they cannot be said to be characterized by any particular form. The memoir contains interesting details respecting the structure and development of the sponge, many of which are exceedingly curious. The memoir is illustrated by good engravings showing well the structure and the varying forms of the sponge cell.

We cite the following observations on the sponge cell, which are exposed on tearing a newly formed sponge, and the protean cells developed from the contents of the seed-like bodies when forcibly expelled.

"The sponge-cell when *in situ*, is ever changing its form, both partially and wholly; its granules also are ever varying their position with, or independently of the movements of the cell, and its pellucid vesicle or vesicles, dilating and contracting themselves or remaining passively distended, and exhibiting in their interior molecules of extreme minuteness in rapid commotion. When first separated from the common mass, this cell for a short time assumes a globular form and afterwards, in addition to becoming polymorphic, evinces a power of locomotion. During its polymorphism it emits expansions of its cell-wall in the form of obtuse or globular projections, or digital and tentacular prolongations. If in progression it meets with another cell, both combine, and if more are in the immediate neighborhood, they all unite together into one common globular mass. Should a spiculum chance to be in the course of a cell, it will ascend it and traverse it from end to end, subsequently quitting it or assuming a globular form, embrace some part of it and remain stationarily attached to it. The changes in shape and position of the sponge-cell and its intercellular mucilage are for the most part effected so imperceptibly, that they may be likened to those which take place in a cloud. Its granules however are more active, but there appears to be no motion in any part of the cell, excepting among the molecules within the hyaline vesicle, which in any way approaches to that characteristic of the presence of cilia.

"It should be understood however, that these remarks are not applicable to every sponge-cell, although fully developed, which appears in the field of the microscope, but they are rather a statement of what a sponge cell may evince, than of what every sponge-cell does evince.

"The polymorphic cells or proteans which appear in the watch-glass after the contents of a seedlike body have been forcibly expelled into it under distilled water, are much more active in their movements. Their cell-walls frequently assume the most fantastic figures, spheroidal, polygonal, asteroid, dendritic, &c. Their green granules move backwards or forwards, to this side or to that with great activity, as the part of the cell to which they are attached is entrained in one direction or another; while their hyaline vesicle or vesicles (in progression) appear occasionally in every part, not only of the body of the cell, but in its tubular prolongations. The contraction of the hyaline vesicle seems to take place most frequently when it arrives at the posterior extremity, that is according to the direction in which the cell is progressing; next in frequency, at the sides, seldom in the anterior or central part of the mass. When contraction takes place, it is effected more or less completely, more or less suddenly; if complete, a dark speck or opacity marks the original position of the vesicle, in the centre of which, if watched, it may be observed to re-appear, and as it is carried forward in the movements of the cell with the portion to which it is attached, it gradually regains its original size, and returning in due course to the point from which it started, again contracts as before.

"In progression, some of the large proteans developed in the way just mentioned appear to be conscious of the nature of certain objects which they encounter in their course, since they will stop and surround them in their cell-wall. It is not uncommon to see a portion of a spiculum in the latter position, the larger germs of the sponge itself, the body of a loricated animalcule the 900th part of an inch in diameter, on which the pressure exerted by the protean may be seen by the irregular form assumed by the animalcule the moment it has become surrounded. I once saw one of these proteans approach a gelatinous body, something like a sluggish or dead one of its own kind, and equal to itself in size, and having lengthened itself out so as to encircle it, send processes over and under it from both sides, which uniting with each other, at last ended in a complete approximation of the two opposite folds of the cell-wall, throughout their whole extent, and in the enclosure of the object within the duplicature. Even while the protean was thus spreading out its substance into a mere film, to surround so large an object, a tubular prolongation was sent out by it in another direction to seize and enclose in the same way, a large germ which was lying near it. After having secured both objects, the protean pursued its course, rather more slowly than before, but still shooting out its dentiform processes with much activity. It took about three quarters of an hour to perform these two acts."

5. *Recherches Anatomiques et Zoologiques faites pendant un Voyage sur les cotes de la Sicile et sur divers points du littoral de la France;* par MM. MILNE EDWARDS, A. DE QUATREFAGES and EMILE BLANCHARD. 3 volumes, 4to, with numerous plates.—These volumes consist of a series of memoirs on the structure, development and physiology of various animals in different departments of zoology. They are the result of investigations of the most minute microscopic character pursued by the first zoologists of France; the observations are profound, and exact, and the illustrations unsurpassed for fullness and beauty. The *First* volume, by MILNE EDWARDS, contains memoirs on the development of Annelids, on the classification of the Gasteropodous Molluscs, and on the general subject of circulation; the *Second* by M. DE QUATREFAGES, treats of the nervous system and History of the Amphioxus, of the Pycnogonidæ, Phlebenterisma, Planariæ, the Nemertes, Echiurus Gærtnerii, with a review of the observations made in 1844 on the Gasteropoda Phlebenterata; the *third* by M. BLANCHARD, is occupied with a memoir of 336 pages on the organization of Vermes, especially the subdivision "Entomozoaires apodes" of Blainville. Many of these memoirs have appeared in some of the later volumes of the Annales des Sciences Naturelles.

6. *Les Alpes, Journal des Sciences Naturelles, Agricoles, Medicales, Physiques et Astronomiques,* contenant un résumé de tous les travaux qui concernent la Suisse et la Savoie et de tous ceux qui sont publiés dans ces deux pays.—Issued in a sheet of 8 quarto pages, on the 1st and 15th of each month. This periodical made its first appearance on the 1st of July last. It is edited by *Gabriel Mortillet* ingénieur civil à Genève, place de la Fusterie, 78 au 2me. The subscription price for foreigners is 12 francs a year. The object of this Journal is to give a brief review of all new scientific matters relating to Switzerland and Savoy and also of whatever may be published in these countries.

G. HECK: Iconographic Encyclopedia of Science, Literature and Art, the text translated and edited by Prof. Spencer F. Baird, and published by R. Garrigue, New York. Part 10, the text on Botany.

ARNOLD GUYOT: The Earth and Man, 2d edition. *Boston,* 1850.

A. DUBREUIL: Cours Elementaire théorique et pratique d'Arboriculture. 2d edition. *Paris.* 9*fr.*

A. D'ORBIGNY: Cours Elémentaire de Palæontologie et de Géologie Stratigraphiques. 2 vols. 18mo, with 500 figs. in the text and 18 tables in 1 atlas 4to. *Paris.* 10*fr.*

A. D'ORBIGNY: Prodromus de Palæontologie Stratigraphique Universelle. 3 vols. 8vo. *Paris.* 24*fr.* Volume 1 is on sale for 8 francs.

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ERRATUM.

In last Number, p. 39, line 13 from bottom, for "JOHN GARRIC, M.D.," *read* "JOHN GORRIE, M.D."

The next No. of this Journal will be published on the first of Nov.

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Vol. X. NOVEMBER, 1850. No. 30.

Published the first day of every second month, price $5 per year.

THE

AMERICAN JOURNAL

OF

SCIENCE AND ARTS.

CONDUCTED BY

Professors B. SILLIMAN, B. SILLIMAN, Jr.,

AND

JAMES D. DANA.

SECOND SERIES.

No. 30.—NOVEMBER, 1850.

WITH AN INDEX TO VOLS. I—X.

NEW HAVEN:

PUBLISHED BY THE EDITORS.

[FOR AGENTS ADDRESSES, SEE SECOND PAGE.]

Printed by B. L. Hamlen—Printer to Yale College.

The postage on this Journal to any distance is 9½ cts.

THE AMERICAN JOURNAL OF SCIENCE, Second Series, which was commenced in January, 1846, is published on the 1st of January, March, May, July, September, and November, of each year, in Nos. of 152 pages each, making Two Volumes a year, fully illustrated by Engravings, and containing a comprehensive bulletin of Scientific Intelligence. Subscriptions $5 per year, in advance. Remittances should be forwarded to B. SILLIMAN, New Haven, Conn.

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April, 1849.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

Art. XXIV.—*Address of Sir David Brewster before the Twentieth Meeting of the British Association at Edinburgh, July* 31, 1850.*

* * * It has been the custom of some of my predecessors in this chair to give a brief account of the progress of the sciences during the preceding year; but however interesting such a narrative might be, it would be beyond the power of any individual to do justice to so extensive a theme, even if your time would permit and your patience endure it. I shall make no apology, however, for calling your attention to a few of those topics, within my own narrow sphere of study, which, from their prominence and general interest, may be entitled to your attention. I begin with Astronomy, a study which has made great progress under the patronage of this Association—a subject, too, possessing a charm above all other subjects, and more connected than any other with the deepest interests, past, present, and to come, of every rational being. It is upon a planet that we live and breathe. Its surface is the arena of our contentions, our pleasures, and our sorrows. It is to obtain a portion of its alluvial crust that man wastes the flower of his days, and prostrates the energies of his mind, and risks the happiness of his soul; and it is over or beneath its verdant turf that his ashes are to be scattered or his bones to be laid. It is from the interior too—from the inner life of the earth that man derives the materials of civilization—his coal, his iron, and his gold. And deeper still, as geologists have proved—and none

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* From the Athenæum, No. 1188.

with more power than the geologists around me—we find in the bosom of the earth written on blocks of marble—the history of primæval times, of worlds of life created, and worlds of life destroyed. We find there, in hieroglyphics as intelligible as those which Major Rawlinson has deciphered on the slabs of Nineveh, the remains of forests which waved in luxuriance over its plains—the very bones of huge reptiles that took shelter under their foliage, and of gigantic quadrupeds that trod uncontrolled its plains, the law-givers and the executioners of that mysterious community with which it pleased the Almighty to people his infant world. But though man is but a recent occupant of the earth, an upstart in the vast chronology of animal life, his interest in the Paradise so carefully prepared for him is not less exciting and profound. For him it was made, he was to be the lord of the new creation, and to him it especially belongs to investigate the wonders it displays and to learn the lesson which it reads. But while our interests are thus closely connected with the surface and the interior of the earth, interests of a higher kind are associated with it as a body of the solar system to which we belong.

The object of Geology is to unfold the history and explain the structure of a planet; and that history and that structure may, within certain limits, be the history and the structure of all the other planets of the system—perhaps of all the other planets of the universe. The laws of matter must be the same wherever matter is found. The heat which warms our globe radiates upon the most distant of the planets, and the light which twinkles in the remotest star is, in its physical, and doubtless in its chemical properties, the same that cheers and enlivens our own system; and if men of ordinary capacity possessed that knowledge which is within their reach, and had that faith in science which its truths inspire, they would see in every planet around them, and in every star above them, the home of immortal natures—of beings that suffer and of beings that rejoice—of souls that are saved and of souls that are lost. Geology is, therefore, the first chapter of astronomy. It describes that portion of the solar system which is nearest and dearest to us,—the cosmopolitan observatory, so to speak, from which the astronomer is to survey the sidereal universe, where revolving worlds and systems of worlds summon him to investigate and adore. There, too, he obtains the great base lines of the earth's radius to measure the distances and magnitudes of the starry host, and thus to penetrate by the force of *reason* into those infinitely distant regions where the imagination dare not follow him. But Astronomy, though thus sprung from the earth, seeks and finds, like Astræa, a more congenial sphere above. Whatever cheers and enlivens our terrestrial paradise is derived from the orbs around us. Without the light or heat of our sun, and without the uniform movements of our system, we should have neither climates nor seasons. Darkness would blind, and famine destroy, everything that lives. Without influences from above, our ships would drift upon the ocean, the sport of wind and wave, and would have less security for reaching their destination than balloons floating in the air and subject to the caprice of the elements. But while the study of Astronomy is essential to the very existence of social life, it is instinct with moral influences of the highest order. In the study of our own globe we learn that it has been rent and upheaved by tremendous forces—here sinking into ocean depths, and there rising into gigantic elevations. Even now, geologists are measuring the rise and fall of its elastic crust; and men who have no faith in science often learn the truth to their cost, when they see the liquid fire rushing upon them from the volcano, or stand above the yawning crevice in which the earthquake threatens to overwhelm them. Who can say that there is a limit to agencies like these? Who could dare to assert that they may not concentrate their yet divided energies, and rend in pieces the planet which imprisons them? Within the bounds of our own system, and in the vicinity of our own Earth, between the orbits of Mars and Jupiter, there is a wide space which, according to the law of planetary distances, ought to contain a planet. Kepler predicted that a planet would be found there—and strange to say, the astronomers of our own times discovered at the beginning of the present century four small planets, Ceres, Pallas, Juno and Vesta, occupying the very place in our system where the anticipated planet ought to have been found. Ceres, the first of these, was discovered by Piazzi, at Palermo, in 1801; Pallas, the second of them, by Dr. Olbers, of Bremen, in 1802; Juno, the third, by Mr. Harding, in 1804; and Vesta, the fourth, by Dr. Olbers, in 1807. After the discovery of the third, Dr. Olbers suggested the idea that they were the fragments of a planet that had been burst in pieces; and considering that they must all have diverged from one point in the original orbit, and ought to return to the opposite point, he examined these parts of the heavens, and thus discovered the planet Vesta. But though this principle was in the possession of astronomers, nearly forty years elapsed before any other planetary fragment was discovered. At last, in 1845, Mr. Hencke, of Driessen, in Prussia, discovered the fragment called Astræa, and, in 1847, another, called Hebe. In the same year our countryman, Mr. Hind, discovered other two, Iris and Flora. In 1848 Mr. Graham, an Irish astronomer, discovered a ninth fragment called Metis. In 1849 Mr. Gasparis, of Naples, discovered another, which he calls Hygeia; and within the last two months, the same astronomer has discovered the eleventh fragment, to which he has given the name of Parthenope.* If these eleven small planets are really the remains of a larger one, the size of the original planet must have been considerable. What its size was, would seem to be a problem beyond the grasp of reason. But human genius has been permitted to triumph over greater difficulties. The planet Neptune was discovered before a ray of its light had entered the human eye; and by a law of the solar system just discovered, we can determine the original magnitude of the broken planet long after it has been shivered into fragments,—and we might have determined it even after a single fragment had proved its existence. This law we owe to Mr. Daniel Kirkwood, of Pottsville, a humble American, who, like the illustrious Kepler, struggled to find something new among the arithmetical relations of the planetary elements. Between every two adjacent planets there is a point where their attractions are equal. If we call the distance of this point on one side of a planet to that on the other, the *diameter* of its sphere of attraction, then Mr. Kirkwood's law is, that in every planet the square of the length of its year, reckoned in days, varies as the cube of the diameter of its sphere of attraction. This law has been verified by more than one American astronomer, and there can be no doubt, as one of them expresses it, that it is at least a physical fact in the mechanism of our system. This law requires the existence of a planet between Mars and Jupiter; and it follows from the law that the broken planet must have been a little larger than Mars, or about 5,000 miles in diameter, and that the length of its day must have been about 57½ hours. The American astronomers regard this law as amuonting to a demonstration of the nebular hypothesis of Laplace; but we venture to say that this opinion will not be adopted by the astronomers of England. Among the more recent discoveries within the bounds of our own system, I cannot omit to mention those of our distinguished countryman, Mr. Lassels, of Liverpool. By means of a fine twenty-feet reflector, constructed by himself, he detected the satellite of Neptune, and more recently an eighth satellite circulating round Saturn—a discovery which was made on the very same day, by Mr. Bond, Director of the Observatory of Cambridge, in the United States.

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* Ceres	1801, January 1	Piazzi.
Pallas	1802, March 28	Olbers.
Juno	1804, September 1	Harding.
Vesta	1807, March 29	Olbers.
Astræa	1845, December 8	Hencke.
Hebe	1847, July 1	Hencke.
Iris	1847, August 13	Hind.
Flora	1847, October 18	Hind.
Metis	1848, April 25	Graham.
Hygeia	1849, April 12	Gasparis.
Parthenope	1850, May 11	Gasparis.

Mr. Lassels has still more recently, and under a singularly favorable state of the atmosphere, observed the very minute, but extremely black, shadow of the ring of Saturn upon the body of the planet. He observed the line of shadow to be notched, as it were, and almost broken up into a line of dots; thus indicating mountains upon the plane of the ring—mountains doubtless raised by the same internal forces and answering the same ends as those of our own globe. In passing from our solar system to the frontier of the sidereal universe around us, we traverse a gulf of inconceivable extent. If we represent the radius of the solar system, or of Neptune's orbit (which is 2,900 millions of miles) by a line two miles long, the interval between our system, or the orbit of Neptune, and the nearest fixed star will be greater than the whole circumference of our globe—or equal to a length of 27,600 miles. The parallax of the nearest fixed star being supposed to be one second, its distance from the sun will be nearly 412,370 times the radius of the earth's orbit, or 13,746 times that of Neptune, which is thirty times as far from the sun as the earth. And yet to that distant zone has the genius of man traced the Creator's arm working the wonders of his power, and diffusing the gifts of his love—the heat and the light of suns—the necessary elements of physical and intellectual life. It is by means of the gigantic telescope of Lord Rosse that we have become acquainted with the form and character of those great assemblages of stars which compose the sidereal universe. Drawings and descriptions of the more remarkable of these nebulæ, as resolved by this noble instrument, were communicated by Dr. Robinson to the last meeting of the Association; and it is with peculiar satisfaction that I am able to state that many important discoveries have been made by Lord Rosse and his assistant, Mr. Stoney, during the last year. In many of the nebulæ the peculiarities of structure are very remarkable, and, as Lord Rosse observes, "seem even to indicate the presence of dynamical laws almost within our grasp." The spiral arrangement so strongly developed in some of the nebulæ is traceable more or less distinctly in many; but "more frequently," to use Lord Rosse's own words, "there is a nearer approach to a kind of irregular, interrupted, annular disposition of the luminous material, than to the regularity observed in others; but his Lordship is of opinion that those nebulæ are systems of a very similar nature, seen more or less perfectly, and variously placed with reference to the line of sight. In re-examining the more remarkable of these objects, Lord Rosse intends to view them with the full light of his six feet speculum, undiminished by the second reflexion of the small mirror. By thus adopting what is called the *front view*, he will doubtless, as he himself expects, discover many new features in those interesting objects. It is to the influence of Lord Rosse's example that we are indebted for the fine Reflecting Telescope of Mr. Lassels, of which I have already spoken; and it is to it, also, that we owe another telescope, which, though yet unknown to science, I am bound in this place especially to notice. I allude to the reflector recently constructed by Mr. James Nasmyth, a native of this city, already distinguished by his mechanical inventions, and one of a family well known to us all, and occupying a high place among the artists of Scotland. This instrument has its great speculum twenty feet in focal length, and twenty inches in diameter; but it differs from all other telescopes in the remarkable facility with which it can be used. Its tube moves vertically upon hollow trunnions, through which the astronomer, seated in a little observatory, with only a horizontal motion, can view at his ease every part of the heavens. Hitherto, the astronomer has been obliged to seat himself at the upper end of his Newtonian telescope; and if no other observer will acknowledge the awkwardness and insecurity of his position, I can myself vouch for its danger, having fallen from the very top of Mr. Ramage's twenty-feet telescope when it was directed to a point not far from the zenith.

Though but slightly connected with astronomy, I cannot omit calling your attention to the great improvements—I may call them discoveries—which have been recently made in *Photography*. I need not inform this meeting that the art of taking photographic *negative* pictures upon paper was the invention of Mr. Fox Talbot, a distinguished member of this Association. The superiority of the Talbotype to the Daguerreotype is well known. In the latter the pictures are reverted, and incapable of being multiplied; while in the Talbotype there is no reversion, and a single negative will supply a thousand copies, so that books may now be illustrated with pictures drawn by the sun. The difficulty of procuring good paper for the negative is so great, that a better material has been eagerly sought for; and M. Niepce, an accomplished officer in the French service, has successfully substituted for paper a film of albumen, or the white of an egg, spread upon glass. This new process has been brought to such perfection in this city by Messrs. Ross & Thompson, that Talbotypes taken by them and lately exhibited by myself to the National Institute of France, and to M. Niepce, were universally regarded as the finest that had yet been executed. Another process, in which gelatine is substituted for albumen, has been invented, and successfully practised by M. Poitevin, a French officer of engineers; and by an ingenious method which has been minutely described in the weekly proceedings of the Institute of France, M. Edmund Becquerel has succeeded in transferring to a daguerreotype plate the prismatic spectrum, with all its brilliant colors, and also though in an inferior degree, the colors of the landscape. These colors, however, are very fugaceous: yet, though no method of fixing them has hitherto been discovered, we cannot doubt that the difficulty will be surmounted, and that we shall yet see all the colors of the natural world transferred by their own rays to surfaces both of silver and paper. But the most important fact in photography which I have now to mention, is the singular acceleration of the process discovered by M. Niepce, which enables him to take the picture of a landscape illuminated by diffused light, in a single second, or at most in two seconds. By this process he obtained a picture of the sun on albumen so instantaneously, as to confirm the remarkable discovery previously made by M. Arago, by means of a silver plate, that the rays which proceed from the central parts of the sun's disc have a higher photogenic action than those which issue from its margin. This interesting discovery of M. Arago is one of a series on photometry which that distinguished philosopher is now occupied in publishing. Threatened with a calamity which the civilized world will deplore—the loss of that sight which has detected so many brilliant phenomena and penetrated so deeply into the mysteries of the material world, he is now completing, with the aid of other eyes than his own, those splendid researches which will immortalize his own name and add to the scientific glory of his country.

From these brief notices of the progress of science I must now call your attention to two important objects with which the British Association has been occupied since their last meeting. It has been long known both from theory and in practice, that the imperfect transparency of the earth's atmosphere, and the unequal refraction which arises from differences of temperature combine to set a limit to the use of high magnifying powers in our telescopes. Hitherto, however, the application of such high powers was checked by the imperfections of the instruments themselves; and it is only since the construction of Lord Rosse's telescope that astronomers have found that, in our damp and variable climate, it is only during a few days of the year that telescopes of such magnitude can use successfully the high magnifying powers which they are capable of bearing. Even in a cloudless sky, when the stars are sparkling in the firmament, the astronomer is baffled by influences which are invisible; and while new planets and new satellites are being discovered by instruments comparatively small, the gigantic Polyphemus lies slumbering in his cave, blinded by thermal currents more irresistible than the firebrand of Ulysses. As the astronomer, however, cannot command a tempest to clear his atmosphere nor a thunder-storm to purify it, his only alternative is to remove his telescope to some southern climate, where no clouds disturb the serenity of the firmament, and no changes of temperature distract the emanations of the stars. A fact has been recently mentioned, which entitles us to anticipate great results from such a measure. The Marquess of Ormonde is said to have seen from Mount Etna, with his naked eye, the satellites of Jupiter. If this be true, what discoveries may we not expect, even in Europe, from a large reflector working above the grosser strata of our atmosphere? This noble experiment of sending a large reflector to a southern climate has been but once made in the history of science. Sir John Herschel transported his telescopes and his family to the south of Africa, and during a voluntary exile of four years' duration he enriched astronomy with many splendid discoveries. Such a sacrifice, however, is not likely to be made again; and we must, therefore, look to the aid of government for the realization of a project which every civilized people will applaud, and which, by adding to the conquests of science, will add to the glory of our country. At the Birmingham meeting of the Association, their attention was called to this subject; and being convinced that great advantages would accrue to science from the active use of a large reflecting telescope in the southern hemisphere, they resolved to petition government for a grant of money for that purpose. The Royal Society readily agreed to second this application; and as no request from this Association has ever been refused, whatever government was in power, we have every reason to expect a favorable answer to a memorial from the pen of Dr. Robinson, which has just been submitted to the Minister. A recent and noble act of liberality to science on the part of the government justifies this expectation. It is, I believe, not yet generally known that Lord John Russell has granted 1,000*l.* a year to the Royal Society for promoting scientific objects. The Council of that distinguished body has been very solicitous to make this grant effective in promoting scientific objects, and I am persuaded that the measures they have adopted are well fitted to justify the liberality of the government. One of the most important of these has been to place 100*l.* at the disposal of the committee of the Kew Observatory. This establishment, which has for several years been supported by the British Association, was given to us by the Government as a depository for our books and instruments and as a locality well fitted for carrying on electrical, magnetical and meteorological observations. During the last six years the Observatory has been under the honorary superintendence of Mr. Ronalds, who is well known to the scientific world for his ingenious photographic methods of constructing self-registering magnetical and meteorological apparatus. On the joint application of the Marquess of Northampton and Sir John Herschel, Her Majesty's government have granted to Mr. Ronalds a pecuniary recompense of 250*l.* for these inventions; and I am glad to be able to state that Mr. Brooke has also received from them a suitable reward for inventions of a similar kind. Under the fostering care of the

British Association the most valuable electrical observations have been made at Kew, and Mr. Ronalds has continued from year to year to make those improvements upon his apparatus which experience never fails to suggest. But I regret to say, that in consequence of our diminished resources, the Association, at its meeting in 1848, came to the resolution of discontinuing the observations at Kew, appropriating at the same time an adequate sum for completing those which were in progress, and for reducing and discussing the five years' electrical observations which had been published in our Annual Reports. I trust, however, that means will yet be found to maintain the Observatory in full activity, and carry out the original objects contemplated by the Committee. Having had an opportunity of visiting this establishment this summer, after having inspected two of the best conducted observatories on the Continent where the same class of observations are made, I have no hesitation in speaking in the highest terms of the value of Mr. Ronalds's labors, and in recommending the institution which he so liberally superintends to the continued protection of the Association and the continued liberality of the Royal Society. From the facts which I have already mentioned, and from many others to which I might have referred, the members of the Association will observe, with no common pleasure, that the Government of this country has, during the last twenty years, been extending their patronage of Science and the Arts. That this change was effected by the interference of the British Association, and by the writings and personal exertions of its members, could, were it necessary, be easily proved. But though men of all shades of political feeling have applauded the growing wisdom and liberality of the State, and though various individuals are entitled to share in the applause, yet there is one statesman, alas! too early and too painfully torn from the affections of his country, whom the science of England must ever regard as its warmest friend and its greatest benefactor. To him we owe new institutions for advancing science, and new colleges for extending education; and had Providence permitted him to follow out, in the serene evening of life and in the maturity of his powerful intellect, the views which he had cherished amidst the distractions of political strife, he would have rivalled the Colbert of another age, and would have completed the systematic organization of Science and Literature and Art which has been the pride and the glory of another land. These are not the words of idle eulogy, or the expressions of a groundless expectation. Sir Robert Peel had entertained the idea of attaching to the Royal Society a number of active members, who should devote themselves wholly to scientific pursuits; and I had the satisfaction of communicating to him, through a mutual friend, the remarkable fact, that I had found among the MSS. of Sir Isaac Newton a written scheme of improving the Royal Society precisely similar to that which he contemplated. Had this idea been realized, it would have been but the first installment of a debt long due to science and the nation, and it would have fallen to the lot of some more fortunate statesman to achieve a glorious name by its complete discharge.

It has always been one of the leading objects of the British Association, and it is now the only one of them which has not been wholly accomplished, "to obtain a more general attention to the objects of science, and a removal of any disadvantages of a public kind which impede its progress." Although this object is not very definitely expressed, yet Mr. Harcourt, in moving its adoption, included under it the revision of the law of patents and the direct national encouragement of science, two subjects to which I shall briefly direct your attention. In 1831, when the Association commenced its labors, our patent laws were a blot on the legislation of Great Britain; and though some of their more obnoxious provisions have since that time been modified or removed, they are a blot still, less deep in its dye, but equally a stain upon the character of the nation. The protection which is given by statute to every other property in Literature and the fine Arts, is not accorded to property in scientific inventions and discoveries. A man of genius completes an invention, and after incurring great expense, and spending years of anxiety and labor, he is ready to give the benefit of it to the public. Perhaps it is an invention to save life—the life-boat; to shorten space and lengthen time—the railway; to guide the commerce of the world through the trackless ocean—the mariner's compass; to extend the industry, increase the power, and fill the coffers of the State—the steam-engine; to civilize our species, to raise from the depths of ignorance and crime—the printing press. But whatever it may be, a grateful country has granted to the inventor the sole benefit of its use for fourteen years. What the statute thus freely gives, however, law and custom as freely take away, or render void. Fees, varying from 200*l*. to 500*l*., are demanded from the inventor; and the gift thus so highly estimated by the giver, bears the Great Seal of England. The inventor must now describe his invention with legal precision. If he errs in the slightest point—if his description is not sufficiently intelligible—if the smallest portion of his invention has been used before—or if he has incautiously allowed his secret to be made known to two, or even to one individual,—he will lose in a court of law his money and his privilege. Should his patent escape unscathed from the fiery ordeal, it often happens that the patentee has not been remunerated during the fourteen years of his term. In this case the State is willing to extend his right for five or seven years more; but he can obtain this extension only by the expensive and uncertain process of an Act of Parliament,—a boon which is seldom asked, and which through rival influence has often been withheld. Such was the patent law twenty years ago. Since that time it has received some important ameliorations; and though the British Association did not interfere as a body, yet some of its members applied energetically on the subject to some of the more influential individuals in Lord Grey's government,—and the result of this was, two Acts of Parliament passed in 1835 and 1839, entitled "Acts for amending the law touching letters patent for inventions." Without referring to another important Act for registering designs which had the effect of withdrawing from the grasp of the patent laws a great number of useful inventions depending principally on form,—I shall notice only the valuable provisions of the two Acts above mentioned, Acts which we owe solely to Lord Brougham. By the first of these Acts the patentee is permitted to disclaim any part either of the title of his invention, or of the specification of it, or to make any alteration in the title or specification. The same Act gives the Privy Council the power of confirming any patent, or granting a new one, when a patent had been taken out for an invention which the patentee believed to be new, but which was found to have been known before, but not publicly and generally used. By the same Act, too, the power of letters patent was taken from Parliament, and given to the Privy Council, who have, on different occasions, exercised it with judgment and discrimination. By the second Act of 1839 this last privilege was made more attainable by the patentee. These are doubtless valuable improvements, which inventors will gratefully remember; but till the enormous fees which are still exacted are either partly or wholly abolished, and a real privilege given under the great seal, the genius of this country will never be able to compete with that of foreign lands, where patents are cheaply obtained and better protected. In proof of the justness of these views, it is gratifying to notice that, within these few days, it has been announced in Parliament that the new Attorney-General has accepted his office on the express condition that the large fees which he derives from patents shall be subject to revision.

The other object of the British Association, mentioned by Mr. Harcourt, the Organization of Science as a National Institution, is one of a higher order, and not limited to individual, or even to English interests. It concerns the civilized world; not confined to time, it concerns eternity. While the tongue of the Almighty, as Kepler expresses it, is speaking to us in His Word, His finger is writing to us in His works; and to acquire a knowledge of these works is an essential portion of the great duty of man. Truth secular cannot be separated from truth divine; and if a priesthood has in all ages been organized to track and exemplify the one, and to maintain, in ages of darkness and corruption the vestal fire upon the sacred altar, shall not an intellectual priesthood be organized to develop the glorious truths which time and space embosom,—to cast the glance of reason into the dark interior of our globe, teeming with what was once life,—to make the dull eye of man sensitive to the planet which twinkles from afar, as well as to the luminary which shines above, —and to incorporate with our inner life those wonders of the external world which appeal with equal power to the affections and to the reason of immortal natures. If the God of Love is most appropriately worshiped in the Christian temple, the God of Nature may be equally honored in the Temple of Science. Even from its lofty minarets the philosopher may summon the faithful to prayer; and the priest and the sage may exchange altars without the compromise of faith or of knowledge. Influenced, no doubt, by views like these, Mr. Harcourt has cited the opinions of a philosopher whose memory is dear to Scotland, and whose judgment on any great question will be everywhere received with respect and attention; I refer to Prof. Playfair, the distinguished successor in our Metropolitan University of the Gregorys, the Maclaurins, and Stewarts of former days, who in his able dissertation 'On the Progress of the Mathematical and Physical Sciences,' thus speaks of the National Institute of France:—

"This Institution has been considerable advantage to science. To detach a number of ingenious men from every thing but scientific pursuits—to deliver them alike from the embarrassments of poverty and the temptations of wealth—to give them a place and station in society the most respectable and independent, is to remove every impediment, and to add every stimulus to exertion. To this Institution, accordingly, operating upon a people of great genius and indefatigable activity of mind, we are to ascribe that superiority in the mathematical sciences which, in the last seventy years, has been so conspicuous."*

This just eulogy on the National Institute of France, in reference to abstract mathematics, may be safely extended to every branch of theoretical and practical science; and I have no hesitation in saying, after having recently seen the Academy of Sciences at its weekly labors, that it is the noblest and most effective institution that ever was organized for the promotion of Science. Owing to the prevalence of scientific knowledge among all classes of the French population, and to their admirable system of elementary instruction, the advancement of science, the diffusion of knowledge, and the extension of education are objects dear to every class of the people. The soldier as well as the citizen—the Socialist—the Republican and the Royalist—all look up to the National Institute as a mighty obelisk erected to science, to be

* Diss. 3rd, Sec. V, p. 500.

respected and loved and defended by all. We have seen it standing unshaken and active amid all the revolutions and convulsions which have so long agitated that noble but distracted country—a common center of affection, to which antagonist opinions and rival interests and dissevered hearts have peacefully converged. It thus becomes an institution of order, calculated to send back to its contending friends a message of union and peace, and to replace in stable equilibrium the tottering institutions of the State. It was doubtless with views like these that the great Colbert established the Academy of Sciences in Paris, and that the powerful and sagacious monarchs on the continent of Europe have imitated his example. They have established in their respective capitals similar institutions—they have sustained them with liberal endowments—they have conferred rank and honors on their more eminent members, and there are now here present distinguished foreigners who have well earned the rewards and distinctions they have received. It is, therefore, gentlemen, no extravagant opinion that institutions which have thus thriven in other countries should thrive in ours—that insulated societies, which elsewhere flourish in combination, should when combined flourish among us—and that men ordained by the State to the undivided functions of science should do more and better work than those who snatch an hour or two from their daily toil or from their nightly rest. In a great nation like ours, where the higher interests and objects of the State are necessarily organized, it is a singular anomaly that the intellectual interests of the country should, in a great measure, be left to voluntary support and individual zeal—an anomaly that could have arisen only from the supineness of ever-changing administrations, and from the intelligence and liberality of a commercial people—an anomaly, too, that could have been continued only by the excellence of the institutions they have established. In the history of no civilized people can we find private establishments so generously fostered, so energetically conducted, and so successful in their objects as the Royal Societies of London, Edinburgh and Dublin, and the Astronomical, Geological, Zoological and Linnean Societies of the metropolis. They are an honor to the nation, and will ever be gratefully remembered in the history of science. But they are nevertheless defective in their constitution, limited in their operation, and incapable, from their very nature, of developing and directing and rewarding the indigenous talent of the country. They are simply subscription societies, which pay for the publication of their own transactions, and adjudicate medals intrusted to them by the beneficence of others. They are not bound to the exercise of any other function, and they are under no obligation to do the scientific work of the State, or to promote any of those national objects which are intrusted to the organized institutions of other lands. Their President and Council are necessarily resident in London, and the talent and the genius of the provinces are excluded from their administration. From this remark we must except the distinguished philosophers of Cambridge and Oxford, who, from their proximity to the capital, have been the brightest ornaments of our metropolitan institutions, and without whose aid they could never have attained their present pre-eminence. It is, therefore, in the more remote parts of the empire that the influence of a national institution would be more immediately felt, and nowhere more powerfully than in this its northern portion. Our English friends are, we believe, little aware of the obstructions which oppose the progress of science in Scotland. In our five universities there is not a single Fellowship to stimulate the genius and rouse the ambition of the student. The church, the law and the medical profession hold out no rewards to the cultivators of mathematical and physical science; and were a youthful Newton or Laplace to issue from any of our universities, his best friends would advise him to renounce the divine gift and to seek in professional toil the well-earned competency which can alone secure him a just position in the social scale and an enviable felicity in the domestic circle. Did this truth require any evidence in its support, we find it in the notorious fact that our colleges cannot furnish professors to fill their own important offices; and the time is not distant when all our chairs in Mathematics, Natural Philosophy, and even Natural History, will be occupied by professors educated in the English universities. But were a Royal Academy or Institute, like that of France, established on the basis of our existing institutions, and a class of resident members enabled to devote themselves wholly to science, the youth of Scotland would instantly start for the prize, and would speedily achieve their full share in the liberality of the State. Our universities would then breathe a more vital air. Our science would put forth new energies, and our literature might rise to the high level at which it stands in our sister land. But it is to the nation that the greatest advantages would accrue. With gigantic manufacturing establishments, depending for their perfection and success on mechanics and chemistry,—with a royal and commercial marine almost covering the ocean,—with steam ships on every sea,—with a system of agriculture leaning upon science as its mainstay,—with a net-work of railways demanding for their improvement, and for the safety of the traveler, and for the remuneration of their public-spirited projectors, the highest efforts of mechanical skill,—the time has now arrived for summoning to the service of the State all the theoretical and practical wisdom of the country,—for rousing what is dormant, combining what is insulated, and uniting in one great institution the living talent which is in active but undirected and unsupported exercise around us.

In thus pleading for the most important of the objects of the British Association, I feel that I am not pleading for a cause that is hopeless. The change has not only commenced but has made considerable progress. Our scientific institutions have already to a certain extent become national ones. Apartments belonging to the nation have been liberally granted to them. Royal medals have been founded, and large sums from the public purse devoted to the objects which they contemplate. The Museum of Economic Geology, indeed, is itself a complete section of a Royal Institute, giving a scientific position to six eminent philosophers, all of whom are distinguished members of this Association. And in every branch of science and literature the liberality of the Crown has been extended to numerous individuals whose names would have been enrolled among the members of a National Institution. The cause, therefore, is far advanced; and every act of liberality to eminent men, and every grant of money for scientific and literary purposes, is a distinct step towards its triumph. Our private institutions have in reality assumed the transition phase; and it requires only an electric spark from a sagacious and patriotic statesman to combine in one noble phalanx the scattered elements of our intellectual greatness, and guide to lofty achievments and glorious triumphs the talents and genius of the nation. But when such an institution has been completed, the duties of the State to science are not exhausted. It has appreciated knowledge but in its abstract and utilitarian phase. It would be of little avail to the peace and happiness of society if the great truths of the material world were confined to the educated and the wise. The organization of science thus limited would cease to be a blessing. Knowledge secular and knowledge divine—the double current of the intellectual life-blood of man—must not merely descend through the great arteries of the social frame. It must be taken up by the minutest capillaries before it can nourish and purify society. Knowledge is at once the manna and the medicine of our moral being. When crime is the bane, knowledge is the antidote. Society may escape from the pestilence and may survive the famine; but the demon of Ignorance, with its grim adjutants of vice and riot, will pursue her into her most peaceful haunts, destroying our institutions, and converting into a wilderness the paradise of social and domestic life. The State has, therefore, a great duty to perform. As it punishes crime, it is bound to prevent it. As it subjects us to laws, it must teach us to read them; and while it thus teaches, it must teach also the ennobling truths which display the power and the wisdom of the great Lawgiver,—thus diffusing knowledge while it is extending education,—and thus making men contented and happy and humble while it makes them quiet and obedient subjects. It is a great problem yet to be solved, to determine what will be the state of society when man's physical powers are highly exalted, and his physical condition highly ameliorated, without any corresponding change in his moral habits and position. There is much reason to fear that every great advance in material civilization requires some moral and compensatory antagonism; but however this may be, the very indeterminate character of the problem is a warning to the rulers of nations to prepare for the contingency by a system of national instruction which shall either reconcile or disregard those hostile influences under which the people are now perishing for lack of knowledge.

ART. XXV.—*On the proper Height of Lightning Rods;* by ELIAS LOOMIS, Professor of Mathematics and Natural Philosophy in New York University.

Read before the American Association for the Advancement of Science at New Haven, August, 1850.

THE rule prescribed by the French Academy of Science (and copied into almost all works on electricity,) for determining the proper height of a lightning rod is that a rod will protect a circle whose radius is equal to twice the height of the rod. A case recently occurred in Tallmadge, Summit County, Ohio, which appears to demonstrate that this rule is unsafe.

On the afternoon of July 27th, about six o'clock, there was a slight shower of rain accompanied by a few flashes of lightning. One flash was remarkably vivid, and was succeeded almost instantly by a loud report. In an instant afterwards, a large pile of shavings lying on the west side of a carriage shop was found in full blaze. The shavings had recently been carried out of the shop and were quite dry; and as no fire had been used in that vicinity for several weeks, and no other mode is known in which the shavings could have been ignited, it is inferred to have been caused by the electric discharge. The carriage shop was furnished with a lightning rod, and it was a matter of surprise that the fluid should have struck the ground so near to the rod. The top of the rod was fifty-nine feet high above the shavings; and the shavings were one hundred feet distant from a point vertically under the top of the rod. According to the rule above quoted, this rod should have afforded complete protection to a distance of one hundred and eighteen feet from its base; whereas the shavings were struck at a distance of one hundred feet, and that too where, being elevated only a few inches above the general level of the ground, they might be presumed to afford no peculiar attraction for the lightning.

This rod appears to have been constructed in accordance with the usual rules. It is terminated by three points which are gilded and appear to be in tolerably good condition. About ten feet from the top is a break in the rod and the two portions are looped together. From this point the rod is continuous to the bottom and enters the ground to the depth of about three feet where the earth at the time referred to was quite moist. The rod is about five-eighths of an inch in diameter.

The preceding case demonstrates to my mind that it is unsafe to rely upon a rod to protect a circle whose radius is more than once and a half the height of the rod, at least upon the west side, being that from which thunder showers generally come in this latitude.

ART. XXVI.—*On the Electrical Phenomena of Certain Houses;* by ELIAS LOOMIS, Prof. of Mathematics and Natural Philosophy in New York University.

Read before the American Association for the Advancement of Science at New Haven, August, 1850.

WITHIN the past few years, several houses in the city of New York have exhibited electrical phenomena in a very remarkable degree. For months in succession they have emitted sparks of considerable intensity, accompanied by a loud snap. A stranger on entering one of these electrical houses, in attempting to shake hands with the inmates, receives a shock which is quite noticeable and somewhat unpleasant. Ladies in attempting to kiss each other are saluted by a spark. A spark is perceived whenever the hand is brought near to the knob of a door—the gilded frame of a mirror—the gas pipes—or any metallic body—especially when this body communicates freely with the earth. In one house which I have had an opportunity to examine, a child in taking hold of the knob of a door received so severe a shock that it ran off in great fright. The lady of the house in approaching the speaking tube to give orders to the servants, received a very unpleasant shock in the mouth, and was very much annoyed by the electricity until she learned first to touch the tube with her finger. In passing from one parlor to the other, if she chanced to step upon the brass plate which served as a slide for the folding doors, she received an unpleasant shock in the foot. When she touched her finger to the chandelier (the room was lighted with gas by a chandelier suspended from the ceiling) there appeared a brilliant spark and a snap as in the discharge of a Leyden Jar of good size. In many houses the phenomena have been so remarkable as to occasion general surprise and almost alarm.

After a careful examination of several cases of this kind, I have come to the conclusion that the electricity is excited by the friction of the shoes of the inmates upon the carpets of the house.

I have proved by direct experiment that electricity is excited by the friction of leather upon woolen cloth. For this purpose I stood upon an insulating stool, and spreading a small piece of carpeting upon a table before me, rubbed a piece of leather vigorously upon it, and then bringing the leather near the cap of a gold leaf electrometer, the leaves were repelled with great violence. The electricity of the leather was of the resinous kind. Electricity therefore must necessarily be excited whenever a person walks with a shuffling motion across a carpet; but it may be thought remarkable that the electricity should be intense enough to give a bright spark. In order to produce this effect there must be a combination of some favorable circumstances.

1. The carpet, or at least its upper surface, must be entirely of wool, and of a close texture, in order to furnish an abundance of electricity. So far as I have had opportunity to judge, I infer that heavy velvet carpets answer this purpose best. Two thicknesses of Ingrain carpeting answer very well. A drugget spread upon an Ingrain carpet yields a good supply of the fluid. The effect of the increased thickness is obviously to improve the insulation of the carpet.

2. The carpet must be quite dry, and also the floor of the room, so that the fluid may not be conveyed away as soon as it is excited. This will not generally be the case except in winter, and in rooms which are habitually kept quite warm. The most remarkable cases which I have heard of in New York have been of close, well-built houses, kept very warm by furnaces, and the electricity was most abundant in very cold weather. In warm weather, only feeble signs of electricity are obtained.

3. The rubber, that is the shoe, must also be dry, like the carpet, and it must be rubbed upon the carpet somewhat vigorously. By skipping once or twice across a room with a shuffling motion of the feet, a person becomes highly charged, and then upon bringing the knuckle near to any metallic body, particularly if it have good communication with the earth, a bright spark passes. In almost any room which is furnished with a woolen carpet, and is kept tolerably warm, a spark may thus be obtained in winter—but in some rooms, the insulation is so good and the carpets are so electrical, that it is impossible to walk across the floor, without exciting sufficient electricity to give a spark.

It may be said that in this case there can be but very little friction between the shoe and carpet. But it must be remembered that the rubber is applied to the carpet with considerable force, being aided by the whole weight of the body, so that a slight shuffling of the feet acts with great energy.

In the London and Edinburgh Philosophical Magazine for February, 1839, is given an account of a leather strap connecting the drum of a Worsted Mill, which gave sparks two inches in length, and charged a battery in a short time. The strap was twenty-four feet long, six inches broad, and one eighth of an inch thick. It crossed in the middle between the two drums, the strap forming a figure eight. Here then was considerable friction, since the strap made one hundred revolutions in a minute.

In the American Journal of Science for July, 1840, is mentioned an instance of a leather band in a cotton factory, which exhibited strong electrical excitement.

These examples show that leather when subjected to considable friction yields an abundant supply of electricity.

In the Proceedings of the American Philosophical Society for December, 1840, are mentioned several cases of individuals who drew sparks of electricity from a coal stove, and from a common grate. I consider it probable that in those cases the experimenter was the electrified body, and not the stove or grate. How is it possible for a grate containing burning coals, to be insulated, so as to retain a charge of electricity? On the other hand, it is presumed that the experimenter was insulated by standing upon a carpet made quite dry by a winter fire.

XXVII.—*On a new method of decomposing Silicates in the process of Analysis*; by Henry Wurtz, of New York.

Read before the American Association for the Advancement of Science, at New Haven, August, 1850.

Having had occasion in the course of some researches upon the greensand of New Jersey, which will be presented to the Association hereafter, to observe the facility with which that substance is decomposed by fusion with chlorid of calcium, it occurred to me that this property of the earthy chlorids might be applicable in the analysis of minerals.

Some experiments were accordingly made with reference to this supposition. Feldspar and hornblende were fused with chlorid of calcium, and it was found that the masses thus formed could be entirely decomposed by hydrochloric acid. The use of chlorid of calcium is however obviously attended with several inconveniences, such as its deliquescent properties, and the unavoidable introduction of a large quantity of ammoniacal salts into the solution, in the separation of the lime from it.

Chlorid of barium was therefore substituted, and the results of experiments upon this reagent were completely successful.

The chlorid of barium of commerce often contains lead, resulting probably from the leaden vessels in which it is crystallized. It is necessary in this case, to pass sulphuretted hydrogen through its solution. This solution is then filtered, recrystallized, and the washed crystals dried on the sandbath and ignited to drive off the water of crystallization. The pure chlorid of barium thus obtained is pulverized and is then ready to be used for the purpose here proposed.

Chlorid of barium may be fused in a platinum crucible by a blast lamp, or by an alcohol blowpipe lamp. A mixture of chlorid of barium and chlorid of strontium in atomic proportions fuses however far more easily than either of its ingredients. Such a mixture is fused by the heat of an ordinary Berzelius lamp, and more easily, I think, than carbonate of soda. This is analogous to the well known fact that a mixture of carbonate of soda with carbonate of potassa fuses more easily than either of the alkaline carbonates by itself. I have found moreover that although sulphate of strontia when precipitated by itself, appears in a form somewhat gelatinous, tedious to wash and difficult to filter, yet when precipitated in the presence of sulphate of baryta, it takes on the finely granular form of the latter, and the combined sulphates are as easily washed as the sulphate of baryta when precipitated alone.

The atomic proportions of a mixture of chlorid of barium and chlorid of strontium, suitable for the fusion of silicates, are about four parts of the former to three of the latter.

The best mode of proceeding was found to be as follows:—

The mineral in fine powder is intimately mixed with four or five times its weight of chlorid of barium, or of the mixture of chlorid of barium and chlorid of strontium, in a platinum crucible, which is then covered, and exposed to a heat sufficient to fuse the mass for twenty or thirty minutes. When cool, the mass is loosened by bending the crucible, and allowed to fall into a beaker glass. Water is then poured on it in sufficient quantity to dissolve the excess of chlorid of barium, or chlorid of strontium, and the undissolved portion allowed to subside to the bottom of the beaker. The clear solution is then poured off into a porcelain dish, and concentrated hydrochloric acid is poured upon the residue in the beaker. The precaution of removing most of the earthy chlorids by means of water before adding the acid, is necessary on account of the difficulty of decomposing the mass by the direct affusion of strong hydrochloric acid, owing to the insolubility of the chlorids of barium and strontium in this acid.

After the application of heat to the beaker until the residue contained in it is decomposed by the acid, its contents are also transferred to the porcelain dish, and the whole contents of the latter evaporated to dryness in the usual manner, for the separation of the silica. The filtrate from the silica is precipitated while hot with a slight excess of sulphuric acid, kept hot for half an hour, and then filtered. The filtrate from the sulphate of baryta, must contain all the constituents of the mineral except the silica, and these may be determined by the ordinary methods.

When the mineral thus treated contains sulphuric acid, it will evidently remain with the silica in the form of sulphate of baryta. A difficulty would also seem to occur when the mineral contains much lime, on account of the insolubility of its sulphate. This last difficulty is however obviated, in some degree, by the very considerable solubility of sulphate of lime in hydrochloric acid, a fact which must have been noticed by many chemists.

The first qualitative experiment was made with a specimen of colorless transparent orthoclase from New York island. This feldspar was fused with chlorid of barium, according to the above process, and the silica thus obtained was found, upon fusion with carbonate of soda, to be perfectly pure.

The next experiment was made upon a black crystallized hornblende, from Franconia, New Hampshire. The silica obtained from this was found to contain considerable more than a trace of iron. I do not therefore venture to recommend this proces at present for minerals which contain a very large quantity of oxyd of iron. Many more experiments which time has not permitted me to make, will soon be made to settle this question, which I do not consider yet determined.

To test the chlorid of barium process quantitatively, the mineral called pink scapolite, of Bolton, Mass., long ago analyzed by Dr. Jackson, was selected. My results agree entirely with his, except as regards the presence of lithia and oxyd of cerium, which careful qualitative examination did not enable me to detect.

This mineral was found, contrary to recorded statements, to be not completely decomposed by concentrated acids, even when finely elutriated. A determination of the silica, made by decomposition with hydrochloric acid, gave the per-centage 50·25; another, made by fusion with carbonate of soda, gave 47·55.

Two determinations of the specific gravity, made upon the identical portion of mineral which was analyzed, dried at 212° in coarse powder, gave the numbers 2·7002 and 2·7046.

The results of a fusion with chlorid of barium were as follows:

		Oxygen.	
Silica, ...	47·67	24·78	24·78
Alumina, . . .	25·75	12·035	12·71
Peroxyd of iron, . .	2·26	·68	
Lime, ...	17·31	4·82	7·08
Soda, ...	7·76	2·26	
Protoxyd of manganese,	*trace.*		
	100·77		

Agreeing nearly with the received formula of scapolite—

$$(\dot{Ca}\ \dot{Na})^3\ \dddot{Si}^2+2\ddot{\overline{Al}}\ \dddot{Si},$$

which requires for the oxygen of the protoxyds, peroxyds and silica, the ratio 1 : 2 : 4, while the above analysis gives the ratio 1·14 : 2·06 : 4.

This method appears to possess advantages for decomposing silicates which contain both the alkalies, over the ordinary methods of fusion with the hydrate or with the carbonate of baryta. Hydrate of baryta generally acts upon the crucible, causing the mass to adhere to it; and upon the affusion of hydrochloric acid, any potash which the mineral may contain, consequently enters into combination as bichlorid of platinum and potassium, and remains with the silica. The carbonate of baryta process requires an intense heat, and is difficult of execution.

The chlorid of barium process proposed in this paper is probably not more laborious than an ordinary carbonate of soda fusion, and is applicable in cases in which the silicate contains both potash and soda.

Art. XXVIII.—*On the availability of the Greensand of New Jersey as a source of Potash and its Compounds;* by Henry Wurtz, of New York City.

Read before the American Association for the Advancement of Science, at New Haven, August, 1850.

The vast importance of potash and its compounds in the arts, long ago impressed upon chemists the necessity of finding some source of these compounds other than the ashes of the forests.

Accordingly, a great multitude of experiments have been made upon feldspar, by various chemists, in the hope of attaining this important end, but owing to the peculiar chemical and physical properties of feldspar, the success of these researches has been doubtful.

It is true that the feeble affinities exerted by mineral waters impregnated with carbonic acid, and even those of pure water itself, acting through hundreds of years, have been found sufficient to break down the constitution of this hard silicate, thus giving rise to vast beds of kaolin, but when it is attempted to effect the same thing within any reasonable time, by artificial means, it is found necessary to bring into play temperatures and forces too enormous in amount to be economical in practice.

I have not been able to ascertain that any one has yet proposed to use, as a means of obtaining the end under consideration, the vast deposits of the potash mineral called *Greensand*, which exist in the United States, and it is the object of the present paper to show that this substance is far superior to feldspar in its adaptation to this purpose.

The existence and common attributes of the greensand are generally known, but a few words on this subject will not be amiss. It exists in greater or less quantity in several States, but has its greatest development, I believe, in the State of New Jersey, where it forms a stratum of variable thickness, covering a great portion of the counties of Monmouth, Burlington, Gloucester and Salem. Wherever it occurs, it is spread upon the land in large quantities as a fertilizer, and is a source of wealth to the farmers, to whom it is known by the name of *Marl*. Its properties as a fertilizer are undoubtedly owing to the ease with which the potash which it contains is abstracted from it by atmospheric agencies, as is suggested by Prof. Henry D. Rogers, in his Report upon the Geology of New Jersey, in which he has devoted about a hundred pages to the greensand, and has given many analyses which indicate the presence of from 10 to 13 per cent. of potash. No analyst has, to my knowledge, found potash in the English greensand, the fertilizing properties of which appear to be due to the presence of phosphates.

The external appearances of the New Jersey *marl* are very various. Some varieties are almost entirely composed of grains of greensand, others contain variable proportions of a red or brown earth and of quartz. A few contain more or less carbonate of lime in such a form that it is not acted upon by dilute acids in the cold, although upon the application of heat a violent effervescence appears. Many contain iron pyrites and some a trace of sulphate of iron. No variety which I have examined has yielded any phosphoric acid.

The greensand grains themselves contain, besides potash, silica, alumina, one or the other or both of the oxyds of iron, and water, with sometimes a little magnesia.

The invariable development of a smell of formic acid by the action of strong sulphuric acid upon them, seems to indicate the presence of a little organic matter. Analyses of two varieties of the marl from the estate of the late Alfred Bishop of Bridgeport, at Shrewsbury, Monmouth County, yielded the following results:

	I.	II.
Silica,	48·24	47·83
Alumina and oxyd of iron, principally peroxyd,	32·89	34·98
Potash,	6·38	4·94
Magnesia,	2·60	
Hydroscopic water,	4·81	11·50
Combined water,	5·69	
	100·61	99·25

It may be remarked here that Prof. Rogers's results were obtained upon the greensand grains separated as much as possible from intermixed earth and sand, while the above were made upon the impure marl itself.

For more detailed information I must refer to the elaborate report of Prof. Rogers before mentioned. I will only state further that the greensand grains are easily pulverized, having only about the hardness of gypsum, and that they are decomposed by dilute acids, and we then come to the immediate subject of this paper.

Considering that the greensand contains the constituents of *alum*, with the exception of the sulphuric acid, it seemed probable that by the action of sulphuric acid upon it, a solution would be formed containing more or less alum. Many experiments were made and much time was spent upon this point without much success. It seemed as if the protoxyd of iron was dissolved by the acid much more readily than any other constituent of the marl, and also that the organic matter interfered in some way. The solutions obtained had generally a dark brown color and a smell resembling that of formic acid. They contained much sulphate of protoxyd of iron, and gave a few impure crystals of alum. A portion of the greensand was next gently ignited, which served the purpose of destroying the organic matter, if any was present, and also of peroxydizing the iron, thus rendering it less soluble in acids. The pulverized and ignited marl presented the appearance of a brownish red powder. It was easily decomposed by dilute sulphuric acid, yielding a solution, the contents of which, upon analysis, proved to be principally common alum, together with small quantities of iron-alum and of the persulphates of alumina and iron. The first crystals of alum obtained from a considerable mass of the solution were almost perfectly pure, and upon the addition of a small quantity of chlorid of potassium to the solution, it was found, as might have been predicted, that all the iron was converted into the uncrystallizable perchlorid, the sulphate of potash thus formed by double decomposition combining with the free sulphate of alumina to form common alum; and even in the last crops of crystals now obtained little or no iron could be detected. The manufacture of alum, therefore, by the action of sulphuric acid upon previously ignited greensand marl, promises to be successful beyond all anticipation. It is obvious that it will be necessary to select varieties of the marl as free as possible from lime and magnesia, which would cause a waste of acid.

My researches were next directed towards the production of chlorid of potassium. Attempts to form this substance by the direct action of hydrochloric acid upon the ignited marl were unsuccessful. A very large quantity of perchlorid of iron was formed, which would give rise to too great a loss of acid.

It was next found that by fusing together greensand and chlorid of sodium at a red heat, a hard mass was formed, which yielded with water a solution containing potash; but owing to the difficulty of separating chlorid of potassium from chlorid of sodium when the latter is present in greater quantity than the former, this observation was considered of little value.

A widely different conclusion was arrived at, when chlorid of calcium was substituted for chlorid of sodium. The pulverized and ignited marl was mixed with a sufficient quantity of chlorid of calcium to form upon the fusion of the latter, a pasty mass. The decomposition of the greensand takes place in this case at a low temperature, and is so complete that I have founded upon this circumstance a method of decomposing minerals in the process of analysis, which I have had the honor of presenting to the Association before.

It is evident that the combined water of the greensand must be expelled by ignition previous to fusing it with chlorid of calcium, otherwise a quantity of the fused chlorid of calcium will inevitably be decomposed by the steam evolved. The fusion may also be performed in close vessels to avoid the decomposition which chlorid of calcium undergoes when fused in contact with the air. The mass, after fusion, falls to pieces in water, yielding to this solvent, in most cases, all the potash which was contained in the greensand employed in the form of chlorid of potassium. The separation of this from the excess of chlorid of calcium is an easy problem, owing to the difference between their solubilities.

This application of the chlorid of calcium will open a market for the large quantities of this substance which are thrown away in some manufactories of soda-ash.

All attempts to procure sulphate of potash by the fusion of various sulphates with the greensand were unsuccessful. In fact, the greensand itself, at a temperature below the fusing points of the sulphates of lime and magnesia, fuses to a black glass which is no longer decomposable by acids.

A great number of other experiments were made upon the greensand, but no results were arrived at, which promise to be of any practical value except the above.

A very great number of experiments were also made, having for their object to obtain sulphate of potash by fusing together chlorid of potassium with alum and with various sulphates, such as those of iron, magnesia and zinc, which gave results of great practical value, but as these researches had but a fortuitous connection with those upon the greensand, I shall not introduce an account of them here.

I will merely remark that if sulphate of potash can be obtained by fusing together alum and chlorid of potassium, both of these being obtained economically from the greensand by the above processes, it is evident that this sulphate of potash may be treated in the same manner for the production of potash as sulphate of soda is in the manufacture of soda-ash, and it seems to me that the desideratum of another source of potash is thus supplied.

Art. XXIX.—*On the Diurnal and Annual Variations in the Declination of the Magnetic Needle, and in the Horizontal and Vertical Magnetic Intensities;* by Prof. W. A. Norton.

In my paper on the Diurnal Variations of the Magnetic elements, published in a former No. of this Journal,* I showed that the variations of the horizontal magnetic intensity which lie between the hours of 10 P. M. and 10 A. M. of the following day proceed *pari passu*, and are undoubtedly in some way physically connected with the variations that take place during the same interval of time in the quantity of moisture immediately at the earth's surface:—or at all events that the deviations from the general law of proportionality to temperature that occur during this interval are effects, direct or indirect, of the deposition of dew during the night, and evaporation of moisture during the morning hours. I accounted for the connection subsisting between such dissimilar phenomena by assuming that particles of water in contact with the earth's surface, (and possibly in the vaporous state,) had a direct magnetic action upon the needle, in accordance with the general theory that I had advanced. This is the simplest assumption that can be made in the case, but it is to be observed that the connection in question may possibly be attributable to some indirect effect of deposition and evaporation; as, for example, the disturbance of the electric equilibrium, which, as is well known, is attendant upon these phenomena, or the varying conduction of electrical currents, or some other cause. For the present, however, it is most philosophical to abide by that view which gives us the highest generalization—which represents, at the same time, the normal state of the earth's magnetism and its periodical variations.

It is true that we have no authority, derived from experiment, for supposing water to have a magnetic action, as the term magnectic is generally understood. It has only been established, by direct experiment, that water detached from the general mass of the earth, has what is called a diamagnetic action. But our first aim in such inquiries should be to obtain the highest generalization possible, from the discussion of the phenomena merely. The reconciliation of this generalization with the results of experiment is to be reserved for subsequent inquiry. Observation and experiment are here two independent paths converging to the same great truth. I would also remark, with regard to the meteorological discussion, incidentally entered into in the paper above referred to, concerning the thermal effect of dew, that this effect may perhaps have been overrated, and that the law of the

* See this Journal, ii ser., vol. viii, p. 35.

nocturnal diminution of temperature may be partially attributable to the unequal cooling action of ascending currents of air, continuing during a portion of the night. Although the fact that this law obtains in the calmest nights, and the assertion so often repeated by meteorologists that in clear calm nights the temperature of the soil falls many degrees below that of the air a few feet above it, would seem to render such a supposition inadmissible. But, whatever view may be taken of the relative part performed by the dew in determining the law of the nocturnal loss of temperature, it is to be observed that it suffices for the explanation of the connection subsisting between this law and that of the nocturnal variations of the horizontal magnetic intensity; since it must be admitted that the tendency of the thermal influence of dew is to produce an inequality of loss of temperature of the same kind with that which actually obtains. However, if the inequality in the nocturnal decrease of temperature be not entirely attributable to the deposition of dew, it must then be admitted that the cause which coöperates meteorologically with the dew, may possibly also coöperate magnetically with it in determining the law of the nocturnal variations of the horizontal force.

Having made these explanations and qualifying statements with reference to my former memoir, I propose now to show, from another point of view, the high probability of the truth of the explanation which I have there given of the diurnal variations of the horizontal force; and subsequently to discuss the annual variations of the magnetic elements.

A short time previous to the date of the publication of that memoir, I was led to make a comparison between the curves showing the diurnal variations of the horizontal force, (as given in the Report of the Meteorological and Magnetical Observations, made at the Girard College Observatory, 1840 to 1845,) with the curves showing the diurnal variations of the height of the barometer; and noticed that the following remarkable relations subsisted between them. *They each have two maxima and two minima, and the maxima of the one set of curves occur at very nearly the same hours as the minima of the other.* The same relations may be observed in the following tabular statement.

Hours of Daily Maxima and Minima of Horizontal Force and Barometer, at Philadelphia, for the year 1844.

	Horizontal Force.	Barometer.
Prin. Maximum,	3 to 4 P. M.	9 A. M.
Prin. Minimum,	10 A. M.	4 P. M.
Sec. Maximum,	5 A. M.	Midnight.
Sec. Minimum,	11 P. M. to Midnight.	4 A. M.

It will be seen that the hours of maxima and minima of the barometer differ by not more than one hour from the hours of minima and maxima of the horizontal force. If we make the comparison for other years, and also for the quarters of years, we find a similar approximate correspondence.

If we descend to a minute comparison, we find that the intervals between the precise hours of maxima and minima in some instances amount to as much as two hours; in fact that the secondary or morning minimum of the barometer sometimes precedes the secondary maximum of horizontal force by three or four hours. If it be imagined that the existence of so large occasional differences between the times of maxima and minima renders it improbable that there is any physical connection between the diurnal variations of the barometer and the diurnal variations of the horizontal force, it is to be observed that the variations both of barometer and horizontal force are comparatively small during the night, and also in general about the times of maxima and minima; and therefore that such comparatively small differences may be expected to subsist, unless the two phenomena be supposed to be identical in their origin. If the times of the maxima of the one element were always precisely the same as the times of the minima of the other, then it would seem highly probable that the diurnal variations of the horizontal force are in some way directly dependent upon the diurnal variations of the pressure of the air; as it is, the more probable conclusion is, that these two different phenomena are two different effects or consequences of the same meteorological phenomenon.

When I had arrived at this point in the progress of my investigations, it at once occurred to me that, as the diurnal variations of the horizontal force had been explained by referring them to the daily changes in the temperature and humidity of the earth's surface, the diurnal variations of the barometer were probably attributable to daily changes in the temperature and humidity of the air. It was seen that the same evaporation by day which tended to diminish the horizontal force, would tend, by adding to the quantity of vapor in the air, to augment the height of the barometer, and that the same condensation of vapor at the earth's surface at night, which tended to increase the horizontal force, would, by diminishing the quantity of vapor in the air, tend to make the barometer fall: also that if these tendencies, in conjunction with those due to variations of temperature, are the actual producing causes of the diurnal variations of the barometer and horizontal force, there would doubtless be an approximate correspondence between the maxima of the one element and the minima of the other. It is somewhat curious that I should have been conducted in this indirect manner, to the explanation of the daily variations of the barometer, which, as I have since found, has been conclusively established, to be the true explanation of this phenomenon, by direct observation. This is known to meteorologists as Dove's theory of the diurnal variations of the barometer. This theory, as stated by Kaemtz, is that the pressure of the atmosphere is equal to the sum of the pressures of dry air and aqueous vapor, and thus the barometric column is composed, so to speak, of two part. ; one of which corresponds to the air; the other to the aqueous vapor. Now when the temperature rises the density of the air diminishes, but the tension of the vapor augments, and *vice versa.* To bring his theory to the test of figures, "he analyzed a set of observations made by Neuber at Apenrade with a Daniell's hygrometer. He calculated the tension of the vapor for each hour of the day, and subtracted it from the barometric column; he thus obtained the pressure of dry air, and found that it had but one maximum and one minimum, each day:"—the former, (at a mean for the year,) occurring at 1 A. M., and the latter at 2 P. M. Kaemtz adduces his own calculations, from other observations, as evidence against the theory of Professor Dove. But the results obtained by several other observers tend to confirm this theory. The following result was obtained from a discussion of the meteorological observations made at the Toronto Observatory, during the years 1841 and 1842. "The diurnal pressure of the gaseous atmosphere has one maximum which occurs about the coldest hour of the day, and one minimum which occurs about the warmest hour of the day. The elastic force of the vapor has also one maximum which occurs at 2 P. M., and one minimum at 4 A. M. The sum of these two pressures however exhibits two daily maxima, viz., at 10 A. M. and P. M., and two daily minima, viz., about 3 or 4 A. M. and P. M. Thus this knotty question respecting the diurnal oscillations of the barometer has been beautifully resolved by simply interrogating nature." A similar conclusion may be drawn from the hourly meteorological observations made during the year 1842 at the following places in Russia, viz., St. Petersburg, Barraoul, Catharinenburg, and Sitka.* It is true that small irregularities are noticed, but as the same general law appears to exist everywhere, it is to be supposed that such irregularities supervene upon it at particular localities by reason of certain local peculiarities.

That the explanation of the diurnal variations of the barometer, upon Dove's theory, corresponds precisely to that which I have given of the diurnal variations of the horizontal force will be readily seen. In the morning, as the temperature rises the pressure of dry air diminishes, but the tension of the vapor which forms over-compensates this effect, and thus the barometer rises. This continues until about 9 A. M. After this the diminution in the pressure of dry air prevails over the increase due to the aug-

* See this Journal for January, 1846, pp. 138, 139, 140.

mented quantity of vapor, and the barometer falls until 3 or 4 P. M. When the temperature begins to fall the barometer also descends by reason of the increase in the density of the air: but in the evening, when a portion of the atmospheric vapor begins to fall in dew, a tendency to a diminution of the barometric pressure arises, but it is not until about midnight that this tendency begins to prevail over the tendency to an increase. From that time the barometer falls, from this cause, until towards the hour of minimum temperature.

The explanation which I have before given of the diurnal variations of the horizontal force is, in substance, as follows. In the morning as the temperature rises the molecular magnetic force increases, and therefore the horizontal force tends to increase, but the diminution arising from the evaporation going on at the earth's surface over-compensates this tendency, and hence the horizontal force, on the whole, decreases. This continues until about 10 A. M. After this the increase, from the rise of temperature, prevails over the diminution produced by the continued evaporation* until about 4 P. M. From that time the horizontal force decreases with the temperature until about 11 P. M.; when the tendency to increase resulting from the deposition of dew begins to prevail over the opposite tendency resulting from the falling of the temperature. This second augmentation continues until about 5 A. M.

In view of what has now been stated it will, I think, be admitted that the diurnal variations both of the horizontal magnetic force and of the barometer are in all probability certain effects resulting from the joint operation of the same two general antagonistic causes—viz., variations of temperature and variations of humidity. The theory of the variations of the horizontal force which I have advanced is in accordance with the fact of the apparent connection subsisting between these phenomena. Other theories may perhaps be devised, equally in accordance with this singular fact; and indeed it must be conceded that it is strongly suggestive of the idea that the cause of the variations of the horizontal force, like that of the variations of the barometric column, must subsist in the atmosphere. In fact if it be admitted that the particles of the atmosphere and of the atmospheric vapor have a magnetic action upon the needle, like those of the solid mass of the earth, then it is a simple consequence of the principles of the general theory under consideration that the horizontal force will vary, by reason of this action, after the manner in which it is observed to do. For, the greater portion of the atmosphere being posited above the needle, the tendency of its

* It is to be observed that we are here speaking of the average state of things in the course of a year, or quarters of a year.

action will be to diminish the horizontal force; and therefore when the number of particles of humid air increases immediately above the needle and the barometer falls, the horizontal force will diminish. Whether the atmosphere has really any effect, and if so, what portion of the observed changes is due to it, and what to the action of the varying temperature and humidity of the surface of the ground, cannot, perhaps as yet be established with certainty. The occasional occurrence in the atmosphere of the luminous magnetic phenomenon, called the Aurora Borealis,—a phenomenon which, (as I think may be clearly shown,) is also dependent upon the same two meteorological elements, viz., *temperature* and *humidity*—seems to favor the idea of atmospheric magnetic action: but, if it be conceded that, as is generally supposed, electrical currents flow along the auroral columns, we have in these currents apparently a sufficient explanation of the magnetic action of the aurora, and have therefore no good ground for supposing that the atmosphere has a magnetic action in its normal state. We may, however, still conjecturally connect the aurora with the diurnal variations of the horizontal magnetic intensity by imagining that the daily changes of temperature and humidity of the atmosphere are attended with electrical currents which traverse the atmosphere without producing any perceptible luminosity, or perhaps any other perceptible effects, except upon the magnetic needle.

The philosophical course for the present, however, is to abide by the theory which furnishes the most direct representation possible of the connection subsisting between the barometric and magnetic variations, and at the same time accords with a general theory that satisfies the conditions imposed by the normal state of the earth's magnetism.

Annual Variations of the Horizontal Magnetic Intensity.

It having been established (or at least rendered in the highest degree probable) that the diurnal variations of the horizontal magnetic intensity are due to changes in the temperature and humidity of the earth's surface (or atmosphere), we naturally seek for the explanation of the annual variations of the horizontal force in annual changes of temperature and humidity. We have moreover seen that the diurnal variations of the horizontal force and barometer are linked together, and due to the same two general causes. It is therefore to be inferred that a similar connection must subsist between the annual variations of the horizontal force and barometer, and that the two phenomena must be other parallel effects of changes of temperature and humidity. Now if we compare the curve showing the variations of the horizontal force from month to month with the corresponding curve for the barometer, we in fact find an approximate correspondence between the maxima of the one and the minima of the other.* In the midst of irregular variations the following general law is very manifest in the curve of the barometric variations, viz., there is a maximum in January (or thereabouts), a minimum in the spring, another maximum about August, and another less decided minimum in the fall. This law has also been revealed by the observations made in Europe. It is stated as follows by Kaemtz, (Cours Complet de Meteorologie, p. 282,) "Setting out from winter the pressure" (of the air) "diminishes until the equinox, then it augments in summer without attaining nevertheless to the winter mean; we afterwards find in autumn traces of the second minimum, then the curve rises again until winter." The same law is also manifest in the curve for the horizontal force, substituting maximum for minimum and minimum for maximum. Before noticing this connection between the annual variations of the horizontal force and of the height of the barometer, I inferred from my previous investigations that the following was probably the true explanation of the annual variations of the horizontal force.

From the winter to the summer this force tends to increase by reason of the increase of temperature, and to diminish by reason of the loss of moisture from the earth's surface (or increase of vapor in the air). For a time the first cause prevails over the second and thus the force augments. But the formation of vapor will be proportionally more and more rapid for the same rise of temperature, and thus it happens that some time during the spring the tendency to a diminution of the horizontal force comes to prevail over the tendency to an increase. This state of things continues until the time of maximum temperature (July or August). After this for a certain time the temperature will fall without a proportionate diminution in the quantity of vapor in the air; and thus the horizontal force will continue to decrease for a certain time beyond the time of maximum temperature. But we may expect that during the autumn the rapid diminution in the quantity of vapor in the air will give rise to an opposite tendency; which will continue until the effect of the average daily loss of vapor (being proportionally less from day to day) comes to be overbalanced by the opposite effect of the diminution of temperature. Thus there may be a second maximum in the fall, followed by a minimum in the winter. While this would be the general course of things, there would be room for material irregularities in individual years.

This explanation of the annual variations of the horizontal magnetic intensity I find to be substantially the same, *mutatis mutandis*, as that given by Professor Dove of the corresponding variations of the height of the barometer. He has established

* See Plates in Report of Observations at Girard College Observatory.

that the pressure of dry air has but one maximum and one minimum during the year,—the former in the winter and the latter in the summer. The tension of the atmospheric vapor, on the other hand, attains its minimum in the winter and maximum in the summer. From the combination of these two pressures there results two maxima and two minima of actual pressure, as previously stated. There is "a minimum in the spring because that then the pressure of dry air diminishes rapidly, while the quantity of vapor is not yet very considerable. We find traces of a second minimum in autumn because the quantity of aqueous vapor diminishes rapidly, while the pressure of dry air increases slowly."

It must accordingly be admitted to be highly probable that the annual variations of horizontal magnetic intensity are attributable to the combined operation of the same two general antagonistic causes as the diurnal variations, viz., variations of temperature and variations of humidity. It must also be admitted that the existence of such a connection between these diurnal and annual magnetic and meteorological phenomena is a necessary inference from the Thermal Theory of Terrestrial Magnetism; and that this theory furnishes a rational and consistent explanation of the laws of the magnetic variations.

Annual Variations of the Vertical Magnetic Intensity.

On examining the curve showing the annual variations of the vertical magnetic intensity at Philadelphia for the years 1841 to 1845, we find that the general law is that the intensity is greatest about June or July, and least about December or January. To this general law an exception occurs in the year 1841—the force decreasing, instead of increasing, from the first of the year until July, and then increasing until November, after which there is a decrease in accordance with the general law. Now it will be recollected that, agreeably to the theory under consideration, the vertical magnetic force is dependent upon the differences of temperature between the station of the needle and all places situated to the north or south of this station, within the circle of sensible magnetic action. We have therefore to enquire whether these differences observe the same law of variation as the vertical magnetic intensity; and also whether there is any exception to the general law, corresponding with that above mentioned, during the year 1841. The observations which I have obtained suited to this inquiry, (viz., the mean monthly temperatures at Washington, Newtown, Port Carbon, and Silver Lake, for 1841 and a part of 1842, and at Trenton for 1842) furnish results which accord, for the most part, with the theory. But as these results are confined to one or two years, no certain conclusion can be drawn from them; I shall accordingly reserve this discussion for a future occasion.

Annual Variations of Declination.

According to the theory under consideration the annual variations of declination are attributable to the annual variations in the position of the isothermal line—or rather, to be precise, of the line of equal molecular magnetic intensity, which must approximate more or less to the isothermal line. Now the annual movements of the isothermal line may be ascertained by discussing the annual variations that take place in the differences between the mean monthly temperature at Philadelphia and at some place to the east or west of Philadelphia;—that is, supposing that the law of the variation is the same whatever place so situated with regard to Philadelphia be taken. The following table shows the mean monthly differences of temperature for 1841, between Philadelphia and several places to the west of Philadelphia.

Mean Monthly Differences of Temperature for 1841.

	Jan.	Feb.	Mar.	April.	May.	June.	July.	A'g.	Sept.	Oct.	Nov.	Dec.
Phila. and Lancaster,	...	3·38°	1·59°	0·06°	0·23°	0·38°	0·91°	...	0·93°	1·50°	4·16°	4·71°
" and Gettysb'g,	...	5·23	3·44	2·05	–0·09	1·89	2·13	...	1·89	–15·88	5·09	4·70
" and Carlisle,	...	...	...	3·5	1·9	–1·5	....	...	....	–15·0	5·9	7·3
" and Harrisb'g,	...	4·9	2·32	–0·70	....	–2·8	–1·1	...	–2·1	– 4·5	3·4	4·7
" and W.Chest'r,	...	...	4·3	2·2	1·3	....	....	...	....	.....	....	4·7

It will be observed that the mean monthly differences of temperature for all these places are greater at the beginning and end of the year than toward the middle of the year. The minus sign indicates that the temperature at Philadelphia is less than at the other place.

It appears from these results that during the year 1841, the temperature at places to the west of Philadelphia, from being less than that at Philadelphia at the beginning of the year, became equal to it, or greater, toward the middle of the year, and less again at the close of the year. The isothermal line through Philadelphia must therefore, on the west side of Philadelphia, have moved towards the north during the first part of the year, and towards the south during the latter part of the year. The tendency of these movements would be to make the declination least toward the middle of the year and greatest at the beginning and end of the year. Now, as a matter of fact, on examining the curve given in the Report of the Observations at the Girard College Observatory, showing the annual variations of declination at Philadelphia, we find that in 1841 the declination was greatest in January and February, and again in November and December, and least in April and May. On inspecting the table given above, it will also be seen that an irregularity occurs in the month of October. In the curve of the declination there is a corresponding irregularity; the declination was less in October than in September, and much greater in November than in October.

The curve of annual variations of declination above referred to, shows that the same general law obtained during the entire period of the observations comprised between June, 1840, and the beginning of 1844. The following table shows that the variations of the difference of temperature between Philadelphia and Lancaster followed the same law during this period.

Mean Monthly Differences of Temperature between Philadelphia and Lancaster, Pa.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1840......	....					−1·4°	−1·9°	−2·3°		−0·8°	−0·7°	−0·3°
1841......		3·38°	1·50°	0·06°	0·23°	0·38	0·91	....	0·93°	1·50	4·16	4·71
1842......	1·1°	2·2	−0·5	−2·3	0·8	1·2	1·7	−0·9	−0·7	1·1	1·9	1·6
1843......	....	....	.....	−0·3	−1·5	−0·4	−1·3	1·0	−0·6	0·4	0·0	−0·2

The temperatures at Philadelphia used in the calculations are those given in the Report of the Girard College observations, except in the case of the year 1841, for which the temperatures published in the Journal of the Franklin Institute were used. The temperatures for Lancaster were obtained from the Journal of the Franklin Institute and from the original manuscripts of Dr. W. M. Atlee's Reports to the Franklin Institute which he had the kindness to place in my hands. The observations for Gettysburg, Carlisle, Harrisburg, and W. Chester were also obtained from the published reports to the Franklin Institute, under the general system of meteorological observation established by this Institute. The daily observations were made at 7 A. M., 2 P. M., and 9 P. M.

It is to be observed that the comparatively large plus differences of temperature that occur in the year 1841, as given in the above table, are to be attributed to the fact that the temperatures at Philadelphia as reported to the Franklin Institute are higher than as given in the Girard College observations. Making use of the latter we obtain for the

Differences of Temperature between Philadelphia and Lancaster for 1841.

Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1·0°	−1·8°	−2·6°	−3·0°	−3·1°	−4·1°	−2·9°	−1·3°	−0·5°	−1·8°	−0·1°	0·6°

The results which we have obtained leave little room to doubt that the principal annual movements of the needle are connected with certain corresponding movements of the isothermal line;

and that the connection is such as our theory calls for. The question of this connection cannot however be definitively settled until we have obtained a longer series of observations, and at a greater number of places.

General Results.

It may be well to recapitulate here the more important results arrived at in this and the previous memoir.

1. The diurnal and annual variations of the horizontal magnetic intensity are due to the joint operation of two general antagonistic causes, viz., variations of temperature and variations of humidity.

2. The diurnal and annual variations of the height of the barometer (or pressure of the air) are attributable to the same general causes;—with this probable difference, that in one case the effects result directly from changes in the temperature and humidity of the air, and in the other from changes in the temperature and humidity of the earth's surface.

3. The variations of horizontal magnetic intensity and of barometric height follow the same law; with the single exception, that the maxima of the one element occur at the same hours as the minima of the other.

4. The observed variations of the horizontal magnetic intensity are legitimate consequences of the thermal theory of terrestrial magnetism; granting that moisture has a magnetic action.

5. The deviations, in the variations of the horizontal force, from the general law of proportionality to temperature, are modifications produced by the deposition of vapor from the atmosphere and the evaporation of the same from the earth's surface. In the case of the diurnal variations these effects (taking the mean of three months' variations) are confined chiefly to the interval between sundown and the hour of 10 A. M. on the following day.

6. Taking the mean for three months, the amount of the decrease of the horizontal force during the morning hours depends chiefly upon the quantity of rain that falls.* The explanation of this dependence is that the diminution of this force in the morning is owing to the evaporation which then occurs, and the average amount of this evaporation must be mainly dependent upon the average quantity of rain that falls.

7. The deposition of dew at night which has the effect to check, and even overcome the diminution of the horizontal force due to the fall of the temperature, exerts also an important influence in moderating the nocturnal decrease of temperature, which does not appear hitherto to have been recognized.†

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* See this Journal, 2nd ser., vol. viii, Number for Nov., 1849, p. 353.

† See this Journal, 2nd ser., vol. viii, Number for Sept., 1849, p. 226.

8. The diurnal variations of the vertical magnetic intensity are, for the most part, in accordance with the idea that they arise from variations in the difference between the temperature at the station of the needle and the temperature at a place to the north or south of it.

The annual variations of vertical force and difference of temperature also appear to proceed *pari passu* with each other; but the observations hitherto discussed are too limited to settle definitively the question of natural dependence.

9. There is an undoubted connection between the diurnal variations of the declination and those of the horizontal force. This connection may be described as follows: when the curve showing the diurnal variations of the horizontal force is concave upward, the declination (westerly) is increasing; and when the curve is convex upward, the declination is decreasing. As a consequence the maxima and minima of declination must be cotemporaneous with the points of inflexion of the curve of horizontal force.*

10. This connection accords with our theory, agreeably to which the declination varies with the shifting position of the line of equal molecular magnetic intensity;—which line must be very nearly coincident with the line of equal horizontal magnetic force. The line in question differs from the true isothermal line by reason of the modifications of the horizontal force induced by changes of humidity.

11. The annual variations of declination appear to be mainly dependent upon an annual oscillatory movement of the isothermal line. This result accords with the theory if we grant that, taking the monthly means, the isothermal line will differ but little from the line of equal molecular magnetic intensity. We know that these lines must approximate more or less to each other. The degree of approximation can only be directly ascertained by a detailed and extended discussion.

Art. XXX.—*On the Analogy between the mode of Reproduction in Plants and the "Alternation of Generations" observed in some Radiata;* by James D. Dana.

Read before the American Association for the Advancement of Science at New Haven, August, 1850.

The very remarkable fact that a Polyp and a Medusa may be in some instances different states of one and the same species, has been well established of late by the researches of Sars, Dalyell, Steenstrup, and others; and recent important observa-

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* See this Journal, 2nd ser., viii, 360, 361.

tions have been made on the subject by Professor Agassiz. The alternations are as follows:—

1. The Medusa produces eggs;—

2. The eggs, after passing through an infusorial state, fix themselves and become polyps, like Corynæ, Tubulariæ, or Campanulariæ;—

3. The polyps produce a kind of bud that finally drops off and becomes a Medusa.

Thus the egg of a Medusa, in such cases, does not produce a Medusa, except after going through the intermediate state of a polyp.

Or if we commence with the polyp, the series is thus:—

1. The polyp produces bulbs that become Medusæ;

2. The Medusæ produce eggs;

3. The eggs produce polyps.

This is what is called by Steenstrup "Alternation of Generations;" and he considers the earlier generation as preparing the way for the latter. It certainly seems to be a most mysterious process:—a parent producing eggs which afford a progeny of wholly different form, (even so different, that naturalists have arranged the progeny in another grand division of the Radiata); and this progeny, afterwards, by a species of budding or gemmation repeating the form of the original parent.

Yet although seemingly so mysterious, is not this mode of development common in the vegetable kingdom? Is it not the prevalent process in the plants of our gardens and fields, with which we are all familiar?

It is well known to us, that in most plants, our trees and shrubs for example, growth from the seed brings out a bud of leaves; from this bud after elongation, other leaf-buds are often developed, each consisting like the first of a number of leaves. It is an admitted fact (as may be found in Treatises on Vegetable Physiology) that each of these buds is a proper plant-individual, and that those constituting a tree are as distinct and independent as the several polyps of a compound zoophyte; and that the tree therefore is as much a compound group of individuals, as the zoophyte. In some cases the plant forms but a single leaf-bud; in others, where there is successive gemmation for a period, the number is gradually multipled, and more or less according to the habit of the species. So among polyps, there is the simple and compound Tubularia, Campanularia, and the like.

After the plant has sufficiently matured by the production and growth of its number of leaf-buds, there is a new development—a flower-bud,—consisting of the same elements as the leaf-bud, but wholly unlike it in general appearance—as much so, as the Medusa is unlike the polyp. The flower-individual starts as a bulb from the leaf-individual, or the group of leaf-individuals, and is analogous in every respect to the bulbs from the Campanulariæ and allied species; and when it has fully matured, it produces, like the Medusa, ovules or seed—these seed to begin the round again of successive or alternating developments.

Thus among plants the seed produce leaf-individuals; these yield bulbs or buds becoming flower-individuals; and these produce seeds; precisely, as the egg produces polyps, the polyps, bulbs that develop into Medusæ, and the Medusæ, eggs.

When we follow out this subject minutely, we find the analogy completely sustained even in minor points of structure and growth. The leaf-bud consists of leaves developed in a spiral order; and in the polyp, as some species show beyond doubt, the tentacles and corresponding parts are spiral in development. The same spiral character is found in the flower, but the volutions are so close as not to be distinguished readily from circles. In the Medusæ referred to, the regularly circular form is far more neatly and perfectly developed than among the polyps—as is clearly seen in a comparison of the polyp Coryna, with the elegant Sarsia, a species of which is described and beautifully delineated in Professor Agassiz's recent memoir, published by the American Academy of Arts and Sciences at Boston. The relations in structure between plants and polyps might be farther dwelt upon; but for other observations the writer would refer to his volume on Zoophytes.

The only point in which the analogy seems to fail, is that the Medusa-bud falls off before its full development, while this is not so with plants. But it is obvious that this is unimportant in its bearing on this subject. It is a consequence of the grand difference in the mode of nutrition in the two kingdoms of nature; for the plant-bud on separation loses its only means of nutriment.

The law of alternating generations is therefore no limited principle, strange and anomalous, applying only to a few Radiata. It embraces under its scope, the vegetable kingdom, and it is but another instance of identity in the laws of growth in the two great departments of life.

Art. XXXI.—*On Electro-magnetism as a Moving Power;* by Prof. Chas. G. Page.

Note.—We are expecting from Prof. Page a description by himself of his new electro-motive engine: and in the mean time give a brief explanation of its fundamental principle, with the following Report to the Secretary of the Navy upon the progress of his investigations.

It is well known that when a helix of suitable power is connected with the poles of a battery in action, that an iron bar within it will remain held up by the induced magnetism although the helix be put in a vertical position: and if the bar be partly drawn out of the helix by the hand, it goes back with a spring when the hand lets go its hold. This power—the action of the helix upon the metallic bar within it,—is the power used in his engine. The power, when a single coil is used, has its points of greatest and weakest force, and in this condition is objectionable. But by making the coil to consist of a series of short independent helices, which are to be brought in action successively, the metallic rod is made to pass through the coil and back again with great rapidity and an equable motion. In all the engines hitherto used, there is a loss of power at the instant of the change of current, owing to the production of a secondary current moving in the opposite direction, and to this loss is owing the fact that these engines cannot be rendered available. Prof. Page had in view the obviating of this difficulty when he commenced his recent investigations, and has full success in his new invention.

The report below is an outline only of his experiments on the application of Electro-magnetism, and is dated, Aug. 3d, 1850. We take the Report from the National Intelligencer of September 4.—Eds.

From the brief time allowed, it will be impossible for me to do more in this respect than to give an outline of the experiments which I have repeated and recorded during the past year. Their full detail and explanation will form a volume, replete with interesting scientific matter, and require much time and labor.

The first principal experiments were made with a small trial engine, built expressly for the purpose, and with the utmost care in reference to mechanical accuracy. Attached to this was a dynamometer of new construction, and admirably adapted to the purpose. This was invented by my principal engineer, and measured in a most satisfactory manner the dynamic power of the engine, at any given velocity—a great desideratum in estimating this new power. With this trial engine the following important questions were tested:

1. The dynamic values of different qualities of soft iron.
2. The dynamic values of steel, hard and soft.
3. The dynamic value of cast iron.

The statical values of all these varieties were tested by a separate apparatus, constructed for the purpose, called the axial galvanometer. Twelve varieties in all were tested, and were in bars of uniform size, one foot in length, and one inch in diameter, and it was found that the statical and dynamic properties corresponded.

4. The proportions of the helices were approximately tested, though much remains unsettled yet upon this important point.

5. The advantages of keeping up the magnetism in the axial bar was most satisfactorily tested.

6. Various modes were tried of reversing the motion of the engine, and with success.

7. Various kinds of cut-off (which is the most critical and important point in the construction of the engine) were tried.

8. The operation of closed circuits and secondary currents was tested by a number of experiments, requiring great care and accuracy.

9. The best working velocity of this engine, and its absolute power with a given battery, was fully tested.

10. The ratio of increase of power, with an increase in the quantity of the current.

11. The values of different kinds of metal in forming the cut-off.

12. Various mechanical points of construction, supposed to have been incompatible with the exhibition of this power, were put to a practical test.

Various other minor points also were the subject of experiment, which will be communicated hereafter.

A second model, of small size and somewhat rude construction, was also made, with a view of testing a new arrangement of the axial bars.

Experiments were then commenced upon a larger scale, with a view to determine whether the same proportion of power could be obtained from large as from small engines, this being the principal question in view at the time of the grant of the appropriation.

With a view to facilitate the construction of helices of large size, a machine which had long been in contemplation was made at a considerable expense. The work was done at the Navy Yard in a creditable manner, and the machine performed its work well, turning out entire helices of copper wire, of large size, from straight bars. But before I had proceeded far, a discovery was made in reference to the helix, which rendered the machine useless; for the present at least.

A number of large helices were then constructed of various sizes, and suitable bars of soft iron prepared, corresponding to the helices. Hollow and solid bars were prepared, from two inches to eight inches in diameter, and generally three feet in length. Some bars of four and five feet in length were also prepared. The bars were all worked at the Navy Yard, and at a considerable expense, as they were required to be of homogeneous metal, accurately turned and bored.

With these bars and helices, a multitude of experiments were performed and recorded, and these were kept up day after day for about two months. My official duties as Examiner in the Patent Office left me only the evening of each day for operation; and, under such circumstances, you will readily appreciate the difficulties and disadvantages under which I have labored. My own zeal has led me beyond my strength; but I have been richly rewarded by the most flattering results.

The experiments here were not such as could be performed upon the laboratory table; but were with large masses of iron, weighing in some cases three hundred pounds, and helices sometimes twice that weight.

Adhering to the same size of battery through a long series of experiments, and varying the coils and bars, I found, to my great gratification, that as I increased the dimensions of each, a corresponding increase of power was exhibited, and the consumption of material, or cost of the power, in some proportion diminished. These results were encouraging and stimulating in the highest degree, and fully justified the undertaking at once of an engine upon a much larger scale than any hitherto tried.

This engine, the frame-work of which was principally built at the Navy Yard, was an upright engine of two feet stroke; and in order to have facilities for comparative trials and experiments, it was necessary that a double engine should be made, the two parts exactly corresponding. Two bars of soft iron, six inches diameter and three feet in length, were the prime movers, and these were balanced by means of connecting rods and cranks upon a fly-wheel shaft. The balance wheel and shaft together weighed six hundred pounds. When this engine was first tried, with the same battery which had before given me one-fifth of a horse power, with a smaller engine, it produced only one-third of a horse power. By careful attention to the adjustments, and particularly to the cut-off, which was a very different thing now from what it had been in smaller engines, the engine soon yielded one horse power. Here was a gain of eighty per cent. as measured merely by the size of the battery. But it was much more; for the cost was found to be less for one horse power than it had been before for one-fifth of a horse power, in a smaller engine; how much less has not yet been ascertained.

A great variety of experiments were continued with this engine, to be hereafter detailed, each having a definite object; and, I am happy to say, each resulting advantageously, so that finally, by little daily increments, I obtained from this engine, by a trifling addition of battery, a full two horse power.

By way of giving a practical character to the engine, it was geared to a circular saw ten inches diameter, the turning-lathe and grindstone of the workshop, all of which it worked simultaneously, as witnessed by a number of visitors, and, if I mistake not, by your predecessor in office, in company with Lieut. Maury, of the National Observatory.

After many satisfactory trials with this engine, it was taken down, and all its available parts used in the construction of the single horizontal engine which I had the honor lately to exhibit before the Smithsonian Institution. This change was made for the purpose of dispensing with the dead weight of one of the driving bars, and more particularly for introducing the important feature of keeping up the magnetism of the driving bar. As soon as this new form was completed and tried, a gain of one-half horse power was at once realized, and by the addition of a few more feet of battery surface, the power was found to be above four-horse. Further addition of battery would still augment the power, and I see no reason why ten horse power might not be obtained from this engine, by the addition of more battery; but whether it would be economical to increase power by this means alone, and to ascertain the point, for this and every other engine, beyond which economy would cease by increasing the battery alone, are matters to be determined by experiment.

The next most important point to be determined was the expense of this power. Much to my own surprise and gratification, the expense was found to be less than the most expensive steam engines, although recently, in Europe, it has been decided by experimenters and men of science, and generally conceded, that it was fifty times the cost of the dearest steam engines; but this is no obstacle to its introduction, considering its immense advantages in other respects. Moreover, if thus much has been done in the very inception of this undertaking, what may we reasonably expect from its further prosecution?*

Before it can be rendered available in practice, much remains to be done with the galvanic battery, to render its action regular and durable, and in other ways to establish a certainty of action, so that the engines may be managed by persons not thoroughly skilled in the subjects of electricity and magnetism.

It remains yet also to be proved whether the power will increase in proportion to the size of the engines. This principle seems to be strongly indicated by past experiments; but yet it cannot be established by calculation or process of reasoning. Experiment upon an extensive scale can alone determine this point. A part of the work preparatory to building a locomotive engine has been done; but it seems necessary to try further experiments before incurring the expense of another large engine upon the plan above mentioned. The rotary form of the engine has not

* Prof. Page, as mentioned in our last number, stated in his remarks before the American Association, that one horse power for twenty-four hours, would cost about 20 cents. Prof. W. R. Johnson observed that his estimate was based upon too high a cost for the zinc, and that 10 cents would be a nearer estimate. In either case, a very great advance is made upon all previous experiments.

Prof. Page also observed, that the cost of electro-magnetic power was not to be reckoned in this comparison by the mere cost of zinc, nor the cost of steam by the pounds of coal consumed. The cost of human life, the sacrifice of millions of property, and risk of many millions more, and all the contingent advantages and disadvantages were to taken into account.

With regard to his mode of measuring the power of the engine, Prof. Page explained as follows after drawing a diagram of the fly-wheel. The brake was loaded to 620 lbs. The power required to barely keep the engine in motion under this load was 126 lbs. The full power being on, the engine made eighty revolutions per minute under this load. The circumference of the wheel being about four feet, it was easy for any one to compute the horse power from these data.

yet been tested, although it possesses advantages not to be found in any form of the reciprocating engine. There are some obvious disadvantages attending its construction; but it is hoped that they will be outweighed, more especially as this form of the engine will occupy less than one-half the room required for the reciprocating form.*

It would seem very desirable that the investigation thus begun, and so far successfully conducted, should be carried at least beyond *an uncertain issue*, and that *every important point* should be settled, and particularly that of its availability on an extensive scale. The power is peculiarly fitted for purposes of navigation, if it can be made subservient; and a trial upon a scale of one

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* The following notice of Prof. Page's experiments is from the Daily National Intelligencer of September 11.

Since the first announcement by Prof. Page of the results of his discoveries, I have seen in several public journals accounts of inventions for the same purpose, by other persons, and in most of them claims to novelty and great superiority. I had some curiosity to refer to Professor P. an article of this kind, from a late number of the St. Louis Republican, in which it was stated that Mr. Bland, of that city, had a new invention, "far in advance," as was shown by calculation, "of that adopted by Professor Page." On making the reference, my attention was called to an article in the 36th volume of Silliman's Journal, page 352, published in 1839, in which Professor Page had explained and figured an electro-magnetic engine, exactly like Mr. Bland's, and which plan, I was informed, was abandoned more than ten years ago.

The fact is, Dr. Page's method is peculiar and entirely new, and distinct from every other hitherto tried; and therein lies the source of his success. Instead of going upon beaten tracks, which, though seemingly fair, he was persuaded would not reach the desired end, he marked out an entirely new one. One great difference between his and other plans, as I understand it to be, is this: In all former electro-magnetic machines, the power is made up of a series of impulses, while in this, which he styles an *axial* machine, or engine, the power is uniform and continuous; and it is just as easy to make a reciprocating engine of twenty-four feet stroke as one of two feet, like that already constructed and recently exhibited.

I saw at the laboratory of Dr. Page, a *rotary axial engine*, which he thinks may, in many cases, supersede the reciprocating. It is really a curious machine; and looking at this, and all his wonderful results, it appears as if we had just entered upon a new era in science and art, promising revolutions in social life and business pursuits as miraculous to the people of the day as have been those effected by the steam engine and the magnetic telegraph. * * *

In order to show that there was something like *power*, he loaded down the engine, placed the crank at half stroke, and then a hook over the end of the crank, to which hook was attached a long rope. Three of the strongest men of the party then took hold of the rope, two of them having their feet braced. The three men could not start the engine a hair's breadth. Four of the men then took hold, and they moved the crank two inches, where it stuck fast. The power was then let on, and the engine started, and made a speed of ninety revolutions in a minute. By taking off fourteen pounds from the end of his friction brake the engine made one hundred and ten revolutions per minute. Professor Page stated that this was not testing the power of the engine, but it showed that what four men could but just move through two inches, the engine carried through one-fifth of a mile, and that, too, in one minute. Understand that, from the change in the position of the crank, the power of four men could go no further than the two inches.

Professor Page expects to make a trial upon a railroad soon. He has sufficient power now to make a *demonstration;* but is not satisfied with it. He would be glad to make the first trip with fifteen to twenty horse power. It is, however, in navigation that he expects the greatest benefits from this invention, and I would like to see the project carried out of an engine and magnetic boat [not *steam* boat] of one hundred horse power. This would settle the question, and enable the hundred horse power seems to be the only mode of arriving at a definite conclusion upon this point. It is obvious that, preliminary to such an undertaking, a great many experiments will be absolutely necessary; and such only as one quite familiar with the difficulties of entering upon an entirely new field of operation can properly appreciate.

ART. XXXII.—*Singular property, and extraordinary size and length of the Secondary Spark;* by Prof. CHAS. G. PAGE, M.D., Washington, D. C.

IN experimenting with my great magnet a new property of the secondary spark has been discovered and some very interesting facts elicited. I will premise that the helix nearly a foot in diameter each way, when charged by the battery, draws up within it in a vertical position a huge bar of iron weighing 300 pounds, through a distance of ten inches, presenting by far the most powerful magnet ever known. When the circuit with the helix is suddenly broken a secondary spark is produced, *eight inches in length.*

The most interesting feature of this spark is the modification of its form and sound by the action of magnetism. When the spark is produced at a distance from the magnet, it is readily elongated to six or eight inches, and I presume might be obtained a foot or more in length if the wires were separated with the velocity of a cannon ball, as suggested by my friend, Mr. Lane. In this case there is little or no noise made by the spark, but as the spark is produced nearer to the magnetic pole the sound increases until at last when close to the pole each spark makes a report as loud as a pistol. The spark also diminishes in length and is spread out as large as the palm of the hand. There is an effect here somewhat analogous to that produced by a magnet upon the arc of flame between charcoal points. I must reserve further remarks for another number of the American Journal.

Washington, D. C., Aug. 27, 1850.

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world to enter upon the benefits of the discovery, or satisfy mankind that the power cannot be made available for "locomotion or navigation;" and thus arrest the further sacrifice of mind and means, in endeavors to find that which (if it cannot be secured by the present plan) does not probably exist. A point has been arrived at in this investigation which indicates, to use the language of Professor Page, in his late report sent to Congress, the importance of carrying the thing "beyond an uncertain issue." I have no doubt of the applicability and economy (especially if we count the risks and value of human life) of this power for the purposes contemplated by Congress in providing for the experiments; but the world may not yet be satisfied, and many experiments have still to be made, and much money expended before the invention will be really available. Fulton's steamboat would bear but a sorry comparison with those of modern construction; yet who will say that Fulton's limited success should have been the signal for suspending all efforts to use steam as a propelling power for ships, or that to his zeal and intelligence we are not indebted for the introduction of what we now enjoy in the way of steam navigation? H.

ART. XXXIII.—*On Rutile and Chlorite in Quartz;* by O. P. HUBBARD, M.D., Prof. Chem., Min. and Geol., Dart. Coll., N. H.

From the Proceedings of the American Association for the Advancement of Science, New Haven, 1850.

SPECIMENS of rutile in quartz have for twenty years past, been found in boulders in several towns in the vicinity of Dartmouth College, none of which have ever been traced to their sources. Localities have been mentioned, but none have furnished specimens resembling these boulders, excepting a single one. This locality was opened two years since at Waterbury, Vt., on the Central Railroad. It was described by Mr. Alger in the Proceedings of the American Association for 1849, and also in the present volume of this Journal, page 14.

In a cut of sixty feet perpendicular through solid talcose slate, and thirty feet from the surface, a vein or pocket of quartz was met, and a considerable number of specimens containing rutile were obtained. The locality is now exhausted. From its position, it never could have furnished the scattered masses heretofore known, and we have yet to discover their origin.

Some of the specimens from this region have comparatively but little beauty; the rutile is in very fine capillary crystals of dark color, two or three inches in length, and the quartz is of inferior quality. But others are exceedingly fine, both in the richness of the quartz and the abundant long needles of the rutile.

There are *three* known American specimens of a remarkable character, one of which is from this Waterbury locality. The other two were found as boulders and are even of superior quality.

One of these has been in the Cabinet of Dr. J. R. Chilton, New York City, for many years, and is reported to have been found in Northern New England. It has the rutile in long acicular crystals and one series of prisms united into a crystal a quarter of an inch wide.

The other is a mass in the writer's cabinet, described by Mr. Alger as "the finest specimen of this mineral found in the United States." It was picked up in this region nearly twenty years ago, but in what town is not known. Specimens from Rochester and Bethel, Vt., resemble it more than any others.

It is about six inches long and three inches in its other dimensions, being of irregular shape, and only a fragment of a larger mass. Two sides have been cut and polished by the lapidary, one retains its polished plane boulder surface, and the remaining part of the exterior is irregular, presenting a conchoidal fractured surface. There are indications of smooth cleavage faces in different parts inclined to each other.

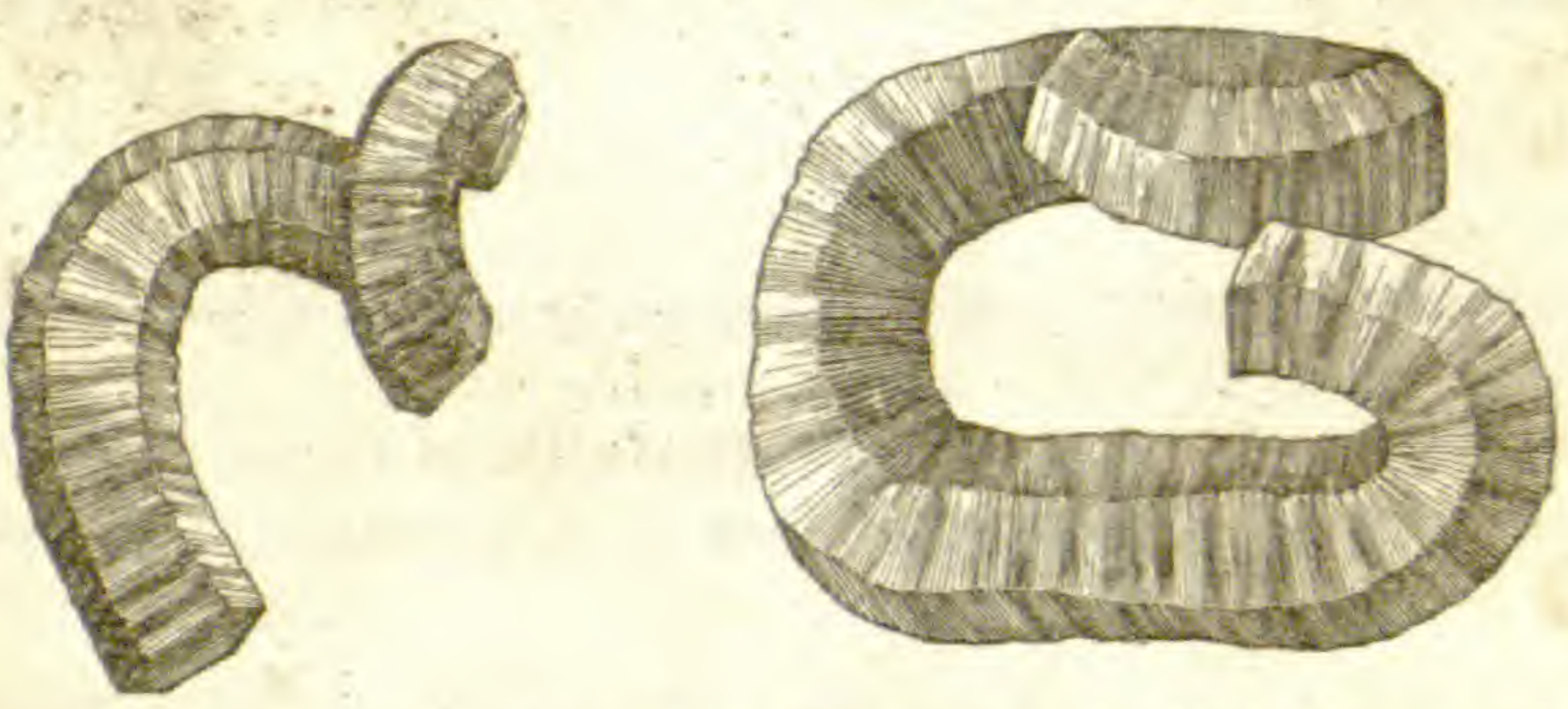
The quartz in mass is transparent and slightly smoky—while the slices cut off are almost colorless. It is questionable whether the color is proper to the quartz, or occasioned by the reflection from the rutile crystals. Mr. Alger finds almost no rutile in the *white* quartz crystals from Waterbury, "while the colored varieties abound with it," and probably, he suggests, owe their color to it.

The rutile crystals are from the size of the finest hair and almost invisible, up to a twelfth of an inch in diameter and five inches long; they are uniformly distributed through the quartz, and intersect and cross each other in all directions. There is no radiation from a centre, but in many instances the crystals have one or more large graceful curves, and sometimes two in opposite directions, and some are bent at an angle either right or oblique. Many are broken at the surface of the quartz, while others are wholly included in it, terminating in a single plane or tapering to a point.

They are all of a uniform bright reddish brown color, and of the lustre of polished copper. Where the ends are seen on the polished faces they have the color and lustre of polished steel.

In numerous cases the surface of the crystals is covered here and there with a brilliant, silver white mineral, sometimes limited to the lateral edges, and again investing parts of the prism at intervals, or with frequent interruptions, giving it the appearance of being made up of numerous short white and brown prisms, the form remaining unchanged. In some cases this mineral occurs like a thin disk, through the centre of which the rutile appears to penetrate. I have not been able to determine with certainty the nature of this mineral, and can only conjecture that it is the same with the curved crystals described below.

In the writer's specimen, as in those described by Mr. Alger, there are numerous vermiform, tortuous and convoluted crystals. By transmitted light, they are sometimes of the color of copper, though faintly so, or of a bronze yellow, or of greenish and yellow shades or even very dark, and by direct light they are almost black. These crystals are regular hexagonal prisms, transversely finely striated,



and appear to be made up of thin plates of slightly varying size, giving the crystals a varying diameter. They occur either singly or in groups of several laterally joined, and united in all their convolutions, and having a single terminal plane, highly lustrous, which often presents a silver white color. The above figures, enlarged views of two of them, give a perfect idea of the originals, the prismatic form of which is obvious to the eye and perfectly distinct with a glass. If we judge from the figures in Mr. Alger's paper in this volume, (p. 14,) the prismatic character of his crystals is much less striking.

Mr. Alger has described the mineral in his specimens, as *mica.* I have been able to obtain only a very small portion of the mineral from one or two protruding curves on my specimen. It readily cleaves parallel to the terminal plane, is apparently softer than mica, and is easily reduced by the pressure of a knife on white paper, into a fine, coherent powder, of a greenish tint. It has no elasticity, and before the blowpipe gives off an abundance of water. From these decided characters, and the rarity of such an association of mica, and the quite frequent one of chlorite and quartz, it seems altogether probable that this mineral is *chlorite.*

If these several minerals were at one time in solution in the fluid quartz, they must have crystallized previous to it. The rutile prisms are so straight or so gracefully curved and bent, that they would seem to have experienced but slight resistance. They intersect and cross each other, and pass *through the loops in the chlorite* crystals or touch them on the outside, and they probably crystallized first. Around most of these convolutions of *chlorite* there is a burr, or a minute spot of imperfectly radiating fractures, occasionally iridescent, which suggests that they were formed before the solidification of the quartz, and that they had occasioned some pressure or disturbance and a slight fracture. But as the chlorite uniformly, and the rutile in very many cases, must have been without any attachment, the density of the fluid quartz to have sustained them was probably great.

On removal of the rutile and chlorite from the gangue the vertical striæ of the former and the transverse striæ of the latter are found figured on the quartz, making it certain that the latter was last solidified.

There must be somewhere in this region north, a rich deposit, for which mineralogists will earnestly seek, until it is found and its treasures are transferred to their cabinets.

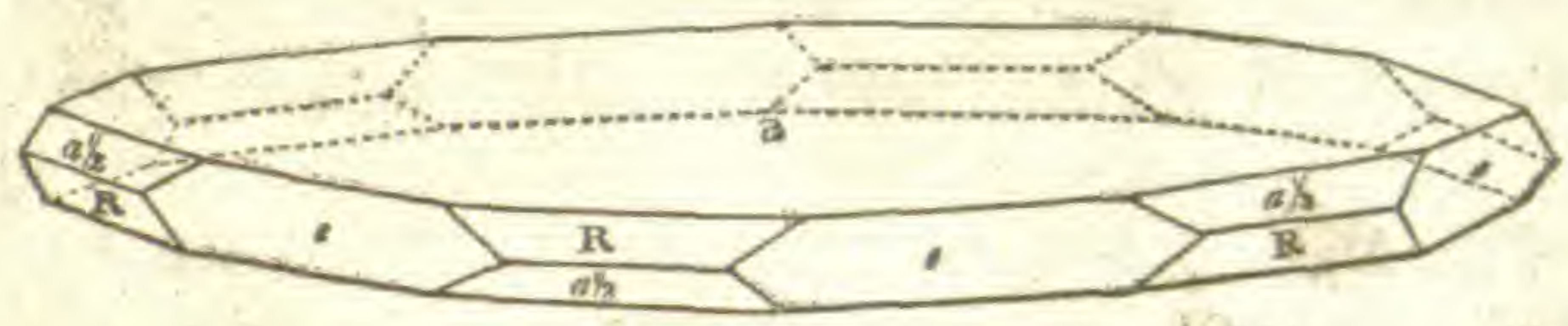
Art. XXXIV.—*Occurrence of Crystallized Oxyd of Chromium in furnaces for the manufacture of Chromate of Potash;* by W. P. Blake.

Read before the American Association for the Advancement of Science, New Haven, August, 1850.

Crystals of the sesquioxyd of chromium have been obtained in small quantities by Wöhler, by passing the vapor of chlorochromic acid through a tube heated to redness.

The crystals which I have examined with the following results, were obtained from a furnace which had been long in operation for the production of chromate of potassa from the mineral chromic iron. A portion of the furnace having been taken down for repairs, I found small but exceedingly brilliant crystals lining the cracks and fissures between the fire bricks and disseminated throughout their substance. They resembled crystals of specular iron, but a blowpipe examination and qualitative analysis proved them to be the sesquioxyd of chromium.

The crystals are of the hexagonal system, and are flat 6 or 12-sided tables or scales, one of which is represented in the annexed figure. The breadth of the largest is 1½ lines, but they are seldom over a line broad, and usually much smaller. In



some of the specimens the crystals are grouped in rosettes of great beauty. The following angles were obtained with the reflective goniometer:

R : a $=121° 55'$ (mean of 14 measurements.)
R : $a\frac{1}{2}=96° 45'$ (mean of 6 measurements.)
a : $a\frac{1}{2}=141° 38' 24''$ (mean of 5 measurements.)

Calculating from a : R, the angle a : $a\frac{1}{2}=141° 15'$, and R : $a\frac{1}{2}$ $=96° 50'$; and as the plane $a\frac{1}{2}$ is exceedingly small, this result is more probably correct than that given above from measurement.

The angle a : R gives for the angle of the rhombohedron 85° 22′, which is but little less than that given for specular iron. The axis $=1{\cdot}39045$. The crystal according to Naumann's notation has the descriptive expression, 0R, R, $-\frac{1}{2}$R, R^{∞}.

The crystals have the hardness of sapphire, equal to 9 on the scale of Mohs. Lustre metallic. Color black; opaque except in thin plates, which are green by transmitted light. The powder of the crystals is dark green.

The specimens were taken from and between the bricks which had constituted the floor of the furnace. The furnace had been in operation for more than a year, and kept at a temperature above redness. As it needed repairs, the fires were drawn, it was allowed to cool undisturbed for ten days, and when the bricks were taken out they were still too hot to be taken in the hand.

The mass of the bricks and the portions on which the sesquioxyd has crystallized, is charged with soluble yellow chromate of potash, and in many or all of the specimens the green color of the uncrystallized oxyd can be seen.

My frequent daily examinations of the furnaces in operation made me familiar with the condition in which the contents were at different times, and considering the facts before stated, I account for the production of the crystals in the following way.

When the furnace, newly constructed or lined with fire-brick, is fired and charged with alkali and chrome ore, much of the fused chromate of potash formed, is absorbed by the porous bricks, and I observed that it had penetrated through three or four courses of bricks and mortar.

After the furnace has been long in operation the bricks become saturated, and vitrified, to a certain depth; and the floor and sides of the furnace become incrusted with a vitreous coating, which is constantly increasing. The parts more remote from the fire are consequently better protected from changes and variations of temperature, and are exempt from the inroads of more fused material.

The chromate of potash is thus kept for a long time at a uniform high temperature, and gradually losing its potash from volatilization, the chromic acid ($\dddot{C}r$) in combination with it loses oxygen, becomes sesquioxyd ($\overline{C}r$) and crystallizes.

Art. XXXV.—*Memoir on Emery;* by J. Lawrence Smith, M.D.—First part—*On the Geology and Mineralogy of Emery, from observations made in Asia Minor.*

Read before the Academy of Sciences of the French Institute, July 15th, 1850, and communicated by the author for this Journal.

Of all the mineral substances employed in the arts, few have offered so little opportunity for geological examination as emery, and consequently our knowledge of it in this particular is very limited.

Aware of the importance of the study of this substance *in situ*, both in a scientific and practical point of view, I did not lose the opportunity afforded by my late position under the Turkish government to develop certain facts that came under my notice the latter part of the year 1846. Prior to that period, emery (which term is here used as in the arts to express that mixed granular corundum employed for abrasion) although known to exist in many places in greater or less abundance, was supplied to the arts almost entirely from the island of Naxos in the Grecian Archipelago; so true is this, that the proprietors of the mines in that island controlled completely the price of this mineral. The emery from Naxos frequently went under the name of Smyrna emery, from the fact of its coming to us from that port, where it is originally carried from the island for future exportation.

Prior to 1846, the existence of emery was not remarked in Asia Minor or any of the contiguous islands except that of Samos, which fact is alluded to in Tournefort's travels in the seventeenth century. In the latter part of 1846, I arrived in Smyrna, and was shown specimens which I recognized as emery that came from a place about twenty miles north of Smyrna; they had been first discovered through the agency of a knife grinder of the country, who had been in the habit of using it to charge his wheels with. The importance of this circumstance to the Turkish government as well as to the arts (emery being at that time sold at a most exhorbitant price) induced me to return to Smyrna in the early part of 1847, for the purpose of examining the supposed locality of this mineral. On this second visit other localities were made know to me that an English merchant by the name of Healy had succeeded in bringing to light.

The first locality towards which I directed my examination was that of *Gumuch-dagh*, a mountain about twelve miles east of the ruins of *Ephesus.* Before, however, arriving there, I discovered this mineral imbedded in a calcareous rock in a valley twenty miles south of Smyrna, called *Allahman-Bourgs;* the position not being very favorable for the study of the geology of this substance, my route was continued to the place originally fixed upon. Obtaining guides at the village of *Gumuch*, I commenced the examination of the mountain, which is composed of bluish marble resting on mica slate and gneiss. On the very summit of the mountain, the emery was found scattered about and projecting above the surface of the soil. After examining the extent of the formation and satisfying myself that it was there *in situ*, I returned to Constantinople, and made a report to the Ottoman government. Although I gave no notice to the scientific world of the result of my examination, the editor of the *Journal de Constantinople* inserted a small note in his journal in May, 1847, to the following effect—

"It is some time since M. Lawrence Smith, American Mineralogist, discovered at Magnesia near to Gumuch-Kuey an emery mine, of which he brought specimens to Constantinople. The government have sent to the place a commission composed of M. Smith and some of the officers of the imperial powder works, to examine thoroughly into the importance of this mine, and according to the report that will be made the government will decide on the steps to be taken with reference to it, &c."

This circumstance, unimportant in itself, has subsequently become of great value to secure to me the priority of the discovery and examination of emery *in situ* in Asia Minor;* and also to show that I have been instrumental in the development which has

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* See Am. Jour. Sci., 2nd ser., vol. vii, 283.

been subsequently given to this emery in a commercial point of view. Since the first discovery other localities have been ascertained by me, all of which will be alluded to in this memoir.

Localities of Emery in Asia Minor and the neighboring islands.

Gumuch-dagh.—In going from Ephesus east to *Gouzel-Hissar* (the ancient *Tralles*) we pass by the ruins of the ancient city of Magnes on the Miandre and near to this latter is a beautiful valley, celebrated for its figs, in which is situated the village of *Gumuch* at the foot of a mountain bearing the same name. It was here that the emery formation was first examined. All the rocks of the surrounding country appear to belong to the old series; the limestone is entirely devoid of fossils and metamorphic in its character; it rests on the older schists of which mica schist appears the most abundant, and this again farther to the north was traced in contact with gneiss. The limestone is of a light blue passing into a coarse grained marble; and on the south side, the rock by its decay leaves in many places precipices of considerable elevation, that add much to the picturesque appearance of the region.

The emery is found in different places in the *Gumuch* mountain; the place, however, to which it is traced in greatest abundance, is on a part of the summit about three miles from the village of Gumuch, and some fifteen hundred or two thousand feet above the level of the valley; it overlooks the magnificent plain of the Miandre, whose curiously tortuous course is seen as if traced on a map. The emery lies scattered on the surface in the greatest profusion, in angular fragments of a dark color, and large masses of several tons weight are seen projecting above the surface; in penetrating the soil, the emery is found imbedded in it and a little farther down it is come to in the rock. In fact by breaking the marble that projects above the surface at this spot we are sure to find nodules of the mineral.

Sometimes the emery forms almost a solid mass several yards in length and breadth. One of these places, opened for the purpose of exploring, is about ten or twelve yards square and all the rock taken out is emery; the spaces between the blocks are filled with an earth highly charged with oxyd of iron. In some places the masses are consolidated by carbonate of lime of infiltration, which must not be confounded with the emery in its original gangue (the marble) in which it is found in nodules sometimes round and at other times fissured so as to represent angular fragments. In no place does it present anything like a vein, nor has it signs of stratification. The largest mass at this locality that I saw unbroken must weigh from thirty to forty tons.

Attached to this mineral, more especially in the fissures and on the surface, are several minerals that will be alluded to hereafter.

Kulah.—This locality of emery is the second in importance in Asia Minor, it is a town situated about a hundred and fifty miles from *Gumuch* and twenty miles from the ancient city of Philadelphia (one of the seven churches). It is near the river Hermes, and on that interesting volcanic district of Asia called *Catacecaumene* or the burnt country, resembling in many respects the volcanic region of Auvergne. The rocks forming the base of this region are of the older metamorphic series, covered to a greater or less depth by lava of different volcanic periods, which has flowed from the numerous craters that form the prominent feature of this region. The most common rocks in the mountain ranges about Kulah are white granular limestone, mica slate, hornblende schist, gneiss and granite; the last four are seen more conspicuously in the mountain two or three miles to the south, which have not been subjected to volcanic action; the limestone overlies these rocks.

Before arriving at the place where I examined the emery, (about two miles to the northeast of Kulah,) an outcropping of gneiss was seen and subjected to the closest scrutiny, without discovering the slightest trace of corundum; and I will here remark that although I have found several thin layers of mica schist engaged in the marble, in no instance was there any trace of corundum in it.

The marble in this region is very compact, of great hardness and I may also add of great purity. I cannot say whether this hardness is traceable to a greater depth than that to which it has felt the influence of the superimposed lava. Here again the emery was found on the surface, but not in such abundance as at *Gumuch-dagh*, and moreover the soil is not as deep as in the latter place. The emery as seen in the marble at Kulah is capable of being studied with the greatest satisfaction, particularly as two or three places in the rock have been quarried.

Adula.—Not far from this town which is about twelve or fifteen miles east of Kulah, I have also discovered emery, only, however, in very small quantity.

Manser.—About twenty-four miles north of Smyrna, emery is found in small quantity in the soil. In this as well as in the former place, white granular limestone is found.

Island of Nicaria, Grecian Archipelago.—I have also been able to examine thoroughly the emery of this island, which promises to be of importance to the arts. It is only within about twelve months that it has been brought to light. The mineral of this locality presents some peculiar features which will be alluded to hereafter. The geology is the same as that of the other localities already alluded to, namely, when found in contact with the rock it is always with limestone.

Island of Samos.—This locality has furnished me with only a few nodules imbedded in the soil with a little calcareous rock attached to the surface.

Island of Naxos.—This old and well known locality is here alluded to, simply because it has furnished me with specimens, the examination of which forms a part of this memoir. It is found in large blocks mixed with a red soil and also imbedded in white marble. It is taken principally from the north and east side of the island—the best comes from *Vothrie*, nine miles from the shore, and is embarked at *Sulionos*. Another good locality is at *Apperanthos*, seven miles from the shore, and it is embarked at a small port called Moutzona. In the south of the island it is found near *Yasso*. It is in such abundance on this island, that notwithstanding the immense quantity carried off it is not yet found necessary to quarry it from the rock.

Conclusions with reference to the Geology of Emery.

The localities at Gumuch-dagh and Kulah are those which afforded me the best means of studying the geology of emery, although in every instance I have found it associated with the old limestone overlying mica slate, gneiss, &c.

It is imbedded either in the earth that covers the limestone or in the rock itself; and exists in masses from the size of a pea to that of several tons weight, generally angular, sometimes rounded, and when in the latter form they do not appear to have become so by attrition.

The masses in the soil possess but little interest for the geologist, as they may have been left there by the decomposition of the rock, or been transported from other positions; still, the latter is difficult of supposition, in reference to what is found at *Gumuch-dagh*, for here it is only on the summit and not on the sides of the mountain that the emery has been traced. But having had the means of studying the emery and rock in contact, I have come to the firm conclusion, that the *emery has been formed and consolidated in the limestone in which it is found*, and that it has not been detached from older rocks as granite, gneiss, &c., and lodged in the limestone at the period of its formation. My reasons for so thinking are the following—

1st. In no instance could the closest investigation of the older rocks of these localities, that are below the limestone, furnish the slightest indication of the existence of emery there; and moreover the masses of emery in the limestone never had fragments of another rock attached to them. A few thin layers of mica slate were found in the limestone, but they were not in contact with the emery, nor contained any traces of corundum. I dwell thus much on this point, because in my specimens the calcareous rock in connection with the emery is under two forms; that of the original rock, and that formed by the infiltration of calcareous water in the fissures which exist near the surface.

2d. The limestone immediately in contact with the emery differs almost invariably in color and composition from the mass of the rock; and at *Kulah*, where the marble forming the rock is remarkably pure (as evinced by analysis), the part in contact with the emery is of a dark yellow color resembling spathic iron, and contains a large portion of alumina and oxyd of iron. The thickness of this interposing coat between the emery and the marble is variable; but what is certain, it passes gradually into white marble, so that their crystalline structures run into each other, showing that they are one and the same rock. Had the masses of emery been broken from an older rock and imbedded in the marble at its formation, there is no reason why the contact should not always be direct and immediate without this transition from ferro-aluminous limestone to pure marble. What we see is just what should be expected in ferruginous and aluminous minerals forming and separating themselves from a limestone not yet consolidated.

This kind of separation between the emery and the marble has been highly useful in the facility that it has indirectly afforded for exploring this mineral. It has been stated that at all the localities under consideration, but principally at Gumuch and Naxos, the emery exists in great abundance detached from the rock in a red earth; now this earth is simply the result of the decomposition of this heterogeneous calcareous envelope, which from its nature is easy of disaggregation by the influence of atmospheric agents. Had the emery been in immediate contact with the marble we could hardly have expected this spontaneous separation in so great a quantity.

I have in some instances seen small nodules of emery in small cavities in the rock but perfectly detached.

3d. The immense mass alluded to as covering several square yards of surface is another evidence of the emery having been formed in the limestone; for this mass does not consist of a single piece, but of a number of different sizes, not lying together irregularly, but with their contiguous surfaces more or less parallel, although removed a little distance from each other; in fact, it is just what we would expect in a large mass that for some cause or other had been fissured in various directions.

4th. Yet another circumstance to be remarked in connection with this part of the subject, is, that in the examination of the surface of contact between the emery and the rock, we do not always see it marked by a distinct outline; but the minerals constituting the emery as well as those associated with it, are more or less disseminated in the limestone at the point of contact; the value of this argument is better understood on examining the specimens in my possession.

Enough having been said to prove that the emery under consideration was formed within the limestone in which it is found, I will allude to the process of segregation which has given rise to this formation.

It would appear that the substances eliminated from the calcareous rock, were silica, alumina, and oxyd of iron, and that these three in the exercise of homogeneous and chemical attractions have given rise to the minerals which constitute and are associated with emery. In my collection, there is a specimen exhibiting this fact in a remarkable manner. It is a nodule, showing emery in the center, with two concentric layers, the inner of *chloritoid* and the outer of *emerylite;* the latter was in contact with the limestone.

Emery—mixture of corundum (alumina a little hydrated) and oxyd of iron.

Chloritoid—silica 24, alumina 40, oxyd of iron 28, water 7.
Emerylite— " 30, " 50, lime 13, water 5.

It is seen that in commencing from the external surface, in which direction we must regard the consolidation of the nodule, that the larger portion of silica eliminated has combined with a large portion of alumina and some lime to form a peculiar mineral; next, the remainder of the silica combines with an additional quantity of alumina and considerable oxyd of iron to form another mineral; and finally the remaining alumina and oxyd of iron crystallize separately. Facts of this kind in geology are not unfrequent, but they are always highly interesting and worthy of remark.

In concluding the geological considerations of emery with reference to the localities in Asia Minor and the neighboring islands, I would remark, that at some future time when the observations become extended, it will doubtless be found that the emery forms the geognostic mark of extensive calcareous formations in that part of the world, just as the flints do in the chalk of Europe.

Mineralogical position of Emery.

Emery has been considered by some as corundum, others suppose it represented by some rock or other, not always the same, in which corundum is disseminated in greater or less quantity; others again consider it a mixture of corundum and oxyd of iron. I am of opinion that the latter is the most correct manner of regarding this substance.

Emery properly speaking is not a simple mineral, but a mechanical mixture of granular corundum and oxyd of iron in which the former usually predominates. It has not the aspect of corundum disseminated in a rock, for it is found in distinct masses of different dimensions and of great hardness; and when broken giving way in the directions of fissures, which exist commonly in the mass. Most frequently there is no other evidence of the presence of corundum in emery but its hardness. The oxyd of iron present is always under the form of magnetic oxyd more or less mixed with oligiste; sometimes it is titaniferous. There are other minerals associated with the emery, all of which will be described hereafter.

The aspect of this substance differs more than is supposed, for until lately, the emery brought from Naxos has been the criterion by which to judge others. The localities that I have discovered furnish me with specimens showing considerable difference not only as regards color but also in the structure.

The *Naxos emery* is of a dark grey with a mottled surface, and with small points of a micaceous mineral disseminated in the mass. It frequently contains bluish specks or streaks which are easily recognized as being pure corundum.

The *Gumuch-dagh emery* is commonly of a fine grain and dark blue bordering on black, not unlike certain varieties of magnetic iron ores. With this variety we frequently find pieces of corundum of some size. The interior of the mass is tolerably free from the micaceous specks found in that of Naxos.

The *Kulah emery* is usually coarse grained, and much darker than that of Gumuch-dagh, its external surface resembling sometimes that of chromate of iron.

The *Nicaria emery* in many instances presents a schistose or lamellated structure to a very remarkable degree, so much so that certain specimens might pass for gneiss. The color is dark blue and somewhat mottled like that of Naxos. There is also much that is quite compact found in the same locality. The lamellated variety contains an abundance of a micaceous mineral, which in this instance appears to have determined its structure.

The *Samos emery*, as yet found only in small quantities, and in the form of nodules, is uniformly of a dark blue color, sometimes of a coarse grained and at other times of a fine grained structure not unlike certain varieties of very compact blue limestone.

Fracture.—The fracture of emery is tolerably regular, and the surface exposed is granular of an adamantine aspect; it is exceedingly difficult to break when not traversed by fissures or not of a lamellated structure as much of that from Nicaria. When reduced to powder it varies in color from that of a dark grey to black. The color of its powder affords no indication of its commercial value.

The powder examined under the microscope shows the distinct existence of the two minerals, corundum and oxyd of iron, which appear inseparable as the smallest fragment contains the two together.

Magnetism.—As it is natural to suppose all specimens of emery affect more or less the magnetic needle; in some the magnetism is barely perceptible, in others it amounts to strong polarity.

Odor.—Emery when moistened always affords a very strong argillaceous odor; even the most compact varieties.

Specific gravity.—The different varieties do not vary much in their specific gravity, it being always in the neighborhood of 4. The specific gravity of various specimens will be given on a following page.

Hardness.—The hardness of emery is its most important property, as to it is due the value of this substance in the arts. For this reason I have devoted much time and attention to the determination of it. In a mineralogical sense its hardness is not difficult to determine; for if we try different varieties of emery by scratching agate or other hard substance, the effect will naturally be very nearly the same; for in every case, it will be some point of corundum that has produced the scratch. If, however, we happen not to rub a point of corundum against the agate no effect will be produced on the latter, but the emery will yield. As this method leads to no practical result, I have sought out another, which may be properly called one for determining the *effective hardness of emery and corundum*, and is as follows.

Fragments are broken from the piece to be examined, and crushed in a diamond mortar with two or three blows of a hammer, then thrown into a sieve, (the one employed had 400 holes to the square centimetre,) the portion passing through is collected, and that remaining on the sieve is again placed in the mortar and two or three blows given, then thrown into the sieve; the operation is repeated until all the emery has passed through the sieve. The object of giving but two or three blows at a time is to avoid crushing any of the emery to too fine a powder.

Thus pulverized it is intimately mixed and a certain portion of it is weighed, (as I operated with a balance sensible to a milligramme, the quantity used never exceeded a gramme.) To test the effective hardness of this, a circular piece of glass about four inches in diameter and a small agate mortar are used. The glass is first weighed and placed on a piece of glazed paper; the pulverized emery is then thrown on it little by little, at each time rubbing it against the glass with the bottom of the agate mortar.

The emery is brushed off the glass from time to time with a feather, and when all the emery has been made to pass once over the glass, it is collected from the paper and made to pass through the same operation which is repeated three or four times. The glass is then weighed, after which it is subjected to the same operation as before, the emery being by this time reduced to an impalpable powder. This series of operations is continued until by repeated weighing the loss sustained by the glass is reduced to a few milligrammes. The total loss in the glass is then noted, and when all the specimens of emery are submitted to this operation under the same circumstances, we get an exact idea of their relative hardness.

The blue sapphire of Ceylon was pulverized and experimented with in this way; it furnished me with a unit of comparison by which to compare the results obtained. This operation is long but certain, and for the harder varieties of emery it is necessary to repeat the rubbing six or seven times and it requires nearly two hours for completion.

The results that I have obtained are interesting and have furnished me with the means of forming conclusions that I could not have otherwise come at.

Glass and agate have not been chosen for this experiment without a certain object, as experiments were first made with two pieces of agate, with two pieces of glass, and with metal and glass. The agates were found too hard, as they crushed the emery without producing hardly any abrasive effect; the others were found not to crush the emery sufficiently, making the experiment tedious and long. With the glass and agate we have a hard substance which crushes the emery, and in a certain space of time reduces it to such an impalpable state that it has no longer any sensible effect on the glass, and on the other hand, the glass is soft enough to lose during this time sufficient of its substance to allow of accurate comparative results. In the employment of this method in the arts, it would not be necessary to go to the sapphire for a standard of comparison; any good emery would answer the purpose quite as well.

It must be understood that this method of coming at the abrasive effects of emery does not furnish the mineralogical hardness of this substance, by which we understand the hardness of any individual particle, as evinced by its effect on a substance of less hardness, without regard to the molecular structure of the mineral. Two minerals possessing the same hardness but differing in structure, one being friable, and the other resisting, will be found very different in their abrasive effects; for instance, break a piece of quartz in two, subject one of the pieces to a white heat, and after cooling, compare the two by rubbing the point against some hard substance; both will be found to scratch equally well: then try the two in a state of powder, by rubbing them between two pieces of glass that have been weighed, and the difference of their abrasive effects will be found very great; because, the one subjected to the fire is exceedingly friable, and becomes readily crushed to an impalpable powder. This fact is eminently true with reference to emery, many specimens of which containing the same amount of corundum differ somewhat in their *effective* *hardness* owing to the more or less compact structure of the corundum.

By the method with the agate and glass I have found the best emery capable of wearing away about one-half its weight of the glass (that used was the common French window glass). The sapphire under the same circumstances wears away more than four-fifths of its weight. A tabular view of the results will be given a little farther on.

Chemical composition of Emery.

This substance consisting of a mixture of corundum and oxyd of iron in various proportions, it is easy to see what its composition must be. Yet the chemical examination of this mineral taken in connection with other properties is not devoid of interest.

For the purpose of analysis, the emery was reduced to a state of powder, in the manner alluded to in speaking of its hardness, with a diamond mortar and sieve. This powder was dried for twenty-four hours over sulphuric acid; a gramme was then weighed in a small platinum crucible of about one-fourth of a cubic inch in capacity, fitted with a cover that adapted itself well to it; this small crucible was placed in another of earth, and the space between the two filled with pulverized quartz which also covered the smaller one to the depth of half an inch. Common sand was not used, because during the heating some particles might adhere to the platinum crucible by a semifusion; nor was powdered charcoal employed because it protected the mineral no better than the pulverized quartz from contact with the air, at the same time a little risk was run in decomposing a small amount of the iron.

Thus arranged the crucibles were heated to a bright red for from thirty minutes to one hour. After cooling, the platinum crucible was carefully withdrawn and weighed. The loss furnished me with the amount of water in the emery.

It requires a continued red heat to drive out all the water, a circumstance which is true for a number of minerals, particularly for those containing a large amount of alumina as diaspore and the micas which will be spoken of in this paper.

The powder, of which the water has been estimated, was next submitted to levigation in a large agate mortar placed on a surface of glazed paper; and when completed, it was carefully detached from the mortar, placed in a platinum capsule, heated gently to drive off any hygrometric moisture and weighed; the increase of weight furnished the amount of silica taken from the mortar.

The levigation of one gramme was accomplished in two operations, each requiring about twenty minutes; and by using a mortar of convenient size and the extremity of a feather or a small brush, it is possible to lose but an insensible quantity of the mineral and to estimate with sufficient precision the amount of silica abraded from the mortar.

Another method by which I accomplished the levigation in some of the analyses, was in a steel mortar of the same form as the agate mortar; and when completed the powder was placed in a glass with nitric acid diluted with thirty times its weight of water and left in it for one hour agitating it occasionally. The iron taken from the mortar was dissolved, and no part of the mineral attached. The next thing was to filter and continue the analysis with the substance thus freed from the iron of the mortar, without any second weighing.

Of these two methods I preferred to employ the first for the emery, as it is more expeditious and almost if not quite as exact as the second. There are, however, occasions in which the steel mortar should be resorted to.

The substance once reduced to an impalpable powder, it was necessary to render it *completely* soluble, and my researches to arrive at this were long and tedious. In trying the various known methods the most successful was found to be that with a mixture of carbonate of soda and caustic soda heated to whiteness for one hour; nevertheless I could not obtain a complete decomposition. The decomposition might probably be completed if the levigation was made more thoroughly, but it is easy to understand, that with a large number of analyses of the same substance to make, it was a desideratum on my part not to consume the best part of a day in the levigation of a single gramme; particular, as I did not wish to confide this operation to another, as much care was required to lose nothing during the levigation. Mixed with carbonate of baryta and heated in a forge, the decomposition of the mineral was far from being complete; the same may be said for the treatment with the caustic alkalies in a silver crucible.

The bisulphate of potash decomposes it almost entirely by a single operation, but unfortunately, a double salt of potash and alumina is formed which is almost insoluble in water or in the acids, and it is only by a solution of potash that it is first decomposed and afterwards redissolved. I will not stop to detail all the disadvantages attending this method, but will at once speak of the method which gave me very easily the most accurate results.

It is by means of the bisulphate of soda that all my analyses of emery, of corundum, and of several aluminates were made. I believe that I am the first who has shown the great advantage of using this double salt in the decomposition of certain substances insoluble in the acids; and very probably it will replace in most cases the use of the bisulphate of potash in analytical chemistry. At present, all the advantages that may arise from the substitution of the soda for the potash salt cannot be mentioned; all that I will say is, that the former in giving a decomposition at least as complete as the latter, furnishes a melted mass quite soluble in water, and in the future operations of the analyses there is no embarrassment from a deposit of alum.

The bisulphate of soda was prepared by adding an excess of pure sulphuric acid to the pure carbonate or neutral sulphate of soda and heating it in a capsule until all the water had been expelled and sufficient of the acid to allow of the mass becoming solid on cooling. That obtained in commerce is not sufficiently pure.

The pulverized emery is placed in a large platinum crucible with six or eight times its weight of bisulphate of soda, and the mixture is heated over a lamp in the same manner and with the same precautions as are employed when using the bisulphate of potash. From fifteen to thirty minutes suffice for the operation. The mass is allowed to cool, and water with a few drops of sulphuric acid are added to it and the whole heated, when it soon dissolves with the exception of a little silica, that renders the solution milky, and a small quantity of undecomposed mineral, that is readily detected by rubbing a glass rod against the bottom of the capsule. The liquid is now filtered, and the filter is washed once with a little water; then with its contents it is placed in a platinum crucible, burnt completely, and the residue is heated with a little bisulphate of soda, which completes the decomposition: and when treated with water and a drop or two of sulphuric acid all except the silica is dissolved. The liquid which passes the filter in this case is added to the first and the analysis continued. The silica obtained is diminished by the quantity taken up from the mortar in order to arrive at what is actually contained in the mineral. The filtered solution is heated with a little nitric acid to convert all the protoxyd of iron into peroxyd, then treated with an excess of caustic soda and a little carbonate of the same alkali; this redissolves the alumina first precipitated and thus separates it from the oxyd of iron and a trace of lime. The iron and lime are separated in the ordinary way; the alkaline solution of alumina was acidulated and the alumina precipitated with carbonate of ammonia.

Thus analyzed, the emery from different places gave the following results:—

No.	Localities.	Effective hardness. Sapphire 100	Specific gravity.	Chemical composition.					
				Water.	Alumina.	Oxyd of iron.	Lime.	Silica.	Total.
1	Kulah,	57	4·28	1·90	63·50	33·25	0·92	1·61	101·18
2	Samos,	56	3·98	2·10	70·10	22·21	0·62	4·00	99·03
3	Nicaria,	56	3·75	2·53	71·06	20·32	1·40	4·12	99·43
4	Kulah,	53	4·02	2·36	63·00	30·12	0·50	2·36	98·34
5	Gumuch,	47	3·82	3·11	77·82	8·62	1·80	8·13	99·48
6	Naxos,	46	3·75	4·72	68·53	24·10	0·86	3·10	101·31
7	Nicaria,	46	3·74	3·10	75·12	13·06	0·72	6·88	98·88
8	Naxos,	44	3·87	5·47	69·46	19·08	2·81	2·41	99·23
9	Gumuch,	42	4·31	5·62	60·10	33·20	0·48	1·80	101·20
10	Kulah,	40	3·89	2·00	61·05	27·15	1·30	9·63	101·13

I ought to mention that the analysis afforded other substances in small quantities in some of the emeries; as titanic acid, oxyd of manganese, oxyd of zirconium, and sulphur (existing in pyrites); but these substances are unimportant in the composition of emery, and are in such minute quantities, that it is necessary to operate on a considerable quantity of the mineral to obtain satisfactory results concerning them.

The analyses marked 6 and 8 were made by decomposing the emery as it came from the sieve, without pulverization in the agate mortar. It was by accident that it occurred and I was not aware of the neglect until it was fused with the bisulphate of soda, but not wishing to lose the analysis, the operations were continued as in the other cases, only using a little more of the bisulphate in the second decomposition; and somewhat to my surprise, the decomposition was quite as perfect as in the other cases. I had nearly completed all my analyses in the manner detailed, when this fact became known, so that I have but these two cases to report. It will simplify the analysis of corundum if pulverization in a diamond mortar be found sufficient, and I propose examining specially into this question.

The water which was found in the emery comes from the corundum, a fact which will be shown when the analysis of pure corundum is given, which will be in the second part of the memoir. A very minute quantity of what has been estimated as water might be a little oxygen lost by the oligiste which is sometimes found in emery. Those emeries which contain the least water, every thing else alike, are the hardest, as instanced by that from Kulah, notwithstanding the quantity of iron it contains. The silica existing in emery is most often in combination with alumina or the oxyd of iron or with both, for this reason we must not always regard the quantity of alumina as an indication of the quantity of corundum in the emery.

Analogies.

Emery at first sight may be confounded with several ores of iron; as magnetic iron, certain varieties of oligiste and sometimes with chromate of iron; but the fracture of emery is stony which differs from these ores of iron, and besides the surface exposed is of a lighter color. From the numerous observations made, I may set it down as a general rule, that any blackish or dark blue rock of a strong argillaceous smell, that scratches agate easily, with a specific gravity in the neigborhood of 4, is sure to be emery.

The mining of Emery.

The mining of this substance is of the simplest character. The natural decomposition of the rock in which it occurs facilitates its extraction. As has already been mentioned, the rock decomposes into an earth in which the emery is found imbedded. The quantity found under these favorable circumstances is so great that it is rarely necessary to explore the rock. The earth in the neighborhood of the blocks of emery is almost always of a red color, and serves as an indication to those who are in search of the mineral. Sometimes before beginning to excavate, the spots are sounded by an iron rod with a steel point, and when any resistance is met with, the rod is rubbed in contact with the resisting body, and the effect produced on the point enables a practised eye to decide whether it has been done by emery or not.

The blocks which are of a convenient size are transported in their natural state, but most frequently they are required to be broken by means of large hammers; when they resist the hammer, they are subjected to the action of fire for several hours, and on cooling they most commonly yield to blows. It, however, happens sometimes that large masses are abandoned from the impossibility of breaking them into pieces of a convenient size; as the transportation either on camels or horses requires that the pieces do not exceed one hundred pounds.

At Kulah, the quantity of emery detached from the rock was not very considerable, as it had been protected from decomposition by the beds of lava that cover it. Here the marble was quarried to get at the emery which was done in the early part of 1847 with profit, although the transportation from Kulah to Smyrna is over a distance of one hundred and ten miles on the backs of camels. Since the diminution of the price of emery, this mine has been abandoned, for the quarrying into the marble is attended with the greatest difficulty as the tools used for boring, &c., are thrown out of use in a very short time, by the pieces of emery which are encountered at every instant. At present all the emery sent from Asia Minor comes from the mine at *Gumuch-dagh*, twelve miles from the ruins of *Ephesus*.

Commercial consideration of Emery.

The use of emery in the arts is of very ancient date, a fact proved by works on hard stones that could not have been executed except by emery or minerals of that nature. It is very probable that emery coming from the localities which have been mentioned, was used in former ages by the Greeks and Romans. For example, the locality of *Gumuch-dagh* is immediately by the ancient Magnesia on the Meandre, and between Ephesus and Tralles, twelve miles from each of these cities, and the same distance from Tyria; in all of these cities the arts flourished, and none more than that of cutting hard stones, if we are allowed to judge from the specimens of their skill in this art that have come down to us.

Nevertheless, the quantity of emery formerly employed was insignificant in comparison to the quantity now required, more particularly within the last twenty years, since the use of plate glass has been extended. The annual consumption at the present time is about *fifteen hundred tons.*

For various reasons, the island of Naxos furnished for several centuries almost exclusively the emery used in the arts, as much for the facility with which it was obtained as for the uniformity of its quality. The emery exists in very great abundance on this island, and notwithstanding the quantity already extracted there still remain immense deposits of it.

The price of this substance at the end of the last century was from 40 to 50 dollars the ton, and between 1820 and 1835 it was at times even less. About this period, the monopoly of the Naxos emery was purchased from the Greek government by an English merchant, who so regulated the quantity given to commerce that the price gradually rose from 40 to 140 dollars the ton, a price at which it was sold in 1846 and 1847. It was at this time that I commenced examining and developing the emery formations of Asia Minor until then unknown. And after making a report to the Turkish government, the monopoly of the emery of Turkey was sold to a mercantile house in Smyrna, and since then the price of this article has diminished to 50 and 70 dollars the ton according to the quality. I speak of the prices in the English market.

The different mines explored are those of *Naxos* of an ancient date, of *Kulah* commenced in 1847 and now abandoned for those nearer the sea, of *Gumuch-dagh* commenced in 1847 and worked largely, and of *Nicaria* commenced in 1850. From all these different places the emery goes to Smyrna, and from there, principally to England, the vessels taking it at a very low price as it serves for ballast.

The various mines belong to the Turkish and to the Greek government. The Greek government now sells its emery in lots of several tons. The Turkish government sells the entire monopoly of its mines, and consequently its operations are controlled by a single interest; but in all probability, this monopoly will be done away with, in virtue of a commercial treaty existing between Turkey and the other powers. If this takes place the price of emery will be still farther diminished.

Of the different varieties of emery employed in the arts that of *Naxos* is still preferred, and with reason, as it is more uniform in its quality than that coming from *Kulah* and *Gumuch*; nevertheless, if the best qualities of that from the island of *Nicaria* are found in abundance and that only sent into market, it will prove at least equal if not superior to that of Naxos.

Art. XXXVI.—*On American Spodumene*; by Geo. J. Brush, of the Yale Analytical Laboratory.

Read before the American Association for the Advancement of Science at New Haven, August, 1850.

Owing to the want of a complete analysis of an American Spodumene, I was induced at the suggestion of Prof. Silliman, Jr., to undertake this research.

The Spodumene from Utö has often been the subject of chemical investigation and has been analyzed by Arfvedson,* Stromeyer,† Regnault,‡ and Hagen.§ That from the Killiney locality has been analyzed by Thomson.‖

These are all the complete analyses recorded of this species. Partial analyses, however, exist of specimens from the Tyrol mountains, and from Sterling, Mass., the former by Hagen and the latter by both Hagen and Bowen.¶

The constitution of this mineral was not correctly understood prior to Hagen's analysis, until which time it had been considered as essentially a silicate of alumina and lithia. Hagen however found a portion of the so-called lithia to be soda, which discovery being confirmed renders the formulas derived from former analyses incorrect, owing to the great difference in the atomic weights of lithia and soda. Hagen's analysis of a specimen from the Utö locality gave,

			Oxygen.		Ratio.	
Silica,	66·136	=	34·36	34·36	12·26	12
Alumina,	27·024		12·63	12·72	4·55	$4\frac{1}{2}$
Peroxyd of iron,	·321		·09			
Lithia,	3·836		2·11	2·79	1	1
Soda,	2·683		·68			
	100·000					

from which he deduced the formula, $\dot{Na}\,\dddot{Si}+3\dot{L}i\,\dddot{Si}+6\ddot{\overline{A}}l\,\dddot{Si}^2$.

My analyses agree with Hagen's in the soda, but lead to a different formula. The specimens selected for analysis were from the Norwich and Sterling (Mass.) localities. A qualitative examination of each, showed the presence of silica, alumina, peroxyd of iron (trace), lime, lithia, and soda.

In the quantitative examination the alkalies were obtained by decomposition by hydrofluoric acid and determined as sulphates; the other constituents were obtained by fusion with carbonate of soda. That from Norwich in two analyses yielded,

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* Schweigger's Jour., xxii, 107.
† Untersuchungen, i, 426.
‡ Ann. des Mines, (III series) 1839, 580.
§ Pogg. Ann., xlviii, 371.
‖ Thom. Min., i, 302.
¶ Sill. Am. Jour., viii, 121.

	I.	II.	Mean.	Oxygen.		Ratio.
Silica,	63·06	62·72	62·39	32·67	32·67	8·04
Alumina,	28·00	28·85	28·42	13·28	13·28	3·27
Lime,	·95	1·13	1·04	·29		
Lithia,	5·67	5·67	5·67	3·12	4·06	1
Soda,	2·51	2·51	2·51	·65		
	100·19	100·88				

And that from the Sterling locality of which also two analyses were made, gave,

	I.	II.	Mean.	Oxygen.		Ratio.
Silica,	62·86	62·67	62·76	32·61	32·61	7·80
Alumina,	28·83	29·83	29·33	13·75	13·75	3·28
Lime,	·56	·71	·63	·18		
Lithia,	6·48	6·48	6·48	3·56	4·19	1
Soda,	1·76	1·76	1·76	·45		
	100·49	101·45				

The mean of the ratios calculated from the four analyses is 1 : 3·27 : 7·92 or quite nearly 1 : 3 : 8, which gives the general formula

$$\dot{R}^3\,\ddot{Si}^2+3\dddot{Al}\,\ddot{Si}^2$$

and the special formula

$$(\cdot0570\,\dot{Ca}+\cdot1333\,\dot{Na}+\cdot8097\,\dot{Li})^3\,\ddot{Si}^2+3\dddot{Al}\,\ddot{Si}^2$$

which requires,

8 atoms of	silica,	4618·48	= pr. ct.	64·14
3 "	alumina,	1925·40		26·76
2·4291 "	lithia,	441·27		6·12
·3999 "	soda,	154·84		2·15
·171 "	lime,	60·10		·83
		7200·09		100·00

This formula corresponds quite well with the analyses, especially in the protoxyd bases, the mean of which is almost precisely that required by the formula.

With the specific gravity 3·18 we obtain from the above the atomic volume 2264. The B atomic volume (see Mr. Dana's memoir, in this Journal, ix, 220) will be 161·7, and the C atomic volume 42·7. The isomorphism of this species with pyroxene is pointed out by Mr. Dana on page 120 of this volume.*

* The analysis by Mr. Brush corresponds in the proportion of silica with the results of Stromeyer's investigation, who found 63·288 of silica and 28·776 of alumina. In the ratio above deduced, as well that of von Kobell's formula ($\dot{R}^3\,\ddot{Si}^2+4\dddot{Al}\,\ddot{Si}^2$), the oxygen of the bases is to that of the silica as 1 : 2, the same ratio as in pyroxene.—J. D. D.

Art. XXXVII.—*Optical Examination of several American Micas*; by B. Silliman, Jr., A.M., M.D., &c.

Read before the American Association for the Advancement of Science, at New Haven, August, 1850.

Prior to the publication of the second edition of Dana's Mineralogy, little had been done in distinguishing the several species among American micas, and in allotting them to the various localities. In connection with Professor Dana, the writer, during the passage of the Mineralogy through the press, made a number of observations respecting the optical properties of such micas as were at that time accessible. A summary of these observations will be found in that volume.* Since that work was published, the writer has continued and multiplied his observations as far as opportunity has been found for prosecuting the investigation, while former examinations have been revised. The results of the whole research as far as they are complete are exhibited in the following tables.

Much yet remains to be done, not only in confirming and extending the present measurements and adding new ones from unexamined localities, but still more in reference to the chemical character of the several compounds, which from their great resemblance in leading physical properties have hitherto been generally confounded under a common designation. This branch of the enquiry is far the most laborious, requiring a large number of rigorous chemical analyses. A beginning in it however, has been made by Mr. Craw of the Yale Laboratory, who has completed three analyses of Phlogopites from New York. The results of his research, which are particularly interesting, will be found on a following page.

The physical questions connected with the micas embrace also the translucency of the several varieties in different directions, the effects of heat and magnetism in varying the angle of the optic axes, and the value of the latter under monochromatic light in all parts of the spectrum;—and investigations on these points would well reward the observer.† I had proposed the subject last-mentioned, to my friend, Mr. W. P. Blake, before my own observations were made, and he has recently planned and constructed for himself an instrument for observations and measurements of this sort. This instrument appears to me particularly

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* Dana's Mineralogy, p. 690.

† A few experiments were made by the author, aided by Mr. W. P. Blake, with Melloni's apparatus, to determine whether any relation in the transmission of heat existed between various micas corresponding at all to the different values of the optic axes. In these trials the mica plates were as nearly as possible of the same thickness, and they were placed so that the normal was parallel to the bundle of rays of

well adapted for this purpose, and with its aid we may hope for important advances in our knowledge of the physical relations of the micas.*

The instrument which I have used for the measurements given in this paper is a modification of the goniometer of Charles and Malus. It has a horizontal circle of about eight inches diameter reading to minutes, with a tangent screw and double readings. To the centre of the instrument has been adapted a simple contrivance for holding two tourmalines, and at the same time for securing the mica plate in the proper position. The tourmalines have both a horizontal and rotary movement, and are so arranged that the mica plate can be conveniently held between them in an unvarying position while the arm of the goniometer makes its revolution. The instrument is adjusted for use by bringing the specimen into such a position that the line connecting the optic axes shall be horizontal; and by turning the arm of the instrument through the requisite number of degrees, the two series of colored rings of a common binaxial mica will come successively into view. A vertical cord placed in an open window is required to complete the arrangement; the instrument is so adjusted, that the cord accurately intersects the black dots of the inner colored circle about one axis; a revolution is then made, till the cord intersects

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heat. The instrument was so adjusted that the Locatelli lamp deflected the needle in 10″ of time 30° of the scale. Thus arranged, the following results were obtained:

Mica examined.	Optic angle.	Color.	Needle deflected.	Per cent. of rays transmitted.
Muscovite of Grafton, . . .	69° 30′	light brown, ...	19°–20°	57–60
Phlogopite, Pope's Mills, .	7° 30′	white glassy, ...	12°-11°30′	36–34·5
" " "	15°	brownish yellow, ...	15°	45
" Edwards, . . .	13° 30′	yellow brown, ...	15°	45
Biotite (?) Topsham, Me., (*probably Phlogopite.*)	. . . .	deep reddish brown, . .	13°–12°	39–36
Biotite, Monroe, N. Y., . .	. . . .	dark green, almost black,	11°	33
Muscovite, Royalston, Ms.,	57° 30′	dark brown, ...	21°	63
" Paris, Me., . . .	72° 30′	nearly colorless clear, . .	21°-21°30′	63–64·5
" Brunswick, Me.,	72° 30′	light brown, ...	21°-21°30′	63–64·5
" Jones Falls, Md.,	67°	dark green, ...	18°–19°	54–57
" Philadelphia, . .	60° 30′–61°	banded in hexag'l figures,	21°36′-22°	64·5–66

When the crystal was placed so that the rays of heat passed parallel to the optic axis, (thus the Grafton mica was placed at an angle of 34° 30′, the arrangement remaining otherwise as before) the needle was on repeated trials, deflected 24°, equal to 72 per cent. of all the rays passing while in the other position, (or with the normal parallel to the rays) only 60° passed.

From these few trials (which are regarded as only preliminary and approximative,) it will be seen that an interesting relation apparently subsists of the sort looked for, and this last experiment is particularly worthy of confirmation by extending it to numerous varieties.

* Mr. Blake presented his instrument and a series of measurements made with it to the Physical Section of the Am. Assoc., at the New Haven meeting.

in the same manner the other axis; the amount or angle of this revolution is the angle between the axes. With this arrangement there is no difficulty after a little practice in obtaining a series of measurements on the same specimen, varying from each other but a few minutes at most, without having recourse to lenses or other means of more accurately defining the field of observation or reducing the area of the colored circles. Such modes of greater accuracy are important for the more delicate physical questions previously suggested; but for the purpose of mineralogical determination, the means just described are quite sufficient, since it is shown that in a series of specimens from the same locality there is generally a difference of angle greater than any error of observation arising from the imperfection of the instrument employed.

Additional interest is given to this enquiry from a comparison of the chemical relations of the various species of mica and their corresponding differences in optical characters. For this reason we briefly recapitulate the divisions which are adopted by Prof. Dana in the late edition of his system, and which are also given with a recapitulation of the chemical formulas on p. 118 of this volume. The species of mica now recognized are muscovite, margarodite, emerylite, euphyllite, margarite, lepidolite, phlogopite and biotite. Of these, all but the last are binaxial. Our observations will be confined mainly to muscovite, lepidolite, phlogopite, and biotite.

1. *Muscovite.*—This name has been proposed by Dana to embrace those binaxial micas whose angle of polarization is between 55° and 75°, excepting however the lithia micas which, having a peculiar composition and a very high angle, are included under the species lepidolite. The terms "oblique mica" "common mica" and "binaxial mica" formerly applied to this species now fail to be distinctive, since we have other oblique and binaxial micas which belong to different species. The optic axes in this species lay in the direction of the longer diagonal of the prism. It is much the most abundant variety and is commonly found in granitic rocks.

2. *Lepidolite.*—This species embraces all the lithia micas, a group presenting however varied chemical characters which will probably be subdivided by future research. They are all binaxial and as far as observed they yield a higher angle than any other of the species of this family, being 75°–76°. The blowpipe reaction for lithia as well as its high polarization angle, enable this species to be very readily distinguished. Many of the varieties are easily recognized by their rosy or peach-blossom color.

3. *Phlogopite.*—This name was first proposed by Breithaupt for the yellowish brown mica associated with serpentine which

is found at Natural Bridge, near Diana in Jefferson county, New York. This species is distinguished by a polarization angle between 7° and 18°, the angle most commonly observed, being 13°–16°; it rarely falls below 10°; in all cases the two axes are so near that both can be distinctly seen in the field at one view, and if examined in thin plates and by a casual observer, it would be esteemed a uniaxial mica. The crystalline form is trimetric, and it occurs often in elongated and tapering hexagonal prisms, sometimes of enormous size as in the well known individuals from Pope's Mills—specimens of which in the writer's possession are 5 by 8 inches in thickness and perfect in form. The color is usually yellowish brown, bronzy yellow, and deep copper red, sometimes greenish yellow and rarely white. Its cleavage resembles that of muscovite, but the laminæ are not generally so elastic. In chemical constitution it is a distinct compound although but few analyses have yet been made of this species. Like the biotite it is remarkable for the amount of protoxyd bases which it contains and the small quantity of alumina—giving for the ratio of the oxygen of its protoxyds, alumina and silica, as deduced by Rose, $18 : 12 : 30 = 1 : \frac{2}{3} : 1\frac{2}{3}$, (more exactly $7 : 4 : 11$, according to Craw,) while in the muscovites it is generally $1 : 12 : 16$. Its localities are much more numerous than was at first supposed; they abound particularly in northern New York, in Canada, and in Morris and Sussex counties in New Jersey. One of the most noted localities of this species is Edwards in St. Lawrence county, N. Y., where it is found both colorless, of an eminently silvery luster, and also of a rich brownish yellow color.

4. *Biotite.*—This species includes the uniaxial or hexagonal micas. Most of the varieties of this species are of a dark color—often black or greenish black and transparent only in very thin laminæ. Owing to this prevalent dark color it is often difficult or quite impossible to obtain satisfactory evidence of the optical character, and there is little doubt that some localities quoted in this article as furnishing uniaxial micas, should be in fact classed among the phlogopites. Only one American variety of this species has yet been analyzed—viz., that from Monroe, N. Y., by von Kobell. They are generally magnesian micas and have for the oxygen ratio of their protoxyds, alumina, and silica, the ratio $1 : 1 : 2 = \dot{R}^3 \dddot{Si} + \bar{\bar{R}} \dddot{Si}$. This species and those anomalous specimens which are classed under it in the present article, but which probably belong elsewhere, offer interesting subjects for chemical examination.

Beside the *phlogopites* and *biotites*, properly so called, there are several micas which have fallen under my observation in this research which are anomalous in character. These present under the influence of polarized light an elliptical colored image, in which however it is not possible to bring out clearly the two poles of a binaxial mica, nor, on the other hand, the symmetrical cross of a uniaxial crystal. The divergence is too constant and too regular to allow the supposition that the ellipticity is due to a mal-position of the laminæ or to a separation between the thin plates (remarked on as a cause of irregularity in certain crystals by Biot). The divergence of the axes in these exceptional cases is too decided not to attract the attention of the experienced observer, and still these specimens would probably, by most persons, be set down as uniaxial, especially in thin plates. Indeed many phlogopites when viewed in thin plates appear so nearly uniaxial as scarcely to excite attention to their binaxial character, while in plates of suitable thickness they are easily measured. Those doubtful cases now under consideration are probably referable to an oblique crystalline form, but even here the study of a large number of specimens from the same locality is required before satisfactory inferences may be drawn. To this head I refer the deep reddish copper-colored mica from Franklin Furnace, Sussex Co., New Jersey, which is found in white dolomite; also a mica of similar character from St. Jerome, Canada; and that, well known to collectors, from Moriah in Essex Co., N. Y., which is more remarkable than any I have seen for its deep smoky red color as seen by transmitted light. But the most interesting specimens of this sort observed by me, are certain large crystals of a deep olive green color from the Yale College Cabinet, and which in our investigations have been referred to Monmouth, N. J., although their true locality is still doubtful. This mica is in very large rhombic crystals oblique from an *obtuse* edge. P : M = 112° - 115½°, M : M = 122° - 125°, the angle of the basal edges is 119° 30′. Plane angle of P 119°. It has a cleavage parallel to the longer axis. The obliquity of the optic axes appears to be nearly as great as that seen in some phlogopites of equal thickness, but the dark color of the mineral prevents a satisfactory examination. Should the character of this mica be confirmed by a set of good analyses, it must in all probability form a distinct species as suggested by Dana.* This variety is not to be confounded with the well crystallized mica of Greenwood furnace which, as seen in ordinary specimens, is oblique from the acute edge (sections of distorted acute rhombohedrons) and which is regarded as a uniaxial mica.

Euphyllite, margarodite and emerylite have hitherto been found in quantities too inconsiderable and in specimens generally too poorly crystallized to furnish many measurements.†

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* Mineralogy, p. 690.

† For the composition of these species, see Dana's Mineralogy, and also this volume, pages 114–118.

TABLE I.—*American Muscovite and Lepidolite Micas. Polarization angle from 55° to 76°.*

Locality of Mica.	From whom received.	Color, Form and Remarks.	Angles of axes.
New York Island, 4 m. from City N. Y.	New York Lyc. Nat. Hist., ...	violet gray, with black grains disseminated in it, ...	56° 20′–56° 40′
Royalston, Mass., ...	E. Hitchcock, Jr., ...	dark brown, fine crystal; locality of the Beryls,........	57° 30′
ib. ib.	ib. ib., ...	ib. ib. another specimen, ...	58°–59°
Pennsbury, Penn., ...	T. Seale, ...	smoky brown, striated; color in blotches, ...	59°
Philadelphia, Penn., ...	Lederer Cabinet, T. Conrad, ..	greenish gray, banded; alternate bars of color, ...	60° 30′–61°
ib. ib. near Fairmount, ...	T. Seale, ...	smoky brown; resembles Pennsbury mica, ...	62°–62 30′
Oxford, Maine, ...	Dr. True, ...	light brown; perfect crystals transparent in transverse direction, ...	62° 42′–63°
Monroe, Conn., ...	Lederer Cabinet, ...	brown with patches, of dark brown mottled, ...	64° 30′–65° 30′
Royalston, Mass., ...	Cambridge Cabinet, by Prof. Lovering,	violet brown; in thick plates, ...	65°
Local. ?	C. M. Wheatley's Cabinet, ...	greenish gray; in perfect crystals, ...	65° 30′–66°
Falls road, 2½ m. from Baltimore, ...	T. Seale, ...	transparent brown, with scales of green, ...	65° 30′–65° 40′
Near Ellicott's Mills, Md., ...	ib. ib., ...	ib. ib. Baltimore and Ohio Railroad, ...	66° 30′
"Jones' Falls," near Baltimore, ...	G. Gibbes and W. S. Vaux, ..	blackish green; symmetrically banded, with dark grains of chrom. iron ?	66° 15′–66° 30′
Greenfield, Conn., ...	Yale College Cabinet, ...	greenish yellow, in granite boulder, ...	66° 30′–67°
Haddam, Conn., (Quarry Hill,)	Mr. Burr, ...	clear brownish green, in granite, ...	67°
Grafton, New Hampshire, ...	Bowers, ...	light brown, transparent, ...	67° 30′
Unionville, Penn., ...	T. Seale, ...	white; corundum locality, ...	67°–67° 28′
Ackworth, N. H., ...	Yale College Cabinet, ...	greenish gray, in granite, ...	67° 15′–67° 30′
Grafton, N. H., another specimen, ...	Bowers, ...	light brown, with flattened quartz and tourmaline, ...	68° 5′–68° 20′
Templeton, Mass., ...	E. Hitchcock, Jr., ...	transparent brown, ...	69° 30′–69° 40′
Orange, Mass.	ib. ib., ...	ib. ib. beautiful crystals, ...	69° 30′–69° 40′
Willimantic falls, Conn., ...	....	brownish green, transparent, in granite, ...	69° 30′–69° 50′
Pennsbury, Penn., ...	T. Seale, ...	brown crystals; another locality, ...	69° 27′–70°
Royalston, Mass., ...	E. Hitchcock, Jr., ...	dark brown; 2d locality, ...	69° 40′–70°.

Table continued.

Locality of Mica.	From whom received.	Color, Form and Remarks.	Angles of axes.
Grafton, N. H.,	Bowers,	light brown; 3d specimen,	69°–69° 30′
Middletown, Conn.,	J. D. Dana,	brownish; feldspar quarry,	70°–70° 30′
Chester, Hampden Co., Mass.,	Yale College Cabinet,	greenish white, in granite boulder,	70°–70° 30′
Norwich, Mass.,	E. Hitchcock, Jr.,	greenish yellow; spodumene locality,	70° 30′
Pennsbury, Penn., (3d Local.,)	T. Seale,	brownish green; in coarse granite,	70°–70° 30′
Goshen, Mass.,	Yale College Cabinet,	greenish yellow; with spodumene,	70°–70° 30′
Greenfield, N. Y.,	Mr. Powers,	brownish; chrysoberyl locality,	70° 45′–71°
Haddam, Conn.,	Mr. Burr,	brownish; in large plates,	70°
Gouverneur, N. Y.,	Dr. B. F. Hough,	brownish-white, in boulder,	70°
Templeton, Mass., (2d spec.,)	E. Hitchcock, Jr.,	transparent brown,	70° 15′
Leiperville, Del. Co., Pa.,	T. Seale,	faint greenish; plicated plates,	70° 30′–71°
Jefferson Co., N. Y.,	Dr. B. F. Hough,	greenish; in a boulder,	71°–71° 30′
Hebron, Maine,	Dr. True,	light brown transparent, in thick masses,	71° 40′–71° 50′
Norwich, Mass.,	E. Hitchcock, Jr.,	yellowish green transparent; 2d specimen,	71° 45′
Haddam, Conn.,	Myself,	ib. ib.; columbite locality,	71° 30′–71° 45′
E. Chester, Westchester Co., N. Y.,	Yale College Cabinet,	ib. ib.; boulder,	71° 30′–72°
Paris, Maine,	Dr. True,	ib. ib.	72° 15′–72° 30′
ib. ib.,	Prof. O. P. Hubbard,	ib. ib.; another specimen,	72° 30′
Brunswick, Maine,	Prof. Lovering,	whitish brown; silvery mica,	72° 37′–72° 50′
Gouverneur, N. Y.?	Cambridge Cabinet,	fair rosy color; no lithia reaction,	73°–73° 5′
Paris, Maine,	Yale College Cabinet,	whitish green; with green tourmaline,	74°–74° 30′
ib. ib.,	ib. ib.	ib. another specimen,	73°
Orange, N. H.,	W. P. Blake,	gray, with flattened tourmaline, quartz and feldspar,	73°–74°
Pounal, Maine,	Dr. True,	nearly colorless; lithia? mica,	74° 50′–75°
Goshen, Mass.,	Yale College Cabinet,	yellowish green; with indicolite,	75°
ib. ib.,		another specimen,	75° 30′–76°
Lenox, Mass.,	Lederer Cabinet,	rose colored lithia mica; with albite,	75°–75° 30′

TABLE II.—*American Phlogopite Micas.*

Locality of Specimen.	Whence obtained.	Color, Form and Remarks.	Angle.
Pope's Mills, St. Lawrence Co., N. Y.,	Lederer Cabinet, ...	a glassy transparent crystal, about ¼ inch thick, ...	7°–7° 30′
Edwards, N. Y., ...	C. M. Wheatley's Cabinet, ...	rich reddish brown in large clear plates and very unlike either of the other varieties from Edwards; resembles the Franklin, N. J., specimens, ...	10° ?
St. Lawrence Co.? N. Y., ...	Lyc. Nat. Hist., N. Y., ...	yellowish, ...	10° ?
Vrooman's Lake, N. Y., ...	Dr. B. F. Hough, ...	in long crystals of a yellow color; resembles the mica of Natural Bridge, ...	10° 30′–10° 50′
Edwards, N. Y., ...	ib. ib., ...	rich yellowish brown color, ...	11°
Warwick, Orange Co., N. Y., ...	C. M. Wheatley's Cabinet, ...	in limestone, yellowish, slightly transparent, uneven, ...	11° ?
Falls of the Grande Calumette, Canada,	T. S. Hunt, ...	yellowish green, in limestone with pyroxene and idocrase; crystals sometimes many inches long; eminently beautiful mica, ...	13°–13° 12′
Pope's Mills, St. Lawrence Co., N. Y.,	Dr. Crawe, ...	large crystals of a fine yellowish brown; specimen measured 9 by 5 inches, ...	13° 30′
Edwards, N. Y.; 2d specimen, ...	ib. ib., ...	yellowish brown, ...	13° 30′
Churches Mills, Rossie, N. Y., ...	Dr. Hough, ...	resembles the Pope's Mills; yellowish brown, ...	13° 30′–14°
Near Skinner's Bridge, Rossie, N. Y.,	ib. ib., ...	silvery yellow mica, ...	14°
Carlisle, Mass., ...	Lyc. Nat. Hist., N. Y., from Dr. Torrey,	in limestone vein with fibrolite, &c.; rich yellowish brown, like Natural Bridge, ...	14°
Rossie, N. Y., near Mrs. Story's, ...	Dr. Hough, ...	light yellowish, with crystals of magnetic iron?	15°
Pope's Mills, St. Lawrence Co., ...	Dr. Crawe, ...	brownish yellow hexagonal crystal 3 inch. diameter; plate examined ⅛ inch thick; axis in the line of the shorter diagonal, ...	15°
Natural Bridge, Jefferson Co., N. Y., .	Lederer Cabinet and Dr. Hough,	rich yellow; associated with serpentine; same as analyzed by Meitzendorff, ...	15°
ib. ib., ...	Dr. Hough, ...	another specimen, ...	16°
Edwards, N. Y., ...	Dr. Crawe, ...	white silvery, curved crystals, often opake, ...	15° 30′–16° 30′
Vicinity of Rossie, N. Y., ...	Mr. Powers, ...	rich yellow brown; probably the same as Gouverneur, ...	16° 7′–16° 15′
Essex, N. Y., ...	Lyc. Nat. Hist., N. Y., ...	in limestone in beautiful crystals not over ¼ to ½ inch in diameter; hexagonal; deep rich brown color, ...	16° 30′
Upper Ottawa, Canada, ...	T. S. Hunt, ...	reddish yellow transparent, in granite, ...	17° 30′–18°

Table continued.

The following are probably of the same species but are generally too dark in color to admit the passage of light in plates of sufficient thickness to define well the two sets of ellipses. Nevertheless in several cases the angle may be approximately estimated, and in others the mica may be more properly arranged with the anomalous varieties which present equivocal evidence of a binaxial character.

Locality of specimen.	Whence obtained.	Color, Form and Remarks.	Angle.
Moriah, Essex Co., N. Y.,	Lederer Cabinet,	very dark smoky red, in thin plates by transparent light,	Estimat'd 16°–17°
Gouverneur, N. Y.,	Dr. Hough,	brownish copper red; yellowish; in hexagonal plates like Pope's Mills,	
Somerville, N. Y.,	Dr. Hough,	faint brownish, with blood red spots, which magnified are seen to be garnet,	Estimated 5°–7°
Burgess, Canada West,	T. S. Hunt,	bronzy, almost metallic luster; a very brownish yellow, semi-transparent if thin; opake in plates a line thick; slightly elastic only; found with apatite in sandstone,	Angle very low.
Franklin, N. J.,	Lederer Cabinet,	bronzy yellow, distinct from the red mica of same place,	About 14°
Burgess, Canada West,	T. S. Hunt,	whitish yellow; imperfect specimen, with particles of foreign matter, resembling specimen from Natural Bridge,	About 10°
Fine, St. Lawrence Co., N. Y.,	Dr. Hough,	very dark olive brown; opake in plates over a line thick; clearly binaxial,	Estimat'd 10°–12°
Amity, N. Y.,	ib. ib.,	opake silvery white; resembles in general character a muscovite,	10°–12°
Sterling Mine, Morris Co., N. J.,	F. Canfield,	rich yellowish brown, inclining to red; in limestone.	
Suckasunny Mine, N. J.,	ib.	deep olive brown, inclining to yellow; in limestone.	
Newton, N. J.,	ib.	yellow; imperfect and very small; in limestone.	
Lockwood, Sussex Co., N. J.,	ib.	deep olive brown, like the mica of Fine, N. Y.; in limestone.	

To the foregoing may be added the anomalous micas before described from Monmouth (?) N. J., (Maine ?), from Franklin furnace, N. J., and several specimens which the author has found in various cabinets, but which being without labels he has been unable as yet to refer to the proper localities. Another phlogopite from Oxbow, N. Y., has been received while these pages are in the press.

In the accompanying tables we have given for each specimen measured, its locality, color, the source whence received, the angle between the optic axes, and other observations bearing on their history. It may afford a more correct impression of the present state of this investigation to remark that specimens from over *one hundred* American localities of mica have been examined besides a considerable number of uncertain locality; and from many of these localities very numerous specimens have been measured. Thus there have been measured of *muscovite* specimens from about 50 localities; of phlogopite over 30; of euphyllite 2; of margarodite 1; of biotite about 12, and of doubtful species 3 or 4.

Biotite.—The number of localities in the United States furnishing uniaxial mica (biotite) has been constantly diminishing since the commencement of these investigations, as increased skill in observation and improved means of examination have shown one after another of the reddish and yellow or brown colored micas to have more or less decidedly the characters of phlogopite.

Thus the dark brown mica of Fine, the red micas of Moriah, of Sterling, of Franklin, and of Gouverneur, have successively been thrown out of the list of biotites into the binaxial species. Others remain in doubt as those of Topsham, Me., and Easton, Pa.

Greenwood Furnace, Monroe, Orange County, N. Y., is the locality of a very remarkable and perfectly crystallized biotite. It is figured by Beck,* and fully described also by Dana,† and it appears to have been analyzed by von Kobell,‡ as quoted by both the authors just cited. The remarks made on a previous page respecting the equivocal optical character of some of the micas apply to this variety also. It is opake in plates over one-twelfth of an inch thick, but of rich olive green in thin plates.

The figure of its rings under polarized light is decidedly elliptical, but not so much so perhaps as to require any other explanation than the remark of Biot, before quoted, respecting the power of thin plates to disturb the ray and produce the effect of binaxial structure in a uniaxial crystal. The author has a new analysis of this variety in progress the results of which he will present on a future occasion. The chemical constitution of very few American biotites has been examined: indeed the locality just referred to is the only one cited. Von Kobell's result corresponds with the formula given on page 375.

Mica of Monroe, N. Y.—This mica from the same town as the last is also uniaxial and gives a figure almost entirely cir-

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* Min., N. Y., p. 37. † Min. (1850), 360.
‡ Kastn. Archv., xii, 29.

cular. It is distinguished by its bronzy lustre and dark greenish olive color inclining to gray or black. It is imperfectly transparent, having by transmitted light a dusky or hazy appearance. It occurs in plates of immense size, which are marked on the cleavage surfaces with rhombic and triangular figures, (no distinct lateral planes have however been found,) as well as with transverse cleavage lines. It is slightly elastic but very tough. One specimen which the author has seen in the cabinet of Mr. C. M. Wheatley in New York is nearly two feet in diameter. Mr. Horton of Monroe has also furnished the writer with numerous very large specimens. No analysis of it has been published, but the author hopes to present one at a future time.

The white mica of Easton, Pa., which is very silvery and slightly elastic and opake in thick plates, is probably a biotite, and, excepting the very similar white mica of Amity, N. Y., is the only white uniaxial variety yet noticed in this country.

The Black micas are almost universally referable to the species biotite, although many micas usually called black in collections are in reality dark brown and olive green and are frequently referable to phlogopite. Unfortunately very few of the localities of this variety of color found in cabinets are labelled. I have one from Moors Slide on the Ottawa in Canada, furnished me by Mr. Hunt of the Canada Geological Commission. Another black mica is found in St. Lawrence Co., N. Y., of which specimens were obtained by the author from the cabinets of Mr. Wheatley and of the N. Y. Lyceum in New York. Two black micas from the Cambridge cabinet are unaxial, locality not known.

Geological relations.—It is worthy of notice that the species muscovite is found almost entirely in granitic rocks; in no instance as far as I have seen, has a specimen of this species been found in a lime rock. On the other hand, the phlogopites, with a single exception, so far as has been ascertained, are found in limestone and often in dolomitic beds. The biotite is less well determined, but, as regards the black micas, they are always, it is believed, found in granitic rocks. Thus New England is the region of muscovites, and northern New York, New Jersey and Canada, that of phlogopite. The few specimens of muscovite from St. Lawrence Co. were obtained from granite boulders. The only specimen of phlogopite yet observed in New England is from Carlisle in Mass., where also it is found in a limestone vein with chondrodite and fibrolite, an interesting confirmation of the suggestion here put forth. Can this distribution be unconnected with the chemical composition of the several compounds? The magnesian character of the phlogopites would seem to indicate the dolomitic position of the species, while the absence of this element in the muscovites is a negative fact of equal significance.

The writer cannot close this paper without tendering his thanks to numerous correspondents who have in the kindest manner responded to his persevering enquiries for specimens. It is intended that the foregoing tables should embrace in all cases the source from whence the specimens were obtained, as it is of the greatest importance that accuracy and authenticity should be obtained in this particular, for the sake of future observers. The author retains all the localities referred to in his own collection so labelled as to avoid error, and they are at all times open to the inspection of those who wish to examine them.*

ART. XXXVIII.—*Analyses of Phlogopite from St. Lawrence County, N. Y.;* by WM. J. CRAW, of the Yale Analytical Laboratory.

Read before the American Association for the Advancement of Science, at New Haven, August, 1850.

THE mica called Phlogopite has been analyzed by Meitzendorff, in Poggendorff's Annalen, volume viii, page 157. An analysis has likewise been published by Svanberg, in the Transactions of the Royal Swedish Academy for 1839, of a mica which has been supposed to be phlogopite, but as it does not agree in atomic proportions with that from New York, it very probably may not come under this species.

Meitzendorff gives the following as the composition of the specimen which he analyzed: it was from Jefferson Co., N. Y., and probably from Natural Bridge.

	$\dddot{\text{Si}}$	$\ddot{\bar{\text{Al}}}$	$\ddot{\bar{\text{Fe}}}$	$\dot{\text{Mg}}$	$\dot{\text{K}}$	$\dot{\text{Na}}$ & some $\dot{\text{Li}}$	Fl	ign.
	41·30	15·35	1·77	28·79	9·70	0·65	3·30	0·28=101·14
Oxygen,	21·46	7·17	0·53	11·31	1·65	0·17		

The specimens analyzed by me were all from Edwards, St. Lawrence Co., N. Y. No. 1, had a deep yellowish brown color, in broad plates.

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* It is worthy of remark that the species Phlogopite has not yet been mentioned as occurring in Europe. Dufrenoy† mentions and figures micas from Clayette in France, another from the department of Finistère and others from Lake Baikal which are probably to be referred to this species, but no optical examination appears to have been made of them. Sir David Brewster quotes a mica of 14° measured by him, but does not say from whence he procured it. Indeed optical observers have seemed heretofore to attach no importance to the localities of the specimens measured by them, and hence their observations are rendered nearly useless to the mineralogist. It cannot be doubted that the limestone rocks of Europe will supply many examples of phlogopite. The author would take this opportunity to suggest that the *light and dark greenish micas of Vesuvius* are referable to *phlogopite* as he has satisfied himself from the examination of authentic specimens in the collection of Yale College. The black Vesuvian micas are undoubtedly uniaxial.

† Traité de Minéralogie, iii, p. 647, 1847, also Dana, p. 359.

No. 2, was a transparent and colorless crystal of a silvery lustre; and

No. 3, was a part of the same crystal rendered quite opaque and silvery by the absorption of water. The crystal which furnished Nos. 2 and 3 was originally 6 by 8 inches in surface dimensions.

No. 1, gave reactions for silica, alumina, magnesia, potash and fluorine, with small quantities of peroxyd of iron and soda, and a doubtful trace of lithia.

Nos. 2 and 3 gave silica, alumina, magnesia, potash and soda, with traces of water and fluorine.

The following are the results of analysis:

	No. 1.		No. 2.	No. 3.
$\dddot{Si}$	40·145		40·358	40·36
$\dddot{\bar{Al}}$ with a little $\dddot{\bar{Fe}}$	17·356		16·450	16·084
$\dot{Mg}$	28·099		29·554	30·247
$\dot{K}$	10·564		7·226	6·066
$\dot{Na}$	0·63		4·938	4·39
Fl	4·202	loss by ign.	0·952	2·65
	100·996		99·478	99·797

Oxygen Ratios.

	No. 1.		No. 2.		No. 3.	
$\dddot{Si}$	20·86		20·97		20·97	
$\dddot{\bar{Al}}$	8·11		7·69		7·52	
$\dot{Mg}$	11·04		11·61		11·89	
$\dot{K}$	1·79	12·99	1·23	14·11	1·03	14·05
$\dot{Na}$	0·16		1·27		1·13	

The ratios are respectively 1·6 : 1 : 2·56; 1·83 : 1 : 2·73; 1·87 : 1 : 2·78; the mean of these is 1·77 : 1 : 2·69, which equals very nearly 7 : 4 : 11, and corresponds with the formula $7\dot{R}^3\dddot{Si}+4\dddot{\bar{Al}}\dddot{Si}$. The equality between the oxygen of the silica and that of the bases will be observed; and if $\dot{R}^3$ and $\dddot{\bar{Al}}$ may replace one another, the formula becomes $(\dot{R}^3, \dddot{\bar{Al}})\dddot{Si}$, a common type among the silicates.

The per-centage corresponding to this formula is as follows, excluding the fluorine, which is not found in analyses 2 and 3.

$\dddot{Si}$	$\dddot{\bar{Al}}$	$\dot{Mg}$	$\dot{K}$
41·60	16·82	30·01	11·57=100·00

In No. 1, the $\dddot{\bar{Al}}$ contains a small proportion of $\dddot{\bar{Fe}}$, which increases the amount of oxygen from the peroxyds so as to make the protoxyds and silica appear smaller in proportion than they really are, and the determination of the silica is also, probably, somewhat too low.

Meitzendorff's analysis affords the ratio 13·13 : 7·70 : 21·46; which equals 1·7 : 1 : 2·78, and corresponds quite closely with the ratio 7 : 4 : 11, the same which is afforded by the analyses above.

The fluorine, however, comes in as an important element, and one which it is somewhat difficult to dispose of, in a perfectly satisfactory manner.

H. Rose considers the fluorine to exist as fluorid of potassium, and gives the formula $KFl+(3\dot{R}^3\dddot{Si}+2\ddot{\bar{R}}\dddot{Si})$ This corresponds to the ratio 3 : 2 : 5—the potash being removed from the other protoxyds, as a fluorid.

Rammelsberg remarks that the fluorine may be considered as silicofluorid of potassium; but Rose is opposed to this view, on the ground that it would change the ratio of the oxygen in the several oxyds. This it would do, if it were considered simply as silicofluorid of potassium, but if, in accordance with the recent views of Rammelsberg published in his last supplement, it is taken as replacing oxygen in the several compounds, the ratio will remain unaltered. In this view, the formula $7\dot{R}^3\dddot{Si}+4\ddot{\bar{Al}}\dddot{Si}$, represents correctly the constitution of the mineral, if we suppose the oxygen partly replaced by fluorine.

The apportionment of the fluorine and oxygen to the several compounds is as follows:—

1. *Meitzendorff's Mica.*

		Fluorine.	Oxygen in residues.			Sum.
Combined with	Si	1·65	$\dddot{Si}$		20·68	22·33
	Al	0·60	$\ddot{\bar{Al}}$	6·92	7·45	8·05
	—	—	$\ddot{\bar{Fe}}$	0·53		
	Mg	0·90	$\dot{Mg}$	10·94		
	K	0·15	$\dot{K}$	1·58	12·68	13·73
			$\dot{Na}$	0·16		

Ratio 1·7 : 1 : 2·78.

2. *Mica from Edwards. Analysis* 1.

		Fluorine.	Oxygen in residues.			Sum.
Combined with	Si	2·101	$\dddot{Si}$	19·97		22·071
	Al	0·764	$\ddot{\bar{Al}}$	7·79		8·554
	Mg	1·146	$\dot{Mg}$	10·56		
	K	0·191	$\dot{K}$	1·71	12·43	13·767
			$\dot{Na}$	0·16		

Ratio = 1·6 : 1 : 2·58.

The ratio of the silico-fluorids to the silicates is 1 : 30 in Meitzendorff's analysis, and about 1 : 24 in the mica of Edwards; according to the first, the detailed formula is—

$$30[7\dot{R}^3\dddot{Si}+4\ddot{\bar{Al}}\dddot{Si}]+1[7(3R\,F+Si\,F^3)+4(\bar{Al}\,F^3+Si\,F^3)]$$

and according to the second—

$$24[7\dot{R}^3\dddot{Si}+4\ddot{\bar{Al}}\dddot{Si}]+1[7(3R\,F+Si\,F^3)+4(\bar{Al}\,F^3+Si\,F^3)].$$

Analyses 2 and 3 correspond closely with Meitzendorff's in the ratio, if this view of the fluorine be taken, and the varieties with fluorine and those without, which are alike in polarization, thus come under one general formula.

*Extracts from the Proceedings of the Twentieth Meeting of the British Association, held at Edinburgh, July, 1850.**

Section A.—Mathematical and Physical Science.

On Atlantic Waves, their Magnitude, Velocity, and Phenomena; by Dr. Scoresby.

During *two* passages across the Atlantic in 1847–8, I had opportunities for investigating certain elements respecting deep-sea waves more favorable than had ever before occurred within my experience in navigation. These observations, it should be noted in the outset, and the results deduced from them, were entirely uninflenced by, and separate from theory. They form but a contribution to this interesting branch of natural phenomena; but I offer them the more readily from the circumstance of their entire independency and speciality. It was in our return voyage from America that the highest seas occurred, when the circumstances adapted for interesting observations were singularly favorable; for, whilst the magnitude and the peculiar construction of the upper works of the ship—the Hibernia—afforded various platforms of determinate elevation above the line of flotation for observations on the height of the waves, the direction of the ship's course, with respect to that of the waves, was generally so nearly similar as to yield the most advantageous agreement or accordance for observations on their width and velocity. These observations I shall extract, in their order, from my journal kept during the homeward passage. My first observation worth recording is under the date of March 5, 1848, when the ship was in latitude about 51°, and longitude (at noon) 38° 50′ W.—the wind then being about W S.W., and the ship's course, true, N. 52° E. At sunset of the 4th the wind blew a *hard gale*, which, with heavy squalls, had continued during the night; so that all sail was taken in but the storm-staysail forward. The barometer stood at 29·50 at 8 P. M., but fell so rapidly as to be at 28·30 by 10 the next morning. In the afternoon of this day I stood some time on the saloon deck or cuddy roof,—a height, with the addition of that of the eye, of 23 feet 3 inches above the line of flotation of the ship,—watching the sublime spectacle presented by the turbulent waters. I am not aware that I ever saw the sea more terribly magnificent. I was anxious to ascertain the height of these mighty waves; but found almost every wave rising so much above the level of the eye, as indicated by the intercepting of the horizon of the sea in the direction in which they approached us, as to yield only the *minimum* elevation, and to show that the great majority of these rolling masses of water possessed a height of considerably *more* than twenty-four feet (including depression as well as altitude,) or, reckoning from the mean level of the sea, of *more* than twelve feet. Exposed as the situation was, I then adventured to the larboard paddle-box, which was about seven feet higher, where the level (as ascertained afterwards at Liverpool, allowance being made for the alteration in the draught of water of the ship,) was twenty-four feet nine inches above the sea.

* From the Athenæum, Numbers 1189, 1190, 1191.

This position, with five feet six inches, the height of my eye, gave an elevation altogether of thirty feet three inches for the level of the view then obtained,—a level, it should be remarked, which was very satisfactorily maintained during the instants of observation, because of the whole of the ship's length being occupied within the clear "*trough* of the sea," and in an even and upright position, whilst the nearest approaching wave had its maximum altitude. Here, also, I found at least *one half* of the waves which overtook and passed the ship were far above the level of my eye. Frequently I observed long *ranges* (not acuminated peaks) extending one hundred yards, perhaps, on one or both sides of the ship,—the sea then coming nearly right aft,—which rose so high above the visible horizon, as to form an angle estimated at two to three degrees (say $2\frac{1}{2}°$) when the distance of the wave summit was about one hundred yards from the observer. This would add near thirteen feet to the level of the eye. And this measure of elevation was by no means uncommon,—occurring, I should think, at least once in half a dozen waves. Sometimes peaks of crossing or crests of *breaking* seas would shoot upward at least ten or fifteen feet higher. The *average wave* was, I believe, fully equal to that of my sight on the paddle-box, or more,—that is, $\frac{30}{2} = 15$ feet, or upwards; and the *mean highest waves*, not including the broken or acuminated crests, about forty three feet above the level of the hollow occupied at the moment by the ship. Illuminated as the general expanse not unfrequently was by the transient sunbeam breaking through the heavy masses of the storm-cloud, and contrasting its silvery light with the prevalent gloom, yielding a wild and partial glare, the mighty hills of waters rolling and foaming as they pursued us, whilst the gallant and buoyant ship—a charming "sea-boat"—rose abaft as by intelligent anticipation of their attack, as she scudded along, so that their irresistible strength and fierce momentum were harmlessly spent beneath her and on her outward sides,—the storm, falling fiercely on the scanty and almost denuded spars and steam chimney raised aloft, still indicated its vast, but as to us innoxious, power, in deafening roarings,—altogether presented as grand a storm-scene as I ever witnessed, and a magnificent example of "the works of the Lord," specially exhibited to sea-going men, "and his wonders in the deep." In the afternoon of the same day the gale again increased, blowing, especially during the continuance of a much protracted hail-shower, terrifically,—roaring like thunder whilst we scudded before it, causing the ship to vibrate as by a sympathetic tremor, and the tops of rolling waves, too tardy, rapid as was their actual progress, for the speed of the assailing influence, to be carried off and borne along on the aerial wings in a perfect drift of spray! But during the period of these most vehement operations of nature, I was fortunately enabled, from familiarity with sea enterprise, to pursue my observations with entire satisfaction. The next day—March 6—added to the interest of these investigations by developing the character of the Atlantic waves under a long and fiercely-continued influence of a little varying wind. It had blown a heavy gale, violent in the showers, from the north-westward, from Saturday evening the fourth, to the evening of Sunday, from twenty-six to thirty hours; during the night, too, of Sunday it had again blown hard (abating towards the morning of Monday), and making a total continuance of the storm, in *its violence*, of about thirty-six hours.* I renewed my observations on the waves at ten A. M.—the storm having been then subdued for several hours, and the height of the waves having perceptibly subsided. Soon I observed, when standing on the saloon-deck, that ten waves, in one case, came in succession, which all rose above the apparent horizon,—consequently they must have been more than twenty-three feet, probably the *average* might be about twenty-six from ridge to hollow. At this period I also found that occasionally (that is, once in about four or five minutes,) three or four waves in succession, as seen from the paddle-box, rose *above* the visible horizon,—hence they must, like those of the preceding day, have been thirty feet waves. But one important *difference* should be noted—viz., that they were of no *great* extent on the ridge, presenting, though more than mere conical peaks, but a moderate elongation.

Another subject of consideration and investigation, on this occasion, was the period of the regular waves overtaking the ship, and the determination, proximately, of the actual width or intervals, and their velocity. 1. The ship was then going *nine* knots only, the free action of the engines being greatly interfered with by the heavy sea running, and the lines of direction of the waves and the ship's course differed about 22½ degrees, the sea being two points on the larboard quarter,—in other words, the true course of the ship was east; the direction *from* whence the sea came was W.N.W. 2. The period of regular waves in incidental series, overtaking the ship, were observed as follows:—

Waves.		Min. Sec.	Mean.
20	occupied	5 03	16″·5
10	"	2 35	15 ·5
10	"	2 50	17 ·0
10	"	2 45	16 ·5
8	"	2 16	17 ·0
General average,			16 ·5

3. The length of the ship was stated to be two hundred and twenty feet. The time taken by a regular wave to pass from stern to stem appeared, on a mean of several observations, to be about six seconds. Hence 6″ : 220 feet (the width passed over in that time) : : 16·5 feet to 605 feet (the width passed over betwixt crest and crest.) But this extent, by reason of the obliquity of the direction of the waves to the course of the ship, is found to be elongated about 45 feet, reducing the probable mean distance of the waves to five hundred and fifty-nine feet. Independently of this process, I had previously estimated the distance of the wave crests, ahead and astern when the ship was in the hollow, as I stood near the center of the ship's length on the paddle-box, at three hundred feet each way, by comparing the intervals betwixt my position and the place of the wave-crest, with the known length of the ship. This comparison frequently re-considered and repeated, subsequently yielded, in much accordance with the former, a total width, in the line

* The barometer on Saturday, at 8 P. M., was at 29·50; at 6 A. M. of Sunday it had fallen to 28·30, being 1·2 inches in ten hours. At 6 P. M. of the latter day it had risen to 30·00 inches.

of the ship's course, of about six hundred feet. 4. But the total distance betwixt the crests of two waves, then reckoned at five hundred and fifty feet, a distance passed by the wave in 16·5 seconds of time, by no means indicates, it is obvious, the real velocity of the wave, as the ship meanwhile was advancing nearly in the same direction at the rate of nine knots, that is, nine geographical miles, or (6,075·6 feet $\times$ 9 $=$) 54,680·4 feet per hour, or 15·2 feet per second. During the time, therefore, of a wave passing the ship $= 16{\cdot}5''$, the ship would have advanced on its course $15{\cdot}5 \times 15{\cdot}2 = 250{\cdot}6$ feet. Reducing this for the obliquity of two points we have 231·5 feet to be added to the former measure, five hundred and fifty nine feet, which gives 790·5 feet for the actual distance traversed by the wave in 16·5 seconds of time, being at the rate of $\left(\frac{3{,}600'' \times 790{\cdot}5}{16{\cdot}5} = \right)$ 17,251·7 feet, or 32·67 English statute miles per hour. To know how far this result is but proximate, it should be considered that, of the several elements employed in the calculation, all but one might be deemed accurate. The interval of time occupied by the transit of a wave with respect to the position of the ship, the *direction* of the ship's motion with relation to that of the waves, and the speed of the ship through the water,—may all be recorded as, essentially, accurate. The element in doubt is that of the average distance from summit to summit of the waves. This distance, it has been seen, was, by a twofold process of observation or comparison accordantly assumed. The value of the judgment derived from rapid comparison of measures by an eye accustomed to such estimations is, it should be observed, far higher than might be generally considered. The practical military commander or engineer officer is able to make, by mere inspection of the ground before him, remarkably close estimates of spaces and distances. When engaged in the Arctic whale fishery, I was enabled, from habit and comparison of unmeasured spaces with known magnitudes, to estimate certain distances with all but perfect accuracy. Thus, as to a circumstance in which we were most deeply interested—the near approach of a boat to a whale—I found it quite practicable, whenever the pursuing boat approached within twice or thrice its length (except when the position was near end on) to estimate the distance to less than a yard. Now, the means of comparison by the eye as to the estimation of the breadth of the Atlantic waves, was that of the ship's length of 220 feet. When the ship was fairly in the middle of the depression betwixt two waves it was assumed, with reference to this known measure, that something obviously less, but not greatly so, than the ship's length, was the distance of each of the two waves then contemplated—giving a total width of about six hundred feet. But the comparison of the time required by a wave to pass from stem to stern, with the average time of transit of an entire wave, yielded a much better result; and, on much consideration of the subject, I am inclined to believe that the estimate is a tolerably close approximation to the truth. It should be observed, too, that as the headway of the ship, in the direction of the course of the wave—being a known quantity—it was favorable to the accuracy of the estimate. For, assuming an error in the width of the waves to have occurred, say to the amount of one-twelfth of the whole, or forty-nine feet—the effect upon the calculated velocity of the wave would have been only about a sixteenth, or 2·16 miles per hour. The form and character of these deep-sea waves became at the same time interesting subjects of observation and consideration. In respect to form, we have perpetual modifications and varieties, from the circumstance of the inequality of operation of the *power* by which the waves are formed. Were the wind perfectly uniform in direction and force, and of sufficient continuance, we might have in wide and deep seas, waves of perfectly regular formation. But no such equality in the wind ever exists. It is perpetually changing its direction within certain limits, and its force too, both in the same place and in proximate quarters. Innumerable disturbing influences are therefore in operation generating the varieties more or less observable in natural sea waves. In regard to my own observations of the actual forms of waves, nothing particularly new could be expected from an inquiry of this kind in regard to phenomena falling within the perpetual observation of sea-going persons; yet, at the risk of stating what might be deemed common, I will venture to transcribe from my notes made with the phenomena before me, the leading characteristics which engaged my attention. During the height of the gale (March 6th) the *form* of the waves was less regular than after the wind had, for some time, begun to subside. Though in many cases when the sea was highest the succession of the primary waves was perfectly distinct, it was rather difficult to trace an identical ridge for more than a quarter to a third of a mile. The grand elevation in such case sometimes extended by a straight ridge or was sometimes bent as of a crescent form, with the central mass of water higher than the rest, and, not unfrequently, with two or three semi-elliptical mounds in diminishing series, on either side of the highest peak. These principal waves, too, it should be noted, were not continuously regular, but had embodied in their general mass many minor, secondary and inferior waves. Neither did the great waves go very prevalently in long parallel series like those retarded by shallow water on approaching the shore; but every now and then changed into a bent cuneiform crest with breaking acuminating peaks. On the following morning (March 7,) after a second stormy night, wind S.S.W. (fine), we had a heavy and somewhat cross sea (from the change of wind from W.S.W. to S.S.W.). But the almost unabated magnitude of the more westerly waves indicated a continuance of the original wind at some distance astern of us. The gale had moderated at daylight, and the weather became fine; but as the sea still kept high, its undulations became more obvious and easily analyzed. At three in the afternoon, when about a third part of the greater undulations averaged about twenty-four feet from crest to hollow, in height, these higher waves could be traced right and left as they approached the ship to the extent of a quarter of a mile on an average, more or less. Traced through their extent the ridge was an irregular round-backed hill, precipitous often on the leeward side. The undulations, indeed, as to primary waves, consisted mainly of these round-backed masses, broken into or modified by innumerable secondary and smaller waves within their general body. The time in which these waves passed the ship was now, on an average, about fifteen seconds, the ship's speed being increased from nine to eleven knots, and the obliquity of the ship's course to the direction pursued by the waves was three points. On the 9th, two days after the above condition of the waves—whilst the sea yet ran high—few waves could be traced, continuously, above three hundred or four hundred yards in extent along the same ridge. The crests often curled over, but none so as to reach the height of a thirty feet wave, and broke for a wide space, estimated at fifty to one hundred yards in continuity.

Miscellaneous Notes and Suggestions.—The mode adopted in these researches of finding the *height* of waves is, I believe, quite satisfactory, and, observed with care and with relation to numbers or proportion of waves, as accurate as need be. The depression of the horizon in respect to the elevation of the observer is too small to form even a correction. As the horizon from the paddle-box $\frac{30}{2} = 15$ feet, had only a depression of 3′ 49″, the distance of the visible horizon, as seen from this elevation, would be 4·45 statute miles, and the actual depression in feet due to the distance of the summit of the wave when the ship was in the midst of the hollow, could only be 0·18 foot or 2·16 inches. Other modes of determining the width of a wave—or the extent betwixt summit and summit—much preferable to that described (the only available one I could devise) might easily be adopted where the management of the ship was in the hands of the observer. In steam ships the simplest mode for high seas, perhaps, would be, altering the speed of the ship when going in the direction of the wave or against the wave; the ratios of the time of transit of wave-crests, under different rates of sailing of the ship might yield results very close to the truth. In moderate sized waves the plan adopted by Capt. Stanley—whose observations I met with before this meeting—seem satisfactory. But in calms, or moderate weather after a storm,—that is, for the determination of the velocities of less elevated waves—a variety of processes might be available.

The author referred, in conclusion, to the *forms* of wave crests, and heights, modified by crossings, interferences, and reflections.

On Metallic Reflection; by Prof. G. G. Stokes.

The effect which is produced on plane-polarized light by reflexion at the surface of a metal shows that if the incident light be supposed to be decomposed into two streams, polarized in and perpendicularly to the plane of reflexion respectively, the *phases* as well as the intensities of the two streams are differently affected by the reflexion. It remains a question whether the phase of vibration of the stream polarized in the plane of reflexion is accelerated or retarded relative to that of the stream polarized perpendicularly to the plane of reflexion. This question was first decided by the Astronomer Royal, by means of a phenomenon relating to Newton's rings, when formed between a speculum and a glass plate. Mr. Airy's paper is published in the Cambridge Philosophical Transactions. M. Jamin has since been led to the same result, apparently by a method similar in principle to that of Mr. Airy. In repeating Mr. Airy's experiment, the author experienced considerable difficulty in observing the phenomenon. The object of the present communication was to point out an extremely easy mode of deciding the question experimentally. Light polarized at an azimuth of about 45° to the plane of reflection at the surface of the metal was transmitted, after reflexion, through a plate of Iceland spar cut perpendicular to the axis, and analyzed by a Nicols's prism. When the angle of incidence was the smallest with which the observation was practicable, on turning the Nicols's prism properly the dark cross was formed almost perfectly; but on increasing the angle of incidence, it passed into a pair of hyperbolic brushes. This modification of the rings was described and figured by Sir D. Brewster, in the Philosophical Transactions for 1830. Now, the question at issue may be immediately decided by observing in which pair of opposite quadrants it is that the brushes are formed. In this way the author was led to Mr. Airy's result, namely, that as the angle of incidence increases from zero, the phase of vibration of light polarized in the plane of incidence is *retarded* relatively to that polarized in a plane perpendicular to the plane of incidence.

On a Fictitious Displacement of Fringes of Interference; by Prof. G. G. STOKES.

The author remarked that the mode of determining the refractive index of a plate by means of the displacement of a system of interference fringes, is subject to a theoretical error, depending upon the dispersive power of the plate. It is an extremely simple consequence (as the author showed) of the circumstance that the bands are broader for the less refangible colors, that the point of symmetry, or nearest approach to symmetry, in the system of displaced fringes, is situated in advance of the position calculated in the ordinary way for rays of mean refrangibility. Since an observer has no other guide than the symmetry of the bands in fixing on the center of the system, he would thus be led to attribute to the plate a refractive index which is slightly too great. The author has illustrated this subject by the following experiment:—A set of fringes produced in the ordinary way by a flat prism were viewed through an eye-piece, and bisected by its cross wires. On viewing the whole through a prism of moderate angle, held in front of the eye-piece with its edge parallel to the fringes, an indistinct prismatic image of the wires was seen, together with a distinct set of fringes, which lay quite at one side of the cross wires, the dispersion produced by the prism having thus occasioned an apparent displacement of the fringes in the direction of the general deviation.

On the Refractive Indices of several Substances; by the Rev. Prof. POWELL.

Having on former occasions endeavored to extend the list of observed indices for the standard rays of the solar spectrum given by prisms of different media, by means of an apparatus described, along with the statement of the results, in my report to the British Association, 1839, I now beg to offer to the Association the indices in like manner obtained for the four following. The rare oil of spikenard I received through a friend from the late Mr. Hatchett, by whom it was carefully prepared, perfectly pure; for the other three I am indebted to Mr. N. S. Maskelyne. The results in each case are the mean of several repetitions. In two instances (the oils of lavender and sandal wood) the absorption of the violet rays (as in so many other oils) was such as to render the line H very indistinct; its index is therefore marked as doubtful.—

MEDIUM.	μ for the the Standard Rays.						
	B	C	D	E	F	G	H
Oil of Spikenard, Temp. 22 deg. Centig.	1·4732	1·4746	1·4783	1·4829	1·4868	1·4944	1·5009
Oil of Sandal Wood, Temp. 20 deg. Centig.	1·5034	1·5058	1·5091	1·5117	1·5151	1·5231	1·5398?
Oil of Lavender, Temp. 20 deg. Centig.	1·4641	1·4658	1·4660	1·4728	1·4760	1·4837	1·4930?
Benzole, Temp. 18 deg. Centig.	1·4895	1·4961	1·4978	1·5041	1·5093	1·5206	1·5310

In my report (1839), I stated the impossibility of obtaining measures in chromate of lead from the absence of all appearance of lines, and the entire absorption of the blue and violet portion of the spectrum. I have since thought that in the absence of any determinations of the kind it might not be useless to give the very rough estimates which my former attempts enable me to obtain by means of the absorption of blue glass, which gave a point roughly corresponding to about B, another to D, and the extreme green space visible might be about E. The most refracted of the two spectra (given by the double refraction of the substance) was the worst defined, and in this the part corresponding to D is extremely uncertain. The mean of two sets of observation was as follows:—Prism of chromate of lead, axis of prism perpendicular to axis of crystal, mean angle obtained by reflexion and by measurement = 14° nearly.—

RAY.	1st Spectrum.		2nd Spectrum.	
	Δ	μ	Δ	μ
Extreme red,				
about B	22°	2·53	26° 30′	2·84
about D	23° 10′	2·55	29° ?	3·00 ?
about E	24° 30′	2·70	30° 30′	3·10

While upon the subject, I may be allowed to remark, that as attempts are now making, with so much promise, for procuring optical glass of a superior quality, it would be highly interesting if specimens were cut into prisms (portions of half an inch cube, or even less, will do, and two sides only need be polished, containing an angle of about 60°), so as to subject the glass to the *very delicate test of the visibility of the finer lines of the spectrum.* I have reason to think that working opticians are not generally aware that in many specimens, *apparently very clear*, only a *few* of the broader lines can be seen and *very often none;* in Fraunhofer's glass nearly six hundred were visible.

On the Magneto-optic Properties of Crystals, and the Relation of Magnetism and Diamagnetism to Molecular Arrangement; by Messrs. J. TYNDALL and HERMANN KNOBLAUCH.

During the investigation carried on more than one hundred natural crystals had been examined. The results were thus briefly summed up :—We have on the one side four new forces assumed,—the optic attractive force and the optic repulsive force, the magno-crystallic force and the magneto-crystallic force ; and on the other side no new force whatever, but simply that modification of existing forces which we have named electro-polarity. By attention to the compression of amorphous bodies, every single experiment cited in proof of these four forces can be reproduced. Exactly the same can be exhibited with wax, dough, artificial layers, gutta percha and ivory. The alternative then appears to be either to explain the action of these substances by the assumption of optic and crystallic forces, or to explain magno-crystallic action by electric polarity.

This paper gave rise to a very animated discussion.—The President said, that although he was ready to admit that Mr. Tyndall's theory was most ingenious, and the arguments and experiments by which he sustained his views were apparently well conceived and sound, yet time must be given to weigh them well before a satisfactory conclusion could be reached.—Prof. Thomson thought that Mr. Tyndall's views would be found to be substantially consonant with Dr. Faraday's and the theory of Poisson.

On the Polarizing Structure of the Eye ; by Sir D. Brewster.

The author said that when he sat down to this paper he was not aware that Prof. Stokes was intending to make the communication which was placed next on the list—as his was an attempt to account by the polarizing structure of the eye for the phenomena of Haidinger's brushes, which would be referred to them immediately by Mr. Stokes. He would, therefore, confine himself to showing that the eye contained within itself amply sufficient to account for the phenomenon, because constituting the eye itself an ever ready polariscope or analyzer of polarized light.—He proceeded by diagrams to show that the crystalline lens of the eye, its posterior enclosing membrane, with the concave parallel membranes immediately in front of the retina, which together, acted similarly to a number of water crystals placed one within the other, constituted a polarizing apparatus, which by analyzing the polarized light from the blue sky, would give two blue skies, bounded by hyperbolic curves, with an interposed space of a yellow of the third order, or a brownish yellow, which would constitute the brushes, or bouchals, of Haidinger. One only difficulty still confronted him in this explanation, viz., that it ought to turn round the brush 45° from the plane of polarization,—in which plane, on the contrary, the brush was found to arrange itself.

On Haidinger's Brushes ; by Prof. G. G. Stokes.

It is now several years since these brushes were discovered, and they have since been observed by various philosophers ; but the author has not met with any observations made with a view of investigating the action of different colors in producing the brushes. The author's attention was first called to the subject by observing that a green tourmaline which polarized light very imperfectly enabled him to see the brushes distinctly, while he was unable to make them out with a brown tourmaline which transmitted a much smaller quantity of unpolarized light. He then tried the effect of combining various colored glasses with a Nicols's prism. A red glass gave no trace of brushes. A brownish yellow glass, which absorbed only a small quantity of light, rendered the brushes very indistinct. A green glass enabled the author to see the brushes rather more distinctly than they were seen in the light of the clouds viewed without a colored glass. A deep blue glass gave brushes of remarkable intensity, notwithstanding the large quantity of light absorbed. With the green and blue glasses the brushes were not colored, but simply darker than the rest of the field. To examine still further the office of the different colors in producing the brushes seen with ordinary daylight, the author used a telescope and prism, mounted for showing the fixed lines of the spectrum. The sun's light having been introduced into a darkened room through a narrow slit, it was easy, by throwing the eye-piece a little out of focus, to form a pure spectrum on a screen of white paper placed a foot or two in front of the eye-piece. On examining this spectrum with a Nicols's prism, which was suddenly turned round from time to time through about a right angle, the author found that the red and yellow did not present the least trace of brushes. The brushes began to be visible in the green, about the fixed line E of Fraunhofer. They became more distinct on passing into the blue, and were particularly strong about the line F. The author was able to trace them almost as far as the line G; and when they were no longer visible the cause appeared to be merely the feebleness of the light, not the incapacity of the greater part of the violet to produce them. With homogeneous light, the brushes, when they were formed at all, were simply darker than the rest of the field, and as might have been expected did not appear of a different tint. In the blue, where the brushes were most distinct, it appeared to the author that they were somewhat shorter than usual. These observations account at once for the color of the brushes seen with ordinary daylight. Inasmuch as no brushes are seen with the less refrangible colors, and the brushes seen with the more refrangible colors consist in the withdrawal of a certain quantity of light, the tint of the brushes ought to be made up of red, yellow, and perhaps a little green, the yellow predominating, on account of its greater brightness in the solar spectrum. The mixture would give an impure yellow, which is the color observed. The blueness of the side patches may be merely the effect of contrast, or the cause may be more deeply seated. If the total illumination perceived be independent of the brushes, the light withdrawn from the brushes must be found at their sides, which would account, independently of contrast, both for the comparative brightness and for the blue tint of the side patches. The observations with homogeneous light account likewise for a circumstance with which the author has been struck, namely, that the brushes were not visible by candle-light, which is explained by the comparative poverty of candle-light in the more refrangible rays. The brushes ought to be rendered visible by absorbing a certain quantity of the less refrangible rays, and accordingly the author found that a blue glass combined with a Nicols's prism enabled him to see the brushes very distinctly when looking at the flame of a candle. The specimen of blue glass which showed them best, which was of a tolerably deep color, gave brushes which were decidedly red, and were only comparatively dark, so that the difference of tint between the brushes and side patches was far more conspicuous than the difference of intensity. This is accounted for by the large quantity of extreme red rays which such a glass transmits. That the same glass gave red brushes with candle-light and dark brushes with daylight, is accounted for by the circumstance that the ratio which the intensity of the transmitted red rays bears to the intensity of the transmitted blue rays, is far larger with candle-light than with daylight.

On the Six Climates of France; by Dr. MARTINS.

Dr. Martins commenced by stating that France partook of the climates both of continental and sea-girt countries. He wished at present to consider six climatorial subdivisions, viz.—1. The northeast or Vosgien.—2. The northwest or Séquanien.—3. That of the west or Armoricain.—4. The southwest or Girondin.—5. The southeast or Rhodanien.—6, and finally, the Mediterranean or Provençal climate. Upon each of these subdivisions he enlarged; detailing the features of the country, the rivers, mountain-ranges, sea-coasts, geological structure, differences of level, and state of cultivation in each case, with the prevailing and most important features in the actual climate of each. Dr. Martins exhibited a map of France with these six regions distinguished. He stated that hitherto the labors of the meteorologists of France had no channel of publicity at their command, but that a journal devoted exclusively to meteorology was about to be established.

Mr. Ronalds inquired whether the state of the dew point had been attended to in this classification, as in his opinion that was one very important element.—Dr. Martins replied that it had been carefully attended to.—The Astronomer Royal inquired whether the difference of climate during the day and during the night, that of the summer as distinguished from the winter, had been attended to, as his experience led him to know that most important distinctions existed between these.—Mr. R. Russell added his testimony to the same effect; and said, that in an agricultural point of view mean temperatures were not so important as was usually supposed. Along the west coast it was now found that the maximum for the summer or ripening portion of the year was of much greater importance to the agriculturist than the mean.—Sir D. Brewster said that means were not to be neglected, for it had been well established that unless the mean rose in the season that might be characterized as the vegetating season to at least 58°, it was not found to be favorable; but of course he did not mean to say that the rising of the mean of the hot season above this, or the falling of the cold much below it, were not important and to be attended to.

On Hourly Meteorological Observations made at Thibet, at an elevation of 18,400 *feet;* by Lieut. STRACHEY, R.E.

Great interest attached to these observations from their having been made during twenty-four hours at an elevation so seldom attained by man, and quite above the clouds and most ordinary disturbing influences, and with a barometer pressure somewhere about fourteen inches of mercury. The chief result was that the curves followed very nearly the same changes as they were observed to do in the lower regions.

Col. Sykes went somewhat into detail regarding these and other observations of Lieut. Strachey, in the course of which he stated that the formula of Dr. Apjohn for the reduction of the wet and dry bulb hygrometers was found to be quite inapplicable to Indian climates; the dry bulb being lowered in its indications by the proximity of the wet, and the wet bulb collecting and retaining a wet atmosphere of its own whose temperature it gave,—not that which it would attain at the lowest were this atmosphere continually removed; one consequence of which was great discrepancy of result according to the part of a room in which it was placed.—Lieut Strachey corroborated this, and said he had found the numerical coefficients of Mr. Glaisher, which varied with the temperature, and which that gentleman had tabulated, much more applicable to Indian hygrometry.

Col. Sykes then gave a brief account of several storms of hail which had occurred in India, collected from various sources by Dr. Buist. The weight of some of these masses of ice was over fourteen pounds. Many of them under a rough external coat contained clear ice within, and with that peculiar radiated structure which he had elsewhere described. Immense aggregated masses of these great hailstones were in some places brought down from the mountain ravines by the succeeding torrents, and in one of these conglomerations a snake was found frozen up and apparently dead, but it soon thawed and revived.

On the Causes of the Rise of the Isothermal Lines in the Winters of the Northern Hemisphere; by Mr. T. Hopkins.

Mr. Hopkins examined some of the isothermal lines exhibited in the maps recently constructed by Prof. Dove, and objected to the theory which is put forward to account for the irregular rise of the winter isothermals in the northern Pacific, Atlantic and Arctic Oceans through the warming influence of the water of these oceans. But if neither the proportional extent of the surface of the sea, as compared with the land, nor the flow of a warm current of water carries high temperature to these northern latitudes, what is the cause of such temperature being found there? A reply is prepared by Prof. Dove himself, where he says,—"This surface," meaning the surface of the globe, "being a highly varied one, the sun's influence on it is also constantly varying, for the impinging solar heat is employed in raising the temperature of substances which do not change their condition of aggregation;—but when engaged in causing the melting of ice or the evaporation of water it becomes latent. When, therefore, the sun, returning from its northern declination, enters the southern signs, the increasing proportions of liquid surface upon which it shines cause a corresponding part of its heat to become latent, and hence arises the great periodical variation in the temperature of the globe which has been noticed above,"—meaning the difference of temperature of the northern and southern hemispheres. Why suppose that this effect of the evaporation of water is experienced only in the relative temperatures of the northern and southern hemispheres? And why not trace the effects of *condensation* of vapor, as well as of the *evaporation* of water? It is evident that heat is absorbed and made latent wherever vapor is produced, and it is equally clear that this heat is given out and made active wherever the vapor is condensed! It does not appear from the atmospheric currents which prevail, that any portion of the vapors of the southern hemisphere passes into the northern, to be condensed within or near to the basin of the Pacific, and there is no reason to suppose that it does;—but in the basin of the Atlantic it is sufficiently evident that vapor does not so pass. The vapor which passes over the Northern Atlantic, and is condensed beyond the British Isles and Norway, is supplied from the tropical and other seas north of the equator. The West Indies constitute the principal point of departure of this vapor, and in the month of January it is carried by southwestern and western winds to those localities where the isothermal lines advance farthest towards the pole. It is accordingly to the condensation of this vapor, and not to the neighborhood of the Atlantic Ocean, in the latitude that we are to attribute the high temperature of this part of the world in the winter. The Atlantic Ocean is as near to Labrador as to Norway, but there is little condensation on the coast of the former, whilst there is much about the latter. Indeed, as far as we know, condensation of vapor is the only influence that operates exclusively on the eastern coasts of the two oceans, the Pacific and the Atlantic, and therefore to it we may attribute the warming of the localities, particularly in the Arctic Ocean, as indicated by the isothermal lines. Condensation, we know, furnishes a constant and abundant supply of heat, not like diffusion by contact, nor radiation from surfaces nearly equal in temperature, but by the energetic chemical action which converts an aeriform substance into a liquid, and consequently changes the heat from a latent to an active state. The greatest irregular rise in the isothermal lines is found in the winter of the northern hemisphere, just at the time that the condensation of vapor produces the greatest effect on the temperature of the air; and the temperature rises most along that line or stripe where the largest amount of condensation takes place; and in that locality the same temperature reaches the highest latitude, showing that condensation of vapor is the cause of the rise of the isothermal lines in the parts.

On the Argument for the Physical Connection of Double Stars, deduced from the Theory of Probabilities; by Prof. FORBES.

The author read a passage from Herschel's 'Outlines of Astronomy,' where this argument is set forth. Mitchell, in the year 1767, in a paper in the Philosophical Transactions, was the first who advanced this argument. He calculates the odds as 500,000 to 1 against the stars which compose the group of the Pleiades being fortuitously concentrated within the space they occupy, and thence infers the probability of some physical connection between them. Struve has pushed this argument much further. In his classification of double stars he has applied it to estimate the improbability of the occurrence of even double stars in close proximity. He calculates the odds as 9,570 to 1 against any two stars from the first to the seventh magnitude falling, if fortuitously scattered, within 4″ of one another. The number of such binary combinations actually observed at the date of the calculations, was 91, and more have since been added to the list. Again, he calculates the odds against any two stars of a number fortuitously scattered, falling within 32″ of a third, so as to form a triple star, as not less than 173,000 to 1, while four such triple stars were known to exist. The conclusion, adds Sir John Herschel, of a physical connection of some kind or other is, therefore, unavoidable. Against the principle of this argument, Prof. Forbes, though with much diffidence, felt himself called on to protest. He owned he could not attach any idea to what would be the distribution of stars, or of any thing else if "fortuitously scattered," and therefore he must regard with hesitation, if not doubt, an attempt to assign a numerical value to the antecedent probability of any given arrangement or grouping whatever. To him it appeared that an equable spacing of the stars over the sky would seem to be far more inconsistent with a total absence of law or principle than the existence of spaces of comparative condensation, including binary or even more numerous groups, as well as regions of great paucity of stars. As an illustration of this, he adduced the representation of stars and their grouping by sprinkling viscid white paint from a coarse brush upon a dark ground; by which, although it was impossible to conceive a nearer approach to "random scattering," yet he had witnessed the production by it of an artificial galaxy, presenting every variety of grouping with double and triple points innumerable; nor could he conceive how on any reasonable theory of chance it should be otherwise.

On Cometary Physics; by Prof. SMYTH.

The author said that points in the Physics of Comets which he had intended to bring in detail under the notice of the Section, might be comprehended in the twelve following axioms or aphorisms—viz. 1. A comet consists of a nucleus and one or more gaseous envelops. 2. The nucleus if solid and material is extremely small. 3. This nucleus is excentrically situated in the gaseous envelop. 4. Comets of longest periods have the largest bodies. 5. The more excentric the orbit of a comet the more excentric is the body of the comet. 6. A comet revolves round its shortest nucleoid axis in the time it revolves round the sun. 7. This axis is not always at right angles to the plane of the orbit. 8. There is also a quicker rotation round its longer axis. 9. A comet shines by reflected light, and shows a sensible phase. 10. In proportion to the excentricity of its orbit a comet increases in density, and decreases in size in approaching perihelion, and *vice versâ*. 11. The longer axis of a comet is straight at perihelion and aphelion; but between these is concave towards the aphelion, the curvature being nearly proportioned to the excentricity of the orbit. 12. (Sir J. Herschel.) The component molecules of a comet are held together only by their mutual gravitation, each constituting almost a separate projectile, and describing its own orbit round the sun.—In consequence of the great press of business before the Section the author confined himself to the illustration of the 9th and 10th of these axioms in connection with the 12th,—showing by diagrams, how the changing appearances both of the head, the nucleus, and the tail, as it swept past perihelion, and particularly the forking observed in the tails of some comets, were simple effects of phase arising from the changing relative position of the illuminating sun, the comet and the observer. The 10th he illustrated by showing that towards perihelion the several parabolic paths of the parts of the comet, by becoming crowded together, caused the condensation of the comet, while the contrary took place by the separation of these several orbits towards aphelion. The author exemplified these principles by reference to the great comet of 1845, which, though visible to the naked eye for about three weeks, and to the telescope for more than five, yet in the very short time of less than twenty-four hours swept through that part of its perihelion path cut off by a plane through the sun parallel to the ecliptic, having approached within about 60,000 miles of the sun:—the nearest approach to that luminary ever actually observed.

Mr. Rankine observed, that if the 12th axiom (attributed to Sir J. Herschel) were a correct representation of facts, he conceived that it would follow that the tail of a comet, which was known to be turned straight from the sun at perihelion, must be turned straight towards the sun at aphelion; and at other parts of the orbit must have intermediate positions. This he proceeded to illustrate by a diagram, in which a number of ellipses with the same major axis were so arranged, respecting the sun occupying a focus common to all, as that their perihelia might all be ranged in one line, embracing the sun also.—Prof. Smyth did not concur in Mr. Rankine's argument; though time would not now admit of his going farther into it than to remark, that in the history of comets no fact was better established than that their tails were always turned, though with a slight curvature, directly from the sun; that this fact was well known to Sir J. Herschel, and was one basis of his induction.

On a new Membrane investing the Crystalline Lens; by Sir D. Brewster.

The author drew a diagram representing the crystalline lens of an ox with its inverted capsule; and said that having lately had occasion to examine the crystalline lens of an ox, which had been killed the day before, he had put it into water,—by imbibing which it had soon swelled, and at length the capsule burst. Before it had burst, however, he had observed distinctly a membrane not before recognized by anatomists, which had at one part detached itself from the body of the lens which it manifestly invested, and risen up within and towards the capsule at one spot.

On some Phenomena of the Polarization of the Atmosphere; by Sir D. Brewster.

The author stated that by the aid of a polariscope, which he had formerly described, formed with two wedges of rock crystal cut in a peculiar way, he was enabled to determine, by an examination of the parallel bands, the neutral points of the sky, and the plane of polarization. This he illustrated by a diagram; and added, that he had now convinced himself that not only was the light from the blue sky polarized by reflection in one plane, but under certain circumstances the refraction caused by some of the clouds polarized the light in quite a different plane; and he had actually in this instrument observed clouds by the action they exerted on the light, which were quite imperceptible to the eye unassisted by it.

Prof. Stokes observed that heretofore it had been universally supposed that the clouds always exerted a depolarizing action on the light which they reflected or transmitted to the eye.

On the Dynactinometer; by Mr. Claudet.

In the introductory portion, the author insisted on the very great importance of distinguishing between the optical foci of the lenses used in photographic cameras and the foci of the photogenic rays. He said that ignorance of this distinction, or inattention to it, was the source of one of the greatest defects in photographic pictures. He had invented a simple instrument, which was exhibited and explained, for accurately distancing the object to be depicted and determining the corresponding foci of the photogenic rays in any given camera. It consisted of a number of marked sectors arranged in a spiral order at several equal distances along a cylinder supported in a frame. By placing this before a photographic camera, the sector of which the image was most distinct could be at once seen, and this determined the distance at which the object should be placed in front of the camera. Since he had invented this, he had found that there was a proper time for exposing an object on a given day, and under given circumstances, before the camera; and that a longer or shorter time than this was injurious to the effect. To ascertain readily this proper time, he had invented the dynactinometer, which he now exhibited. It consisted of a square frame of card, with a circle of card capable of being turned round either by hand or by clock-work; in one position of this circle, the whole surface of the frame exposed to the camera at the proper photogenic distance was black; but as the circle turned, a neatly divided sector of white card was exposed, and by causing the circle to turn so as to expose a given number of divisions each successive equal number of seconds, the part of the sector whose image was most clearly defined on examination of the photogenic drawing, gave the number of seconds best for exposing the object to the camera. But as the several photogenic plates were not equally sensitive, the sensitiveness of the plates was determined by placing them in a small frame, and allowing them to descend along an inclined plane, during a certain part of which descent small circular spots were exposed to the action of light, the rest being quite protected. The action of the light on these spots gave a ready and exact means of comparing the sensitiveness of the several plates.

Attempt to explain the occasional distinct vision of rapidly revolving colored sectors; by Prof. Stevelly.

He exhibited an instrument for whirling cards with colored sectors on them, devised by Mr. Grattan, of Belfast, to teach his children the effect of combining colors. He had shown this at the Natural History Society with an application for enabling painters to determine, experimentally, the exact mixture of any number of colors, and their relative proportion to produce the exact effect which they required. This apparatus he had lent to Prof. Stevelly to show his class; and while doing so, he was surprised to observe that while the cards were revolving rapidly, if he suddenly turned away his head he caught a distinct view of the individual colored sectors at the instant he was losing sight of them by a side view. A few weeks before this, he had attended the lectures of Prof. Carlile, of the Queen's College, Belfast, on the anatomy of the eye and of the ear; and had then become acquainted with a fact connected with the arrangement of the optic nerves and their relation to the retina, which seemed to him to afford an explanation of this curious fact. The optic nerve which originated in the right side of the brain, crossed over to the left eye, but on entering that eyeball only spread out into that part of the retina which spread over the portion of the eyeball next the nose, and the similar portion of the retina of the right eye was supplied by the optic nerve which sprang from the left side of the brain. These nerves, however, were united in their action by a commissure nerve, which stretched in an arch from one to the other. The other and larger portion of the retina of each eye, and that on which the images of objects as usually seen were depicted, was formed by nerves which sprang from the brain in each case on the side next the eye to which they went; these, after accompanying the optic nerve of the other eye to the place where it crossed the optic nerve going to its own eye, turned round with a bend and accompanied it in its passage into the eyeball. These portions of the retina of the different eyes were also united into one nervous action by the "commissure of the retina." So that, the retina of each eye was divided into two portions,—the portion next the nose, and the outer and larger portion; and these two portions of each eye were supplied by nerves springing from opposite sides of the brain, and not united in their action by any commissure or connecting nerve. Now, the consequence of the sudden turn of the head was, to throw the image from its usual place on to the portion of the retina next the nose, affecting a new and fresh part of the retina for an instant only,—for the motion of the head instantly interposed the socket of the eye and shut off the object. The sectors therefore became distinct at that instant, for a similar reason that in the beautiful experiment of Prof. Wheatstone the electric spark showed them distinct,—namely, the instantaneousness of the impression.

The following are the titles of other memoirs presented to the Section of Mathematical and Physical Science.—

Report on the observations and experiments at the Kew Observatory; by F. Ronalds.

Report on Luminous Meteors; by the Rev. B. Powell.

On the Laws of Elasticity of Solids; by W. J. Macquorn Rankine.

Notice of the working of the new Integrating Anemometer during the past year; by Mr. Follet Osler.

On Magnetic Forces; by Mr. J. A. Broun.

On the construction of the Silk Suspension Threads for the Declination Magnetometer; by Mr. J. A. Broun.

On the Mechanical Compensations for the effect of Temperature on the Bifilar and Balance Magnets; by Mr. J. A. Broun.

On some Phenomena of Mirage on the east Coast of Forfarshire; by Rev. C. F. Lyon.

Experiments on the Expansion of Glass, Woods, and Metals from changes of Temperature; by Mr. Roberts.

Report of the committee on the Instruments for the measurement of earthquake waves; by Mr. Mallet.

Report on the Meteorology of the Azores; by Mr. J. C. Hunt.

Report on the effect of a stroke of lightning on a tree near Edinburgh; by Prof. PHILLIPS.

On the climate of the valley of the Nile; by Mr. T. S. WELLS.

On the means of computing the quantities of Aqueous vapor in the Atmosphere at various places and heights; by Mr. T. HOPKINS.

On the daily formation of Clouds at Makerstoun; by Mr. T. HOPKINS.

On the passage of Storms across the British Islands; by Mr. R. RUSSELL.

On remarkable Thermometrical Maxima at or near the Moon's first quarter, from 1839 to 1850; by Mr. R. EDMONDS.

On some extraordinary electrical appearances observed at Manchester on the 16th of July, 1850; by Mr. P. CLARKE.

On Meteorological Phenomena at Huggate, Yorkshire; by Rev. T. RANKINE.

On Isoclinal Magnetic Lines in Yorkshire; by Prof. PHILLIPS.

On a question of Probabilities which occur in the use of a fixed collimator for the verification of the constancy of Position of an Azimuth Circle; by Prof. AIRY.

On the Lunar Surface, and its relation to that of the Earth; by Mr. NASMYTH.

On a Tissue woven by Caterpillars; by J. DENNISTOUN.

On a new solid Eye-piece; by the Rev. J. B. READE.

On Polygons inscribed on a Surface of the Second order; by Sir W. R. HAMILTON.

On the Theory of Magnetic Induction; by Prof. THOMSON.

On the reduced observations for six years of the Winds in the regions of Glasgow; by Prof. NICOL.

On some powerful Magnets made by a process devised by M. Elias, and manufactured by M. Logeman, optician, at Haerlem; by Sir D. BREWSTER.

On the Optical Properties of Cyanuret of Magnesia and Platina; by Sir D. BREWSTER.

On a Geometrical Rotation between Ten Points, on a surface of the second order; by Sir W. R. HAMILTON.

On the mode of Disappearance of Newton's Rings in passing the Angle of total internal Reflexion; by Prof G. G. STOKES.

On the Distribution of Shooting Stars in the Interplanetary Spaces; by Mr. H. HENESSY.

On Electrical Figures of Dust on Plate Glass; by Mr. J. A. BROUN.

Magnetic Chart, exhibited by Mr. BESWICK.

Section B.—CHEMISTRY, INCLUDING ITS APPLICATIONS TO AGRICULTURE AND THE ARTS.

On the per-centage of Nitrogen as an Index to the nutritive Value of Food; by Dr. A. VOELCKER.

The object of this paper was to show, that the usual estimation of the nutritive qualities of an article of food is frequently attended with inaccuracies, which renders it desirable to modify our present methods in this respect in many cases. A circumstance which leads to considerable error is, the presence of ammoniacal salts in the juices of plants. In order to prove experimentally the presence of ammoniacal salts in larger quantities than hitherto suspected, and to avoid the objection that they might result from a partial decomposition of albuminous substances during the analysis, the author chose fungi for his experiments, which are rich in nitrogen and known as being highly nutritious. The species used was *Agaricus prunellus*, a species which is edible, and remarkable for forming most beautiful fairy rings. After having separated all soluble proteine compounds by means of basic acetate of lead, which re-agent throws down these completely, the amount of nitrogen still present in the juice of these agarics, in the form of ammoniacal salts, was found to be 0·204 per cent. for the fresh fungi, or 1·82 per cent. for the dry fungi. The whole amount of nitrogen in the same agarics, collected at the same time, determined by combustion, was found to be 0·74 per cent. for the fresh fungi, or 6·61 per cent. for the fungi dried at 212° F. Deducting from the last stated numbers the quantity of nitrogen found to exist in the juice in the form of ammonia, we find that only 0·536 per cent. of nitrogen in the fresh, or 4·799 per cent. of nitrogen in the dry fungi, exists in the state of proteine compounds, and that nearly one third of the nitrogen obtained by direct combustion exists in the form of ammonia in the juice, or at all events in a form in which the nitrogen adds nothing to the nutritive value of the fungi. The nutritive value of the fungi has thus been overrated considerably; and there can be little doubt that the same is the case of many vegetables, which according to the author's experiments contain sometimes considerable quantities of ammonia in the form of ammoniacal salts.

Dr. Christison remarked that he had long been convinced that there was a considerable fallacy in the methods of determining the value of nitrogen, and he hoped Dr. Voelcker's communication would give inquiry a more satisfactory direction.—Dr. Daubeny made some observations on this paper; and particularly noticed the researches of Prof. Hoffman on the substitution of ammonia, or of its elements, for carbon, which it appeared to him pointed to some laws in connection with the process of assimilation of nitrogenous materials by growing vegetables.—Dr. R. D. Thomson offered some objections to the reception of the doctrine that nitrogen was the principal source of nutrition, since it is found that blood and the other animal constituents contain many other substances.—Dr. L. Playfair was pleased that Dr. Voelcker had pointed out a source of error in the determination of nitrogen. Having been engaged in examining the dietaries of a large number of extensive establishments, he should lay the results before the Meeting.

On a peculiar form produced in a Diamond when under the influence of the Voltaic Arc; by J. P. Gassiot.

M. Jaquelin was the first to show that when the diamond is submitted to the high temperature and influence of the voltaic arc, it quickly becomes converted into a black carbonaceous matter having all the appearance of coke:—the diamond when in a native state is an insulator or non-conductor of electricity, but when thus changed into coke it becomes an excellent conductor. At the Chemical Section of the British Association, held at Oxford, in 1847, Dr. Faraday exhibited some specimens of the diamond coke which had been forwarded to him by M. Jaquelin, and subsequently, on the 16th of June, 1848, he publicly showed the experiment in London, in the theatre of the Royal Institution. On repeating the experiment a short time since before a few friends, I obtained a product so totally different from that of M. Jaquelin, that I am induced to bring the subject before this Section, in the anticipation that it may tend to elicit some observations on a phenomenon which at the time attracted the attention of many electricians. The apparatus I used in the experiment consisted of forty series of the usual size of Grove's nitric acid battery; the terminals were made from two pieces of well burnt box-wood charcoal, that attached to the positive or platinum end of the battery being formed in the shape of a small cup or crucible, in which the diamond was placed; to the negative or zinc end of the battery, a piece of the same charcoal (but *pointed*) was attached. The experiment was then made in the same form as described by M. Jaquelin, by first making contact with the two charcoal terminals, then bringing the flame in such a position as to cause it to *surround* the diamond ;—in less than one minute, the diamond as well as the electrode became in a state of intense ignition. The diamond gradually increased in size, rolling about in the heated crucible ; when it suddenly expanded, forcing itself upwards on the negative terminal, at which moment I separated the electrodes. The diamond, which was in a state of intense ignition, remained attached to the negative terminal. When cool it exhibited the same state as it now presents. It was expanded to eight or ten times its original bulk. Instead of becoming a black carbonaceous substance, and a good conductor, it has a vitreous white opaque appearance, and remains a non-conductor. It has also a deep circular cavity on that portion which was opposite and nearest to the positive electrode ; that part which was in contact with the negative electrode being clearly discernible by a small portion of the box-wood charcoal remaining attached to it. The centre of the cavity appears to be still brilliant, as if that portion of the diamond had not been in a complete state of fusion. In one or two other experiments the diamonds disintegrated, the fragments remaining in a carbonaceous state. Since which I have not had an opportunity of repeating the experiment.

Report on the present State of our Knowledge of the Chemical Action of the Solar Radiations ; by Mr. R. Hunt.

In this report the author gave an historical sketch of the progress of inquiry on this subject, from the period when Scheele first observed that the chlorid of silver was blackened much more speedily by the rays at the blue end of the spectrum than by those at the least refrangible, or red end, to the announcement of the discovery of the sensibility of the iodized tablets to the solar influences by Daguerre and the discovery of the action of gallic acid in the Calotype process by Mr. Fox Talbot. He then proceeded to show the extent of knowledge we had obtained as to the peculiarities of the phenomena, which may be summed up as follows. The chemical action of the sun's rays is proved, by its influence on organic and inorganic bodies, to extend over all the luminous rays of the prismatic spectrum—and slightly beyond them at the least refrangible end, and considerably beyond them at the most refrangible extremity. Living organisms and the products of organic life appear to be influenced by light—the luminous power—as distinguished from the purely chemical or calorific powers. The vitality of plants is stimulated by light ; and although many functions are performed in the absence of luminous radiations, they all appear to be quickened by its exciting powers ; at the same time we have evidence to show that the chemical principle is necessary to the processes of assimilation, and consequently to the production of many of the proximate constituents of plants. The author is of opinion—though he regards the subject as open to serious inquiry—that the processes of germination and budding are essentially influenced by the chemical principle, *Actinism* :—that the decomposition of carbon is peculiarly due to the luminous principle ;

and hence that the formation of wood in plants is a function of their vitality excited by *Light*:—that the development of the flower is due to a delicate balance of the forces, *Actinism* and *Light*, since we find that both the luminous and chemical agencies are very active during the process, and that the ripening of fruit and the perfecting of the healthful conditions of the seed are due to a combination of the calorific and chemical forces—as evidenced in the so-called parathermic rays,—many of the properties of which have been examined by Sir John Herschel and Mrs. Somerville. Returning to the consideration of the influence of the solar rays upon inorganic bodies, the author thought it established beyond a doubt—1st. That the maximum of chemical (actinic) phenomena was to be found where there was the least quantity of light and heat.—2. That as the luminous power increased—either in the spectrum or in natural phenomena—the chemical (actinic power) diminished, until it came to its minimum, where light—luminous power—existed at its maximum.—3d. That although the chemical influence extended to the red or heat-giving rays, its operations were materially modified, and to all appearance changed, by the combined operation of the calorific power, and that results standing in direct opposition to those obtained by the pure chemical rays were given by the chemico-calorific rays. In conclusion, the author pointed out the wide field for investigation which was opening to the experimentalist, and he showed that, although much had been achieved by the experiments already undertaken, there yet remained an extensive ground for inquiry which may be considered as absolutely unbroken:—chemical action, vital power, electrical phenomena and phosphorescence were proved to be directly dependent on the solar influences; but we yet want the researches which shall satisfactorily show whether these phenomena are due to one great principle modified by the matter on which it acts, or whether they result from the operation of forces combined in action, although very different in their resulting effects.

New Researches on the Conductibility of the Earth; by Prof. MATTEUCCI.

Although the good conducting power of the earth is at present generally admitted and is advantageously applied to the construction of electric telegraphs, it must be confessed that nothing has been hitherto known of the laws and theory of this singular phenomenon. In England, Germany, and Russia, it has been found advisable, for several years past, to form the telegraphic circuit partly with the earth and partly with the metallic wire, instead of forming the whole circuit with metallic wire only. I was, I believe, the first to show, by exact experiments made in 1844 at Pisa, and by others performed according to my propositions at the Scientific Congress of Milan, that *the resistance of the earth* to the passage of the electrical current, which is sensible in short distances, ceases to increase and remains constant when the distance between the electrodes plunged in the earth has attained a certain length. Having laterally renewed my studies on this subject, I have confirmed and extended in a complete and general manner the conclusions drawn from my former researches; I have also demonstrated the principal result, given above, by different experimental processes. I have compared the resistance of a mixed telegraphic circuit with that of an entirely metallic circuit, containing a length of wire twice as great as that employed in a mixed circuit. I have also formed metallic circuits of very fine brass wires, having the same resistance as the metallic portion of the very long mixed telegraphic circuit; and finally, by making use of long metal wires covered with gutta percha, I have been able to compare the resistance of an entirely metallic circuit with that of a mixed circuit, in which the metallic portion remained constantly the same, and to which were added different lengths of earth. The following are the principal conclusions drawn from experiments which have occupied me for about a year.—The resistance of a layer of earth to the passage of the electrical current varies according to the quantity of water contained in the earth of which it is composed,—according to the specific gravity of that earth,—according to its depth beneath the surface,—according to the nature of the electrodes and extent of their surface. This resistance does not increase with the increased length of the layer of earth; on the contrary, beyond a certain limit of length, which varies according to the different circumstances just indicated, but which in all cases is of little extent, the resistance of a layer of earth *remains constant* whatever be its length. It is unnecessary to say that I could not prove this fact by experiment on circuits exceeding eighty miles in length, such being the average of the telegraphic circuits in Tuscany. In making the experiment near the surface of the soil, it is difficult to plunge the electrodes in earth of exactly the same conducting power; different portions of the surface of soil possessing either better or worse conductibility than that on which I began to operate, it follows that in increasing the distance between the electrodes we may find either an increase or diminution in the resistance of the earth. Likewise, in operating on a long mixed telegraphic circuit, which is not perfectly isolated, owing to the effect of the different derived circuits formed between the posts and the earth, the electric current is stronger near the piles than at a distance, and stronger than in a circuit which is formed only of metal wire equal in length to that which enters into the mixed circuit. This explains the results which I had obtained from my former uncompleted experiments. The resistance of a layer of earth appears to diminish as its length increases only in cases where we meet with other layers of better conducting power. In every layer of earth of a certain *constant* conducting power, the resistance which at first increases very feebly with the increased length of the layer, becomes very soon constant, and continues the same for all the subsequent lengths, however great, on which experiments have been made. Now, it is evident that as the increase of resistance in a long metallic circuit is scarcely perceptible when we add to the circuit, by means of two large electrodes, a thin stratum of water; so we ought to find in the long mixed telegraphic circuits that the resistance of the earth is null or nearly so, since it is equal to that of a thin stratum of water of a very large section. The law of the conducting power of the earth being established, it remains to give the theory of this phenomenon. The opinion of the scientific world is divided on this point. Some explain the good conducting power of the earth by the almost infinite section of the earth compared with the distance of the electrodes; others, again suppose that the electricities at the extremities of the pile are dissipated in the earth, in the same manner as the electricity of the conductor of an electrical machine. This second explanation, will not bear the slightest examination, nor can it be made to tally with the results of the most elementary experiments relative to the conducting power of the earth. In fact, we cannot on this supposition explain why the resistance of the earth increases at first with the length of the layer; why it varies with the depth and the degree of moisture of that layer; why it changes if the mass of earth interposed between the two electrodes happen to decrease or to be wanting, as I have proved by experiments made in mountainous districts; why the interposition of a portion of earth of a different conducting power produces a variation in the resistance of the entire mass; why this resistance becomes infinitely greater when we keep this layer in a wooden trough separate from the earth, but in communication with it by means of large metallic plates. Finally, according to this explanation, the resistance of the metallic part of a mixed circuit ought to disappear,—a thing which never happens. I think that I may be able to give a satisfactory explanation of the good conducting power of the earth, founding my assertions on very simple experiments and on theoretical views already known. As long ago as 1837, I proved in a memoir published in the *Annales de Physique et de Chimie*, that in operating on a certain liquid mass, very considerable compared with the distance of the electrodes plunged in it, *the length of the intermediate liquid stratum has no sensible influence on the intensity of the current.* I have recently verified this result on a very large scale. I had a wooden case made seven metres in the side. I keep this case isolated from the earth, and filled with water. Operating on this mass of water, we find that the resistance of a certain stratum of water, variable within certain limits, is independent of its length. In like manner, in studying the conducting power of spherical masses of water varying in diameter from $2^{c\cdot m}$ to 30 or $40^{c\cdot m}$, I have found that the resistance of these spherical masses of water was the same, and independent of their diameter. I have already said that this result may be deduced from the theory, and this is done as follows:—From the same differential equations given first of all by Fourier in his celebrated theory of heat, and which Ohm has applied to electricity, suppressing in the latter case the terms which expressed the dispersion of heat in the air, are deduced in the case of the sphere the results which I have obtained by experiments on the propagation of electricity in the earth. Although we are as yet ignorant of the physical value of that variable U which figures in the fundamental equation of Ohm at three partial differentials, which is the same as that of Fourier in the propagation of heat, and although that equation would really be more applicable to the case of the metallic wire which communicates at one extremity with the conductor of an electrical machine in action, and at the other extremity with the earth, than to the case of the electrical current defined by its electro-chemical, and electro-magnetical action; it is no less true that a certain number of the phenomena of the electrical circuit are explained by representing the propagation of the electrical current by the same equation given by Fourier in his theory of heat. Among these phenomena, may be placed the fundamental law of the propagation of electricity in metallic wires according to their section and length, and the other more general cases of the propagation of the electrical current, and of derivation, in large metallic plates, or in spherical masses and in the earth, such as they have been found by MM. Kirchhoff and Smaaeen in Germany, and in Italy by my friends Ridolfi and Felici.

The reading of the communication from Prof. Matteucci led to a conversation on the various methods employed by the Electric Telegraph Company and others, and on the question of the investigations of Messrs. Bain and Wheatstone in England, and several experimentalists on the continent prior to these investigations of M. Matteucci as to the power of the earth to conduct electricity.—Mr. R. Hunt explained that in speaking of the conductibility of the earth it should be distinctly understood that the water contained in the superficial stratum is the conducting medium; since he has proved that non-metalliferous rocks and dry earth will not conduct an electric current.

On the Sugar Produce of the South of Spain, chiefly in connection with the employment of the Acetate of Lead and Sulphurous Acid as purifying agents; by Dr. Scoffern.

On the southern coast of Spain, in a region limited by Almeria on the east and Malaga on the west, bounded on the north by mountain ranges and on the south by the Mediterranean, is a tract of land which, so far as its climate and productions are concerned, may be aptly denominated tropical. In it, the date, palm, indigo, cotton, and sugar-cane flourish with vigor, yielding products equal both in quantity and quality to those of the tropics themselves. The sugar-cane, first introduced by the Arab conquerors, is not only consumed in large quantities as a dessert, but also gives rise to a considerable manufacture of raw and refined sugar, a circumstance which beyond Spain itself seems to be very little known. There is perhaps no example on record of any operation involving a commercial result attended with such an enormous destruction of materials as the operation of extracting sugar from the cane. One portion of this loss is due to mechanical, another to chemical causes. The sugar-cane has been stated by most writers who have found opportunities of practically examining the subject to contain no more than 10 per cent. of solid non-saccharine matter, leaving 90 per cent. of juice to be extracted. Of this 90 per cent., most writers concur in testifying that in practice scarcely 50 per cent. are actually obtained; at least in the British West India possessions. Cane juice itself has usually been stated to contain from 17 to 23 per cent. of crystalline sugar, of which scarcely 7 per cent. in practice is actually extracted. Considerable doubts having been expressed as to these statements of the amount of juice in the cane, and sugar in the juice, I have lately gone through a series of experiments having for their object the settlement of the doubt, and with the result of amply confirming the testimony of other experimenters. Having operated on canes from various parts of this district, by slicing them, exhausting first by hot water and then by hot alcohol, and finally drying, I obtained as my mean result about 10 per cent. of woody or insoluble matter; whilst the sugar extracted and crystallized ranged from 17 to 23 per cent., as had previously been stated. It would consequently appear that 40 per cent. of juice is actually lost in the practice of our West India workings; and now arises, as a most important consideration, the question as to what extent this loss is inevitable, and to what extent it might have been obviated by altered machinery or improved manipulation. Instead of 50 per cent. of juice extracted, 70 per cent. is much nearer the average amount yielded by the sugar-mills of this coast, although occasionally the result is as high as 75 per cent., and this, in some cases, with mills of very inferior construction. The cane, however, is passed between the rollers of the mill four times, until the refuse or megass, as the pressed cane is called, has been reduced to a state of disaggregation resembling ground tan, whereas the West India cane refuse is represented to be in the form of long strings, a sufficient proof that the pressure applied has been very inadequate. After the cane has finally left the mill, it is immediately, in the Spanish sugar regions, subjected to the operation of pressing, sometimes by the agency of a screw, but in many cases by hydrostatic force. By the latter method, I have seen 13 per cent. of juice extracted from megass which had already yielded up 73 per cent. of juice to the mill, thus elevating the total quantity extracted to 86 per cent. out of the original 90, and consequently as a manufacturing operation leaving very little more to be desired. The hydrostatic press I consider to be an apparatus which is indispensable to the economy of every sugar estate:—not only does it largely contribute to the amount of juice extracted,—but what is most remarkable, the juice resulting from hydrostatic pressure of megass is invariably, so far as my observations have gone, richer in sugar than juice yielded by the mill,—a fact which seems to be only explicable on the supposition that the hydrostatic press in virtue of its great power is enabled to extrude those particles of sugar which microscopic examination demonstrates to exist in the cane in the solid and crystalline form. The subsequent stages of the sugar manufacture as carried on in Spain do not materially differ from those in operation in Cuba, and many other tropical countries. The juice is defecated or purified by lime, skimmed, evaporated to the requisite degree, and poured into earthen ware moulds, the contents of which are finally exposed to the operation of claying. In one manufactory, however, witnessed by me at Almunecar, lime is no longer used on account of its well-known injurious effects on sugar:—no other agent having been substituted in its stead, but sole reliance being placed on the coagulation by heat of albuminous matters present in the juice, and their final removal by skimming. Under this system of manufacture, the sugar produced is light colored, but badly grained, and the unseparated albuminous matters are present in such quantity that every 100 parts of the concentrated saccharine juice as it comes from the teache, or last evaporating pan, yield only 40 parts of crystallized sugar on cooling, the other 60 per cent. remaining in the condition of molasses perfectly uncrystallizable until some adequate means for defecation be had recourse to. The chief object of my residence in this sugar district was to superintend the erection of machinery for manufacturing sugar by means of my own process. The site of our operations is Montril, about forty-five miles south of Granada, in a manufactory furnished with apparatus of the rudest character. Up to this period (July 9) our own vacuum apparatus has not been sufficiently advanced to enable us to pursue our operations by its aid; nevertheless, owing to the superior defecating power of the subacetate of lead, we have, even with the old and rude machinery, obtained a result of more than 16 instead of 7 per cent. of sugar. Our striking teaches, or final evaporating pans, we were under the necessity of removing in order to afford the requisite space for our own machinery; hence we were reduced to the necessity of concluding our process of concentration in a brass pan of conical form and holding about 600 imperial gallons, thus materially increasing the difficulty of the evaporative process. Hitherto only one-sixth per cent. on the juice of sub-acetate has been used,—but I imagine the quantity may be advantageously increased. As filtration is indispensable for the conducting of this process, considerable fear was entertained lest fermentation might supervene. This fear, however, practice has demonstrated to be groundless, inasmuch as we possess in sulphurous acid an agent most antagonistic to fermentation. Another speculative fear was lest danger might arise from the lead employed: this fear, too, practice demonstrates to be entirely without foundation, for not only is the sulphite of lead most easily removed,—but even were it to remain no injury could supervene, inasmuch as this agent is as harmless as chalk.

In continuation:—Observations on the Sulphite of Lead were made by Dr. GREGORY,—who stated that he had made experiments on the sulphite of lead formed in this process. He admitted that an infinitely small portion might still remain in the sugar, but that he considered it quite innocuous. He had indeed fed rabbits and dogs with food which had been united with this sulphite of lead, and the result was that they thrived amazingly, showing no symptom of any of the known effects of lead. Dr. Gregory also remarked that in testing sugar for lead with the hydro-sulphuret of ammonia, iron was often mistaken for the former metal.

Dr. CHRISTISON contended that we had no evidence that the sulphite of lead was innocuous. It was true that in case of poisoning by carbonate of lead sulphuric acid was administered to convert it into the comparatively insoluble sulphate; but this was a case widely different from the slow accumulation of lead upon the system. Dr. Christison adduced some examples of exceedingly small doses of lead being taken in water for more than twelve months before its evil effects became apparent. He, therefore, thought it yet remained to be proved that the sulphite of lead was without action on the system, since we know nothing of the influences of the solvents it would meet with in the system, or of the influences of vital action. Rabbits, he was prepared to say, should be entirely rejected in these inquiries, since he had found that they were not affected by many poisons. Dogs and cats were the only animals which could, from their internal structure, be regarded as the representatives of the human system in these investigations.

On the Air and Water in Towns, and the action of Porous Strata on Water and Organic Matter; by Dr. R. A. SMITH.

It is a matter of great importance to find from what source it is best to obtain water for large towns, and how it is to be collected. To these points Dr. Smith particularly directs attention. Regarding the conditions of many springs, which never become muddy, but possess a constant brilliancy and a very equal temperature at all seasons of the year, the author thinks that there is a purifying and cooling action going on beneath. The surface water from the same place, even if filtered, has not the same brilliancy; it has not the same freedom from organic matter, neither is it equally charged with carbonic acid or oxygen gas,—there are other influences therefore at work. The rain which falls has not the purity, although it comes directly from the clouds; it may even be wanting in cleanness, as is often the case. Springs rise through a great extent of soil, and collect a considerable amount of inorganic salts; and it is shown by Dr. Smith that their purity is due entirely to the power of the soil to separate all organic matter, and at the same time to compel the mixture of carbonic acid and oxygen. The amount of organic matter removed in this way is surprising, and it is a most important and valuable property of the soil. The change even takes place close to cess-pools and sewers; at a very short distance from the most offensive organic matter there may be found water having little or none in it. As an agent for purifying towns, this oxydation of organic matter is the most extraordinary, and we find the soil of towns which have been inhabited for centuries still possessing this remarkable power. St. Paul's churchyard may be looked upon as one of the oldest parts of London, and the water from the wells around it is remarkably pure, and the drainage of the soil is such that there is very little of any salts of nitric acid in it. If the soil, says Dr. Smith, has such a power to decompose by oxydation, we want to know how it gets so much of its oxygen. We must, however, look to the air as the only source, and see how it can come from it. When water becomes deprived of oxygen, it very soon takes it up again,—as may be proved by experiment. This shows us that as fast as the oxygen is consumed by the organic matter, it receives a fresh portion, conveyed to it by the porous soil. Several experiments of the following character were given, to show the filtering power of the soil. A solution of peaty matter was made in ammonia; the solution was very dark, so that some color was perceived through a film of only the twentieth of an inch in thickness. This was filtered through sand, and came out perfectly clear and colorless. Organic matter dissolved in oil of vitriol was separated from it by a thickness of stratum of only four inches. A bottle of porter was by the same process deprived of nearly all its color. The material of which this filter is made is of little importance. One of the best, according to Dr. Smith, as far as clearing the water is concerned, being of steel filings,—oxyd of iron, oxyd of manganese, and powdered bricks all answering equally well. This shows that the separation of the organic matter is due to some peculiar attraction of the surfaces of the porous mass presented to the fluid. This paper was a continuation of Dr. Smith's Report published last year,—and he proposes continuing the inquiry.

On the Proportion of Phosphoric Acid in some Natural Waters; by Prof. VOELCKER.

The object of this paper was to draw attention to a natural source from which many of our fields may be economically supplied with phosphoric acid. Prof. Fownes has shown that traces of phosphoric acid are met with in many rocks of igneous origin, but also in stratified rocks, particularly in limestone rocks, the presence of phosphoric acid has been indicated by several chemists. The author found the proportion of phosphoric acid in graptolite, from the neighborhood of Cirencester, amounting to 0·124 per cent., equal to 0·260 of bone-earth, and in Stonesfield slate from the same locality amounting to 0·117, equal to 0·244 per cent. of bone-earth. As water, charged with carbonic acid, is capable of dissolving bone-earth, this important fertilizing substance is found in many natural waters, percolating rocks which contain phosphoric acid. Such waters, therefore, may be applied with advantage for irrigation. The advantages derived from this too often neglected natural source, are strikingly exhibited in the irrigated meadows in the neighborhood of Cirencester; and it is the opinion of the author that one of the chief causes of the beneficial effects which follow the application of the water for irrigation in this locality, is to be found in the phosphate of lime it contains. In a tea-kettle incrustation formed in a short period by this water, the proportion of phosphoric acid was found to amount to 1·25 per cent., showing a considerable quantity of this acid present in the water. A very hard water from Edinburgh likewise proved to contain phosphoric acid, but its proportion was not so large as that in the Cirencester water, the quantity of phosphoric acid in a boiler incrustation formed by this Edinburgh water being only 0·427 per cent. Sea-water also contains phosphoric acid, but the proportion amounts to mere traces. A quantitative determination of phosphoric acid in the boiler deposit of a Canada steamer gave only 0·0306 per cent., and that in a boiler incrustation of a steamer plying between Dublin and Liverpool 0·0424 as the per-centage of phosphoric acid. In conclusion, the author recommended Svanberg's test, molybdate of ammonia, as a ready means for deducing the presence of phosphoric acids in natural waters.

The Phosphorescence of Potassium; by Mr. W. PETRIE.

While speculating on the consequences of the dynamical theory of heat, I was led to the conclusion that cold potassium *ought* to be found *luminous;* and farther, that it ought to be only about a *tenth part* as *luminous* as *phosphorus.* On testing this experimentally, with the precautions for sensitive vision which the anticipated feebleness of the light indicated to be necessary, the result was, that on dividing a bit of potassium, (which was quite dry, being protected only by a coating of bees' wax,) the halves showed two *distinctly luminous sections;* the light being about *a tenth* of that from a similar surface of phosphorus, as far as the eye could make the comparison. The light diminished, naturally, as a protecting coating of oxyd was formed, but remained just perceptible to the most sensitive sight, as long as half an hour.

On the presence of Fluorine in Blood and Milk; by Dr. G. WILSON.

In 1846 I announced to the Royal Society of Edinburgh that after finding that fluor spar was soluble in water, and occurred in many natural waters, I thought it well to seek for it in milk and blood, and found distinct evidence of its presence in both. The proofs however were not so decisive as I could have wished. This summer, however, I have employed the fresh drawn blood of the ox. About 26 imperial pints or 3 gallons of blood were made use of. From the large scale on which the experiment was conducted, and the simplicity of the process followed, the evidence in favor of the presence of fluorine in the blood of the ox seems unexceptionable; and it cannot be doubted that the blood of other animals will be found to contain the same element. I presume it to be present in the state of fluoride of calcium, and that its amount is very small; but I have not attempted its quantitative determination. Milk was examined in a similar way, with nine imperial pints of rich milk from a country farm. The vapor which they evolved etched glass distinctly. The ashes of twelve pound of new skim-milk cheese made this spring treated in the same way occasioned deep etching of glass. The ashes of four imperial pints of whey treated in the same way have barely marked glass so as to show the faintest outlines when breathed upon. In all probability the fluorid of calcium is associated with the phosphate of lime, and when milk is coagulated separates along with the caseine. Dr. Wilson also stated that he had repeated the inquiry into the solubility of fluorid of calcium in water, reported to the Association at its Southampton meeting, and with the same result, viz., that 16 fluid ounces, or 7,000 gr. of water, at 60°, dissolve 0·26 gr. of fluor spar.

Another New Planet.—On the 13th of Sept., 1850, another new planet was discovered by Mr. J. R. Hind, of London. Its place, Sept. 13, $11^h\ 29^m\ 3^s$ Gr. m. t. was R. A. $23^h\ 44^m\ 45^s{\cdot}08$, and N. decl. $14°\ 6^m\ 42^s{\cdot}9$. It is probably one of the Asteroidal group.

Supposed Staurotide of Norwich, Mass.—Notwithstanding the resemblance to Staurotide in these crystals, and the identity with that species in the prismatic angles, they are found by Mr. W. J. Craw, on chemical examination, to be a phosphate of manganese, iron and lithia. Mr. Craw has them under investigation, but has not yet completed his analyses. Several of the crystals appear to have a slight obliquity between the terminal and lateral planes.—J. D. D.

General Index.—We close this volume with a general Index to the first ten volumes of this series of the Journal, intending to continue the plan with every succeeding tenth volume, believing that the value of the Journal will be thus enhanced, as well as its convenience to our readers. Some articles now on hand, are consequently deferred to another volume.—*Eds.*

Errata.—The Errata of the ten volumes of this series are to be found on page viii of each volume, also in volume iii, p. 464; also for additional errata of vol i, see vols ii. and iii, p. viii; of vol. iii, see vol. iv, p. viii; of vol. v, see vol. vi, p. viii; of vol. vi, see vol. vii, p. viii; of vol. viii, see vol. ix, p. viii.

Other Errata not mentioned, are as follows:—

Vol. iv, p. 278, for Tautolite, read Tachylite,
p. 356, in note, for vol. i, read vol. ii.

Vol. v, p. 337, 8 l. from top, for $\dot{C}a\,\ddot{C}$, read $\dot{C}a\,\dddot{S}$.

Vol. vii, p. 114, 2 l. from top, for Marbury, read Marburg.
20 l. from top, for $4\overset{...}{\bar{A}l}\,\dddot{Si}$, read $5\overset{...}{\bar{A}l}\,\dddot{Si}$.

Vol. viii, p. 428, 7 l. from top, for Ephipphora, read Ephippiphora; and for Phil. Mag., read Ann. Mag. Nat. Hist.

Vol. ix, p. 399, last line, for Journal, read Proceedings.

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The book before us is, in short, a graphic encyclopædia of the sciences—an atlas of human knowledge done into maps. It exemplifies the truth which it expresses—that he who runs may read. The Thermal Laws of Leslie it enunciates by a bent line running across a map of Europe; the abstract researches of Gauss it embodies in a few parallel curves winding over a section of the globe; a formula of Laplace it melts down to a little path of mezzotint shadow; a problem of the transcendental analysis, which covers pages with definite integrals, it makes plain to the eye by a little stippling and hatching on a given degree of longitude! All possible relations of time and space, heat and cold, wet and dry, frost and snow, volcano and storm, current and tide, plant and beast, race and religion, attraction and repulsion, glacier and avalanche, fossil and mammoth, river and mountain, mine and forest, air and cloud, and sea and sky—all in the earth, and under the earth, and on the earth, and above the earth, that the heart of man has conceived or his head understood—are brought together by a marvellous microcosm, and planted on these little sheets of paper, thus making themselves clear to every eye. In short, we have a summary of all the cross-questions of Nature for twenty centuries—and all the answers of Nature herself set down and speaking to us voluminous system *dans un mot*. . . . . Mr. Johnston is well known as a geographer of great accuracy and research; and it is certain that this work will add to his reputation; for it is beautifully engraved, and accompanied with explanatory and tabular letterpress of great value.—*London Athenæum.*

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We possess, indeed, the valuable Physical Atlas of Mr. Keith Johnston, which may well be associated with Mrs. Somerville's book, for their mutual illustration. But this Atlas is itself a recent undertaking, and by no means yet known or studied commensurately with its merits. . . . Such works are as essential to the study of Physical Geography as are experiments to the chemical student, or models and diagrams to instruction in the mechanical sciences—or, what is more pertinent in this case, as common maps to common geography. Their linear delineations to the eye are an admirable short-hand writing, conveying impressions to the mind far more explicit and forcible than any mere descriptions can afford—suggesting comparisons and relations, and giving facilities of reference which can in no other way be equally attained. The Atlas ought to have a place in every good library.—We know no work containing such copious and exact information as to all the physical circumstances of the earth on which we live, nor any of which the methods are so well fitted for the instruction of those who come ignorantly to the subject.—*Quarterly Review.*

Although we have thus endeavored to give our readers an idea of the valuable contents of the Physical Atlas, yet we are persuaded that it is only by an examination and study of the work itself, that they can form anything like an accurate estimate of the amount of instruction and even amusement which it affords. In public libraries and reading-rooms the work will be of inestimable value; and in our public, and even private schools, the teacher can scarcely perform his duties to the youth under his charge, unless he gives them the advantage of studying the phenomena of the material universe through the medium of its graphic representations.—*North British Review.*

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