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* This Art. should have been viii. and the following ix., &c. &c.

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MISCELLANIES.—FOREIGN AND DOMESTIC.

Natural Philosophy.

Chemistry.

Geology and Mineralogy.

Miscellaneous Intelligence.

ERRATA.

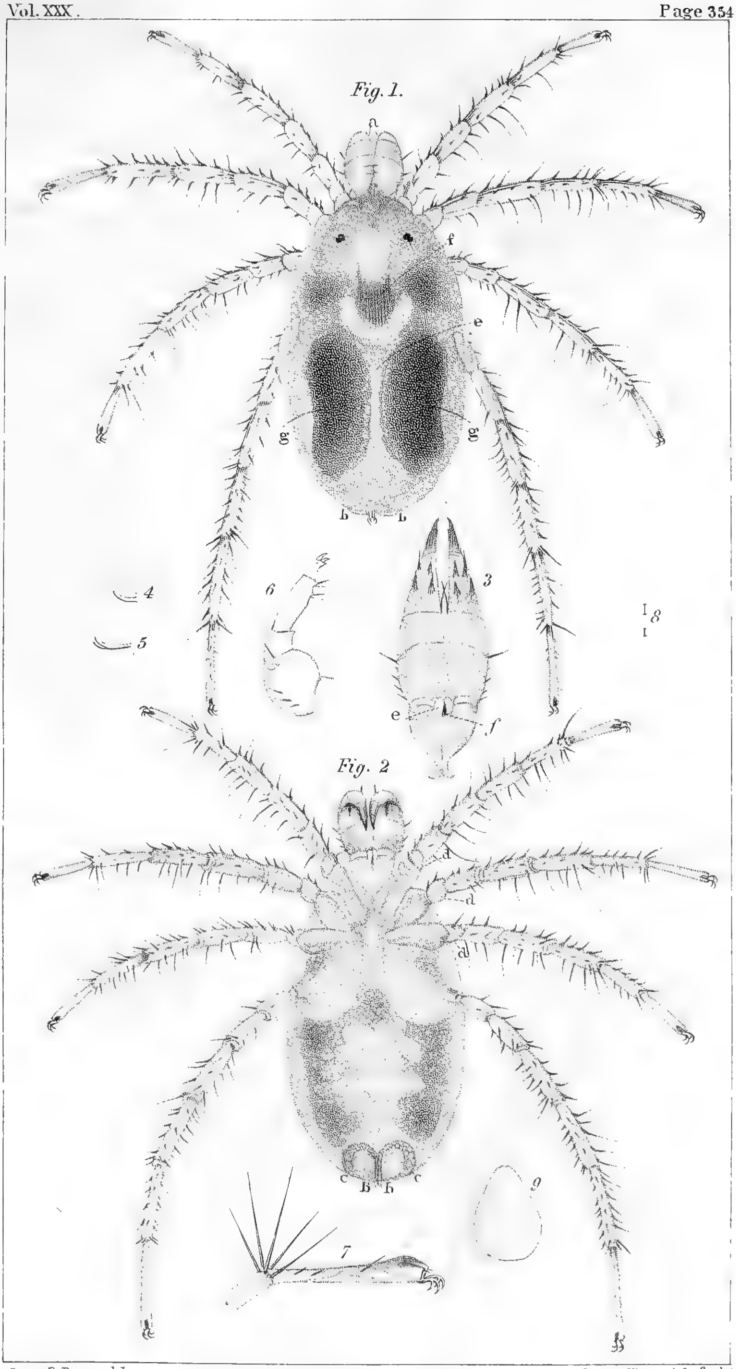
P. 85, l. 3 fr. bot. for *imperious* read *impervious.*—p. 86, l. 18 fr. top, for *three* read *tree.*—p. 89, l. 18 fr. bot. for *liue* read *luce.*—p. 92, l. 29 fr. top, for *Anderson* read *Audubon.*—p. 95, l. 2 fr. top, for *Cypselus* read *Cypselsus.*—p. 104, l. 13 fr. top, for *are* read *were.*—p. 105, l. 8 fr. top, for *pure* read *free.*—p. 177, l. 10 fr. top, for *Plenakite* read *Phenakite.*

ERRATA.

The reader is requested to correct the following errata in Vol. XXX. of the American Journal of Science. The writer of that article, at the time of its publication was in the Indian country, on Red river, several hundred miles west of the Mississippi, where the proof sheets could not be sent to him; and it is only since his return from that part of the country, that he has seen the article in print. There are several errors in punctuation, but the verbal errors only will be noticed.

W. W. Mather, *Mining Engineer.*

P. 326, l. 3 fr. bot. for *quality*, read *equality.*—P. 328, l. 9 fr. top, for *exhaustible*, read *expansible;* l. 11 fr. top, for *will thrust*, read *will be thrust.*—P. 329, l. 7 fr. top, for *those*, read *that.*



James D. Dana del. Daggett, Hinman & Co. Sculp.

HYDRACHNA FORMOSA

THE

AMERICAN

JOURNAL OF SCIENCE, &c.

Art. I.—*Geological and Miscellaneous notice of the region around Fort Winnebago, Michigan Territory; by* Lieut. D. Ruggles,* of the U. S. army.

Fort Winnebago is situated 43¾° N. lat. and 12° W. lon. from Washington. The general character of the country is secondary; and the soil within the section this survey contemplates, varies between calcareous, argillaceous and siliceous. The most striking features are the various combinations of water, marsh and upland, which, perhaps, render this particular spot as interesting to the geologist, as any equal portion of a country but partially explored.

Here is a summit level from whose sources the waters of the St. Lawrence and Mississippi flow to the extremes of our republic. This summit level is called the Winnebago Portage, and is a marsh of one mile and a quarter in breadth, between the Wisconsin and Fox river, over which at times of high water, boats freighted with from twenty to forty tons, pass without interruption; and at ordinary stages of water, this, although the principal, is very far from an in-

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Fort Winnebago, M. T., May 20, 1835.

* To Prof. Silliman.—Sir—Having been assured that a brief geological survey of this section of U. S. Territory, would interest scientific men generally, and be considered worthy, perhaps, of the medium which your valuable Journal affords, I am induced to throw into form, some observations I have made during my residence here.

The observations, I must add, do not profess to be purely geological, but of a mixed nature, so as to embrace the most prominent objects of interest which surround us. The accompanying map, is the result of observation and not of admeasurement, consequently it cannot be rigidly accurate. It has been submitted to the examination of several gentlemen well qualified to judge, but no material errors have been pointed out. I submit it, Sir, although with diffidence, to be disposed of at your pleasure. Yours, &c. D. Ruggles.

surmountable obstruction in the connected navigation of the Fox and Wisconsin rivers.

Fox river is generally termed a navigable stream, although there are some rapids and obstructions rather difficult to surmount. They are overcome, however, by ascending boats, by making short portages and by 'cordelleing.' This sluggish river is fed principally by springs in this vicinity, and is bordered by low wet marshes, in which wild rice grows in luxuriant profusion.

The Wisconsin river is broad, rapid and interspersed with a great many wooded islands, partaking of the character of the Mississippi. It is moreover filled with quicksands, which are obstacles rendering the navigation rather troublesome, especially in low water. I mention the character of these rivers, on account of its having been contemplated, and indeed proposed to connect them by a canal at the portage. Having some knowledge of the nature of this design, I am induced to give my opinion of its practicability.

1st. The canal must be supplied with water principally from the Wisconsin and Fox rivers; the former of which will give the best supply with the minimum length of canal. In this case the length would not exceed one mile and a half.

2nd. Two river locks would be required, in addition to which two lift locks would be advantageous and perhaps absolutely necessary.

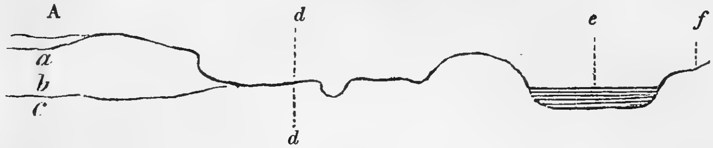
3rd. Owing to the soluble nature of the soil, revetment walls or wooden curbs would be required, both for economy and durability. Revetments of masonry would be too expensive; there being no fit materials within at least eight or ten miles of the ground. It would be advisable to resort to curbing, for which materials are easily obtained, partly by driving piles and partly by planking. The width should be about twenty feet—depth eight feet.

4th. Allowing for the disadvantages of such an undertaking in a wild country, I feel sure fifty thousand dollars would cover all expenses.

At present such an investment would be unprofitable, but in a few years it must necessarily be profitable, unless the tide of emigration ceases towards the west.

I will now return to the main subject. There are no elevations within the district under consideration exceeding two hundred and fifty or three hundred feet, and no very abrupt terminations, cliffs or deep excavations; consequently these observations will be confined to the minutiæ of the subject, rather than its most striking features and illustrations; nor can I obtain so perfect a section of the inequalities that do exist as I desire.

The following section is cut from a small hill of about one hundred feet in height, called stonequarry hill, of a range E. N.E. and W. S. W.



A, Stonequarry hill.—*a*, Limestone.—*b*, Sandstone.—*c*, Limestone.—*dd*, Calcareous soil.—*e*, Swan lake.—*f*, Prairy.

Stonequarry hill appears to be an uplifting of the earth, a peculiarity common to most if not all the elevations in this country. There are some fragments of tolerably pure limestone imbedded in the surface near the greatest elevation, but more generally they are impure calcareous sandstone, and pure sandstone of a very peculiar and interesting character, always found in place or horizontal. This sandstone is so far suitable for architectural purposes, as to bear exposure to the atmosphere, where it hardens after slight disintegration. I consider most kinds of wood superior to it for hydraulic purposes.

This stone appears in laminæ, three or four feet in thickness, with frequent vertical fissures, as if affected by some powerful convulsion, and when exposed, the surface has become hard. The predominating color is white, often intersected by portions which are blood-red, pale-red, pink, pale-orange and black.

The structure is very peculiar. The mass consists of an aggregation of pure siliceous particles, about three fourths the size of mustard seed, devoid of every appearance of cement. I cannot conceive of a more clear and beautiful illustration of cohesion.*

The fracture is conchoidal and uneven—small fragments, after exposure to a moist atmosphere, crumble between the fingers, and heat renders it a very excellent sand. I do not give this as a general description of sandstone of the country, but as a remarkable exception. I recently had an opportunity of examining its general structure to such a decided advantage, that I am induced to present the result of my examination, although the location is not embraced

* Very accurately described as appears by a specimen forwarded, which greatly resembles a very pure arenaceous quartz.—*Ed.*

by this view. I refer to excavations made for a shot tower, belonging to Daniel Whitney, Esq. upon the Wisconsin, about sixty miles below this, to the politeness of whose agent, I am indebted for assistance and facilities during my examination. The 'drop' is one hundred and eighty two feet. The building is situated upon a cliff, terminated by the river, of which the lower projection extends somewhat beyond the upper portion, which is fifty five feet in perpendicular height, and upon the verge of which the building stands. From the bottom of this portion of the cliff, a vertical shaft is sunk one hundred and twenty seven feet, through variegated sandstone, which, for about four feet in one place, is so much indurated, as to become as hard as flint. This shaft is about six feet in diameter at top, and eight feet at bottom. There is a horizontal shaft or 'drift' at the base, meeting the vertical one, of eighty seven and one half feet in length; in which the sandstone appears in horizontal laminæ of different colors, textures and thickness, presenting an agreeable aspect to the eye. The stone is so hard as to bear exposure to the atmosphere; is of uneven fracture, and composed of siliceous particles, united by a cement of the same substance, very much attenuated. As sandstone appears on most of the elevations, and is subject to disintegration, the surrounding soil is siliceous rather than calcareous, although at a distance the reverse is often, if not generally observed.

It is believed, although observations sufficient to demonstrate the fact have not been made, that the sandstone is underlaid by secondary limestone. I entertain this opinion and ground it in part on the following circumstances. The soil in this vicinity resembles that of a great part of the mining country which lies south of this, and there, shafts have been sunk more than a hundred feet, at which depth secondary limestone was the prevailing rock. From this comparison, the enquiry suggests itself to the mind, whether mineral beds may not be found within the space this view embraces. If the following indications—the above-mentioned resemblance of soil,* the small particles of galena which have been found, transported perhaps by the natives, and prevalence of minerals often associated with galena are conclusive, I can bear evidence to their existence; yet these indications convince me only of the probability of the existence of mineral lead in this district.

* The prevalence of what is called 'mineral weed,' which abounds where mineral lead (or lead ore) is found, although mineral lead is not always found where it abounds.

The limestone which abounds about the surface, is subject to rapid decomposition, and is often found passing into marl, which produces a luxuriant soil. On the other hand, it occasionally presents the closeness of aggregation, and hardness of marble, and will receive a fine polish. I have been credibly informed that marble of a superior quality is found at a place called Four Lakes, about forty miles south of this place.

Limestone is found in this district on the elevations, sufficiently pure to make lime for architectural purposes. The only organic remains that have been found in this rock to my knowledge, are encrini, and indications of bivalve shells of some kind, the character of which has disappeared. There are, however, some indications of vegetable substances often observed in the indurated fragments; they resemble the fern leaf.

Among the elevations of this country, we often meet with extensive prairies. The origin of prairies is doubtless attributable to the extensive fires which scour the whole country when vegetable matter has become dry; and I believe it is the prevailing opinion among men of observation that this is the principal, if not the only cause. We have constant evidence of the operation of this cause around us—the country is very thinly wooded, and it is still diminishing; the dry and decayed trees, are often felled to the ground by the flames, and the most flourishing arrested in their progress.

In this northern climate, the fires are more destructive perhaps, than in the southern, because vegetation is of a shorter duration here, yet prairies abound in the south west, resulting however from the operation of nearly the same causes.

In some instances, prairies are found stretching for miles around, without a tree or shrub, so level as scarcely to present a single undulation; in others, those called "rolling prairies," appears in undulation upon undulation, as far as the eye can reach, presenting a view of peculiar sublimity, especially to the beholder for the first time. It seems when in verdure, a real troubled ocean, wave upon wave, rolls before you, ever varying, ever swelling; even the breezes play around to heighten the illusion; so that here at near two thousand miles from the ocean, we have a fac-simile of sublimity, which no miniature imitation can approach. On many of the prairies, the soil is equal, probably, to any in the world—vegetation is rapid and luxuriant; yet they meet with any thing but cordial salutations from the passing emigrant, who turns his anxious gaze, and bends his course towards the nearest grove.

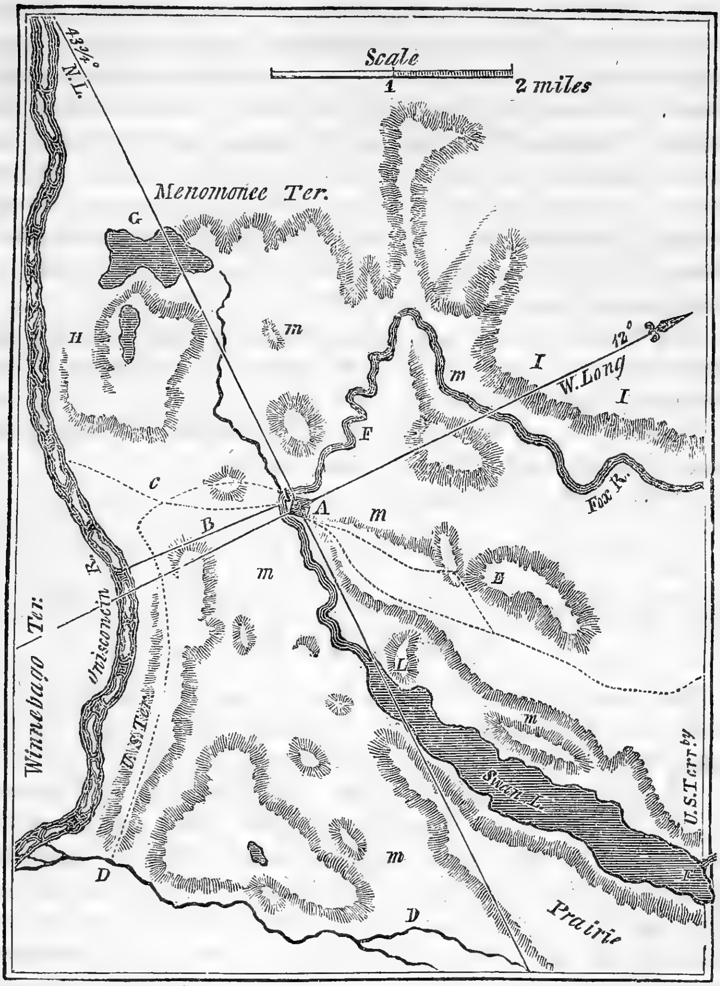
This want of wood, however, is not an irremediable evil, since by securing the surface from the ravages of the flames, wood springs up and grows rapidly. The marshes which are very extensive about us, are overflowed only in times of high water, and then not entirely. They are in many places, covered with bog, to considerable depths below the surface, which has been found suitable for fuel. Below the bog, layers of plastic clay and sand make their appearance, which are underlaid by light quicksands. The marshes are supplied with water by an abundance of cold, clear springs, which flow through them into the neighboring rivers—a circumstance to which we must attribute the absence of stagnation and putrid exhalations, and in consequence of which we find, as far as health is concerned, no inconvenience in being almost entirely surrounded by marshes, for we are subject to no diseases attributable to our particular location.

Luxuriant grass, often very excellent, grows upon the marshes, which supply us with grazing and hay. Wild rice, (Zizania cloverlasa,) is very plentiful, especially along and near the shores of Fox river. It is said to be as palatable and nutritious as cultivated rice. It is gathered in great quantities by the natives, and, at times, forms their principal subsistence; it is very often used by the border-settlers, and supports an innumerable quantity of wild ducks, some geese, and other water fowl.

The waters in our lakes and rivers, are frequently so pure that we can see to a surprising depth with distinctness. They abound with several kinds of fish, which are often very large and excellent. There are two or three small lakes in this vicinity, without outlets, well supplied with very fine fish; they are so situated, however, that in times of extremely high water their margins would probably overflow.

Several species of shells are found in the rivers and lakes; they are many of them interesting. Both univalves and bivalves are found.

An extensive bed of clay has been found, situated on the bank of the Wisconsin river, which is intersected by strata of sand, forming, by mixture, a suitable combination for brick of a superior quality. A gentleman of experience, and well qualified to judge upon the subject, assures me that this bed is an excellent potters' clay.



References.—A. Fort Winnebago; B, Portage line; C, Proposed canal; D, D, Duck creek; E, Stonequarry hill; F, Fox river; G, Loch Mogie; H, Clay bed; I, Indian territory; *m*, *m*, Marsh.

There is but a small variety of minerals about us, and few of those we have are not at all rare; the following enumeration embraces the principal. Native copper, amorphous, pure, and malleable; rare. Ferruginous sulphuret of copper, purple copper, in small quantities; also, slight traces of blue and green carbonate. These minerals are rare, but the indications are such as almost to confirm the prevailing opinion, that extensive veins of copper ore may be found in this vicinity. Brown hematite is found in small shining nodules, and in confused crystallization, among sandstone, limestone, and scattered over the surface of the small elevations. Hornblende is occasionally found; tourmaline also, but always imbedded. I believe they are erratic. Hornstone, small crystals of quartz, incrusting small cavities in limestone, and also gneiss and granite, both of which are erratic. The granite is composed of red feldspar, transparent quartz, and slight traces of mica. It comes, perhaps, in boulders, from the shores of Lake Superior. There are indications of coal, but no satisfactory examination has been made.

As I intended to give a mere enumeration of facts, relating to the most interesting points, I have been rather brief, believing such a course the most judicious.

My sincere thanks are due to several gentlemen, for their judicious views and assistance in my examinations.

Fort Winnebago, M. T. May 20, 1835.

Art. II.—*Remarks on Indian Summers; by* Dr. Lyman Foot, of the United States' army.

The article on Indian summer in your Journal (Vol. xxvii, page 140,) has excited a good deal of attention here; but, we do not agree with the writer of that article in many particulars.

1st. As to the origin of the name "Indian summer." He says it "is derived from the circumstance of this period of the year, being selected by the aborigines of the country, as their hunting season, &c." Now, so far as our knowledge extends, and we have been pretty well acquainted with the western and north western Indians for the last thirteen years, it is the season of all others in which Indians hunt the least. We have taken considerable pains to ascertain from the Chippeways, Menominees, Winnebagoes and others, whether they know, or notice, what we call Indian summer, and if so, what they call it.

All who are acquainted with the western and north western Indians, know, that during the month of April or May, according to the latitude in which they reside, they collect together at what they call their villages, or towns. These towns are always situated on good land, and near some fine lake or river which abounds with fish. The general local advantages of these old Indian villages are so well known, that when the land is brought into market, the sections covering them are sought for with avidity by land speculators.

Here they plant their corn, and a few other vegetables, the squaws performing all the labor, while the men spend most of their time in fishing. They rarely hunt, during the summer months, till ducks and geese begin to abound and to be in good condition, which is from the latter part of August to the first of November, during which time they kill great numbers of them in the waters contiguous to their villages.

Thus they live from about May to November, collected together by hundreds—sometimes even hundreds of families.

After gathering their corn and wild rice, if in a rice country, drying their fish, and packing in small sacks provisions for a long march; they prepare for what they call their winter's hunt. That is, they entirely desert their villages, and disperse in small bands to every part of the country, diving into the darkest forests, and ascending the various streams to the remotest parts of their territory, where they pass the winter in hunting and trapping animals, whose skins are valuable, and the flesh of which serves them for food.

Thus much to come at the origin of the name, "*Indian summer.*" If you ask an Indian in the fall when he is going to his hunting ground, he will tell you, when our fall summer comes, or when the Great spirit sends us our fall summer—meaning the time in November which we call Indian summer. And they actually believe that the Great spirit sends this mild season in November, after the cold fall rains, for their special benefit. Thus you see, the poor untutored Indian has *faith* in the goodness of the great "I am." May it not be counted to him for righteousness?

We agree with the writer of the article as to the usual time of the appearance of Indian summer. It is, in all latitudes in which we have served, (from Fort Brady, outlet of Lake Superior, to Jefferson Barracks, Missouri,) sometime in November, or not far from that time. We leave it to others to explain the cause or causes of our autumnal rains. We mean that succession of storms and rains, commencing with what is commonly called the equinox, about the last week in September, and ending, usually about the middle of October, a little time after which, Indian summer commences. We cannot subscribe to the assertion, that the south winds prevail during Indian summer. So far as our experience goes, and we have kept a diary of the weather for more than sixteen years, the prevailing winds in November are some point west of south and north, or perhaps west and north of west during Indian summer. We have before us the meteorological register for the years 1822, 1823, 1824, and 1825, from observations made by the surgeons of the army at the military posts of the United States, prepared under the direction of Joseph Lovell, M. D., Surgeon General, U. S. army, from which we here give the following extract.

Place of observation.	Prevailing winds in November			
	1822.	1823.	1824.	1825.
Fort Brady, (Outlet Lake Superior,) -			S. E.	S. E.
" Snelling, (Falls St. Anthony,) -	N. W.		S.	N. W.
" Sullivan, (Eastport, Maine,) - -	N.	N. W.		W.
" Howard, (Green Bay, M. T.,) -	N. E.	S. W.	S. W.	S. W.
Council Bluffs, (Missouri river,) - -	N.	N.	S.	S. W.
Fort Crawford, (Up. Miss.,) - -	N. W.		N.	
" Wolcott, (R. I.,) - - -	S. W.	N. W.	S. W.	S. W.
" Columbus, (N. Y.,) - - -	S. E.	N. W.	N. W.	N. W.
" Miflin, (Delaware,) - - -		N. W.		
Washington, (D. C.,) - - -		N. W.	N. W.	N. W.
Fort Johnson, (N. Carolina,) - -	W.	N.	N.	N.
" Moultrie, (S. Carolina,) - -		N. E.	N. E.	
Cant. Jesup, (Louisiana,) - - -		S. E.	N. E.	S. E.
" Clinch, (Florida,) - - -	S. E.	N. E.	S. E.	

From the above table it appears, that the winds bearing north and west, prevail over the south, and we believe that west and north west winds would be found to prevail over all others during Indian summer.

We agree with the writer, "that electrical causes are negatively concerned in the production of Indian summer," or rather that they are *positively* concerned. For during the summer months the earth, probably from its dryness, often becomes the negative, and the atmosphere, being loaded with moisture, the positive conductor of electricity. Hence the frequent occurrence of thunder gusts.* The vapor collecting, forms clouds highly charged with electricity, which give it out to the earth.

After the autumnal rains have completely wet the surface of the earth, things are reversed. The earth now becomes the positive, and the atmosphere the negative conductor of electricity, and thun-

* Thunder gusts are much more frequent, and more severe on the extensive prairies of the west, than in the Atlantic states.

der gusts cease. The earth's surface soon absorbs all the rain which has fallen, the atmosphere is neither hot nor cold, *it* also soon becomes dry, and Indian summer commences. This leads us to differ with the writer of the article on Indian summer, in another view which he takes of the subject, viz: He says, "one of the most remarkable phenomena of Indian summer, is the peculiar redness of the sky, &c." So it is—but how does he account for it? He speaks of "the foggy stratum near the earth's surface." Does fog in May or June, when it is much more prevalent than in November, produce that redness of the sky, so peculiar to Indian summer? Here it certainly does not, and we suspect not about Baltimore. Besides, according to our experience, the atmosphere is remarkably dry during Indian summer. We have never kept a hygrometer, but have remarked that whenever a rain falls, this smoky aspect of the atmosphere soon disappears, which it should not do, if it depended on fog or vapor. Again he says, "this redness of the air together with the mechanical irritation produced by the denseness of the aerial vapor, excites a painful affection of the eyes." This does not accord with our experience. We know that inflammation of the eyes is very common in the west, particularly during Indian summer. We know it by sad experience ourselves, and by many hundred cases which we have treated. But we have always found that pure aerial vapor, and even dense fog, so far from producing inflammation of the eyes, was congenial to, and always relieved those patients affected with it. The *real* cause, in our opinion, of the smoky appearance of the atmosphere, and the painful affection of the eyes, is what the writer calls an "optical illusion." That is, by the "burning of the decidua which are collected together in the fall season," and "the firing of the neighboring forests." We do not mean to be understood that the burning of the forests, prairies, &c. is the *cause* of Indian summer, but a consequence of it, and the smoky appearance of the atmosphere is *caused* by it. Now we come to speak of the *real* cause of Indian summer. Or rather, to state the facts as they occur, according to our observation, and give such explanations as seem to us rational. We have before said that during the summer months, the earth becomes dry and the atmosphere is surcharged with vapor, the former being often the negative, and the latter the positive conductor of electricity, &c. Hence the frequent occurrence of thunder gusts. But these thunder gusts do not fully restore the equilibrium, either of electricity or moisture. The earth's surface is on the whole becoming dryer and dryer. September comes, and with it a reduction of temperature from a well known cause. The vapor begins to condense into clouds. Currents and counter currents of air are formed, and, aided no doubt by electrical phenomena in some way we do not understand, the autumnal rains commence, with what is called the "equinoctial storm." These continue generally till past the middle of October, when the equilibrium seems, from some cause, to be restored. The elements cease to contend, a mild bland atmosphere ensues, and the earth soon absorbs the rain which has fallen. The sun has yet influence enough to keep up a mild temperature, under this quiet state of the atmosphere, and Indian summer commences. As to the *increased* temperature, during Indian summer, we cannot agree to it. From the document we have quoted, (Meteorological register,) it appears, that the mean temperature for November, is somewhat lower than that of October. It is from the quiet placid state of the atmosphere, that some are led to suppose it is actually warmer. But he who keeps an accurate record of the thermometer will find it is a mistake. Frosts have already put a stop to vegetation. The leaves have fallen, annual plants have become dry, and the fields, the swamps, the forests and the prairies are set on fire by Indians and hunters. The smoke arising from them is abundantly sufficient to produce all that peculiar redness of the sky so common to Indian summer. We have seen at a glance, thousands and tens of thousands of acres on fire; the smoke of which no doubt, affected the atmosphere for three or four hundred miles. This appearance of the decline of Indian summer in the eastern states, of which the writer speaks, may be thus accounted for. The forests there are disappearing. What are left, (Indians, there are none,) hunters dare not set on fire; therefore the smoky atmosphere, so identified with Indian summer, is of less frequent occurrence. Real Indian summer probably continues about the same; but without that peculiar redness of the sky, it is not noticed. We arrived at this post the third day of last November. We had three weeks of Indian summer, with all that peculiar redness of the sky, mentioned above, in great perfection. The prevailing winds were west and north of west, with a dry atmosphere. The country was on fire in various places for forty miles around us.

I have thus given you some of my views of Indian summer, founded on practical observation. As the writer of the article on this subject in Vol. xxvii of your Journal, declares it to be his object to "elicit further enquiry from others," so it is ours. We have long observed, with curious attention the Indian summer, and should like to see it discussed by abler pens than ours.

Fort Winnebago, May, 1835.

Diary of the Weather at Fort Winnebago, (M. T.) for the quarter ending the 31st day of March, 1835.

Jan.	Therm'eter. 7, A.M.	2, P.M.	9, P.M.	Winds	Weather	Remarks.
1	22	27	13	N.	Cloudy.	
2	-2	6	-1	N.	Clear.	
3	-8	10	-3	N.	Clear.	
4	-8	16	2	N.W	Clear.	
5	-5	19	4	N.	Clear.	
6	6	26	16	S.	Clear.	
7	20	30	22	S. W.	Cloudy.	
8	20	30	17	S.	Clear.	
9	8	31	18	S. W.	Clear.	
10	15	38	36	S.	Cloudy.	High wind in the night.
11	30	39	32	S.	Cloudy.	Light snow during the night.
12	30	34	30	S.	Cloudy.	Light snow in the afternoon.
13	27	33	28	S. W.	Cloudy.	Snow in the night.
14	28	30	26	N.	Cloudy.	Snow all day and night, high wind in the night.
15	19	25	7	W.	Clear.	High wind in the night.
16	17	22	6	W.	Clear.	
17	00	22	4	S. W.	Clear.	
18	-1	28	24	S. W.	Clear.	
19	26	36	27	W.	Clear.	I slept in the woods this night.—L. Foot.
20	24	31	30	S. W.	Cloudy.	Snow.
21	26	27	17	N.W.	Clear.	
22	18	30	30	W.	Clear.	
23	12	26	13	N.	Clear.	
24	22	34	30	S. W.	Cloudy.	Snow and hail during the day.
25	32	36	32	N.W.	Cloudy.	Rain all night, sleet through the day, high wind.
26	30	30	30	N.	Cloudy.	Snow and sleet.
27	26	30	27	N.	Cloudy.	Light snow during the day.
28	25	32	27	N.W.	Cloudy.	Snow continued at intervals.
29	25	31	26	N.	Cloudy.	Snow continued, high wind.
30	20	28	20	N.	Cloudy.	A severe snow storm cont'd all night & day, high wind.
31	10	24	16	N.	Clear.	Snow storm ceased during the night.
	504	861	605	Total number of degrees.		

Feb.	Therm'eter. 7, A.M.	2, P.M.	9, P.M.	Winds	Weather	Remarks.
1	11	18	2	N.	Cloudy.	Light snow in the morning.
2	–10	2	–8	N.	Clear.	
3	–14	2	–10	N.W.	Clear.	
4	–5	6	00	W.	Clear.	
5	–6	8	–4	N.	Clear.	
6	–12	–4	–12	N.W.	Cloudy.	High wind, snow from 3 o'clock, P.M. till 5 P.M.
7	–24	–10	–19	N.W.	Clear.	High wind.
8	–26	–6	–15	S.	Clear.	High wind.
9	–8	7	–4	N.	Clear.	Snow during the night and forenoon.
10	–10	12	–6	S.	Cloudy.	
11	8	25	9	N.W.	Clear.	
12	16	30	28	S. W.	Cloudy.	High wind.
13	30	26	13	N.	Clear.	Light snow in the night, continued till 7 A. M.
14	–3	12	–3	S. E.	Clear.	
15	–15	17	2	S.	Clear.	
16	–10	26	18	S.	Clear.	
17	21	30	30	S.	Cloudy.	
18	24	29	18	N.W.	Cloudy.	
19	3	21	12	S. W.	Clear.	
20	25	31	40	S. W.	Cloudy.	Hail in the forenoon, high wind.
21	36	36	30	W.	Clear.	Light rain in the night.
22	14	28	16	S. W.	Clear.	
23	10	30	30	S.	Clear.	
24	28	40	10	S.	Cloudy.	Sleet began 3 p.m. followed by snow, cont'd all night, high w'd.
25	–3	9	–4	N.W.	Clear.	
26	16	–4	–12	N.W.	Clear.	High wind continued.
27	–12	14	–5	N.W.	Cloudy.	Light snow in the afternoon.
28	–10	3	–2	W.	Clear.	
	42	438	154	Total number of degrees.		

March.	Therm'eter. 7, A.M.	2, P.M.	9, P.M.	Winds	Weather	Remarks.
1	–5	14	4	S. W.	Clear.	
2	–4	14	6	N.W.	Clear.	
3	–3	16	5	N.	Clear.	
4	–4	24	10	S.	Clear.	
5	3	28	15	S.	Clear.	
6	12	37	22	S.	Clear.	
7	27	41	26	S.	Clear.	
8	21	44	30	N.	Cloudy.	
9	26	42	30	N.	Clear.	
10	25	42	32	N.	Clear.	
11	23	44	36	S.	Clear.	
12	35	41	31	N.W	Cloudy.	Rain and high wind.
13	30	42	33	S. W.	Clear.	
14	31	51	30	N.	Clear.	
15	24	46	33	S. W	Cloudy.	
16	33	32	19	N.W.	Cloudy.	A flock of ducks seen to-day, high wind, light snow.
17	14	33	32	S.	Cloudy.	High wind.
18	33	44	29	N.	Cloudy.	Light snow in the night.
19	28	44	38	S. W.	Clear.	Five pigeons seen this day.
20	40	45	28	N.W.	Clear.	High wind.
21	22	31	24	N.	Clear.	
22	19	32	27	N.	Clear.	
23	25	39	26	S. W.	Clear.	
24	30	54	38	S.	Clear.	
25	43	57	50	S.	Cloudy.	A flock of Brant seen this day, high wind.
26	46	45	32	N.W.	Clear.	Rain in the night.
27	30	52	42	S. W.	Cloudy.	High wind, rain in the night.
28	34	42	28	N.	Clear.	High wind.
29	33	47	32	N.	Clear.	
30	35	53	39	S.	Clear.	
31	40	64	42	S.	Clear.	
	746	1240	869	Total number of degrees.		

Abstract of the Meteorological Register kept at Fort Winnebago, for the quarter, consisting of January, February and March*, 1835.

Months.	Thermometer. Highest degree. 7 o'c.	2 o'c.	9 o'c.	Lowest degree. 7 o'c.	2 o'c.	9 o'c.	Mean temperature. 7 o'c.	2 o'c.	9 o'c.	Hottest day.	Coldest day.	Winds. Days, N.	Days, N.W.	Days, S. E.	Days, S.	Days, S. W.	Days, W.	Prevailing.	Weather. Days, fair.	Days, cl'dy.	Days, rain.	Days, snow.	Prevailing.
January, .	32	39	36	–8	6	–3	$16\frac{3}{31}$	$27\frac{24}{31}$	$19\frac{16}{31}$	11	3	11	4	-	5	7	4	N.	16	15	-	11	Fair.
February, .	36	40	40	–26	–10	–19	$1\frac{14}{28}$	$15\frac{13}{28}$	$5\frac{14}{28}$	21	7	5	8	1	7	4	3	N. W.	19	9	-	5	Fair.
March, .	46	64	42	–5	14	4	$24\frac{2}{31}$	40	$28\frac{1}{31}$	31	1	10	5	-	10	6	-	N.&S.	23	8	1	-	Fair.

* This table does not bear directly upon the Indian Summer, but having published in a former volume various details of the extraordinary cold weather of the late winter, we annex this table, communicated by Dr. Foot, along with a full account of every day.—*Ed.*

Art. III.—*Inquiry in relation to the alleged influence of Color on the Radiation of non-luminous heat; by* A. D. Bache, Prof. of Nat. Philos. and Chem., University of Pennsylvania.

In the following essay I propose to submit a few remarks upon a paper by Doct. Stark of Edinburgh, first published in the Transactions of the Royal Society of London, for 1833, together with an experimental inquiry into the alleged influence of color on the radiation and absorption of *non-luminous* heat.

The experiments were commenced soon after the paper referred to, reached this country, and in them was adopted what seemed to me the less exceptionable of two methods used by Doctor Stark, which actually bear upon the question of the radiation of non-luminous heat. It was my intention to examine the matter more fully than had been done by Dr. Stark, and to procure a more satisfactory induction by experimenting on a considerable variety of substances. In this I had the kind assistance of my colleague, Prof. Courtenay.

While these experiments were in progress, the remarks of the Rev. Professor Powell, of Oxford, on the paper of Doctor Stark, appeared in the Edinburgh New Philosophical Journal. They confirmed me entirely in the view of the inapplicability of most of the experiments made by Dr. Stark, to the determination of the question of the influence of color on the radiation or the absorption of heat.—Of this class were the absorption of heat, *radiant* heat being understood, as tested by the inverse of Count Rumford's method for comparing the conducting powers of substances used for clothing; also, as tested by the effect of the heat from the *flame* of an argand gas burner, thrown by a mirror upon the bulb of an air thermometer, which was variously coated. Of the same class were the experiments on radiation, as tested by the method used by Count Rumford, as above referred to; the enveloping materials of the inner thermometer being wools of different colors, and colored wheaten paste.

Not included in this class are the methods of ascertaining the rate of cooling of a thermometer of which the bulb was coated with different pigments, and of a glass globe filled with warm water and variously coated. I gave the preference to a modification of this latter method from the greater extent of radiating surface which may, without inconvenience, be commanded by it. The glass globe used by Dr. Stark, was one inch and a quarter in diameter; it was coated at different times with Prussian blue, red lead, and white lead, and in a room at 50° Fah., the fall, from 120° through 25 degrees, was in seventeen minutes, eighteen minutes and nineteen minutes.

I am constrained to differ from Professor Powell in his remarks upon the method just referred to, and, with great deference to so high authority would state why I consider them inconclusive. Professor Powell deems it necessary, or at least highly important to the determination of the question, that the radiating coatings of the globe should be equalized in respect to thickness, conducting power, density, &c., and refers to the experiments of Prof. Leslie, in which equal quantities of different radiating substances were dissolved and spread upon a surface, for comparison. That equal thicknesses of substances possessing different radiating powers should be compared together, seems to me to be disproved by the law established by Sir John Leslie's own experiments, namely, that radiation takes place not only from the surface, but in a thickness which is appreciable in good radiators, Thus when different coatings of jelly were applied, in succession, upon one of the sides of the cube in Prof. Leslie's experiments, the radiation increased with the thickness, up to a certain point. The effect of conducting power appears by this same experiment to be so small that an increase of the thickness in the bad conductor was actually more than compensated by the increased radiating power. The influence of density on conducting power is well known, but the effect of either as controlling the radiating power of a substance, or as modifying it, is, I apprehend, yet to be appreciated. If these views be correct, and they are, I believe, founded upon the authorities so ably illustrated by Professor Powell in his report on radiant heat, to the British Association, the radiating powers of substances would not be rightly compared by equalizing their thicknesses upon a given surface, nor by equalizing their weights; but by ascertaining, for each substance, that thickness beyond which radiation does not take place. This will be placed in a clearer point of view in the sequel.

I do no. nowever, consider the question at issue as the less difficult to determine, "no substance can be made to assume different colors without at the same time changing its internal structure,"* and I believe with Professor Powell that "a very extensive induction is perhaps the only means open to us of ascertaining this, (the circumstances and properties wherein the coatings differ) considering how totally ignorant we are of the peculiarities on which their color depends."

* Prof. Leslie's Essay on Heat.

This *very* extensive induction I do not pretend to have made, but I have multiplied our experiments so much beyond the number made by Dr. Stark, as to be able to show that the supposed influence of color on the absorption and radiation of heat remains yet to be demonstrated, and thus to prevent the admission as proved of what is more than doubtful.

The principal object was to select a considerable variety of pigments of the same color differing chemically, and of different colors chemically allied, and, as subsidiary, to ascertain the effect of changes of color produced by chemical means on different substances, and the effect of the material used to apply the pigment to the radiating body.

Several tin cylinders were procured, two inches high, and $1\frac{1}{2}$ in diameter, closed at the bottom, and having fitted to the top a slightly conical tube, to receive a perforated cork, through which to pass the stem of a thermometer. One of these vessels having been selected was coated in successive layers with a pigment. Water which was boiling in a porcelain capsule was then poured into the cylinder, which was suspended by means of two lateral hooks to cords attached to the canopy covering the lecture table. A thermometer introduced through a cork had its bulb nearly in the middle of the axis of the cylinder, and the thermometer by displacing part of the water proved that the quantity contained was the same in each case. A temperature was selected for beginning the experiments, sufficiently below that which the introduction of boiling water produced, to permit the rate of cooling to have become uniform, and one for ending which was high enough to prevent uncertainty from the slowness of the fall of temperature. The instant of the arrival of the mercurial column at any degree on the scale, and of its leaving the same, was noted, and a mean taken for the time of being at that temperature ; a precaution which though superfluous in such experiments as these, will, I am persuaded, be found of importance where minute accuracy is desired in investigating the motion of heat. One of us observed the thermometer, the other noted the time by a pocket chronometer.

The time of cooling of the cylinder coated with coloring matter having been ascertained, an additional layer of the same substance was put upon it and the cooling again observed. The time of cooling diminished, of course, until that thickness was attained beneath which no radiation takes place, the time then slowly increased with each additional coat, the conducting power entering as an appreciable element into the rate of cooling. To show the decided nature of the results, I subjoin an account of one series towards the beginning of our experiments, when a want of experience rendered us cautious in applying the successive coatings, lest we should pass the thickness of determinate radiation. The necessity for thus feeling our way, rendered the labor of the experiments very considerable.

Cylinder coated with Prussian blue:

Time of cooling from 180° to 140° Fah.

1. Thick coating,			1011½ seconds.
2.	ditto.	added,	965
3. Additional coat,		do.	910¾
4.	do.	do.	829½
5.	do.	do.	805
6.	do.	do.	842

Another series, in a further advanced stage of our experiments is subjoined:

Cylinder coated with Litmus blue.

Time of Cooling from 180° to 140° Fah.

1. First thick coating,			985 seconds.
2. Additional coat,			855
3.	do.	do.	827½
4.	do.	do.	834½

Besides the necessity of making several experiments to obtain a single result, it sometimes occured that particular results required to be repeated for verification, when apparent discrepancies occurred; this was done to ascertain if they were real or not.

As it was obvious that the experiments must necessarily extend through a considerable time, during which the circumstances attending the cooling of the cylinders could not be expected to remain uniform, a standard for comparison was provided, in a cylinder of which the coating was not changed, and which was observed in regular turn with the other cylinders. At first a vessel without coating was used for this purpose, but as it was found liable to tarnish, a cylinder was substituted having a coating of aurum musivum, which was one of the smoothest and most uniform of the colored substances used.—The numbers obtained on the different days from a mean of the trials made of the cooling of the standard cylinder, were applied to compare the the results of one day with those of another. This assumes that the times of cooling of the different vessels would be affected proportionately by a given change in the circumstances of the experiment. This inability to preserve the circumstances constant is the real objection to this method, and one which most affects the certainty of the results.*

The following example shows the application of this method. The observed times of cooling of the standard cylinder, from 180 to 140° in two experiments on the 31st of October, were 969½ and 968½ seconds, mean 969. Three experiments on the first of November, gave 898, 892, 893½ seconds, mean 894½.

Cylinder, number four, coated with cochineal (crimson) gave for the time of cooling from 180 to 140° on the 1st of November, 848½. To compare this with a result obtained with the same cylinder on the 31st of October we have 894½ : 969 : : 848½ : x, the equivalent number for October 31st, 916.3 seconds.

The results obtained with the same cylinder on different occasions of experiment, having been thus rendered comparable, the comparison of experiments with different cylinders, was effected by determining the time of cooling with the same coating upon different cylinders. Thus, numbers one and two having been coated with carbonate of lead, and their times of cooling through forty degrees having been ascertained, all the results with the various other coatings applied to these cylinders were comparable.

The numbers thus obtained will not be strictly proportional to the radiating power of the substance used, for the whole surface of the cylinders, including the ends, was not coated, and the contact of the air, and its consequent circulation exert a most important influence on the rate of cooling. This latter element has been shown by the experiments of Petit and Dulong, to be independent of the nature of the surface, and as the amount of uncoated surface remains constant, the greater effect of radiation will appear by the more rapid rate of cooling, and the less by the less rapid rate.

I proceed now to examine the degree of approximation which may be expected from the results of the experiments.

First.—A comparison of different observations on the same day under the same circumstances of the cylinders, and nearly or quite the same as to the temperature of the room, will show how far accuracy is possible under the most favorable suppositions. The following table presents the results of this kind obtained during the entire series of experiments, with the ratios of the times of cooling:

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* If the circumstances could be retained the same, three observations of the temperature at equal known intervals, would give a numerical expression for the radiating power of the coating.

Nature of Coating.	Time in seconds.	Ratio.	Nature of Coating.	Time in seconds.	Ratio.
Cylinder No. 3.			Cylinder No. 1.		
No Coating.	1281¾	1.000	Sulphuret of antimony.	849½	1.000
	1300	1.014		972¾	1.145
Chalk.	909¼	1.000	do. additional.	871½	1.000
	939¼	1.034	Coating on another occasion	878½	1.008
Prussian blue.	909½	1.000	Red lead.	886½	1.000
	932½	1.025		894½	1.009
Litmus blue.	920½	1.000	do. blackened	911½	1.000
	956	1.038	by Sulphuretted Hydrogen.	924½	1.014
Cylinder No. 5.			Cylinder No. 4.		
Aurum Musivum.	892	1.000	Gamboge.	932	1.000
	893½	1.001		942½	1.011
	898	1.007			
			Chromate of lead.	938½	1.000
do. on another occasion.	937½	1.000		954½	1.017
	959	1.023			
			Vermilion.	845	1.000
do.	943½	1.000		850	1.006
	957	1.014			
			Sulphate of Baryta.	740½	1.000
do.	818	1.000		778	1.051
	820½	1.003			
			Cylinder No. 1.		
do.	850	1.000			
	860	1.012	No coating.	1396½	1.000
	897	1.055		1425½	1.020
				1445½	1.035
do.	851	1.000			
	872½	1.025	do. another occasion.	1313½	1.000
				1315½	1.002
			do.	1303	1.000
				1320	1.013

In the foregoing table, ten of the ratios are about 1.01 to 1, six 1.02 to 1, three 1.03 to 1, one 1.04 to 1, and two 1.05 to 1: it is therefore fair to infer that the single ratio of 1.14 to 1 results from an error of record or observation, and the table fully shows, that *under the same circumstances the results could readily be reproduced within about two per cent.*

Date.	No. Cylinder.	Nature of Coating.	Reduced time of cooling.	Ratio.	No. Cylinder.	Nature of Coating.	Reduced time of cooling.	Ratio.	No. Cylinder.	Nature of Coating.	Reduced time of cooling.	Ratio.
Oct. 21	II.	Not coated.	1406	1.00	III.	Prussian blue.	914	1.00				
24			1422½	1.00			953¼	1.04				
24		Do.	1422½	1.06		do.	953¼	1.05				
25			1314½	1.00			910¾	1.00				
28	I.	Ammoniacal Sulphate of copper,	853½	1.01	V.	No coating.	1342	1.02	V.	Litmus blue.	855	1.00
29			849½	1.00			1311½	1.00			827	0.97
31			862	1.01			1359½	1.04				
31	II.	Ammoniacal Sulphate of copper,	930	1.13	IV.	Cochineal.	877½	1.03	V.	Aurum Musivum.	968½	1.08
Nov. 1		(not the same as above.)	826½	1.00			848½	1.00			894½	1.00
1	II.	Ammoniacal Sulphate of copper,	808½	1.00	IV.	Chromate of lead.	907	1.00	V.	Aurum Musivum.	894½	1.00
6		(not the same as above,)	831½	1.03			944½	1.04			948½	1.06
6	I.	Red lead.	890½	1.00	IV.	Alkanet.	980½	1.00	V.	Aurum Musivum.	948¼	1.00
11			912¼	1.02			926¾	0.95			950¼	1.00
15	V.	Aurum Musivum.	865½	1.06	VI.	Black lead.	870	1.09				
17			819¼	1.00			799	1.00				
17	III.	India Ink.	788½	1.00	V.	Aurum Musivum.	819½	1.00				
18			836½	1.06			869	1.06				
20		India Ink.	834	1.00	V.		816	1.00				
21			890	1.07			861¾	1.06				

Second.—The correction for the altered circumstances of temperature of the room, &c., may be tested by comparing the experiments made with different cylinders having the same coatings on different days. In the table on the preceding page, is given the various results of this kind furnished throughout the series of experiments. The date is given in the left hand column, and applies to all the results on the same horizontal line with it. A comparison of the numbers in the columns marked ratio, and on the same horizontal lines will show how far the same reduction to a standard would have been given by different cylinders: in other words, how far the influence of currents of air, local temperature, and radiation from or to adjacent bodies might have interfered with the particular resluts.

Of the ratios thus brought into comparison it will be found that in one case the results are identical, in four others that they differ one per cent., in two others two per cent., in four others three, in one four, in three five, in two seven, and in one ten per cent: omitting this latter the accordance is much less satisfactory than was shown by the former table, and the average amount of error is nearly four per cent.

Having now shown the probable limits of accuracy in the experiments, I proceed to compare together the reduced times of cooling of the same cylinders with different coatings. In the table will be given the observed time of cooling through forty degrees, and the time of cooling of the standard, from whence the reduced times are deduced. As the colors of the substances were not in all cases what would be expected, the color is designated in a separate column.

Cylinder No. 1, variously coated.

Nature of Coating.	Color.	Date.	Observed time of cooling.	Time of cooling of standard.	Reduced time of cooling.	Remarks.
			Secs.	Secs.	Secs.	
Carbonate of lead.	White.	Oct. 24	864	1014	864	Smooth.
Vermilion.	Red.	25	806	937	872	Smooth, with minute cracks.
Golden Sulphuret of Antimony.	Brown, nearly black.	31	868.5	969	909	Rough, peels easily.
Red oxide of lead.	Orange.	Nov. 6	890.5	948.2	952	Smooth.
do. additional coat.		11	932.7	950.2	995	For comparison with following.
Do. blackened by hydro sulphate of potassa.	Brown.		917.8	"	966	Red shows through.
Plumbago.	Black.	17	787	819.2	974	Uniform, but not glossy.
Gamboge.	Olive.	20	808.7	816.	1005	Smooth, but in streaks.

The radiating power being greater, as the time of cooling is less, we have the order of radiating power of the different colored substances, as follows: white, red, brown, orange, black, green. Omitting in this enumeration the blackened surface of the red oxide of lead, which had passed in thickness the maximum radiating thickness, and is only comparable with the result which precedes it. The change of color effected by changing the surface sulphuret of lead, (black or rather brown) increases the radiating power in the ratio of 1.03 to 1, which is within the average of error.

The following results given in order of time, and reduced by the standard, were obtained with cylinder No. 2.

Nature of coating.	Color.	Date.	Observed time of cooling.	Time of cooling of standard.	Reduced time of cooling.	Remarks.
			Sec.'s	Sec.'s	Sec's	
Ammoniacal sulphate of copper.	Bluish green.	Nov.6.	808.5	948.2	856	Streaked and peels off rough.
Indigo.	Blue.	11	928	950.2	990	Very smooth.
Carbonate of lead.	White.	14	883.2	956.	937	Smooth.
do. do.	-	15	910	856.5	982	For comparison with following.
do. blackened by hydro sulphate of potassa.	Black.	15	874	-	944	
Per oxide of manganese.	Dark br'wn	18	747	869	872	Uniform, but not smooth.

The variety of color is here small; the radiating powers rank, blueish green, dark brown, white, blue; omitting the second experiment with the carbonate of lead which is only comparable with the one in which the surface was blackened by hydro sulphate of potassa. Comparing these two results the change of surface appears to have increased the radiating power in the ratio of 1.04 to 1.

The coatings applied to cylinder No. 3, were more varied than those of either of the foregoing.

Cylinder No. 3.

Nature of coating.	Color.	Date.	Observed time of cooling.	Time of cooling of standard.	Reduced time of cooling.	Remarks.
			Sec.'s	Sec.'s	Sec's	
Carbonate of magnesia.	Yellowish white	Oct. 11	859.5	862	1011	Rough, in specks projecting.
Carb. of lime, (chalk)	White.		879		1034	do.
Carb. of lead.	White.		877		1032	Smooth and somewhat shining.
Prussian blue.	Blue.	25	805	937	871	Rough.
Litmus.	Blue.	31	831	969	870	Not uniform.
Bichromate potassa	Reddish brown.	Nov. 1	854	894.5	986	Streaked and not smooth.
Alkanet.	Crimson.	11	926.7	950	989	Uniform.
Do. rendered blue by potassa	Blue.		938.2		1001	
India ink.	Black.	17	776	819	959	Not smooth.
do.		18	836	869	976	More uniform, (mean 697)
Carb. of lead in oil of lavender.	White.	21	843.5	862	992	Uniform, but not glossy on surface.
Do. blackened by hydro sulphate of potassa.	Black.		850		1000	

The effect of changing the crimson of alkanet to a blue was apparently to decrease its radiating power about one per cent. or the change of color in reality did not alter the power. The carbonate of lead lost also slightly, or rather was not affected, by the change not only of its surface, but of a considerable part of its mass, for the oil of lavender having evaporated, the hydro sulphate of potassa penetrated the coating. The substance by means of which the coating was applied, seems not to have sensibly affected the radiating power; the carbonate of lead applied with gum differing in radiating power but four per cent. from that applied with oil of lavender.

The colors rank from the foregoing table, blue, two varieties; black, brown, crimson, white, black, blue, white, three varieties. There is no certainty that the litmus and alkanet, changed to blue by potassa, were originally the same in color. The surfaces were very different in regard to uniformity and smoothness; the alkanet was perfectly uniform, but not at all glistening; it may be described as of a uniform minute roughness. In this table, we have the greater number of whites at the bottom of the scale of radiation, and of blue and black at the top; but this is all that can be said, for a white, a black, a blue, are in close proximity near the middle of the scale.

The results, with cylinders Nos. 4 and 5, were few in number. They are subjoined.

Nature of Coating.	Color.	Date.	Observed time of cooling. Sec's.	Time of cooling of standards. Sec's.	Reduced time of cooling. Sec's.	Remarks.
Cylinder, No. 4.						
Cochineal,	Crimson,	Nov. 1	8485	894.5	962	Not uniform.
Chromate of lead,	Yellow.	6	931.7	948.5	996	Very smooth and uniform.
Bi-sulphuret of mercury, (vermilion)	Red.	11	843.7	950.2	888	Uniform and smooth.
Sulphate of baryta,	White.	15	759.2	865.2	889	Rough.
Ditto.	"	21	829	861.7	975	Smooth, freshly precipitated.
Cylinder No. 5.						
Gamboge,	Olive.	Oct. 29	845.5	934	917	Smooth.
Bi-sulphuret of tin, (aurum musivum.)	Yellow,	31	969	969	1014	Very even.

The order from cylinder No. 4, is red, white, crimson, white, yellow; the influence of the roughness of surface is here plainly shown, by which the place of the white material, sulphate of baryta, is entirely changed; this is a quality difficult to appreciate, and yet here we find it exceeding in influence any other property of the coating.

A review of these results will show that we have been able to establish, among the separate series, no order of color; we have the different orders as follows:

From No. 1.	No. 2.	No. 3.	No. 4.
White,	Green,	Blue,	Red,
Red,	Brown,	Black,	White,
Brown,	White,	Brown,	Crimson
Orange,	Blue,	Crimson,	White,
Black,	White to black, an increase of 4 per cent. in radiating power.	White,	Yellow,
Green,		Black,	No. 5.
White to black, an increase of 3 per cent. in radiating power.		White,	Green,
		No effect from changing white to black, or purple to blue.	Yellow.

A more satisfactory comparison, in respect to the number of substances employed, will be had by using the means, heretofore described, for comparing together the results obtained with different cylinders. For example, Nos. 1, 2, and 3, were each coated with carbonate of lead, and through the numbers given by these coatings, those found for the other coatings can be compared; Nos. 1 and 4 were coated with vermilion, and Nos. 1 and 5 with gamboge.

The following table presents the comparison, the substances being arranged in the order of their radiating powers.

Number.	Nature of Coating.	Color.	Number of Cylinders.	Date.	Time of cooling. sec's.	Remarks on surface.
1	Litmus blue,	Blue,	No. 3	Oct. 31	728	
2	Prussian blue.	Blue,	3	25	729	Rough.
3	Ammoniacal Sulphate of copper,	Greenish blue,	2	Nov. 6	789	Rough.
4	Per-oxide of manganese.	Brownish black,	2	18	804	Not shining, but uniform.
5	India ink,	Black,	3	17	804	Not smooth.
6	Bi-chromate of potassa.	Brown,	3	1	810	Streaked, streaks smooth.
7	India ink.	Black,	3	18	817	Smooth.
8	Alkanet,	Crimson,	3	11	828	Not shining, but uniform.
9	Carbonate of lead in oil of lavender,	White,	3	21	830	Smooth, not shining.
10	Sulphuret of lead,	Black,	3	21	837	
11	Alkanet blue,	Blue,	3	11	838	
12	Carbonate of magnesia,	White,	3	Oct. 13	846	Rough.
13	Carbonate of lead in gum,	White,	1	24	864	Smooth.
14	Carbonate of lime,	Dingy white,	3	11	865	Medium.
15	Vermilion,	Red,	1	25	872	Smooth.
16	Sulphate of baryta,	White,	4	Nov. 15	873	Rough, blueish white.
17	Golden sulphuret of antimony,	Brown,	1	Oct. 31	909	Smooth, in streaks.
18	Indigo,	Blue,	2	Nov. 11	912	Smooth.
19	Cochineal,	Crimson,	4	1	944	Smooth.
20	Red lead,	Orange,	1	6	952	Smooth.
21	Sulphate of baryta.	White.	4	21	957	Medium.
22	Plumbago,	Black,	1	17	974	Not shining, but uniform.
23	Chromate of lead.	Yellow,	4	6	977	Smooth.
24	Gamboge.	Olive green,	1	20	1005	Smooth, in streaks.
25	Bi-sulphuret of tin.	Yellow,	5	Oct. 31	1085	Smooth.

The results thus exhibited are decidedly unfavorable to the specific effect of color in determining the radiating powers of bodies. Blue is above black at the beginning of the table, and occurs again in the eighteenth place. Although the first seven numbers are blue or black, the ninth, tenth, eleventh, and twelfth, are white, black, blue, and white respectively. Red occupies the eighth and nineteenth places, and then an intermediate one, namely, the fifteenth. White is in the greater number of cases in the middle part of the table, ranging close to black.

The alleged advantages of dark clothing during cold weather, thus seem to have been too hastily inferred; and it appears that, provided the person is not exposed to the sun, the particular color of the clothing is not of real importance.

If color is not a determining quality, neither does roughness appear to be so, for though generally the smooth surfaces are lower on the list, this is not universal. The rough sulphate of baryta is lower on the list than the smooth carbonate of lead. Plumbago occupies a low place, and India ink a comparatively high one.

The best radiators do not appear to belong to any particular class of bodies; litmus blue and Prussian blue are side by side, while sulphuret of lead, and the bi-sulphuret of tin, are fifteen numbers apart.

If the results be admitted as decisive of the radiating powers of the bodies used, they show that each substance has a specific power not depending upon chemical composition, nor upon color. I do not claim to found such a conclusion upon the experiments; their object has been before stated, and if they prevent the introduction of an inference from an imperfect induction, as a law of science, the labor bestowed upon them will be amply recompensed.*

Art. IV.—*On Definitions;* by the Rev. D. Wilkie, of Quebec.

No. I.

As the adoption of correct logical definitions lies at the foundation of many of the sciences, and is of the greatest utility in all of them, and in every branch of knowledge, I hope no apology is necessary for laying before the readers of this Scientific Journal a few remarks, which observation has long suggested, or been suggesting to me. I need not appeal to the great Locke for confirming the importance of a subject, which, whether in the promotion, in the acquisition, or the communication of knowledge, is equally manifest. Without a constant reference to well defined terms, mistakes on the part of the learner are innumerable; errors in the teacher, or the writer, are inevitable.

In order to throw as much light as possible on this subject, let us first observe its necessity, or the necessity of some substitute for it,

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* The scientific reader need not be reminded that these remarks do not bear upon the radiation or absorption of heat accompanying light.

in such individuals of the human race as might meet together, unacquainted with each other's language, or unacquainted with the use of any language whatever.

If we imagine two human beings to meet together, after having grown to the age of manhood, without having ever had the opportunity of exercising the faculty of speech, having till then lived in absolute solitude: if we imagine them both, upon this rencounter, to be actuated by the impulse to make known their desires and feelings to each other: in such a case it is difficult, or perhaps impossible, for us to say which of the senses these two individuals would select as the instrument of their communications. We, from the fullest experience, know that the voice and the ear are by far the most efficacious; but what time might elapse before the two individuals in question would stumble upon this discovery, it is impossible for us to form the slightest conjecture. From the observations and practice of those who are engaged in the truly humane task of communicating to the dumb the blessings of social intercourse, it should seem that the looks, the hands and the eyes, would, for no inconsiderable period, continue to be the principal means for maintaining such interchange of thought, as in such a state could be practiced.

But, passing over this part of the subject as the region of mere conjecture, let us consider the case of two persons meeting, who were unacquainted with each other's language, but having each his own language, and consequently well aware that words, or speech of some kind or other, were by far the readiest means of maintaining social intercourse. In such a case, how would they proceed to the adoption of such terms as both might understand? No satisfactory conversation, it is manifest, could be carried on, till they came to be agreed as to the meaning and force of such a number of terms, as might be sufficient to express their more common thoughts and intentions.

It is easy to perceive that the first names which would be adopted by common agreement between the persons thus situated, would be the names of external objects, of such objects as could be seen or pointed at, or which could be felt, or smelt, or tasted. About the names of such objects there could be no difficulty in coming to an agreement. The presence of the object itself would easily remove all ambiguity, and might at any time be referred to, when any doubt might arise. In this way the names of trees, plants, animals of every description, rivers, fields, hills, of different kinds of food, and of all other common objects, would be settled by convention in such a manner, as to leave no room for doubt. There is every reason to believe also, that the names first employed, even for these objects, would be the names of individuals; they would be what we now call proper names, and they would only cease to be used as proper names when other objects of the same kind came to be observed, and received, as they would do, the same denominations.

The next class of names which would probably be introduced and agreed upon, would be those which we now call the names of qualities. A very short experience in attempts at conversation, would render it manifest that there must be words to denote the properties of objects; for example, to describe the colors of bodies, their size, their form, their position, and innumerable other qualities, of which we might find it desirable to speak to our friend. Hence are invented, or introduced in some way, such words as green, blue, sweet, sour, small, large, high, low, long, short, and innumerable others. These, at their introduction, are all adjectives. The further progress of speech forms substantives from all of them, and verbs from many. The agreement as to the meaning of these terms, between the two persons whom we have supposed, must be obtained by a comparison of two or more objects possessing the same qualities with others possessing different ones. When two persons thus situated, had a *green* and a *red* leaf before them, or both tasted of a *sweet* and a *sour* fruit, or both handled a *smooth* and a *jagged* stone, there could remain no doubt, as long as their memory was correct, of the application of these epithets. The definition of the terms was obtained by comparing, in actual perception or use of the senses, the qualities which they were intended to denote. We shall find, as we advance, that the highest degree of art and improvement does not furnish us with any better means of explaining these terms, or others of the same description. We can never distinguish white from yellow by any other means than actual inspection; nor salt taste from bitter, by any other means than making the experiment. But, in the progress of society there come into use other epithets of a more complicated signification, that may be explained by other phrases of a simpler nature.

The next class of words, that, in all probability, were found indispensable for the purpose of social intercourse, were those that denote actions and events. These are verbs, words that express change, that is, either some action or some event, or some change of situation. If all things remained constantly in the same situation, no verbs would be used. We require them to indicate that something has undergone a change. Thus the phrases, to fall, to rise, to strike, to build, to sail, to reward, to see, and all verbs whatever, imply some event or change of situation. Even the verb *to be*, the least active of all, it might seem, would never be used, if no change had taken place. We should never say, "it is cold," if it had not previously been warm. All the original verbs denoting natural events, occurring in a primitive situation, must have had their meaning fixed by convention between the parties, upon actual perception of the event. But, in the progress of society, a great number of verbs are introduced, which imply complex actions, and may be explained by a combination of other simpler words. Of this kind are such words as *to liberate, to cultivate, to obtain, to navigate, to pacify, to govern.* Such phrases as these may be, and commonly are, explained by the use of other and simpler words, which simpler words have originally had their meaning settled by convention, and upon the actual inspection of the event.

We come next to a class of objects, of which the names cannot have their meaning determined in same manner with those we have hitherto contemplated. Their meaning must indeed be determined by convention, but it cannot be brought about by the actual view or perception of the things themselves, for they are not objects of sense. It will easily be seen that I allude to the feelings of the mind. Our feelings of pleasure or pain, of grief or joy, of hope or fear; our thoughts, our recollections, our contemplations, our resolutions, are objects of sense to no living being. To ourselves they are the objects of consciousness, not of sense. They are not objects of perception.

How then are the two persons whom we supposed to meet without any previous knowledge of names, to agree in their names of these invisible objects? How are they to mention to each other their hopes, their desires, their preferences, their remembrances, since they have no names for such things, nor any means of fixing by agreement upon any names for objects which they cannot compare? They cannot place the object before their eyes, or any of their senses, and say, here is the object to which I give this or the other name.

And I may observe in passing, that here is an everlasting barrier to the knowledge of those who assert, as is now confidently done, that we can have no knowledge of things which are not objects of sense. If that doctrine be correct, or have any foundation at all, we must henceforth abandon all reasoning upon such things as gratitude, love, fear, happiness, loyalty, treason, falsehood. These are not objects of sense. They can never be submitted to the eye, the ear, or the touch.

To us who admit the existence of things which cannot be seen, it is competent to state, that there are two methods, by which, along with great care and attention, we may arrive at some knowledge of the feelings of others, and consequently agree upon some terms which may represent them in conversation. The first method is, to judge of the effect by the cause which produced it; and the second is, to judge of cause by the effect which we see produced; and comparing both with what we remember to have happened to ourselves. If one of the two persons above supposed, had chanced to have his hand severely bruised by a stone, we all know what the effect would be: he would suffer much pain. This would be the first effect. The second would be, to cry, to make wry faces, to jump, and to shed tears; for he could not yet have learned the propriety of suppressing these modes of giving vent to his emotions. If we now suppose, that in the course of time his wound is healed, and all his pains removed; and after sometime farther, his companion meets with a similar accident; it is now tolerably easy for the first sufferer, if his memory be at all correct, to understand from what has happened, what must be the painful situation of his friend: he judges of the effect by the cause which produced it. It is equally easy for him to derive the same conclusion from the groans and cries which he hears, from the contortions and tears which he sees, and all which he understands from his own experience: he judges of the cause (his friend's painful situation) from the effects which it produced. During the conversations to which these two painful incidents would give occasion, it is easy to see that suitable names and terms would come to be employed, which, on all future occasions, would serve to designate the mental situations that had been developed.

Such are the only two methods, as far as I can discover, by which the thoughts and feelings of one mind may be laid open to another. They are both indirect and imperfect; and nature affords no direct communication between minds. They both proceed upon the admission of several principles, which, in the operation, are taken for granted, and, in fact, can never be proved. First, they suppose, that nature is constant in her operations; namely, that the same causes always produce the same effects, and vice versa. Secondly, they suppose in us, that any other man's mind has the same susceptibility with our own. And, thirdly, they suppose we take for granted, that every event, or every change in mind or body, is preceded by some other event which we consider as its cause, and I may add, is succeeded by another still, which we consider as its effect. These are principles which all mankind admit, but which none can prove, which lie at the foundation of all philosophical enquiry, and without the admission of which, we cannot proceed one step in the unravelling of nature's operations.

The force of the terms employed to denote the more simple mental operations, being thus settled or agreed upon, in the manner that has been mentioned, which never could be done by verbal description; the names of the more complex feelings, may possibly have their meaning explained by words, the simple feelings of which they consist having been determined in the former method. Many of the terms introduced and employed in this manner, imply a combination of inward feelings with many external actions. Thus, the words, justice, government, liberty, aristocracy, proneness, baseness, murder, imply a complicated assemblage of mental feelings joined to many habitual acts. Verbal descriptions, and detailed explanations of the numerous ideas implied in each of these terms, may be of great use in leading us to form more correct conceptions of their meaning; but unless the simpler feeling originally implied in each of them be previously understood, no verbal elucidation whatever can ever lead us to the ideas intended.

Art. V.—*Fossil Fishes.*

This interesting class of fossils is now in a way to be developed far more thoroughly than before.

The labors of Prof. Agassiz, which we have had frequent occasion to mention, are presenting us with many new facts and new views of the greatest interest.

Prof. Jameson, of Edinburgh, in his Journal for October, has given an able analysis of the work of Prof. Agassiz as far as it is published, and we gladly avail ourselves of the opportunity to present it to our readers, especially as our own series of the livraisons of this work is interrupted.

If the general reader should be repelled by the new names which the author has found it necessary to introduce, he will find no difficulty in going along with the wonderful development of geological formations, in which the numerous races of fishes are found, and with the progressive alteration in their forms in the different epochs. Fishes begin very early even below the coal, soon after the slaty rocks of the primary family, as early as the grauwacke, if not before; and they continue (changing however their races,) as the creation advances, quite to our own times. All, that sober minded geologists believe of the epochs of deposition and formation, and of the extent of time, is fully established by the history of fossil fishes* alone.

Work of Agassiz on Fossil Fishes.†

Of the great work on "Fossil Fishes," by Professor Agassiz, four numbers have already appeared, eminently distinguished by the accuracy and elegance of the engravings, and the very interesting nature of the letterpress. The fifth number is finished, and will appear during the course of next month.

In the first number our author informs us, that by an attentive examination of the *scales*, fishes may be divided into orders more natural than those hitherto adopted by naturalists. In this manner he has established four orders, which bear some relation to the divisions of Artedi and Cuvier, but one of which, hitherto misunderstood, is almost exclusively composed of genera whose species are found only in the older formations of the crust of the earth. These four orders are the following:—

"Order I. Placoides.—The tribes of this order are so named on account of the irregularity of the solid parts of their integuments; these are, masses of enamel, often of considerable size, or sometimes reduced to small points, as in the shields of the ray and the different

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* We again call on the American public to patronize this great and difficult work, which ought to be in every public library, and in those of opulent individuals: the cost is about $100.

† Recherches sur les Poissons Fossiles, in quarto livraisons; the plates in folio. The booksellers in London who furnish the work, are Black & Armstrong, 2 Tavistock Street, Covent Garden, and J. B. Bailliere, 219 Regent Street, London.

kinds of shagreen of the shark. It comprehends the cartilaginous fish of Cuvier, with exception of the sturgeon tribe.

"Order II. Ganoides.—This order comprehends families apparently very different from each other, but which, notwithstanding, when minutely examined, have many points of agreement. The character common to them all is the angular form of their scales, which are composed of two substances, one of corneous or bony laminæ, superimposed on each other, and covered with a thick coat of enamel. These scales are constructed precisely like the teeth. In this order are arranged the *Lepidoides*, Agass., all of which are fossil; the *Sauroides*, Agass., fossil, with the exception of two genera, viz. the Lepidostees and the Bichir; the *Pycnodontes*, Agass., also fossil; the *Sclerodermes*, *Gymnodontes*, *Lophobranches*, *Goniodontes*, *Siluridæ*, and *Sturiones*.

"Order III. Ctenoides.—In this order the common character consists in their laminated scales being toothed at their posterior edges—those which are externally visible. The teeth of these numerous laminæ, which are so placed above each other that the lower always project over the upper, make the scales rough to the touch. This structure is particularly obvious in the *Chetodontes* and *Pleuronectes*. In this order are arranged the *Percoides*, *Polyacanthes*, *Scienoides*, *Sparoides*, *Scorpenoides*, and *Aulostomes*. There are the Anthopterigians of Cuvier and Artedi, with exception, however, of those having smooth scales, and with the addition of the Pleuronectes.

"Order IV. Cycloides.—The families belonging to this order are provided with scales formed of simple laminæ with smooth edges, a circumstance which does not prevent their external surface being frequently ornamented with different designs, which are imprinted on all the laminæ, where they are exposed to view and are not covered over. The scales of the lateral line are formed like all the others; but, in place of being mere laminated plates, these are funnels, placed the one within the other, and of which the narrow part, applied to the disc of the scale, forms a tube, from which the mucus which covers the fish is poured out. This tube is sometimes bifurcated, or even ramified. In this order are placed the *Labroides*, *Muges*, *Atherines*, *Scomberoides*, *Gadoides*, *Gobioides*, *Murenoides*, *Lucioides*, *Salmones*, *Clupes*, and *Cyprinidæ*."

If we estimate the number of species of fishes, now known to amount to about 8000, we may state that more than three-fourths of this number belong to two only of the above-mentioned orders, namely, *Cycloides* and *Ctenoides*, whose presence has not been discovered in the rock formations below chalk. The other fourth part of living species is referable to the orders *Placoides* and *Ganoides*, which are now far from numerous, but which existed during the whole period which elapsed since the earth began to be inhabited, to the time when the animals of the greensand lived. The remarkable conclusion to which M. Agassiz had come from the study of more than 600 fossil fishes on the Continent, has been corroborated by the inspection of more than 250 new species, found in the British collections.

In the first, second and third numbers, there are descriptions and admirable figures of genera and species of the different orders, both Continental and British. General discussions also occur; one in particular we recommend to the attention of our readers, viz. that on the colors and scales of fishes.

The fourth number contains a critical review of the numerous tribes of fossil fishes found in the famous Monte Bolca, and a tabular view of the fishes of the chalk formation. M. Agassiz, in the same number, informs us that he obtained vast additions to his former stores in the British collections, of which the following are noticed: viz. British Museum, Museum of the Geological Society of London, of the College of Surgeons, and of the United Service Club; the beautiful collection of Mrs. Murcheson; the cabinets of Messrs. Lyell, Stokes, Fitton, Sharpe, Yarrell, and Richardson; all in the vicinity of London. Great additions were obtained from the collection of Dr. Buckland, and the splendid cabinets of Sir Philip Egerton and Lord Cole. The fine collection of Mr. Witham, and the Museums of Whitby, Scarborough, York, Leeds, Birmingham, Liverpool, Bristol, also proved productive sources of new and interesting species. The private collections of Miss Philpot and Mr. Cumberland, and the well known museum of Dr. Mantell, contributed an ample supply of species entirely new to M. Agassiz. At Edinburgh the collections of the Royal Society and the College Museum; the cabinets of Professor Jameson, Lord Greenock, Dr. Hibbert, Dr. Traill, Mr. Copland, and Mr. James Torrie, proved not less interesting than those visited by our author in England and Ireland. After enumerating the above and other collections in very courteous terms, he adds the following observations.

"These notices concerning the rich and splendid materials with which, from so many quarters, I have been favored during the past year, naturally suggest some additional remarks concerning that portion of our work which has been already executed, and also regarding that which still remains for the furtherance of the science of fossil fishes.

"The study of Ichthyology has, in all past ages, been much neglected, in comparison with that of the other branches of natural history. The extreme difficulty which exists in observing fishes in their watery haunts, and in collecting authentic facts regarding their habits, and the whole of their animal economy, has rendered this science much less attractive than the history of the great mammifera, and of the feathered tribes. Even reptiles, hideous and oft times dangerous as they are, have found more admirers than fishes; and concerning the attractions of entomology and conchology, we need say nothing. In the midst of so many favorite fields of research, fishes have remained hid from us in the vast oceans which they inhabit, for the number of those already described is comparatively small; and if the great work on Ichthyology of Cuvier and Valenciennes promised us the description of from six to eight thousand species, the greater is our regret that the volumes which have hitherto appeared contain no more than a fifth part of the number. And now, notwithstanding all these attending difficulties, the first steps being taken, and an entrance effected into these new regions, what a world of wonders presents itself in the depths of the ocean, and in the inaccessible haunts of the creatures which inhabit it? In approximating to these results, we unfortunately cannot repose confidence on any guides whom we now possess, since the older amongst them reveal but a few species, and the best of the more modern, leave us in the midst of the investigation. Accordingly, I have had to pursue my researches, in a great degree, independently of every thing which was previously accomplished, in establishing an equilibrium in the various branches of Ichthyology, and in making the whole of this labor nothing more than a simple introduction to the examination of those fossil species I sought to determine; for it will now be readily conceived, that those memoirs concerning Ichthyolites which were published some twenty years ago, do not now exhibit results in keeping with the knowledge which may be easily acquired regarding the existing species, in the many great collections throughout Europe.

"From this state of things, and from the manner in which I have been obliged to study living fishes, that I might compare them with fossil ones, a great advantage has resulted in the complete independence I was required to maintain concerning all the former reputed alliances of different fishes; because the great number of new species which have been discovered since the commencement of the present century, for the most part represented in the *Regne Animal* of Cuvier, and which it was necessary to insert in the groups of the natural families of this class, has caused all the alliances proposed by the older Ichthyologists entirely to disappear. In afresh reviewing their characters, I have been led to adopt a classification which differs considerably from any arrangement which has hitherto been proposed, and which is founded upon important considerations which have hitherto been neglected.

"It admits of no doubt, that one of the distinctive characters of the class of fishes, consists in the skin being possessed of scales of a peculiar form and structure. This covering, which protects the animal externally, has, according to all the observations I have made up to the present moment, the most direct relation to its interior organization, and to the external circumstances in which the animal is placed. So that, in this point of view, the scales acquire a primary importance, and may be regarded as a superficial reflection of all that passes within and around the fish. Accordingly, upon attentively examining them, I have found that fishes may be arranged in an order much more natural than any hitherto proposed, by allowing ourselves to be regulated by the structure of the scales. Acting on this principle, I have established four orders, which present some resemblance to the great divisions of Artédi and Cuvier, but one of which, hitherto almost wholly unknown, is nearly exclusively formed of genera, the species of which are found solely in the older strata of the crust of the globe. These four divisions are—the *Placoïdes*, which includes all the cartilaginous fishes of Cuvier, with the exception of the Sturgeons; the *Ganoïdes*, which comprehends more than fifty extinct genera, and with which it is necessary to ally the Plectognathes, the Syngnathes, and the Acipenser; the *Ctenoïdes*, which are the Acanthopterygiens of Cuvier and Artédi, to the exclusion, however, of all those which have smooth scales, and including with them the Pleuronectes; and, lastly, the *Cycloïdes*, which are principally the Malacoptérygiens, but which likewise comprehend all the families which are excluded from the Acanthoptérygiens of Cuvier, and from which it is necessary to separate his Pleuronectes, that they may be carried back to the preceding order.

"The better to comprehend the general results which I propose to present, it will be necessary to say a few words on the existing species.

"We are now acquainted with about 8000 species of fishes. Of this number more than three fourths belong to two orders of that class, the presence of which has not yet been discovered in formations older than the cretaceous one, viz. to the Cycloïdes and to the Ctenoïdes; so that, truly, there is nothing analogous to them in the whole series of the secondary rocks, even to greensand; whilst the remaining fourth belongs to the orders Placoïdes and Ganoïdes, not at all numerous now, but which existed *alone*, during the whole period which elapsed from the time the earth began to be inhabited, till the moment in which the animals found in the greensand appeared. The same precise proportion existing in the orders of the class to which attention is more especially solicited in this work, is a fact which is truly remarkable; it is almost inconceivable, but still undoubted, since it is a mere matter of calculation; and moreover, we may remark, it is not only in general that this regular arrangement of the groups may be noticed; but in each order, and in each family even, the genera produce in their affinities analogous series, so that the differences of organization become the distinctive characters for the geological epochs, even in those species which are seen for the first time. I can now state this result with confidence, after having reviewed the general conclusions to which I have arrived in the study of fossils, and supported by the examination of 250 new species discovered in British Collections, without having met with a single exception in the 800 species with which I am now acquainted. These essential organic differences have an especial reference to the nature of the integuments, and to the mode in which the vertebral column terminates in the caudal fin, in other words, to the relations which subsist in the animal to the material world which surrounds him, and the structure of that organ which is essential to his locomotion. I shall now very briefly point out those distinctions, and, at a future time, will fully enumerate all the fishes belonging to each great formation; for in thus presenting a general description, it will easily be understood that I cannot enter largely into detail.

"That we may appreciate at its just value the study of fishes in general, and of fossil fishes in particular, we ought never to lose sight of the true position of this class in the scale of living beings. Placed higher than the radiata and the mollusca, they present peculiarities of organization more numerous, and also subject to greater differences; and in them also we remark, within narrower geological limits, more marked differences than among animals lower in the scale. In the class of fishes, we do not see genera, nor even families, run throughout a whole series of formations with species which often differ but very little in appearance, as happens in the Zoophytes; on the contrary, this class, from one formation to another, is successively represented by very distinct genera, referable to families which themselves soon vanish, as if the complicated apparatus of a superior organization could not be long perpetuated without intimate modifications; or rather, as if animal life had a more rapid tendency to change in the higher orders of the animal kingdom, than in those lower in the scale. In this respect, it is with fish nearly as it is with the mammifera and reptiles, whose species, in general very limited, belong in the series of formations with little verticla distance, to genera which are different, without passing insensibly from one formation to another, as is often witnessed in certain shells. This is one of the most interesting facts which I have observed, namely, that I do not know a single species of fossil fish which is successively found in two formations, whilst I know a great number which have a considerable horizontal distribution. Besides, the class of fishes moreover presents to zoological geology the immense advantage of extending across all the formations, and of presenting in a class of vertebral animals a point of comparison regarding the differences which exhibit themselves in the longest lapse of known time, of animals constructed generally on the same plan—of animals of a class which already counts a very great number of fossil species, for the most part referable to types which exist no longer; and whose affinities with the living species are as distinctly marked as those which ally the Crinoidea to the ordinary Echinodermata, the Nautili, and the Sepia to the Belemnites, and the Ammonites, the Pterodactyli, the Ichthyosauri, and the Plesiosauri to our Saurians; and the living Pachydermata to those which of old, inhabited the borders of the lakes around Paris, or the planes of Siberia.

"The fishes of the tertiary rocks, are those on which I the least dilate, because they approach nearest to the living species, and because their study may be undertaken by means of works which are already published upon Ichthyology. At the same time, considering the prodigious number of living species to which they approximate, it is often very difficult, in the condition in which they are discovered, to identify them, or rather exactly to appreciate their distinctive characters. I will only remark, in general, that up to the present moment, I have not found a single species which is perfectly identical with those of our seas, except that little fish which is found in Greenland, in the géodian clay, and the geological age of which is unknown to me.

"The species of the Crag of Norfolk, of the superior subapennine formation, and of the molasse formation, for the most part approach to the common genera of tropical seas; such as the Platax, the Great Carcharias, the Great Myliobates, &c.

"In the inferior tertiary formations,—in the London clay, the coarse limestone of Paris, and of Monte-Bolca, we find that at least a third of the species already belong to *genera* which no longer exist. In the comparative tables of all known fishes which I propose soon to publish, I shall give the names of all the fossil genera and species of all geological epochs, and at the same time shall point out all the localities in which they are found, and the corresponding genera of the actual existing creation, in a separate column.

"The Chalk group includes more than two-thirds of those species which are referable to *genera* which have entirely disappeared; and here we already see some of those singular forms which prevail in the oolite series. At the same time, as a whole, the fishes of the chalk formation much more resemble the general character of the fishes of the tertiary series than that of those belonging to the oolite group; and this to such an extent, that did I regard only the fishes, in a general classification of geological formations, it would appear to me more natural to associate the chalk and greensand series with the tertiary formations, than to rank them with the secondary rocks. Underneath the chalk there is no longer a single genus which possesses existing species; and even those of the chalk which have them, possess a much greater number of fossil ones.

"The oolite group to the lias, also included, forms a very natural and very well defined series, which must also comprehend the Wealden rocks, in which I have not found a single species referable even to the chalk genera. From that epoch, always descending, the two orders which prevail in the present creation are no longer found, whilst those which are in small numbers in our days suddenly present themselves in great abundance. As to the Ganoïdes, the genera with the symmetrical caudal fin are those which are here found; and among the Placoïdes, it is especially those with teeth furrowed on their two faces, and with large spinous rays, which predominate. For it is now certain that those great rays which Dr. Buckland and M. de la Beche have denominated Ichthyodorulithes belong neither to the Silures nor to the Balistes, but are the rays of the dorsal fin of the great Squali, the teeth of which are found in the same beds.

"In quitting the Lias, and proceeding to the lower formations, a great difference is observed in the form of the posterior extremity of the body of the Ganoïdes. All of them have the vertebral column at its extremity prolonged into an imperfect lobe, which reaches to the extremity of the caudal fin, and this peculiarity extends to the most ancient fishes. Another observation, which is worthy of remark, is, that down to the coal formation, no fishes that are evidently carnivorous are found; that is to say, with large conical and sharp teeth. The others appear to have been omnivorous, their teeth being rounded, or obtusely conical, or pencil-shaped.

"The day is assuredly not far distant when we shall be able to collect a great number of facts respecting the habits of these animals, and their interior organization. The discovery of coprolites already enables us to discover the organized beings which preyed upon the pirates of the ocean; for in these coprolites, which are sufficiently numerous in those deposits which contain Sauroid fishes, we easily discover the scales of the fishes which they ate, and sometimes we can determine the scales. Their intestines even are preserved in some cases, as for an example in a specimen of Megalichthys, where a portion of intestine is visible; and bundles of pyloric appendices, and the *cul-de-sacs* of intestines of the species Leptolepis and Thrissops of Solenhofen, known by the name of *Lumbricaria*, are not rare in the schists of this interesting locality. In the fishes of the chalk group, there may be seen, in Dr. Mantell's collection, specimens of Macropoma, in which the whole stomach is preserved with its different membranes, which are separated, as it were, into as many leaves. In a great number of the fishes of Sheppey, of the chalk and oolite series, the capsule of the eye-ball is still preserved; and in many species of Monte-Bolca, of Solenhofen, and of Lias, there are very distinctly to be seen all the little laminæ which form the gills. It also appears evident that the constitution of some rocks is much more favorable for the preservation of animal remains than that of others.

"It is in the series of deposits inferior to the Lias that we begin to discover the largest of those monstrous Sauroid fishes, whose osteology approximates them so much to the skeletons of the Saurians, by the stronger sutures of the cranial bones, by their great conical teeth with longitudinal striæ, and also by the mode in which the spinous apophyses are articulated with the bodies of the vertebræ, and the ribs also at the extremity of the transverse apophyses. The analogy between these fish and the Saurians goes further than the skeleton: in one of the two species which is now alive, I have found an interior organization of soft parts, which is very peculiar, and which approximates these groups of animals more than at first was anticipated. In the *Lepidosteus osseus* there is a real glottis, like that of the sirens and the salamander reptiles, also a cellular swimming bladder, with a trachea, as in the lungs of an ophidian. Finally, these integuments have often an appearance so like to those of the crocodile, that it is not always easy to distinguish them.

"The small number of fishes found in the transition rocks appear not as yet to enable us to assign to them any peculiar character. At the same time, the species in the collection of Mr. Murchison exhibit types which are not found even in the coal measures.

"What is very remarkable in all the fishes below the oolite species, besides their resemblance to reptiles, is, on the one hand, the greatest uniformity of their types; and on the other, the very great uniformity of the parts of the same animal among themselves; so much so, that the scales, the bones, and the teeth of one are with difficulty distinguished from those of the other. And if we might here hazard a conjecture on this state of matters, we should be naturally led to fancy that the principle of animal life, which developes itself at a later period under the form of our common fishes, of reptiles, birds, and beasts, is at first, entirely confined to these singular sauroid fishes, at the same moment participating of the nature of fishes and of reptiles; and that this mixed character is only lost in this class upon the appearance of a greater number of reptiles, as we see the Ichthyosaurus and Plesiosaurus participate, as to their bony structure, in the characters of the cetaceæ, of the class of mammifera; and the great terrestrial saurians to those of the Pachydermæ, which would appear to have been called into existence at a much later period.

"We are thus by observation conducted to those ideas of the philosophy of nature which are presented to us by an organic and regular development of all created beings, in fixed relation to all the different conditions of existence which are realized on the surface of the globe, at the conclusion of those changes to which it has itself been subjected.

"According to all these facts, I perceive in these series of all the geological formations two grand divisions, which have their limits at the greensand. The former, and more ancient, includes only the Ganoides and Placoides; the latter, more closely allied with the existing species, comprehends forms and organizations much more diversified; and these are principally the Ctenoides and the Cycloides, and a very small number of the species of the two preceding orders, which insensibly disappear, and the living resemblances of which are very considerably modified. As I do not find in the fishes of the first grand period the differences which correspond to those which we now observe between fresh-water and marine fishes, it appears we perhaps go farther than facts warrant when we admit in the oolite series, and in those which are lower than it, distinct formations of fresh and salt water. I rather think that the waters of these remote periods, confined in less secure basins, did not then present the same striking differences which we observe in our own days.

"Such is a very meagre sketch of a history of the deepest interest, and full of curious episodes, which are, however, somewhat difficult to relate. The exposition of the details which it includes will be the task of my life."

The fifth number contains chiefly histories and descriptions of genera and species met with in the formations of England and Scotland.* The plates in this number are even more admirably executed than those in the fourth livraison. The following enumeration of the new British species may prove acceptable to our geological readers:—1. *Palæoniscus*. Of this genus the following species are described: 1. *P. Robisoni*, found in the limestone of Burdiehouse, near Edinburgh, and by Professor Jameson in the Burntisland district. 2. *P. striolatus*, found at Burdiehouse and in Fifeshire. 3. *P. ornatissimus*, in the limestone of the Burntisland district. 4. *P. elegans*, in the magnesian limestone of Durham. 5. *P. comatus*, Durham. 6. *P. glaphyrus*, magnesian limestone. 7. *P. macroph-*

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* Although the fifth livraison is not yet published, we have, through the kindness of the author, obtained a sight of it.

thalmus, magnesian limestone. 8. *P. longissimus*, magnesian limestone. 9. *P. carinatus*, in the clay-ironstone of Wardie.

Lord Greenock found that many of the balls of clay-ironstone at Wardie, to the westward of Newhaven, contained, as a nucleus coprolite, or portions of fish. He also found the following species of the genus Amblypterus at Wardie, viz. *A. ambnemopterus*, *A. punctatas*, *A. striatus*. Of the genus Osteolepis, the following species are mentioned as British: *O. macrolepidotus*, in the slate of Caithness and Pomona; 2. *O. Microlepidotus*, in Caithness and Pomona; 3. *O. arenatus*, at Gamrie, in Banffshire. Of the genus Acanthodes, the *A. sulcatus*, is mentioned as having been found by Lord Greenock at Wardie. Of the genus Cheiracanthus, the following species are enumerated as British: 1. *Ch. Murchessonii*, found at Gamrie by T. Jameson Torrie, Esq. and others; 2. *Ch. minor*, by Dr. Traill in Pomona. Of the genus Cheirlepis, the following British species are described: 1. *Ch. Traillii*, found by Dr. Traill in the slates of Pomona; 2. *Ch. Uragus*, found at Gamrie, where there is an interesting deposit of fossil fishes in a red sandstone, which, in the regular succession, is below the coal formation. Of the remarkable genus Cephalaspis, the following British species are mentioned. 1. *C. Lyellii*, of which fine specimens were found in the old red sandstone of Forfarshire many years ago by Professor Jameson, and since by Mr. Lyell and others; 2. *C. Lewisii*, found at Whitebach; *C. Lloydii* common, in the old red sandstone in Wales. The consideration of this genus leads our author to the following observations *on the strata in which the mostancient fish are found.*

" The truly astonishing character of the genus (Cephalaspis) afford me a renewed opportunity of remarking how much the several portions of the frame of the animals of the most ancient epochs, exhibit a uniformity of structure, and at the same time, types of the animal kingdom which are but little distinguishable among themselves. Here, for example, the bones of the head are all as one, the scales, are united in very elevated bands, and the rays of the fins remain covered by the membrane which elsewhere surrounds them; whilst the whole animal, to a most extraordinary extent, resembles the Trilobites, which have somewhat preceded the *Cephalaspis*, in the series of their creation. This example alone would suffice to manifest the constant laws which regulate the succession of living beings, and their progressive development, if the whole class of fishes were not itself a continual demonstration of it.

"All the species of the genus *Cephalaspis* have been found in the old red sandstone of England and Scotland. It is not therefore, as has been asserted, in the mountain-limestone, and consequently still less in the Zechstein or Magnesian limestone, that the vestiges of the most ancient fishes have been found. Their presence mounts up to an epoch yet more distant; for it is now certain that they are found in very considerable number in the old red-sandstone. And moreover, even this formation is not the oldest in which fossil fishes have been discovered. But since the *Cephalaspis* itself belongs to an epoch so very remote, and since it is of the highest importance for the science of Paleontology to determine exactly the formation in which the first traces of fish are found, I deem it proper now to enter into some details concerning all the species of fossil fishes, the debris of which have been discovered in the most ancient beds of the crust of the globe, even although their organization will compel me, in part at least, to postpone their description to the 3d volume of this work.

"In order that I may be able the better to fix and point out my indications concerning the strata in which the most ancient fish occur, and also that no uncertainty may remain concerning the geological age of their beds, it will be useful here to transcribe in a summary way the results of Mr. Murchison's researches into the fossiliferous stratified rocks below the coal formation—and these indications will be the more exact, inasmuch as it is to Mr. Murchison that I am indebted for the most of the specimens of fossil fish which I have examined, belonging to formations below the coal measures, and it is also his valuable communications which, in a great degree, have enabled me to digest this notice....In the synoptical table of these stratified deposits which Mr. Murchison has published, he commences with the carboniferous limestone, and descends successively to the schistose system of the southern parts of Wales. The English old red-sandstone is the most recent formation whose beds are examined in detail in this table. The upper division of this formation is wholly destitute of organic remains.* The whole of these beds, formed of a red conglomerate, and of different sandstones, has a thickness of many thousand feet, as may be ascertained by visiting the escarpment—the slopes—of the counties of Brecknock and Caermarthen;

* Mr. Murchison has lately discovered scales of fishes in the upper part of the old red sandstone.

they support the coal formation in the southern parts of Wales. The middle division of the old red-sandstone consists of red and green marls, with numerous beds of concretionary limestones called *cornstone*, and some beds of very hard sandstone; it is this part of the formation which contains the debris of the *Cephalaspis*. There has not been the slightest trace of any other kind of organic body discovered in this division of the formation, these fragments of fishes excepted, which, at the same time, characterise it in a very peculiar manner. They have been found by Mr. Murchison himself in the different parts of this division, in the counties of Salop, Hereford, Worcester, Monmouth, and Brecknock, over an extent of nearly 3000 square miles, and always occupying the same geological horizon. This group of cornstones has likewise a very considerable thickness, equal probably to that of the division immediately above it. At the same time, Mr. Murchison thinks that the remains of the fishes are especially abundant at the lower portion of this middle division. In the lowest division of the old red-sandstone, below the horizon of the *Cephalaspis*, Mr. Murchison found, at Downton-Hall, near Ludlow, not more than the fragment of a head, with a portion of the scaly cuirass, evidently belonging to the *Dipterus Macrolepidotus*; and at Tinmill, near Downton-Castle, some small Ichthyodorulites, accompained by a new species of *Pileopsis*, and a new species of *Avicula*. In this last locality, the transition from the old red-sandstone to the Ludlow-rock sandstone which it covers, was very distinctly perceived. The situations in which the fossils of this formation occur most frequently, are Whitbach near Ludlow, the Whyle, the Bromyard Road, Sutton-Hill, Downton-Hall, Menaibridge, and Abergavenny. Mr. Murchison imagines that the concretionary nature of the limestones of the old red-sandstone formation, and their being broken into small pieces so as somewhat to resemble a conglomerate rock, has prevented the discovery hitherto of native fishes, but he does not despair of discovering them in the more compact sandstones. Neither has he found any in the vast masses of concretions which have somewhat of a subcrystalline structure, and an occasional thickness of about twenty feet. In Scotland, however, and, amongst other places, at Glammis in Forfarshire, some specimens of *Cephalaspis*, in fine preservation, and almost entire, have been discovered, and have been transmitted to me by Professor Jameson and Mr. Lyell. Lastly, the scales of the old red-sandstone of Fifeshire, which have been described by Dr. Fleming, belong to a gigantic species of *Gyrolepis*. Thus the old red-sandstone contains debris of many species of *Cephalaspis*, of a species of *Dipterus*, and of a species of *Gyrolepis*, (consequently of those genera which belong to the order Ganoides), and many species of *Ichthyodorulites*, which are the bony rays of different fishes of the order Placoides. These bony rays present such remarkable differences, that it is impossible to suppose that they have belonged to the same genus, any more than to any genus, the rays of which are found in the superior geological formations. I shall describe some under the denomination of *Ctenacanthus ornatus*, and others under that of *Onchus Murchisoni* and *Onchus erectus*.

"In descending further into the greywacke group, the debris, of fossil fishes are still to be found at different depths; and as the researches of Mr. Murchison have led him anew to subdivide this series of deposits into many formations, I deem it expedient yet further to point out in this place the limits, of his subdivision, in order that we may accurately define the layer of the crust of the globe which incloses the first traces of the presence of fishes.

"Immediately under the old red-sandstone we find the upper portion of the superior greywacke series, which Mr. Murchison calls the *Ludlow rock*, and regards as the first formation of his series. This system is characterized in its upper subdivision, which Mr. Murchison designates the Upper Ludlow rocks, by a new species of *Avicula*, and by the *Avicula retroflexa* of Hisinger, by the species of *Alrypa*, a new species of *Cypricardia*, the *Homonolotus Knightii* (a new genus of Konig), the *Leptana lata* of Buch, by many new species of Orthis, two new species of *Orbicula*, different new species of *Orthocera* two of *Pleurotomaria*, a new species of *Turbo* and *Gigantic serpuleæ*. These fossils, which will be described and figured in the work now preparing by Mr. Murchison, are contained in a slightly micaceous, grey-colored, thin-bedded sandstone. The environs of Ludlow Castle, in Shropshire, and those of Croft Castle in Herefordshire, the western flanks of the Malvern and Abberly Hills in Worcestershire, the western slopes of May-Hill, Pain Castle, in Radnorshire, and the Tewerne Hills, belong to this subdivision. In its central subdivision, the Ludlow rock formation comprehends the Aymestry and Sedgley limestone, subcrystalline, grey and blue argillaceous limestone, characterized by the *Pentamerus Knightii* Sow., the *Pileopsis vetusta*, Sow., a new species of *Bellophon*, a species of *Lingula* one of *Atrypa*, the *Terebratula Wilsoni* Sow., the *Cala-*

mopora fibrosa, Goldf., and some other corals. This subdivision is remarkably well developed near Aymestry in Herefordshire, in some parts of Shropshire, and at Sedgley in Staffordshire. The third and last subdivision is that of the lower Ludlow rocks; these are concretionary and sandy limestones, and sandy shales of a dark color; and are especially developed in the escarpments of Monktree and Bridgwood in the valley of Woolhope in Herefordshire, in the escarpments, in Montgomery and the Radnor Forest and are characterized by three species of *Phragmoceras* (a new genus of Mr. Broderip), by the *Asaphus caudatus*, Brong.; two species of *Cardiola* (new genus of Bron.), a new species of *Nautilus*, two of *Spiralites*, a *Pentamerus*, the *Atrapa galeata*, Dalm., a new species of this same genus, one of *Pleurotomaria*, the *Orthocera pyriformis*, and many other fossils. The teeth of fishes have also been found in this formation, though not in great numbers, and lying most commonly in the upper Ludlow rocks. No part of the body of a fish was ever discovered in this formation till this present year, when the upper beds being removed at Ludford, in digging the foundation of some houses, a heap of scales were discovered, as also the rays of fins, and teeth completely broken lying huddled together, and forming a bed between the strata of sandstone which contain an immense number of Serpulæ also, *Leptæna lata*, and of other fossils which characterize this formation.

"These fragments are too incomplete, to enable us at the present moment to arrange them in a systematic table of classification. It is, however certain, that they do not present any specific analogy to the fishes of the old red sandstone formation. The remains of the fins belong to a different species of Ichthyodorulites, and the scales appear to belong to different fishes of the family Lepidoidians, for their appearance is very varied; the teeth, moreover, are less numerous, and none have been found entire. The nature of these strata, their disintegrated condition, and the fossils which they contain, have, altogether, led Mr. Murchison to suppose that they have been deposited in very shallow waters. Only a very few Ichthyodorulites have been found in the lower Ludlow rocks; and since Mr. Murchison has not collected them himself, he thinks that their presence in this subdivision should be admitted with some degree of hesitation; and the more so, as they have not been found in the Aymestry limestone, which contains such an immense quantity of organic debris, and amongst others of the *Pentamerus*.

"The second formation, arising from the division of the greywacke group, is composed of the *Wenlock* and the *Dudley rock*, which swarm, so to speak, with corals, shells, and Trilobites.* Mr. Murchison, divides it into two subdivisions: the upper comprehends the Wenlock and Dudley limestone, which is a subcrystalline limestone, highly concretionary, of a grey or blue color. It contains an immense quantity of corals and crinoidea,—the *Bellerophon tenuifascia*, Sow., the *Euomphalus rugosus* and *discors*, Sow., the *Conularia quadrisulcata*, Sow., a new species of *Pentamerus*, also of the *Natica*, the *Natica spirata*, Sow., the *Leptæna euglypha*, Dalm., the *Spirifer lineatus*, Sow., and a new species of this last genus, the *Terebratula cuneata*, Dalm., the *Producta depressa*, Sow., many species of Orthocera, the *Asaphus caudatus*, Brong., the *Calymene Blumenbachii*, Brong., and other trilobites. This subdivision prevails remarkably in the neighborhood of Wenlock, in Shropshire, in Caermarthenshire, at Dudley, and in Gloucestershire. The second subdivision comprehends the Wenlock and Dudley argillaceous shales, which are of a dark grey color, rarely micaceous, and contain nodules of earthy limestone. In these strata are especially found a variety of the *Asaphus caudatus*, the *Calymene Blumenbachii*, a new species of *Lingula*, and a new species of *Orthis*; also the *Cyrtia trapezoidalis*, Dalm., a new species of *Delthyris*, an *Orthocera*, the *Orthocera annulata*, Sow., besides *Crinoidea*, &c. Mr. Murchison has never found the least trace of fishes in this formation.

"Mr. Murchison's third formation includes the *Horderley* and *May-hill rocks*, which descend as far as the Black Rocks of Llandeilo and Builth. It is entirely devoid of the debris of fishes, and is especially characterized by the *Pentamerus lævis*, Sow., the *P. oblongus*, Murch., a new species of *Leptæna* one of *Pileopsis*, a new terebratula, many crinoidea, some corals, undescribed species of trilobites including the genus *Cryptolithus*, which have been discovered in North America, and fourteen species of the genus *Orthis* different from those which have been found in the overlying formations, including the *Orthis Callacis* of Dalm., and his *O. Aperturatus*. The *Asaphus Buchii*, Brong., his genus *Agnostus*, and other undescribed species, characterize the Llandeilo and Builth rocks, which form the fourth formation of the series.†

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* Mr. Murchison now names this second formation the *Wenlock Limestone.*

† Mr. Murchison now names the third formation simply *Cardoc limestone* from Caer Cardoc, where it is well seen, and the fourth simply *Llandeilo formation.*

"By descending still lower, we arrive at the system of slaty rocks of the southern part of Wales, which Professor Sedgwick has examined with such minute care, and in which he has never discovered the least trace of fishes. Thus we have under the old red sandstone a *geological scale* of many thousand feet extent, the several degrees of which have been most assiduously examined by geologists of the first-rate eminence, and upon which the commencement of the history of fishes may be with certainty inscribed at the height of the Ludlow rocks' formation, and possibly even at that of the Dudley rock formation. But, however this may be, we may safely state that it is in the Greywacke group that fishes begin to appear.

"These facts are not of a kind in any degree to countenance the ideas which are at present most generally received concerning the succession of organized beings, and the consecutive appearance of animals of the radiata, then the mollusca, the articulata, and the vertebrata, since we find them here commingled together. Their progressive development, on the contrary, presents phases peculiar to each of these classes, and is expressed by various metamorphoses, to which each of them is subjected in their specific characters, and in their mutual arrangements."

Of the genus *Eurynotus*, the following British species are described:—1. *E. crenatus*, found by Professor Jameson in the limestone slate of the Burntisland district, and also in the limestone of Burdiehouse, by Dr. Hibbert. 2. *E. fimbriatus*, in the clay-ironstone of Wardie, where it was noticed by Lord Greenock. Of the genus *Platysomus*, the following British species are described:—1. *P. striatus*, in the English magnesian limestone. 2. *P. macrurus*, English magnesian limestone. 3. *P. parvus*, magnesian limestone of Low-Pallion, Northumberland. Of the genus *Gyrolepis*, the following British species are described:—1. *G. Albertii*, at Wickwar, near Bristol. 2. *G. tenuistriatus*, in osseous breccia at Wickwar. 3. *G. maximus*, at Wickwar? 4. *G. giganteus*. The immense scales, found by Professor Jameson and others in the old red sandstone of Fifeshire and Angus-shire, belong to this species. Of the genus *Dipedius*, the following are British species:—1. *D. politus*, a species characteristic of the lias of Lyme-Regis: Then follow geo-

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These four formations of Murchison, interposed between the English old red sandstone and the transition rocks of the Wernerian school, as described by Jameson, are considered as forming a grand group, and named *Silurian* from the Silures the ancient inhabitants of the district where these rocks occur.—EDIT.

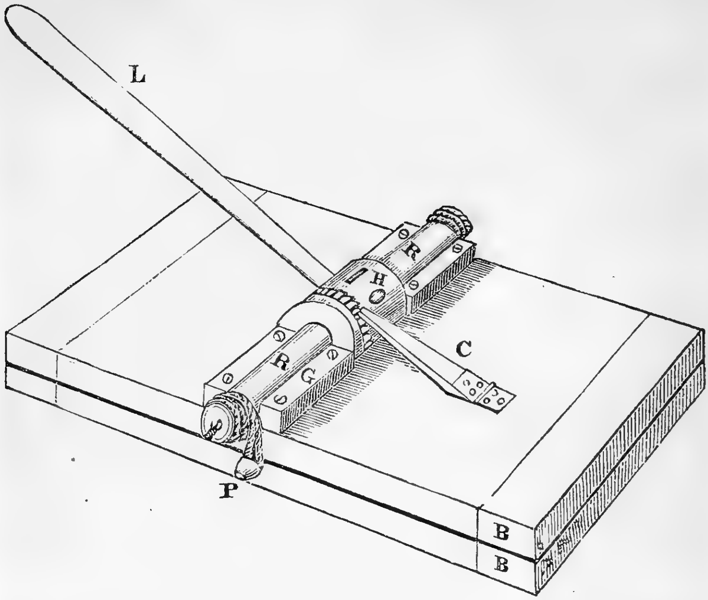
logical observations on the contemporaneous formation of masses of strata, p. 188. 2. *D. granulatus*, the rarest species of the genus found at Lyme-Regis. 3. *D. punctatus*, is peculiar to the lias of Lyme-Regis. 4. *D. Colei*, Lyme-Regis. Of the genus *Tetragonolepis*, the following British species are described:—1. *T. confluens*, in the fine collection of Lord Cole, from Lyme-Regis. 2. *T. speciosus*, in the collection of Lord Cole, from Lyme-Regis.

But we must conclude, for the present, our extracts from Agassiz' work, with the following curious observations connected with the genera Cephalaspis, Dipterus, Osteolepis, Acanthoides, Cheiracanthus, Cheirolepis, Amblypterus, Gyrolepis, Palæoniscus, Platysomus, and Eurynotus, in all of which the tail is *unsymmetric*, having the one lobe longer than the other.

"There is one very remarkable fact concerning the relation of these genera with the geological formations which they characterize; it is this, that all the known species, without exception, have been found in the formations which are anterior to the formation of the lias. Nor can this circumstance be accidental, it is again observed, within the same kind of limits, and upon almost an equal number of species in the Sauridian family; at the same time that all the fish of the Placoidean order, which accompany them in the same formations, have also the same kind of structure of the tail. Some unknown condition of existence, therefore must have been operating in those remote times upon the development of organic life, and must have effected a conformation which is at once so peculiar and so general: For we are not permitted to regard phenomena so constant as these as simple exceptions, since nature in her doings, never admits them on so extended a scale. We can only regard these forms as necessary antecedents of those which have followed; and the traits which characterize and distinguish them as the differences in the progressive developments. These differences consist especially in the transition of an unsymmetric structure to a structure more and more perfect in its symmetry, which has gradually prevailed in subsequent epochs, in which the unsymmetric forms have successively disappeared. To attempt to point out the causes of such a state of things, would be to endeavor to fathom the motives of the Almighty Creator; yet at the same time we may venture to offer some conjectures concerning the relations of the form of these fishes with the external world in which they were destined to live.

"If we regard the whole organized beings which have lived simultaneously with the Heteroceric Lepidoidians, or those with unsymmetric tails, we shall remark that they were for the most part fixed at the bottom of the waters, or at least that they crawled about there, without being able to elevate themselves freely and at their wills towards the surface, and move about at large. With the exception of some reptiles, the appearance of which on the earth was much posterior to that of fishes, all these animals were *aquatic,* and the soil bore only those plants which are analogous to those of great archipelagoes or of low plains. Fish, therefore, were the first of the animals to which the power was given of spontaneously gliding through space in all directions, in water; whilst the movements of the Crustacea were only irregular and imperfect. Among the Mollusca, the Cephalopodes, which have the most power of motion, ascended to the surface of the water, and remained, the sport of the winds in their aquatic ascensions; the Gasteropodes were still more bound to the soil, and the Acephales and Brachiopodes were frequently fixed to it. All the Polypi and Crinoidians of that period appear to have been attached by their base to different solid bodies. At the same time, even the fishes, with their unsymmetrical tails, could not execute the accurate movements of the symmetrical fishes of the following epoch, and their progressive movements therefore would be still irregular. The whole of these animals respiring by branchiæ, could as yet utter no cry, and lived in the most absolute silence. A long period assuredly elapsed ere the surface of the earth was replenished with birds and beasts, and ere man could reflect upon the events which have produced these changes in organic life. We can scarcely conceive that, surrounded with such facts as these, it would be possible to doubt that there was a regular order of succession, and a constant progression in the work of creation."

Art. VI.—*Botanical Press*; by J. Locke, M. D.



TO PROF. SILLIMAN.

The little press figured above, which is an improvement by myself on one figured and described in the Western Journal for July, 1834, by Dr. Riddell, has been so decidedly approved by our western botanists, that I have determined on sending you a description of it for publication.

It consists of two boards drawn together by cords passing from the edges of the lower one, and winding on the projecting end of an axle or roller, crossing the upper one. The action is produced by means of a lever inserted in a capstan head, which constitutes the middle portion of the axle or roller. The winding is held at every point by a click resting in a racket wheel cut around the capstan head. This is the chief improvement alluded to above.

The figure: BB are the boards $1\frac{1}{8}$ by 12 by 19 inches. P, a steel pin with a conic head driven firmly into the lower board by its shank, which is three-sixteenths of an inch in diameter.* The rope or cord, which is of the same diameter, is spliced in a loop, which buttons upon the head of this pin, and can be easily slipped

* A large wood screw will answer if the head be rounded.

off. The ropes are 12 to 18 inches long, and wind around the ends of the axle RHR, which is exactly 1¼ inches in diameter at R and R, and 3 inches at H, which forms the capstan head, having two holes ⅞ in diameter, perforating it in the middle at right angles for the lever L. The racket wheel which is cut in this head consists of 20 teeth cut three-sixteenths deep on the perpendicular side, and about half an inch long, not at the end, but about half an inch from it, to give sufficient support to the teeth. The click C is 4½ inches long, ½ inch thick, 1 inch wide at the hinge end, and half an inch at the wheel, planted 3½ inches from the head, and fastened by a brass table-hinge. The lever L is 16 to 18 inches long. The whole length of the axle is 16 inches. The head part, 3 inches long, is sunk into the board, and rolls between guides at G.

When the press is to be opened, the lever is inserted, the click loosened and turned out, the cords slackened and slipped from the heads of the steel pin, and the upper board lifted off. Although this press is so portable as to be packed in a common travelling trunk, it will exert a force by the application of one hand, of half a ton. When neatly made of mahogany and polished, it is not unsightly in the parlor; and the pressure being applied to the pile of papers containing the specimens, the click holding the last force, the lever may be removed, and it may be set on one end at the side of the room, scarcely incommoding any other operations. It is peculiarly adapted to the purposes of the travelling botanist. It is capable of being applied to other uses than that of pressing plants for an herbarium. On a large scale, it would be an excellent cheese press, and it has been already adopted for some parts of book-binder's operations. Printers will find it convenient to apply to their paper, in wetting it down. In this use, a longer lever should be applied, and a weight attached to it. The cords are ordinarily passed through the ends of the roller, and fastened by a knot; but I have lately made one, intended as a present to a foreign botanist, in which I have passed the cord quite through the whole length of the axis, thus allowing it to be adjusted whenever one cord shall be the longer. The figure shows the oblong hole near H, through which the two ends of the cord were intended to emerge at the extremes of the axle, where they are turned out in a slit, and made to wind on the outside. It should be observed that the axle should be placed so far to one side of the centre, that the cord shall run off from it exactly at the middle, between the two ends of the press.

ART. VII.—*Meteorological Journal for the year* 1835, *kept at Marietta, (Ohio,) in Lat.* 39° 25′ *N., and Long.* 4° 28′ *W. of Washington;* by S. P. HILDRETH.

Months.	Thermometer.				Fair days.	Cloudy days.	Rain and melted snow.		Prevailing winds.	Barometer.		
	Mean temperature.	Maximum.	Minimum.	Range.			Inches.	100ths.		Maximum.	Minimum.	Range.
January, .	34.20	52	2	50	17	14	2	42	W. & N. W.	29.80	28.85	.95
February, .	25.00	55	−15	70	13	15	1	50	W. & S. W.	29.75	29.10	.65
March, . .	41.30	70	5	65	16	15	2	00	N. W. & S. E.	29.92	28.70	1.22
April, . .	49.70	79	24	55	17	13	3	87	S. & S. W.	29.60	28.92	.68
May, . .	63.00	85	42	43	18	13	3	13	S. S. W. & N.	29.63	29.05	.58
June, . .	69.00	86	44	42	18	12	5	50	S.W. N. & N.W.	29.60	29.02	.58
July, . .	69.70	89	42	47	22	9	2	58	W. N. W. & N. E.	29.62	29.25	.37
August, .	68.00	89	44	45	24	7	6	54	S. W. N. & N. E.	29.60	29.30	.30
September,	57.00	88	34	54	21	9	2	75	W. N. N.W. & S. E.	29.75	28.88	.87
October, .	55.00	80	32	48	23	8	4	80	W. N.W. E. & S. E.	29.80	28.95	.85
November,	45.00	76	12	64	14	16	5	50	W. & N.W.	29.73	28.80	.93
December,	31.00	56	6	50	18	13	1	87		29.80	29.	.80
Mean, .	50.65				221	144	42	46		Mean range,		29.31

Remarks.

The past year has been a peculiar one in several particulars; but is the more remarkable for the diminution of heat, and its effects on vegetable life. The temperature for the year is 50°.65; and is about four degrees less than the mean annual amount of heat for this climate, and nearly two degrees less than the preceding year, which was thought to be an extraordinary period in this respect.—In February the mercury fell to 15° below zero, Fahr., a depression considerably greater than has been experienced since the year 1818, when it sunk on the 9th of the same month to 22° below. The effects on the peach in this vicinity were similar, but not so universal; in 1818 it destroyed the *whole*, of whatever age, but in 1835 the old trees only were killed, while the more vigorous and younger trees escaped with the loss of frost-bitten extremities. The mean temperature of the winter months is 30°; of the spring months 51°. 30; of the summer months 68°.90; of the autumnal months 52°. 30. The temperature of the summer is three degrees less than that of the preceding year, which was considered as notably below that of this climate. The winter months are more than five degrees colder than those of the year 1834. The spring and autumn are not so strongly marked, varying but little from the preceding year. The quantity of rain and melted snow amounts to forty two inches and forty six hundredths; being eight inches greater than that of the year before; and is nearly the mean annual supply for a series of years in this part of the valley of the Ohio. The falls of snow have been light, not more than two or three inches at any one time. But even a very slight covering of the earth's surface has a very perceptible influence in diminishing the amount of heat radiated from the earth. The mercury rarely sinks to zero when there is *no snow* on the ground. In January, 1835, when the cold in New Lebanon, N.Y. froze the mercury in the thermometer, and all through the eastern States was of almost unprecedented severity, the depression at Marietta was at no time less than two degrees above zero, and most of the time at 6° or 8°. The earth was *bare*, and there was a free radiation of heat from its surface. In February following, under a different aspect, the earth being *covered with snow*, the mercury sunk to 15° below zero, demonstrating at once the powerful effect of a checked radiation. Snow being one of the most perfect non-conductors of heat, and furnishing a much better material for the winter huts of the inhabitants of the frozen arctic regions, than wood, of which we have evidence in the Voyages of Parry and Franklin. The temperature of April, which is esteemed in some measure an index for the year, was 49°.70; being nearly 6° less than the temperature of the same month in the preceding year, but nevertheless closely approximating to the mean of the current year, which is 50°.65. The blossoms of the peach and the pear were retarded this spring to the 26th of April, and the apple to the 2d of May; whereas in common seasons the peach blooms the fore part of the month, and the apple by the middle. The same retardation in the *ripening* of fruits was continued through the season. The "Drap d' or" apple and "Early Chandler," usually fit for eating by the first of July, were not ripe until the middle of the month.—Wheat harvest on the Ohio river, commences about the fourth of July, and Rye the last of June. This season they were not fit for the sickle until near the middle of that month, in the earliest fields, and the harvest was continued until the last of the same; the grain ripening very unequally, and many of the heads partially or entirely blighted, being destitute of seeds. This was probably owing to some violence or injury inflicted on the pollen, by powerful rains, or a hot sun after a thunder shower. Indian corn, or maize, requiring much more heat for its welfare and healthy growth than the cereal grains, was still more retarded in ripening than wheat. The heat of August being nearly two degrees less than that of July, and four below the usual temperature for this month, checked the ripening of the grain, until many fields, planted at the usual period, were much injured by the frosts, which occurred early in September. It was a wide spreading and common calamity, felt through all the western States, to the serious injury of the farming interest. It was remarked that not a month passed during the summer without frost, on two or more nights of each; a fact not noticed since the cold summer of 1816, when so many spots on the sun's disk interrupted or lessened solar heat. The season has been congenial to the growth of oats and potatoes, assimilating more to that of the eastern States; and large crops of both these articles were produced. Grass was also abundant, but late in ripening. Fruit, especially apples and pears, was in great profusion, and very excellent in quality. The fluctuations of the mercurial column in the barometer, have been more varied than common, being subjected to unusual depressions. It was the lowest on the 22d of March, when it fell to $28\frac{70}{100}$ inches, although unattended with any remarkable phenomenon in the weather. It was at its maximum for the year on the 5th of the same month, standing at $29\frac{92}{100}$ inches; making the greatest range $1\frac{22}{100}$ inch. A remarkable depression took place on the 10th and 11th of November, when it sunk to $28\frac{80}{100}$, and continued at and below 29 inches for forty eight hours. It was then attended by a high wind from the west. This depression was noticed through the western and middle States. Snow fell to the depth of three inches on the 22d Nov., and much floating ice appeared in the Ohio, checking, though not entirely stopping, steam-boat navigation. Early in Dec. the rivers closed above Pittsburgh, and the Ohio was full of floating ice below that place, nearly all the month. At Marietta on the 17th, the mercury sunk to 6° above zero, which is the greatest depression yet felt this winter. January commenced mildly, and has continued so to this 7th day of the month. Rivers clear of ice, and navigation free, with a depth of water sufficient for boats of the largest class. The supply of water has been more abundant than usual the whole season.

Art. VII.—*Caricography;* by Prof. C. Dewey.

Appendix, continued from Vol. xxix. p. 253.

No. 159. Carex *blepharophora*, Gray.*

Tab. Aa. fig. 85.

Spica staminifera solitaria erecta; pistilliferis tristigmaticis ternis vel quaternis oblongis cylindraceis nutantibus; fructibus ovatis subconicis rostratis bidentatis, squamam ovatam oblongam obtusiusculam paulo longioribus; foliis bracteisque ciliatis.

Culm twelve to eighteen inches high, erect, striate; glabrous, scabrous above; leaves short, and shortest below, lanceolate, with sheaths purple at the base, and bracts and leaves slightly ciliate on the margin; stigmas three; pistillate spikes 3—4, pedunculate, pendulous, and short-sheathed; fruit ovate, conic-terete, rostrate, equal or a little shorter than the obtusish and ovate oblong scale. This is a handsome and rather slender species; scales of the stamens like those of the pistils; color of the plant a light green.

Found in the middle parts of the State of New York, by Dr. A. Gray.

No. 160. C. *stenolepis*, Torrey.

Tab. Aa. fig. 86.

Spica staminifera solitaria brevissima et minuta; pistilliferis subquinis oblongis cylindraceis perdensifloris erectis, inferioribus exserte pedunculatis; fructibus oblongis obovatis basi teretibus apice obtusissimis et longorostratis bifurcatis divergentibus subretrorsis, squama lineari basi subdilata in aristæ forma paulo longioribus.

Culm eighteen to twenty four inches high, triquetrous, smooth, striate, erect, stiff; leaves linear-lanceolate, scabrous on the edge, nerved, with long and striate sheaths, and both leaves and sheaths shorter below; bracts very long and leafy, and the upper leaves and lower bracts far surpassing the stem; staminate spike small and short, (sometimes wanting,) with scales long and linear-cuspidate and scabrous; pistillate spikes four to six, cylindric, rather large, very densely flowered, on stiff and shortish peduncles, the upper being nearly sessile; fruit, in the younger state, round and ovate

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* In the Annals of the Lyceum of Nat. History, New York, Vol. iii. p. 235.

lanceolate and rostrate, but in its natural state, very crowed, inflated and obovate, and very obtuse, and yet extended in a long and scabrous beak, two-forked, nerved, horizontal, or retrorse; the scales long, linear, scabrous, awnlike, dilated at the base so as to be small ovate. On the leaves and bracts the midrib and two nerves are very distinct. Color a bright green.

Found at Lexington, Ky., by Dr. Short, from whom it was received and named by Dr. Torrey: related to C. pseudocyperus, C. hystericina, and C. retrorsa. Grows in marshes, and flowers late in the season.—Dr. *Short.*

No. 161. C. *Shortiana,* Dewey.

Tab. Aa. fig. 87.

Spicis quaternis vel quinis androgynis inferne staminiferis, suprema semi-staminifera, cæteris maximé pistilliferis, inferioribus exsertè pedunculatis, omnibus longo-cylindraceis erectis densifloris tristigmaticis; fructibus obovatis obtusis convexo-compressis basi attenuatis et substipatis, squama ovata oblonga acuta vix longioribus.

Culm twelve to twenty four inches high, triquetrous, scabrous on the edge, striate, leafy; lower leaves shorter, with striate sheaths, and upper leaves linear-lanceolate, and nearly as long as the culm; bracts with short sheaths, leafy, and about equalling the culm; spikes four and five, long cylindric, the highest at least half covered with staminate flowers below, and the others with from *four to twelve* staminate flowers below; staminate scale oblong and obtusish, and green on the keel; fruit obovate and obtuse, smooth, tapering to the base and substipitate, yellow, convex on both sides, and yet somewhat compressed, and with an ovate, oblong and acutish scale, and green on the keel. Color light green; stigmas three.

Found in Lexington, Ky., by Dr. Short, and named to his honor. It is a beautiful species, and allied to C. formosa, C. gracillima, and C. virescens. Grows in marshes, and flowers early in the season.—Dr. *Short.*

No. 162. C. *Careyana,* Torrey.

Tab. Bb. fig. 88.

Spica staminifera solitaria, erecta oblonga; fructiferis tristigmaticis binis vel ternis ovatis laxifloris paucifloris, suprema staminifera approximata subsessili, cæteris distantibus exserté pedunculatis, bracteatis; fructibus ovatis triquetris subinflatis nervosis acuminatis ad basin attenuatis lævibus ore integris, squama ovata mucronata duplo longioribus.

Culm one to two feet high, erect, triquetrous, smooth, leafy at the base; leaves linear-lanceolate, soft, with deep reddish brown sheaths; staminate spike one, half an inch long, cylindric, with oblong and obtuse scales deep reddish brown; pistillate spikes 2—3, short, ovate, few-flowered, upper one sessile and near the staminate, with a short cuspidate bract; the lower exsert-pedunculate, with a striate sheath ending in a leafy bract; fruit ovate, tapering at both ends, scarcely rostrate, full of nerves, and with an ovate and mucronate scale, reddish on the edge, green on the keel, and about half as long as the fruit.

Auburn, N.Y.; in the herbarium of Dr. Torrey. It is between C. plantaginea and C. oligocarpa. Grows in marshes, and flowers early in the season.—*Dr. Short.*

No. 163. C. *Greeniana,* Dewey.

Tab. Bb. Fig. 89.

Spica staminifera solitaria vel binis erecta; spicis fructiferis tristigmaticis oblongis bracteatis pedunculatis binis vel ternis oblongis; fructibus ovato-lanceolatis triquetris nervosis rostratis bifurcatis subdensifloris, squama ovato-cuspidata subæquantibus.

Culm one to two feet high, triquetrous, scabrous above, leafy towards the base; staminate scale oblong and obtuse, tawny, white on the edge; pistillate spikes 2—3, with sheathed peduncles, the lowest of the three long-exsertly pedunculate, with large and leafy bracts; fruit ovate and lanceolate, rostrate, smooth, nerved; pistillate scale ovate, cuspidate, or mucronate, tawny, green on the keel; color of the plant light green.

Found in the neighborhood of Boston, by B. D. Green; described from specimens in Dr. Torrey's herbarium.

No. 164. C. *binervis,* Smith.

Schk. Tab. Rrr. fig. 160.

Spica staminifera solitaria erecta; pistilliferis tribus tristigmaticis cylindraceis, inferioribus exserte-pedunculatis; fructibus ovatis rotundis brevi-rostratis bicuspidatis lævibus binervosis, squama ovata subacuta duplo longioribus.

Culm a foot or more high, triquetrous, leafy; bracts leafy, linear-lanceolate, shorter than the culm; staminate spike single, rarely two, one very short, with oblong and obovate obtuse scales; stigmas three; fruit ovate, roundish, terete, subrostrate, two forked; pistillate scales ovate, acutish, half the length of the fruit; color light green.

Found near Boston, by B. D. Green; probably introduced like C. *panicea*, from Europe; it is very like the C. *binervis* of Europe.

No. 165. C. *Columbiana*, D.

Tab. Bb. fig. 90.

Spicis pluribus subsenis androgynis inferne staminiferis tristigmaticis pedunculatis subnutantibus approximatis oblongis cylindraceis densifloris, suprema in medio pauci-fructifera, inferioribus inferne pauci-staminiferis; fructibus obovatis acuminatis subrostratis compressis oblongis, squama lineari-lanceolata angusta multo longioribus.

Culm two to three feet high, striate, triquetrous, scabrous above, with a leafy bract under the lowest spike, and with other bracts cuspidate; leaves long, linear, narrow, flat, striate; stigmas three; spikes about six, an inch or more long, the upper staminate except a few fruit in the middle, the others with a few staminate flowers at the base of each, all pedunculate and somewhat recurved, large and cylindric, and densely flowered; staminate scale oblong, obovate, obtuse and black; pistillate scale linear-lanceolate, rather obtuse, narrow and deep brown; fruit oblong, and obovate, acuminate, subrostrate, compressed, broader and one half longer than the scale.

Found at Columbia River by Dr. Scoreler, and in the herbarium of Dr. Torrey.

No. 166. C. *Martensii*, Prescott.*

Spicis pluribus 4—7, androgynis inferne staminiferis approximatis linearibus pendulis distigmaticis; fructibus ovato-lanceolatis compressis membranaceis ore integerrimis, squama lanceolata latioribus et longioribus.

Culm three to four feet high; spikes linear; the fruit or covering of the seed very delicate, hyaline, entire, without any teeth.—*Prescott.*

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* Memoirs of the Academy of Sciences at St. Petersburgh, VI. Sec. tom. 2. p. 168, in a paper by *M. Bougard* on the plants of *Sitcha*, Russian America.

Found at Sitcha in Russian America, and though the number of stigmas is different, is clearly related to the preceding.

C. *Sitchensis*, Prescott,* is the same as C. *cryptocarpa*, Meyer, described in the last No. of this Journal.

No. 167. C. *Houghtoniana*, Torrey.

Tab. Bb. Fig. 91.

Spica staminifera solitaria erecta; spicis fructiferis tristigmaticis sublimis oblongis cylindraceis, infima exserte pedunculata, omnibus folio-bracteatis sublaxifloris; fructibus ovatis rotundis subinflatis rostratis bifurcatis nervosis hispido-pubescentibus, squama ovata mucronata subduplo longioribus.

Culm a foot a more high, triquetrous, scabrous above, striate; leaves short, and shortest at the base; bracts leafy, long, with short sheaths; staminate scale oblong, obtuse, white on the edge, short-mucronate; stigmas three; pistillate spikes 2—3, oblong, upper sessile; often a single fruit between the staminate and next pistillate spike; fruit ovate, round, dirty brown, rough-pubescent; pistillate scale ovate, mucronate, about half as long as the fruit; color a light green.

Found at Lake La Biche, near the sources of the Mississippi River by Dr. Houghton; in the herbarium of Dr. Torrey; is related to C. *dasycarpa*, and C. *Schweinitzii*.

No. 168. C. *mirabilis*, D.

Tab. Bb. fig. 92.

Spica composita; spiculis androgynis *distigmaticis* inferne staminiferis suboctonis ovato-globosis alternis sessilibus sub-densé aggregatis; fructibus ovatis sublanceolatis e margine scabris concavo-convexis rostratis bidentatis subdivergentibus, squamam ovato-lanceolatam vix duplo longioribus.

Culm sixteen to thirty six inches high, erect, rather stiff, obtuse angled, striate, scabrous above, slender; leaves flat, striate, linear-lanceolate, scabrous on the edge, sheathed towards the base, nearly as long as the culm, shorter below; spikelets 6—10, roundish, ovate, sessile, usually close together, with two stigmas, and with a few sta-

* See note on preceding page.

mens at their base; staminate scales ovate and rather obtuse; fruit ovate, convex above, narrowed and scabrous above, rostrate and two-toothed; pistillate scale ovate-lanceolate, tawny, about two-thirds as long as the fruit; color of the plant a fine green.

Found along fences and hedges, not very abundant; confounded heretofore with C. *fœnea*, C. *straminea*, or C. *festucacea*. From the first two it is widely separated by its fruit and scale, and general habit, and from the last by the shape of its spikelets especially, as those of C. *festucacea* are *clubform*.

Figures of the following species are given in this volume.

C. *blepharophora*, Gray.	Fig. 85.
— *stenolepis*, Torrey.	86.
— *Shortiana*, Dewey.	87.
— *Careyana*, Torrey.	88.
— *Greeniana*, Dewey.	89.
— *Columbiana*, Dewey.	90.
— *Houghtoniana*, Dewey.	91.
— *mirabilis*, Dewey.	92.
— *siccata*, Dewey, Vol. x. p. 278.	93.

Art. VIII.—*Notice sur la Vie et les Ouvrages de M. le Comte Lagrange;* par M. le Chevalier Delambre, Secrétaire Perpétuel de l' Institut Royal de France. (Lue le 3 Janvier, 1814.)

(Translated and communicated for this Journal by F. Furber, Boston, Mass.)

Joseph Louis Lagrange, one of the founders of the Academy of Turin, Director during twenty years of the Academy of Berlin, for the physico-mathematical sciences, Foreign Associate of the Academy of sciences of Paris, member of the Institute of France and of the board of longitude, Senator and Count of the Empire, Grand Officer of the Legion of Honor and Grand Cross of the imperial order of the reunion, was born at Turin on the 25th of January 1736. His father was Joseph Louis Lagrange, Treasurer of War; his mother, Maria Theresa Gros, only daughter of a wealthy physician of Cambiano.

His great-grandfather, captain of cavalry in the service of France, had gone over to that of Emmanual II, King of Sardinia. Through the latter he was fixed at Turin, by marriage with a lady *Conti*, of an illustrious Roman family: he was of Parisian extraction, and relative of one Maria Louisa, tire-woman of the mother of Louis XIV, and afterwards wife of François Gaston de Bethune.*

These details are of no importance to the illustrious Geometer, whose renown dispenses with shewing forth a genealogy, but not so to France. She is eager to recal him, and reestablish him under her ancient sovereignty. His own name, and that of his mother also, attest a French origin; all his works are written in French; the city which saw his birth too had become French. France then, has incontestably the right of being proud of one of the greatest men who has honored the sciences.

His father was wealthy, had made an advantageous marriage, but was ruined by hazardous enterprises. Let us not hence pity M. Lagrange. He himself received this misfortune as the first cause of all which afterwards befell him most happily. *S'il avait eu de la fortune*, said he himself, *il n'eût probablement pas fait son état des mathématiques.* And in another career, what advantages could he have found, that had entered into comparison with those of a calm and studious life, with that brilliant train of success, uncontested in a department reputed eminently difficult, and with that personal esteem, which he saw increase till his last moment.

A taste for mathematics, however was not that which he first manifested. He had a strong passion for Cicero and Virgil before being able to read Archimedes and Newton. Soon he became an admirer no less passionate of the geometry of the ancients, which he at first preferred to the modern analysis. A memoir which the celebrated Halley had long before composed, expressly to show the superiority of analysis, had the glory of converting M. Lagrange, and revealed to him his true destination.

He then gave himself up to this new study with the same success which he had obtained in synthesis, and which had been so marked, that at the age of sixteen† years he was professor of mathematics in the royal school of artillery. The extreme youth of a professor is for him but a greater advantage, when he has shown extraordinary talents and at the same time his élèves are not children. All those of Lagrange were older than himself and were not thence less atten-

* Eulogy of Lagrange by Cassali. *Padua*, 1813.

† Others say fifteen or nineteen.

tive to his *lessons*. He selected some of them whom he made his friends.

From this association sprang the Academy of Turin, which published in 1759 a first volume, under the title of *Actes de la société privée*. We therein see Lagrange directing the physical researches of doctor Cigna, and the works of the *Marquis de Saluces*. He furnished to Foncenex the analytical part of his memoirs, at the same time leaving to him the care of developing the arguments on which his formulas rested. In effect, we notice already in these memoirs this pure analytical step which afterwards characterised the great productions of Lagrange. He had found a new theory of the lever. It constituted the third part of a memoir that had much success. Foncenex in return, was put at the head of the navy which the king of Sardinia was then forming. The two first parts seem of the same style and from the same hand. Are they alike from Lagrange? He has not positively claimed them. What however, can direct our conjectures upon the real author, is, that Foncenex soon ceased to enrich the collections of the new Academy, and that Montucla, ignorant of what has been revealed to us by Lagrange at his last moments is astonished that Foncenex, after being so favorably announced, broke off researches that could have obtained for him a great name.

Lagrange abandoning to his friend isolated solutions, published at the same time under his own name some theories which he promised to follow out and develope. Thus after having given new methods for *maxima* and *minima* of every kind, after having shewn the insufficiency of the the known formulas, he announced that he would treat this subject, which otherwise appeared to him interesting, in a work which he was preparing, and in which, too, are seen deduced from the same principles all the mechanics of bodies, whether solid, or fluids. Thus, at twenty three years he had already laid the foundation of great works which have since caused the wonder of savans.

In the same volume, he brings back to the differential calculus, the theory of recurring series, and the doctrine of chances, which, until now, had been treated only by indirect methods, and which he establishes upon the most natural and the most general principles.

Newton had undertaken to submit to the calculus the motions of fluids: he had made researches on the propagation of sound. His principles were insufficient and even defective; and his suppositions inconsistent with themselves: Lagrange so demonstrated. Lagrange founded his new researches on the known laws of dynamics; by considering in the air only the particles found in a straight line, he reduced this problem to that of vibrating cords, about which the greatest geometers were divided; he showed that their calculations were insufficient to decide the question; he undertook a general solution by an analysis as new as interesting, since it permits of resolving at once an indefinite number of equations, and since it extends even upon discontinued functions: he established more firmly the theory of the mixture of the simple and regular vibrations of D. Bernouilli: he shows the limits between which this theory is exact, and beyond which it is defective; *then he* arrives to the construction given by Euler, a true construction, although the author had arrived to it only by calculations which were not sufficiently rigorous: he answers objections raised by D'Alembert; he demonstrates that whatever figure we give to the cord, the duration of oscillations will be always the same, a truth of which for experiment D'Alembert had judged the demonstration very difficult or even impossible; he passes to the propagation of sound; treats of simple and compound echoes, of the mixture of sounds, of the possibility that they spread in the same space without disturbing one another, and demonstrates rigorously the generation of harmonical sounds; he announces in fine, that his object is to destroy the prejudices of those who still doubt if mathematics could ever shed true light in physics.

We have given the above extent to the extract of this memoir, because it is the first by which Lagrange became known. If its analysis is of the most transcendent kind, the object at least has some thing in reality. It recalls names and facts, which are not foreign to the greater part of the audience. We have done so, because it is surprising that such had been the commencement of a young man. He it was, that, seizing upon a subject treated by Newton, Taylor, Bernoulli, D'Alembert and Euler, appeared suddenly in the midst of these great geometers, as their equal; as an umpire too, who, to put an end to a difficult case, showed to each of them wherein he was right and wherein he was wrong; judged them, reformed them, and gave to them the true solution, which they had seen faintly, without being able to attain to it.

Still, however, solid and well grounded as his calculations appeared to him, the author avows that they render only an imperfect account of phenomena observed, so far as concerns the theory of wind instruments, the size and position of their holes, and the velocity of the sound in general. It is probable in effect, that in these instruments particularly, the air should no longer be considered as divided into straight lines; at least, however, the solution explains the famous experiments of Tartini, if we admit that this celebrated Professor could have been deceived in putting the octave in the place of the real sound which he heard.

Euler felt the worth of the new method, and selected it for the object of his most profound meditations. D'Alembert did not coincide. In his private letters, as in his printed memoirs, he proposed numerous objections, to which Lagrange has since answered but which can at least leave this doubt; . . . How in a science to which we grant universally the merit of exactness, can it be that men of the first order are divided against themselves, and for a long time dispute? The reason is, that in problems of this kind, the solutions of which cannot be submitted to the proof of a direct experiment, besides the part of the calculus which is subjected to rigorous laws, and upon which it is not possible to have two opinions, there is always a *metaphysical* part which leaves doubt and obscurity. The reason is, that in the calculations themselves, geometers are often content with *pointing out* the steps of demonstrations, while they *suppress developments* that are not always so *superfluous* as they have been thought; while the care of filling up their gaps require a labor which the author only has courage to undertake, and while, in fine, he himself led on by his subjects, and by the habit which he has acquired, permits himself to pass over intermediate ideas, and anticipates his resulting equation, instead of arriving at it, step by step, with an attention that would escape all mistake. It is that very great geniuses cannot be made to harmonize together at first, for want of being read with sufficient attention to be well understood.

The first answer of Euler was to cause Lagrange to be associated with the Academy of Berlin. Upon announcing to him this nomination, on the 2nd of Oct. 1759, he said to him: "*votre solution du problême des isopérimètres ne laisse rien à desire et je me réjouis que ce sujets, dont je m'etais presque seul occupé depuis les premières tentatives, ait été porté par vous au plus haut degré de perfection. L'importance de la matierè m'a excité à en tracer à l'aide de vos lumières, une solution analytique à laquelle je ne donnerai aucune publicité jusqu à ce que vous-même ayer publié la suite de vos recherches pour ne vous enlever aucune partie de la gloire qui vous est due.*"

If these delicate procedures, and the testimonies of the highest esteem should flatter a young man who was not twenty four years old, they do no less honor to a great man, who, holding then the sceptre of mathematics, knew how to receive in this manner the work which pointed to him his successor.

But these eulogies are contained in a letter: hence we might think that the great and good Euler may have suffered himself to go on in some of the exaggeration permitted in the epistolary style; let us see then how he afterwards expressed himself in the dissertation which his letter announced. Here is the beginning.

"After I had long and vainly fatigued myself in seeking for this integral, (postquam diu et multum desudassem......nequicquam inquisivissem) what was my astonishment (penitus obstupui) when I learned that in the Memoirs of Turin, this problem is found resolved with as much ease as excellence. This fine discovery caused me the more admiration as it is the more different from the methods which I have given, and as it surpasses them considerably in simplicity." It is thus that Euler begins the memoirs in which he explains with his usual clearness, the reasons of the method of his young rival, and the theory of this new calculus, which he has called the *calculus* of *variations.*

To render more sensible all the different motives which gave birth to the admiration that Euler showed with so noble a candor, it will not be useless to recur to the origin of the different researches of Lagrange, such as he gave it himself two days before his death.

The first attempt to determine the maximum and minimum in all indefinite integral formulas, had been made on account of the curve of the swiftest descent, and the isoperimeters of Bernouilli. Euler had reduced them to a general method, in an original work, wherein shines throughout a deep knowledge of the calculus; but, however ingenious was his method, it had not all the simplicity which we can desire in a work of pure analysis. The author concluded so himself; he perceived the necessity of a demonstration independent of geometry and of analysis.

In an appendix to the volume having for its title *du Mouvement des projectiles dans un milieu non resistant,* he seemed wholly to distrust the resources of analysis, and finishes by saying *Si mon principe* (it is that which Lagrange has since named the principle of the last action) *n'est* pas suffisamment démontré, comme cependant it est conformé à la vérité, je ne doute pas qu'an moyen des principes d'une saine métaphyssique on ne puisse lui donner la plus grande évidence, et j'en laisse le soin à ceux qui font leur état de la métaphysique.

This appeal to which metaphysicians did not answer, was understood by Lagrange who excited their jealousy.

In a short time the young man found the solution of which Euler had despaired. He found it by analysis; and in giving an account of the way which had led him to this discovery, he said positively, to answer the doubts of Euler, that he viewed it, not as a metaphysical principle, but as a necessary result of the laws of mechanics, as a simple corollary of a more general law, which he afterwards made the base of his *Mecanique Analytique.* (See this work, page 246 of the second edition, or 189 of the first.)

This noble spirit that excited him to triumph over difficulties regarded as insurmountable, and to rectify or complete theories still imperfect, appeared to have constantly directed Lagrange in the choice of his subject.

D'Alembert had thought it impossible to submit to the calculus the motions of a fluid contained in a vessel, if this vessel had not a certain figure. Lagrange demonstrated on the contrary that there would be no difficulty except in the case when the fluid is divided into many portions. Yet then we can determine the places where the fluid ought to be divided into many portions of which we can determine the motions as if they were isolated.

D'Alembert had thought that in a fluid mass such as the earth might have been originally, it was not necessary that the different layers should be on a level: Lagrange shews that the equations of D'Alembert were themselves only those of strata on a level.

In opposing D'Alembert with all the respect due to a geometer, of that order, he often employed very fine theorems which he owed to his opponent; D'Alembert, on his side added to the researches of Lagrange. "Your problem appeared to me so fine," wrote he to him, "that I have sought for it another solution; I have found a more simple method to arrive at your elegant formula." These examples, which it would be easy to multiply, prove with what courtesy these celebrated rivals corresponded. Vying with each other incessantly, conquered as well as conquerors, they found at every moment in their discussions themselves, reasons to esteem one another the more, and each supplied for his antagonist opportunities that were to lead him to new triumphs.

The Academy of sciences of Paris, had proposed for the subject of one of its prizes, the theory of the libration of the moon: that is to say, it asked the cause why the moon, in turning around the Earth always shows the same face, with the exception of some variations observed by astronomers, and of which Cassini I. had well explained the mechanism. The point was, to find the means of calculating the phenomena, and of deducing them analytically from the principle of universal gravitation. Such a chance was an appeal to the genius of Lagrange; one, which was held out to him of applying his principles and his analytical discoveries. The hope of D'Alembert was not blasted. The piece of Lagrange is one of his highest titles of glory. Therein are seen the first developments of his ideas and the germ of the *Mecanique Analytique*. D'Alembert wrote to him; *j'ái lu avec autant de plaisir que de* FRUIT *votre belle pièce sur la libration, si digne du prix qu'elle a remportée.*

His success inspired in the Academy the confidence of proposing the theory of the satellites of Jupiter. Euler, Clairaut, and D'Alembert had engaged about the problems of the three bodies on account of the motions of the moon. Bailly applied then the theory of Clairaut to the problems of satellites. It led him to results strikingly interesting; yet this theory was insufficient. The earth has but one moon, Jupiter four, which must be reciprocally troubled and deranged in their orbit. The problem seemed that of six bodies, the Sun, Jupiter, and the four moons. M. Lagrange attacked the difficulty in front, triumphed over it happily, demonstrated the cause of inequalities observed by astronomers, and pointed out some others too feeble to be noticed by observations. The shortness of time fixed for the session, and the immense calculations, both analytical and numerical did not allow the subject to be wholly exhausted in a first memoir. The author himself announced this, promising farther researches to which other labors and perhaps his own taste, always prevented him from devoting himself. Twenty four years after, M. le Compte Laplace assumed this difficult theory, made in it some interesting discoveries which completed it and put astronomers in the situation of banishing all empiricism from their tables.

About the same time a problem of a totally different kind attracted the attention of M. Lagrange. Fermat, one of the greatest geometers of France and of his time had left, on the properties of numbers, some very remarkable theorems to which he had arrived perhaps by means of induction, but of which he had promised demonstrations. These were not found at his death, being perhaps suppressed as insufficient, or for some other cause hard to be conjectured. These theorems, in other respects may appear more curious than useful. We know however, that difficulty is an attraction, for all men and especially for geometers. Without such attraction, can we believe that they would have placed so much importance on the problems of the brachystochrone, of isoperimeters, and of orthogonal trajectories? In truth, they wished to create the science of the calculus, and to invent or bring to perfection methods which could not fail of finding one day useful applications. Under this light, they would devote themselves to the first question which required the employment of new resources.

Such was for them as fine a fortune as the system of the world discovered by Newton. Never had transcendant analysis been able to find a more worthy or grand subject. Whatever progress is made therein the first inventor will preserve his rank. Lagrange who often called him the greatest genius which had ever existed, added himself also *et le plus heureux; on ne trouve qu'une fois un systême du monde à établir.* It required a *hundred years* of labors and of discoveries to raise the edifice of which Newton had laid the foundations. Yet he has received the praise of all, and has been supposed to have finished entirely the career which he simply began; began, however, with an *éclat* which should encourage his successors.

Many geometers, undoubtedly, practised upon the theorems of Fermat, but not one ever succeeded. Euler alone had made some progress in this difficult path wherein have since distinguished themselves M. Legendre and M. Gauss. Lagrange, upon demonstrating or correcting some attentive glimpses of Euler resolved a problem which appeared to be the *knot* of all the rest, and from which he made flow a useful result, that is to say, the complete resolutions of equations of the second degree, with two unknown quantities that must be entire numbers. The memoir, printed like the preceding among those of the Academy of Turin, is nevertheless dated at Berlin, the 20th September, 1768. This date points out to us one of the events, (few indeed,) which show that the life of Lagrange is not all in his works.

The residence at Turin pleased him little. He saw there no one that cultivated mathematics with success: he was impatient to see the savans of Paris with whom he corresponded. M. de Caraccioli, with whom he lived in the greatest intimacy, had just been nominated to the embassy of England, and was to pass through Paris where he purposed to tarry awhile. He proposed this journey to Lagrange. Lagrange consented to it with joy, and as was right to expect, was welcomed by D'Alembert, Clairaut, Condorcet, Fontaine, Nollet, Marie, and other savans. Having fallen dangerously sick in the course of a dinner, when Nollet had served to him only dishes prepared *a l'italienne,* he could not follow to London his friend, M. Caraccioli who suddenly received the order of repairing to his post, and was obliged to leave him in a furnished hotel, to the care of a confidential person, directed to supply all his wants.

This event changed his purposes. He dreamed of nothing but of returning to Turin. He gave himself up to mathematics with a new ardor, when he learned that the academy of Berlin was threatened with the loss of Euler, who was intending to return to Petersburgh. D'Alembert spoke of this intention of Euler in a letter to Voltaire, the 3d of March, 1766; *j'en serais* fâché, added he, *c'est un homme peu amusant, mais untres-grand géomètre.* It was of little consequence to D'Alembert that the *homme peu amusant* should remove seven degrees from Paris towards the pole. He could read the works of the *great geometer* in the Transactions of the Academy at St. Petersburg, as well as in those of the one at Berlin. What troubled D'Alembert was, the fear of seeing himself called upon to replace him; and the embarrassment of replying to offers which he was well resolved not to accept. Frederic, in fact, proposed anew to D'Alembert the place of president of his academy which he held for him in reserve after the death of Maupertuis. D'Alembert suggested to him the idea of placing Lagrange in the place of Euler; and if we believe the secret history of the court of Berlin (tom. II, p. 474), *Euler had already pointed out Lagrange as the only man capable of following in his track.* And in effect, it was natural that Euler, who wished to obtain leave to quit Berlin, and D'Alembert who sought a pretext for not going thither, should both, without corresponding with each other, have cast their eyes upon the man most fit to sustain the glory which the labors of Euler had shed upon the Academy of Prussia.

M. Lagrange was engaged with the title of director of the Academy for the physico-mathematical sciences. We can be astonished that Euler and Lagrange, placed successively in the stead of Maupertuis, should have received but half of the salary which the king wished to give, apart from every thing to D'Alembert. The reason is, that this prince, who in his leisure, cultivated poetry and the arts, had no idea of the sciences which he thought himself obliged notwithstanding to protect as king: the reason is, that in reality he *placed* little value upon geometry, against which he sent three pages of verse to D'Alembert himself. D'Alembert delayed answering him until the end of the siege of Schweidnitz, by the reason that it *ce serait trop d'avoir a-la-fois l'autriche et la géométrie sur les bras*; and in fine, notwithstanding the immense reputation of Euler, we see by the correspondence with Voltaire, that Frederic designated him only by the qualification of his *géomètre borgne, dont les oreilles ne sont pas faites pour sentir les délicatesses de la poésie:* to which Voltaire added; *nous sommes un petit nombre d' adeptes qui nous y connaissons, le rest est profane:* a remark more witty than fair, and which Euler, in speaking of geometry, might have been able to retort against Voltaire and Frederic. We see plainly that Voltaire who had so *worthily lauded Newton,* sought in this expression to flatter Frederic. He entered out of courtesy into the ideas of a prince. For Frederic wished to put at the head of his Academy a savant only, who had at least some reputation in literature, under the fear that a geometer would not take sufficient interest in the direction of literary works; and at the same time, that a man of letters would not be more out of place at the head of a society, composed in part of savans whose language he did not understand. He was then right in dividing the office in order that it might be completely filled.

Lagrange took possession the 6th Nov. 1766. The procès-verbal which makes mention of it, gave him the names of Lagrange-Tournier. *Il avait eté bien reçu par le roi, mais il s'apperçut bientôt que les Allemands n'aiment pas que les étrangers viennent occuper des places dans leur pays; il se mit a bien étudier leur langue: il ne s'occupa sérieusement que de mathematiques: il ne se trouva sur le chemin de personne, parce qu'il ne demandait rien, et força bientôt les Allemands à lui accorder leur estime. La roi me traitait bien,* added he himself, *je crois qu'il me préférait à Euler qui était un peu Lévot, tandis que moi je restais etranger à toute discussion sur le culte, et ne contrariais les opinions de personne.* This prudent reserve, by depriving him of the advantages of an honorable familiarity, necessarily somewhat inconvenient, left to him all his time for his mathematical labors, that had drawn out for him until then only the most flattering and unanimous eulogies. But once was this harmony of praises disturbed.

A French geometer, who united to much sagacity, a still greater self-esteem and scarcely gave himself the trouble of studying the works of others, accused Lagrange of having wandered in a new path, that he had marked out, without having well understood the theory; he reproached him with committing mistakes in his assertions and his calculations. Lagrange, in his answer showed some astonishment at these uncourteous expressions, to which he was so little used; he expected at least to see them explained on some good or bad reasons. But he found none of any kind. He shows that the solution proposed by Fontaine was defective and illusory in certain respects. Fontaine had boasted of having taught geometers, the conditions which render possible the integration of differential equations with three variables. Lagrange shewed him, by many citations, that these conditions were known by geometers long before Fontaine was ever able so to inform them. He does not deny, in other respects, that Fontaine could have found these theorems himself, "*at least I am persuaded*" added he, "*that he was as able as any one to find them.*"

With these regards and this moderation he answered to the aggressor. Condorcet, in the eulogy of Fontaine, in relation to this dispute, is obliged to confess that his brother had strayed from that *politesse*, with which he never before dispensed: but that he thought it perhaps unimportant with opponents of eminence, and those whose glory had no need of these trifling courtesies. We feel the worth of this excuse, especially when we present it in favor of a man, who, according to his own confession, applied himself, to study the vanity of others, in order to wound it at a fit opportunity. It must be agreed at least that he, who was seen attacked in this manner when he was in the right, and who knew how to preserve this politesse towards an adversary, who had dispensed with it, had gained a double advantage over him whose imprudent attacks he had otherwise successfully repelled.

We must not be expected to follow Lagrange step by step, in the learned researches with which he has filled the Memoirs of Berlin, and even some volumes of the Academy of Turin, that owed to him in all respects its existence. But we cannot omit pointing out, at least in a few words, the most remarkable which they contain. We will cite:—

A great memoir wherein are found the demonstration of a curious proposition that Euler could not demonstrate, a new extension given to this theorem and direct proofs of many other propositions, to which Euler had arrived only by way of induction, and in which, after having enriched the analysis of Diophantus and Fermat, the author passes to the theory of equations with partial differences explains a striking paradox noticed by Euler, makes known an entire class of equations of which there were only some isolated examples, and puts out of sight the paradox by showing to what belong, both the complete integral of these equations, and the *singular solution* which is not comprised in this integral.

A formula for the return of series, remarkable by its generality and the simplicity of the law, of which he makes a happy application to the problem of Kepler, and thence succeeds in rendering sensible the convergency of the analytical expression of the equation of the center, a convergency which we had always supposed, without being able to demonstrate.

An important memoir on the solution of numerical equations, containing also new remarks on that of algebraical equations. This work served as the base to a treatise which he afterwards published, under the same title, and of which he gave two editions.

Another memoir, no less important, and still more original, where he reduces to operations of pure algebra, every process of the differential and integral calculus, which he separates from every idea of infinitely small, of fluxions, of limits and of vanishing, and demonstrates the lawfulness of the abbreviations permitted in these two *calculs*, which he also frees from all difficulties, and from all paradoxes that had sprung up in an imperfect and suspected *metaphysique*.

The demonstration of a curious theorem on primal numbers; a demonstration that no one had been able to find, and the more difficult, as we know how to express algebraically propositions of this kind.

The integration of partial differences of the first order, by a fruitful principle, sufficient for the greater part of cases where this integration is possible.

A purely analytical solution of the problem of the rotation of a body of any figure, wherein he at last surmounts difficulties that had long stopped him, and by which geometers seemed to expect, with curiosity, some ulterior developments, that they hoped to find in the second volume of his new *Mecanique Analytique*.

Many memoirs on the obscure and difficult theory of probabilities, wherein we admire the integral that forms its base, the number and importance of the problems it resolves; the application that the author makes of it to the question, recurring every day in astronomy; of the degree of confidence that can be allowed to the mean result of a great number of observations; and wherein is found this remarkable property, and so favorable to the circles of Borda, that each of the even numbers states as probable, by the odd number immediately above, that the error will be comprised within certain limits. M. le Comte Laplace had on his part labored on the same theory. M. Lagrange resumed it, on his part, by means which extend to equations of all orders. Of these, they give finite integrals, and facilitate, in all cases, the determination of arbitrary functions.

Maclaurin had treated, after the method of the ancients, the attraction of elliptic spheroids. Lagrange thought this work comparable with all that Archimedes had left of ingenuity and excellence; he showed then that analysis can treat this difficult subject with the same success; he succeeded in it, but was stopped at the same point as the English geometer. M. Legendre and M. Laplace have since been farther. But Ivory has just shewn us, that an extremely simple view can render useless many calculations, and reach even theorems to which the most tedious calculations lead only with difficulty. Formerly, geometers, in every question, tried at first to gain these insights, that could simplify them or reduce them to questions already solved, thus shortening the calculations or rendering them even entirely useless. Since the discovery of the infinitesimal calculus, the facility, the generality of the method, which often dispenses with the calculator's having genius, has caused, that in the most difficult cases, the object chiefly in view was to perfect the universal instrument. But now, that the resources of this kind have been entirely exhausted, by the labors of Euler, of Lagrange, and of their worthy rivals, it will be perhaps time to return to the ancient method, and to imitate D. Bernouilli, whom Condorcet has praised with having distinguished himself upon calculations. Lagrange made more constantly another use of his sublime talents; he drew all from analysis. Yea, it is still more true, to say that he has united each method to the highest degree. The proof of it is in the calculus of variations, to which cannot be compared, either for greatness or for generality, any of the most happy ideas of other geometers. But if it is a question of these ingenious glimpses, of which all before was limited to simplify a single question, it is thus that from the first steps he had reduced the phenomena of sound to the theory of vibrating cords. It is thus that in the last work which he has presented to the class, he succeeded in simplifying remarkably his theory of the variations of the elements of the planets, and in drawing from his solution a general method for all problems of mechanics, where the disturbing forces are small in comparison with the principal forces. But as we often see him make the most happy efforts to generalize a solution, to exhaust a subject, so also we sometimes see him create difficulties where none existed, and apply his adroit and learned methods to the solution of elementary problems that required only a construction of the most simple nature.

Thus, at the time of the last transit of Venus, he treats analytically curves of entrance and of departure, for the different countries of the earth. But to arrive at the very easy and moderately exact solution given by Delille and Lalande, he is obliged to employ successively crooked shifts, remarks full of subtlety, to cause his co-ordinates to undergo a number of transformations, whilst by a trigonometrical calculation of some lines, we arrive at a more complete formula, wherein are found terms neglected by Lagrange, and which, though very small, are not yet absolutely insensible. Let us notwithstanding avow, that he knows how to gather from his formula, for calculating the parallax of the sun, a very advantageous part, of which neither Delille nor Lalande had obtained a glimpse, but which still proceeds with more facility from the trigonometrical calculus. Let us add, still, that this memoir, which had been wholly unknown to me, even to the time when I was obliged to read all that had issued from his pen, appeared to have served some modern astronomers, in establishing which they were obliged to acknowledge, that Lagrange therein gave the first example, somewhat extended, of an elementary problem of astronomy, solved by the method of three rectangular co-ordinates, of so great and so indispensable use in transcendant astronomy.

He made then a similar attempt for the problem of eclipses; he found that the methods, somewhat prolix, of Duséjou, had neither the simplicity nor the facility that ought to have been expected from the actual state of analysis. He shows, in this work, all his resources and all his address. The reading of his memoir is very touching, to an astronomer, who had no idea of these methods. I have not forgotten the effect it produced on myself, thirty years ago, when I first read it; I remember still with what praises, some years after, M. Oriani spoke to me of this work. The author attempted to facilitate the practical part of it, by the aid of ingenious tables. Still, however, we do not see that astronomers have adopted this method. For, beginning with formulas apparently, the most direct, and the most rigorous, and most proper, for application to all cases, it nevertheless terminates in one merely approximative, and what is still worse, indirect.

Another attempt of the same kind was not more happy, because success was impossible. The problem was very simple. It was proposed to find the difference between the heliocentric and geocentric longitudes of a superior planet.

Among the sports of his genius which sought difficulties to display best its powers, ranked still the memoir, wherein he points out the means of constructing astronomical tables, after a series of observations, and without knowing the law of celestial motions. It is a problem that astronomers of all ages have resolved by means the most elementary. The methods of Lagrange, are more analytical and learned; yet in the very example which he has selected, and which is most simple, it is doubtful whether those employed are the most sure and easy. Undoubtedly he wished only to show us the resources which he had found in analysis, since Kepler and Newton had unfolded to us the system of the world, and the laws by which the planetary motions are accomplished. For it is not possible to suppose that he could have had the least doubt about that law of universal gravity, of which he himself had given so many fine developments. Nevertheless in many passages of his works, he took care to establish his formulas for any law of attraction, in order to render them independent of every hypothesis.

Geometers will read with pleasure his analytical researches on the problem of projections, which had never before been treated in a manner so general or complete; astronomers and geographers will find in it for practice, only what they had previously learnt by the merest elements. If these latter memoirs offer no results in reality useful, otherwise than that they furnish pleasant reading, they still give us this lesson, which may have frequent applications, viz.:— that easy questions should be treated only by means equally plain; that learned analysis, should be reserved for questions that need powerful means, and that in doing otherwise, we may resemble the man in the fable, who, to get rid of a flea, wished to borrow of Hercules his club, or of Jupiter his thunderbolt.

We must believe that on these occasions Lagrange wished not seriously to propose to astronomers these troublesome means in the place of these more easy and exact of which they were in possession. He made of these plain, common, and already solved problems, the same use that other analysts have made of questions of pure curiosity. For the questions furnished them examples of calculations, and opportunities of unfolding new artifices of analysis that were always well to be known.

But a work grand in its object, useful by its continual applications, and worthy entirely of his genius, is that in which he has calculated the successive changes that operate in the dimensions and the positions of the planetary orbits. All the geometers since Newton, had engaged upon this problem ; their differential formulas applied successively to each planet, could until a certain period and during a certain time, satisfy the wants of astronomy ; but after some time they were found insufficient, and the calculations to be made again upon new data. M. Lagrange viewed the question under a point of sight that embraced it entirely, and permitted its most complete solution. Instead of combining the orbits two by two, like his predecessors, he considered them all together and whatever be their number, he succeeds in giving to the equation a form which permits of integration, supposing on one side, the fundamental principle of gravitation, and on the other, the orbits known as they were, for a certain epoch. His analysis determines what they have been and what they will be in all past and future ages. The solution leaves nothing to be desired, unless it be a more exact knowledge of the mass of the planets that have no satellites. But this knowledge itself, in time, can be obtained by his formulas. In the mean time M. Laplace drew from his work a more limited but more easy solution ; since, allowing him to go back to the first years of astronomy, it extends into the future the same number, namely two thousand.

To be concluded in the next No.

Art. IX.—*On the Migration of the Birds of North America.* Read before the Literary and Philosophical Society of Charleston, (S. C.) March 15th, 1833; by Rev. J. Bachman.

For ages past, the migration of birds has been a subject of great interest to naturalists. The mysterious appearance and disappearance of many species, at different periods of the year, while many of them have never been seen in their migrations; the remote or unknown situations to which they retire; the sudden appearance of some birds in the spring, after one or two days of warm weather, and their equally sudden disappearance on the first cold day; all have conduced to create many vague and superstitious notions, in the minds of the uninformed, and have often left the intelligent student of nature in perplexity and doubt. Examples are seen, in the accounts so often published, of the swallows having been found, in great numbers, in caves and hollow trees, and in lakes and ponds; and of the common Rail or Sera, (*Rallus Carolinus*, L.) having been discovered in gutters and hollow banks.

Some have supposed that birds, like some animals, are, by their internal organization, capable of becoming dormant, during winter, and hence they readily listen to stories of birds having been found, concealed, in great numbers, in caverns, the hollows of decayed trees, recesses of old buildings, and other secluded situations; whilst others have contended, that they were, during the winter, preserved under the water, beneath the mud.

Amidst such contradictory opinions, on a subject concerning which the most intelligent naturalists are not yet agreed, there is a wide field open for inquiry and observation. The works of God, amidst the wonders of nature, are always worthy of investigation. If he has given to the birds of the air, instincts which cannot be equalled by the boasted reason of man—if he has communicated to them some mysterious faculties, which have hitherto baffled the researches and wisdom of the wise,—may it not be well for us, at least, to record the facts, so that, although we may not be able to explain these hidden mysteries of nature, we may be humbled under a sense of our inferiority, and thus be led to adore the wisdom of God.

Very little appears to have been written on the migration of North American birds; a topic probably regarded as of too little importance, to merit the research necessary to a satisfactory result on such an intricate subject; for the elucidation of which, I have myself possessed some opportunities, by witnessing the migration of birds, in three very distinct portions of America.

That instinct is truly mysterious, which, at particular seasons of the year, teaches birds to take wing and leave their native haunts, pursuing their onward course, sometimes across arms of the sea, and in most cases over rivers, mountains and forests, into far distant countries. It is equally surprising, that many of them, commencing their migrations in summer, should thus anticipate the cold; while others return from southern climes, before the snows of the north have disappeared, and whilst winter still "lingers in the lap of spring."

Among animals and birds, we often discover a train of actions, all adapted to produce a certain effect, by the agency of certain means, without the exhibition of any part of a regular chain of thought, the essential characteristic of reason; this substitute for reason is called instinct, a term which has given rise to many unsatisfactory theories. I shall, therefore, pass them over with a few brief remarks, on the difference between instinct and reason.

When certain species of birds, at their first season of breeding, being without experience, build all their nests alike, both in form and materials, this may be called the result of instinct. On the other hand, when man guards against danger, or makes provision for the wants of life, or seeks relief from diseases, by the application of medicines, he acts from reason, because he is instructed by the experience of the past. When birds, at certain seasons of the year, change the climate, in anticipation of cold or heat, they act from instinct, because, to many of them, it is their first migration; and as they often migrate singly, and not in flocks, in such cases no experience can aid them. On the other hand, when man makes provision for the changes of seasons and climates, he acts from reason, and is instructed by his own experience, or the experience of others.

Whatever difficulties there may be, in accounting for that mysterious principle in birds, called instinct, and which induces them, at certain seasons, to change their abode, and again, after an interval of six months, to return to the neighborhood where, the year before, they reared their young; the facts of these migrations are incontrovertible, and the reasons why they take place are becoming more and more apparent.

Those birds that migrate, are, from the very structure of their bodies admirably adapted to rapid and continued flight. Their feathers are so light, that they float in the atmosphere, for many hours, with very little artificial support. The tubes of these feathers are hollow; the bones are specifically lighter than those of quadrupeds; the bones, also, are hollow, and instead of marrow, are filled with air. They are furnished with lungs of an unusually large size, adhering to the ribs, and provided with aerial cells, insinuating themselves into the abdomen. These, added to the great length and strength of wing, enable them, with ease and rapidity, to navigate the air—to elevate themselves above the clouds, and pass from one country and climate to another.

We perceive, then, from the very structure of birds, that they are admirably formed for rapid flight and migration. From a variety of accurate experiments, which have been made, at different periods, it appears, that the Hawk, the Wild Pigeon, (*Columba migratoria*,) and several species of wild ducks, fly at the rate of a mile in a minute and a half; this is at the rate of forty miles an hour, four hundred and eighty between the rising and setting of the sun, and nine hundred and sixty miles in twenty four hours. This would enable birds to pass from Charleston to our distant northern settlements in a single day, and this easily accounts for the circumstance, that geese, ducks, and pigeons, have been taken in the northern and eastern states, with undigested rice in their crops, which must have been picked up in the rice fields of Carolina or Georgia but the day before.* There is a well attested account of a falcon from the Canary Islands, sent to the Duke of Lerma, which returned from Andalusia to the Island of Teneriffe in sixteen hours, which is a passage of seven hundred and fifty miles. The story of the falcon of Henry the second is well known, which, pursuing with eagerness, one of the small species of bustards at Fontainbleau, was taken, the following day, at Malta, and recognized by the ring which she bore. Swallows fly at the rate of a mile in a minute, which would be one thousand four hundred and forty miles in twenty four hours. That many birds continue their migrations by night, as well as by day, and are thus enabled to make an additional progress, may be easily ascertained from their notes, which, in autumn and spring, the seasons of their migration, we often hear by night. The cries of geese,

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* I had an opportunity, several years ago, in the state of New York, of examining the contents of the craws of several pigeons, taken from the same flock, which were pronounced by the country people to be rice. It proved, however, to be a different grain, the wild rice of the western lakes, (*Zizania aquatica*.)

cranes, and some species of land birds, are distinctly heard, and others fly silently. Wild pigeons are frequently seen, at early dawn, in the higher atmosphere. They fly higher by night than by day, and thus experience less inconvenience from darkness. The great Hooping Crane scarcely ever pauses in his migrations, to rest, in the middle states. I have heard his hoarse notes as he was passing over the highest mountains of the Alleghany; but he was always too high to be seen by the naked eye. This bird seems to take wing from his usual winter retreats in the south, ascends into the higher regions of the air, and scarcely halts, until he arrives at his breeding places, in or near the polar regions.

There are very few birds that do not migrate, either on account of food or climate. The observations of Captains Parry and Franklin, of Dr. Richardson and their associates, who wintered in the polar regions, prove, that birds which never visit temperate climates and which naturalists formerly supposed were wholly confined to the arctic circle, leave the intensely cold regions of the north in winter and migrate southerly to the distance of many hundred miles.—These adventurous explorers of the polar regions speak of the dreariness and desolation of these countries in the winter and the almost total absence of animal life. During the whole winter, spent at Melville Island, a pair of ravens, (*Corvus Corax,*) alone were seen and these they state had frequently a white ring around their necks, "caused by the accumulated encrustments of their own breath and giving them a very singular appearance." The snow Buntings, (*Emberiza nivalis,*) the Ptarmigan, (*Tetrao Lagopus,*) and two other species of Artic Grouse, (*Tetrao Salicti* and *T. rupestris,*) were their earliest visitants in the spring; and these birds are in Europe and in the fartherest northern settlements of our continent found only in the coldest winters, and on the highest mountains; still we perceive that even they find limits beyond which they cannot live in winter.

Birds migrate, either to avoid the cold of winter, or to find more congenial, or more abundant food, and I am induced to believe that in general, the latter is a stronger principle than the former. The small number that remain amidst the snows of the north are either carnivorous, such as a few of the Owls and Hawks, the Ravens. (*Corvus Corax,*) the Canada Jay, (*Corvus Canadensis,*) and the northern Shrike, (*Lanius borealis,*). These pick up a scanty subsistence by feeding on a few of the smaller birds that remain, or by following the hunters and the wolves, and supporting life by picking the bones of the animals which they have left. Or they are composed of those birds that feed on the buds of trees, such as the Grouse, that live on the buds of the birch, (*Betula*,) Poplar, (*Populus*,) and several species of willow, (*Salix*.) Or those that feed on the seeds of the pine and spruce, (*Abies*,) as the Crossbills, (*Curvirostra*,) and pine Grosbeaks, (*Phorhula enucliator*,) or they are birds that are able to find subsistence on the seeds of plants that are protruded above the snow, or on the seeds of grass found in the barn yards or haystacks of the farmers, such as a few species of the sparrow. But those immense numbers of birds, that feed on insects and worms all migrate to those countries where they are abundantly supplied with this kind of food. These are the Swallows, (*Hirundo*,) the night Hawks and the Whippoorwill, (*Caprimulgus*,) the Tanagers, the fly catchers, and warblers. To them migration is essential to the support of life. The insects at that season disappear, the earth is bound in frost, or covered over with snow, and all the means of subsistence are removed; but long ere this these lively tenants of the air, have obeyed the impulse of some mysterious instinct, and have migrated to more congenial climes. To these may be added all the birds that obtain food from the muddy and moist places of the earth, such as the different species of Curlew, (*Numenius*,) the Snipes, (*Scolopax*,) and the sand birds, (*Tringa*,) as well as those ducks that obtain subsistence from fresh water ponds and rivers; these finding the swamps, brooks, and shores frozen over, migrate from the north to milder regions where they can procure suitable food.

Those birds that migrate but partially and spend their winters in the northern states, though in a milder temperature than their places of summer retreat, such as the eagles, hawks, owls, and grouse, are enveloped in a warm thick, and downy plumage, which in most of the species extends even over the legs and toes. Other birds are exposed to the water, as well as the cold, such as some species of wild ducks, (*Anas*,) gulls, (*Larus*,) petrels, (*Procellaria*,) and puffins, (*Puffinus*.) These, gaining a subsistence from the sea, are not obliged to migrate on account of food. In addition to their warmth of covering, which shelter them from the cold, they are supplied with sacs, containing an oleagenous substance with which they regularly lubricate their feathers, thus rendering them imperious to moisture. Whilst floating on the surface of the water, they often draw up their feet beneath their warm covering of down, and thus every part of the body is protected against the influence of the cold. There is another circumstance, with regard to the capacity of birds, to endure cold, which is not generally taken into consideration, it is the high degree of temperature. The temperature of the human body is generally placed at 97 or 98 of Fahrenheit, that of warm blooded, animals two or three degrees higher, and that of birds as high as 106 making a difference of 8 or 9 degrees, between birds and men. A large mass of air penetrates the lungs and all the aerial sacs and canals of the bird, increasing the action of the heart and propelling the tide of circulation with great rapidity. The pulsation in birds follow each other in such quick succession that they can scarcely be counted. The heat of their bodies being much greater than that of animals enables them to bear with ease the rigorous cold in the distant north and in the elevated regions of the air.

Some birds migrate only from one extreme of our union to the other. Thus the many species that go under the name of Sparrows that breed at the North, with the exception of three, the Snow, Bunting, (*Emberiza nivalis,*) the three Sparrows, (*Fringilla arborea,*) and the white crowned bunting,* (*Fringilla Leucophrys,*) spend their winters in tens of thousands in Carolina. When the meadow Lark, (*Sturnus Ludovicianus,*) and the brown Lark, (*Anthus spinoletta,*) find the snows of the north covering the earth, and hiding their favorite food, they retreat before it and seek sustenance in our Southern states. Other families of birds, such as feed on ripe berries, that abound in the winter also remain with us; these are the Robins, (*Turdus migratorius,*) the wax bird, (*Bombycilla Americana,*) and the blue bird, (*Saxicola sialis,*) which feed on the berries of the Tupelo, (*Nyssa aquatica,*) the Holly, (*Ilex opaca,*) the Cassena, (*Ilex cassina,*) and the small black and red berries of several species of Smilax and Prinos. The yellow crowned Warbler, (*Sylvia coronata,*) is the only Sylvia out of fifty species inhabiting the U. States, that remain with us in the winter, and even this bird could not find a subsistence among us were it not that it almost changes its nature in winter and lives on the fruit of the wild myrtle, (*Myrica cerifera.*) This is also the case with the only fly catcher, that winters in Carolina, the Pewee, (*Muscicapa fusca,*) which sometimes fattens on the seeds of our imported tallow tree, (*Styllingia cerifera.*)

* It has been commonly believed, that this very rare species which breed at Labrador, does not migrate far to the south in winter. It however passes through Carolina early in autumn, and winters farther south.

It is doubtful whether there are any birds that never migrate during the changes of the season. Hawks and Crows are infinitely more abundant in the north during summer, than in winter; the greatest number of them retreat southerly; those of the south may at the same time proceed still farther toward the Equinox. Our cardinal Grosbeaks, (*Fringilla cardinalis,*) are found in New Jersey during summer, and are abundant in Virginia, hence the name of Virginia nightingale and yet during winter very few remain in those states. In the mean time as our number of birds, of this species does not increase, it is very probable that those which have been raised among us, remove still father to the south. As our summer birds, such as the blue Grosbeak, (*Fringilla cœrulea,*) painted bunting, (*Fringilla ciris,*) and our warblers and fly catchers, abandon us towards the close of autumn, we receive at the same time fresh supplies of feathered hordes from Canada and the northern portions of the United States. Many of these remain in our mild climate of Carolina, during the whole winter. Some of them such as the Fox colored sparrow, (*Fringilla iliaca,*) the Siskin, (*F. Pinus,*) the Purple Finch, (*F. purpurea,*) and the Woodcock, (*Scolopax minor,*) only approached our southern climates in proportion as they are pursued by the cold. These seem to beg their subsistence on their passage, and linger among us no longer than their necessities require.

When our winter birds return to their breeding places in the north, they are in the early period of spring replaced by analogous species from the tropics, which resort to South Carolina and principally along our maritime districts, to engage in the affections and cares of reproduction. Of the many species of northern hawks, the red shouldered, (*Falco lineatus,* Wilson,) one of the most common species in the United States, is the only one that remains in our low country during summer. In the mean time, several interesting species from the south, arrive among us of gentler and less destructive habits, feeding principally on insects and lizards. The beautiful swallow-tailed hawk, (*Falco furcatus,*) a Mexican species which seems to be ever on the wing, builds its nest on the highest trees of our forests. The Mississippi Kite, (*Falco plumbeus,* Gmel.) with similar habits and also feeding whilst on the wing, is found occasionally in groups of four or five, soaring high in the air. This bird is so gentle when not on the wing, that it generally suffers you to walk under the tree without being disturbed. The black winged Hawk, (*Falco dispar,* Temm.) is another of our visitors. It bears so strong a resemblance to an Asiatic species, (*Falco melanopterus*, Daud.) that, although it is described under another name, I have never been able to detect the slightest difference. It is occasionally met with as early as the beginning of February and breeds on a few of our islands along our sea-board. This species it has hitherto been supposed never migrated north of Florida. When the Gannets, (*Sula bassana*, Lacep.) leave us for their northern rocks, we are visited by the two species of Pelicans, (*Pelicanus onocrotalus*, and *P. fuscus*, L.) and by immense flocks of the wood Ibis, (*Tantalus Loculator*, L.) The latter commence regular systematic attacks upon our rice fields and on the fish in our ponds, first muddying the water and then killing ten times as many as they can consume, leaving a rich repast for the alligator. As strange as it may appear in birds so large and numerous, their nests have never been found. No sooner do the Virginia Rail, (*Rallus Virginianus*, L.) and the Sora leave us, than their place is supplied by two species of kindred genera, the purple and common Gallinules, (*Gallinula martinica*, Gmel. *G. Chloropus*, Lath.) The latter is found breeding in nearly all the back waters of our rice fields; the former is seen but sparingly, and the large family of northern finches is succeeded by several interesting species, among the most beautiful of which are the Nonpareil, or painted Bunting and the blue Grosbeak. Thus by a wise benevolent provision of providence, the varying seasons bring along with them a succession of the feathered tribe, that either contribute to our sustenance or minister to our pleasures.

Whilst some of our northern birds make Carolina their southern limit in the winter, there are others that make it their northern boundary, beyond which they dare not go at that season. Thus the Cat bird, (*Turdus Felivox*,) the white eyed flycatcher, (*Muscicapa Cantabris*,) the green Swallow, (*Hirundo bicolor*,) and several other species appear among us in small numbers after one or two warm days in winter. A few of these linger along our sea-board in sheltered situations during the winter, and they are found in great abundance through the whole of that season in Florida and Mexico. The whole crane and heron family, (the latter composed of twelve American species,) all spend their winters south of Carolina, with the exception of a few stragglers, from among the great blue heron, a very small number of the white heron, and a few of the young of the hooping crane; yet all of these species of birds are numerous in Florida in winter.*

Many birds make occasional and partial migrations only, to procure a supply of food. Thus, the common partridge, (*Perdix Virginiana*,) in seasons when there is a scarcity of grain in New Jersey, crosses the Delaware River, into Pennsylvania. The same has been observed along the Susquehanna and Hudson. The flight of these birds is so heavy, that they are seldom able to reach the opposite shores on the wing, but drop into the water, when they are weary, and swim across. This is also the case with that most delicious of all birds, the wild turkey. Along the Ohio, Missouri, and Mississippi rivers, numbers of these, in the seasons of a scarcity of their accustomed food, cross those rivers, partly by flying, and then by swimming, and in their wet and exhausted state are taken in great numbers, either in the rivers, or as they arrive on the opposite shores. The wild pigeon is another of those birds, that are supposed to be driven among us only by the extreme cold of the north. This is a mistake. These birds appear in Carolina, only at very long and uncertain intervals. Sometimes they visit us in cold, but frequently in mild winters. I have seen wild pigeons, in immense flocks, in Canada, in the coldest winters, when the thermometer was below zero. It is to be remarked, that the previous autumn had produced an abundance of beech nuts and buckwheat, their favorite food, and

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* The following herons breed in Carolina, and all of them in communities with the exception of the least bittern, (*Ardea exilis*,) a rare species, which conceals its nest among the rushes in fresh water ponds, where it deposits three nearly white eggs. Great heron, (*Ardea Herodias.*) Great white heron, (*A. liue*, Tem.) Snowy heron, (*A. candidissima.*) Louisiana heron, (*A. Ludoviciana.*) Yellow crowned heron, (*A. violacea.*) Night heron, (*A. nocticorax.*) Blue crane or heron, (*A. coerulea.*) The young of this species, are white until they are two years old. Green heron, (*A. virescens*,) and least bittern, (*A. exilis.*) The American bittern, (*A. minor*,) remains in our marshes during the spring, until about the 12th of May, when it retires to its breeding places in the farthest north. The *Ardea Pealii* of Bonaparte, as has been ascertained by Audubon, is the young of the *Ardea rufescens* of Buffon. Having had living specimens in my possession for some time, I am enabled to state, that the downy feathers of the young, whilst in the nest, are brown—the birds then continue white until the second year, when they assume a rufescent color. They are found breeding in great numbers on the islands of the southern extremity of Florida. In the same places are found also, the newly discovered heron, (the largest of all our American species,) which Audubon describes under the name of *Ardea occidentalis*. The brown crane, (*Grus Canadensis*, of Temm. and Bonaparte,) is undoubtedly the young of the great hooping crane, as I have ascertained in a pair kept in confinement, which either in the second or third year of their age assumed the form and plumage of the adult bird, the *Grus Americana.*

that the ground was not covered with snow. It is only when the forests of the west have failed in their usual supply of mast and berries, that the wild pigeons come among us, to claim a share of the acorns and berries of our woods, and the refuse grains scattered over our rice fields.

It is, perhaps, not improbable, that the occasional changes in the migrations of the birds of our continent, may, in the course of time, introduce among us some species of birds from the south and west, that are not now found here. A large number of the feathered race follow the improvements of civilized man. No sooner does cultivation commence, than many birds, which were unknown in the forest around him, are seen in his fields and orchards. A new species of grain attracts the graminivorous bird—a particular plant or tree, on which certain caterpillars or insects feed, invites the Sylvias, Vireas, and Muscicapas; and the tubular flowered plants of the West Indies, transplanted into the soil of Florida, are already beginning to attract some of the many species of humming birds of the south. In the days of Wilson, (one of the most observing of our American ornithologists,) the great Carolina Wren, (*Troglodytes Ludovicianus,*) and the pine creeping warbler, (*Sylvia Pinus,*) together with several other species that are now common in the northern states, (where I sought for them for many years in vain,) were there unknown. They have now extended their summer migrations, as far north, at least, as Boston. The cliff swallow, (*Hirundo Lunifrons*, Say,) a Mexican species, was first seen on the banks of the Ohio, in 1815. These birds excited much interest, from the peculiar structure of their nests, built of mud, and clustered together, resembling a bunch of gourds. From year to year, they continued to increase and advance eastwardly in their migrations, until they have now extended across the continent, as far as Canada and Maine. The olive-sided flycatcher, (*Muscicapa Cooperii*, Nutt.) has but recently made its appearance in the north, and on the mountains of Virginia; and in the latter situations, the newly described Bewick Wren of Audubon, (*Troglodytes Bewickii,*) has supplanted all the other species of that genns. The fork-tailed flycatcher, (*Muscicapa savana*, Bonap.) has, only within a few years, commenced leaving the tropical wilds of Guiana, and a few stray birds of that species are almost annually seen in the middle states. The solitary flycatcher, (*Vireo solitarius*, Vieill.) which was so rare with us ten or twelve years ago, that scarcely a bird of that species could be found in a year, has of late become so abundant, that in the month of February five or six can be counted in particular situations, near our city, in a single day, and their sweet notes form a considerable addition to the concerts of our feathered choir. The orange crowned warbler, (*Sylvia celata*, Say,) so long confined to the far west and the orange groves of Florida, has become equally common in our immediate neighborhood. The pectoral sandpiper, (*Pelidna pectoralis*, Say,) and the long-legged sandpiper,* (*Tringa himantopus*, Bonap.) which were formerly so exceedingly rare, that Wilson knew nothing of their existence, are now found every summer, in small numbers, along our sea coast. It may not be unworthy of remark, in this place, and in confirmation of the views now advanced, that no less than eight or nine species of birds, either wholly undescribed or not previously known to exist in the United States, have recently been discovered in the neighborhood of this city. A few of these may have long existed in the country, and escaped the researches of former naturalists, but I am under an impression that some may have but recently come among us.† From these facts, we may easily perceive, that after all the additions that have been made to our American ornithology, by Wilson, Bonaparte, Cooper, Nuttall, Richardson, and especially the indefatigable Audubon, the field still remains open to the investigation of the student of nature, and promises a rich reward.

There is one singularity in the migration of American birds that is as yet involved in some obscurity. A vast number of northern warblers and fly catchers do not pass over the low countries of Carolina in their migrations and the closest observers have not been able to find a single specimen of many species that are abundant in the north, and that all migrate southerly in autumn. It is possible that migratory birds pass southerly in two immense channels, one leading from Hatteras, or some of our capes a little farther south, and then across the gulf of Mexico to the West India Islands where, they spend the period of our winter in immense numbers. They

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* From specimens, in various stages of plumage, which I possess, of the long legged sandpiper, I am disposed to believe, that Swainson and Richardson, in their Fauna Boreali-Americana, have been deceived by the variations in the plumage and size to which this bird is subject, and have described it three times under the names of *Tringa himantopus*, *T. Audubonii*, and *T. Douglassii.*

† Some of these birds have since been figured and described by Audubon, under the following names: *Muscicapa dominicensis*, Briss. *Parus Carolinensis*, Aud. *Fringilla Bachmanii*, Aud. *Fringilla Macgillivraii*, Aud. *Sylvia Swainsonii*, Aud. *Sylvia Bachmanii*, Aud. *Rallus elegans*, Aud.

are often met at sea during the period of their migration and are frequently known to alight on the rigging of vessels, where they rest, for an hour or two, and then again pursue their onward course. The other path of migration and probably the most common, to which I refer, is along the Alleghany mountains which extend through the whole interior of our country. They vary occasionally in their flight so as to follow not only the range of mountains, but the courses of rivers. In these views I am supported by Audubon and Nuttall, and they are strengthened by the fact that the Rose-breasted Grosbeak, the Baltimore Oriole, the Scarlet Tanager and a number of species of warblers that seldom visit the maritime districts of Carolina are found to pass along our mountains and from thence through the states of Louisiana, Mississippi, and Arkansas. Some of these birds remain in Mexico; some enter within the Tropics, and others possibly pass beyond them in order to find a climate similar to that which they have left.

It has recently been ascertained that some of the birds that are found in the north of Europe and have hitherto not been known to exist in America, migrate from the polar regions, along the Rocky mountains sometimes as far south as Mexico and in their spring migrations return by the same route. The Magpie, (*Corvus Pica,*) and the Bohemian wax-wing, (*Bombycilla garrula,*) and a few others, are of that number. Several other birds, peculiar to the American continent, never visit the cultivated districts of the United States, but take the same course in their annual migrations; among these are the black water-ousel, (*Cinclus Pallassii,* Tem.) the evening Grosbeak, (*Fringilla Cooperi,*) Clarke's Crow, (*Corvus columbarius,*) and the Columbia Jay, a most splendid bird, figured by Anderson, rivalling in beauty the bird of Paradise. The spotted thrush of Latham, (*Turdus nævius,*) the arctic Blue-bird, (*Erythaca arctica,* Swain.,) the *Emberiza picta,* Swain., and the saffron headed Troophial, (*Icterus xanthocephalus,* Bon.) are also of this number. Those birds only, that breed in the arctic circle visit both Continents. It is computed that out of almost four hundred and fifty species already known in North America, only twenty seven land, and eighty one* water birds are natives of both continents; conse-

* This number has been considerably increased since the publication of Richardson and Swainson's Fauna Boreali-Americana, in which it is to be feared the mania for adding new species of birds has been indulged to a considerable extent.

quently three hundred and forty two species are peculiar to our own continent. The land birds that visit both continents are composed of Eagles, Hawks, Owls, and most of the genus corvus, and a few other species possessing great strength of wing, and warmth of covering, enabling them to migrate with ease, and to bear the rigors of the polar regions. The water birds are either composed of Ducks, which breeding far north, are enabled to reach the regions of Norway and Russia, and visit the shores of Europe, or they are of the Gull, Tern, and Petrel, species which find sustenance every where on the bosom of the ocean, and may therefore, with great facility, navigate the widest seas. Still, it will be observed, that the number of birds that migrate from one continent to another, is very small, and I am under an impression, that these migrations take place but seldom. Such is also the case with our animals of which very few are found on the eastern continent. In fact, our whole kingdom of nature not even excepting the insects and plants, presents peculiarities which well entitle it to the name given it, by its first discoverers of "the new World."

Whether many of our migratory birds that leave us early in the season, may not pass beyond the tropics, and retire to latitudes in the southern hemisphere, of the same temperature with that which they left, is a subject that remains for the investigation of future naturalists. Why may they not take advantage of the reversion of the seasons, and rear a second brood in South America? The purple martin which is found in our whole country during summer as far as the 60th degree North latitude, is known to breed in South America during our season of winter, and this is also the case with several of our rarest sylvias. Even admitting that our birds do not migrate to the southern hemisphere, it is probable that some of the species, may breed in two distinct portions of North America. The stork, after it leaves Europe, is known to raise another brood in Africa. Audubon found the white headed Eagles, (*Falco leucocephalus,*) and the Fish Hawk, (*F. Haliætos,*) having nests with their young full fledged and able to fly in the month of November in Florida. The Barn Owl, (*Strix flammea,*) sometimes lays its eggs in the unoccupied buildings of this city in November, and I last year had a young bird, of the great horned Owl, (*Strix virginiana,*) sent me on the 3d of December, which had been taken from a nest in this vicinity. Now this is a season when our northern countries are blocked up in frost and snow, and it is not improbable that many of these birds, following the opening of spring, may raise a second brood in the more northern climates.

The Rail, (or Soree as it is called in Virginia) and the swallows have occasioned more speculations and created more superstitious ideas with regard to their winter residence than any other of our American birds. The erroneous opinions with regard to the Rail have probably arisen from the sudden manner in which it appears and disappears in the middle states, and the unphilosophical notions, with regard to the swallows have originated in Europe and from thence been transmitted to our country.

Rails, after having been absent during the whole summer from the middle States, suddenly make their appearance early in August in immense numbers along the Delaware, Schuylkill and James rivers. In a single night, their clamorous voices are heard in tens of thousands, on those reedy shores, where but the day before not one could be found. Here they remain till about the middle of October, when suddenly their well known cackle ceases and in the places where the day before many hundreds were seen, not a solitary one remains. They seem so heavy of flight that they are sometimes taken by hand and hence the oft repeated inquiry whence come and whither goes the Soree. Many believe that these birds, are scarcely capable of flight, and must find some retreat in hollow banks or perhaps under the ice or mud. The truth is they migrate altogether by night, and like the Woodcock, and other kindred species, fly admirably in twilight or in the dark. They breed very far north. An intelligent Indian trader informed me that he had found great numbers of their nests, whilst hunting for the egg of the wild goose, (*Anser Canadensis*,) along the reedy marshes of the most northern lakes. It is not generally known that when they leave the middle states, they appear in the rice fields and marshes of Carolina, where they remain a short time, before they migrate, still farther south and in the spring again visit us, as they are passing on to their northern breeding places. There is then nothing in the migrations of the Rail that cannot be accounted for on the principles of nature.

All the absurd theories with regard to the hibernation of Swallows have originated from the habits of a few species common to our country and to Europe. The chimney swallow of Europe, (*Hirundo rustica*,) resembling our barn swallow, (*Hirundo rufa*,) in every thing but its habits of building in chimnies so perfectly, that they cannot be distinguished from each other, and the bank swallow, (*Hirundo riparia*,) which is a native of both continents, and our chimney swallow, (*Cypselus pelasgius*, Temm.) have occasionally been found in holes on the banks of rivers, in the hollows of decayed trees, or in the recesses of old buildings, clinging together sometimes in great numbers, nearly in a torpid state. Hence it was asserted that these were their winter retreats and that here they remained in a state of torpidity, from the cold of autumn, to the sunny days of spring. This doctrine has been espoused by a number of intelligent naturalists of Europe from the amiable and observing White of Selborne, even to the great Cuvier, who makes use of the following language. "Some birds retire into remote places, to some desart cave, some savage rock, or ancient fortress." He evidently had no opportunities for a satisfactory examination. Dr. Good has also asserted of the chaffinch of Sweden, (*Fringilla coelebs*,) that many of the males indulge in a profound sleep in Sweden whilst the females migrate to Holland towards the winter and duly return to them in the spring.

From dissection, (the details of which it is unnecessary to give here) it has been ascertained that from the internal structure of swallows, and the same may be said of all birds, it is impossible for them to live beyond a day or two in a torpid state. In this declaration I am supported by the investigations of the celebrated John Hunter. I have seen the American chimney swallow as well as the rail placed under the water to try the experiment whether they could exist in that element, and they have invariably been drowned in a few minutes and no warmth or even electricity could afterwards revive them. The habits of swallows drinking from brooks and rivers, while they are on the wing and of their picking up flies and insects, whilst skimming the surface of the water, has no doubt given rise to the deceptions in persons, supposing that they had seen them going under the water as a winter retreat. When birds of this species have been found in nearly a dormant state, it was either in the autumn or early in the spring, generally the latter. These are the seasons of their migration. At night they sought those retreats, as usual, to sleep; here, they were overtaken by a cold change in the atmosphere, and here they would have died in a very short time, if the weather had not become milder. These birds have, I apprehend, never been found in this situation in winter. Besides, our senses can satisfy us where the swallows spend their winters. Of the six species of swallows that inhabit the United States, all of them but the cliff swallow which has but recently made its appearance in the country are seen in thousands performing their annual migrations, along the environs, and even the very streets of our city. The green swallow, (*Hirundo bicolor*,) is found in Florida, during the coldest weather of that country, and was during the last winter (1832) seen every day with the exception of about two weeks, in considerable numbers in the neighborhood of Charleston. The barn swallow and purple martin, (*H. purpurea*,) leave us earlier and return later, the chimney swallow follows last in the train on its return from the south as it is the first to leave us in autumn. Thus we perceive that there is nothing mysterious, nothing unnatural in the migration of the swallows.

When the period of migration arrives, birds evince an uncontrolable restlessness of disposition, as if conscious that an important undertaking, was at hand. The Snow and Canada Geese, (*Anser hyperborea* and *A. Canadensis*,) which I have had for some years in a state of domestication, (although in other respects perfectly tame) make constant efforts, on the return of every spring, to obey the impulse of nature, and take their departure for the north. Although a joint from a wing of each has been removed, yet they make attempts at flying, and when at this season they are enabled to escape from their enclosure, they hurry off in a northern direction, as if determined to make their long journey on foot. Wilson gives a well authenticated anecdote of a female wild goose having been domesticated by Mr. Platt of Long Island, which, after flying off on the following spring, returned in the autumn with three of its comrades or young and the birds were all living several years afterwards. I have preserved in an aviary, robins, finches and orioles that had been procured when young at the north, and no sooner did the spring (the time of migration) arrive, than they exhibited by their constant fluttering a disposition to escape and the moment this was effected they flew off not to the south or west but as directly in the line of migration as if guided by a compass. These are facts of which the humblest individual may inform himself, but which neither our wisdom or philosophy can explain.

The manner in which birds perform their migrations is also deserving of notice. At the approach of autumn, when the cold is beginning to drive the insects to their winter retreats, when the earth begins every where to present the image of desolation and death, when many terrestrial animals are preparing for themselves a shelter from the cold, it is then and sometimes a few weeks earlier (as if in anticipation of this season) that birds assemble, in troops, to set out on their annual aerial voyage to southern climes. The young in most species instinctively flock together as if disdaining to enquire the path of migration from the old. Some taught by an instinct of nature, which way to bend their course depart singly, and make their long and weary journey alone, others go in straggling flocks, sometimes you see the air almost darkened with the swallows and night hawks, (*Caprimulgus Virginianus,*) other species crowd into close columns during their flight. This is particularly the case with the wild pigeons, wax birds, (*Bombycilla Carolinensis,*) black birds, (*Icterus Phœnicus,*) the cow bunting, (*I. Pecoris,*) the wild geese, ducks and several species of Tringas or sand birds. Some species move slowly and seem only urged along either by the cold or by a scarcity of their accustomed food. Others pass rapidly and effect their migration in a very few days. Some flit along the earth's surface and rest, here and there, as if to take a glance at the fields, gardens and habitations of man, whilst others mount high in the air and soar almost among the clouds as if scarcely deigning to cast an eye on the cities and villages and the puny efforts of their inhabitants and on the mountains and vallies beneath them. These aerial voyagers, by an admirable instinct, seize upon a favorable moment in which winds and the weather are fitted for these migrations; they are not carried along by the wind, but are obliged from the construction of their feathers to fly against it. They have a foreknowledge of frosts and snows for weeks before they arrive and they have a mysterious but sure monitor within them to tell them the coming of spring. They require no chart and no compass to enable them to navigate the air and pass through the region of clouds, the thunder and the storm. They arrive at the end of their destined voyage, and there in the grove, the forest, the mountain, the field or the garden, they find food, shelter and a home prepared by the hand of providence; there in all probability, they revisit the very neighbourhood and probably build their nests in or near the same tree, or bush, or tuft of grass in which the year before they reared their young. This too may have been the scene of their infancy and here they may have carroled their earliest song. The disposition of birds to revisit, annually, the place where they have once bred is remarkable. A blue bird that was marked so as to be known, built its nest, for ten successive years, in a box that had been prepared for the purple martin. A pewee, (*Muscicapa fusca,*) has been known to revisit the same cave for nine successive years. A robin, bred for a still longer time in the same apple tree, and a red tailed hawk, (*Falco borealis,*) which is distinguished from all others of the species on account of its plumage having accidentally become white, has for the last twelve winters, kept possession of a dead pine in an old field in Colleton district, (South Carolina.)

Whilst many species of birds perform their migrations during the day a great number travel by night. The lover of nature who in the seasons of the migrations of birds, sees flock after flock passing over his head, all day long, or witnesses the wrens, blue birds and creepers just stopping for a few moments to seize a worm or insect and then as if impelled by destiny, rising again on the wing and urging onwards; has also the evidence that many pass over him at night. He hears unusual sounds in the air. The single sharp note of the rice bird repeated all around him is succeeded by the crake of the snipe resembling the grating of a wheel repeated at long intervals, and the woodcock, (*Scolopax minor,*) wheels around him uttering notes like the loud tickings of a watch so rapidly repeated that they cannot be counted. He ascends higher and still higher in the air like the lark of Europe till he seems to have risen above the clouds, when suddenly his voice is hushed and in zigzag lines he descends rapidly to the earth and alights near the same spot from whence he arose. This is repeated for several successive evenings and at early dawn till suddenly, he commences his annual migration and is seen no more. The yellow crowned and the night herons utter their hoarse croak as they pass high and rapidly on, and at a still greater distance like unearthly sounds, is heard the not unmusical cry of the Canada goose. In the mean time the rails, owls, thrushes, warblers, and many other birds glide silently by him like spirits of the air; and without being superstitious, there comes over him a sensation of mingled admiration and fear and he feels the truth of the language of inspiration. "Great and marvellous are thy works Lord God Almighty."

The arrival and departure of birds affords a pretty sure indication of the state of the weather and the advance of the seasons. Living constantly in the air and exposed to all its variations, they become, either from instinct or habit, acquainted with the changes of the atmosphere, with the winds, the weather and the seasons. Captain Parry and Dr. Richardson inform us of the anxiety with which the northern Indians watched the approach of the first bird, the harbinger of spring. On the 12th of April, says Dr. Richardson, the arrival of the swans, geese and ducks gave certain indications of the return of spring. On the 14th a robin appeared; this bird is considered by the natives as an infallible precursor of warm weather;" and Capt. Parry says " the snow bunting was the first precursor of spring that appeared. When the well known notes of the whip-poorwill, (*Caprimulgus vociferus*, and *C. Carolinensis*,) are heard, the farmer is reminded that the time for the planting of corn is at hand. The fish hawk's return to the rivers of the north is regarded by the fisherman as a proof that the season for the taking of shad has arrived. When the swallow appears, the danger of frost is believed to be over; and if the Cuckoo of Europe is hailed by the old and the young as an evidence of the return of spring, and if we have in common with them admired the beautiful sentiment of the poet,

> " Sweet bird thy bower is ever green,
> Thy sky is ever clear;
> Thou hast no sorrow in thy song,
> No winter in thy year,"

the inhabitants of the middle and northern states of our country feel equally interested and pleased when they hear the soft and melodious note of the blue bird, the robin and the wood thrush, (*Turdus mustelinus*,) reminding them that " the winter is past and gone and that the time of the singing of birds has come."

Previous to a storm, the birds give indications of its approach. Our vultures, in great numbers, rise in circles till they are almost lost in the region of the clouds, the stormy petrels, (*Thlassidroma Wilsonii*, Bon.) crowd in great numbers around vessels and follow in their wake as if seeking the protection of man, the sea gulls and terns make the shores reecho with their hoarse clamorous notes, the loon, (*Colymbus glacialis*,) is excessively restless and his screams are heard at the distance of more than a mile and the barred owl, (*Strix nebulosa*,) utters his funeral cries even in the day. But when fine weather is about to return the whole scene is changed, and every hedge and copse and grove is rendered vocal and the whole feathered tribe seem to rejoice at the prospect of the cessation of the storm and the anticipation of bright skies and sunny days.

But although our subject is far from being exhausted I am admonished that it is time to bring these desultory remarks to a close. If I shall have fortunately succeeded in throwing even a ray of light on that which has hitherto appeared mysterious in nature; or if I have been enabled to awaken in a single mind a sentiment of admiration and gratitude to that superintending providence who teaches "the stork in the heavens to know her appointed time, and the turtle and the crane and the swallow to know the time of their coming." I shall be doubly recompensed for those pleasing studies of nature which have enabled me to offer these remarks.

The farther we pursue this subject the more we shall be convinced that there is a wise arrangement in nature which governs instinct and action and creates being and beauty and happiness. The laws by which the whole system of nature is governed are equally simple and majestic and are equally visible in the minutest as well as in the most stupendous of Gods works. From the beauty and harmony of that system of nature by which we are surrounded, the mind is insensibly led to admire and adore that mighty cause the fountain of wisdom and perfection, who though unseen, is ever present who is "the source of all matter and mind and modes of existence."

The temple of nature, wide and wonderful as it is stands ever open, inviting the ignorant as well as the wise to enter and learn those lessons which are calculated not only to enlighten the mind but to improve the heart, and the chief object of science and Philosophy should be to lead us to the Altar of the benevolent Author of all things, and to make our experience and knowledge subservient to his grand designs.

Art. X.—*Chemical examination of the water of the Gray Sulphur Springs of Virginia; by* Charles Upham Shepard, Professor of Chemistry in the Medical College of the State of South Carolina.

In consequence of an opportunity afforded me by a gentleman of this city, I have been led to a chemical investigation of the water of the Gray Sulphur springs of Virginia; and having recognised several ingredients in their constitution not before detected, I am induced to believe that a brief sketch of my examination will not be unacceptable to some of the readers of this Journal. Not having visited these springs, I must premise, that the topographical and other details, aside from the chemical research, are derived from the person who placed the water at my disposal.

The Gray Sulphur Springs are situated among the spurs of that portion of the Alleghany mountains which passes through Virginia on the borders of Munroe and Giles counties. They are nine miles from the Red Sulphur, and twenty miles from the Salt Sulphur, springs. The surrounding country, is much broken by hills and vallies, while at a distance of only two miles, rises the lofty and continuous range of Peters mountains. Between three and four miles from the spring, New River breaks through the chain, giving rise to several striking views, as well as the most instructive geological sections.

The springs have not been opened for visitors until very recently, though the water had for a long time attracted the attention of the inhabitants of the vicinity. On excavating the rock from which the water issued, about three feet, it was discovered that the supply was derived from a vertical seam, which was bored into, to the depth of five feet. The rock is excessively hard, yielding with the utmost difficulty to the mattock; and is commonly called in that region a *slate rock.* A wooden box is inserted into the excavation, to serve as a reservoir for the water. The medical qualities of this spring, have gained for it the name of the Anti-dyspeptic Spring.

The water is in general extremely clear, with a temperature of about 67°. Aerial globules line the sides of the wooden box, and occasionally large bubbles of gas ascend from the bottom through the water, and break at its surface.

"There is one thing" says my informant "remarkable in the appearance of this water, when the weather is cold; at least I have never noticed it during summer, and only after the cool weather; but my attention was not drawn to it early enough to ascertain positively whether it invariably followed cold nights, though I am inclined to think it does. It is this. Early in the morning, before it has been disturbed, a bluish gray precipitate is seen floating in distinct veins throughout the water, resembling more nearly blue smoke, floating in the atmosphere after a shower, than any thing else to which I can compare it. On being disturbed, the whole of the water becomes clouded." He adds that when the water becomes thus troubled, "the smell of sulphuretted hydrogen, entirely disappears from the water near the surface; but water taken from the bottom, where it issues from the rock, and which I procured by having the receptacle bailed out possesses it as strong as ever. I have to observe also that the precipitate which causes this cloudiness appears to be redissolved, as no deposit is found on the rock, when the water becomes clear again, although it may have remained troubled for many days.

A second excavation was made last autumn, by boring into the rock at a little distance from the well just described; both springs being protected by a common roof. The boring was carried down eighteen feet, and a wooden box of the capacity of about two and a half cubic feet, placed at the top to retain the water. "A very considerable white deposit was soon collected on the bottom of the box. About this time I was taken sick, and did not see the spring for a week. On visiting it, I was surprised to observe that the bottom was apparently covered with stalactites and small globular bodies from one quarter to one and two inches in diameter, and four and five inches in length. These I found to be nothing more than the deposit under which the gas had risen, and which was prevented by the viscous nature of the precipitate from making its escape. Some of these bodies had the appearance of small balloons attached to the rock, and others presented a botryoidal surface. They were beautifully white on the outside, but invariably black on the interior. On being lightly touched, they were detached and rose rapidly to the surface, when the bubble burst and the envelop immediately collapsed and sunk to the bottom. On heating this deposit on a red hot shovel, a blue flame was emitted and a strong sulphurous odor."

Two small vials containing this sediment were submitted along with the water for examination.

1. The water was received in closely corked, and well cemented bottles. When held between the eye and the light, the water appeared perfectly transparent, with the exception of a few blackish flocculi floating near the bottom of the bottle.

2. On withdrawing the cork from a bottle, the odor of hydro-sulphuric acid was very strong; but on being allowed to stand uncorked for a short time, it ceased to emit this smell.

3. A part of the contents of a bottle was poured into a glass retort, having, the capacity of about 16 cubic inches, until the vessel was filled. It was perfectly diaphanous. Half its contents were then poured off, the thumb placed over the mouth of the retort and the vessel inverted and subjected for several minutes, to violent agitation, taking care to hold the glass in a manner not to communicate to it the warmth of the hand. It was obvious from the appearance of the fluid that it held aerial matter, in solution; and on suffering it to become quiet, the orifice was inverted and opened under water. As the retort was so held that a portion of the water was in the bulb of the instrument, while the remainder occupied the part near the

beak, this last portion was seen to descend perceptibly on removing the thumb from the opening, farther showing that the water holds gas in solution.

4. A few ounces of the water were warmed in a flask over a spirit lamp: bubbles of air immediately began to form at the bottom, and to ascend to the surface where they broke with a slight report after the manner of carbonic acid.

5. The specific gravity of the water is 1.003.

6. Its taste, when newly uncorked, is decidedly that of a weak solution of hydro-sulphuric acid, modified by a cooling alcalino-saline flavor; and is far from being unpleasant. On being allowed to stand for a few minutes in an open vessel the hepatic taste disappears.

7. On being evaporated to three quarters its bulk, it afforded a distinct precipitate, and as it approached a state of dryness, it emitted the odor of extractive matter. The residuum was perfectly white, without any signs of crystallization, even when examined with a microscope, and remained exposed to the air for several days without change of color or deliquescence.

8. 31.7 cubic inches of freshly opened water were transferred to a glass flask, to which was fitted a recurved tube leading to a graduated air-jar, over the mercurial cistern. Heat was applied, and the water maintained at a boiling temperature for fifteen minutes, the operation having been so conducted as to prevent the smallest quantity of water from passing over into the jar. The process yielded 1.8 inches of gas; which is 5.67 for one hundred cubic inches of the water, or 13.11 to the gallon.

9. One part of this gas (8) on being poured into a solution of lime produced a cloud, and another portion into a solution of acetate of lead, afforded a brownish precipitate; from whence the existence of carbonic and hydro-sulphuric acid in the water was demonstrated.

10. A portion of the gas (8) was treated with a solution of potassa, whereby it was proved that a part of it was unabsorbable, but the proportion remaining was not ascertained. And since oxygen and hydro-sulphuric acid do not coexist in waters, it follows, that the unabsorbed gas must have been nitrogen.

The water was then subjected to the following examination, in order to ascertain whether the principles successively enumerated, enter into its composition.

A. *For Sulphuric Acid.*—To the water was added chloride of barium ; it produced cloudiness, but became transparent on the addition of nitric acid. The experiment was repeated on water considerably concentrated by evaporation, in which state the cloudiness would not be wholly removed by nitric acid. Sulphuric acid is therefore present.

B. *For Carbonic Acid.*—The boiling of the water produced a precipitate (7) which was soluble with effervescence on the addition of hydrochloric acid. A clear portion of the water evaporated to one quarter its bulk, effervesced perceptibly on dropping into it hydrochloric acid. Hence carbonic acid is an ingredient of the water.

C. *For Nitric Acid.*—To half a pint of the water, whose salts, are decomposed by sulphuric acid as far as they are capable of alteration by this agent, was added hydro-chloric acid, then gold leaf; heat was afterwards applied, and the water kept boiling for one hour. The addition of porto-chloride of tin caused no precipitation. Hence the absence of nitric acid was inferred.

D. *For Boracic Acid.*—A portion of the water was evaporated to dryness the residuum decomposed by sulphuric acid, and alcohol added. A gentle heat was applied and the alcohol inflamed : no green color was perceptible. Hence boracic acid is not present.

E. *For phosphoric acid.*—To a portion of the water evaporated to dryness, redissolved in nitric acid and precipitated by nitrate of silver, ammonia was added, without affording indications of phosphoric acid.

F. *For Silicic Acid.*—A portion of the residuum from evaporation of the water, was heated to redness in a platina crucible, and hydro-chloric acid was affused, which left a distinct silicious precipitate. The water having exhibited, (7) during evaporation, the odor of extractive matter, I was led to test it for the newly discovered acids of Berzelius.

G. *For Apocrenic Acid.*—Six ounces of the water, (from the new spring,) containing a considerable portion of the flocculent matter above described, were evaporated to a gelatinous consistency and treated, at a boiling heat, with a strong solution of potassa. The solution was suffered to become clear and the supernatant liquid decanted. It possessed a slightly brown color. To this fluid, saturated to excess with acetic acid, acetate of copper was added, without occasioning a precipitate, from whence the absence of apocrenic acid was inferred.

H. *For Crenic Acid.*—To the liquid thus treated in G, was added, in excess, the bi-carbonate of ammonia, and afterwards acetate of copper: a green precipitate was thrown down, proving the presence of crenic acid.

I. *For Combined Hydro-Sulphuric Acid.*—The freshly opened water was added to a solution of sulphate of zinc, without occasioning any precipitate; from whence it was inferred that the hydro-sulphuric acid present, exists in a pure state.

J. *For Fluorine.*—To half a pint of the water, was added acetate of lead, in order to convert its carbonates into acetates, so that the fluo-silicic acid, if present, might not be carried off too rapidly along with the carbonic acid gas; the solution was evaporated to dryness and treated in a platina crucible with sulphuric acid,—a moistened evaporating glass being held over it; no indication of hydro-fluoric, or fluo-silicic acid gas was afforded.

K. *For Chlorine.*—To the water was added a solution of nitrate of silver; it instantly produced a brownish precipitate, which was insoluble in nitric acid. Hence chlorine is an ingredient in this water.

L. *For Iodine.*—The residuum from half a pint of the water was transferred to a test tube, and upon it was poured two inches of gelatinous starch: sulphuric acid was carefully suffered to trickle down the side of the tube, so long as any action was observable on the residuum: no change of color was noticed in the starch, and hence iodine is not present.

M. *For Bromine.*—Half a pint of the water was evaporated to one twentieth its bulk, introduced into a vial one inch and three quarters wide, and rendered yellow by passing through it a stream of chlorine. Sulphuric ether was added; the mixture well shaken and suffered to rest. The ether mounted to the top of the fluid, but without exhibiting the characteristic color of bromine.

N. *For Alkaline bases.*—Infusion of purple cabbage was added to the water. In one hour it had changed to green near the surface; and after the expiration of twelve hours, the alkaline reaction was obvious throughout the fluid. Tincture of alkanet was instantly changed to blue.

O. *For Potassa.*—The residuum of the water previously cleared of lime was dissolved in nitric acid, and to the solution, tartaric acid was added: no crystals of the bi-tartrate of potassa made their appearance, hence no potassa is contained in this water.

P. *For Soda.*—Experiments N and O taken together prove the presence of soda.

Q. *For Lithia.*—The residuum from evaporation was treated with excess of carbonate of soda, and heated before the blowpipe on platina foil, without evincing the characteristic indication of this base.

R. *For Baryta and Strontia.*—To the dry residuum from evaporation, hydro-chloric acid was added. The clear solution was treated with a saturated solution of sulphate of lime: no cloudiness was apparent. Hence these bases are not present in the water.

S. *For Lime.*—Oxalate of ammonia produced a precipitate before and after boiling. Lime is therefore present.

T. *For Ammonia.*—The addition of potassa to a hot, concentrated solution of the water, afforded no odor of ammonia.

U. *For Magnesia.*—From the partially evaporated water, the lime was cleared by oxalate of ammonia and to the decanted, supernatant fluid was added bi-carbonate of ammonia and phosphate of soda, without troubling its transparency. Hence no magnesia is present.

V. *For Alumina.*—A portion of the residuum was heated before the blowpipe on charcoal, and moistened by nitrate of cobalt: a bright blue color made its appearance at a single point of the mass, indicative of the presence of alumina.

W. *For Iron.*—Tincture of nutgalls caused no discoloration in the water, even after it was reduced to three quarters its bulk by evaporation. Ferrocyanide of potassium merely produced a yellowish green color after standing many hours. No iron therefore exists in the clear water.

X. *For Manganese.*—The absence of all color in the residuum from evaporation of the water, both before and after ignition, proves the absence not only of iron, but of manganese.

Examination of the flocculent precipitate from the water of the new spring.—It was contained in about three ounces of the water, and occupied the lower third of this fluid, as contained in a five-ounce vial. Its appearance was that of grayish films, intermingled with black particles. On opening the vial, a strong hepatic odor was emitted, accompanied by the smell of extractive matter. The water was much more saline in its taste, than that of the spring which had not been kept in contact with the precipitate. It was examined for the following substances.

a. For Sulphur.—A portion of the precipitate was treated with nitric acid in a flask: a strong odor of hydro-sulphuric acid was emitted, attended by the production of nitrous acid fumes. The clear solution on being decanted, gave a copious precipitate with chloride of barium, thereby evincing the presence of sulphur.

b. For Fluorine.—A portion of the precipitate was introduced into a platina crucible and treated with sulphuric acid as in J without obtaining any evidence of this principle.

c. For Iron.—Water was added to the residuum in *b* and the solution tested with tincture of nutgalls, and also with the ferrocyanide of potassium; with the former it instantly struck a dark ink and with the latter, a blue precipitate. Iron therefore, is an ingredient of the precipitate.

d. For Silicic Acid.—There remained behind after the addition of water in *a* and *c*, a copious siliceous deposit.

e. For Manganese.—A portion of the precipitate from the vial was treated with borax before the blowpipe, but only yielded a glass stained with iron. Therefore, no manganese is present.

The water through which the precipitate was diffused in the vial, was filtered and evaporated in a porcelain capsule. It emitted a distinct odor of extractive matter, and soon gave rise to a pellicle on its surface. The evaporation having been pushed nearly to dryness, it was suffered to cool, when it shot into minute crystals of sulphate of lime, intermingled with those of sulphate of soda; the former preponderating in quantity, and when viewed with a microscope seem to be in slender prisms, crossing each other at 60 and 120°, and forming six-rayed stars.

The striking difference in the results of evaporation between the water from the other bottles, and that in the present case can in no other way be accounted for, than by attributing it to the oxygenization of the sulphur by the atmospheric air in the upper part of the vial; whereby sulphuric acid was formed, which decomposed the carbonates of lime and soda.

The foregoing experiments prove that these springs contain the following ingredients: viz.

Nitrogen,	Crenic acid,
Chlorine,	Soda,
Carbonic acid,	Lime,
Sulphuric acid,	Alumina,
Hydro-sulphuric acid,	Iron,
Silicic acid,	Sulphur.

It next becomes a subject of inquiry in what manner these different principles are combined among themselves.

It cannot be determined whether this water contains carbonic acid, before the quantities of soda and lime are ascertained by analysis. It may be regarded as doubtful however, whether there is any considerable excess of the acid generally present over and above what is necessary to constitute the carbonate of soda a bi-carbonate, and to render the carbonate of lime soluble.

The sulphuric acid, which must be present in very small quantity, is probably engaged with the soda, forming sulphate of soda ; inasmuch as this acid has a stronger affinity for soda than for lime, and as the sulphate of soda is a more soluble salt than sulphate of lime.

The chlorine may be regarded as divided between the calcium and the sodium as is usual in mineral waters, forming the chloride of calcium and the chloride of sodium.

The silicic acid, though capable of solution in water to a small extent, is probably maintained in solution by the carbonate of soda ; and when in a state of precipitation, appears to be partly free, and in part in combination with oxide of iron, forming a silicate of iron.

The crenic acid when in solution must be engaged by soda or lime ; but that it also exists in the precipitate, appears from the odor of the heated precipitate, in which condition it is probably united with per-oxide of iron with which base it forms an insoluble compound.

In what way the alumina is combined, or whether it be merely a mechanical element, I shall not attempt to explain.

It would seem most consistent with chemical theory to suppose, that the iron was originally held in solution by the crenic and carbonic acids ; but that the access of the hydro-sulphuric acid had completely decomposed these salts and given rise to the sulphuret of iron.

The appearance and disappearance of the precipitate in the water, as mentioned at the commencement of this article, and which is chiefly silicic acid, may probably be explained by supposing the quantity of carbonic acid subject to fluctuation ; when most abundant, the bi-carbonate of soda may be formed, and the silicic acid previously held in solution by it, deposited. When less abundant, or when its escape is formed by a more elevated temperature in the atmosphere as from frequent agitation, the carbonic acid may be expelled and the silicic, redissolved. Or, probably a hydro-sulphuret of sodium may be formed, which occasions the precipitation of the silicic acid, and on the decomposition of the hydro-sulphuret from a higher temperature in the water or agitation, carbonate of soda may combine anew with the silicic acid.

The following is the most satisfactory view which my experiments enable me to present of the constitution of these waters.

Soluble ingredients.

Nitrogen,	Chloride of sodium,
Hydro-sulphuric acid,	Sulphate of soda,
Bi-carbonate of soda,	An alcaline, or earthly crenate, or both,
A super-carbonate of lime,	Silicic acid.
Chloride of Calcium,	

Insoluble ingredients.

Sulphuret of iron,	Alumina,
Crenate of per-oxide of iron,	Silicate of iron.
Silicic acid,	

My examinations do not permit me to point out the difference between the two springs with precision. The new spring appears to give rise to a greater amount of hydro-sulphuric acid, as well as of iron and silicic acid, possibly it may differ in still other particulars. I have not examined it for iodine or bromine.

The medical effects of these springs are in a measure ascertained, and their virtues especially those of the old spring strongly attested by many persons of the highest intelligence and respectability.

They are both described as being very light, and as not creating uneasy sensations even when first made use of by invalids. In their operation on the system, they are thought to differ considerably from each other; that from the anti dyspeptic, acting principally as a diuretic and but gently on the bowels, while the new spring is a powerful aparient. The latter spring, however, not having as yet been fairly tested in its medical properties, I shall simply relate those which seem to have been clearly ascertained in the old spring, and which may be set down as follows. It relieves nausea and headaches originating in a disordered state of the stomach and bowels, corrects acidity, and is an excellent tonic, exciting the digestive organs, to a more healthy action. It allays irritation of the stomach and bowels, and possesses a powerful control over the arterial action; very perceptibly when excited by disease. Its effect in allaying nervous irritability is no less striking; and besides possessing gentle diaphoretic qualities and operating as a pleasant soporific, it has been observed to exert an important action upon the liver without exciting the system generally, and thus to afford relief in many cases of torpidity in this organ, when more stimulating medicines could not be employed.

Charleston, Jan. 19, 1836.

Art. XI.—*Earthquake and rising of the sea coast of Chili, in November*, 1822.

The well known account by Mrs. Graham of the rising of the land on the coast and in the interior of Chili, during the great earthquake of 1822, having been controverted by Mr. Greenough, late President of the Geological Society, a vindication of her statements was made, and by the authoress herself, now Mrs. Calcott, (formerly Mrs. Graham,) in a letter addressed to the President and members, of the Geological Society. These papers were published in this Journal, Vol. xxviii, at p. 236 and Mrs. Calcott was, we believe, regarded by the geological world as having fully sustained herself in the controversy. This subject having been publicly mentioned by Professor Silliman at Nantucket, during a residence there, in August and September, 1835, a full confirmation of the first account of Mrs. Graham was accidentally obtained from Captain Robert M. Joy a highly intelligent and most respectable man, for many years, a commander of a whale ship. This gentleman having expressed great surprise that any one personally acquainted with the facts, should doubt of the rising of the land, kindly furnished to Professor Silliman the following documents.

1. A printed letter addressed to the editor of the Nantucket Inquirer, written at Valparaiso at the request of Captain Joy, by Mr. I. Robison, a gentleman of Virginia, who was present at the time of the earthquake; this letter is dated December 15, 1822.

2. A letter by Captain Joy, to Professor Silliman, dated Nantucket, Sept. 23, 1835.

Our limits allow us to give only an abstract of the principal facts contained in Mr. Robison's letter; those facts relate to the convulsions of the earthquake which produced the rising of the ground described in Captain Joy's letter.

1. *Notices of the earthquake in Chili in November,* 1822, *by I. Robison, Esq. of Virginia, U. S. Am.*

The catastrophe occurred in the night of the 19th of Nov., 1822. It was summer in that climate, the weather had been hot and dry and during the preceding month, many slight shocks of earthquakes had been perceived, which were most sensible on alluvial ground. The town of Valparaiso stands on the margin of a bay of the same name about five miles deep, and is built at the feet or the sides and summit of hills or on the plain Almeadrall, formed by the sand washed up by the waves from the ocean and borne down by the torrents, that in the rainy season, descend from the hills.

The bases of the hills are either solid rocks, or rocks, sand and clay. On the 19th of Nov. 1822, the sun rose with great splendor—a haze followed—then at noon intense and oppressive heat—sea breezes in the afternoon, and a brilliant star-light night succeeded, with a delightful calm. But at half past 10 o'clock, P. M., appalling shocks of an earthquake began; every thing on the surface was in motion; the hills near at hand, oscillated; the ground rose and fell; houses reeled, like ships on the ocean; trees waved as if bent by a blast of wind; waters before still, began a rustling movement; the earth opened; the distant mountains rocked; the sea retired, and the tremendous convulsion seemed to threaten universal dissolution. The animals, wild with terror, ran away, or sought the protection of man; who, himself, fled, in consternation, he knew not whither. In a moment more, the crash of falling roofs and walls—the cries of those imprisoned in their fallen houses and the shrieks of those buried beneath the ruins, invoking their saints, or imploring mercy of God, filled the air. The moon was just setting when the convulsion began, and now dropped beneath the horizon, leaving more sensible the horrible darkness of that night, increased by the dust of the prostrated buildings, which now obscured the stars or caused them to shed a ruinous light, and producing also a difficult respiration; the motion created nausea and vomiting, as in sea sickness. Neither men or animals could move without staggering, and horses were thrown on their knees. This greatest shock seemed to most persons to come from the north and west, and continued about two and one half minutes. Subsequent shocks were heavy and appeared to recede toward the south and east, but none of them were very destructive.

During the remainder of the night and the following day, they were repeated at intervals of twenty five to thirty minutes, and continued to the date of the letter, (twenty seven days,) but with diminished force and frequency. On board ships, it was observed by sounding, that the water shoaled two and a half fathoms; vessels of war at their moorings, swung five or six times in shore and out again; as there was no wind and as it was low water with a smooth sea, the movement must have been that of the earth under the ocean. When the lead was thrown, it seemed to catch, as if pinched in a fissure, first made and then closed by the earthquake; it sometimes required two persons to draw it up, and it was observed to swing with an extraordinary vibration. A captain of a ship at San Antonia, forty leagues from Valparaiso, observed immense quantities of dead fish of all kinds floating, and the ship which was at some distance from the shore, under weigh, was shaken as if she would fall to pieces, proving that the movement was submarine. Rivulets of water, formerly running, but now, on account of a drought almost dry, were made to flow again, and exhausted springs became, on the second day, living fountains.

By this earthquake, a large part of Valparaiso was ruined; chiefly the part erected on the alluvial plain Almeadrall, while the houses built upon solid ground or firm foundations, were much less injured. Many hundred dead bodies were taken from the ruins. Towns and villages, at the distance of ten, twelve and thirty leagues, were sensibly affected; the Andes and the country east of them, were agitated, and it was supposed that one quarter of South America was shaken.

On the night of the great earthquake, meteors or blazing stars and flakes of fire, are said to have been seen in the heavens; one very vivid meteor shot from the south west towards the south east. Three deep and wide cracks were opened on the beach and many others intersected mutually like the furrows of a ploughed field.

In a little more than three hundred years, Chili has been visited by eight* destructive earthquakes. In 1520, some villages were destroyed in the southern provinces: in 1647 many houses in St. Jago were ruined, and 1657 the greater part of that city was destroyed. In 1730 the city of Conception was much injured; the sea overflowed its walls, and in May, 1751, St. Jago was entirely destroyed together with all the villages between the 34 and 40° of latitude. The shock then came from the northward and was announced by a slight movement and a ball of fire. In 1819 Copiaco was destroyed: in 1821 a place near Coquimbo was injured; and in 1822 Valparaiso, Cassa Blanca and several other places were nearly ruined.

The inhabitants of the country, received the impression that the bitumen which greatly abounds there, and the electric fluid which is very active in those regions, aided by internal fire were concerned in producing the earthquake. It was afterwards ascertained that

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* Other very severe ones occurred in that country last year which nearly destroyed Conception and other towns.—*Ed.*

the shock was very violent on the Pampas, fifty leagues on the eastern side of the Cordilleras. There was a hollow and terrific sound in the air and a gentleman on the Combre, the highest peak of the Cordilleras thought it was falling with the surrounding cliffs, while from below the report as of a thousand cannon, resounded through the mountains.

Such are some of the leading circumstances attending this remarkable earthquake. We now quote

2. *An extract from the letter of Captain Joy.*

To Professor Silliman—*Respected Sir.*—I find by referring to my Journal, that I arrived at Valparaiso on the thirtieth of Nov. ten days after the earthquake and that I left it on the seventeenth of Dec. for the United States of North America. While I lay at Valparaiso, not a day passed without one shock, or more of an earthquake; and by leaning against any permanent fixture, we could feel the earth tremble like the boiling of a pot over the fire. The rising of the shore above the usual tide was visible on the whole margin of the bay; it was the cause and the subject of daily remark, among many of my acquaintances, and frequent visits were made to the shores to observe the effects and alterations made by the earthquake upon them. They were most visible on the rocky parts where we found the rocks to be elevated, from two to six feet above the usual tide water mark: and at the N. E. point of the bay near the town, there were found, after the earthquakes, a great quantity of the round clam (vulgarly called *quahogs*) in four or five feet of water, where the water was formerly deep, and these shell fish were not before known to exist there.

The depth of the bay was found to be in many places from one fathom to two fathoms less than before the shock.

The remainder of the letter is occupied with statements similar to those contained in Mr. Robinson's letter and we therefore omit them. The statement of Captain Joy, (a perfectly competent observer and one committed to no theory,) is full to the point that the country was elevated as Mrs. Graham, now Mrs. Calcott, has stated.

We must therefore, in connexion with all previous evidence, and and after a careful review by Mr. Lyell of all the circumstances and a verdict by him in the affirmative, consider the fact of the rising of a part of Chili as fully established.

ART. XII.—*Remarks on a "Critical examination of some passages in Gen.* I.*; with remarks on difficulties that attend some of the present modes of Geological reasoning. By M. Stuart, Prof. Sacred Lit. Theol. Sem. Andover." Biblical Repository and Quarterly Observer for January,* 1836.

THE subject of geology, in its relation to the Mosaic cosmogony, has, within a few years, occasioned considerable discussion; and the geologists have been thought by some theologians, and by Prof. Stuart, as it appears, among others, to have advanced in their speculations quite beyond the limits of their own province. In the number of the Biblical Repository and Quarterly Observer for January of the present year, Prof. Stuart has in due form warned the geologists to abstain from their encroachments; and given them very clearly to understand, that he considers them mere intruders on Hebrew ground. He announces the important truth, that "the digging of rocks and the digging of Hebrew roots are not yet precisely the same operation, and are not likely soon to be;" and to give, as it would seem, a practical demonstration of this proposition, has dug up, for the edification of all concerned, a quantity by no means inconsiderable of the roots in question. It is the object of the following remarks to ascertain, as far as possible, the present state of the controversy between the professor and the geologists; or, in other words, whether the geologists have lost or gained by this new assault on their positions. This inquiry will be conducted with all possible respect for Prof. Stuart; but that freedom will be used with his reasoning, which the subject requires, and which he, no doubt, would be among the first to grant.

The writer of these remarks, if he had any claims to be ranked among geologists, might well be alarmed at the warning given by Prof. Stuart to certain philosophers of this class, "to keep a good look out how they meddle with Hebrew philology;" but he has no such claims. It ought likewise to be added, that as to Hebrew philology, he makes no pretensions to those high attainments, which are so generally and so justly ascribed to Prof. Stuart. The question then occurs,—if the writer is armed with no weapons, either geological or philological, how he dares enter a field, in which he will be exposed to such fearful odds? Prof. Stuart shall himself furnish the answer. He observes (p. 103) "that the *logic* of men, who reason from certain facts real or supposed, as connected with a science, may be a fair and legitimate object of critical examination, even by some who are not versed in the detail of the science in question." Under this shield, then, the following remarks will be hazarded.

Prof. Stuart, that his readers may be fully aware of the grounds of his reasoning, has laid down, before entering on his main topic, a canon of criticism applicable to the interpretation of all ancient writings, and particularly as he maintains, of the writings of Moses. "I am unable" he says, (p. 49.) "to see how the discoveries of modern science and of recent date, can determine the meaning of Moses' words. Nothing can be more certain, than that the sacred writers did not compose their books with modern sciences in view, or, indeed, with any distinct knowledge of them." Again (p. 51.) he remarks, "I am now concerned merely to show, that modern science not having been respected [?] in the words of Moses, it cannot be the arbiter of what the words mean which are employed by him. Indeed, this proposition is so plain, as to its general nature, that it does not need any confirmation." He adds, (p. 81.) "Is he [the interpreter of scripture] to resort to a recent science, in order to explain what was writen some 4000 years ago? Then the state of modern Greece under the Turks may interpret the Iliad; and that of present Italy, the Æneid, or the works of Livy. But all must see, that this will never do."—The same idea is repeated again and again, in other parts of his dissertation, and in a great variety of forms. Here, then, if the meaning of any writer can be clear, Prof. Stuart would be understood to say, that no principles of science, discovered by philosophers after an author has written, can be applied to explain the meaning of that author; and the reader is led to infer, that the Professor is about to interpret the first chapter of Genesis in strict accordance with this great rule of critical exposition.

It will be attempted in what follows to show, in the first place, that Prof. Stuart, in the interpretation of some parts of this chapter, has left his own rule above stated entirely out of sight, or has made no intelligible application of it; and in the second place, that if certain expositions given by Prof. Stuart are according to his rule, then, agreeably to the same rule, every thing is granted which the geologists need, or can, ask. The first inquiry, then, will be, whether Prof. Stuart, in his explanation of certain passages of the first chapter of Genesis, has not neglected his own canon;—Or, in other words, whether, in expounding the first chapter of Genesis, as he says *philologically*, without the aid of modern science, Prof. Stuart has not thrust aside, or actually transgressed, the very rule which he has laid down for his own guidance?

He says (p. 50.) "In Gen. 1 : 7, the *firmament* is represented as an expanse, as it were solid and extended, which retains the waters above, that is, those which fall in showers of rain." And he observes afterwards, "in accordance with this, the *windows* or *lattices* of heaven are said to have been opened, that the waters to cause the deluge might descend (Gen. 7 : 11); and they are in like manner said to be *closed*, when the diluvial rain was restrained. Gen. 8 : 2. So in Ps. 148 : 4, *the waters which are above the heavens*, that is, the expanse or firmament, are called upon to praise Jehovah."—Prof. Stuart remarks in reference to the first passage now quoted, and he would, most probably, include the others with it, "there can be no good reason for doubt here, that the welkin or apparent arch of the heavens, or the clouds over our heads with the atmosphere, is meant." In a case where the reader has been led to expect the most exact *philological* interpretation, without any assistance derived from later discoveries in science, it is not a little strange, and it is much to be regretted, that the steps by which this conclusion has been reached, are not more particularly stated. The reader at once looks about for the mode, in which a firmament "solid and extended" is *philologically* made to mean "the clouds over our heads with the atmosphere." After the formal annunciation of a canon for the interpretation of the Mosaic writings, he very naturally expects to see as formal an application of it; and to find the whole process of exposition adjusted to the standard erected by the author himself.

When Moses says that God made the firmament, that is, according to Prof. Stuart, "an expanse as it were solid and extended," if he *means* only, that God made what is now called "the welkin or apparent arch of the heavens, or the clouds over our heads with the atmosphere," the question at once occurs, whence does this appear? The conclusion in this instance, considering the nature of the discussion and the expectation raised, is altogether too remote from the premises. There is also some appearance, from the exposition here given, that Prof. Stuart has made an application of later science to explain the language of Moses. His readers wish to be well assured that this is not the fact; and that there is not here, at the very outset, what he calls "a ὕστερον πρότερον in hermeneutics." Prof. Stuart was at liberty to bring to this interpretation, no science, but such as was current among the Israelites in their passage through the wilderness. It was his business to inform his readers, what the cotemporaries of Moses, what Aaron, Joshua and Caleb understood by this "solid and extended" firmament, and whether they believed this language to mean precisely what is now meant by the *welkin*. This was the point to be ascertained, and it was what Prof. Stuart, on his own principles was bound to show. Or, if he felt himself unable absolutely to prove, that the cotemporaries of Moses understood this language in the sense in which he says that Moses meant it, he should have attempted to make out some such probability as to warrant a belief, that the language of Moses, when first published, or at some time during the Jewish commonwealth, was understood to mean what he now understands it to mean.

Prof. Stuart adds, "we are not to assume the fact, that Moses taught or designed to teach the doctrine, that the apparent celestial arch above our heads is of solid matter." But why? If Moses actually says, as according to Prof. Stuart he does say, that the celestial arch above our heads is of "solid matter," why is there a greater impropriety in assuming that he means what he says, than in assuming, as Prof. Stuart does, that he means something else, on the face of it, altogether diverse? Prof. Stuart comes before the public with the declared design of interpreting Moses *philologically*. But which assumption is most philological, to take language in its obvious sense, unless a reason is assigned why it should be understood in some other, or to take it in a sense deduced only, so far as appears, from the application of the discoveries of science, and possibly, of comparatively recent science? If ever a reason was necessary, for a departure from the obvious meaning of language, such a reason seems to have been required, in the present instance. It should be recollected, that the question now, is not, whether the construction put upon the words of Moses, in this instance, is correct. For the purpose of the present discussion, it is unimportant, whether it is correct or not. The simple inquiry now is,—Has Prof. Stuart interpreted Moses in consistency with his own principles? Until additional light is thrown upon this matter, and if a judgment must be formed from the exhibition of his reasoning here made, the inference seems fully warranted, that he has not.

But it may be said, that if Prof. Stuart has not, in the present case, made use of his canon of criticism in form, he has yet adduced what he considers parallel cases of interpretation, and in this way confirmed the meaning which he has put upon the language of Moses. It is true, that he has brought forward what he appears to intend for illustrations of his construction of the passage in question, and these shall now be considered. Thus he says, "the evangelist speaks of *lunatics* being healed by Christ, Math. 4: 24. 17: 15," and adds, that we are not to assume the fact that Matthew designed to teach, "that the moon has a real and actual influence in creating disease." But where is the parallelism between the two cases? Moses speaks of the beginning of the firmament, its creation; and represents it, according to Prof. Stuart, to be "solid and extended." Matthew says nothing directly of the origin of lunacy, or of the nature of this disease. The two cases are, therefore, wholly unlike. The latter, in no respect, illustrates the former. Moses, as Prof. Stuart asserts, speaks of the firmament as having been created "solid and extended." If Matthew had been speaking directly on the origin of lunacy, and had ascribed this disorder to the influence of the moon, the two cases, then, would have had some points of resemblance, and might perhaps be compared. But, if Matthew had here directly treated of the origin of lunacy, and had described its nature, it would be just as difficult to show, that he did not mean what he said, as it now is to do the same office for Moses: and if Matthew might be brought to explain Moses—Moses, with the same propriety, might be brought to explain Matthew; but

"Nil agit exemplum litem quod lite resolvit."

Prof. Stuart likewise says, "Paul asks the Galatians, who had *bewitched* them, that they should not obey the truth. Gal. 3: 1.;" and adds, that we are not to assume the fact from this language, that Paul taught, "that the doctrine of witchcraft is something, which is to be truly and philosophically credited." Here again the case is not such a one, as to throw any light on what it is brought to illustrate. If St. Paul had been speaking of the origin of witchcraft, and its characteristics, there might have been some similarity between the two passages. As the fact is, there is no similarity. But perhaps Prof. Stuart will say, that the words *lunatics* and *bewitched* meant, in their original application, something different from what they mean as used by Matthew and Paul; and that on this account he referred to them. Be it so. These words shall be examined a little more particularly. Matthew employs a name given to a disease in the language in which he wrote. It was the common belief of his countrymen, that this disease, which is called in English lunanacy, was occasioned or greatly affected by the influence of the moon. Matthew may have been, or may not have been, wiser on this subject, than other Jews of his age. His great object, as appears from the narrative, was to record the fact, that Christ *healed* those who were afflicted with this malady; not to teach any thing respecting its origin and nature. Now if Prof. Stuart will show, that it was not the design of Moses to teach any thing respecting the origin and nature of the firmament, more than it was the design of Matthew to give an account of the origin and nature of lunacy; and if he will show further, that Moses had another object as distinctly in view in speaking of the firmament, as Matthew had in speaking of lunatics, he will make some progress in his argument.

As to the word *bewitched* as used by St. Paul, its use from its connection, is plainly figurative; and the original Greek word, of which this is a translation, is employed in the same way in the Greek classics. *Bewitched* is as obviously used by St. Paul in a figurative sense, as it is by Shakespeare in his Henry VI. Cardinal Beaufort, is thus represented as saying,

> "Look to it, lords; let not his smoothing words
> *Bewitch* your hearts; be wise and circumspect."
> 2. *Act. I.* 1.

And again, Queen Margaret,

> "Heaven grant, that Warwick's words *bewitch* him not."
> 3. *Act. III.* 3.

This word was in common and reputable use when the present translation of the bible was made; and its figurative meaning agreed sufficiently with the corresponding Greek word. Let Prof. Stuart show, that the words employed by Moses, in his account of the firmament, are as plainly figurative, and he will do something to his purpose.

But Prof. Stuart speaking of the firmament of Moses and the language of Matthew and Paul, says (p. 50.) "all these things and others like them, are referred to as things *apparently* existing, or else as *supposed* to exist. Realities in all cases are in one sense described by such language, that is, something that is real and true; but the *manner* in which these things do actually exist, is not described, and in my apprehension is not intended to be described." Here we are told, that there is an important difference in the writings of Moses, and in other parts of the scriptures, between *realities* and *manner*; and are led to infer, that this difference is of great value in the interpretation of the first chapter of Genesis. What more necessary, then, than some criterion, by which *realities* and *manner* may be distinguished from each other? And without some such criterion of what use is this rule? Yet to aid the inquirer in applying this specious rule of sacred criticism, not one word is said; and the rule, so far as appears, is worthless. But perhaps Prof. Stuart expected, that his readers would look to what he has himself done with the rule, and learn its application from the use which he has made of it in the particular exposition under consideration. Let the inquiry then be instituted, how Prof. Stuart has practically distinguished between *realities* and *manner*. According to his exposition, the firmament is spoken of by Moses as "solid and extended," and this firmament is represented as retaining "the waters above, that is those which fall in showers of rain;" and the *windows* or *lattices*, of heaven as being opened and shut, as it rains or ceases to rain. But all that is *meant* here is, "the welkin or apparent arch of the heavens, or the clouds over our heads with the atmosphere." Here if Prof. Stuart is rightly understood, the "solid and extended" firmament is *manner*, except so far as there are *realities* in "the clouds over our heads with the atmosphere;" and the *waters above* the firmament, and the *windows in* the firmament are *realities* in no respect, but *manner* absolutely. Now let the reader look at this, and discover if he can, what criterion Prof. Stuart here employed to distinguish *realities* from *manner*. If he had not expressly banished all modern science from the interpretation of Moses, there would appear to be little difficulty in the case. The belief would then be irresistible, that whatever, in the Mosaic account of the creation, agrees with modern science, as Prof. Stuart understands it, is reckoned among *realities*; and that whatever in this account, is contradicted by modern science, as Prof. Stuart understands it, is placed to the account of *manner*. But this conclusion the reader is not at liberty to adopt, as it would bring Prof. Stuart into direct collision with his great critical canon for interpreting ancient writings. He says, moreover, (p. 79.) "When we inquire simply and philologically what Moses said, and testified and *meant*, we know of no rule which obliges us, nay of none which permits us, to accommodate his words to the deductions of modern science." The criterion, therefore, which he has actually applied in making this important distinction, must be left among things yet to be ascertained.

Prof. Stuart proceeds to say, "Do not we, after the Newtonian philosophy has so long been spread before the world, and our popular calendars all constructed on its basis, do not we still speak of the sun as *rising* and *setting*? And who is deceived or misled by this popular usage—a usage adopted even by philosophers themselves, because the exigences of language demand it? Even so with the sacred writers. They could refer to natural objects and phenomena in the popular language of the times in which they wrote. They did so; for on what other ground could they have been understood?" That the sacred writers adopted popular language is without doubt true; but that they supposed this language, in reference to natural phenomena, not to correspond to the reality of things, is not to be admitted, certainly by Prof. Stuart, without proof. In speaking of the rising and setting of the sun, does Prof. Stuart suppose, that they made any distinction between the *appearance* and the *reality*, or that such a notion ever entered their minds? When the author of Ecclesiastes wrote, "the sun also ariseth, and the sun goeth down, and hasteth to his place, where he arose," will Prof. Stuart say, with his own rule of interpretation before his eyes, that the writer did not intend to imply the *fact* of the sun's rising and setting, as well as to assert the *appearance*? What proof has he, or ground of presumption, to the contrary, except, that a distinction between the appearance and fact is *now* made? And the same rule holds good with respect to many other celestial phenomena. Prof. Stuart appears to forget, that he is bound, or ought to be, by his own principles of exposition, and, that according to these, he can introduce no science to explain the language of a writer, which is of a later date, than the age of that writer; and science, moreover, which the writer may be supposed, on at least probable grounds, to have possessed. But what evidence is there, that the true theory of the earth, on which the distinction in question is founded, was known to a single writer of either the old or the new testament? Prof. Stuart will hardly undertake to maintain, that the descendants of Abraham, at the period during which the books of the Hebrew canon were written, were better informed as to the actual arrangement of the planetary system, than the nations of Europe were, at the time of the publication of the theory of Copernicus. And how was the language of the sacred writers understood in Europe, when this theory was first publicly taught?

But it is asked, "Do not we, after the Newtonian philosophy has so long been spread before the world, and our popular calenders all constructed on its basis—do not we still speak of the sun as *rising* and *setting?*"—Most certainly, this language is still used; and as certainly, by a large part of mankind, it is used to declare their belief in the *fact*, no less than in the *appearance*. When one, who admits the truth of the Copernican system, speaks of the rising and setting of the sun, he means the appearance only; and when one, who is ignorant of that system, or who does not admit its truth, uses the same words, he means both the appearance and the fact. The *science* of the individual makes the difference; from the *words* no certain conclusion can now be drawn. Just so far, therefore, as Prof. Stuart can make it probable, that the sacred writers were acquainted with the true solar system, or any other which would require the same distinction to be made, he may conclude, that they distinguished between fact and appearance in the phenomena of the heavens; and beyond this, on his own principles, he cannot go. His reasoning, in the present case, if correctly apprehended, is this. Those who adopt the Copernican system, *now* make a distinction between fact and appearance in celestial phenomena, and this distinction is founded on a belief in the truth of that system; therefore Moses, who as far as appears was unacquainted with that system, or any other which would separate appearance from fact, made the same or a similar distinction. It might perhaps be hazardous to characterize such ratiocination; but if it is not bringing modern science to illustrate the first chapter of Genesis, it bears a striking resemblance to such a process. Prof. Stuart's exposition of the language of Moses respecting the firmament, may be, or may not be, right. On this point nothing will be said. The simple inquiry now is, whether if he had employed no science in this exposition, which is not as old as the Pentateuch, he would ever have inferred *philologically*, that Moses did not intend to be understood to say, that there are waters above the firmament, or windows in the firmament, or that the firmament itself is a solid and extended covering of the earth?

Prof. Stuart still farther to illustrate this part of his subject, adds, "that the description of the work of creation, as a whole, contains several things, that are said altogether in accordance with things as viewed by the physical eye, I have not the least doubt." Why this is said, or what strength it adds to his argument, is not very apparent. That there are "several things" represented in the manner now mentioned, may be true; and it may likewise be true, that there are "several things" represented, not altogether, or exclusively, as viewed by the physical eye. The remark, to be of any value in the interpretation of Moses, needs some direction appended, to show how it should be applied. Would Prof. Stuart include in his list of the "several things" which are "represented as viewed by the physical eye"—the "waters above the firmament?" Are these mentioned as something *visible?* and not rather, if a judgement is to be formed from his exposition, as something to *account* for the falling of rain?

Prof. Stuart proceeds to interpret another passage. "Verses 15, 16." he says "describe the creation of the sun, moon and stars, and all as designed for the service of the earth. The countless host of heaven occupies but a single clause in the writer's account—*he made the stars also.* As an astronomer Moses did not surely write." Here, as well as in the preceding comment, there is some reason to complain of the course which Prof. Stuart has adopted. He has laid down a rule of criticism applicable to the interpretation of the Mosaic writings; but when he comes to interpret them, of his rule he says nothing; or if he makes use of it, he does this so obscurely, that the application is not perceived. Thus, in the present instance, his comment on the fifteenth and sixteenth verses is, "as an astronomer Moses did not surely write." But on what ground is this inference made? How does it appear, that Moses did not write what is contained in these verses, with all the astronomical knowledge current in his time? This was the very point about which it was important that information should be given; but yet Prof. Stuart has put down with extreme brevity the result of his examination of this passage, without any part of the process by which he obtained it. If he expected his readers without aid, to supply the *hiatus* in his reasoning, he has taxed their ingenuity and patience most unwarrantably. Is it possible, that Prof. Stuart has come to this conclusion, by comparing the account of Moses with astronomy as developed in the Copernican system? He must mean either, that Moses did not write as an astronomer of the present age, or as an astronomer of his own age. But to say, that Moses did not write as an astronomer of the present age, would be, to say the least, to give a very unnecessary piece of information, or would be to recur to modern science for the purpose of determining in what character he wrote; which would be to encroach, as it should seem, upon the author's critical canon.

It must be kept in mind, that Prof. Stuart is interpreting the first chapter of Genesis *philologically*, having ruled out all discoveries in science, since the time of Moses, as irrelevant, and inapplicable to the case. His meaning therefore must be, that Moses did not write as an astronomer of his own time. But here the inquiry immediately suggests itself, what probability is there, that a system of astronomy was current at the time of the writing of the Pentateuch, of which Moses was ignorant, or which, if it was known to him, he did not think it proper to notice, in his history of the creation? He certainly speaks of the sun, moon and stars, the great subjects about which astronomy is conversant. The heavenly bodies are not introduced into the narrative incidentally or cursorily, or as by a writer who did not comprehend astronomy in his plan, and who notices the subject indirectly and by reference. The sun, moon, and stars are mentioned as a part of the creation; nor does it at once appear from the language, though perhaps Prof. Stuart could show the contrary, that they have not their relative importance assigned them, according to the views of the historian; and that he did not write with all the astronomy in view, of which he was possessed, or which was known by those among whom he lived, and for whom he wrote. Prof. Stuart himself (p. 80.) says of Moses, "the distances, magnitudes, orbicular motions, gravitating powers, and projectile forces, of the planets and of the stars, are all out of the circle of his history, and were probably beyond his knowledge." When Moses, therefore, after having described the firmament, had related, that the sun, moon, and stars were set "in the firmament of the heavens, to give light upon the earth;" and having before affirmed, that these "lights in the firmament of the heaven," were to divide the day from the night," and to be "for signs, and for seasons, and for days, and years," what more as an astronomer, that is, of his own age, could he have to say? After Moses had described such a firmament, as, Prof. Stuart says, he has described, is not his system of astronomy such an one, as corresponds to that firmament, and, on the plan of the writer, as complete as any thing in the narrative? It is to be regretted, that Prof. Stuart did not turn his attention more particularly to the elucidation of this point; and especially, that he has not made it more clear, that his conclusion, in this case, has been *philologically* deduced.

Prof. Stuart, as has been several times stated, represents the firmament of Moses, according to the words of the sacred historian, as "solid and extended;" and he says, (p. 70.) that the *luminaries*, that is, the sun, moon, and stars, were placed "in the firmament of heaven in order to give light." He asserts likewise, as appears from quotations already made, that, according to Moses, there were waters *above*, that is, as it should seem, beyond the firmament. This is Prof. Stuart's own exposition, if he is correctly understood; and the unavoidable inference appears to be, that those waters were beyond the sun, moon, and stars. What is asked here is, that Prof. Stuart will remove, and perhaps he can, some of the difficulties of his own statements; or such as follow necessarily from them. He will satisfy, however, no one by simply saying, as an *astronomer* or as a *meteorologist*, "Moses did not surely write." If the author should recur to his distinction between what is "real and true," and the "manner" in which things exist, it is certainly no more than reasonable in his readers to ask, that the ground of that distinction should be fully and clearly explained, and especially, that it should be made to appear, that his conclusions, whatever they may be, have not been drawn by the aid of any modern science. Or if he should say, that "the description of the work of creation, as a whole, contains several things that are said altogether in accordance with things as viewed by the physical eye," his readers have a right to require, that the particular difficulties, which this fact removes, should be marked, and that nothing should be included here, to which the physical sight does not extend.

Prof. Stuart explains another passage. He says, (p. 51.) "The rise of plants and fruits is described, in verses 11 and 12, as occasioned by the earth. God commands, and the *earth* brings forth all these things. So in verse 20, the waters bring forth abundantly fish, and fowl, and reptiles, at the command of God. *Optical*, therefore, in some good measure, all this description is. Plants and trees in their origin appeared to spring forth spontaneously from the earth; and in accordance with this, the writer represents the earth and water as producing them. Still the voice of the creator is after all to be heard. God said let this and that produce the objects of creative power." The difficulty, which Prof. Stuart is here endeavoring to remove, if he is rightly understood, is this. The language of Moses represents the *earth* as bringing forth herbs, and the *waters* as bringing forth fish, at the command of God, as if they acted by some inherent power. To prevent a mistake in this matter, Prof. Stuart says, this is *optical.* But how? Plants are seen to spring out of the ground, but the *cause* of their springing is invisible. There seems to be an inference here; at least, this is as probable, as that the whole is *optical.* Will Prof. Stuart explain, *philologically,* how, when Moses says, that the waters brought forth *fowl,* this can be said to be an *optical* description, and not rather an *inference* from what is optical?

The reader is disappointed likewise, at the course which Prof. Stuart has adopted in his explanation of this passage. From his principles of interpretation, the expectation is excited, that he will endeavor to ascertain, how this language was understood among the cotemporaries of Moses, or the Israelites of later days; but of this he says nothing; nor has he in this case, more than in the former, made any use, which is discernible, of the great rule of expounding the Mosaic writings, with which he sets out in his inquiries. Especially, he says not a word to remove the impression, which some might receive, that the philosophy of the time of Moses, might agree with the literal interpretation of the text. Indeed, there is some appearance, that his own philosophy has, in this instance, as heretofore, got the better of his philology, and that the latter has been insensibly accommodated to the former.

But Prof. Stuart, without doubt, will maintain, that all his conclusions, as to the meaning of the first chapter of Genesis, which have now been considered, are correct; and that they have been philologically deduced. As his skill in Hebrew criticism is not denied, perhaps he will be able to show this. Let it then be conceded for the present, that his reasoning is throughout sound and legitimate; and the question will be, What rules of interpretation can be derived from these examples of true philological exposition, to direct interpreters through other parts of this chapter; and as the case may be, through the rest of the Old Testament. These examples should be considered as patterns, according to which, similar investigations may be pursued; and as these examples have important relations to modern science, no reason appears, why geologists may not profit by them as well as others.

It appears, then, by reference to Prof. Stuart's exposition of parts of the first chapter of Genesis, made, as is for the present admitted, on the purest principles of philology, that a firmament said to be solid and extended, and retaining water above it, except when this water is suffered to fall through windows opened for the purpose, *means* nothing more than the welkin or the clouds over our heads, with the atmosphere, and must have been so understood by Moses, and by those to whom the Pentateuch was first delivered; and that a representation of such a firmament, was the most direct and obvious mode, when this history of the creation was written, of conveying a notion of the atmosphere, without any additional idea; that is, of saying what was meant. It appears further, that the sun, moon, and stars, placed in this firmament for the service of the earth, *means* nothing, (and when first published, would convey no idea,) inconsistent with the fact, that all these bodies are at immense distances from the earth, and most of them at distances inconceivable, and baffling all calculation; that the distinction between appearance and reality in the phenomena of the heavens, was, in the time of Moses, generally understood; and that when the earth and the sea are said to *bring forth* their productions, this is optical; and not inconsistent with the present prevailing philosophy on this subject.

It is worthy of remark, that the philological deductions of Prof. Stuart, happen to be all in accordance with his own philosophy. How this should occur, is not now a subject of inquiry; it is sufficient to mark the fact, that till he comes to geology, he has no collision with modern science. When he has arrived at this point, his philology and his philosophy, both forbid him to go further. But this opposition to the geologists, seems not to be warranted by his own principles. If his expositions of the first chapter of Genesis, which have now been enumerated, are correct, the geologists have nothing to fear; to show which, was the second thing proposed in these remarks.

According to the Mosaic history, the creation was accomplished in successive periods. This, as the geologists affirm, is indicated, or, rather as they would probably say, *proved*, by the appearance of the earth; and some think it a matter of no little importance, that this coincidence is established. Moses says, that these periods were *days*; and it is evident from the narrative, so evident from the common English version, that there seems to be little need of going back to the Hebrew original to make it more so, that his literal meaning is, days of twenty four hours. Some geologists wish to explain the language of Moses so as to *mean*, not definite but indefinite periods; not portions of time limited to twenty four hours, but long enough to admit of such natural processes, as they say must have been carried on; and which could not have been completed in the time stated by Moses interpreted to the letter. Here Prof. Stuart directly meets them. "Our inquiry is," he says, (p. 54.) "What does the *language* of Moses mean? We propose to solve this question simply by *philology*: they [the geologists] tell us we must not so construe Moses as to contradict their *geology*, and that geology must be called in as the final umpire, where doubt and dispute may arise. We make the appeal from such a court." And again. "They are sure that their decision of a scientific nature about the earth must be well grounded. As philologists we say: Be that so or not, it is nothing to the question, what the record of Moses means. If they please, let it be a question whether Moses has taught wrongly or rightly; but it never can be a question with philologists, whether modern science is to be the final judge of what an ancient writing means. This is as settled as the first principles of interpretation, and as the first laws of reason and the human mind in relation to this subject." And again, (p. 55.) "One simple thing is his [the philologist's] business; and that is merely to seek, by the aid of usual, well known, and established principles of interpretation, after what his author has said or declared. This done, his work is at an end." The geologists respond, that they wish to apply no rules of interpretation, which are not sanctioned by high authority; and that as to the exact limit of twenty four hours to a day—"As a *geologist* Moses did not surely write," substituting the word *geologist* for the word *astronomer*, in Prof. Stuart's short method of freeing himself from all the difficulties of the Copernican system. They say likewise with Prof. Stuart, (p. 50.) "there are many things adverted to and spoken of in the scriptures, which by no means constitute of themselves a revelation," that "the sacred writers were not commissioned to teach geology or any of the natural sciences," and that "so often as any of these subjects are adverted to in the Bible, it is altogether in a popular way of speaking;" and they ask, if Prof. Stuart himself will urge such considerations as these in support of *meteorologists*, why he is not equally liberal to *geologists*.

But says Prof. Stuart, (p. 74.) "Moses tells us expressly in Ex. 20: 11. that in six days God made heaven and earth, and all that is in them; and then he rested on the seventh day;" and (p. 76.) "To the law and to the testimony, then, I answer, for we are not discussing now what geology has found out to be true, or guesses to be true, but simply what Moses has written and what he meant.

Is the word *day* susceptible of comprehending Mr. Faber's thirty six thousand years of creation; or the six hundred thousand years of Mr. McCulloch; or the quadrillions of millions of others; or the *indefinite* periods of a more cautious and less extravagant class of geologists?" The geologists reply, that according to Prof. Stuart himself, "in Psalm 148: 4, the waters which are above the heavens, that is, the expanse or firmament, are called upon to praise Jehovah;" and if this allusion to the first chaper of Genesis is an accommodation to received opinions and popular language, that is precisely their own view of the reference in Exodus. They add, that as to the language of Moses, in his history of the creation, "realities in all cases are in one sense described, that is, something that is real and true; but the *manner* in which these things do actually exist, is not described;" that the *distinct periods* they consider as "realities," the *twenty four hours* or the length of these periods, as "manner;" and they put the question with some emphasis, what in their application of this principle, is more inconsistent with philology, than in Prof. Stuart's own application of it? If a "solid and extended" firmament may mean only *thin air;* and if "water above the firmament," and "windows" in the firmament, may mean *nothing at all,* why may not twenty four hours be understood as a *definite* time for an *indefinite?* As to quadrillions of millions of years, Prof. Stuart himself says, that the heavenly bodies are represented by Moses as placed "in the firmament," and he would no doubt maintain, that this is not inconsistent with the immeasurable distance of the fixed stars; i. e. he would admit that their actual distances are incalculably greater, than would be inferred from a literal interpretation of the text. Grant us, say the geologists, to be as free in construing the length of the Mosaic day, as Prof. Stuart, from his admissions, is in construing the distance of the Mosaic firmament; that is, allow us to take the same liberties with *time,* that he necessarily must with *space,* and we will rest satisfied.

But Prof. Stuart rejoins, (p. 52.) "Any speculation that leaves untouched the real affirmations which Moses himself makes, I can easily concede that any one should indulge; and this without theological or philological offence. But if the philosopher or the geologist bids me pass by, or wink out of sight, or turn awry, any of the declarations that Moses has actually made as to particulars, then I must beg leave to demur, or to deny the correctness of his theory." The geologists reply, Do we not read in the first Chapter of Genesis, "God made the *firmament*," that is, an expanse "solid and extended," "and divided the waters which were under the firmament from the waters *which were above the firmament?*" Do we not read, "The earth *brought forth* grass," that God set two great lights "in the firmament of the heaven," and that the waters "*brought forth*" fish and fowl?—Are not these "real affirmations?"—and affirmations "as to particulars?" as much so, at least, as "the evening and the morning were the first day," and "the evening and morning were the second day?"—Now if Prof. Stuart, claims, that *any* of these affirmations can be *philologically* modified, he is bound to show why, on clear, distinct, and acknowledged *philological* principles, *all* cannot be; and, his *philosophy* being out the question, that his *philology* would still make a difference. If in his exposition of the former passages, he has not passed by, winked out of sight, or turned awry "any of the declarations that Moses has actually made as to particulars," the geologists claim, that in their exposition of the latter passages, they are equally innocent of these high offences. "Where among them all," [the geologists,] asks Prof. Stuart, (p. 54.) is one profound critic and interpreter of the Scriptures; or where has there ever been one?" We have now, respond the geologists, what is better; a profound critic and interpreter of the scriptures, among our opponents, virtually making every concession we ask for.

Prof. Stuart himself says, (p. 55.) that "the common principles of interpreting words must be carried through and through," and the geologists agree with him in this position. Their objection is, that he has gone only *half through;* that he has adopted principles of interpretation in reference to meteorology and astronomy, which he has lost sight of when he comes to geology; and that instead of going "through and through," he has stopt short, and been guided by principles essentially different, in reference to this latter science.

These remarks have sprung from no disposition to cavil. What have been pointed out as inconsistencies in Prof. Stuart's examination of the first Chapter of Genesis, have been honestly felt to be such; and this statement is now made public, with the hope, that it will lead to a clearer elucidation of the subject, when the Professor shall again write on geology. The writer of these remarks, honors Prof. Stuart for his literary zeal, and would willingly sit at his feet, to be instructed in the true Mosaic cosmogony, or in any other subject. K.

Art. XIII.—*Account of an Aurora Borealis, with a notice of a Solar Phenomenon;* by Capt. R. H. Bonnycastle, R. En., Toronto, Up. Canada. Communicated for this Journal.

I. *Aurora Borealis.*

Having witnessed from the days of boyhood, the splendid phenomena of the Boreal Aurora, in almost all the latitudes under which it is usually seen, as far north as to have observed the sun at midnight, and particularly during a long sojourn in Shetland, where the people imagine, from its extremely swift changes and inexpressible vividness, that they can actually hear its rushings, I have ever been anxious to seize all opportunities of endeavoring to catch its Protean forms and to describe them, in hopes that by exciting attention to facts concerning this wonder of northern skies, science might be more attentive to its appearances, and that at length it might become a portion of the duty of meteorologists to detail in their columns, all circumstances concerning it, which they might observe.

The Aurora in the high northern latitudes, when at its extreme, is almost dazzling, and the quickness of its motions, approaches that of lightning. In other situations, it has also been observed to assume irised colors. But although all these combined are eminently wonderful, and strike the spectator with profound admiration and awe, yet perhaps the regions of Upper Canada, bordering on Lake Ontario,* exhibit, though not so splendid and varied a display of this mystery, yet one equally, or perhaps more interesting, to the philosopher. I have now witnessed the Aurora at Kingston for upwards of four years, and in a former volume of the Transactions, have described a magnificent scene, which occurred there two years ago.

During the winter months, on lake Ontario, the Aurora may be said to be almost a constant companion of the dark and cheerless nights, and it occasionly presents itself at all other times of the year, nor is it, in winter a mere display of a glorious phenomenon, the utility of which has not yet been exemplified by science, for it sheds a continued and pleasing light, which resembles that of the crepuscular. This light does not, as in Europe, emanate from the vivid streamers which dance over the starry floor of the heavens, in ever changing and

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* Not having observed it elsewhere in Canada, I speak only of locality as a personal observer.

inexplicable mazes, but proceeds from the northern horizon, over which a pale, luminous, low, and depressed arch embracing an extent of from sixty to ninety degrees, is commonly thrown. This arch is generally luminous in its whole body, not on the rim or verge only, which fades away into ethereal space, but from its superior circumference to the chord formed by the horizon itself, and varies in its elevation, from ten to fifteen and twenty degrees. Wherever it embraces stars, these luminaries are either veiled or dimly seen, being strongly contrasted on a fine star light night, with their fellow orbs of the southern heavens, which appear to shine out with double brilliancy.

Within the space comprehended by this arch of light, continual changes are operating, if the Aurora assumes a splendid shape. Dark volumes of vapor, not like clouds, but blackening in a moment, rise and fall, whenever a ray or an interior arc begins to form, and it is remarkable, that this darkness usually accompanies the commencement of every change in the scene, thereby increasing the majesty and beauty, as well as the brilliancy of the spectacle.

But it is impossible for any pen adequately to describe a phenomenon, which is continually presented in these regions, and it is with diffidence that I continue a task imposed on myself. It will, therefore, be more satisfactory to detail the circumstances attending a very recent repetition of one of the most beautiful of those which have been seen at Kingston this winter, nearly the whole of which I saw, and whatever escaped me was related by a very accurate observer.

On the evening of the 11th of December, 1835, the sky, after the sun had sunk, was dark and gloomy, and although there were but few clouds visible, and the stars were rapidly brightening, a change of weather was apparent. Snow had fallen, for the first time, on Wednesday, the 8th, after a short space of great cold, to the depth of about five inches, and the thermometer had sunk afterwards to 16°, at which it stood on Monday, the 13th. On Tuesday, it rose to 30°, and rain in abundance falling, removed the snow entirely. It was exactly midway between the extreme cold and the thaw, that the Aurora took place, the thermometer at the time standing at about 26°, and the wind, a gentle breeze from the north west. The barometer stood at 29.9, at 9 P. M., at an elevation of forty feet above the lake, which is two hundred and nineteen feet above the sea.*

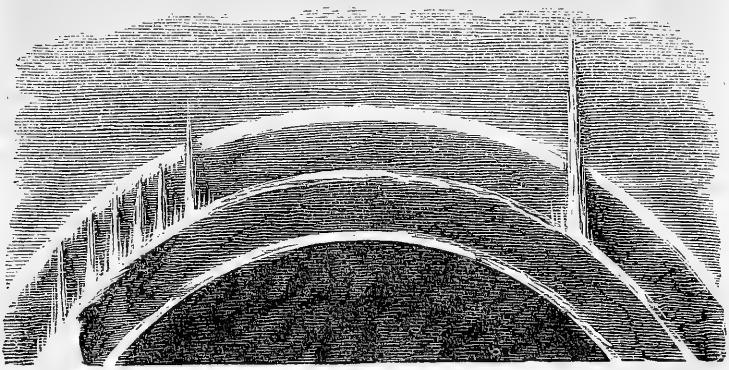
* The barometrical observations were made at the Hospital on Point Henry, by a very accurate observer. On the 10th of December, it indicated, at 9 A. M. 29.5, at 9 P. M. 29.7; on the 11th, at 9 A. M. 29.8, at 9 P. M. 29.9; on the 12th, at 9 A. M. 30.1, at 9 P. M. 30.1.

Its first appearance, after darkness had completely set in, was by the luminous arch above mentioned assuming its wonted place. From this arch, in the north, arose almost incessant streamers of bright white light, which shot upwards to the zenith, and streaked the dark sky with their silvery lines.

Once a mass of light suddenly opened in the zenith, and from it darted out innumerable pencils of bright rays, overspreading the dark vault of heaven with their glories, and seeming for a moment to illuminate the sky with a star which its vast space was scarcely capable of containing.

Again, rods of white light would dart forth from the northern horizon, and one single one, in particular, spanned the whole arch of heaven, touching the southern horizon over the great lake.

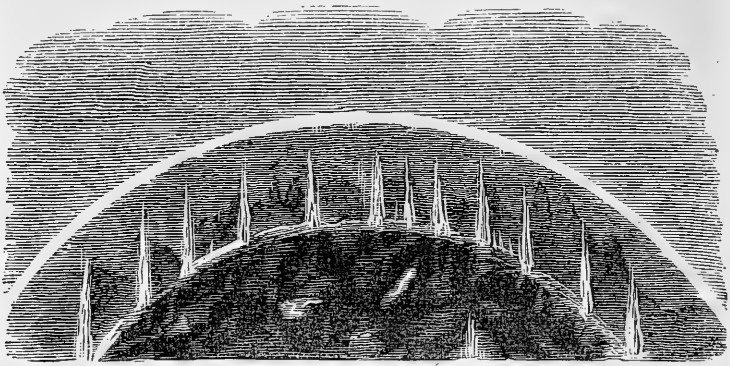
This play of the Aurora continued from seven until near nine, and was most brilliant and magnificent about nine, when it assumed another and not less singular attitude, of which the following is a faint attempt to delineate.



These arches are not so flat as they should be, but the space is insufficient to shew them exactly. The lower one was usually the boundary of a very dark black, changing mass; between the lower arch and the second, the space was not so dark; and between the second and third, or upper arch, it was still lighter, excepting where the coruscations shot upwards out of the second arch, and there it was very dark. The second arch was incomplete.

The ray shooting up on the right was brilliant in the extreme. Stars were partially visible above the third arch, but the bright ones in Ursa Major, on the left, had lost all their splendor, and the constellation could just be traced. The obscuration of the heavenly bodies reached almost to the zenith, above the center of the arch, and was less over the extremities.

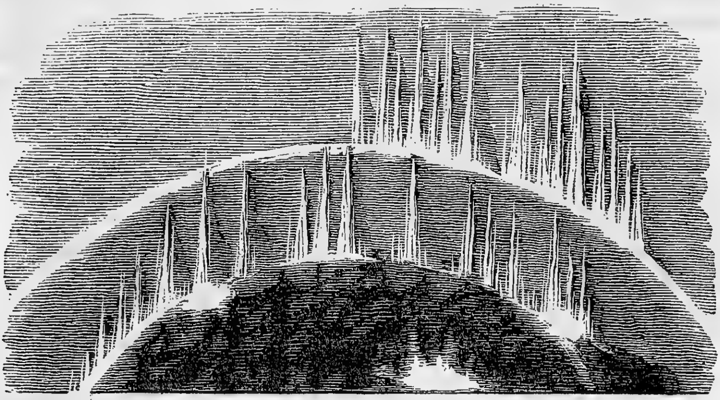
This first appearance lasted long enough to enable me to go into another part of the house and make a hasty sketch; on my return to the window, it was altering to the following form.



The lower arch had somewhat heightened and become darker, with here and there spots of light in it, whilst from its circumference shot out brilliant rays and pencils of light. The second arch had altogether disappeared, but the upper one held its wonted place. It must be observed, that the upper arch was always paler, and more indistinct in its outline, than the others. Faint stars now appeared through the darkish vapor, between the two bands or arches of light, and the lower band was indistinct, excepting to the left of its central space, where it was vividly depicted and extremely well defined, by a sharp band of bright light, cut off, both above and below, by very black vapory masses. This second appearance lasted, also, long enough to enable me to make a hasty sketch of it.

None of the pencils or rays, which shot out of either of these changes of the Aurora, were so quick or so intensely vivid in their action or light, as those seen in the more northern regions, nor were they colored; but they were always accompanied by the black vapory shroud, which hid every thing else from view, and added greatly to the lustre of their exodus from the horizon.

Having made the foregoing sketch, I again returned to view the Aurora, which had somewhat changed its appearance.

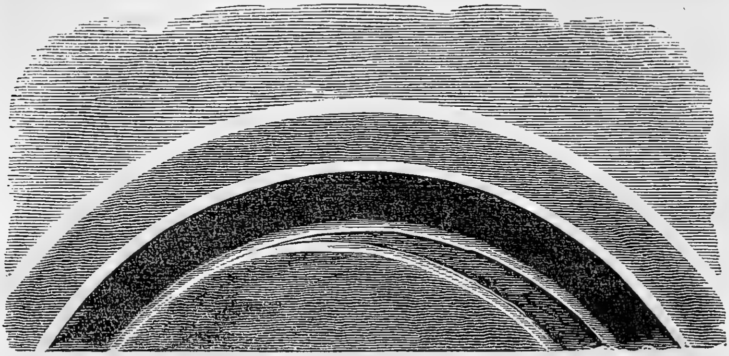


Both arcs or belts were now less distinct, the lower one almost obliterated, but still its place was well marked by the arch of vapor below, which was darker than ever. Three large spots of intense light now displayed themselves, one on the horizontal chord, and one on each side of the lower arch, whilst this lower zone shot out innumerable pencils and floods of light from its dark nucleus, the upper zone also darting forth long lines of brilliant rays; all these rays from both bands, moving in a very stately march or progression from east to west.

Towards the southern and western portions of the heavens, all was clear blue-black starlight, Orion being particularly brilliant; the north was as if overspread with a thin veil, through which the stars were barely visible.

I watched these alterations of the phenomenon until after ten; and the last I observed presented this form; after which the arches became less distinct, and eventually, with the exception of the great arch, passed away.

In this fourth change the Aurora, it will be observed, resumed its three arches, but they were no longer concentric, the third being broken on the right into a portion of a fourth. Between the second and third the darkness was the darkness of blackness, whilst the third arch was light itself; but the lower arches were not so bright, and the lower nucleus was only darkish, which was contrary to every state that it had presented, under any former observations for several years.



The constant arch of the Aurora of the Lakes has, I believe, never been noticed in any scientific publication, and is well worthy the attention of the learned. Whether it is created by a peculiar locality of the matter, of which the substance of the Aurora is composed, or whether the Aurora itself, as the magnetic influence, has a peculiar pole from whence its effluences emanate, can scarcely be, at present, determined; but it is at all events highly singular, that in a latitude so low as 44°, the Aurora should assume forms, unknown in the higher northern regions where its powers were hitherto supposed to have developed themselves in the highest possible state.

Not having been very well when this singular scene occurred, I did not take all that notice of it which it deserved. I trust I shall be able during the winter to note the atmospheric phenomena which accompany it, more particularly, as well as to give more detailed accounts, and more perfect drawings.

II. *Solar Phenomenon.*

Immediately previous to the alteration of the weather at Kingston on Lake Ontario, after an unusual duration of severe frost, and about the middle of March, at near four o'clock in the afternoon of Sunday, I observed a singular species of halo or rainbow.

The day was mild, and there was scarcely any wind, and no rain, but the face of the sky was overclouded, and the sun appeared as it does through a slight fog.

Around the luminary, at a radial distance of perhaps twenty degrees, there was a dark halo of the usual defined character and appearance; and circling this halo in various places, a rainbow was visible. This rainbow was brightest in the eastern and western parts of the halo, where it assumed that peculiar appearance which seafaring men call weather dogs, and which are of very frequent occurrence in the northern division of the Atlantic ocean.

It was evident from the dull whitish light, that was diffused about those portions of the circumference of the halo on which the prismatic colors were not perfectly defined, that, in some situations, an observer might witness the singularly interesting spectacle of a circum-solar rainbow, in which the prismatic colors formed a complete circle, concentric with the sun.

In the course of the winter season, during changes of the weather from frost to a thaw, I have frequently observed a small portion of a vertical arch of the above description, although the sun was hardly visible. Usually these occurrences have taken place when the sun has been at the same elevation, as in the instance here described. They have always happened when there was no rain.

I am unable to say whether the appearances might not be created by reflection from the brilliant surface of such a vast body of ice, unincumbered by snow, as has been presented by Lake Ontario during the last winter, as it is difficult to account for the formation of a rainbow of so small a diameter on the usual principles, since the sun at the time was forty degrees above the horizon.

I have used the word rainbow in the above description, although it is not a correct one, as there were no appearances of rain during the presence of the phenomenon, although it is true there was a slight mist or fog.

Since writing the above, I have seen an almost complete circum-solar rainbow, which appeared at Toronto, (U. C.) July, 1834, at 7 in the morning.

ART. XIV.—*Essay on Calcareous Manures, by* EDMUND RUFFIN. Second Edition. Shellbanks, Va., 1835. 8vo. pp. 116.

On the use of Lime as a Manure, by M. PUVIS, translated for the Farmer's Register. Shellbanks, Va., 1835.

THERE is no one of the useful arts to which the application of Chemical Science may be made of as much importance as to agriculture. We cannot indeed inquire in the minute and delicate processes by which nature elaborates the inert matters of the soil, and converts them into the living plant; but we can examine that organized vegetable, and find what elements enter into its composition, and by a similar examination of soils can determine whether they contain the substances from which the plant must obtain its growth, or not. If they do not, the addition which will be efficient in promoting the growth being thus determined, chemical researches will again show the source whence it can be derived in the most economical manner. So also soils may contain compounds which, if the proper food of some plants, may be noxious to others; chemistry will detect these, and point out the means of neutralizing their injurious action.

Instead then of the decreasing fertility of soils which political economists assume, in opposition to some well known facts, or which the general experience of our own country would seem to demonstrate, we might infer that good soils could be kept up to their original state, and inferior soils improved until they became equal to the best: that nothing in fact except climate would oppose a limit to the approach of agricultural product to the maximum.

Such results, however probable in appearance, have been far from being attained, or even approached. Agriculturists rarely take the trouble to learn even the elements of science, and if the direct force of obvious example occasionally leads to the introduction of new machines and improved processes which are merely mechanical, those which chemical science would indicate are rejected as unintelligible and visionary. On the other hand the student of science can rarely or never acquire the practical skill, the knowledge of the mode of performing and directing agricultural labor, on which the practical farmer properly prides himself, and without which the best theory will lead to no profitable result.

In the application of chemistry to the analysis of vegetables, chemists have usually neglected to examine substances of the greatest importance which they know to exist in them, but which they were willing to consider as merely adventitious. Thus the person who reads a treatise on general chemistry, may rise from its perusal in the belief that plants contain no other essential elements but carbon, hydrogen, and oxygen, with the occasional addition of nitrogen, while a course of actual experiment could show earthy and saline substances of large amount wholly neglected in the estimate. Phosphorus too, which forms so large a portion of the mass of those animals whose whole subsistence is derived from the vegetable kingdom, is never named* among the elements of vegetables, yet it has on some occasions been detected in them, and there can be no doubt that if it were diligently sought it must be found in almost every case. When the gigantic bones of the elephant are known to consist to so great an extent of phosphate of lime, it would be vain to deny that the phosphorus and calcium exist in some state or other in the herbage he feeds upon, as well as the oxygen which forms the other ingredient of the phosphate. So far in fact from those substances which are neglected by chemists being unimportant in the constitution of plants, they must modify the manner in which the other elements combine, and although the vital action does in many cases compel them to enter into combinations in direct opposition to the ordinary laws of chemical affinity, we may in many instances safely attribute the great difference which exists among compounds said to be of the same elements, to the very matters that are usually rejected in the examination.

"It is said that putrescent manures serve for the nutriment of plants. But the same might be also stated in relation to substances which improve the soil, which furnish to it matters necessary to render it fertile; which impart to vegetables, the earth and saline compounds which enter as essential elements into their composition, texture, and their products. Such improving substances well deserve to be regarded as nutritive."

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* Probably the author refers to the necessary elements of plants, among the adventitious bodies; we believe it is usual to name the bodies which he has designated, e. g.—from a work now lying before us, take the following passage.—"Besides the elements above named, that are essential to organized bodies, (carbon, oxygen, hydrogen, and nitrogen,) there are others, which are present in different cases, in greater or less quantity: such are phosphorus, sulphur, chlorine, iodine, bromine, potassium, sodium, calcium, silicium, magnesium, iron, manganase, &c. but generally they are in minute quantities," &c. Silliman's Chemistry, Vol. II. p. 391. Within these remarks, both organic kingdoms are included.—*Ed.*

"Thus lime, marl, and all the calcareous compounds employed in agriculture, since they furnish to plants lime and its compounds, which sometimes form half of the fixed principles of vegetables, ought also to be considered as aliments, or, which comes to the same, as furnishing a part of the substance of vegetables. Thus, again, wood ashes, pounded bones, burnt bones, which furnish to vegetables the calcareous and alkaline phosphates which compose a sixth part of the fixed principles of the stalks, and three fourths of their seeds, ought well to be considered, and surely are nutritive."

"What thus particularly marks the distinction between the manures which improve the soil (*amendemens*) and those which are alimentary (*engrais*) is, that the former furnish, for the greater part, the fixed principles of vegetables, the earths and salts, the latter the volatile matters which are abundantly diffused through the atmosphere, whence vegetables draw them by suitable organs: and what is more remarkable, is, that the vegetable by receiving the fixed principles of which it has need, acquires as we shall see, a greater energy to gather for its sustenance, the volatile principles which the atmosphere contains."—*Puvis.*

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"The greater part of improving substances are calcareous compounds. Their effect is decided on all soils which do not contain lime, and we shall see that three fourths, perhaps, of the lands of France are in that state. Soils not calcareous, whatever may be the culture, and whatever may be the quantity of manure lavished on them, are not suitable for all products, are often cold and moist, and are covered with weeds. Calcareous manures, by giving the lime that is wanting in such soils, complete their advantages, render the tillage more easy, destroy the weeds, and fit the soil for all products."

"These improving substances have been called *stimulants;* they have been thus designated because it was believed that their effect consisted only in stimulating the soil and the plants. This designation is faulty, because it would place these substances in a false point of view. It would make it seem that they brought nothing to the soil nor to plants, and yet their principal effect is to give to both, principles which are wanting."

"Thus the main effect of calcareous manures proceeds from their giving on the one hand, to the soil the calcareous principle which it does not contain, and which is necessary to develop its full action on the atmosphere, and on the other hand, to vegetables the quantity which they require of this principle, for their frame work, and for their intimate constitution. It would then be a better definition than that above, to say that to *improve* the soil, is to give to it the principles which it requires, and does not contain."—*Puvis.*

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"In the neighborhood of great cities, alimentary manures being furnished on good terms, may well vivify the soil, but animal manures cannot suffice but in a few situations and of small extent, and in every country where tillage is highly prosperous, improving manures are in use. The department of the North (*France*,) Belgium, and England owe to them in a great measure their prosperity. The department of the north (which is, of all Europe, the country where agriculture is best practised and the most productive,) spends every year upon two thirds of its soil a million of francs in lime, marl, ashes of peat and bituminous coal, and it is principally to these agents, and not to the quality of the soil, that the superiority of its production is owing. The best of its soil makes part of the same basin, is of the same formation and same quality as a great part of Artois and Picardy, of which the products are scarcely equal to half the rate of the North. Neither is it the quantity of meadow land which causes its superiority; *that* makes the fifth part of its extent, and Lille, the best *Arrondissement*, has scarcely a twentieth of its surface in meadow, Avesne the worst of all, has one third. Nor can any great additional value be attributed to the artificial meadows, since they are not met with except in the twenty sixth part of the whole space. Neither can this honor be due to the suppression of naked fallows, since in this country of pattern husbandry, they yet take up one sixth of the ploughed land, every year. Finally the Flemings have but one head of large cattle to every two hectares, a proportion exceeded in a great part of France. Their great products are due to their excellent economy in the use of manures, to the assiduous labour of the farmers, to courses of crops well arranged, but above all, we think, to the improvers of the soil, which they join to their alimentary manures. Two thirds of their land receive these regularly: and it is to the reciprocal action of these agents of melioration that appears to be due the uninterrupted succession of fecundity, which astonishes all those who are not accustomed continually to see the products of this region."—*Puvis.*

The agriculture of adjacent parts of Belgium is even more instructive as an example. Those parts of that rich country which are now most remarkable for fertility, as for instance the *Pays de Waes*, were originally barren hills of blowing sand. They now yield a greater product than any other part of Europe. Great industry and labor have no doubt been expended in obtaining and applying putrescent manures, and in this region the clover husbandry had its rise; but with all this, no permanent fertility could have been given without the use of lime, and those substances which contain its compounds. At present a light friable mould of greater depth than can be reached by the plough, and which is therefore occasionally trenched by the spade, covers the whole of the ancient *dunes*, and the whole country presents an aspect, of which no part of ours can even furnish an idea, with the exception of the garden grounds of the *Shakers* at Lebanon.

We fully concur with the opinion of M. Puvis in respect to the importance of earthy and saline matter, not only as forming a basis of proper character to support the plant, but as itself forming an essential part of the food. We on the other hand cannot but express our dissent from the opinion, which derives the other elements necessary to growth of plants wholly from the atmosphere. If he mean indirectly, through the intervention of the soil, he is to a certain extent right, for most of the water which is the vehicle of all their nutriment, yields at least one of their essential elements, and is itself incorporated with the plants, is derived from the atmosphere in the shape of rain and dew. But the gases it dissolves and carries with it, and which yield the carbon and part of the hydrogen of plants, are all derived from the decomposition of organic matter in the soil itself. When therefore we can find, in the absorption of water by the roots; its ascent in the form of sap; its elaboration in the leaves, where so far from an absorption from the atmosphere taking place, oxygen is evolved; in the subsequent descent of the altered sap in the form of gum, resin, &c. a sufficient reason for the supply of the parts M. Puvis seeks in the atmosphere: we are compelled to dissent from his conclusion. There is one curious fact in connexion with this subject, that we have never seen mentioned. It is obvious that while lands still retain a large quantity of vegetable matter, arising as it may, from their having been for ages in the state of forest, little benefit is to be hoped from putrescent manures, while there may be earthy substances essential to the growth of particular plants, which the soil does not contain. In such a case earthy manures are likely to be extremely serviceable.

The practical farmer in some parts of our country, finding that in a new soil, putrescent manures injure his crops, carts them out upon frozen rivers, in order that they may be carried away when the streams thaw. He thus loses all recollection of the European tradition of the necessity of restoring vegetable or animal matter to the soil, and continues a cultivation growing yearly more exhausting, until his land will no longer afford him a subsistence. His successor coming from a longer settled district, or from an European country, trusts wholly to putrescent manures, and is astonished that they often fail in their effects; but that an inert earthy matter should in some cases be a substitute for organic manure, and in others should be absolutely necessary to make it efficient, he will not believe, and laughs at the idea of carting an absolutely barren soil to an unproductive field. The earlier settler, who thinks no soil worthy of cultivation which demands any manure, would be even more astonished were he told, that by the use of a small quantity of a substance which his experience tells him destroys vegetation, he might have easily maintained his lands in their original fertility, and instead of losing his whole labor in clearing the soil, have continued to reap crops equal to the earlier ones whose prospect tempted him to that arduous task. Yet these propositions are strictly true.

One only mineral manure has been of any extended use in our country, namely the sulphate of lime, usually called plaster of Paris. This was luckily forced into notice by scientific farmers, and produced such obvious effects that the most sceptical could not doubt. Yet the reasons of its benefits are not generally understood, the causes of its failure in some positions not accounted for, and the injury that an unskilful use of it may produce, is rarely guarded against.

The agriculture of America has proceeded from three distinct centres, and may be divided into three distinct and separate characters. The settlers of New England were thrown upon a bleak shore, incapable of yielding any valuable agricultural product for export. They found and adopted the culture of maize as practiced by the Indians; this consisted in planting it in hills, each of which was manured by one or more fishes; and unable at first to see its value as a fallow crop, repeated it in continual succession upon the same field. To this they added the culture of European corn of various descriptions, following without alteration the husbandry introduced by the Romans into Great Britain, and which is described almost identically in the Georgics of Virgil. With a less command of labor, however, the fallows were less perfect than in England, and presented rather the aspect of plantations of weeds, than of the naked pulverized soil which ought to characterize them. Wheat was first attempted, but gradually abandoned as unprofitable; rye followed, and is still cultivated, while a great improvement has taken place in making it or oats follow corn, and thus introducing the rotation of crops. The failure of wheat was in New England ascribed to any thing but the true cause; and it has been usual to lay the blame to the presence of the barberry bush. This indeed infests the fields from which wheat is banished, but is no more than the natural growth of a soil from which the earthy matter necessary to the nutriment of wheat is exhausted. This Eastern mode of farming has derived great improvement from the introduction of sown meadows instead of naked fallows, and the climate admits of grazing them with benefit to the soil rather than injury. At a distance from the sea, clover has been introduced for this purpose, and has by the aid of plaster extended its beneficial influence along the southern shore of the great Lakes, almost to the Mississippi.

It is to this lucky accident, as it may in fact be termed, that it is owing that the more newly opened regions in the northern states have not depreciated as much as those which were cultivated more early. Still the habits of the pioneers of civilization, of eastern origin, are such as to make sad havoc with the native bounty of the earth.

The settlers of Virginia, on the other hand, found in their soil and climate the capacity of yielding tobacco, which formed an article of such value as to dispense with their raising any other, for by its sale they could provide themselves even with bread stuffs. Not only did it do away with this as a necessity, but it furnished them with the means of purchasing slave labor, by which they were enabled to increase the extent of ground brought into cultivation, in a ratio far greater than was at first attempted in the eastern states. While in the latter regions, the settlers were at first collected in villages and hamlets, around which their arable lands were opened at the least possible distances, and every consideration of interest led the inhabitants to attempt to keep them in tolerable condition, the settlers of Virginia, after the first struggle with the natives was over, spread themselves in separate plantations at great distances from each other. Each planter took into possession an extensive district of wood land, of which he cleared as much as his slaves were able to cultivate. So soon as by a continual succession of hard cropping with the same article, the power of raising tobacco was exhausted, new woodland was cleared and brought into cultivation, and thus in the older regions there has been no single acre of land of tolerable promise, that has not at some period or other been subjected to the plough. The virgin soil having been thus completely explored, it remained to abandon the country altogether, or bring it back to cultivation by enriching manures, and in the manner of applying, the proposed remedy became almost as fatal as the disease itself. The putrescent manures furnished by the whole stock of a plantation were lavished upon a few acres, and applied to the continual cultivation of the favorite staple, until they were again exhausted, and thus in succession until a second round of ruinous cropping had been completed.

As the staple fell in value from the increased population employed in its culture, Indian corn and wheat became objects of attention, but they were merely secondary, and did not enter into rotation with tobacco. Were we to hazard an opinion founded upon analogy, we should feel almost certain that tobacco well manured would form a substitute for a fallow crop, and might, in a well planned rotation, impair in no degree the native fertility of a soil. We have ascertained, in fact, that it is an admirable preparation for wheat, but the temptation to continue the tobacco culture is such, that it is rarely intermitted until the land becomes a *caput mortuum.*

The same system has been pursued in all the southern states, which may in this respect be considered as colonies of Virginia, and in all have the same consequences inevitably followed; immense products at first, in consequence of the intrinsic value of the peculiar staple, whether cotton or tobacco; improvident expenditures, arising from the difficulty of distinguishing what part of the annual income was in fact an encroachment upon the capital; and finally, impoverishment or ruin. To the latter event the large families of slaves, which the apparent profits of the earlier culture induced the planters to acquire, contribute in no small degree, as well as their disproportionate increase in a state of being free from all care and anxiety. This state of things is well described by Mr. Ruffin, in one of his notes.

"A gang of slaves on a farm, will often increase to four times their original number in thirty or forty years. If a farmer is only able to feed and maintain his slaves, their increase in value may double the whole of his capital originally vested in farming, before he closes the term of an ordinary life. But few farms are able to support this increasing expense, and also furnish the necessary supplies to the family of the owner—whence, very many owners of large estates in lands and negroes, are throughout their lives too poor to enjoy the comforts of life, or to incur the expenses necessary to improve their unprofitable farming. A man so situated may be said to be a slave to his own slaves. If the owner is industrious and frugal, he may be able to support the increasing number of his slaves, and to bequeath them undiminished to his children. But the income of few persons increases as fast as their slaves, and if not, the consequence must be, that some of them will be sold that the others may be supported; and the sale of more is perhaps afterwards compelled, to pay debts incurred in striving to put off that dreaded alternative. The slave at first almost starves his master, and at last is eaten by him—at least, he is exchanged for his value in food."

The waste of productive capital due to the use of slave labor, does not appear in this statement. By the estimate of Mr. Ruffin himself, the annual *wages* incurred by the employment of a slave amount to $38; the whole of the expenses to $86 50. By the account of one of his southern correspondents, in the Farmer's Register, an agricultural hired laborer in the state of Rhode Island does the work of 2½ slaves, and his wages and food we presume will not cost his employer more than $173. The northern farmer, therefore, saves one fifth; but it is, in addition, to be recollected, that the southern planter must support the children, the aged, and females who can do no work. We therefore think we are safely warranted in saying, that every piece of agricultural labor performed by a slave in a southern state, costs one half more than if performed in an eastern state by a freeman. The difference, indeed, would not be perceptible, if they labored side by side, for the freed negro or white hireling would disdain to work in such a case; but as the comparative result between different portions of our country, it is unquestionable. The effect upon the general prosperity is, however, far greater than would appear from the comparison, for every laborer in the eastern states saves a part of his wages, to add to the national wealth; the children, the women, find profitable occupations, and thus a barren county of New England, in which there may not be a single wealthy individual, may notwithstanding possess a far greater collective wealth than an equal surface of the richest cotton region in the southern states.

The settlers of Pennsylvania commenced the cultivation of that state under different circumstances, and with different views from those either of the east or of Virginia. Penn's plan made cities or boroughs, the seats of traffic and markets of produce, the nuclei of his agricultural settlements, and by a mutual dependence thus created, fixed the agriculturist in the neighborhood of the merchant and manufacturer. The very state of things which a slow course of events has brought about in New England, was contemplated by that enlightened proprietor from the first. The dairy and the beeve were therefore the staples, and other products became the accessories; thus it has happened, that the farmers of the neighborhood of Philadelphia, are the only settlers of English blood, who have resisted the migratory habits of other parts of the country. In that neighborhood, the original fertility has been kept up by the manure yielded by the stocks of cattle which formed the basis of the system, and that which is afforded by the stables and streets of the city. A similar dread of change influenced the Germans, who followed the Quakers, in the occupation of the more remote districts of Pennsylvania; and while bread stuffs naturally became the only profitable objects of culture, they avoided the exhaustion which their growth produced in other districts, by a most valuable secret they brought with them from Europe. We call it a secret, for those of other blood, who see it used in their presence, do not discover its value. This is neither more nor less than the use of lime. By this simple but efficient aid, the farms of Pennsylvania have generally maintained their original character for fertility, and in some places have increased in products, beyond the early crops that are given by the proverbial energy of a virgin soil. To show how slowly an agricultural process, however valuable, passes from one race of settlers to another, we may mention what we ourselves saw, during the last summer, in Hunterdon County, New Jersey. The southern part of this county is settled by Germans, who have entered from Pennsylvania; the northern, by those of various races who have mounted the valley of the Raritan. The soil, in many places, is to appearance identical, being formed by the decomposition of the red shale; the Germans, by the use of lime, raise from thirty to forty bushels of wheat per acre, and their other products are in proportion; the settlers of Yankee and Dutch blood, are happy to get from fifteen to twenty. We hope, for the sake of our argument, that the same German race has carried the same practice along with it, in its progress through the valley of Virginia to North Carolina; for in the last named state we noticed a contrast even stronger than the one we have stated. After traveling for many miles through wastes of old fields, we entered the German settlement of Salem, which, without any perceptible change in the original character of the soil, presented the rich appearance of Pennsylvania.

In the foregoing statement, we have not included the settlers of the Dutch province of the New Netherlands, not because they brought with them little agricultural skill, but because their example has been of little influence upon the present agriculture of the United States. It is, however, but justice to them to mention, that many of them being Protestants, expelled from the southern Netherlands by the persecutions of the Spanish crown, the modes, implements, and practice of husbandry which they introduced, were the very best which then existed, and that although part of these were necessarily abandoned under the new circumstances in which they were placed, their implements in particular were superior, if applied to light soils, such as those of their native Brabant, to any brought to America by other races. Long Island, to which these were well fitted, bore, in consequence, previous to the Revolution, the well merited epithet of the garden of the colonies. But the Flemish, Walloon, and Frisian blood was not excited by the same inventive spirit as the Anglo-Norman, and unable to accommodate themselves to new circumstances, the Dutch settlers of rocky and rugged districts fell behind the New England yeomanry in agricultural skill.

We have called the use of lime a secret; it was not only so to the practical farmer, but to the man of science; for even the researches of Davy, who was well aware of its importance, have not completed the theory of its action. It is, therefore, with no small pride, that we can refer to a countryman of our own, as having finished what the most acute researches of European chemists had left unaccomplished. This credit is due to Mr. Ruffin, and with him as a guide, and with some little aid from the article of which he is the translator, we shall endeavor to exhibit, in a succinct form, the theory of calcareous manures.

In order to make this complete, we shall be compelled to state facts, which to the majority of the readers of this Journal will appear trite and hacknied. As our main object, however, is to call to this subject the attention of intelligent farmers, who may not recollect familiarly the elements of science, our more learned readers must pardon us, if we devote a few lines to matters in which they need no instruction.

Pure lime is so rarely found in nature, that it may be stated as a general rule, that it can be obtained only artificially, in the process of calcination, as practiced in preparing limestones for making mortar. Thus obtained, lime retains the original figure of the stone from which it is manufactured, is acrid and caustic, soluble in small proportion in water, and possesses alkaline properties, that is to say, it is capable of neutralizing acids, and forming with them substances of the class to which chemists give the name of salts. Of these salts, those most important to the agriculturist are, the carbonate, which forms the principal part of common limestones, chalk, and the shells of testaceous fish; sulphate of lime, which in combination with water constitutes gypsum or plaister of Paris; and phosphate of lime, which is the basis of the bones of animals, and has been found in the ashes of plants.

When exposed to air, lime attracts carbonic acid, and passes back to the state whence it was reduced by fire, but loses its figure and falls slowly to powder. When water is thrown on lime, it undergoes the process called slaking, and falls rapidly to powder, producing a hydrate, which is a combination with water. This powder, when exposed to the air, also rapidly passes into carbonate of lime. Both lime and its combination with water promote the decomposition of animal and vegetable substances, and absorb the gases which are generated by their putrefaction. The latter property is also possessed by the carbonate of lime, when in a state of fine powder, or even when it merely exposes a large surface to their action, and although rarely mentioned by chemists, and in itself purely mechanical, is of the most familiar character, being habitually called into use in our domestic economy, to correct offensive effluvia, and absorb the miasmata which produce disease.

The two other earths, as usually found in soils, (silica the base of flint and sand; and alumina, the base of clay,) appear to possess this mechanical property, either not at all or in a very inferior degree.

Lime, when mixed with these earths, gives them this property, and at the same time modifies their characters in a most remarkable manner. Silica, which has no attraction for moisture, is rendered retentive of water by lime; and clay which forms with abundance of water, a plastic paste, and hardens, on partial drying, into a tough clod, loses its plasticity and is rendered friable.

Lime in the form of carbonate, still possesses these mechanical properties, and thus limestone, or shells even coarsely powdered, will absorb the gases of putrescent bodies, render sand more retentive of moisture, and convert a stiff clay into a friable loam.

The alteration of the mechanical character of soils, is the first and most obvious effect produced by lime. For this purpose, it may be applied either caustic, as prepared by burning, merely pulverized from chalk or limestone, or in the form of disintegrated shells. The caustic form is more rapid but not more sure in its action, and the burning of limestone, when this is the object, is no more than a cheaper mode of reducing it to powder, through the intervention of slaking.

When the soil contains inert vegetable matter, which has ceased to undergo fermentation, caustic lime will promote the putrefactive process, and prepare it for the food of plants. If the process of fermentation has stopped after the acetic stage, and left the soil acid, either caustic or carbonated lime will neutralize the acid; it will not only thus prepare the soil to bear plants, which would not otherwise grow, but, by removing the antiseptic action of the acid, permit other vegetable matter to undergo putrescence, and thus supply an appropriate food for plants.

The principal part of the food of all plants is derived from the gases and soluble matters furnished by the decomposition of organic bodies. These gases tend to expand and distribute themselves through the atmosphere, with the exception of such as are soluble in the moisture of the soil, or such as the earth may retain by mechanical attraction. Lime and its compounds have by far the greatest powers in the last respect, and thus, if a soil is calcareous, it will retain these elements of fertility, and give them out only as they are required by the wants of the vegetables that grow upon it; it will be but little injured by exposure to the air when uncovered, and will retain its fertility longer. It will receive a greater quantity of animal and vegetable manure without poisoning the plants by excess of nutriment, and the effects of a given quantity will be longer sensible. If the soil contain no carbonate of lime, this property may be given artificially by manuring with slaked lime, which rapidly becomes carbonated; with powdered limestone or chalk; with marl in the proper sense of the term; or with recent or fossil shells.

Here it might at first be imagined, that the more free a soil is from calcareous matter, the greater the quantity of carbonate of lime which might be applied; but this is exactly the reverse of what is pointed out by experience. We do not consider that this fact is difficult of explanation. Lime cannot alter the mechanical texture of soils, nor give to the whole mass, the property of absorbing gases, except by entering into chemical combination with their other constituents. Such changes of character are not only the universal consequence of chemical action, but the strongest proofs of its having taken place. The case before us, is therefore, no more than one of a large class in chemistry, where it is necessary to apply a chemical agent much diluted at first, while, after the action has once commenced, the strength of the agent may be gradually increased with advantage; and in the same way, upon poor and exhausted soils, a large dose of calcareous matter may produce no benefit, or even positive injury, while a lighter dressing may produce immediate good effects, and prepare the soil to receive with advantage, larger quantities. The addition of putrescent manures, enables the lands to receive a larger dose of lime even from the first, and the successive dressings may in like manner be increased if green crops are ploughed in, or stable manure added. This is also consistent with theory, for the surplus lime may be advantageously employed in absorbing the gases generated by the decomposition. Still these are facts that, although we may explain them in conformity with chemical principles, could not readily have been predicted before hand.

One curious fact was observed by Mr. Ruffin, in his chemical analysis of the soils of Virginia, namely, that even in regions where the rock, by whose decomposition the soil was formed, was limestone, not only did no carbonate of lime manifest itself by effervescence with acids, but no precipitate was formed by the tests of lime in the acid solution. He could not admit that in such soils, lime was absolutely wanting, and therefore inferred, that it was in such a state of combination, as to be neither soluble in water, or decomposable by nitric acid. Such a combination is the oxalate of lime, but as he has made no direct inquiry into its presence, and some of his correspondents have questioned the probability of such an union existing, he has with the true spirit of an inductive enquirer, modified his assertion until farther proof be obtained. We however, do not think that so much modesty was necessary, for it can be shown that the formation of oxalic acid, and its consequent combination by superior affinity with lime, whatever may be its previous state of combination, are at least probable, if not certain.

The formation of nitric acid in calcareous soils, by the absorption of its two elements from the atmosphere, is not only rendered probable by the presence of nitre and nitrate of lime in the soils of many warm countries, but made certain by the construction of artificial nitre beds in France, when the foreign supply was cut off by the preponderance of the British navy. When this acid is formed in a soil abounding with vegetable matter, not yet decomposed, as is the case with many of our newly opened regions, oxalic acid *must* be formed.

Such then are the properties of lime, considered as a constituent of the soil: to bring its texture to that best suited for tillage, and for conveying the moisture, which is the vehicle of the food of plants, neither in excess or in defect; to condense and store up that gaseous food until needed; to promote the decomposition of inert organic matter; to neutralize acidity, and counteract its antiseptic action, thus removing a noxious principle, and opening a new supply of vegetable food.

One other use of lime in soils remains to be mentioned, and this does not seem to have occurred to Mr. Ruffin, but is strongly urged by M. Puvis, in a passage we have quoted. The ashes of all plants, when lixiviated, are composed either of the pure earths or earthy salts; of the latter, those of lime are by far the greatest in quantity, although probably altered by the fire from their original state. Thus, wood ashes contain the carbonate, sulphate, and phosphate of lime. In addition, the liquor by which the alkaline matter is separated by lixiviation carries with it earthy matter, which is rendered soluble by the alkali. Now it cannot be doubted, that if this alkaline, earthy and saline matter is not contained in the soil, the plant must be stinted in its growth or actually die. Thus, to many plants, lime or some of its compounds forms an essential article of food; it may be required in less quantity than carbon, hydrogen, oxygen, and water, but is not the less necessary to their growth. Now, former analyses did not shew, that lime and its compounds are contained in the stalks and seeds of the cereal gramina, but the existence of earthy matter, undissolved by water or alcohol, or left as a residuum of combustion, was well known. More recent investigations have shewn, that lime is one of the constituents of this residuum. Silex certainly exists in the stalks, and particularly at the joints of the gramina, being in some actually visible to the naked eye.

It is upon the same principle only, namely, that it serves as the food of plants, that we can account for the effects of gypsum upon certain crops, which in any other mode of viewing them would appear miraculous. This sulphate of lime, if merely ground, has no attraction for moisture, and if burnt would rather absorb it from the soil than the air; it cannot therefore act, as some have supposed, to increase the deposit of dew; it is so sparingly soluble in water, and so inert, that it cannot act as a stimulus, nor is it certainly followed by exhaustion, as all stimuli must be. But although sparingly soluble, it is still conveyed by water into the bodies of plants, although in small quantities, and as that water undergoes the chemical changes, which we know are induced by the vital action of plants, the sulphate of lime must be deposited in them, injuriously perhaps to some, but as a necessary food to others. Thus, it has been found, that the ashes of clover, lucern, and many of those plants whose growth is known to be most certainly promoted by gypsum, uniformly contain sulphate of lime; it is, therefore, their essential and appropriate food. Some soils may contain it naturally—here an addition will not increase the crop of such plants; some may contain a substance which will decompose the sulphate, and thus prevent its action. The earth baryta will separate the sulphuric acid from lime, and thus may produce this effect; but this is so rare, that injury from this cause can hardly be anticipated. Oxalic acid will separate the lime from the sulphuric acid, and thus will not only render the application of gypsum inefficient, but will also set the acid free to act injuriously. But if the oxalic acid has been previously neutralized by lime, the sulphate remains unaltered, and is capable of aiding the growth of clover and other plants of the sort. In confirmation of this view of the subject, we may quote the experience of Mr. Ruffin. His soil would not produce clover, even with the aid of plaister; and this being known, he did not attempt to use it, until encouraged by a spontaneous and luxuriant growth of white clover. We may also state a fact, within our own knowledge. It is a general belief, that in the neighborhood of the sea plaister is useless, and it has been so found in general on the Island of New York and in the adjacent county of Westchester, yet upon a narrow ridge of magnesian marble, which lies between the layers of gneiss and mica slate which form the greater part of this region, we have seen plaister used with great success.* Yet clover, when aided by putrescent manures, is a successful crop, on the gneiss, without the artificial aid of plaister; but this is readily accounted for, as sulphuret of iron is not rare, which being decomposed, and thus yielding sulphuric acid to combine with the lime of the feldspar, supplies the necessary food of the plant. In England, too, where a saline air, prevailing almost universally, is generally supposed to forbid the use of gypsum, there are calcareous soils on which it is beneficial, as in Kent, which is as much exposed to the blast of the sea as almost any part of the kingdom. Among the many advantages of calcareous manures, then, may be mentioned, as not the least, that it will prepare the way for the introduction of the clover husbandry, in regions where it would otherwise be impracticable.

Mr. Ruffin's own experience is limited to the regions of Virginia within reach of the tide, and to these his remarks are, with a just philosophical spirit, restricted, yet his extensive experience and accurate observation furnish the ground for a theory which must be useful, if properly understood and applied, in any district whatsoever.

The soils of this region were, on examination, found to be wholly destitute of calcareous earth, except a few isolated spots inclosed within them, which were observed to manifest, even to the eye, fragments of shells, and yielded lime on analysis. These spots were proverbial for their fertility, and remarkable for the fact, that on some of them long continued successions of the same crop had been annually cultivated, without absolute exhaustion. On a careful examination, Mr. Ruffin found that these shells, supposed to be the relics of Indian encampments, were the outcrop of fossil layers, and he succeeded in tracing these layers to his own land.

"My use of calcareous earth, as a manure, has been almost entirely confined to that form of it which is so abundant in the neighborhood of our tide waters, the beds of fossil shells, together with the earth with which they are found mixed. The shells are in various states—in some beds generally whole, and in others reduced nearly to a coarse powder. The earth which fills their vacancies, and makes the whole a compact mass in most cases, is principally siliceous sand, and contains no putrescent or valuable matter, other than the calcareous. The same effects might be expected from cal-

* It has been used, with entire success, on lands washed by the sea, at Stamford, in Connecticut, by the late Mr. Moses Rogers.—*Ed.*

careous earth in any other form, whether chalk, limestone, gravel, wood ashes, or lime, although the two last have other qualities besides the calcareous."

"During the short time that lime can remain quick or caustic, after being applied as manure, it exerts (as before stated) a solvent power, sometimes beneficial, and at others hurtful, which has no connexion with its subsequent and permanent action as calcareous earth."

"These natural deposits of fossil shells are commonly, but very improperly, called marl. This misapplied term is particularly objectionable, because it induces erroneous views of this manure; other earthy manures have long been used in England, under the name of marl, and numerous publications have described their general effects and recommended their use. When the same name is here given to a different manure, many persons will consider both operations as similar, and perhaps may refer to English authorities for the purpose of testing the truth of my opinions and the results of my practice. But no two operations, called by the same name, can well differ more. The process which it is my object to recommend, is simply the application of calcareous earth, in any form whatever, to soils wanting that ingredient, and generally quite destitute of it; and the propriety of the application depends entirely on our knowing that the manure contains calcareous earth, and what proportion, and that the soil contains none."

This being his object, he enforces the use of calcareous manures, both by example and a well grounded theory.

The comparative effects of the marl in various proportions, of stable and cowpen manure, and of marl mixed with manure, were fairly tried, and tested with the product of land without addition. We have not time to enter into the detail of the operations, and of the several products. It is sufficient to say, that by the mixture of marl and manure, the crop of corn was raised from twelve to thirty six bushels per acre; by eight hundred bushels of marl, from twelve to an average of twenty three; by four hundred and fifty bushels of marl, from twelve to somewhat less than twenty seven; that the mixture and the last named dressing of marl alone, showed a subsequent increase when corn returned in a four years' rotation, while the others showed an average decrease; that the wheat crops were increased in about the same ratio; but that clover, which before had not grown at all, became a good crop on the marled patches. This was upon soil which had before been in cultivation. Similar results followed upon land newly reclaimed from the forest. The next important inference to which we shall refer, is the advantage derived from the use of gypsum, upon soils which have been dressed with marl. This inference was reached, by observing the effect of a layer of the fossil shells, which contained sulphate of lime.

On one point only are we compelled to dissent from Mr. Ruffin. He states it broadly as his belief, that a soil may be either so constituted naturally, or so improved by the artificial application of carbonate of lime, that it shall never thereafter need a farther supply. There will no doubt be a limit reached in both ways when an addition of lime will be injurious, but as this substance is in fact a necessary part of the nutriment of many plants, their growth will slowly remove it, and the time may at last come when a new supply will be necessary, to restore or retain the original natural fertility. But soils, thus prepared by nature or improved by art, may retain for long periods of time their power of giving value to the original vegetable matter in the soil, or the putrescent manures applied, either in the form of green crops or of stable manure, and the English saying, that no man need marl his field a second time is founded on sound observation. Soils of such a character either native or artificial are to the husbandman, what labor sawing machines are to the mechanic, they are in fact engines by which the greatest possible return may be obtained by the least expenditure of labor. No part of the sea board of the United States, with the exception of small and isolated districts, presents soils of this character. It is only after passing the first range of mountains that a limestone formation is met with. This is or was proverbial for its fertility, of which the Fishkill, Swetara and Shenandoah vallies are well known instances. The same formation can be traced to the valley of the Schoharie, in New York, and it is recorded by tradition to have had even a higher character than any of the others; its glory has however, departed, and the Genesee, which is a similar formation, now holds the rank in public estimation, the Schoharie once possessed. Both the valley of the Schoharie, and the vicinity of Lancaster, Pa., were occupied by German settlers. The former Palatines, the latter of more northern birth; the former were ignorant of the use of lime, the latter brought it with them, and being fortunately no chemists, saw no impropriety in applying it in small quantities, and at intervals, to a soil originally calcareous. The difference of the results, is an interesting illustration of the value of this manure. In the valley of the Schoharie, land tilled with equal industry and economy, will not command a fourth of the price of that of the vicinity of Lancaster.

The experiments of Mr. Ruffin, were made, as we have stated, upon the soils of lower Virginia. He introduces his description of them in the following striking manner.

' " During several days of our journey, no spot was seen that was not covered with a luxuriant growth of large and beautiful forest trees, except where they had been destroyed by the natives for the purpose of cultivation. The least fertile of their pasture lands, without seeding, are soon covered with grass, several feet in height; and unless prevented by cultivation, a second growth of trees rapidly spring up, which without care or attention, attain their giant size in half the time that would be expected in the best soils in England." '

" If the foregoing description was met with in a "journey through Hindostan," or some equally unknown region, no European reader would doubt that such soils were fertile in the highest degree—and even many of ourselves would receive the same impression. Yet it is no exaggerated account of the poorest natural soils in our own poor country, which are remarkable for producing luxuriant growths of pines and broom-grass, as for their unproductiveness in every cultivated or valuable crop. We are so accustomed to these facts, that we scarcely think of their singularity; nor of the impropriety of calling any lands barren, which will produce a rapid growth of any one plant. Indeed by the rapidity of that growth, (or the fitness of the soil for its production,) we have in some measure formed a standard of the poverty of the soil."

" With some exceptions to every general character, the tide-water district of Virginia, may be described as generally level, sandy, poor, and free from any fixed rock, or any other than stones, rounded apparently by the attrition of water. On much the greater part of the lands, no stone of any kind is to be found of any larger size than gravel. Pines of different kinds, form the greater part of a heavy cover to the siliceous soils in their virgin state, and mix considerably with oaks and other growth of clay land. Both these kinds of soil after being exhausted of their little fertility, by cultivation, and "turned out" to recruit, are soon covered by young pines, which grow with vigor and luxuriance. This general description, applies more particularly to the *ridges*, which separate the *slopes* on different streams. The ridge lands are always level, and very poor, sometimes clayey—more generally sandy, but stiffer than would be inferred from the proportion of siliceous earth they contain, which is caused by the fineness of its particles. Whortleberry bushes as well as pines, are abundant on ridge lands, and numerous shallow basins are found, which are ponds of rain water in winter, and dry in summer. None of this large proportion of our lands, has paid the expense of clearing and cultivation, and much the greater part still remains under its native growth. Enough however has been cleared and cultivated in every neighborhood to prove its utter worthlessness, under common management. The soils of ridge lands vary between sandy loam and clayey loam. It is difficult to estimate their general product, under cultivation; but judging from my own experience of such soils, the product may be from five bushels of corn, or as much wheat to the acre, on the most clayey soils, to twelve bushels of corn and three of wheat on the most sandy—if wheat were there attempted to be made."

"The *slopes* extend from the ridges to the streams, or to the alluvial bottoms, and include the whole interval between neighboring branches of the same stream. This class of soils, forms another great body of lands of a higher grade of fertility, although still far from valuable. It is generally, more sandy than the poorer ridge land, and when long cultivated, is more or less deprived of its soil, by the washing of rains on every slight declivity. The washing away of three or four inches in depth, exposes a sterile subsoil (or forms a "gall,") which continues thenceforth bare of all vegetation; a greater declivity of the surface serves to form gullies several feet in depth, the earth carried from which, covers and injures the adjacent lower land. Most of this kind of land has been cleared and greatly exhausted. Its virgin growth, is often more of oak, hickory and dog wood, than pine; but when turned out of cultivation, an unmixed growth of pine follows. Land of this kind in general, has very little durability; its usual best product of corn, may be for a few crops, eighteen or twenty bushels, and even as much as twenty-five bushels, from the highest grade. Wheat is seldom a productive or profitable crop on the slopes, the soil being generally too sandy. When such soils as these, are called rich or valuable, (as most persons would describe them,) those terms must be considered as only comparative, and such an application of them proves, that truly fertile and valuable soils, are very scarce in Lower Virginia."

"The only rich and durable soils, below the falls of our river, are narrow strips of highlands along their banks, and the lowlands formed by the alluvium of the numerous smaller streams, which water our country. These alluvial bottoms, although highly productive, are lessened in value by being generally too sandy, and by the damage they suffer from being often inundated by floods of rain. The best highland soils, seldom extend more than half a mile from the river's edge, sometimes not fifty yards. These irregular margins are composed of loams of various qualities, but all highly valuable; and the best soils are scarcely to be surpassed, in their original fertility, and durability under severe tillage."

"The simple statement of the general course of tillage, to which our part of the country has been subjected, is sufficient to prove that great impoverishment of the soil, has been the inevitable consequence. The small portion of rich river margins, was soon all cleared, and was tilled without cessation for many years. The clearing of the slopes was next commenced, and is not yet entirely completed. On these soils the succession of crops was less rapid, or from necessity, tillage was sooner suspended. If not rich enough for tobacco when first cleared, (or as soon as it ceased to be so,) land of this kind was planted in corn, two or three years in succession, and afterwards every second year. The intermediate year between the crops of corn, the field was "rested" under a crop of wheat, if it would produce four or five bushels to the acre. If the sandiness, or exhausted condition of the soil, denied even this small product of wheat, that crop was probably not attempted, and instead of it the field was exposed to close grazing, from the time of gathering one crop of corn, to that of preparing to plant another. No manure was applied, except on the tobacco lots; and this rotation of a grain crop every year, and afterwards every second year, was kept up as long as the field would produce five bushels of corn to the acre. When reduced below that product, and to less than the necessary expense of cultivation, the land was turned out to recover under a new growth of pines. After twenty or thirty years, according to the convenience of the owner, the same land would be again cleared, and put under similar scourging tillage, which would then much sooner end as before, in exhaustion. Such a general system is not yet every where abandoned, and many years have not passed, since such was the usual course on every farm. How much our country has been impoverished during the last fifty years, cannot be determined by any satisfactory testimony."

"But, however, we may differ on this head, there are few who will not concur in the opinion, that our system of cultivation has been every year lessening the productive power of our lands in general, and that no one county, no neighborhood, and but few particular farms have been at all enriched, since their first settlement and cultivation. Yet many of our farming operations, have been much improved within the last fifteen or twenty years. Driven by necessity, proprietors direct more personal attention to their farms—better implements of husbandry are used, every process is more perfectly performed—and, whether well or ill directed, a spirit of inquiry and enterprize has been awakened, which before had no existence.—Throughout the country below the falls, and perhaps thirty miles above, if the best land be excluded, say one tenth, the remaining nine tenths, will not yield an average product of ten bushels of corn to the acre; although that grain is best suited to our soils in general, and far exceeds in quantity, all other kinds raised. Of course, the product of a large proportion of the land would fall below this average."

"Such crops can not in many cases remunerate the cultivator. If our remaining woodland, should be at once brought into cultivation, the gross product of the country would be greatly increased, but the net product, very probably diminished—as the general poverty of these lands would cause more expense than profit, to accompany their cultivation under the usual system. Yet every year we are using all our exertions to clear woodland, and in fact, seldom increase either gross or net product—because nearly as much old exhausted land is turned out of cultivation, as is substituted by the newly cleared. Sound calculations of profit and loss would induce us to reduce the extent of our present cultivation, by turning out every acre that yields less than the total cost of its tillage."

The green sand formations which yield the marl of Mr. Ruffin, are to be found also in Maryland and in Jersey. In the latter State, its use was commenced as early as 1805, by the Rev. John Singleton of Talbot County, and has been continued to the present time with great success. His example has been followed by his neighbors in the same county, and the practice has extended into Queen Ann's; but it does not appear that it has yet been introduced on the western shore of the Chesapeake. In Jersey, the county of Monmouth, at one time considered almost irretrievably barren, has been raised to great productiveness by the use of this manure.

This formation, or one analogous in its fertilizing character, is however of no frequent occurrence, it therefore remains for us to enquire whether other forms of calcareous matter may not answer the same purpose. Such is the extensive diffusion of lime in nature, that there are few or no regions where it cannot be procured. Even in primitive regions, beds or veins of granular carbonate of lime, are of occasional occurrence, and in those which are alluvial, the remains of recent shell fish are not wholly wanting. Much, however, will depend in the success of its application, upon the quantity which is required, as it may happen that when this is considerable, the expense of transportation may equal, or more than counterbalance the benefit it is calculated to produce. This question can be solved only by reference to the practice of those countries, in which lime has been applied with success.

In the north of England, and in Scotland, the use of lime as a manure, may almost appear to be excessive. Two hundred bushels per acre are often applied to sandy soils, and from three to four hundred on clay. Dressings of this amount are renewed once in every term of twenty one years. This high rate of application could not however be practised upon land not yet habituated to its use, and would in most parts of the United States, be too costly to yield any profit.

In England the lime is usually laid in small heaps on the fields, in its caustic state, and spread as soon as it becomes air-slaked.

In the department of l'Ain in France, the dressings are about eighty bushels to the acre, and are applied as a preparation for every grain crop.

The lime here, is also laid in heaps in its caustic state, but these are immediately covered with earth, which remains until the lime is slaked, when the earth and it are intimately mixed, and after having rested for a fortnight, are again thoroughly incorporated. In this state they remain for another fortnight, when the whole is uniformly distributed over the ground.

In Flanders the quantity of lime applied, is from forty to fifty bushels to the acre, and the dressing is not repeated oftener than once in ten or twelve years. The lime is usually mixed with the ashes of bituminous coal or of turf, or formed into a compost with other manures.

In the department of La Sarthe, the lime is applied once in three years, and in the form of compost.

"For that, there is first made a bed of earth, mould or turf, of a foot, or thereabout, in thickness. The clods are chopped down, and then a layer of unslaked lime is spread over them, at the rate of a hectolitre for twenty cubic feet, or a ton to forty five cubic feet of earth.* Upon this a second layer of earth, of the same thickness as the first, is placed; on this is laid a second layer of lime; and the whole covered by a layer of earth. If the earth is moist, and the lime recently burnt, eight or ten days will suffice to slake it completely. Then the heap is cut down and well mixed—and this operation is repeated afterwards before using the manure, which is delayed as long as possible, because the power of the effect on the soil is increased with the age of the compost, and especially if it has been made with earth containing much vegetable mould."—*Puvis.*

The quantity used in La Sarthe is not more than twelve bushels to the acre, and is laid upon the land in alternate rows, with barn yard manure. In the opinion of M. Puvis, this method, although the least expensive, is the best, and it may be said to be within the reach of almost every American agriculturist.

The advantage of the use of lime may be stated in a few words: it is an essential part of the seed of wheat, and that valuable grain will not grow in any soil which does not contain it. It may, therefore, be reasonably hoped that the culture of this plant may, by the aid of lime, in this comparatively cheap mode, be restored in those districts whence it has long been banished.

In the United States the use of lime is limited to the districts into which the descendants of the Germans, who settled in Pennsylvania, have introduced the method they brought from their native country. It is usual to apply from thirty to forty bushels per acre, and in some instances one hundred bushels have been used to advantage. The limestone, by the analysis of Dr. Cooper, yielded in some instances as much as 16 per cent. of magnesia. It therefore comes into the class of magnesian limestones, the employment of which requires caution, for this earth absorbs carbonic acid from the atmosphere much more slowly than lime, and so long as it is uncombined may be injurious to plants.

The high price of grain in Great Britain, during the long wars of the French Revolution, acted as a stimulus upon the use of inferior

* These two quantities do not correspond, but as we have not the original to refer to, we are compelled to take the translation as we find it.

soils, and these were rendered arable by the use of lime; but the improvement thus produced has been permanent, and although a fall in the price of agricultural products may have lowered rents, or ruined those who had contracted to pay high ones, we have heard of no instance of its becoming expedient to abandon the soils once brought into tillage by the aid of lime. The most remarkable fact of all is, that many of the newly reclaimed lands, in districts formerly considered as sterile both by nature and climate, were raised to a higher value than those of ancient fame for fertile soil and favorable skies. Thus, twenty years since, the rents of Northumberland, Berwick, and Dumfries, were higher, per acre of arable soil, than those of Gloucestershire, and we were witnesses of the migration of tenants, from the northern to the southern region, at the request of landlords, who wished to introduce the Scottish husbandry, of which lime is the main support, into the fertile vales of the Severn.

We cannot conclude, without recommending the perusal of the works whose titles stand at the head of this article, to the attention of every American landholder. A skillful application of the principles they illustrate, would go far to check the annual decrease in fertility, which takes place in many of our districts; a decrease which has already made some of the regions once most productive in bread stuffs, importers of that necessary element of existence, and which subjects us most deservedly to the reproach of being unable to retain the blessings which Providence has showered with a lavish hand upon our country.

One other important consideration requires notice, and this is the change in healthfulness which the application of calcareous earth to soils abounding in vegetable matter, is likely to produce. Mr. Ruffin cites facts in which he is corroborated by M. Puvis, which would almost warrant the conclusion, that this will be an efficient remedy for the malaria, whose influence is extending itself over every part of our middle and southern seaboard. There are indeed causes, such as stagnant waters, mill ponds, and rice cultivation, which are beyond its reach; but there are other cases, where, if there be any reliance on the usual theory of the causes, calcareous manures ought to be efficient in checking an infliction, which drives the white population, during the summer and autumnal months, from many of the fairest portions of the United States, and in others materially shortens the average duration of human existence.

Art. XV.—*On the Resistance of Fluids; by* Prof. George W. Keely.

Waterville Coll. Feb. 22, 1836.

TO PROF. SILLIMAN.

Sir,—Mr. Blake has obliged your readers with a communication on the resistance of fluids. With your permission I will make a few remarks upon it. It is not my intention, however, to follow him through all his statements and reasonings. To save time, I propose merely to examine a few essential points.

Mr. B. asserts that the common theory assumes what is a *fundamental error,* viz., that the force of a fluid particle is as the velocity of the plane which it strikes perpendicularly; that the nature of the resistance Prof. Wallace proposed to determine is different from that of the resistance which I had in view, in my former communication; that Prof. Wallace's reasoning does not differ from that of the common theory, so far as I examined it; and that, at the same time, we are both wrong in our conclusions.

Whatever be the effect of Mr. Blake's reasonings on others, I am not convinced by them. Each of the above assertions I hold to be incorrect. I will consider them in order.

When Mr. Blake denies that the force of a particle is as the velocity of the plane, he must mean that momentum is not a measure of the moving force; a truth so obvious, that if we are to give it up, we give up the whole theory of Mechanics. This truth Mr. B. would not have denied, but for the supposed accuracy of his argument that the force of a fluid particle, in an indivisible instant, is as the square of the velocity of the plane. Mr. B. defines Force of resistance to be the action, "*in an indivisible instant of time,*" and he undertakes to prove that this force of resistance for a particle, is as the square of the velocity of the plane. Now I mean to show that, in attempting this, the force he determines *is not* the force of resistance according to his own definition; and, however surprising it may seem, that Mr. B. has actually assumed the same fundamental proposition as the common theory, and that too in the very argument he uses to prove it is a fundamental error: this argument I will now examine. It consists of three "analogies," which, for brevity, I will state by symbols; using v', v, f, and t, to express respectively the velocity communicated to a particle, the velocity of the plane, the force of resistance, and the time of the action of the plane on a particle. (Note, that Mr. Blake applies these analogies, to determine the action on the plane, of a stratum of fluid, of the thickness of a particle, and then infers the conclusion true of a particle, because the action of the stratum is as the action of a particle.) The analogies are these, $v' \propto v$, $v' \propto ft$, $t \propto \frac{1}{v}$; hence $v'^2 \propto f$; from which it is *inferred* that the force of the particle is as the square of the velocity of the plane. Now what do these analogies separately mean as applied to determine the "*force of resistance*" of a fluid particle. The first means that the whole velocity communicated to the particle is as the velocity of the plane. This is very true; and, by the way, it is remarkable that Mr. Blake did not see that as the force of the particle is as its velocity by its mass, it follows that the force of the particle is as the velocity of the plane. But what means the second analogy? It is a dynamical relation between velocity, accelerating force, and the time of the action of the force. It expresses the fact, that the whole velocity communicated to the particle, is as the accelerating force (f) into the time (t) *in which it acts.* Now, as a general truth how is that analogy obtained by the mathematicians? Why by *taking it for granted* that the force (f) acting on a body, produces, in any "*indivisible instant*" a certain velocity *which is its measure;* that in the second "*indivisible instant*" there is added another equal velocity *which is the measure* of the second impulse, &c. &c.; and thus they deduce finally that in any time (t) consisting of an infinite number of indivisible instants $v' \propto ft$. Now if Mr. B. uses this analogy, he must use it in the same way in regard to the fluid particle, viz.: *by taking it for granted* that the force (f) in any "*indivisible instant,*" produces a certain velocity in the particle *which is its measure;* and that, in the second "*indivisible instant*" ...... but here is a dilemma; will Mr. Blake stop here, or go on? If he stops, then *the force is as the velocity,* and so indeed he acknowledges it is, if he goes on, as every one must see; but if he goes on, then the force of the particle which he determines is not the "*force of resistance,*" for it does not take place in "an indivisible instant." But Mr. B. prefers to go on, and now, what more, I would ask is necessary to prove what I proposed to do, viz., that he has actually allowed to be true what he himself styles the fundamental error of the common theory; and, moreover, that setting out to determine the "*force of resistance,*" he has deduced a quantity of a very different nature. But I have not yet done with this analogy, $v \propto ft$; is (t) constant? In other words, does the time of the action of the force on the particle vary according to the velocity of the plane? Mr. B. must say that it does, for if not, we have $v \propto f$, the conclusion he takes so much pains to avoid. Indeed he does say so, by giving the ratio of the variation in the third analogy, $t \propto \frac{1}{v}$; which means that the times of the action, on a particle, of planes moving with different velocities, are inversely as the velocities: so here we have another difficulty; this "*indivisible instant*" (t) not only consists of an infinite number of indivisible instants, but varies with every varying velocity of the plane.

But disregarding the conflict between the definition and the argument, and admitting that the kind of action on a particle, implied in the argument is possible, and takes place, the conclusion is true and important that the force (not Mr. Blake's force of resistance) is as the square of the velocity. The following is a question to the solution of which that very argument may be applied. Suppose a plane to act with different velocities, and perpendicularly to its line of motion on a fluid mass of constant or given amount, how would the resistances be? The answer is, as the squares of the velocities. Indeed this is the very question which Mr. B. has unconsciously solved; he may and does call his fluid mass a particle, or stratum of the thickness of a particle, but if this particle or stratum acts (as I have shewn he virtually admits) as a uniformly retarding force, it unquestionably has the property of a fluid constant mass of any extent. The truth is, when Mr. Blake calls the measure of the simple velocity a fundamental error, affirms it to be the square of the velocity, and offers the above argument to prove it, he raises the very question which unaccountably agitated all scientific Europe for forty years about the measure of forces, whether it was the velocity or the square of the velocity, and which at length died away by a tacit admission of the parties, that the Leibnitzians universally considered an element in their calculations as variable, which the Newtonians as universally considered constant.

Mr. B. moreover asserts, that Prof. Wallace and the common theory determine two very different things. "Prof. Wallace considers the number and effect of the particles at any (given) instant." "Prof. Keely and the common theory consider the effect and number of the particles encountered in a given time." Now, I ask what is a given instant but a given time? I will venture to say that Prof. Wallace would not admit that there is any difference between either his method or his result and those of the common theory, when the question relates to the action on planes perpendicular to their line of motion, and moving with different velocities. The difference between Prof. Wallace and the common theory is this: when equal planes are differently inclined to the line of motion, and moving with equal velocity, the common theory says that the number of particles striking the plane in a given time, depends on the inclination of the planes; Prof. W. says it does not. Prof. W. reasoned rightly from his premises; but they were wrong; to show which was the object of my former communication. I ought to remark that all I know of Prof. Wallace's views is from the paper of Mr. Gibbes.

Mr. B. further asserts that I pursue the subject in opposition to the views of Prof. Wallace, only to the point at which he first begins to diverge from the common theory. The truth of this assertion depends on that of the preceding, which is yet to be proved.

Mr. Blake, at last, says, we are both wrong, and indeed that all that has been published on this subject is more or less wrong. Let the readers of the Journal decide. I wish to be brief. I have therefore made the truth of Mr. Blake's first assertion the main point in this discussion. He considers it fundamental; so do I; if I have proved what I proposed in regard to that, those who are interested will apply it, and they will find that Mr. B.'s error there has vitiated his reasonings.

It is unnecessary for me to deny or to admit the justness of the distinctions among forces which is made at the outset by Mr. Blake, since I have shewn that these distinctions are not regarded by him.

On first reading Mr. B.'s communication, I determined not to comment on it. But it occurred to me that there are many readers of the Journal who are not so well informed in theoretical philosophy, or, so much disposed to do it justice as Mr. Blake, and that his statements, if not contradicted, were just such as would promote that hostility of theoretical and practical science, which, I am sure, Mr. Blake himself has done much to destroy. I do not wish to conceal the fact, that the common treatises on Natural Philosophy are very defective, but let us not charge them with defects that do not belong to them; and above all, if the accuracy of the demonstrations and conclusions of such minds as Newton's must be impeached, let it be done, I will not say timidly, but cautiously, and with respect.

MISCELLANIES.

FOREIGN AND DOMESTIC.

1. *Alum* may be used for ornaments, like alabaster. When of a proper degree of solidity, it may be wrought with tools, polished, &c. When melted by heat, it may be cast into pasteboard moulds, and then polished or wrought. While in a melted state, it may be colored to suit the fancy. If rubbed with an excaustic of yellow wax, the appearance of marble or alabaster may be given to it. (J. G.)

2. *Cement.* (J. G.)—Calcined and pulverized shells, mixed into a paste with coarse or refuse oil, makes a cement, used in India for stopping the joints of boats, &c.

3. *A heater* or *calorifactor*, for preserving the heat of the body in attacks of cholera, or severe and protracted chills, is made with advantage, by forming a semi-cylindrical case of tin, which will cover the body when in bed, leaving an opening at one end for the neck, so that the head may protrude. This case is made double, with a space of four inches between the inner and outer sheet. One opening is left at the top, for the insertion of a funnel, through which hot water is to be poured, and another small opening for the escape of air. This case is to be pressed down, over the patient, when in bed, and the clothes packed round it. If covered with a blanket, it will, when charged with hot water, retain the heat a great while. It need not be *filled* with hot water. The steam which rises, keeps the upper part hot. The two sides should be connected by a tube, to equalize the flow of the water. In fifteen minutes the pulse has been raised from sixty one to eighty seven per minute. In rheumatism, and all cases in which sweating is indicated, this instrument may be effectually used. The water is drawn off by a stop cock at the bottom. (J. G.)

4. *Freezing mixture.* (J. G.)—Four pounds of pulverized sulphate of soda, (not efflorescent,) and three pounds of cold dilute sulphuric acid, (seven pounds strong acid and five pounds of water, mixed the day before using.) I have prepared by this process more than three hundred pounds of artificial ice.—Boutigny. *D'Evreux.*

5. A good *Safe,* or victual preserver, is prepared, by making it of a double case of wire gauze, and filling the interval with fresh charcoal, in fine pieces. Fresh meat, when suspended by hooks from the top, will keep good and sweet for a week in this safe, in the hottest weather. (J. G.)

6. *Cure for Cramp.* (J. G.)—A bar of iron, placed across the bed on which the person sleeps, under the mattrass, about as high from the foot as the calf of the leg, is said to be an effectual preventive. The bar may be an inch square. In defect of a bar, a poker or other iron will answer temporarily. If there be two mattrasses, it may be placed between them. This remedy was strongly recommended by Dr. Chretienne, of Montpelier, and has proved availing in a vast number of cases.

7. *Excellent ink, and easily made.* (J. G.)—Into a ten gallon keg, put three pounds of copperas, well pulverized. Take three pounds of logwood, and boil it in six or seven gallons of rain or pure river water, and when it has boiled half an hour add four pounds of nut galls, broken up, and a quarter of a pound of alum. After another half hour's boiling, pour the whole of the materials into the keg, stir the contents well together, and let it remain a week, stirring the whole several times a day. Then put into the keg half a pound of gum arabic, in powder, and one pound and a half of sugar candy. Leave the mixture a week longer, stirring frequently. After three weeks' rest and settling, the ink may be used at pleasure, growing better with age.

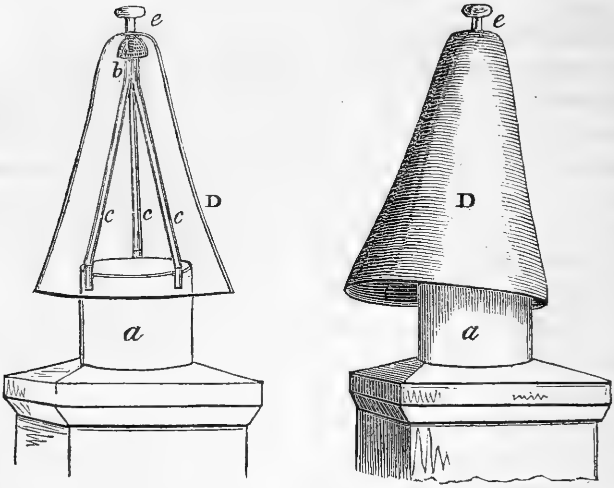
To keep it from moulding, add a dram of cloves and cinnamon, in powder, with an ounce of anise seed.

To render the ink of a beautiful blue black, add to the above contents a quart of sulphate of indigo. The latter is prepared by taking a quarter of a pound of indigo, reducing it to small pieces, sprinkling a little water on it, and the next day add to it two pounds of sulphuric acid, and leave it to digest in a warm place.

8. *To silver iron.* (J. G.)—Add to a solution of silver in nitric acid, a portion of common salt. Wash the precipitate thoroughly on a filter, and let it dry. By rubbing this powder on the iron or steel, (previously coppered, by plunging it, with a clean surface, into a warm solution of sulphate of copper, and rubbing it with a polisher,)

with a little cream of tartar, a coating of silver may be established, which admits of a fine polish.

9. *Movable hood, for smoky chimnies.* (J. G.)—The following s described as a simple and effectual cure for smoky chimnies.



The flue of the chimney terminates in a cylinder of cast or strong sheet iron, (*a*,) one foot in diameter, firmly set in the top of the masonry. Three light iron rods, (*c*, *c*, *c*,) riveted to the cylinder *a*, rise about two feet above it, and unite in a piece of iron, (*b*,) of a triangular shape, and three or four inches long, and having a hemispherical termination. This half ball has a hole bored in its upper part, at least an inch deep and one fourth of an inch in diameter, and well tapered to receive the screw *e*, which is provided with a good thumb piece. This screw holds the hood *b* in its place, and serves as its axis of motion.

The cone D is of sheet iron, two feet long and two feet in diameter. When at rest, its base is horizontal. It has a truncation or flattening at the top, four inches at least in diameter, with a hole for receiving the screw. It is made somewhat concave, and the hood must be so adjusted as to turn freely on its axis.

When the wind blows strongly, the hood is pressed against the chimney on the windward side, and the smoke freely escapes on the opposite side.

When the chimney is to be swept, the hood is unscrewed and removed for the purpose, if necessary.

When the wind is variable, the hood is liable to rattle against the cylinder, and occasion an unpleasant noise. This may be prevented, by punching holes round the cylinder, and attaching to it, by means of wire, a band of thick list or double piece of cloth. The hood must extend at least an inch below the top of the cylinder.

10. *Method of coating Busts and Plaster Casts, so as to give them the appearance of marble; by* M. Pleuvarre. (J. G.)—Into a wooden tub or trough, put a strong and warm solution of alum. Into this plunge the bust or plaster cast, previously made perfectly dry, and let it remain therein from fifteen to thirty minutes; then suspend it over the solution, that the superfluous portions may drain off, and when it is cold, pour over it a fresh portion of the solution, and apply it evenly by a sponge or cloth. Continue this operation until the alum has formed a crystallized coating over the whole surface. Put it aside, and when perfectly dry, polish it with fine sand paper, or glass paper, and complete the polish with a cloth slightly moistened with pure water.

A wooden vessel is best for the solution, warmed by steam from a boiler, because metals are apt to color the solution. This coating gives greater solidity to the substance, and possesses the whiteness and transparency of the finest marble. It stands the attacks of moisture in any apartment,—is less subject to become soiled, and is as easily cleaned as marble.

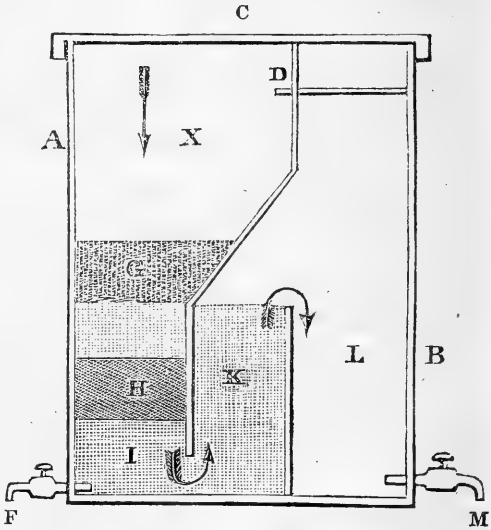
In this manner, excellent copies may be obtained of antiques, as well as moderns, at a price little exceeding common plaster casts.

11. *Iron Cement.* (J. G.)—The Fountaineers of Paris, make use of an iron cement, for uniting the stones which form their fountains. It is very strong, and may be employed in a variety of occasions.

Take one part of vinegar, and four parts of pure iron filings, stir them well together every hour for six hours, or until the mixture begins to form a good paste. To unite stones by this cement, clamps are to be first attached to the stones, which are to be very dry. The surfaces (of the stones,) to be united, need not be more than two lines (at farthest,) apart at the top, and to terminate below at the depth of five or six lines, at the distance of one line. The mastic once introduced into this space, the stones are to be pressed together, and the cement allowed to set. In a few hours, the surface may be polished, and the joint becomes as firm, as the stone itself.

12. *Filtration of Water for Domestic purposes.* (J. G.)—Many families and individuals are subjected to great inconvenience and often to the injury of health by the use of impure water. The water of wells and springs, is very frequently impure, not only from the ingredients which it holds in solution, but from earthy and foreign matters suspended in it. From the former, that is, the saline and calcareous matters which are completely dissolved in it, and which render it hard and unsavory, it cannot be deprived by filtration merely, but from those foreign substances, which destroy its transparency and make it turbid and unpleasant, the filter is an effectual remedy. Besides, there are few dwellings, whether in situations where the well water is naturally hard and injurious, to those who drink freely of it, or otherwise defective, from which, with a little attention, rain water may not be caught in sufficient quantity to answer for cooking and drinking, and this when passed through the filter we are about to describe, is perfectly fit for these uses.

The cheapest kind of filter, and at the same time, one of the best ever used, is the following.



A B, is a wooden box made of pine plank, which should be previously boiled several times in water, to remove all the resinous, or soluble parts which give taste to the water. C is a cover with a rim, and D is a sliding board, to keep the filtered water from dust. X receives the water to be filtered, which first passes through a bed of coarse gravel G, terminating in fine sand, and then through a bed of charcoal H, coarsely pounded, and again through sand, resting on fine gravel I. From this, the water passes through an opening one and a half inches high, at the bottom of the partition, into the compartment K, which contains only fine sand. The compartment L, receives the filtered water, which is drawn off through the stop cock M. F, is a cock for emptying the machine when necessary for cleaning.

The sand and gravel must be carefully washed before using, and the charcoal carefully selected and free from taste. If this box be nine inches square at the bottom, and thirteen inches high, it will be sufficient for an individual. Larger dimensions may adapt it to family use, and if made higher in proportion to its breadth than the above ratio, the filtration would be more thorough. The materials should be renewed once in five or six months, or oftener, if necessary.

13. *To render Oil casks impermeable.* (J. G.)—When the cask is new and ready to receive the oil, pour into it a concentrated and hot solution of sulphate of soda, (Glauber's salt,) spread it well over the whole interior surface by a sponge, cloth or broom, so that the wood may become thoroughly impregnated with the liquor. When it begins to grow cold, withdraw it, heat it again to boiling and renew the operation three or four times. Wipe off the superfluous salt with a coarse cloth, let it dry a few hours, replace the head, (the inside surface of which should have been treated in the same manner,) and it will be found that the pores have been effectually stopt by the salt, so that the oil may be safely introduced.

14. *To purify cold short iron,* a very simple process is practised in some bloomeries, which consists in throwing on the loupe at the moment when it is formed, half a shovel full of powdered flux, and keeping it afterwards exposed to the air of the bellows for a few moments, before it is carried to the hammer. The flux thus employed, is a limestone, which yields lime of good quality. Its effects on the loupe, are very prompt, depriving the iron of the siderite or phosphate of iron, which as is well known, renders the iron brittle when cold.—(J. G.)

15. *Method of Bronzing Iron and Gun barrels.* (J. G.)—Gun barrels when damasked, are less liable to rust, and any of them, of whatever price, may be treated by a very simple method, which will diminish their readiness to oxydize. When the iron is well scraped and cleaned, cover its surface with a coating of butter of antimony. If one is not sufficient, two or three coatings may be given. The iron thus acquires a horny reddish brown color, which is not unhandsome, and

which preserves it from rust. When the iron has acquired the desired tint, wipe it carefully, warm it a little and then rub it with white wax, until there remains no longer any visible traces of the wax. This renders its preservation complete.

16. *Medal of the Royal Society conferred on Mr. Lyell.*—Mr. Greenough, president of the Geological Society, at the annual meeting in February, announced that one of the Royal Medals, was awarded by the Royal Society, to Mr. Lyell, as the author of the most important discoveries or series of investigations sufficiently established or completed to the satisfaction of the council within the last five years, and for which no honorary reward had been previously received. The council of the Royal Society, premising that they decline to express any opinion on the controverted positions taken in Mr. Lyell's work, entitled "Principles of Geology," state the following as the grounds of their award.

1. The comprehensive view which the author has taken of his subject, and the philosophical spirit and dignity with which he has treated it.

2. The important service he has rendered to science by especially directing the attention of geologists to effects produced by existing causes.

3. His admirable description of many tertiary deposits, several of these descriptions being drawn from actual observations.

Lastly, the new mode of examining the tertiary deposits, which his labors have greatly contributed to introduce; namely, that of determining the relative proportions of extinct and still existing species, with a view to discover the relative ages of distant and unconnected tertiary deposits.

A notice is then given by Mr. Greenough, of the improvements in the third edition of Mr. Lyell's Geology, but we omit this notice because a fourth edition has recently appeared of which we have already made mention.

17. *Maize Sugar.* (O. P. H.)—Mr. Pallas has after repeated experiments succeeded in procuring a crystallized sugar from the stalks of Indian Corn, which bears a strong analogy to that extracted from beet root.—*Lond. Ath. April* 11.

18. *Introduction of Burden's Boat into France.* (O. P. H.)—Baron *Séguier*, member of the Institute, has constructed a boat

after the plan of Burden's, of two double cones, one hundred feet long, with the engine between them, which with the boiler presents some improvements.

M. *Cavé*, a mechanical engineer, has also constructed a double boat, for the navigation of the canal of Somme. It differs from the preceding in being open at the surface covered with a flooring and has two keels and two helms.

A similar boat has been constructed for the navigation of the Loire, between Nantes and Angers.—*Bul. Soc. Enc. l'Ind. Nat.*

19. *Meteorites.* (O. P. H.)—On the 8th of June, 1834, a stone fell at Charwallas, a village twenty three coss. (thirty nine three fourth miles) west of Hissar, near Delhi, Hindoostan. At about 8 o'clock in the morning, the sky was cloudy, and the weather gusty, or approaching to a north wester, but no rain,—very loud thunder, similar to constant discharges of heavy artillery was heard for about half an hour before it fell, and in the direction *with* the wind, to a great distance. When the stone fell, it was accompanied by a trembling noise, similar to a running fire of guns. It fell in the jungle close to a pelec or herdsman, who was out with his cattle. The original weight of the stone was twelve seers—but it was broken into fragments by visitors, and scattered. It bore the usual external appearances of meteoric stones. Sp. gr. 3.6, and affects a magnet.—*Jour. Asiatic Soc. Bengal.—Lon. and Edin. Phil. Mag., May*, 1835.

20. *Fall of a Meteorite in Moravia.* (O. P. H.)—At a quarter past six, P. M., on the 25th of Nov., 1833, M. Reichenbach witnessed the fall of a meteoric stone, accompanied by a brilliant light, and a noise like thunder, in the neighborhood of Blansko, in Moravia. The county was woody, and the larger mass could not therefore be discovered; but he succeeded in finding some fragments, weighing about half a pound, which resemble the stones that fell at Benares, L'Aigle, Berlongville, &c., so closely, that they cannot be distinguished from them. According to Berzelius, in one hundred parts, there are 17.15 of meteoric iron, separable by the magnet, containing small quantities of nickel, cobalt, tin, copper, sulphur and phosphorus—42.67 silicate of magnesia and protoxide of iron, in which the silica and base contain equal quantities of oxygen, together with some sulphuret of iron—39.43 silicate of magnesia and

protoxide of iron, mixed with silicates of potash, lime and alumina, in which the silica contains twice as much oxygen as the bases—0.75 chromate of oxide of iron mixed with oxide of tin. These proportions are subject to some variations in different portions of the stone. 100 parts of the meteoric iron, contain

Iron,	93.816
Nickel,	5.053
Cobalt,	0.347
Tin and copper,	0.460
Sulphur,	0.324
A trace of phosphorus.	

The remaining or stony portion of the mass, is partly soluble in hydrochloric acid. The soluble part consists of,

Silica,	33.084
Magnesia,	36.143
Protoxide of iron,	29.935
Oxide of manganese,	0.465
Oxide of nickel, containing tin and copper,	0.465
Alumina,	0.329
Soda,	0.857
Potash,	0.429
Loss, principally sulphur,	1.273

The insoluble part contains,

Silica,	57.145
Magnesia,	21.843
Lime,	3.106
Protoxide of iron,	8.592
Oxide of manganese,	0.724
Oxide of nickel, containing tin and copper,	0.021
Alumina,	5.590
Soda,	0.931
Potash,	0.010
Chromate of iron, containing tin,	1.533
Loss,	0.505

Pogg. Annalen.—Lon. and Ed. Phil. Mag. Feb. 1835.

21. *Carrara marble.* (O. P. H.)—It appears by the recently published account of this marble, by Professor Hoffinan, "that this pure saccharine limestone, in which no trace of organic matter has been discovered, although in its cavities are occasionally found crystals of quartz, *is only transformed oolite.*" A very wide expanse of ser-

pentine is seen in its neighborhood, and yet the Carrara marble is not magnesian. In the Isle of Skye, veins of serpentine sometimes penetrate the lias, where, in the vicinity of numerous whin dykes, it assumes the whiteness, and occasionally the sparkling grain, of statuary marble—and here again the marble is unadulterated with magnesia.

M. Dufrenoy, in a late number of the "Annales des Mines," has described a similar transformation of lias into saccharoid limestone, seen in the Pyrenees.—*Lond. and Ed. Phil. Mag. Sept.* 1835.

22. *Plenakite—new locality.* (O. P. H.)—Plenakite has been found, in very perfect crystals, accompanied by quartz, in the brown ore of Framont. Its specific gravity is 3.00; hardness equal to that of topaz; cleaves readily, parallel to the faces of a rhombohedron of 116° 40′, according to M. Beirich. It has been analyzed by Prof. G. Bizchof, of Bonn, who finds that its composition is expressed by the formula $\dddot{Be}+2\dddot{Si}$.—*Ibid.* (*Pog. Ann.*)

23. *A new Antimonuret of Nickel.*—(Ann. des Mines, T. VII, 3e liv. de 1835. Extracted from a report to the Soc. de Sc. de Göttingen, by M. STROMEYER.)—This mineral was discovered in the mountains of Andreasberg, with calc spar, galena, and cobalt. It resembles copper-nickel, but is distinguished by its color. It occurs in minute tables, with six faces, either grouped or isolated, and presenting the appearance of dendrites. Sometimes it is found in grains, rarely in large masses. The crystals—seldom longer than a line—appear to be hexahedral prisms, but do not admit of exact measurement. Their lustre is brilliant metallic. The fracture is brilliant, uneven, and conchoidal, and presents a copper red color, with a slight violet tinge; streak, a reddish brown, deeper than the surface of fracture. Its hardness is nearly that of copper-nickel, as it is scratched by feldspar, and scratches fluor spar. No odor is given off under the blowpipe, and fusion can be effected only in small pieces. Heated in a glass tube, antimony is sublimed. The analysis of this mineral makes it probable, that it is composed of an atom of nickel and an atom of antimony, and consequently it is analogous to copper-nickel, which is composed of equal parts of arsenic and nickel.—D.

24. *On a double Sulphuret of Antimony and Lead;* by C. BOULANGER. (Ann. des Mines, T. VII, 3e liv. de 1835.)—This mineral

occurs quite abundantly at Molières, department of Gard, where it is found in masses, whose fracture exhibits a crystalline structure; distinct crystals have not been obtained. It has a bluish gray color, with a metallic lustre, and a specific gravity of 5.97. It is frequently covered in spots with hydrate of iron, and also a yellow substance, which appears to be composed of the antimonic acid and the oxyd of lead, and to have proceeded from the decomposition of the mineral. Its gangue is quartz and iron pyrites.

It fuses readily under the blowpipe, with exhalations of sulphurous acid and the white vapors of oxyd of antimony. On charcoal, a yellow circle indicates the presence of lead. It is easily attacked by nitric acid, and gives rise to an Antimonate or Antimonite of Lead. Boiling, concentrated hydro-chloric acid dissolves it completely, with the extrication of sulphuretted hydrogen.

Its composition is as follows:

Sulphuret of antimony, - - - -	35.0
Sulphuret of lead, - - - -	62.1
Sulphuret of iron, - - - -	01.9
Sulphuret of copper, - - - -	01.1
	100.1

Neglecting the copper and iron, its formula is $\underline{\overset{'''}{Sb}}\,\dot{Pb}^{3}$.—D.

25. *Brevicite, a new Mineral;* by M. Sonden. (Neues Jahrbuch für Min. Geog., &c., von Leonhard und Bronn, *3es* H., 1835.)—This mineral was sent to Berzelius by M. Ström, from Brevig in Norway, where it exists, filling the cavities of a trachytic rock, and from which place it received its name. It occurs in white lamellar radiated masses, with transparent prismatic crystals, usually in the interior of the cavities. It is crossed by large bands of a deep dirty red color. Its composition is as follows;

Silica, ...	43.88
Alumina, ...	28.39
Soda, ...	10.32
Lime, ...	6.88
Magnesia, ...	0.21
Water, ...	9.63
Loss, ...	0.69
	100.00

Its formula is consequently $(\dot{N}a^3, \dot{C}a^3)\ \dddot{S}i^2 + 3\overset{\cdots}{\underline{A}}\ \dddot{S}i + 6\underline{\dot{H}}$. This substance appears to be a new zeolite ranking near Prehnite.—*Ann. de Pogg. B.* 33, *s.* 602.—D.

26. *Oerstedite;* by Forchammer. (Karsten Archiv f. Min. B. viii, S. 229, ff.)—The form of this mineral scarely differs from that of Zircon. The inclination of two adjacent terminal faces, is 123° 16′ 30″. Its hardness is between apatite and feldspar; specific gravity =3.629.—69 per cent of this mineral is titanate of zirconia, the remaining 31 per cent are constituted, as represented in the formula $(\dot{C}a, \dot{M}g\ \dot{F})\ \dddot{S}i^2 + Aq^3$.—D.

27. *Electricity of Peroxyd of Manganese.* (L'Institut, No. 117.)—M. Becquerel in a memoir on the particular electrical properties which mineral substances, conductors of electricity, acquire in contact with water, states that when a mass of peroxyd of manganese composed of a group of irregular crystals, was introduced, its half into water, the part of the specimen most distant from the water, was found in a short time to be negatively electrified, whilst that near the water, contained positive electricity, and that there was a point in which neither kind was apparent.—D.

28. *Paramorphine and Pseudomorphine.** (L'Institut, No. 107.) —M. Pelletier announces the discovery of two new substances in opium, which he terms Paramorphine and Pseudomorphine. The former is a white solid of an acid and styptic taste, scarcely soluble in water, but very much so in alcohol and ether. It differs from morphine in not reddening with concentrated nitric acid, in not forming crystallizable salts, and in not becoming blue in contact with the salts of iron. It resembles *Codeine*, in its solubility in alcohol and ether, and in its alkalinity; but unlike it, its salts do not crystallize, and it is always precipitable from its solutions by ammonia. It has no analogy with *Meconine* and *Narceine*, and a slight resemblance only to *Narcotine*, from which it is easily distinguished by its taste, fusibility and solubility in alcohol.

Pseudomorphine is nearly insoluble in water, still less soluble in alcohol, and entirely insoluble in ether. The most singular property

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* The discovery of this substance, was first announced, about a year since.—*Trans.*

of this substance, is that of becoming of a very intense blue, when in contact with the persalts of iron. This color disappears when there is an excess of acid, as is the case with morphine. It is not volatilizable, and not even completely fusible by heat, it being decomposed when about to melt. The following are the analyses of these two substances and morphine as made by M. Liebig:

	Carb.	Hyd.	Nit.	Ox.
Paramorphine,	71.31	6.290	4.408	17.992
Pseudomorphine,	52.74	5.81	4.08	37.37
Morphine,	72.20	6.24	4.92	16.66–D.

29. *A new Carburetted Hydrogen.* (L'Institut, No. 114.)—Dumas and Peligot, announce the discovery of a new compound of carbon and hydrogen, which like the carburet $C^4\ H^4$ and $C^8\ H^8$, will unite with two parts of water, and form a peculiar alcohol, and with the various acids, and produce compounds analogous to the common ethers. The composition of this new compound is represented by the formula $C^{64}\ H^{64}$. Thus there are four compounds of carbon and hydrogen, which contain these elements in the same ratio, but vary in their state of condensation, as the numbers 1, 2, 4, 16. The existence of the compound $C^{16}\ H^{16}$ has been proved, but its compounds remain yet unexamined.

The compound $C^{64}\ H^{64}$, is obtained by the distillation of ethal in connection with vitreous or anhydrous phosphoric acid. It is a colorless oily liquid, whose boiling point is about 260° C.

From the preparation of this substance, as also from the analysis of ethal, it is inferrible that this latter compound is composed of two atoms of water, and one of the new carburet, and may therefore be represented by the formula $C^{64}\ H^{64}+H^4\ O^2$; consequently ethal is a compound analogous to alcohol. To this base the name cetina is applied, as it is obtained during the saponification of spermaceti.

Dumas and Peligot have obtained a compound of cetina similar to the hydrochloric ether, represented by the formula $C^{64}\ H^{64}+Ch^2\ H^2$; also a substance, analogous in composition to the sulpho-vinate of potash, represented by $C^{64}\ H^{64}\ SO^3+KO,\ SO^3+H^2\ O$, and termed sulpho-cetate of potash. This compound resembles much a soap. The sulphocetic acid may be formed by mingling sulphuric acid and ethal. They have also found that spermaceti is a definite compound, consisting of one atom each of oleic and margaric acids, with three atoms of cetina, and three atoms of water. The hypothesis of Chevreul, that the fatty substances are compounds analogous to the ethers, is thus verified in the case of spermaceti.—D.

30. *On the improvements lately introduced into the Iron Founderies of Russia;* by M. Sobolewski. (Ann. des Mines, T. vii, 3e liv. de 1835.)—M. Sobolewski opposes the principle that has gained some ground, that any advantage results from the employment of heated air in furnaces, apart from the increased elastic force it thus receives, and states that a much greater heat is obtained, and a less consumption of coal required, when the air is made to enter with great force, that is, from a previous state of compression. M. Knauff, member of the scientific Society of Mines at St. Petersburg, by experiments made at the expense of government, finds that a hundred cubic feet of air, thrown into the furnace, under a pressure of two inches of mercury, produce the same result as two hundred cubic feet of air, under the pressure of an inch of mercury, with this difference, that in the latter case double the quantity of charcoal is consumed.

The economy in the use of coal, attained by some of the Russian mines, by the above means, is worthy of remark. In the founderies of the heirs of Raztorgonief, where are thrown out, in twenty four hours, seven hundred poods (nearly twenty five thousand three hundred pounds avoirdupois) of cast iron, only five hundred poods (nearly eighteen thousand pounds avoirdupois) of charcoal, (principally of birch wood,) is consumed, whilst formerly twice this quantity was used, for the same quantity of iron. In fact, since 1806, eighteen founderies of the Oural have annually economized nearly five thousand two hundred cubic feet, principally by a proper regulation of the quantity and pressure of the air—the former of these particulars is attained by varying the size of the tuyere, and the latter determined by a manometer.

The above results seem to prove a much greater effectiveness and economy in the process above described, than results from the use of heated air.—D.

31. *Cause of Dynamic phenomena.* (L'Institut, No. 113.)—M. Pelletier states the following facts, in support of the opinion he has advanced, that the transitory motion produced by every change in the molecular state of a body, is the immediate cause of dynamic phenomena.

When a wire of copper, gold, silver, or platinum, several yards in length, bent in the form of an arc, and connected with the ends of a galvanic multiplier, is gradually heated, an energetic current is excited, the positive fluid passing from the heated point through the end not heated, and the negative in the contrary direction. If the heat is applied to a part of the wire which is not curved, no current is produced. Iron, on the contrary, affords it in all circumstances. Having arranged a circuit of ten, or even twenty yards of unheated iron wire, the touch of the finger, or of any other object, or even the motion produced by a change of position of any part of the apparatus, was sufficient to induce an instantaneous current, dependant apparently on the unequal pressure of the molecules in different portions of the wire, and seeming to prove the truth of the supposition, that a change of equilibrium merely is necessary to produce an electric current.—D.

32. *On the use of locust wood for the timber work of subterranean galleries;* by M. Francois. (Ann. des Mines, T. vii, 3e liv. de 1835.)—It has been a source of much expense, and not less inconvenience, in the mines of France, that the timbers employed in the subterranean galleries have been rapidly destroyed by the "dry rot." The wood that is usually employed is the oak, and although the timbers were of considerable size, (from three and a half to eight inches in diameter,) they have seldom lasted longer than fifteen months, and usually the greater part have been rendered unfit for further use in the course of from three to seven months, and some in fifteen or twenty days. The substitution of the locust was first attempted by M. Chassignet, in 1830, whose experiments on this subject proved satisfactorily its superiority to the oak.

Under the influence of the subterranean heat, a yellowish viscous substance is formed, which perfectly protects the alburnum from the influence of the surrounding air. This viscous covering affords a protection to the wood for about eight months. The alburnum is then converted into a porous ligneous substance, to which the ulterior preservation of the wood is probably owing, the inner parts retaining to an indefinite period the healthy soundness and firm texture they possessed when first used.—D.

33. *On the cause of the "dry rot;"* by M. Aubuisson. (Idem.) The first cause of this disease appears to be a fungous vegetation,

arising in the interior parts of the wood, from the sap which remains in it. At first, the vegetation is scarcely perceptible. Shortly, the white filaments increasing in number, extend themselves towards the surface, and interlacing with one another, present an appearance somewhat like leather. The fibre of the wood is now attacked and corroded, and in the course of a short time the ligneous mass becomes a loose cellular tissue, readily falling into powder. Frequently the surface remains sound, while the whole central part is thus decomposed; but occasionally this process commences at the surface.

The immediate causes of the disease are dryness, heat, and a vitiated atmosphere. The presence of water, or even moisture, in the air, will prevent or lessen the rapidity of the development of the disease; though, if this moisture is accompanied by mephitic exhalations, it rather contributes to it.

If it is true that the sap promotes this disease, the wood which is stripped of its bark in the spring, and cut the following winter, is probably, as has been supposed, less subject to it.

In the mines of the Hartz, it is a very general practice, according to M. Regnault, to preserve the timbers by means of water, carried over them in troughs or lead tubes, which permit the water to pass out in small quantities on the timbers. This has been found to be quite a successful method. It has also been proposed to cause the wood that is to be used to absorb water, by placing it in this fluid under a strong pressure. The wood retains the absorbed water for a considerable length of time, and thus is not so soon attacked by the disease.—D.

34. *Taxidermy.* (L'Institut, No. 115.)—M. Gannal, proposes a solution of the following salts, for the preservation of animal substances, which from its cheapness and superior preservative qualities, seems to be preferable to the materials heretofore used.

Alum, - - - - - -	2 parts.
Chlorid of soda, - - - -	2 do.
Nitrate of potassa, - - - -	1 do.

Two dead bodies were immersed in a liquid containing these salts in solution, and at the end of two months were found to have undergone no change in their appearance. In general, the tissues and internal organs are perfectly preserved. Sometimes those immediately in contact with the fluid, lose their natural color, but farther than this, no change takes place. The muscular fibres offer less resistance to pressure, than is usual, in a body forty eight hours dead. It seems to be peculiarly well adapted for the preservation of the brain, as this organ, although thus kept for some months, will still serve for the demonstrations of the anatomist. This solution has also been used as an injection in anatomical preparations, and with perfect success.—D.

35. *Statue of* CUVIER *at Montbéliard.* (L'Institut, No. 122.)—This statue, erected by the Academy of Sciences, is of bronze, and a little larger than nature. CUVIER is represented in a standing posture, with a pencil in his hand, meditating on the remains of different fossil animals that lay before him, and appears to have just succeeded in determining a new species, by a union of its parts. The statue has been erected in the public square, before the Hotel de Ville of Montbéliard. On one side, may be seen the College where Cuvier first received his education, and on the other, the house in which he was born, on which is inscribed the date 23 Aout, 1769. The erection of the statue took place on the anniversary of this day.—D.

36. *Extraordinary application of Gas.*—[From the evidence of Richard Smith, Esq., before the Parliamentary Committee appointed to report upon accidents in mines.] Speaking of the coal mines of Nova Scotia, (observes the *Mining Journal,*) Mr. Smith says, "When we first struck the coal at the depth of about one hundred and eighty feet, it was highly charged with water; the water flew out in all directions with considerable violence; it produced a kind of mineral fermentation immediately. The outburst of the coal, crossed the large river which passed near the coal pit. We were not exactly aware of the precise outcrop, on account of a strong clay paste eight or ten yards thick. It is rather difficult to find the outburst of coal, when clay paste is thickly spread over a country. At the river, the water boiled similarly to that of a steam engine boiler, with the same kind of rapidity; so that on putting flame to it on a calm day, it would spread over the river, like what is commonly termed setting the Thames on fire; it often reminded me of the saying. It is very common for the females, the workmen's wives and daughters, to go down to the river with the washing they have to perform for their families. After digging a hole in the side of the river, about ten or twelve inches deep, they would fill it with pebble

stones, and then put a candle to it; by this means, they had plenty of boiling water without further trouble, or the expense of fuel. It would burn for weeks or months, unless put out. I mention this to show how highly charged the coal was with gas. What I am now going to describe, may be worth a little attention. There was no extraordinary boiling of the water, or rising of the gas, before we cut the coal at the bottom of the pit, more than is usually discernible in a common pond of stagnant water, when a long stick is forced into the mud. As soon as the coal was struck at the depth of one hundred and eighty feet, it appeared to throw the whole coal mine into a state of regular mineral fermentation. The gas roared as the miner struck the coal with his pick; it would often go off like the report of a pistol, and at times I have seen it burst pieces of coal off the solid wall, so that it could not be a very lightly charged mine, under such circumstances. The noise which the gas and water made in issuing from the coal, was like a hundred thousand snakes hissing at each other."—*Atheneum, No.* 427.

37. *Fossil Wax.*—Dr. Meyer has forwarded a specimen of fossil wax to the French Academy of Sciences, with all the details concerning it which he had been able to procure. It was found in Moldavia, at the foot of the Carpathian Mountains, covered with a stratum of clay slate, mixed with bitumen. M. Udreizky, a German, had bored a mine there, and in it found pieces weighing from eighty to one hundred pounds. The texture varies considerably; sometimes its fracture is fibrous, at others leafy; occasionally it is rippled; it is very pure and transparent at the edges, melts at a temperature of 40°, and yields a bituminous odor, by no means disagreeable. When washed in several waters, this substance assumes a deep yellow tint, and in this state, is employed in the manufacture of candles. Not far from the place where it was found, are several layers of brown amber, which leads M. Meyer to believe that it may be yellow amber disturbed while joining. Cold alcohol has no action upon it; when boiling it dissolves a small quantity, which in cooling precipitates itself in white flakes. The residuum acquires a deeper color and more tenacity. Ether, at an ordinary temperature, dissolves that part which gives the yellow color, leaving an almost colorless residuum. Alcohol and ether mixed, precipitate the dissolved portion, and this precipitate, exposed to fire, melts at a low temperature, and stains paper in the manner of oil. It is per-

fectly dissolved in oil of turpentine, and the solution coagulates in cooling. The alkalies do not turn it into soap. Sulphuric acid carbonizes it, even at a temperature which causes it to melt. It does not emit a flame when exposed to a candle. M. Paravey has been seeking among Chinese authors for an account of this fossil wax. He states, that in the book of Pen-Tsao, the *hou-pe* or *khou-pe*, is said to be formed as follows: the resin or grease of the wild pine or larch, left in the earth a thousand years, gives the fouling, a sort of excrescence from the roots of these pines or larches, which have been cut down even with the soil, and the presence of which is discovered by a luminous vapor rising over the spot. It is a rare and expensive substance, employed in medicine, and when combined with the still more precious roots of the *quiseng*, and left a thousand years, or a very long time, in the earth, gives the *hou-pe*, and if, after becoming *hou-pe*, it is again left for a thousand years, it gives the black stone *to*, or *to-pe* (evidently jet.) M. Brongniart says, in his 'Mineralogy,' "that with the Prussian amber, are often found the fruits of the Pinus abies; and the tree called in Chinese *Song*, from which the *hou-pe* is said to come, is the Pinus abies."—*Id.*

38. *Circulation in Insects.*—M. de Blainville has, in his own name, and that of MM. Dumeril and Bory, reported to the French Academy of Sciences some observations on circulation in insects. If the foot of a young insect of the genera Notonecta, Naucoris, and those of the family of Hydrocorisæ, order Hemiptera, of Linnæus, be magnified one hundred times, taking care that the foot be always attached to the living animal at the articulation with the thigh, a motion, more or less distinct, will be seen, varying in rapidity, but always regular, and capable of being accelerated or retarded, and even suspended for a time. It is to be observed as long as the animal lives, and even a short time after the limb has been separated from the body. M. Dufour, who has attempted to verify these observations, thinks, that this motion is merely vibration of the muscular fibres, and is even of opinion, that it is impossible there should be any thing like circulation in Hexapoda.—*Id.*

39. *Iron.*—Twenty years back, Dr. Portal, when analyzing some fragments of ancient lava near Mount Etna, found iron ore in them; more recently, Dr. Benedetto has discovered, close to the volcano, an extensive vein of this metal, presenting groups of octahedral figures.—*Ath. No.* 423.

40. *Rain.*—An abundant rain of Mollusca, genus Bulimus, species truncatus, took place at Montpellier, after a violent storm, which came from the west. The noise of the falling shells, resembled that of hail, and they might have been collected in thousands.—*Id.*

41. *Yale Natural History Society.*—An association bearing this name has recently been formed in this city, the object of which is the pursuit and critical investigation of Natural Science. The plan of such an institution originated about two years since, with a few individuals interested in natural science, and a society was then formed under the title of the "Yale Institute of Natural Science." From some unfavorable circumstances, however, it accomplished but little, and was fast becoming extinct, when a few months since, it was revived and *re*-formed under its present name. From the interest manifested by its members since its reorganization, it is presumed that this Society will now go forward and do honor to itself, and to our country.

Our country is one of vast resources, in every branch of Natural History, which yet remain to be rendered available to science, and it is only by the combined efforts of members of scientific associations, affording mutual incitement to exertion, that this object can be attained. Every institution of this kind, therefore, should receive the aid and patronage of the friends of improvement.

The Yale Natural History Society comes into existence, with as promising prospects, as—under present circumstances—its friends could expect for it. Funds to a considerable amount have already been pledged towards the establishment of a library of natural history, and a splendid collection of eastern birds has recently been received from one of its members, the Rev. Peter Parker, M. D. missionary at Canton. The Society already numbers, too, amongst its members, some of the most zealous and successful cultivators of natural science in this country, and it is hoped that with such prospects and in such hands, it will attain a high station amongst the scientific associations of the present age.

42. *Academy of Natural Sciences of Philadelphia.*—This Institution has just received a munificent donation from its President William Maclure, Esq. It consists of the whole of the Library purchased by him in Europe, for his establishment at New Harmony. This splendid collection of books embraces many of the most costly works in Natural History, the Fine Arts, Antiquities, Literature

and general science. The number of volumes is upwards of two thousand, viz:

Folios, - - - - - -	377
Quartos, - - - - - -	533
Octavos, - - - - - -	772
Duodecimos, - - - - -	577
	2259

When it is recollected that Mr. Maclure, on a former occasion, presented the Academy with fifteen hundred volumes, we cannot too much admire and commend the spirit of liberality by which he is actuated in the promotion of science. This gentleman is now resident in the city of Mexico, where, for the benefit of a genial climate, he is passing the evening of his life; and although at an advanced age, he seems to have lost none of his characteristic zeal for the promotion of useful knowledge.

43. *Medical and Physical Researches, or original Memoirs in Medicine, Surgery, Physiology, Geology, Zoology and Comparative Anatomy, illustrated by plates containing* 160 *figures;* by R. Harlan, M. D., F. L. S., &c. &c. &c.—This splendid volume, remarkable both by its size, (nearly 700 pages, large 8vo.) and by the number, and importance of the memoirs which it contains, deserves more than a passing notice; but neither our time nor space will suffice for more. There can be no doubt, that the volume will do great credit to American Science, as many of the memoirs which it contains, have already done.

Dr. Harlan's name, and deservedly high reputation as a naturalist, are too well known, both at home and abroad, to require our tribute of commendation.

We cannot however suppress our regret, that the *spurious rhinoceros jaw*, is again crowded into company, to which it has no claim whatever, among the undoubted fossil remains of extinct animals, some of which were remarkable by their magnitude, their structure and their habits.

No person admits the genuineness of the supposed rhinoceros relic, or perceives in it any proofs of animal origin.

Why then should Dr. Harlan continue to give the weight of his eminent name to countenance error, when he has done so much to enlarge the bounds of real truth, which is only another name for science.

44. *Peat, (turf,) its application to gas light;* by Dr. Lewis Feuchtwanger, of New York.—Great advantages may be anticipated from the introduction of peat, in making gas for gas light. 1, it is less expensive than the gas from either coal, oil or resin; 2, the produce is nearly as much as from those substances; 3, the gas is quite harmless and inoffensive, and has in respect to healthfulness great advantages over the others; 4, the peat after having been used for the production of gas, may be used for fuel, and is equal to any charcoal.

According to the experiments of Merle, who is director of a Gas Company in France, one thousand kilogrammes of peat when distilled like the stone coal for two hours, yields eight thousand cubic feet of gas, which is of rather weak luminating power, and contains much carbon, and which although apt to be purified by water, loses a great deal more of its strength; but if the same quantity is distilled for three fourths of an hour only, five thousand and five hundred cubic feet of a pure gas are obtained, which is said to afford a stronger and whiter light than coal or oil gas.

An apparatus, consisting of a condensator with eighteen tubes is fixed for purifying the gas completely; each tube stands in a reservoir of flowing water, so that the gas has to pass eighteen times through the water, and is not deprived of its carbon; before the gas arrives in the large gasometer, it has to pass through two layers of dry lime; the gas thus purified, may be respired without any difficulty.

The construction of all other apparatus, may be made like that for other gases.

45. *New mode of preparing Supercarbonate of Soda;* by Dr. Lewis Feuchtwanger.—It has a short time ago been stated that supercarbonate of soda, may be prepared by mixing ten parts of crystallized sal soda, and four parts carbonate of ammonia, and exposure to atmospheric air, which method was then considered too expensive for practical use, but more so, since the ammonia which ought to be collected at the same time, could not be well obtained on account of the quantity of crystalline water, which keeps the mixture always soft, and when evaporated again, gives a loss in carbonic acid gas; Schoy however, recommends to use the dried soda in combination with the crystalline, and ammonia, so that one atom of the soda should after the process, be combined with one atom of water. According to him, 5.0 dried soda, 1.5 crystalline carbon-

ate of soda, (sal soda) and 4.1 subcarbonate of ammonia are mixed together in a powder and put in a still, which heated in water or a vapor bath, carries off the vapors of ammonia and all the carbonic acid attaches itself to the soda, and we obtain 8.4 parts of beautiful white and pure bicarbonate of soda ; the ammonia so obtained is perfectly pure, and strong; but if it is not intended to be made use of, the above mixture may be made, covered with paper, and left for a few days exposed to the heat of a room, and the product will likewise be as good.

46. *On Veratria;* by Dr. Lewis Feuchtwanger.—Since Veratria has since the last year, become so important a medicine, having been recommended first by Dr. Trumbull, and now fairly ranks among the most salutary ingredients in materia medica, it must naturally be of great interest to the pharmaceutical and medical world, to obtain so valuable a substance in its perfect purity, that is, very white without being adulterated with foreign articles ; the author of these lines, takes this opportunity of comparing the methods hitherto pursued for the preparation, with the one lately recommenced by the pharmaceutist Simon of Berlin.

Veratria was dicovered in 1819 by Pelletier and Caventou, and at the same time by Meissner, in several plants of the genus Veratrum, and particularly in the root of Veratrum album or white hellebore, and in the seed of Veratrum sabadilla, sabadilla seed.

The sabadill seed was treated with sulphuric ether, which dissolved a volatile crystallizable acid, and a fatty and other substances; the residuum treated with boiling alcohol, a deep brown coloring matter is obtained, which is filtered off and evaporated to the consistency of an extract. Cold water will now dissolve this extract, except a small quantity of fatty matter which is filtered off; the solution however, has to be concentrated by evaporation, and filtered again, and then precipitated with sugar of lead, which yields a copious yellow precipitate and an almost colorless liquor, which, after having passed some sulphuretted hydrogen for removing any excess of lead, and filtered and evaporated again, is treated with magnesia. This precipitate when dissolved in boiling alcohol, which is afterwards to be distilled off again, yields a pulverulent substance, the veratria, which is yellowish, but which may be whitened by repeated treatments with alcohol and precipitations of water.

Couerbe's method is to make an alcoholic extract from which alcohol is distilled off, and this brownish red extract is now boiled with water acidulated with sulphuric acid, until a mineral alkali does not indicate any precipitate; by adding now a solution of potassa or ammonia, the base or veratria is precipitated in its yet impure state.

For obtaining it still purer, it is dissolved in very diluted sulphuric acid, and to the sulphate of veratria, so obtained, are added some drops of nitric acid, and the liquor is decomposed by potassa dissolved, and we obtain the alkaline matter, which is washed with cold water, and redissolved again with boiling alcohol, &c. Simon's method, however, as described in the Berlin Annals, is very simple; the seed is treated with boiling alcohol, which is distilled off afterwards, and the extract boiled with water acidulated with sulphuric acid, until subcarbonate of soda will no more produce a precipitate; the whole liquor is set aside for settling, during which period, the oil of sabadilla is separated and filtered from it, and then it is precipitated by subcarbonate of soda, so as to leave the fluid alkali; then put the kettle over the fire when the froth will at once be removed; before it begins to boil, veratria coagulates together, and may easily be removed. It is washed out with water, and discolored in the following manner: after having dissolved it in boiling alcohol, add then animal charcoal, and after agitating for some time, filter the fluid, which will, at last, become quite clear; evaporate the spirits of wine over a sand bath, and the remaining mass in a porcelain dish by means of water vapors. It is obvious now, in what an improved manner veratria may be obtained by this last process; according to the first, when the veratria was filtered off, and washed out with water and then redissolved again, concentrated and precipitated again with soda, the alkaloid was separated by pressing it between blotting paper, and must naturally suffer a great loss in the product; whereas in the latter, the alkaloid is separated from its acid solution, and it runs from itself by means of heat in the basic fluid to its proper substance. The product by the former process, was forty grains from the ounce of clear seed, and that obtained by the last process is fifty four grains, which is thirty three per cent more.

47. *Impressions of the feet of Mammalia in Sandstone,* (grês bigarré)—Extract of a letter to the editor, from Dr. Mantell, dated Brighton, England, Aug. 24, 1835.—A very interesting discovery

has been communicated by Humboldt, to the French Academy; the impressions of the feet of some kind of *mammalia*, on sandstone, belonging to the *grês bigarré*, at Hildburghausen in Germany: the account given by Humboldt, is excellent; he thinks the animal was marsupial, but the thumb was so much prolonged, so separated from the other fingers and toes, that it more resembles the quadrumana, than any other recent animals.

We observe in Prof. Jameson's Journal for July, a notice of this discovery, which states that the impressions were on the inferior surface of a bed of free stone, at the depth of fifteen and eighteen feet. The traces both of large and small animals, are easily discovered, which appear to have been going sometimes in the same direction, and sometimes in contrary. Invariably the point of a large foot, is succeeded by that of a smaller; the larger, which are about eight inches long and four broad, have at first sight, the appearance of a human hand, in which the thumb was placed far back; this is probably the indication that is thought to point to the quadrumana.

48. *Movements on the surface of water produced by the vibration of glass.*—Extract of a letter from C. G. Page, to the editor, dated Salem, Sept. 2, 1834.—I was moving my finger on the edge of a glass bowl about six inches diameter, for the purpose of observing the appearances on the surface of the water. During the vibration of the glass, the surface of the water was strongly agitated, presenting a reticulated appearance between four points, at, and about which there was little or no motion. It would seem then, that the vessel during vibration, was divided into four parts, each vibrating about these fixed points or nodes. But the following phenomenon which I noticed, is still more beautiful. When the agitation was more violent, and the bowl nearly full, the water would start up from between the nodes, in fine drops or spray, so as to rise high above and cover the top of the bowl, making altogether a beautiful experiment.

49. *Maryland Academy of Science and Literature.*—The Maryland Academy of Science and Literature, having lately had the misfortune to lose its valuable Museum and Library by fire, respectfully and earnestly appeals to those who feel a common interest in its pursuits, for aid in repairing its loss.

The assiduous labor of nearly thirteen years had amassed for it a large number of costly literary and scientific works, an extensive collection of minerals, specimens in almost every department of Natural History, and many coins, medals and antiquities. In commencing anew the work of accumulation, much, it is persuaded, can be done by the contributions of the public and similar Institutions. Most such societies and many individuals possess superfluous specimens and duplicates of Books, of but little or limited use to themselves, while of value to those who are without and need them. To associations or individuals who have it thus in their power to aid the Academy, it cannot be necessary, for securing that aid, to address any considerations. The cause of science and literature, is one of no merely local or private concern, but possesses an interest and importance of the widest character, and its followers, in what place soever they urge its extension, must be counted as of one brotherhood. To the same end all co-operate, and it no doubt must be the impulse of feeling, as it certainly is the dictate of duty, that each, where it is possible, shall lend encouragement and assistance to the other.

The collections which the Academy seeks to make, embrace all that can claim the attention of the literary and scientific. They include minerals, shells, fossils, specimens in Natural Science, books, coins, aboriginal antiquities, maps and documents illustrative of the history, geography, or literature of any portion of the world, and in particular of Maryland. Unpublished barometrical or thermometrical observations—descriptions of celestial or terrestrial phenomena, and State Statistics which have never been given to the world, are likewise among the means of information which it seeks to gather and make useful. Donations coming under the above description, or aught else that can attract the observation of the curious, or stimulate the inquiries of the learned, will be thankfully received and acknowledged. By order,

P. Macaulay, M. D., President.
Wm. R. Fisher, Secretary.

Baltimore, February 29th, 1836.

Residents in Baltimore and the country adjacent, are respectfully informed that Messrs. Tyson & Fisher in Market, between Charles and Hanover streets, and George W. Andrews, Esq. in the same street, near the Bridge, will take charge of any donations left with them, or sent to their address. Those who desire to transmit donations from a distance, will please address the Secretary.

For the information of those who feel disposed to aid in the attainment of the Academy's wishes as expressed above, it is observed, that quadrupeds and birds which cannot be sent entire, should be skinned, leaving the head and feet attached. The skins should be freely rubbed on the flesh side with arsenic or some other preservative, and afterwards filled with tow or cotton. Reptiles, fish, crabs and other crustaceous animals, spiders, the hard-winged insects, and soft animals of small size, may be preserved in spirits. Other insects should be impaled through the body with a small pin, and put up in closed boxes, their wings being properly expanded. Shells and minerals may be wrapped in paper, or when delicate and fragile, in cotton or some other soft texture. Plants should be pressed and dried between sheets of spongy paper. It is desirable that a note should be made of the locality of all specimens.

50. *Meteorological Observations*, made at the Apartments of the Albany Institute, Albany Academy, for thirty-seven successive hours, commencing 6 A. M. of the 21st Dec. 1835, and ending 6 P. M. of the following day. (Lat. 42° 39′ 3″, Lon. 73° 46′ 38″ W.)——The Albany Institute having learned that, "on four fixed days in each year, the 21st of March, 21st of June, 21st of September, and 21st of December, (unless any of these days should fall on Sunday, in which case for the 21st substitute the 22d,) horary observations of the barometer, thermometer, wet and dry thermometer, clouds, winds, meteors, &c., were to be made by scientific men in different parts of the globe, at the commencement of each hour, (per clock,) mean time at the place, for 37 hours: beginning at 6 o'clock on the morning of the 21st, and ending at 6 o'clock on the evening of the 22d;" and that it was deemed highly desirable that the points of observation should be multiplied by the co-operation of societies and individuals: have, by a committee of their body, made the following observations on the 21st and 22d of December. It is, however, to be regretted that the accuracy of the instruments employed has not been verified by comparison with acknowledged standards. The thermometers which were used, agree with that kept at the Academy, and by which the observations there taken under the direction of the Regents of the University, are made; but the barometer, although probably the best in the city, (the property of the President of the Institute, by whom it was loaned for this occasion,) is defective in not having any adjustment to ascertain the height of the mer-

cury in the cistern—the bottom of which is of leather—and in not having a neutral point marked upon it. The bulb of the wet thermometer was covered with cambric, and previously to each observation dipped into water.

While, therefore, the results exhibited in the following table, may not be rigidly correct, it is hoped that the publication of them may be the means of spreading information, and of inducing others to lend their assistance on the specified days to the collection of materials, which will tend to advance a knowledge of the science of the weather, so highly important to the seaman and the farmer, and indeed to every class of the community.

The form of the following table, with the exception of a column for rain, which is omitted, as none fell within the period embraced in the observations, is copied from that employed in recording the observations taken on those days, under the direction of the Royal Society of London, of which those for June and September last, have been published in the London Athenæum.

Hours of observation.	Baromet'r corrected.	° Attach'd therm'r.	° Extern'l therm'r.	° wet bulb therm'r.	Winds.	Remarks.	
6 A. M.	29 759	34.	36.	33.	S.W.	Overcast—thick clouds—	wind very light.
7 "	29 739	34.	37.	33.	"	" clouds breaking and moving N.	do. do.
8 "	29 739	34.	36.	33.	"	Haze,	"
9 "	29 746	35.	36.	34.	"	"	"
10 "	29 751	37.	38.25	37.	"	"	"
11 "	29 744	40.	39.	38.	"	Fog rising—clouds moving NE.	"
12 "	29 733	40.50	40.	37.	"	Fine—light cirrous clouds.	"
1 P. M.	29 719	42.	42.	39.	"	" upper clouds moving E.	"
2 "	29 719	42.	42.	39.	"	" cloudless,	"
3 "	29 734	40.25	41.	38.	"	" "	"
4 "	29 741	37.	38.	35.	"	" upper clouds moving NE.	"
5 "	29 719	34.	35.	33.	"	Haze,	"
6 "	29 734	32.	33.	31.	"	"	"
7 "	29 739	30.	32.	29.	"	Thick fog,	"
8 "	29 721	29.	30.	29.	"	Haze,	"
9 "	29 714	28.	29.50	28.	"	Fine, and cloudless,	"
10 "	29 706	27.	28.	25.	"	Thick fog,	"
11 "	29 706	27.	28.	25.	"	" "	"
12 "	29 706	27.	29.	25.	"	" "	"
1 A. M.	29 712	25.	26.	25.	"	Fine—light stratus clouds in horizon,	"
2 "	29 702	25.	26.	23.	"	" " " " " "	"
3 "	29 702	25.	27.	25.	"	" cloudless,	"
4 "	29 702	25.	26.	23.	"	" "	"
5 "	29 707	23.	26.	19.	"	" "	"
6 "	29 712	21.	25.	19.	"	" stratus clouds in horizon,	"
7 "	29 712	21.	23.	18.75	N.E.	" fog over river,	"
8 "	29 726	23.50	24.50	19.	"	" mottled cirro-stratus clouds moving NE.	"
9 "	29 751	25.50	26.	22.50	NNE.	"	"
10 "	29 755	27.50	27.50	24.	"	" light clouds moving E.	"
11 "	29 769	30.	29.	24.50	NNW.	" " " " "	"
12 "	29 745	31.50	31.	26.	"	" " " " "	"
1 P. M.	29 735	31.50	30.	25.	"	"	"
2 "	29 736	31.	31.	25.	"	"	"
3 "	29 751	29.	29.	24.	"	"	"
4 "	29 759	26.	27.25	24.	"	" thin cirro-stratus clouds moving NE.	"
5 "	29 812	25.	24.	19.	"	Cloudy—cirro-stratus, and cirro-cumulus clouds,	"
6 "	29 816	23.50	24 50	19.50	"	Slightly overcast,	"
Mean.	29 734	30.23	31.15	27.72		Height of cistern of barometer above low water mark of Hudson River at Albany, 148 feet, 4.44 inches.	

51. *Note to the article by* Dr. Hildreth *in the last volume of this Journal;* by S. G. Morton.—It is due to myself to state, that my remarks on the *Vegetable Organic Remains*, published in the last number of this Journal, were furnished without having seen the specimens, the wood cuts alone having been submitted to my inspection. The *Fossil Shells* there described are in my possession, but the drawings were made, and the wood cuts executed, before I received the specimens,—a circumstance which will serve to explain the unsatisfactory nature of some of the references. Dr. Hildreth thought it would be best to figure some fragments, which, upon inspection, were found entirely indeterminate.

52. *Eyes of flies changed to red by nitric acid;* by Lieut. F. R. Baddeley, R. E.—Reading Article 40th of the Philosophical Magazine for October, 1831, on the red coloring matter produced by the action of nitric acid upon alcohol, &c., by M. Rouches, I was strongly reminded of a fact which I have often noticed, (as indeed it is probable that many others have also,) viz., that when flies become immersed in nitric acid, their eyes, or eye-like protuberances, become changed to a deep sealing-wax color. Will any of M. Rouches' observations explain (or apply to) this phenomenon?

53. *Death of* Mr. David Douglas.* (Extracted from a letter by J. Goodrich and J. Diell, published in the Ke Kumu Hawaii, a paper printed at Honōlulu, Nov. 26, 1834.)

"From Edward Gurney, an Englishman, we received the following account of the tragical scene: About ten minutes before six o'clock in the morning, Mr. Douglas arrived at his house on the mountain, and wished him to point out the road to Hilo, and to go a short distance with him. Mr. Douglas was then alone, but said that his man had given out the day before; (this man was probably John, Mr. Diell's colored man.) After taking breakfast, Edward accompanied Mr. Douglas about three fourths of a mile, and after directing him in the path, and warning him of the traps, went on

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* Mr. Douglas was born at Perth, Scotland, and had traveled in various parts of the world as a naturalist, connected with the Horticultural Society of London. He was engaged in his scientific pursuits when he met with the fatal accident. It took place on the 12th of July, 1834.

about half a mile further with him. Mr. Douglas then dismissed him, after expressing an anxious wish to reach Hilo by evening, thinking that he could find out the way himself.

"Just before Edward left him, he warned him particularly of three bullock traps, about two miles and a half ahead; two of them directly on the road, the other on one side.

"Edward then parted with Mr. Douglas, and went back to skin some bullocks which he had previously killed. About 11 o'clock, two natives came in pursuit of him, and said that the European was dead, and that they had found him in the pit in which the bullock was. They mentioned that as they were coming up to this pit, one of them observing some of the clothing on the side, exclaimed *lole*, but in a moment afterwards discovered Mr. Douglas within the cave, trampled under the feet of the bullock. They went back immediately for Edward, who left his work, ran to the house for a musket and ball, and hide, and on coming up to the pit found the bullock standing upon Mr. Douglas's body. Mr. Douglas was lying upon his right side. He shot the animal, and after drawing him to the other end of the pit, succeeded in getting out the body. His cane was with him, but the bundle and dog were not. Edward, knowing that he had a bundle, asked for it. After a few moment's search, the dog was heard to bark, at a short distance ahead, on the road leading to Hilo. On coming up to the place, he found the dog and the bundle. On further examination, it appeared that Mr. Douglas had stopped for a moment and looked at the empty pit, and also at the one in which the cow had been taken; that after passing on up the hill some fifteen fathoms, he laid down his bundle, and went back to the pit in which the bullock was entrapped, and which lay on the side of the pond opposite to that along which the road runs, and that whilst looking in, by making a misstep, or by some other fatal means, he fell into the power of the infuriated animal, who speedily executed the work of death."

54. LIST OF NEW PUBLICATIONS.

American.

Natural History of Insects, 2d Series; being vol. lxxiv of Harper's Family Library. 18mo. *New York.*

Troost, (G.) M. D., Prof. of Chem. Min. and Geol. in the Nashville University: *Third Geological Report to the twenty first General Assembly of the State of Tennessee.* Made Oct. 1835. 8vo. 32 pp. *Nashville.* 1835.

The Elements of Geometry. 114 pp., 8vo. *New York.* 1836.

Catlin, (John Ruggles,): Synopsis of Lectures on Geology. 120 pp. 8vo. *Taunton.* 1835.

Gibbes, (Lewis R.): *A Catalogue of the Phenogamous Plants of Columbia and its vicinity.* 13 pp., 8vo. *Columbia, S. C.* 1835.

Monography of the Family Unionidæ or Naiades of Lamarck (fresh water bivalve shells) *of North America.* Illustrated by figures drawn on stone from nature. By T. A. Conrad, Cur. Acad. Nat. Sc. of Philadelphia. Nos. 2, 3, 4; for January, February and March. 12 pp. each.

Contents of No. 2.—Unio Mortoni, fragosus, costatus, retusus, parvus, Glans, siliquoideus, radiatus.

3.—U. pectorosus, fasciolus, congaræus, Masoni, coccineus, catillus, productus, lanceolatus, rectus.

4.—U. crassus, viridis, ochraceus, nasutus, icterinus, cariosus.

This work is very neatly executed. Of each species there is given a painted lithographic drawing, exhibiting very exactly the minutest peculiarities of the different shells. The drawings are accompanied with a scientific description of the species, followed by general observations.

Reprints.—Moffatt, (J. M.): *The Scientific Class Book,* or a Familiar Introduction to the principles of physical science, for the use of schools, &c.; with additions, by Walter R. Johnson, A. M. Part II. *Philadelphia*: Key & Biddle.

Foreign.

GENERAL NATURAL HISTORY.

Nightingale, (Thomas) Esq.: *Oceanic Sketches,* with a Botanical Appendix by Dr. Hooker. Post, 8vo. *London.* 1835.—— Brown, (Capt. Thomas) F. L. S. &c.; assisted by eminent scientific and literary men: *The Edinburgh Journal of Natural History* *and of the Physical Sciences.* With the Animal Kingdom, by Baron Cuvier. 4to. No. I.; 8 pp. inclusive of one wood cut, and exclusive of one plate, bearing 6 figures, species of Trogon, L. *Edinburgh.* 1835. pr. 4*d*, uncolored; 6*d*, colored.——*Transactions of the Cambridge Philosophical Society.* Vol. v. Part I. 1835.——*Expédition Scientifique en Morée;* 36[e] livr. in–fol. de 12*f*., plus 5 pl. *Paris,* chez F. Didot, rue Jacob, 24. 1835. Pr. 12*f*.——*Voyage de* MM. de Humboldt et Bonpland; 10[e] liv., in fol., 12*f*. plus une carte. *Paris,* chez Gide, rue Saint-Marc, 23. 1835. Pr. 36*f*.——*Memoirs of the Society of Natural History at Strasburg,* Vol. ii. 1st part, in 4to. *Paris* and *Strasburg.* 1835.——*Memoirs of the Société de physique et d'histoire naturelle at Geneva.* Vol. vii. 1st part, in–4. *Geneva.* 1835.——*Transactions of the Zoological Society of London.* Vol. i. 3d part. 4to. *London.* 1835.——*Annales de la Société Entomologique de France;* T. iv. 1[er] trimestre de 1835. *Paris.*——*Actes de la Société Linnéenne de Bordeaux;* tome vii. 3d liv. 1835.

GEOLOGY AND MINERALOGY.

Minerals and Metals; their Natural History and use in the arts, with incidental Accounts of Mines and Mining. A pocket volume. *London.* 2*s*. 6*d*.——*Cenni di Statica Mineralogica* degli stati S. M. il re di Sardegna, &c. A descriptive catalogue of the minerals of the states of the king of Sardinia. 8vo. 350 pp. *Turin.* 1835. ——Hartmann, (C.): *Grundzüge der Mineralogie und Geologie.* 1ste Th. Mineralogie (1ste Lief. 1–14b. mit tafeln, 8°) *Nurnberg.* 1835. 22½*s. gr.*——Triger: *Cours de Géognosie appliquée aux arts et à l' agriculture.* Liv. 3 à 5, chacune d' une feuille in–12. *Au Mans, chez Pesche.* 1835.——Burat (Amedee): *Traité de Géognosie;* tome iii. in–8, de 42 feuilles, plus 7 pl. *Paris et Strasburg,* chez Levrault. 1835. Pr. 8*f*.——Salneuve, (H.): *Essai sur les eaux Minérales de Chateauneuf;* in–8, de 6 feuilles ¼. Imp. de Goninfaure-Arthaud, à *Paris.* 1835.——Cuvier, (G.) et Brongniart, (Alex.): *Description Géologique des environs de Paris;* 3d éd. in–8, 43 f., plus un atlas d' une ½ f. in–4, et 17 pl. *Paris,* chez d' Ocagne, rue des Petits-Augustines, 12. 1835. Pr. 21*f*.——Alberti: *Beitrag zu einer Monographie des bunten Sandsteins, Muschelkalks und Keupers* und etc., mit 2 lith. *Stuttgart* et *Tubingen.* 1835. 2*th*.——Gutbier: *Geognostische Beschreibung des Zwickauer Schwarzkohlengebirges und seiner Umgebungen.*

Zwickau. 1835. 2*th.* 20*s. gr.*——KIRCHER: *De petrifactis et fossilibus, quæ Soraviæ et in vicinis agris reperiuntur, commentatio cum imaginibus.* Lithog. 1*f. Sorau.*——LINK: *Die Urwelt und das Althertum, erlautert durch die Naturkunde.* 1ste. th. in 8°. 29½ b. *Berlin.* 2*th.*–2te. th. 1*th.* 10*s.*——REICH: *Beobachtungen über die Temperatur des Gesteins in verschiedenen Tiefen in den Gruben des Sachsischen Erzgebirges in den Jahren* 1830 *bis* 1832. Nebst 2 Beilagen: 1. über die Churprinzer lauwarme Quelle: 2. über das perennirende Eis im Saubirge. 8°. 13¼ b. *Freyberg.* 1835.——BOUE: *Guide du Géologique Voyageur.* 2 vols. in–8. *Paris.* Levrault. 1835.——AGASSIZ, (L.): *Rapport sur les Poissons fossiles decouverts en Angleterre* (extrait de la 4me livraison des "Recherches sur les Poissons fossiles.") *Neuchâtel.* 72 pp. in–8. 1835. ANKER, (J.): *Kurze Darstellung der mineralogisch-geognostischen Gebirgs–Verhaltnisse der Steyermark.* 8°. 84*ss. Gratz.* 1835. ——KAUP, (J. J.): *Description d' ossemens fossiles de Mammifères inconnus jusqu' à présent, qui se trouvent au Muséum grand-Ducal de Darmstadt* IVme cahier in–4., avec Atlas in fol. *Darmstadt.* 1835.——PELOUZE, *père: Les merveilles et les richesses du monde souterrain,* ou les mines, les metaux, les pierres precieuses, la houille, le sel, etc. Ouvrage destiné à la jeunesse, suivi de quelques notions de géologie et de géognosie. in–16. *Paris.* 1835.——REBOUL, (H.): *Essai dé géologie descriptive et historique: prolégomènes et période primaire;* in–8. *Paris.* 1835.——DUFRENOY: *Mémoire sur les terrains tertiares du bassin du midì de la France;* in–8. *Paris.* 1835.

ZOOLOGY.

GRAY, (GEORGE ROBERT) M. E. S.: *The Zoological text book;* or, an explanation of all the terms employed by Zoologists, in the description of *Beasts, Birds, Fishes, Reptiles, Insects, Shells, Worms, Corals,* &c. &c., illustrated by numerous Plates, representing the various parts in their natural situation and in detail. *London.* 1836.——EYTON, (T. C.): *A History of the Rarer British Birds.* Intended as a Supplement to the History of British Birds, by the late T. BEWICK,—with wood cut figures. To be completed in three parts; pr. 3*s.* 6*d.* each in demy 8vo.; 7*s.* royal 8vo. *London.* 1836. ——MEYER, (H. L.): *Illustrations of British Birds;* consisting of colored Figures of all the Birds that are indigenous to Great Britain. Nos. 1—8. Roy. 4to. Each No. contains five plates. *London.*

1835. Pr. 14*s.* 6*d.* each: to subscribers, 12*s.* 6*d.*——STANLEY, (Rev. E.): *A Familiar History of Birds,* their Nature, Habits, and Instincts. 2 vols. 8vo. *London.* 1835.——HEWITSON, (W. C.): *British Oology;* being Illustrations of the eggs of British Birds, accompanied by letter press. Nos. xxiii and xxiv. 1835.*

BOTANY.

EVERARD, (ANNE): *Flowers from Nature.* 1 vol. imp. 4to. Containing 13 colored plates, with the Botanical Name, Class and Order of each flower, and Instructions for copying. *London.* 1835. 1*l.* 11*s.* 6*d.*——BAXTER, (GEORGE): *Agricultural and Horticultural Annual for* 1836. *London.* 12*s.* Embossed sides and gilt edges.——BOITARD: *Manuel de botanique* 1re partie; 3e edition, in–12. *Paris.* 1835.——DUMONÇEAU, (DUHAMEL): *Table des arbres fruitiers;* nouvelle edition, par A. Poiteau et P. Turpin; 7e liv. 1835.——WATSON, (HEWETT COTTREL) F. L. S.: *On the Geographical Distribution of British Plants.* Small 8vo. *London.* 1835. 6*s.* 6*d.* in cloth.——WATSON, (H. C.): *The new Botanist's Guide to the localities of the rarer plants of England and Wales;* on the plan of Turner and Dillwyn's Botanist's Guide. *London:* Longman, Rees & Co. 1835. 10*s.* 6*d.* in cloth.——HENSLOW, (J. S.): *Botany;* forming vol. lxxv of Lardner's Cabinet Cyclopedia. Foolscap. 8vo. *London.* 1835.——PARTINGTON, (C. F.): *Introduction to the science of Botany.* 1 vol. 8vo. 150 pp. and 1 plate of 4 colored figures, and several wood engravings. *London.* 1835. Orr & Smith.——LINDLEY, (J.): *A Key to Structural, Physiological and Systematic Botany. London.* 1835

CHEMISTRY AND NATURAL PHILOSOPHY.

GRAHAM, (J. T.) M. D.: *A Chemical Catechism.* Illustrated by notes and numerous engravings. 1 vol. 8vo. *London.* 16*s.* in boards. HENRY, (O.): *De l' action du Tannin sur les bases salifiables organiques et applications qui en dérivent;* in–8. 1 feuille ½. Imp. de Fain, à *Paris.* 1835.——*Manuel de chimie amusante;* traduit de l'Anglais par AD. VERGNAUD. *Paris,* chez Roret, rue Hautefeuille. 1835. Pr. 3*f.*——GUERIN, (TH.): *Le Chimiste Populaire;* in–18, 2f. *Paris,* chez Garnier, passage de la Cour des Fontaines, 1.

* The late works on Entomology are again omitted for reasons which it is unnecessary to mention.

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APPENDIX.

On the collection of Geological Specimens and on Geological Surveys; by CHARLES T. JACKSON.

Boston, March 12, 1836.

TO PROF. SILLIMAN.

Sir—Having mentioned to you in private conversation, a plan which I had some time since conceived, for making an universal collection of the objects of Natural History of the United States of America, which project seemed to you feasible, I beg leave, by your kind invitation, to suggest it to the public, through the medium of your valuable Journal of Science.

The Geological surveys which have been made, or are now in progress, and the numerous calls that are made, on our state governments, for similar investigations, of their respective territories, demonstrate that the community are fully aware of the advantages, which must necessarily accrue to them, from a scientific examination of their mineral resources.

It is certainly a source of congratulation, that we find the American people so liberal and enlightened, in this respect. No other people in the world, I may safely affirm, have ever called on their governments, to furnish information of this kind; from which fact we may conclude, that the American people are more enlightened respecting the application of science to the arts, than the people of any European state. This is no doubt to be attributed, to the general diffusion of knowledge in our country.

The geological surveys that have been made, in Europe, have generally been executed by private exertion and enterprize, the governments rarely patronizing them, or assisting in the task.

In no instance, I believe, has any entire European state, been wholly surveyed by the orders of its government.

The first geological map of England, it is well known, was made by the enterprising miner, William Smith, and it is a monument of geological labor, which will forever preserve his name in the annals of British science.

Greenough, Conybeare, and Phillips, have added many new observations to this map, and have rendered it still more worthy of confidence. France, at the present time, is engaged in constructing an accurate geological map of her territory, and the direction of the work is confided to the care of two of her most learned and industrious geologists, Messrs. L. Elie de Beaumont and Dufrenoy, who are aided by the members of the *Ecole des Mines*, and the researches of the members of the Geological Society of France. This map, when complete, will be the most accurate and comprehensive work of the kind, ever executed.

To Massachusetts belongs the honor of having made the first geological survey of an entire state, under the authority of its government, and the public are greatly indebted to Prof. Hitchcock, for the faithful manner in which he has executed the task assigned to him. It cannot be supposed, however, that he has exhausted the subject, nor does he by any means pretend that he has presented all the interesting details. They remain to be discovered by continued research. Since the publication of his valuable report, he has himself discovered many very remarkable facts, concerning the new red sandstone, of the Connecticut valley. Other geologists have also added some important discoveries, to enrich the geology of the state. Anthracite has been found at Mansfield, within the limits of the state, associated with the usual fossil plants of the coal measures, and some which appear to be new species. All the discoveries which are made concerning our geology, should become incorporated in the state report, a new edition of which will hereafter be called for. Specimens of all the rocks and minerals, as they are discovered, should also be preserved for the use of the state.

I should think it also advisable, to extend the observations in the economical department of the work, and specimens of our metallic ores which are wrought should be accompanied by the ores in their various states of reduction, and the metal in its various states of refinement. Slags produced in smelting ores, with fluxes, should also be preserved. A suite of economical mineralogy would thus be formed of great value to practical workmen.

An imperfect collection of this kind is kept in the School of Mines at Paris, which is found to be valuable in teaching.

In making the above remarks, I have no intention of depreciating the valuable report of Prof. Hitchcock, but merely suggest additions which, in my opinion, will make the work of greater practical use.

I hope the government of the state will authorize that gentleman to continue his researches, until every desideratum in our geology shall be accomplished. The plan which I have now to suggest, is to furnish a complete collection of the mineralogical, geological, botanical, and zoological specimens of the United States, for the capital of each of the individual states, and one for the general government at Washington. Some of my friends have smiled at this project, and have evidently considered it Quixotic. Your sanction, however, is to me a sufficient proof of its feasibility.

We all know how easy it is for individuals to form collections by exchange of duplicate specimens, and why cannot the state governments, with their ample means, do as much by similar operations? A student of any department of natural history knows, that if he is in possession of duplicates of thirty or forty rare species, he can command the scientific resources of the world wherever his pursuits are followed, and can form an universal collection by a system of judicious exchanges. This system is exactly what I now propose to recommend to the state governments. We may calculate with certainty, that at least twelve of the United States will be surveyed within ten years from the present time, and perhaps many more will enter upon the work within that time. If then the plan which I now propose be kept in view, it is evident that nothing can be easier than to effect it in a satisfactory manner.

Let every surveyor be authorized and required, to furnish at least fifteen specimens of every object of natural history which comes under his observation, and falls within his department. When these specimens are numbered and arranged with a descriptive catalogue, let them be carefully packed in separate cases, marked, and numbered, so that complete sets may be disposed of without the trouble of again unpacking and assorting them.

The collections arranged in this manner should then be sent to to the state depository, and a distinctive mark should be placed on those which are to be reserved for the use of the state surveyed. The most perfect suite should, of course, belong to the state cabinet. The other collections may then be disposed of by exchanging them with the other states, and the collections received in return will form a complete cabinet of the various natural productions of the states with which the exchanges are made. Both parties will be gainers by the transaction, and the expense will be comparatively trifling, it being only, for the little additional trouble, in collecting and arranging duplicates, and the cost of transportation of the specimens. Each state should also send several copies of their descriptive catalogues bound up separately, and copies of their reports on their respective districts.

Each state should then provide ample accommodations for their collections of objects of natural history, and a museum on a liberal scale would be formed. A large and commodious building should be purchased or erected for this purpose. If a new building should be erected, then it might be formed into very ample and convenient galleries or halls, where the specimens should be all put up, in separate glazed cabinets, to protect them from dust and from injury. The most appropriate arrangement would evidently be according to the states; beginning with the northern states, and arranging the cabinets accordingly. The subordinate arrangement may, however, be systematic, according to any respectable authority.

Some scientific society should be entrusted with the care of the state collection, and it should be their duty to arrange the specimens in order. The capital of every state has, I believe, some such scientific association, and they would gladly avail themselves of the privilege of superintending the great state museum. A cabinet keeper should, however, be employed by the government to keep the rooms in order, and to remain in the museum during visits, to prevent mischief, and to explain the arrangement or to supply the visitors with catalogues to be used in viewing the collections.

Whoever reflects on the value and importance of science to the rising generation, will duly appreciate the utility of such an institution as I have described; it will be a practical school where our children will, at all times, be able to learn to read the book of nature thus opened before them.

The stranger, who visits our country, will find in the capital of each individual state, an entire collection of the curious and useful objects of natural history from every part of the Union, and will contemplate with admiration, the liberality and intelligence of a people who have provided such ample means of instruction, surpassing in extent and utility, the far famed cabinets of European monarchies.

When we visit a foreign land, we are naturally interested in the objects peculiar to that country; and it would certainly be delightful, to see its various natural productions all arranged in order for our observation, constituting a sort of epitome of the whole state; presenting the materials forming its rocks, mountains, soils, and valuable minerals, besides exhibiting the various organized beings indigenous to the soil. There is no such collection in the world, and it would be an honor to America to take the lead of Europe in effecting this object. The utility of a collection of this kind, must be obvious to every intelligent person who reflects on the subject. Whoever is interested in any particular district of country, would be thus enabled to study its products in a well arranged cabinet, where he might learn the names and properties of all the valuable natural substances which that section presented. Should any one, for instance, imagine he had discovered indications of coal on his estate, he would be enabled to satisfy himself on the subject by comparison of his specimens with those of the well known coal fields of Pennsylvania, Virginia, or Ohio, and thus be directed in his researches; while, if he found he was in error, it might save him much time that would have been lost in vain search, and great expense in mining operations would have been prevented. The probable value of a metallic ore might also be estimated, by comparing it with specimens from well known localities where such ores are wrought. Those who visited the cabinet, would also learn to distinguish the various objects of value in the arts, and would be enabled to add to our stock of knowledge by these observations.

I have no doubt, that an institution of the kind I have mentioned, would prove a most valuable acquisition to every state, and would furnish employment to young persons who would desire to learn the natural history of their country, instead of wasting their time in frivolous pursuits.

Such an institution would favor the growth of science; and I may safely add, that the morality of our country would be very much improved by the diversion of young and ardent minds, from idle or vicious amusements, to solid and useful learning.

I know from observation, that since the museum of the Boston Society of Natural History has been opened, freely to the public one day in the week, young persons throng to the cabinet for instruction and amusement; and that many a germ of science has begun to unfold itself in their minds, the fruits of which no man can calculate.

Can any one doubt, that such pursuits are in the highest degree salutary; or, that a knowledge of the works of God, showing wisdom and benevolence in their design, can fail to improve the morality of mankind? For my part, I have no doubt of the good influences thus exerted.

P. S. A portion of the surplus revenue of the United States, might be usefully appropriated in founding state museums of the kind I have described; and it would also be desirable for the general government, to have a complete suite of European specimens at Washington for the use of Congress.

Confirmation of Judge Woodruff's account of the influence of the ash on the rattlesnake; by Mr. William R. Morris.

Dover, (Del.) Jan. 25, 1836.

TO PROF. SILLIMAN.

Dear Sir—I observed recently, in the Baltimore Chronicle, a republication of an extract from the Journal of Science, which I had met with about a year ago, giving an account of the surprising effects of the twigs and branches of the white ash in disarming and subduing the rattlesnake, as tested by an experiment of Judge Woodruff, some years since, in the northern part of the state of Ohio.

Judge Woodruff, according to my apprehension, seems to have felt his experiment to have been merely the establishment of what he had hitherto deemed a popular superstition, as a positive matter of fact. He seems not to have suspected its being, as it indeed was, a modern test (and applied to a reptile not included even within the wide range of his knowledge) of a statement made by Pliny the naturalist, more than seventeen hundred years ago, and in a language now dead, that "a serpent will rather creep into the fire than over a twig of ash." I have not taken the trouble to point out this coincidence between ancient authority and recent experience, in order to disparage the credit of Judge Woodruff's experiment, (for which I have no doubt that he is entitled to the claim of perfect originality,) but rather to support the suggestions made by him upon this subject, by an argument depending upon his very unconsciousness of such a coincidence, and moreover to suggest, on the authority of Pliny, a similar application to other noxious varieties of the serpent kind. I confess that I also felt amused and interested by this example of the manner in which phenomena brought before one age of the world, are, after having been neglected and forgotten, again presented, with the same aspect of novelty, to another.

Fig. 85. C. blepharophora, Gray.
Fig. 86. C. stenolepis, Torrey.
Fig. 87. C. Shortiana, D.

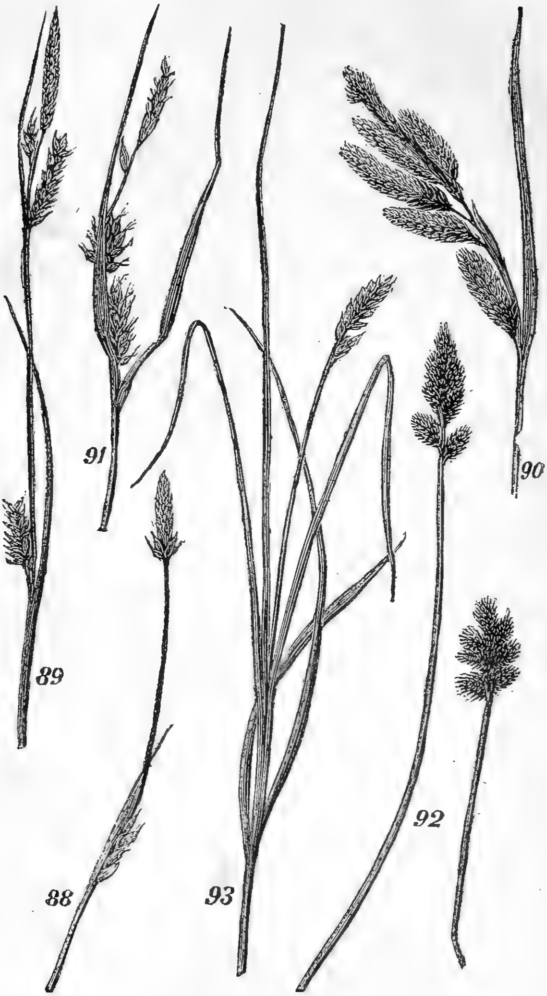

Fig. 88. C. Careyana, Torrey.
Fig. 89. C. Greeniana, D.
Fig. 90. C. Columbiana, D.
Fig. 91. C. Houghtoniana, Torrey.
Fig. 92. C. mirabilis, D.
Fig. 93. C. siccata, D. Vol. X, p. 274.

THE

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Art. I.—*Observations on the Comet of Halley, made at Yale College;* by Elias Loomis.

In No. 268 of Prof. Schumacher's Astronomische Nachrichten, Dr. Olbers has given reasons for supposing that Halley's comet might possibly have been seen as early as February or March, 1835. He was of course aware that the comet would then be at a greater distance from the earth and the sun than it had ever before been seen; yet considering that it would be high up in the evening sky, even after the disappearance of the twilight, and also that very great improvements had been effected in the manufacture of telescopes since 1759, he concluded it was not altogether impossible that the comet might be discovered at this early period. No account, however, has been received of the comet's discovery before the month of August. The first announcement which has come to our knowledge, is from Rome. In the number of L'Institut for August 19th, it is stated that M. Bouvard had received a letter from M. Dumouchel, Director of the Observatory at Rome, announcing that he, in company with M. Vicot, his assistant, saw the comet distinctly about three o'clock on the morning of August 5th. Its position they determined to be in Right Ascension 5h. 26m.; Declination 22° 17′ North. On the following morning, at about the same hour, they saw the comet again, but were prevented by clouds from taking accurate observations. The comet had, however, advanced sensibly to the east.

From about this time, the presence of the moon interfered with observations, and we hear that the comet was next seen at Dorpat in Russia, by Prof. Struve, on the 20th of August. It was seen by M. Boguslawski, at Breslau, on the 21st; by M. Kunowski, at Berlin, on the 22d; by Sir James South, in England, and by M. Arago, at Paris, on the 23d; by M. Valz, at Nismes, on the 24th; and by Prof. Schumacher, at Altona, on the 25th. The comet was first seen at Yale College, on the morning of August 31st. A fruitless search had been made for it, on the preceding Monday morning, (Aug. 24,) and each succeeding morning was cloudy, until the 31st.

When the comet was first discovered, it appeared to be merely a faint nebulous mass. M. Dumouchel describes it as very faint, much resembling Biela's comet. Sir James South describes it, on the 23d of August, as a round, well-defined nebulous body, extremely faint and perhaps about two minutes of space in diameter. M. Arago describes it, on the 31st, as having a sensible nucleus, and a nebulosity of two or three minutes in diameter, but he could distinguish no appearance of a train. Mr. E. J. Cooper of the county of Sligo, Ireland, first saw the comet, on the 26th of August, in the *finder* of his great telescope. His telescope is 25¼ feet focal length, 13.3 inches aperture; the finder is 6½ feet focal length, with an object glass of 4.9 inches aperture. He describes it as appearing faint in the finder, but by no means so much so as other objects he had seen through it. In the great telescope, it was beautifully shewn, and its nucleus perfectly distinguishable.

When first seen at this place, the comet was nearly circular, about two minutes in diameter, brightest in the middle, fading away upon the borders. No distinct nucleus was observed. As the large telescope, with which the comet was discovered, has no micrometer, nor an equatorial movement, no measurements could be taken, until the comet became visible in smaller instruments. On the 21st of September, I noted in my journal,—the nucleus is as bright as a star of the sixth magnitude—coma about six minutes in diameter, although its outline is quite uncertain—think there is more coma on the lower (through an astronomical eye-glass) than the upper side of the nucleus—comet quite conspicuous in the finder. Sept. 22d, *sure* there is more coma on the side next the sun. Sept. 25th, comet distinctly visible to the naked eye. Oct. 3d, comet as bright to the naked eye as a star of the fourth magnitude. Oct. 10th, comet quite as conspicuous in the light of the moon as α Ursæ Majoris. Oct. 11th, train nine degrees in length, extending to 4 Draconis of Flamsteed.

Coma almost entirely fills the field of view, whose diameter is 38′. The nucleus has much the appearance of one of Jupiter's satellites, but with scarcely any sensible magnitude. Oct. 12th, nucleus distinctly visible to the naked eye—train about six degrees in length, points one degree west of α Draconis—slightly hazy.

On the 22d of September, I commenced observing with a small equatorial, belonging to the College. It has a telescope of eighteen inches focus, with a declination circle, graduated to half degrees, and reading by a vernier to minutes, manufactured by Banks of London. To prepare for observations, I procured a thin plate of silver, with a rectangular cavity, made as accurately as possible, which I secured firmly in the focus of the instrument, in the place of the parallel wires, so that the field of view became rectangular, instead of circular. The wires were useless, there being no contrivance for illuminating the field, and indeed most of the time the comet would not bear illumination.

The several adjustments were made substantially, according to the directions of Vince, and for verification I observed the differences of Right Ascension and Declination of two known stars near each other, obtaining a result which differed little from the tables. As the instrument has no micrometer, it was necessary to observe the two Declinations independently of each other. In observing the Right Ascension of the comet, I noted merely the time of egress from the field, as also that of the star with which I was comparing the comet. When it could conveniently be done, I selected for observation, a star upon each side of the comet; noting the egress of the first star, then the comet, then the second star, repeating the operation half a dozen times or more, until I was satisfied with the result. Whenever the nucleus of the comet was distinctly visible in the equatorial, I observed *its* egress; when this was not the case, I noted the egress of the point of greatest brightness, which I could generally determine to a second or two of time. On one evening I noted the times of egress of both sides of the coma, and afterwards took their mean; but I found I could bisect the comet more accurately than I could note the egress of its border.—The clock was regulated to sidereal time, and its error each evening determined by observing the meridional transit of some known star. The following table contains a summary of the observations before the perihelion passage. The differences of Right Ascension and Declination, are the means of several observations. The places of the stars marked (A) are from the Catalogue of the Royal Astronomical Society, with the exception of 1305 which is taken from the Nautical Almanac. The places of the remaining four are from Bode's Catalogue, and were furnished by Lalande.

1835.	Sidereal time at N Haven	Stars.	Mag.	Apparent places of the stars.		Comet *minus* star.	
				A. R.	Dec.	A. R.	Dec.
	h. m. s			h. m. s.	° ′ ″	m. s.	° ′ ″
Sept. 23	0 53 5	800 A.	6	6 17 59.10	+30 35 17.87	+ 32.30	+1 13 40.0
" "	1 7 48	808 A.	7	6 21 42.38	32 33 53.68	− 3 12.00	− 43 40.0
" 24	1 59 1	808 A.	7	6 21 42.38	32 33 53.68	− 53.40	0 0
" 30	1 54 40	60 Aurigæ.	6	6 41 54.83		− 53.12	
" "	2 30 30	"	"		38 38 30.23		− 4 29.8
Oct. 10	21 41 45	1305 A.	1.2	10 53 29.07		+18 54.00	
" "	" " "	260 Bode.	6	11 19 37.89		− 7 12.33	
" "	" " "	273 Bode.	6	11 22 57.70		−10 31.33	
" "	22 53 47	1305 A.	1.2		62 38 11.72		+1 2 50.0
" "	" " "	260 Bode.	6		62 40 11.54		+1 1 10.0
" "	" " "	273 Bode.	6		61 59 33.40		+1 44 0
" 12	22 51 20	17 Bootis.	6	14 7 32.67		+ 3 23.33	
" "	" " "	1639 A.	4	14 19 33.07		− 8 40.60	
" "	23 57 20	17 Bootis.	6		52 34 2.97		+1 50 35.0
" "	" " "	1639 A.	4		52 36 58.02		+1 53 10.2
" 14	21 27 37	1782 A.	5	15 33 9.37		+ 3 42.33	
" "	21 43 48	"	"		37 10 37.78		+ 41 18.0
" 16	21 8 50	1896 A.	2.3	16 23 7.41		− 5 8.62	
" "	21 31 17	"	"		21 51 20.20		+1 26 36.0
" 19	20 27 21	1932 A.	4	16 46 12.54		+ 50 66	
" "	" " "	1940 A.	4	16 49 52 07		− 2 50.50	
" "	20 33 56	"	"		+ 9 38 22.55		0 0
" 25	20 50 16	1998 A.	5 6	17 17 53.46		− 8 32.20	
" "	21 36 22	"	"		− 4 55 59.25		+1 52 37.0
" 29	21 9 39	2021 A.	5	17 28 53.04		−14 22.50	
" "	21 40 18	"	"		8 0 37.21		+ 43 43.0
" 31	20 44 27	"	"	" " "		−13 7.00	
" "	21 51 21	"	"		" " "		− 47 0
Nov. 4	21 4 2	1985 A.	4.5	17 11 33.95		+ 4 40.00	
" "	21 22 35	"	"		12 38 17.84		+1 34 0
" 9	21 17 24	"	"	" " "		+ 2 41.20	
" "	21 38 3	"	"		" " "		− 40 4.0

The places of the comet thus determined were corrected for refraction, parallax and aberration; the Right Ascensions and Declinations, after being reduced to the same instant of time, were converted into longitudes and latitudes; the longitudes were referred to the mean equinox of Jan. 1, 1836, and the results are given in the table on page 216. Having obtained the true geocentric places of the comet, I proceeded to compute the elements of its orbit. I first made trial of the method of Olbers for a parabolic orbit, but it gave me a result differing very much from the truth. I obtained the longitude of the perihelion about two degrees too great; the perihelion distance about one hundredth part too great; and the time of passing the perihelion almost a day too early. This was obviously the result of the parabolic hypothesis. If I had employed observations made after the perihelion passage, the effect on the time of perihelion, and the longitude of the perihelion, would have been reversed. Rejecting, therefore, the parabolic hypothesis, I resorted to the elliptic, adopting the method which is particularly explained by Dr. Bowditch, in the first volume of his translation of the Mecanique Celeste, pages 470–3. It so happened, that I had three observations on the comet, in which I had more confidence than in the others, viz. those of Sept. 24th, Oct. 10th and 19th. They were made under favorable circumstances, and I was sure they could not be liable to any great error. I decided, therefore, to correct the elements of the orbit, by these three observations. Taking the mean of each element, according to the calculations of Pontecoulant, Damoiseau and Lubbock, I assumed these as the *approximate* elements of the orbit, with the exception of the time of perihelion passage, which I had already determined to be very nearly Nov. 16th.

With these elements I calculated the comet's places for the three times of observation above mentioned; I repeated the calculation in five successive operations, varying one of the elements at each operation, while the others remained unaltered.

The eccentricity I assumed to be constant, (.967392,) as I doubted whether my observations were accurate enough to enable me to correct this element.

I thus obtained six equations, containing five unknown quantities, from which the corrections of the elements were to be deduced.

Applying these corrections to the assumed elements, the result was as follows:

Perihelion distance, .586016

Perihelion passage, Nov. 15.947622, Greenwich mean time from noon.

Place of perihelion on the orbit,	304°	26′	54″
Longitude of the ascending node,	55	13	5
Inclination of the orbit, -	162	15	53

With these elements, I computed the comet's places for the times of all my observations, and found the differences to be no greater than might perhaps be fairly ascribed to errors of observation. I therefore computed an ephemeris of the comet, for the months of January, February and March, and waited for additional observations, to enable me to correct still further the elements. The comet was rediscovered by Prof. Olmsted on the morning of Dec. 31st. Yet, it was then almost immediately lost in the morning twilight, and there was no opportunity for locating it with accuracy. The entire month of January was remarkably unfavorable for observations. Besides the uncomfortable severity of the weather, there was scarcely a clear morning during the month; so that, although I saw the comet repeatedly, I was yet unable to obtain a single satisfactory observation. On the morning of January 29, Prof. Olmsted saw the comet distinctly with his naked eye; and during the months of February and March, I saw the comet with my naked eye, about a dozen different mornings. The last of these was Monday morning, March 21st. About the middle of February, the comet passed the meridian at five o'clock in the morning; and from this time, I made my observations with a small transit instrument of twenty inches focus. The instrument was carefully adjusted to the meridian by the method of high and low stars. The comet would bear no illumination, and it was necessary to sit in the dark for five or ten minutes before the comet entered the field. The wires were entirely useless. I therefore contented myself with noting the time, when as I judged the comet was in the middle of the field, and also the instant of egress. The altitudes were measured at the same time by a graduated circle which read by a vernier to minutes. Immediately afterwards, I observed in the same way, the passage of two or three stars with which the comet was to be compared. I thus obtained six transit observations about the middle of February, and five about the middle of March. The mean of all the observations made the comet's Right Ascension in February, 35.9 seconds greater than by my ephemeris, and the Declination 1′ 13″ greater; for March the Right Ascension was 55.1 seconds greater, and the Declination 5′ 14″. Having applied these corrections to the computed places for February 18, and March 16, I converted the places thus found into latitudes and longitudes. Taking these in connection with the observations of September 24, October 10th and 19th, I determined by means of them, to obtain the final correction of the elements. Proceeding again as in the former case, I varied the perihelion distance by .0001; the time of perihelion passage by .01; and the other three elements, each by one minute. The resulting equations were:

$$444 = -295\delta - 150\tau + 511\pi + 45\nu - 75\iota$$
$$1016 = -245\delta + 180\tau + 418\pi - 284\nu + 272\iota$$
$$778 = -840\delta + 3609\tau + 1843\pi - 146\nu + 135\iota$$
$$960 = -1669\delta + 1086\tau + 2451\pi - 371\nu + 539\iota$$
$$397 = -802\delta + 16\tau + 857\pi + 150\nu - 361\iota$$
$$270 = -139\delta + 720\tau + 382\pi + 261\nu - 879\iota$$
$$4241 = +337\delta + 249\tau + 679\pi + 25\nu - 176\iota$$
$$706 = +138\delta + 50\tau + 306\pi - 183\nu + 520\iota$$
$$8350 = +447\delta + 175\tau + 1080\pi - \nu - 201\iota$$
$$334 = +95\delta - 3\tau + 264\pi - 203\nu + 753\iota$$

Applying the method of minimum squares, the resulting values of the unknown quantities are; $\delta = +5.2755$, $\tau = -.9785$, $\pi = +4.3792$, $\nu = -5.1624$, $\iota = -2.8864$. These corrections give the following final elements:

Perihelion distance, .586544
Perihelion passage, November 15.937837, Greenwich mean time from noon.

Longitude of perihelion,	304° 31′ 17″
Longitude of ascending node,	55 7 55
Inclination of orbit, -	162 13 0

With these elements, I recomputed the comet's place for each time of observation, and the result is shown in the following table:

Comparison of the Observations with the results deduced from the elements.

M. T. at Greenwich, from noon.	Time from perihelion.	True anomaly.	Log. of comet's radius vector.	Sun's longitude.	Log. of earth's radius vector.	Calculated.		Observed.	
						Longitude.	Latitude.	Longitude.	Latitude.
1835.	D								
Sept. 23.737816	53.200021	93° 9′ 43″	0.0860057	180° 27′ 30″	0.0011252	93° 59′ 39″	+8° 28′ 40″	93° 58′ 56″	+8° 26′ 0″
24.777373	52.160464	92 24 41	0.0802372	181 28 41	0.0009957	94 27 31	9 11 19	94 26 55	9 11 18
30.756320	46.181517	87 40 18	0.0454505	187 21 15	0.0002381	98 20 17	15 23 0	98 18 48	15 22 13
Oct. 10.603746	36.334091	77 52 34	9.9818620	197 4 21	9.9990009	135 12 13	52 9 22	135 11 51	52 9 52
12.642296	34.295541	75 27 7	9.9677199	199 5 29	9.9987520	176 28 47	60 53 49	176 27 11	60 56 17
14.544357	32.393480	73 2 29	9.9542464	200 58 39	9.9985213	216 45 20	55 0 16	216 44 30	54 54 55
16 530228	30.407609	70 21 31	9.9399188	202 56 57	9.9982820	236 49 22	43 54 53	236 49 26	43 52 6
19.482319	27.455518	66 1 27	9.9182095	205 53 5	9.9979268	248 44 56	31 47 45	248 44 39	31 45 46
25.509175	21.428662	55 41 27	9.8731845	211 53 35	9.9972044	256 33 41	19 45 17	256 34 36	19 49 0
29.500977	17.436860	47 34 31	9.8440178	215 52 58	9.9967331	258 17 36	15 43 54	258 17 17	15 43 21
31.502758	15.435079	43 5 5	9.8300779	217 53 10	9 9965021	258 43 37	14 14 40	258 44 3	14 14 50
Nov. 4.472325	11.465512	33 19 28	9.8049148	221 51 51	9.9960604	259 2 24	11 54 35	259 2 5	11 59 12
9.469388	6.468449	19 31 39	9.7807605	226 52 59	9,9955418	258 43 7	+9 40 26	258 43 4	+9 42 8
1836.									
Feb. 18.	94.062163	112 27 5	0.2624681	328 55 37	9.9951174	226 3 13	−16 30 27	226 2 38	−16 31 43
March 16.	121.062163	119 31 19	0.3434309	355 57 47	9.9981105	195 10 41	−23 41 14	195 12 9	−23 41 35

The Longitudes are referred to the mean equinox of January 1, 1836.

The agreement between the observed and computed places is as good as had been expected. The errors in latitude, as was foreseen, are greater than those in longitude; for the instrument with which the Declinations were measured was graduated only to minutes; the Declinations were all taken independently of each other, and at considerable intervals, so that any error in the adjustment of the instrument commonly had a greater influence on the Declinations than on the Right Ascensions. The errors are, however, so small that it was thought useless to attempt to carry the approximation any further. I have taken no account of the planetary perturbations of the comet, and the effect of a resisting medium. Without observations more accurate than my own, this labor would be superfluous.

The times of perihelion passage, as predicted by five different astronomers, were as follows:

Mr. Lubbock, Oct. 29.7. Paris, M. T. from noon.

M. Damoiseau, Nov. 3.82.

Prof. Rosenberger, Nov. 11.

M. De Pontecoulant, Nov. 12.6.

Dr. Lehrmann, Nov. 26.

The date here assigned to Pontecoulant, is that which he has himself given in the Connaissance des Tems for 1837. A brief note, however, in L'Institut of Sept. 23, 1835, announces that in that calculation he had employed the value of the earth's mass which is given in the third volume of the Mecanique Celeste, but that in revising his calculation and employing the value of the earth's mass which is now generally received, he found the time of perihelion about a day later than before, which makes his final calculation differ but about two days from the truth.

Halley's comet has made six visits to the sun, at which observations have been made with sufficient accuracy to calculate its orbit. The perihelion passages have been as follows:

O. S. 1456, June 9.		
	Interval,	75 years, 77 days.
" 1531, Aug. 25.		
	do.	76 " 53 "
N. S. 1607, Oct. 26.		
	do.	74 " 323 "
" 1682, Sept. 14.		
	do.	76 " 180 "
" 1759, March 13.		
	do.	76 " 248 "
" 1835, Nov. 16.		

The attractions of the planets, then, have been sufficient to vary the times of revolution, to the amount of nearly two years. Knowing as we do but imperfectly the masses of the planets, and possibly their number also, it is not strange that astronomers should have been led to different results, in estimating the amount of these disturbing influences.

A highly interesting question here arises, *Is the comet gradually wasting away,* so that there is reason to apprehend that its matter may ultimately become entirely dissipated in space?

According to the usually received theories, it seems necessary to suppose that much of the matter which is thrown off to form the comet's train, must be forever dissipated, so that a gradual diminution of the comet's mass appears inevitable; yet observations shew that in the case of Halley's comet, at least, this diminution has been very gradual, if indeed it has been perceptible. It is certain, that the comet has not, at every return, presented the same splendid appearance, yet perhaps these various appearances may be accounted for, from the different positions of the comet with reference to the earth and sun, without admitting an actual diminution of its mass. At its last return, it was visible to the naked eye for five or six weeks, before its perihelion passage, and for several evenings exhibited a train from 10° to 12° in length. In 1759, the comet attracted little attention in Europe, except from astronomers. Most observers were doubtful whether it had any train. This however, is very easily explained. The comet was at too great a distance to be seen by the naked eye, until after the perihelion passage. In April, 1759, it approached somewhat nearer to the earth, than it did at its last visit; yet it had then passed its descending node; had a great southern declination, so that its meridian altitude at London was only about 10° or 15°, was consequently almost entirely lost in the mists of the horizon, and was still further obscured by the presence of the moon. In the southern hemisphere, however, where the comet had a great elevation, it shewed a conspicuous train, varying, according to La Caille, from 10° to 47°. The appearances in 1682, were not greatly different from those in 1835. Flamsteed's observations are the following: Aug. 19, train about 5° in length—Aug. 22, train 10° in length—Aug. 30, train scarcely 2° in length—Sept. 1, scarcely any train could be seen by the naked eye.—Hevelius at Dantzic observes: Comet first seen Aug. 25. The head was rather brighter and larger, than that of the year 1681, but it had a much shorter train. At first, it was about 12° long; afterwards rather shorter, and sometimes longer, as far as to 15° or 16°;

but towards the end it diminished continually. Hooke also, notices particularly the splendor of the nucleus. At this time, the comet did not approach so near the earth, as at its last two visits; yet it was then much nearer the sun, and this will account for the remarkable splendor of its nucleus. Almost every elementary treatise on astronomy, gives the length of the train in 1682, at 30°, I know not on what authority. The observations of Flamsteed and Hevelius, shew that such was not the appearance in the north of Europe. In 1607, the comet is described as having a train of considerable length; yet the accounts do not seem to indicate that it was either then, or in 1531, more splendid than it was in 1835.

The only other return which can be certainly identified, is that of 1456; for although a remarkable comet appeared in 1305, (an interval sufficient for about two revolutions of Halley's comet) its identity with the comet of Halley, is not established. Every year a new comet is discovered, and we can only identify a comet on its return to the sun, by means of its elements. These elements cannot be computed for the comet of 1305, for want of accurate observations. At its return in 1456, the comet is described as uncommonly splendid. Its tail is represented as 60° in length; and although the accounts do not bear the marks of philosophic accuracy, yet it may be true, that it then exhibited a train more splendid than on any subsequent return. Its position was on one account more favorable to its splendor, than it has ever been since that time. The comet, when it approached nearest the earth, had just passed its perihelion; and this, as is well known, is the time when the train acquires the greatest length. On the whole then, it is by no means settled, that this comet has become sensibly reduced in matter since the time of its first known visit to the sun. That it is more substantial than many known comets is certain; and it is highly probable that even its long train of 60° bore but a very small proportion to the comparatively solid matter of its nucleus.

Another interesting question here presents itself. Can this comet be seen at the same distance from the earth and sun after, as before the perihelion passage? To furnish the means of settling this question, I have computed the following table of relative intensities of the comet's light, supposing this intensity to vary as $\frac{1}{R^2 D^2}$, where R and D denote the distances from the sun and earth.

1835.	Intensity.		Intensity.
August 5, - -	.04	November 7, - -	2.44
" 23, - -	.10	" 13, - -	1.91
" 31, - -	.15	1836.	
September 6, - -	.23	January 1, - -	.25
" 13, - -	.40	" 15, - -	.20
" 19, - -	.67	February 1, - -	.17
October 1, - -	3.16	" 15, - -	.17
" 7, - -	10.71	March 1, - -	.15
" 13, - -	33.96	" 16, - -	.12
" 19, - -	15.44	April 1, - -	.08
" 25, - -	6.64	" 16, - -	.05
November 1, - -	3.32	" 21, - -	.04

After its perihelion, the comet was first seen here December 31st. It was then elevated less than 10° above the horizon, and moreover had to contend with the morning twilight; yet the sky in that quarter of the heavens was unusually transparent. It is evident, that under these circumstances, no very exact estimate of the intensity of its light could be made; but it is also certain that if its light had not been considerable, the comet could not have been seen at all. A better judgment can be formed by comparing the times at which the comet was visible to the naked eye. I saw the comet distinctly with my naked eye about a dozen times during the months of February and March. The intensity of its light by theory, may be stated at .16. Before the perihelion passage, the comet was first faintly visible to the naked eye on the 19th of September. This is probably the very earliest date, at which it could have been seen; and the intensity of its light at that time, by theory was .67, four times as great as in February. This leads us directly to the conclusion, that the intensity of the comet's light was very much increased by its approach to the sun, being nearly four times greater after the perihelion passage than before. On the evening of April 5th also, although the comet could not probably be seen by the naked eye, it was still visible in the finder of Clark's telescope, being itself a small telescope, of less than twelve inches in length. The comet has appeared larger and less distinctly defined since its perihelion than before. When first seen in August, the outline of the coma was very regular, being almost an exact circle, and its margin very well defined. Since its perihelion, it has appeared *smoky*, and irregular, with its margin so indefinite that it was impossible to estimate its diameter with any degree of confidence. It is however certain, that its diameter has been greater than before the perihelion. On the 22nd of February, I observed with Clark's telescope a central part quite bright, at least two minutes in diameter, and surrounded by a coma nearly circular, whose entire diameter was at least six minutes. At this time, the comet was about as distant from us as it was September 8th, when its apparent diameter did not exceed three minutes. These facts will suffice to shew that Halley's comet is far more substantial than Encke's comet, which in passing its perihelion becomes so rarefied, as to be scarcely able to reflect to us at all, the light of the sun.

Art. II.—*Observations on the Variation of the Magnetic Needle, made at Yale College, in* 1834 *and* 1835; by Elias Loomis.

About the middle of October, 1834, I commenced a series of observations on the diurnal variation of the magnetic needle. The instrument employed was a Variation Transit, by Dollond, belonging to the College. The needle is 5.4 inches in length, and the compass circle is graduated to quarter degrees. The azimuth circle is graduated to half degrees, and has three verniers, each reading to single minutes. The instrument was placed by a north window in North College upon a solid block of wood, resting on the floor, and so secured as to be free from all motion, except the unavoidable agitation of the building. There was no fire in the apartment where the instrument was placed, although its temperature was somewhat affected by a fire in an adjoining room. Before commencing the observations, all movable iron was removed from the vicinity of the needle; and no change was made in this respect during the continuance of the observations. The several adjustments were carefully attended to. The levels were first corrected, so that the instrument might be turned quite round in azimuth, without sensibly moving the bubble in either level. I ascertained that the perpendicular wires of the transit, were truly perpendicular to the horizon, by pointing the instrument towards a star and moving the telescope in altitude. I ascertained that the horizontal wire was truly horizontal, by causing a star to travel upon it, when the instrument was in the meridian. To ascertain if the line of collimation was perpendicular to the axis of rotation, I noted the instant of Polaris' passage at the first two wires; then reversed the axis and noted the third passage. The two intervals were very nearly equal. I ascertained that the line of collimation was in the same vertical plane with the meridian, as marked on the compass. This was done by pointing the telescope downwards towards the divisions on the compass, the focal distance being regulated by a small lens fitted to the object glass, and the central wire of the transit was made to coincide with the two zeros on the compass. Having made these preparatory adjustments, the instrument was finally placed upon the meridian, by noting the passage of Polaris. A meridian mark was fixed upon, at a considerable distance, and for verification, the passage of Polaris was repeatedly observed. As it required considerable time to turn the instrument in azimuth so as to read off by means of the verniers, these were seldom employed, but instead of them I used a compound microscope, and estimated the fraction of a quarter degree, (the smallest division on the compass,) by my eye. After some practice, I was able to do this with considerable accuracy, so that, as I judged, I was not liable to an error of more than one minute. As the first observations were necessarily imperfect, those which were made during the month of October, 1834, were rejected. A Fahrenheit's thermometer was placed about two feet above the transit, within the building, and was always observed at the same time with the needle. These observations were intended to be made at every hour of the day, from five or seven o'clock in the morning, till ten at night; yet some failures were absolutely unavoidable. At some hours of the day, the observations were contined uninterruptedly for a month together; while at a few hours, the observations were made only about half the time. This fact will explain some apparent anomalies in the following tables, particularly in the observations with the thermometer. During the period embraced in these observations, the needle has repeatedly suffered a sudden and irregular deflection to the amount, in three instances, of more than a degree. A particular account of these irregularities will be given in the latter part of this article: and they have been excluded from the means in the following table, the object there being to exhibit the regular diurnal variation. To determine the influence of the building, I took the instrument out into the President's garden, where it was supposed the local attraction must be small, if any, and made repeated observations. The influence of the building was determined to be $1^\circ\ 21'\ 41''$, which has accordingly been added to all the observations. The instrument was also carried out of the city, to a situation remote from any building, and nearly the same result obtained.

Mean Monthly Declination of the Magnetic Needle at Yale College.

	5 A. M.	6 A. M.	7 A. M.	8 A. M.	9 A. M.	10 A. M.	11 A. M.	12 A. M.	1 P. M.	2 P. M.	3 P. M.	4 P. M.	5 P. M.	6 P. M.	7 P. M.	8 P. M.	9 P. M.	10 P. M.
1834.	′ ″	° ′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″
Nov.,		5 37 10	36 33	35 46	36 3	36 50	37 22	39 2	39 52	40 21	40 37	38 57	37 10	37 3	35 50	35 13	36 5	35 25
Dec.,		37 14	37 47	36 40	36 36	36 40	37 9	39 45	38 46	39 6	38 13	38 36	37 51	36 51	36 3	35 54	35 30	35 5
1835.																		
Jan.,		37 1	36 59	36 6	35 24	35 48	37 1	39 41	39 30	39 51	39 23	39 43	37 55	38 36	37 41	36 34	36 36	36 43
Feb.,		36 27	36 29	36 9	36 18	36 28	37 5	37 55	38 32	39 1	38 9	38 27	37 25	38 1	37 32	37 9	36 43	36 15
March,		36 57	36 29	34 49	34 53	35 43	36 29	37 33	38 56	39 48	40 14	38 59	37 56	38 28	37 49	37 38	36 44	36 38
April,		35 28	34 39	33 20	33 48	35 53	37 12	38 16	40 0	40 53	39 30	39 28	37 44	36 41	36 48	36 28	36 36	35 17
May,	40 11	39 1	36 57	37 14	37 52	40 11	44 7	48 43	49 7	48 49	48 41	46 9	43 0	42 38	40 11	41 46	41 8	41 33
June,	37 7	35 10	34 41	34 50	36 49	38 20	40 49	42 14	45 52	44 7	44 41	41 1	40 13	38 52	39 46	36 1	38 24	39 54
July,	35 41	33 50	34 41	33 0	34 45	36 57	38 32	39 5	42 3	41 52	43 3	39 14	38 44	38 15	37 29	37 47	37 35	36 4
August,	35 14	32 36	31 45	34 8	34 54	37 9	42 21	42 19	45 15	44 57	42 41	44 18	41 34	39 26	37 41	38 46	37 45	35 34
Sept.,			37 18	38 3	39 48	45 21	47 48	51 11	51 21	50 26	49 41	46 18	45 41	48 1	45 41	46 41	43 1	41 41
Oct.,			43 46	40 18	41 31	43 23	46 1	50 2	50 32	50 54	50 57	49 58	48 38	49 1	48 11	49 30	46 6	49 56
Nov.,			48 0	46 11	47 36	48 0	50 46	52 38	52 48	53 50	52 59	52 47	49 32	51 6	49 21	47 17	46 11	49 18
Mean,			37 23	36 40	37 24	38 59	40 59	42 57	44 3	44 9	43 45	42 37	41 2	41 0	40 0	39 45	39 6	39 11

The mean of all the above observations, (excluding the morning observations of five and six o'clock) is 5° 40′ 34″ W. These observations shew that the north end of the needle has in the morning a motion eastward amounting to from one to three minutes, when the declination is usually less than at any other hour of the day, and may therefore be called the minimum. This minimum during the winter, is attained about nine o'clock, but during the summer months commonly as early as seven. The needle then gradually deviates to the west, and attains its greatest westerly bearing about two o'clock in the afternoon, when the declination is greater than at any other hour of the day, and may therefore be called its maximum. This maximum declination is attained during the winter months, about three o'clock; and during the summer, commonly as early as one. From this time, the needle again returns to the eastward, till it attains its original bearing about ten o'clock, and then continues nearly stationary until the next morning. The mean of the observations at nine o'clock in the evening, is a little less than at ten, agreeing with the results of other observers, who had remarked an evening minimum. The difference in this case is however, so slight, that it might be presumed accidental.

The following table exhibits the differences between the minimum and maximum of each month. It is remarkable that the amount of this variation is less in July than in either of the preceding or following months, a circumstance which seems to have been first observed by Colonel Beaufoy, in 1818, and which was confirmed by the observations of five years. It appears somewhat improbable that such a coincidence should be accidental.

1834.			
November, - -	4′ 51″	May, - -	12′ 10″
December, - -	3 9	June, - -	11 11
1835.		July, - -	10 3
January, - -	4 27	August, - -	13 30
February, - -	2 52	September, - -	14 3
March, - -	5 25	October, - -	10 39
April, - -	7 33	November, - -	7 39

The following table will show to some extent how far these changes are connected with variations of temperature.

Mean temperature at each hour of the day.

1834.	5	6	7	8	9	10	11	12	1	2	3	4	5	6	7	8	9	10	Mean.
	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
Nov.		44.7	44.0	45.4	45.9	47.1	48.8	49.6	49.4	49.3	49.6	48.4	48.9	46.5	47.4	47.5	46.9	46.9	47.4
Dec.		36.4	36.9	39.1	37.1	38.8	40.1	41.4	41.3	41 7	42.1	40.2	39.6	38.3	38.8	38.6	38.9	38.6	39.3
1835.																			
January,		37.2	34.4	29.8	34.1	32.7	35.5	36.6	36.6	37.7	38.2	37.5	35.6	32.0	35.2	36.5	32.9	32.0	35.0
February,		33.7	33.4	34.4	35.7	36.7	37.3	38.2	38.0	37.7	38.9	36.8	36.2	35.5	35.7	37.3	36.7	37.0	36.4
March,		38.8	39.6	43.9	43.3	44.6	44.5	44.7	46.5	44.8	45.3	44.8	42.5	39.1	42.4	44.2	41.0	42.6	43.1
April,		49.3	51.9	51.7	51.7	52.3	55.7	53.4	54.6	54.8	53.9	55.3	53.8	52.9	52.1	51.2	50.7	51.2	52.7
May,	68.9	68.7	62.0	63.0	63.7	64.1	63.2	65.5	65.0	68.4	63.8	66.4	66.1	63.9	61.7	63.8	64.0	63.6	64.8
June,	68.1	68.5	70.0	68.8	69.6	70.8	70.6	71.1	72.9	70.9	73.3	71.4	70.9	71.3	67.8	69.6	71.2	68.5	70.3
July,	71.9	71.3	71.4	71.8	72.7	73.3	74.9	75.5	75.9	76.3	78.1	76.3	75.2	75.7	74.1	73.3	73.1	72.5	74.1
August,	68.8	70.6	70.3	70.3	69.9	71.8	72.1	73.4	75.2	73.2	74.7	73.7	74.4	72.8	67.5	72.0	70.5	72.9	71.9
Sept.			60.1	60.2	62.5	63.9	62.7	63.7	65.5	64.5	68.4	64.1	66.0	63.7	64.1	62.6	60.0	64.1	63.5
October,			61.5	63.3	61.2	62.0	61.6	62.3	62.0	63.6	64.4	64.2	63.9	64.0	64.7	63.5	65.2	62.3	63.1
Nov.			52.8	54.5	52.4	52.3	51.5	55.1	53.2	52.9	56.3	51.9	54.6	54.1	51.6	55.8	52.2	54.2	53.5

That temperature has an influence on the amount of the diurnal variation can hardly be doubted. Thus, in November, 1834, this variation was less than in November, 1835. The thermometer indicates it to have been a colder month. The variation, during the winter months, is uniformly less than during the summer months. Yet it does not appear that this variation is strictly *proportioned* to the temperature, for then the variation must have been greatest in July. To attempt satisfactorily to explain the cause of this diurnal variation, with the present limited number of observations, seems almost hopeless. The fact of the daily variation was first discovered by Mr. George Graham, in 1722. The discovery, however, attracted little attention until 1750, when the subject was taken up by Wargentin, secretary to the Swedish Academy of Sciences, and in 1759, Mr. John Canton, an English philosopher, made about four thousand observations on the same subject.

Since this time, like observations have been made by Van Swinden, Gilpin, Hansteen and Beaufoy. The following table exhibits the mean diurnal variation for each month of several years, as found by different observers.

	Canton, in 1759.	Gilpin, in 1787.	Gilpin, in 1793.	Beaufoy, in 1817, 8, 9.
January,	7′ 8″	10′ 12″	4′ 18″	5′ 3″
February,	8 58	10 24	4 36	6 3
March,	11 17	15 0	8 30	8 22
April,	12 26	17 24	11 42	11 48
May,	13 0	18 54	10 24	9 53
June,	13 21	19 36	12 36	11 15
July,	13 14	19 36	12 30	10 43
August,	12 19	19 24	12 6	11 26
September,	11 43	15 30	9 48	9 44
October,	10 36	14 18	7 0	8 46
November,	8 9	11 6	3 48	7 10
December,	6 58	8 18	3 48	4 7

Mr. Canton first attempted to explain the cause of the diurnal variation. He established by experiment the following principle, viz. that the attractive power of a magnet decreases while the magnet is heating, and increases while it is cooling. He then assumes that the magnetic parts of the earth in the north, on the east side and on the west side of the magnetic meridian, equally attract the north end of the needle. If then the eastern magnetic parts be heated faster by the sun in the morning than the western parts, the needle will move westward, and the absolute variation will increase; but when the western magnetic parts are either heating faster or cooling slower than the eastern, the needle will move eastward, or the absolute variation will decrease. This explanation seems to account satisfactorily for the principal motion of the needle as exhibited at London, but it is not obvious how it can account for the slight easterly motion in the morning. Mr. Barlow has adopted this hypothesis, with some modification. He observes: while the sun is between the magnetic east and south, those parts being then most heated, their power will be diminished, and the south end of the needle ought to incline to the west, or the north end to the east, and we ought to expect that the greatest declination eastward should take place when the sun is equally distant between those points; as the sun approaches nearer the south, the parts to the west of the magnetic meridian, as well as those to the east, become heated, and the eastern deviation ought to decrease and disappear entirely as the sun passes the magnetic meridian, because then the effects on each side of that meridian are equal to each other.

Beyond this period, the southwestern parts will receive the greatest power of the solar rays, become weakened in their action, and the south end of the needle will deviate to the eastward, or the north end to the westward, and continue increasing in its deviation till the sun becomes S. W. (magnetic,) which happens between one and two o'clock in the afternoon, and its effect will be greater than the morning easterly deviation, because it happens when the sun has a greater altitude, and consequently a more intense action. From this period, the western deviation ought to diminish till the sun becomes west, (magnetic,) when it ought to cease entirely; because then the parts on the western side of the needle being equally heated, both to the north and to the south, there can be no tendency in either end to incline from the meridian.

Now the preceding theory may account for the variation of the needle at London, where the declination is about 24° W., but it does not agree at all with the observations at this place. Thus, according to his theory, the morning minimum should occur when the sun is in the S. E. (magnetic.) In June this occurs with us after ten o'clock, which is about three hours after the morning minimum, as indicated by observation.

Indeed both of these theories seem much better suited to the latitude of London, than to our own; for according to either theory, the needle should occupy its mean position when the parts of the earth, both to the east and west of the magnetic meridian, are equally heated, which happens about the time, or soon after, the sun passes the magnetic meridian, that is about twelve o'clock at this place. But the observations shew that the needle occupies its mean position between ten and eleven o'clock, which is about the same time as at London, although at London the magnetic meridian makes an angle with the astrononomical meridian nearly eighteen degrees greater than at New Haven. The times, both of minimum and maximum declination, are about the same at both places; so that as far as the observations go, they seem to prove that the diurnal variation is independent of the sun's position with reference to the magnetic meridian. It is highly desirable that these observations should be repeated in other parts of our country, particularly in the extreme Western States. The difference between the declination of the needle at London, and in Illinois for instance, is about thirty two degrees; and although it is difficult to determine exactly the times of the minimum and the maximum, still it would seem that a difference of two hours could not fail of being detected. It surely would seem possible to determine, whether the needle occupies its mean position between twelve and one o'clock in the afternoon, as it should do according to either of the preceding theories. Some observations made by Prof. Bache, during ten days in September, 1832, exhibit results different from my own, both as to the times and amount of the maxima and minima. It is possible that these results might be modified by observations continued for a year.

The discovery that the magnetic needle was agitated during the presence of an aurora, has usually been ascribed to Wargentin. He states that on the 28th of February, 1750, the needle was disturbed by an aurora, so as to vibrate between 6° 50′ and 9° 1′ of west variation; and on April 2nd, it shifted from a like cause backward and forward, between 4° 56′ and 9° 55′. I have repeatedly witnessed a similar effect on the needle, but have never seen the effect so great as is here stated. This disturbance of the needle by an aurora is not merely occasional, but almost invariable. During the continuance of my magnetic observations, I paid particular attention to the aurora, and in every instance when the aurora was considerable, there was a palpable agitation of the needle, and almost always a deflection to the amount of ten, twenty or thirty minutes, and in two instances of more than a degree. On the other hand, whenever the needle has experienced any unusual deflection, I have uniformly seen reason to ascribe it to an aurora. The aurora, indeed, has not always been visible; and there are several reasons why it should not be. It might occur in the day time, when it would be wholly invisible, or during moonlight, or a cloudy night, when it would be nearly if not wholly obscured. But there has not occurred an instance, during the period embraced in these observations, when the needle has suffered an unusual deflection, without an aurora being visible, unless observations were frustrated by one of the causes above mentioned. I will now enumerate all these cases, and in the chronological order:

Nov. 3, 1834.—6 h., A. M., Declination 5° 41′; 7 h., 5° 41′; 8 h., 5° 57′; 9 h., 5° 47′; 10 h., 5° 51′; 10 h. 50 m., 6° 4′; 11 h., 6° 1′; 12 h., 5° 49′; 1 h., P. M., 5° 45′. During the remainder of the day, the needle was tolerably regular, although not quite so much so as usual. Professor Olmsted observed about 8 o'clock last evening, an uncommon brightness, like the dawn in the north, much brighter than the common aurora—lasted with fluctuations all night. At 5 o'clock in the morning, it was nearly as bright as it had been in the evening. The needle was not observed during the evening of the 2nd; nor did I notice the aurora myself; if I had, I should have watched the needle at the same time. On the evening of the same day, a very brilliant aurora was seen in England, of which a description was given in the New York Observer of December 27, 1834. It is described as an arch of light, six or seven degrees in breadth, extending from the eastern to the western horizon, nearly through the zenith. The observers represent it as unusually splendid. See also Loudon's Magazine for 1835, p. 94.

Nov. 5.—6 h., P. M., Declination 5° 42′; 7 h., 5° 30′; 7 h. 40 m., 5° 28′; 8 h., 5° 30′; 9 h., 5° 38′; 10 h., 5° 36′. At half past seven in the evening, although cloudy, the entire horizon, from the west point almost to the east, was lighted up like the dawn, with very considerable brightness. The brightest point, about N. 30° W.

Nov. 6.—7 h., P. M., Declination 5° 36′; 8 h., 5° 29′; 9 h., 5° 29½′; 10 h., 5° 34′. Between eight and nine, an auroral bank of light in the north west.

Nov. 10.—7 h., P. M., Declination 5° 34′; 8 h., 5° 36′; 9 h., 5° 35′; 10 h., 5° 26′. Quite foggy. No aurora visible.

Nov. 28.—7 h., P. M., Declination 5° 37′; 8 h., 5° 32′; 9 h., 5° 37′. At eight o'clock, a faint auroral light extends along the northern horizon from the east almost to the west points.

Dec. 4.—7 h., P. M., Declination 5° 44′; 8 h., 5° 30′; 9 h., 5° 36′. At eight o'clock, a slight auroral appearance in the north; not remarkable. Aurora seen in England, (Loudon, 1835, p. 96.)

Dec. 6.—5 h., P. M., Declination 5° 44′; 6 h., 5° 31½′; 7 h., 5° 35′; 8 h., 5° 31½′; 9 h., 5° 6′; 10 h., 5° 34′; 10 h. 30 m., 5° 42′; 11 h., 5° 40′. Rainy through the forenoon—cloudy during the remainder of the day. At eight o'clock in the evening, a very evident illumination in the east. At nine o'clock, from north to east, the openings in the clouds are quite luminous. At ten o'clock, the clouds broke away and shewed the horizon from N. W. to N. E., to be all in a glow, a very bright and extensive bank of light. No arches or streamers. At half past ten, the aurora fades in the east, and brightens up in the north and north west. At eleven o'clock fades away. 'Vivid' aurora seen in England, (Loudon, 1835, p. 96.)

Dec. 8.—6 h., P. M., Declination 5° 38′; 7 h., 5° 23′; 9 h., 5° 35′; 10 h., 5° 36′. Thick clouds and the light of the moon, prevented any observations on the aurora.

	A. M.	Declination.		P. M.	Declination.
Dec. 21.—	7 h.,	5° 36′	*Dec.* 21.—	1 h.,	5° 39′
	8	6 9		2	5 39
	8 15 m.,	6 18		3	5 39
	8 40	6 37		4	5 40
	8 55	6 22		6	5 39
	9	6 7		8	5 38½
	9 15 m.,	5 54		9	5 36
	9 30	5 57		9 15 m.,	5 32
	10	5 44		9 30	5 27
	11	5 38		10	5 29½
	12	5 38			

Yesterday the air was very mild; in the evening somewhat hazy; in the night it became clear and cold—very clear all day, with a fresh breeze. At a quarter past nine in the evening, a faint aurora in the north. At half past nine, illumination very bright directly in the north, extending about 30° in azimuth, and 6° or 7° in altitude. Mere *bank* of light. Brightest point a little west of north. At 10 o'clock, the center of the aurora is a little east of north. Moon rose at a quarter past ten. This aurora was seen at Hanover, N. H., (Am. Jour., Vol. xxviii, p. 178,) and also in England, where it was described, as most brilliant, (Loudon's Magazine for 1836, p. 33.)

Dec. 22.—Needle somewhat irregular during the whole day, particularly in the evening. 6 h., P. M., Declination 5° 34′; 7 h., 5° 11′; 8 h., 5° 27′; 9 h., 5° 23′; 10 h., 5° 37′. Cloudy all day; in the evening, aurora very bright through partial openings in the clouds, a few degrees E. of N. Aurora very splendid in England, (Loudon, 1836, p. 33.)

Dec. 23.—5 h., P. M., Declination 5° 37′; 6 h., 5° 30′; 7 h., 5° 29½′; 9 h., 5° 29′; 10 h., 5° 29′. Cloudy, yet a small spot in the N. E. horizon, at 6 o'clock, very bright, about 10° in breadth, its center is about 15° north of Pollux. Not faded at all at seven.

Jan. 29, 1835.—7 h., P. M., Declination 5° 52′; 8 h., 5° 51′; 9 h., 5° 34′; 10 h., 5° 27′. Slightly hazy—manifest illumination; brightest point about 20° E. of north. At nine o'clock very bright in the same quarter. Seen at Hanover, (Am. Jour., Vol. xxviii, p. 179.)

Feb. 7.—6 h., A. M., Declination 6° 9′; 7 h., 5° 49′; 9 h., 5° 42′; 10 h., 5° 52′; 11 h., 5° 54′; 12 h., 5° 50′; 1 h., P. M., 5° 50′; 3 h., 5° 37½′; 4 h., 5° 38′; 5 h., 4° 56′; 6 h., 5° 40′; 8 h., 5° 37; 9 h., 5° 37′. The needle, it will be seen, was very irregular during the day, the extreme variation being 1° 13′, but quite regular in the evening. The evening was clear and no aurora was seen, although the light of the moon would have obscured any thing but a splendid aurora. Such a one was seen in England, (Loudon, 1836, p. 34.)

Sept. 4.—2 h. 30 m., A. M., Declination 5° 22′; 2 h. 45 m., 5° 21′; 2 h. 55 m., 5° 18′; 3 h., 5° 12′; 3 h. 15 m., 5° 21′. At half past two, a bright auroral bank of light. A streamer shoots up from north point of the horizon to γ Ursæ Minoris, about 5° in breadth; another shoots up perpendicularly to ζ Draconis. At a quarter before three, a most brilliant streamer 10° in breadth, extending up to β and γ Ursæ Minoris. Cloud stretching along on the horizon, with an arch of light extending all along upon the cloud. At five minutes before three, the streamers all moved to the east, about 6°. Highest extends up a little above γ Ursæ Minoris. This aurora was seen by Mr. E. C. Herrick, between Philadelphia and New York, from half past twelve to three o'clock. It appeared a little to the west of north; a low arch about three degrees high resting upon a cloud; beams shot up about 30° high; moved laterally to east; brightest between two and three o'clock, when at South Amboy, N. J. Produced a very sensible illumination of the village.

	P. M.		Declination.		P. M.		Declination.
Nov. 17.—	7 h.,		5° 52′	*Nov.* 17.—	11 h.,	19 m.,	5° 52′
	8		5 37		11	21	5 42
	9		5 52		11	24	5 37
	10		5 52		11	27	5 22
	10	55 m.,	5 17		11	30	5 12
	11		6 22		11	34	5 22
	11	4 m.,	5 37		11	44	5 37
	11	6	5 36		11	52	5 25
	11	10	5 47		11	59	5 37
	11	14	5 52		12	9	5 45
	11	15	6 7				

	A. M.		Declination.		A. M.	Declination.
Nov. 18.—	7 h.,		6° 53′	*Nov.* 18.—	9 h.,	5° 36′
	7	30 m.,	6 38		10	5 53
	8		6 35		11	5 52
	8	30 m.,	6 2		12	6 7

This was the most remarkable aurora I have ever witnessed, and the most remarkable disturbance of the magnetic needle, the entire range being 1° 41′. A particular account of the appearances, has been given in this Journal, Vol. xxix, p. 388. The needle was little, if at all affected by the auroral arch which appeared during the forepart of the evening, but was very violently affected by the crimson columns which formed about eleven o'clock. It is doubtful whether I observed the greatest agitation of the needle at this time, for I did not commence my observations until the corona was almost completely formed. This auroral arch was the only instance observed during the year, of an arch completely spanning the heavens. As very careful observations were made upon it at Dartmouth College, which lies almost due north from New Haven, at a distance of about one hundred and sixty four miles in a right line, we have the materials for calculating its height. This arch at Dartmouth College, appeared in the south at eight o'clock, having an altitude of 38°. At New Haven, at the same time, its altitude was 75°, from which we at once obtain its height to be about one hundred and sixty miles.

On the evening of Nov. 18th, there was a slight repetition of the aurora. A diffuse light was spread all along the northern horizon, and rose to a considerable elevation. The appearances, however, were at no time splendid. I was absent from my room during the principal part of the evening, and could not therefore observe the needle constantly; yet at seven, eight and eleven o'clock, the needle was as regular as usual.

In England an aurora was observed on the night of the 17th, and early in the morning of the 18th, and so much did the appearance resemble a natural fire at a distance, that we are told at London, 'sixty men and twelve fire engines hastened towards some dreadful conflagration.' About midnight, clouds intervened, and the fire became extinguished, but the aurora again burst forth about 3 A. M., so that the firemen were again on the alert. On the evening of the 18th, the aurora was uncommonly splendid, consisting of beams and coruscations which shot up to the zenith. The light was, however, almost entirely white. (Loudon's Magazine for 1836, pp. 23—36.)

The preceding catalogue contains all the instances in which the aurora was observed here during the year, and also all the instances in which the needle was decidedly irregular. These observations lead us to the conclusion, that auroras are most common during the months of November and December. That when the aurora consists merely of a *bank of light* like the dawn, and rises but little above the horizon, the disturbance of the magnetic needle is very little, and is generally proportioned to the vividness and extent of the aurora. The needle has sometimes appeared to veer towards the point of greatest brightness, and sometimes to recede from it. This is a question which deserves more consideration.

Auroral *beams* cause a disturbance of the needle, at least, when the beams are themselves in active motion.

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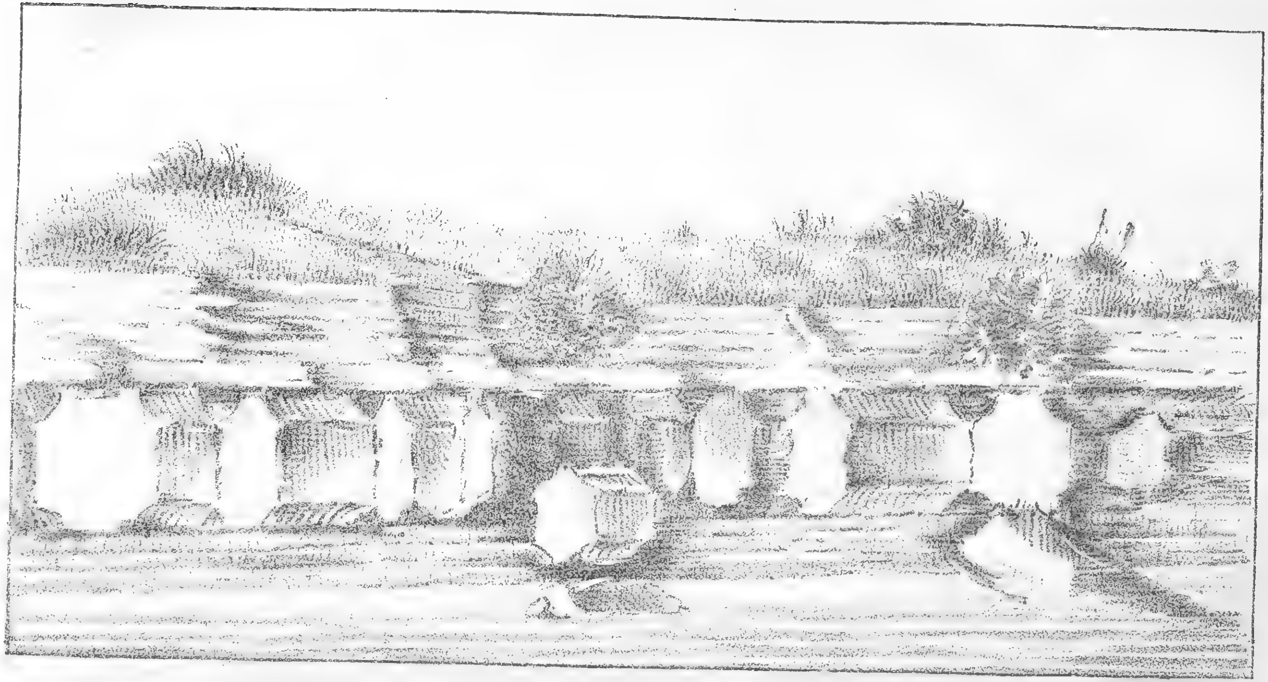
No. 1

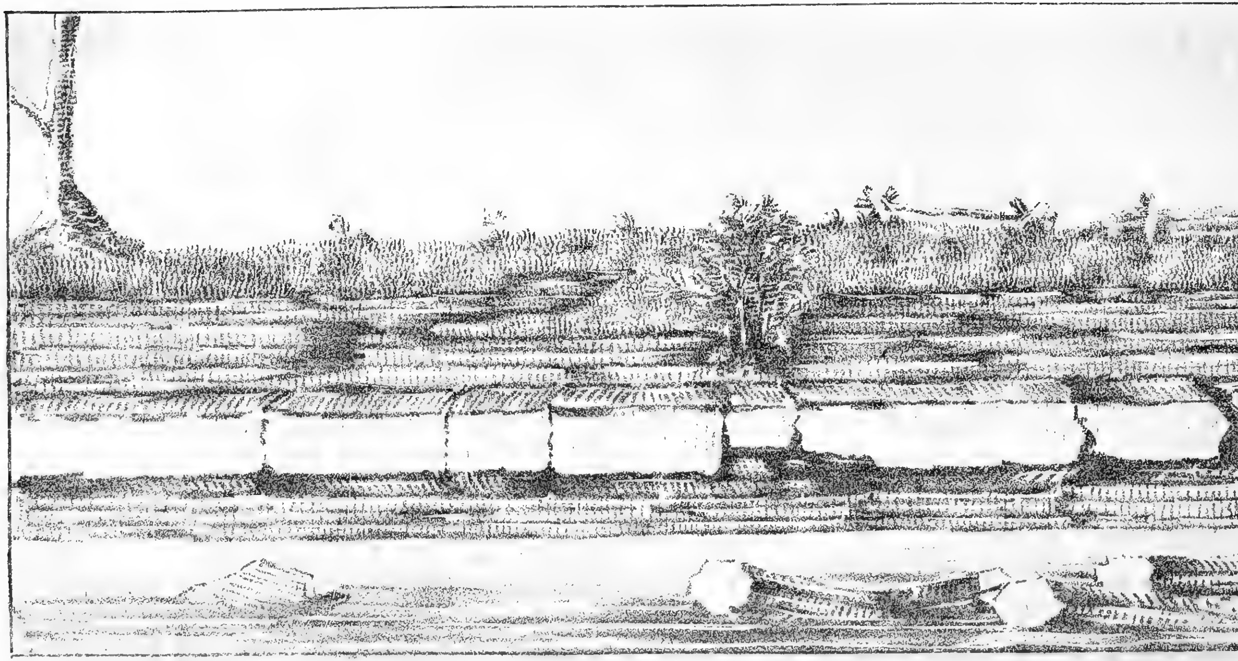
Transition Rocks of the Cataraqui



Basaltiform Lithographic Limestone of Kingston U.C. near Murney's Point.

N.° 2

Transition Rocks of the Cataraqui



From Drawings by Captn. Bonnycastle R.E. — H.B. Junr. del.

Basaltiform Lithographic Limestone of Kingston U.C near Nickall's Hop Ground

Tazewell Lith. from Canadian Stone York U.C. 1833.

Auroral *waves or flashes*, when rising to the magnetic pole, cause a violent agitation of the needle, which consists of an irregular oscillation, sometimes to the amount of nearly a degree, on each side of its mean position. When the aurora ceases, the needle soon returns to its former state.

An auroral *arch* has little, if any, influence on the magnetic needle.

During snow storms and thunder storms, I have commonly observed considerable *agitation* of the needle, like that arising from a shaking of the whole building, but have never seen any *deflection* of the needle. No great weight, however, can be attached to this observation, for it is by no means uncommon for the needle to shake with a very tremulous motion, even when there is no agitation of the building, and no perceptible cause for the disturbance.

Art. III.—*On the Transition Rocks of the Cataraqui;* by Capt. R. H. Bonnycastle, R. En.

Continued from Vol. xxiv, page 104 of this Journal.

Having described as well as I was able, the singular horizontal basaltiform or prismatic limestone of Kingston, I now request the reader's attention to the accompanying lithograph, as although it is very rudely executed, it is interesting from its having been drawn on a new species of lithographic stone, which contrary to all others hitherto used, is nearly black and resembles marble, when smoothed for the printer's use.

It is however, highly adapted to the art, particularly in diagrams and maps, and not so subject to fracture under pressure.

These drawings represent the basaltiform lithographic limestone of Kingston, as viewed at two points, near the western end of the town.

The upper one, shews the beds, as they appear from the edge of the water forming the bank; the under one, the beds viewed at their extremities, left open by quarrying, and in this view the octagonal figure is completely displayed.

The third view heretofore mentioned, was a bird's eye one, and shewed the basaltiform beds extending into the lake, but to what distance I have not however, been able to ascertain. This drawing it is not now deemed necessary to introduce, as the others are sufficiently explanatory.

Limestone of so very ancient a class, being formed into prismatic shapes, so similarly to basalt, but taking a nearly horizontal position, presents a new feature in geology, and although at first sight, it might go far to prove the theories which have been advanced concerning the igneous origin of the Cataraqui rocks in its vicinity, yet on a more careful examination of them, it does not appear to justify those theories, or to cause me to waver from the opinion originally given, that the Cataraqui granitic rocks are of an age assimilating to the transition limestone, with which they are so closely connected, and the singular appearance this lithographic limestone has here assumed, in a limited locality, may be traced satisfactorily to the same causes which have made the greenstone of Lake Superior associated with sienite to assume the columnar form, which is at Kingston, after all, perhaps, merely a modification of cleavage on a grand scale, or a mere deviation from ordinary appearances, similar to the beautiful minute columnar limestone of the adjacent beds which we have already described, and which may as well be brought forward to testify that these enormously thick masses of horizontal fossiliferous rocks owed their origin to volcanic agency.

In fact, I see nothing after several years experience, to alter the opinions which have been assumed by the geologists already adverted to, that these are granitic rocks of the families posterior to the primary class, and I think that these opinions are very strongly assisted and developed in Upper Canada, from the extremity of Lake Superior to the shores of the United States, near the Thousand Islands of the St. Lawrence, and in further investigating this very interesting subject, I shall hereafter endeavor to undertake a description of the *Lacustrian* chain, under which term is embraced the ridge which bounds Superior, Huron and the ancient shores of Ontario, and endeavor to prove that the primary rocks scarcely exist in this chain, which appears to me to be of a much more recent date than that class, and to have owed its origin to the same influence which formed the transition rocks of the Cataraqui, and has been but little affected by that igneous agency which created the decided trappose masses, occasionally blended with it, and I am the more inclined to support this opinion, from the absence of mica, either schistose, or forming a considerable share of ingredients in the granitic aggregates of this immensely long chain, which strikes across the St. Lawrence, and is split into the Thousand Islands, before it sweeps onward towards the Rocky Mountains, the true Andes of North America.

The want of altitude in the Lacustrian chain, is very remarkable, until it reaches Lake Superior, where the transition limestones are not so observable, and where igneous agency is more apparent.

It is well known, that the greater portion of the United States and of Canada, is decidedly, either of the transition or secondary class, and that the beds of these rocks are there of enormous thickness, and are either very little elevated above the ocean, or in many places below its level, whilst the primary rocks are comparatively of little extent in those regions, and never lofty.

If it be true therefore, as BAKEWELL has ingeniously advanced, that volcanic action does not always (and perhaps it never does,) take place, in what we have hitherto considered the lowest rock formation or granite; from what an inconceivable depth must that action have originated in this vast tract of country, and how likely that it formed the low ridges of the Lacustrian range, by a partial upheaving of the older transition rocks which it occasionally pierced through, and what extraordinary power must have been exerted where it has fractured the granite and its superincumbent beds, to eject and form recognized trap.

BAKEWELL observes, that in Auvergne and a large part of Central France, granite is the foundation rock, and that it has been pierced through by numerous ancient volcanoes, which have poured currents of lava over its surface, and covered other parts with loose scoria and black volcanic sand, some of the currents of lava appearing as fresh as the recent ones from Etna or Vesuvius. In other parts of Auvergne, he thinks, only, that the granite has been acted upon by subterraneous fire in situ, and in some mountains, as in the Pay de Chopine near Rione, granite and volcanic rocks are intermixed, one part being true granite, and the other volcanic porphyry, or trachyte, and this is also the case on Lake Superior.

Where the seat of the igneous agency is very deep, as it no doubt is, in the enormous basin of North America, and covered by the primary rocks and their superincumbent masses, it would be not at all unlikely, that in travelling to find a vent for the pent up gases, it would upheave and finally crack long tortuous lines, spending the utmost of its force wherever it formed spiracles by which to eject its confined vapors.

These spiracles, no doubt, in the case of the Canadas, must be sought for to the westward of Lake Superior, and to the eastward of the St. Lawrence, and hence the wild and disjointed masses which create such a display of magnificent scenes on the northern coasts of Superior, and on the borders of the Gulf of St. Lawrence, in Labrador and Gaspesia, whilst all the intervening country from Quebec to Huron is tame and level, with the exception of the Lacustrian chain, and a few isolated trappose mounds, as those at and near Montreal.

Earthquake, even in our own days, exerts its influence on part of this line near the *Saguenay,* where the appropriate names of *Mal Baie* and *Les Eboulemens* testify its best known localities. Whether similar phenomena occur in the northern region of Superior or not, cannot easily be ascertained, as that country, from the broken nature of its surface, is a complete desert even to the Indians, who are unable to exist in it.

The nature of the shocks in the Saguenay country has been subjected to as much investigation as a territory so thinly inhabited and so rarely visited by men of research, could afford proper opportunities for, but it has been ascertained beyond doubt, that at Mal Baie, their direction is easterly, or proceeding to the convulsed line of the Gaspé country. A singular noise like the roaring of a chimney on fire precedes them, accompanied by distinct concussions, and it appears that there is very little reason to disbelieve the assertion of the natives and settlers, that an actual volcanic eruption has happened within the memory of man, in the unexplored back country.

It would be very interesting to trace minutely the connection of the volcanic rocks in Lower Canada and Labrador, where there are most interesting facts to be studied regarding the formation, denominated trappose, and the singular columnar basalt of Castle Reef rock in Henley Harbor on the Labrador coast; it merits that observation, which we trust Captain Bayfield has given to it, should his survey of the Gulf, have yet extended there, for, although on a smaller scale, it appears by the description given of it in the first volume of the Quebec Transactions, to be equally interesting with the similar formations of Staffa, and the north coast of Ireland, and that there are several caves in its vicinity, which indicate that it may extend over a much larger tract of country, than has hitherto been noticed. The columns there, are stated to be vertical, extending in circumference from two to seven or eight feet, and jointed by the cup shaped sockets at every foot or eighteen inches; their number of sides varying from pentagonal to the hexagon, heptagon and octagon, whilst their height reaches to twenty five feet, at one place, where they support an enormous roof or cap of amorphous basalt fifty feet in thickness, resembling an irregular fortification.

The explored course of this basaltic formation was from east to west, and the columns to the westward were of greater magnitude than those to the eastward.

That Canada has been subject to the influence of earthquakes, there can be no doubt, not only from the configuration of the country, but from actual observation.

The greatest convulsion on record, was that mentioned by the Jesuits, as having occurred in 1663, which lasted six months, or from January to July, overturning mountains, altering the course of rivers, and rendering the mighty St. Lawrence white. Such an event too, (probably,) from the direction that river takes, created its present channel, and particularly on the Niagara Frontier, where the mural precipices of sandstone and shale, seem to have been formed by the rending of the rocks asunder into a vast longitudinal fissure.

At Mal Baie and the country adjacent, it appears that since 1663, the inhabitants have noticed that a recurrence of this dreadful visitation in its greatest vigor, occurs periodically once in twenty five years, lasting about forty days at each return; these exact periods are probably not accurately defined. The greatest shocks felt of late years, were in 1791, which date is however within the calculation, as it embraces the fifth quarter century from 1653. I had at one time, thought that the phenomena of the dark days of Lower Canada, might be ascribed to this influence, but the recorded dates do not agree, for 1785 and 1814 do not come within the quarter centuries, although neither are far from them. Charlevoix observes, that it rained ashes for six hours at the mouth of the Saguenay in 1663, and there were such clouds of light dust, resembling smoke, that an universal conflagration was dreaded. This looks as though the grand outlet of the gases in the earthquake of 1663, was near Mal Baie, where perhaps, or in Labrador, an active volcano will yet be found.

But well established as the circumstances connected with the Saguenay country may ever be, in the case of the *Lacustrian* range the outlets for the gases must be sought for either in the Rocky

Mountains or in the unexplored countries between the Canadas and the Arctic Ocean. The Indians assert that there is an active volcano in the interior, behind the settled ranges of townships on the northern shore of the St. Lawrence, and a crater of a small extinct volcano is marked on the Upper Canada maps, in the township of Mulmur, near that elevated range called the Blue Mountains, which border the shores of Lake Huron in Nottawasaga Bay, from Cabot's Head to the termination of that vast gulf.

These mountains, which from a distant view I obtained of them last summer, are the highest land in those parts of Upper Canada yet laid out for settlement, and will, I have no doubt, when examined, prove to be a spur of that chain which runs along the opposite coast of Huron, and is connected with the Rocky Mountains on one side, and with the Atlantic, through the Thousand Islands of the St. Lawrence, on the other.

That vast tracts of country have been upheaved, even in our own times, is well known, and that the continued action of volcanic fires exists at an immense depth, in given directions, is also clearly established, and is no where better exemplified than by looking at the map of Mexico, where from Tuxtla on the Gulf of Mexico to the Revillagegido Islands, on the Pacific shore, is nearly a straight line, under the same parallel of longitude, of active craters. From Mount St. Elias, on the Pacific side of the Rocky Mountains or North American Andes, to Cape Horn, or almost the whole semi-circumference of the globe, is another but more tortuous chain of burning mountains, the number of which is not even yet known.

These are the valves by which the destructive gases are liberated, and to which the New World owes its safety, and in the original effort to reach them, on the north, have the rocks been rent, and the trappose formations of the Canadas been called into existence.

In Lower Canada, it is probable, as we have already stated, there has been an offset to these breathing holes of the Fire King, for there, in the country lying along the southern shore of the Gulf, and in some parts of its northern littoral, near the Saguenay, there are those dome-shaped mountains, which were no doubt originally active, and which, when explored, will exhibit traces of their former use. Nothing can be more wild, dreary, or magnificent, than the scenery of this region. Lofty cones, (such as those named the *Paps of Matane*,) in the interior, show themselves to the observer, from the deck, as he coasts along from Anticosti up the Great River, whilst the shore is bounded by tremendous mural precipices, hundreds of feet in perpendicular altitude, and the country appears broken and disrupted into every imaginable form that the most awful idea of earthquake could suggest.

The highest mountain known in this portion of Canada, has been ascertained to be three thousand seven hundred feet above the sea;* but as the country has never been much explored, it is not improbable there are others yet higher.

During a tour, in 1831, to the Labrador coast, Anticosti, the Gulf of St. Lawrence, the Bay of Chaleurs, and the Restigouche River, undertaken by His Excellency Lord Aylmer for scientific purposes, as well as for administrative information, I was much struck by the volcanic appearance of the extensive regions which are opposite to the island of Anticosti, on both sides of the Gulf of St. Lawrence.

The island itself is not without much geological interest, but access to it is difficult, owing to the horror with which sailors look upon its desolate shores, particularly since the melancholy wreck of the Granicus, when from mismanagement the passengers and crew were compelled to become anthropophagi, and out of a numerous company but one unmutilated or undevoured corse was found, which had been once the last surviving relic of a mortality unprecedented in the annals of maritime adventure.

In my visit to Anticosti, I certainly saw no very striking features of igneous agency in the rocks, which were chiefly fossiliferous, excepting that at the south west point or provision post, at the mouth of the Jupiter River, where there is a high range of coast, visible at a great distance at sea, and called the White Cliffs, which have an appearance, although they consist of limestone, of vertical stratification. On landing, however, and examining them as near as they could be got at, (for there is a great mass of debris at their foot, and they are perfectly perpendicular,) I had reason to think that the stratification was horizontal, and the vertical or pilastre appearance, which is very marked and almost basaltically well defined, arose from cleavage, and certainly may have been caused, although the inference is not very conclusive, by the undoubted volcanic or Plu-

* By Captain Bayfield, R. N.; and on the opposite coast of Labrador, Mount Thoresby, an island south of Kiglapyed, was found by the officers of H. M. ships Medusa and Thalia to be two thousand seven hundred and thirty three, whilst the Kiglapyed, the Kaumayok, and Nachwak, were much higher.

tonian struggle, which is so visible, both at the Bay of the Seven Islands, on the opposite coast of Labrador, and on the Gaspé shore of the St. Lawrence, although they are both nearly a day's sail distant from these cliffs.

Here, in the highly picturesque scenery of Jupiter River, where a shed filled with provisions, unlocked and having a few utensils for cooking, with a painted board showing that it is twelve long leagues to the next depot of stores, we deeply felt, amid the stern solitude, where no animate being seemed in existence, save the seal or the sea bird, our own insecurity, heightened by the storm-beaten wreck of the large brig Bonito, which formed a prominent and sad feature in the otherwise beautiful and grand display of coast landscape.

But to return to our present object, or the influence which formed the dome-shaped and sugar-loaf cones in the vicinity of this dreaded island.

The Paps of Matane, which we have already mentioned as being very advantageously seen from the sea, are not, however, easy of access, owing to the desolate and howling wilderness in which they rear themselves.

But there is a cone which can be approached without difficulty, and which has been seen by several travelers.

It is situated on the New Brunswick side of the Restigouche River, exactly opposite the opening of the *Kempt Road,* from the Indian mission of Point a la Croix, or Restigouche, in Canada, to Metis on the shore of the St. Lawrence. It was visited by our party in 1831, and again by Captain Baddeley soon afterwards, when it was ascertained to be of trap, and to rise to the height of one thousand two hundred and thirty feet. I am not aware, however, (for I have not seen that officer's account of his exploratory journey,) whether he was able or not to ascend to its summit, which I conceive could not be accomplished without extreme labor and the actual cutting of a passage, as it is densely covered with the eternal forest and is very steep. From the views we were able to obtain, both of it and of the singular pointed mountains in the rear of the mission, I should suppose that no vestiges of a crater are to be found,—such appearance perhaps, however, will be discovered in the interior, amid the more numerous and more lofty domes, particularly as the fragments of trap, which lay along the streams crossed by Captain Baddeley, exhibited undoubted signs of a lava-like formation, in the actual vicinity of these domes.

There is no place in the world more strongly exhibiting signs of volcanic action of a very ancient era, than the country bordering the Restigouche River, which separates Canada from New Brunswick; and there is no part of Canada where the pencil of the artist may meet with fitter employment, in delineating sublime and interesting geological scenery, than there.

At *Tracadegash*, in the Bay or rather Gulf of Chaleurs, there is another very lofty dome, remarkable for steepness, and the whole region, on entering the mouth of the Restigouche, at its confluence with the ocean in the bottom of Chaleurs Bay, exhibits in the distance a series of strongly marked and brightly colored cones, standing in deep blue relief against the sky, when viewed on a placid autumnal day, and beautifully contrasted with the islands, which protect the entrance of that fine harbor.

The direction of the regular rocks in this portion of country, which are chiefly transition and secondary, is the same as that in the Atlantic region of the United States, or northeast and southwest, or that of the course of the St. Lawrence from Newfoundland to the state of Ohio; and here we find those curious vesicular or amygdaloidal wackes, which accompany the porphyries and greenstones of Lake Superior, and which appear to pass so easily into each other; and here we also find the conglomerates of Superior and Huron, a variety of which may even be collected on the shores of Ontario, near the mouth of the Niagara. These conglomerates are chiefly remarkable for containing that bright red mineral, which has hitherto passed for a species of jasper, and is now supposed to be zeolitic and has received the name of *Huronite*, from its abundance amid the rocks of upper Huron.

It is very interesting to find the extreme points of so vast a range as that from Gaspesia to the western country of Lake Superior, or nearly three thousand miles, exhibiting the same geological facts and features, which only proves the truth of the remark of an eminent writer, that to American geologists we may look for the development of many of the still unsettled and mysterious parts of the science.

Whilst generalizing, however, on some of the features of this extensive range, we must not wholly lose sight of the object with which the present essay was commenced, which was merely to elucidate the circumstances connected with the transition rocks of the Cataraqui, or that portion of the transition formation of the great Lacustrian chain, which is connected by the Thousand Islands with the primary and transition rocks of the eastern states on the one side, and with the same classes on Lake Superior on the other, classes distinguished in the granitic families by the absence of mica, the presence of amphibole, the abundance of feldspar, and the facility with which it appears to have passed into greenstone, rendering it questionable whether it has ever been connected with what has hitherto been termed the primary rocks.

Captain Bayfield, in his excellent succinct account of the geology of Lake Superior, which he enjoyed the enviable advantage of examining under favorable circumstances, is of opinion that the greenstone of Lake Superior, which occurs in immense beds, forming perpendicular and columnar precipices upwards of a thousand feet in altitude above the great lake, and which passes into sienite and what he terms sienitic granite, should not be confounded with the trap, greenstones, and sienitic masses, associated with the amygdaloids and porphyries, and which in general occupy, excepting in veins, a comparatively lower situation, from the circumstances of its alternating with and sometimes overlying the granite, and from appearances of stratification in it, as well as its forming chains of hills and immense beds.

That it should not be classed with the overlying rocks, the trap, greenstones, amygdaloids, and clay porphyries, is very evident, but as, according to Dr. Bigsby, and from what Capt. Bayfield observed, as well as the appearances in the sienite of the Cataraqui of stratification, it can scarcely be said to belong to the primary family, and may, therefore, with much probability, come under the transition, submedial, or Hemilysian era, particularly if any facts can be substantiated which connect it with the known limestone strata of that epoch.

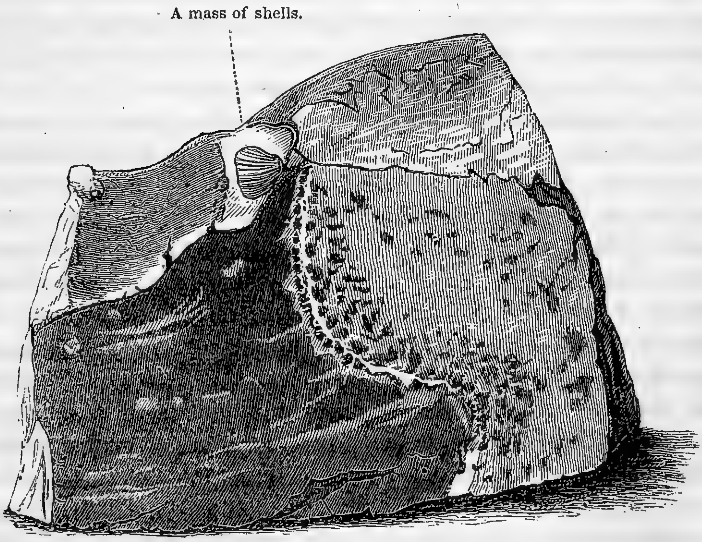
And in order to pursue this investigation, we shall again visit the shores of the Cataraqui, and endeavor to find amidst the never ending varieties of the amphibolic rocks there, whether or no such facts are to be elicited.

The sienite, of a bright red color, which has been already mentioned as forming a large mass of the Kingston rock, and as being joined as it were to the transition trilobitic limestone, exhibits some remarkable features, both on Cedar Island, and on the main land, in the township of Pittsburgh.

Here, as we have already stated, the sienite actually penetrates the limestone, and that in a very remarkable manner, without having changed the form or nature of the rock, by hardening, baking, or calcining it, and without even altering its usual dark color and aspect, but is detached into it, in large and small nodules or masses, conjoining with it at the lines of contact of the two rocks, and shooting crystals of quartz and hornblende into the lime, in such a manner as would naturally occur, if the substance of the sienite and of the limestone were both in a soft or jelly-like state originally, and gradually hardened together.

Every variety of appearance is thus assumed which the sienite and limestone could afford, but the limestone has yielded few or none of the prominent features of its original character. On examining a vast number of fragments, taken from the places where these rocks were in contact, I was surprised in polishing some of them, in order to exhibit their singular appearance better, to find fossils at the point of junction.

The best defined of these I have selected, and the accompanying is a drawing from one of them.



This is about half the real size of the specimen, taken from the southwest end of Cedar Island; the little round spots on the limestone side denote the quartz, as well as the long curved lines, whiter than the other portions, which have shot into the calcareous body from the sienite; the whitish undefined band, between the sienite and the lime, seems also, from its great hardness and the difficulty of polishing it down to a level with the feldspar and lime, to be very pure quartz. Here and there, a small hornblende crystal may be observed in the lime. The feldspar of the sienite is of nearly a deep flesh color, and the lime and sienite are so closely connected that they cannot be separated. The limestone, at this locality, has the same appearance as the rock on the main land, and contains terebratulæ, two of which shells may be observed in the drawing, at the very junction of the two rocks and almost in the sienite, and there are traces of very large ones visible. On close inspection of cut and polished specimens, the limestone, instead of looking like a conglomerate, which it otherwise resembles, from the weathering of the calcareous matter in the rock, proves to be porphyritic, the chief character of which is that of quartz nodules, pasted as it were in lime, the larger spots of sienite interspersed being only occasional, and probably the nuclei whence the quartzose particles emigrated. It is only in the immediate neighborhood of the conjunction of the limestone and sienite, that this porphyroid structure is, however visible; the limestone soon regains its independence, and exhibits only its usual features of dark blue stone, with occasional glimmerings of crystalline lime and few organic remains.

In its neighborhood are observed the detached tables of a grayer and highly crystalline limestone, with abundance of those specimens of orthoceræ which have received the name of Huronia, and which look so very like vertebræ as to be called back-bones by the uninitiated. On these tables are stuck, as it were, fragments of sienite and of quartz, which project considerably, and have no doubt been subject to the action of water, since their original contact with the limestone, which has been uniformly worn away, so as to discover the orthoceratites imbedded, and have been thus preserved from farther harm by the surrounding protuberances of the harder material.

I am well aware that the mass of impressions of marine shells, nearly in and actually touching the sienite, may not be considered a very strong argument in favor of the aqueous origin of that sienite, as instances are said to have occurred wherein such casts of fossil animal remains have remained perfect in a high state of igneous action; but yet I think it will scarcely be asserted, that at the point of contact and actual junction, between a highly crystalline substance in a state of fusion, and a subcrystalline and very calcinable one, in a soft state also, that perfect casts of fossils would remain unobliterated, or that the limestone would not entirely lose its usual dark appearance, and become white, more crystalline, or incinerated.* It is certainly dangerous ground, at present, to attempt to combat the numerous hosts of ignipotent knights, who have appeared in the lists of modern geology; but still theirs is yet only the armor of theory, and may not prove impervious to the keen-edged weapons of fact.

That all the singular appearances in the granitic aggregates should be easily accounted for, by the rocks containing them, and chiefly the amphibolic ones, having undergone their changes of character and situation from protrusion and the other supposed effects of volcanic agency, is a matter I must confess not all so easily settled to my own conviction, and although I have lately and carefully perused the arguments adduced in Professor Hitchcock's Report to the state legislature of Massachusetts, which, with its numerous diagrams, embraces more details of the igneous theory of the early rocks than I have elsewhere observed, I am not convinced that the numerous instances there adduced of the appearances of granite, gneiss, and mica slate, being pierced by small or large dykes and veins of granite, may not just as easily be accounted for by the one theory as the other, and may in fact all have arisen from the mere modification of the different elements of which rocks so intimately connected in their relations are composed, either from unequal pressure, a greater proportion of the particles in one situation than another, chemical action in the act of crystallization, and other well known causes.

Why in fact, should not granite, gneiss, quartz rock and mica slate, be all classed under one head, instead of being divided as they now are, for they are all mere modifications of the constituent parts of the old rocks, and may as well be reckoned as one family, as the numerous and almost endless varieties of limestone from chalk to the oldest transition limes are.

It appears to me that, although there are still great difficulties in the classification of the early rocks, there is not any absolute disproof, that they may extend far into the Hemilysian period, and that their appearance there, may be as satisfactorily explained, as the appearance of the transition lime containing few or no fossils, which

* The fact of lava containing shells, where it was in contact with the ocean, does not militate against this, as the gradual cooling would be sufficient to allow shells to sink into it, without much alteration.

although totally different in appearance, is evidently a mere modification of the same substance in its most crystalline state, which has received the name of primitive, and which alters in its aspect so wonderfully as it passes through all its stages into chalk.

If we then admit a posterior formation of granite and its family, we have only attentively to mark the difference between it and those rocks which so closely resemble it in the volcanic regions, to establish the limit between them, and this, no doubt, in the advanced state of the science, will soon be discovered.

I have as yet seen, even in the boulders so extensively spread over Canada, very little appearance of a true granite, and am much inclined to think, that the rocks in situ of that family are there newer than is generally imagined, and to believe also that they have been not much disturbed by volcanic agency, but as this article has already reached beyond the bounds, I should assign to it, I shall merely at present observe, that I think the opinion that volcanic agency is carried on, not in the granite, but very far below it, has not been sufficiently attended to, and that in the case of the trachytic and trap rocks with all other varieties of protruded and overlying unconformable masses of admitted igneous origin, most frequently the substances came from beneath the granite altogether, and that it has not, except in the upheaving, been so much altered by them as is supposed. If therefore there are enormous stores of amphibole and pyroxene above those vast laboratories under the granite, and if the materials from which the granite itself has been created, exist in an independent state, it is easy to suppose that the endless varieties of interminglement and apparent confusion in the igneous rocks, may be accounted for, and that the crystallizations of pyroxene, feldspar, amphibole, mica, leucite, &c. in the petro-siliceous basalt, may be simply accounted for by chemical arrangement, and the cooling of the lava, and the reason of the difference of bases between those kinds of lava distinguished by DOLOMIEU as argillaceous, siliceous, granitic or rather feldspathose and leucitic, assuming such distinct features, may be found in the volcanic action, having affected the regions below the granite, in different dispositions of its force amongst the different deposits of these elements. It may be fanciful to suppose so, but I confess I cannot see why what is called the crust of the earth is limited to the granites, and why beneath those granites, there may not be rocks of a nature more individualized, with large deposits of the metals, and if this should ever be proved by the foundations of the granite being once observed, it would not be deemed very difficult, thus to account for all the appearances which the trap rocks put on without having recourse to the more far fetched notions of the gradual relaxation of volcanic action, from the creation down to the era of the newer traps and lavas.

Moreover, as it is known that every substance in the mineralogical world, is capable of being reduced into its constituents, and as those constituents are further reducible into metallic bases, it is just as easy to imagine therefore, that granite and its compounds had an aqueous as an igneous original, and it appears more probable to the chemist, that the particles of feldspar, mica and quartz formed by their elements of silex, alumina, lime and alkali would assume their present confused crystallizations in the absence of great heat, rather than in its intensity, and as all the substances entering into the composition of granite, have some of each other's elements, it is not difficult to believe that where the element of silex predominated, a quartzose granite would be formed, where the aluminous element was most attracted, a feldspathose or micaceous, and the presence of calcareous and magnesian particles, would create hornblende, and consequently be sienites. The paper maker takes three or four colors and drops them on a basis of water or oil, arranged in a bed or stratum, the stick is then applied, and the colors stirred about. When they have thus mixed, a sheet of paper is dipped on to them, and the artist shews you a beautifully variegated imitation of marble, in which you in vain try to trace the original blots of distinct color. This is an operation a child may perform, and yet nature is equally simple in hers, and the varieties of granite were no doubt made in a manner equally easy to be comprehended, chemical affinity, or disturbance of the fluid masses under pressure, creating all those beautiful displays of arrangement which the original elements there assume, whilst in the trappose rocks, the suddenness with which they would cool on reaching the ocean, or the air, would equally well account for the singular perfection of some of their contained crystals, particularly in the dolerites and lavas.

Let us not, therefore, too hastily abandon altogether the Wernerian doctrine, for although parts of it are indefensible, and have been disproved by facts, yet on the whole, it has not been so much shaken as some of its antagonists are willing to believe. It remains to be seen, whether or not this theory may not be so interwoven with the Vulcanian, as to render each capable of assisting towards the development of the other, for that a great portion of the present state of the crust of the earth has resulted from the agency of heat, no one in possession of the facts, and exercising a sound judgment, will now attempt to deny, and also that the agent which caused these appearances is still in active operation. The trident of Neptune would never have been reared over his oceans, had not Vulcan forged it for the Sea King, yet it is ever to be borne in mind that the liquid realms of that monarch contain in their constitution the principles over which in another mode of combination, the master of fire rules with equal power.

(To be continued.)

Art. IV.—*Theory of the Variations of the Arbitrary Constants in Elliptic Motion*; by Prof. Theodore Strong.

We shall (for simplicity,) consider a system of three bodies only, whose dimensions are so small when compared with their distances, that they may be neglected, so that their masses may be regarded as united at their centres of gravity.

Let then M', m, m' denote the masses of the bodies collected at their respective centres of gravity, and suppose that m, m' revolve in the same direction around M' considered as at rest, and that the bodies attract each other with forces, which are expressed by their quantities of matter divided by the squares of their distances; to determine the circumstances of the motion of m.

Put t for the time, and x, y, z; x', y', z' for the rectangular coordinates of m and m', when referred to any fixed system of rectangular axes whose origin is at M', also r, r', r'' severally for the distances of m, m' from M, and for their distances from each other, at the time t; then $r^2=x^2+y^2+z^2$, $r'^2=x'^2+y'^2+z'^2$, $r''^2=(x'-x)^2+(y'-y)^2+(z'-z)^2$, (1).

By supposition, $\frac{M'}{r^2}, \frac{m}{r^2}, \frac{m'}{r'^2}, \frac{m'}{r''^2}$ are the forces with which the bodies attract each other, and if dt denotes the differential of the time, it is evident that $\frac{M'}{r^2}dt, \frac{m}{r^2}dt, \frac{m'}{r'^2}dt, \frac{m'}{r''^2}dt$ will denote the velocities which the attractions considered as constant will communicate to the attracted bodies in the time dt; $\therefore \frac{M'+m}{r^2}dt$ is the velocity with which M′ and m approach each other at the end of the instant dt, by their mutual attractions during the same time, and $\frac{M'+m}{r^2}$ denotes the force of attraction of m towards M′ regarded as at rest.

Put $M'+m=M$, then resolving the attractions in the direction of the axis of x, we have $\frac{Mx}{r^3}+\frac{m'x'}{r'^3}-\frac{m'(x'-x)}{r''^3}$ for the whole force with which m is attracted in that direction towards M regarded as at rest, for $\frac{m'x'}{r'^3}$ draws M′ towards m and $\frac{m'(x'-x)}{r''^3}$ draws m from M′, and therefore has been written with the sign minus; in the same way, we have $\frac{My}{r^3}+\frac{m'y'}{r'^3}-\frac{m'(y'-y)}{r''^3}$, $\frac{Mz}{r^3}+\frac{m'z'}{r'^3}-\frac{m'(z'-z)}{r''^3}$ for the whole forces of attraction of m towards M′ in the directions of the axes of y and z; and by multiplying these attractions by dt we shall have the velocities which they will severally communicate to m towards M′ in the same directions.

Now $\frac{dx}{dt}$, $\frac{dy}{dt}$, $\frac{dz}{dt}$ denote the velocities of m in the directions of x, y, z severally at the time t, and we shall suppose that they tend to increase the coordinates; it is evident, that at the end of the time dt, the velocities will become $\frac{dx}{dt}+d\frac{dx}{dt}$, $\frac{dy}{dt}+d\frac{dy}{dt}$, $\frac{dz}{dt}+d\frac{dz}{dt}$, which are evidently less than the former velocities since m is attracted towards M′, hence we shall have $-d\frac{dx}{dt}$, $-d\frac{dy}{dt}$, $-d\frac{dz}{dt}$, for the velocities received by m in the directions of x, y, z severally in the time dt; but the velocities communicated and received in the same directions in the instant dt, are evidently equal to each other, $\therefore$ by making dt const. we have $\frac{d^2x}{dt^2}+\frac{Mx}{r^3}+\frac{m'x'}{r'^3}-\frac{m'(x'-x)}{r''^3}=0$, $\frac{d^2y}{dt^2}+\frac{My}{r^3}+\frac{m'y'}{r'^3}-\frac{m'(y'-y)}{r''^3}=0$, $\frac{d^2z}{dt^2}+\frac{Mz}{r^3}+\frac{m'z'}{r'^3}-\frac{m'(z'-z)}{r''^3}=0$, (2).

Multiply the forces in the directions of x, y, z by dx, dy, dz severally, and put $-dQ$ for the sum of the products, then by taking the integral relative to x, y, z, we have $Q=\frac{M}{r}-\frac{m'(xx'+yy'+zz')}{r'^3}+\frac{m'}{r''}+c$, c being a function of x', y', z', which may be neglected or it may be considered as included in Q; it is evident that the partial differential coefficients $-\frac{dQ}{dx}$, $-\frac{dQ}{dy}$, $-\frac{dQ}{dz}$, equal the forces in the directions of x, y, z, hence (2) will be changed to $\frac{d^2x}{dt^2}=\frac{dQ}{dx}$, $\frac{d^2y}{dt^2}=\frac{dQ}{dy}$, $\frac{d^2z}{dt^2}=\frac{dQ}{dz}$, (3), which are the fundamental equations used by La Place in his theory of the moon.

Again, multiply the forces which depend on m', and which act in the directions of x, y, z by dx, dy, dz severally, and put the sum of the products $=d\mathrm{R}$, then take the integral relative to x, y, z, and we have $\mathrm{R}=\frac{m'(xx'+yy'+zz')}{r'^3}-\frac{m'}{r''}$, (4), whose partial differential coefficients taken as before, will change (2) to $\frac{d^2x}{dt^2}+\frac{Mx}{r^3}+\frac{d\mathrm{R}}{dx}=0$, $\frac{d^2y}{dt^2}+\frac{My}{r^3}+\frac{d\mathrm{R}}{dy}=0$, $\frac{d^2z}{dt^2}+\frac{Mz}{r^3}+\frac{d\mathrm{R}}{dz}=0$, (5), which are the equations used by La Place in his theory of the planets. It may be observed, that if M′ and m should be attracted by another body whose mass is m'', revolving about M′ in the same direction as m, and whose distances from M′, m are denoted by r''', r'''', and rectangular coordinates x'', y'', z'', its effect would be to add the terms $\frac{m''(xx''+yy''+zz'')}{r'''^3}-\frac{m''}{r''''}$ to the terms in the value of R, and so on for any number of bodies whatever; and it may be remarked that R is called the disturbing function, and $\frac{d\mathrm{R}}{dx}$, $\frac{d\mathrm{R}}{dy}$, $\frac{d\mathrm{R}}{dz}$ the disturbing forces.

Put $\frac{yd\mathrm{R}}{dx}-\frac{xd\mathrm{R}}{dy}=\frac{dc}{dt}$, $\frac{zd\mathrm{R}}{dx}-\frac{xd\mathrm{R}}{dz}=\frac{dc'}{dt}$, $\frac{zd\mathrm{R}}{dy}-\frac{yd\mathrm{R}}{dz}=\frac{dc''}{dt}$, (6), multiply the first and second of (5) by $-y$, x severally, add the products and we get $\frac{xd^2y-yd^2x}{dt}=dc$, similarly $\frac{zd^2x-xd^2z}{dt}=-dc'$, $\frac{yd^2z-zd^2y}{dt}=dc''$ whose integrals are $\frac{xdy-ydx}{dt}=c$,

$\frac{zdx - xdz}{dt} = -c'$, $\frac{ydz - zdy}{dt} = c''$, (7); where $\frac{cdt}{2}$, $\frac{-c'dt}{2}$, $\frac{c''dt}{2}$ are evidently the areas described by r in the instant dt, when orthographically projected on the planes (x, y), (x, z), (y, z) severally.

Multiply (7) by z, y, x, add the products, we have $zc - yc' + xc'' = 0$, (8), similarly by (6), we get $zdc - ydc' + xdc'' = 0$, (9), $\therefore$ by taking the differential of (8) having regard to (9), we derive $cdz - c'dy + c''dx = 0$, (10), this added to (8) gives $c(z+dz) - c'(y+dy) + c''(x+dx) = 0$, (11); which with (8) show that by neglecting quantities of the order $dcdz$, $dc'dy$, $dc''dx$, m is moving in the same plane when its coordinates are x, y, z, that it is when they become $x+dx$, $y+dy$, $z+dz$, and that the plane passes through the origin of the coordinates. Imagine the plane in which m is moving (when its coordinates are x, y, z,) to be produced to cut the plane of x, y, then will the line of common section be the line of the nodes; put $\theta =$ to the angle which the line of the nodes makes with the axis of x reckoned in the direction of the motion, and $\varphi =$ to the inclination of the two planes. Let x', y' denote the coordinates of m when referred to the line of the nodes, as the axis of the abscissas; we have $x' = x \cos.\theta + y \sin.\theta$, $y' = y \cos.\theta - x \sin.\theta$, $z = y' \tan.\varphi$, $\therefore z = y \cos.\theta \tan.\varphi - x \sin.\theta \tan.\varphi$, (12).

By comparing the last of (12) with (8), we get $c' = c \cos.\theta \tan.\varphi$, $c'' = c \sin.\theta \tan.\varphi$, $\therefore \tan.\theta = \frac{c''}{c'}$, $\tan.\varphi = \frac{\sqrt{c'^2 + c''^2}}{c}$, (13); put $p = \frac{c''}{c}$, $q = \frac{c'}{c}$, (14), multiplying (6) by dt, and taking the integrals, we find the values of c, c', c'' at any time, and thence θ and φ will be found by (13), also p and q become known by (14), they also give $dp = \frac{dc'' - pdc}{c}$, $dq = \frac{dc' - qdc}{c}$, (15).

Taking the plane of x, y for that of the primitive orbit of m, it is evident that z and $\frac{dz}{dt}$ will be of the order of the disturbing force; $\therefore$ neglecting quantities of the order of the square of the disturbing force, we have by (6) $dc = \left(y\frac{d\text{R}}{dx} - x\frac{d\text{R}}{dy}\right)dt$, $dc' = -x\frac{d\text{R}}{dz}dt$, $dc'' = -y\frac{d\text{R}}{dz}dt$, (16), also (15) become $dp = \frac{dc''}{c}$, $dq = \frac{dc'}{c}$, or by (16), $dp = -\frac{y}{c}\frac{d\text{R}}{dz}dt$, $dq = -\frac{x}{c}\frac{d\text{R}}{dz}dt$, (17).

The squares of (7) when added, by putting $c^2+c'^2+c''^2=b^2$, give $r^2\left(\frac{dx^2+dy^2+dz^2}{dt^2}\right)-\left(\frac{rdr}{dt}\right)^2=b^2$, (18); also put $x\frac{dR}{dx}+y\frac{dR}{dy}+z\frac{dR}{dz}=rR'$, multiply (5) by x, y, z, add the products, and we get $\frac{xd^2x+yd^2y+zd^2z}{dt^2}+\frac{M}{r}+rR'=0$, (19), by $xdx+ydy+zdz=rdr$, and (18), we have $\frac{xd^2x+yd^2y+zd^2z}{dt^2}=\frac{d(rdr)}{dt^2}-\frac{dx^2+dy^2+dz^2}{dt^2}=\frac{rd^2r}{dt^2}-\frac{b^2}{r^2}$, (20); hence (19) becomes $\frac{d^2r}{dt^2}+\frac{Mr-b^2}{r^3}+R'=0$, (21), put $Mr-b^2=s$, $\frac{dR''}{ds}=MR'+\frac{d^2b^2}{dt^2}$, (22), and (21) will be changed to $\frac{d^2s}{dt^2}+\frac{Ms}{r^3}+\frac{dR''}{ds}=0$, (23).

Put $s\frac{dR}{dx}-x\frac{dR''}{ds}=\frac{df}{dt}$, $s\frac{dR}{dy}-y\frac{dR''}{ds}=\frac{df'}{dt}$, $s\frac{dR}{dz}-z\frac{dR''}{ds}=\frac{df''}{dt}$, (24), then multiply (23) by x, and the first of (5) by $-s$, add the products, and we get after multiplying by dt, $\frac{xd^2s-sd^2x}{dt}=df$, whose integral is $\frac{xds-sdx}{dt}=f$, similarly $\frac{yds-sdy}{dt}=f'$, $\frac{zds-sdz}{dt}=f''$, (25), these give $cs=yf-xf'$, $c's=zf-xf''$, $c''s=zf'-yf''$, (26), by the first two of these $s(c'f'-cf'')=zff'-yff''$, which compared with the third gives $cf''+fc''-c'f'=0$, (27); we also have by (25) $cds=fdy-f'dx$, $c'ds=fdz-f''dx$, $c''ds=f'dz-f''dy$, (28), then by taking the differentials of (26) having regard to (28), we get $sdc=ydf-xdf'$, $sdc'=zdf-xdf''$, $sdc''=zdf'-ydf''$, (29). By the first and second of (26) we have $cz-c'y+\left(\frac{c'f'-cf''}{f}\right)x=0$, which reduces by (27) to $cz-c'y+c''x=0$, and agrees with (8), as it evidently ought to do.

Now, since $r^2=x^2+y^2+z^2$, by restoring the value of s in (26), then substituting the value of z from (8) in the first, the value of y in the second, and the value of x in the third, we shall have three equations of the second order; the first in terms of x and y, the second in terms of x and z, the third in terms of y and z; which show that m is constantly moving in some conic section, whose elements are constantly changing, since c, c', c'', f, f', f'' are continually varying by the action of the disturbing force; but if the disturbing force $=0$, then c, c', &c. are const., or if the disturbing force is very small, they vary very slowly, and it is evident by (26) that the origin of r is always at the focus, and that whether the elements vary or not. In what follows, we shall suppose that the variable section is constantly an ellipse.

By substituting the value of s in the first of (26) it is easily changed to $r=\frac{b^2}{M}+y\frac{f}{cM}-x\frac{f'}{cM}$, (30), put $s=$ the tangent of the angle which r makes with the plane of x, y, and $v=$ the angle that the projection of r on the plane x, y makes with the axis of x, v being reckoned in the direction of the motion; then $x=\frac{r\cos. v}{\sqrt{1+s^2}}$, $y=\frac{r\sin. v}{\sqrt{1+s^2}}$, $z=\frac{rs}{\sqrt{1+s^2}}$, (31); put $\frac{f'}{cM}=e\cos.\varpi$, $\frac{f}{cM}=-e\sin.\varpi$, (32); by (31) and 32) we easily reduce (30) to $r=\frac{b^2}{M}\sqrt{1+s^2}\div[\sqrt{1+s^2}+e\cos.(v-\varpi)]$, (33). If we suppose that the plane x, y is taken so as to coincide with the plane of the curve in which m is moving, (when its coordinates are x, y, z,) it is evident that the curve described by m will remain the same as before, and that $\frac{b^2}{M}$ will remain the same, put $p'=\frac{b^2}{M}$, (34); then since $s=0$, (33) becomes $r=\frac{p'}{1+e\cos.(v-\varpi)}$, (35), which is the well known equation of the conic sections, the origin of r being at the focus; hence we obtain the same conclusions as by the previous method; and it may be remarked, that (33) agrees with the equation which La Place has obtained by a very different method at pa. 155, Vol. I of the Mec. Cel., when we suppose the disturbing force $=0$, or the elements to be invariable as he has done. Again, if we take the plane of the primitive orbit of m for that of x, y, by neglecting s^2, which is of the order of the square of the disturbing force, and using p', (33) becomes $r=\frac{p'}{1+e\cos.(v-\varpi)}$, (36), which is of the same form as (35); and it is evident that by neglecting quantities of the order of the square of the disturbing force, all the quantities in (36) may be considered as belonging to the curve described by m, when it is orthographically projected on the plane x, y, p' being the semi-parameter, e the ratio of the excentricity to half the greater axis, and ϖ the longitude of the perihelion of the projection of the variable section, reckoned in the direction of the motion from the axis of x.

Adding the squares of (26) and substituting the value of s, we get $b^2(\mathrm{M}r-b^2)^2=r^2(f^2+f'^2+f''^2)-(xf+yf'+zf'')^2$, (37), also since m moves momentarily in the ellipse to which b belongs, we shall have $ds=\mathrm{M}dr$, ∴ substituting this value of ds in (25), also restoring the value of s, then multiplying (25) by x, y, z and adding the products we get $b^2\frac{rdr}{dt}=xf+yf'+zf''$, which reduces (37) to $b^2(\mathrm{M}r-b^2)^2=r^2(f^2+f'^2+f''^2)-b^4\frac{r^2dr^2}{dt^2}$, (38). Put $2d\mathrm{R}=\mathrm{M}d\frac{1}{a}$, or $da=-\frac{2a^2}{\mathrm{M}}d\mathrm{R}$, (39), then multiply (5) by $2dx$, $2dy$, $2dz$, take the integral of the sum of the products, and we get $\frac{dx^2+dy^2+dz^2}{dt^2}-\mathrm{M}\left(\frac{2}{r}-\frac{1}{a}\right)=0$, (40), this with (18) gives $\frac{\mathrm{M}}{a}\times(2ar-r^2)-b^2=\left(\frac{rdr}{dt}\right)^2$, (41), this and (38) give $b^2(\mathrm{M}r-b^2)^2=r^2(f^2+f'^2+f''^2)-b^4\left(\frac{\mathrm{M}}{a}(2ar-r^2)-b^2\right)$, (42), which must be an identical equation, hence $\frac{f^2+f'^2+f''^2}{b^4}+\frac{\mathrm{M}}{a}=\frac{\mathrm{M}^2}{b^2}$, (43).

To find the greater axis and excentricity of the ellipse in which m is moving at any instant, we shall have (supposing it to continue its motion in the section,) when it arrives at the extremities of the greater axis $\frac{dr}{dt}=0$, ∴ (41) becomes at those points $r^2-2ar+\frac{ab^2}{\mathrm{M}}=0$, hence we have $a+\sqrt{a^2-\frac{ab^2}{\mathrm{M}}}$, $a-\sqrt{a^2-\frac{ab^2}{\mathrm{M}}}$ for the distances of the extremities of the greater axis from the focus, whose half sum $=a=$ half the greater axis, and whose half difference $=\sqrt{a^2-\frac{ab^2}{\mathrm{M}}}=$ the excentricity; and $\sqrt{1-\frac{b^2}{a\mathrm{M}}}=$ the ratio of the excentricity to half the greater axis, put this $=e$, and we get $\frac{b^2}{\mathrm{M}}=a(1-e^2)=p'=$ the semi-parameter of the ellipse; neglecting quantities of the order of the square of the disturbing force, (supposing the plane of x, y to be taken for that of the primitive orbit of m,) $b^2=c^2+c'^2+c''^2=c^2$, $\therefore p'=a(1-e^2)=\frac{c^2}{M}$, (44), hence $dp'=\frac{dc^2}{M}$, and $d\sqrt{a(1-e^2)}=\frac{dc}{\sqrt{M}}=$ [by (16)], $\left(y\frac{dR}{dx}-x\frac{dR}{dy}\right)\frac{dt}{\sqrt{M}}$, (45), which will enable us to find the variation of e; and it may be observed that the variation of a is found by (39). Since $\frac{b^2}{M}=a(1-e^2)$, by putting $n=\sqrt{\frac{M}{a^3}}$ or $n^2a^3=M$, (46), we get by (41) $ndt=\frac{rdr}{a\sqrt{a^2e^2-(a-r)^2}}$, (47); (46) gives $dn=\frac{3na}{M}\times\frac{d}{2}\left(\frac{M}{a}\right)$ or by (39) $dn=\frac{3na}{M}dR$, and using $\int$ for the sign of integration $n=\int\frac{3na}{M}dR$, put $dn'=ndt$ and we have $n'=\int n\,dt=\frac{3}{M}\iint na\,dt\,dR$, (48), which gives the variation of the mean motion; supposing $\int n\,dt$ to denote the mean motion of m. By putting $a-r=ae\cos. u$, or $r=a(1-e\cos. u)$, (49); (47) becomes $ndt=\frac{(1-e\cos. u)\times d\,[a(1-e\cos. u)]}{ae\sin. u}$, (50); since a and e belong to the ellipse in which m is momentarily moving, we have by (50) $ndt=(1-e\cos. u)\,du$, (51), by taking the total differential of $a(1-e\cos. u)$ in (50), having regard to (51), we get $(1-e\cos. u)\,da-a\cos. u\,de+ae\sin. u\,d'u=0$, (52), where $d'u$ denotes the differential of u which arises from the variability of the elements of elliptic motion.

Put $d(E-\varpi)=(1-e\cos. u)d'u-\sin. u\,de$, (53), adding this to (51), then taking the integral, we have $\int n\,dt+E-\varpi=u-e\sin. u$ (54); where $u=$ the excentric anomaly reckoned from the perihelion of the ellipse, E the longitude of the epoch, and ϖ the longitude of the perihelion, which are reckoned in the plane of the orbit in the direction of the motion from any straight line drawn at pleasure through the focus where M′ is situated.

Again, by taking the plane of x, y, for that of the primitive orbit of m, by neglecting quantities of the order of the square of the disturbing force, we may suppose that (54) is the orthographic projection of the curve described by m, when reduced to the plane of x, y; hence we have $\int n\,dt+\text{E}-\varpi=u-e\sin. u$, (55), and by (49) $r=a(1-e\cos. u)$, (56), also by substituting the value of p' in (36), we have $r=\frac{a(1-e^2)}{1+e\cos.(v-\varpi)}$, (57), where we shall suppose E, ϖ, and v, to be reckoned from the axis of x in the direction of the motion.

By comparing (56) and (57), we have $\tan.\frac{v-\varpi}{2}=\sqrt{\frac{1+e}{1-e}}\tan.\frac{u}{2}$, (58), since the elements are constant in the ellipse in which m is momentarily moving, we have by (58), $\frac{dv}{\cos.^2\frac{v-\varpi}{2}}=\sqrt{\frac{1+e}{1-e}}\frac{du}{\cos.^2\frac{u}{2}}$, (59); by taking tbe total differential of (58), and noticing (59), we get $-\frac{d\varpi}{\cos.^2\frac{v-\varpi}{2}}=\frac{d'u}{\cos.^2\frac{u}{2}}\times\sqrt{\frac{1+e}{1-e}}+\frac{2\tan.\frac{u}{2}de}{(1-e)\sqrt{1-e^2}}$, (60); by (58) $\frac{1}{\cos.^2\frac{v-\varpi}{2}}=1+\frac{1+e}{1-e}\tan.^2\frac{u}{2}$, hence (60) is easily reduced to $(1-e^2)\,d'u+\sin. u de+\sqrt{1-e^2}\times(1-e\cos. u)d\varpi=0$, (61). Eliminating $d'u$ from (52) and (53), we get $(1-e\cos. u)^2 da+ae\sin. ud(\text{E}-\varpi)+a(e-\cos. u)de=0$, also eliminating $d'u$ from (52) and (61), we have $ae\sin. u\sqrt{1-e^2}\times d\varpi+a(e+\cos. u)\,de-(1-e^2)\,da=0$; by adding these equations, we get $d\text{E}=(1-\sqrt{1-e^2})\,d\varpi+\frac{d[a(1-e^2)]-(1-e\cos. u)^2da}{ae\sin. u}$, (62), which will enable us to find the variation of the longitude of the epoch.

Put $\frac{r}{\sqrt{1+s^2}}=r'$, and (31) become $x=r'\cos. v$, $y=r'\sin. v$, $z=r's$, (63), hence $c=\frac{xdy-ydx}{dt}=\frac{r'^2dv}{dt}$, (64), which by taking the plane of x, y for the primitive orbit of m, becomes $\frac{r^2\,dv}{dt}=$

$\sqrt{Ma(1-e^2)}$, (65); regarding R as a function of x, y, z, and by (63) as a function of r', v, s, we have $\frac{dR}{dx}dx+\frac{dR}{dy}dy+\frac{dR}{dz}dz=\frac{dR}{dr'}dr'+\frac{dR}{dv}dv+\frac{dR}{ds}ds$, which by substituting the values of dx, dy, dz from (63), becomes $\left(x\frac{dR}{dx}+y\frac{dR}{dy}+z\frac{dR}{dz}\right)\times\frac{dr'}{r'}+\left(x\frac{dR}{dy}-y\frac{dR}{dx}\right)dv+r'\frac{dR}{dz}ds=\frac{dR}{dr'}dr'+\frac{dR}{dv}dv+\frac{dR}{ds}ds$, which must be an identical equation, $\therefore x\frac{dR}{dx}+y\frac{dR}{dy}+z\frac{dR}{dz}=r'\frac{dR}{dr'}$, $x\frac{dR}{dy}-y\frac{dR}{dx}=\frac{dR}{dv}$, $r'\frac{dR}{dz}=\frac{dR}{ds}$, (66).

Since $p=\frac{c''}{c}$, $q=\frac{c'}{c}$, we have by (8), dividing by r', and substituting from (63 , $s=q\sin. v-p\cos. v$, (67), $\therefore$ regarding R as a function of s, and by (67) as a function of p and q, we have $\frac{dR}{ds}ds=\frac{dR}{ds}(\sin. vdq-\cos. vdp)=\frac{dR}{dp}dp+\frac{dR}{dq}dq$, which must be an identical equation, $\therefore\frac{dR}{ds}\sin. v=\frac{dR}{dq}$, $\frac{dR}{ds}\cos. v=-\frac{dR}{dp}$, or by (66), $y\frac{dR}{dz}=\frac{dR}{dq}$, $x\frac{dR}{dz}=-\frac{dR}{dp}$, (68); hence and by (66), the first of (16), and (17), become $dc=-\frac{dR}{dv}dt$, $dp=-\frac{dR}{dq}\cdot\frac{dt}{c}$, $dq=\frac{dR}{dp}\cdot\frac{dt}{c}$, (69); by neglecting quantities of the order of the square of the disturbing force, we may evidently write r for r' in all the above equations.

Assuming M for the unit of masses, (46) becomes $n^2a^3=1$, (70), also $c=\sqrt{a(1-e^2)}$, (71); substituting from these equations in (69), they will be changed to $d\sqrt{a(1-e^2)}=-\frac{dR}{dv}dt$, $dp=-\frac{andt}{\sqrt{1-e^2}}\times\frac{dR}{dq}$, $dq=\frac{andt}{\sqrt{1-e^2}}\times\frac{dR}{dp}$, (72), by the first of these, we have $ede=\sqrt{\frac{1-e^2}{a}}\frac{dR}{dv}dt-a(1-e^2)\times d\frac{1}{2a}$, which by (39) and (70) becomes $ede = andt\sqrt{1-e^2}\times\frac{dR}{dv} - a(1-e^2)dR$, (73), which will enable us to find the variation of the excentricity.

Again, since $r=a(1-e\cos. u)$, we have $(1-e\cos. u)^2 = \frac{r^2}{a^2}$, $ae\sin. u=\frac{dr}{du}$, $d[a(1-e^2)]=2cdc=-2r^2\frac{dR}{dv}dv$ by (69) and (65), since $M=1$, also by (39) $da=-2a^2dR$, and since we neglect quantities of the order of the square of the disturbing force $dR=\frac{dR}{dv}dv + \frac{dR}{dr}dr$; by substituting these values in (62), it becomes $dE = (1-\sqrt{1-e^2})d\varpi + 2r^2du\frac{dR}{dr}$, but $du=ndt\div(1-e\cos. u)=\frac{andt}{r}$, $\therefore$ we have $dE=(1-\sqrt{1-e^2})d\varpi+2andt\,r\frac{dR}{dr}$, (74); since $r=a(1-e\cos. u)$, by considering R as a function of r and then of a, we have $\frac{dR}{dr}dr=\frac{dR}{dr}\times\frac{r}{a}da=\frac{dR}{da}da$, $\therefore r\frac{dR}{dr}=a\frac{dR}{da}$, (75), hence (74) becomes $dE=(1-\sqrt{1-e^2})d\varpi+2a^2ndt\frac{dR}{da}$, (76).

It is evident by (54) and (58), that $v=\int ndt+E+\varphi(\int ndt+E-\varpi)$, (77), φ denoting a function of the quantity that follows it; regarding R as a function of v and then by (77), as a function of $\int ndt$, we have $\frac{dR}{dv}dv=\frac{dR}{ndt}ndt$, (78,) but by (77) $dv=ndt[1+\varphi'(\int ndt+E-\varpi)]$, $\varphi'(Sndt+E-\varpi)$ denoting the differential coefficient of $\varphi(\int ndt+E-\varpi)$ taken relative to $\int ndt$; but by (77) $\varphi'(\int ndt+E-\varpi)=-\frac{dv}{d\varpi}$, hence $dv=ndt\left(1-\frac{dv}{d\varpi}\right)$, $\therefore$ (78) becomes $\frac{dR}{dv}-\frac{dR}{dv}\times\frac{dv}{d\varpi}=\frac{dR}{ndt}$, or since $\frac{dR}{dv}\frac{dv}{d\varpi}=\frac{dR}{d\varpi}$, we have $\frac{dR}{dv}=\frac{dR}{ndt}+\frac{dR}{d\varpi}$, (79); and because that $\int ndt$ is always accompanied by E in (77), it is evident that $\frac{dR}{ndt}=\frac{dR}{dE}$, (80), hence (79) becomes $\frac{dR}{dv}=\frac{dR}{dE}+\frac{dR}{d\varpi}$, (81). Substituting the value of $\frac{dR}{dv}$ from (79) in (73), since $dR=\frac{dR}{ndt}\times ndt$, (82), it will be changed to $ede=andt\sqrt{1-e^2}\left(\frac{dR}{ndt}+\right.$

$\left.\frac{d\mathrm{R}}{d\varpi}\right) - andt(1-e^2)\frac{d\mathrm{R}}{ndt}$, or $de = \frac{a\sqrt{1-e^2}}{e}\left(1-\sqrt{1-e^2}\right)d\mathrm{R} + \frac{a\sqrt{1-e^2}}{e} \times \frac{d\mathrm{R}}{d\varpi}ndt$, (83).

Again, since R is a function of $\int ndt$, a, E, ϖ, e, p, q, we shall have $d\mathrm{R} = \frac{d\mathrm{R}}{ndt}ndt = \frac{d\mathrm{R}}{ndt}ndt + \frac{d\mathrm{R}}{da}da + \frac{d\mathrm{R}}{d\mathrm{E}}d\mathrm{E} + \frac{d\mathrm{R}}{d\varpi}d\varpi + \frac{d\mathrm{R}}{de}de + \frac{d\mathrm{R}}{dp}dp + \frac{d\mathrm{R}}{dq}dq$, (84); by (72) $\frac{d\mathrm{R}}{dp}dp + \frac{d\mathrm{R}}{dq}dq = 0$, and by (80) $\frac{d\mathrm{R}}{d\mathrm{E}} = \frac{d\mathrm{R}}{ndt}$, also (39) gives $da = -2a^2\frac{d\mathrm{R}}{ndt}ndt$, substituting these values and that of dE from (76), we easily reduce (84) to $\left((1-\sqrt{1-e^2})\frac{d\mathrm{R}}{ndt} + \frac{d\mathrm{R}}{d\varpi}\right) \times d\varpi + \frac{d\mathrm{R}}{de}de = 0$, which gives by (83) since $d\mathrm{R} = \frac{d\mathrm{R}}{ndt}ndt$, $d\varpi = -\frac{andt\sqrt{1-e^2}}{e} \times \frac{d\mathrm{R}}{de}$, (85). Substituting the value of $d\varpi$ in (76), then collecting the results which we have obtained, we have $da = -2a^2 d\mathrm{R}$, $dn' = 3\int an\,dt\,d\mathrm{R}$, $d\mathrm{E} = -\frac{andt\sqrt{1-e^2}}{e}(1-\sqrt{1-e^2})\frac{d\mathrm{R}}{de} + 2a^2ndt\frac{d\mathrm{R}}{da}$, $de = \frac{a\sqrt{1-e^2}}{e}(1-\sqrt{1-e^2})d\mathrm{R} + \frac{a\sqrt{1-e^2}}{e}ndt\frac{d\mathrm{R}}{d\varpi}$, $d\varpi = -\frac{andt\sqrt{1-e^2}}{e} \times \frac{d\mathrm{R}}{de}$, $dp = -\frac{andt}{\sqrt{1-e^2}} \times \frac{d\mathrm{R}}{dq}$, $dq = \frac{andt}{\sqrt{1-e^2}} \times \frac{d\mathrm{R}}{dp}$, (A).

It is evident that by neglecting quantities of the order of the square of the disturbing force, we may take the integrals of (A) on the supposition that a, e, &c. in their right members are invariable, hence by using $\int$ for the sign of integration, and δ for the characteristic of variations; we have $\delta a = -2a^2\int d\mathrm{R}$, $\delta n' = 3a\iint ndt\,d\mathrm{R}$, $\delta\mathrm{E} = -\frac{a\sqrt{1-e^2}}{e}(1-\sqrt{1-e^2})\int ndt\frac{d\mathrm{R}}{de} + 2a^2\int ndt\frac{d\mathrm{R}}{da}$, $\delta\varpi = -\frac{a\sqrt{1-e^2}}{e}\int ndt\frac{d\mathrm{R}}{de}$, $\delta e = \frac{a\sqrt{1-e^2}}{e}(1-\sqrt{1-e^2})\int d\mathrm{R} + \frac{a\sqrt{1-e^2}}{e}\int ndt\frac{d\mathrm{R}}{d\varpi}$, $\delta p = -\frac{a}{\sqrt{1-e^2}}\int ndt\frac{d\mathrm{R}}{dq}$, $\delta q = \frac{a}{\sqrt{1-e^2}}\int ndt\frac{d\mathrm{R}}{dp}$, (B), for it is manifest that the integrals of (A) are variations.

By (49) $1-e\cos.u=\frac{r}{a}$, which gives $e\sin.udu=\frac{dr}{a}$, and (51) gives $du=\frac{andt}{r}$, $\therefore\sin.u=\frac{rdr}{a^2endt}$, (87), and $a^2e^2\sin.^2u=\frac{r^2dr^2}{a^2n^2dt^2}$, also $a(1-e\cos.u)=r$ gives $a^2e^2\cos.^2u=(a-r)^2$, $\therefore$ since $\sin.^2u+\cos.^2u=1$, we get $a^2e^2=(a-r)^2+\frac{r^2dr^2}{a^2n^2dt^2}$, or $\frac{a^2(1-e^2)}{r}=2a-r-\frac{rdr^2}{a^2n^2dt^2}$, whose differ'l gives $\frac{a^2(1-e^2)^{\frac{1}{2}}}{r^2}=\frac{\frac{2rd^2r+dr^2}{a^2n^2dt^2}+1}{\sqrt{1-e^2}}$; since $M=1$, we have by (65) and (70) $\frac{r^2dv}{dt}=\sqrt{a(1-e^2)}$, $n^2a^3=1$, $\therefore\frac{dv}{ndt}=\frac{a^2\sqrt{1-e^2}}{r^2}$, hence $\frac{\frac{2rd^2r+dr^2}{a^2n^2dt^2}+1}{\sqrt{1-e^2}}=\frac{dv}{ndt}$; (88).

Substituting n' for $\int ndt$ in (54), then taking the variations we have $\delta u=\frac{\delta n'+\delta E-\delta\varpi+\sin.u\delta e}{1-e\cos.u}=\frac{a}{r}(\delta n'+\delta E-\delta\varpi+\sin.u\delta e)=\frac{a}{r}\left(\delta n'+\delta E-\delta\varpi+\frac{rdr\delta e}{a^2endt}\right)$, (89); the variation of (49) or (56) gives $\delta r=(1-e\cos.u)\delta a-a\cos.u\delta e+ae\sin.u\delta u=\frac{r}{a}\delta a+\frac{dr}{ndt}(\delta n'+\delta E-\delta\varpi)+\left(a-\frac{a^2(1-e^2)}{r}\right)\times\frac{\delta e}{e}=\frac{r}{a}\delta a+\frac{dr}{ndt}(\delta n'+\delta E-\delta\varpi)+\left(a-\frac{rdv}{ndt}\sqrt{1-e^2}\right)\times\frac{\delta e}{e}$, (90), substituting the values of $\delta n'$, δE from (B) in (90), we get $\delta r=\frac{r}{a}\delta a+\frac{dr}{ndt}\left(3a\int\int n\,dt\,dR+2a\int ndt\,a\frac{dR}{da}-\sqrt{1-e^2}\,\delta\varpi\right)+\left(a-\frac{rdv}{ndt}\sqrt{1-e^2}\right)\times\frac{\delta e}{e}$, (C), or $\delta r=\frac{r}{a}\delta a-\frac{dr}{ndt}\sqrt{1-e^2}\,\delta\varpi+\frac{dr}{ndt}\left(3a\int\int n\,dt\,dR+2a\int ndt\,a\frac{dR}{da}\right)+\left(a-\frac{a^2(1-e^2)}{r}\right)\times\frac{\delta e}{e}$, (D).

By (58) $\frac{1}{\cos.^2\frac{v-\varpi}{2}}=1+\frac{1+e}{1-e}\tan.^2\frac{u}{2}=\frac{1-e\cos.u}{(1-e)\cos.^2\frac{u}{2}}$, hence by taking the variation of (58), we get $(\delta v-\delta\varpi)\frac{r}{a}=\sqrt{1-e^2}.\,\delta u+$

$\frac{\sin. u}{\sqrt{1-e^2}}\delta e$, or $\delta v = \delta\varpi + \frac{a\sqrt{1-e^2}.}{r}\delta u + \frac{dr}{ndt}\times\frac{\delta e}{ae\sqrt{1-e^2}} = \delta\varpi + \frac{a^2\sqrt{1-e^2}}{r^2}(\delta n' + \delta E - \delta\varpi) + \frac{dr}{ndt}\left(\frac{\sqrt{1-e^2}}{r} + \frac{1}{a\sqrt{1-e^2}}\right)\times\frac{\delta e}{e}$, (91), or by (B) we have $\delta v = \delta\varpi + \frac{dv}{ndt}\left(3a\int\int ndt\, dR + 2a\int ndt\, a\frac{dR}{da} - \sqrt{1-e^2}.\,\delta\varpi\right) + \frac{dr}{ndt}\left(\frac{rdv}{a^2 ndt} + \frac{1}{a\sqrt{1-e^2}}\right)\times\frac{\delta e}{e}$, (E); $\delta v = \left(1 - \frac{dv}{ndt}\sqrt{1-e^2}\right)\delta\varpi + \frac{dv}{ndt}\left(3a\int\int ndt\, dR + 2a\int ndt\, a\frac{dR}{da}\right) + \frac{dr}{ndt}\left(\frac{\sqrt{1-e^2}}{r} + \frac{1}{a\sqrt{1-e^2}}\right)\times\frac{\delta e}{e}$, (F); also by taking the variation of (67), neglecting quantities which depend on the square of the disturbing force, we shall have $\delta s = \sin. v\delta q - \cos. v\delta p$, (G); it may here be observed that the formulae which we have found for δr, δv, explain in a very simple manner what La Place proves by an elaborate and not very obvious process at pp. 291, 292, 293, Vol. I of the Mec. Cel., we would also remark that they are new to us, and that we believe they are better for calculating δr, δv, than any formulae with which we are acquainted.

Let r', v', s', denote the radius vector, longitude, and tangent of the latitude of m' above the plane x, y, at the time t, v' being reckoned from the axis of x in the plane x, y, and in the direction of the motion of the bodies m, m'; let a' denote half the greater axis of the ellipse in which m' is moving at the time t, and $\int {}_{,}ndt$ its mean motion at the same time, then we shall have as in (46), ${}_{,}n^2 a'^3 = M' + m'$; if m, m', are very small when compared with M', we shall have very nearly $M' + m = M' + m' = M'$, hence by putting $M' = 1$, we shall have by (46), and by what has now been proved, $n^2 a^3 = {}_{,}n^2 a'^3 = 1$ very nearly, (92).

In order to apply the above formulae, we must substitute the values of x, y, z, x', y', z', or $r\cos. v$, $r\sin. v$, rs, $\frac{r'\cos. v'}{\sqrt{1+s'^2}}$, $\frac{r'\sin. v'}{\sqrt{1+s'^2}}$, $\frac{r's'}{\sqrt{1+s'^2}}$ in (4), and putting for brevity $\frac{r'}{\sqrt{1+s'^2}} = \rho$, we shall have

$$R = \frac{m'[r\rho\cos.(v'-v) + zz']}{(\rho^2 + z'^2)^{\frac{3}{2}}} - \frac{m'}{[r^2 - 2r\rho\cos.(v'-v) + \rho^2 + (z'-z)^2]^{\frac{1}{2}}},$$

(93), or $R = \frac{m'r[\cos.(v'-v)+ss']}{\rho^2(1+s'^2)^{\frac{3}{2}}} - \frac{m'}{[r^2-2r\rho\cos.(v'-v)+\rho^2+(\rho s'-rs)^2]^{\frac{1}{2}}}$, (94); we must now express r, v, s, ρ, v', s', in terms of $\int ndt, a, E, e$, &c., $\int {}_{,}ndt, a', E', e'$, &c., on the supposition that the disturbing force $=0$, in which case the ellipse described by m would be invariable, and the elements a, E, &c. are constant, also $\int ndt = nt$ since $n=$ const., in the same way a', E', &c. the elements of the motion of m' corresponding to those of m, are to be considered as constant, also $\int {}_{,}ndt = {}_{,}nt$, as before; for the method of expressing r, v, s, ρ, v', s', as directed, we shall refer to Vol. I of the Mec. Cel., p. 181, &c. then for R, as given in (93) or (94), we shall refer to p. 263, &c. of the same volume, where it is expressed in a function of nt, a, E, &c. ${}_{,}nt, a'$, E', &c. Then observing that the characteristic d of differentials in the above formulae refers only to the t or nt in the invariable ellipse described by m, but that the integral sign $\int$ refers to t, whether it is introduced into R by the values of r, v, s, or those of ρ, v', s', we shall readily find $\delta r, \delta v, \delta s$; and in the same way we might find the variations of r, v, s arising from the action of another body m'', revolving around M', and so on for any number of bodies whatever, then by adding all the variations of r, v, s, according to their algebraic signs we shall get the total variations of r, v, s arising from the disturbing bodies m', m'', &c. which being applied to the values of r, v, s, in the invariable ellipse at the time t, will give the correct values of r, v, s, at the same time by neglecting quantities which depend on m'^2, m'^3, &c. m''^2, m''^3, &c., and so on; it may be observed that as the invariable ellipse is taken for the plane of x, y, the complete value of δs will be the latitude of m, but if the invariable ellipse makes a very small angle with the plane x, y, we must add the complete value of δs to the value of s in the invariable ellipse at the time t, as stated above.

The application of what has been done to the solar system is easy, for in the case of a primary planet or a comet disturbed by the attractions of the other planets, we are to consider M' as denoting the sun's mass, m that of the disturbed planet or comet, and m', m'', &c. as the masses of the disturbing planets; but in the case of a secondary planet revolving around its primary, and disturbed in its motion by other secondaries revolving around the same primary, M′ will denote the mass of the primary, m that of the secondary, which is disturbed, and m', m'', &c. will denote the masses of the disturbing secondaries; but as the method of finding the integrals which are indicated in (B), and in the values of δr, δv, δs, is too long to be inserted here, we shall refer to p. 362, Vol. I of Pontecoulant's Systeme du Monde, where the value of F which he has given, denotes the value of R, that is to be used in computing the secular variations of E, ϖ, e, p, q; and for finding the periodical variations, we shall refer to pp. 346, 463, where the value of R that is to be used in finding the periodical variations of the above quantities together with those of a and n' is given; the value given at p. 463, will enable us to find the variations which involve the first powers of e, e', p, q; then for finding δr, δv, by (C) and (E), we shall refer to pp. 474, 475, and for finding δs, to p. 483, of the same volume, or Mrs. Somerville's Mechanism of the Heavens may be consulted, *where the above subjects are treated after the manner of Pontecoulant.*

We will now proceed to obtain other formulae for finding δr, δv, δs, which will be useful in many cases.

Put $\frac{xd^2x+yd^2y+zd^2z}{dt^2}+\frac{M}{r}=T$, then from $r^2=x^2+y^2+z^2$, we get $\frac{d^2(r^2)}{2dt^2}=T-\frac{M}{r}+\frac{dx^2+dy^2+dz^2}{dt^2}$, or by (40), we shall have $\frac{d^2(r^2)}{2dt^2}=T+\frac{M}{r}-\frac{M}{a}$, whose variation gives $\frac{d^2(r\delta r)}{dt^2}+\frac{Mr\delta r}{r^3}=\delta T+\frac{M\delta a}{a^2}$, by (39) $\frac{M\delta a}{a^2}=-2\int dR$, also by (19) $\delta T=-rR'$, for in the invariable ellipse $T=0$, and in the variable ellipse it $=-rR'$, but since the first power of the disturbing force is only considered, we have by (66) $rR'=r\frac{dR}{dr}$; hence we shall have $\frac{d^2(r\delta r)}{dt^2}+\frac{Mr\delta r}{r^3}+2\int dR+r\frac{dR}{dr}=0$, (H), which agrees with the equation given at p. 257, Vol. I of the Mecanique Celeste.

It is evident by (52), that if we take the differential of δr as expressed in the first and second forms of (90), relative to δa, δe, δu, only, we shall get $\frac{d\delta r}{ndt}=\frac{dr}{ndt}(3a\int dR)$, (95); substituting the value of $\frac{dv}{ndt}$ from (88) in (F), then since $a\frac{dR}{da}=r\frac{dR}{dr}$, by comparing the result with (D), having regard to (95), we shall have $\delta v = \frac{\frac{2r\,d\delta r + dr.\delta r}{a^2\,ndt} + 3a\int\int ndt\,d\text{R} + 2a\int ndt\,r\frac{d\text{R}}{dr}}{\sqrt{1-e^2}}$, (I), if M is not supposed $=1$, we must divide the two last terms of the numerator of (I) by M, since $\frac{m'}{\text{M}}$ enters as a factor into R, or if there are several disturbing bodies as m', m'', &c., then the factors $\frac{m'}{\text{M}}$, $\frac{m''}{\text{M}}$, &c., will enter into the several parts of R which depend on the disturbing bodies m', m'', &c. severally; hence (I) will become the same as the formula (Y), given by La Place at p. 258, Vol. I of the Mec. Cel., which was found by him in a very different manner from the above.

Again, substituting r^2 for r'^2 in (64), then taking the hyperbolic logarithms, we get $\log. dv = \log. dt + \log. c - 2\log. r$, whose variation (since $dt =$ const.) gives $d\delta v = \left(\frac{\delta c}{c} - \frac{2\delta r}{r}\right) dv$, and by taking the integral $\delta v = \int\left(\frac{\delta c}{c} - \frac{2\delta r}{r}\right) dv$, (K); which is the formula given by Pontecoulant at p. 474, Vol. I of his Systeme du Monde, and by Mrs. Somerville at p. 296 of her Mechanism of the Heavens, but their methods of investigation (*which are exactly the same,*) are by no means so simple as the above.

By (64) $\frac{rdv}{dt} = \frac{c}{r}$ which reduces (K) to $\delta v = \int\left(\frac{\delta c - 2\frac{dv}{dt}\cdot r\delta r}{c}\right) dv$, (96), since we have $c = \sqrt{a(1-e^2)}$, $n^2a^3 = 1$, $dv = \frac{dv}{ndt}ndt$, (96) is easily changed to $\delta v = \frac{na}{\sqrt{1-e^2}} \times \int \frac{dv}{ndt}\left(\delta c - 2\frac{dv}{dt}r\delta r\right)ndt$, (L); which appears to us to be a better form for calculating δv than that given by (K).

By taking the plane of the primitive orbit of m for that of the plane x, y, the last of (5) which is $\frac{d^2z}{dt^2} + \frac{\text{M}z}{r^3} + \frac{d\text{R}}{dz} = 0$, (M), will enable us to find z, and then by (63), since $r' = r$, we shall get $s = \frac{z}{r}$, or if we please we may put rs for z in the two first terms of (M), and we have $\frac{d^2(rs)}{dt^2}+\frac{Mrs}{r^3}+\frac{dR}{dz}$, (N), which will also enable us to find s; see Mec. Cel., Vol. III, p. 17.

We will conclude this paper by showing how to find the integral of the equation $\frac{d^2u}{dt^2}+n^2u+P=0$, (a), where $n=$ const. $dt=$const. and $P=$ a function of t and given quantities; which will be useful for finding the integrals of (H) and (M), which can easily be shown to depend on equations of the form of (a).

Multiplying (a) by cos. $nt.\ dt$ and taking the integral we get $\frac{\cos. nt. du}{dt}+nu \sin. nt+\int P \cos. nt.\ dt=a=$ const., also multiply (a) by sin. $nt.\ dt$ and take the integral we shall have $\frac{\sin. nt.\ du}{dt}-nu \cos. nt+\int P \sin. nt.\ dt=b=$ const., then multiply these equations by sin. nt, $-$ cos. nt respectively add the products, and we shall have $nu=a \sin. nt-b \cos. nt+\cos. nt\int P \sin. nt.\ dt-\sin. nt\int P \cos. nt.\ dt$, (b), which gives the form of the integral as required; see Mec. Cel., Vol. I, p. 240, where it is found in a much less simple manner.

If $P=K \cos. (mt+e)$, where K, m, &c. are invariable quantities, we shall easily obtain $\int P \cos. nt.\ dt=K\int \cos. (mt+e) \cos. nt.\ dt=\frac{Km \sin. (mt+e) \cos. nt-Kn \cos. (mt+e) \sin. nt}{m^2-n^2}$, and $\int P$ sin. $nt.\ dt=K\int \cos. (mt+e) \sin. nt.\ dt=\frac{Kn \cos. nt \cos. (mt+e)+Km \sin. (mt+e) \sin. nt}{m^2-n^2}$, hence (b) becomes $u=\frac{a}{n} \sin. nt-\frac{b}{n} \cos. nt+\frac{K \cos. (mt+e)}{m^2-n^2}$, (c); if $P=K \sin. (mt+e)$, we shall have in a similar manner $u=\frac{a}{n} \sin. nt-\frac{b}{n} \cos. nt+\frac{K \sin. (mt+e)}{m^2-n^2}$, (d).

If $P=K \cos. (nt+e)$, we shall have $\int P \cos. nt.\ dt=K\int \cos. (nt+e) \cos. nt.\ dt=\frac{K}{2}\int [\cos. e+\cos. (2nt+e)]\,dt=\frac{K \cos. e.t}{2}+\frac{K \sin. (2nt+e)}{4n}$, also $\int P \sin. nt.\ dt=K\int \cos. (nt+e) \sin. nt.\ dt=\frac{K}{2}\int [-\sin. e+\sin. (2nt+e)]\,dt=-\frac{K \sin. e.t}{2}-\frac{K \cos. (2nt+e)}{4n}$, hence we have by (b) $u=\frac{a}{n}\sin. nt-\frac{b}{n}\cos. nt-\frac{\text{K}\cos.(nt+e)}{4n^2}-\frac{\text{K}t\sin.(nt+e)}{2n}$, (e), in a similar way if $\text{P}=\text{K}\sin.(nt+e)$ we shall get $u=\frac{a}{n}\sin. nt-\frac{b}{n}\cos. nt-\frac{\text{K}\sin.(nt+e)}{4n^2}+\frac{\text{K}t\cos.(nt+e)}{2n}$, (f); since the third terms of u in (e) and (f), when developed are of the same forms as the first two, they may be comprehended in those terms by changing a and b accordingly; see Mec. Cel., Vol. 1, p. 241.

Note.—When in the course of this article, it is said that quantities of the order of the squares, &c. of the disturbing masses m', m'', &c. are rejected; the meaning is, that quantities which depend on the squares, products, &c. of the masses are rejected.

Art. V.—*On Definitions;* by Rev. D. Wilkie, of Quebec.

No. II.

Having proceeded thus far to trace analytically, to the best of my ability, the natural process of the mind, in settling the terms necessarily required for carrying on social intercourse, I shall next proceed, as well as I can, to illustrate synthetically, the use that is made of definitions, by the greatest improvers of human knowledge, in laying the foundations of the various sciences of which they treat. The fiction of the two persons meeting in ignorance of each other's language, may now be laid aside. We proceed to consider the general practice of philosophical inquirers.

The use, then, of a definition, is to give such a verbal description of the object or objects, implied in any term, as shall enable us to distinguish it from every other term, so that there can no longer exist any doubt, or ambiguity, or uncertainty, as to its meaning. The definition further enables us to ascertain and enumerate all the individual objects which this class comprehends; and, further, to trace the various and sometimes very numerous consequences, to which the property thus ascribed to the object may lead. The definition, as I observed, is effected by a verbal description, and not by the inspection of the object, as in the former case, nor by reasoning from causes to effects, or from effects to cause, as in another case already alluded to.

Now we shall find, by a careful consideration of the subject, that there is only one way in which this verbal description can be obtained; only one way in which a strictly logical definition can be formed. I say, strictly logical, because there are innumerable explanations to be found in dictionaries and other books, which are not definitions, but for the most part mere illustrations, serving by means of synonyms, and other analogous words, to produce a vague and uncertain approximation to the meaning intended. What then is the way by which alone this purpose can be accomplished? It is by stating the genus, or higher class, to which it belongs, and then the differences which distinguish it from other species, or from other objects. It must specify the genus, and the proper difference. If either one or more of these terms is not already perfectly understood, the definition is of no use.

Thus, if I would define the word Iron, I must state that it is a metal, black, of a certain specific gravity, malleable, admitting a fine polish, and becoming liquid at a certain temperature, &c. Here, metal is the genus, or higher class, and the other qualities that follow, form the characteristic differences, which distinguish this from other metals. But, if either the word Metal, which expresses the genus, or any of the qualities next mentioned, are unknown to the person we seek to inform, we must make up that deficiency, or lay aside the definition. But if I would define a metal, I must state, that it is a solid, and then mention all the properties of metals.

If I am required to define the word Square, I may call it a rectangle, having two adjacent sides equal. Here, a *rectangle* is the genus, *having two adjacent sides equal*, the characteristic difference. If either term is unknown, the definition is useless, or rather not one. I might also define it, a four-sided figure, having two sides parallel and one right angle.

A definition is also deficient, or not a proper definition, if either the genus does not comprehend the species or individual intended; or, if the enumeration of differences be incomplete, so as not to distinguish it from all the other species or individuals comprehended under the genus mentioned. It would be redundant, if any of the differences were implied in the term denoting the genus; or, if any of the differences comprehended another difference mentioned.

In all the sciences, before attempting to lay down definitions, it is necessary that the meaning of certain terms and expressions, be previously settled by agreement, according to the methods already explained for that purpose. The number of these terms, in different branches of knowledge, is widely different. And in many branches, numerous and complicated inquiries are to be made before we proceed to the use of definitions.

Of all the sciences, Arithmetic is that in which definitions are the most precise, and in which the least preparatory instruction is required.

Before proceeding to lay down the necessary definitions in this science, it is only requisite to settle by agreement the meaning of the words, one, sum, and difference. When the force of these expressions is thoroughly understood, we can proceed with advantage to the definitions of all the common numbers. Thus, two is a number equivalent to the sum of one and one; three, to the sum of two and one, and so throughout the scale. When we have, for example, defined five to be the sum of four and one, our reason or our recollection informs us, that it is also equal to the sum of three and two. And operations analogous to this are, to a boundless extent, mastered with ease, by every human being. Another set of arithmetical definitions, are the names of the different modes of reasoning upon numbers. Thus, when the same number is several times added to itself, this operation is termed multiplication; and of this kind are all the various rules or operations in this science, and in this manner they are defined, or receive their name.

It is proper to add, that the various branches of Algebra, or analytical science, is only a continuation of Arithmetic; and that the whole doctrine of Fluxions, Infinitesimals, and Functions, are merely an extension of the same great subject. They are all founded in accurate definitions, and require nothing further to be taken for granted, than the meaning of the three terms mentioned above.

Arithmetic, in this extensive sense, possesses also the singular property, that it is, in the strictest view, independent of all the other sciences, while its rules and results are applicable to every one of them.

Geometry requires the previous admission of more principles than numbers, and though independent of material objects, has yet a closer affinity with them. It requires that we be agreed as to what is meant by a point, a line, a surface, a body, and, in the opinion of most geometricians, by a straight line. The term Angle ought, perhaps, to be also added to the list. When the meaning of these terms has been clearly determined, and understood by those who are to use them, the whole remaining superstructure of geometrical reasoning is established upon accurate definitions. Upon these are supported Plane and Solid Geometry, Conic Sections, Trigonometry, and, in some measure, all the sciences illustrated by the proportionality of lines and figures.

In order to show, with great brevity, how such a superstructure can be raised out of definitions alone, let us take one very plain example from the square. The square may be defined, a parallelogram having two adjacent sides equal, and one angle a right angle. By its being a parallelogram, the opposite sides are equal, and two of the adjacent ones being equal, they must thus be all equal. Also, one of the angles being right, and the two opposite angles of a parallelogram being equal to two, the opposite one must be right; and the two remaining angles being equal, and amounting to two right angles, must be severally right angles. Its diagonals are equally the property of triangles having two sides, and the included angle in the one equal to the corresponding parts in the other.

In the sciences denominated Mixed Mathematics, a much greater number of principles must be admitted, in particular all those which are learned from experiment. In Mechanics, we must take for granted, the inertia of matter, its motion by impulse in a straight line, the equality of the motion produced to the impulse, the composition of forces, and their resolution, with some others. Upon these and a few other assumptions, and by means of accurate definitions, the various branches of the science of forces is founded.

In Astronomy, in addition to the other principles of dynamics, or forces, it is admitted that the power of gravitation diminishes as the square of the distance, and that it acts equally on every particle of matter. On these admissions, and by the aid of a powerful analysis, founded on accurate definitions, and the co-operation of innumerable observations, is founded the most splendid of all the sciences.

In Optics must be admitted, the motion of light in straight lines, the reflection of the rays from the plane surfaces of non-transparent surfaces, and refraction to or from the perpendicular, according to the change of medium.

It is remarkable of all these principles of the mixed sciences, that they admit of strict definitions. The truth which they announce, and which is ascertained by experiment, is coupled with the definition.

In those sciences which treat of Fluids, whether elastic or non-elastic, there is considerable difficulty in fixing upon proper definitions, and those that have been fixed upon, or adopted by all inquirers, refer to qualities which do not precisely or perfectly exist in these substances. The best definition given of a fluid is, that it is a substance which admits a free passage to other bodies among its parts. But no liquid is actually found corresponding to this definition, and water, the liquid contemplated in these sciences, makes so powerful a resistance to the motion of other bodies among its parts, that some of the most improved instruments we possess, as the steam-boat, are founded upon this principle. To render the demonstrations in hydrostatics perfectly satisfactory, water should also be entirely non-elastic, and incompressible, neither of which qualities it perfectly possesses. Nor is air perfectly elastic, which is requisite to complete the principles of pneumatics.

In the departments of knowledge just mentioned, matter is considered in masses. In chemical science, the affinities which govern its principles are understood to act either on the ultimate particles of matter, or on such portions of it as are altogether imperceptible to the senses. When gravity acts on a piece of iron, it acts on the whole mass; but if an acid act upon the same metal, it acts only on the portion with which it absolutely combines; and in doing so, it acts upon portions which are not discoverable even to the microscope.

Definitions in Chemistry are either of the names of simple quantities, or of compound quantities, or of instruments and operations.

It is an important observation, that the definition of a simple quantity, in order to be complete, must contain a distinct enumeration of all its properties. These are its distinguishing characteristics, if any one of which be wanting, it is not the substance we mean. A definition of gold, which should omit its solubility in *aqua regia*, would manifestly be imperfect; for if another yellow metal should be found, having the specific gravity of 19.3, and possessing the other properties of gold, except that it was not soluble in *aqua regia*, it would then appear not to be gold, but some new species of metal, differing from gold in this particular quality. The same consequence would follow from any other characteristic property of that metal being found wanting in any supposed specimen that might be produced. A definition, therefore, which should not distinguish this new and hitherto unknown species of metal from that usually styled gold, would be an imperfect and defective definition. The definition must, therefore, comprehend all the known properties of the substance in question. But to avoid excessive and inconvenient length, every property may be expressed by a single term, and the full development of these terms, afterwards annexed in detail.

On the other hand, the definition of a compound body should state merely the simple substances, and the proportions of them which enter into the combination. These form the characteristic differences that distinguish it from all other bodies, and serve this purpose altogether independently of the new properties which the compound body may possess. If we define copperas to be the sulphate of iron, or a combination of a certain portion of iron, with a definite portion or quantity of sulphuric acid, this definition is complete; for it distinguishes the substance in question from every other substance that can possibly exist. The properties of the compounded mass may be few or many, simple or most extraordinary, but the substance itself can never be any thing else, than the product of the two constituent principles of which it is the combined result. That compound bodies should possess properties so extremely different from those of their simple elements, and even from the same elements in different proportions, is a very striking demonstration, added to some others, that we are yet extremely ignorant of the constituent forms and properties of matter, and indeed that we are far behind in our knowledge of the bodies in the universe.

The definitions of instruments and operations, in this as in other branches of knowledge, are best taken from the purposes which they serve; and, if there are different instruments for the same purpose, it may be taken from the principle of their construction. These, and similar terms in other departments of knowledge, do not admit of logical definitions. They belong more properly to the class of proper names, to be considered hereafter.

In the three great branches into which Natural History is commonly divided, the want of accurate definitions is, if I mistake not, a great and serious inconvenience, and one of the greatest difficulties to be overcome in the prosecution of these studies. Where the objects of investigation are so extremely numerous, or rather innumerable, exceedingly little progress can be made without distinct classification, and classification is of little avail, without clearly defined terms. To arrange all existing plants and animals into certain classes and orders, and to distinguish these divisions and subdivisions by as clear definitions as possible, so as to avoid all ambiguity in the description of species and individuals, has, if I am not mistaken, been the object of all the systems of botany and zoology that have from time to time been proposed. It is especially the object of the system, which, for more than half a century, gained general ascendancy, that of Linnæus. The botanical definition of a plant, then, as may be seen by looking into any book on the subject, consists of a statement of the class, of the class and order, or of the class, order and species, to which the plant belongs, (that is, the genus,) and then of an enumeration of all the various peculiarities, often extremely numerous, which distinguish it from every other plant. The distinction or distribution into classes is taken as much as possible from one part of the plant, by which means the science of botany appears to have been reduced to a greater degree of precision than the corresponding branches treating of animals and minerals. Still there appears to remain a degree of vagueness in the characteristics of all the organized productions of nature, which embarrasses to a certain degree all the definitions that can be adopted, which necessarily leaves the species and individuals to encroach indefinitely upon one another, has hitherto formed a very serious impediment to those studies, and must fill us with no small admiration of the genius and perseverance of those individuals, who, notwithstanding all the difficulties in their way, have yet made great advances in this extensive circle of investigation. It is true, that all the qualities of a plant, or of an animal, or of any substance found near the surface of the earth, might be described in the fullest detail, without taking any notice of the scientific arrangements that naturalists have adopted; but it is equally true, that it would in this case remain for ever uncertain and doubtful, to what individual object the description was designed to be applied. A conspicuous instance of the confusion which must ever attend all descriptions of this kind, in which scientific arrangement is neglected, is to be found in the writings of the ancients. Their descriptions of plants, and even their allusions to them, except those of the most common species, are for the most part incapable of being distinctly traced, with any degree of satisfaction. Their accounts of animals are much more intelligible, and much more useful, as there is generally some subject by which the account may be verified, and even many of the names have been handed down, with little or no alteration.

The only remaining class of names of visible objects, to which I wish at present to draw your attention, is that of proper names. These, it is believed, were the first of all names, and they deviated into common names solely by being applied, in the infancy of human knowledge, to all the objects which possessed a striking resemblance to each other. These were never defined. They continue to be learned, as they were first applied and understood, either by actual inspection, or by some other verbal description or indication, which, in this case, is the only substitute that can be had for a definition. This is Montreal, that is Madrid, says custom, and these names remain. It might be supposed, that, by determining the latitude and longitude of a place, we obtain a definition of its name. But it is manifest, that this is not the object of such determination, for then the meaning of every name of a place, of which the latitude and longitude were either unknown, or erroneously given, would be ambiguous. This, we know, is not at all the case. It is the position of the place, that determines the meaning of the name. The names of rivers and mountains, in the same manner, are determined by their course and position, and admit of no further definition. The names of men and women, are of the same description. So also are the names of horses, elephants, dogs, and all other animals to which names are given; so also the names of stars and books. In all these cases, the individuality of the object is of itself so precise, that no other definition, or rather no other explanation is necessary, than simply to say, this is Charles, that is Bucephalus.

Analogous to proper names, are the names of the parts of bodies, especially in the case of animals and plants. The names of such objects are determined by the place which they hold in the body to which they belong, and in the relation which each holds to its own body, to the body to which it belongs, they are, strictly speaking, proper names. Thus, the words Head and Heart have their meaning determined by their position or locality in the body to which they belong; and being single in each body, they are, as far as that body is concerned, proper names. When considered as parts of numerous bodies, they cease to be proper, and become common names. But they always agree with the former in having their application defined by locality. The same observations apply, in every respect, to plants and trees, to all natural bodies as far as they consist of distinct parts, and also to machines and other works of human construction. In the latter case, indeed, it is the use of the part, perhaps, to which our attention is more particularly directed. Thus, the roof of a house, the stem of a plant, are defined by their position; the oars of a boat, by their position and by their use.

Having thus gone over the names of the principal classes of visible objects, we shall not be long detained with the consideration of those which are invisible. These objects may, in general, be comprehended under the following classes, the impressions made upon the mind by external objects, the capacities or habits of mind in judging of these, and the principles of action which we form. For examples of these, we may mention gratitude, or the feeling arising from receiving a favor; memory, or the record of past impressions; and fortitude, or the determination to bear suffering with firmness. They are moral and intellectual impressions, or virtuous resolutions, and the contrary. Now the manner of ascertaining or limiting the signification of such terms, can scarcely be any other than the natural method of doing so, described in the former part of this paper. It must be, principally and almost solely, by observing the causes by which they are produced, and the conduct, actions and behavior to which they lead. These are the only two ways which we have for obtaining any knowledge of what passes in the minds of others. They are, for that reason, almost the only means which we have for discovering the sense and meaning of the terms employed by others. Having felt a very strong feeling of aversion, (excited in my mind,) when something is imputed to me which I despise, and of which I deem myself incapable, I give to that feeling the name of indignation. And if I afterwards witness another person suffering from the same cause, I give to his feeling the same name, and I never entertain any doubt that the feeling is of the same kind, of the same general nature, although it may differ in many subordinate particulars. I argue from cause to effect. But the cause may vary indefinitely, as to its extent, and therefore so also must the effect. Again, if I observe a person speaking ill of another, and can, upon inquiry, find no good ground that he had for doing so, I here reason from the effect to the cause, and consider him as entertaining an intention to injure the other, without proper reason. I give to his conduct the name of slander. This term serves for ever after to designate this crime; but the guilt which we necessarily attach to it, varies through every possible degree, from the slightest disapprobation up to the most consummate detestation. These two examples serve to show how moral names are applied, limited, and defined. They also serve to exemplify the ever varying nature of moral qualities, which, Aristotle has long ago repeatedly mentioned, renders that branch of knowledge for ever unsusceptible of scientific demonstration. In geometry, if two lines are shown to be parallel, they must possess all the properties of parallel lines. There can be neither exception nor degree. But in morals, good actions exist or are performed, challenging every possible or conceivable degree of approbation, according to their varied merit; as, on the contrary, bad ones are performed, which must receive every possible variety of disapprobation, according to the infinitely varied degree of demerit which they possess. The names of intellectual objects, such as attention, memory, imagination, &c. stand upon the same footing, and are open to the same observations with those of moral impressions and moral determinations.

Besides the knowledge of invisible objects, there are some branches of it, which are founded upon the great instrument, viz. language, which keeps up the communication between visible and invisible objects, as far at least as that communication depends upon our acquaintance with the thoughts of other men. The principal sciences, the most noted sciences, that come under this description, are grammar, rhetoric, and criticism. These may be considered as comprehending under them, all the rules, directions, and observations that relate to the use, the improvement, and the embellishment of language, regarded as a vehicle of thought.

But as this paper has already been extended far beyond the limits within which I at first expected it to be confined, I shall reserve the observations I intended to make on these subjects, with some others on moral definitions, which I have not completed, for the ground work of some future speculations.

Art. VI.—*On the formation of Compound or Twin Crystals;* by James D. Dana.

Read before the Yale Natural History Society, March 3, 1836.

The nature of atoms or those invisible particles which have been supposed to constitute matter, has long received the attention of the philosophic world. Till within a few years, the theories on this subject have been a collection of mere speculations. Like the metaphysics of the mind, thought, aided it may be by the sensations, but unassisted by any inquiries into the nature of matter itself, has been considered fully capable of furnishing both facts and principles; as if attempts to deduce conclusions from the visible to the invisible, were as futile in material as in mental investigations. The more philosophical methods of scientific inquiry of modern times, which have resulted in the erection of the most splendid and lasting monuments of the age, the various sciences, have occasioned besides other effects, the complete overthrow of theories so weakly founded. It is no longer considered indispensable to provide atoms with hooks, to account for their various combinations; nor to suppose them either "round, oval, lenticular, flat, gibbous, oblong, conical, smooth, rough, bristly, quadrangular,"* &c., to account for the different phenomena in which they are the agents.

Yet our knowledge of the nature of the ultimate particles of bodies is still quite limited. The rapid advancement however of the various sciences, affords from time to time some slight elucidation of their secret nature, and leads us to expect that ere long we may speak of them, as we do of the natural objects apparent to our senses.

Attempts have of late been frequently made to advance one step towards a complete understanding of these bodies by a determination of their form, which accomplished would be a valuable accession to the few ideas of their nature already acquired. The present state of Science seems to render this a favorable time for a consideration of this subject, and if I mistake not, the time has already arrived, when correct conclusions may be deduced not only respecting their form, but also other qualities of these invisible constituents of our world. If the truth is attained on these points, there will necessarily flow from it a satisfactory explanation of the formation and structure of those Siametic prodigies in Mineralogy, called *Compound* or *Twin Crystals,* otherwise, although improperly, ***Hemitropes.***

Among the theories that have been proposed relative to the forms of atoms or molecules, the following are the principal. Many others have at different times been brought forward, but like the Leucippian, they appear to be too absurd, viewed with the present light of science, to deserve enumeration.

According to one author,† Molecules have the same form as those polyhedral solids that may be obtained by the cleavage of crystals.

Another,‡ seeing the inability of the Haüyan hypothesis to account for the very facts that gave rise to it, adopts the conclusion,

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* Epicurus.

† *L'Abbé Haüy.* His *integrant* molecules were however only three in number, the Tetrahedron, cube, and three sided prism.

‡ *Wollaston* in the Phil. Trans. of the Roy. Soc. of London, for 1813.

remarkable for its simplicity, that they are either *Spheres* or *Spheroids.*

A theory of later date,* but certainly not an improvement on the last, gives to all simple atoms a *Tetrahedral* form, from combinations of which solids, the molecules of compounds result.

A fourth,† and that which embraces among its advocates the greater part of the Chemists of the day, is founded on the supposition that the atoms of the elements are spheres and those of compound bodies, an aggregation of these spheres.

Without remarking on any of these theories at present, I shall select from them, what appears to be correct and reply to the rest in the course of the communication.

The simplicity of the hypothesis of Wollaston, that the molecules of matter are either spheres or spheroids, and its capability with some modifications of explaining the numerous facts in Natural Philosophy and Natural History, as also its *consistency,* at least, with the principles of Chemical Science, seem to render its correctness highly probable.

It may be objected to it, that a *permeation* of the molecules by one another, or an *intimate union* of them, is required in order to form the molecules of compounds; that is, a combination in which the molecules of the elements, (Hydrogen and Oxygen for instance, in water,) unite in a manner which may be represented by the combination of two globules of mercury. As a consequence of this supposition, what are called atoms, are in fact *not* atoms, but on the contrary either *infinitely divisible* particles, or if *finitely,* capable of exceedingly minute division, as is evinced in every instance of the action of chemical affinity. The former is the most probable supposition. If it be advanced, that we cannot conceive of matter with such a quality, equally imperfect, it may be replied, are our powers of conception with regard to the existence of the Creator of matter. The argument against the infinite divisibility of matter which has been derived from the limits of vaporization, and that also founded on observations of the eclipses of Jupiter's satellites, are not

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* By *J. G. Macvicar,* in a work entitled "*Inquiries concerning the medium of Light and the form of its molecules.*" 8vo. 132 pp., London, 1834.

† This general theory admits of a great variety in its particulars and at present is in a very indefinite state. An attempt to systematize it, has however been lately made by *M. Gaudin,* who has published on the subject in the Revue Encyclopedique, Nov., 1832, Paris.

applicable to this hypothesis, inasmuch as the elementary molecules are not supposed to be divisible except at the instant of entering into combination, and then it is merely a mutual divisibility.

Arguments in favor of this hypothesis might be drawn from *chemical* considerations. I might speak of the difficulty of comprehending how the *juxtaposition* of two atoms, absolutely indivisible particles, could produce a change in the quality of these atoms. I might allude to the difficulty of accounting for the different densities of isomeric bodies, on the hypothesis of juxtaposition. Analogical conclusions in its favor might be derived from the fact that some compounds act an elementary part. The difficulty of accounting for the peculiarities of chemical combination and solution, might be adduced, or at least the superior simplicity of the supposition, that in the former case, there is an actual and intimate combination of the particles, and in the latter merely a juxtaposition due to the general attractions of matter for matter, modified to some extent in each species of matter, by the idiosyncrasies of each; and also from this would arise the explanation of the fact that a saturated fluid may still dissolve a third body; for we may suppose that the particles of the two former may each of them, have this general attraction for a third body, although not more for each other. Reference might also be made to the greater simplicity of the idea that the molecules of alcohol for instance, are simple molecules of alcohol, instead of an aggregate of four atoms of carbon, five of hydrogen and one of oxygen. Indeed the facts are numerous which lend some probability to the truth of the hypothesis. But *Crystallography* appears to be more properly the judge of the forms of molecules. From this science seems to come that ray of light which is to guide us to this of nature's most hidden secrets. The geometrical regularity and perfect symmetry of the forms of which it treats, clearly indicate that they are dependent on certain forms in the ultimate particles of matter, and we may hope, that by an attentive study of the laws and facts of this science, conclusions may be deduced respecting their forms that will admit of but little if any doubt. If the various facts brought to light by the science of Crystallography, admit of a simple and satisfactory explanation on the above hypothesis, and if more than this, the facts absolutely require its admission, we can hardly hesitate in giving it our credence. Whether this is so or not, will be considered in the following remarks, in which the truth of the above hypothesis will be assumed and its sufficiency as the ground work of a theory tested.

To produce those regular solids which are the primary forms of crystals, the spherical and spheroidal molecules of matter must be endowed with some peculiar power of attraction competent to cause that arrangement of them necessary to the production of these solids. We cannot suppose with WOLLASTON, that spheres aggregating themselves as they are "*naturally disposed*," that is under the influence of the attraction of cohesion, would ever give rise to a cube or an octahedron.* This is contradicted in every globule of mercury or drop of water, instances which may be cited as examples of the forms resulting from the mutual action of spheres, gifted with an equal attraction in every direction. If then the regular crystalline solids cannot result from so general and equable a diffusion of attraction in the molecules, must we not suppose the cohesive power to be exerted in certain directions only? This hypothesis has been lately advanced in Germany by M. VOLTZ.† As stated correctly by this author, attraction in *one* direction, will cause an addition of particles in a straight line, in *two* directions will give rise to a plane; a third is required and is sufficient to produce a regular solid. Although this supposition is made by M. VOLTZ, the development of the proposed theory will manifest several points of discrepancy with his views.‡ If there were no other argument on this point, it would seem sufficient to prove the inadequateness of the attraction of cohesion to produce regular solids, that an axial attraction will accomplish this result.

The molecules probably assume their axes, and take on a corresponding degree of eccentricity, at the moment the mineral leaves the fluid state, for we see no evidence of their existence previous to that time; and we may suppose this to be that great change which

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* In Thomson's Annals of Philosophy, Vol. I, new Series, p. 84, there is an attempt by Mr. Emmett to prove on Mathematical principles, attraction of cohesion to be sufficient to account for the formation of crystals. His reasoning however is quite unsatisfactory.

† Transactions of the Strasburgh Natural History Society for 1833, and L'Institut, for 29th of March, and 8th of August, 1834.

‡ Some suggestions have lately been thrown out on the existence of axes of attraction in the ultimate particles of matter, by Prout in a valuable work entitled "*Chemistry, Meteorology and the function of digestion,*" forming Vol. viii of the Bridgewater Treatises. But although I had supposed on first perusing it, that his views coincided with those here adopted, the similarity is in fact very slight. According to the theory of Mr. Prout, atoms have axes of attraction. Only two of them however are cohesive; the other is chemical and in its direction *juxtaposition*, in the formation of Chemical Compounds, takes place.

attends the passage of a body from the state of a fluid to that of a solid.

The three axes indispensable to the construction of a Cube, may be represented by lines connecting the centres of the opposite faces. A sphere being inscribed in this solid, these axes will be three diameters at right angles with one another, (Fig. 1.) Spherical molecules influenced by such forces would be arranged on one another as in Fig. 2, being in contact at the extremities of these axes, where the attraction for one another is exerted.

In the Right Square Prism, because of the inequality of the vertical axis to the other two which are equal, the length of the molecule is unequal to its breadth, and consequently it is an *ellipsoid of revolution,** to which the rectangular axes of the prism are conjugate axes, (Fig. 3.) The action of such molecules on one another would

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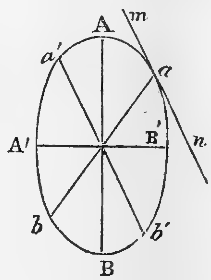
* A few remarks may possibly be required in explanation of the solids termed ellipsoids or solid ellipses, and their axes.

In the plain ellipse AA′ BB′, the lines AB, A′B′ at right angles, are termed *conjugate axes*, and *ab*, *a′b′*, *conjugate diameters*. The peculiarity of these lines is, that if a line as *mn* is drawn touching the ellipse at the extremity of one diameter, it will be parallel to the other diameter. When $ab = a'b'$, these lines are called the *equal* conjugate diameters. The axes are conjugate diameters at right angles with one another.



The revolution of a plain semi-ellipse, as AB′B, around one axis, describes the surface of a solid which is denominated an ellipsoid of revolution. Suppose the semi-ellipse AB′B to revolve on AB as an axis, all the sections of the described solid, which pass through AB will be ellipses, of the same curvature as the above plain ellipse, their curvature being determined by that of AB′B. Again as every point in the curve AB′B describes a circle in its revolution about AB, the sections parallel to A′B′ are circles, and consequently the lateral axes which lie in the section A′B′ are equal. The ellipsoid of revolution has therefore its sections in one direction *circles*. If these sections are *ellipses*, the figure is still an ellipsoid but not one of *revolution*, as the simple revolution of a plain ellipse will not describe it.

The *axes* in the ellipsoids, are three in number, and as in the plain ellipse, are lines at right angles with one another. The three *conjugate diameters* may have any position, with this restriction, that if a plane touches the ellipsoid at the extremity of one, it must be parallel to the plane in which the other two diameters are situated. The *axes* are consequently *diameters*, but only the *rectangular diameters*, axes. A moment's thought will make it evident that each face of a crystal (tangent at the extremity of one axis,) is parallel to the plane in which are situated the lines connecting the centres of the other faces; and these lines are the other axes.

produce the same arrangement as in Fig. 2, as the same kind of forces are in operation.

It may be remarked here, that when the axes of a crystal are hereafter referred to, those lines are understood which connect the centres of the opposite faces. They may be called *Crystallogenic* axes to distinguish them from the *Crystallographic* or those found convenient in the descriptions of crystals and calculations connected with them. Their nature is precisely expressed in the above adjective, derived from κρυσταλλος and γεννάω, to make.

In the Right Rectangular Prism, the axes being unequal, the molecule is an ellipsoid with unequal axes. Fig. 4, is a horizontal section through the centre of the Prism and molecule. It will be observed that the faces of the Prism in this and the preceding instances are necessarily tangents to the curved surface of the molecule at the poles of the axes. Such is also the case in all the primary forms, and hence these molecules in their combinations will give rise to the same solids that would proceed from the union of the Prisms themselves.

The molecule of the Right Rhombic Prism, is similar to that of the last mentioned solid. The lateral axes are however the *equal* conjugate *diameters*, (see Fig. 5,) instead of conjugate *axes*, and have a mutual inclination equal to the lateral interfacial angles, (inclination of the lateral faces.) The vertical axis is still at right angles with the lateral, and is therefore one of the conjugate *axes* of the ellipsoid. This is also the fact in the Right Rhomboidal Prism, which differs from the last in this only, that the lateral axes are *any conjugate diameters not equal*, (see Fig. 6,) the axes of the crystal being unequal.

In the oblique prisms all the axes of the solid are conjugate *diameters* (not axes) of the ellipsoid, their angles with each other being oblique.

The molecules of these prisms differ in the comparative lengths of the crystallogenic axes and their angles of inclination.

In the Rhombohedron the three axes (crystallogenic,) are equal and incline to one another at equal angles, and consequently are the *equal conjugate diameters* of an ellipsoid of revolution.* When the

* The construction of the Rhombohedron proposed by Wollaston, is quite different from that produced by the axes here given. Influenced by these axes the arrangement would be similar to that in the cube, Fig. 2, whilst this author adop-

angles of inclination, the axes continuing equal, are 90°, the ellipsoid becomes a sphere with the axes of the cube, and generally the vertical conjugate axis of the ellipsoid is greater or less than the lateral, as this angle is greater or less than a right angle.

Thus, then all the Primary forms of Crystals* proceed from one simple solid, an ellipsoid (a sphere being a solid of this kind with equal rectangular axes,) and all may result from a variation merely in the length and direction of the conjugate diameters of a solid of this kind. It will be noticed that all possible positions of these diameters occur in the forms of crystals, from an equality and rectangularity in the Cube, through different variations in length and situation, to a general inequality in length, and a like inequality in their mutual inclinations as in the Oblique Rhomboidal Prism.

A few remarks on the situation of secondary planes, relatively to the axes, in support of the hypothesis of the existence of these axes, will finish the general exposition of the theory after which the main

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ted the same construction as in his Octahedron, the only "natural way," as seemed to him, for union to take place.

* The Octahedron and Dodecahedron have not been included above, it being yet a subject of doubt, whether they depend on the three axes of the cube or have peculiar axes of their own. The latter seems to be the most probable conclusion, as solids then result cleavable parallel to their primary faces. The *peculiar axes* of the Octahedron will be six in number, connecting the centres of the opposite edges, the molecules touching one another in twelve points in this direction. Such is Wollaston's Octahedron. The axes of the Dodecahedron will be four in number, connecting the opposite obtuse solid angles. These appear to be the only methods of constructing these figures so that they may have their natural cleavage. It is possible however that some peculiar modification of the attraction in the axes of the cube may give rise to the same arrangement of the particles as would result from these peculiar axes. The Hexahedral Prism being a distinct primary form, it will result from molecules with four axes, one of which is at right angles with the other three. Fig. 8, is a horizontal section of the crystal and molecule.

If the existence of Crystallogenic axes is admitted, the Tetrahedron cannot be considered one of the primary forms; for an unmodified axis must cause addition of particles at each of its extremities, whereas in this solid, each face is opposite a *solid angle*. Its origin is undoubtedly connected with the cause of the dissimilar modifications at the opposite extremities of crystals of Tourmaline, at the opposite angles of Cubes of Boracite, and also with that of the inequilateral Tetrahedrons of Yellow Copper Pyrites. The electric polarity of such crystals, favors the supposition that it is connected with some peculiar disposition of the electric fluid. The Tetrahedron of Copper Pyrites, deserves equally with the Regular Tetrahedron, a place among the Primary forms.

object of the memoir, the explanation of twin crystals according to the principles of the theory, will be attempted.

Similar secondary planes are invariably situated at equal distances from similar crystallogenic axes, and when a point in a molecule has a secondary plane tangent to it, or rather parallel to a tangent to this point, all similarly situated points will also have their tangent planes. This is but another statement of the fundamental principle in Crystallography, that similar parts of a crystal are similarly modified. For instance, in Fig. 3 of the Right Square Prism, if the central point between two of the lateral poles is opposite a secondary plane, a similar secondary plane will be opposite to each one of the other three similarly situated points, or in the ordinary language of Crystallography, if one of the lateral edges is replaced by a tangent plane, the others will be similarly replaced. Again, if the point opposite the secondary plane be between the same poles, but is not central, there are two similarly situated between these two poles. Consequently both of these points will be opposite to a plane, and more than this the similar points between the three other pairs of poles will also have their tangent planes; or using Crystallographic instead of Crystallogenic language, if, one of the lateral edges of a Right Square Prism be beveled, the others will be similarly beveled.

These facts are a necessary consequence of the existence of axes of attraction. Any cause which may effect a variation in the force of attraction (the cause of secondary planes,) in the direction of one axis will naturally and necessarily produce the same result in all similar axes, and therefore all points similarly situated will be equally affected by the poles of these axes. Dissimilar parts of a crystal are not of necessity modified at the same time, because of the generally admitted principle, the same cause cannot produce the same effect in dissimilar circumstances, or as here, *on unlike axes.* The cause which might produce a variation in the attraction along the lateral axes of the Right Square Prism, and thus give rise to a replacement of the lateral edges, would not necessarily so alter the attraction in the vertical axis as to effect a modification of the basal edges.

This affords a strong argument,—if any in addition to the cleavage is necessary—that the Rhombohedron does not contain four Crystallogenic axes, (the four axes of a Hexahedral Prism,) since if the latter was the case, the secondary planes would not be symmetrically arranged relatively to these axes. According to the hypothesis of three axes, the planes on the terminal edges are tangents to the point of equilibrium between two poles at the same extremity of the ellipsoid, as (Fig. 7,) between N‴ and *N*′, (the italic letters are here used instead of the dotted posterior ones in the figures,) *N*′ and N″, and N″ and N‴, and also the same at the other extremity; those on the lateral edges, to similar points between a pole at each extremity, or between S′ and N″, S′ and N‴, *S*″ and N‴, *S*″ and *N*′, &c. The plane truncating the terminal solid angles touches the point of equilibrium between the three poles N″, *N*′, N‴, or S′, *S*″, *S*‴; those on the lateral angles, (prismatic planes,) similar points between one pole at one extremity of the ellipsoid, and two at the other as between S′, N‴, N″, &c. The same law is throughout followed, as in the construction of the secondaries of the Cube and the Prisms, which could not be the fact with any other diposition of the axes.

The hemihedral modifications, that is, those in which but half the number of secondary planes occur, which perfect obedience to law would require, are exceptions to the general principle laid down, that all similarly situated points in a molecule are opposed to similar planes. The Cubes of Pyrites, modified with the planes of the Pentagonal Dodecahedron are instances. Also those crystals of Quartz in which every other pair only of the lateral solid angles of the prisms are replaced by a plane. These planes are the result of a bevelment of only the alternate lateral edges, or those inclined in the *same* direction, for instance the edges *a* in Fig. 7, or at other times the edges *b*. The attraction in each pole at one extremity of the ellipsoid appears to have been modified only in its relation to one at the other extremity instead of two, from which it is equally distant. Some peculiar kind of influence, probably electrical, seems to have been exerted between the poles at one end and those to the right or those to the left at the other. There is an undoubted connection between the cause and that of the peculiarly interesting optical phenomenon observed by Mr. Herschel in similar crystals having oblique faces on each of the solid angles; I refer to their turning the planes of polarization to the right or left according to the inclinations of these faces to the right or left hand. A complete knowledge of the facts that electricity may hereafter develop, will probably elucidate fully this point. These irregularities do not militate with the general theory, and may be considered as evidence merely, that other powers operate on matter besides Crystallogenic attraction.

A general explanation of the formation of secondary planes arises from the nature of axial attraction, and has been adduced by Voltz. The force of attraction in the line of these axes is necessarily inversely proportioned to the length of the axes; that is representing the axes by a, b, c, the force of attraction in the direction of each, will vary as $\frac{1}{a} : \frac{1}{b} : \frac{1}{c}$ or $1 : \frac{a}{b} : \frac{a}{c}$. If then any power should cause a variation in the proportion of the strength of attraction to the length of the axes, we might expect secondary planes to occur, as the addition of particles would not take place in the same manner as if no such variation had taken place. Such then must be considered the origin of the modifications of the Primary forms. The variation in the attraction necessary for each new plane may be stated simply on mathematical principles. But I do not now propose to enter on the consideration of this part of the subject, my object at present being merely a general exposition of the theory with the view of explaining the formation of Twin Crystals.

Before proceeding, however, to the consideration of Compound Crystals, a few remarks may here be made on the theory relative to the forms of molecules, which seems to be so generally in vogue among Chemists, and more particularly on the views of M. Gaudin, who has lately attempted to reduce this *Chemical* theory, as it may be called, to some shape and limits.

This author has undertaken to point out the particular arrangement of the atoms which takes place in the construction of the molecules of crystals. He has even proceeded so far in his investigations as to have succeeded in obtaining a compound molecule from the elementary atoms of Feldspar, differing but 6′ in the inclination of its faces from the actual inclination in crystals of this mineral.

In the first place, it may be remarked with regard to the theory that the assumptions required by it are numerous and improbable. To obtain the above result, the form and relative size of the atoms ought to be known, as the primary forms of minerals have constant proportional dimensions as well as angles of inclination between their faces. To determine the former of these particulars, recourse might be had, and it would seem justly, to the crystalline forms of the elements, that is, such of them as are or may be rendered solids.

But if it is possible to determine the form of a molecule in one instance from a crystal, why not, it may be asked, in all? A deduction from the Cube of Copper that its molecule was a sphere, would lead to the same inference from the *Cube of Pyrites*, or that of *Common Salt*, which are *compound* bodies. This method, and it would seem to be the only possible one, therefore, proceeds on the incorrectness of this theory.

It may be assumed (with what propriety, it is difficult to say,) that the molecules of the elements are equal spheres. In doing so, however, the inconsistency between the supposition and the crystalline forms of Sulphur, Selenium, Tellurium, Iridium, Arsenic, Antimony, &c. is forgotten. If the assumption is correct, the unavoidable conclusion must be drawn, that there is no similarity in any instance, between the primary form of a mineral and its molecule; for that relation cannot be supposed to exist elsewhere if not in the above substances. But this resulting principle is too improbable to be earnestly brought forward. The invariable proportional dimensions and angles of the primary forms, the symmetrical arrangement of secondary planes, as also the optical properties of minerals, are opposed to such an hypothesis.

The mind may indeed possess a vague indefinite idea of the aggregation of particles by some law acting independently of their forms; but it is a peculiarity of this idea, the greater the effort to bring it distinctly before the mind and comprehend it, the more incomprehensible it becomes.

M. Gaudin states that in the construction of the molecule of Feldspar, he was guided by the rules of "*equilibrium and symmetry.*" These rules however could have been derived from no other source but the unassisted judgment, whereas in a question of this kind, they ought to have proceeded from some fixed laws, a consequence of causes analogous to, if not identical with those known to exist. The following are his three classes of crystals in his own words: "1. Ceux où les files d' atomes n' existent pas plus que dans les liquides transparens, mais où les grandes espaces vides sont regulièrement répartis (crystals of the Tesseral system); 2. Ceux où les files d' atomes ont une direction unique (cristaux à une axe); 3. Ceux où les files d' atomes affectent plusieurs directions, (cristaux à deux axe.")—

The formation of a molecule of a compound resembling the primary form, requires a separation of the atoms of the constituent parts of this compound,—that is, of its acid and base, or salts,—either at the time of crystallization or previously at the formation of the compound itself. For the union of the compound molecules as such, if the requisite form could be produced, could not give rise to regular symmetrical molecules. The compound molecule of Sulphuric acid and Lime for instance, would contain different atoms as to kind and number in similar parts. Such molecules seem to be inconsistent with the regularity of the Crystalline solids, and more particularly with the occurrence of similar secondary planes on homologous parts. But can it indeed be believed that the molecule of the above acid placed along side of the base, should produce a Right Rhomboidal Prism of the same angles and proportional dimensions as in a crystal of Sulphate of Lime? If this method is then not practicable, a rearrangement is necessary, and this seems to have been the principle on which M. Gaudin has proceeded. But it may be objected to this, that the power which causes sulphuric acid to unite with Lime resides in these two compounds as such, and would not exist were the sulphur separated from its oxygen or the elements of Lime to be disunited.

But were all other difficulties removed, it might be asked how can an atom of Sulphur united to an atom of Lead—probably dissimilar in size and form—produce a molecule having any similarity to the Cube of Galena. At least eight *equal* spheres would be required. A remark that has been already made may hence be here repeated; the relation between the primary form and the molecule cannot be supposed to exist elsewhere, if not in instances similar to the above, and consequently we are forced again to the improbable deduction that they never exist, a deduction which appears to be generally rejected by Chemists, judging from their explanations of Isomorphism, although perhaps not always as much disapproved of in accounting for other occurrences, as a general survey of facts and an extended view of the simplicity of the operations of nature, lead us to suppose would be consistent with truth.

Admitting however that the molecule could be formed of the requisite dimensions and angles, as might possibly take place provided the molecules of the elements are supposed to be sufficiently polyatomic, (a supposition that has been advanced,) how is it possible that several additional atoms can be united to such a molecule without changing in the least the ratio of its edges, or the inclinations of its faces? Instances of this fact are numerous.

Similar objections exist to the hypothesis of Mr. PROUT. It may be farther objected to it that it is hardly philosophical in accounting for the formation of a solid by the existence of three axes in atoms, to consider one of these axes "*chemical*," while the two others are "*cohesive*." Where there is a sameness of effect we must suppose the existence of the same cause. The perfect resemblance in all the physical properties of the face of a cube, ought therefore to be ascribed to the operation of the same kind of attraction. Again Mr. PROUT states, and correctly, (p. 41,) that particles endowed with a single axis of attraction would arrange themselves in a straight line, and if with two axes, in a plane. But consequently his compound molecule of water instead of being spheroidal, as stated on page 148, can only be either a straight line or a plane, (that is, leaving out of consideration the thickness of the molecule.) The molecules of Oxygen and Hydrogen are supposed by Mr. PROUT to be triatomic, and hence water will contain nine sub-molecules "which may be associated, in the first place the hydrogen with the oxygen *chemically*, and afterwards the three sub-molecules of water with one another *cohesively*." The latter process, as the axes of cohesion are but two in number, will therefore according to the above principle, give rise to a plane, and it is hence impossible that thus should be "constituted a spheroidal molecule." It is also an impossibility that any but rectangular molecules and rectangular crystals, should proceed from rectangular axes of attraction, (see p. 41.) The rhombic and rhomboidal prisms right and oblique, as also the Rhombohedrons require some other hypothesis to explain their construction. It is of no avail either, to suppose the existence of oblique axes in the molecules of some of the elements, for combinations of those which in every probability have not such, will still at times produce an oblique form; and for the same reason nothing is gained by the supposition that some molecules may have a spheroidal form.

But I will not delay longer on this subject although it admits of a much greater extension. I proceed with the explanation of the formation of Compound Crystals.

The existence of axes of attraction supposes also the existence of opposite poles at the extremities of these axes. To these poles may be applied the ordinary names of *North* and *South*. In the Rhombohedron, the three poles about one of the dominant solid angles, are North, and those about the other, South. Thus in Crystals of Tourmaline there is no opposition between the Crystallogenic poles and the electric induced by heat. Probably also in the oblique prisms, the poles about a dominant solid angle are of the same kind. It may be also inferred that the poles about an acute edge in the right prisms are of the same kind, as marked in Fig. 5. Farther than this, it is at present impossible to distinguish the poles of the axes in the different Primary forms.

The molecules of crystals, governed by the usual principles of attraction, *the repulsion of like poles and the attraction of unlike,* will assume the arrangement given in Fig. 9. The general action of the poles on one another, will cause the axes of the molecules to assume a parallel position, and also a uniformity in the direction of similar poles. Such is the general principle in the architecture of the Crystalline Solids. We may now notice the apparent exceptions to it, exceptions which are a consequence of the general principle, and of which it may be correctly said, "exceptio probat regulam."

Two molecules assuming simultaneously their axes of attraction, would have any situation towards one another, were there no mutual influence between them. But since, from the very nature of attraction, they must necessarily influence one another, guided by this attraction, they will always assume the position in Fig. 9, unless they are nearly or entirely inverted as in Fig. 10. In this case the strong attraction between the adjacent north and south poles, and the not unfavorable position of the other axes to the occurrence, may counterbalance the tendency of the molecules to invert themselves in order that the joining axes may be in the same straight line, and hence may bring them together as in Fig. 11, a position nearly as natural as that of Fig. 9. Consequently a compound nucleus is formed, each half of which now commences to act independently of the other, although in connection with it, and the issue is a *compound* or *twin crystal.* Such may be considered the origin of those compound crystals, whose composition has taken place parallel to a primary face. That the accident to which they owe their origin should have happened, results as has been said, from the nature of the molecules, and the non-occurrence of it, might have been adduced as a strong argument against the whole theory.

This species of composition can only be detected among those crystals which have at least one oblique interfacial angle, unless it be in some cases of hemihedral modification. When these angles are all oblique and unequal, as in the Oblique Rhomboidal Prisms, this kind of composition may produce a different twin crystal, on each of the primary faces.

Instances of compound forms of this kind, are of very frequent occurrence in Arragonite and Carbonate of Lead or White Lead ore, which sometimes present stellated forms, owing to a repetition of the composition. Fig. 13, represents a crystal of Arragonite thus compounded parallel to the face P. A vertical profile of the same is exhibited in Fig. 13 *a*, in which it is more distinctly seen that the composition takes place parallel to a lateral face. The peculiarity of this form is owing in part to the truncation of the acute lateral edges by the planes *e*, *e*. Fig. 13 *b*, shows the relative situation of the molecule and the lateral planes *e*, *e*. Other minerals in which these forms have been observed, are Periclin, White Iron Pyrites, Albite, Epidote, Gypsum, Feldspar, &c. For other figures of these forms, reference may be had to most works on Mineralogy. A treatise by C. U. Shepard,* is quite full in them, as also in the figures of crystals generally. A valuable article on compound crystals by Haidinger in Brewster's Edinburgh Journal, is accompanied with numerous figures. Naumann's treatise on Crystallography,† contains figures of upwards of one hundred and fifty different forms of Twin Crystals.

In the formation of other crystals, which are exceptions to the general principle, two molecules unite in points of equilibrium of attraction between *two* poles in some instances, and in others in the same point between *three* poles. In the first case the situation of the molecules is similar to that given in Fig. 12, where they are retained in combination by the action of two north poles of one, on the two south poles of the other. It is apparent that this is an instance of a composition parallel to an edge, as the edges in the Primary forms, lie opposite the points of equilibrium of attraction between two poles.

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* Treatise on Mineralogy, consisting of descriptions of the species and tables illustrative of their natural and Chemical affinities. By Charles U. Shepard, Lect. on Nat. Hist. in Yale College, &c. 2 vols., 12mo., with 500 wood cuts. New Haven, 1835.

† Lehrbuch der reinen und angewandten Krystallographie, von Dr. Carl Friedrich Naumann, 2 vols., 8vo., with 39 copper plate engravings, containing 900 figures of Crystals. Leipzig, 1830.

This is shown by the rectangular figure described about the molecule.

In the other case, in which the molecules are in contact in a point of equilibrium between three poles, these poles of one molecule are adjacent to three opposite poles of the other, and to their combined attraction is owing the union of the molecules. Such is the source of those twin crystals in which the face of composition is parallel to a plane on a solid angle of a prism.

These are two common species of twin crystal, and they result from the nature of Crystallogenic attraction. In the action of particles on one another just assuming their axes of attraction and commencing to obey their influences, it is an occurrence to be expected that two should combine elsewhere than at their poles, provided their mutual attractions remain balanced; and as was said of a similar occurrence, did it not occasionally take place, it might be justly concluded that some other arrangement of the attracting power beside that here supposed to exist, was engaged in the formation of crystals.

Instances of composition between two axes, or on an edge, are of common occurrence in Hornblende, Feldspar, Staurotide, Pyroxene, Iron Pyrites, &c. Fig. 14, represents a crystal of Pyroxene, in which the union has taken place parallel to an edge between M and M, the acute edge of the prism. This kind of composition is sometimes repeated on all the similar edges of a crystal as in Iron Pyrites.

Geniculated crystals of Rutile and Manganite are examples of Twin Crystals of the third kind. Fig. 15, is a sketch of a crystal of the latter mineral compounded parallel to a plane on an angle, (*a*). Tin ore affords other examples; also native gold and silver, &c. In Rhombohedral minerals, composition usually, although not always, takes place on the vertical solid angle. Fig. 16, is a twin of Calc Spar of this kind. In the nucleus the north poles of one molecule are near and opposite the south poles of the other. The lateral edges of the Rhombohedron, have in this instance been beveled, so that the present form is a scalene dodecahedron with the upper half turned around apparently 60° on the vertical axis. A repetition of this kind of composition, also, takes place at times on all the similar solid angles.

Distinguishing terms being convenient for designating these three species of composition, which are the only kinds that occur in simple twin crystals, the following are proposed: 1st. *Adaxal*, the axes of the two simple crystals being united: 2d. *Interaxal*, the union taking place at a point of equilibrium of attraction between *two* axes of each molecule: 3d. *Mediaxal*, the combination being effected at a central point as to attraction between the poles of three axes. The first is applied to instances of composition parallel to a face, the second where it is parallel to a plane on an edge, the third where parallel to a plane on an angle.

Compound crystals sometimes occur whose composite character has evidently been received subsequently to the commencement of, yet during their formation. Such are the doubly geniculated crystals of Rutile and other mineral species. To cause such forms is required merely a reversion of the polarity of the crystal, (that is, of its molecules,) by electrical influence or some other cause.

Other twins occur which have been joined after a previous state of separate existence. They are always united by their homologous parts, and consequently owe their union to the attraction of opposite poles of similar axes in the molecules of the two crystals. Crystals of Quartz are very subject to such accidental combinations. It is not improbable that distant crystals may have an influence upon one another to cause a similar direction of similar poles.

In the preceding remarks, the statements have necessarily been very general. To have entered minutely into all the particulars and explained the formation of each twin crystal that occurs would have required more time than is proper for me in this place to employ; besides, this appears to be unnecessary, as with a little thought any of these forms, although some are quite complex, may be easily understood and referred to one of the above classes. It will be observed that in the explanations given, no new principles have been added to the theory first proposed. On the contrary, these solids of so peculiar forms, have been shown to be a necessary result of the same simple law, that gives to the ordinary crystal its geometrical regularity. This may then be added as an important argument for the truth of the adopted theory, *its ample ability to explain apparently anomalous occurrences.*

That the above explanation of the formation of twin crystals is correct, will also appear from the simplicity with which, on the same theory, we may account for numerous other facts connected with crystals.

We perceive from the theory some reason for the fact that dissimilar faces of a crystal are unlike in their lustre and cleavage, and at times in hardness and color, they owing their peculiarities to the action of dissimilar axes.

We understand why crystallizations in veins are usually fibrous, the power of attraction in the sides of the vein causing the addition of particles principally in lines perpendicular to these sides. For the same reason cubes and crystals generally, occur lengthened in the direction of one or more of their axes, and are often variously distorted. The attraction in the direction of an axis may be augmented by that residing in the rock supporting the crystal, and thus give rise to long and slender forms, or the attraction may be diminished by a similar cause and produce unusually short crystals. The kind of pole (whether north or south,) that attaches itself to the rock will depend on the polarity of the rock at the commencement of the crystallization. On the same principle, we understand why the introduction of a solid into a liquid about to crystallize, may commence or accelerate the process.

From it also seems to be at least in part apparent, the cause of the curious acoustical phenomenon noticed by Savart, in connection with Crystals of Quartz,* that is, that the tone and acoustic figures obtained by striking three alternate faces of the terminating pyramid, differ from those obtained on the other three, a fact which enabled him to determine the primary planes of the crystal. In one instance, the crystal was struck in the direction of the Crystallogenic axes, in the other, at points equidistant between them. This ought not to have occurred had there existed in the crystal four crystallogenic axes—those of the Hexahedral prism, Fig. 8.

By the theory is also afforded a probable explication of the facts arranged under the head of Isomorphism and Plesiomorphism. The ability of one element or compound to replace another in a series of combinations, without changing the primary form, depends on their similar crystallogenic relations, (or possibly electric relations) in consequence of which the same arrangement of the axes takes place in the different compounds.

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* See Brewster's Ed. Journal, Vol. I, new series, p. 144, in an article entitled Researches on the Elasticity of regularly crystallized bodies. By M. Felix Savart.

There may be also adduced a satisfactory solution of the occurrences which are included under Dimorphism. M. Voltz has attempted to deduce by mathematical considerations one of the forms of a Dimorphous body from the other. His process is as follows:

Let a, b, c represent the force of attraction in the axes of a Rectangular prism, adding these quantities as follows:

$$\begin{array}{ccc} a, & b, & c \\ a, & c, & b \\ \hline \end{array}$$

we have $2a$, $b+c$, $b+c$, three sums, of which *two are equal.* They therefore represent the attraction in the axes of a Right Square Prism, the other form of the dimorphous body of which the above Rectangular prism was one. By adding these quantities again in the following manner:

$$\begin{array}{ccc} a, & b, & c \\ c, & a, & b \\ b, & c, & a \end{array}$$

we obtain three equal sums $a+b+c$ which therefore are the axes of a cube, another form of the same dimorphous body. Thus the form of Rutile has been determined by him from that of Anatase, and the proportional dimensions of the crystals of Carbonate of Lime, from those of Arragonite, in which he has approximated very nearly to the truth. The process however appears to be quite too mechanical, and probably if the deductions should prove correct, it will be found that they depend on a different cause from that which appears in the author's writings on this subject. He says in accounting for it, "*the different polar forces combine with each other as in a Chemical Compound.*"

He even proceeds so far as to suppose that a cube results in all instances from a combination of three inequiaxal solids. This seems to be so unnecessary as well as improbable a supposition, if *permeation of particles* is admitted, that I somewhat distrust whether my own opinions on this subject, and the discovery of the similarity in many parts of his theory to my own previously formed views, may not have led me to suppose incorrectly that his theory was founded on this hypothesis. Still it is difficult to conceive, how the remaining parts of the exposition of it are consistent with any other hypothesis.

If it is supposed that the axes are fixed lines, of a determinate and unalterable length, it might be necessary for the sake of the existence of the different forms, that this supposition respecting the cube should be made. But if, as they appeared to be, they are mere directions in which attraction is exerted, subject to alteration by the peculiar qualities of any substance with which combination takes place, the hypothesis must be considered wholly incorrect. The union of two molecules of sulphur in one, may be the source of one of the forms of this dimorphous substance. But if so, the peculiarity of form is probably owing to the mutual influence of the different axes. In general, however, Dimorphism appears to be due to the different circumstances under which a substance crystallizes. It is not an improbable conclusion that the nature of the solvent, the degree of heat during crystallization, combined with some other causes, may effect a change in the direction of the axes, although generally, their only effect is a production of secondary planes. The probability of this supposition will appear from the following account (others similar are numerous,) by Mitscherlich, of a change produced in Sulphate of Zinc by the influence of heat. (Brewster's Ed. Journal of Science, Vol. iv, p. 301.) "When a *Right Rhombic Prism* of this salt is heated above a temperature of 126°, F., we may observe certain points at its surface become opaque, and then bunches of crystals shoot out from these points in the interior of the original specimen. In a short time, the whole is converted into an aggregate of those crystals, diverging from several centres, that are situated on the surface of the original crystal. No water escapes during this process except what may have been accidentally included in the lamellæ of the specimens, a circumstance which proves the identity of the chemical composition of the two species, and the difference merely to depend on the arrangement of their particles." The small crystals formed were *Oblique Rhombic Prisms*, which form is also produced when the solution crystallizes at a temperature above 126°, F.

The prismatic form of Arragonite may possibly then be owing to the presence of but a small quantity of Carbonate of Strontian, which is sufficient to change the direction of the axes, or even to the presence of this mineral in the solvent, or perhaps some other cause with which we are not acquainted.

The expansion of water previous to freezing admits of an easy explanation. In the fluid state the molecules would assume that compact position which the attraction of gravitation and a general mutual attraction would give them. But at 40°, F., commencing to act in obedience to the axes of attraction, which they are just assuming at the same time with their ellipticity, they would combine by their opposite poles merely, (see Figs. 9 and 2,) and consequently a greater space would be required to contain them.

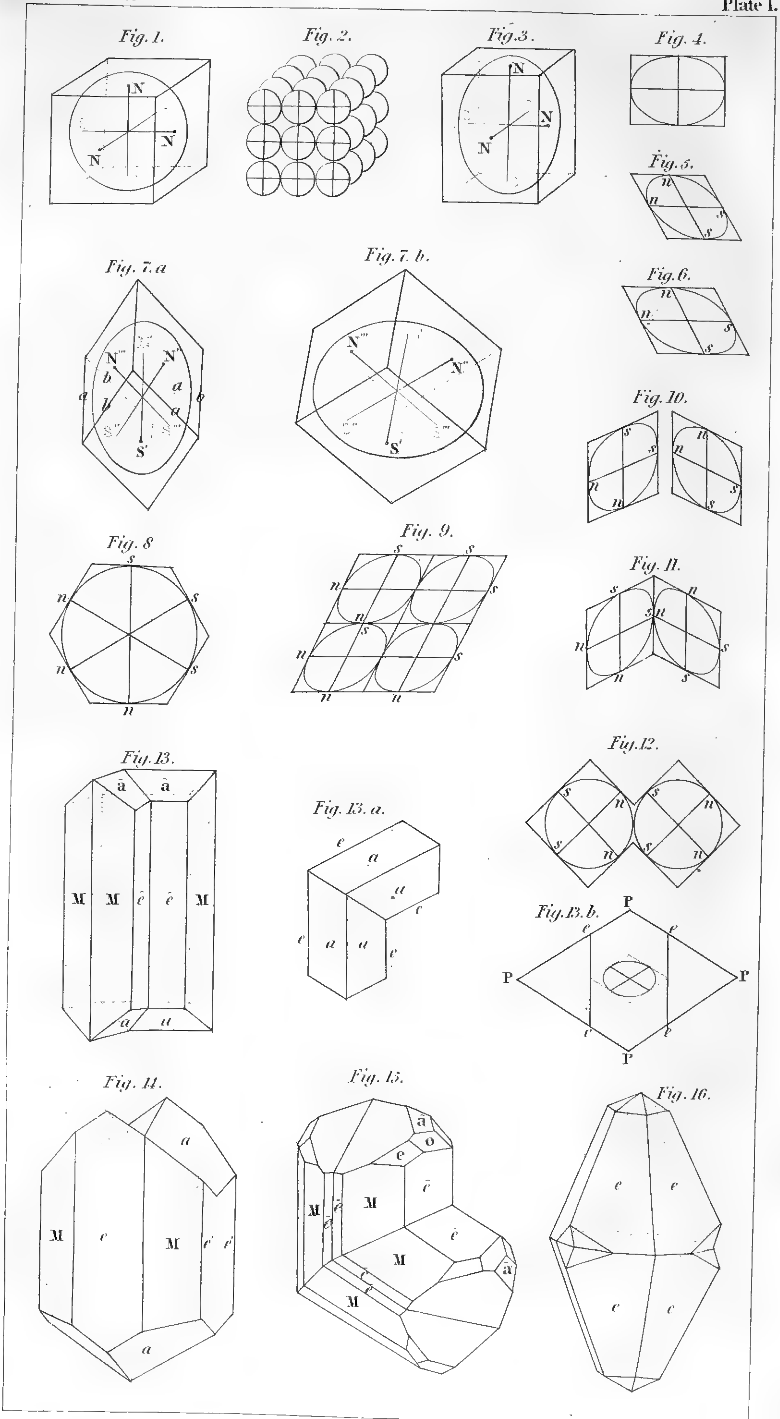
The facts in Optics connected with the Crystalline Solids are perfectly consistent with the theory adopted. When the molecules of a crystal are spheres, a solid all of whose sections are circles, there is no axis of double refraction; when the same are ellipsoids of revolution, a solid with its vertical section an ellipse but its horizontal a circle, there is one axis of double refraction. Such is the case in Right Square Prisms and the Rhombohedrons. When they are ellipsoids not of revolution, in which both the two sections, the vertical and horizontal, are ellipses, there are two axes of double refraction. This is the case in the right and oblique, rectangular, rhombic and rhomboidal prisms.

The application of this theory to Natural Philosophy might be continued to considerable length. This subject, however, and also the bearing of the principles on facts in Chemistry, I defer for the present. They may form the subject of a future communication.

Continuation on the Formation of Compound Crystals; by JAMES D. DANA.

Read before the Yale Nat. Hist. Society, April 6th, 1836.

In the communication on the subject of twin crystals, read before this Society at its last session, the structure of the stellated groups of crystals and doubly compounded forms generally, were stated to be owing to a repetition of the composition on one or more of the similar parts of a molecule. This is true; still the situation of the molecules in these compound nuclei, is in some instances not immediately apparent. I therefore propose to explain a few of them, particularly those which occur as Compounds of Right Rhombic Prisms, they being among the most complex in their internal structure, although not perhaps as complicated as some others in their exterior. I would also make some additional remarks on the formation of geniculated crystals, which in the former communication



FORMATION OF TWIN CRYSTALS.

Fig. 1.

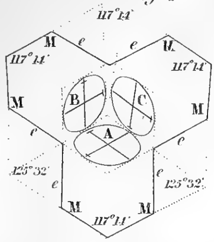


Fig. 2.

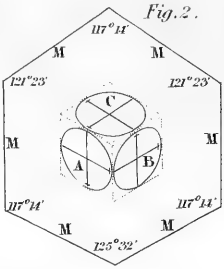


Fig. 3.

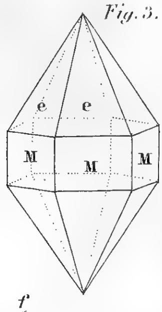


Fig. 4.

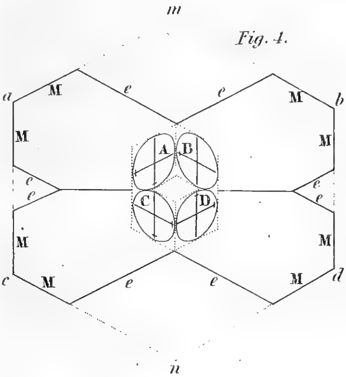


Fig. 7.

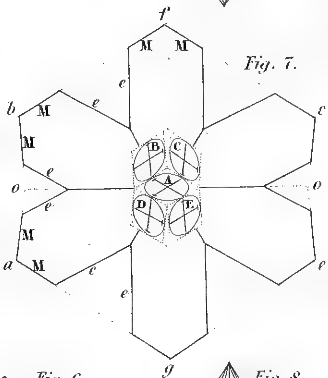


Fig. 5.

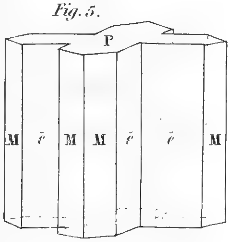


Fig. 6

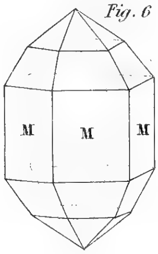


Fig. 8.

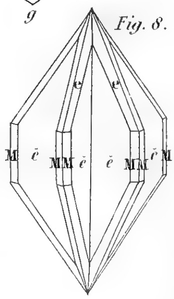


Fig. 9

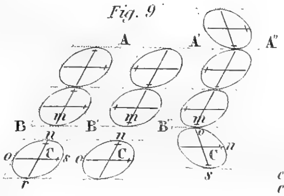


Fig. 11.

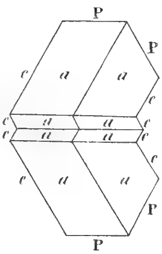


Fig. 12.

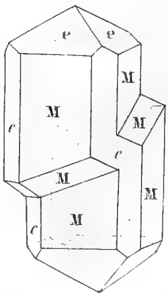
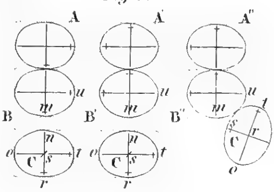


Fig. 10.



G.E. Sherman Sc.

FORMATION OF TWIN CRYSTALS.

was referred to a general principle without any accompanying elucidation.

In Fig. 1, *a m b o c n* represents a vertical section of a Hexahedral Prism of Carbonate of Lead or White Lead ore, having its angles as follows; *a, b, c* and *o* equal to 117° 14′ the angle of the primary rhombic prism; *m* and *n* equal to 125° 32′ or twice the acute angle of the prism. This form may proceed from the compound nucleus represented in the same figure, and is a consequence of the occurrence of adaxal composition on two faces of a molecule, as is shown in the figure, A being united to B and C by its faces about an obtuse angle. This is an occurrence analogous to that which gives rise to the simple twin crystals shown in Fig. 13, Pl. I. A simple calculation founded on the angles of the circumscribing rhombs and their situation will prove the angles above given, to be those which would result from such an arrangement of the molecules. The same arrangement is usually given in explanations of the formation of this prism, and is in fact the only one which is possible. There can then be no doubt as to its correctness, and only a doubt, if any is possible, as to the existence of the power which is here supposed to unite the molecules.

When the component prisms of this Hexahedral Prism have their acute lateral edges replaced by a tangent plane, (a very common modification of the Rhombic Prism,) a stellated figure with three rays is formed. The relative situation of these rays to the molecule, is shown in Fig. 13 *b*, Pl. I. It will be seen there, that the direction of the ray is in the line of the shorter diagonal of the rhomb, and the sides of the ray are parallel to this line. The same is manifest in Fig. 1. This stellated form is not of unusual occurrence among the crystals of White Lead ore.

A Hexahedral Prism of different angles is represented in Fig. 3, and with its pyramidal terminations resembles much a secondary to a Rhombohedron, from which however it is readily distinguished by the inequality of its lateral angles. This form occurs in the same mineral, White Lead ore. It has three alternate angles, (see its horizontal projection in Fig. 2,) *a, b, c,* each equal to 117° 14′ the primary angle, two *m* and *n* equal to 121° 23′, and another *o* of 125° 32′. In this instance adaxal composition has first taken place between A and B, after which the molecule B was drawn to its situation by the operation of the opposite poles, which in the figure are represented nearly in contact. In the preceding prism, the union of B and C with A was effected at the same instant, and consequently they have the same situation on A; but here the addition of C was subsequent to the union of A and B, and from this has arisen its equal inclination to the other molecules. Calculation will here also show that the prism from such a nucleus would have the angles above stated.

Crystals of Arragonite often present a similar composition.

Fig. 6, is a representation of a six-sided prism of Witherite, depending on the compound nucleus in Fig. 3. The Hexagon in this figure has the same angles as the horizontal section of this prism. In the nucleus, the molecules A and B, and C and D were first united adaxally as in Fig. 11, Pl. I. These compound molecules were then brought in contact by the attraction in the adjacent poles, (of opposite polarity,) whose axes are consequently in the same straight line. Of the angles of this prism, *a, c, b* and *d* will be the obtuse primary angles of the primitive form, and *m, n,* equal to twice the acute.

When the lateral edges of the component prisms are truncated, there results the stellated form in Fig. 4, which is a horizontal section of the cruciform crystal in Fig. 5. This is a very common form both of Arragonite and White Lead ore.

A stellated crystal of six individuals radiating from a centre, is represented in Fig. 8, of which a horizontal section is given in Fig. 4, together with the prismatic form that would result from the included nucleus. It will be observed that composition has taken place at all the lateral poles of the central molecule *A*, or parallel to *all* the faces of the circumscribed rhomb, instead of *two* as in Fig 1. Thus B and C, D and E are united to A. The prism *a o b f c o e g* proceeding from this nucleus, has a reentering angle at *o*, corresponding to the same reentering angle in the nucleus.

The truncation of the lateral edges of the four crystals B, C, D, E, would give rise only to a cruciform crystal. But the action of the central molecule A, together with that of the poles *m, n,* would cause an addition of particles parallel to A and thus give rise to the two other rays. The commencement of these rays will be by an interaxal composition with the particles B, C and D, E, the added particle having two of its poles near *m* and *n*. The rays it will be observed, have the direction before pointed out, that is, are in the line of the short diagonal of the rhomb, or the minor axis of the ellipse. This form occurs frequently in Arragonite and Carbonate of Lead.

These are the principal varieties of the doubly compound crystals belonging to Right Rhombic Prisms. The formation of all is found to be referrible to the principles already laid down. Thus we see that all those accidents, if they may be so called, which would naturally result to molecules endowed with axes of attraction, do actually occur, and precisely as they might have been predicted, had the nature of these molecules been previously known. And with a perfect acquaintance with the operations of other agents besides Crystallogenic attraction, the occurrence of *doubly geniculated* crystals, which we are now about to consider, could have been predicted with the same certainty.

In accounting for these forms, in my former communication, it was supposed that they resulted from a reversion of the original polarity in the molecules of the crystal. The causes of this reversion are probably agents that are not unknown to us. Heat will have this effect on crystals of Tourmaline, their polarity varying with the temperature. Electricity is equally an efficient agent in producing similar results.

A reference to a figure will prove the capability of this hypothesis to account for these occurrences. Let AB, (Fig. 9,) represent a line of molecules in a crystal which is now in the process of formation. The poles may be situated as there marked, (the marked poles are north.) The particle C is supposed to be on the point of obeying its axes of attraction, by uniting the point *n* with *m*. At this moment the poles of the crystal are reversed, and consequently their situation is, as in A′B′. The molecule C that was about to join itself, finds now a repellent pole opposing it, *m* and *n* being both north. A change of the position of the molecule, will therefore ensue, and the nearest south, *o*, will be brought in contact with *m*, which is so represented in A″ B″. The same would take place in every line of molecules, and in opposite parts of the crystal, and thus give rise to two geniculations. These are instances of adaxal composition. A horizontal section of a crystal of Arragonite thus compounded is represented in Fig. 11. Fig. 13, Pl. I, exhibits nearly the form of the crystal.

At other times the composition is interaxal, the situation of the molecules of the crystals in these instances may be represented by Fig. 10, A B, in which C is again the next particle that is to be added. These molecules are those of a Right Square Prism, which form is peculiarly subject to these accidents. Only one of the lateral axes is represented in the figure. The other is at right angles with the one given, (*r n* in C,) and is pointed towards the observer. In the molecule C, its north pole, as the mark indicates, is in front.

If the polarity is reversed, at the moment C is about to unite itself, *m* being thus rendered a pole of the same kind with *n*, as in A′ B′, will repel the latter, and attract the nearest north of a similar axis, which is S. A revolution of 90° must hence take place. But during this time the unlike poles *t* and *n*, (of like axes, the vertical in the prism,) are also acting on one another and tending towards an union. Consequently, combination will not take place at either pole, but at the point of equilibrium of attraction between them, that is, the point of contact in the crystal will be between *m* and *t*, and in the molecule between *p* and *n* as in Fig. 10, A″ B″.

A similar explanation might be given of the postgenital *mediaxal* compositions. But as it proceeds directly from the above, it seems unnecessary to consider the subject farther. Fig. 12, may be considered for illustration, an instance of either mediaxal or interaxal composition, it depending on which are selected as primary planes. The planes *e* being the primary, it is an instance of composition of the latter kind; but if M are the primary faces, the composition is mediaxal, or on an angle.

It is certainly quite amusing to follow the molecules of crystals guided by their instinctive faculty, polar attraction,—and it may be influenced, at times by other powers,—in the construction of those beautiful specimens of architecture, the crystalline solids; and equally interesting the fact, and we might say surprising, were it not in accordance with the usual simplicity of the operations of nature, that all their varied forms may proceed from the mutual action of solids of a single kind, by varying the situation and length of the axes of these solids, and the force of attraction in the direction of these axes. That such *may be* the fact, cannot be doubted; whether it is in reality so, the opening facts of science will soon disclose, if they are not already sufficient for a decision.

ART. VII.—*On the late efforts in France and other parts of Europe to restore the Deaf and Dumb to hearing;* by GEORGE E. DAY, late Instructor in the New York Institution for the Deaf and Dumb.

THE common opinion that in the deaf and dumb, the vocal organs and the nerves of hearing are simultaneously affected, is by no means of recent origin or confined exclusively to the unreflecting and ignorant. It is related by Itard that at a public exhibition of the Paris Institution, a distinguished prelate opened the mouth of one of the pupils and took hold of his tongue, with the view of discovering the cause of his dumbness. Previous to the sixteenth century, it seemed to have been the general, if not the universal opinion of both medical and philosophical observers, that dumbness was in all cases the result of organic defect in the organs of speech. The fact, however, that deaf mutes have the power of uttering vocal sounds, and that in many of the European institutions they are taught to enunciate words and to speak, demonstrates beyond the possibility of doubt the incorrectness of this conclusion. It is now universally admitted, among the well informed on the subject, that *deaf mutes are dumb, simply because they are deaf.*

Without examining fully the long and dismal train of evils which the want of hearing, whether congenital or contracted in infancy, produces, it is at present sufficient to remark, that the deaf and dumb, though not by nature inferior to their more fortunate fellow men, are yet in fact immensely below them.

Before instruction, they know nothing of the past or future: nothing of the history of other times, or the experience of other men: nothing of those great truths of man's immortality, of his spiritual being, and his relations to God. Even the existence of God is to them unknown and unsuspected. In the midst of a Christian community, they are heathens: living in a civilized nation, they are barbarians: surrounded by men of cultivated intellect, they yet remain in mental infancy.

If, now, hearing could be given to these children of misfortune, a change would instantly take place in their mental character. Once in the possession of the power of hearing, *language* would shortly be acquired, and with it that multitude of ideas of which its terms are the signs. The deaf mute would then possess the means of learning the truths which have been wrought out by the study of ages, and of becoming acquainted with those higher and more important truths revealed from God. He might mingle on equal terms with the community about him, and be urged onward in improvement by that ceaseless activity of the minds of men, which their union in society produces. He might take his place in the social and family circle, and participate in those kindly thoughts and feelings which do so much to refine the disposition and soften the heart. He might learn the way of life and truth, and be prepared for an "inheritance incorruptible, undefiled, and that fadeth not away."

With these facts in view, the attention of a number of skillful and distinguished European physicians, who were under the most favorable circumstances for making experiments, was directed, a few years since, to the possibility of restoring the deaf and dumb to hearing. Among others are found the names of *Sir Astley Cooper*, *Curtis*, *Itard*, *Deleau*, and *Guyot*,—men who to acknowledged surgical and medical skill, united a degree of enthusiasm and perseverance, which afforded the surest pledge, that the obstacles in their way, if vincible, would certainly be overcome. Now that the excitement which then existed in relation to the subject has passed away, we propose to make an impartial examination of the results of their labors, with the view of ascertaining to what extent, and in what cases, if any, deafness when so great as to prevent the acquirement of language, may, in the present state of medical science, be cured.

The first object was to ascertain, if possible, the *causes* of deafness, by *post mortem* examination. The anatomical observations which had previously been made in this department, were too few and incomplete to render any conclusions which might be founded upon them, of any great value. In view of the necessity of more accurate and extensive observations, the institution for the deaf and dumb at Copenhagen announced to the world, a few years since, their intention of requesting the bodies of their deceased pupils from their friends, for the purpose of dissection. What were the results of the examination, or how far they succeeded in obtaining their consent to this request, we are not able to state, as nothing has since been published in relation to the matter. The able physician, however, of the Paris Institution, M. *Itard*, has materially added to the knowledge which formerly existed in this obscure region of physiology. For several years he entertained the opinion, from the total absence of any perceptible defect in the organ of hearing, that deafness, when so great as to occasion dumbness, was always caused by paralysis of the labyrinthic nerve. Such, in fact, is the negative condition in which the ear and the parts connected with it, present themselves to the eye of the dissector, in the great majority of deaf mutes. Farther and more accurate observation, however, enabled him to discover, in some cases, palpable causes of this defect. He twice found the cavity of the tympanum filled with concretions of a chalky appearance, and in two other instances with fungous excrescences, in connection with the loss of the membrane of the tympanum and the little bones. A fifth subject presented a mass of gelatinous matter, which filled not only the cavity of the tympanum, but the semi-circular canals of the labyrinth. In another, who died after two years of malignant fever, the auditory nerve had little more consistence than mucus. Others have found the Eustachian tube in some cases filled, and in others completely obliterated. The partial or total imperforation of the meatus auditorius has been observed. Morbid affections of the tympanum of a nature opposed to the transmission of sound have been met with. Other organic defects have been discovered; but the requisite scientific technicality would render it improper to describe them here.

The results thus obtained, inform us only of the defects of the organ of hearing, and the manner in which they prevent it from becoming the vehicle of sound. If we search farther and inquire how these defects arise: the answer is, that in many cases they are congenital, and in many others are produced by disease or accident after birth. From inquiries instituted by several of the institutions for the deaf and dumb in Europe, and in this country by the American Asylum, it appears that of four hundred and forty five deaf mutes, respecting whom inquiry has been made, two hundred and forty four became deaf after birth, and two hundred and one were born in that condition; and that the causes to which the loss of hearing was most commonly attributed, were fevers, especially the scarlet fever, epileptic fits, convulsions, inflammation of the brain, the small pox and measles, blows on the head, violent falls, etc. Copious details on this subject will be found in the third biennial circular of the Paris Institution.*

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* *Troisième circulaire de l'Institut royal des sourds-muets de Paris, a toutes les institutions de sourds-muets de l'Europe, de l'Amérique et de l'Asie.* Paris, 1832.

On the whole, there can be little doubt, that the causes of that degree of deafness which is followed by dumbness, may be the same which weaken or destroy the sense of hearing in adults. In two respects, however, a difference exists, which for practical purposes, nearly destroys the comparison. In the first place, organic defects as material causes of deafness, are of much more unfrequent occurrence among the deaf and dumb, than among adults; and, secondly, in the case of the former, deafness is nearly always connected with paralysis, either natural or acquired, of the organ of hearing.* To restore an organ, for years unused and paralyzed to its full and perfect exercise, must be, under the most favorable circumstances, extremely difficult; but the difficulty is greatly increased in the case of the deaf and dumb, from the carelessness and frequently the resistance of the patient; from his want, in many cases, of intelligence and the fear with which prolonged operations inspire him, and from the absence of a perfect understanding between him and the physician. It thus not unfrequently happens, that notwithstanding the most rigid examination of the membrane of the tympanum by the rays of the sun, and after the most careful means have been taken to ascertain the permeability of the Eustachian tube, by blowing the nose, and by expiring strongly with the mouth and nostrils closed, the physician is obliged to act in the dark: to choose at random a mode of operation which is frequently painful, sometimes fatal, and rarely if ever successful.

It would, however, be equally incorrect and discouraging to infer that the sense of hearing has never, under any circumstances, been restored to the deaf and dumb. Itard† has given an account of all the cases in which congenital deafness had been cured, previous to the efforts of himself and his contemporaries. The number is so small, that we propose to present them to our readers, before entering on the examination of the recent and what may be termed, the more scientific efforts to accomplish the same result.

Case 1. To Amatus of Portugal we owe the first account of the cure of deafness when connected with dumbness. His observation however is by no means a full description. He only informs us that a child who was dumb till twelve years of age, at the end of that period, began to talk easily and plainly; and that her cure was ow-

* Itard.

† *Traité des maladies de l'Oreille et de l'Audition.* Tom. II. Paris, 1821.

ing to a seton applied to the back of the neck, which dried up, in the course of time certain feculent humors (certaines humidités excrémentitielles) with which the head was filled. Although he makes no mention of deafness, it is impossible to attribute her dumbness to any other cause. The supposition is confirmed by the fact that he relates it in connection with another cure of accidental deafness.*

2. The next observation was communicated to Lazarus Rivière by Desgrands Prés, a physician of Grenoble. A wandering beggar arrived by night at Pousenac, with his sick (deaf mute) child, who was suffering under a continued fever. For several days, they were charitably entertained and provided for, but at length the father, despairing of the child's life, abandoned him to his fate and secretly left the place. The patient however was cured, and on his full recovery was employed to take care of the sheep. Some years afterwards, he received a blow on the occiput, which fractured the bone in several places; but the wound, under the care of an able surgeon, was fortunately healed. In proportion as the cure advanced, the sense of hearing recovered the exercise of its functions, so that the man began to mutter a few words, and in a short time he was able to hear and speak distinctly. This power he retained to the end of his life.

3. The third case is that of a young man, deaf and dumb from birth, the son of a laborer of Chartres. At the age of twenty four years he suddenly began to speak, to the great astonishment of the whole town. It was ascertained from him that three or four months before, he had heard the sound of the bells, and was extremely surprised by this new and unknown sensation; and that subsequently, an aqueous discharge had taken place from his left ear, after which he heard perfectly with both. For three or four months he listened without speaking; this time he spent in repeating to himself the terms which he heard, and in becoming acquainted with the pronunciation of words and the ideas attached to them. At the end of that period believing himself sufficiently acquainted with language, he broke silence and began to speak, although very imperfectly.

Able theologians immediately questioned him with respect to his past condition, especially his ideas of God, the soul, and the moral quality of actions. Of these last subjects, he seemed not to have the slightest notion. Although he had been present at mass and

* *Curationum medicinalium Centuriæ septem.*

had been taught to make the sign of the cross, he had neither attached any meaning to these ceremonies, nor understood the object for which they were practiced. He had not formed a distinct idea of death, and in fact had never thought of it. He had passed a purely animal life, with not a single idea, except those derived from the senses.*

4. An instance is mentioned by M. Varroine, a French physician in the suite of Lucien Bonaparte, in which the application of the *moxa*† seems to have been successful. The patient was a young lady of Malaga, twenty years of age, who was born deaf. On carefully examining the organs affected, the tongue appeared to him a little thicker than usual. M. Varroine, regarding the deafness therefore as the result of a simultaneous paralysis of the ear and *tongue*, applied two moxas ; one on the back of the neck, and the other under the chin, as near as possible to the root of the tongue. The two moxas, each of the diameter of a crown, produced about the seventh day a lively inflammation : an extraordinary swelling appeared on the anterior part of the neck, and extended down to the breasts, accompanied with a violent fever, which continued 24 hours and ended in a copious perspiration. The scabs fell off on the twelfth or fourteenth day, and their loss was followed by a very considerable suppuration. The operator affirms that the tongue, at this period, was more free in its movements, and was *diminished in thickness*. In consequence of fumigations made in the *meatus auditorius*, the membrane which lines it was excoriated and furnished, about the twenty second day of the treatment, a thick, yellowish humor which flowed abundantly during six days ; these depuratory efforts were succeeded by a voracious appetite, and an increase of cheerfulness and intelligence.

About two months and a half after the application of the moxas, the young lady, to her great joy and astonishment, began to hear the ringing of the bells. From that period, her hearing continued to improve, and in a short time her deafness was completely dissipated. At the same time her dumbness ceased, and when the mother of the young lady communicated this happy result to M. Varroine, who had left Malaga, she articulated distinctly the words which she heard.‡

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* *Histoire de l'Académie des sciences, année* 1702.

† A lanuginous or cottony substance, which is burnt slowly in contact with the skin, for the purpose of producing cauterization.

‡ *Mémoires sur les bons effets du moxa dans les cas désespérés.*

"In reading this observation," observes M. Itard, "it is impossible not to see the error of the author in supposing that he had a case of simultaneous paralysis of the organs of hearing, and the organs of speech to prescribe for: and the degree to which he was prepossessed with the idea, since to support it, he imagined that the tongue was too thick, and that after the application of the moxa, its thickness was diminished. If, notwithstanding this mistake, the mode of treatment really succeeded; if the moxa under the chin partly contributed to the cure of the deafness, the sympathy which exists between this region and the ear easily explains the whole secret of the success. Paralysis of the tongue never causes complete dumbness: the articulation is defective, but there are some sounds which are distinctly heard. The same thing is true of the paralysis of the muscles of the larynx, which never causes a total privation of speech. The utterance is indeed feeble, and destitute of inflection, but yet it is intelligible. The vocal organs, then, in the case above cited, were not injured, and the cure of the deafness was sufficient to restore them to their functions."

5. In the year 1786, a man named Felix Merle, a botanical physician, as he styled himself, appeared at the Institution for the Deaf and Dumb at Bourdeaux, and commenced a course of treatment for deafness on all the pupils, amounting at that time to twenty six or twenty seven. It consisted in introducing, morning and evening, into each ear, a drop of a certain liquid of his own composition, which was kept there by a bit of cotton. This treatment was continued a month, but with no effect, except in two instances. The first is that of a young lad, eight or nine years of age, who in infancy had possessed the power of hearing, and had become deaf by an accident, but who yet heard a little with one ear. On the twenty third or twenty fourth day of the treatment, he experienced in both ears a very sharp pain. The pain gradually increased till the introduction of the liquid into the *meatus auditorius* became insupportable: two or three days after the first attack, a purulent discharge took place, in the middle of the night, from both ears; the child immediately began to hear more distinctly, so that the ear affected with total deafness, occupied the place of that which had retained some little sensibility, and the sense of hearing in the latter was still more improved. Though the hearing was by no means perfect, it was sufficient to enable the child to learn to speak and to make use of language; which he has ever since retained. It should be re-

marked however, that he has never heard or spoken as well as other men. The discharge from the ear, which continued only a few days, was not very copious and ceased spontaneously.

6. The second case in which this mode of treatment appears to have been successful, is that of a young lady, sixteen years of age, who was born with the organs of hearing in a sound condition, and at the end of fifteen or sixteen months, began to talk. The mother of the child was in the habit of taking her to the vineyard in which she was employed, and leaving her on the grass at a time when the weather was damp, while she herself was at work. It was not remarked at the time that the child suffered any inconvenience: but soon after, it was noticed that instead of improving in hearing and speech, she appeared to have lost both the one and the other. She had since remained deaf and dumb, and was making rapid progress from the instruction she was receiving in the Institution. About the twenty-fifth day of the treatment, she experienced in both ears a very sharp pain, which was so intolerable, especially when the liquor was introduced into the *meatus auditorius*, that she was obliged to be forcibly held. On the twenty-eighth day while in school, she felt an inclination to sneeze, which was immediately followed by a simultaneous discharge from both ears of a large quantity of very fetid purulent matter. The perfect re-establishment of hearing immediately took place, so that the young lady experienced a feeling of extreme terror, and firmly clung to what was at hand, under the apprehension, as she afterwards said, that the house was about to fall upon her. By degrees this feeling left her, but her hearing continued the same. As soon as she heard, she forgot or at least had no desire to use the common signs employed by the deaf and dumb, and rapidly learned to speak. At the end of six weeks she was able to ask for every thing that was necessary, and at the end of six months she spoke very well. Having, at that time, returned into the country, she lost somewhat of her facility in speaking. The discharge from the ears continued fifteen days or three weeks, and ceased shortly after.

Such is the account of all the well authenticated instances in which, previous to more recent efforts, the deaf and dumb have been restored to hearing. If too limited in number, to warrant any certain conclusion with respect to the extent to which congenital deafness might be cured, they yet were sufficient to show the *possibility* of so desirable an event, and to point out the means from which success might be hoped. Of the six cases of cure cited above, one was spontaneous, and the remaining five were produced by extreme irritation of certain parts of the head by means of the moxa, the seton, a certain aqueous composition introduced into the ear, or a blow on the head.

Although many of these remedies may properly be called cruel, and are not unfrequently attended with danger, the object to be gained was too important, while any probability existed that their use would be followed by success, to suffer the experiment to be neglected. Accordingly, they were all employed by M. Itard a sufficient number of times to test abundantly their efficacy; but with how much success the following account will show.

The *moxa* was applied, under his direction to nine or ten deaf mutes: and he affirms that several of the pupils of the Paris Institution, before their connexion with it, had been submitted to the same treatment; but the case mentioned by M. Varroine is the only known instance in which its application has been attended with success. The employment however of the *actual cautery,* a remedy similar to the moxa, seems to have been accompanied with more fortunate results. The patient was a child of the age of four and a half years, possessing a good constitution, and in perfect health, but completely destitute of hearing and speech. The application of a cautery on each of the mastoid processes, with an iron heated white, was shortly followed by an abundant suppuration, and an eruption of purulent matter. At this time, signs of the re-establishment of hearing began to be observed. "The child turned his head, whenever a noise was made behind him, or any one spoke on an elevated tone: and it was noticed that he took pleasure in striking on the window with his hands and making the glass sound. As his restoration to hearing became more apparent, the child began to repeat a few words: though it was necessary to pronounce them very distinctly and in a loud tone of voice. Eighteen months after the operation the child spoke, or rather pronounced words, with considerable distinctness: for his deafness not having been completely dissipated, his case would properly be classed among those of the semi deaf." This restoration to hearing, although partial was sufficient to inspire a hope that the same treatment on others would produce similar results: but, as if to destroy all confidence in it as a means of cure, in three other cases of congenital deafness in which it was employed, it was not attended with the least success.

The two extraordinary cures accomplished at the Institution at Bordeaux, early attracted the attention of M. Itard, and induced him to make the greatest efforts to become acquainted with the composition of the remedy employed. This, however, its possessor utterly refused to disclose, but consented to send him a certain quantity of it, prepared by himself. This was employed on three deaf mutes, but without any effect whatever. When informed of its want of success, he alledged in excuse that it was owing to the *alteration of the liquid,* which was of such a nature, that after two or three days, it would lose its power. M. Itard then offered to buy the secret, but was refused on the ground that the discovery was one, which only the government should know and recompense. On the death of the inventor, however, his wife communicated to M. Itard the composition of the remedy.* As several of the ingredients were such as were known to be useful in cases of deafness, and as its use had been attended with such flattering success at Bourdeaux, it was employed on all the deaf mutes in the Paris Institution, who had lost the power of hearing in infancy. But in this, as in so many other instances, his hopes were completely frustrated; since none of the effects observed at Bourdeaux took place. It was subsequently used in a number of other cases, but, with one slight exception, with the same want of success.

M. Itard, with a degree of perseverance in the midst of such continual failures, which few men would have had, and which does him great honor, both as a benevolent and scientific man, was determined to leave no means untried, from which any rational hope of success could be drawn. On a child, therefore, of three or four years of age, whose deafness was attributed by his parents to violent convulsions, caused by dentition, he resolved to employ a new experiment. Without describing it minutely, it will be sufficient to say, that it consisted principally in the application of blisters. In this instance it was happily successful; but in forty cases in which it was subse-

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* For the information of physicians, who may be supposed to take an interest in it, we subjoin the recipe.

R. Pulverized Asarabacca, - - - - - two drams.
Rose leaves, - - - - - - - - one pinch.
Horse Radish, - - - - - - - one dram.
Parsley Pert, or Stonebreak Parsley, - - - one pinch.
White wine, - - - - - - - - eight ounces.
Boil to one half, strain and add
Sea salt, - - - - - - - - two drams.

quently employed, no similar instance occurred: thus, demonstrating on the one hand, in conjunction with other experiments, the *possibility* of restoring the deaf and dumb to hearing, and on the other, the great improbability in any given instance of so desirable an event.

In addition to the remedies above mentioned, a multitude of others have long been in popular use, but with nothing to support their pretensions. Of these, the most rational are various essences, alcohol, ether, and ammonia, to which electricity and galvanism may also be added; and the least so, are preparations of earth-worms, snails, ants' eggs, hog lice boiled alive, etc.

Although the employment of stimulating means was attended with so little success, a wide field for experiment yet remained, in the removal of the material causes in the ear, which prevent the free admission or circulation of sound. To accomplish this result, the two principal operations relied upon, were the *perforation of the tympanum*, and the *injection of the Eustachian tube*. The *tympanum* is a small cavity at the extremity of the canal which leads from the *auricle*, or, as it is commonly called, the ear, into the head. Between this canal, called the *meatus auditorius externus*, and the tympanum, is a thin membrane, termed the *membrane of the tympanum:* it is commonly known as the drum of the ear. Running obliquely downwards from the tympanum, and opening into the *pharynx* or back part of the mouth, is a small passage called the *Eustachian tube*. The office of the tympanum, is to communicate sound to the region of the ear which lies behind it. For this purpose, it is made to resemble in many respects the common *drum:* the membrane of the tympanum may be regarded as the drum-head, and the Eustachian tube as the orifice through which air passes into the drum. If now this tube be obstructed, so that no air can pass through it into the tympanum; or if the tympanum itself be filled with mucus or any other material substance; or if its membrane becomes ossified, or so thick that it cannot communicate the vibrations of sound, the hearing will inevitably be destroyed. Such accidents often occur, and are a frequent source of the deafness of the deaf and dumb. The two operations relied upon to accomplish a cure in these cases, will be treated of in their order.

I. *The perforation of the tympanum.*—The advantage of this operation may be considered as the result either of the free entrance of sound, striking immediately upon the sensitive parts of the organ of hearing, and thus becoming perceptible; or of the renewal of the air in the cavity of the tympanum, which, in consequence of the obstruction of the Eustachian tube, had undergone certain physical changes, of a kind to injure the transmission of sound. The two cases, then, in which it would seem to be useful, are when the membrane of the tympanum hass acquired an unnatural thickness, and when the interior aperture of the ear has ceased to be permeable.

The utility of this operation was originally suggested by *Riolan*, and subsequently by the celebrated *Cheselden;* but M. *Eli*, a Paris surgeon, is supposed to be the first who actually performed it. Eli, however, died young, and his experiment and even his name were nearly forgotten, when *Sir Astley Cooper*, in the year 1800, again revived the practice, and performed the operation on a number of deaf persons. His success for a time appeared so promising, that his reputation was at once extended, and the perforation of the tympanum, in the mode which he pointed out,* was immediately practiced in France and Germany. But its very popularity finally proved its ruin; for it was soon discovered, from the numerous cases in which the operation was performed, without producing any favorable result, that little reliance could be placed upon it, as a means of cure.

The same operation was also performed a number of times by *Hymly*, a German physician, but with no better success. Instead of the simple *trocar* of Cooper, he employed a very sharp instrument in the form of a punch, the object of which was not only to perforate the tympanum, but to remove a portion of the membrane. His utmost efforts, however, could not prevent the aperture from closing, and becoming healed even more rapidly than that made by the trocar. On one individual, he performed the operation *four times*, without being able to preserve the opening. He hence inferred that if ever the operation is successful in restoring the deaf to hearing, the cure is always temporary.

The idea, however, of restoring the sense of hearing to the deaf and dumb by means of perforating the tympanum, was not yet abandoned. Cooper and Hymly had indeed met with little success, but it was hoped that some modification or improvement of the instruments they employed, would be productive of more fortunate results. In place, therefore, of the trocar of Cooper, a similar instru-

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* *Phil. Trans. of the Royal Soc. of London, for the year* 1801.

ment, but of larger dimensions, was introduced; that of Hymly was abandoned; and a piece of cat-gut, or the end of an India-rubber probe was inserted in the aperture made in the membrane, for the purpose of preserving it: but it was soon found that notwithstanding all the efforts of ingenuity and skill, the object to be attained was as distant as ever. Itard indeed invented an instrument, which was in a great measure free from the objections urged against those of his predecessors, since its use was followed neither by cicatrization, nor by dangerous inflammation of the internal ear. Although, with these advantages, he performed the operation of perforating the tympanum in a number of cases of accidental and congenital deafness, he frankly confesses that his success was completely ephemeral. He has therefore entirely renounced its employment.

M. *Deleau*, a young French surgeon, who has made himself as much noted by his bold assertions of the cures of deafness he has performed, when others were confident there were none, as by his ingenuity and perseverance, was not however discouraged by the failure of his predecessors. Having contrived an instrument more complicated in its structure, than any which others had previously used, and which, he alledged, if skillfully employed, would render impossible the obliteration of the aperture in the membrane of the tympanum, he commenced a new series of experiments. The results of twenty five of these, which he deemed most successful, he published in 1822 in a work entitled *Mémoire sur la perforation de la membrane du tympan*, etc. In reading this essay, it is difficult to avoid the conviction, notwithstanding the constant effort he makes to show the remarkable success he has met with, that even if truly related, it is scarcely worth mentioning. In some cases, to his great disappointment, the aperture closes; in others, a promising subject, when just about to demonstrate the complete success of his operation, is afflicted with a cold, or some form of disease, and again plunged into his original state of deafness. Sometimes the parents are perverse enough to deny that the hearing of their children is improved, and sometimes the children hear well enough, but utterly refuse to talk! To judge from the cases before us, he seems to have succeeded in every thing, except restoring his patients to the full and permanent use of the sense of hearing. In this, it is perfectly evident, he met with no success. *He has not recorded a single instance, in which a patient was so far restored to hearing, as actually to have acquired the use of language.* At the same time, it is equally evident, that the hearing of some of his patients was somewhat improved, although probably in most of the cases, the cure was merely temporary. The fact that he has abandoned the use of the instrument he invented, and that in his later writings scarcely any mention is made of the operation itself, more conclusively proves its inutility, and the little success with which it was actually attended, than any arguments which others could possibly advance.

At the institution for the deaf and dumb at Groningen in Holland, the operation was performed by the celebrated Professor *Hendrisksz* and Dr. *C. Guyot* on *eighty one* individuals. Of these eighty one, there were only seventeen, whose hearing seemed to be in the least improved; and even of these, fourteen, before the expiration of nine months, relapsed into their original state of deafness. The remaining three preserved their artificial hearing, but not to such an extent, as to be of any use to them in the acquisition of language.*

The results, then, of the operation of perforating the tympanum,—an operation which has been performed in a great number of cases, and by a large number of skillful surgeons, have been such, that no rational hopes can be founded upon it, as a means of restoring the deaf and dumb to hearing and the use of speech. Dr. *Wright*, an English surgeon, who has written a very candid work on deafness and its remedies,† strongly objects to the practice. He affirms that atmospheric air, which passes through the membrane of the tympanum, does not become regulated in temperature, as it does when passing naturally through the Eustachian tube; that by perforating the tympanum, the painful sensibility of hearing, which at first takes place, is shortly followed by a partial or total obliteration of the faculty, occasioned by the unnatural and immediate vibration of sound, striking upon the fine membrane and producing an excessive degree of tension; and that atmospheric air, in its passage through the Eustachian tube, probably undergoes certain physical changes, which it cannot do, when entering the cavity through the membrane of the tympanum.

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* *Deuxième circulaire de l'Institut royal des sourds-muets de Paris*, etc. Paris, 1829.

† *An essay on the human ear.* London, 1817.

Although other distinguished physicians have not coincided with Dr. Wright, in regard to the injurious effects of the operation, they have with great unanimity testified to its inutility. "Profesor *Dubois*," says M. *Richerand*, "has performed the puncture of the *membrana tympani* four times without success, on subjects aged from thirty to fifty years. This inutility of the operation, proved by the four instances so well authenticated, will tend to make the correctness of other observers doubted—at least to show that one should not always promise himself success."* "I entertain, in that respect," observes M. *Saissy*, "the opinion of Professor Richerand, and I will also add, that there are a great many circumstances which may defeat the operation. In other cases, success will be but temporary. There will be but few cases in which it will be successful."† In speaking of the operation, M. *Berjaud* remarks;‡ "The merited neglect into which it has fallen on the part of the most distinguished of the Paris practitioners, and the judicious opinion long since expressed by Professor Richerand, confirm in a stronger manner than any thing that I can say, the impotence of the perforation of the tympanum against congenital deafness, and the discredit into which it is gone, even in the cases of occasional infirmities of the same nature. A most conclusive proof, and one which I ought not to omit, is this;—that when the operation was so common in France, it was performed on nearly all the deaf-mutes at that time in the Paris Institution, as well as on others who were afterwards admitted, without the least advantage."

M. *Itard*, to whom we have alluded as having met with no success in the perforation of the tympanum, devised a new mode of operation, which seemed for a time to promise success. Having found in two deaf-mutes, who had died within a few months of each other, the internal ear completely obstructed by concretions, composed in one, of thick mucus, and in the other, of a matter resembling chalk, he inferred that congenital deafness might be produced in certain cases by a material cause, and that this cause might be removed. To do this, he decided on injecting the cavity of the

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* *Nosographic chirurgicale*, Tome ii, p. 132; as cited by Saissy.

† *An essay on the diseases of the internal ear, translated from the French, by Nathan R. Smith, M. D., Prof. of Surgery in the University of Maryland.* Baltimore, 1829.

‡ *Examen critique de cette question; Dans l'état actuel des sciences médicales, peut-on rendre l'ouïe et la parole aux sourds-muets de naissance?* Paris, 1827.

tympanum *through the membrane*, with the view of expelling the concretions through the Eustachian tube. By a fortunate chance, the first deaf-mute, upon whom he undertook to perform the experiment, was precisely of that small number, who owe their defect to the cause in question. The patient was a child, twelve years of age, and deaf from birth, named Christian Dietz. His restoration to hearing was almost complete, and he would probably have recovered the use of speech, had his life been continued. He was attacked, however, with a disease which baffled medical skill, and occasioned his death a few months after the operation.* M. Itard was induced by the encouraging result of this experiment, to perform the same operation on twelve other deaf-mutes, but its inutility from these cases, became so evident, that he abandoned it in despair.†

II. *Injection of the Eustachian tube.*—The attention of physicians was next directed to the injection of the Eustachian tube. The end to be attained was the same as before—the free admission of air into the cavity of the tympanum. The possibility of this operation was first suggested by *Guyot*, a post master at Versailles, who was afflicted for many years with deafness caused by mucus which obstructed the tube. He performed the operation on himself, and in 1724 presented the instrument he had employed to the Royal Academy of Sciences. "The most important part of this instrument," say the committee, "is a curved tube, which is passed deep into the mouth, behind and above the palate, so as to be applied to the extremity of the canal to be injected. It will serve at least to wash the mouth of the Eustachian tube, and will thus, perhaps, in certain cases, be useful."‡ This language sufficiently indicates the distrust with which the committee regarded the alledged injection. The faults of the operation were in fact so prominent, and the impossibility of performing a perfect injection through the mouth was so apparent, that it was abandoned for nearly twenty years. The idea however had once been suggested, and it required only that some improvement should be proposed again to bring it into notice. This was done by *Cleland*, an English surgeon. He recommended

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* A minute and interesting report of this case will be found in Itard's *Traité des maladies de l'Oreille et de l'Audition:* Tome ii, p. 464, et seq.

† *Revue médicale Française et Etrangère, et Journal de Clinique*, etc., Avril, 1827, p. 34.

‡ *Hist. de l'Acad. Royale des Sciences, année* 1724. p. 37.

that in place of the leaden tube employed by Guyot, a flexible one of silver should be substituted and directed into the Eustachian tube *through the nose.** This operation was first actually performed by *Douglas*, on several dead subjects, and shortly afterwards by *Wathen*, on a number of living patients. The latter published an account of five cases in which he supposed more or less benefit had been derived from the operation.† But subsequently, as we learn from Dr. Sims, a late President of the Medical Society of London, "he became less sanguine in his hopes of cure from it, than he was originally."‡ As late as 1791, it was proposed by *Lentin*, who had somewhat modified the original method of Guyot, to pass the injection through the mouth,§ but the conviction of its impossibility, or at least its extreme difficulty had become so general, that no one appears to have adopted his suggestion. The injection through the nose, on the contrary, was performed by *Sabatier, Leschevin, Desault, Saissy, Boyer, Itard* and others, nearly all of whom either invented new instruments or proposed some modification of those already in use. But although the operation had now been practised for nearly a century, it may be considered as having demonstrated nothing, except the practicability of the injection of the Eustachian tube, and the little reliance which could be placed upon it as a means of cure, when *M. Deleau* undertook another series of experiments, which he affirmed had at length proved that the deaf and dumb might be restored to hearing and speech. The committees appointed by the Royal Academy of Sciences, reported as favorably of his success as Deleau could possibly have desired. The newspapers eagerly seized hold of what so nearly approached the marvellous, and circulated the most exaggerated reports of his magic power. As these accounts were extensively copied into the papers in this country, it is not improbable that many of our readers will remember them.

There were some, however, who, in the midst of this general enthusiasm, had the boldness to doubt the reality or at least the extent

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* *Phil. Trans. of the Royal Soc. of London, for the year* 1741. Vol. xli. p. 847.

† *Ibid, for the year* 1755. Vol. xlix. p. 213.

‡ *An essay on the deaf and dumb; shewing the necessity of medical treatment in early infancy; with observations on congenital deafness.* By John Harrison Curtis, Esq. London, 1829. 8vo. p. 173.

§ *Tentamen vitiis auditvs medendi*, etc; *in Commentationibus societatis regiae scientiarum Gottingensis ad* 1791 *et* 92. Vol. xi. p. 39.

of the wonderful cures alledged to have been performed. Themselves physicians and surgeons, and many of them distinguished for their knowledge of the anatomy and diseases of the ear, they were too well acquainted with the little success which had attended the efforts of others, and with the intrinsic difficulty of the subject, to believe that it had all at once been so entirely surmounted, that henceforth deafness would be as easy of cure as other diseases. In reply to the numerous cases of restoration to hearing which M. Deleau was constantly publishing, they affirmed that some of his operations were precisely such as had been repeatedly performed before him, with not the least success; and that others were anatomically impossible. They complained that no information was given of the condition of the patients after the operation; and denied, in short, that any cures had really been effected. In answer to these objections, he seems to have relied in a great measure upon his apparent success in the case of a boy named Trézel. As this case excited great interest at the time (1825) both in England and on the continent, we present the report of the committee of the Academy of Sciences nearly entire.

"Claude-Honoré Trézel, at this time ten years of age, born at Paris, of poor parents, was of that class of the deaf and dumb which cannot hear the loudest noises nor the most violent explosions. His countenance had little expression; he dragged his feet in walking, and his gait was tottering. He did not know how to wipe his nose, and he made his principal wants known by a certain number of signs.

"Nothing remarkable occurred during the operation, which is very simple and by no means new. It consisted in the injection of liquids into the Eustachian tube, by means of a small flexible *sound.* The first few days after the development of hearing, was a season of continual delight to the child. Every kind of noise gave him inexpressible pleasure, and he sought for them with great eagerness. It was not, however, till after some time that he perceived that speech was a means of communication: this he still attached not to the sounds that issued from the mouth, but to the movement of the lips. Accordingly, for some days, he thought that an infant of seven months old spoke, because he saw the movements of the lips. He was soon taught his error, and informed that it was only to the sounds that importance belonged. It happened, unfortunately, that he heard a magpie pronounce some words,—then generalizing this fact, he thought that all animals could articulate, and actually endeavored to make a dog speak. He employed considerable violence to make him say *papa, du pain,* the only words which he himself could pronounce. The cries of the poor animal alarmed him, and he desisted from his attempt.

"The earlier period after the development of hearing wrought a considerable change in the physical state of Trézel. His step became firmer; the mournful air of his countenance changed to one gay and smiling; he learned to wipe his nose, and ceased in walking, to drag his feet. A month elapsed, and Honoré, absorbed in his new sensations, remained in nearly the same state. He could not seize the different syllables of which compound words are formed,—much less know their signification, or even that of short and simple phrases. He required much time also to enable him to distinguish the direction of sounds. His instructor, having concealed himself in a room in which the child was, called him by name; and it was only with the greatest difficulty, that he could discover the person who spoke, and even then, it was rather by his eyes and reason than from the sound, that he discovered it.

"The first sounds which Trézel pronounced without difficulty, were *a, o, u,* etc.; and the first words which he formed, were *papa, tabac,* etc.; but when he wished to pronounce more complicated words, he made great contortions of the lips, tongue, and all the parts concerned in articulation. By repeated efforts, he became able to pronounce a few compound words, which at first were far beyond his power. When advanced thus far, he believed himself on an equality with other children of his own age: and satisfied with himself and proud of his new situation, he looked with great disdain on his former companions in misfortune. Notwithstanding, however, this slight exhibition of vanity, Trézel made but little progress in pronunciation; a vast number of syllables escaped him, or he articulated them in a very defective manner. Perhaps he would never have surmounted this difficulty, had he not ceased to depend entirely upon his ears, and assisted himself by his sight. Several words were written, and he pronounced them much more distinctly, catching with considerable clearness, the assemblage of the vowels and consonants, and their reciprocal influence. Another very remarkable fact may also be stated, viz. that the association between the sight and the movements of the larynx, was always prompt and easy; while that of hearing, and the organ of voice was always difficult, and slowly exercised. For instance, as soon as Honoré perceived the written syllables, he pronounced them, if at the same time they were repeated to him; but if the writing was removed, the syllables were in vain articulated in the most distinct manner; he could not follow them.

"His pronunciation is very defective; the *r* rolls disagreeably upon his tongue, and the differences in accent appear unknown to him. He exhibits also a phenomenon which has engaged the attention of the commissioners. When they spoke a word distinctly to him, he repeated it immediately; but if his instructor wished to address his understanding, signs and expressions of countenance were employed. It would have been thought, that after having acquired a new mode of expressing his wants and ideas, he would have neglected that which had formerly served him, and which is inferior to speech; but hitherto, the contrary has happened. The natural language of Honoré, *i. e.* by signs, instead of going gradually into disuse, and being replaced by speech, has rapidly gained a degree of perfection and force which it did not possess before he had acquired the sense of hearing.

"In recapitulation, Honoré Trézel, who, a year ago, was so completely deaf as not to be able to hear the loudest noises, understands all kinds of sounds, knows when they come from a distance, distinguishes their character, avoids carriages and horses, and proceeds to open the door when any one knocks. He is pleased with music, and can appreciate and repeat all the articulations of the French language. He obeys the spoken commands of his instructor, but does not yet sufficiently understand other people: and he learns, analyzes and repeats a number of phrases at length."*

This report, plausible as it is, did not excite, in the minds of physicians who were most interested in the subject, that confidence, which ordinary readers would probably give it. It was asserted that some of the statements of M. Deleau, with regard to the operation, were to any one acquainted with the anatomy of the ear, utterly incredible; and that the brevity and obscurity with which the operation was described, were such as to destroy, or at least very much to impair all confidence in its truth. It was affirmed too, that a palpable contradiction existed between the report presented to the Academy of Sciences and the account of Deleau; for while in the

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* *L'ouïe et la parole rendues a Honoré Trézel, sourd-muet de naissance,* etc. Paris, 1825.

former of these documents, the operation is said to have consisted in the injection of *liquids*, in the latter we are told that it consisted in the injection of *air*.

The true subject of inquiry, however, and the only one worth settling, is the actual degree to which the child acquired the sense of hearing. The operation was performed in April, 1824; and the report we have cited above, was made in June, 1825. During this period, he had learned, we are told, "to appreciate and repeat all the articulations of the French language. He learns, analyses and repeats a number of phrases at length, and obeys the spoken commands of his instructor." But M. Berjaud, in 1827, two years after, gives a somewhat different account of his progress. "I have seen Honoré Trézel," he says, "at two different times. At my second visit, which took place a year after the first, the child was no farther advanced in his education, than when I first saw him. He continued to recite the three or four first verses of La Fontaine's fable of the fox and the crow; but when I distinctly asked him this simple question: *do you love sugar plums?* (bonbons) he was so perfectly confounded that he could only look at his instructor, and stupidly repeat *bo, bo*."* In one of Deleau's works, however, published in 1830, we find the following note: "Honoré Trézel has forgotten his former language, (of signs): he speaks and talks with the greatest facility. He is constantly increasing his knowledge by reading books, such as are usually put into the hands of children of eight or ten years of age."†

The question still occurs, was an actual cure in this instance performed?—was the child really restored to the full and perfect exercise of the sense of hearing? Even if the most favorable account of the child's facility in speaking be admitted not to be at all exaggerated, it by no means follows that the sense of hearing was really recovered; for nearly six years, it must be remembered, had passed since the operation had been performed, and during the whole of this period, he had been under constant instruction in articulation. In this time, he might have learned to "talk and read juvenile books," even if his hearing had not been in the least improved; since the same acquisition is made in half the Institutions in Europe,

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* *Examen Critique*, etc., p. 46.

† *Extrait d'un ouvrage inédit intitulé Traitement des maladies de l'Oreille moyenne qui engendrent la surdité.* Paris, 1830, p. 25.

and within the same period, by those who are profoundly deaf. If the child's hearing had been perfectly restored, it is not for a moment to be admitted, that nearly *six years* would have been necessary to enable him to speak. As that time, however, was actually employed, and the period required to teach those who remain deaf to speak, is no greater, we are compelled to conclude, either that his hearing was entirely useless to him in acquiring the use of spoken language, or that he never, in fact, recovered it. Itard and Berjaud have adopted the latter supposition, and have endeavored to show that this alledged cure was simply a successful instance of instruction in artificial articulation. The conclusion derives considerable plausibility, from the fact that Deleau strongly insists upon the necessity of a special education of several years, for those whom he has *cured*, and that in this case, he does not deny, that six years of constant labor have been spent to accomplish the result produced. It seems more natural, however, on the whole, to suppose that the hearing of the child was really improved, but in so slight a degree, that nearly the same course was necessary to teach him to speak, as that employed in Europe with the deaf and dumb for the attainment of the same object.

We have been thus minute in examining the case before us, because it has already begun to appear in the medical books, as an undoubted instance of perfect restoration to hearing. But in this, as well as in the subsequent cases of success, published by Deleau, we look in vain for the evidence on which to found such a conclusion. Of the numerous cures which he claims to have performed, it is at present sufficient to remark, that none of them are even so plausible as that of Trézel, and that in France, by men who are best capable of judging, little or no credit is given to them. A full examination of the several publications of M. Deleau, would properly belong to the pages of a medical journal. It may here be remarked, however, that judging from his later works, injections of air (douches d' air,) seem finally to have taken the place of nearly all other remedies. Of the little value of this harmless operation, physicians can easily judge.

The success which seemed for a time to have crowned the efforts of Deleau, induced the Council of Administration of the Royal Institution for the deaf and dumb at Paris, to request of M. Itard, a report on the advantages which might reasonably be expected from various remedies, if employed, on a large number of their pupils.

No man in Europe was better prepared to make such a report than Itard. For nearly thirty years he had been at the head of the medical department of the Institution, and during that time, had been unwearied in his efforts to restore the deaf and dumb to hearing. He had made himself acquainted with all that had been known by his predecessors on the anatomy and diseases of the ear, and had written the most elaborate work which had ever been published on this difficult subject.

In consequence of his report, it was decided that a certain number of the mutes in the Institution should be subjected to medical treatment. As nearly every other remedy had previously been employed in vain, Itard determined to make a thorough experiment of the utility of injecting the Eustschian tube. He therefore, performed the operation in *one hundred and twenty* cases, the results of which were, to use his own language, "just nothing, with regard to hearing, in the great majority of the mutes, and in the rest, temporary and of little advantage.*

With this conclusive experiment, the great efforts which for several years, had been made in France to restore the deaf and dumb to hearing, may be considered to have ended. M. Deleau, indeed, still continues to practice in cases of deafness, but no important facts of a more encouraging nature, so far as is known, have since been brought to light. It is now, we are informed, the general opinion among those in France, who are capable of judging on the subject, that these numerous and long continued experiments have proved that the sense of hearing cannot be perfectly restored to the deaf and dumb, although it is admitted that it may sometimes be improved. This conclusion, the efforts of both Itard and Deleau, we think, have abundantly shown. Although a few cases of perfect recovery have undoubtedly occurred, they can be regarded only as isolated exceptions, and do not destroy the general principle, that *congenital deafness cannot be cured.*

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* *Deuxième rapport, lu, le* 9 *février*, 1827, *au Conseil d'Administration*, etc., p. 11.

ART. VIII.—*On an instrument proposed for measuring the expansion of Solid Bodies, and which may also be used as a Thermometer;* by W. W. MATHER, A. M., and Lieut. U. S. Army.

IT has long been a desideratum to measure the expansion of bodies, and the changes of temperature, more accurately than these can be done by the instruments which are or have been in use. Every scientific man is aware of the practical utility of a solution of the two points mentioned, and more particularly, in geodesic operations, where the accuracy of extensive surveys is dependent on a rigid determination of the length of the base line, and in the true determination and verification of weights, and of measures of length and capacity. An instrument for such purposes, becomes more valuable in proportion to its accuracy and its capacity, for rigid verification. The mode of determining the expansion of solids employed by Messrs. Lavoiser and Laplace, is of all the methods that have been employed, the least exceptionable, and probably their determinations, as far as they experimented, are close approximations to the true expansions.*

Mr. Hassler's method of measuring the expansion of his measuring rods by means of micrometer screws, is very ingenious, and it is surprising how close his approximations are to those of Lavoisier and Laplace, when we consider the possible error arising from the probable slight flexture in his long wire rods.†

Thermometers all labor under an objection, which it has not, hitherto, been practicable altogether to obviate, and the one that I shall propose, will probably labor in a slight degree, under the same objection, if employed at a higher temperature than the ordinary ranges of atmospheric heat and cold. This objection, in the thermometers hitherto used, arises from the different rates of expansion of the bodies used in their construction, and from there being no means of testing rigidly, the rates of expansion in each individual case. The fact is notorious, that scarcely any two thermometers, however carefully constructed are strictly comparable, and hence, the utility of the instrument in minute scientific investigations is much less than might be expected. To the same defect are to be attributed, in

* A description of their instrument and mode of experimenting may be seen in Biot, Traité de Physique, or more in detail in the Memoirs de l'Institute.

† Vide Hassler on the Coast Survey of the U. S. in the Am. Phil. Transactions.

part, the different results of philosophers in some of their investigations on temperature, and as an example of this, the temperature of of distilled water at its maximium density may be quoted.

Although the flint glass experimented on by Biot and Arago, had such a rate of expansion as to counteract almost exactly the increasing rate of expansion in mercury and thus produce the effect of a uniform expansion of the mercury, yet the flint glass manufactured for thermometer tubes is not composed of the same proportions of the materials in different manufactories and is of different densities and rates of expansion, and hence it follows, that its rate of expansion in most cases differs from that of mercury, and consequently, the compensation will not be exact.

Again, glass is highly elastic, and the increasing length of the column of mercury, causes the bulb, which is of thin glass, to increase its capacity by the effect of hydrostatic pressure, and as all the bulbs have not the same thickness in relation to their capacities, it follows, that in different thermometers with even the same length of columns of mercury, the effect of the hydrostatic pressure, in increasing the capacities of the bulbs, would prevent them from being strictly comparable. There are other sources of error too well known to require notice.

Of all the metallic thermometers, those of Borda and Breguet, are perhaps least objectionable. The first, to be very accurate, requires the bars to be very long and it then becomes cumbrous: the other has too many sources of error to be regarded as an accurate instrument.

Air thermometers are the most accurate of all, yet they labor under this objection, that while the air expands uniformly for equal increments of temperature, the containing body expands in a slightly increasing ratio as the temperature increases, thus enlarging its capacity, so that a less temperature is indicated than the true one. This is true as well with the differential as with the other air thermometers, but the inaccuracy is so slight as to render it inappreciable at ordinary ranges of temperature.

The published details of the measurement of the base lines of various extensive trigonometrical surveys, of the measurement of the arcs of the meridian, and of the establishing of standards of weight, length and capacity, show how much labor, thought and science have been employed in endeavoring to arrive at rigid accuracy. The thermometer has been the principal stumbling block, in consequence of no means having been contrived to insure uniformity in the indications of this instrument, and there being no means of testing its accuracy to the extent that many scientific researches demand; and as corrections for changes of temperature enter into all the investigations mentioned above, uniformity in the results, can be obtained only when temperatures, and the changes of volume resulting from varied temperatures can be accurately measured.

In all accurate measurements of lineal expansion in solids, the first object is, to have two points which shall remain invariably equidistant at all temperatures within the range of experiment: and the second, is, to provide the means of accurately measuring the variations in the length of the body under examination, when it is placed between these fixed points.

I propose to accomplish the first of these objects, by making use of *two bars of different metals, whose lengths are inversely proportional to their expansibilities,* on the principle of the compensation pendulum; that is, if both the bars be equally heated, the shorter bar shall expand exactly as much in length as the longer, and the distance $a\,b$ in the annexed diagram shall be equal, at one temperature, to $a'\,b'$ at another.

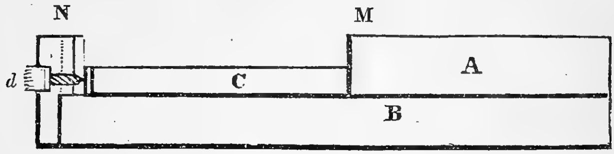
To verify the accuracy of this equal expansion, I propose to use the combined bars fastened together at (c) as a balance beam; the masses being so adjusted, as to throw the center of gravity in the vertical plane passing through (a), and perpendicular to the axis of the beam. A delicate knife edge is attached to the end of the short bar at (a), like that of a balance, and it rests on polished cylinders of glass, transversely to their axes. The friction is thus rendered null, by the contact of the knife edge and the supporting cylinders being reduced almost to a mathematical point. After the center of gravity shall have been brought indefinitely near to the knife edges, and below them by the ordinary adjustments of a balance, so as to render it as delicate a balance as the inertia of the mass will allow, the combined mass is ready for experiments to test the quality of expansion. If it be now subjected to varied temperatures, (being exposed to each a sufficient time for its mass to be uniformly heated,) and it remains in equilibrio, the compensation is exact, and the expansion of each bar is equal in length.

This method, although sufficiently accurate for most purposes, has its limit in delicacy, and is not as exact as many philosophical investigations require. I propose to employ another and a better method of verifying the accuracy of the compensating expansibilities, one that is capable of demonstrating to any degree of exactness that may be desired, whether the expansion of one of the bars compensates perfectly for the expansion of the other; or in other words, whether the distance between the extremities of the two bars be uniformly the same at all temperatures to which the mass may be subjected.

This mode is, to use the combined mass as a pendulum the knife edge of suspension being on the plane of the end of the longer bar, and the mass being so adjusted as to throw its centre of gravity in the plane of the extremity of the shorter bar. It is a well known mechanical principle that the product of the distance from the point of suspension to the centre of gravity, into the distance from the centre of gravity to the centre of oscillation is a constant quantity. From this it follows, that a pendulum keeping uniform time, must have its centre of oscillation remain at a constant distance, else, the length of the pendulum varying, the time kept by it will vary. If the pendulum under consideration keeps uniform time at varied temperatures, it demonstrates that the centres of oscillation and gravity remain at invariable distances from the point of suspension, and consequently, that the distance from the extremity of the longer to that of the shorter bar remains of a constant length and solves the proposition, viz. to obtain two points which shall remain at an invariable distance at different temperatures. Suppose the pendulum at a mean temperature of 32° Farenheit, beats (m) times in a week, and at a mean temperature of 100° Farenheit it also beats (m) times in the same period; it follows that the distance of the centres of oscillation and gravity remain at a constant distance from the point of suspension, and that the compensation for unequal expansibility is exact. This mode of verification may be carried to any degree of exactness that circumstances may render expedient. One thing has been supposed that is not rigidly true, viz. that the metals used for the bars each expand uniformly for equal increments of temperature; but as the uses for which the instruments will be most valuable, do not require higher ranges of temperature than those of the atmosphere, this source of inaccuracy, if metals of high fusing points be selected, becomes almost infinitely small, and would scarcely be appreciated, even by the rigid verification above proposed.

The second point is, to provide the means of measuring accurately the variations in length of bodies placed for experiment between the fixed points.

This may be done by means of a micrometer screw with a graduated head and vernier attached: or in a better way by the bar of experiment C, which abuts firmly against the more exhaustible bar of the instrument A acting at its free end against the screw *d*. This screw is made with a very oblique thread so that it will thrust out with a small force and turn at the same time on its axis. By means of the rotation of this screw, the expansion of the bar C between the two invariable points MN may be measured.



The rotation of the screw *d* may be measured by a system of wheel work like that of a watch, or, (as this is liable to some inaccuracies from complication) by means of a telescope mounted on the axis of the screw and perpendicular to it, and ranging over a graduated arc with a vernier, at a convenient distance. In this way a minute of a degree equal to $\frac{1}{21600}$ of a rotation, and perhaps a second of a degree equal to $\frac{1}{1296000}$ of a rotation of the screw might be measured. The limits of the delicacy of this determination will depend on the smallness of the screw which must be strong enough for the purpose named, and on the distance of the arc at which minute divisions can best be seen by the telescope. This method seems to be susceptible of much greater accuracy and more rigid verification than any one hitherto employed. In the practical use of the instrument there are only two corrections to be made and they are of the nature of constants which are to be applied in all the experiments except when the experimental bar is of the same metal as the screw. They are, 1st, the expansion of the screw *d* from *o* to *o′*, and 2d, the correction for the expansion of a steel plate of known thickness between the screw and end of the bar C. This steel plate is acted against by the point of the screw, because this point would otherwise indent the bar and the screw be thrust out a less distance than the actual expansion.

The thermometer proposed is the same instrument as that which has been described, except that the same experimental bar C is always used, and that a spring is connected with the oblique threaded screw *d* so as to cause the point of this screw to be always pressing upon the end of the bar C. The temperature may be indicated by means of a graduated plate connected with the screw with a vernier attached analagous to those of a theodolite, by means of a system of wheels like a watch; or by means of a telescope mounted as before described. This thermometer labors under one objection, viz. that the different metals do not expand with perfect uniformity as their temperatures increase: but as in this instrument no temperatures much beyond the limits of atmospheric variation are proposed to be measured, this objection vanishes, for, the metals of difficult fusibility are said to expand infinitely near to uniformity by equal increments of temperature within the range of atmospheric changes.

Another application of the principles of the same instrument is proposed for use, in connection with scales of equal parts, for making mathematical drawings. Every one who has attempted to make very accurate mathematical drawings, must be aware that a distance of any number of divisions of equal parts laid off at one temperature, differs from that laid off from the same number at another. It follows, that in accurately plotting the triangulations of extensive surveys, a practical difficulty would be experienced, and it *is* often experienced, when the plotting is continued through various atmospheric changes. The method proposed for obviating this difficulty, is, to have several of the pairs of bars described as maintaining a uniform distance between two points at different temperatures, and let the distance MN, in them respectively, be one inch, one foot, one yard, one metre, or any other convenient units. By means of these units, the common brass scale of equal parts usually seen in boxes of French mathematical instruments, *may be set* each day when commencing to draw. If the temperature of the room remain uniform during the day, and the scale be not handled to increase its temperature, the difficulty arising from expansion so far as it depends on the uniformity of the scales for measurement is obviated.

Those gentlemen engaged in plotting the triangulations of the coast survey of the United States, under the direction of Mr. Hassler, frequently experience the inconvenience arising from the expansion of their scales and paper. I trust that the above suggestion may tend to remove one of the many difficulties which they have to encounter.

Being under the impression that the instrument which I have proposed will obviate some of the difficulties in the measurements of dilitation and temperature, I may be excused for adding another to the long catalogue of those which have been heretofore described.

Fort Gibson, January 31st, 1836.

Art. IX.—*Notice of a Scientific Expedition.* Communicated by Prof. E. Emmons, of Williams College.

Since the appearance of the publication of Dr. C. T. Jackson and of Mr. Alger, on the mineral riches and geological structure of Nova Scotia, a strong desrie has been felt by us to visit the same interesting region. Though the harvest had been principally reaped by those gentlemen, yet the variety of the mineral productions, the instructive geological formations, the scenery, the wonderful tides, and even the fogs of those shores, all conspired, together with other less weighty considerations, to give the proposal of a visit to that region, during our succeeding vacation, an unusual popularity.

Accordingly arrangements were made in the latter part of the summer term, by the young gentlemen of the Society of Natural History,* and three instructors, to put in execution the proposed project of an expedition to Nova Scotia, during the fall vacation.

Circumstances obliged us to limit our absence to twenty days from Boston: a period much too short to accomplish all we had proposed to ourselves.

In this place it is proper to remark, that the following account must be brief for two reasons, viz.: 1st. the story of the voyage has already appeared in several weekly publications. 2d. A former number of this Journal contains nearly a full description of the mineral riches and geological formations of this province.

The party left Boston for Lubec, in the sloop Flight, Captain Hallett, on the afternoon of the 25th of August, and a fair wind carried us to that port in about forty hours.

Our object in visiting that place was to obtain a more favorable clearance for the British Provinces. This we were enabled to do

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* Of Williams College, Berkshire, Massachusetts.

by the kindness of Mr. Thayer, the collector of the port, to whom letters had been addressed by John Tappan, Esq. of Boston, to whom also we are indebted for many of the comforts of the voyage, if not for much of its success.

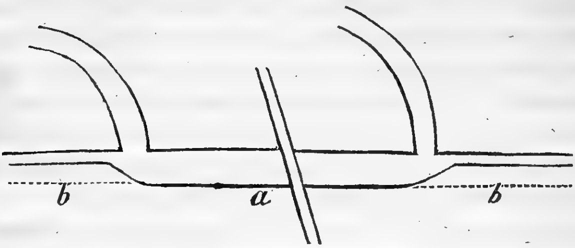
Our stay at Lubec was short. We however visited a few interesting places. The first is near the plaster mills; it is a deposit belonging to the newer tertiary. The body of this formation is a stiff marly clay, in which fossils are thickly imbedded; the remains of molluscous animals are of the same species as those now inhabiting this sea. The elevation of this bed of marl is about forty feet above high water; it occupies in part a ravine which extends inland about half a mile. It rests on a porphyritic greenstone.

A further examination of this coast, and also of the Grand Nassau is desirable, and this we propose to make at some future time. Whether similar formations to the above skirt this coast we were unable to determine. The single instance of a formation of this character, raises the question of the recent elevation of this coast, and so far as we can judge, settles it; but the discovery of similar deposits would remove some objections and add much in establishing its certainty. Connected with this view is the fact of a multitude of islands on the coast, and in Passamaquaddy Bay. It is even asserted that there are in it no less than 360 islands. Now a slight elevation of the coast, and the bottom of its neighboring sea would convert much of it into dry land. This would give rise to a great complexity in the geological structure of this coast. It is not our intention to follow out the thoughts which these few facts suggest; it is sufficient to say that the elevation of extensive territories is not unfrequent in our day, and of course may have occurred here.

Another place of geological interest is Comstock's Point, four miles from Lubec. Here occurs a junction of greenstone with what appeared to be transition limestone. There is not so much change in the structure of the two rocks at the line of junction as some would *wish* to see. Neither of them have a vesicular structure; but the limestone is lighter colored, much fractured, and more compact near the line of junction. That which will excite the attention of the geologist the most is the existence of trap dykes, wherever the limestone is uncovered.

Fig. 1 is a representation of one of these. The whole width of the main branch is two feet. The curved portions are segments of circles nearly thirty feet in diameter. *a* is a portion of the dyke entirely exposed. *b* is another part which is partially concealed by a layer of limestone shelving over it. It is evident, therefore, that this dyke has been brought to light by the wearing away of the upper layers of limestone. The circumstances accompanying this

Fig. 1.



dyke suggest the thought that it was projected into the limestone while it was yet forming, and in comparatively a soft state; and that that portion of the limestone which shelves over the dyke, and other portions which have been worn away, were deposited after the dyke was driven through the rock. If so, then we have a clue to the comparative age of the greenstone. Whatever theory we may have on these points, it is plain that dykes may sometimes exist when they do not appear at the surface.

The last mineral locality visited by us in the neighborhood of Lubec was the lead mine. It is about eleven miles from that place, and is owned by Mr. Thayer. The immediate gangue is much like that at the Southampton lead mine. The ore is the common sulphuret accompanied by the sulphuret of zinc. There are three vertical veins in the neighborhood of each other. Their width varies from one inch or two to a foot, and in some places they bulge out to three or four feet. Judging from appearances, we should infer an abundance of lead, but the gangue is a very tough variety of quartz; the expense of working the mine is so great that it is quite unprofitable at the present price of lead. We were disappointed in not finding any of the salts of lead at either of the veins, though we have been informed that the sulphate and carbonate have been procured here.

Returning from a visit to the mines just before sunsetting, we were gratified with the sight of a fine atmospheric phenomenon. The day had been very foggy much to our discomfiture. But at this time the atmosphere began to clear up. The fog seemed to gather into strata of different densities, with surfaces as distinct and well defined as if they had been cut with a sharp instrument. These were often tinged with the brighter hues of the rainbow. The scene, however, was always changing. The stratum of mist which one moment rested on the bosom of the bay soon rose from the silvery expanse, and as it rose became more and more tinged with the golden beams of the setting sun. Rising still higher it gradually separated into fleecy masses which still rose and finally formed long slender flaxen clouds, which stretched across the firmament in different directions. Thus in a very short time an atmosphere loaded with vapor clears up, and the gloom which shrouds one of the most beautiful bays disappears as by the influence of magic.

We found it to be a common observation here and it was verified by our own experience, that the south and south west winds bring a fog. This is easily accounted for from the known fact that the wind blowing from these directions, arrives here loaded with vapors; this vapor the atmosphere can no longer sustain; the temperature of this region being constantly lower than the more southern and western, hence the superabundant moisture is deposited in the form of fog or mist.

The next day, August the 27th, we left Lubec for St. John, distant about sixty miles. Failing to reach there the first day, we were obliged to lie at anchor in Caliph's Cove. Here on a sandy beach we were rewarded for our delay by finding the *Lithospermum maritimum*, *Cakile Americana*, Nutt. the *Bunias edentula* of Bigelow; but what was quite unexpected the *Carex Davisii* of Dewey, a small plant which we had supposed was confined to a few little patches on Stone Hill, about a mile south of the College at Williamstown, Mass. St. Johns, at which we arrived the next day, is built on graywacke slate, highly inclined. It is a place of considerable trade, especially in lumber. Its immediate vicinity is not particularly interesting. The fall of St. John's river is worth visiting. It is through a deep gorge. The fall is said to be both ways, and may be passed at about midtide. The neighborhood of St. John's is strewed with boulders of different rocks, as granite, gneiss, hornblende, and a conglomerate, made up of primitive masses, consisting of fragments six to twelve inches in diameter.

After remaining at St. John's one day we departed with a clearance for Parsboro, situated in the basin of mines. The only incident on our passage worth recording was a beautiful exhibition of the mirage. Our course was to the east leaving behind us the distant headlands of New Brunswick. During the afternoon, having but little wind, and not making much, if any head way, we were occupied in letting down empty bottles into the sea, that they might be broken by the pressure or filled with water forced through the pores of the cork. The temperature of the water at the surface was 52° F. That which was drawn from a depth of thirty fathoms was 50°. As we were about finishing these experiments, the attention of our party was accidentally called to a white silvery stripe like mist far in our stern. Those who had the best eyes soon recognized this as the sea elevated by extraordinary refraction. It so happened that a vessel was sailing in this direction. Soon this vessel appeared sailing upon this mist in the sky. Presently the scene is changed; the vessel before upright is now inverted, upon its keel the ocean appears to rest, bearing another vessel in position erect. A variety of illusions of the kind continued to present themselves until the evening closed in upon us. Similar phenomena are probably common; but such complicated cases of extraordinary refraction are undoubtedly rare even in this region of mists. Coming to anchor, we were soon made sensible of the force and rapidity of the tide in this bay; it swept by us with the ripple and flow of a river's current. We were assured by the pilot that it was equal to nine knots an hour; requiring, of course, a very strong wind to give a vessel head way against it.

By the next morning, a favorable wind and a returning tide brought us near the shores of Nova Scotia, and we were soon permitted to tread on the field which we had so long viewed only in prospect. The place before us was Peters' Point; we found it among the most interesting localities we visited. That we may speak of the subjects which came under our observation with conciseness and without repetition, we shall arrange our geological remarks under a few general heads.

Appearance and structure of the Coast.—The coast is uniformly bold and runs nearly in a continuous line. Here and there are indentations formed by the action of the tide, which, during certain winds, may be used as harbors for the small fishing vessels employed in the bay. Approaching near the shore, we behold a high perpendicular range of dark colored rocks, fissured and broken, frequently overhanging their bases and apparently without support and ready to fall. As far as can be seen, sharp angular fragments of rocks, from the towering cliffs, cover the shore. A sandy, smooth beach, is of rare occurrence.

Materials forming the Coast.—Trap rocks form the coast for about one hundred and thirty miles. The breadth of the range, scarcely exceeds three miles. Beneath the trap is the new red sandstone, which occasionly appears in nearly horizontal strata. Reckoning in the upward order, the rocks are sandstone, amygdaloid and greenstone. The lines of demarkation between these rocks are remarkably distinct, so much so that they may be seen at a distance of many rods.

The amygdaloid is quite vesicular, more so, and more lava-like, than that of Connecticut river. We speak of this character as general; many portions of it resemble so perfectly cinders from a forge, that the difference would pass unnoticed by a cursory observer. The greenstone, exhibits more or less tendency to a columnar structure; it is compact, heavy, and sonorous when struck with a hammer. Its compactness is, it appears to us, between that of the basalt of the giant's causeway and of the greenstone of Connecticut river. The particles of feldspar are less; in fine, it is more homogeneous. At no place which we visited, did we meet with any porphyritic greenstone. This variety we have already remarked, occurs at Lubec, and is known also to exist at various places in the valley of the Connecticut.

Imbedded Minerals.—There is a great uniformity in the distribution of the minerals. Amygdaloid is the repository of the zeolite family, or the genus kouphone spar, while the greenstone contains the silicious or quartz family. There are rarely exceptions to this order of distribution. The only mineral known to belong to the the sandstone is selenite or gypsum.

Degradation.—The abrading action of the sea and of other destroying agents, is not the least instructive phenomenon of this region. The shores as has been remarked, are strewed with angular boulders. They extend into the sea beyond low water, but how far, we were unable to determine. From an inspection of the coast, it is easy to see how this degradation is carried on. As the amygdaloid is the rock which is exposed to the sea, and the action of its heavy tides, the porosity of its structure, enables it to resist but feebly its furious attacks. Cavities with shelving banks are formed which are sooner or later crushed in by the enormous weight of the greenstone above; or the greenstone is forced from its perpendicular sides by water freezing between its columns. As these shores have been exposed for ages to these and other kinds of destructive agents, no one can doubt that great changes have been produced both in the configuration of the coast, and in the width of the adjoining bay. If it is admitted that this bay was once narrower than it is now, the inference would follow that its tides must have risen much higher; when it therefore received into a narrow channel the great tidal wave of the Atlantic, it must have swept with a force inconceivable, and have carried on its undermining operations, with a fearful rapidity. The comminuted fragments too, would be borne to a greater distance, and be spread out upon a far greater surface. The tendency, however, of all this would be to erect finally, barriers against its own powers, and to create a limit to its own destructive agencies; so that probably, the real width of the bay does not now increase, what is lost on one side being gained on the other, with the exception of what may yet be spread out on the bottom of a wide ocean.

Stratification.—Trap rocks are rarely if ever stratified. If, however, we are not deceived, there are places where these rocks assume such an appearance, or are in fact stratified. The only place to which we can safely refer the observer is Cape Split. The stratification is, on what may be termed a large scale; the strata are thick and heavy, but the parallel lines* separating the strata, may be seen one fourth of a mile. See Fig. 3.

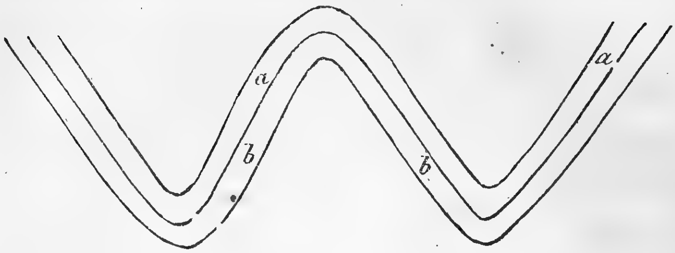
Junction of the trap with sandstone.—We are not satisfied with what we saw at those places of junction, which came under our observation. There is a sort of blending of the two rocks, and they form a species of *breccia*, but did not present marks of a partial fusion, by exhibiting vesicles or cavities. The effects of trap on the adjacent rock, are not uniform. They seem to depend on circumstances, which we can only conjecture.

Contortion of strata.—Remarkable instances of the contortion of strata, may be seen between Partridge Island and Cape Sharp. Here, for the distance of six miles, the shale and sandstone appear in alternating layers. At many places, the strata are nearly vertical, but there is no regularity in the dip of the rocks, or in the arrangement of the strata among themselves. Fig. 2, is a representation of one instance among many, of the contortions occurring between P. Island and Cape Sharp. This disorder among the strata of shale and sandstone, is supposed to be produced by its proximity to the trap, by dykes which have been forced between the layers of those rocks, and of course have acted very unequally on different points.

Scenery.—The scenery is peculiar and characteristic. Its characters arise from two causes; the nature of the rocks themselves,

* Are these lines any thing more than fissures produced by cracking?

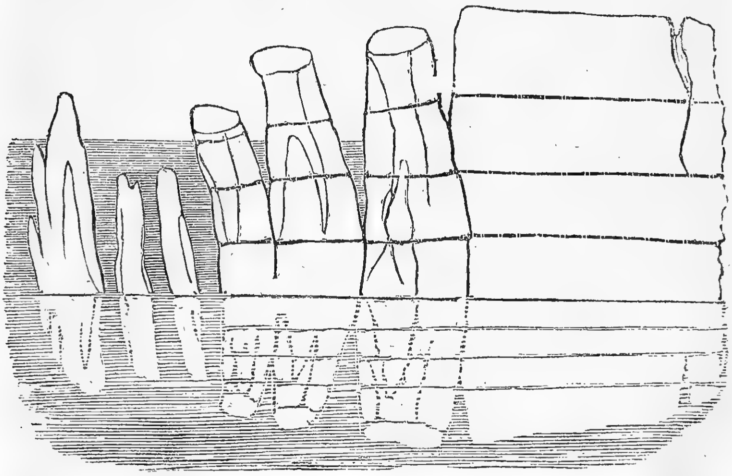
Fig. 2.



a a. Shale.—*b b* Sandstone.

and the manner they are acted upon by external agents. The scenery of a trap region, is usually bold, and it is rendered *picturesque* by the outstanding columns which have as yet resisted the attacks of time. The reader may obtain an imperfect idea of this kind of scenery from Fig. 3. It is an outline of the termination of Cape Split.

Fig. 3.



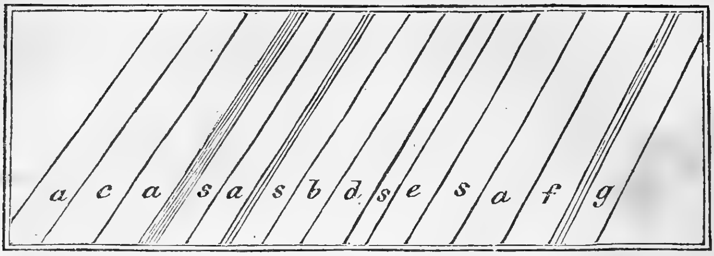
This figure is intended to show too, the manner in which rocks of this kind, disintegrate and fall to decay. There is first a separation into irregular columns; these gradually dwindle away into rude shaped masses, till finally they are overthrown by some convulsion.

Much might be said of the splendid and romantic scenery of Nova Scotia, but the narrow limits imposed on us in this paper, prevent our offering any thing satisfactory on this subject. We take pleasure in referring the reader to Jackson and Alger's work on the Geology and Mineralogy of this province, where he will find a delineation of the scenery in the plates accompanying that work.

Cumberland Bay.—Having seen as much of the trap formation as our time and circumstances permitted, the party were ready to proceed to the coal districts of the Province. It was the original intention to visit Pictou, but so much time had already been spent, it was now abandoned and we were obliged to be satisfied with an examination of a similar formation at the Joggins on Cumberland Bay. South Joggins, the place where most of our time was spent, when in the bay, presents to the sea a mural precipice of an uniform height, for the distance of five or six miles. The height of this range is about seventy five feet. The edges of the strata, stand directly to the sea; they dip at an angle of 30° to the west. The strata are sandstone and bituminous shale, with some other subordinate layers; the whole of which are exhibited in the section of the coast. The sandstone is grey and is made up of angular grains of quartz, generally not larger than a mustard seed; intermixed are a few scattered particles of feldspar and spangles of mica. The texture of the different layers varies somewhat, sometimes fine and sometimes coarser than is represented. These grains are united by an argillaceous cement. It is proper to remark here, that the color of this rock is different from that between Partridge Island and Cape Sharp. There, the sandstone and shale is brick red in many places. This difference must be ascribed to its proximity to the trap; to the heat which the latter rock communicated to the shale and sandstone at some former period. Certain it is, if the gray sandstone and dark shale are exposed to heat, they become red. The sandstone of Cumberland Bay, is employed for grindstones, most of which are brought to the United States. The best kinds are taken from the deep-seated strata; they are more easily wrought, as they are softer when first exposed to light.

The grindstones are sold at the quarries for 3*s.* and 3*s.* 6*d.* a stone—a stone measures twenty four inches in diameter, and four inches thick. There are few places where stratification is so perfect as at the Joggins. The lines of separation are perfectly parallel, and as strait as if they were drawn with a rule. Fig. 4, exhibits a section of the coast at South Joggins. The kinds of layers which occur for four or five miles are brought into this sketch. One deposit of bituminous coal, four feet thick, is now opened. It yields coal of a tolerable quality now, and its value increases the farther it is explored.

Fig. 4.



a Sandstone.—*b* Shale containing small bivalve shells.—*c* Coal.—*d* Sandstone and Lignite.—*e* Nodular argillaceous Iron.—*f* Argillaceous Iron with Lignites.—*g* Thin layers of coal.—*s* Shale.

Fossils.—The fossils are mostly of vegetable origin. They are generally casts, or substitutions of sandstone for the vegetable matter, the latter having been removed without leaving a trace of vegetable structure, except the external markings. Those which we consider the most remarkable at the South Joggins, were large trunks of what might be called trees, belonging to the order *sigillariæ.* We saw several of these trunks, some lying in a horizontal position, others erect, or rather they pierce the strata at right angles. They vary in size from eighteen to thirty inches in diameter. The one we had an opportunity to examine carefully, was about eighteen inches in diameter, exclusive of a thick carbonaceous bark. Beneath the bark, it is superficially grooved, and resembles slightly a fluted column. The carbonization of the bark in this instance and in other similar ones, does not appear to have been produced by exposure to heat, but it is rather a chemical process, more like the action of sulphuric acid on wood, or it may be the result of long immersion in water. The lower portion of that trunk, which was at right angles to the strata, was broken away; there were no appearances of roots, hence the idea, that it grew here, may be doubted. The sandstone was deposited around it where it was vertical, but whether it was a living or a dead tree, or whether its trunk was thrown into and fixed in a vertical position as some are now, when washed from a bank and are floating down a river, cannot now be determined, but the circumstances of the case would lead most observers to the conclusion, that it might have grown on or near the place it now occupies.

The most common vegetable relics of the Joggins are Lepidodendra Calamites and Cactæ. The first are confined mostly to beds of slate, yet the matter of substitution is a fine sandstone, while the Calamites are generally found in the sandstone and are frequently erect; some of them are short conical stems which appear to have been young plants whose growth was arrested before they came to maturity. The Cactæ are rare.

Having completed our collections at the Joggins we bent our course in a homeward direction. The first anchorage was at Grindstone Island, at the head of Chepody Bay. It belongs to New Brunswick. It is an inconsiderable island, but its quarries are valuable and inexhaustible, and the grindstones are of a superior quality. We found here the same species of vegetable relics as we had just obtained at Cumberland Bay. We were however fortunate in discovering several fossil trees belonging to the dicotyledonous order of vegetables. One of these trunks may yet be seen on the west side of the island. It lies in the face of a cliff of sandstone which is about fifty feet high. It reposes obliquely in the strata in a kind of trough formed by the contortion of the layers. It is about twenty-five feet above the base of the cliff. This noble relic of ancient vegetation is uncovered for about forty feet, and probably twenty feet towards its base is yet concealed in the rock and debris. There are some obscure appearances of branches near its small extremity, so that when these branches waved in the breezes of gone by ages, its trunk was full sixty feet in height. This trunk still preserves a conical shape though in places it is flattened by compression. Its annual layers, which are distinct, are of unequal thickness but this inequality is more evident when observed in the compressed portions and it may be possible that a farther examination of different parts of it will prove that the greater thicknes of some layers is the result of compression, especially as the thickest ones are near the outside. It is the opinion of some, if not all geological writers, that the vegetables of the coal formation enjoyed an equality of the seasons, or at this period there was an equality or uniformity of temperature and moisture, &c. which favored an equality of vegetable growth, or that the seasons were similar to those of tropical climates. It is well known that the vegetables of that climate increase equally every year, while those of temperate climates are influenced in their growth by the temperature. In a cold season the annual layer is thinner than when the season has been warm and moist. The diameter of this trunk near its middle is about twelve inches, but many layers have probably been removed in the process of decay, as its outside is formed or has passed into a loose friable oxide of iron so tender, that it may be brushed off with the least touch; the diameter evidently increases downward towards its base. Color brown. Specific gravity=2·87. Effervesces in some places with acids. It is probably composed of carbonate of lime, oxide of iron and other carbonates. Narrow seams of calcarous spar traverse it in various directions. There are numerous cavities running longitudinally or in the direction of its fibres and which are partially intersected by transverse portions; these cvaities are lined by crystals of calcareous spar. It is mineralized, as has already been remarked by carbonate of lime and the oxide of iron, a fact somewhat inexplicable, and unexpected, for it would be more likely, we should think, to be mineralized by silex as it is imbedded in rock composed principally of siliceous particles. The examination of this relic has led us to imagine that it was in a decayed state when the process of mineralization commenced, and that it was in a state similar to the dead and decayed hemlock, so common in the open fields of New England. The question most interesting in regard to this fossil is, to what order of plants does it belong? To determine this, I have prepared sections of it agreeably to the method of Messrs. Witham and Nicol. These sections, it is generally known, are thin slices ground and polished and cemented to glass by Canada Balsam. They must of course be sufficiently thin to transmit light. A transverse section of this tree prepared as above stated, is represented in Fig. 5. This section is seen by transmitted light, and is magni-

Fig. 5.

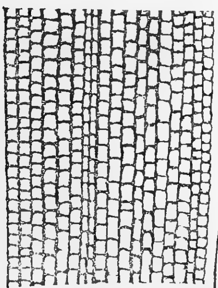
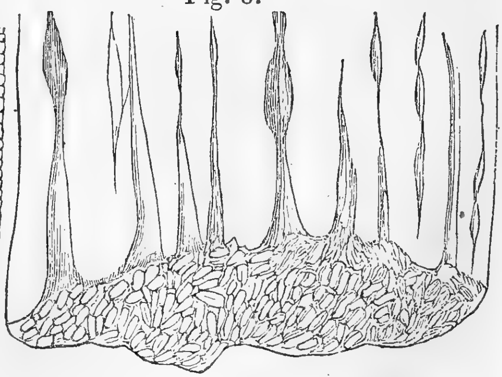


Fig. 6.



fied fifty times. On comparing its structure with the fossils of which Mr. Witham has given figures in his work on vegetable fossils. I find that it very nearly resembles in its structure the wide, open tree, which belongs to the same formation as this. As sufficient advances has not as yet been made in vegetable anatomy to distinguish species by their anatomical structure, it would be premature to attempt to refer vegetable fossils to particular species. So much as this, however, may be known of the ordinal relations of a plant. In respect to this one under consideration, we do not hesitate to pronounce it a *coniferous plant.* The woody structure is discernible by the naked eye, especially when the section is held between the eye and a strong light. It is remarked by Mr. Witham, and the same fact has been noticed by myself, that the texture of fossils is coarser than that of vegetables of the present day, a fact which proves in part, that the climate in which they grew, was at least milder than the present temperate zones. Fig. 6, is an outline of a vegetable fossil I obtained at South Joggins. It was taken from the shale and appeared to have been flattened by pressure, but a careful examination will convince any one that this was its natural shape. This relic belongs to the monocotyledonous order of vegetables, for the following reasons; 1st, it has no central axis or pith; 2d, it is destitute of medullary rays; 3d, it has no bark. The exterior is black, and when the shale is entirely removed, shining. The end which had been exposed to the weather exhibits a dark portion penetrating it in various directions, and another somewhat fibrous in appearance and which run through it longitudinally; the first shows no organic structure. A transverse section magnified about four times is shown in Fig. 7. In the figure both of the portions spoken of are seen. Fig. 8, is a longitudinal section magnified with the same power. It would be difficult to distinguish this stem from a Lepidodendron by the microscope simply; but the latter has a bark and pith, while this has neither; it however resembles the Lepidodendron in being destitute of medullary rays. Fig. 9, is a transverse section of the same stem magnified fifty times. This stem is mineralized by the oxide of iron mixed with a large quantity of carbon. Specific gravity 2·53. The vessels are generally of a polygonal shape and quite regular in this respect, throughout the whole stem. Wishing to pursue this subject one step farther, I prepared a section of fossil wood from the greensand of New Jersey. It is represented in Fig. 10, magnified fifty times. That it belongs to the dicotyledonous order of vegetables cannot be doubted, and it does not appear perfectly like a coniferous plant. It has medullary rays and distinct anual layers. Its structure as seen under the microscope shows it to be more allied to the

Fig. 7.

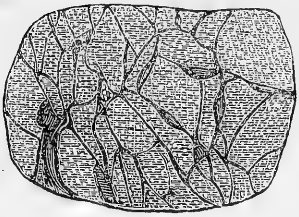
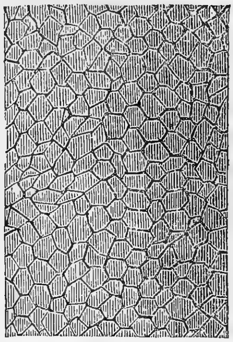


Fig. 9.



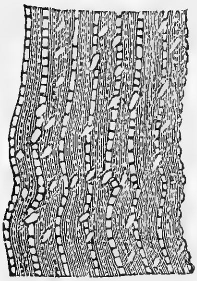


Fig. 10.

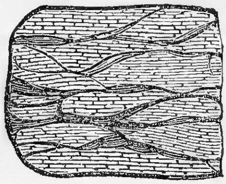


Fig. 8.

oak than the pine family. Its vessels have two sizes, and throughout the whole surface of the section, there are numerous oval spots which appear to have been *lacunæ* for the lodgment of concrete juices. The texture of the wood of this plant is finer than that represented in Fig. 5. Whether this is owing to a cold climate is uncertain. The pieces from which the section was taken is small, or a mere fragment, and probably belonged to an earlier period than the greensand. It shows the marks of the borings of a small species of Teredo.

In concluding our remarks on vegetable fossils we would say that it is quite satisfactory to know that the study of this branch of Botany has brought to light the existence of what are usually called the higher order of vegetables in the ancient formations. Taking this and other facts into consideration, such as the existence of Birds as early at least as the new red sandstone, can we doubt that even then the state of the globe was such, that it might have been inhabited by man. That warm blooded animals of the mammiferous tribe did exist, may not be proved for a great time by the discovery of their remains in the ancient strata; but it ought to be remembered that where one relic is preserved, hundreds must have perished under circumstances unfavorable to the preservation of their hard parts.

On leaving Grindstone Island, our homeward course still lay along the shores of New Brunswick. These shores are steep, but slope to the bay, they are not a bare perpendicular ledge of rocks like the opsite coast of Nova Scotia. There were only two places which circumstances permitted us to examine, and at one of these we could not land. The first I shall mention, and which we examined with some care, is near St. Martin's Head. The minerals we observed were asbestus, serpentine and indifferent specimens of schorl. The predominant rock is talcose slate, against which, we found the remains of a stratum of the old red sandstone reposing; remains which have as yet resisted the warring elements. A conical shaped mass about forty feet high, and twenty or thirty feet across it, where it plunges into the sea, is one of the largest of the remains of this stratum yet remaining *insitu.*

The shore is strewed with boulders of primitive rocks and a conglomerate, which may have belonged to the new red sandstone. Some of these were evidently members of the graywacke formation. The texture of this sandstone is coarse and harsh, is made up of angular particles which adhere firmly; it is tough and not easily broken. We proceeded twenty miles farther and came to anchor near Quaco. This place appeared still to be underlaid by talcose slate. A thin stratum of sandstone may still be seen resting unconformably on it. The light house which is built on a tongue of land running several rods into the bay, is founded immediately on the ruins of this rock yet in place, and still exhibiting its stratification; the diameter of these ruins is not much greater than the base of the lighthouse and about ten feet thick. The stratum dips to the east at an angle of 20° or 25°. These few remarks on the geological structure of the coast of New Brunswick are not made with a view to settle the interesting points on which they bear, but to call the attention of future observers to them. It would be interesting to know what and how great have been the changes on the coasts of New Brunswick and Nova Scotia in comparatively modern times.

From Quaco we proceeded directly across the bay to Digby. It is situated on the Annapolis basin, its location is pleasant and the people are kind and hospitable, as we found all Nova Scotians to be: but it is inconsiderable in point of size and population. Here we renewed our acquaintance with the trap formation, but as nothing new or peculiar came under our notice, we shall drop for the present our geological remarks, and take up the mineralogical part of our journal. In doing this, we shall commence at Peter's Point, the place on which we first landed.

Peter's Point.—This locality will furnish the collector of minerals with *Laumonite, Thomsonite, Apophyllite, Mesotype* and *Heulandite.*

The Laumonite is the most abundant. Its colors are white and flesh red. The crystals are small and unmodified. They occur in compound masses, the individuals of which radiate from a common centre, forming short stellated groups similar to stilbite. They may be distinguished from the latter by the terminating planes, (as they appear to be in the masses,) which stand in the Laumonite obliquely to the lateral edges. Many large cavities, and even deep recesses may be found lined with Laumonite in a good state of preservation. Peter's Point is the best place to lay in a stock of Laumonite, although it may be found very frequently at other localities. It is however, more abundant here, and in a better state for keeping than elsewhere.

Apophyllite occurs both massive and crystallized. The former variety is greenish and might be mistaken for phosphate of lime. The faces of fracture are generally striated parallel to the faces of composition. The crystallized variety presents the primitive form of the species modified by replacements of the solid angles by single triangular planes similar to Fig. 39, Shepard's Mineralogy, p. 37, also, in thin tables similar to Fig. 42 of the same work. The latter intersect each other in all directions and stand upon their narrow edges with the plane P, vertical.

The size of those occurring under their primary form, varies from one eighth of an inch to an inch and a half. They are slightly tinged red, cleave easily parallel to the plane P, and occur in groups of different sizes, and which intersect each other in the manner of crystals of Chabasie.

The collector will do well to lay in a full supply of this rare and interesting mineral while at this locality; for so far as our experience goes, it is not so abundant nor so fine at any other place.

Thomsonite.—This substance is found in long slender prisms of a grayish white color in compound forms, the individuals of which radiate from a common centre. Their extreme length is about four inches.

Mesotype.—Like the former mineral, it occurs in compound radiating masses of extremely thin individuals, more so in the last respect than Thomsonite; they are not so long, and their external portions are weathered and partially decomposed, and often pass into a variety of snow white asbestus. These snow white masses were not seen to occur at any other locality, but are abundant at this place. A broken mass of this substance presents numerous points from which individuals diverge, so that in a single mass there is no common centre of radiation.

Heulandite.—This fine mineral is not so abundant at this locality as those already mentioned. It is, however, frequently present in the cavities of amygdaloid. Colors both white and flesh red. The size of the crystals is good, varying from a quarter to three fourths of an inch in the longest diagonal. Their beautiful pearly lustre will always distinguish it from every other mineral. In some respects the most interesting variety of Heulandite at this locality occurs in long cylindrical cavities in the amygdaloid. These cylinders are often about the size of a pipe stem, sometimes larger, not smooth but beset with projecting points. It is usually the case, that cavities formed in partially fused masses are oval or almond shaped. May not cylindrical cavities be produced by currents of air forced through the oval cavities in a continued stream, while the rock is in a yielding state.

The vesicular structure of amygdaloid is undoubtedly produced by confined aerial fluid, and if this pent up air or vapor could find a vent in one direction, the flowing of it in that direction might convert all the vesicles into one contiuous cavity.

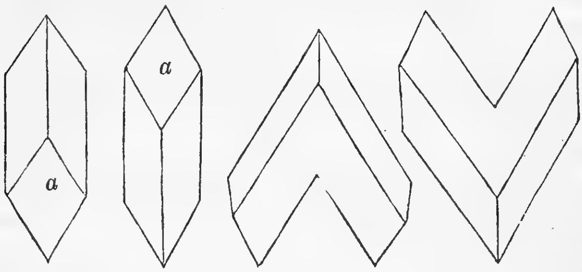
Patridge Island, about one fourth of a mile from Parsboro, and to which it is joined by a narrow ridge of sand, furnishes at present but few minerals. We were informed by Dr. Gesner, an industrious and successful cultivator of Nat. Science, that minerals, which a few years since, were abundant, have now become scarce. The truth seems to be that from its proximity to the village, this enchanting little spot is much exposed to visits, hence every thing which falls from the cliffs is soon carried away. We were unable to obtain more than two minerals in any tolerable quantity, these were stilbite and calcareous spar. Chalcedony and a decomposed variety of it, usually called cacholong, and a coarse red jasper, are common to this, and most every other locality where trap rocks occur.

The Stilbite forms an almost perpendicular vein in the amygdaloid and greenstone three or four inches thick, and extends up the cliff from thirty to fifty feet. Colors white and flesh red. It occurs in its usual form of compound masses of individuals radiating from a common centre. Intermixed is the wine colored carbonate of lime. The specimens are exceeding rich and vie in beauty with minerals of the same kind from the deep mines of Europe.

Calcareous Spar.—The more common forms of this substance, are the *primary*, the *chaux carbonateé inverse* of Haüy, and the *metastatique*. The *inverse*, somewhat elongated, forms a remarkable variety of intersected or hemitropic crystals.

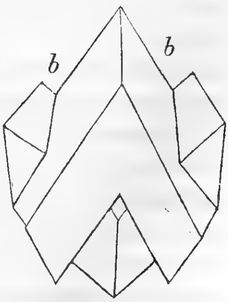
Figs. 1 and 2, are the most common of the simple forms. Occasionally, however, they form hemitropes. The manner in which these are found, may be understood supposing the faces *a*, *a*, joined in composition with their similar edges parallel, or rather forming one continuous line. Now if these crystals thus applied, are made to perform an angle of revolution equal to 180°, the hemitropes as Figs. 3 and 4, will be produced. This is one kind, and is of com-

Figs. 1 and 2. Figs. 3 and 4.



mon occurrence, but it more frequently happens that two of these hemitropes are joined or connected together in composition, and form by this union the remarkable *intersected* or double hemitropes as in Fig. 5. Often the last described hemitropes become more complex by embracing another crystal within the angle *b*, *b*.

Fig. 5.



All the forms and varieties mentioned above, are found frequently in one specimen.

Patridge Island, is the only locality which furnishes these peculiar and interesting forms. It ought to be remarked, however, that at Cape Sharp, the same may be found, as fine specimens of wine colored spar, are known to occur there. We were prevented visiting the latter place by the state of the tide, at the only time when it lay in our power to examine into its productions.

Sprinkled over the surfaces of the crystals of this place, is a curious variety of stilbite, it is much like a reddish sand. It is evidently a more recent formation than those minerals on which it is encrusted, and possibly it is now deposited from the waters which percolate through the fissures and cavities of the rock.

Cape Blomidon, is fifteen miles from Parsboro. It is the termination of the North Mountains on the east, is quite abrupt and rises to the height of about four hundred feet. Along the shore for the distance of many miles, the new red sandstone is conspicuous with its parts arranged in regular layers, which to the eye appear almost horizontal. It slopes off moderately to the south west and passes under the trap. Its thickness between low water mark and the line of junction with the amygdaloid, cannot be far from three hundred feet. The termination of this range of mountains, is crested with greenstone, which rises full one hundred feet above the sandstone.

We sought diligently for organized remains in the sandstone, but were unsuccessful. Messrs. Jackson and Alger discovered casts of culminiferous plants in an highly carbonized state. Veins of selenite composed of thin or slender individuals are common, traversing the body of the sandstone, but seem to be more in place near the line of junction of the sandstone with the amygdaloid. The collector may supply himself with the *foliated, fibrous* and granular varieties of selenite. Colors generally snow white, but the fibrous is often flesh red and bent, as if one side of the vein had been raised since the deposit of the mineral.

Along the coast in the neighborhood of the Cape, are minerals more interesting than the above, as the whole genus of the kouphone spar may be gathered, and besides these, the crested summit will furnish *amethyst, agatized hornstone, coarse heliotrope, imperfect bloodstone* and numerous varieties of agate, besides cacholong, calcedony and fine jasper of different colors. Probably a diligent search would bring to light the whole quartz family in great perfection, more particularly the genus uncleavable quartz. The more interesting species of kouphone spar, are apophyllite in tabular crystals. Heulandite in crystals nearly an inch and a half in length, also in minute crystals associated with calcareous spar, laumonite, chabasie, analcime and stilbite.

Two Islands, are situated four miles N. E. of the village of Parsboro. They are composed of amygdaloid and greenstone, and beneath, is the red sandstone which just appears at low water. At one of the Islands, there is a splendid arch way which has been formed by the action of the tide on the porous amygdaloid. The minerals which may be sought for, are needlestone in snow white and brownish implanted crystals of the lustre of satin, analcime and heulandite colored greenish gray by some foreign substance. The heulandite is modified by replacement of the acute lateral edges, and the obtuse solid angles. Generally both modifications are combined in the same crystal, the replacement of the acute lateral edge, is often so deep that the crystal becomes a six sided prism.

Swan's Creek in the immediate neighborhood of Two Islands, furnishes fine needlestone in white crystals, and also a variety which is hair brown in extremely thin individuals. It is sometimes called *hairstone.* The base of those compound masses is generally analcime, and needlestone stands out from all the spaces between the crystals like small mats of bristles.

Chabasie.—Three circumstances combined render it necessary to speak particularly of the mineral; 1st, its abundance; 2d, the occasional size of the crystals; and 3d, the modifications which they present. Chabasie seems to take the place of stilbite, and the other zeolites in this neighborhood. Although the crystals are generally small, yet occasionally they may be found in the debris from one half to an inch in diameter.

The following are some of the modifications of the primary form, which may be obtained here. 1st, the primary with two adjacent solid angles replaced by single tangent planes. 2d, the primary having its upper edges and lateral angles replaced by tangent planes. 3d, the primary with its superior edges replaced by two planes producing a dodecahedron with isosceles triangular planes. The latter solid is occasionally found complete. The manner in which crystals of chabasie are grouped or clustered, adds much to the difficulty of studying the modifications of the primary form.

Siliceous Sinter occurs in balls or ovoid masses, generally white, but frequently purple. The interior is often lined by chabasie and small dodecahedral crystals of carbonate of lime.

About two miles from Swan's Creek there is a locality of tolerable good pyroxene in the usual form of six sided prisms; on the surface of these crystals and in the interstices, is a small reddish mineral in the form of an octahedron with a rhombic base. We were unable to visit the place, but from what we saw in Dr. Gesner's cabinet, we feel confident that future travelers will do well to go there.

Spencer's Island.—Situated at the entrance of the Bay of Fundy into the *Basin of Mines* is, to appearance a barren island and destitute of interest to the mineralogist. But a few hours spent here, will not be lost time. It is composed of columnar trap. It has not been raised sufficiently high to bring up the amygdaloid. The island furnishes agates of different varieties, and abundance of fine siliceous sinter, *white*, *gray* and *purple*. The sinter occurs frequently in solid balls, presenting on fracture, layers like the coats of an onion. When the balls are hollow, very small crystals of chabasie are found lining them. Frequently, the cavities are studded with quartz crystals under *the primary form*. Sometimes they have suffered a modification by a replacement of the lateral angles by a single plane parallel to the axes. The surfaces of these crystals are always dull, like ground glass, but the fracture is highly vitreous. The dullness was probably produced by the subsequent action of the solvent from which the crystals were deposited.

Cape D'Or furnishes the usual zeolotes, particularly a beautiful pearly variety of apophyllite. This place is visited chiefly for its masses of *native copper*, but we regret to say that it is exceeding scarce, and obtained with difficulty.

Digby.—The neighborhood of Digby will be found as interesting perhaps, as any other section of country along the Bay of Fundy, but as we have spoken with sufficient minuteness already, of the family of minerals peculiar to this formation, we shall finish what we have to say, by a mere reference to a few more localities.

At Nichols Mountain, four miles from Digby, there is a fine locality of protoxide of iron under the form of a regular octahedron. It is associated with amethyst of a good color, which lines the interstices of the ore in geodes. At Gulliver's Hole, two miles farther, agate of a very superior kind, is quite abundant. Sandy Cove, twenty miles from Digby, on St. Mary's Bay, furnishes good laumonite, but other minerals of the zeolite family appear to be exhausted. The greenstone at this place is worth a critical examination. It is columnar, and the columns are four, five and six feet in diameter, or such is the distance between the lines marking the exterior of the columns. The structure is sometimes concretionary;

the same structure may be seen at the Digby light house. The fusion seems not to have been sufficient to produce a freedom of motion among the particles of composition. It is well known that a concretionary structure is sometimes given to hearth stones, which have long been subjected to heat. So far as this fact goes, it may be applied as an explanation of the kind of structure here spoken of. Different degrees of heat, we believe, we are warranted in saying, have acted on the greenstone of Nova Scotia. Sometimes it is perfectly columnar, in masses composed of separate columns piled up one above another; again, the same structure is distinct, but there is no separation of the mass into separate columns, there is still an adherence of their sides. It would be interesting to describe all the appearances the trap assumes, but the field is too wide for us at present. We wish others to go and see for themselves. The formation is on a large scale,—it is grandly exposed and laid open to view,—frequently the whole thickness of the amygdaloid and greenstone, is raised up for inspection, and every year they scatter their treasures profusely on the shore. It is fitted, therefore, by nature to instruct and enlighten, and to open to us the great mysteries which shroud the origin of these rocks. In conclusion, we ask the indulgence of the reader, while we make a few remarks on two or three topics which have been briefly alluded to or connected with subjects discussed in the preceding pages.

1st. Geologists as a body, seem to have overlooked too much, those causes now in operation, which tend to modify the surface of the globe. It may never be proved absolutely, that the earth has been brought into its present state by the slow operations of agents now at work, although these very agents working at their present slow rate, have done much to alter the earth's surface. But does it contradict either experience or observation, or both, to assert, that a given agent accomplished more formerly than now, although it operated with the same energy only. If an extensive island should be thrown up from the deep, would not the first rain which fell upon it, wash down more sediment to the sea than the same quantity of falling water afterwards. May we not suppose that the upper layers of rocks were softer and more easily abraded than the deeper ones? if so, they must have been more deeply worn and furrowed soon after their elevation than in these days. If the reasoning is correct, then we have furnished a greater quantity of detritus in early times for the formation of transition, secondary and tertiary deposits. With materials thus abundant, (if the position is admitted,) rocks might rapidly form of great thickness, a thickness, which if estimated on the slow progress of increase at the present day, would require vast periods. The only objection which we can see to this reasoning is, that continents were raised gradually, or by successive throes, as it were, from the bosom of the deep. Hence, the surface which would be exposed to abrasion, at any one time could not furnish materials for a single stratum. As yet, this point is left for decision to future discoveries, although there are many facts in its favor.

2d. We are gratified, to have it in our power to add to the evidence, that many dicotyledonous vegetables existed as early at least, as the coal formation which rests on the mountain limestone, and also that on this side the Atlantic, there is much reason to suppose they are equally numerous as on the other. This fact breaks in upon the *harmonious order of creation*, as stated by many geologists of the present day, viz., "that the acotyledonous tribe of vegetables first appear in the oldest rocks, then follow the monocotyledonous plants, and that the dicotyledonous were not created until all the rocky strata were deposited." Discoveries, however, establish so much as this, that the number of acotyledonous and monocotyledonous plants, predominated in early times; there is this to be considered, however, and it is a draw back on the discoveries, that the ferns, reeds, &c. grew probably, in marshy places, and under circumstances better fitted to ensure their preservation than the higher order of plants.

The success which has followed investigations in fossil botany, may also follow in fossil zoology, in that department which relates to mammiferous quadrupeds. If it is right to reason or speculate on the existence of these animals independent of geological facts, we should be disposed to maintain the affirmative. It seems more agreeable to the harmonious order of creation which we know now exists. That numerous tribes of animals should have been brought into existence *at the same time*, that these tribes and families taken as a whole, should form a complete series, and the different orders and genera form a lineal succession, would be more agreeable to the present order of things, and also that they should be formed on different types or models, would be another point in which we have reason to expect an agreement. Analogy, then, lends its support to the view here taken. But an objector says the earth was not in a settled state. When alligators and crocodiles were the lords of creation, nought prevailed but a wide muddy waste. It is admitted, however, indirectly, if not directly, by all geologists, that there were bays, estuaries and rivers ; the latter must have drained districts more or less extensive. There were vegetables too, and those of a large size, which we have reason to suppose grew on the dry land. It seems more agreeable then, to what we know of the wisdom and power of the CREATOR, that single, solitary races of animals were not created at different times and at different intervals. It seems better, that the waters should teem with numerous orders of fish—that the air should be filled with feathered tribes and winged insects—and that the *earth* too, should be made the abode of beasts and creeping things.

Admitting that mammiferous animals existed as early as shell fish and crocodiles, are there any reasons for the absence of their remains in the same deposits ? There are, in the first place, two reasons why *shell fish* and the other lower orders are abundant in some deposits, viz. They were numerous from the first, and they lived in a medium which would insure their preservation. Again, these two reasons why mammiferous quadrupeds *should not* be found in the older strata, viz.: their probable unfrequency and their modes of life. Whether more than a single pair were created, is a question we cannot settle. This we know, that their habits generally, lead them instinctively to shun the water, and it appears probable, that when, under the necessity of resorting to it, the instances of their perishing would be rare. Mammiferous animals would die in the open fields or on the woody hills and their bodies would decay, and the disjointed limbs would be separated to the four winds, and their bones would whiten and crumble to earth, without leaving a trace of their existence. Not so with shell fish, crocodiles and amphibious animals ; they lived in a medium the best calculated to preserve their bodies whole after death—they would be surrounded soon with mud and gravel, the basis of all fragmentary rock, and when in process of time the strata were to be raised from the deep, their remains would be found locked up in the slates and marbles. But how fortunate is that geologist who is able to enrich his cabinet with an ancient crocodile, numerous as they were. Why need he wonder then, that mammiferous quadrupeds are not yet discovered in the older strata ?

3d. There is an inconsistency in the doctrine of those geologists who maintain that God created all things in six days of twenty four hours each, and in proof of it appeal to the supposed order of vegetable and animal relics in the rocky strata. Cryptogamous plants and the lowest orders of animals, say they, are found in the transition and oldest secondary rocks, but the bones of quadrupeds are found only near the surface in the last deposits; and this they maintain is agreeable to the order of Moses, and the fact is cited as furnishing a striking coincidence between scripture and science—between revelation and modern discoveries. But can there be much in this—is it possible that twenty four hours' difference in the ages of two animals should make such a wide difference in the disposition of their remains—that the reptile which is only a few hours older than the beasts of the field, the remains of the first should be entombed thousands of feet below the last—that cryptogamous plants only a few days older than the coniferæ, should be found so much deeper in the bowels of the earth?

Art. X.—*On two American Species of the Genus Hydrachna;* by James D. Dana and James Whelpley.

Read before the Yale Nat. Hist. Soc., May 5, 1836.

The Hydrachnæ, or *water spiders,* which are here described, were found as parasites on several species of Unionidæ (fresh water clams) collected in the canal near this city.* They usually occur on the branchial leaflets within the mantle of these animals, where they live, extracting their nutriment by means of a sucker-like mouth.

The examinations of the species of this genus, seem as yet, to have been hardly sufficient in number or accuracy for an exact determination of generic characters. In the following description, it is therefore thought advisable, to state first the characters which these species possess in common, without reference to the generic distinctions heretofore proposed; and afterwards, their *distinguishing* peculiarities.

The body of these animals is enveloped in a coriaceous membrane extending undivided over the head, thorax and abdomen.

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* New Haven.

Beneath the membrane in front, are situated two distant, dark brown eyes, visible only from above. By a close microscopic examination, these eyes are observed to be composed of two concurrent eyelets,* of which the anterior is the larger. They are capable of a slight and quite peculiar motion to and from the shell.

Beyond the anterior margin of the body, between the palpi, extends a beaked projection which is connected with the organs of manducation beneath. These organs are situated on a broad and somewhat triangularly shaped pedicel which is capable of a slight lateral motion independent of the body. From two sockets on each side on its front margin arise the palpi (Fig. 3.), between which are observed two short rounded projections (*e*, *e*,) which have been considered a bifid linguette. Immediately below projects a rostriform mouth, (*f*).

The *palpi* are quinqui-articulate and usually arcuately inflexed, the terminal joints being bent downwards. Their general form is obclavate. The *basal* joint is small, short and cylindrical. The *second* is laterally compressed. In a perpendicular view, it appears to increase with a curve from its base to the apex, where the antennæ, as seen in this view, attain their maximum diameter. When amputated and placed on its side, (Fig. 6.) it exhibits a breadth about equal to its length and more than double that of the succeeding joint. Two or three setæ are observed on both the interior and exterior sides. The *third* is a short subcylindrical joint, provided with a long seta near its outer base and another on its inner apex. The *fourth* is more than double the preceding in length. On its inferior surface arise three or four setæ. The *fifth*† is a movable corneous digit, bearing at its apex two minute hooks, one of which only can be seen in a vertical view, and this with difficulty, unless the antennæ are separated from the body.

The rostriform *mouth* is inserted nearly at right angles with the body, and is curved forward, (Fig. 4.) This rostrum is composed of two laminæ, which form a sheath to an exsertile ligula. The laminæ when protruded separate and exhibit a lancet-like form, (Fig. 5.)

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* A reexamination of the species heretofore described, would probably prove that the eyes are in all instances double; the eyelets being separate when there are four eyes, and concurrent when apparently but two.

† This has been considered an appendage to the preceding; as it is itself however terminated with appendages, it is here recognized as an independent joint.

The *legs* have six joints, exclusive of the basal or coxal, which is immovable, and are covered with long, transparent, setaceous hairs. The fourth or posterior pair is the longest, being one and one fourth the length of the body; the second and third about equal the body in length, and the first is three fourths of the same. The first joint of the six anterior legs is very short, (Fig. 2, *d.*); the fourth and fifth in each are the longest. The immovable basal joint of the fourth pair is expanded into a broad lateral breast plate, and is united by a suture with the corresponding joint of the third pair. The last joint or tarsus in each is bifid at its apex and terminated by two retractile claws, articulated with the inferior extremity of the foot.

The genitalia are situated at the inferior extremity of the body. In the *female* there appear externally two conchoidal plates, (Fig. 2, *b, b,*) with their intermediate edges, or those by which they unite, elevated in the form of a ridge. The extension of this ridge forms a short tail which is terminated by four setæ—two on each side. These laminæ open laterally. In the *male* the body is terminated by a broad oval plate, conforming nearly to the curvature of the abdomen, and having an elevation near its center.

The colored internal organs of the body are visible through the transparent membrane covering the body; but as these organs are mostly opaque their true nature cannot with certainty be determined. Without however applying names they may be described to be, 1, a broad white or yellowish white, central abdominal vessel, (Fig. 1, *e.*) extending from a point anterior to the middle of the body to the apex of the abdomen, where there is a connection with the anus; 2, a brown or brownish black vessel, anterior to the commencement of the preceding, (Fig. 1, *f.*) extending nearly to the line between the eyes, whence it is apparently continued colorless to the mouth; 3, two lateral abdominal vessels which appear to be united with the preceding, and lie along side of and partially overlap the central vessel or organ first described, (Fig. 1, *g, g.*) The abdominal vessels are also visible below.

To this *partial* overlapping of the white central abdominal vessel by the two lateral, is owing the appearance of a narrow, irregular white line along the centre of the abdomen, more or less forked in front. The lateral vessels are sometimes so extended as to conceal the central one, and consequently the medial white line is then interrupted.

The interior organs fill but partially the coriaceous covering of the body, and the usual appearance presented, is that of a small abdomen within an enveloping shell. From the extremity of the inner abdomen a duct is visible passing to the anus; others proceed from the same point to the exterior margin of the plate, (Fig. 1, *b.*) described as terminating the body where there are small holes (Fig. 2, *c, c.*) which are the *stigmata*. The fæces of these animals are white.

The space between the coriaceous covering of the venter and the internal organs above described, is occupied in the gravid female, either in part or entirely, with ovary sacs. The largest have an oval form, an amber yellow color, and a length equal to about one fourth the breadth of the abdomen. The smallest are white and spherical. Between the two extremes of size, there are corresponding variations in the shades of color. When extracted from the body they appear slightly translucent.

The young animal shortly after birth has somewhat the appearance of a transparent globe with a brownish nucleus. It has only the three anterior pairs of legs. The palpi project at one extremity in the form of a beak, and a short distance back on each side, there are two concurrent eyelets. The animal in this state scarcely moves itself, when taken from the branchiæ of the clam in which it lies partially imbedded. When more developed its back is of a dirty brown color, the central white line not being apparent.

The following are the two species from which the above description has been derived.

Hydrachna formosa.

Body oval, in length twice its breadth; slightly the broadest posteriorly; irregularly punctate; frontal parts and lateral edges yellowish, translucent; marked on its back with a forked line somewhat resembling the letter Y; extremity of the abdomen white above; dark internal vessels varying from a light to a dark chesnut-brown. *Palpi* and *legs* diaphanous, light green. When the palpi are flexed in their natural position, a spine proceeds *apparently* from the extremity of the fourth joint exterior to the fifth; this spine is one of the four that project from the inferior surface of the fourth joint. *Legs* covered with long setaceous hairs (the tarsal joint of the three anterior pairs excepted); diameter of the tarsus, nearly one half that of the basal joint, greatest at its apex. *Claws* doubly hooked; hooks equal. (Fig. 7.)

Length of the male, including the palpi $\frac{1}{20}$—$\frac{1}{25}$ of an inch; of female $\frac{1}{16}$—$\frac{1}{20}$ of an inch. Found most abundant in the *Anodonta cataracta*, in one specimen of which there were forty four individuals; also in the *Unio purpurata*. Collected in the course of April. The females usually contained numerous ovary sacs.

To the unaided eye this animal has a considerable resemblance to a minute Coleopterous insect, the longitudinal white line appearing to mark the separation of the elytra.

H. pyriformis.

Body, broad ovate, sub-pyriform, sub-globose posteriorly, breadth across the abdomen, nearly twice that in a line with the eyes; medial abdominal line, white, scarcely forked, but broader anteriorly; dark internal vessels black. *Palpi* diaphanous: when inflexed, apparently terminated by three spines of which the corneous fifth joint is the central one; the lateral are two out of the four which proceed from the inferior surface of the fourth joint. *Legs*, diaphanous, covered with setaceous hairs; tarsus of each leg slightly hairy; one third the diameter of the basal joint, smallest one fourth the distance from the apex. *Claws* simple, small.

Length $\frac{1}{20}$ of an inch. Found in the Alasmodonta undulata, during the month of April. Fig. 9, is an outline of the body.

Many points which should be noticed in the description of the species, are probably described in the preceding remarks. The reason for this arrangement, has already been stated.

These species, appear better fitted for their peculiar parasitic habits, than for the life of animalculæ hunters, which Müller ascribed to the species he examined. Provided with long slender legs unfit for natation, they sink almost immediately, when brought to the surface of the water, notwithstanding a rapid motion of these organs, and are only able to make their way along the bottom of the vessel that contains them. In every part of their structure, they appear to be destined to lead the life of prowling adventurers, roving over the bottom of streams for prey, and entering the habitation of any unfortunate clam that may be open to them. Under the mantle which covers the body of the clam, they find a snug retreat, where they may revel in all the luxuries of which their sensual appetites are capable. The mouth being composed of a pair of lancet-like blades, they are enabled to prepare a place for this minute and delicate organ, previous to inserting it.

It is not impossible that these animals feed also on animalculæ, if unable to obtain what may be a more delicious morsel to them, the body of the clam. Indeed this is quite probable, as we have kept several of them nearly two months, in a cup of water, and they still retain all their usual activity, and appear really to have fattened on the animalculæ which the stagnated water has afforded them. In this state of confinement, they show no disposition to molest one another, but are continually occupied in walking, with occasional attempts to swim, along the bottom and sides of the cup. In walking they rely principally on their second and third pair of legs, occasionally using however, the others.

Explanation of the plate.

Fig. 1. Back view of female. *a*, palpi. From *b* to the extremity of the inner abdomen, appear lines which represent the vessels connected with the stigmata below, (fig. 2, *c*, *c*.)

Fig. 2. Under view of the same. *c*, *c*, stigmata.

Fig. 3. Under view of the palpi. *e*, *e*, two prominences forming the bifid linguette of authors. *f*, sucker, situated nearly at right angles with the body.

Fig. 4. Profile view of the sucker.

Fig. 5. The same protruded.

Fig. 6. Side view of the palpi.

Fig. 7. Tarsus of a posterior leg, exhibiting the hooks which terminate the same joint of each pair of legs.

Fig. 8. Length of female and male.

Fig. 9. Outline of the body of H. pyriformis.

Art. XI.—*On the Resistance of Fluids, in reply to Professor Keely, with remarks on the measure of Mechanical Power;* by Eli W. Blake.

TO PROFESSOR SILLIMAN.

Prof. Keely, in the last number of the Journal of Science, has amused your readers with some strictures on my communication to the preceding number, on the resistance of fluids. His arguments in these strictures are founded, so far as they have any foundation, on a misapprehension of my meaning. I solicit therefore the favor of a page or two in your ensuing number to correct this misapprehension, and to urge still further the general views which it was my principal object in that communication to express.

In penning the article referred to, it was far from my wishes to array myself in opposition to the views of any individual, for I had not then, nor have I now either leisure or inclination to engage in a polemic discussion. Nor was it my principal object to develop the truth in relation to the Resistance of Fluids; but to exhibit the necessity of using language, in the discussion of such subjects, with greater precision than has been generally practiced. Of this necessity, I considered the communications of Professor K. and Mr. Gibbes, as affording ample proof and an apt illustration; and with these views I referred to them with perfect freedom indeed, but certainly with no intentional disrespect.

In writing an article with this object in view, I made an effort, as I was bound to do, to use language myself with entire precision, and am therefore the more surprised that I should have been misunderstood. I commenced by pointing out the ideas which I should attach to several terms that are in common use, but without any fixed meaning. Among these was the term *force,* which I defined to be "simple pressure or effort at any point or indivisible instant of time." I stated that "in this sense its magnitude is expressed simply in pounds." In recapitulating my definitions I varied the form of this one, retaining the same idea. I said "force is simple pressure or effort, irrespective of duration or motion." Now by all this Prof. K. understands me to mean that force is a magnitude which results from the product of simple pressure or effort by a unit or element of time; a quantity totally different in its nature from that which I intended to define, and a quantity too which surely is not irrespective of duration. He not only misunderstands but misquotes my definition. He says, "Mr. B. defines force to be the pressure "*in an indivisible instant.*" Not so by any means. I said "AT *any indivisible instant;*"—referring to time as an *epoch* or *date,* and not as a *period* or *duration.** The *pressure* AT *any instant* differs from the *pressure* IN *an instant* as widely as a linear inch differs from a square inch. As a linear inch has magnitude without breadth, but a square inch has not, so the *pressure* AT *any instant* has magnitude independently of duration, but the *pressure* IN *an instant* has none.

I am bound to presume, and am happy to do so, that this misquotation of my definition was made through inadvertency. And since

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* It was necessary in a comprehensive definition to refer to time, because forces are sometimes *variable;* and if the magnitude of such a force be expressed simply in pounds, some particular instant must be pointed out *at* which its magnitude is estimated.

to presume this is to presume that my piece was read inattentively, I think it fair to infer that the misunderstanding did not arise from any defect in the definition, and therefore that further illustration of my meaning, were it possible, would be unnecessary.

The distinction here insisted upon between simple pressure and the product of pressure by unity of time, is highly important to clear and just views on the subject. Still however, the truth of the proposition on which Prof. K. considers the question between him and myself to turn, viz. that the force of a particle is as the square of the velocity, does not depend on this distinction; for although the multiplication of a quantity of one species by unity of another changes the *nature* of the quantity, it does not alter its *numerical magnitude;* and therefore, numerically measured, it will still retain the same magnitudal relations to other quantities. The proposition that the force of a particle is as the square of its velocity is true in either sense of the term *force*. The fallacy of Prof. K.'s reasoning in opposition to my views on this point, consists in his having hastily overlooked for a moment, the fact that the duration of the action of a given quantity of fluid is not given in my argument; a fact which he recognizes immediately afterward.

If Prof. K. will reexamine the subject with more careful attention, keeping in view that by *force* in every instance in which I used the term, I meant precisely what I defined it to mean, he will see that when I determined the force of a particle on the plane, I determined, not its *whole action,* but only its action *at* any instant—he will see that there is nothing in my argument which assumes that the whole action of a particle takes place in an indivisible instant, or even that it may not last for hours. He will see, therefore, that there is no conflict between my definition and my argument, and that I have not set out to determine one kind of force and determined another. He will find that the "dilemma" into which he seems to himself to have pursued my argument, will vanish when he calls to mind the fact that velocity is not a measure of the force which generates it, except when the time is given; and that the time was not given in my argument. He will see that the truth that *the resistance of a given quantity of fluid is as the square of the velocity;* which truth he takes some pains to show that I virtually admit, and which he says that I unconsciously proved, is not deducible from the common theory; and that instead of supporting that theory it disproves it. He will see also that instead of proving this truth unconsciously, the proof of it was one of the main points aimed at in my argument; for a given plane when it moves a given distance encounters a given quantity of fluid; and I showed that when it moves a given distance it encounters a quantity of resistance which is as the square of the velocity.

In short, if Prof. K. will examine the subject with careful attention, he will find that not one of the objections which he urges against my views is well founded, and that the logic of my argument is pure, and its conclusions irresistible.

I deem it unnecessary to say more in reply to the strictures of Prof. K. There are however among these strictures one or two incidental remarks, which invite, or rather seem to demand an expression of my views on a point of great interest in mechanical philosophy, and in reference to which, if I mistake not, a most fatal error almost universally prevails among men of science. Prof. K. says, "The truth is, when Mr. Blake calls the measure of the simple velocity a fundamental error, affirms it to be the square of the velocity, and offers the above argument to prove it, he raises the very question which unaccountably agitated all scientific Europe for forty years, about the measure of forces, whether it was the velocity or the square of the velocity, and which at length died away by a tacit admission of the parties, that the Leibnitzians universally considered an element in their calculations as variable, which the Newtonians as universally considered constant."

Before commenting upon this remark, I will quote one from Gregory, alluding to the same dispute. He says, in his Treatise of Mechanics, Vol. I. Article 214,—

"We must not omit observing, that about a century ago there was a warm dispute among the mathematicians, in order to determine whether we ought to consider the force of bodies in motion proportional to the velocity or to the square of the velocity; it is easy from what has preceded, to reduce this question to a simple enunciation which will remove all difficulty. The word *force*, denoting any cause of which the nature is unknown, and of which the effects are the only things we can measure, it is evident that by the term *measure of force*, we can only mean that of its effects: now the effects may be considered under different aspects, each comporting with a species of measure particular and conformable to its nature. If we consider the effect of the force as consisting in the destruction of a certain sum of obstacles or of quantities of motion, this sum must, as above shown, be expressed by $BV+bv+$ &c. that is, it is proportional to the velocity simply. But if we consider the effect of the force, not with relation to the sum of the obstacles, but to their number, this number, as will appear further on, will be represented by $\frac{1}{2}Bv^2$, and will be proportional to the square of the velocity when all the obstacles are equal. Hence this famous question is only a dispute about words; and as such we need dwell no longer upon it."

Now who does not see that a *sum* of equal obstacles is as their *number?* If therefore their *sum* is as the velocity, their *number* surely cannot be as the square of the velocity. "Hence this famous question" was not settled by Gregory, notwithstanding he seemed to dispose of it so easily. Nor has this "unaccountable" dispute been accounted for to my satisfaction by Prof. K.; for I have not been able to discover any element which when assumed by one party to be *constant* and by the other to be *variable,* will account for the different results at which the parties arrived. The real difference between the Newtonians and Leibnitzians was simply this: they attached different ideas to the term *force.* One party by the *force* of moving bodies meant the power inherent in them by means of their mass and motion, to effect *changes of motion* in other bodies; in this sense their force is as their velocity. The other party by the same term meant the power to penetrate or make impressions on other bodies; in this sense the force is as the square of the velocity. When I affirmed, in my article on the Resistance of Fluids, that the force of a particle on the plane at any instant of its action is as the square of the velocity, I neither concurred with nor dissented from the views of either of these disputants, for the simple reason that I attached a different idea to the term force from that attached to it by either of them. The term was used by me in its elementary sense, and confined to that sense by my definition. In this sense it is a magnitude whose nature is independent of the nature of the effect which it may be concerned in producing; and in this sense it enters as an element and only as an element into mechanical agency in every other aspect in which it can be contemplated. The dispute between the Newtonians and the Leibnitzians is another apt illustration of the necessity of confining the term *force* and other similar terms to some one fixed meaning. The two meanings attached to it by these disputants are only two out of perhaps a dozen that might be pointed out; and their dispute is only one out of a thousand that have arisen from this loose manner of using the term.

Prof. K. in another place remarks, "When Mr. Blake denies that the force of a particle is as the velocity of the plane, he must mean that momentum is not a measure of the moving force; a truth so obvious that if we are to give it up we give up the whole theory of mechanics." It was not my intention either to affirm or deny the proposition that "momentum is the measure of the moving force:" nor did my argument bear at all on that point, for I affixed to the term force a narrower meaning than it has in that proposition. My views however on that point are fully settled, and indeed I have in effect already partially expressed them in the preceding paragraph. Believing that a further development of them may subserve the interests of Mechanical Philosophy, I avail myself of this opportunity to express in a few words the substance of what I had heretofore intended to make the subject of a separate communication to the Journal of Science.

If in the proposition that momentum is the measure of the moving force, the term force means the power of a moving body to effect changes of motion, that is, to generate or destroy momentum in other bodies, then momentum is unquestionably the measure of the moving force: but if it means power to penetrate or make impressions on other bodies, measuring the magnitude of the effect by the magnitude of the impression, or if it means power to grind grain, saw lumber, drive a steam boat or rail road car, or to impede the progress of a steamboat or car, measuring the magnitude of the resistance by the amount and cost of the steam or other power necessary to overcome it; or, in short, if it means the power either to aid or to hinder the production of any of the effects for which mechanical power is used in any of the various processes of engineering and the arts, then momentum is *not* the measure of the moving force; and "the whole theory of mechanics," so far as it is based on the supposition that it is so, "MUST BE GIVEN UP." The proposition, as I understand it, means that the momentum of a moving body is a universal measure of its power to produce mechanical effects. If Prof. K. understanding it in this way, believes it a truth, he is not alone in that opinion. So far as my reading and observation extend, it is generally admitted or taken for granted that momentum is a universal measure of mechanical power. This idea pervades the standard Treatises of Mechanics, and in some of them is laid down in plain and unequivocal terms. Gregory, Vol. I. Article 477, says, "the quantity of motion extinguished or produced, is the true, unequivocal measure of mechanical power really expended, or the mechanical effect actually produced." Now I freely and deliberately incur whatever of responsibility there may be in affirming that this is a most egregious error; and that momentum is *never* a measure of power in the mechanics of engineering and the arts. I know that on this error much of the theory of mechanics is based; and I repeat that so far as it is thus based "*it* MUST *be given up.*" It is this error pervading treatises of mechanics which has rendered them worse than useless as guides to practical men on subjects relating to the application and use of mechanical power. The doctrine of the "maximum effects of machines" owes its origin entirely to this error; a doctrine so absurd that if a mere tyro in practice were to construct machinery on the principles there recommended, he would render himself ridiculous among practical men.

Let it not be imagined that I would promote what Prof. K. calls the "hostility of theoretical and practical science." I seek a reconciliation between them on the only ground on which permanent peace can be established;—the purgation of the theory from this grand error. When this is effected, I pledge myself as a friend to both, that all the other errors which Prof. K. admits to exist, shall not seriously interrupt their future harmony.

Prof. K. concludes his strictures with the following very just remark: "and above all, if the accuracy of the demonstrations and conclusions of such minds as Newton's must be impeached, let it be done, I will not say timidly, but cautiously, and with respect." I am not aware that in the article referred to I impeached the demonstrations or conclusions of Newton. I imagine that the points which I called in question, were rather inferences illegitimately drawn by others from Newton's reasonings. However this may be I intended to state my views in a respectful manner. I certainly did it with great caution, and not without having first established in my own mind a perfect conviction of their truth. If therefore I have actually arrayed myself against Newton I shall not retreat, nor seek refuge behind any *name*, but take my stand upon the *immutable laws of nature*. If these will not sustain me let me be put down.

In conclusion I will only remark, that if my former communication appeared to Prof. K., as would seem from the paragraph last quoted, to lack that modesty which ought to characterize the production of one who can make but little pretension to mathematical knowledge, in comparison with the distinguished men who have before written upon the same subject, it is not likely that this communication will remove that impression. Let him however be assured that I have yet left, too much of timidity of my own powers, too much of caution lest I should commit myself in error, and too much of respect, I will not say for Newton, but for all other mathematicians, however humble, to assail their conclusions hastily, and without reflection. And as I have taken care to advance no crude and ill digested views myself, so I shall feel myself called upon to reply to none which may be advanced by others.

Art. XII.—*Analyses of Chabasie.*

To Chas. T. Jackson, M. D., member of the Geolog. Society of France—the Boston Society of Natural History, &c.

Dear Sir—In compliance with your wishes, I hand you herewith a translation of an article published in Poggendorff's Annals, relative to our Nova Scotia Chabasie, which no doubt will prove interesting, and will, I hope, induce you to examine more minutely this variety of the mineral species. With feelings of friendly regard, I remain, dear Sir, ever truly yours, Chas. Cramer.

New York, May 2, 1836.

Analyses of some Chabasies, by E. Hoffman, Dr. Ph. (Poggendorff's Annals, 1832, No. 7.)

It may appear unnecessary to publish analyses of Chabasies, as good ones have already been made known by Arfvedson. The following circumstance however, induced me to do so.

Mr. G. Rose,* on his return from Russia, brought home a Chabasie from Parsboro', Nova Scotia, which he obtained in St. Petersburg.†

It is of a brick color and crystallized in large and distinct single and twin crystals. Having made two successive analyses of this mineral, and not obtaining Arfvedson's formula

$$\left.\begin{matrix}\dot{C}^3\\ \dot{N}^2\\ \dot{K}^3\end{matrix}\right\}\dddot{S}i^2+3\,\dddot{Al}\,\dddot{S}i^2+18H$$

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* Humboldt's Companion.

† From myself—I found it in 1827, as subsequently ascertained, about three weeks after your first visit to the Basin of Mines.—*Chas. Cramer.*

but that of Berzelius, $\left.\begin{matrix}\dot{C}\\ \dot{N}\\ \dot{K}\end{matrix}\right\} \dddot{Si}+3\dddot{Al}\ \dddot{Si}^{2}+6H$

which however was rejected, as the crystals did not rest on quartz, by which they might have been penetrated, for in this way Berzelius accounts for the surplus of silica in his analysis, compared to that of Arfvedson, (Edin. Phil. Journal, Vol. 7. p. 11.) I likewise examined the Chabasies from Aussig in Bohemia, and the Fassathal, Tyrol. These however, I found to accord with the formula of Arfvedson.

Having with equal minuteness, followed the same method in all these three analyses, I cannot, (unless some analysis should hereafter correct mine,) but declare that the mineral from Parsboro', is "*no* Chabasie," particularly as the specific gravity too, differs in a measure, and as this mineral is not as easily affected by muriatic acid, as the others. I proceeded in the analysis in the following manner.

After reducing the mineral to a powder, in a mortar of agate, it was levigated, weighed and treated with muriatic acid; the fluid being diluted with water, the silica was collected on a filter, calcined and weighed, and then boiled with carbonate of soda. The portion which did not dissolve, was deducted from the quantity used. The alumina was precipitated by ammonia from the fluid, freed of silica, afterwards weighed, and again dissolved by muriatic acid. This left a trifling quantity of silica.

In No. III, the oxide of iron was separated by succinate of soda, from the dissolved alumina. The lime was precipitated by oxalate of ammonia, from the solution which had been freed of alumina, the fluid was evaporated to dryness, the muriate of ammonia dissipated by calcining, and the weight of the chloride of soda and chloride of potash, ascertained. These were hereafter again dissolved in water, a solution of chloride of platinum, being added; and evaporated with a moderate heat. The residue was treated with alcohol, by which process the soda alone was dissolved, the quantity of potash was determined from the chloride of potash and platinum, and that of the soda ascertained by the loss. The quantity of the water was determined, by calcining another portion of the mineral. I convinced myself, that none of the chabasies I analyzed, contained either muriatic or fluoric acid.

I. *Chabasie from Biebendörfel near Aussig, Bohemia.*

Specific gravity at +7° 7 R. =2.127. The quantity of the powdered mineral used, was 2.6712 gr. The result of the analysis was the following:—

		Per cent.	Oxygen.	
Silica,	=1.2869	48.18	25.027	8
Alumina,	=0.5148	19.27	8.998	3
Lime,	=0.2580	9.65	2.710	
Soda,	=0.0414	1.54	0.393	1
Potash,	=0.0058	0.21	0.035	
Water,	=	21.10	18.753	6
		99.91		

II. *Chabasie from the Fassathal, Tyrol.*

Specific gravity at +8° 3 R. =2.117. The quantity of the powdered mineral used, was 2.7381 gr. The analysis produced the following result.

		Per cent.	Oxygen.	
Silica,	=1.3318	48.63	25.262	8
Alumina,	=0.5345	19.52	9.118	3
Lime,	=0.2799	10.22	2.870	
Soda,	=0.0155	0.56	2.142	1
Potash,	=0.0079	0.28	0.047	
Water,	=	20.70	18.339	6
		99.91		

III. *Chabasie from Parsboro', Nova Scotia.*

Specific gravity at +70° 6 R. =2.075. The quantity of the powdered mineral used, was 2.5878 gr. The result of the analysis was the following:—

		Per cent.	Oxygen.	
Silica,	=1.3318	51.46	26.732	9
Alumina,	=0.4570	17.65	8.242	3
Lime,	=0.2308	8.91	2.502	
Soda,	=0.0284	1.09	0.278	1
Potash,	=0.0043	0.17	0.028	
Oxide of iron,	=0.0284	0.85		
Water,	=	19.66	17.473	6*
		99.79		

* According to a communication of Prof. G. Rose, however, there appears to be no difference in the angles or structure of the chabasie from Parsboro' compared to that of other localities.

Art. XIV.—*On the Origin of Shooting Stars.*

1. *Letter from* Rev. W. A. Clarke, *addressed to the Editor.*

Stanley Green, near Poole, Dorsetshire, Eng. Jan. 6, 1836.

Sir—I observe in your Journal for October last, (Vol. XXIX, p. 168,) that Prof. Olmsted has done me the honor of quoting from the Magazine of Natural History for December, 1834, some observations of mine relative to a few meteors seen at this place by me on the morning of November 13th, 1834. As Prof. Olmsted quoted my notice in testimony of his supposed comet, I think it only due to myself and such of your numerous readers in England and America, as do not see the Magazine of Natural History, and who dissent from Prof. Olmsted's views, to state distinctly, that so far from thinking the occurrence of meteors here, at the time alluded to, any evidence in favor of the supposed comet, I have in the Magazine of Natural History, for March, 1835, (Vol. VIII, p. 140, in No. 6, of a series of essays "On certain recent meteoric phenomena, vicissitudes in the seasons, prevalent disorders, &c. contemporaneous, and in supposed connection with volcanic emanations")—shown, that the difference in time (6*h.* 47*m.* 28*s.*) of the first appearance of meteors at Poole and New Haven, makes the supposed comet move *westwardly*, which contradicts Mr. Olmsted's hypothesis—and I have added to my calculation: "If, then, these meteors betoken the presence of a cometic body, it moves *westardly*; and the position of Professor Olmsted is untenable. This is a fatal argument for the hypothesis; and we are driven to conclude, that the idea of the meteors being altogether electrical, arising from a certain state of the atmosphere, in certain years, at a certain period of the year, is sufficient to explain their occurrence. The subsequent agitations of the atmosphere in 1834, as well as in 1833, the gales that occurred, and the volcanic phenomena that preceded, all lead to the same conclusion. Till, therefore, these difficulties shall have been reconciled, I shall adhere to my own supposition, (p. 141.) In the course of this and my other essays, I have advanced evidence to support my conclusions, and have canvassed nearly every argument advanced by Prof. Olmsted, his friends, and opponents, and I cannot but confess, that however ingenious may be his theory, and however great his skill in astronomical calculation, and whatever may be the fate of my own hypothesis respecting the cause of these occasional displays of meteoric phenomena, I do not consider that Mr. Olmsted has established his position respecting his imaginary comet, either by sound philosophical induction, consistency of conditions, or sufficiency of evidence. At the same time, I beg to add, that I wish not this letter, or these remarks, to be considered as in the slightest degree disrespectful to Prof. Olmsted's personal or scientific character.

I have the honor to be, sir, your obedient servant,

W. A. Clarke.

2. *Remarks on Shooting Stars, in reply to Rev. W. A. Clarke; with additional observations on the present state of our knowledge respecting the* origin *of these meteors.*

By Denison Olmsted; Professor of Mathematics and Natural Philosophy in Yale College.

I have noticed the strictures of the Rev. Mr. Clarke, published at different times, in Loudon's Magazine of Natural History, on my views of the origin of Meteoric Showers, and it might seem due to so respectable a writer to have replied at an earlier date than the present; but not feeling disposed to undertake a refutation of his theory,—that the meteoric showers of November are the consequence of "volcanic emanations,"*—and perceiving, that in the very objection which he pronounces *fatal* to my views, he had committed errors so obvious, that they could not escape the notice of astronomers, I was rather disposed to consider him, from his assiduity in collecting facts, as a fellow laborer, and to avail myself of his labors, than to meet him in the field of controversy.

It appears, however, by the foregoing letter, which, by the kindness of the editor, I have been permitted to read, that Mr. Clarke fears lest the readers of this Journal should suppose, by my quoting his facts, that he is a believer in my theory. I certainly did not intend to claim him as such, but felt at liberty still to use the facts as common property, and indeed supposed that they would be considered as of more value to me, because they were so favorable to my views, although furnished by one whose opinions differed so widely from my own.

* Not that the meteors themselves are thrown out of volcanoes, but that such agitations and derangements of the atmosphere are produced by volcanic emanations, as, with the aid of *electricity*, engender the meteors.

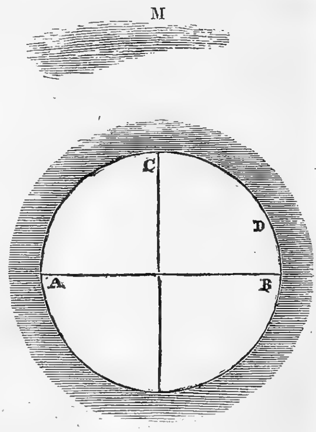
Having now adverted to certain "errors" committed by Mr. Clarke, I ought perhaps to point them out more particularly. The argument of Mr. C. is as follows :*

"They [the meteors] were first seen [at New Haven] at 1*h.* 4*m.* A. M. November 13. They were seen in Dorsetshire, at 11 P. M. on the 12th.

The Longitude of New Haven is	72° 50′†
The Longitude of Poole is	1° 58′
	70° 52′=4*h.* 43*m.* 28*s.*

Therefore, 1*h.* 4*m.* New Haven time is equivalent to 5*h.* 47*m.* 28*s.* Poole time; which added to 1 hour, the difference of time between the hour of observation here and midnight, gives 6*h.* and 4*m.* 28*s.* the interval which elapsed between the first appearance of the meteors in England and America. If then these meteors betoken the presence of a cometic body, it moves *westwardly*, and the position of Professor Olmsted is untenable.‡ This is a fatal argument for the hypothesis; and we are driven to the conclusion that the idea of the meteors being altogether electrical, arising from a certain state of the atmosphere, in certain years, at a certain period of the year, is sufficient to explain their occurrence."

Now the argument of Mr. Clarke, goes to prove just the opposite of what he asserts. For, let ABC represent the earth and atmosphere turning on its axis from west to east in the direction BCA, and let M represent the *extreme portions* of a nebulous body situated in space. Let C be the meridian of Poole, and D that of New Haven.



At 11 o'clock, on the night of November 12th, the point C came so near the nebulous body that meteors began to descend from it to the earth. Four hours and forty three minutes afterwards, that is, when it was 11 o'clock at New Haven, the point D comes into the same position; and provided M remains at rest relatively to the earth, and its extreme portions remain unaltered, then we might expect that the meteoric display would commence at New Haven at the same hour of the night, or at 11 o'clock. But it commenced about

* *Loudon's Magazine of Natural History*, for March, 1835.

† More accurately, 72° 57′ 46″.

‡ Namely, that the supposed nebulous body moves in an orbit around the sun *from West to East.*

two hours later; therefore the point D must have gone so much farther in order to come under the meteoric body; or, what amounts to the same thing, that body must have travelled about 30 degrees *eastward*, during the 6*h*. 47*m*. 28*s*. the difference, *in absolute time*, between the first appearances of the phenomenon at the two places. We do not suppose that the body actually made such an advance eastward; but such would be the consequence of Mr. Clarke's argument.

The fact is, however, that the *commencement* of such a phenomenon, cannot be employed to determine the position of the body at the beginning and end of so long an interval. Were the body to sustain no change of place relatively to the earth, (which would imply that its angular motion in its orbit was in the same direction and precisely at the same rate with the earth's,) yet the actual *loss* of some of the extreme portions might cause the time of night to be later to places lying westward of a given meridian.

From comparing a great number of observations made at places differing many degrees of longitude, in the meteoric shower of November 13th, 1833, it was found, that *the time of arriving at the maximum*, was nearly the same in them all,—indicating that the body was, *apparently*, nearly stationary in respect to the earth, and consequently moving along with it in its annual revolution. From calculations made to determine the *real* velocities of the two bodies, it was inferred, that the motion of the earth in its orbit, was to the motion of the body at the time of the occurrence, as 18.92 to 12.15.* Still, on account of the great distance of even the nearest portions of the body, (a distance which is now believed to be much greater than was at first estimated,) it would have had only a small parallactic motion, although portions descending from it to the earth, would have a great relative motion westward.

I avail myself of this opportunity to add a few observations on the present state of the controversy, respecting the origin of shooting stars.

In the French journals which have lately reached us, are contained a series of *Instructions*, drawn up by M. Arago, of the Observatory of Paris, well known as one of the ablest astronomers of his age, for the use of the officers of the *Bonite*, a French ship which was about to sail on a voyage of circumnavigation. In this article,

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* American Journal, XXVI. 167.

among other topics of meteorology, the writer introduces some remarks on shooting stars; and it cannot but be very gratifying to me to find the leading conclusions at which I had arrived on this difficult subject, sustained by so high an authority as M. Arago.

It may be recollected that in some observations on the Meteors of November 13th, 1833, published in the 25th and 26th volumes of this Journal, I endeavored to show, that these meteors had their origin beyond the limits of the atmosphere,*—that they consist of light, transparent, combustible bodies, which take fire on falling into the atmosphere, having existed together in space in the form of a nebulous cloud of great extent,†—that this cloud, or nebulous body, has a periodical revolution around the sun, constituting a distinct member of the solar system.‡

On these several points, M. Arago remarks as follows:§

"Since men of science have thought of observing shooting stars with accuracy, we may have seen how these phenomena, so long disregarded,—how these pretended atmospheric meteors, these would be trains of hydrogen gas on fire, show their claims to attention. Their parallax has always placed them much higher than, according to prevailing theories, the sensible limits of our atmosphere would seem to allow. In examining the direction in which shooting stars move, *most usually*, we perceive, by other means, that if they take fire in our atmosphere, *they at least do not take their rise there, but come from beyond it.*"‖

Again M. Arago observes:

"We cannot get the smallest glimpse of an explanation of the astonishing exhibition of fire balls, observed in America on the night of the 12th and 13th of November, 1833, unless by supposing that besides the great planets, (and in this number we comprehend even Ceres, Pallas, Juno and Vesta,) *there revolve around the sun* myriads of small bodies, *which do not become visible except at the moment when they come into our atmosphere and take fire.*"

After giving an account communicated to him by M. Berard, an officer in the French Navy, of a remarkable fall of meteors seen in the Mediterranean on the coast of Spain, November 13, 1831, M. Arago concludes with the following remark:

"Thus is confirmed, more and more, the existence of a zone composed of millions of little bodies, whose orbits meet the plane of the ecliptic near the point that the earth occupies every year, from the 11th to the 13th of November. *This is a new planetary world, which is beginning to reveal itself to us.*"

The account of M. Berard alluded to, is as follows; and it is deserving of the greater attention because it confirms the statement of

* Am. Journal of Science, XXVI. 140. † Ib. p. 161 and 172. ‡ Ib. p. 165.

§ See Bibliotheque Universelle, for September, 1835, pp. 71—75.

‖ Ib. p. 72.

Dr. Wright, of Ohio, given in this Journal, (Vol. XXVII. p. 419.) upon whose authority alone we until now rested our knowledge of a repetition of the meteoric shower as far back as the year 1831.

"November 13th, 1831, (says M. Berard,) at 4 o'clock in the morning, the sky was perfectly pure, and the dew very copious. We had seen a considerable number of shooting stars and luminous meteors of great size. During three hours there fell on an average, two per minute. One of these meteors which appeared in the zenith, leaving an enormous train, extending from east to west, presented us with a very large zone, equal in diameter to half the breadth of the moon, and exhibiting several colors of the rainbow. A trace of it remained visible more than six minutes. We were then on the coast of Spain, near Carthagena."

All accounts received respecting the recurrence of the meteoric shower last November agree in this—that it was on the morning of the 14th and not on the 13th as usual. The London Athenæum for Feb. 27, 1836, contains the following notice of the observations of Sir John Herschel at the Cape of Good Hope.

"ANNUAL METEORIC PHENOMENON.—Our transatlantic brethren have, for the last two or three years, indulged us with accounts of some most extraordinary meteoric appearances that have taken place in America about the middle of the month of November of each year, and generally on the same day. The phenomenon in question consists of a most brilliant display in the heavens of a great quantity of that class of meteors called *shooting stars*, which, during the whole of the night above alluded to, keep up a constant discharge, and illuminate the whole hemisphere. The most remarkable circumstance, however, attending this affair is, that the phenomenon always occurs on or about the same day of the month, and that the direction of the meteors is generally the same, which has induced many persons to imagine that it is connected with some extraneous body revolving about our globe (?) Mr. Bailly, in the course of his correspondence with Sir John Herschel, noticed these remarkable statements, and requested Sir John to notice any remarkable appearance of the kind that might occur during his residence at the Cape of Good Hope. The following is an extract from a letter which Mr. Bailly has just received from that distinguished philosopher. "In all my sweeps in November, I was on the look out for shooting stars, viz. on the 10th, 11th, 13th, 14th, and 18th. On the 13th, and especially on the day mentioned in your letter, I told Stone (my assistant) to keep a sharp look out for them; his attention being disengaged whilst I was occupied at the telescope. He saw none. On the 14th I still desired him to keep watch for them. The sweep commenced at 0 hours, sideral time, and we went on from 4*h.* 8*m.* without his or my noticing any. At 4*h.* 8*m.* 19*s.* sideral time, he called out, 'There goes the largest I ever saw.' It fell in azimuth north about one half west perdendicularly. At 4*h.* 42*m.* 59*s.* he cried out again for another great one. This fell north about two points east, not quite vertical; but rather inclining eastward. This was as large, he said, as Jupiter. At 4*h.* 46*m.* 39*s.* another great one falling east of Jupiter, and still more obliquely, elicited another call. At 4*h.* 53*m.* 59*s.* I absolutely started from the eye-piece of the telescope, at the glare of a superb one, which fell about 20° azimuth west of south, obliquely. Stone thought that it lightened, though his back was to it, and it was hid from him by trees. It left a narrow, vivid, and distinctly crooked train, which lasted twenty seconds, and admitted of being distinctly contemplated. This meteor was equal to Venus at her brightest here; and I ought to mention that Venus *here* casts a strong shadow, in which all the most minute parts of objects, as the leaves of trees, &c. are perfectly well distinguished, not only against the white wall of a house, but on the ground. You may be sure that I shall look out again next 13th and 14th of November, should I still be here; though I can hardly suppose the thing to be more than an accidental coincidence: however, I have seen no considerable meteor since." "

By the side of Sir John Herschel's observations, I am happy to place the following accounts of the same phenomenon as witnessed at several places in our own country. They were transmitted to me soon after the occurrence, and would sooner have been given to the public, through the medium of this Journal, but for want of room.

1. From Mr. *Frederick Merrick.**

"*Amenia Seminary, Amenia, Dutchess Co. New York, Nov.* 18, 1835.

"To Professor Olmsted,—

"Sir,—On the morning of the 14th instant, I observed a slight exhibition of meteors, very much resembling those of 1833, although the number of meteors was comparatively small. From the time when I first observed them, until they were lost in the light of the sun, a period of only fifteen minutes, judging from the number which fell in that part of the heavens to which I directed my attention, I think they must have fallen at the rate of about six or eight per minute, although at one time I counted seven within the space of about half a minute; so that had they fallen equally fast in all parts of the heavens, they must, at that time, have fallen at the rate of not less than fifty per minute. Some were quite brilliant, though none remarkably so. They evidently radiated from a point not far from the constellation Leo, although I was unable to fix the point with any considerable degree of accuracy."

2. From *Professor John Mc Caffrey,* Vice President of Mount St. Mary's College, Maryland, dated November 24, 1835.

"Professor Olmsted,—

"Dear Sir,—In this district of country, the morning of the 13th was cloudy, and of course no return of the meteoric phenomenon of 1833 could be discovered. On the morning of the 14th, which was very clear, I observed, from a window in my room, the eastern portion of the heavens, from about five o'clock until day light. During this time I noticed the descent of seven meteors, which seemed to me to fall towards the earth at angles with the horizon of 70° or less. They all appeared to me to proceed from the vicinity of the constellation Leo; and one of them, which was very bright, and admitted of more satisfactory observation, seemed to start from a point in the heavens not far from the radiant point of the meteors seen on the 13th November, 1833;—in other words, it fell in a line which, produced upwards, would terminate in or near Gamma Leonis.

"A laboring man, living at this Institution, has informed me, that being up earlier than 4 o'clock on that morning, he witnessed a very brilliant, and quite unusu-

* Mr. Merrick made some very good observations on the meteors of Nov. 13, 1833, at Middletown, Conn. being then a member of the Wesleyan University. (American Journal, XXVI, 340.)

al display of shooting stars. To him they appeared to radiate from a common center, which, however, he places somewhere in the north western part of the heavens.

"Two gentlemen of my acquaintance, both tutors in this Institution, together observed and counted five meteors, which appeared in rapid succession, and did not cross each other's paths. They also fix the radiant point in the north western portion of the firmament. They saw them a little before 5 o'clock; consequently, at a time intermediate between my observations and those of the laboring man referred to above."

3. From the *Carolinian*, published at Salisbury, North Carolina, December 12, 1835.

"A brilliant display of *shooting stars*, was witnessed by a gentleman from this town on the 14th ult. They were seen on going out of doors, about 5 o'clock in the morning. Upwards of a dozen were counted in fifteen minutes. The same gentleman saw the magnificent meteoric shower, which took place about two years ago; and he says, that the meteors of last month, though much fewer in number, were in all other respects similar to those formerly seen."

Such are the accounts which have reached us of the "anniversary of the Meteoric shower," in 1835, agreeing in too many particulars, and all too remarkable, to permit the supposition that it was any ordinary atmospheric occurrence.

MISCELLANIES.

FOREIGN AND DOMESTIC.

NATURAL PHILOSOPHY.

1. *Atmospherical Electricity.*—(Rec. of Gen. Science. No. XIV. 1836.—Bibliothèque Universelle, May, 1835.)—In some investigations by M. Matteucci on this subject, he has found that whenever the electricity of the atmosphere was positive, (which is always the case in warm weather,) it is impossible to have any traces of electricity in the center of a wood or forest; whilst, not ten paces out of the wood, traces of electricity were apparent. On returning from this distance, the first tree is scarcely reached, when the electroscope, ceases again to indicate the presence of electricity. These general results can only be explained by two hypotheses; either that the electricity of the atmosphere is discharged by the trees and vapor, and thus escapes to the earth, or the respiration of plants develops sufficient negative to neutralize the positive electricity of the sur-

rounding air. The second appears most plausible, since it is difficult to admit the first, when we attend to the conducting power of the flame, and of the column of hot air, which is much superior to that of the leaves. The results of a great number of observations, proved that in the night, signs of electricity are often absent, both in the air and the interior of a wood. At the approach of day, before the sun appears above the horizon, decided indications of negative electricity appear among the trees, while none are detected in the open air. We can readily understand this observation, if we admit that oxygen is disengaged from the leaves, before the rays of the sun strike them directly. In this case, negative electricity appears. From the above it may be inferred, that negative electricity is disengaged by vegetation during the day, which is constantly neutralized by positive electricity.

2. *Effect of Sound on the Barometer.*—(Records of General Science, No. XIV. 1836, p. 113.)—Sir H. Englefield, while at Brussels in 1773, made some experiments on this subject. The barometer was fixed in the opening of a window, in the north east tower of the church of St. Gudule, about seven feet from the summit of the bell. Mr. Pigott found the height of the barometer, 29.478 inches. It did not vary until the clapper was loosened, when the mercury rose and continued to undergo a kind of starting, every time the clapper struck the bell. Mr. Pigott observed the height of the mercury during the sound 29.469. Sir H. Englefield, found its maximum height, during the sounding 29.480, minimum 29.474; maximum 29.482, minimum 29.472. Hence the effect of sound upon the barometer, extends to the $\frac{1}{6000}$ and $\frac{1}{10000}$ of an inch. It is remarkable that Pigott generally made the height $\frac{1}{5000}$ less than Englefield. The latter attributes such discordances to the difference in the eyesight.

CHEMISTRY.

1. *Phloridzine; a new organic substance.* (L' Institut. No. 143.) —This vegetable principle was obtained by MM. de Koninck and Stas from the bark of the root of the apple, pear and cherry trees. It may be procured by boiling the roots for four or five hours in water, decanting the liquid and continuing the ebullition with an addition of pure water for two hours, and again decanting. The decanted liquid, in each instance, after twenty four or thirty six hours repose,

will deposit small crystals of a more or less brown color. It may also be obtained from an alcoholic solution.

Phloridzine is but slightly soluble in cold water, but more so in warm. At 100° C. it is dissolved in every proportion. It is more soluble in cold alcohol than cold water, but equally in the two liquids at their boiling temperature. Heated above 100° C. it slowly melts, at 177° boils, and at 197° is decomposed, producing benzoic acid which sublimes. It is decomposed by the sulphuric, nitric and hydrochloric acids. Ammonia and the other caustic alkalies in solution dissolve it without alteration. The deutosulphate of iron colors the solution of phloridzine a deep brown, causing at the same time an ochre-yellow precipitate—no change in the color is effected by the protosulphate. Its composition is carbon 14, oxygen 9, and hydrogen 18 atoms.

The author, M. de Koninck, suggests in his memoirs, that phloridzine may be ranked with the most valuable febrifuges and will rival in utility the sulphate of quinine.

2. *Gastric Juice.*—This liquid has been lately analyzed by M. Henri Braconnot, who found it to contain

1. Free hydrochloric acid, in a remarkable quantity.
2. Hydrochlorate of Ammonia.
3. Chlorid of Sodium.
4. Chlorid of Calcium.
5. Chlorid of Iron.
6. Chlorid of potassium, (slight traces.)
7. Chlorid of Magnesium.
8. A colorless oil of an acrid taste.
9. Animal matter soluble in water and alcohol, in large quantities.
10. Animal matter soluble in the diluted acids.
11. Animal matter soluble in water and insoluble in alcohol, (salivary matter of Gmelin.)
12. Mucus.
13. Phosphate of Lime.

The fluid that was operated upon by M. Braconnot was obtained from dogs, by means of sponges which they were made to swallow.

The results obtained confirm the observations of Prout, Tiedmann, and Gmelin, that the stomach when stimulated by foreign or alimentary substances, has the remarkable power of secreting a large quantity of free hydrochloric acid. This analysis, as its author observes, affords no explication of the fact experimentally determined by Brugnatelli, that rock crystal and agate, introduced into the stomach of a turkey were so attacked as to lose from twelve to fourteen grains of their weight. This if true would prove the existence of hydrofluoric acid in the gastric juice of gallinaceous birds.—*Ann. de Ch. et de Ph. T.* 59. 1835.

3. *Thebaine; a new alkali in Opium.*—This new substance was discovered by M. Couerbe in the solution from which the muriates of morphine and codeine had been separated by Gregory's process; that is, by evaporating this solution to the consistency of a syrup, and after purifying it by hydrochloric acid, adding ammonia, which occasions a black deposit of morphine and thebaine. Ether slightly dissolves the latter and thus may be used in separating them. Thebaine thus prepared, is perfectly white, strongly alkaline, and soluble in alcohol and ether. It fuses at 266° and resolidifies at 130°, whilst narcotine fuses at 338° and solidifies at 266°. Codeine fuses at 302° and meconine at 194°. Its composition, according to M. Couerbe, is

Carbon,	71.976	25	equivalents,	nearly.
Nitrogen,	6.385	2	"	
Hydrogen,	6.460	27	"	
Oxygen,	15.279	4	"	

GEOLOGY AND MINERALOGY.

1. *Subsidence of the coast of Greenland.*—In a letter from Dr. Pingel, of Copenhagen, to the President of the Geological Society of London, it is stated, that the first observations which led to the supposition that the west coast of Greenland had subsided, were made by Arctander, between 1777 and 1779. He noticed in the firth called Igalliko (lat. 60° 43′ N.) that a small, low, rocky island, about a gun-shot from the shore, was almost entirely submerged at spring tides, yet there were on it the walls of a house fifty two feet in length, thirty feet in breadth, five feet thick, and six feet high. Half a century later, when Dr. Pingel visited the island, the whole of it was so far submerged that the ruins alone rose above the water.

The colony of Julianahaab was founded at the mouth of the same firth in 1776; and near a rock, called the Castle by the Danish colonists, are the foundations of their storehouse, which are now dry only at very low water.

The neighborhood of the colony of Frederickehaab (lat. 62° N.) was once inhabited by Greenlanders; but the only vestige of their dwelling is a heap of stones, over which the firth flows at high water.

Near the well known glacier which separates the district of Frederickehaab from that of Fiskenass, is a group of islands called Fulluartalik, now deserted; but on the shore are the ruins of winter dwellings, which are often overflowed.

Half a mile to the west of the village of Fiskenass (lat. 63° 4′ N.) the Moravians founded, in 1758, the establishment called Litchtenfeld. In thirty or forty years they were obliged once, perhaps twice, to move the poles upon which they set their large boats, called Umiak, or Women's boats. The old poles still remain as silent witnesses, but beneath the water.

To the north east of the mother colony, Godthaab, (lat. 64° 10′ N.) is a point called Vildmansnass by St. Egede, the venerable apostle of the Greenlanders. In his time, 1721—1736, it was inhabited by several Greenland families, whose winter dwelling remains desolate and in ruins, the firth flowing into the house at high tide. Dr. Pingel says, that no aboriginal Greenlander builds his house so near the water's edge.

The points mentioned above, the writer of the letter had visited; but he adds, on the authority of a countryman of his own, highly deserving of credit, that at Napparsok, ten Danish miles (forty five English) to the north of Nÿ-Sukkertop (lat. 65° 20′ N.) the ruins of ancient Greenland winter houses are to be seen at low water.

Dr. Pingel is not aware of any instance of subsidence in the more northern districts; but he suspects that the phenomenon reaches at least as far as Disco Bay, or nearly to 69° north lat.—*Proceedings of the Geol. Soc. of London.* Vol. II. No. 42.

2. *Dreelite; a new mineral species.* By M. Dufrenoy, (Ann. de Chim. et Phys. T. 60, p. 102.)—Dreelite occurs in small crystals disseminated on the surface and in the cavities of a quartzose rock, which contained also a white mineral supposed to be halloysite. The crystals were unmodified rhombohedrons of 93° or 94°, of a white color and pearly lustre; the lustre is quite brilliant on a surface of fracture. Its cleavage is indicated only by lines parallel to the faces. In hardness it is somewhat superior to carbonate of lime. Sp. gr.=3.2—3.4. Under the blowpipe it fuses into a white bleb-

by glass, which is colored blue by nitrate of potash. Its analysis proves it to be composed of

Sulphate of baryta,	61.701
Sulphate of Lime,	14.274
Lime in excess,	1.521
Carbonate of Lime,	8.050
Silica,	9.712
Alumina,	2.404
Water,	2.308
	100.00

This mineral is named after M. de Drée, a liberal patron of science.

3. *Albite of Chesterfield.*—This mineral has lately been analyzed by MM. Aug. Laurent and Ch. Holms, who have found its composition to be identical with that of common albite, contrary to the result of Stromeyer's analysis. The following is the composition determined:

Silica,	68.4
Alumina,	20.8
Iron and Manganese,	0.1
Lime,	0.2
Soda,	10.5
	100.0

from which is deduced the formula $(3Si^3O^3+Al^2O^3)+(Si^3O^3+ONa)$ identical with that for the common variety of albite.—*Ann. de Ch. et de Ph.* T. 60, p. 331.

4. *Analysis of Tabasheer from India.* By Dr. Thos. Thomson.—(Rec. of Gen. Sc. No. XIV. 1836.)

Moisture,	4.87
Silica,	90.50
Potash,	1.10
Peroxyd of Iron,	0.90
Alumina,	0.40
	97.77

5. *Corals and Entomostraca in Chalk.*—Mr. Lonsdale has discovered that the common white chalk, especially the upper portion of it taken from different parts of England, (Portsmouth and Brighton among others,) is full of minute corals, foraminifera and valves of a small entomostracous animal resembling the Cytherina of Lamarck. From a pound of chalk he has procured in some cases at least, a thousand of these fossil bodies. They appeared to the eye like white grains of chalk, but when examined by the lens are seen to be fossils in a beautiful state of preservation.—*Address before the Geol. Soc. of London by C. Lyell.* 1836.

MISCELLANEOUS INTELLIGENCE.

1. *Sopra i Vulcani estinti del val di Noto, del Professore Carlo Gemellaro; memoria secunda.* 35 pp. 4to. Catania, 1835.—The former memoir in the Val di Noto by this active geologist of Sicily, established two eras of calcareous formation, and two of volcanic action. That now before us contains an elaborate account of each variety of rock which this tertiary and volcanic region affords.

2. *Vibration of Railways, by Capt.* Denham, *R. N.*—Capt. Denham ascertained that the vibrating effects of a passing, laden rail road train in the open air extended laterally on the same level 1,110 feet, (the substratum of the positions being the same,) whilst the vibration was quite exhausted at 100 feet when tested vertically from a tunnel. The tunnel was through a stratum of sandstone rock: the rails being laid on a substratum of 12 feet of marsh over the sandstone rock. The method of testing was by mercury reflecting objects to a sextant. The experiments were made in the neighborhood of Liverpool.

3. *The Geological Society of London,* has awarded the Wollaston Medal to M. Agassiz, of Neuchatel, for his work on Fossil Ichthyology; and also the sum of 25*l.* from the donation fund to M. Deshayes in promotion of his labors in fossil conchology.

4. *New Scientific Journal.*—On April 1st, 1836, will appear the London Geological Journal, No. I, with colored engravings, by J. de Carl Sowerby, F. L. S. of new Fossil Echinidæ from the English strata.—*Lond. and Ed. Phil. Mag. and Jour. of Sc. No.* 43.

5. *Fertilizing properties of Limestone.*—Communicated by a gentleman in Geneseo, State of New York.—An additional fact in relation to the fertilizing quality of lime seems to be rendered very probable, if not certain, by some experiments which have been recently made in this town. Mr. Moore, in digging a well, hit upon a formation of soft or friable limestone, combined with fossil shells* of great diversity of formation. Specimens were sent in different directions, and there was but one opinion that it was a limestone formation. A bed of gypsum is very valuable. Mr. Moore and his neighbors believe that they had discovered a valuable gypsum formation on their farms. They sent waggon loads to plaster mills and grist mills, and caused what they pronounced gypsum to be spread on a great number of fields, during last fall and this spring. The result has been, in every instance, that the clover, wheat, and spring crops have been essentially benefitted by the application; and Mr. Moore and his neighbors still believe the substance which they are selling as gypsum, surpasses in efficacy either the Wheatland or the Cayuga plaster. That this formation is equally efficacious with the plaster which is generally used in this section of the State, there seems no reason to question—and that it is a limestone formation, is beyond the possibility of doubt.

Is it possible that the mechanical operation of grinding or pulverizing crude or unburnt limestone renders it equally fertilizing with gypsum? It appears difficult to avoid this inference. Mr. Moore has erected a windmill, and is digging and vending in great quantities, what he calls plaster; and the farmers, from hundreds of experiments, entertain the most entire confidence in its efficacy.

With a view of obtaining some additional facts on this subject, which, in its present stage, is a little perplexing, a person called on Mr. Moore with a vial of muriatic acid. Mr. Moore showed him specimens of Chitteningo, Cayuga, Phelpstown and Wheatland plaster. Each of these specimens effervesced on the application of the acid.† This fact seems to add to the perplexity of the subject,

* Chiefly terebratulæ of great distinctness and beauty.

† Owing to a mixture of carbonate of lime with plaster of Paris, it is not uncommon that the latter effervesces. It is not difficult to decide which prevails. The substance being pulverized and mixed with diluted muriatic or nitric acid, will dissolve entirely if it is a pure carbonate of lime; if there is a residuum, it may be plaster of Paris, or something else not soluble in the acids. The specimen sent to us by our correspondent was so treated, and indicated about half of carbonate of lime without plaster of Paris, but with a large clayey residuum.—*Ed.*

and would appear to indicate that we are using (and certainly deriving great benefit from the use of) a certain description of limestone, but which is not gypsum. If this is a fact, it goes to confirm the idea, that limestone, in a pulverized state, is equally fertilizing as gypsum.

The subject is important to the farming interest, and certainly merits further investigation. It would not be difficult to erect machinery which would crush the hardest limestone and prepare it for grinding in a common plaster mill.

Mr. Moore's limestone formation is on a hill about five miles north east of this village. In digging his well, the first three feet was common soil; the next three feet shelly, thin limestone, (this is a common covering of beds of gypsum); from six feet from the top to twenty feet, which is as far as he has dug, it has the appearance at a short distance of blue clay; it is brittle, and easily dug up with a pick, and consists of fossils in vast abundance, which would seem to have been interspersed originally with blue earth, which became indurated, and finally limestone. Among the specimens is a large piece of the blue clay limestone. Almost precisely similar formations have been found on several farms. I have never seen any thing similar to it, but am not enough of a geologist to describe it more minutely—one thing is certain, which seems to comprise the main chance, it is equal to the best plaster in its fertilizing qualities.

Geneseo, June 1, 1836.

6. *A few Observations on the Reply of Professor Shepard,* which was published in this Journal, Vol. XXVII, No. II. January, 1835.—Our College of Mines did not receive number 2, of the American Journal until the first of this month, and I could not, of course, make an answer earlier.—I *omitted,* indeed, *giving the reader a fair opportunity of judging of the matter through the aid of an example,* in order to leave the choice to Professor Shepard, and he could not choose a worse one; I know not any rutile which is fine granular or impalpable: even Mohs only says that it is granular of various sizes. Now any small fragment whatever breaks under the blow of a hammer into square prisms, as I have elsewhere observed. Moreover, Breithaupt (a good authority, in my opinion) says in the fourth volume of his German manual of Mineralogy, 255th page, that *its fragments not very*

acute become regular prismatic by cleavage. Rutile does not therefore belong to the third class of Professor Shepard, or the uncleavable class; and he might have spared the student the trouble of searching it among seventy two species, sixty four of the 314th page and twelve of the following one, by absolutely excluding the rutile from the list of his third class. The same will be more or less applicable to the minerals of lamellar, columnar if not impalpable, and large granular composition; and thus the list of his third *indexlike* class would be much curtailed, and consequently, the first order of the same class. What a chaotic order, which comprehends all the solid minerals! Yet with all this tedious labor of the student, he cannot be finally certain, whether it is *rutile* or *ostranite,* if he does not appeal to the rarity of the latter, which is unknown to him. I hear Professor Shepard reply: *The color of the ostranite is clove brown, not liver brown, and the fracture and lustre are quite different.* Indeed; but these are not *natural properties* for Professor Shepard. He is therefore at a loss to understand how the large granular, the fine granular or compact, and the crystallized galena are, *per se, for the student, toto cœlo different* in their habit, and he very victoriously affirms at the 318th page, *that minerals not differing in their natural properties are identical,* so that the different lustre, streak, fracture and color of the three galenas are not natural properties. If they are not in the present case to distinguish two species of galena, the *common* and the *compact,* why should they in the other distinguish the genus *ostranite* from the genus *rutile?*

In my logic I do not see that *the frequent division of the species is a consequence* (page 317) *of the necessity of providing means for the determination of imperfect minerals.* Mohs himself, though invested with a triple coat of mail from Kant, has never drawn such a conclusion. Professor Shepard must at least confess, that he triplicates the number of the species. Well, *he makes the hospital, but he made the poor.*

I never talked of *any confusion* (page 320) *to be experienced in the determination of the leucite from the fact that it has a dodecahedral cleavage;* I only said, that for the student's sake, in the system of Professor Shepard himself, the leucite, which always occurs trapezohedral, should have been put in a new order, *the trapezohedron,* like *the octahedron,* the *rhombic dodecahedron,* together with the analcime, and the garnet, the perfect trapezohedron of which is as common here in *Pachuca*, *Zimapan*, &c. as in Cornwall; and the quartz, which never occurs as rhombohedron, should have been placed in the order of the *regular hexagonal prisms* for the sake, I repeat, of the student per se.

I did not allude (nor could I do so if in my right senses) to the law of *isomorphism* of Mitscherlich, when I said that it is to be inferred from his observations on biphosphate of soda, sulphur, carbonate of lime in calcareous spar and in arragonite, and arsenious acid and probably metallic arsenic also, that *elementary and compound bodies are capable of assuming two distinct crystalline forms.* Professor Shepard may find it in Turner's Chemistry, page 688.

The European sulphuret of manganese is the same species, or genus, as the Mexican; the 17 per cent of sulphur in the European was a mistake; Arfwedson has recently found 37: consequently, the same species, or better to say, genus, crystallizes in two different systems, the hexahedron and the rhombohedron.

I am sorry for my inapplicability of the 70th, 77th and 78th pages, for broken or imperfect crystals, (page 323.) The fact is, that it is said in the 70th page, *if we arrive at the knowledge of the lateral faces of a prism, we possess independently of the cleavage, means for determining the base, whether it be horizontal or oblique.* Now looking for these means, I found at the 76th page a remark thus commencing: *In consequence of the irregularity of crystals,* when it is said, page 78, *let us suppose a crystal to be contained within a series of vertical planes, and to be terminated, not by a horizontal plane, but by a single oblique plane, it will belong to the oblique rhombic prism, &c.* My crystal is now broken at the extremities, so that no terminal plane, either horizontal or oblique, is to be discovered; and *it is clear that from one or two lateral faces or vertical cleavages, no regular solid must result.* What is to be done? The means, in spite of his copiousness, are no where to be found in Professor Shepard's Mineralogy.

I know pretty well, that *the shorter the character,* (page 324,) *the greater the facility and certainty it will afford in the distinction.* Very true by essential characters; but how many such are to be found in the mineral kingdom? Even the crystals cannot be such; since, according to Professor Breithaupt, the prisms of the genus *pyroxene* vary no less than two degrees, and those of the genus *amphibole* nearly five.

I pass over the other remarks, because I see that, unhappily, we do not understand one another, so that I can say, with more reason than Rousseau to the archbishop of Paris, *that we have not a common language.* (Signed) A. Del Rio.

Mexico, December 15, 1835.

7. *Notice of a large crystal of Columbite;* in a letter to the editor, from Prof. Johnston, dated Wesleyan University, Middletown, April 27, 1836.—It is generally known, that a new deposit of columbite was discovered a little more than a year since, at the feldspar quarry about three miles from this city. Mr. Shepard says in his Mineralogy, (Vol. II. p. 323) "the crystals [found at this locality] are occasionally distinguished for their regularity and high degree of lustre, and are generally of unusual dimensions for the species. One of these (he continues) weighs three or four ounces."

"Associated with the crystals of columbite, are apatite, uranite, and albite."

Below, I have attempted to give a description, accompanied by a figure, of a crystalline mass of columbite, obtained at this locality some time the last summer, by Mr. P. E. Hubbard, the present proprietor of the quarry. It is probably of much larger dimensions than has before been seen; weighing 6 lb. 12 oz. avoirdupois. The whole mass, of which this is the largest piece, I am informed, weighed 14 pounds. It had fallen into several pieces, however, when discovered by the workmen.

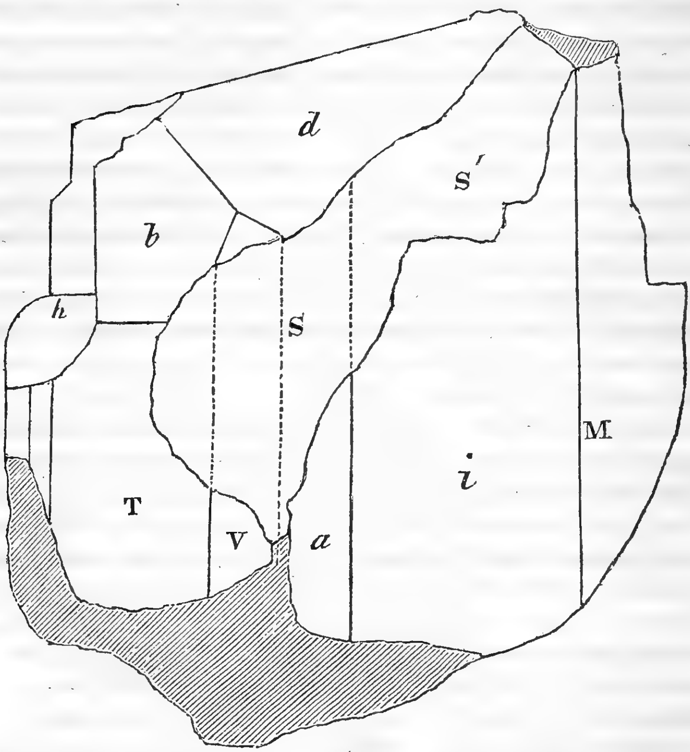
This piece, though it is considerably irregular, as is always to be expected in such overgrown crystals of any species, yet distinctly shows itself to be a part of a regular crystal. There are attached to it, or rather imbedded in it, several pieces of feldspar and quartz; and several of the fractured surfaces are coated with minute crystalline masses of uranite. The specific gravity of the whole is 5.4.

The figure below is one half the natural size, and is lettered as far as practicable, to correspond with Mr. Shepard's figure in his appendix, which was drawn from a specimen from this locality. As the faces are not perfectly smooth, and the angles were necessarily measured with the common goniometer, the results of course cannot be considered perfectly accurate. It will be seen, however, that they correspond very nearly with those of the specimen measured by Mr. Shepard.

The part S S′ has been removed by a fracture. At *h* too there is a defect, but the faces appear to be crystalline faces.

Inclination of the faces—

M on T,	- - - - -	90°	0′
T on V,	- - - - -	157	0
T on *a*,	- - - - -	128	30
T on *i*,	- - - - -	113	30
V on *a*,	- - - - -	150	00
a on *i*,	- - - - -	163	00
M on *a*,	- - - - -	137	00
b on *d*,	- - - - -	157	00
T on *b*,	- - - - -	149	00



8. *New species of* Argulus.—At the session of the Yale Natural History Society, on the evening of June 2d, 1836, a communication was presented by Messrs. J. D. Dana and E. C. Herrick, comprising a detailed description of a new parasitic crustaceous ani-

mal, which they call the *Argulus Catostomi*, found within the opercula of the fish here called a sucker.* The fish were taken during April and May from Mill river, just below the falls at Whitneyville. The animal is undoubtedly congeneric with the *Argulus foliaceus*, described at large by Jurine (in the *Annales du Museum d'Histoire Naturelle* for 1806,) which is hitherto the only species known. The following particulars of the description are given to show the difference between these two species.

The length of the *A. foliaceus* is about two lines and a half; that of this new animal is often six lines. Of the former, the shell is oval; in this the breadth exceeds the length.

The thighs of the prehensile legs of the *A. foliaceus* are each furnished with four curved spines :—in this, the posterior margin of the thigh is occupied by three broad and flat teeth, with interstices between them about equal to half their medium width. These teeth are irregularly quadrilateral with rounded angles.

In this, the outer pinnula of the first pair of natatory legs is twice articulated near its extremity; and the marginal portions of the inferior surface of the shell, (the clypeus excepted,) are thickly set with recurved spines ;—characters of importance, which are not stated in regard to *A. foliaceus*, and of which it is therefore presumed to be destitute.

The *A. foliaceus* is found attached to the surface of the body of various fish,—this has hitherto been detected within the gill covers only.

Many other points of difference were indicated, especially in the organs of manducation, portions of which Jurine erroneously considered the heart; but as these particulars cannot be clearly shewn without figures, they are not here given. Full details, with illustrations, may be expected in the memoir, which may appear in the October number of this Journal.

9. *Annual Report of the Regents of the University of the State of New York; made to the Legislature, Feb.* 29, 1836. 245 pp. 8vo. Albany, 1836.—The State of New York is distinguished for its efforts in the extension of education and the promotion of science, of which no stronger evidence can be adduced than the annual Report of the Regents of the University. The commence-

* The fish appears to be the *Catostomus Bostoniensis* of Le Sueur.

ment of this volume for the past year is occupied with the reports of the various colleges of the State, and also of the several public academies, giving an account of the number of students, subjects of study pursued, class or text books used, peculiarities in the instruction at particular academies, &c. &c. The academies thus derive mutual benefit from each other's experience.

We next open to the meteorological reports from these academies, which it will be observed are under the direction of the State, and as yet form the only instance in our country in which the advancement of meteorological science has been taken under the superintendence of government. These reports occupy sixty pages and contain accounts of the mean temperature for each month, highest and lowest degree of the thermometer, winds, weather, and observations with the rain gage at the several places where the academies are located. Recapitulatory tables follow, containing the general results deduced from the observations of this year and the preceding. Occurrences of any remarkable atmospheric phenomena, as auroras, halos, parhelia, meteors, &c. are next noticed.

The remaining part of the volume is occupied with an article of sixty pages, on auroras, halos and other atmospheric phenomena, by Benjamin F. Joslin, M. D., Prof. Nat. Phil. in Union College, Schenectady, in which the author proposes a theory to account for these unsatisfactorily explained occurrences. This article is valuable because of the facts it contains, and is of some considerable interest from the novelty of the theory it presents. After an enumeration of the results of his observations, Prof. Joslin draws the following general conclusions:

That the temperature of the air is falling, and the atmospheric pressure increasing, on the day in which an aurora appears:

That generally after an aurora, the atmospheric pressure falls, the temperature rises, and water either in the form of snow or rain, falls within two days:

That the air at the earth's surface, if not saturated with moisture at the time of an aurora, is much nearer than usual the point of saturation.

He next proceeds to show the *connection between the Aurora Borealis and the crystallizations of the vapors of the atmosphere.* The following are the principal steps in the argument.

The production of light by crystallization is a common occurrence and has been observed even during the congelation of water. If then crystallization of aqueous vapor actually takes place in the upper regions of the atmosphere, it is *possible* that light should be produced. But on the nights of the auroral exhibitions the air is unusually moist, and from its coolness the line of perpetual congelation is, for the season, unusually low. It is therefore probable that the vapor of the atmosphere is in fact undergoing continual crystallizations as it rises above the line of congelation. Such then is the origin of the light. Other arguments are adduced on this point which we have not now time to mention. Prof. J. next supposes that the molecules of crystals have a peculiar kind of polarity which governs them in their crystalline arrangements. If so, they would be acted on mutually by their own polarities, and also by the magnetism of the earth ; and thus would be produced the columns, waving from the unsteady nature of the atmosphere and the continual reproduction of light from the crystallization of new quantities of vapor.

This theory supposes the aurora to be nearer to the earth than facts seem to allow; and on this ground principally rests its improbability. It appears however to be deserving of more attention than that which ascribes it to reflections from polar ice, or that attributing it to the existence, in the upper regions, of *magnetic particles of that unvaporizable substance, iron.*

10. *Third Geological Report to the* 21*st General Assembly of the State of Tennessee, made Oct.* 1835. By Prof. G. Troost, M. D. 32 pp. 8vo. Nashville, 1835.—The following passages from this Report contain the result of Prof. Troost's investigations respecting the extent of the coal formations of the State of Tennessee.

"I have ascertained that the places in which coal may be expected, belong exclusively and entirely to that group of mountains which are known by the name of *Cumberland Mountain*, and are composed of Walden's ridge, Crab-orchard mountain, Brimstone mountain, and some other subordinate ridges of the same system.

"Commencing with the most southern extremity of our coal-fields, we find that coal first makes its appearance in Mount Sano, east of Huntsville, Alabama, where it crops out in several places. This Mount Sano is a ramification of the Cumberland mountain, which is there divided or terminates in several ridges running generally in a north and south direction. The coal crops out in several of these ridges, which join and form the main Cumberland mountain near the line which separates Tennessee from Alabama. The first out-crop in the main Cumberland mountain is near Battle creek, in Tennessee, ten or twelve miles southwest from Jasper, Marion county. Several out-crops of coal are towards the west and northwest from Jasper. The coal seems there to be deposited in horizontal strata of great extent, and it is, therefore, probable, that every where there in the mountain, coal may be found by boring. In Walden's ridge, on the eastern ridge of the Sequatchee valley, coal is seen. A few miles east from Pikeville, in Bledsoe and Rhea counties, are several out-crops of coal, some of which are opened and furnish the coal for several of the blacksmiths' shops of Washington, Pikeville and the surrounding country. Continuing on Walden's ridge from these coal banks in a direction from southwest to northeast, we find several beds, one lately opened by Mr. Gillenwater, and another near the turnpike road from Kingston to the Crab-orchard, belonging to Mr. J. Kimbrow. They are worked at present, and the coal transported to New Orleans by boats, which are loaded in the Tennessee river, which is only three miles from the coal bank.

"Not only are the out-crops found along the eastern slopes of the mountain, as I have described them, running from Huntsville, Alabama, to the above mentioned turnpike, (and it is probable that they may continue to the Cumberland river,) but they extend also towards the west. Travelling from the above mentioned Kimbrow's coal mine in a western direction, we find abundance of out-crops, as opposite Mr. Burke's on the Crab-orchard,—we find it still more westerly, or rather north westerly, near the head of the Calf-killer creek, on the north western declivity of the mountain in White county, where the liquid bitumen (*petroleum*) oozes out of the rocks. Continuing northward, the coal strata crop out in several places. In Fentress county several pits have been opened by General Rodgers, who sent the coal down the Obey and Cumberland rivers.

"Near the northern limit of the State, the breadth of the coal formation seems to be the greatest; it comprises here part of Overton, the whole of Fentress, Campbell and part of Claiborne counties. Besides the above named counties, coal may be found in White, Morgan, Anderson, Roane, Warren, Bledsoe, Rhea, Hamilton, Marion and Franklin counties."

A map accompanies the Report, giving the limits of the coal formation of this State.

11. *Report on the new map of Maryland*, 1835.—The Geological survey of the State of Maryland has been undertaken in connection with a Topographical survey of the same. The above is the second report to the government of the State on this subject. The first 34 pages are occupied by the report of the State engineer, Mr. J. H. Alexander; the remainder, about 50 pages, contain the geological observations of Prof. Ducatel. The examinations are as yet but partially completed: "twelve out of the nineteen counties of the State have been visited, six have been thoroughly examined, at least as far as the purposes of the survey require; two are nearly completed; and in the four remaining, examinations that will tend to facilitate ulterior researches have been commenced." In a report made at this period of the work, we are not to expect those general considerations on the relative situation of strata, their nature and contents, which are of the most importance to science. It is principally occupied with the *economical* geology of the country examined.

We look with some considerable interest for the conclusion of this survey, and the final and general report. It embraces a considerable portion of the tertiary formations, which abound in organic remains, a perfect acquaintance with which will tend to a more complete elucidation of the relation of American and European strata.

Considering the rapid extension of that spirit for geological investigations which is manifesting itself throughout the country, we may reasonably expect that hardly a score of years will elapse before the whole of the United States will have been examined by geologists. Already one survey has been completed, and the survey of eight other states has been commenced or projected. Maine has made her first appropriation of $5,000. New York is about to devote $26,000 annually for four years, and is engaged in immediate preparations for the commencement of the survey. Dr. J. G. Percival and Prof. C. U. Shepard have made their first report on the Geology and Mineralogy of Connecticut, and a new appropriation of $2,000 has been made by the Legislature for the present season. The survey of New Jersey is soon to be undertaken. That of Maryland has already been alluded to. Prof. Rogers, of Pennsylvania, has been appointed the geologist for Pennsylvania, and Prof. Rogers, of the University of Virginia, is already engaged in that of Virginia. Finally, the survey of Tennessee under Prof. Troost, is far advanced. This list will probably be much extended in the course of the coming year.

It is to be hoped that liberal appropriations of both *time* and *money* may be made for these surveys by the states that undertake them. Otherwise, instead of being economically advantageous, they will be comparatively useless to the community, and more so to science.

12. *Remarks on the Geological features of Ohio, and some of the desiderata which might be supplied by a Geological survey of the State.* By John L. Riddell, Adjunct Professor of Chemistry, &c. in Cincinnati College. 8vo. 12 pp. The nature of these few pages is fully expressed in the title. Among the interesting facts mentioned, the following is particularly worthy of note.

"De la Beche remarks, (Manual, p. 197,) that 'the relative age of the deposit, in which the remains of the *Mastodon maximus* are found, cannot be considered as very satisfactorily ascertained.' I am happy in being able to contribute any thing which may throw light on this matter. Three years since, the tusks and decayed bones of an unusually large mastodon, commonly called mammoth, were brought to light in ditching a quagmire, half a mile south of Massillon. I visited that locality on the ninth of last August. The remains were found in a boggy morass, of less than two acres area. Around it on three sides at least, are stationed rounded knolls, made up of sand, gravel, argillaceous earth, pebbles, nodules of iron ore, and water worn bowlders of grauwacke and primitive rocks. The same diluvium is continued beneath the quagmire, the proper soil of which is a black vegetable loam, approaching the nature of peat, and about three feet in thickness. The precise situation where the tusks were found, is near an old deer lick, just within the margin of the morass, and about two rods west of the Ohio Canal. They were enveloped in the loam and rested on the gravel and pebbles at the bottom. Upon searching, my companion Mr. Lindsly, found only a small friable piece of bone. This quagmire, indubitably belongs to the group of modern formations, the gradual production of similar quagmires being often observed in our own day. The North American mastodon became extinct then, in comparatively modern times; doubtless long since the distribution of the ancient diluvium."

13. *Note by Dr. S. P. Hildreth, on the Lias of the West.*—On a recent visit made to the iron ore deposits in Scioto and Lawrence counties, Ohio, I also examined that *great rock formation*, underlying the iron ores, and which is noted in the article on "the coal measures of the valley of the Ohio," as a lias. I had not at that time, seen the rock in place, but had examined hand specimens, and also possessed numerous fossil organic remains, imbedded in the rock, from its surface strata to a depth of one hundred and fifty feet. From the fact that the lower portions are argillaceous, and have a slaty structure, and the whole series being formed in thin layers or beds of one and two feet in thickness, in addition to which, one of the fossil remains being apparently of the saurian family, I was led to class it as a lias. But from careful personal inspection, I have changed that opinion, and now consider it one of the lower members of the coal series. A thick bed of coal is found near its inferior portion, lying beneath the main rock, and petroleum and carburetted hydrogen gas were freely discharged from the wells bored through it in search of salt water, and also subsequently in searching for coal. The upper strata consist of a fine grained white sandstone, and afford one of the most beautiful materials for architectural purposes and for sculpture, that I have ever seen, not inferior in splendor to the richest white marble.

I am still of the opinion, that the rock noted as a lias, in the highlands, on the heads of the Little Muskingum, and lying over the coal, is a true lias, or at least *an equivalent* deposit. The calcareous rock deposits, west and north of the coal measures in Ohio, I have little doubt will be found to be transition rocks, or the oldest secondary and underlying the coal.

Marietta, Ohio, May 26, 1836.

14. *Transactions of the Maryland Academy of Science and Literature*, 1836.—At the sitting Jan. 1st, the officers elected for the present year are—*P. Macauley*, M. D. President.

E. Geddings, M. D., *Mr. P. T. Tyson*, Vice Presidents.

D. Keener, M. D. Treasurer.

J. J. Cohen, M. D. Librarian.

Wm. R. Fisher, Secretary.

A. B. Cleavland, M. D., *Mr. Geo. W. Andrews*, *Geo. Frick*, M. D., *Mr. Jas. Green*, Curators.

Jan. 14.—A series of specimens of zinc ore, were received as a donation from Mr. J. Hitz, which were referred to Mr. P. T. Tyson for examination.—Dr. Geddings, delivered a lecture upon the respiration and circulation of fishes, which he illustrated, by drawings, preparations and dissections.—A committee was appointed to prepare a system of classification, by which the various departments of the sciences may be assigned to the members according to some fixed order.

Jan. 28.—The following system of classification, was reported by the committee, charged with that duty at the last meeting, and adopted, and the members were subsequently assigned to each class respectively, by the President. It is expected that every member will enrol himself, under one class at least, but he may co-operate with as many of the classes as inclination will permit. Each class is considered a standing committee, upon the particular department of Natural Science, whose title it bears, and all communications and specimens submitted to the Academy are to be referred to that class, having particular cognizance of the subject.

1st. Class.—Mathematics, astronomy and physics, the latter including natural philosophy and mechanics.

2nd. Class.—Chemistry.

3rd. Class.—Mineralogy and geology, including physical geography and the history and classification of fossil remains.

4th. Class.—Zoology, embracing the comparative anatomy and physiology of animals. This class is further divided into six sections, viz.

1. History and classification of mammalia.
2. " " birds.
3. " " reptiles.
4. " " fishes.
5. " " insects, including crustacea.
6. " " mollusca, including conchology and the zoophytic productions.

5th. Class.—Botany, including vegetable physiology.

The first named member of each class, is chairman of that class, and at present the chairmen are, 1st. class, B. H. Latrobe; 2d. do. J. T. Ducatel, M. D.; 3d. do. P. T. Tyson; 4th. do. E. Geddings, M. D.; 5th. do. W. E. A. Aikin, M. D.

A paper was received from T. Phillips Allen, corresponding member, residing in North Carolina, "on the chemical composition of the Prussian Blue of commerce," which was read and referred to the section of chemistry.

The following donations were received and referred to the different sections, viz. From Prof. Ducatel, a circular steel plate, marked with various colors, by the agency of galvanism, by M. Nobili of Florence, referred to section 1st.—From Dr. J. K. W. Dunbar and Dr. Edward A. Warrell, a cougar (*Felis concolor*,) prepared by them; referred to section 4th.—From Dr. P. Macauley, a fossil vegetable from the Virginia coal mines; referred to section 3d.

Feb. 4.—Donations were received and referred as follows. From Messrs. Alexander and Ducatel, a copy of their report on the new geographical and geological Map of the state, for 1835; deposited in the Library.—From Dr. Macauley, a series of minerals and fossils from the coal formation of Virginia, which having been examined and labelled, were referred to the curators to be placed in the cabinet.—From Dr. Dunbar, two Vols. of the Transactions of the American Philosophical society;" deposited in the Library.—From Mr. George W. Andrews, a copy of "the Manual of the Practical Naturalist;" deposited in the Library.—From Dr. Geddings and Dr. Dunbar, a Cougar; referred to section 4th.

A report on Mr. Allen's paper, read at last meeting, was received from Mr. W. R. Fisher of the section of chemistry, which was read and ordered to be filed with the original paper.—Prof. Ducatel, gave a lecture on the chemical phenomena, which occur during respiration, and offered some strictures on the explanation of that function, as described in "the treatise on Vegetable and Animal Physiology, by P. Mark Roget, M. D."

Feb. 11.—Dr. W. E. A. Aikin deposited in the Library, "Rennie's Elements of Mechanics."—Mr. W. R. Fisher, read a lecture on "the detection of Arsenic, in Medico-legal investigations," which was accompanied by experimental illustrations, of many of the processes and phenomena described.—Dr. T. Edmondson, Jr., reported a meteorological table for the month of Jan. 1836.—Mr. Jas. Green, of the first section, made a report on the steel disk, referred to that section at a former meeting. The experiments of Mr. Green, had afforded him various brilliant colors, resembling in beauty and intensity, those produced by the process of M. Nobili, although not arranged with the same precision, in regular forms. The process of the author of this species of galvanic etching, has not been disclosed, but there is a prospect, that the continuance of Mr. Green's investigations, will enable him, if not to discover a means identically the same, at least to furnish a mode by which analogous effects may be produced. The report was accompanied by illustrations, exhibiting the manner in which the results obtained, had been produced. Mr. Green was requested to continue the investigation.

Feb. 18.—Dr. Geddings presented to the Library, a copy of "Lea's observations on the genus 'Unio,' and a copy of "Genera Crustaceorum et Insectorum," by Latreille.

Prof. Ducatel submitted to the Academy, a series of experiments undertaken under the direction of Mr. Nicollet, with a view to determine the magnetic intensity at this meridian. He described the manner in which the experiments were performed, and exhibited one of the instruments employed. This consists of a highly sensitive magnetic needle, suspended in a glass vessel, by a single strand of silk, perfectly free from any twist, so that no motion may be produced by the torsion of the silk. The intensity was determined by marking the time, during which any given number of vibrations was accomplished, through a given arc of amplitude; the temperature, barometrical pressure and direction of the wind, being noted. The consideration and further examination of the subject was referred to section first.—Prof. Ducatel, also presented a memoir on a system of meteorological observations, prepared by Mr. Nicollet, and submitted by him to the secretary of war, being the basis of the observations now being made at the different military and naval stations of the United States, by order of the government.

Feb. 26.—Donations for the library, were received from Mr. Alexander and Dr. Geddings. From the former a copy of the "Histoire des Oiseaux d'Afrique," by Levaillant, two vols. folio; from the latter, copies of "Lehmann's Jungermannia," "Balt. Med. and Surg. Journal," and "North American Archives."—Mr. W. R. Fisher, read a short memoir on "Amalgams for electrical machines," and exhibited specimens of Mosaic gold, and Baron Krimayer's mercurial amalgam, prepared by the processes described in the paper; all referred to section first.—Mr. Green exhibited some electro-magnetic phenomena, and accompanied the experiments with an account of the fact first observed by Mr. Ritchie, that the length of time during which an electro-magnet retains its armature, after the connection is destroyed, depends upon the length of its arms.—A donation was received from Dr. Geddings, of a collection of Southern plants; referred to section 5th.

15. *Elements of Botany; by* Asa Gray, *M. D., Memb. of the Cæsar. Leopold.-Car. Nat. Curios., and of the Lyceum of Nat. Hist. of New York.*—A volume bearing the above title has just been presented to the public, by Dr. Asa Gray, of New York, a gentleman well known for his devotion to the study of plants. It treats extensively, of vegetable organography and physiology, and of the principles of classification. It is furnished with a dictionary of botanical terms, and is enriched with a large number of illustrative wood cuts. In an appendix are given directions for collecting and preserving plants, and a catalogue of the Natural Orders of the vegetable kinddom. It is the best work on the philosophy of Botany that has appeared in this country, and we trust that its merits will be appreciated by the numerous students of this science among us.

The volume contains 428 pages in duodecimo, and is published by G. & C. Carvill & Co. of New York City.

16. *New Medical Work by G. W. Carpenter.*—Mr. G. W. Carpenter, long advantageously known as an active and successful cultivator of pharmacy, has just published a "Family Medicine Chest Dispensatory." It contains a select catalogue of drugs, chemicals and family medicines, with the doses and properties of each article most approved of in domestic medicine, adapted and proportioned to the various ages of life. It contains also, directions for the treatment of accidents and disorders destructive to life, when a physician is not at hand, or until his assistance can be procured; it shows also the best immediate means to be adopted for obviating the effects of accidents, from excessive doses of medicines, or where poisons have been taken.—It contains in addition, a concise description of all the celebrated mineral springs of Great Britain and the United States, with observations on the various kinds of baths and bathing, &c. &c. We doubt not it will prove a useful volume.

17. *Notice of Dr. Hildreth's article on the coal deposits of the Ohio, &c.* in No. 1, Vol. xix. of this Journal; from No. 58 of Loudon's Magazine of Natural History.—This is, perhaps, the most important geological memoir that has been recently published, if we take into the account the amount and extent of the mineral treasures which it develops, and their immense value to the rapidly increasing population of the United States on the western side of the ranges of the Alleghany Mountains. The memoir occupies the whole

of the last number, (for Oct. 1835,) of that valuable work, "*The American Journal of Science and Arts, conducted by Professor Silliman, of Yale College, Connecticut.*" The extent of the region described comprises about five degrees of latitude, and as many in longitude, on each side of the river Ohio. The mineral treasures are precisely those which are most available to the comfort and prosperity of an industrious and enterprising people: rock salt, or rather brine springs, containing 15 per cent. of salt; coal in numerous beds, some many yards in thickness, and of an excellent quality; ironstone, of various qualities; lead ore, limestone, and millstone, &c. The coal of this vast region occurs in regular strata, scarcely troubled by the faults and dislocations which so much impede mining operations in other districts. In general the strata have very little inclination, and present great facilities for working. The salt springs are bored for, in many situations, to the depth of 600 feet, and even 900 feet. When the bed which contains the salt water is first pierced, there is generally a great rush of carburetted hydrogen gas emitted: it has, in some instances, been so violent as to blow up the boring rods. The memoir is accompanied by explanatory sections, which presents at one view the thickness and succession of the strata, and is, in every point, deserving of the attention of the geologist, and of all who feel an interest in studying the resources and future prospects of the western United States. The memoir occupies 154 pages, and is accompanied by 36 plates, besides a large map of the region."

Dr. Hildreth's memoir is highly commended in the London and Edinburgh Philosophical Magazine, and an abstract is given of its contents.

Dr. J. L. Comstock, M. D., has published a second edition of his "Outlines of Geology, &c. designed for the use of schools and general readers." The work is enlarged, particularly in facts relating to American Geology, and it is furnished with new illustrations, by wood cuts.

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ACKNOWLEDGMENTS TO FRIENDS, CORRESPONDENTS AND STRANGERS.

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

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

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

DOMESTIC.

The American Monthly Magazine for January, 1836. From John Stark.

The Western Journal of the Medical and Physical Sciences; edited by Dr. Daniel Drake and Dr. Wood. October, 1835.

Catalogues of Oberlin Institute. From Professors Cowles and Ingersoll.

Library of the Medical Sciences; edited by Dr. Isaac Hays. Parts 8 and 9.

Report on the subject of a mineralogical and geological survey of the state of Ohio.

Quarterly Anti-Slavery Magazine; edited by Elizur Wright, Jr.

First Annual Report of the directors of the New York and Erie Rail Road Company, 1835; with a separate map.

Report of Hon. John Q. Adams and others, on the Smithsonian Bequest, for founding a literary institution at Washington. Two copies; one from G. W. Chapin, M. C. and the other from F. Granger, M. C.

Report on the proposition to erect a monument to the memory of Capt. Nathan Hale. From A. T. Judson, M. C.

Report on the Chenango Canal, by and from the chief engineer, J. B. Jarvis.

Medical and Physical Researches, &c. by R. Harlan, M. D. F. L. S. Lond. &c. &c. Large 8vo. pp. 658; with numerous plates.

Report on a geological survey of the state of New York. Jan. 1836. From the Secretary, J. A. Dix.

The same; from —— Seymour, Esq. Troy.

Common School Assistant, Vol. I. No. 1. Edited by J. O. Taylor.

"Maclure's opinions" on various subjects. New Harmony, Indiana. For the American Geological Society, from the author.

Biographical Sketch of the late Thomas Say, by Benjamin H. Coates, M. D. Philadelphia, 1835. From the Acad. Nat. Sci.

Supplement to the Farmer's Register. Second edition. James Wadsworth, Esq.

Report of the progress of the Pennsylvania Lyceum.

Report of the Secretary of State, in relation to a geological survey of the state. From Geo. W. Paterson, Esq.

A Geological Ramble; by John L. Riddle. From the author.

Fourteenth Annual Report of the Board of Canal Commissioners to the 34th General Assembly of the state of Ohio.

Remarks of the Hon. H. L. Pinckney, on the abolition of slavery, House of Representatives, Feb. 8, 1836. Anon.

Third Annual Report of the Seamen's Aid Soc. Boston. Anon.

Third Geological Report to the 21st General Assembly of the State of Tennessee. Made Oct. 1835, by G. Troost. From the author.

Western Monthly Magazine, Vol. V, No. 2. From Wm. Wood, M. D.

Brief View of the American Bible Society. Anon.

Dr. Beecher's Address before the Union Literary Soc. of Miami University. From the author.

The Elements of Geometry. New York, 1836. Anon.

Speech of Mr. Evans, of Maine, on the failure of the bill making appropriations for fortifications, at the last session of Congress. Delivered in the House of Representatives, Jan. 28, 1836. Washington. Anon.

Cotting's Geological Text-Book. Taunton, 1836. From Jos. Dixon, Esq.

Catalogue of the Oberlin Collegiate Institute, 1835–6. Anon.

The Year-Book; by M. Conant, Esq. 1836. From the author.

Catalogue of the Phenogamous Plants of Columbia, S. C. From Prof. Gibbes.

Report of the Board of Managers of the Lehigh Coal and Navigation Company, Jan. 11, 1836.

William Jackson's Catalogue of English Books, 53 Cedar Street, New York.

Catalogue of the Library of the Mechanics' Institute, &c. New York. From Dr. L. D. Gale.

Constitution of the Mechanics' Institute of New York—the same.

First Annual Fair of the Mechanics' Institute of New York—the same.

Advocate of Moral Reform, Nos. 13, 14, 15.

The Farmer's Register, by Edmund Ruffin, ap. 1835.

The Cultivator, Vol. II. Albany. From the Editor, J. Buel, Esq.

Trilobites, encrinital stems, terebratulæ, corals, &c. fossilized in sandstone, from Mauch Chunk, Pennsylvania. From Ch. Barnard, Civil Engineer.

Monography of the Family Unionidæ of North America, Nos. 2, 3, and 4; by T. A. Conrad. Philadelphia, 1836. Two copies—from the author.

FOREIGN.

Transactions of the Society of Arts, &c. London, Vol. 50, Part 1. From the Society.

Prospectus of Professor Agassiz's Recherches sur les Poissons Fossiles—many copies.

Instruction sur la Manière de faire des Observations Météorologiques; par P. C. Morin. From the author.

Observations on Chronometers, from the Appendix of Capt. Sir John Ross. From Messrs. Parkinson & Frodsham, London.

Neues Jahrbuch für Mineralogie, &c. of Professors Leonhard and Bronn; 3 Heft, 1835. From the editors.

Prospectus of Memoires de la Société des Sciences Naturelles de Neuchâtel, premier volume, in quarto, avec planches.

Mémoire sur les Encombrements des Ports de Mer; par P. E. Morin. Paris. From the author.

Rapport sur les Poissons Fossiles, decouverts en Angleterre; par L'Agassiz. From the author.

Brighton Gazette and Lewes Observer, Nov. 19, 1835. G. M.

L'Institut, Journal Général des Sociétés et travaux scientifiques de la France et de l'étranger, for July, August, and Sept. 1835.

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